Mentors or Teachers?

Microenterprise Training in Kenya[*]

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Abstract

We use a randomized controlled trial to demonstrate that inexperienced female microenterprise owners in a Kenyan slum benefit from mentorship by an experienced entrepreneur in the same community. Using seven rounds of surveys, we find that mentorship increased profits by 20% on average with initially large effects that fade out as matches dissolve. We conduct a formal business education intervention, which has no effect on profits despite changes in business practice. Our results demonstrate that missing information is a salient barrier to profitability, but the type of information matters: access to the localized, specific knowledge of mentors increases profit while abstract, general information from the class does not.

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1 Introduction

In urban areas of developing countries, microenterprises are ubiquitous and a large fraction of the workforce is engaged in entrepreneurship.[1] Many of these very small firms generate low profit and have few or no employees. Understanding the reason that these firms have low profits, and why they generate so little employment, is key for designing policy to improve the welfare of the urban poor. One possibility is that microenterprise owners lack what Bloom and Van Reenen (2007) and Bruhn, Karlan and Schoar (2010) refer to as *managerial capital*. That is, they lack the skill or know-how to run a business, which limits their profitability and operational scale.

If this lack of information or managerial capital is a serious impediment to growth, business training classes offer an appealing solution: a well-designed business training curriculum may increase business profit, and may be scalable and easily portable across environments. However, as summarized by McKenzie and Woodruff (2014) and Blattman and Ralston (2015), where formal business classes have been offered to entrepreneurs, they have had limited impact. One possible interpretation of these results is that, in fact, a lack of information or business knowledge is not an explanation for the lack of profitability in these small firms.

In this paper, we provide evidence that a lack of information is a salient limitation for the operators of microenterprises even in an environment in which classes have no significant effect on profits. Our insight is that a distinction must be made between the abstract principles commonly taught in formal business classes (such as bookkeeping, marketing, managing costs, and selection of product lines) and market-specific, localized information (such as what location has highest demand, what supplier sells at lowest cost, or what products can be sold for the greatest profit). We show that the local expertise provided by a mentor can significantly benefit young entrepreneurs, even when formal classes fail to do so.

We use a randomized controlled trial to evaluate the effects of two different types of business training: formal business classes and mentorship from a successful entrepreneur

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[1] According to Gollin (2008), developing countries have substantially higher rates of entrepreneurship than rich countries. The proportion of workers that identify as entrepreneurs is 75.3% in Nigeria, 74.5% in Bangladesh, and 50.4% in Guatemala. This compares to 8.2% in the United States and 10.6% in the United Kingdom.

in the same community. We randomly assign young and inexperienced microenterprise owners into three groups. The first group receives one-to-one matching with a mentor from the same community and that operates in the same industry, the second group receives formal business classes, and the third group receives neither. Mentors are drawn from the set of successful business owners within the same community.[2] Rather than direct the content of mentor-mentee interactions, we allow the mentors to relay whatever information they believe will be beneficial to their mentees. The business classes are conducted by instructors from a local university, and cover general business topics such as accounting, marketing, cost control and business planning.

We find that mentorship is effective at increasing profit among young microenterprises. Over the seventeen months following treatment, weekly profits are on average 20 percent higher among the mentor treatment than the control, but the effect fades over time as mentor-mentee matches dissolve. We find changes in business practice from mentorship that are consistent with our interpretation that mentees are gaining local, market-specific information from their mentors. Mentees are more likely to have switched suppliers in the aftermath of the treatment, which generates greater profitability per unit sold. We find that this increase in profitability is driven by lower costs rather than greater revenue or a more profitable product mix. We find little evidence of changes in firm quality (customer relations, marketing, production quality, etc.) for the mentor treatment.[3] We further exploit a discontinuity in our mentor selection procedure and find no statistically significant effects on profit, suggesting that the effect on mentees is not coming at the expense of mentor profit.

These benefits from mentorship are obtained even though we find muted results from formal business training. Our results relating to formal business classes are consistent with the large and growing literature studying their effects. Formal business training generates no statistically significant change in profit relative to the control, which is in line with studies summarized in McKenzie and Woodruff (2014) and Blattman and Ralston (2015). However, we do find short run changes in business practices.[4] The lack of profit increase

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[2] Our mentor selection procedure is based on age and experience thresholds, as well as a profit cutoff. This procedure is detailed in Section 3.1.

[3] Interventions that do find these changes either concurrently increase demand (Atkin, Khandelwal and Osman, 2017; Hardy and McCasland, 2016) or focus on larger firms (Bloom et al., 2013).

[4] We interpret this as evidence against the possibility that these classes were of categorically lower quality than classes

coupled with a short run change in business practices is consistent with previous work on microenterprise training, including Bruhn and Zia (2013) and Giné and Mansuri (2014). These results demonstrate that the mentorship treatment impact is not simply driven by an initial absolute lack of knowledge relative to contexts previously studied, but that mentorship is effective even in circumstances where classroom study has the same effects as found previously.[5]

We finally turn to understanding the dynamics of the treatment effect. The vanishing effect seems at odds with a basic human capital accumulation story in which mentors augment or accelerate the process by which mentees acquires managerial capital, following Becker (1964). Instead, it appears that mentors provide a flow benefit to mentees in the short run which does not lead to permanent changes in firm productivity. These stylized facts more closely resemble the model of Garicano and Rossi-Hansberg (2006), where agents vary in their knowledge of how to solve specific problems. The set of problems that arise in the operation of the entrepreneur's business is constantly evolving, such as the loss of suppliers, the need to change locations, or changing demand for products. In Garicano and Rossi-Hansberg (2006), lower level workers within an organization can seek help from their managers when they lack the knowledge to solve their problem. In a microenterprise, no such manager exists, but when the entrepreneur has access to a mentor, she is able to draw on her mentor's knowledge. Hence, mentorship improves profits by helping to solve the constantly evolving set of problems that entrepreneurs face, which is a flow information benefit. When the mentorship relationship dissolves, the entrepreneur is no better off than she was before she had a mentor.

To provide evidence for this interpretation, we utilize data on the topics discussed in mentee-mentor meetings. We show that the issues faced by small businesses vary substantially and there is little autocorrelation in problems faced over time. When we ask business owners to define the three largest difficulties facing their business, 93 percent of the control group provides a different list just seven months after initially responding at baseline, while 79 percent add at least one new major constraint. The average control

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studied in the previous literature on formal business training.

[5]It is worth emphasizing that our goal is not to make the mentorship and class treatments as similar as possible. Instead, we want to show that (1) interacting with a successful business has a positive effect and (2) that a standard training intervention generates similar results to the literature within the same population.

owner no longer mentions 59 percent of the problems mentioned at baseline 7 months earlier. Consistent with these results, we find no relationship between critical issues facing mentee businesses at baseline and the topics discussed with mentors. Moreover, the likelihood of a mentor-mentee pair talking about the same topics two waves in a row is only 10 percent. These results provide evidence that the problems microenterprises face change constantly. While the topics discussed with mentors have an immediate effect of limiting the impact of current shocks faced by the business, they provide less help with future and as of yet unencountered problems.

We further exploit variation in outcomes between mentees who continue to meet with mentors and those who do not.[6] Despite nearly identical average profits at baseline, those who continue to meet with their mentors twelve months after the official end of the treatment period are on average 55 percent more profitable than those who are no longer meeting. This result could be explained by survivorship bias if matches with future benefits are the only ones that continue. We provide evidence that works against this explanation by showing that continued meeting cannot be predicted by previous changes in profit. Nearly 70 percent of matches were ended by the mentor as opposed to the mentee, which limits the effect of mentee selection. Our interpretation is that mentees benefit from mentorship and matches typically end only when mentors lose interest. This again suggests that our positive treatment effects are driven by the flow of information from mentor to mentee, but that the benefit of the information is short-lived.

1.1 Related Literature

The literature on increasing managerial capital in microenterprises and small firms has overwhelmingly focused on in-class training. McKenzie and Woodruff (2014) provide an excellent and comprehensive review of previous studies. The overriding theme of this research is that business practices do change, but translate into little impact on revenue and profit (for example, Bruhn and Zia, 2013; Giné and Mansuri, 2014), despite billions of dollars spent by governments and international organizations. Our in-class training intervention is consistent with these results. The mentorship intervention, however, overcomes

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[6]Meeting of mentors and mentees after the treatment period was not incentivized. We discuss the persistence of mentor-mentee relationships in Section 5.2.

two issues in this literature. First, in-class training requires diagnosing what skills the entrepreneurs are missing, and then designing a curriculum to effectively address those deficiencies. This has proved difficult, in part because of the large number of different constraints facing microenterprises (Bruhn, Karlan and Schoar, 2017; Karlan, Knight and Udry, 2014). Our intervention allows each mentor-mentee pair to diagnose and discuss information or skills they believe to be most useful. Second, diagnosing the key constraints or problems facing businesses is expensive. Classroom training usually can cost over 100 U.S. dollars per student (Blattman and Ralston, 2015), while more personalized consultants can cost as much as 11,000 U.S. dollars, as in the case of Bruhn, Karlan and Schoar (2017). Our mentors were paid 9.83 U.S. dollars for one month of meetings, yet despite this, 45 percent were still meeting 17 months after the official treatment period when they were being paid nothing. Our training class cost approximately 40 U.S. dollars per student.

Our solution to these issues – leveraging the skill of other individuals facing the same local economic conditions – is closely related to work on the transmission of knowledge through networks. Foster and Rosenzweig (1995), Munshi (2004), Bandiera and Rasul (2006), and Conley and Udry (2010) all document social learning in various contexts, while BenYishay and Mobarak (2015) and Beaman et al. (2015) experimentally vary the entry point of information and track its impact through existing networks. While these papers all show the importance of existing networks for information and technology flow, much less is known about the impact of (potentially) learning from an exogenously selected partner. More closely related to our exercise are Atkin, Khandelwal and Osman (2017), who study this in the context of supplier-customer relationships by randomly allocating new foreign rug orders to small Egyptian rug makers, and Cai and Szeidl (2016), who study the creation of randomly formed business groups in China. Interestingly, despite the fact that they focus on much larger firms, we find many of the same channels. However, they find more persistent effects, highlighting potentially important differences among firms of different scale.

Last, our results highlight a particular micro-foundation for distortions to managerial capital accumulation, albeit in a relatively specialized setting. Bhattacharya, Guner and

Ventura (2013) and Da-Rocha, Mendes Tavares and Restuccia (2014) show that frictions limiting managerial capital accumulation can have an important aggregate effect in the context of policy distortion models developed in Restuccia and Rogerson (2008) and Hsieh and Klenow (2009). Our results suggest a link between these distortion models and recent work by Lucas (2009), Lucas and Moll (2014), and Perla and Tonetti (2014) in which growth is generated through spillovers from other firms.

## 2 Microenterprise Characteristics in Dandora, Kenya

Dandora is a dense, urban slum to the northeast of Nairobi. It is approximately four square kilometers, and as of the 2009 census, contained 151,046 residents. To assess the business characteristics in the area, we conducted a street-level survey of 3,290 randomly selected business.[7] Table 1 provides summary statistics for business. Column three also includes the same information for "young" firms with owners under 40 years old and less than 5 years of experience, as we eventually draw our sample from this group. These businesses make up 43 percent of all businesses surveyed. The rest of the businesses — those with owners older than 40 or more than 5 years of experience — we classify as "experienced."

The average business in our survey has profit of 16,899 Kenyan shillings (Ksh), or 167 U.S. dollars, in the previous month. This is approximately 72 percent above GDP per capita in Kenya. However, while the average young owner earns 14,266 Ksh, the average experienced owner earns nearly 42 percent more profit per month or 20,168 Ksh. Figure 1 plots the distribution of the natural logarithm of profit for young and experienced enterprises.

This result is driven by selection. However, despite the substantial difference in profit, there is not much difference in observable formal business practices. They are equally likely to offer credit to customers, have a bank account, have taken a loan at some point in the past, or engage in formal accounting or advertising. Moreover, they are roughly equally educated.

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[7]Our sampling procedure was intended to reduce convenience sampling, and worked as follows. We generated 200 points randomly throughout Dandora, and then gave each enumerator a list of randomly selected numbers. Starting from a randomly selected point, they were instructed to count businesses until they reached a number on their list, and survey the business owner of that establishment.

Figure 1: Log profit distribution for young and experienced enterprises
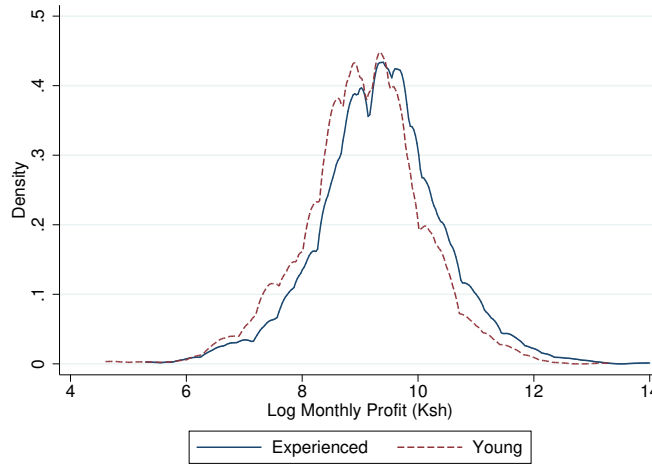
We focus on female microenterprise owners, as they make up 71 percent of inexperienced owners. As Figure 2a shows, they are less profitable than their male counterparts at every business experience level. Interestingly, this percentage difference in profits is roughly constant over the first fifteen years of the firms' operating lives (Figure 2b).

Figure 2: Gender differences over the lifecycle

(a) Average lifecycle profit

(b) Average lifecycle profit growth
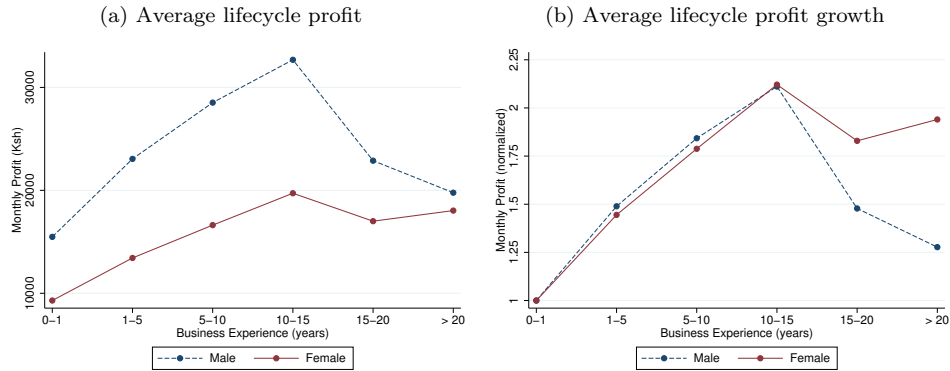
3   Experimental Design

We use the baseline survey discussed in Section 2 to construct our experimental sample. We restricted our sample to business owners who are under 40 years old and have been running a business for less than 5 years. This includes 1,094 business owners, 787 of whom

were operated by females.[8] Out of these 787 female entrepreneurs, we contacted 723 to participate in the study after dropping some with a high fraction of missing baseline data or extreme outliers in the baseline. Of these, 538 (68%) accepted our invitation to participate in the program without knowing which treatment group they would be assigned to. The time commitment was emphasized to participants, including attendance at an in-person orientation, multiple meetings where they would receive business advice, and the follow-up surveys over the telephone. Of the 538 individuals, 372 attended orientation (69% of 538, or 51% of the original 723). Randomization took place among these 538 individuals, and no one was given any indication of their assigned group until arrival at orientation. All groups received a cash payment of 4,800 Ksh (48 U.S. dollars) to encourage participation, which is equal to approximately two weeks of average profit. The classroom treatment also received one month of business classes. The mentor group received the cash payment and assignment to a mentor drawn from local successful business owners. Of the original 372 individuals at orientation, 369 business owners answered at least one post-treatment survey.[9]

The business classes were conducted by faculty from Strathmore University, a leading university in Kenya that is located in Nairobi. The classes have been used as part of a small and medium size business training program by the Strathmore University School of Management and Commerce. The instructors are all experienced in teaching this course, which they believe to be the best curriculum available to teach microenterprise owners. The treatment consisted of four two hour classes that covered marketing, accounting, cost control and business planning. These topics are similar to programs used in other studies.[10] Classes were offered six times over the course of the week at a local hall in Dandora to accommodate individual schedules.

Individuals assigned to the mentor treatment were matched with a mentor drawn

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[8]To the best of our knowledge, all of these factors have been used to select population in other studies of classroom training, except for age (and experience, which is naturally correlated). McKenzie and Woodruff (2014) provide a detailed breakdown of selection procedures used in other microenterprise training papers. Of the RCTs surveyed in that paper, the average age is 38 years old, while ours is 29. This is roughly the tenth percentile of papers they survey, so indeed our average age is toward the low end. However, given that we find very similar results to the majority of the class training literature, it is difficult to imagine that our results are driven by age selection.

[9]Table 20 in Appendix D provides a detailed breakdown of the business types included in this sample and a comparison to the overall composition of the baseline businesses surveyed.

[10]We find similar results to previous formal training research using other training programs, suggesting that there is nothing specifically different about our class design that generates our results.

from a set of successful local female business owners (mentor selection is detailed in the next subsection). Once the pool of mentors was chosen, mentees were matched based on narrowly defined business sectors. For example, we match perishable food sellers with perishable food sellers, tailors with other tailors, and so on. Conditional on matching business sectors, mentors were randomly assigned. Mentees were required to meet with the mentor each week at the mentor's business, which was designed to minimize the cost to the mentors. The meetings, however, were relatively unstructured. We put no constraint on minimum meeting time nor the topics that must be discussed. To facilitate discussion, participants were given optional prompts, including "What were some of the challenges the mentee faced this week?" and "What was a time the mentor faced a similar challenge, and how did she respond to it?"

The treatment was completed at the end of November 2014. To understand the dynamics of the response across different treatment arms, we conducted six follow up surveys in the middle of December 2014 (preceding the Christmas holiday) and then in the last week of January, February, March, June, and November of 2015, and a much shorter followup in April 2016. The last two surveys contained a longer set of followups with more detailed business practice questions. Throughout the rest of the paper these surveys will be numbered by months since treatment, so that the surveys will be numbered by the months since baseline as $t \in \{1, 2, 3, 4, 7, 12, 17\}$.

3.1 Mentor Selection

The pool of mentors was selected from our baseline survey. We first constrained our sample to female business owners who were over 40 years old and had been operating the same business for at least 5 years. This left 366 individuals. We then sorted based on profit after controlling for sector-specific differences, and recruited mentors until we had enough to link each mentee to a mentor in the same tightly defined business sector. Mentors were offered 1,000 shillings (9.83 U.S. dollars) to participate, which is equivalent to approximately two days of profit for the average firm in this group. We also told both mentors and mentees that meetings would take place at the mentor's business to minimize inconvenience to the mentor and maintain some similarity with the travel requirement of

the class treatment. These incentives were sufficient to generate high take up, as 95 percent accepted our invitation to take part in the program.[11]

3.2 Sample Size and Balance across Survey Waves

Of the 372 individuals who attended orientation, 369 (99%) answered at least one follow-up. The response rates by wave were 352 (95% of 372), 318 (85%), 319 (86%), 323 (87%), and 325 (87%). In total, 4 individuals completed exactly one follow-up, 6 completed two, 21 completed three, 41 completed four, and 108 completed five, and 194 completed all six. In Appendix A we provide survey round-specific balance tests. There is no evidence that attrition generates any observable differences across the groups. We further provide the correlation coefficients of baseline observables with number of surveys answered in Table 17 of Appendix A. A few observables are correlated with answering surveys. However, the differences are small and we find little difference in estimation results with or without controlling for baseline factors.[12]

3.3 Take-up of Treatments

Attendance at the business class was recorded, and the average person in the classroom treatment attended 3.1 out of 4 classes. One person attended no classes, 11 percent attended one of four classes, 17 percent attended two, 32 percent attended three, and 40 percent attended all four. This is broadly in line with attendance in other studies (McKenzie and Woodruff, 2014). The mentorship treatment was used by all individuals at least once during the intended treatment period. In the last week of the official treatment, 85 percent had met with their mentor within the past week.

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[11]See Appendix C for more details. Note that this procedure is used to construct the regression discontinuity control group as well, so they were also offered the same cash incentives to facilitate comparibility.

[12]To give some examples, manufacturing business owners answer 6.8 surveys on average, compared to 6.0 for the rest. Restaurants answer 5.7 surveys, compared to 6.1 surveys for non-restaurants. The other two observables correlated with answering are firm age and owner age, which are naturally highly correlated. A business owner in the bottom 25 percent of the age distribution answers 6.1 surveys on average, compared to 6.3 in the top 25 percent of the age distribution.

4 Empirical Results: Profitability

We begin by considering the impact of our treatments on business profitability and scale in Section 4.1. We find that mentorship increased average profit relative to control, while in-class training had no statistically significant effect. In Section 4.2 we use a regression discontinuity design to show that there is no statistically significant change in profit for mentors. Taken together, we interpret this to mean that entrepreneurs face a constantly evolving set of challenges in the operation of their business that their mentor helps them to solve. This is a flow benefit that mainly accrues to the firm in the short run, and the withdrawal of the mentor leads to its loss.

4.1 Treatment Impact

We begin by looking at weekly profit for each treatment group. Figure 3 plots the time series of average weekly profit by treatment arm.

Figure 3: Profit time series
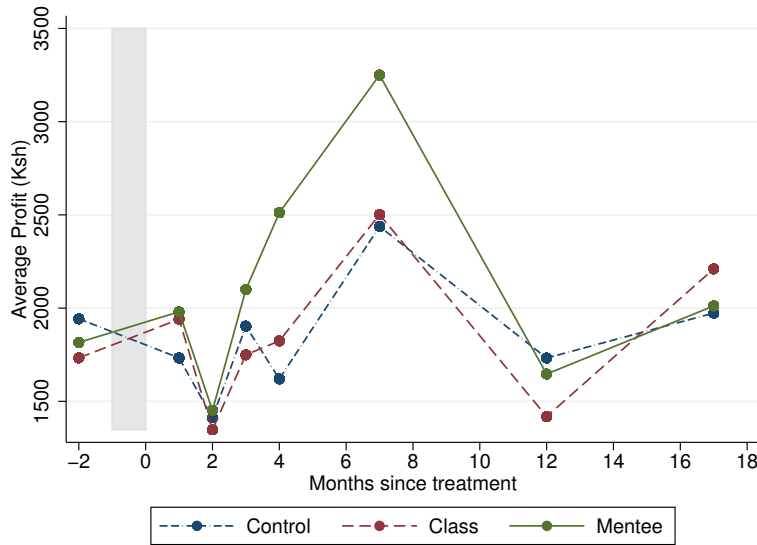
The classroom treatment mimics the control group closely throughout the study period.[13] The mentor treatment, however, sees a substantial growth in profit relative to both

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[13]There is an obvious decline in profit from December to January ($t = 1$ to $t = 2$) across all groups. This is the seasonal effect of a slow down in sales after December holidays, which we confirmed with numerous business owners in the study. Note, however, that the positive effect of mentorship is not driven by seasonality, as one can see from the lack of effect at $t = 17$.

the control and class that first appears in month four and lasts through month seven. This effect fades out by the twelve month follow-up as mentor-mentee matches dissolve, which is discussed in detail in later sections. To make statistical inferences about these effects, we employ a series of regressions. First, following McKenzie (2012), we pool the data and run the ANCOVA regression to measure the average treatment effect:

$$y_{it} = \alpha + M_i\beta + C_i\gamma + y_{i0}\delta + X_i\eta + \theta_t + \epsilon_{it}. \tag{4.1}$$

Here, y_{it} is the outcome for individual i at time $t \in \{1, 2, 3, 4, 7, 12, 17\}$ months since the treatment, while y_{i0} is the baseline outcome value. $M_i = 1$ if i is in the mentor group, and $C_i = 1$ if i is in the classroom treatment. X_i is a vector of baseline controls including secondary education, log age, and business sector fixed effects, and an indicator for whether the firm employs any workers, while θ_t is a time fixed effect. All pooled regressions have standard errors clustered at the individual level. To understand the dynamics of the response, we supplement the pooled regression with wave-by-wave regressions:

$$y_{it} = \alpha_t + M_i\beta_t + C_i\gamma_t + y_{i0}\delta_t + X_i\eta_t + \varepsilon_{it} \quad \text{for} \quad t \geq 1 \tag{4.2}$$

Table 3 reports the impact on business profit. We find statistically significant increases in profit from mentorship in the pooled regression, but no significant effects from classroom training. The time series of profit across the three groups shows that the average results are driven by a large increase that begins 4 months post-treatment. Overall, the mentorship program generates a large average increase in profit relative to the control, while the in-class training program delivers almost no change in profitability. However, the effect of mentorship fades over time, which we discuss in detail in Section 5.

Next, we look within the mentorship treatment to see if the mentor's baseline characteristics affect the magnitude of effects. If mentors utilize their own skill or ability to increase mentee profit, better mentors should generate a larger treatment effect. We test this with the following estimating equation:

$$y_{it} = \alpha + M_{1it}\beta_1 + M_{2it}\beta_2 + M_{3it}\beta_1 + C_{it}\gamma + X_i\eta + \theta_t + \varepsilon_{it}. \tag{4.3}$$

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where $M_{1it} = 1$ if i has a mentor from the bottom 25 percent of the baseline mentor profit distribution, $M_{2it} = 1$ if i's mentor is in the 25th to 75th percentile, and $M_{3it} = 1$ if i's mentor is in the top 25 percent. The results are in Table 4. These tests do not have sufficient power to statistically distinguish between these groups. However, the point estimates are ordered: the most profitable mentors had the biggest treatment effects on average, though, again, these differences are not statistically significant.[14]

4.2 Impact on Mentors

The mentorship treatment generates a significant increasee in profit for mentees, and we next wish to determine if there is any impact on the mentor. One possibility is that the mentor is an experienced agent who can impart knowledge on a less experienced business without receiving any return benefit from the interaction. This assumption is made in a large body of recent theoretical and quantitative work (e.g. Jovanovic and Rob, 1989; Lucas, 2009; Lucas and Moll, 2014; Buera and Oberfield, 2015). Alternatively, the mentor-mentee relationship may be better described as a collaboration or business group, where both sides gain from interacting with the other (e.g. Cai and Szeidl, 2016). To distinguish between these two possibilities, we explot our procedure for selecting mentors and employ a regression discontinuity design.

As discussed above, mentors were paid 1000 Ksh for their participation. Therefore, when selecting mentors at the beginning of the program, we also contacted the 150 female business owners closest to the cutoff to participate as well. Ninety five agreed to participate, and they were also paid 1000 Ksh at the start of the program, so that we can compare the two groups without the effect of the cash transfer as a potential confounding factor. Four months after the treatment, we surveyed all mentors, along with these 95 other non-mentor business owners.[15] We then assess the impact of being chosen as a mentor on profit. First, Figure 4 plots profit along with a fitted quadratic and its 95 percent confidence interval. Figure 4a uses the entire sample, while Figure 4b uses the Imbens and Kalyanaraman (2012) procedure to choose the optimal bandwidth. Both use

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[14]In the appendix, Table 22 splits up the mentor effect by the mentor's education and business experience. Those mentor characteristics have no effect on realized treatment effects in mentees.

[15]Note that this happened to be a wave with large treatment effects for the mentees. This is fortunate, since a failure to find statistical effects on mentors in a wave where we also failed to find them in mentees would be harder to interpret.

15 bins on either side of the cutoff.

Figure 4: Profit for mentors and non-mentors

(a) Full sample   (b) IK Optimal Bandwidth
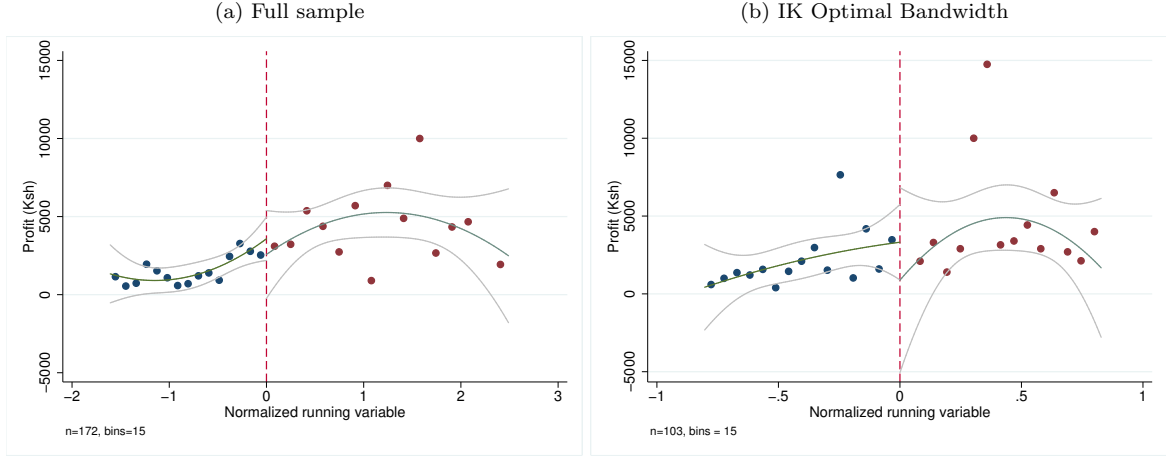
Figure 4 suggests no statistically discernible discontinuity around the cutoff, which we next assess formally. In particular, letting $\bar{\varepsilon}$ be the cut-off value for mentors derived from regression (C.1), we run the regression

$$\pi_i = \alpha + \tau D_i + f(N_i) + \nu_i \tag{4.4}$$

where π_i is profit, $D_i = 1$ if individual i was chosen as a mentor ($\widehat{\varepsilon}_i \geq \bar{\varepsilon}$ in regression C.1), $f(N_i)$ is a flexible function of the normalized running variable $N_i = (\widehat{\varepsilon}_i - \bar{\varepsilon})/\sigma_\varepsilon$, and ν_i is the error term. The parameter τ captures the causal impact of being chosen as a mentor. We use local linear regressions to estimate the treatment effects on profit and inventory, along with business practices of record keeping and marketing. The results are in Table 5, and we find that being a mentor has no statistically significant effect on profits. Moreover, there is no change in marketing or record-keeping practices. There is some evidence that inventory spending decreases, but it cannot be statistically distinguished from zero. Overall, we find little evidence that mentorship changes either business scale or business practices for the mentors. In Appendix C, we use the MSE-optimal bandwidth procedure proposed in Calonico, Cattaneo and Titiunik (2014), along with varying the order of the polynomial for $f(\cdot)$. We find similar results.

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5 Understanding Treatment: Channels and Dynamics

Section 4 demonstrates two key results. First, mentorship generates a substantial increase in profit while the class does not. Second, the treatment effect has non-trivial dynamics. We find that the changes among the mentorship group primarily relate to market-specific information, not the more general business skills covered in the class. We first show that these results are driven by reductions in costs rather than increases in revenue. We then provide evidence on the mechanism underlying this reduction in cost by showing significant changes in on supplier choice and inventory. We then examine the dynamics of the treatment effects by considering the dissolution of matches over time.

5.1 Effects on Revenue and Costs

The increase in profit in the mentorship treatment could arise from increased revenue, decreased costs, or a combination of both. We first show that neither total revenue nor sale prices of output changed significantly, which suggests that increased profits must result from reduced costs. We verify this by showing that input costs declined significantly for firms in the mentorship treatment. The subsections that follow provide evidence on the mechanism by which costs fell by showing that mentees exhibited significantly different inventory and supplier behavior.

Columns (1) and (2) of Table 6 show the treatment effects on revenue in the classroom and mentorship groups. Both with and without controls, neither group experiences a significant change in revenue.[16] The fact that we find no evidence of a change in revenue mitigates the concern that treatment effects on mentees spilled over to the control group through competition. If mentorship caused mentees to increase sales and gain market share, that would negatively impact competing firms in the control group and bias our estimated treatment effects upward. Given that there is no evidence of such an effect, we are less concerned about this possibility.

Even though there is no evidence that revenue changed, it is possible that prices rose while quantities declined, perhaps due to increases in quality. To evaluate that possibility,

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[16]A direct question about total revenue was only posed in waves 3 though 7, and questions on the input and output prices of the firms' most profitable products were only posed in waves 5 through 7, resulting in fewer available observations, as reported in Table 6.

we would like to know if firms raised the price of their output. However, this is difficult to measure in multi-product firms, since there is no single output price to be measured. Instead, beginning in wave 5 we ask firms to identify the product which accounts for the most of their sales and the output price of that product. We then ask if this is their most profitable product, and 92.16 percent confirm that it is. We then proceed by using the price of their most profitable product for the analysis. We can then see how the price of that product changed over time. This result is given in columns (3) and (4) of Table 6. Again, we see no significant effect in either group. Taken together with no evidence of a change in revenue, we interpret this to mean that the most important mechanisms for mentees did not operate through the demand side of the firm's problem.[17]

If increased profits for mentees did not arise from increased revenue, it must have come from reduced costs. We verify this by looking at the supply costs paid by firms. The dependent variable in columns (5) and (6) of Table 6 is the supply cost paid for the firm's most profitable product, defined as in columns (3) and (4). Now we find that the supply cost in the mentee group falls significantly, while there is no significant change in the class group. This is consistent with what was suggested by the previous results: profits increased in the mentee group from reduced costs, not from increased revenue.

In the next subsections we provide evidence on the mechanisms that generate this reduction in costs. We show evidence on supplier churning as a mechanism for reduced costs. We also show that mentors seem to be helpful in giving mentees guidance on how to solve the constantly-evolving set of problems that arise in the course of running their businesses.

5.2   Supplier Churning

To understand why costs declined, we consider the effect of each treatment on product sourcing by studying the propensity of firms to switch to new suppliers. The treatment effects are presented in Table 7 where the outcome variable is an indicator for whether or not the firm switched suppliers between the first and seventh survey wave. First, we note

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[17]As these results suggest, we find no evidence that mentorship relaxes firms' credit constraints, which would arise if mentors helped mentees secure financing. As shown in Table D of the appendix, we also find no evidence that mentees made more large investments in their firms than did members of the control group. Only 2 loans were made from mentors to mentees, both for about 50 USD.

that supplier churning is very high in this environment. In the control group, 62 percent of firms switched suppliers, which suggests that the ability to acquire information about suppliers is salient for these firms. Second, we find a strong effect on the likelihood of switching suppliers in response to the mentorship treatment. Mentees are 19 percentage points ($p = 0.00$) more likely to switch suppliers than the control group. The class treatment, on the other hand, generates both a statistically and economically insignificant effect of -0.00 ($p = 0.98$).

The results suggest that mentorship has the effect of connecting mentees with better suppliers, since we find a greater propensity to switch and lower supply costs for mentees.

All of these results confirm the importance of suppliers and cost as a critical margin changed by interacting with high quality business owners. In Appendix D, we include other measures of business scale (employment, wages, hours of operation, etc.) and show that these do not change. The last part of this paper therefore focuses then on why the dynamics of the treatment effects. We utilize the separation of mentee-mentor pairs, along with the substantial changes in suppliers that occur among the control group, to show that the profit increase is driven by the flow of information, not by permanent changes in practices.

5.3 Dynamics of Mentorship Treatment

We have shown that having a mentor increases profit, and primarily works through realizing lower costs. However, as previously noted, the treatment effect from mentorship fades out over time. In this section, we use details of the discussion topics from mentee-mentor pairs to probe this result in more detail. We find that businesses face an ever-evolving set of problems, implying that information provided to solve today's problem may not be the same information required to solve tomorrow's problem. Moreover, within the mentee group, we also show that the topics of discussion between mentor and mentee have little autocorrelation. Taken together, we interpret this to mean that entrepreneurs face a constantly evolving set of challenges in the operation of their business that their mentor helps them to solve. This result echoes recent work by Bruhn, Karlan and Schoar (2017), who find substantial heterogeneity across Mexican businesses in the specific con-

straints limiting growth. In contrast, however, our results show that there is substantial heterogeneity in challenges facing the *same* business over time.

To further emphasize this result, we utilize the heterogenous length of time that mentees and mentors met. We find little persistence in profit benefits (relative to control) once a match ends, but high profits for those that continue to meet. Of course, the choice to continue meeting or not is endogenous. However, we show that the results are not driven by low benefits to the mentee, which eliminates one critical form of selection.[18] These results are consistent with our interpretation: mentees face a vast array of potential problems that are constantly evolving, which their mentor can help them solve when they are meeting. However, when they have stopped meeting, the mentee faces new problems that the mentor did not previously teach her how to solve, so that she is no better off now in solving the new problem than she would have been without having ever worked with her mentor.

5.3.1   Business Challenges and Mentor Advice

We begin by using the control group to understand the underlying churn in business problems in Dandora. At both baseline and $t = 7$, we asked business owners to identify (up to) the three most critical challenges facing their business, from a list of 11 possibilities. The results imply an incredible amount of turnover in critical problems in just seven months. Ninety two percent of control business owners drop at least one problem from $t = 0$ to $t = 7$ while 79 percent introduce a new problem at $t = 7$. The average control business owner drops 59 percent of baseline problems by $t = 7$. Moreover, of those topics mentioned at $t = 7$, 53 percent of them were not mentioned at baseline. These results suggest the underlying problems facing Dandoran business owners vary substantially, even over a short time horizon.

This has important implications for the mentee-mentor relationship. If topics covered during meetings are focused on the current issues facing the business, there is a high probability those issues will no longer be critical to profitability once meetings end. Figure 5a plots the fraction of mentee businesses that discuss each of a variety of topics over the

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[18]Nearly 70 percent of matches are ended by the mentor. There are no observables correlated with match dissolution, but we naturally cannot rule out dissolution based on unobservables.

course of their relationship. The different color bars indicate businesses who mention the three most common key problems at baseline – lack of funds, the inability to find inventory or supplies, and customers that do not pay. We see that the topics discussed are remarkably consistent across baseline problems. One possibility is that mentors give advice that is simply orthogonal to contemporaneous business problems. This, however, is inconsistent with fact that profit increases. Since business problems vary over time, mentors are discussing many different topics with mentees. Indeed, Figure 5b shows that about half of those still meeting at $t = 12$ discuss at least half of the topics listed, while 20 percent discuss all ten topics.

Finally, Figure 5c plots the persistence of topics across meetings. This figure shows the conditional probability of discussing each topic given that it was discussed at the previous meeting. Since the panel is unbalanced, we do this in two different ways. First, we measure persistence based on two consecutive observations of the same mentee, no matter how many months are between them, which is labeled "consecutive observations." Second, we restrict the sample to only those firms that we observe in consecutive waves, and those probabilities are labeled "consecutive waves." These results are again consistent with the low autocorrelation of challenges faced by the business. Attracting customers and keeping records are the most commonly discussed topics overall, but all other topics seem to represent the emerging challenges that the mentees are facing on an ongoing basis.

Taken together, these results suggest that Dandoran business owners face a large number of potential problems. Moreover, mentorship responds to the economic conditions by covering a large number of issues as they arise in mentee businesses. We next turn to the dissolution of mentee-mentor matches to assess how the underlying economic environment affects the dynamics of treatment effects found in Section 4.

5.3.2   Evidence from Match Persistence

If the critical challenges facing microenterprises are constantly varying, the information provided by the mentor may not be useful once new problems are faced in the absence of a mentor. One implication of this is that benefits to mentees should end quickly after matches end. Figure 6 plots the fraction of mentees still meeting with their mentor over

Figure 5: Mentee-Mentor Discussion Topics

(a) Topics Discussed at Any Point

(b) CDF of No. of Topics (if meeting at $t = 12$)

(c) Persistence of Discussion Topics
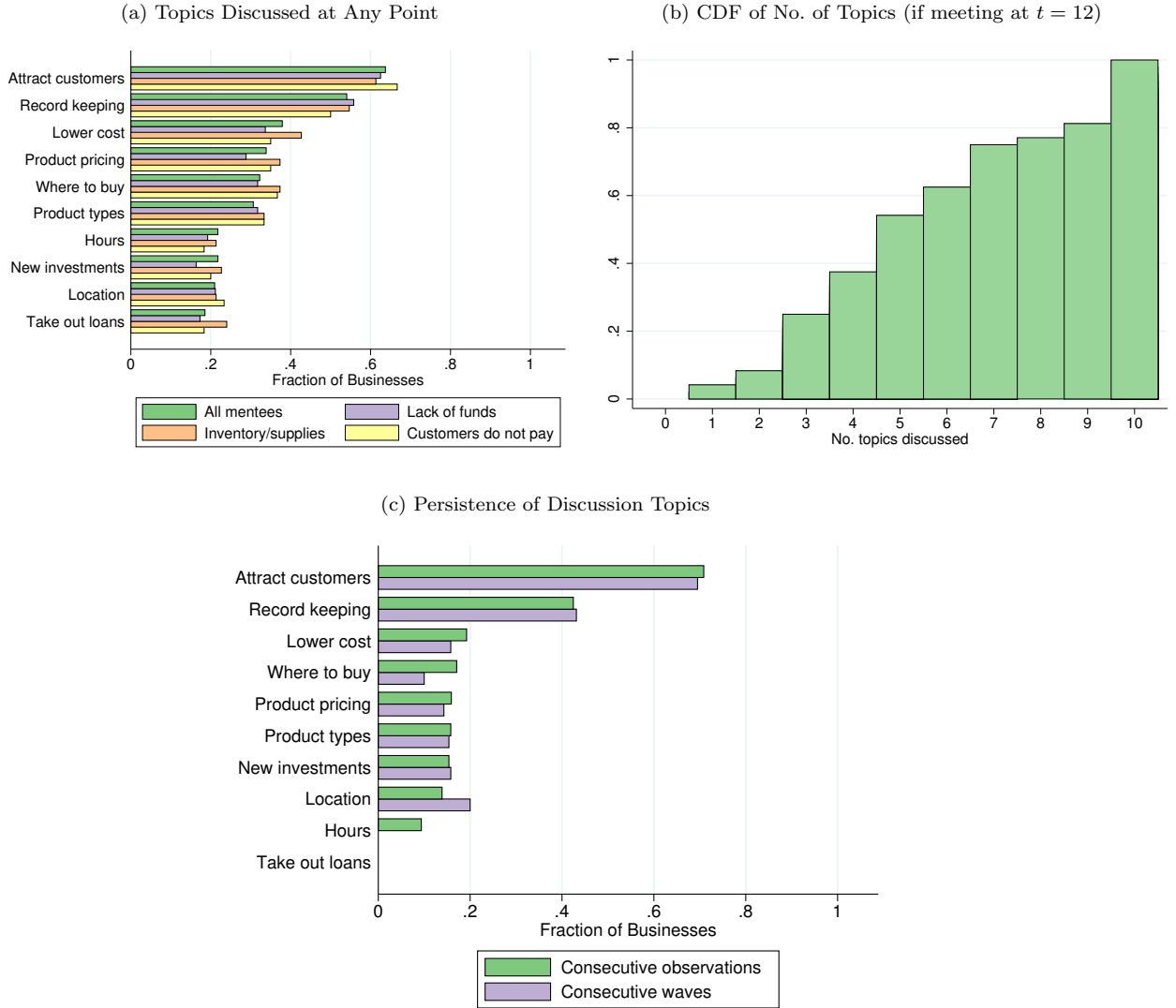
the course of the study. All mentees met with their mentor in the official treatment month. This fraction declines over time with 45 percent still meeting after twelve months. No incentives were provided to continue the relationship beyond the treatment period.

Twelve months after the treatment, average profit for those still meeting with their mentor is 2071 Ksh compared to 1339 Ksh among those not meeting - a difference of 55 percent (and statistically significant at 0.05). This result holds despite the fact that the

Figure 6: Fraction of mentees still meeting with mentor
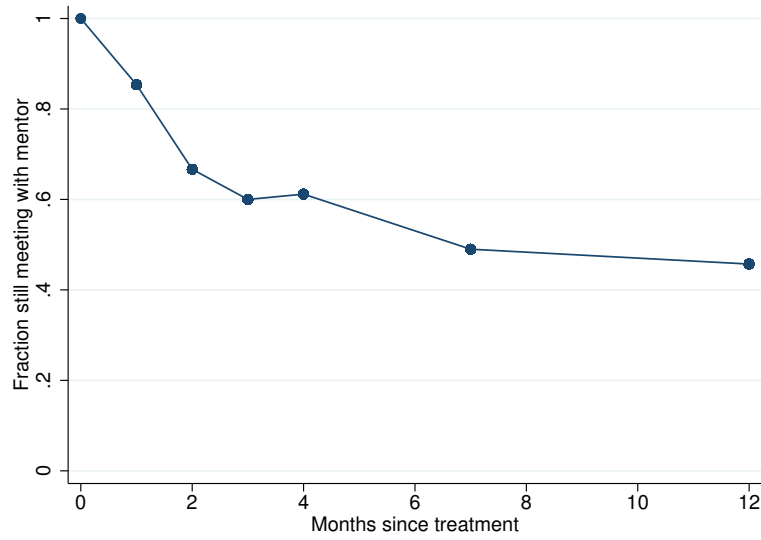
two groups on average have nearly identical baseline profit levels. Figure 7 shows the time series of average profit for those meeting and not meeting in each wave. Though we mostly lack power to find significant differences between those meeting and not, the point estimates are of similar magnitudes across the waves that exhibited the largest effects in our main results.

Figure 7: Average Profit for Mentees
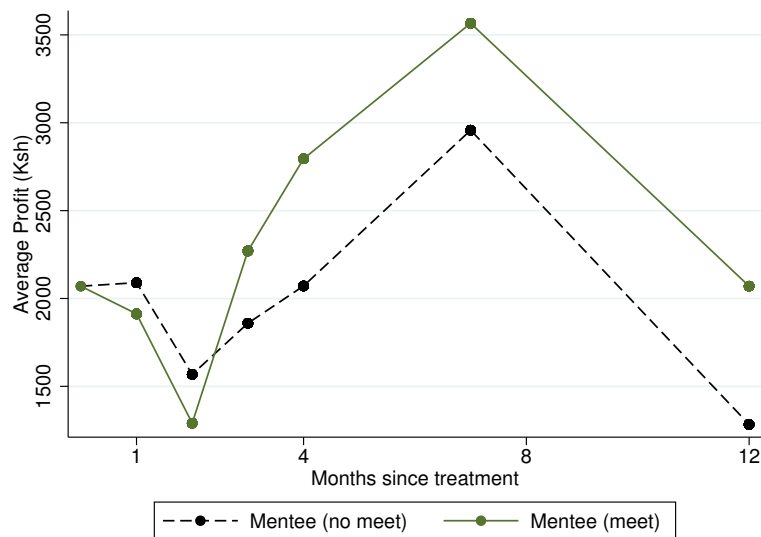
Because we did not generate variation in the length of the mentor-mentee relationship with an experiment, we cannot apply a causal interpretation to these findings. We can rule out some of the most obvious confounding factors, such as a lack of interest from mentees, or time series correlation with profit realizations. Nearly 70 percent of mentees claimed that mentors ended the relationship, suggesting strategic exit from the relationship by the mentee is not the main issue. In Appendix D.10 we provide additional evidence that continued meetings are not predicted by changes in profits.

However, we are unable to rule out that selection by mentors both drives match dissolution and is correlated with mentor profit. Future studies of mentorship should seek to introduce experimental variation in the duration of mentor-mentee pairings to see if these profit dynamics are caused by differences in the duration of matches.

6 Other Explanations and Discussion

6.1 Skills Taught in Class

We turn back to studying the business practices covered in the training classes. If the classes simply transferred no skill or knowledge, then it is certainly possible that standard training would have been beneficial had it been successful in changing practices. We refute that argument here, and show that our results are consistent with previous work on training classes. In particular, we see changes in underlying business practices covered in class, but they do not translate into increases in profits.

In every survey, we asked about accounting and advertising practices, and Table 8 provides the time series of estimates using regressions (4.1) and (4.2). Marketing practices do not change relative to the control for either treatment. Accounting practices do change across treatments, and in fact we find a significantly larger impact among the class than the mentees. On average, 74 percent of the control does some sort of record keeping, compared 86 percent of those who receive in-class training (19 percent increase) and 77 percent of the mentees (7 percent increase). However, this effect is only present in the first four months following the treatment for the class treatment. This is consistent with short run changes in behavior found in other studies as well (e.g. Karlan, Knight and

Udry, 2014), and implies that the in-class training does in fact change behavior without changing business outcomes.

For further evidence that classes change business practices, in our $t = 7$ and $t = 12$ surveys we asked a much longer battery of business practice questions. The questions are primarily drawn from the survey instrument first used in de Mel, McKenzie and Woodruff (2014) and McKenzie and Woodruff (2016) show that these questions are positively correlated with profit in a number of countries. Table 9 provides four aggregate measures of business practices. The *Aggregate Score* variable is the sum of *Marketing score*, *Stock score*, and *Record keeping score*. Each is presented as a standardized z-score to facilitate comparability, but we present the raw numbers in Appendix D for disaggregated categories. We combine the two waves of data and run the regression

$$y_{it} = \alpha + M_i\beta + C_i\gamma + X_i\eta + \theta_t + \epsilon_{it} \qquad \text{for } t = 7, 12$$

We do find changes associated with the stock score, which includes the likelihood of haggling with suppliers, whether or not the business owner compare suppliers, and how often she runs out of stock. This is consistent with the importance of suppliers and cost for generating changes in profit. Interestingly, however, we find similar effects between the class and mentorship group. The difference is that this information does not translate into profitability among the training class. Taken together, the results highlight the fact that the class was indeed successful in generating changes in behavior, but that they did not translate into increased profit. Moreover, they demonstrate that the mentorship treatment impact is not simply driven by an initial absolute lack of knowledge relative to places previously studied, but that mentorship is effective even in circumstances where classroom study has the same effects as found previously.

6.2 Other Possible Channels

We compute the treatment impact on a number of other results, and find no effect. These include measures of business scale, borrowing, changing of product mix, and business exit. The results are available in Appendix D. A last and related possibility is that mentors provided monetary help to the mentees. While interesting in its own right, it suggests

constraints other than local information driving the results. Qualitatively, when asked about the most important benefit of having a mentor, only 6 percent mentioned anything related to financial help or transfers. More objectively, only two percent reported receiving any financial help from their mentor at any point between $t = 1$ and $t = 12$. Financial transfers do not seem to be critical in increasing mentee profit.

7 Conclusion

The prevalence of small, unprofitable businesses in developing countries presents a persistent and difficult problem. To investigate whether local expertise and business experience can benefit these entrepreneurs relative to a class, we conduct a randomized controlled trial in which all subjects receive a cash grant, some subjects are enrolled in a business course, some receive an experienced mentor, and some receive neither. Our results show that interacting with a successful, local business owner generates a 20 percent increase in profit, but that the effect fades over time, while the class generates no change in profit. We use the dynamics of the response to show that the result is driven by the dissolution of matches over time, as those that still meet earn higher profit. We argue that the reason for the treatment effect and its amelioration is that the value of information and supplier relationships are short-lived, and access to more experienced mentors who have a better understanding of the local market has substantial advantages. This also implies a rationale for the lack of success of formal training classes (at least in terms of higher profit). Most current training programs are designed to be easily portable and replicable, and therefore do not focus on the local information we have shown to be important.

While we view the results as an important first step, there is more work implied by these results. One important question implied by these results is based on the fact that mentorship varies both the teacher (local instead of outsider) and the method (one-on-one instead of group) of training. While evidence suggests the former is important, we cannot all together rule out the role of the latter. Further refinement of this mentorship program will provide more detailed policy prescriptions. This further experimentation is facilitated by the high cost effectiveness relative to classroom training. Per dollar spent, the mentorship treatment increased profit by 1.63 USD, while the class increased profit by

0.30 USD. This is driven by both the higher benefits and 20 percent lower cost compared to the class treatment. These calculations are discussed further Appendix E.

This paper joins a recent literature that shows specific and individualized interaction with peer groups, consultants, and mentors can be harnessed to improve business outcomes. Still unknown is how the the scale of the business determines which type of interaction is most effective. While Bloom et al. (2013) finds that professional consultants have positive effects on large businesses in India, Karlan, Knight and Udry (2014) find no increase in profit from a similar intervention targeting microenterprises in Ghana. We use mentors from the same community and find positive effects on microenterprises in Kenya. Cai and Szeidl (2016) show that peer groups have a positive effect on medium-scale businesses in China. Further research should focus on how the effectiveness of these different types of arrangements interacts with firm scale.

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Main Tables for Text

Table 1: Baseline Characteristics

	Overall (3290)	Young Firms (1405)
Firm Scale:		
Profit (last month)	15,084	13,431.38
Firm Age	5.6	2.1
Has Employees?	0.21	0.18
Number of Emp (if $n > 0$)	1.8	1.5
Business Practices:		
Offer credit	0.67	0.69
Have bank account	0.36	0.30
Taken loan	0.21	0.15
Practice accounting	0.11	0.12
Advertise	0.10	0.09
Owner:		
Age	34.0	28.9
Female	0.65	0.71
Secondary Education	0.58	0.58

Table notes: Trimmed profit drops the top and bottom 1 percent of answers. 3171 establishments answered about profit.

Table 2: Balancing Test at Baseline

	Control Mean (1)	Class - Control (2)	Mentor - Control (3)
Firm Scale:			
Profit (last month)	10,054	-360.95 (1175.44)	-975.25 (1186.76)
Firm Age	2.39	0.19 (0.23)	-0.05 (0.23)
Has Employees?	0.21	-0.06 (0.05)	-0.02 (0.05)
Number of Emp.	0.21	-0.05 (0.06)	0.02 (0.06)
Business Practices:			
Offer credit	0.74	0.00 (0.06)	-0.02 (0.06)
Have bank account	0.30	-0.03 (0.06)	-0.03 (0.06)
Taken loan	0.14	-0.03 (0.04)	-0.05 (0.04)
Practice accounting	0.11	-0.07 (0.04)	0.00 (0.04)
Advertise	0.07	-0.02 (0.03)	0.04 (0.03)
Sector:			
Manufacturing	0.04	0.00 (0.02)	-0.03 (0.02)
Retail	0.69	-0.12 (0.06)**	0.03 (0.06)
Restaurant	0.14	0.06 (0.05)	-0.02 (0.05)
Other services	0.17	0.06 (0.05)	0.06 (0.05)
Owner Characteristics:			
Age	29.1	0.87 (0.65)	-0.25 (0.64)
Secondary Education	0.51	-0.04 (0.06)	-0.00 (0.06)
Observations	119	129	124

Table Notes: Columns 1-3 are the coefficient estimates from the regression $y_i = \alpha + \gamma C_i + \beta M_i + \varepsilon_i$, where C_i and M_i are indicators for the class and mentorship treatments. Column 1 is $\hat{\alpha}$. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 3: OLS Estimates on Profit at Different Time Periods (ANCOVA)

| | Pooled | Months since treatment | | | | | | |
		(1)	(2)	(3)	(4)	(7)	(12)	(17)
Mentee	371.83	266.06	50.22	321.06	994.68	915.59	20.81	35.86
	(133.02)***	(203.36)	(208.94)	(272.72)	(279.15)***	(336.22)***	(210.16)	(289.39)
Class	124.51	193.72	17.05	34.84	312.58	141.01	-150.85	284.21
	(144.90)	(200.90)	(203.32)	(264.57)	(275.37)	(330.48)	(210.19)	(0.32)
Control mean	1803.48	1733.11	1412.43	1903.30	1620.28	2437.84	1764.84	1973.37
p-value, $H_0 : M = C$	0.102	0.719	0.872	0.285	0.014	0.018	0.418	0.383
Obs.	2578	345	311	312	316	302	318	307
R^2	0.108	0.056	0.028	0.057	0.081	0.076	0.102	0.053
Controls	Y	Y	Y	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. The top and bottom one percent of dependent variables are trimmed. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. The p-value provided is the p-value for testing equality of treatment effects across the two treatment arms.

Table 4: Heterogeneous Mentor Effects from OLS Profit Regression

	Pooled	Months since treatment					
		(1)	(2)	(3)	(4)	(7)	(12)
Mentee: mentor in $(0, 25)$ pctile	356.037 (186.90)*	235.92 (256.74)	-43.99 (259.35)	232.89 (336.44)	747.57 (360.20)**	1000.83 (412.41)**	-18.91 (265.46)
Mentee: mentor in $(25, 75)$ pctile	461.92 (170.93)***	429.69 (270.31)	93.80 (278.65)	596.22 (372.14)	1151.34 (370.33)***	629.19 (461.45)	-108.49 (278.64)
Mentee: mentor in $(75, 100)$ pctile	539.42 (260.32)**	-88.66 (406.72)	285.35 (450.86)	-117.46 (372.14)	1317.05 (567.77)**	1338.68 (666.64)**	542.90 (432.43)
Class	95.27 (142.49)	192.74 (201.10)	14.91 (203.85)	32.28 (264.82)	304.47 (275.82)	140.60 (331.04)	-152.32 (211.56)
Control mean	1803.48	1733.11	1412.43	1903.30	1620.28	2437.84	1764.84
p-value, $H_0 : M_H = M_L$	0.541	0.457	0.490	0.557	0.355	0.634	0.225
p-value, $H_0 : M_L = C$	0.184	0.865	0.817	0.543	0.211	0.033	0.615
Obs.	1902	345	315	312	316	302	318
R^2	0.090	0.059	0.003	0.095	0.085	0.079	0.108
Controls	Y	Y	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. The top and bottom one percent of dependent variables are trimmed, though results are robust to other (or no) trimming procedures. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. The first p-value is for test of equality of "high" (percentile 75-100) mentor and "low" (percentile 0-25) mentor effects, while the second is the p-value for equality of means of "low" mentor and class.

Table 5: Regression discontinuity results for mentor treatment effect

% of IK optimal bandwidth	Scale		Practices	
	Profit	Inventory	Marketing	Record keeping
100	-503.18 (1321.82)	-3105.87 (2698.11)	0.01 (0.11)	0.02 (0.18)
150	300.19 (1407.26)	-2585.22 (2291.34)	0.01 (0.09)	0.07 (0.14)
200	322.09 (1324.17)	-123.59 (1964.08)	0.01 (0.08)	0.10 (0.13)
Treatment Average	4387.34	8435.79	0.08	0.85
Control Average	1794.09	4039.20	0.13	0.63

Table notes: Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Profit and inventory are both trimmed at 1 percent.

Table 6: Effects on Revenue, Output Prices and Supply Costs

	Revenue (1)	Revenue (2)	Output Price (3)	Output Price (4)	Supply Cost (5)	Supply Cost (6)
Mentee	-95.44	162.77	-3.47	-7.80	-341.73	-326.54
	(428.95)	(445.52)	(70.83)	(65.59)	(109.88)***	(110.09)***
Class	-154.88	193.30	9.78	2.29	-104.02	-86.83
	(457.90)	(440.64)	(70.62)	(71.26)	(136.78)	(144.01)
Control mean	5813.7	5813.7	315.8	315.8	645.1	645.1
p-value, $H_0 : M = C$	0.896	0.944	0.843	0.875	0.016	0.023
Obs.	1,400	1,398	867	867	821	821
R^2	0.013	0.066	0.001	0.030	0.024	0.035
Controls	N	Y	N	Y	N	Y

Table notes: Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Standard errors are clustered at the firm level.

Table 7: Likelihood of Switching Suppliers

	Switch suppliers?
Mentee	0.187
	(0.065)***
Class	-0.002
	(0.069)
Control mean	0.617
p-value, $H_0 : M = C$	0.002
Obs.	304
R^2	0.070
Controls	Y

Table notes: Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. p-values given are for the t-tests comparing equality of the different treatment effects.

Table 8: OLS Treatment Effects for Business Practices at Different Time Periods

Panel A: Record Keeping		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	0.072	-0.008	0.119	0.099	0.143	-0.022	0.105
	$(0.027)^{***}$	(0.054)	$(0.057)^{**}$	(0.059)	$(0.061)^{**}$	(0.069)	(0.063)
Class	0.146	0.198	0.185	0.122	0.308	-0.047	0.098
	$(0.027)^{***}$	$(0.052)^{***}$	$(0.057)^{***}$	$(0.059)^{**}$	$(0.060)^{***}$	(0.067)	(0.063)
Control Mean	0.654	0.719	0.683	0.689	0.561	0.629	0.636
p-value, $H_0 : M_L = C$	0.003	0.000	0.238	0.688	0.007	0.710	0.909
Obs.	1941	351	317	318	322	308	325
R^2	0.052	0.090	0.099	0.057	0.090	0.062	0.049
Controls	Y	Y	Y	Y	Y	Y	Y

Panel B: Marketing/Advertising		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	-0.004	0.003	-0.022	0.077	-0.014	-0.015	-0.058
	(0.019)	(0.052)	(0.048)	$(0.044)^{*}$	(0.036)	(0.055)	(0.046)
Class	-0.014	0.030	-0.050	-0.008	0.007	0.010	-0.086
	(0.020)	(0.052)	(0.047)	(0.043)	(0.035)	(0.053)	$(0.045)^{*}$
Control Mean	0.201	0.202	0.163	0.097	0.075	0.186	0.182
p-value, $H_0 : M_L = C$	0.643	0.611	0.541	0.050	0.551	0.634	0.554
Obs.	1941	351	317	318	322	308	325
R^2	0.028	0.050	0.040	0.052	0.052	0.056	0.040
Controls	Y	Y	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 9: OLS Treatment Effects on Aggregated Business Practice Measures used in McKenzie and Woodruff (2016)

	Aggregate z-score	Score Components		
		Marketing z-score	Stock z-score	Record keeping z-score
Mentee	0.140	0.006	0.248	0.088
	(0.098)	(0.096)	$(0.099)^{**}$	(0.095)
Class	0.118	-0.022	0.248	0.063
	(0.093)	(0.096)	$(0.096)^{**}$	(0.095)
p-value, $H_0 : M = C$	0.810	0.779	1.000	0.794
Obs.	633	633	633	633
R^2	0.076	0.059	0.061	0.062
Controls	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

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A Further Balance Tests and Attrition

Table 10: Wave 1 Balance Test

	Control (114)	Class (125)	Mentor (113)
Firm Scale:			
Profit (last month)	10252	9783	9268
Firm Age	2.4	2.6	2.4
Has Employees?	0.09	0.10	0.13
Number of Emp (if $n > 0$)	1.3	1.3	1.3
Business Practices:			
Offer credit	0.75	0.75	0.75
Have bank account	0.30	0.28	0.27
Taken loan	0.15	0.10	0.09
Practice accounting	0.01	0.01	0.01
Advertise	0.06	0.05	0.11
Sector:			
Manufacturing	0.04	0.05	0.01
Retail	0.69	0.57	0.65
Restaurant	0.14	0.19	0.12
Other services	0.16	0.23	0.24
Owner Characteristics:			
Age	29.3	29.8	28.9
Secondary Education	0.52	0.48	0.51

Table 11: Wave 2 Balance Test

	Control (104)	Class (113)	Mentor (101)
Firm Scale:			
Profit (last month)	9675	9355	9161
Firm Age	2.49	2.59	2.38
Has Employees?	0.09	0.08	0.12
Number of Emp (if $n > 0$)	1.00	1.44	1.33
Business Practices:			
Offer credit	0.74	0.77	0.72
Have bank account	0.32	0.27	0.28
Taken loan	0.14	0.11	0.08
Practice accounting	0.01	0.01	0.00
Advertise	0.05	0.05	0.11
Sector:			
Manufacturing	0.05	0.04	0.01
Retail	0.67	0.57	0.69
Restaurant	0.15	0.19	0.09
Other services	0.15	0.22	0.22
Owner Characteristics:			
Age	29.2	29.4	28.9
Secondary Education	0.54	0.49	0.51

Table 12: Wave 3 Balance Test

	Control (103)	Class (115)	Mentor (101)
Firm Scale:			
Profit (last month)	9942	9802	9547
Firm Age	2.40	2.63	2.31
Has Employees?	0.11	0.10	0.12
Number of Emp (if $n > 0$)	1.27	1.36	1.5
Business Practices:			
Offer credit	0.73	0.76	0.72
Have bank account	0.29	0.28	0.29
Taken loan	0.15	0.10	0.08
Practice accounting	0.01	0.01	0.01
Advertise	0.07	0.03	0.09
Sector:			
Manufacturing	0.05	0.05	0.01
Retail	0.70	0.57	0.66
Restaurant	0.14	0.19	0.11
Other services	0.16	0.22	0.24
Owner Characteristics:			
Age	29.1	29.6	28.7
Secondary Education	0.51	0.45	0.53

Table 13: Wave 4 Balance Test

	Control (107)	Class (113)	Mentor (103)
Firm Scale:			
Profit (last month)	10380	9452	9371
Firm Age	2.38	2.67	2.37
Has Employees?	0.09	0.10	0.15
Number of Emp (if $n > 0$)	1.30	1.36	1.40
Business Practices:			
Offer credit	0.75	0.75	0.69
Have bank account	0.30	0.28	0.27
Taken loan	0.15	0.11	0.09
Practice accounting	0.01	0.01	0.01
Advertise	0.07	0.05	0.09
Sector:			
Manufacturing	0.05	0.05	0.01
Retail	0.69	0.54	0.66
Restaurant	0.14	0.20	0.13
Other services	0.17	0.23	0.22
Owner Characteristics:			
Age	29.7	29.7	29.2
Secondary Education	0.53	0.49	0.50

Table 14: Wave 5 Balance Test

	Control (101)	Class (110)	Mentor (104)
Firm Scale:			
Profit (last month)	10198	8986	9195
Firm Age	2.45	2.60	2.26
Has Employees?	0.09	0.09	0.15
Number of Emp (if $n > 0$)	1.33	1.40	1.40
Business Practices:			
Offer credit	0.74	0.75	0.71
Have bank account	0.31	0.26	0.25
Taken loan	0.15	0.10	0.07
Practice accounting	0.01	0.00	0.01
Advertise	0.05	0.05	0.12
Sector:			
Manufacturing	0.05	0.05	0.01
Retail	0.69	0.54	0.66
Restaurant	0.14	0.20	0.13
Other services	0.17	0.23	0.22
Owner Characteristics:			
Age	29.6	29.6	29.4
Secondary Education	0.50	0.49	0.51

Table 15: Wave 6 Balance Test

	Control (110)	Class (109)	Mentor (104)
Firm Scale:			
Profit (last month)	10293	8986	9167
Firm Age	2.48	2.60	2.31
Has Employees?	0.21	0.16	0.21
Number of Emp (if $n > 0$)	1.33	1.03	1.27
Business Practices:			
Offer credit	0.75	0.75	0.70
Have bank account	0.31	0.26	0.26
Taken loan	0.15	0.10	0.07
Practice accounting	0.01	0.00	0.01
Advertise	0.05	0.05	0.11
Sector:			
Manufacturing	0.04	0.05	0.01
Retail	0.70	0.54	0.66
Restaurant	0.15	0.17	0.12
Other services	0.14	0.23	0.25
Owner Characteristics:			
Age	29.6	29.6	29.3
Secondary Education	0.52	0.48	0.51

Table 16: Wave 7 Balance Test

	Control (101)	Class (111)	Mentor (99)
Firm Scale:			
Profit (last month)	9739.60	9896.85	9240.30s
Firm Age	2.44	2.71	2.42
Has Employees?	0.26	0.23	0.22
Number of Emp (if $n > 0$)	0.94	0.93	1.18
Business Practices:			
Offer credit	0.73	0.73	0.74
Have bank account	0.32	0.27	0.28
Taken loan	0.16	0.09	0.07
Practice accounting	0.01	0.00	0.00
Advertise	0.07	0.05	0.12
Sector:			
Manufacturing	0.05	0.05	0.01
Retail	0.67	0.58	0.67
Restaurant	0.14	0.16	0.09
Other services	0.19	0.25	0.26
Owner Characteristics:			
Age	29.0	30.0	29.0
Secondary Education	0.50	0.47	0.52

Table 17: Correlation of baseline observables with number of surveys completed

Variable	Correlation coefficient
Firm Scale:	
Profit (last month)	0.029
Firm Age	0.137***
Has Employees?	0.054
Number of Emp (if $n > 0$)	0.049
Business Practices:	
Offer credit	0.069
Have bank account	0.078
Taken loan	-0.055
Practice accounting	-0.055
Advertise	-0.032
Sector:	
Manufacturing	0.118**
Retail	-0.006
Restaurant	-0.129**
Other services	0.050
Owner Characteristics:	
Age	0.106**
Secondary Education	0.054

Statistical significance at 0.10, 0.05, and 0.01 are denoted *, **, and ***.

B   Motivation: Baseline Learning Methods

To motivate our study, we assess how profitability varies in the cross-section based on self-reported learning methods. Fifty-five percent of all firms claimed they were self-taught, while the rest claimed to learn either from another business operator, in school, or through an apprenticeship. We then consider various measures of business success across these two groups, and find that those who are self-taught make substantially less profit and operate at a smaller scale. Figure 8 plots three measures of business scale over the lifecycle.[19] First, Figure 8a shows that the self-taught earn less profit at any point over the lifecycle. The average profit of a self-taught firm is 18 percent less than firms that learn from others. Other measures show similar patterns of self-taught firms operating at a lower scale than those who learned from others. Figures 8b and 8c show that self-taught firms are less likely to have employees and pay a smaller total wage bill.

Figure 8: Business scale differences over the lifecycle

(a) Average profit

(b) Share with employees

(c) Total wage bill

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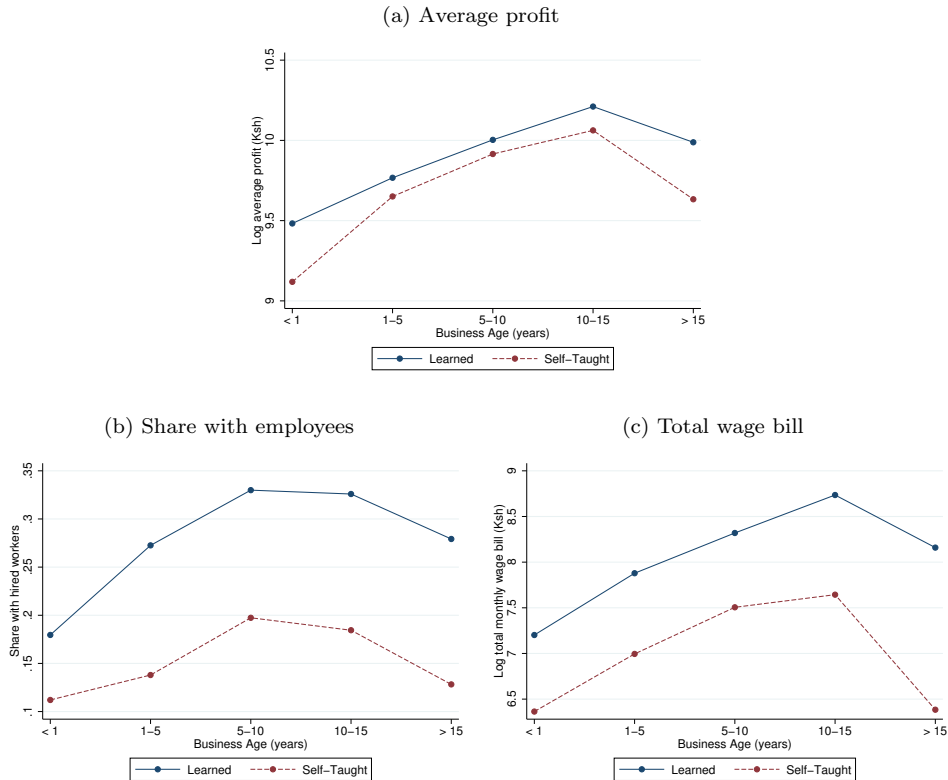
[19]Total employment looks quite similar to Figure 8b given there are so few firms that have more than one worker.

The results provide suggestive evidence that learning from others plays an important role in the profitability of business, though not all businesses have access to it. However, confounding factors (e.g. selection) limit our ability to make causal inferences about the relationship between profitability and the type or source of information. Understanding this link is the purpose of our experiment.

C Details of Regression Discontinuity

C.1 Details of Mentor Selection and Characteristics

The pool of mentors was selected from our baseline survey. We first constrained our sample to female business owners who were over 40 years old and had been operating the same business for at least 5 years. This left 366 individuals. We then ran a simple regression to control for age and sector-specific differences:

$$\log(\pi_i) = \alpha + \delta_i + \gamma \log(age_i) + \varepsilon_i \tag{C.1}$$

where π_i is baseline profit for individual i, δ_i is a sector fixed effect (manufacturing, retail, restaurant, other services) and age_i is age in years. Our mentors are chosen based on having the highest residual profit $\widehat{\varepsilon}_i$. Given this, we simply sorted potential mentors by residual profit, then, starting with the most profitable, we recruited mentors until we have enough to link each mentee to a mentor that is in the same tightly defined business sector.[20]

Mentors were offered 1,000 shillings (9.83 U.S. dollars) to participate, which is equivalent to approximately two days of profit for the average firm in this group. We also told both mentors and mentees that meetings would take place at the mentor's business to minimize inconvenience to the mentor and maintain some similarity with the travel requirement of the class treatment. These incentives were sufficient to generate high take up, as 95 percent accepted our invitation to take part in the program. Figure 9 plots the distribution of $\widehat{\varepsilon}$ along with the cut-off.

As expected, mentors run substantially more successful businesses than those not chosen, as can be seen in Table 18. Moreover, they are nearly twice times as likely to have employees.

C.2 Robustness to Bandwidth Procedure

In this Appendix, we use the MSE-optimal bandwidth procedure in Calonico, Cattaneo and Titiunik (2014) instead of the Imbens and Kalyanaraman (2012) procedure used in

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[20]The sector-specific estimates were small and statistically insignificant, as the correlation between the natural logarithm of baseline profit and $\widehat{\varepsilon}_i$ is 0.98. Using residual profit rather than raw profit did not change the set of potential mentors.

Figure 9: Distribution of $\widehat{\varepsilon}$ and cut-off
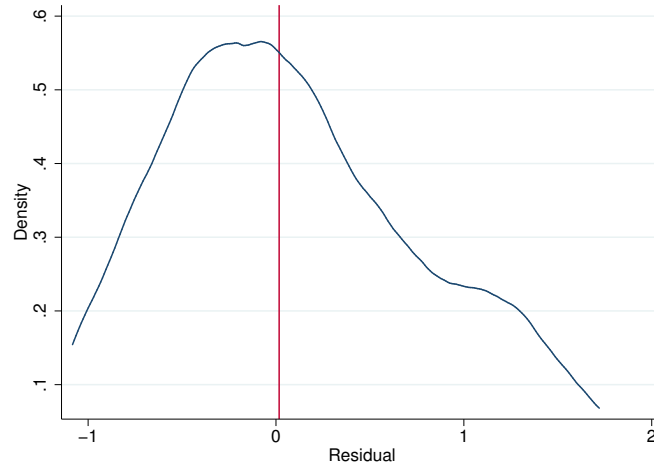
Table 18: Mentor vs. non-mentor baseline characteristics

	Mentors	Non-mentors
Firm Scale:		
Profit (last month)	21,430	6,883
Firm Age	13.0	13.2
Has Employees?	0.29	0.16
Number of Emp (if $n > 0$)	2.3	1.5
Business Practices:		
Offer credit	0.67	0.73
Have bank account	0.52	0.35
Taken loan	0.48	0.25
Practice accounting	0.11	0.08
Advertise	0.05	0.08
Owner:		
Age	44.3	43.8
Secondary Education	0.57	0.48

the text. We also allow for higher-order polynomials. In no specification do we find any statistically significant effects.

Table 19: Regression discontinuity results using Calonico, Cattaneo and Titiunik (2014)

Polynomial order	Scale		Practices	
	Profit	Inventory	Marketing	Record keeping
0	-265.39	-1739.00	0.050	0.042
	(900.51)	(2322.60)	(0.089)	(0.175)
1	-724.40	-5388.50	0.086	0.003
	(2277.10)	(3855.90)	(0.125)	(0.295)
2	-4071.80	-4107.20	0.232	0.222
	(4232.50)	(5356.30)	(0.142)	(0.505)
Treatment Average	4387.34	8501.58	0.08	0.85
Control Average	1791.94	4005.06	0.13	0.63

Table notes: Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Profit and inventory are both trimmed at 1 percent.

D   Further Results on Composition, Business Scale and Skills

D.1   Baseline Business Types

In Table 20 we provide more detail on the types of businesses included in the sample of firms assigned to the three treatment groups. Overall, retailers make up nearly two thirds of the sample, and the retail sector is mostly composed of clothing and food sellers. Firms were allowed to select more than one category, so these numbers sum to 104%.

Table 20: Detailed Composition of Businesses

Business Type	Fraction of Businesses (Entire Baseline Sample)		Fraction Female (Entire Baseline Sample)		Fraction of Businesses (Study Sample)	
Retail	0.59		0.70		0.64	
Clothes		0.22		0.74		0.32
Electronics		0.02		0.27		0.00
Fruits and vegetables		0.30		0.84		0.28
Non-perishable food		0.19		0.65		0.17
Perishable food		0.19		0.62		0.14
Medicine		0.02		0.64		0.04
Cell phones, mobile		0.00		1.00		0.00
Books, stationary		0.01		0.60		0.02
Bicycle or moto		0.00		0.50		0.00
Materials (wood, glass)		0.04		0.56		0.03
Other retail		0.12		0.57		0.08
Production	0.06		0.48		0.03	
Furniture		0.21		0.05		0.00
Cooking pots		0.01		0.00		0.00
Animals		0.03		0.60		0.09
Metal work		0.10		0.29		0.00
Tailor, seamstress		0.41		0.75		0.55
Other		0.24		0.49		0.36
Services	0.25		0.56		0.21	
Clothes repair		0.21		0.76		0.32
Carpenter		0.03		0.00		0.00
Hair stylist		0.42		0.73		0.52
Bicycle repair		0.01		0.14		0.00
Auto, moto repair		0.01		0.00		0.00
Moneylender		0.03		0.55		0.02
Clothes washer		0.04		0.23		0.02
Other		0.25		0.25		0.12
Food Prep	0.14		0.73		0.16	
Roadside food prep		0.33		0.77		0.41
Small meals, breakfast and lunch		0.40		0.69		0.36
Coffee shop		0.13		0.58		0.11
Larger meals (lunch, dinner)		0.15		0.79		0.13
Larger meals with interior seating		0.05		0.57		0.00
Bar with exterior seating		0.01		0.50		0.00
Bar with interior seating		0.03		0.62		0.02
Other		0.01		0.60		0.00

Table notes: Fractions of individual sectors are written as share of total. For example, *Furniture production* is 21 percent of total *Production* businesses. *Fraction Female* is the fraction of businesses in each sector operated by a woman.

D.2 Alternative Regression Specifications

In our main specification we included baseline values of control variables. Here we provide two alternative specifications. First, we consider the case where instead of baseline controls we include individual fixed effects. Second, we estimate treatment effects with neither baseline controls nor individual fixed effects. However, each specification does include time fixed effects. Given that our sample is balanced at baseline, it is unsurprising that we find similar results in all three specifications.

Table 21: OLS Estimates on Profit at Different Time Periods

Panel A: Fixed Effects		Months since treatment						
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)	(17)
Mentee	474.39	313.17	50.22	321.06	994.68	915.59	20.81	35.86
	$(206.65)^{**}$	(200.26)	(208.94)	(272.72)	$(279.15)^{***}$	$(336.22)^{***}$	(210.16)	(289.39)
Class	229.76	255.82	17.05	34.84	312.58	141.01	-150.85	284.21
	(212.31)	(196.04)	(203.32)	(264.57)	(275.37)	(330.48)	(210.19)	(282.78)
Control mean	1803.60	1733.11	1412.43	1903.30	1620.28	2437.84	1764.84	1973.37
p-value, $H_0 : M = C$	0.236	0.719	0.872	0.285	0.014	0.018	0.418	0.383
Obs.	2608	345	311	312	316	302	318	307
R^2	0.345	0.056	0.028	0.057	0.081	0.076	0.102	0.053
Panel B: No Controls		Months since treatment						
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)	(17)
Mentee	302.71	247.02	39.02	196.90	891.09	811.96	-85.95	38.45
	$(94.96)^{***}$	(203.92)	(203.71)	(273.78)	$(277.69)^{***}$	$(331.86)^{**}$	(216.26)	(286.07)
Class	26.50	206.93	-64.30	-153.74	203.73	63.91	-313.75	236.68
	(92.69)	(199.71)	(198.09)	(263.89)	(271.29)	(325.75)	(213.24)	(278.16)
Control mean	1803.60	1733.11	1412.43	1903.30	1620.28	2437.84	1764.84	1973.37
p-value, $H_0 : M = C$	0.003	0.841	0.605	0.189	0.013	0.021	0.291	0.479
Obs.	2608	345	311	312	316	302	318	307
R^2	0.042	0.005	0.001	0.006	0.034	0.024	0.007	0.003

Table notes: Standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. The top and bottom one percent of dependent variables are trimmed. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. The p-value provided is the p-value for testing equality of treatment effects across the two treatment arms.

D.3 Other Dimensions of Mentor Heterogeneity

We now include two other potential measures: business age and an indicator for whether the mentor was a high school graduate. Neither provides the same results. Interestingly, they are also both only somewhat correlated with mentor profitability.

Table 22: Heterogeneous Mentor Effects from OLS Profit Regression

	Business age	High School graduate
Mentee: mentor in $(0, 25)$ pctile	507.06 $(192.85)^{***}$	489.10 $(184.00)^{***}$
Mentee: mentor in $(25, 75)$ pctile	418.77 $(173.63)^{**}$	n.a.
Mentee: mentor in $(75, 100)$ pctile	329.62 (241.34)	374.40 $(158.89)^{**}$
Class	95.70 (144.64)	97.03 (144.46)
Control mean	1803.48	1803.48
p-value, $H_0 : M_H = M_L$	0.532	0.5818
p-value, $H_0 : M_L = C$	0.053	0.0472
Obs.	2271	2271
R^2	0.121	0.120
Controls	Y	Y
Corr. with mentor profit	0.059	0.122

Table notes: Robust standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. The top and bottom one percent of dependent variables are trimmed, except for indicators. For the "high school graduate" indicator, the first row denotes non-high school graduates, while the third row denotes a high school graduate mentor. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, *** for both p-values and correlation. The first p-value is for test of equality of "high" (percentile 75-100) mentor and "low" (percentile 0-25) mentor effects, while the second is the p-value for equality of means of "low" mentor and class.

D.4   Details on Supplier Switching

Table 23: OLS Treatment Effects on Supplier Switching

	Switch Suppliers	Switch Suppliers
Mentee	0.203***	0.187***
	(0.063)	(0.065)
Class	0.001	-0.002
	(0.069)	(0.069)
log(age)		-0.192
		(0.160)
Retailer		0.095
		(0.143)
Manufacturer		0.001
		(0.188)
Services		0.179
		(0.142)
Food Preparation		0.046
		(0.155)
Control mean	0.617	0.617
p-value, $H_0 : M = C$	0.001	0.002
Obs.	304	304
R^2	0.042	0.070
Controls	N	Y
Time F.E.	Y	Y

Table notes: Robust standard errors are in parentheses. Results are for switches between $t = 0$ and $t = 7$. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees at baseline. The first regression reports the results from the main text for comparison. We report the coefficient on sectoral controls and age in the second regression. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

D.5 Business Exit

In the final survey wave ($t = 17$) we ask respondents if they are still operating the business from baseline. We find no evidence that members of either the classroom or mentorship treatment exit at a greater rate than the control group as summarized in Table 24.

Table 24: OLS Treatment Effects on Exit

	Exit by $t = 17$	Exit by $t = 17$
Mentee	-0.029	-0.024
	(0.028)	(0.028)
Class	-0.023	-0.023
	(0.028)	(0.029)
Control mean	0.059	0.059
p-value, $H_0 : M = C$	0.836	0.972
Obs.	311	311
R^2	0.003	0.026
Controls	N	Y
Time F.E.	Y	Y

Table notes: Robust standard errors are in parentheses. Results are presented for pooled regressions from $t = 1$ to $t = 12$. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees at baseline. The top and bottom one percent of dependent variables are trimmed for all dependent variables except the 0-1 indicator. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

D.6 Product Switching

Table 25: OLS Treatment Effects on Product Mix

	New Product?	New Product?
Mentee	-0.009	-0.010
	(0.023)	(0.023)
Class	-0.007	-0.007
	(0.025)	(0.025)
Control mean	0.198	0.198
p-value, $H_0 : M = C$	0.915	0.908
Obs.	1921	1917
R^2	0.041	0.043
Controls	N	Y
Time F.E.	Y	Y

Table notes: Standard errors clustered at individual level are in parentheses. Results are presented for pooled regressions from $t = 1$ to $t = 12$. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees at baseline. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

D.7 Measures of Business Scale

At $t = 7$ and $t = 12$, we included more detailed measures of business scale, including inventory stock, employment, and hours of business operation. In each wave, we asked about any big investments made in the business. We test whether these margins change in response to either of the treatments. We find that they do not, again highlighting the importance of the supplier and cost channels emphasized in the main text. To test them, we run a variation of the pooled regression (4.1) with the caveat that the variables are only available for two periods. The results are in Table 26.

Table 26: OLS Treatment Effects on Business Scale Measures

Panel A: No Controls	Stock of inventory (Ksh)	Any employees?	Number of employees	Total wage bill (Ksh)	Hours open (last week)	Any investment?
	(1)	(2)	(3)	(4)	(5)	(6)
Mentee	851.04	-0.038	-0.006	224.43	0.82	0.005
	(2275.16)	(0.025)	(0.038)	(231.73)	(2.24)	(0.006)
Class	-1166.94	-0.032	-0.034	117.44	1.51	0.002
	(1907.59)	(0.026)	(0.034)	(183.64)	(2.10)	(0.006)
Control mean	11030.83	0.092	0.099	360.00	49.42	0.011
p-value, $H_0 : M = C$	0.290	0.792	0.405	0.678	0.760	0.671
Obs.	629	633	628	630	629	1911
R^2	0.002	0.07	0.002	0.005	0.005	0.012
Controls	N	N	N	N	N	N
Panel B: With Controls	Stock of inventory (Ksh)	Any employees?	Number of employees	Total wage bill (Ksh)	Hours open (last week)	Any investment?
	(1)	(2)	(3)	(4)	(5)	(6)
Mentee	1304.43	-0.032	0.001	244.98	0.42	0.005
	(2104.53)	(0.022)	(0.034)	(212.08)	(2.29)	(0.006)
Class	-398.74	-0.027	-0.029	112.56	2.21	0.002
	(1881.65)	(0.022)	(0.034)	(165.48)	(2.08)	(0.006)
Control mean	11030.83	0.092	0.099	360.00	49.42	0.011
p-value, $H_0 : M = C$	0.364	0.806	0.334	0.579	0.423	0.578
Obs.	629	633	628	630	629	1911
R^2	0.067	0.240	0.206	0.162	0.048	0.018
Controls	Y	Y	Y	Y	Y	Y

Table notes: Standard errors are in parentheses. Results for regressions 1–6 are available for $t = 7$ and $t = 12$, while regression 6 is available for $t = 1$ to $t = 12$. Controls include a time fixed effect, secondary education, age of owner, sector fixed effects, and an indicator for any employees at baseline The top and bottom one percent of dependent variables are trimmed for all dependent variables except for 0-1 indicators. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

D.8 Formal and Informal Borrowing

One possibility is that mentors provide informal financing for mentees. At $t = 7$ and $t = 12$ we asked mentees if their mentor had ever given them an informal loan. At $t = 7$, no one answers yes and at $t = 12$, only 2 percent of mentees affirm financial help from their mentor. A second possibility is that mentors advise mentees to access the formal financial market. Regression 1 and 2 are an indicator equal to one if a business had taken a loan at any point in the previous year, asked at $t = 12$. We find no significant effect on formal borrowing.

Table 27: OLS Treatment Effects on Borrowing

	Loan in last year ($t = 12$)	Loan in last year ($t = 12$)
	(1)	(2)
Mentee	0.011	0.015
	(0.023)	(0.024)
Class	0.009	0.015
	(0.023)	(0.023)
Control mean	0.020	0.020
p-value, $H_0 : M = C$	0.937	0.995
Obs.	298	298
R^2	0.001	0.015
Controls	N	Y

Table notes: Standard errors are in parentheses. Results are presented for pooled regressions from $t = 1$ to $t = 12$ with standard errors clustered at the individual level. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees at baseline. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

D.9 Decomposition of Business Scores

Table 28: Marketing Practices Decomposed

Panel A: $t = 7$		Marketing Score Components				
	Marketing Score	Check competitor price	Check competitor products	Have sales	Upsell	Advertise
Mentee	0.185	0.036	0.096	0.064	0.072	-0.083
	(0.195)	(0.061)	(0.060)	(0.067)	(0.073)	(0.061)
Class	-0.263	-0.058	-0.031	-0.017	-0.064	-0.092
	(0.161)	(0.054)	(0.053)	(0.061)	(0.071)	(0.058)
Control mean	1.505	0.206	0.186	0.289	0.536	0.289
p-value, $H_0 : M = C$	0.015	0.082	0.020	0.200	0.048	0.867
Obs.	308	308	308	308	308	308
R^2	0.104	0.036	0.060	0.072	0.048	0.094
Controls	Y	Y	Y	Y	Y	Y
Panel B: $t = 12$		Marketing Score Components				
	Marketing Score	Check competitor price	Check competitor products	Have sales	Upsell	Advertise
Mentee	-0.203	-0.111	-0.072	-0.038	0.041	-0.023
	(0.185)	(0.063)*	(0.065)	(0.056)	(0.053)	(0.055)
Class	0.248	0.106	0.108	0.020	0.132	-0.117
	(0.202)	(0.067)	(0.068)	(0.058)	(0.058)**	(0.050)**
Control mean	1.400	0.373	0.391	0.227	0.191	0.218
p-value, $H_0 : M = C$	0.022	0.001	0.008	0.292	0.127	0.061
Obs.	325	325	325	325	325	325
R^2	0.081	0.064	0.047	0.046	0.090	0.037
Controls	Y	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Marketing score is computed by summing all its components.

Table 29: Stock Practices Decomposed

Panel A: $t = 7$		Stock Score Components		
	Stock Score	Haggle with suppliers	Compare suppliers	Run out of stock
Mentee	0.521	0.141	0.158	-0.221
	$(0.118)^{***}$	$(0.061)^{**}$	$(0.067)^{**}$	$(0.052)^{***}$
Class	0.445	0.107	0.119	-0.220
	$(0.116)^{***}$	$(0.061)^{*}$	$(0.066)^{*}$	$(0.048)^{***}$
Control mean	1.021	0.691	0.598	0.268
p-value, $H_0 : M = C$	0.482	0.517	0.534	0.952
Obs.	308	308	308	308
R^2	0.099	0.041	0.048	0.125
Controls	Y	Y	Y	Y
Panel B: $t = 12$		Stock Score Components		
	Stock Score	Haggle with suppliers	Compare suppliers	Run out of stock
Mentee	-0.12	-0.03	-0.05	0.02
	(0.13)	(0.07)	(0.07)	(0.06)
Class	-0.05	-0.04	0.02	0.01
	(0.13)	(0.07)	(0.07)	(0.06)
Control mean	1.51	0.21	0.19	0.29
p-value, $H_0 : M = C$	0.004	0.042	0.021	0.067
Obs.	306	306	306	306
R^2	0.025	0.010	0.015	0.008
Controls	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Aggregate stock score is computed as *Haggle + Compare - Run out of stock.*

Table 30: Record Keeping Practices Decomposed

Panel A: $t = 7$		Record Keeping Score Components		
	Record Keeping Score	Record every sale	Consult records	Budget costs
Mentee	0.239	0.053	0.033	0.153
	(0.187)	(0.069)	(0.070)	(0.068)**
Class	0.070	-0.026	0.042	0.054
	(0.179)	(0.066)	(0.068)	(0.069)
Control mean	1.711	0.598	0.557	0.557
p-value, $H_0 : M = C$	0.346	0.243	0.901	0.131
Obs.	308	308	308	308
R^2	0.088	0.091	0.070	0.073
Controls	Y	Y	Y	Y
Panel B: $t = 12$		Record Keeping Score Components		
	Record Keeping Score	Record every sale	Consult records	Budget costs
Mentee	-0.001	0.090	0.033	-0.125
	(0.140)	(0.060)	(0.065)	(0.060)**
Class	0.094	0.021	0.032	0.401
	(0.151)	(0.062)	(0.066)	(0.066)
Control mean	1.391	0.700	0.345	0.345
p-value, $H_0 : M = C$	0.509	0.254	0.983	0.007
Obs.	325	325	325	325
R^2	0.071	0.052	0.054	0.065
Controls	Y	Y	Y	Y

Table notes: Standard errors are in parentheses. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Record keeping score is computed by summing all its components.

D.10 Mentor-Mentee Match Continuation

We run an additional test not subject to the biases that potentially entered into mentee answers (though we were careful to explain that the program had ended, and any answer would have no positive or negative effects on anything in the future). The idea of this test is that the mentee cannot perfectly predict whether an additional meeting with the mentor is worth the cost, since the value of the meeting is determined by information that she does not yet possess. But if the mentee decides to end the relationship because the mentor is providing increasingly less valuable advice, this should translate into increasingly smaller improvements in her business, which is observable in the data. Similarly, if the mentor's participation is driven by the satisfaction of seeing improvements in the mentee's firm over time or since baseline, lack of firm growth or performance should predict exit by the mentor. In either case, if a mentee has high profit at t_1 and low profit at t_2, then the selection explanation implies the match should end. Therefore, the likelihood of meeting with her mentor at t_3 should be positively related to the change in profits $\pi_{t_2} - \pi_{t_1}$: those who continue to generate profit growth through meetings continue to meet, while those who do not are less likely to meet. Alternatively, if matches end for reasons unrelated to mentee profitability (i.e. the mentor tires of time away from her own business, mentor moves to different city, etc.) then there should be no relationship between meeting and profit changes. We therefore ask whether changes in profitability affect the likelihood of meeting with a mentor in the future with the regressions

$$\Delta Meet_{it} = \alpha + \beta \Delta \pi_{i,t-1} + \varepsilon$$

run on just the mentees, with standard errors clustered at the individual level. The variable $Meet_t = 1$ if the mentee is still meeting with her mentor and $\Delta Meet_t = Meet_t - Meet_{t-1}$. We define $\Delta \pi_{i,t-1}$ in three different ways, to allow mentees to potentially react to different profit fluctuations. The first is $\Delta \pi_{i,t-1} := \log(\pi_{i,t-1}) - \log(\pi_{i,t-2})$, so that we test whether lagged profit changes impact meeting likelihood. The second is $\Delta \pi_{i,t-1} := \log(\pi_{i,t-1}) - \log(\pi_{i,0})$ which test whether changes from baseline profit predict meetings. Lastly, $\Delta \pi_{i,t-1} := \log(\pi_{i,t-1}) - \log(\overline{\pi}_{i,t-1}^{control})$. This regression considers that mentees may react to how well they are doing relative to the control group. If mentees are responding

Table 31: Relationship between Meeting with Mentor and Previous Profit Realizations

	$\Delta Meet_t$					
$\log \pi_{t-1} - \log \pi_{t-2}$	-0.017	-0.014	–	–	–	–
	(0.026)	(0.028)				
$\log \pi_{t-1} - \log \pi_0$	–	–	-0.002	0.000	–	–
			(0.019)	(0.019)		
$\log \pi_{t-1} - \log \overline{\pi}_{t-1}^{control}$	–	–	–	–	-0.004	0.005
					(0.025)	(0.026)
Constant	-0.188	-0.383	-0.291	-0.508	-0.300	-0.504
	(0.317)	(0.326)	(0.336)	(0.353)	(0.340)	(0.351)
Obs.	414	414	423	423	423	423
R^2	0.012	0.027	0.009	0.024	0.000	0.024
Wave F.E.	N	Y	N	Y	N	Y
Controls	Y	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses, and are clustered at individual level. Controls include secondary education, age of owner, sector fixed effects, and an indicator for any employees. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. The variable $Meet_t = 1$ if an individual has met with their mentor in period t.

to changes in profit as is required for the selection explanation, we would expect to see $\widehat{\beta} > 0$ for at least one of these regressions. The results are presented in Table 31, and we find no evidence that meeting likelihood is responding to mentee profit realizations. In all specifications, we have no statistically significant effects. Moreover, the point estimates are small. Combined with the previous evidence, this suggests that the cause of the decline in average effect is driven by the dissolution of matches by mentors, not necessarily by a decrease in the impact of continued mentorship. These results suggest the differences in Figure 7 are not simply a result of selection, but that the *flow* of information is critical to continued success of the mentee.

D.11 Self-Reported Usefulness

Within the set of mentees, we asked them to report what they felt was the greatest benefit they derived from having a mentor. Their answers are tabulated below:

Table 32: Self-Reported Benefits from Mentorship

Benefit	Proportion
Motivation to work harder	22%
Greater confidence in decision-making	17%
Specific business skills	17%
Introduced to new suppliers	13%
Introduced to new customers	12%
Change hours of operation	7%
Gave financial help (loans or cash)	6%
Other	6%

E   Cost Effectiveness

From Table 3, the average treatment effects pooled over survey waves are 371.83 Ksh for mentees and 124.51 Ksh for the class. However, this is potentially too generous a treatment effect to use when calculating cost effectiveness here. Because the effect is zero by month 12, we instead use the average effect over just the first year of the program. We re-run the same profit regression and find treatment effects of 422.57 Ksh and 96.47 Ksh for mentees and class, respectively. In Table 33 we translate this into USD and provide the 95 percent confidence interval using an exchange rate of 101.5 Ksh = 1 USD. The total effect takes this average effect and multiplies by 48 (4 weeks for 12 months).

Table 33: Treatment Effect on Weekly Profit for First Year

	Pooled treatment effect (USD)	Total Effect (USD)
Mentee	4.16 (2.26, 6.06)	199.84 (108.68, 291.00)
Class	0.95 (-0.92, 2.82)	45.62 (-44.00, 135.24)
Control Mean	18.11	1231.77

Since mentorship provides a relatively large change in profit, we next turn to the cost of the program. Total program costs – net of the direct costs of the follow-up surveys – are provided in Table 34. Even though mentorship provides substantially larger benefits than training, the last two lines of Table 34 show that it was also substantially cheaper to implement. On a per-person basis, classroom training was over four times as expensive: 41.10 USD per student compared to only 9.85 USD per mentee.

Table 34: Total Program Costs

Cost	USD
Baseline Survey	22,216
Orientation	1,480
Participation incentives	18,295
Class operation	5,302
Mentor incentives	1,222

However, to compute impact per dollar spent we require a measure of total cost per person, which as not as immediately obvious as computing the direct cost of the class or mentor. In particular, it depends on how we share the cost of the baseline survey across treatment arms. We include three cost sharing assumptions in Table 35. The first does not include the baseline cost. This assumes that we could identify mentors without the baseline survey, and therefore provides an upper bound on cost effectiveness. The second cost sharing assumption is our baseline. It splits the cost of the baseline survey across all three arms. This measure is preferred for two reasons. First, the survey is used to define the treatment groups as well as mentors. Therefore, it is reasonable to split it across all three groups. Second, the number of mentor-mentee pairs was constrained by the need for class and control groups. Put differently, had we only been interested in the mentorship program, we could have had three times as many mentee-mentor pairs with no additional cost. Thus, the mentorship group we select should only bare one-third of the cost. However, for the sake of completeness, we also include an extreme cost-sharing scenario in column three that assigns the entire 22,216 USD baseline survey cost to the mentorship program. This assumption is much too extreme. It assumes we could costlessly target and recruit young female-operated businesses without the baseline survey and that we required a 3,290 firm survey to recruit 124 mentors, which we view as unreasonable. We include it here as an absolute lower bound on the cost effectiveness of the program.

First, note that implementing the mentorship program costs only slightly more than implementing the control group, as both groups receive cash participation incentives. In cost scenario one, the average mentorship cost is only 19 percent higher than the control, compared to 77 percent higher for the class. That number drops to 9 percent under scenario two for mentees, and 36 percent for the class. Thus, the net cost of mentorship is quite small relative to control. When combined with the annualized benefit, our baseline results (cost scenario 2) show that the mentorship program returns 1.63 USD for every dollar spent on the program, compared to 0.30 USD for training. Even when we attribute the entire baseline survey cost to mentees, the program still returns 0.83 USD per dollar spent, higher than the point estimate for both our class treatment and the estimates in

Table 35: Cost-Benefit Calculation Under Different Cost Assumptions

	(1)	(2)	(3)
	No baseline cost	Baseline cost divided evenly	Baseline cost only to mentees
Per-Person Cost			
Control	53.16	112.89	53.16
Class	94.26	153.98	94.26
Mentee	63.01	122.73	242.17
Δ Profit per Dollar Spent			
Class	0.48 (-0.47, 1.43)	0.30 (-0.29, 0.89)	0.48 (-0.47, 1.43)
Mentee	3.17 (0.89, 4.62)	1.63 (0.89, 2.37)	0.83 (0.0.45, 1.20)

many other training studies summarized in McKenzie and Woodruff (2014).