

# Keeping it Simple: Financial Literacy and Rules of Thumb

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January 2011

## Abstract

Individuals and business owners engage in an increasingly complex array of financial decisions that are critical for their success and well-being. Yet a growing literature documents that in both developed and developing countries, a large fraction of the population is unprepared to make these decisions. Evidence on potential remedies is limited and mixed. Two randomized trials test the impact of financial training on firm-level and individual outcomes for microentrepreneurs in the Dominican Republic. We find no significant effect from a standard, fundamentals-based accounting training. However, a simplified, rule-of-thumb training produced significant and economically meaningful improvements in business practices and outcomes.

**Keywords:** financial literacy, entrepreneurship, business training, microfinance, adult education

**JEL Classification Codes:** C93, D12, I21, J24, O12

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# 1 Introduction

Individuals are asked to make financial decisions in many areas of life, whether in their personal finances in the form of savings decisions and retirement planning or in a business context as small business owners or investors. However, a growing literature shows that a large fraction of the population is woefully unprepared (or underprepared) to make these decisions. Lusardi and Mitchell (2007b) or Lusardi and Tufano (2009), for example, find low levels of financial literacy in the US population, an inability to understand basic financial concepts such as the importance of retirement savings, and poor judgment in borrowing decisions. Similarly, Cole, Sampson, and Zia (2009) document very low levels of financial literacy for households in India and Indonesia. In addition, these studies find a strong association between understanding financial concepts, better financial decisions, and household well-being.

The challenge is to determine whether and how financial literacy can be taught and, closely related, whether there is causal link between improving financial literacy and financial outcomes. The evidence so far has been mixed, with large heterogeneity in the estimated success of training programs. For example, Bernheim and Garrett (2003) and Lusardi (2004) provide survey evidence that people who attend financial counseling programs subsequently make better financial decisions, especially those attendees with low income and education levels. The estimated effects of the program are large, but due to the non-random treatment assignment might be overstated due to selection bias. In contrast, Duflo and Saez (2003) conduct a randomized control trial to expose employees to a benefits fair to raise awareness about retirement savings, but they find only a small effect on savings plan enrollment. Similarly, Cole, Sampson, and Zia (2009) find only modest effects from a financial literacy training program in Indonesia.

The contribution of our paper is twofold. The first is methodological: we conduct a randomized control experiment to test the impact of financial training on firm-level and individual outcomes for microentrepreneurs in the Dominican Republic. It is important to keep in mind that any study of this kind tests not only the impact of financial literacy but implicitly also whether these skills can be transmitted via classroom training. Therefore our second contribution is conceptual: we test whether the type of program determines the effectiveness of the training. The impact of a program might be crucially driven by the complexity of the materials, since any training program faces a trade-off between ease at which participants can grasp the concepts and the depth of understanding.

In order to analyze what are the most effective ways of teaching financial accounting skills to small business owners, we developed two distinct types of financial accounting training that rely on different approaches to training. We focus on the tradeoff between a standard approach to small business training, which teaches the fundamentals of financial accounting, and training based on simple rules of thumb. The former aims to provide a relatively complete understanding of financial decision making, with concepts and materials targeted towards the typical microfinance client. Similar programs are used around the world by groups such as Freedom for Hunger, the International Labor Organization, and BRAC. The latter provides a simplified view of financial decision making, teaching easily implemented decision rules without explaining the underlying accounting motivation. For example, instead of teaching the details of working capital management at even the rudimentary level of traditional accounting training, the rule-of-thumb training instructs micro-entrepreneurs to assign themselves a wage at the beginning of each month, which they pay out to themselves on a weekly basis, but apart from this they cannot take any money out the firm. This way the owner can learn how profitable the business is without having to do any cash flow analysis. In contrast, the basic accounting training is designed to teach micro-entrepreneurs the basics of double-entry accounting, cash and working capital management and investment decisions. This class follows the traditional approach of teaching first principals. Our aim is to quantify the effectiveness of training when trading off the complexity of the material versus the depth of the concepts that are taught.

Between November 2006 and July 2008, we implemented a randomized control trial of these different financial accounting classes in collaboration with ADOPEM, a microfinance institution that lends to individuals and small businesses in the Dominican Republic. We selected 1200 existing clients of ADOPEM who had expressed an interest in training and randomly assigned them to one of the two accounting trainings or a control group that did not receive any training. In order to begin understanding the potential limitations to classroom-based, financial training, we also randomly assigned half of the people in each of the treatment and control groups to receive follow-on training consisting of in-person visits of a financial trainer to the micro-entrepreneur's business. When necessary, the trainers reviewed the class materials with the entrepreneurs and helped clarify any questions they might have had. The purpose of the on-site visits was to ensure that individuals understood the material and were capable of implementing their newly-acquired financial accounting skills in their businesses.<sup>1</sup> This structure al-

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<sup>1</sup>The control group received placebo follow-up visits to control for possible monitoring effects.

lows us to differentiate the channel by which training affects the participants: If we do not find an effect of training we can determine whether this result is due to the inability of the participants to understand what was taught in class or whether the material itself, even when properly understood, is not helpful.

Our results show an asymmetric impact of the rule-of-thumb training compared to the basic accounting training. People who were offered rule-of-thumb based training showed significant improvements in the way they managed their finances as a result of the training relative to the control group which was not offered training. They were more likely to keep accounting records, calculate monthly revenues and separate their books for the business and the home. Improvements along these dimensions are on the order of a 10% increase. In contrast, we did not find any significant changes for the people in the basic accounting training. It appears that in this context, the rule-of-thumb training is more likely to be implemented by the clients than the basic accounting training.

When looking at the impact of training on the outcomes of the business we again find a more significant change in the group that received the rule-of-thumb training compared to the group in the basic accounting training. We see an especially large improvement in the level of sales during bad weeks—30% for people in the Rule-of-Thumb based training—and a substantial but not statistically significant increase in average sales. The basic accounting training produces no significant effects. We do not see any discernible effects on investment behavior or profitability of the firms in either treatment group; however, these variables are reported with such noise that we are unable to reject even large effects. Taken together, these results suggest that effective training may operate by helping individuals to better manage negative shocks or by alerting them to such shocks such that they can counteract the effect of slow weeks.

We also find an economically large increase in savings of 6% for the rule-of-thumb trainings, but the result is only significant at the 10%-level. We do not find any effect on savings for the group that received the basic accounting training.<sup>2</sup>

Finally, we find that in-depth follow-on training at the business of the borrower

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<sup>2</sup>We also investigate whether there are heterogeneous treatment effects of the treatment for people with different levels of educational background and for borrowers that have individual loans versus group loans. We do not find any consistent differences in outcomes for the borrowers with two different loan types. But we find some heterogeneous treatment effects for more educated clients in the basic accounting training. More educated clients tend to show significant improvements when allocated to the basic accounting training, e.g. their savings and likelihood of record keeping increases. However, the effects are not significant across all outcomes. In contrast we do not find any differential effect of education for clients in the rule of thumb based training.

did not affect the outcomes for clients in the rule-of-thumb based training. We neither see a change in the likelihood of implementing the accounting methods learned in class nor an impact on actual outcomes for the business. In contrast, people who received the follow-on training in the basic accounting group did show a significant increase in the probability of implementing the accounting practices taught in class. They also had a significant increase in savings levels of about 10%. However, we did not find an improvement on real outcomes of the businesses such as sales or investments. These results support the idea that the rule-of-thumb training is more effective because it is easier to understand, but it also generates larger results conditional on understanding, which was ensured through follow-on visits. This difference may stem from either the rule-of-thumb techniques being more effective once implemented or from individuals being more likely to implement these techniques, even conditional on understanding. We believe that a potential channel how these results occur is that better financial controls allow a business owner to adjust the effort level more effectively or manage inventory and product offering better.

The findings from this study also have important implications for programs designed to help micro entrepreneurs. International development organizations, NGOs and others spend a lot of effort on financial literacy training in their technical assistance programs but often report only mixed success. But if micro-entrepreneurs are unable to effectively control the finances in their businesses, it is very difficult to efficiently scale up operations even if the firm has access to other resources. Thus our results suggest that lack of knowledge in finance and financial accounting might impede the growth of small businesses.

The rest of the paper is organized as follows. Section 2 briefly describes the related literature, and Section 3 details the experimental design. Section 4 describes the data and empirical strategy, Section 5 presents the results, and Section 6 concludes.

## **2 Related Literature and Background**

A growing literature has documented the low level of financial literacy in the general population and its impact on individual decision making. Lusardi (2008) finds widespread lack of financial literacy among large sections of the U.S. population, especially among people with low levels of education, women, and ethnic minorities. This lack of financial literacy is associated with poor financial decision making, in particular regarding retirement planning (Lusardi and Mitchell 2007a),

borrowing decisions (Lusardi and Tufano 2009, Stango and Zinman 2009), investment choices (Lusardi and Mitchell 2007b), and participation in the formal financial system (van Rooij, Lusardi, and Alessie 2007).

Yet despite the strong association between financial literacy and a range of measures of financial well-being, little is known about the efficacy of financial literacy training programs in improving these outcomes (Braunstein and Welch 2002). Causal inference for many studies is hindered by endogenous selection into training programs.<sup>3</sup> Where causal effects can be clearly identified, the results are mixed. Bernheim, Garrett, and Maki (2001) exploit variation across states and time in mandatory financial education for high school students and find that mandates increased exposure to financial curricula and asset accumulation; however, subsequent work by Cole and Shastry (2009) uses a larger sample and finds little effect. Cole, Sampson, and Zia (2009) conduct a randomized control trial of a financial education program in Indonesia. They find that while financial literacy is strongly correlated with the demand for financial services, financial literacy education had at most modest effects on demand and was dwarfed by the effect of even a small subsidy to opening a savings account.

Moreover, most studies use the term “financial literacy” training to refer to a myriad of different programs, varying from one-day consultation sessions in the field to one year of detailed in-class training. This variation makes it difficult to interpret results and compare the impact of training across studies. In particular, these studies do not allow one to test which features of literacy training are more effective than others. In contrast, in our work we explicitly test the impact of different types of financial literacy training—standard accounting and a simplified, rules-of-thumb approach—with the aim of beginning to understand the mechanisms through which training programs may or may not work.

We also focus on a specific type of training aimed at small business owners. Surprisingly few studies have looked at financial literacy for this population, even though significant resources are devoted to accounting and financial literacy training for them.<sup>4</sup> One notable exception is Karlan and Valdivia (2010), which studies

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<sup>3</sup>Meier and Sprenger (2008), for example, document that individuals who choose to acquire personal financial information through a credit counseling program discount the future less than individuals who choose not to participate.

<sup>4</sup>Organization’s Know About Business Programme, and the Financial Education for the Poor (FEP) project sponsored by Microfinance Opportunities, the Citigroup Foundation, and Freedom From Hunger and many others aim to teach financial skills at huge expense every year. The SBA training includes modules on finance and accounting, business planning, business start up, business management, government contracting, marketing and advertising, and how to survive in a slow economy. The training is available online at <http://www.sba.gov/training/>. The FEP targets microfinance clients, many of them having only subsistence level business activity.

the impact of teaching basic finance concepts to micro-entrepreneurs.<sup>5</sup> The study finds a large impact on the MFI clients' knowledge of financial terms and reported business practices. Results are more mixed on real outcomes such as sales or consumption, but the microfinance institution benefited from increased retention and repayment. Field, Jayachandran, and Pande (2010) evaluate a two-day training program for clients of an Indian microfinance institution. Their study focuses on constraints to women's entrepreneurial choices and finds that being invited to the training program increased both borrowing and the likelihood of personal labor income. A recent program evaluation by Berge, Bjorvatn, and Tungodden (2010) evaluates the effect of financial grants and a wide-ranging business training program for clients of a microfinance institution in Tanzania. They find little effect on female clients, but a substantial impact on men's business practices and outcomes.

There is a related strand in the literature on capacity building for small- and medium-size enterprises that focuses on providing consulting and management services to firms. Bloom, Eifert, Mahajan, McKenzie, and Roberts (2010) study the impact of intensive consulting services from an international management consulting firm on the business practices of medium- to large-size firms in the Indian textile industry. Even these large firms were unaware of many modern management practices, and treated plants significantly improved their management practices. Bruhn, Karlan, and Schoar (2010) conduct a randomized control trial of consulting services in which small businesses were paired with a local management consultant for one year. The study assigned firms to a wide range of management consulting services, with financial literacy was an integral part of the intervention. More than 30% of the firms requested financial advice as one of the main inputs.

We contribute to this literature by conducting a randomized control experiment which explicitly compares the impact of standard accounting training with a simplified, rule-of-thumb-based program. In this vein, we build on a growing literature that supports the merits of simplification in settings as varied as retirement savings plan enrollment (Beshears, Choi, Laibson, and Madrian 2010, Choi, Laibson, and Madrian 2009), Medicare drug plans (Mullainathan and Shafir 2009), weight loss (Mata, Todd, and Lippke 2010), and college student loan applications (Bettinger, Long, Oreopoulos, and Sanbonmatsu 2009). Research in cognitive psychology offers additional evidence that simpler rules and less feedback

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The FEP project includes five modules: credit administration, savings, financial negotiation, budgeting, and bank services.

<sup>5</sup>The micro-entrepreneurs in their study are part of a group lending program with weekly meetings. In these weekly sessions, clients in the treatment group also receive training.

may be preferable in certain learning environments (Maddox, Love, Glass, and Filoteo 2008, Maes and Eling 2007). As Feldman (2003) notes, it is not surprising that more complex tasks are also often more difficult to learn. However, this seemingly obvious idea has until recently played little role in theories of concept learning. Similarly, the trend in business and financial literacy training appears to have been towards increasing complexity. In the context of Dominican microentrepreneurs, our results suggest that optimality may lie in the direction of simplification.

### 3 Experimental Design

ADOPEM is a savings and credit bank based in Santo Domingo, Dominican Republic and serving primarily low-income, urban individuals and small businesses throughout the country. ADOPEM was founded in 1982 as a non-governmental organization providing a range of programs aimed at reducing poverty levels in the Dominican Republic. Since then, they have increased their focus on financial services and related activities, incorporating as a bank in 2004. Large by Dominican standards, in 2006 ADOPEM had approximately 59,000 clients in 19 branches. The bank offers a wide range of lending products; in 2006, 90% of loans were for amounts between RD\$2,500 and RD\$50,000 (US\$70-1,400). Over that same period, 56% of loans were made to individual persons or businesses and 44% were made to solidarity groups of two to five borrowers.<sup>6</sup> Approximately 80% of these clients were women.

In addition to extending loans, ADOPEM offers savings, insurance, and remittance products. It also operates a training center, with programs ranging from basic computing, entrepreneurship, and specific trade skills. In the year before this experiment was launched, ADOPEM was actively planning to launch a dedicated financial education program and was interested in evaluating different approaches.

We worked with ADOPEM and Dominican training experts to develop two alternative financial education training programs. The Accounting treatment offered a traditional, principles-based course in basic accounting techniques. Topics covered included daily record keeping of cash sales and expenses, aggregation of daily records into weekly and monthly reports, inventory management, accounts

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<sup>6</sup>ADOPEM's solidarity groups follow the traditional joint liability model. Each borrower takes out his or her loan as an individual, but all group members are jointly responsible for one another's repayment. Should any member fail to repay, each member suffers the default consequences as if she herself failed to repay.



receivable and accounts payable, calculating cash profits, and investment planning. The materials and capacitor training program for the Accounting treatment were based on the financial education program designed by Freedom from Hunger, a US-based non-profit organization, and the Citigroup Foundation and adapted to local conditions.<sup>7</sup>

The Rule-of-Thumb treatment taught participants simple rules for financial decision making, focusing on the need to separate business and personal accounts. Account separation is a staple rule in developed country entrepreneurship. In developing countries, where the tax and legal motivations for account separation often are weaker, it continues to receive a great deal of attention. The proposed benefits of account separation are twofold. On the one hand it is seen as a very crude but easy way to monitor whether the business is self sustainable and provides an estimate of the profitability of the business. The second rationale is more behavioral: keeping accounts separate serves as a commitment device for the business owner (or the family members and relatives) not to overconsume and deplete the working capital in the business. In addition to presenting several strategies for physically separating business and personal funds, the Rule-of-Thumb treatment taught how to estimate business profits by simple changes in business cash on hand, paying oneself a fixed salary, distinguishing business and personal expenses, and easy-to-implement tools for reconciling accounts when business funds have been used for personal expenses or the reverse. In both treatments, clients received handouts and homework assignments to reinforce ideas or techniques from the meetings. Both classes were offered once a week for three hours at a time. The Accounting treatment lasted for six weeks and the Rule-of-Thumb treatment for five. As described in Table A1, the first three classes of both treatments covered consumption, savings, and debt management. The final three classes of the Accounting treatment comprised basic cash accounting, distinguishing business and personal expenses, calculating profits, and working capital management. Classes four and five of the Rule-of-Thumb treatment focused on separating business and personal money and estimation techniques for calculating profits. Attendance for classes one through five did not differ across the two treatments.

The sample consisted of 1,193 existing ADOPEM business or personal loan

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<sup>7</sup>The ADOPEM training program is most closely related to the budgeting module of the FFH training program. This module includes training on: how to develop a financial plan for the household expenses, how to adapt the spending to a restricted income, how to develop a budget for the house and the business, how to prioritize spending, how to record income and expenses, how to use income and expenses book keeping to make financial decisions, and how to store financial documents. Importantly, both ADOPEM training programs focused on maintaining a clear separation of business accounts.

clients from Santo Domingo.<sup>8</sup> Of these, we assigned 402 to the Accounting treatment, 404 to the Rule-of-Thumb treatment, and 387 to a control group which received no additional training services. The treatment was assigned at the individual level and administrative data was used to stratify according to loan size, years of borrowing, and whether or not a client maintained a formal savings account with the bank. ADOPEM made no additional policy changes concurrent with the training program. The treatment was conducted in two waves. The first wave, comprising 302 treatment assignments, was conducted from March to May 2007, and the second wave comprising the remainder ran from July to August of the same year.<sup>9</sup>

We also randomly assigned both treatment and control individuals to follow-up visits of varying intensity. This begins to unpack the mechanisms through which classroom-based training works or does not work. If the training does not change management practices or improve outcomes, it could be that individuals did not understand or were unable to implement new management techniques after classroom training. Alternatively, it could be that individuals understood the management techniques but chose not to implement. Finally, it could be that even when the material is understood and implemented, it does not affect business performance. In the intensive follow-up, training personnel visited participants eight times over three months in order to answer any questions that students have about the materials, to verify and encourage completion of accounting books, and to correct any mistakes made in completing these books. The intermediate follow-up comprised five visits over six weeks. These treatments were randomly assigned conditional on a client attending the first class. In order to assess potential Hawthorne Effects induced by the follow-up, randomly selected members of the control group also received a “dummy” follow-up, in which they were visited by training staff and asked questions about their business performance over a period of six weeks.<sup>10</sup>

All courses were taught by qualified local instructors. The majority had univer-

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<sup>8</sup>At the request of ADOPEM, group loan clients with loans smaller than \$RD15,000 were excluded from the study. The original sample comprised 1,200; however, 7 observations were discarded due to errors in the baseline survey.

<sup>9</sup>A third wave of 800 individuals across all three assignment categories was planned for late 2007, but was cancelled due to the disruption caused by Hurricanes Dean and Noel and Tropical Storm Olga.

<sup>10</sup>While the visits in the intermediate follow-up were initially intended only to verify understanding and not implement techniques, in practice it was not feasible for training personnel to deny requests for assistance when visiting treated households. At the request of training personnel and ADOPEM, the intermediate follow-up was implemented as a lower-intensity version of the full follow-up. In the analysis that follows, we group together treatments of both intensity levels.

sity degrees and experience with adult education, in most cases with ADOPEM directly. Courses were offered at seven schools throughout Santo Domingo and scheduled based on preferences elicited during the baseline survey. In addition, the course was heavily subsidized. Fees were randomly assigned at RD\$200 (approximately US\$6) or zero, relative to an overall program cost of approximately RD\$700. We varied fees in order to test for selection effects. As noted in Karlan and Valdivia (2010), the emerging approach to business development services calls for pricing training services at or above marginal costs. However, if those entrepreneurs who would most benefit are uncertain of the program's benefits or subject to tighter credit constraints, this approach may induce adverse selection.

## 4 Data and Empirical Strategy

We constructed the original sample frame based on administrative data collected by ADOPEM in the ordinary course of operations. In November 2006, we conducted a baseline survey of each study participant using a professional survey firm unaffiliated with ADOPEM. We collected information on household and business characteristics, business practices and performance, business skills, training history, and interest in future training. The endline survey was conducted during the summer of 2008, at least 12 months after training was completed. We augmented the surveys with administrative data from ADOPEM.

Table 1 reports summary statistics for the full sample and each of the three assignment groups from the baseline data collected in November 2006. Given that the treatments were randomly assigned, we expect individuals in the three assignment groups to be similar in the baseline.<sup>11</sup> As shown in the table, this expectation generally holds; however, individuals assigned to the Accounting treatment are marginally less likely to report keeping accounting records or separating their business and personal accounts. Individuals in the Rule-of-Thumb training also report lower revenues in average and bad weeks, although these differences fall below the 10%-significance level. Therefore, we control for these characteristics in the regression analytics that follow. Based on our sample size of approximately 400 individuals per assignment group, any small-sample bias introduced by inclusion of these baseline characteristics as covariates is minimal.

As shown in the table, the average loan size for all participants in the study was RD\$26,514, approximately US\$750; the median was RD\$20,000. The median

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<sup>11</sup>As described above, stratification utilized administrative records. Baseline survey data was not available at the time of assignment.

borrower in the sample reported revenues during an average week of RD\$3,000 (US\$85). Median good week and bad week revenues were RD\$4,000 and RD\$1,500, respectively. Approximately 60% of the businesses were sole proprietorships—with no employees in addition to the borrower. Of the rest, 80% have one or two employees in addition to the borrower and few have more than five. Typical businesses include small retail shops, general stores (*colmados*), beauty salons and food service. Approximately half of participants operate businesses engaged in retail sales and trading.

The endline survey conducted in mid-2008 reached 87% of participants reporting in the baseline. Intensive efforts were made to contact all participants using bank and phone records, and we believe that many of the individuals we were unable to reach in the endline had migrated outside of the Dominican Republic. Although attrition rates are relatively low considering the endline survey follow-up window, there is some evidence for selective attrition. Treatment group individuals who were not reached for the endline survey have higher baseline revenues than those who dropped from the control group. The differences in reported weekly sales range from 0.27 standard deviations (average weekly sales) to 0.45 standard deviations (bad week sales). This suggests that the reported results for business outcomes may understate the program’s true effect.<sup>12</sup>

Random assignment of treatment allows us to obtain unbiased estimates of the effect of being offered the training program by estimating the following equation:

$$y_i^E = \alpha + \beta Treat_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \quad (1)$$

where  $y_i^E$  is the endline value of the outcome variable of interest;  $Treat_i$  is an indicator for being assigned to the treatment;  $X_i$  is a matrix of baseline-measured covariates including business types, loan size, and participation in an ADOPEM savings account. The pre-treatment measure of the outcome variable,  $y_i^B$ , explains a substantial share of the variance in outcomes across individuals and is included where available. We estimate equation (1) separately for each training type, alternately excluding participants assigned to the other training program. The parameter  $\beta$  is an estimate of the program’s average effect on outcome  $y$ . For binary outcome variables, we estimate a linear probability model following the same specification in (1), which allows interpretation of  $\beta$  as the difference in the mean level of an activity, e.g., keeping formal accounts, conditional on assignment

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<sup>12</sup>Table 14 reports non-parametric bounds for the treatment effect across a range of assumptions for the pattern of attrition following Horowitz and Manski (2000) and Lee (2002).

to the particular treatment group. For all business outcome and performance measures (e.g., weekly revenues or keeping business and personal accounts separate), the sample is restricted to only those individuals who report owning a business, so answers to these questions are well defined. The rate of business ownership is 78.1% and does not differ significantly across the various treatment groups. Standard errors are clustered at the barrio level, to account for community-level shocks to business conditions. While covariates were specified in advance of final data collection, we also estimate the simple cell means regression,

$$y_i^E = \alpha + \beta Treat_i + \delta y_i^B + \varepsilon_i, \quad (2)$$

to verify that the choice of covariates is not affecting parameter estimates.

We test for heterogeneous treatment effects with respect to education, business type, loan type (individual or group), and prior interest in training re-estimating equation (1) while restricting the sample in turn to each of the partitioning subgroups. Each of these subgroups was specified in the analysis plan before the endline data was collected.

Because follow-up for the treated participants was assigned conditional on attending the first class, we estimate the effect of the follow-up with the following specification, restricting the sample to only those participants who were randomly assigned to one of the follow-up conditions:

$$y_i^E = \alpha + \beta Follow_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \quad (3)$$

where  $Follow_i$  is an indicator for assignment to either the intensive or intermediate follow-up. To assess the possibility that the act of training personnel visiting participants affected outcomes independent of training content, we also estimate (3) for those assigned to the placebo follow-up.

We also estimate the effect of treatment on the treated by estimating the equation,

$$y_i^E = \alpha + \beta AttendAny_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \quad (4)$$

where  $AttendAny_i$  is an indicator for whether individual  $i$  attended any of the training classes. Because attendance is endogenous, we instrument for attendance in (4) with assignment to the treatment.

While we focus on a few key business practice and performance outcomes, we consider the effect of training of 38 distinct outcomes. Because testing multiple outcomes independently increases the probability that we will reject at least one

outcome, we follow Kling, Liebman, and Katz (2007) and Karlan and Valdivia (2010) in constructing summary measures of standardized treatment effects for four classes of outcomes: business practices, business performance, personal outcomes, and personal financial practices. Within each category, we rescale each outcome such that larger values indicate better values for the individual or business and convert each measure to a z-score such that  $z_{ki} = (y_{ki} - \mu_k) / \sigma_k$ , where  $\mu$  and  $\sigma$  are the mean and standard deviation of  $y_k$  for the control group. For each category, we then construct a summary measure  $z_i = \sum_k z_{ki} / k$ . We then estimate equation (1) for each of the four categories in order to test whether the training treatments affected the set of outcomes within the category. We then estimate

$$z_i^E = \alpha + \beta Treat_i + \gamma X_i + \delta z_i^B + \varepsilon_i. \quad (5)$$

Self-reporting bias raises concerns about our measures of business management practices. Treated individuals may, for example, report maintaining separate business and personal accounts because they were told this was important and not because they actually do so. To allay such concerns, we construct an objective index of financial reporting errors. We classify as an error any report of (i) bad period sales greater than average or good, (ii) average period sales better than good, or (iii) average period profits better than good period sales for each of daily, weekly and monthly reported outcomes. In the baseline, 45% of subjects make at least one mistake and 11% make three or more. We then estimate the effect of each treatment on reporting errors following equation (1). We also consider the interaction of education and training, estimating

$$y_i^E = \alpha + \beta Treat_i + \beta_2 HighEd_i + \beta_3 Treat_i \times HighEd_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \quad (6)$$

where  $HighEd_i$  is an indicator for whether or not individual  $i$  completed high school or better and the coefficient  $\beta_3$  reflects whether more highly educated subjects respond differentially to the treatment.

Finally, although attrition in our sample was relatively low (13%), we follow Lee (2002) in constructing non-parametric bounds on the category aggregate treatment effects using a range of assumptions for the pattern of attrition. To compute lower bounds, we assign to all those who attrited from the treatment group the mean value of the non-attriters minus some fraction of the standard deviation for the group. For all those who attrited from the control group, we assign an outcome equal to the mean value of the non-attriters from the control group plus some

fraction of the reported standard deviation. We then estimate equation (1) on the imputed values for missing observations. Upper bounds on the treatment effect are computed following the same procedure, *mutatis mutandis*.

## 5 Results

Table 2 demonstrates a clear pattern of selection into training. Conditional on assignment to the treatment group, those who attend are more well educated (high school graduates are 10 percentage points more likely to attend). They are also more likely to have expressed an interest in accounting training during the baseline survey; however, a prior interest in increasing savings or improving cash management is not associated with increased attendance. They also tend to have lower revenues but bigger plans, as measured by the share of the loan intended for fixed asset purchases. Attendance does not vary with individuals' business type. Interestingly, we see some evidence of the reverse of an "Ashenfelter dip": individuals reporting that their business had improved in the month preceding the baseline survey were 6.4 percentage points more likely to attend the training. These results underline the importance of using an intent to treat design as discussed above.

Table 3 presents the effect of each training program on business practices and performance. Assignment to the rule-of-thumb training substantially increases the likelihood that individuals report separating business and personal cash and accounts, keep accounting records, and calculate revenues formally. Each of these measures increases by 6% to 12% relative to the control group, which did not receive training, and all estimates are significant at the 5%-level or better. In contrast, we find no statistically significant effects on the business practices of those assigned to the Accounting treatment.

Individuals assigned to the Rule-of-Thumb treatment report a substantial increase in revenues during bad weeks. This increase of RD\$967 is economically large, 25% of mean endline reports and nearly 60% of the median, and significant at the 5%-level. As is shown in columns 5 and 6 of Table 3, those assigned to the Rule-of-Thumb training also reported higher revenues in both average weeks and the immediately preceding week; however, neither result is statistically significant. These results should be interpreted with some caution. As noted, individuals assigned to the Rule-of-Thumb training reported lower revenues in these periods than those assigned to the control group. These differences in baseline characteristics are not significant at conventional levels; however, the treatment effect is insignificant when the controls for baseline revenues are dropped. With

this caveat in mind, these results parallel those of Karlan and Valdivia (2010) and Berge, Bjorvatn, and Tungodden (2010), both of which find revenue improvements in bad periods. The findings remain consistent with the possibility that effective training may operate by helping individuals to better manage negative shocks or by alerting them to such shocks such that they can counteract the effect of slow weeks. There are no discernible effects of the accounting program on revenues.

We do not find an impact of either program on total firm expenses. However, as shown in Table 3 and consistent with De Mel, McKenzie, and Woodruff (2009), standard errors for the estimates are large. As a result, we cannot rule out economically large impacts, either positive or negative.

Table 4 describes the effects of training on institutional outcomes. The Accounting treatment had no discernible effects on loan size, loan type, savings, or dropout. Those assigned to the Rule-of-Thumb treatment are approximately 6% more likely to save, with the result marginally significant. Point estimates for effect of training on their savings in the month immediately prior to the endline survey are large—an increase of RD\$829 or nearly 20% of the endline mean—but not statistically significant. There is no evidence that the Rule-of-Thumb training causes any other changes in institutional outcomes.

In Tables 5 and 6 we now want to test whether there are heterogeneous treatment effects for different subgroups of the population. In particular we focus on four dimensions: (1) we differentiate participants with high school education or above from those with less education in order to test whether the effectiveness of training depends on the participants schooling level; (2) we compare firms that are predominantly in trade (buying and selling of goods) versus small manufacturing and services since the former businesses might show results more quickly due to the faster working capital cycle in these firms; (3) we compare participants who have group loans versus individual loans since one might be concerned that the difference in the structure of these two loan groups could interact with the effectiveness of training; (4) we compare individuals in the lower and upper quartiles of baseline business management practices.

Table 5 reports the impact of the Rule-of-Thumb training for these different subgroups while Table 6 repeats the regressions for the accounting training. Each of the cells in these tables reports the coefficient on the treatment dummy in separate regressions for the outcome variables indicated. In the first two columns of Table 5 we compare the impact of the Rule-of-Thumb treatment when splitting the sample into clients with at least a high school education and those who completed less than high school. The treatment had a larger effect on more educated



clients' likelihood to separate business and personal cash and likelihood to save, but otherwise there is not a consistent difference in the treatment effect between these two groups. The Rule-of-Thumb treatment had positive effects on both groups. In columns 3 and 4 we split the sample into trading businesses (buy and sell) versus others. There is some suggestive evidence that the Rule-of-Thumb training had a larger effect on trading businesses; however, only the difference in savings rates is significant at conventional levels, and the aggregate difference is inconclusive. Similarly, and in contrast to the expectations, columns 5 and 6 demonstrate that treatment effects are nearly identical for group versus individual borrowers. We find a heterogeneous interaction of the Rule-of-Thumb treatment and prior interest in training across various business and personal financial practice measures—with individuals demonstrating a prior interest exhibiting a substantially larger response on some dimensions (e.g., setting aside cash for business expenditures) and a lower response on others (e.g., separating accounts or keeping accounting records). Point estimates for the impact on sales and savings are also strongest for those expressing less interest in training, but these differences are not statistically significant. In contrast, the accounting training does appear to have a greater benefit on those who expressed a prior interest in training, with those who expressed interest in the baseline improving on the aggregate measure of business practices by 0.16 standard deviations relative to no improvement for those who did not. This stands in contrast to the results of Karlan and Valdivia (2010). We hypothesize that this difference stems from the voluntary nature of ADOPEM's training program—individuals who were not sufficiently interested in training could opt out at any time—versus the mandatory program studied by Karlan and Valdivia. It suggests that in certain circumstances the price mechanism may effectively allocate training programs.

Columns 9 and 10 show that the Rule-of-Thumb training had a substantially larger impact on businesses with the worst management practices in the baseline. Those beginning in the lowest quartile improved by 0.20 standard deviations in their aggregate measure of business practices relative to a modest improvement for those beginning in the top quartile. There is no distinguishable difference across the quartiles for those receiving the accounting training.

In Table 6 we now repeat the exact same set of regressions for the different subsamples as in Table 5 but for the sample of participants who received the accounting training. Parallel to the overall results reported in Table 3 we do not find a significant impact of the Accounting treatment on the different subgroups of clients and their outcomes. However, there is one notable exception: Less educated

clients seem to experience a significant drop in their weekly sales as measured by “last week sales” and also when asked about their “sales in a bad week”. The effect is substantial, 0.2 standard deviations from the baseline reported value. This result is quite surprising but could be driven by several different channels besides a causal effect of lower sales from accounting training. We conjecture that one possible interpretation for this finding is that clients either are more realistic about their actual sales once they went through the training while prior to the training they might have inflated the number.

Table 7 reports the impact of follow-up visits, conditional upon attending the first class, at which follow-up treatments were randomly assigned. Overall we do not find evidence of any positive impact from these visits. The follow-up visits do not reinforce the positive level effects we documented for the rule-of-thumb training, nor do they seem to help clients who received the standard Accounting training to achieve better outcomes. Most of the coefficient on the interaction of the level effect with the intense follow dummy up are close to zero or estimated with large error. One interpretation of these results is that problems with implementation of the materials did not contribute to the lack of effect for the Accounting training maybe because customers realized from the start that this material was not going to be useful for them. For the Rule-of-Thumb training we can conjecture that the material was simple enough that additional help with implementation through follow up visits were not needed (but also did not persuade any additional recipients to adopt the treatment). A less flattering interpretation for us would be that the follow up visits themselves were not effective and maybe more substantive hand holding might have been needed. However, we think an even more involved follow up may have been disruptive to the small business owners.

Table 8 reports the effects of the treatment on the treated for both the accounting and Rule-of-Thumb training according to equation (4). These estimates represent the Wald Estimator for the treatment effect, effectively rescaling the intention to treat effect by the probability of attending the course conditional on assignment to the treatment. Consistent with the results reported in Table 3, we see large and statistically significant effects from the Rule-of-Thumb treatment on business practices and an economically and statistically significant increase in reported sales in bad weeks. While the effects of the accounting training lack statistical significance, there is a consistent pattern of negative reported effects on measures of sales performance.

Table 9 reports the results for regression of standardized treatment effects for each component and aggregate family totals grouped as business practices,

business performance, personal outcomes, and personal financial practices. As shown in the table, the Rule-of-Thumb training substantially improved aggregate measures of business and personal financial practice. While the effect on aggregate business outcomes is not statistically significant, the Rule-of-Thumb training did improve aggregate personal outcomes. Large increases in treated individuals' self-reported economic situation and their subjective economic situation relative their neighbors drive these results.<sup>13</sup> There is no demonstrable effect from the accounting training.

Finally, we consider the effect of both training programs on the objective measure of financial reporting quality. Table 10 reports the results of estimating equations (1) and (6) where the outcome of interest is the index of reporting errors as described in the prior section. As shown in columns (3) and (4), the Rule-of-Thumb training reduced the incidence of reporting errors by 8 to 9 percentage points relative. This effect appears to be independent of education levels. In contrast, the main effect of the accounting training shows little effect; however, those individuals with at least a high school education who were assigned to the accounting training committed 16 percentage points fewer errors than the control group. This result suggests that even seemingly simple training programs may require relatively high levels of existing education to be effective. In contrast, well-chosen, easily-learned rules of thumb appear to be more robust and more likely to be followed.

Table 11 reports the results of bounds estimation on the treatment effect for the Rule-of-Thumb training. While the bounds span a large range of potential effects, the estimated effect on business practices is quite robust. Even with the relatively severe assumption that those attriting from the treatment group are 0.25 standard deviations below the mean and those attriting from the control group are 0.25 standard deviations above, we still find a significant, positive effect from the Rule-of-Thumb training.

## 6 Conclusion

The results from this study suggest that improved knowledge of finance and financial accounting indeed has a positive effect on the management practices of small businesses in an emerging market such as the Dominican Republic. However, we show that the impact of such training crucially depends on the form in

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<sup>13</sup>See appendix table A3 for detail. Tables A2 through A4 report the disaggregated elements for each component.

which financial literacy training is provided. In this setting, training that relies on the standard approach to small business training, teaching the fundamentals of financial accounting, had no measurable effect. But the training program based on simple rules of thumb led to significant improvements in the way businesses managed their finances relative to the control group that was not offered training. Businesses in the Rule-of-Thumb training were more likely to implement the material that was taught, keep accounting records, calculate monthly revenues and separate their business and home financial records. Improvements along these dimensions are on the order of ten percentage points.

These changes in management practices translate into business outcomes. We find larger improvements for the group receiving the rule-of-thumb training compared to the group in the basic accounting training. In particular, we see a large increase in the level of sales during bad weeks—30% for people in the rule-of-thumb-based training—and a substantial but not statistically significant increase in average sales and an aggregate measure of business outcomes. We also find an economically large increase in savings of 6% for the rule-of-thumb training, but the result is only significant at the 10%-level. In contrast the basic accounting training produces no significant effects.

Based on these findings, it appears that significant gains could be made by simplifying training programs and relying more on easy-to-implement, practical “rules of thumb.” On a day-to-day basis, the rule-of-thumb-based approach performs better than teaching accounting and finance from first principles. However, more research is needed to investigate how the results generalize and how rules of thumb can be optimized for maximum impact and adjusted to the level of experience and expectation of different types of business owners. Moreover, we believe that going forward it will be important to understand in more detail the potential costs and benefits of Rule-of-Thumb based learning, e.g., are there situations where rule-of-thumb-based training make it more difficult for businesses to adjust to new circumstances or make sense of unforeseen developments.

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**Table 1: Baseline Summary Statistics**

	Obs.	Full Sample Mean	Control	Accounting Treatment	Diff. from Control	Rule of Thm. Treatment	Diff. from Control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Borrower Characteristics</i>							
Age	1,189	40.2 (10.4)	40.1 (10.5)	40.7 (10.3)	0.58 [0.44]	40.0 (10.5)	-0.08 [0.92]
Female	1,193	0.90 (0.30)	0.90 (0.30)	0.90 (0.30)	0.00 [0.86]	0.90 (0.30)	0.01 [0.75]
Number of Children	1,193	2.9 (1.7)	2.9 (1.7)	3.1 (1.8)	0.17 [0.17]	2.9 (1.7)	0.00 [0.98]
Any Savings	1,193	0.66 (0.47)	0.68 (0.47)	0.62 (0.49)	-0.06 [0.08]	0.68 (0.47)	-0.01 [0.85]
High school education or more	1,193	0.35 (0.48)	0.37 (0.48)	0.36 (0.48)	-0.01 [0.69]	0.33 (0.47)	-0.04 [0.27]
Expressed interest in financial training	1,193	0.63 (0.48)	0.65 (0.48)	0.59 (0.49)	-0.06 [0.09]	0.65 (0.48)	0.00 [0.99]
Sales and trading business	1,193	0.50 (0.50)	0.48 (0.50)	0.50 (0.50)	0.02 [0.49]	0.52 (0.50)	0.04 [0.27]
<i>B. Loan Characteristics</i>							
Individual loan	1,183	0.61 (0.49)	0.61 (0.49)	0.60 (0.49)	0.00 [0.89]	0.62 (0.49)	0.01 [0.70]
Amount of last ADOPEM loan	1,191	26,514 (17,411)	26,702 (18,126)	26,500 (17,366)	-202 [0.87]	26,349 (16,790)	-353 [0.78]
<i>C. Sales Performance, \$RD</i>							
Weekly Average	972	6,591 (10,719)	6,855 (11,087)	6,791 (11,737)	-64 [0.94]	6,133 (9,199)	-722 [0.37]
Last Week	940	5,317 (9,804)	5,923 (10,480)	5,264 (10,085)	-659 [0.42]	4,760 (8,742)	-1163 [0.13]
Good Week	961	8,111 (13,765)	8,188 (13,980)	8,254 (14,344)	66 [0.95]	7,886 (12,962)	-302 [0.78]
Bad Week	960	3,730 (8,253)	4,275 (10,588)	3,708 (7,735)	-567 [0.44]	3,207 (5,701)	-1067 [0.11]
<i>D. Business Practices</i>							
Sep. business and personal cash	1,159	0.74 (0.44)	0.75 (0.43)	0.74 (0.44)	-0.01 [0.82]	0.72 (0.45)	-0.03 [0.30]
Keep accounting records	1,163	0.66 (0.47)	0.68 (0.47)	0.61 (0.49)	-0.07 [0.05]	0.68 (0.47)	0.00 [0.95]
Sep. business and personal acct.	1,160	0.53 (0.50)	0.56 (0.50)	0.50 (0.50)	-0.07 [0.07]	0.54 (0.50)	-0.02 [0.51]
Calculate revenues formally	1,161	0.80 (0.40)	0.80 (0.40)	0.82 (0.39)	0.02 [0.50]	0.79 (0.41)	-0.01 [0.82]
Observations		1,193	387	402		404	

Notes: This table presents summary statistics based on baseline survey data. Standard errors of variables appear in parenthesis and p-values for differences of means appear in square brackets. Section 3 describes both treatment groups, columns (4) and (6), in detail.



**Table 2: Determinants of Attendance**

	Attend any class <sup>/a</sup>
Female	0.023 (0.066)
Number of children	0.029** (0.012)
Any savings	0.026 (0.042)
High school education or more	0.092** (0.043)
Index of spending behavior <sup>b</sup>	-0.163*** (0.049)
Interested in accounting training <sup>/c</sup>	0.080** (0.039)
Interested in saving more <sup>/c</sup>	-0.045 (0.050)
Interested in cash mgmt. <sup>/c</sup>	0.047 (0.052)
Current loan (0000)	-0.001 (0.013)
Planned loan amount (0000)	0.000 (0.005)
Loan planned for fixed assets (0000)	0.025** (0.012)
Weekly Average, Sales (0000)	-0.044* (0.023)
Aggregate business practice measures <sup>b</sup>	-0.039 (0.039)
Buy-sell business in baseline	0.003 (0.040)
Reports business improving	0.064** (0.027)
Constant	0.287*** (0.089)
N	653

## Notes:

<sup>/a</sup> OLS regression of attending any class on the dependent variables indicated, conditional on treatment assignment. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Aggregate z-score indices. Index of spending behavior based on gambling, regretting purchase decisions, buying from door-to-door vendors, meals away from home, and spending on furniture. Higher scores indicate less spending discipline. Revenue measure based on aggregate of all reported revenue measures. Business practice measures detailed in table A1.

<sup>/c</sup> Baseline reported interest in specific forms of training as indicated.

**Table 3: Impact of Training on Business Practices and Performance<sup>/a</sup>**

	Obs.	Control		Accounting		Rule of Thumb		p-value for test of equality <sup>c</sup>		Any Treatment	
		Mean	Treatment	Incl.	Treatment	Incl.	Treatment	Incl.	Treatment	Incl.	
		(2)	Only	Covariates <sup>b</sup>	Only	Covariates <sup>b</sup>	Only	Covariates <sup>b</sup>	Only	Covariates <sup>b</sup>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
<b>Business and Personal Financial Practices</b>											
Sep. business and personal cash	794	0.56 (0.50)	0.00 (0.03)	0.00 (0.03)	0.08*** (0.03)	0.08*** (0.03)	0.010	0.013	0.04 (0.03)	0.04 (0.03)	
Keep accounting records	795	0.46 (0.50)	0.04 (0.05)	0.04 (0.05)	0.11*** (0.03)	0.11*** (0.03)	0.128	0.095	0.08** (0.04)	0.08** (0.04)	
Sep. business and personal acct.	792	0.40 (0.49)	0.04 (0.05)	0.04 (0.05)	0.11*** (0.03)	0.11*** (0.03)	0.141	0.103	0.08** (0.03)	0.08** (0.03)	
Set aside cash for business exp.	794	0.39 (0.49)	0.07** (0.03)	0.07** (0.03)	0.12*** (0.04)	0.12*** (0.04)	0.161	0.170	0.10*** (0.03)	0.10*** (0.03)	
Calculate revenues formally	795	0.57 (0.50)	0.02 (0.04)	0.02 (0.04)	0.06** (0.03)	0.06** (0.03)	0.211	0.235	0.04 (0.03)	0.04 (0.03)	
Aggregate business practices <sup>/d</sup>	804	-0.04 (0.60)	0.07 (0.05)	0.07 (0.06)	0.14*** (0.04)	0.15*** (0.04)	0.193	0.163	0.11*** (0.04)	0.11*** (0.04)	
<b>Business Performance</b>											
Weekly Average, Sales <sup>/e</sup>	571	8,711 (11,710)	-582 (794)	-685 (808)	566 (886)	450 (865)	0.264	0.276	21 (669)	-92 (657)	
Last Week, Sales <sup>/e</sup>	507	6,880 (10,229)	-970 (645)	-1,017 (640)	412 (799)	408 (779)	0.037	0.039	-258 (641)	-286 (620)	
Good Week, Sales <sup>/e</sup>	568	10,219 (13,647)	-839 (930)	-833 (948)	28 (955)	-59 (891)	0.391	0.409	-393 (791)	-433 (785)	
Bad Week, Sales <sup>/e</sup>	551	5,232 (7,880)	-669 (507)	-660 (514)	967* (523)	979* (524)	0.003	0.002	176 (438)	190 (451)	
Weekly expenses <sup>/e</sup>	497	3,192 (6,422)	-68 (758)	-153 (720)	184 (733)	228 (698)	0.732	0.584	57 (650)	37 (619)	

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (1) for columns (3) and (6) and equation (2) for columns (2) and (5). Standard errors, clustered at the barrio-level, in parentheses. Regression includes only those individuals with own business. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account.

<sup>/c</sup> p-value for F-test of equality of Accounting and Rule of Thumb treatment effect coefficients.

<sup>/d</sup> Aggregate is unweighted sum of z-scores for all business practices as detailed in Table A1.

<sup>/e</sup> Variable winsorized at 1%.

**Table 4: Impact of Training on Institutional Outcomes<sup>/a</sup>**

	Obs.	Control Mean	Accounting		Rule of Thumb		p-value for test of equality <sup>/c</sup>		Any Treatment	
			Treatment	Incl.	Treatment	Incl.	Treatment	Incl.	Treatment	Incl.
			Only	Covariates <sup>/b</sup>	Only	Covariates <sup>/b</sup>	Only	Covariates <sup>/b</sup>	Only	Covariates <sup>/b</sup>
(1)	(2)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Loan size, \$RD	1,027	36,572 (25,439)	-447 (1,035)	-377 (937)	824 (1,429)	593 (1,331)	0.353	0.386	186 (1,040)	105 (1,001)
Any savings	1,030	0.53 (0.50)	0.01 (0.04)	0.01 (0.04)	0.06 (0.04)	0.06 (0.04)	0.141	0.127	0.03 (0.03)	0.03 (0.03)
Savings last month, \$RD <sup>/c</sup>	977	1,755 (6,808)	276 (508)	285 (517)	829 (572)	869 (581)	0.342	0.323	552 (458)	576 (466)
Individual loan	1,020	0.61 (0.49)	0.01 (0.02)	0.01 (0.02)	0.00 (0.03)	0.00 (0.03)	0.770	0.847	0.00 (0.02)	0.01 (0.02)
Dropout <sup>/d</sup>	1,191	0.46 (0.50)	0.02 (0.05)	0.01 (0.05)	0.05 (0.04)	0.04 (0.04)	0.508	0.527	0.03 (0.04)	0.03 (0.04)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (1) for columns (3) and (6) and equation (2) for columns (2) and (5). Baseline level of dependent variable excluded for dropout regression. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account.

<sup>/c</sup> Results reflect OLS regression of savings amount on treatment indicator, unconditional on any savings. Results of CLAD and Tobit regressions are not significant at the 10%-level.

<sup>/d</sup> No loans taken in prior twelve months.

**Table 5: Impact of Rule of Thumb Training, by Subgroup<sup>a</sup>**

	Education level <sup>b</sup>		Business Type		Loan Type, Baseline		Prior Interest in Training		Baseline Bus. Prac	
	Low	High	Buy-Sell <sup>b</sup>	Other	Group	Indiv.	Yes	No	<25th Pctle	>75th Pctle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b>Business and Personal Financial Practices</b>										
Sep. business and personal cash <sup>c</sup>	0.06 (0.04)	0.12** (0.05)	0.05* (0.03)	0.12* (0.07)	0.08* (0.04)	0.08** (0.04)	0.08* (0.04)	0.09* (0.04)	0.15* (0.09)	-0.01 (0.04)
Keep accounting records <sup>c</sup>	0.11*** (0.04)	0.11 (0.08)	0.10*** (0.03)	0.12** (0.06)	0.11* (0.06)	0.10** (0.05)	0.08 (0.05)	0.14** (0.06)	0.12* (0.07)	0.07 (0.06)
Sep. business and personal acct. <sup>c</sup>	0.11*** (0.04)	0.11* (0.06)	0.09** (0.04)	0.14** (0.06)	0.15** (0.07)	0.09* (0.05)	0.06 (0.05)	0.16*** (0.05)	0.11 (0.07)	0.05 (0.06)
Set aside cash for business exp. <sup>c</sup>	0.11** (0.04)	0.15** (0.07)	0.09* (0.05)	0.16*** (0.06)	0.06 (0.07)	0.14*** (0.04)	0.19*** (0.05)	0.06 (0.05)	0.25*** (0.06)	0.04 (0.07)
Calculate revenues formally <sup>c</sup>	0.09** (0.04)	0.02 (0.06)	0.09** (0.04)	0.02 (0.06)	0.08* (0.04)	0.06 (0.04)	0.07 (0.04)	0.06 (0.05)	0.06 (0.07)	0.02 (0.06)
Aggregate business practices <sup>c</sup>	0.16*** (0.05)	0.10 (0.07)	0.14*** (0.04)	0.15** (0.07)	0.12** (0.06)	0.15*** (0.06)	0.17*** (0.04)	0.12* (0.06)	0.20** (0.08)	0.05 (0.06)
Any savings	0.01 (0.05)	0.15** (0.07)	0.05 (0.06)	0.07 (0.05)	0.05 (0.05)	0.07 (0.05)	0.04 (0.06)	0.08 (0.05)	0.09 (0.07)	0.06 (0.07)
Savings amount, \$RD <sup>e</sup>	1,825 (3,100)	4,470 (5,615)	-692 (3,498)	5,270 (4,534)	1,813 (2,476)	1,690 (3,499)	-2,258 (2,709)	4,757 (4,129)	1,843 (4,190)	5,184 (8,026)
<b>Business Performance</b>										
Total number of employees <sup>c</sup>	-0.28*** (0.10)	0.27* (0.16)	-0.03 (0.11)	-0.07 (0.17)	0.08 (0.13)	-0.11 (0.12)	-0.01 (0.11)	-0.09 (0.15)	-0.22 (0.17)	0.04 (0.21)
Weekly Average, Sales <sup>c/d</sup>	741 (1,173)	-143 (1,571)	732 (1,328)	215 (1,118)	-1,955 (1,236)	1,837 (1,118)	578 (1,094)	539 (1,512)	359 (1,869)	1,273 (1,315)
Last Week, Sales <sup>c/d</sup>	-387 (1,037)	931 (1,544)	-57 (1,052)	906 (1,317)	-990 (1,223)	1,102 (928)	-110 (694)	873 (1,437)	-1,517 (2,344)	1,817 (1,814)
Good Week, Sales <sup>c/d</sup>	236 (1,278)	-664 (1,601)	295 (1,168)	-244 (1,220)	-1,212 (1,484)	728 (1,133)	-345 (1,111)	426 (1,571)	-579 (1,723)	1,704 (1,482)
Bad Week, Sales <sup>c/d</sup>	563 (614)	1,281 (1,128)	1,018 (678)	857 (972)	-808 (901)	1,845*** (667)	853 (723)	1,066 (974)	-75 (1,188)	1,161 (1,312)

Notes:

<sup>a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (1). Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>b</sup> Education subgroups separated by high school or above (High) or less than high school (Low); trading business or other type of business; and participation in individual or group loan in baseline.

<sup>c</sup> Regression includes only those individuals with own business.

<sup>d</sup> Variable winsorized at 1%.

<sup>e</sup> Results reflect OLS regression of savings amount on treatment indicator, unconditional on any savings. Results of CLAD and Tobit regressions are not significant at the 10%-level.

**Table 6: Impact of Accounting Training, by Subgroup<sup>a</sup>**

	Education level <sup>b</sup>		Business Type		Loan Type, Baseline		Prior Interest in Training		Baseline Bus. Prac	
	Low	High	Buy-Sell <sup>b</sup>	Other	Group	Indiv.	Yes	No	<25th Pctle	>75th Pctle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b><i>Business and Personal Financial Practices</i></b>										
Sep. business and personal cash <sup>c</sup>	-0.02 (0.05)	0.03 (0.05)	-0.01 (0.04)	0.01 (0.06)	-0.03 (0.06)	0.02 (0.04)	0.05 (0.04)	-0.05 (0.05)	0.02 (0.08)	-0.11* (0.06)
Keep accounting records <sup>c</sup>	0.04 (0.05)	0.05 (0.11)	0.08 (0.05)	-0.02 (0.08)	0.12** (0.06)	0.00 (0.06)	0.06 (0.08)	0.03 (0.06)	-0.01 (0.07)	0.00 (0.10)
Sep. business and personal acct. <sup>c</sup>	0.04 (0.04)	0.04 (0.09)	0.05 (0.06)	0.03 (0.07)	0.08 (0.06)	0.02 (0.06)	0.09 (0.07)	0.01 (0.05)	-0.03 (0.07)	-0.13 (0.10)
Set aside cash for business exp. <sup>c</sup>	0.06 (0.05)	0.09 (0.06)	0.05 (0.05)	0.09* (0.05)	0.01 (0.06)	0.09** (0.04)	0.11** (0.04)	0.04 (0.05)	0.07 (0.07)	-0.04 (0.07)
Calculate revenues formally <sup>c</sup>	0.02 (0.04)	0.01 (0.07)	0.11*** (0.03)	-0.10 (0.07)	0.05 (0.05)	0.00 (0.05)	0.06 (0.04)	-0.01 (0.06)	-0.05 (0.06)	0.00 (0.06)
Aggregate business practices <sup>c</sup>	0.07 (0.04)	0.09 (0.12)	0.13** (0.05)	0.00 (0.08)	0.11* (0.06)	0.05 (0.07)	0.16** (0.07)	0.00 (0.06)	-0.03 (0.07)	-0.08 (0.09)
Any savings	-0.03 (0.05)	0.07 (0.07)	0.03 (0.05)	-0.03 (0.05)	0.00 (0.05)	0.02 (0.06)	-0.01 (0.06)	0.03 (0.05)	-0.11 (0.07)	-0.06 (0.08)
Savings amount, \$RD <sup>e</sup>	2,213 (7,233)	2,734 (4,836)	-3,509 (3,204)	9,137 (11,533)	-2,288 (2,781)	5,520 (8,404)	4,484 (8,369)	-1,111 (4,308)	-4,242 (2,818)	-3,575 (6,703)
<b><i>Business Performance</i></b>										
Total number of employees <sup>c</sup>	-0.16* (0.09)	0.47** (0.20)	0.12 (0.12)	0.01 (0.14)	0.10 (0.19)	0.06 (0.11)	0.23 (0.17)	-0.04 (0.15)	-0.19 (0.18)	-0.07 (0.21)
Weekly Average, Sales <sup>c/d</sup>	-821 (1,019)	-548 (1,707)	-265 (992)	-963 (1,295)	253 (1,862)	-821 (1,021)	1,710 (1,519)	-2,360** (1,181)	-3,128** (1,302)	2,098 (1,755)
Last Week, Sales <sup>c/d</sup>	-1,749** (765)	-175 (1,335)	-918 (653)	-895 (1,005)	-96 (1,291)	-1,538* (787)	-256 (666)	-1,507 (1,046)	-4,303** (1,767)	-1,076 (948)
Good Week, Sales <sup>c/d</sup>	-1,945 (1,279)	1,046 (2,004)	-1,177 (1,241)	-474 (1,606)	1,228 (1,869)	-1,462 (1,092)	-1,414 (1,340)	-641 (1,564)	-2,235 (1,628)	1,672 (1,743)
Bad Week, Sales <sup>c/d</sup>	-1,381** (543)	380 (1,068)	-527 (670)	-876 (744)	28 (1,058)	-1,037* (617)	-337 (701)	-678 (801)	-2,815** (1,183)	-777 (996)

Notes:

<sup>a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (1). Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>b</sup> Education subgroups separated by high school or above (High) or less than high school (Low); trading business or other type of business; and participation in individual or group loan in baseline.

<sup>c</sup> Regression includes only those individuals with own business.

<sup>d</sup> Variable winsorized at 1%.

<sup>e</sup> Results reflect OLS regression of savings amount on treatment indicator, unconditional on any savings. Results of CLAD and Tobit regressions are not significant at the 10%-level.

**Table 7: Impact of Training on Business Practices and Performance, by Intensity Conditional on Attending First Class**

	Accounting	Rule of Thumb
	Intense <sup>/a</sup>	Intense <sup>/a</sup>
	(1)	(2)
<b><i>Business and Personal Financial Practices</i></b> <sup>/c</sup>		
Sep. business and personal cash <sup>/d</sup>	0.06 (0.09)	-0.11 (0.07)
Keep accounting records <sup>/d</sup>	-0.03 (0.09)	0.00 (0.09)
Sep. business and personal acct. <sup>/d</sup>	-0.05 (0.09)	-0.06 (0.08)
Calculate revenues formally <sup>/d</sup>	-0.11* (0.06)	0.07 (0.09)
Has employees <sup>/d</sup>	0.07 (0.07)	-0.04 (0.07)
Any savings	0.07 (0.07)	-0.18** (0.09)
Savings amount, \$RD <sup>/f</sup>	524 (6,255)	-7,721 (5,515)
Dropout <sup>/g</sup>	-0.06 (0.09)	-0.06 (0.10)
<b><i>Business Performance</i></b>		
Total number of employees <sup>/d</sup>	-0.19 (0.29)	0.07 (0.25)
Weekly Average, Sales <sup>/d/e</sup>	349 (1,306)	2,477 (2,148)
Last Week, Sales <sup>/d/e</sup>	567 (1,187)	1,344 (1,654)
Good Week, Sales <sup>/d/e</sup>	1,537 (1,715)	-621 (2,184)
Bad Week, Sales <sup>/d/e</sup>	1,024 (712)	1,767 (1,432)

Notes:

<sup>/a</sup> Values in each row in each set of basic and intense columns (e.g., (1) and (2)) represent the coefficients from a regression of the form  $y_{it} = \alpha + \beta_1 x_{it} + \gamma x_{it,B} + \epsilon_{it}$  as shown in equation (3). Sample restricted to those attending first class, where intensity was assigned. Intensity is an indicator for additional training follow up visits, as described in Section 4. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Low education is defined as less than high school. High education includes completing high school or greater.

<sup>/c</sup> See section 3 for detailed description of treatments.

<sup>/d</sup> Regression includes only those individuals reporting own business in baseline survey.

<sup>/e</sup> Variable winsorized at 1%.

<sup>/f</sup> Results reflect OLS regression of savings amount on treatment indicator, unconditional on any savings. Results of CLAD and Tobit regressions are not significant at the 10%-level.

<sup>/g</sup> No loans taken in prior twelve months.

**Table 8: Impact of Training on Business Practices and Performance**  
**Treatment on the Treated<sup>a/b</sup>**

	Obs.	Accounting		Rule of Thumb		Any Treatment	
		Treatment	Incl. Covariates <sup>b</sup>	Treatment	Incl. Covariates <sup>b</sup>	Treatment	Incl. Covariates <sup>b</sup>
		(1)	(2)	(3)	(4)	(5)	(6)
<b><i>Business and Personal Financial Practices</i></b>							
Sep. business and personal cash	794	0.00 (0.06)	-0.01 (0.06)	0.17** (0.07)	0.17** (0.07)	0.08 (0.05)	0.08 (0.05)
Keep accounting records	795	0.08 (0.10)	0.07 (0.10)	0.23*** (0.06)	0.23*** (0.06)	0.15** (0.07)	0.15** (0.07)
Sep. business and personal acct.	792	0.08 (0.10)	0.07 (0.10)	0.24*** (0.06)	0.24*** (0.06)	0.16** (0.06)	0.15** (0.07)
Calculate revenues formally	795	0.03 (0.07)	0.03 (0.07)	0.13** (0.06)	0.13** (0.06)	0.08 (0.05)	0.08 (0.06)
Has employees	794	0.05 (0.07)	0.06 (0.07)	-0.07 (0.10)	-0.06 (0.09)	-0.01 (0.06)	0.00 (0.07)
Total number of employees	794	0.14 (0.17)	0.13 (0.17)	-0.11 (0.19)	-0.07 (0.19)	0.02 (0.13)	0.03 (0.13)
<b><i>Business Performance</i></b>							
Weekly Average, Sales <sup>c</sup>	571	-1,138 (1,522)	-1,341 (1,538)	1,201 (1,893)	973 (1,854)	43 (1,358)	-188 (1,323)
Last Week, Sales <sup>c</sup>	507	-1,826 (1,219)	-1,920 (1,223)	817 (1,567)	815 (1,526)	-498 (1,225)	-552 (1,183)
Good Week, Sales <sup>c</sup>	568	-1,600 (1,758)	-1,583 (1,792)	57 (1,956)	-115 (1,824)	-780 (1,566)	-857 (1,547)
Bad Week, Sales <sup>c</sup>	551	-1,293 (955)	-1,284 (967)	2,045* (1,131)	2,086* (1,123)	357 (887)	382 (900)

<sup>a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (4), instrumenting attendance with assignment to the treatment. No individuals assigned to the control group attended training sessions. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>b</sup> Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account.

<sup>c</sup> Variable winsorized at 1%.

**Table 9: Standardized Treatment Effects  
Business Practices**

	<u>Accounting</u>	<u>Rule of Thumb</u>	<u>Any Treatment</u>
	(1)	(2)	(3)
Aggregate business practices	0.07 (0.06)	0.15*** (0.04)	0.11*** (0.04)
Aggregate business outcomes	-0.03 (0.03)	0.04 (0.04)	0.01 (0.03)
Aggregate personal outcomes	0.00 (0.03)	0.06** (0.03)	0.03 (0.02)
Aggregate personal financial practices	0.04 (0.04)	0.05* (0.03)	0.05 (0.03)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (5). Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text. Aggregates based on unweighted sum of all components, as detailed in tables A1 to A4. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.



**Table 10: Impact of Training on Reporting Quality**

	Accounting		Rule of Thumb		Any Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent Variable: Any Reporting Errors<sup>/b</sup></i>						
Treatment	-0.03 (0.04)	0.02 (0.06)	-0.09*** (0.03)	-0.08* (0.04)	-0.06* (0.03)	-0.03 (0.04)
High Education		0.02 (0.07)		0.02 (0.07)		0.02 (0.07)
Interaction		-0.16 (0.10)		-0.02 (0.09)		-0.09 (0.09)
Observations	804	804	804	804	804	804

Notes:

<sup>/a</sup> Values in columns (1) and (3) and columns (2) and (4) are from a single regression. High Education is an indicator equal to 1 if the individual has a high school education or better. Includes only those individuals reporting own business. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Error defined as reporting bad period revenues better than average or good period; average period revenues better than good; or average profits greater than good period revenues.

**Table 11: Bounds estimates for standardized treatment effects**  
**Rule of Thumb Treatment**

	Lower Bounds <sup>/a</sup>				Unadjusted Treatment Effect	Upper Bounds <sup>/b</sup>			
	0.50 sd (1)	0.25 sd (2)	0.10 sd (3)	0.05 sd (4)		0.05 sd (6)	0.10 sd (7)	0.25 sd (8)	0.50 sd (9)
Business practices	0.022 (0.036)	0.064 (0.036)	0.088 (0.036)	0.097 (0.036)	0.108 (0.042)	0.113 (0.037)	0.122 (0.037)	0.146 (0.038)	0.188 (0.039)
Business outcomes	-0.044 (0.031)	-0.005 (0.031)	0.019 (0.030)	0.026 (0.030)	0.040 (0.033)	0.042 (0.030)	0.050 (0.030)	0.073 (0.031)	0.113 (0.031)
Personal outcomes	-0.012 (0.022)	0.015 (0.022)	0.032 (0.022)	0.037 (0.022)	0.045 (0.025)	0.048 (0.022)	0.053 (0.022)	0.069 (0.022)	0.096 (0.023)
Personal financial practices	0.003 (0.024)	0.030 (0.025)	0.046 (0.025)	0.051 (0.026)	0.052 (0.029)	0.062 (0.026)	0.067 (0.026)	0.083 (0.027)	0.110 (0.028)

Notes:

- <sup>/a</sup> Columns (1) through (4) imputes attrited treatment group as mean of non-attrited treatment minus the indicated fraction of the standard deviation for the non-attrited treatment. Attrited control are imputed as mean of non-attrited control plus the indicated fraction of the standard deviation for the non-attrited control.
- <sup>/b</sup> Columns (6) through (9) imputes attrited treatment group as mean of non-attrited treatment plus the indicated fraction of the standard deviation for the non-attrited treatment. Attrited control are imputed as mean of non-attrited control minus the indicated fraction of the standard deviation for the non-attrited control.
- <sup>/c</sup> Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text.

**Table A1: Summary of Training Programs**

	<b>Rule of Thumb</b>	<b>Accounting</b>
<b>Class 1</b>	<b>Savings</b> <ul style="list-style-type: none"> <li>- Why we should save</li> <li>- Set saving goals</li> <li>- Save for emergencies</li> <li>- Decide how to save</li> <li>- Compare saving services</li> <li>- Plan your future savings</li> </ul>	<i>Same</i>
<b>Class 2</b>	<b>Consumption</b> <ul style="list-style-type: none"> <li>- Financial burden</li> <li>- Study your income and expenses</li> <li>- Plan your future expenses</li> </ul>	<i>Same</i>
<b>Class 3</b>	<b>Debt Management</b> <ul style="list-style-type: none"> <li>- Why borrowing</li> <li>- How much debt I can afford</li> <li>- Default, what is it and how it happens</li> <li>- Cost of default and excessive debt</li> </ul>	<i>Same</i>
<b>Class 4</b>	<b>Account Separation</b> <ul style="list-style-type: none"> <li>- Why separate money for the household from money for the business</li> <li>- Separating house and business money</li> <li>- Setting ourselves a salary</li> <li>- How to keep records of flows between business and household</li> </ul>	<b>Basic Accounting 1</b> <ul style="list-style-type: none"> <li>- Relevance of Accounting</li> <li>- Estimating profits using itemized records or cash accumulation</li> </ul>
<b>Class 5</b>	<b>Estimation Methods</b> <ul style="list-style-type: none"> <li>- Estimate total monthly flow of money between household and business</li> <li>- Estimate increase/decrease of money in the business between beginning and end of the month</li> <li>- Estimating profits</li> </ul>	<b>Basic Accounting 2</b> <ul style="list-style-type: none"> <li>- Including personal income and expenses into the business daily records</li> <li>- Using daily records to estimate daily profit</li> <li>- Review estimating profits using itemized records or cash accumulation</li> <li>- How to include fixed costs into the profit calculations</li> </ul>
<b>Class 6</b>	<i>None</i>	<b>Basic Accounting 3</b> <ul style="list-style-type: none"> <li>- Aggregating daily records into monthly records</li> <li>- Estimating monthly profit</li> <li>- Accounts payable record keeping</li> <li>- Accounts receivable record keeping</li> </ul>

**Table A2: Standardized Treatment Effects  
Business Practices**

	Accounting	Rule of Thumb	Any Treatment
	(1)	(2)	(3)
Keep accounting records	0.08 (0.11)	0.22*** (0.06)	0.15** (0.07)
Sep. business and personal acct.	0.07 (0.11)	0.23*** (0.06)	0.15** (0.07)
Sep. business and personal cash	-0.01 (0.06)	0.16*** (0.06)	0.08 (0.05)
Plans cash needs	0.11 (0.08)	0.18** (0.07)	0.15** (0.07)
Set aside cash for business expenses	0.14** (0.06)	0.24*** (0.07)	0.19*** (0.06)
Calculates profits	0.08 (0.11)	0.15** (0.06)	0.12 (0.08)
Keeps accounts for Acct Receivable	0.05 (0.10)	0.19*** (0.07)	0.12* (0.07)
Keeps accounts for Acct Payable	0.04 (0.10)	0.15** (0.07)	0.09 (0.07)
Keeps accounts for Expenses	0.11 (0.09)	0.17** (0.07)	0.14** (0.07)
Keeps accounts for Sales	0.13 (0.08)	0.06 (0.07)	0.09 (0.07)
Keeps accounts for Inventory	0.06 (0.08)	-0.02 (0.08)	0.02 (0.07)
Accuracy of financial reporting	0.07 (0.09)	0.19*** (0.07)	0.13* (0.07)
Aggregate business practices <sup>/b</sup>	0.07 (0.06)	0.15*** (0.04)	0.11*** (0.04)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (5). Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Aggregate value is unweighted sum of all individual measures.

**Table A3: Standardized Treatment Effects  
Business Performance**

	Accounting	Rule of Thumb	Any Treatment
	(1)	(2)	(3)
Sales last day <sup>b</sup>	-0.07 (0.06)	-0.03 (0.06)	-0.05 (0.05)
Sales average day <sup>b</sup>	-0.04 (0.07)	0.03 (0.08)	0.00 (0.06)
Sales last week <sup>b</sup>	-0.10 (0.06)	0.04 (0.08)	-0.03 (0.06)
Sales average week <sup>b</sup>	-0.05 (0.06)	0.03 (0.07)	-0.01 (0.05)
Sales good week <sup>b</sup>	-0.06 (0.07)	0.00 (0.06)	-0.03 (0.06)
Sales bad week <sup>b</sup>	-0.08 (0.07)	0.12* (0.07)	0.02 (0.06)
Sales last month <sup>b</sup>	0.05 (0.08)	0.05 (0.06)	0.05 (0.06)
Sales average month <sup>b</sup>	-0.01 (0.08)	0.04 (0.06)	0.02 (0.06)
Sales good month <sup>b</sup>	-0.04 (0.07)	0.02 (0.05)	-0.01 (0.05)
Sales bad month <sup>b</sup>	-0.05 (0.09)	-0.01 (0.07)	-0.03 (0.07)
Plan any innovation in business	-0.14* (0.08)	-0.02 (0.08)	-0.08 (0.07)
Total employees	0.05 (0.06)	-0.02 (0.06)	0.01 (0.04)
Prefers own business to RD\$10,000 salary/mo	-0.02 (0.06)	-0.01 (0.05)	-0.01 (0.05)
Aggregate business outcomes <sup>c</sup>	-0.03 (0.03)	0.04 (0.04)	0.01 (0.03)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (5). Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Winsorized at 1%.

<sup>/c</sup> Aggregate value is unweighted sum of all individual measures.

**Table A4: Standardized Treatment Effects  
Personal Outcomes**

	<u>Accounting</u>	<u>Rule of Thumb</u>	<u>Any Treatment</u>
	(1)	(2)	(3)
First child in school	-0.12* (0.06)	-0.07 (0.10)	-0.09 (0.07)
First child working	-0.13* (0.07)	0.07 (0.09)	-0.03 (0.07)
Spending on furniture for home	0.10 (0.09)	0.13 (0.09)	0.11 (0.08)
Owns home	0.12** (0.05)	-0.03 (0.06)	0.04 (0.05)
Reports improving economic situation	0.03 (0.08)	0.12* (0.07)	0.08 (0.07)
Total savings <sup>/b</sup>	-0.09 (0.09)	0.04 (0.07)	-0.03 (0.07)
Dining out or eating meat	-0.09 (0.08)	-0.01 (0.08)	-0.05 (0.07)
Economic situation relative to neighbors	0.13* (0.07)	0.16** (0.07)	0.15*** (0.06)
Aggregate personal outcomes <sup>/c</sup>	0.00 (0.03)	0.06** (0.03)	0.03 (0.02)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (5). Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Winsorized at 1%.

<sup>/c</sup> Aggregate value is unweighted sum of all individual measures.

**Table A5: Standardized Treatment Effects  
Personal Financial Practices**

	<u>Accounting</u>	<u>Rule of Thumb</u>	<u>Any Treatment</u>
	(1)	(2)	(3)
Buy from door-to-door vendors	0.03 (0.11)	0.03 (0.09)	0.03 (0.09)
Regret purchase decisions	-0.01 (0.08)	-0.05 (0.09)	-0.03 (0.08)
Save regularly	0.03 (0.09)	0.16* (0.08)	0.10 (0.07)
Amount saved last month	0.12 (0.14)	0.09 (0.12)	0.10 (0.11)
Any gambling	0.13 (0.09)	0.05 (0.08)	0.09 (0.07)
Use remittances for business purposes <sup>b</sup>	0.05 (0.07)	0.15* (0.08)	0.10 (0.06)
Aggregate personal financial practices <sup>c</sup>	0.04 (0.04)	0.05* (0.03)	0.05 (0.03)

Notes:

<sup>/a</sup> Each coefficient reported in the table is from a separate regression of the form described in equation (5). Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account. All measures converted to standardized z-scores and scaled such that positive values indicate desirable outcomes, as described in text. Standard errors, clustered at the barrio-level, in parentheses. \* Denotes significance at the 10%-level, \*\* at the 5%-level, and \*\*\* at the 1% level.

<sup>/b</sup> Baseline value not available.

<sup>/c</sup> Aggregate value is unweighted sum of all individual measures.