Business Literacy and Development: Evidence from a Randomized Controlled Trial in Rural Mexico

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I. Introduction

Self-employed, nonagricultural workers make up about 45% 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). For that reason, many governments and nongovernmental organizations (NGOs) promote the use of business-training programs to help

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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 reached more than 4.5 million potential and existing entrepreneurs between 2003 and 2010 (van Lieshout, Sievers, and Aliyev 2012).

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

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

Women who were offered the training have larger profits, revenues, and household income, and they also serve more clients. Treatment induced an increase in the use of formal accounting techniques and an increase in the likelihood of formally registering with the government, which requires not only paying taxes but also allows firms to issue legal bills of sale. Treated firms were also able to reduce their costs and change the mix of products they sold: specifically, they increased the number of items sold, dropping higher-cost goods and adding lower-cost ones. Importantly, we collected several distinct measures of business outcomes, such as profits and revenues. The treatment effects are consistent across these measures, suggesting that estimated impacts are not simply arising because treatment induced more precise accounting of self-assessed profits and revenues. Furthermore, we collected two rounds of post-intervention data, at 1 year and 2.5 years postimplementation, and found that the effects of the treatment do not fade out in the medium run.

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

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

This model offers two testable implications: first, among 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 pretreatment profits, we find some support for the fact that that low-ability entrepreneurs are indeed more likely to quit their businesses as a result of the training and strong evidence that the largest positive effects are recorded among the best entrepreneurs.

This paper contributes to the growing literature on the effects of business-literacy training on firms' profitability. For example, empirical evidence is presented by Field, Jayachandran, and Pande (2010) in India; Karlan and Valdivia (2011) and Valdivia (2011) in Perú; Drexler, Fischer, and Schoar (2014) in the Dominican Republic; Berge, Bjorvatn, and Tungodden (2011) in Tanzania; Bruhn and Zia (2013) in Bosnia-Herzegovina; Giné and Mansuri (2014) in Pakistan; de Mel, McKenzie, and Woodruff (2014) in Sri Lanka; and Fairlie, Karlan, and Zinman (2012) in the United States.

At the same time, our intervention is distinct from this literature in several important dimensions. First, the pedagogy focuses on the practical application of the skills and topics in the entrepreneurs' own businesses. Second, compared with other training programs (McKenzie and Woodruff 2012), the course is relatively long and intensive, with a total of 48 hours of classes over more than

6 weeks. Third, our sample is rural, while most of the other study populations are urban. Fourth, the entrepreneurs in our sample do not receive any other treatment; for example, none are involved in microfinance or other targeted business interventions.[1] This last feature increases the external validity of our results and distinguishes them from other studies in this literature (e.g., Field, Jayachandran, and Pande 2010; Karlan and Valdivia 2011; Drexler, Fischer, and Schoar 2014).[2]

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

The remainder of this paper proceeds as follows: Section II describes the business-literacy training and our experimental design. Section III describes the data and the sample. Section IV presents the empirical methodology. Section V presents the results. Section VI provides the simple theoretical framework for the interpretation of the results, and Section VII concludes.

II. Description of the Business-Literacy Training and Experiment

A. The Business-Literacy Classes

In 2009, we partnered with Creating Resources for Empowerment in Action (CREA), an NGO, to develop and implement a business-literacy training program for small, female-headed firms in the retail or production sector.[3] CREA operates in small villages in the Mexican state of Zacatecas, a poor, high-altitude, dry agricultural region. Although there is good road access to all villages in which CREA operates, 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 4-hour classroom meetings per week and runs for 6 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 certificate of completion from CREA, the Institute for Women of Zacatecas (a government agency), and the Autonomous University of Zacatecas (the local university); weekly in-class

[1] Only 4.5% 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 among female entrepreneurs in Sri Lanka.

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

raffles for small prizes (e.g., a CREA hat or stationary supplies), 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. Because 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 the competition. The final module is a discussion of how to be an effective salesperson.

The content and teaching style of CREA's course are intentionally simplified 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 that 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 was modeled as follows: first, the main concepts were introduced, and then the concepts were applied to simple examples relevant to the participants' own businesses.[4]

B. *Experimental Design and Population of Study*

Our experimental design contained two stages. In the first, villages were randomized into either treatment or control, and in the second, entrepreneurs within treatment villages were randomized to receive or not receive an invitation to attend the classes.[5] This design allows us to estimate the direct effect of

[4] An in-class example and exercise can be seen in the appendix.

[5] Our randomization algorithm involved first choosing a seed group of potential treatment villages and then choosing 50% of entrepreneurs in each seed village as potential treated individuals. We repeated this assignment 10,000 times so as to minimize the (squared) sum of the distances of predicted profits between treated and control units. The approach we use is that of running a baseline regression of profits over a set of conditioning variables (number of workers, the age and sector of the enterprise, the replacement value of business capital, whether the entrepreneur states that she lacks business

the program, by comparing invitees in treatment villages to entrepreneurs in control villages, and the indirect effects, by comparing those not invited to attend classes in treatment villages to entrepreneurs in control villages. However, as described in the online appendix, the estimated indirect effects are indistinguishable from zero yet rather imprecisely measured, leaving us with little ability to draw meaningful insights from this aspect of the experiment.

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: (i) they had 100–500 female entrepreneurs, as identified by the 2005 Mexican census; (ii) they are within a 2-hour drive of the city of Zacatecas; and (iii) they contained fewer than 1,500 households (also identified by the 2005 Mexican census).[6] This selection process identified 25 villages. To accommodate our survey budget as well as CREA's institutional capacity, we randomly drew a sample of 17 villages from this set of 25 to be included in the study.

Within the chosen villages, we identified female entrepreneurs who produced and/or sold goods using 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 or her to introduce us to at least three knowledgable local women (the "seeds"). Second, we asked this group to list all the women in the village who (i) worked for themselves and (ii) sold a good. None of the local seed women were entrepreneurs themselves, and enumerators emphasized to the seed women the importance of identifying as close to a census of women entrepreneurs as possible. This process yielded about 50 entrepreneurs per village, to whom we applied a preintervention questionnaire between July and September of 2009.[7]

We did not have the resources to survey male entrepreneurs, which limits our ability to estimate the full indirect effects of treatment (spillover effects). However, our experience in these villages is that the majority of the goods 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 reported selling their goods outside of their own village, suggesting it is unlikely

skills, whether she is risk averse, her age, education, number of rooms in her home, and her score on the business skills exercise), which we then include in our analysis as controls.

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

[7] The difference between the 100–500 entrepreneurs identified in the census and the number of entrepreneurs identified in our sample is explained by the fact that we include neither farmers who do not retail their produce (the vast majority) nor those in the service sector.

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.

C. *Expected Effects of the Intervention*

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

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

III. Data and Sample

A. *Data*

Our data include an array of indicators of business performance, entrepreneurial ability, and socioeconomic characteristics. In addition to the preintervention survey, two waves of data were collected postintervention, approximately 18 months apart (the first between July and September of 2010 and the second between March and May of 2012). These multiple postintervention 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 microenterprises; intentionally, no connection was established with CREA or the intervention, and interviewers knew neither entrepreneurs' treatment status nor program participation decisions.

Our main measures of business performance include self-reports of profits, revenues, and the number of clients served over various stretches of time. As some of the women do not work/sell every day or at regular intervals, 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 that likely have less measurement error. Importantly, we also collected and analyzed self-reports of total household income at baseline and in the first posttreatment wave. We see at least two main advantages to collecting household income in a study like ours: first, such a measure does not suffer from firms' attrition (and closure) as it is recorded for the household rather than the firm; second, household income is potentially the relevant measure in terms of overall household welfare in the absence of consumption measures.[8]

Although 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 follow-up. We first asked the entrepreneur to list all the goods that she sold (up to a maximum of 14 items).[9] We then asked for the number of units sold for each good 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, these data represent an unbalanced panel at the goods level. As such, they contain three types of goods: new goods for sale, old goods no longer sold, and goods sold both pre- and postintervention. From this data, we calculate aggregate measures of the stock of goods an entrepreneur sold, including total revenue, total profit, the total number of goods sold, and the mean across all goods of both unit cost and price. These aggregate measures are useful because they capture optimizing decisions in terms of product stock, which could have been affected by the intervention. For example, a woman may learn that one product is losing money and drop that product; she may also decide to sell a new product with a larger profit margin. (The measures are also useful to help assuage concerns that the treatment simply teaches entrepreneurs to more accurately report their profits and costs, which is outlined in Sec. V.)

We also use the goods-specific data to examine how the product mix changes over time in response to the business training. Specifically, we examine treatment effects on revenues, profit, and mean unit cost among (i) the goods that the

[8] Bernhardt et al. (2019) show the importance of collecting household-level information on outcomes.
[9] Approximately 6% of the sample reported selling 14 goods; thus, 6% of the sample could have had more than 14 different goods for sale, information that we do not capture.

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 postintervention round (added goods). These outcomes help us understand what changed in the daily operations of the treated businesses and therefore allow us to look into some of the mechanisms.

Several other outcomes will give us more insight into how the intervention affects the performance of the business, including the number of employees (both paid and unpaid), the number of co-owners, the average number of hours worked per week by the owner, and whether the entrepreneur is registered with the Secretaría de Hacienda y Crédito Público (Secretary of Finance and Public Credit), the government agency in charge of collecting taxes and regulating business activity. We administered a simple exercise related to production and sales (see the appendix) to directly examine the effect of the treatment on business math knowledge; this same exercise was applied to both pre- and posttreatment. We observed whether each of the four sections scored correct and summed to create a total score. Furthermore, we asked the entrepreneurs how they kept accounts for their business—through personal notes or a formal accounting method—or whether they did not keep any accounts.

Additional pretreatment data include the owner's age, education, and asset ownership (e.g., type of dwellings and number of rooms), a measure of risk aversion; reservation wages, credit availability, and the cost of credit; the type of activity engaged in; the age of the business; and the replacement value of the firm's capital stock. Finally, in both postintervention surveys, we elicited the firm's survival by asking whether the entrepreneur still sells any goods and defined a firm as quitting accordingly.

B. *Sample and Summary Statistics*

Our sample includes 17 villages—seven treatment and 10 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 preintervention. The majority of firms (about 65%) 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 and other resale items comprise a little more than 25% of the sample; handicrafts and clothing sum up about 10%.

Table 1 contains pretreatment summary statistics by village type and treatment group. The randomization appears to have been successful in that the mean preintervention characteristics are for the most part indistinguishable across groups: five comparisons out of 48 are significantly different at the 10% level, although two out of 48 differ at the 5% level.

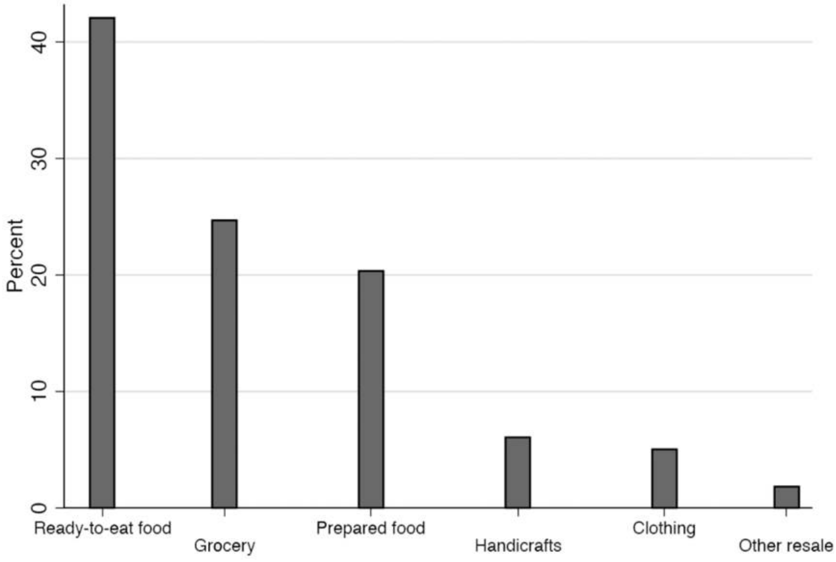

Figure 1. Sectors of microenterprise activity pretreatment.

These data paint a sobering picture of the economic lives of these entrepreneurs. Daily profits average around Mex$140 (approximately US$11).[10] Revenues are about four times the size of profits, and it is interesting to note that this is the same order of magnitude as found among firms in Sri Lanka by de Mel, McKenzie, and Woodruff (2009).

Business owners are on average 46 years old and have about 6 years of education. Approximately one-third have a temporary roof on their residence (e.g., thatch or cardboard), which is 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 US$570. Businesses are small: on average, there are 1.6 workers, including the owner, and employees work about only one-quarter of the hours the owner works (i.e., about 10 hours per week). About 60% of businesses have no workers other than the owner. The average age of a firm is about 7 years, again with large variation, and in particular, 25% of the firms are less than 1 year old with a median age of 4 years.

The women in our sample know how to make basic calculations but are less proficient at determining profits or optimally setting prices. For example, 93% said that they know how to make simple math calculations (not shown in the table), although the average score on the math exercise was 39% (less

[10] The dollar-peso exchange rate in 2008–9 was approximately Mex$13 to US$1.

TABLE 1

PRETREATMENT CHARACTERISTICS, BY TREATMENT GROUP

	Treatment Villages						
	Firms Offered Treatment (1)	Control Firms (2)	All Firms in Control Villages (3)	(1) = (2) *p*-Value (4)	(1) = (3) *p*-Value (5)	(2) = (3) *p*-Value (6)	Number of Observations (7)
Personal characteristics:							
Age	46.04	46.28	45.45	.830	.467	.484	869
	(.48)	(.96)	(.63)				
Years of education	5.96	6.05	6.08	.666	.743	.895	846
	(.32)	(.21)	(.15)				
Roof is made of temporary material	.33	.31	.32	.775	.947	.962	844
	(.09)	(.08)	(.07)				
Score on math exercise (% correct)	.39	.44	.48	.114	.134	.511	864
	(.04)	(.03)	(.04)				
Keeps formal business accounts	.01	.03	.04	.441	.092*	.537	873
	(.01)	(.01)	(.01)				
Weekly hours worked in enterprise	39.43	35.82	40.40	.196	.803	.088*	866
	(3.19)	(1.35)	(2.11)				
Household income, daily	158.71	173.24	182.96	.500	.508	.778	826
	(18.86)	(14.99)	(30.32)				
Business characteristics:							
Produces goods for sale	.62	.69	.66	.024**	.452	.628	875
	(.03)	(.03)	(.05)				
Last day's profit	132.24	145.54	158.52	.553	.458	.717	760
	(16.05)	(17.29)	(30.50)				
Last day's revenue	456.16	404.74	406.42	.341	.508	.976	840
	(55.14)	(28.09)	(48.34)				
Last day's number of clients	14.03	15.70	13.95	.488	.971	.469	808
	(1.47)	(1.88)	(1.41)				
Number of employees	.49	.64	.52	.138	.539	.255	874
	(.03)	(.09)	(.04)				
Weekly hours worked by employees	10.27	12.42	9.79	.341	.846	.205	872
	(2.26)	(1.79)	(.86)				
Age of business (years)	6.77	7.17	7.79	.496	.402	.596	874
	(.84)	(.77)	(.83)				
Replacement value of business capital	8,062.61	10,714.60	8,704.49	.031**	.688	.374	875
	(1,008.94)	(1,832.23)	(1,200.87)				
Registered with the government	.15	.20	.22	.242	.085*	.630	844
	(.03)	(.04)	(.03)				
Assigned observations	164	189	522				

Note. Sample includes all subjects interviewed in the baseline survey. Standard errors (in parentheses) are clustered at the village level; *p*-values in cols. 5 and 6 are calculated from *F*-tests of the equality of means with 15 degrees of freedom (see Sec. IV). All monetary variables are measured in Mexican pesos (Mex\$13 ≅ US\$1). Government registration is with the Secretaría de Hacienda y Crédito Público.

* $p < .10$.

** $p < .05$.

than two of the four questions answered correctly).[11] Less than 5% of entrepreneurs keep formal business accounts, and about only one-fifth of the sample is registered with the government.

C. 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 not to prescreen invitees on the basis of the stated desire to accept the classes. As such, among the 164 entrepreneurs who were offered the classes, about 35% (57 entrepreneurs) did not attend any classes. Among 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).

Table A1 (tables A1–A4 are available in the online appendix) compares the mean preintervention characteristics of entrepreneurs who attended classes and those who did not and shows that no variables are significantly different across groups at the 5% level. However, despite this lack of significant differences (partly driven by the small sample size), on average, attendees appear to be less successful entrepreneurs than nonattendees. For example, daily profits and revenues are about 50% higher for entrepreneurs who did not attend classes; similarly, nonattendess have larger business capital and household incomes. Again, such findings are consistent with the literature (see, e.g., de Mel, McKenzie, and Woodruff 2014; Drexler, Fischer, and Schoar 2014).

The effect of treatment (being offered the class) on the treated (class attendees) can be estimated by instrumenting attendance status (which is presumably endogenous) with treatment status (which is exogenous due to randomization). However, we instead focus our empirical analysis on the intention-to-treat (ITT) parameter for parsimony. In general, one can easily scale up the ITT 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 (.65), or a factor of about 1.53 (1/0.65).

D. Attrition

Some entrepreneurs attrited from our sample between the baseline and the first and second follow-up surveys; however, attrition rates do not vary significantly across treatment groups (on average). Specifically, at the time of the

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

first postintervention survey, sample attrition was 12.8% in the treatment compared with 15.3% in the control (p-value of the difference = .58). During the second follow-up survey, we were able to survey some of the attrited entrepreneurs from the first follow-up, although some new subjects attrited: relative to the baseline sample, attrition in the second follow-up was 16.5% in the control group compared with 18.3% in the treatment group (p-value = .77). Virtually, all the attrited entrepreneurs either moved out of the village or were unavailable on the day of the interview; only three subjects ever refused to participate.[12]

IV. Empirical Strategy

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

$$y_{it} = \alpha + \beta T_i + \delta \mathrm{Post}_t + \gamma(T_i \times \mathrm{Post}_t) + \lambda \mathrm{Wave2}_t + \mathbf{X}_i \Omega + \varepsilon_{it}, \quad (1)$$

where y is the outcome interest, T is an indicator for living in a treatment village, Post is an indicator for the postintervention period, Wave2 is an indicator for the first postintervention survey, $\mathbf{X}$ is a vector of preintervention business and demographic characteristics, and ε is an error term. Preintervention characteristics are included as covariates to increase precision, and we include only covariates that were used in the randomization algorithm; in the following, we demonstrate that results are robust to the exclusion of these controls.[13]

Several issues are of significance: first, the direct effect of the offer of treatment, or the ITT effect, is identified by γ when equation (1) is estimated on the sample of all entrepreneurs in control villages and entrepreneurs in the treatment villages who were offered the classes (this identification strategy is immune from within-village spillover effects). The indirect effect of the offer of treatment, or the indirect treatment effect (ITE), is identified by γ when equation (1) is estimated on the sample of all entrepreneurs in the control villages and entrepreneurs in the treatment villages who were not offered the classes. Discussion of the ITEs can be found in the appendix.

[12] Comparing entrepreneurs who ever attrited with those who did not reveals that, preintervention, attrited entrepreneurs have less education, have significantly lower revenues, employ fewer workers, and are less likely to produce goods rather than resell goods (see table A2); these relationships hold equally in both the treatment and control groups (results available on request).

[13] These preintervention 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.

Second, with two postintervention survey waves, we are able to estimate models that permit different treatment effects over time. However, as shown further down, estimated treatment effects do not differ significantly across the two postintervention survey waves; therefore, we pool the postintervention surveys together to increase statistical power and include an indicator for the first postintervention period (Wave2) to absorb any time-specific effects.

Finally, statistical inference is complicated by the small number of clusters (i.e., villages), implying that the standard (asymptotic) method for hypothesis testing may be incorrect. We therefore use the Donald and Lang (2007) adjustment for all tests of statistical significance, which entails calculating *p*-values from tests with degrees of freedom equal to the number of clusters minus the number of group constant variables (in our case, this is $17 - 2 = 15$ degrees of freedom).[14]

V. Results

A. The Effect of Business Training

We first explore the effect of business-literacy classes on firm survival. Column 1 of table 2 shows the ITT on quitting one's business is an insignificant 1.6 percentage points, suggesting the offer of classes did not differentially induce entrepreneurs to quit their business (on average). Quit rates overall (in both treatment and control groups), however, were nonnegligible: by the first and second follow-up surveys, 18.6% and 41.1% of the sample had stopped running their business, respectively.[15] Firm survival rates of this magnitude are typical for small businesses but pose problems in longitudinal studies of entrepreneurs as business-related outcomes are unobservable for those who quit; we note, however, that household income is immune from such an issue.[16] To account for potential selective quitting (and attrition) by treatment status, we estimate bounds on the treatment effects in columns 3 and 4 of table 2, using Lee's (2009) methodology for continuous variables and Manski's (1990) methodology for binary outcomes.

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

[15] Perhaps not surprisingly, there are significant differences between those who ever quit and those who did not (see table A3); e.g., compared to nonquitters, quitters were younger in age, worked fewer hours in their business, had fewer employees, and had been in business for less time. These relationships hold equally in both the treatment and control groups.

[16] For example, the 5-year survival rate for small businesses of similar age to our sample in the United States and other member countries of the Organisation for Economic Co-operation and Development is about 50%–70% (Bartelsman, Scarpetta, and Schivardi 2003).

TABLE 2

EFFECTS OF BUSINESS TRAINING ON MAIN BUSINESS OUTCOMES

	ITT Effect (1)	Number of Observations (2)	Lower Bound on ITT (3)	Upper Bound on ITT (4)	Number of Observations (5)	Survey Waves (6)
Quit her business	.016	1,836	−.152***	.168***	2,058	1–3
	(.032)		(.043)	(.032)		
Measures of profit and revenue:						
ln(last day's profit)	.213*	1,183	.144	.329***	1,177	1–3
	(.110)		(.123)	(.107)		
Standardized profits	.200**	1,322	.152*	.268**	1,317	1–3
	(.077)		(.079)	(.094)		
ln(last day's revenue)	.253**	1,357	.164	.358***	1,350	1–3
	(.115)		(.116)	(.118)		
Standardized revenue	.232***	1,421	.187**	.297***	1,415	1–3
	(.067)		(.071)	(.071)		
Other business outcomes:						
ln(number of clients last day)	.220*	1,312	.143	.335**	1,301	1–3
	(.121)		(.120)	(.121)		
ln(household income, daily)	.194*	1,080	.138	.314**	1,075	1, 2
	(.109)		(.106)	(.122)		
ln(number of goods for sale)	.204**	1,145	.108	.469***	1,118	1, 2
	(.089)		(.086)	(.097)		
ln(mean unit cost)	−.273*	907	−.292*	−.212	905	1, 2
	(.148)		(.152)	(.142)		
ln(mean unit price)	−.021	1,139	−.053	.047	1,135	1, 2
	(.085)		(.090)	(.097)		
Percentage correct on business practices exercise	.051	1,210	.008	.146**	1,180	1–3
	(.063)		(.062)	(.059)		
Uses formal accounting methods	.048*	1,432	−.190***	.262***	1,652	1–3
	(.023)		(.034)	(.060)		
Hours worked per week by owner	2.686	1,411	−1.107	3.935	1,403	1–3
	(2.639)		(2.800)	(2.724)		
Hours worked per week by employees	.756	1,143	−3.473	14.915**	1,081	1, 2
	(3.833)		(3.652)	(5.344)		
Number of employees	.091	1,419	−.002	.986***	1,308	1–3
	(.108)		(.109)	(.154)		
Registered with the government	.087***	1,399	−.155***	.298***	1,618	1–3
	(.029)		(.044)	(.048)		

Note. Values in cols. 1, 3, and 4 are means and standard errors (in parentheses). Samples include firms offered treatment in treatment villages and all firms in control villages. Coefficients are estimated by eq. (1), including an indicator for the first follow-up wave. Covariates include the preprogram 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 postintervention period independently and then estimating our difference-in-differences model with this trimmed data and the full preintervention sample. For binary outcomes, lower and upper bounds are calculated using Manski's methodology. Standard errors are clustered at the village level; *p*-values are calculated from *t*-tests with 15 degrees of freedom. ITT = intention to treat.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

We next explore the effect of treatment on profits and revenues. All the available measures of profit and revenue—from the last day, the last week, and the aggregate calculated from goods-specific last-day and last-month values—are self-reported and thus may be measured with error. As such, we normalize all individual measures of profit and revenue and calculate the mean standardized profit and revenue for each business (Kling, Liebman, and Katz 2007); we note that the use of standardized measures of profits also addresses concerns due to multiple-hypothesis testing across a series of outcomes (Romano and Wolf 2005). For parsimony, we present in our main analysis only results on (the logarithm of) the last day's profit and revenue as well as the standardized profit and revenue measures; estimates using the other available measures as outcomes are of similar magnitude and are included in table A4.

Column 1 of table 2 shows that the direct effect of the offer of classes is 22 log points (p-value $< .1$) on the last day's profit and 0.209 standard deviations (p-value $< .05$) on standardized profit. The ITTs on revenues are of a similar magnitude to profits: the last day's revenue increased by 25.3 log points (p-value $< .05$) and standardized revenue increased by 0.209 standard deviations (p-value $< .01$). These effects are quite large, yet comparable to the impact of other business-literacy courses in the literature (McKenzie and Woodruff 2012; Bruhn, Karlan, and Schoar 2013).

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

The remainder of table 2 presents a complementary set of outcomes that help us understand the mechanisms through which profits and revenues were affected by the program. We first find a positive and significant effect on the number of clients on the last working day (22.4 log points, or about four extra clients) and the number of goods for sale (20 log points, or approximately two extra goods for sale). We find this result important not only because it is bound to be measured with less error than profits and revenues but also because it suggests that part of the overall effects on profits and revenues arises from larger quantities sold.

Household income—which was collected from all subjects regardless of whether they continued their business and is therefore not subject to missing information in cases where a business closed down—increased significantly by

19.4 log points under the program. (Recall that household income was collected only in survey waves 1 and 2.) The similar magnitude of the increase in profits and household income suggests that the program did not have a meaningful income effect on overall household labor supply (e.g., spouses or children of entrepreneurs do not appear to reduce their labor supply in response to the increase in entrepreneurial profits).

It appears that the observed increase in profits is being driven by reduced costs and increased quantities sold rather than increased prices: unit costs fall by 27 log points (p-value $< .1$), and unit prices were not meaningfully impacted. Also, firms are neither changing the number of employees nor the hours worked by either the owner or employees.

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

Although we focus mainly on the pooled effect of the treatment across survey waves, ITTs are of very similar magnitude in both the short run (1 year postintervention) and the medium run (2.5 years after the intervention). This can be seen in table 3, which contains by-wave ITTs estimated from a version of equation (1) that includes indicators for each postintervention wave and their interaction with the treatment indicator. (Note that this table includes only variables that we observed in both posttreatment waves.) In general, point estimates for ITTs in wave 3 have larger standard errors than those in wave 2 (in part because of the smaller sample size in wave 3), but we cannot reject the hypothesis that the ITTs are equal across waves. This finding is important in that the one-time intervention appears to have long-lasting positive effects, which do not seem to decay 2.5 years after the classroom training took place.

B. Robustness and Validity of the Main Results

These treatment effects are robust to alternative specifications, as shown in table 4. First, column 1 shows that excluding preprogram covariates does not change point estimates meaningfully but increases standard errors as expected.

Second, we further explore the concern that differential quitting or attrition by treatment status is biasing our results. One important piece of evidence suggesting that our results are not being driven by selective observability of

TABLE 3
EFFECTS OF BUSINESS TRAINING BY WAVE

	ITT Wave 2	ITT Wave 3	p-Value (H_0: ITT Wave 2 = ITT Wave 3)	Number of Observations
Measures of profit and revenue:				
ln(last day's profit)	.216*	.208	.971	1,183
	(.057)	(.345)		
Standardized profits	.200**	.198	.986	1,322
	(.047)	(.160)		
ln(last day's revenue)	.240**	.278	.814	1,357
	(.027)	(.160)		
Standardized revenue	.222***	.249*	.835	1,421
	(.006)	(.053)		
Other business outcomes:				
ln(number of clients last day)	.237**	.189	.779	1,312
	(.044)	(.355)		
Percentage correct on business practices exercise	.037	.127*	.122	1,210
	(.572)	(.090)		
Uses formal accounting methods	.030	.078	.477	1,432
	(.124)	(.214)		
Hours worked per week by owner	4.066**	.237	.322	1,411
	(.037)	(.961)		
Number of employees	.178	−.058	.204	1,419
	(.193)	(.692)		
Registered with the government	.073*	.110**	.520	1,399
	(.054)	(.029)		

Note. Samples include firms offered treatment in treatment villages and all firms in control villages. Outcomes include only those observed in all three survey waves. Covariates are included. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. Standard errors (in parentheses) are clustered at the village level; p-values are calculated from t-tests with 15 degrees of freedom. ITT = intention to treat.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

business outcomes is that our measure of household income is observable for all entrepreneurs, regardless of quit status, and we see in table 2 that household income increased with a similar magnitude as did profits. Furthermore, we present results in column 2 of table 4 that invoke a very strong assumption that can help bound estimates from below: that is, firms that quit or attrited had zero profits and revenues, served no clients, worked no hours, did not use formal accounting methods, and were not registered with the government. Applying this assumption (and using 0.1 clients and Mex$1 in profits and revenues in logarithmic specifications), we see that treatment effects are of similar magnitude and less precisely estimated but still suggest that differential quitting or attrition is not driving the results. For example, ITTs on standardized profit and revenue (those measured with the least error) are still marginally distinguishable from zero with p-values of .103 and .07, respectively.

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

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

Are classes simply teaching entrepreneurs to more accurately report business outcomes? One concern is that the intervention taught entrepreneurs to accurately measure business outcomes but did not change actual outcomes themselves. Three pieces of evidence suggest that this is not the case.

First, it is unlikely that the treatment affected how entrepreneurs measure revenue, the number of clients served, or the number of products sold. Second, we elicit unit costs directly in the good-by-good analysis and use those to calculate aggregate profits. This measure is immune to mislabeling of household expenses as business costs, which would tend to make business profits artificially low. Indeed, table A4 shows that treatment effects on the goods-specific profits are of similar magnitude to those on reported overall profits, suggesting that there are real program effects on profits.

Third, the good-by-good analysis provides a set of alternative measures of business performance, which help alleviate concerns that nonclassical measurement error, or systematic downward bias in reporting, is driving our results.

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

TABLE 4
ROBUSTNESS OF THE MAIN EFFECTS

	Excluding Pretreatment Controls		Imputed Values for Quitters and Attriters		Including Control Firms in Treatment Villages		
	ITT	Number of Observations	ITT	Number of Observations	ITT	Number of Observations	
	(1)		(2)		(3)		Survey Waves
Quit her business	.017	1,836			.020	2,348	1–3
	(.032)				(.027)		
Measures of profit and revenue:							
ln(last day's profit)	.163	1,183	.109	1,637	.254***	1,531	1–3
	(.102)		(.109)		(.083)		
Standardized profit	.159*	1,322	.128	1,713	.209***	1,699	1–3
	(.080)		(.075)		(.065)		
ln(last day's revenue)	.226*	1,357	.101	1,779	.252**	1,699	1–3
	(.115)		(.124)		(.106)		
Standardized revenue	.194**	1,421	.150*	1,812	.209***	1,828	1–3
	(.076)		(.078)		(.057)		
Other business outcomes:							
ln(number of clients last day)	.251*	1,312	.095	1,728	.204*	1,690	1–3
	(.128)		(.114)		(.110)		
ln(household income, daily)	.160	1,080			.192*	1,403	1, 2
	(.105)				(.109)		

ln(number of goods for sale)	.179*	1,145			.195**	1,474	1, 2
	(.102)				(.079)		
ln(mean unit cost)	−.394*	907			−.320**	1,174	1, 2
	(.194)				(.113)		
ln(mean unit price)	−.085	1,139			−.050	1,467	1, 2
	(.087)				(.086)		
Percentage correct on business practices exercise	.037	1,210			.054	1,551	1–3
	(.065)				(.052)		
Uses formal accounting methods	.045*	1,432	.040**	1,825	.032	1,844	1–3
	(.021)		(.015)		(.022)		
Hours worked per week by owner	2.003	1,411	2.727	1,804	1.572	1,816	1–3
	(3.069)		(2.762)		(2.388)		
Hours worked per week by employees	.512	1,143	.706	1,536	−.012	1,472	1–3
	(3.588)		(2.608)		(3.311)		
Number of employees	.096	1,419			.089	1,827	1–3
	(.095)				(.105)		
Registered with the government	.077**	1,399	.091***	1,790	.092***	1,809	1–3
	(.031)		(.030)		(.024)		

Note. Samples in cols. 1 and 2 include firms offered treatment in treatment villages and all firms in control villages; the sample in col. 3 also includes control firms in treatment villages. The estimates in col. 1 exclude all pretreatment control variables. The estimates in col. 2 include control variables and impute outcomes for individuals who quit or attrited as follows: 0 for linear and binary outcomes, ln(.1) for clients served, and ln(1) for other logarithmic outcomes. Standard errors (in parentheses) are clustered at the village level; p-values are calculated from t-tests with 15 degrees of freedom. ITT = intention to treat.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Having two classes of measures for business profits and revenues—one self-reported and one calculated from the goods-specific data—allows us to test whether the extent of measurement error in these outcomes is systematically linked to the offer of the classes. Specifically, we cannot reject the equality of the correlations in the two measures, neither at the individual level, for either profits or revenues between the control and treatment groups in the ex post period, nor in a difference-in-differences specification. These results are inconsistent with systematic measurement error being the main driver of the positive treatment effects we find.[18]

C. *Changes in the Composition of Goods for Sale*

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

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

D. *Heterogeneity by Pretreatment Profits*

We next explore the hypothesis that that the effect of business-literacy training is stronger for better-performing enterprises at baseline (or higher-ability entrepreneurs). This set of tests is the empirical counterpart of the simple model of entrepreneurship we discussed earlier in Section II.C and will further detail more formally in Section VI. For ease of presentation, we split our sample into those above and below the median of the standardized pretreatment profits

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

TABLE 5
EFFECTS ON GOODS THAT WERE DROPPED ACROSS WAVES, KEPT ACROSS WAVES, AND ADDED POSTINTERVENTION

	Intention to Treat	Number of Observations
Standardized profit:		
Dropped	−.040	489
	(.106)	
Kept	.139	511
	(.176)	
Added	−.080	138
	(.119)	
Standardized revenue:		
Dropped	−.094	519
	(.099)	
Kept	.095	710
	(.132)	
Added	.149	320
	(.104)	
ln(mean unit cost):		
Dropped	.167*	512
	(.086)	
Kept	−.300*	533
	(.170)	
Added	.018	145
	(.249)	

Note. Samples include firms offered treatment in treatment villages and all firms in control villages. Dropped goods specifications use data from the pretreatment wave only, kept goods specifications use data from the pretreatment wave and first posttreatment wave, and added goods specifications use data from the first posttreatment wave only. Covariates are included. Standard errors (in parentheses) are clustered at the village level; p-values are calculated from t-tests with 15 degrees of freedom.
* $p < .10$.

and present in table 6 separate ITT estimates for above versus below the median of the baseline variable running equation (1). Comparing ITTs in the samples above and below the median of pretreatment standardized profits, we see quite striking differences: by and large, the positive effects of the intervention consistently arise from those above the median of pretreatment standardized profits, which can be seen as a proxy of entrepreneurial quality.

Although we cannot reject the equality of the effects between the top and bottom halves 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 among those above the median of pretreatment profits. For example, the ITT on standardized profits is 0.236 (significant at the 5% level) for those above the median and 0.057 for those below the median (which is largely insignificant). The difference between the two estimated parameters of 0.179—although clearly economically nonnegligible—is marginally significant with a p-value of .132. A similar story is present for our various

TABLE 6

HETEROGENEOUS TREATMENT EFFECTS BY PREINTERVENTION PROFITS

	Above Median of Standardized Baseline Profits		Below Median of Standardized Baseline Profits			
	ITT	Number of Observations	ITT	Number of Observations	(1) = (2) *p*-Value	
	(1)		(2)		(3)	Survey Waves
Quit her business	.014	876	−.016	912	.700	1–3
	(.029)		(.058)			
Measures of profit and revenue:						
ln(last day's profit)	.281*	609	.042	561	.240	1–3
	(.150)		(.154)			
Standardized profits	.236**	669	.057	640	.132	1–3
	(.107)		(.099)			
ln(last day's revenue)	.338**	680	.113	653	.334	1–3
	(.131)		(.188)			
Standardized revenue	.287***	703	.107	688	.187	1–3
	(.082)		(.105)			
Other business outcomes:						
ln(number of clients last day)	.332**	646	.150	644	.272	1–3
	(.145)		(.127)			
ln(household income, daily)	.247	537	.022	528	.308	1, 2
	(.182)		(.162)			
ln(number of goods for sale)	.193**	562	.122	556	.364	1, 2
	(.084)		(.094)			
ln(mean unit cost)	−.151	459	−.270	434	.713	1, 2
	(.190)		(.239)			
ln(mean unit price)	.090	557	−.153	555	.065*	1, 2
	(.094)		(.116)			
Percentage correct on business practices	.058	583	.035	598	.468	1–3
	(.075)		(.061)			
Uses formal accounting methods	.068*	707	.042	692	.581	1–3
	(.037)		(.028)			
Hours worked per week by owner	4.635	697	−.283	682	.087*	1–3
	(3.409)		(2.963)			
Hours worked per week by employees	6.786	565	−5.126*	552	.046**	1, 2
	(6.136)		(2.906)			
Number of employees	.216	701	−.044	685	.106	1–3
	(.180)		(.099)			
Registered with the government	.112**	690	.082	678	.606	1–3
	(.050)		(.051)			

Note. Samples include firms offered treatment in treatment villages and all firms in control villages. Covariates are included. Standardized profits (revenues) are constructed as the mean of standardized *z*-scores of the four profit (revenue) measures. Standard errors (in parentheses) are clustered at the village level; *p*-values are calculated from *t*- and *F*-tests with 15 degrees of freedom. ITT = intention to treat.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

measures of revenues and the number of clients served: good businesses benefit from the intervention more than bad businesses.

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

One striking observation is the large and significant differential effect in terms of hours worked per week by the owner of almost 5 hours (or 13% over the baseline). We find an even larger differential effect in terms of hours worked per week by employees, close to a 6-hour increase for those above the median compared with a 5-hour decrease for those below the median (with a difference of almost 12 hours significant at the 5% level). These effects on hours worked by employees seem not to be driven by differential changes in the number of employees. We also find a small differential effect in terms of registering with the government.

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

VI. A Simple Model of Entrepreneurial Experimentation and Business Literacy

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

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

$$\max_{c_{it}} V \equiv E_0 \sum_0^\infty \beta^t U(c_{it}), \tag{2}$$

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

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

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

For simplicity, we assume that there are only two types of technology, new and old, denoted by x_h and x_l, respectively, which cost x_h and x_l (with $x_h > x_l$). For the more productive types of entrepreneurs, the more expensive technology is more profitable than the less expensive technology, although 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 payout 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.[19]

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

[19] We assume that a nonempty 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.

"non" entrepreneur. Recall that all the subjects in our study are already entrepreneurs so their status quo is the use of the old technology in the model environment. Importantly, about a quarter of our entrepreneurs have been running their business for less than 1 year, although about half of them have been in the current business for less than 4 years.

More formally, the entrepreneur's value function is as follows:

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

The entrepreneur will decide to invest in the new technology rather than stick with the old technology if the following condition holds:

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

That is, the enterprenuer will choose to experiment if she is sufficiently optimistic about her productivity type being high, namely, large p^h.[20]

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, namely, $p_i^h \pi_i^h(x^h) + p_i^l \pi_i^l(x^h) + p_i^0 \pi_i^0(x^h) < \pi_i^l(x^l)$. The reason is that

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

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

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 quitting, as follows:

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

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

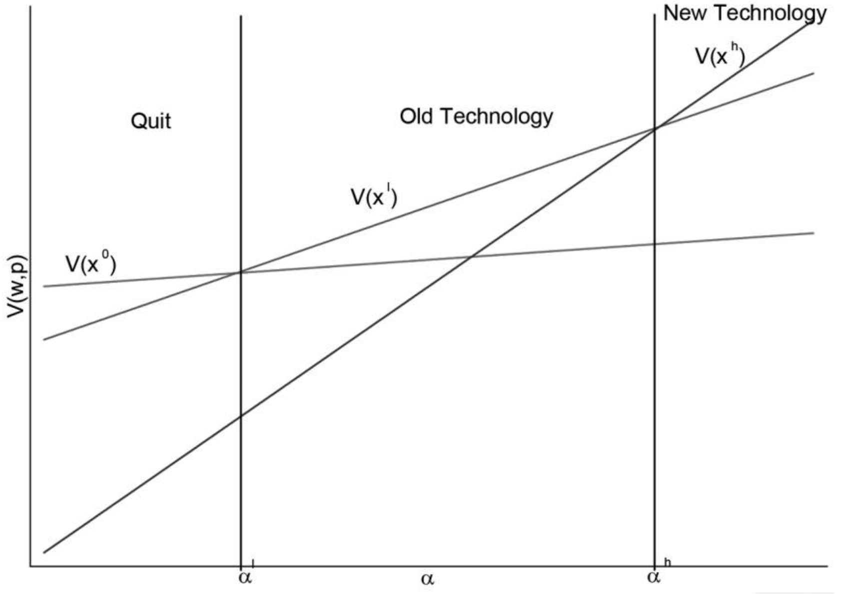

Figure 2. Entrepreneurial choice. A color version of this figure is available online.

Under the assumption that the probability of success is positively related to one's ability—that is, p^h is positively related to α—the treatment will induce more optimistic entrepreneurs to try the new technology relative to the control. This implies that the average difference between the treated and control groups in quit rates and profits cannot be signed ex ante, as some of the treated are low-ability types who are trying out the new technology. Thus, the average effect of the treatment (i.e., offering business-literacy classes) is ambiguous on firm profits and quit rates, as we would require knowledge of the distribution of types and beliefs in the population as well as the relative productivity gains the new technology offers. Ultimately, it is an empirical matter whether

$$\Pr(\text{Quit}|T = 1) - \Pr(\text{Quit}|T = 0) \gtreqless 0,$$

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

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

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

$$E(\pi \big| T = 1, \alpha > \alpha^h) - E(\pi \big| T = 0, \alpha > \alpha^h) > 0. \tag{5}$$

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

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

Testing these two predictions requires knowledge of α. As productivity and type are difficult to measure empirically, a potential proxy for productivity could be pretreatment profits, π_0. Thus, the two testable implications of this model are that the ITT effect on quitting should be nonincreasing in pretreatment profits and the ITT effect on profits should be nondecreasing in pretreatment profits:

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

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

The empirical support for the first hypothesis (eq. [7]) was presented in table 6, albeit—as discussed earlier—from tests with low power. The effect of treatment on profits, as well as other indicators of profitability, is larger among entrepreneurs with higher pretreatment profits. We do not find empirical support for the second hypothesis in table 6, in that the propensity to quit one's business in response to the treatment is not differential on average between those with higher and lower pretreatment profits (a small differential in magnitude and strongly insignificant).

However, a closer look at the distribution of the propensity to quit one's business as a function of pretreatment profits shows that the excessive experimenters are in fact located in the far left tail. It is precisely these entrepreneurs who have the lowest ability that are induced to quit their businesses. This can be seen in figure 3, which presents—separately for treatment and control firms—the distributions of pretreatment profits in the whole sample compared with the distribution of pretreatment profits among those who did not quit by the second follow-up survey. It is clear that the survived sample (i.e., those who did not quit) is similar in terms of pretreatment profits to the whole sample in the control group. In the treatment group, however, the distribution of the survived sample is significantly shifted to the right; this is consistent with the prediction that those with the lowest ability will be induced to quit on learning—they are in fact a low-ability type. Kolmogorov-Smirnov tests for the equality of

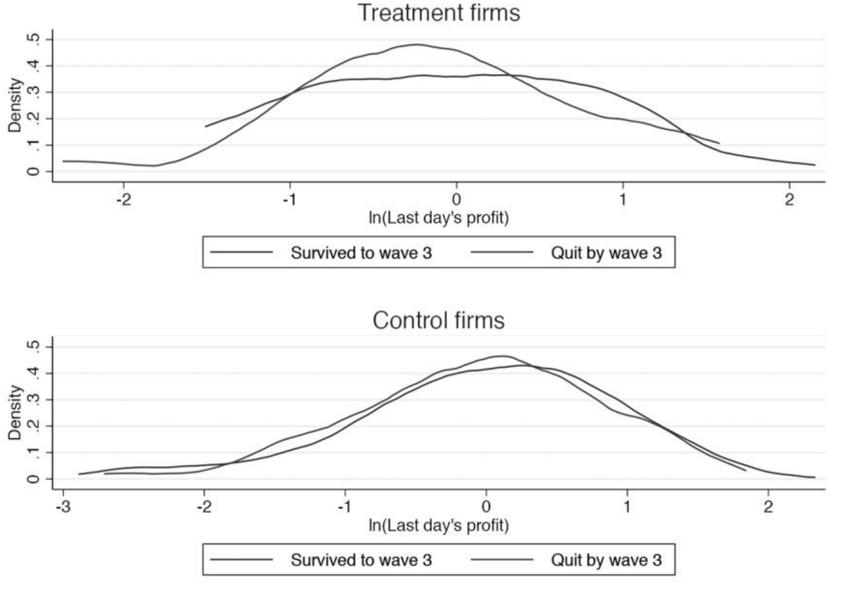

Figure 3. The distribution of baseline (log) daily profits among the whole and survived samples of the treatment and control groups. A color version of this figure is available online.

the distribution functions in figure 3 yield *p*-values of .07 in the treatment group and .97 in the control group. We also test this prediction in a parametric framework (results available on request). In the context of the model, the exercise we undertake amounts to searching for where α^l is located within the distribution of baseline profits. We conduct a grid search over percentiles of the distribution of baseline profits by regressing an indicator for quitting (and attriting) by the second follow-up survey on a treatment indicator, an indicator for being a given percentile of the last day's profits pretreatment, and the interaction of these two indicators. The interaction term is large and significant up to the 2nd percentile and smaller in magnitude and insignificant—yet positive—up to the 5th percentile. Interaction terms for all percentiles greater than 5 are small in magnitude and insignificant. Thus, it appears α^l is around the 2nd percentile of pretreatment profits.

VII. Conclusion

Growing evidence suggests that firms in developing countries are often run inefficiently. This paper focuses on whether a lack of entrepreneurial business skills is impeding business success, and it uses data from a randomized controlled trial in Mexico that offered business-literacy classes to poor women microentrepreneurs.

We find that a basic training in business management and accounting is capable of significantly increasing profits. This increase appears to be driven by a combination of higher revenues, lower costs, a change in the composition of goods sold to higher-profit ones, more clients served and quantities sold, and an increased use of formal accounting methods. Importantly, knowledge gained through the intervention does not appear to fade out over time, as we observe positive effects persisting into the medium run.

The full social impacts of the training must include any spillover impacts on untreated firms in treatment villages. Theoretically, spillovers could be either positive or negative. For example, positive spillovers could result from the dissemination of efficient business practices, which could lower costs and increase profits of all businesses. In contrast, treated firms could use their new business acumen to outcompete untreated firms. In practice, and as detailed in the appendix, our experimental design was underpowered to precisely estimate spillover effects, although the point estimates on profits were large in magnitude and negative—the indirect effect on the last day's profit was −0.119 log points. Policy makers interested in both efficient and equitable interventions must not ignore spillovers when designing and scaling up interventions such as these, and more research is needed to estimate theses spillovers precisely.

To justify the intervention from a social point of view, program impacts must be weighed against implementation costs. CREA classes are rather inexpensive to run, as local teachers are hired for a modest wage, minimal materials are provided to the students, and community centers are used to hold classes at no cost. Specifically, each of the seven treatment villages had two teachers who taught for a total of 48 hours and were paid about $10 per hour, yielding $6,720$(7 \times 2 \times 48 \times \$10)$ in salaries. Although only 65% 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 teachers' salaries with 164 invitees is $49.97($6,720/164). Materials (photocopies of lessons, pens, paper, calculators, and CREA logo hats that were used as prizes) totaled about $5 per participant; conservatively assuming materials were purchased for all invitees, the total per-invitee cost of CREA's program is approximately $54.97($49.97 + $5).

The program impacts were positive among the treated but negative among the control in treatment villages: the direct effect on daily profits was a 23.4% increase (log(0.215)), although the indirect effect—albeit imprecisely measured—was a 12.6% decrease (log(0.119)). Mean pretreatment daily profit in the treatment villages was $10.68, which implies that the program increased average net daily profits per entrepreneur by $1.15 ($10.2 × [23.4 % −12.6 %]). Pretreatment, 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 7% annual discount rate, the present discounted value of the perpetuity of average increased profits is $2,214.93([$1.15 × 5.17 × 26]/0.07). Given this large disparity in program costs and benefits, it should be clear that it would be very difficult to find a scenario under which increased profits do not outweigh the program costs, even if we were to include the opportunity cost of missed work when taking the classes.

We conclude with an important question: Why do we not observe private firms offering business-training courses to microentrepreneurs? Given the large returns to training that we find, some entrepreneurs should demand the product at a price above cost. However, informational asymmetries and credit constraints in these poor, rural villages may well be sufficiently large to impede the emergence of a market. Business owners may not know the value or even the existence of better management skills; further, they might lack the initial capital (credit and savings constraint) for paying for such services upfront, although at the individual level, those costs are substantially higher than at the group level due to fixed costs. It is also possible that potential suppliers of such services to small entrepreneurs may lack the capital required to build demand for the product through advertising or subsidized courses. Future research on the demand for business-literacy training among entrepreneurs—and how demand evolves with knowledge of its effectiveness—would help governments and NGOs in deciding the optimal amount and type of subsidized intervention that should be provided.

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