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## Keeping it simple: financial literacy and rules of thumb

## Discussion paper

# Keeping it Simple: Financial Literacy and Rules of Thumb

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## Abstract

Individuals and micro-entrepreneurs often lack sufficient financial literacy to make the complex financial decisions they face. We conduct a randomized control trial with a bank in the Dominican Republic to compare the impact of two distinct financial literacy programs. The first is a standard, fundamentals-based accounting training. The second is a simplified, rule-of-thumb training that teaches basic heuristics to manage finances. We find that only the latter produced significant improvements in business practices and outcomes. The impact is especially pronounced for micro-entrepreneurs with poor financial literacy upfront. These results suggest a possible advantage to reducing the complexity of training programs.

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

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

# 1  Introduction

Individuals and micro-entrepreneurs alike are asked to make complex 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) and 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 programs are large; however, self-selection into training makes it hard to interpret the results as causal. 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 et al. (2009) find only modest effects from a financial literacy training program in Indonesia. One challenge in studying the impact of such financial literacy programs is that measured impacts conflate the usefulness of the financial skills themselves with whether they can be transmitted via the specific training methodology. To date we have only very limited systematic knowledge about the dimensions that determine a financial literacy program's effectiveness. The impact of a program might be crucially driven by the complexity of the materials, since any training program faces a trade-off between the ease with which participants can grasp the concepts and their potential depth of understanding.

To advance our knowledge we conducted a randomized control experiment to compare the impact of two financial training programs on firm and individual-level

outcomes for micro-entrepreneurs in the Dominican Republic. By randomizing access to the training, we overcome issues of selection bias that confounded earlier studies. In order to understand the mechanisms through which financial training may or may not affect recipients, we also developed two distinct types of training that are at different points along the spectrum between comprehensiveness of the material and ease of understanding. The first training program closely follows a standard approach to small business training, which usually teaches relatively detailed material on the fundamentals of financial accounting. It is designed to teach micro-entrepreneurs the basics of double-entry accounting, working capital management, and investment decisions. Similar programs are used around the world by groups such as Freedom from Hunger, the International Labor Organization, and BRAC. In contrast, the second program, rule-of-thumb training, focuses on very simple rules of thumb or routines for financial decision making without aiming to provide comprehensive accounting knowledge. For example, both the standard accounting and rule-of-thumb trainings taught participants to separate their business and person accounts. In the standard accounting training, this was followed by instruction for how to calculate business profits in accordance with a typical accounting curriculum for micro-entrepreneurs. In contrast, the rule-of-thumb training gave them a physical heuristic of how to keep money in two separate drawers (or purses) and to only transfer money from one drawer to the other with an explicit "IOU" note between the business and the household. This gave entrepreneurs a simple way to figure out the profits of their businesses: at the end of the month, count how much money was in the business drawer. Our aim in designing and evaluating these two distinct programs was assess the possibility that there may be advantages to such reductions in the complexity of training programs.

Between November 2006 and July 2008, we implemented a randomized control trial of these two training programs in collaboration with ADOPEM, a microfinance institution that lends to individuals and small businesses in the Dominican Republic. We selected 1193 existing clients of ADOPEM who had expressed an interest in training and randomly assigned them to one of the two trainings or a control group. In addition to this core comparison, we designed the study to better understand potential limitations to either type of classroom-based financial training. If a treatment were to have no effect, it could be because participants did not understand and implement what was taught in class or because the material itself, even when properly understood, was not helpful. Therefore, we randomly assigned approximately half of those attending each treatment to receive

supplemental, one-on-one training. A financial trainer visited their place of business to review the class materials and, when necessary, 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.[1] The remaining participants did not receive any training beyond what was offered in the classroom. If we do not see an effect for even those receiving the intensive follow-up, we can conclude that the material itself does not have an impact. Differences between those receiving the intensive follow-up and those receiving the standard treatment can be ascribed to the delivery mechanism.

Our results show an asymmetric impact of the rule-of-thumb training compared to the standard accounting training. People who were offered rule-of-thumb-based training showed significant improvements in the way they managed their finances and in the accuracy and internal consistency of the numbers they reported. 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 10 percentage points. In contrast, we did not find any significant changes for those in the standard accounting training. Overall, it appears that the micro-entrepreneurs in our study were more likely to implement what they learned in the rule-of-thumb training.

When looking at the impact of training on business outcomes, we again find a more significant change in the group that received the rule-of-thumb 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 standard 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. We also find an economically large increase of 6% in the likelihood of having any personal savings for those in the rule-of-thumb training, but the result is only significant at the 10%-level. In contrast, we do not find any effect for the group that received the accounting training.

In studying training programs, measurement effects are a natural concern. Respondents may report what they believe surveyors want to hear. Therefore, we developed and looked for changes in objective measures of reporting quality.

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[1]Approximately one-fourth of the control group received placebo follow-up visits to control for possible monitoring effects; however, these visits had no discernible effect on outcomes.

Consistent with the belief that SMEs lack financial controls, the quality of self-reported firm data is poor. In the baseline survey, nearly half of the respondents make at least one error when asked to report sales and profits over different time horizons and levels of aggregation. Similarly, self-reported profits are substantially lower than what one would calculate from respondents' own revenue and expense detail.[2] The standard accounting training generates small and not statistically significant improvements in both dimensions. In contrast, the rule-of-thumb training significantly improves objective reporting quality. The error rate falls by nine percentage points, and the mean difference between the two profit measures drops by more than 50%. These improvements in objective reporting quality suggest that the rule-of-thumb training changes actual business management practices.

Taken together, these results suggest that effective training may operate by helping individuals to better manage negative shocks or by improving their financial controls, which may allow them to predict and counteract the effect of slow weeks more proactively.[3] However, it is important to note that improvements for firms in the rule-of-thumb treatment could stem from net business growth or from a redistribution from other enterprises in their area. Our study design allows us to test for these negative spillovers. We find a small but statistically significant drop in sales for firms located near more treated firms if these treated firms are in the same general line of business. While these results are suggestive that some of the growth in the treated firms was due to crowding out of other firms, our set up was not designed to definitively quantify the size of these spillovers.

Finally, we find that one-on-one follow-on training did not affect the outcomes for clients in either training. We see neither a change in the likelihood of implementing the accounting methods learned in class nor an impact on actual business outcomes. These results support the idea that the rule-of-thumb training is not only more effective because it is easier to understand, but it also generates larger

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[2]The direction of this reporting bias goes in the opposite direction from what De Mel, McKenzie and Woodruff (2009) found in their sample of small firms in Sri Lanka, for which self-reported profits are larger than those calculated from revenue and expense detail. We speculate that the firms in our sample have less incentive to underreport revenues but still fail to record and have poor recall over expense detail.

[3]In addition, we 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 between outcomes for borrowers 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.

effects 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. One could imagine that the mental energy and time cost required to implement standard accounting methods might simply be too high for a small business to justify.

What are the potential channels by which improved financial literacy can affect business outcomes and savings? Better financial controls might allow a business owner to use resources more effectively, e.g., improve inventory and product offerings to focus on higher margin items and expand inventory during times of high customer demand. Similarly, better financial controls might enable business owners to predict revenue shortfalls more effectively and adjust their effort levels within a period accordingly. The fact that the rule-of-thumb training reduces the drop in sales during bad periods is consistent with the hypothesis that better financial controls allow micro-entrepreneurs to manage negative shocks proactively.

The findings from this study also have important implications for programs designed to help micro entrepreneurs. Our results support the belief that financial illiteracy might impede the growth of small businesses. If micro-entrepreneurs cannot effectively control their finances, they may find it difficult to scale up operations even when given access to finance and other resources. To address this constraint, 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. A natural response to such muted results is to redouble one's efforts, offering longer and more complex courses. Our results suggest that in some cases simplification can be the better route.

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

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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.[4] 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 et al. (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 open 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.[5] One notable exception is Karlan and Valdivia (2011), which studies

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[4]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.

[5]Organization's Know About Business Programme, 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

the impact of teaching basic finance concepts to micro-entrepreneurs.[6] 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 as 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 applica-

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in a slow economy. The training is available online at http://www.sba.gov/training/. The FEP targets microfinance clients, many of whom have only subsistence level business activity. The FEP project includes five modules: credit administration, savings, financial negotiation, budgeting, and bank services.

[6]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.

tions (Bettinger, Long, Oreopoulos and Sanbonmatsu 2009). Research in cognitive psychology offers additional evidence that simpler rules and less feedback 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 micro-entrepreneurs, 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.[7] 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 standard accounting treatment offered a traditional, principles-based course in basic accounting techniques.

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[7]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.

Topics covered included daily record keeping of cash sales and expenses, aggregation of daily records into weekly and monthly reports, inventory management, accounts receivable and accounts payable, calculating cash profits, and investment planning. The materials and capacitator training program for the standard 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.[8]

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 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 record-keeping books, 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 standard 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 standard 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

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[8]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.

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 clients from Santo Domingo.[9] 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.[10]

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.[11]

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[9] 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.

[10] 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.

[11] 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

All courses were taught by qualified local instructors. The majority had university 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 (2011), 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.[12] As shown in the table, this expectation generally holds; however, individuals assigned to the standard 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 inclu-

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the full follow-up. In the analysis that follows, we group together treatments of both intensity levels.

[12]As described above, stratification utilized administrative records. Baseline survey data was not available at the time of assignment.

sion 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 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 the 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.[13]

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

$$y_i^E = \alpha + \beta Treat_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \tag{1}$$

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

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[13]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).

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 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,

$$y_i^E = \alpha + \beta Treat_i + \delta y_i^B + \varepsilon_i, \tag{2}$$

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:

$$y_i^E = \alpha + \beta Follow_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \tag{3}$$

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,

$$y_i^E = \alpha + \beta AttendAny_i + \gamma X_i + \delta y_i^B + \varepsilon_i, \tag{4}$$

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 (2011) 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

$$z_i^E = \alpha + \beta Treat_i + \gamma X_i + \delta z_i^B + \varepsilon_i. \tag{5}$$

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). Along the same lines, we compare self-reported profits to profits calculated from respondents' own revenue and expense detail. These differences are large; self-reported profits are only 60% of those calculated from the disaggregated components. While these differences could result from misreporting any of the components, we believe the most plausible explanation is that respondents fail to remember and hence underreport their various detailed business expenses. This poses challenges when interpreting the impact of either treatment on profits. For example, training could increase actual profits while improving recall of business expenses, leaving reported profits unchanged. For this reason, we are cautious when interpreting any profit measure as a stand-alone outcome. However, we expect that if either treatment improves financial controls, it should reduce the difference between these two profit mea-

sures. Therefore, we also estimate the effect of each treatment on the raw and absolute difference between self-reported and calculated profits.

Finally, although attrition in our sample was relatively low (13%), we construct bounds on the category aggregate treatment effects using a range of assumptions for the pattern of attrition following an approach based on Lee (2002). To compute lower bounds, we assign to all those who attrited from the treatment group the mean value of the non-attritors minus some faction 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-attritors from the control group plus some faction 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*.

Appendix 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.

# 5 Results

## 5.1 Business Practices and Performance

Table 3 presents the effect of each training program on business practices and performance. All the regressions in this section follow the estimation strategy laid out in the prior section. 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 standard 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 10%-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 (2011) and Berge et al. (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 et al. (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 along which we expected training may have differential effects: (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 and the nature of competitive interaction could interact with the effectiveness of training; (4) we compare individuals across the 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 (2011). 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 through 12 show that the rule-of-thumb training had a larger impact on businesses with poorer management practices in the baseline. Those beginning in the first three quartiles improved by 0.14 to 0.20 standard deviations in their aggregate measure of business practices relative to a modest improvement for those beginning in the top quartile. The accounting training had a comparable effect on those in the middle quartiles but no demonstrable effect for those in the lowest quartile. An F-test for equivalent treatment effects by both treatments on those in the lowest quartile rejects with a p-value of 0.006. The ability of the rule-of-thumb training to benefit even the most poorly managed businesses is primarily responsible for its larger average treatment effect and supports the hypothesis that there may be advantages to reducing the complexity of training programs.

In Table 6 we 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 standard 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 are more realistic about their actual sales once they went through the training while prior to the training they might have inflated the number.

## 5.2 Accuracy of Reporting

Finally, we consider the effect of both training programs on the objective measure of financial reporting quality. If micro-entrepreneurs indeed gain better financial understanding and more control over the cash flows through the training, we would expect that also the internal consistency with which they report these items to the survey goes up. We construct the index of reporting errors as described in the data section to measure whether business owners have inconsistencies across different budget items or time horizons, e.g., do weekly earnings add up to reported

monthly earnings.

Table 10 reports the results of estimating equation (1) where the outcomes of interest are those objective measures of reporting quality described above. We see that the rule-of-thumb training reduced the incidence of reporting errors by 9 percentage points relative to receiving no training, a 20% drop. The standard accounting training has a small and insignificant effect.[14] We also find that training reduces the gap between self-reported and calculated profits. Column (2) of Table 10 shows that the profits which micro-enterprises report in the survey are lower than the profits we calculate based on the revenue and expense details of the business. The dependent variable in column (3) is the difference between the profits the business reported in the endline survey and the profit that we calculate ourselves by using the responses on detailed questions about earnings and expenditures which we asked in the survey. This gap is consistent with underreporting of detailed expenses and is sensible in a world where businesses do not have sufficient oversight of their finances. When looking at the treatment effects, we see that assignment to the rule-of-thumb treatment significantly reduces both the raw and absolute differences in these profit measures. This is consistent with either an increase in reported profits or more comprehensive reporting of expenses for the detailed calculation. Point estimates for the accounting training also suggest some improvement, but they are smaller in magnitude and not statistically significant.

## 5.3   Impact of Follow-on Training

We now want to test if the impact of financial literacy training is hampered by difficulty in conveying this material through classroom training. As described above, randomly assigned follow-up visits that ensure participants understand and are able to implement the material allow us to distinguish this explanation from the possibility that the material itself, even when properly understood, is not helpful. 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

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[14]This effect of the rule-of-thumb training is independent of education levels. In contrast, while the main effect of the standard accounting training shows little effect, 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 (p-value: 0.11). This suggests that even seemingly simple training programs may require relatively high levels of existing education to be effective. Results available on request.

achieve better outcomes. Most of the coefficients on the interaction of the level effect with the intense follow-up dummy 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 standard 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 conjecture that the material was simple enough that additional help with implementation through follow-up visits was needed (and also did not persuade any additional recipients to adopt the management practices taught in class).[15]

## 5.4 Robustness Checks

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 the 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 to their neighbors drive these results.[16] There is no demonstrable effect from the accounting training.

Table 11 reports the results of bounds estimation on the treatment effect for

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[15] A less flattering interpretation for us would be that the follow-up visits themselves were not effective and maybe more substantive handholding might have been needed. However, we think an even more intensive follow-up would have been disruptive to the small business owners.

[16] See Appendix Table A3 for detail. Tables A2 through A4 report the disaggregated elements for each component.

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

Notes:

/a 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[a] |
| --- | :---: |
| 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[b] | -0.163*** |
|  | (0.049) |
| Interested in accounting training[c] | 0.080** |
|  | (0.039) |
| Interested in saving more[c] | -0.045 |
|  | (0.050) |
| Interested in cash mgmt.[c] | 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[b] | -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:

[a] 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.

[b] 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.

[c] Baseline reported interest in specific forms of training as indicated.

## Table 3: Impact of Training on Business Practices and Performance[a]

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

Notes:

[a] 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.

[b] Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account.

[c] p-value for F-test of equality of Accounting and Rule-of-Thumb treatment effect coefficients.

[d] Aggregate is unweighted sum of z-scores for all business practices as detailed in Table A1.

[e] Variable winsorized at 1%.

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

Notes:

[a] 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.

[b] Covariates include variables used for stratification: business types, loan size, and participation in an ADOPEM savings account.

[c] 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.

[d] No loans taken in prior twelve months.

## Table 5: Impact of Rule-of-Thumb Training, by Subgroup[/a]

| | Education Level[/b] | | Business Type | | Loan Type, Baseline | | Prior Interest in Training | | Baseline Bus. Prac (by quartile) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Low | High | Buy-Sell[/b] | Other | Group | Indiv. | Yes | No | 1st | 2nd | 3rd | 4th |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| ***Business and Personal Financial Practices*** | | | | | | | | | | | | |
| Sep. business and personal cash[/c] | 0.06 | 0.12** | 0.05* | 0.12* | 0.08* | 0.08** | 0.08* | 0.09* | 0.15* | 0.19*** | -0.01 | -0.01 |
| | (0.04) | (0.05) | (0.03) | (0.07) | (0.04) | (0.04) | (0.04) | (0.04) | (0.09) | (0.07) | (0.05) | (0.04) |
| Keep accounting records[/c] | 0.11*** | 0.11 | 0.10*** | 0.12** | 0.11* | 0.10** | 0.08 | 0.14** | 0.12* | 0.07 | 0.18*** | 0.07 |
| | (0.04) | (0.08) | (0.03) | (0.06) | (0.06) | (0.05) | (0.05) | (0.06) | (0.07) | (0.08) | (0.06) | (0.06) |
| Sep. business and personal acct.[/c] | 0.11*** | 0.11* | 0.09** | 0.14** | 0.15** | 0.09* | 0.06 | 0.16*** | 0.11 | 0.16** | 0.09 | 0.05 |
| | (0.04) | (0.06) | (0.04) | (0.06) | (0.07) | (0.05) | (0.05) | (0.05) | (0.07) | (0.08) | (0.07) | (0.06) |
| Set aside cash for business exp.[/c] | 0.11** | 0.15** | 0.09* | 0.16*** | 0.06 | 0.14*** | 0.19*** | 0.06 | 0.25*** | 0.13 | 0.08 | 0.04 |
| | (0.04) | (0.07) | (0.05) | (0.06) | (0.07) | (0.04) | (0.05) | (0.05) | (0.06) | (0.08) | (0.07) | (0.07) |
| Calculate revenues formally[/c] | 0.09** | 0.02 | 0.09** | 0.02 | 0.08* | 0.06 | 0.07 | 0.06 | 0.06 | 0.07 | 0.11*** | 0.02 |
| | (0.04) | (0.06) | (0.04) | (0.06) | (0.04) | (0.04) | (0.04) | (0.05) | (0.07) | (0.06) | (0.04) | (0.06) |
| Aggregate business practices[/c] | 0.16*** | 0.10 | 0.14*** | 0.15** | 0.12** | 0.15*** | 0.17*** | 0.12* | 0.20** | 0.14 | 0.20** | 0.05 |
| | (0.05) | (0.07) | (0.04) | (0.07) | (0.06) | (0.06) | (0.04) | (0.06) | (0.08) | (0.09) | (0.08) | (0.06) |
| Any savings | 0.01 | 0.15** | 0.05 | 0.07 | 0.05 | 0.07 | 0.04 | 0.08 | 0.09 | 0.10 | -0.02 | 0.06 |
| | (0.05) | (0.07) | (0.06) | (0.05) | (0.05) | (0.05) | (0.06) | (0.05) | (0.07) | (0.08) | (0.06) | (0.07) |
| Savings amount, $RD[/e] | 1,825 | 4,470 | -692 | 5,270 | 1,813 | 1,690 | -2,258 | 4,757 | 1,843 | -2,061 | 406 | 5,184 |
| | (3,100) | (5,615) | (3,498) | (4,534) | (2,476) | (3,499) | (2,709) | (4,129) | (4,190) | (5,825) | (5,014) | (8,026) |
| ***Business Performance*** | | | | | | | | | | | | |
| Total number of employees[/c] | -0.28*** | 0.27* | -0.03 | -0.07 | 0.08 | -0.11 | -0.01 | -0.09 | -0.22 | 0.18 | -0.29 | 0.04 |
| | (0.10) | (0.16) | (0.11) | (0.17) | (0.13) | (0.12) | (0.11) | (0.15) | (0.17) | (0.18) | (0.19) | (0.21) |
| Weekly Average, Sales[/c/d] | 741 | -143 | 732 | 215 | -1,955 | 1,837 | 578 | 539 | 359 | 237 | -639 | 1,273 |
| | (1,173) | (1,571) | (1,328) | (1,118) | (1,236) | (1,118) | (1,094) | (1,512) | (1,869) | (1,864) | (1,625) | (1,315) |
| Last Week, Sales[/c/d] | -387 | 931 | -57 | 906 | -990 | 1,102 | -110 | 873 | -1,517 | 666 | -799 | 1,817 |
| | (1,037) | (1,544) | (1,052) | (1,317) | (1,223) | (928) | (694) | (1,437) | (2,344) | (793) | (977) | (1,814) |
| Good Week, Sales[/c/d] | 236 | -664 | 295 | -244 | -1,212 | 728 | -345 | 426 | -579 | -1,672 | -4 | 1,704 |
| | (1,278) | (1,601) | (1,168) | (1,220) | (1,484) | (1,133) | (1,111) | (1,571) | (1,723) | (2,116) | (1,492) | (1,482) |
| Bad Week, Sales[/c/d] | 563 | 1,281 | 1,018 | 857 | -808 | 1,845*** | 853 | 1,066 | -75 | 630 | 784 | 1,161 |
| | (614) | (1,128) | (678) | (972) | (901) | (667) | (723) | (974) | (1,188) | (1,164) | (692) | (1,312) |

Notes:

[/a] 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.

[/b] 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.

[/c] Regression includes only those individuals with own business.

[/d] Variable winsorized at 1%.

[/e] 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 Standard Accounting Training, by Subgroup[a]**

| | Education level[b] | | Business Type | | Loan Type, Baseline | | Prior Interest in Training | | Baseline Bus. Prac (by quartile) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Low | High | Buy-Sell[b] | Other | Group | Indiv. | Yes | No | 1st | 2nd | 3rd | 4th |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| *Business and Personal Financial Practices* | | | | | | | | | | | | |
| Sep. business and personal cash[c] | -0.02 | 0.03 | -0.01 | 0.01 | -0.03 | 0.02 | 0.05 | -0.05 | 0.02 | 0.14* | -0.05 | -0.11* |
| | (0.05) | (0.05) | (0.04) | (0.06) | (0.06) | (0.04) | (0.04) | (0.05) | (0.08) | (0.07) | (0.06) | (0.06) |
| Keep accounting records[c] | 0.04 | 0.05 | 0.08 | -0.02 | 0.12** | 0.00 | 0.06 | 0.03 | -0.01 | 0.04 | 0.11 | 0.00 |
| | (0.05) | (0.11) | (0.05) | (0.08) | (0.06) | (0.06) | (0.08) | (0.06) | (0.07) | (0.09) | (0.08) | (0.10) |
| Sep. business and personal acct.[c] | 0.04 | 0.04 | 0.05 | 0.03 | 0.08 | 0.02 | 0.09 | 0.01 | -0.03 | 0.14* | 0.15* | -0.13 |
| | (0.04) | (0.09) | (0.06) | (0.07) | (0.06) | (0.06) | (0.07) | (0.05) | (0.07) | (0.08) | (0.08) | (0.10) |
| Set aside cash for business exp.[c] | 0.06 | 0.09 | 0.05 | 0.09* | 0.01 | 0.09** | 0.11** | 0.04 | 0.07 | 0.19*** | 0.09* | -0.04 |
| | (0.05) | (0.06) | (0.05) | (0.05) | (0.06) | (0.04) | (0.04) | (0.05) | (0.07) | (0.07) | (0.06) | (0.07) |
| Calculate revenues formally[c] | 0.02 | 0.01 | 0.11*** | -0.10 | 0.05 | 0.00 | 0.06 | -0.01 | -0.05 | 0.10 | 0.06 | 0.00 |
| | (0.04) | (0.07) | (0.03) | (0.07) | (0.05) | (0.05) | (0.04) | (0.06) | (0.06) | (0.07) | (0.07) | (0.06) |
| Aggregate business practices[c] | 0.07 | 0.09 | 0.13** | 0.00 | 0.11* | 0.05 | 0.16** | 0.00 | -0.03 | 0.16** | 0.23** | -0.08 |
| | (0.04) | (0.12) | (0.05) | (0.08) | (0.06) | (0.07) | (0.07) | (0.06) | (0.07) | (0.08) | (0.09) | (0.09) |
| Any savings | -0.03 | 0.07 | 0.03 | -0.03 | 0.00 | 0.02 | -0.01 | 0.03 | -0.11 | 0.12 | 0.07 | -0.06 |
| | (0.05) | (0.07) | (0.05) | (0.05) | (0.05) | (0.06) | (0.06) | (0.05) | (0.07) | (0.09) | (0.09) | (0.08) |
| Savings amount, $RD[e] | 2,213 | 2,734 | -3,509 | 9,137 | -2,288 | 5,520 | 4,484 | -1,111 | -4,242 | -7,800 | 21,732 | -3,575 |
| | (7,233) | (4,836) | (3,204) | (11,533) | (2,781) | (8,404) | (8,369) | (4,308) | (2,818) | (5,489) | (15,951) | (6,703) |
| *Business Performance* | | | | | | | | | | | | |
| Total number of employees[c] | -0.16* | 0.47** | 0.12 | 0.01 | 0.10 | 0.06 | 0.23 | -0.04 | -0.19 | 0.71** | -0.12 | -0.07 |
| | (0.09) | (0.20) | (0.12) | (0.14) | (0.19) | (0.11) | (0.17) | (0.15) | (0.18) | (0.33) | (0.15) | (0.21) |
| Weekly Average, Sales[c/d] | -821 | -548 | -265 | -963 | 253 | -821 | 1,710 | -2,360** | -3,128** | -767 | -1,096 | 2,098 |
| | (1,019) | (1,707) | (992) | (1,295) | (1,862) | (1,021) | (1,519) | (1,181) | (1,302) | (2,506) | (2,081) | (1,755) |
| Last Week, Sales[c/d] | -1,749** | -175 | -918 | -895 | -96 | -1,538* | -256 | -1,507 | -4,303** | -686 | 388 | -1,076 |
| | (765) | (1,335) | (653) | (1,005) | (1,291) | (787) | (666) | (1,046) | (1,767) | (1,664) | (1,286) | (948) |
| Good Week, Sales[c/d] | -1,945 | 1,046 | -1,177 | -474 | 1,228 | -1,462 | -1,414 | -641 | -2,235 | -3,345 | -133 | 1,672 |
| | (1,279) | (2,004) | (1,241) | (1,606) | (1,869) | (1,092) | (1,340) | (1,564) | (1,628) | (3,174) | (1,421) | (1,743) |
| Bad Week, Sales[c/d] | -1,381** | 380 | -527 | -876 | 28 | -1,037* | -337 | -678 | -2,815** | -536 | 699 | -777 |
| | (543) | (1,068) | (670) | (744) | (1,058) | (617) | (701) | (801) | (1,183) | (1,320) | (929) | (996) |

Notes:

[a] 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.

[b] 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.

[c] Regression includes only those individuals with own business.

[d] Variable winsorized at 1%.

[e] 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

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

Notes:

[a] 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 $y_E = \alpha + \beta_1$ x Intensity $+ \gamma$ x $y_{i,B} + \varepsilon_i$ 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.

[b] See section 3 for detailed description of treatments.

[c] Regression includes only those individuals reporting own business.

[d] Variable winsorized at 1%.

[e] 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.

[f] No loans taken in prior twelve months.

# Table 8: Impact of Training on Business Practices and Performance
## Treatment on the Treated[/a/b]

| | Obs. | Standard Accounting | | Rule-of-Thumb | | Any Treatment | |
|---|---|---|---|---|---|---|---|
| | | Treatment Only | Incl. Covariates[/b] | Treatment Only | Incl. Covariates[/b] | Treatment Only | Incl. Covariates[/b] |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| ***Business and Personal Financial Practices*** | | | | | | | |
| Sep. business and personal cash | 794 | 0.00 | -0.01 | 0.17** | 0.17** | 0.08 | 0.08 |
| | | (0.06) | (0.06) | (0.07) | (0.07) | (0.05) | (0.05) |
| Keep accounting records | 795 | 0.08 | 0.07 | 0.23*** | 0.23*** | 0.15** | 0.15** |
| | | (0.10) | (0.10) | (0.06) | (0.06) | (0.07) | (0.07) |
| Sep. business and personal acct. | 792 | 0.08 | 0.07 | 0.24*** | 0.24*** | 0.16** | 0.15** |
| | | (0.10) | (0.10) | (0.06) | (0.06) | (0.06) | (0.07) |
| Calculate revenues formally | 795 | 0.03 | 0.03 | 0.13** | 0.13** | 0.08 | 0.08 |
| | | (0.07) | (0.07) | (0.06) | (0.06) | (0.05) | (0.06) |
| Has employees | 794 | 0.05 | 0.06 | -0.07 | -0.06 | -0.01 | 0.00 |
| | | (0.07) | (0.07) | (0.10) | (0.09) | (0.06) | (0.07) |
| Total number of employees | 794 | 0.14 | 0.13 | -0.11 | -0.07 | 0.02 | 0.03 |
| | | (0.17) | (0.17) | (0.19) | (0.19) | (0.13) | (0.13) |
| ***Business Performance*** | | | | | | | |
| Weekly Average, Sales[/c] | 571 | -1,138 | -1,341 | 1,201 | 973 | 43 | -188 |
| | | (1,522) | (1,538) | (1,893) | (1,854) | (1,358) | (1,323) |
| Last Week, Sales[/c] | 507 | -1,826 | -1,920 | 817 | 815 | -498 | -552 |
| | | (1,219) | (1,223) | (1,567) | (1,526) | (1,225) | (1,183) |
| Good Week, Sales[/c] | 568 | -1,600 | -1,583 | 57 | -115 | -780 | -857 |
| | | (1,758) | (1,792) | (1,956) | (1,824) | (1,566) | (1,547) |
| Bad Week, Sales[/c] | 551 | -1,293 | -1,284 | 2,045* | 2,086* | 357 | 382 |
| | | (955) | (967) | (1,131) | (1,123) | (887) | (900) |

[/a] 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.

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

[/c] Variable winsorized at 1%.

# Table 9: Standardized Treatment Effects
## Business Practices

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

Notes:

[a] 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

| | Obs. | Control Mean | Standard Accounting | Rule-of-Thumb | Any Treatment |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Any Reporting Errors**[b] | 804 | 0.48 | -0.03 | -0.09*** | -0.06* |
| | | | (0.04) | (0.03) | (0.03) |
| **Raw difference between reported and calculated profits**[c] | | | | | |
| Weekly | 427 | -2,154 | 761 | 1,060* | 918* |
| | | | (711) | (579) | (548) |
| Monthly | 534 | -4,180 | 1,708 | 3,690** | 2,710** |
| | | | (1,413) | (1,646) | (1,337) |
| **Absolute difference between reported and calculated profits**[d] | | | | | |
| Weekly | 427 | 3,844 | -173 | -668 | -434 |
| | | | (602) | (518) | (494) |
| Monthly | 534 | 11,913 | -1,225 | -1,919* | -1,575 |
| | | | (1,244) | (1,053) | (1,002) |

Notes:

[a] 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.

[b] 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.

[c] Raw difference equals self-reported profits for period minus profits calculated for period using reported revenues minus expenses, winsorized at 1%.

[d] Absolute value of raw difference described above. Tobit and CLAD regressions generate similar estimates.

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

| | Lower Bounds[a] | | | | | | Unadjusted Treatment Effect | Upper Bounds[b] | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Worst Case | Worst, No Reversal | 0.50 sd | 0.25 sd | 0.10 sd | 0.05 sd | | 0.05 sd | 0.10 sd | 0.25 sd | 0.50 sd | Worst, No Reversal | Worst Case |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
| Business practices | -0.248 | -0.115 | 0.022 | 0.064 | 0.088 | 0.097 | 0.108 | 0.113 | 0.122 | 0.146 | 0.188 | 0.306 | 0.430 |
| | (0.042) | (0.039) | (0.036) | (0.036) | (0.036) | (0.036) | (0.042) | (0.037) | (0.037) | (0.038) | (0.039) | (0.048) | (0.052) |
| Business outcomes | -0.609 | -0.517 | -0.044 | -0.005 | 0.019 | 0.026 | 0.040 | 0.042 | 0.050 | 0.073 | 0.113 | 0.626 | 0.702 |
| | (0.079) | (0.078) | (0.031) | (0.031) | (0.030) | (0.030) | (0.033) | (0.030) | (0.030) | (0.031) | (0.031) | (0.079) | (0.081) |
| Personal outcomes | -0.306 | -0.167 | -0.012 | 0.015 | 0.032 | 0.037 | 0.045 | 0.048 | 0.053 | 0.069 | 0.096 | 0.251 | 0.391 |
| | (0.038) | (0.032) | (0.022) | (0.022) | (0.022) | (0.022) | (0.025) | (0.022) | (0.022) | (0.022) | (0.023) | (0.033) | (0.041) |
| Personal financial practices | -0.288 | -0.170 | 0.003 | 0.030 | 0.046 | 0.051 | 0.052 | 0.062 | 0.067 | 0.083 | 0.110 | 0.289 | 0.426 |
| | (0.036) | (0.035) | (0.024) | (0.025) | (0.025) | (0.026) | (0.029) | (0.026) | (0.026) | (0.027) | (0.028) | (0.046) | (0.052) |

Notes:

[a] Column (1) imputes value of attrited treatment group as minimum value of for non-attrited treatment and attrited control group as maximum value of non-attrited control. Column (2) is the same as (1) for control group and for variables without baseline observations. For variables with baseline observations value, value for attrited treatment group is imputed as maximum of individual's baseline value and minimum for non-attrited treatment group. Columns (3) through (6) impute 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.

[b] Columns (8) through (11) impute 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. Column (13) imputes value of attrited treatment group as minimum value of for non-attrited treatment and attrited control group as maximum value of non-attrited control. Column (12) is the same as (13) for treatment group and for variables without baseline observations. For variables with baseline observations value, value for attrited control group is imputed as maximum of individual's baseline value and minimum for non-attrited control group.

[c] 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**

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

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

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

Notes:

[a] 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.

[b] Aggregate value is unweighted sum of all individual measures.

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

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

Notes:

[a] 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.

[b] Winsorized at 1%.

[c] Aggregate value is unweighted sum of all individual measures.

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

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

Notes:

/a  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.

/b  Winsorized at 1%.

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

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

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

Notes:

[a] 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.

[b] Baseline value not available.

[c] Aggregate value is unweighted sum of all individual measures.