Complementing Business Training with Access to Finance: Evidence from SMEs in Kenya

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Abstract

This paper investigates the complementarity between business training and access to financial capital for small and medium enterprises (SMEs) in Kenya. All participants in a business training program are offered training. One-third of participants are offered loans immediately after training (Concurrent Loan group), one-third are offered loans six weeks after training (Delayed Loan group), and the remaining third are offered loans after another four weeks (Control group). While a long time lag may reduce knowledge retention and application by SMEs, concurrent access to loans and associated business spending may crowd out the entrepreneurs' attention from improving business practices. We find evidence for the latter in both intention-to-treat and treatment-on-the-treated estimates. While SMEs in both Control and Delayed Loan groups improve their business practices, SMEs in the Concurrent Loan group who take loans do not improve their practices at all. Moreover, entrepreneurs who take loans spend less time on their businesses and their business revenue falls. Our evidence is consistent with the entrepreneurs in our study using loans to substitute for their income.

JEL-Codes: O120, L260, M530.

Keywords: business training, access to finance.

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1 Introduction

Business training for micro, small, and medium enterprises in low-income countries has yielded limited performance improvements despite strong evidence that they operate with low managerial knowledge (Bruhn et al., 2010; McKenzie and Woodruff, 2014). Recent studies have explored whether a lack of complementary inputs prevents these firms from implementing the business practices they learn during training. In light of evidence that such firms are often capital-constrained (Banerjee and Duflo, 2014; de Mel et al., 2008, 2012), several papers have studied and found evidence of complementarities between small business training and financial capital (Karlan and Valvidia, 2011; de Mel et al., 2014; Fiala, 2018). Indeed, training that teaches, for instance, how to create and keep track of a budget may be of limited use if businesses do not have the cash to allocate across expense categories.[1] Recent increase in access to microloans in low- and middle-income countries may help to relieve this constraint (e.g., Bank, 2022), but potential time lags between training and access to financial capital may reduce knowledge retention and application and undo the benefits of complementary access to capital (Lyle et al., 2020).

In this paper, we test the extent to which timely access to financial capital impacts the effectiveness of business training among small and medium enterprises (SMEs) in Kenya. We evaluate a program implemented by Technoserve Kenya that offers participating SMEs an interest-free loan if they complete a financial literacy micro-course. In the program cohort we evaluate, all participants were offered training at the same time, but Technoserve randomized when participants received access to the loan. In particular, one-third of participants were offered the loan immediately after training (*Concurrent Loan* group), one-third were offered the loan six weeks after training (*Delayed Loan* group), and the remaining participants were offered the loan after another four weeks (*Control* group). The phased-in access to loans allows us to examine whether the timeliness of loan access augments any complementarities between financial literacy training and access to finance.

About 14,000 SMEs in Kenya signed up to receive access to the program we evaluate. Of those SMEs that signed up, a randomly selected sample was surveyed before and after the intervention.[2] Importantly, the endline survey is conducted before the Control group receives access to loans. These two rounds of surveys enable us to estimate short-run treatment effects of the timing of

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[1]For instance, firms that make small or negative profits may not plan and, rather, pay expenses as they become due (e.g., Mani et al., 2013).

[2]The baseline sample consists of 526 SMEs while the endline survey consists of 415 SMEs surveyed during baseline.

access to loans on business outcomes as within-firm changes before and after the intervention. Additionally, a second follow-up survey conducted seven months after the Concurrent Loan group received loan access enables us to study longer-term effects.

Our analyses provide three related takeaways. First, contrary to our expectation, providing loans immediately after training reduces improvement in business practices. To be concrete, the Control and Delayed Loan groups report an average improvement of around 18% in financial-management best practices whereas the Concurrent Loan group improves their financial practices by only half as much as the Control group. Almost all of our sample entrepreneurs complete only the financial-management module of the training and do not participate in other modules of the training. Therefore, we conduct a placebo test using best practices related to a separate training module - the marketing module - to verify that the improvement in practices related to financial management is driven by the training. Reassuringly, we find no changes in marketing practices in any of the treatment or control groups.

We also confirm that the lower average improvement in business practices among Concurrent Loan firms is driven by loan uptake. Loan uptake among treated businesses is an endogenous decision.[3] Therefore, we instrument uptake of loans with random assignment to the Loan treatments and estimate treatment-on-the-treated effects. Our estimates suggest that entrepreneurs in the Concurrent Loan group who take loans do not improve their financial practices *at all*. On the other hand, the entrepreneurs who take loans after a time lag in the Delayed Loan group show improvement in business practices that persist even after they take loans. Additionally, evidence from knowledge tests administered before and after the training confirms that the lack of improved practices in the Concurrent Loan group is not because of less knowledge absorbed from the training.

Second, we find that access to loans reduces the time treated entrepreneurs spend on their businesses. Entrepreneurs in the Concurrent (resp. Delayed) Loan group who take loans leave their businesses closed for nine hours (resp. five hours) a week more than the Control group on average.[4] They also serve fewer customers. Along with our results on business practices, the evidence suggests that the treated entrepreneurs invest less effort in their businesses compared to those in the Control group and might use the loan as a substitute for their income. Indeed, only

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[3]In our sample, around 75% of firms complete training, and conditional on training completion, around 60% of treated firms take loans. To be clear, access to loans was conditional on the successful completion of the training module on financial management.

[4]Control group businesses remain open for 77 hours per week on average at baseline.

around 50% of borrowing firms in our sample pay a part of their loan back while almost none of them repay their loan in full.[5]

Lastly, we find negative treatment effects on performance, consistent with treated entrepreneurs' reduced effort translating into lower business performance. In particular, we find that immediate access to loans substantially reduces firm survival and sales revenue. In the post-intervention period, firms in the Concurrent Loan group are 4% less likely to be operational on average compared to those in the Control group. We do not find any effect of the Delayed Loan treatment on firm survival. However, conditional on survival, both treatments reduce sales revenue. The change in sales revenue for the Concurrent Loan group between pre- and post-intervention is about 2,500 KES (USD 23) lower than that for the Control group, with the average Concurrent Loan firm earning 41% less after intervention than at baseline. Sales in the Delayed Loan group also decrease relative to their baseline and relative to the Control, although by less substantial amounts. Again, treatment-on-the-treated estimates confirm that the negative performance effects are driven by loan uptake. We verify the robustness of our estimates to outliers and provide bounds of treatment effects while accounting for attrition. Finally, while these findings capture relatively short-run effects, we descriptively explore longer-run business performance among treatment and control firms and find that the declines in sales persist in magnitude for as many as seven months after the Concurrent Loan group receives access to loans.[6]

Our paper contributes to two strands of the economics literature. First, we contribute to the literature on business training for micro and small enterprises in developing countries. Such training generally has positive but limited and short-lived effects on business practices and performance.[7] One stream of research explores whether lack of access to financial capital that complements training can explain the limited impact of business training (Karlan and Valvidia, 2011; de Mel et al., 2014; Berge et al., 2015; Fiala, 2018).[8] While they generally demonstrate that complementary access to capital can improve the effects of training on business practices, it is relatively less understood when

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[5]The loans provided by Technoserve are effectively soft loans with no strong punishments for non-repayment. The primary, and to our knowledge, only, downside of non-repayment is that it precludes business owners from accessing subsequent, potentially larger, loans through the program.

[6]Our analysis is limited by the fact that the Control group receives access to loans by the final follow-up survey.

[7]See McKenzie and Woodruff (2014) and McKenzie (2020) for extensive reviews on this literature.

[8]Other attempts to explain and improve training performance have explored individual consulting (Bruhn et al., 2018; Dalton et al., 2021), simplified training (Drexler et al., 2014), personal initiative training (Campos et al., 2017), mentoring (Brooks et al., 2018; Bruhn et al., 2018), and network effects (Fafchamps and Quinn, 2018; Cai and Szeidl, 2018).

such access to capital should be provided. On one hand, delaying access to loans after training could lead to lower retention and application of training knowledge by trained entrepreneurs (Lyle et al., 2020). On the other, concurrent access to loans may crowd out entrepreneurs' attention from implementing best practices and toward business spending. Using a novel variation in the timing of loans, our paper finds evidence consistent with the latter. Immediate access to loans distracts entrepreneurs from implementing best practices taught during training while a few weeks' time lag helps them to focus on and improve best practices first. Such improvements persist even after the businesses receive loans.

Second, we contribute to a large literature on access to finance for micro and small enterprises in developing countries.[9] This literature has considered both grants and loans. While access to grants has a large positive impact on business performance (de Mel et al., 2008, 2012), access to loans or microloans has often led to weak or no effects (e.g., Banerjee et al. (2015a,b).[10] In fact, Meager (2019) presents quantile estimates based on seven studies on microfinance and shows that the impacts are quite precisely zero for all firms from the 5th to 75th quantiles and imprecisely positive on firms in the right tail. One posited explanation for the contrasting effects of loans and grants is that the latter allows enterprises to make riskier but more rewarding investments as they have no obligation to repay the grant (Field et al., 2013). On the other hand, recipients of loans make less rewarding but safer investments as they have to pay them back, often within a short horizon. The program we evaluate provides soft loans with limited liabilities, potentially enabling enterprises to take high risks if necessary. We find that the loans lead to a decline in business survival and revenue, potentially driven by income substitution by the loan recipients (Attanasio et al., 2015). Our findings are broadly consistent with Karlan and Zinman (2011) who document a similarly negative effect of loans on business survival in the Philippines 11 months after their intervention. Additionally, we show that treated businesses perform worse - even conditional on survival - and that this effect has an early onset relative to loan receipt.

The rest of the paper is organized as follows. Section 2 describes the empirical setting. Section 3 discusses the data and estimation strategy for our analysis. Section 4 reports the main findings while Section 5 presents their robustness tests. Section 6 concludes.

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[9]See Quinn and Woodruff (2019) and Jayachandran (2021) for excellent reviews on this literature.

[10]One exception to this is Fiala (2018) who shows that loans to microentrepreneurs in Uganda outperform grants.

2 Empirical Setting

We partnered with Technoserve Kenya to evaluate the impact of providing loans to small and medium enterprises (SMEs) in Kenya in conjunction with training on business practices. The program we evaluate is part of Technoserve's regular organizational activity in the country and was carried out among retail and service SMEs in Mombasa and Nairobi in early 2021. SME owners were invited to apply to participate in a training program with the option to access a loan. Around 14,000 eligible SMEs across 419 neighborhood-clusters responded to the invitation and registered with Technoserve for the business training.

The training is a standardized training program that Technoserve provides SMEs across several countries. It has modules that include topics on financial literacy and marketing. To enhance its scalability and eliminate the risks of in-person training due to COVID-19 pandemic at the time of the intervention, the training was provided in April 2021 using short messages through WhatsApp or SMS, depending on business owners' preferences.

In addition to access to the training program, participating SMEs could request loans in the range of 2,000 to 80,000 Kenyan Shillings (KES), or about 20 to 725 US Dollars. However, they were required to complete the financial literacy module of the business training and pass a knowledge test to be eligible for loans. Completion of additional training modules was encouraged but not required for access to loans. The loans were to be repaid over two months following disbursement and no interest was charged. Business owners who repaid their loans were eligible for subsequent loans through Technoserve.

The loans were funded separately as a one-time intervention. They could be potentially continued in the long run if they proved to be sustainable and effective in helping SMEs grow. Therefore, Technoserve was keen to assess the impact of the loans through a randomized design.[11]

The eligible SMEs were assigned to one of three groups based on a clustered randomization design. All the groups received training starting in April 2021. However, a total of 4,791 applicants were provided access to loans immediately after training (*Concurrent Loan* group), 4,246 were provided access to loans about six weeks after training (*Delayed Loan* group), and the remaining 4,966 were provided access to loans about 10 weeks after training (*Control* group).[12] The randomization

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[11]Technoserve led the effort to implement the intervention. We advised them on their randomization and data collection process and helped them conduct the subsequent analysis.

[12]SMEs in the Delayed Loan and Control groups were told that they would receive their loans in six and 10 weeks,

was clustered by geographical neighbourhoods with an average of 33 SMEs in each cluster.

We use the variation in the timing of loan access to identify two treatment effects. First, comparing the two Loan groups separately to the Control allows us to test if receiving loans immediately after training - that focuses on improving business practices - affects the firms' business practices differently than receiving loans with a delay. On one hand, under strong complementarities between the training and access to capital, a time lag may reduce the retention of knowledge from the training and its implementation. On the other, concurrent access to loans (and associated business spending) may divert the entrepreneurs' attention from improving business practices and they may focus relatively more on business spending. Second, the fact that the Control group does not receive access to loans until the end of the study period allows us to estimate the impact of loans on SMEs by comparing the Control group to the Loan groups.[13]

We use data from two rounds of surveys that Technoserve conducted. The first was a baseline survey in mid-February 2021 before training began in April. Because of budgetary limitations, the survey sampled a random subset of 4% SMEs from each of the three experimental groups, leading to a total *baseline sample* of 526 SMEs. The baseline sample was selected to be geographically representative of the whole sample and led to 185 SMEs in the Control group, 181 in the Concurrent Loan group, and 160 in the Delayed Loan group. The surveys were administered over the phone.

The second survey - a post-intervention survey - followed up on the baseline sample in the third week of June, before the Control group received loans. We call this survey the *endline* survey. A total of 415 SMEs responded to the endline survey: 155 from the Control group, 133 from the Concurrent Loan group, and 127 from the Delayed Loan group.[14] We will begin our analysis with the full sample of program participants to explore training and loan uptakes. However, our analysis of treatment effects on business outcomes will rely on survey data and therefore focus on the SMEs that responded to both baseline and endline surveys.

respectively, because of constraints on how many loans Technoserve can process in a given time frame. Thus, all SME owners who applied and qualified for a loan could reasonably expect to receive one and what varied across groups is the timing with which they received it relative to when they completed the training.

[13]Funding deadlines meant that Technoserve could not delay the loans for the Control group any further. As a result, with this roll-out design, we can rigorously identify the intervention's impacts over only a short term. However, we descriptively explore longer-run impacts in Section 4.3.

[14]See Table A1 for a summary of the sample sizes at different stages of the study.

3 Empirical Strategy

In this section, we discuss the data and empirical specifications we use to estimate treatment effects of the intervention. We begin by describing the SMEs in our sample and showing that they are largely similar across experimental groups. We then discuss the empirical specifications that we use to derive our main results.

3.1 Data

As mentioned earlier, we use baseline and endline survey data to evaluate the treatment effects on SME performance. As a result, most of our analysis will focus on the 415 businesses that responded to both rounds of surveys (*respondent sample*). Therefore, below, we primarily focus on data on firm characteristics collected through the surveys for the respondent sample.[15]

Panel A in Table 1 reports a broad set of characteristics for firms in the respondent sample using data collected during the baseline survey. Almost all the firms in our sample are owned by a single entrepreneur and employ an average of 0.60 additional employees at baseline. A small majority are *dukas* (small convenience shops). The next most common type of business is grocers (23%). The remaining is composed of salons, tailors, and chemists or pharmacists. Just over half of the firms are licensed. A majority of our firms' owners are female (68%). The businesses we have in our sample are the primary source of income for almost all the owners, although about a quarter of them own at least one other business as well. The median owner is between 31 and 40 years of age.[16]

The primary outcomes of interest given our motivation and empirical design are (i) business practices covered by the financial literacy training, (ii) business owners' effort investment in the business, and (iii) business performance. We measure relevant financial management practices as the average of whether or not business owners keep a budget, whether they are aware of different sources of business financing, whether they have cash savings, and whether they keep business records. For each category, enumerators score businesses on a scale of 1-3.[17] We also measure busi-

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[15]However, Table A2 in the Appendix verifies that the respondent sample, as well as the baseline sample, are similar to the full sample of firms in terms of a handful of business characteristics collected during the program registration process: business type, formalization status, and daily sales revenue. These data were collected from all 14,000 SMEs during registration with Technoserve.

[16]Age is measured on a scale of 1-5 where 1=18-30 years old, 2=31-40 years old, 3=41-50 years old, 4=51-60 years old, 5=61+. A distribution of entrepreneurs' age can be found in Figure A1 in the Appendix.

[17]Our results are unchanged if we use the sum of these reports rather than the mean.

ness marketing competencies (awareness of online marketing and deliveries). To capture whether the treatments lead to adjustments in business owners' effort along dimensions other than business practices, we measure the number of hours a week the business is open and the average number of customers the business serves per day, both of which are self-reported by the business. To assess whether the treatments lead to changes in business performance, we examine business survival and business sales. Average sales measures were either collected from business records read out by respondents or backed out in conversation between the enumerator and the business owner. Whether or not a business is operational is self-reported by business owners.[18]

Panel B of Table 1 reports average firm performance across these measures at baseline. On average, firms in our sample were open for 78 hours per week at baseline, indicating how time-consuming SMEs in Kenya are, and they earn about 7,000 KES per day in revenue while serving about 36 customers.[19]

Table 2 compares average firm characteristics across treatment and control groups. The businesses in the three experimental groups appear to be largely balanced except in terms of the nature of businesses and whether they are formalized. In particular, firms in the Delayed Loan group comprise relatively more dukas (and fewer grocers) than firms in the Control group and are more likely to have business licenses. Firms in the Concurrent Loan group are less likely than those in the Control group to be licensed. The average business in the Concurrent Loan group generates somewhat lower revenue than those in the Control group, although the difference is not statistically significant at traditional levels. Business practices, measured with Financial Management and Marketing scores are similar across the three groups.

3.2 Analysis Plan

Because of non-trivial attrition between baseline and post-intervention surveys and some differences in firm characteristics across groups at baseline, we limit our sample to firms that we could reach in both baseline and endline surveys and estimate *within-firm* changes in outcomes between the two surveys for the treated firms relative to Control firms.[20] A difference-in-difference approach controls for observable and unobservable differences in baseline firm characteristics across experimental

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[18]Note that businesses not in operation by the endline survery do not have sales, hours of operation, or number of customers to report.

[19]One US Dollar was equivalent to a little more than 110 Kenyan Shilling (KES) on average in 2021.

[20]We will explore the issue of attrition separately later in the paper.

groups and allows us to estimate causal effects of the loan and loan timing on business performance.

Our primary objective is to estimate the intention-to-treat (ITT) effects of providing access to loans to sample firms. Thus, our main estimation specification is:

$$Y_{it} = \alpha + \beta_1 ConcurrentLoan_i * Post_t + \beta_2 DelayedLoan_i * Post_t$$

$$+ \beta_3 Post_t + SME_i + \varepsilon_{it}, \quad (1)$$

where Y_{it} is an outcome for firm i in period t. We use two periods: pre-intervention (baseline) and post-intervention (endline). $Post_t$ is a dummy that takes the value of zero for baseline and one for endline. $ConcurrentLoan$ and $DelayedLoan$ indicate whether a business has been assigned to the Concurrent or Delayed Loan groups, respectively. We include SME fixed effects in our regressions to control for firm-specific characteristics. Thus, β_1 and β_2 estimate within-firm changes in outcome among treated firms between baseline and endline compared to that for Control firms.[21] We cluster standard errors by the firms' geographical neighborhoods - the same variable used to randomize them into experimental groups.

We validate our estimation approach by testing for parallel trends. Although we do not have data on business practices or entrepreneurs' effort from more than one point in time prior to the intervention, we do have information on sales revenue - a key summary measure of business performance - prior to the baseline survey because they were captured during the business' registration with Technoserve. We test for parallel trends in sales using data from (i) the baseline survey, and (ii) registration form entries. Reassuringly, Figure 2 confirms that sales revenue in the Control and the Loan treatment groups evolve similarly before the intervention and begin to diverge substantially only after the intervention.

We also estimate treatment-on-the-treated (TOT) effects. In our context, this is the effect of loans on firms that take them up. To estimate TOT effects, we conduct a 2-SLS estimation that uses assignment to the treatment groups as an instrument for taking loans. We report both ITT and TOT estimates wherever appropriate.

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[21]Later, we also confirm that a traditional differences-in-means analysis on post-intervention outcomes does not change our estimates.

4   Main Results

This section presents the main results of the intervention. First, we establish that the uptake of training and loans is large among our sample firms. Next, we estimate the effects of the intervention on business practices covered in the training, and on other business activities. Lastly, we estimate whether the intervention leads to any changes in firm performance. We estimate both the effect of being assigned to the loan treatments (intention-to-treat effects) and the effect of loans on firms that eventually take loans (treatment-on-the-treated effects). We then report a series of subsequent tests that verify the robustness of our findings.

4.1   Training and Loan Uptakes

To ensure that there is sufficient treatment take-up to estimate treatment effects, we begin by investigating the uptake of training and loans in our full sample of firms. Recall that the training was offered to all three experimental groups in the intervention at the same time. Around 75% of the 14,003 firms in our sample completed at least one module of the training. In all cases, this is the financial module of the training. There is, however, some difference in the uptake across experimental groups. As the left panel of Figure 1 demonstrates, a relatively higher share of firms in Delayed and Concurrent Loan Treatments finish the training (81% and 77%, respectively) compared to those in Control (66%). It is possible that knowledge of a relatively long delay in access to loans for Control firms discourages some of them from completing the training.[22]

Although subsequent loan uptake is also large, not all firms that are eligible for loans eventually take them up. The right panel of Figure 1 shows that roughly 50% of all firms in the treatment groups take loans (46% in Delayed and 51% in Concurrent Loan treatments). Recall, however, that only firms that complete the financial module of the training are eligible to take loans. Therefore, the last two bars in Figure 1 estimate loan uptake within treatment groups conditional on completion of the financial module training. The uptake of loans is higher conditional on eligibility but still, perhaps surprisingly, low at 56% in Delayed and 66% in Concurrent Loan treatments. Conditional on taking a loan, firms in the Concurrent Loan group borrow 11,903 KES on average while those in the Delayed Loan group borrow 11,641 KES. These are roughly equivalent to the firms' sales revenue from 1.5 days. The microfinance literature generally reports a much lower uptake of loans

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[22]We will return to this issue in Section 5.

when microenterprises are offered one (e.g., see Banerjee et al. (2015b)). However, the less-than-perfect uptake of loans in our context is nonetheless interesting from a theoretical perspective as the loans were offered at a 0% interest rate (that is, no cost of borrowing) as opposed to higher interest rates charged by microfinance institutions. It is possible that some firms were more interested in the training than the loan, and the training that focused on debt management discouraged some from taking additional debt burden.[23]

4.2 Effect of Loan and Loan Timing on Business Practices and Effort

We now turn to an assessment of how the interventions affect the business practices targeted by the required training module, such as having a budget, keeping business records, and understanding basic finances. To do so, we rely on data obtained through detailed surveys during baseline and endline. As a result, the analysis in this section and in all subsequent ones focus on firms that responded to both baseline and endline surveys (respondent sample).[24]

Estimates in Column 1 of Table 3 suggest that, on average, the program improves financial practices in all of the three groups of businesses in our sample. The mean score for financial practices for businesses in the Control group increased by 0.38, an increase of around a fifth from a mean score of 2.12 at baseline. In comparison, businesses in the Delayed Loan group perform marginally worse (although statistically indistinguishable from the Control group) but yet improve their score from baseline by 0.32 (p-value=0.00). Businesses in the Concurrent Loan group, however, improve their practices by significantly less than the Control group. Although they show an increase in scores on average as well, their financial practices improve by only half as much as those in Control.

Because loan uptake is below 100%, next, we test whether the negative intention-to-treat effect on SMEs in the Concurrent group in Column 1 is indeed driven by loan timing rather than another, potentially unobserved, difference between the groups. We estimate treatment-on-the-treated (TOT) estimates to estimate the effect of treatment on those who take loans. To be concrete, we conduct a two-stage-least-squares estimation by instrumenting loan uptake by businesses with

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[23]To be strict, entrepreneurs would need to complete the training and pass a subsequent knowledge test to access loans. However, the number of entrepreneurs who failed the test was very low and would not explain the low uptake of loans conditional on training completion.

[24]In the previous section, we confirmed that firms in the respondent sample are similar in characteristics to firms in the full sample. Along the same lines, Figure A2 confirms that the uptakes of training and loans are also similar in the two samples, even when split by experimental groups.

their random assignment to Loan treatments.[25] Column 2 presents the TOT effects on business practices and confirms that the ITT effects are driven by the borrower businesses. In particular, we find that businesses that received loans immediately after training show almost *no improvement* in business practices compared to the pre-intervention period. In contrast, businesses that received loans a few weeks after training exhibit a positive increase in their practices compared to the pre-intervention period, by a score of 0.27 (p-value<0.01).

The evidence in Columns 1-2 suggests that loans provided immediately after training distract entrepreneurs from implementing best practices taught during training. Those who received the loans a few weeks later, on the other hand, may have been able to keep their focus on improving business practices and once they implemented new practices, they were sustained even after taking loans. Yet, in principle, it is possible that the difference in implemented practices between the two treatments is not driven by distraction caused by loans, but by a difference in learning potentially induced by the two treatments. Perhaps entrepreneurs in the Concurrent Loan group paid less attention to training from the onset.

Column 3 shows that, if anything, businesses in the Concurrent Loan group learned more on average than either Control or Delayed Loan group. To test how much they learned from the training, we compute Knowledge Scores as the mean scores from tests that they had to take on lessons they learned during the training. The tests were conducted at regular intervals throughout the sample period. We control for month fixed effects to control for time trends. The positive, although statistically insignificant, coefficient for the Concurrent Loan group suggests that they were no less likely to absorb knowledge from the training than the Control group. Their lower score on business practices is a result of lower implementation *after* training.

An alternative possibility is that the improvements in financial practices among Control and Delayed Loan group SMEs were unrelated to the training they received. Indeed, while changes in financial practices among the Concurrent and Delayed Loan groups can be identified by comparing them to the Control group, the estimation of changes in practices among the Control group relies on a before-after comparison as we do not have a pure control group that did not receive training. To verify that improvements in financial management practices are indeed driven by the training, as

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[25] Whenever we report TOT estimates, we report Kleibergen-Paap F-statistic at the bottom of the table to confirm that our instrument strongly predicts loan uptake in the first stage (Kleibergen and Paap, 2007). An F-statistic with a value higher than 10 would indicate a sufficiently strong first stage.

opposed to a general improvement over time, for instance, we conduct a placebo test. In particular, we test whether the training program similarly influenced marketing competency. Because the businesses were required to complete only the training module on financial literacy to access loans, most businesses did not complete the module on marketing. A general improvement of business practices between pre- and post-intervention, as opposed to a training-driven improvement, is likely to improve marketing practices as well as financial management. However, we do not find any effect of the intervention on Marketing Competency (Column 4), even for the Control group. The placebo tests support our interpretation that improved financial practices in our sample are driven by the training.

While the results from Table 3 are consistent with an interpretation that providing immediate post-training loans to businesses distracts entrepreneurs from implementing best practices, perhaps they instead allowed business owners to redirect their effort towards other important business activities. The placebo test with marketing practices suggests that this is unlikely. Nonetheless, the remaining specifications in Table 3 test the impact of loan treatments on other measures of entrepreneurs' effort in business.

Columns 6-7 present the ITT and TOT effects on the average number of hours businesses are open, conditional on being operational at endline. The estimates in Column 7 suggest that businesses that received the loans immediately after training operated for around nine fewer hours per week than businesses in Control. Interestingly, businesses that receive loans after a few weeks' delay also operate their businesses for fewer hours than those in Control. The average weekly operation hours of businesses in the Delayed Loan group are not statistically different than that of the Control group. Nonetheless, they exhibit more than half of the drop that businesses in the Concurrent Loan group do.

Reduced hours for business operations do not appear to be consistent with treated business owners reducing unproductive business hours either. As Columns 8-9 show, the number of daily customers that these businesses serve is significantly lower, particularly for businesses that take loans. For instance, borrowers in the Concurrent Loan group serve about 50% fewer customers per day compared to the Control mean at baseline, while borrowers in the Delayed Loan group serve 25% fewer customers.

In sum, our estimates demonstrate that businesses that received the loans immediately after

completing training implemented fewer best practices taught during training. Moreover, our estimated TOT effects are consistent with this negative effect being driven by the loan itself rather than the training independently of the loan.

We also find that entrepreneurs who received loans spent less time and effort on their businesses overall. In contrast to the effects on business practices, this negative effect persists for entrepreneurs in both Loan treatments, although it is quantitatively higher for the Concurrent Loan group. Nonetheless, it is plausible that the entrepreneurs who receive loans make profitable investments in their businesses that are not captured by business practices and operation hours. In the next section, we analyze the effects of our treatments on business performance which, in any case, is likely to capture the effects of the overall effort of the entrepreneurs on their businesses.

4.3 Effect of Loan Intervention on Business Performance

After examining the effects of loans and their timing on business practices and entrepreneurs' efforts in business, we now test for the overall effects of loans and their timing on business performance. In particular, we consider two key indicators of business performance - business survival and, conditional on the business's survival, sales revenue.

Column 1 of Table 4 reports the ITT estimate of the effect of being drafted into the Loan treatments on the likelihood that a surveyed business reports being operational at the endline. Because we cannot estimate changes in firm survival as a within-firm change, we report differences in means at endline. We find that firms in the Delayed Loan treatment are no more or less likely to be operational than the Control group. However, firms in the Concurrent Loan treatment are four percentage points less likely to be operational than both the Control group and the Delayed Loan group.[26] Column 2 reports TOT estimates on firm survival at endline. The point estimate for TOT is double the magnitude of the ITT estimate. Along with the fact that loan uptake was around 50%, the TOT estimates suggest that the entire ITT effect is driven by loan uptake.

Next, we estimate the effect of the intervention on business revenue conditional on survival. We estimate within-firm changes in average daily sales revenue. Column 3 reports the estimates. Concurrent Loan treatment reduces average daily sales by about 2,500 KES, equivalent to about a third of daily sales for Control firms at baseline. Similar to our results on operation hours, firms

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[26]For comparison, 98% of businesses in Control group surveyed during endline are operational. Baseline values for Control group are reported at the bottom of the table.

in the Delayed Loan treatment also experience a decline in sales but by a lower and statistically insignificant margin - about 1,600 KES. Control firms, on the other hand, report a small increase in sales over the same period. We are statistically underpowered to reject that the two loan treatments have the same effect. In terms of magnitudes though, Concurrent Loan treatment reduces sales 1.5 times more than Delayed Loan treatment. Column 4 estimates the TOT effect on sales and finds a similar pattern as the previous column. Access to loans reduces sales revenue.[27] Importantly, the treated firms not only perform worse compared to the Control firms, but they also perform worse relative to their pre-intervention performance.

In sum, entrepreneurs in the Concurrent Loan group generate lower sales revenue when they remain operational. They are also more likely to have shut down their businesses altogether. There is a similar negative effect on effort and sales revenue for the Delayed Loan group but by a smaller magnitude. In other words, access to loans generally depresses business performance for both treatment groups. This is consistent with our findings in the previous section that entrepreneurs exert less effort in their businesses after receiving loans. It is useful to highlight that the adverse effect on performance is in addition to the negative effect of immediate access to loans on the improvement of business practices. The former is unlikely to be driven by the latter as business practices in both Control and treated firms appear to have either improved or remained stable between the pre- and post-intervention periods.

Do the negative effect on business performance persist beyond an immediate short-run? As we mentioned earlier, our experimental design allows us to rigorously identify treatment effects only over a short run (6-10 weeks) before firms in the Control group are provided access to loans. However, Technoserve conducted an additional follow-up survey in early November 2021 - about seven months after the Concurrent Loan group received access to loans and 4.5 months after the Control group received similar access immediately after the endline surveys. We cannot use this survey to identify treatment effects as the Control group is already treated at the time these data were collected. Nonetheless, this enables us to test the persistence of treatment effects over this period with a descriptive analysis.

To be concrete, Figure 3 extends Figure 2 to show reported sales value across experimental

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[27]The estimates of treatment effects on sales revenue are conditional on firm survival. Given that the treated firms are more likely to be closed because of the intervention, we would obtain larger estimates had we imputed a value of zero for sales of closed businesses.

groups in the last follow-up survey. We make two observations from this graph. First, the declines in sales revenue for firms in the Concurrent Loan group (and the Delayed Loan group) appear to persist as many as seven months after the Concurrent Loan group receives loan access. The levels, moreover, appear to be remarkably similar as in the endline surveys. Second, reported sales for firms in the Control group - which was similar to firms in the Delayed Loan group at baseline - decline by a similar margin after being treated as for firms in the Delayed Loan group. This reinforces our previous estimates of treatment effects and lends further support to our interpretation that access to loans reduces firm sales.

The adverse effect of loans on business performance is a stronger result compared to null effects of loans as traditionally found in the microfinance literature (e.g., Meager (2019)). This is reminiscent of the finding in Karlan and Zinman (2011) that access to loans leads to business closures in the Philippines. We find similar results on business survival. Additionally, we document an adverse effect on the intensive margin of business performance: even treated businesses that survive perform worse than Control businesses. In the next section, we conduct extensive tests to verify our interpretation of the business performance results.

5 Robustness Tests and Alternative Interpretations

Given the importance of our findings on the adverse effect of loans on business performance, it is important to establish the robustness of these results. We conduct several tests in this section to do so. First, we establish that the large treatment effects on sales are not driven by outliers or a few treated businesses reporting unusually low sales during endline surveys. Second, given non-trivial attrition from endline surveys, we compute conservative bounds for treatment effects under the assumption of no attrition and show that the treatment effects are always (at least weakly) negative. We also test for several alternative interpretations of the results. Additionally, we test for heterogeneity in treatment effects by businesses likely to be more or less dependent on owner time and show that businesses that rely more on their owners' time experience bigger declines in performance. This provides additional support to our interpretation that business performance declines because the treated entrepreneurs invest lower effort on their business.

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5.1 Outliers

We establish the robustness of our estimates to outliers in two ways. First, we winsorize the values of daily sales at their 1% and 99% levels separately for baseline and endline. Subsequently, we re-estimate ITT effects on sales using the winsorized values. This reduces the likelihood that our estimates of treatment effects are driven by extremely low or high sales values reported by the firms. Reassuringly, as Column 1 of Table 5 shows, the new estimates are at least as large as that with raw values.

Second, we re-estimate treatment effects using log-transformed sales instead of levels. Monotonic transformations such as the log-transformation reduce the distortion in estimates from extremely small or large values in the outcome. As before, we estimate a larger treatment effects for both treatments using the transformed values than in the original specification. The estimates for the two treatment groups are also statistically different with this specification (p-value<0.03).

5.2 Attrition

Our difference-in-difference approach in our analysis allows us to estimate the causal impacts of the treatments on firms that we could reach in baseline as well as endline surveys. However, it is unclear whether our average treatment effects would change had we been able to capture the attritors in the endline surveys. In other words, besides estimating treatment effects on the respondent sample, we would also like to understand the treatment effects on the entire baseline sample of firms that we started with.

Under the assumption that the sample attrition between baseline and endline surveys is similar across experimental groups in both attrition rates and firm characteristics, the treatment effects without attrition are likely to be similar to what we have already estimated. However, Column 3 of Table 5 shows that there is relatively higher attrition, especially in the Concurrent Loan group: 22% in the Delayed Loan group and 30% in the Concurrent Loan group could not be surveyed at the endline as opposed to 18% in the Control group (see also Table A3). If the attrition differs across experimental groups because of treatment status, it becomes less clear what the treatment effects would be in the full baseline sample. We conduct two sets of tests to understand how attrition may affect our estimation of treatment effects.

First, we explore how firm characteristics vary across baseline firms that did or did not respond

to the endline survey. We test the same baseline characteristics as we did in Table 2 but now make a distinction between *respondents* and *non-respondents* in Table A3. Non-respondent firms are similar to respondent firms in all characteristics within the Control group. There are, however, a few exceptions within the treatment groups. Non-respondents in both Loan groups are about 5% less likely to be their firms' sole proprietors than respondents. Additionally, respondents in the Concurrent Loan group are marginally more likely to be a grocery business than non-respondents, and, perhaps relatedly, employed half an employee less than non-respondents at baseline. In any case, it appears unlikely that the small differences in business types and additional employees across respondents and non-respondents in the treatment groups would substantially change our estimates had there been no attrition from the baseline sample.

Nonetheless, we compute Lee-bounds for our ITT estimates (Lee, 2009) to derive the upper and lower bounds of the treatment effects. Comparing one experimental group to another, Lee's bounds-estimator trims the experimental group that has the least attrition so that the share of observations with observed outcomes is equal for both groups. In the group with the least attrition, observations with the largest or smallest outcome values are considered, somewhat conservatively, "excess observations" and excluded from the analysis to derive the lower or upper bounds for estimates of treatment effects. Lee's bounds estimation in its original form can only be computed for differences in means. Therefore, Column 4 of Table 5 re-estimates ITT effects on sales but in a traditional differences-in-means framework. This serves two purposes. First, it provides an estimate in a more conventional estimation method for experiments. Second, it provides a benchmark for the bounds estimates that follow. The estimates in Column 4 are similar to difference-in-difference estimates from earlier. If anything, the estimate for Concurrent Loan treatment is now larger in magnitude.

Column 5 estimates the lower and upper bounds for ITT estimates for Concurrent Loan treatment. Given attrition is particularly different for the Concurrent Loan group (see Tables 5 and A3), we primarily focus on attrition for this group. Both the lower and upper bounds for treatment effect from the Concurrent Loan are negative in magnitude although the upper bound is imprecisely estimated. Column 6 provides tighter bounds by controlling for the nature of business (e.g., whether a business is a *duka* or not). Both bounds increase in magnitude although the upper bound remains statistically insignificant.[28]

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[28]The bounds for the Delayed Loan group is similar to the original estimate and tighter compared to the Concurrent

5.3 Heterogeneous Treatment Effects by Owner Time Dependence

We now conduct tests of heterogeneous treatment effects to check how they vary across firms that may be more or less sensitive to an owner's work time. If our interpretation that owners of the borrowing firms consider the loans as an income substitute and hence reduce their effort, we would expect that firms that rely relatively more on their owners' time experience the most decline. First, we distinguish between businesses that are duka and those that are not, because duka in our setting are less permanent structures that customers associate with more flexible schedules and inventories than the other business categories in our sample. The duka-owners might find it easier to substitute away from work time relatively more easily than other firms. Second, we split our sample by businesses that have employees and those that do not because employees may reduce the impacts of owner work time reductions. Third, we split our sample into firms that have owners who also own another business at baseline. These firm owners may have more demands on their time overall and, thus, be more responsive to opportunities to reduce their work time. These analyses are presented in Table 6.

Columns 1-2 of Table 6 splits the sample by dukas and non-dukas (grocers, chemists, salons, and tailors), and demonstrates that the treatment effects of the Concurrent Loan group on average sales are larger for dukas than for non-dukas. Moreover, there is a similarly larger negative impact of the Delayed Loan group on average sales for dukas than for non-dukas as well. On the other hand, in Columns 3-4 we find similar estimates of treatment effect on firms with and without employees. Lastly, Columns 5-6 demonstrate that firms owned by individuals who own another business at baseline are more negatively impacted by both the concurrent and delayed loans than those owned by individuals who do not own another business. Overall, businesses that are more sensitive to owners' time flexibility appear to be more negatively impacted by the training and loan program.

5.4 Alternative Interpretations

Our preferred interpretation of the relatively worse business performance of the treated groups is that the treated entrepreneurs who take loans reduce effort into their business leading to a decline in business performance. In principle, there could be a few alternative interpretations of the results. We carefully test them below.

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Loan group.

Low Training Completion in Control: Although training was offered to all three experimental groups, a relatively higher share of firms in the Loan treatment groups completed training than in the Control group (see Section 4.1). As a result, the Control and the treatment groups differ not only in access to loans but also in training completion rates. This does not affect the estimation of our ITT effects, as we compare the experimental groups in their entirety regardless of training completion. However, this has an implication on how we interpret the estimates. In theory, it is possible that the difference in performance between the Control and treatment groups is not driven by the loans but by a perverse effect from the training. Our TOT estimates in Table 4, however, suggest that it is in fact the loans that drive the decline in firm performance. Nonetheless, we conduct additional tests to disentangle the effects of loans and training.

Specifically, we split the sample into firms that complete the training but do not borrow money and firms that do borrow money after completing training. We use Control firms who do or do not eventually take loans after the endline survey as corresponding counterfactuals and re-estimate treatment effects for two sets of firms separately. This allows us to verify whether it is indeed the loans that lead to lower business performance while accounting for potentially endogenous firm characteristics that correlate with the firms' decisions to borrow money. Table A4 reports the results. Firm performance declines heavily among treated firms in Column 1 when firms take loans after completing their training. Column 2, reassuringly, shows that there is no substantial difference in business performance between treatment and Control group firms that do not take loans.

Misreporting of Sales in Treatment Groups: Even if the drop in reported sales among treated businesses is caused by loans, it may be a difference in how they report sales and not necessarily in actual sales. For instance, business owners may believe that a drop in sales could help them get away without paying back their loans (though, as we show later, loan repayment did not seem to be a primary concern among loan takers). If the drop in sales reported by firms who took loans is simply driven by the firms hiding their sales in order to avoid loan repayment, we expect this to be more likely among business owners who borrow larger sums. Instead, in Appendix Table A5, we find the reverse—there is a negative correlation between the likelihood that a firm reports a decline in sales at endline (compared to baseline) and the size of the loan they borrow. This is true on average among all treated businesses and as well as within each treatment group.

Misreporting of Sales in Control Group: A related concern is that firms in the Control

group over-report their sales at endline to ensure that they obtain loans after the endline. It is difficult to rule out this concern completely as we do not have a counterfactual group that was not offered loans at all. Nonetheless, we conduct the following test. As before, we split our Control group into two sets of firms - those who eventually take loans when they are offered loans at the end of the study (n=32) and those who do not take loans even when offered (n=120). We then re-estimate treatment effects for the two treatment groups in comparison with the two separate subsets of Control group firms. This is similar to what we did in Table A4, but now, we do not split the treatment groups and compare them in their entirety to the split Control groups.

Table A6 in the Appendix reports the results. Column 1 estimates ITT effects for the two treated groups while comparing them with firms in the Control group that eventually take loans after the end of the study period. Column 2 estimates similar ITT effects but uses Control group firms that did not take loans. The estimated treatment effects are similar in both columns. We do find some evidence that would-be borrower Control firms in Column 1 report a higher increase in sales between baseline and endline relative to non-borrower Control firms in Column 2. However, importantly, we find almost equally strong treatment effects against non-borrower Control firms that would not have the incentive to over-report sales at endline. This confirms that our previous estimates of treatment effects are unlikely to be driven by the over-reporting of sales by Control firms.

Loan Repayment Burdens: Finally, it is possible that access to loans reduces business performance because the borrowing businesses need to repay the loans. Note that we estimate treatment effect on business sales and not business profit. Thus, we measure business performance overall and not what the entrepreneurs earn net of loan repayments. Nonetheless, the need to repay loans could place additional burden and stress on the owner and reduce performance. However, we find that only around 1% repay their loans in full. This does not differ by treatment. Also, only about 50% of the borrowers repay some part of their loans. Conditional on some repayment, the firms repay only 38% of their loan amounts on average. We measure repayment as any repayment made over the seven months following the Concurrent group's loan access. These patterns perhaps reflect the fact that Technoserve did not enforce strict measures to ensure loan repayment and the main incentive for repayment was access to subsequent loans.[29] Importantly, they also suggest

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[29]Lending programs that credibly enforce repayment are likely to yield a higher repayment ratio (e.g. as in Hsu et al., 2021).

that given the upfront knowledge of soft nature of the loans, the businesses might not have taken repayments seriously. Thus, the negative effects from the loan treatments are unlikely to have been driven by duress caused by loan repayment requirements. These results further reinforce the interpretation that loans were treated as income substitutes that distracted borrowing entrepreneurs from their businesses.

6   Conclusion

In this paper, we study the effects of combining an SME financial literacy training program with access to an interest-free loan, where the loans are provided either immediately after the training or after a few weeks delay. We find that there is an improvement in financial-management practices among businesses that receive delayed access to loans while providing access to loans immediately after training leads to no improvement. This is consistent with immediate access to loans diverting entrepreneurs' attention from improving their business practices.

We also document a strong negative effect of providing access to loans on business performance. The negative effect is driven primarily by businesses that took out the offered loan rather than those that took up the training but declined the loan. Findings from additional analyses are consistent with the interpretation that entrepreneurs who take loans focus less on their businesses which in turn lead to a fall in business performance. The loans might have reduced pressure on SME owners to generate income (Evans and Nguyen, 2019). Moreover, the negative performance effects of the loan treatments persist for as many as seven months after the loans are provided suggesting that even short-term reallocation of effort away from businesses can have meaningful longer-run effects (Tohidi et al., 2022). Our findings suggest that limited access to finance is not a barrier to the success of SME financial literacy training, at least in our context. Moreover, they suggest that policies that seek to provide access to complementary financial capital to help SMEs grow could benefit by carefully considering when such access is provided after the training, and could even condition this access on specific business investments (such as improvement in business practices) being made.

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Figure 1: Training and Loan Uptakes in Full Sample
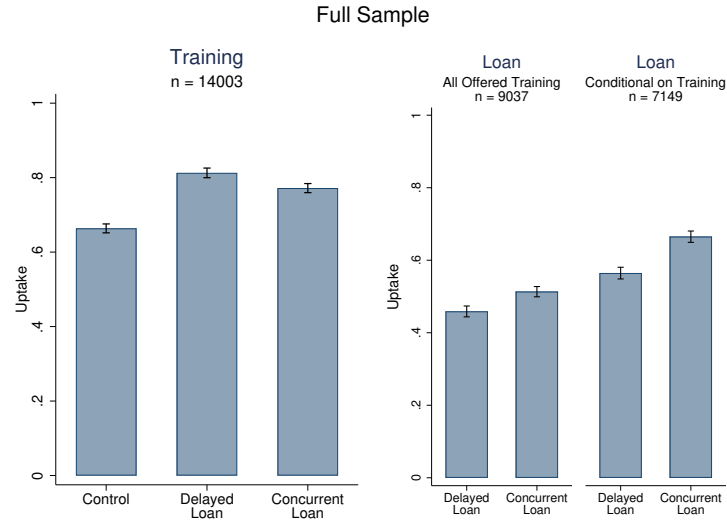
Note: The figure shows the share of businesses who undertook training (left panel) and applied for loan (right panel) from among the full sample of SMEs in the study. The first two bars on the right panel show the uptake of loans among SMEs who were offered training and loan. The last two bars show the uptake of loans among SMEs who successfully finished the training. The vertical bars depict 95% confidence intervals.

Figure 2: Test of Identification Assumption
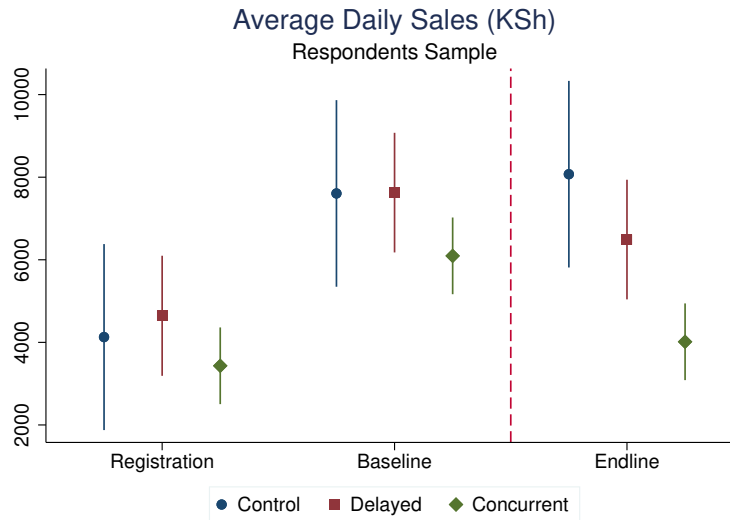
Note: The figure depicts point estimates of average daily sales for businesses in Control and Loan treatments during registration, baseline survey, and endline survey. The dashed vertical line represents the implementation of the intervention. Only businesses that could be reached during the endline survey are considered. Solid vertical lines depict 95% confidence intervals.

Figure 3: Longer-Run Effect
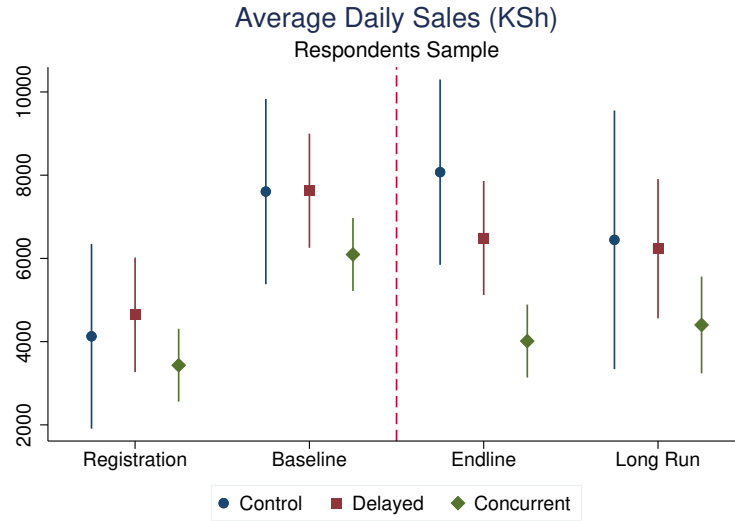
Note: The figure depicts point estimates of average daily sales for businesses in Control and Loan treatments during registration, baseline survey, endline survey, and a final follow-up survey. The dashed vertical line represents the implementation of the intervention. Only businesses that could be reached during the endline survey are considered. Solid vertical lines depict 95% confidence intervals.

Table 1: Summary Statistics

	N	Mean	SD
Panel A: SME Characteristics at Baseline			
Sole Proprietorship	415	0.99	0.10
Number of Employees	415	0.60	1.15
Duka	415	0.52	0.50
Grocer	415	0.23	0.42
Business is Licensed	415	0.57	0.50
Business Owner Female	415	0.68	0.47
Business is Primary Income Source	415	0.98	0.15
Business Owner has Other Businesses	415	0.26	0.44
Panel B: Performance at Baseline			
Average Sales per Day	415	7045.65	13363.29
Hours Open per Week	415	78.34	18.45
Average Customers per Day	415	35.92	32.47
Financial Competency Score	415	2.14	0.53
Marketing Competency Score	415	1.71	0.58

Note: The table presents summary statistics for SME characteristics at baseline (Panel A) and business performance at baseline (Panel B). Only respondents of the endline survey are considered.

Table 2: Baseline SME Characteristics by Loan Groups

	(1)	(2) Control	(3) Concurrent	(4) Delayed	(5)	(6)
	N	Mean	Mean	Mean	(2)-(3)	(2)-(4)
Sole Proprietorship	415	0.99	0.99	0.99	0.005	0.005
Number of Employees	415	0.68	0.53	0.57	-0.144	-0.110
Duka	415	0.45	0.53	0.60	0.075	0.147**
Grocer	415	0.26	0.26	0.14	-0.001	-0.123**
Business is Licensed	415	0.53	0.43	0.76	-0.100*	0.235***
Business Owner Female	415	0.70	0.69	0.64	-0.011	-0.065
Business is Primary Income Source	415	0.97	0.97	0.99	-0.004	0.018
Business Owner has Other Businesses	415	0.26	0.23	0.28	-0.031	0.019
Average Sales per Day	415	7549.68	5920.68	7608.62	-1,629.001	58.945
Hours Open per Week	415	76.84	78.63	79.87	1.793	3.035
Average Customers per Day	415	36.94	36.12	34.48	-0.815	-2.455
Financial Competency Score	415	2.11	2.11	2.22	0.001	0.107
Marketing Competency Score	415	1.69	1.71	1.74	0.020	0.050

Note: The table presents mean of SME characteristics by experimental groups. Only respondents of the endline survey are considered. Concurrent (resp. Delayed) refers to the Concurrent (resp. Delayed) Loan group. The last two columns report the differences in mean characteristics of SMEs in Control group and Concurrent Loan group (Column 5) or Delayed Loan group (Column 6). and *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 3: Treatment Effect on Business Practices

	(1) Financial Competency ITT	(2) Financial Competency TOT	(3) Knowledge Score ITT	(4) Marketing Competency ITT	(5) Weekly Hours ITT	(6) Weekly Hours TOT	(7) N. D. Customers ITT	(8) N. D. Customers TOT
Concurrent Loan X Post	-0.19*	-0.33**	0.09	-0.01	-5.23	-9.07**	-11.23**	-19.49***
	(0.11)	(0.14)	(0.07)	(0.11)	(3.55)	(4.41)	(4.99)	(7.44)
Delayed Loan X Post	-0.05	-0.10	0.01	0.07	-2.61	-5.10	-4.73	-9.24*
	(0.10)	(0.14)	(0.02)	(0.10)	(3.20)	(4.73)	(3.53)	(4.71)
Post	0.38***	0.38***		0.07	-1.42	-1.42	2.92	2.92
	(0.08)	(0.05)		(0.07)	(2.18)	(1.53)	(2.81)	(1.97)
Observations	803	803	3,728	803	804	804	804	804
Control Mean at Baseline	2.12	2.12	.75	1.69	77.28	77.28	37.01	37.01
KP Wald F Stat.	-	26.2	-	-	-	26.21	-	26.21
SME Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	No	No	No	No	No

Note: *Post* is a dummy variable that takes the value one for post-intervention period. Competency scores are the total number of best practices a firm implement in financial management or marketing. *Knowledge Score* is computed from regular tests of knowledge that firms are supposed to learn during training. *Weekly Hours* is the total number of hours a firm operates during a week. *N.D. Customers* refers to the number of customers a firm serves daily. *ITT* refers to intention-to-treat estimation, while *TOT* refers to treatment-on-the-treated effects. TOT estimation estimates the treatment effect on firms that take loans, using treatment assignment as an instrument for loan uptake. Standard errors are in parentheses, clustered by SME locations. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 4: Treatment Effects on Business Performance

	(1) Firm Survival	(2) Firm Survival	(3) Daily Sales	(4) Daily Sales
	ITT	TOT	ITT	TOT
Concurrent Loan Treatment	-0.04*	-0.08*		
	(0.02)	(0.05)		
Delayed Loan Treatment	0.00	0.01		
	(0.02)	(0.03)		
Concurrent Loan X Post			-2,546.68**	-4,421.32***
			(1,056.34)	(1,369.99)
Delayed Loan X Post			-1,602.88	-3,130.62*
			(1,448.20)	(1,844.69)
Post			466.12	466.12
			(761.24)	(534.26)
Observations	415	415	804	804
Control Mean at Baseline	.98	.98	7607	7607
Concurrent - Delayed = 0	[0.035]	[0.06]	[0.51]	[0.48]
KP Wald F Stat.	-	56.68	-	26.21
SME Fixed Effect	No	No	Yes	Yes

Note: *Post* is a dummy variable that takes the value one for post-intervention period. *Firm Survival* is a dummy variable that takes the value one if a firm reports being operational during endline survey. *Daily Sales* is average daily sales revenue in Kenyan Shillings (KES). *Full (resp. part) Repayment* is a dummy variable that takes the value one if a firm repays its loan in full (resp. part). Columns 1-2 consider all respondents of endline surveys, Columns 3-4 consider firms that report to be operational, and Columns 5-6 consider firms who took loans following their training. *ITT* refers to intention-to-treat estimation, while *TOT* refers to treatment-on-the-treated effects. TOT estimation estimates the treatment effect on firms that take loans, using treatment assignment as an instrument for loan uptake. Standard errors are in parentheses, clustered by SME locations. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 5: Robustness to Outliers and Attrition

	(1) Winsorized D. Sales	(2) Ln D.Sales	(3) Attrition	(4) Daily Sales	(5) Daily Sales	(6) Daily Sales
Concurrent Loan X Post	-3,142.50***	-0.54***				
	(937.01)	(0.14)				
Delayed Loan X Post	-2,076.30*	-0.19				
	(1,140.99)	(0.14)				
Post	1,059.54*	0.07				
	(584.66)	(0.09)				
Concurrent Loan			0.12***	-4,058.46***		
			(0.04)	(1,327.30)		
Delayed Loan			0.04	-1,583.10		
			(0.04)	(1,439.66)		
Concurrent Loan: Lower Bound					-5,335.44***	-5,572.22***
					(1,609.66)	(1,727.01)
Concurrent Loan: Upper Bound					-210.77	-606.95
					(695.74)	(708.40)
Observations	804	804	524	402	364	364
Control Mean	6126	7607	.18	8072.7	-	-
SME Fixed Effect	Yes	Yes	No	No	No	No
Other Controls	No	No	No	No	No	Yes

Note: All specifications report intention-to-treat estimates. *Post* is a dummy variable that takes the value one for post-intervention period. The outcome variable in Column 1 is average daily sales winsorized at the bottom and top 1% values. Column 2 uses a logarithmic transformation of daily sales. *Attrition* is a dummy variable that takes the value one if a firm was surveyed at baseline but did not respond during endline surveys. Columns 5-6 report Lower and Upper Bounds of treatment effects for the Concurrent Loan group using Lee-bounds (Lee, 2009). *Other Controls* in Column 6 include a control for whether a firm is a duka or not. Standard errors are in parentheses, clustered by SME locations. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 6: Heterogeneous Treatment Effects

|	(1) Daily Sales Duka	(2) Daily Sales Not Duka	(3) Daily Sales Empl.=0	(4) Daily Sales Empl.>0	(5) Daily Sales Oth. Bus.	(6) Daily Sales No Oth. Bus.
Concurrent Loan X Post	-3,489.22*	-1,432.91	-2,782.60**	-2,212.96	-4,332.28**	-1,856.38
	(1,768.54)	(994.51)	(1,111.64)	(2,623.52)	(1,749.93)	(1,243.32)
Delayed Loan X Post	-2,619.15	74.26	-1,456.10	-1,584.69	-6,164.50*	159.39
	(1,993.99)	(2,036.06)	(1,234.99)	(3,455.80)	(3,309.07)	(1,571.24)
Post	814.94	108.00	1,029.50	-617.31	2,825.61**	-405.41
	(1,342.86)	(758.83)	(646.64)	(2,286.84)	(1,220.97)	(956.64)
Observations	460	344	524	280	214	590
SME Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: All specifications report intention-to-treat estimates. *Daily Sales* is average daily sales revenue in Kenyan Shillings (KES). *Post* is a dummy variable that takes the value one for post-intervention period. Column 1-2 considers sample conditional on whether a firm is a duka or not. Column 3 considers firms that do not employ an additional employee. Column 4 considers firms that employ at least one additional employee. Columns 5-6 considers sample conditional on whether an entrepreneur has additional businesses. Standard errors are in parentheses, clustered by SME locations. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.
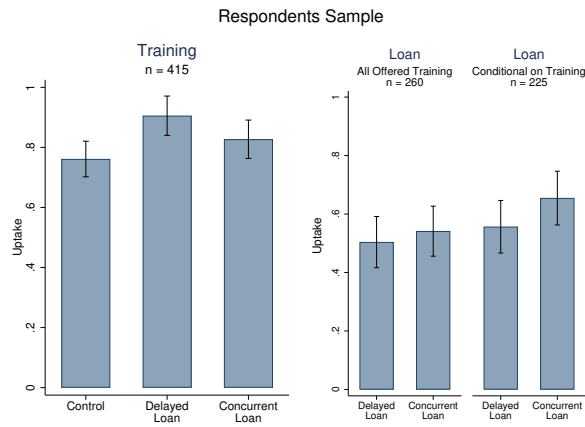
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A   Additional Figures and Tables

Figure A1: Distribution of Entrepreneurs' Age
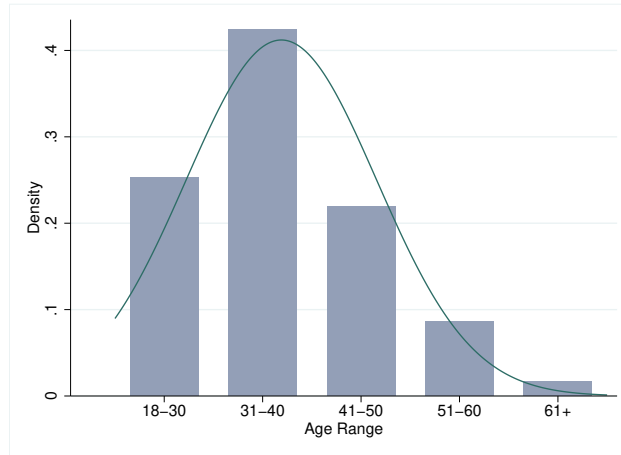
Note: The figure shows the distribution of age of entrepreneurs who could be reached in both baseline and endline surveys (n=415).

Figure A2: Training and Loan Uptakes in Respondent Sample

Note: The figure shows the share of businesses who undertook training (left panel) and applied for loan (right panel) from among the sample of SMEs who responded to the endline survey and their businesses were operational. The first two bars on the right panel show the uptake of loans among SMEs who were offered the training and loan. The last two bars show the uptake of loans among SMEs who successfully finished the training. The vertical bars depict 95% confidence intervals.

Table A1: Sample Sizes

	Total N	Control N	Concurrent Loan N	Delayed Loan N
Full Sample	14003	4966	4791	4246
Baseline Sample	526	185	181	160
Respondent Sample	415	155	133	127

Note: *Full Sample* refers to the entire sample of firms that registered with Technoserve in 2020-21 to avail its training and loan services. *Baseline Sample* refers to a subset of firms that were randomly sampled for a detail baseline survey. *Respondent Sample* refers to firms in Baseline Sample that consented to an endline survey.

Table A2: Comparison of Full Sample & Survey Samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		Baseline Sample		Respondents	
	N	Mean	N	Mean	N	Mean
Duka	14003	0.53	525	0.57	415	0.56
Grocer	14003	0.26	525	0.22	415	0.22
Business is Licensed	14003	0.58	525	0.56	415	0.57
Business Owner Female	14003	0.70	525	0.69	415	0.70
Business is Primary Income Source	14003	0.99	525	0.98	415	0.98
Average Sales per Day	14003	4173	525	3950	415	4013

Note: The table compares characteristics of firms in the full sample, baseline sample, and respondent sample. Data on the reported characteristics were collected when the firms in the full sample registered with Technoserve to be able to access training and loan.

Table A3: Baseline Characteristics of Respondents and Non-respondents in Endline Survey

	Control		Concurrent Loan		Delayed Loan		Control	Concurrent	Delayed
	Resp.	Non-Resp.	Resp.	Non-Resp.	Resp.	Non-Resp.	Difference	Difference	Difference
N	155	30	133	48	127	33	125	85	94
Sole Proprietorship	0.99	1.00	0.99	0.94	0.99	0.94	-0.013	0.055**	0.053**
Number of Employees	0.68	0.53	0.53	1.02	0.57	0.33	0.144	-0.487*	0.234
Duka	0.45	0.53	0.53	0.58	0.60	0.58	-0.082	-0.057	0.023
Grocer	0.26	0.40	0.26	0.15	0.14	0.24	-0.135	0.117*	-0.101
Business is Licensed	0.53	0.50	0.43	0.35	0.76	0.73	0.029	0.074	0.037
Business Owner Female	0.70	0.70	0.69	0.65	0.64	0.64	0.003	0.046	0.001
Business is Primary Income Source	0.97	1.00	0.97	0.96	0.99	1.00	-0.026	0.012	-0.008
Business Owner has Other Businesses	0.26	0.30	0.23	0.31	0.28	0.21	-0.035	-0.079	0.071
Average Sales per Day	7549.68	8456.67	5920.68	6246.33	7608.62	5636.36	-906.99	-325.66	1972.26
Hours Open per Week	76.84	79.53	78.63	83.90	79.87	83.91	-2.695	-5.264	-4.035
Average Customers per Day	36.94	43.10	36.12	34.83	34.48	32.52	-6.165	1.287	1.965
Financial Competency Score	2.11	2.17	2.11	2.04	2.22	2.14	-0.065	0.074	0.080
Marketing Competency Score	1.69	1.70	1.71	1.63	1.74	1.62	-0.010	0.086	0.119

Note: The table reports means of baseline characteristics for firms that responded to endline surveys (*Resp.*) and firms that did not respond to endline surveys (*Non-Resp.*), separately for each experimental group. The last three columns report differences in means in baseline characteristics of respondent and non-respondent firms within corresponding experimental groups. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A4: Decomposition of Training & Loan Effects

	(1) Daily Sales Loan	(2) Daily Sales No Loan
Concurrent Loan X Post	-3,479.44**	-972.18
	(1,481.09)	(1,669.18)
Delayed Loan X Post	-2,866.33	-575.47
	(1,791.80)	(2,523.57)
Post	806.25	366.47
	(1,229.03)	(1,255.47)
Observations	336	340
SME Fixed Effect	Yes	Yes

Note: All specifications report intention-to-treat estimates. Column 1 considers firms that take loans following their training. Column 2 considers firms that receive training but do not take up loans. The corresponding counterfactuals are Control firms who do and do not take loans after the endline survey, respectively. *Daily Sales* is average daily sales revenue in Kenyan Shillings (KSh). *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A5: Misreporting of Sales in Treatment Groups

	(1) Sales Decline	(2) Sales Decline	(3) Sales Decline Concurrent	(4) Sales Decline Delayed
Above Median Loan Size	-0.32***	-0.29***	-0.49***	-0.19
	(0.11)	(0.10)	(0.12)	(0.16)
Baseline Sales		0.00***	0.00**	0.00***
		(0.00)	(0.00)	(0.00)
Observations	136	136	64	72

Note: All specifications report intention-to-treat estimates and considers only treated firms that take loans following their training. *Sales Decline* is a dummy variable that takes the value one if a firm reports a decline between baseline and endline. Column 3 (resp. Column 4) considers only the Concurrent (resp. Delayed) Loan group. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A6: Treatment Effects against Borrowing & Non-borrowing Control Firms

	(1) Daily Sales (KSh) Borrower Control	(2) Daily Sales (KSh) Non-Borrower Control
Concurrent Loan X Post	-2,886.81**	-2,455.98**
	(1,426.68)	(1,163.31)
Delayed Loan X Post	-1,943.01	-1,512.18
	(1,738.29)	(1,528.17)
Post	806.25	375.42
	(1,223.29)	(903.65)
Observations	564	740

Note: All specifications report intention-to-treat estimates and control for SME Fixed Effects. Both specifications consider treatment groups in their entirety and compare them to either Control firms who take loans (Column 1) or do not take loans (Column 2) following the endline survey. *Daily Sales* is average daily sales revenue in Kenyan Shillings (KSh). *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.