Learning Managerial Skills:

Evidence from Kenyan Microenterprises*

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Abstract

We use a randomized controlled trial to evaluate whether interaction with successful firms increases managerial skill and profit among Kenyan microenterprises. Owners are assigned to receive classroom business training, meet with a randomly assigned successful firm ("mentorship"), or neither. Over the course of a year, mentee weekly profit is on average 20 percent higher than the control while no such change occurs among the class treatment. However, the effect fades over time as matches dissolve. The gains are driven by the fact that mentees are 40 percent more likely to switch suppliers in the aftermath of the treatment, increase inventory spending by 20 percent, and have 50 percent lower inventory costs relative to both the class and the control. We exploit our mentor selection procedure with a regression discontinuity design to show that there are no changes in scale or business practices among mentors. This implies that the observable gains from the interaction accrue only to the mentee, consistent with models in which learning occurs only in one direction.

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1 Introduction

Microenterprises account for a large share of businesses in many developing countries, despite the fact that many never grow. Understanding why this is the case not only has important implications for the welfare of the poor, but also has important aggregate development consequences.[1] One possibility is that microenterprise owners lack what Bloom and Van Reenen (2007) and Bruhn et al. (2010) refer to as managerial capital. That is, they lack the skill or know-how to run a business, which limits their profitability and scale. In response, business training has received significant attention from both academics and policy makers. International organizations spend over a billion dollars per year pursuing various forms of training while large scale programs such as the International Labor Organization's Start and Improve Your Business program have reached over 4.5 million people (van Lieshout et al., 2012; Blattman and Ralston, 2015). Despite this effort, formal business training has generated only marginal impact on business profit or operational scale among microenterprises (McKenzie and Woodruff, 2014; Blattman and Ralston, 2015).

Of course some businesses ultimately succeed, even in economies in which the above facts are most salient. These business owners then – at least in part – embody the skills and knowledge required to successfully grow a business in that specific economy, which potentially differ from topics covered in standard training classes. In this paper we ask two questions. First, can successful business owners transfer their business knowledge to young, inexperienced business owners, and thus increase managerial capital and profit? Second, if that is the case, to what extent does it differ from skills covered in training classes? If they differ substantially, this could provide a rationale for the relatively small impact of training.

We attempt to answer these questions with a randomized controlled trial in the Kenyan slum of Dandora, in which we assess the importance of interacting with significantly more successful business owners and compare it to a standard in-class training program. We randomly assign 378 young, female-run businesses to receive a mentor drawn from the most successful business owners in Dandora ("mentees"), attend in-class training ("trainees"), or a control group. These mentors – randomly assigned conditional on business type – are on average twice as profitable as their mentees, are almost twice as likely to have employees, and have been in business for almost ten years longer. Mentees meet with their mentor weekly for one month (though we find that many meet well after the program ends) at the

[1] See Restuccia and Rogerson (2008) and Hsieh and Klenow (2009), among many others, for evidence of the aggregate impact of distortions that limit firm growth.

mentor's business in a relatively unstructured setting. We provided no guidance on topics or important issues to discuss, as the goal is to let the mentors and mentees jointly decide on relevant topics. Classroom training, on the other hand, provided a more structured learning environment. Classes were taught in Dandora using a well-established microenterprise training curriculum developed by Strathmore University in Nairobi, and taught by instructors with substantial previous experience. The course covered the four broad topics of marketing, accounting, business plans, and cost structures. While comparing mentorship to the control identifies the absolute impact of interaction with successful businesses, the comparison to the training treatment gives the relative impact and allows us to deconstruct the relevant channels of both treatments.

We find that mentorship is an effective means to increase business outcomes among microenterprises. Over the course of the twelve months following treatment, weekly profit is on average 20 percent higher among mentees than the control, compared to a (statistically insignificant) 0.2 percent increase among the class treatment. However the average effect masks important heterogeneity. After four months, the average mentee treatment impact is 30 percent and remains approximately the same up to seven months post-treatment. After twelve months, however, there is no significant difference across any of the three groups, so that the average effect fades over time.

We use the panel dimension of our data to understand the erosion of the average effect, and find that it is due to the dissolution of matches over time. While all mentees meet with their mentors during the treatment month, that number drops to about 50 percent twelve months after the program officially ended. Revealed preference then suggests that at least this half of mentees were receiving some benefit from the program despite the lack of an average effect. The data support this hypothesis, as weekly profit one year post-treatment was on average 55 percent higher among those that were still meeting with their mentors relative to those that were not, despite the fact that there is no difference in profit between these two groups at baseline. However, the implication of this result depends on why the matches ended. If meetings ended when matches no longer generated any benefit, then the result is simply selection bias - mentees meet with mentors until benefits run out, then stop. We test this and find no relationship between changes in mentee profit and the likelihood of meeting with a mentor over time. Moreover, nearly 70 percent of matches were ended by the mentor as opposed to the mentee. This suggests that it is not the effect of mentorship fading over time, but instead that mentee-mentor matches were not sufficiently persistent to generate a sustained average impact.

We then turn to assessing the underlying causes of the increase in profit among mentees. The key difference is that mentees are nearly 40 percent more likely to have switched suppliers in the aftermath of the treatment. This translates into a decrease in the unit cost of inventory. On their main product, mentees have a unit inventory cost that is half that of the class or control group one month after the treatment, and in response, increase their spending on inventory. In the months immediately following treatment (a time when profit is the same across the three groups), mentees spend approximately 50 percent more on inventory than the control or class groups. Taken together, we view this as evidence of the value of local information available to the mentees relative to more generic information available to training students. Moreover, our results imply that this information can indeed be transferred from more successful businesses, though a prolonged impact requires sustained matches.

Despite the fact that only mentorship generates higher profit, both classes and mentorship affect behavior, which is consistent with previous studies of in-class training (e.g. Karlan and Valdivia, 2011; Bruhn and Zia, 2013; Giné and Mansuri, 2014). In the immediate aftermath of the treatment, both mentees and in-class trainees are more likely to take up formal bookkeeping, and the effect is in fact larger among the trainees. Moreover, both mentees and trainees are less likely to run out of stock, suggesting that the accounting practices allow business owners to better manage inventory. After a few months however, both groups stop their new accounting practices, consistent with short-run experimentation found in other studies (Karlan et al., 2014 find a similar result in Ghana). However, we find little differential changes in business practices for mentees in terms of marketing, record keeping, and customer relations. The classes were therefore effective at conveying (some of) their content to the trainees, but the effect was short-lived and never translated to higher profits.

Lastly, since we find that mentorship has a positive impact on mentees, we assess the impact of the being chosen as a mentor. We cannot directly test mentors against non-mentors as we specifically choose mentors for their relatively high profit and business experience. Since mentors are chosen based on ranking of residual profit after taking out sector-specific fixed effects, we instead exploit this procedure with a regression discontinuity design. After resurveying the mentors and 95 female business owners just below the cutoff, we find no impact on profit or business practices. This suggests that knowledge is flowing only from mentors to mentees, consistent with the idea that mentors are transmitting knowledge derived from their larger stock of local business knowledge. Moreover, the result is consistent with models of knowledge transfer, which typically assume that the gains from an interaction between two firms accrue solely to the less productive member of the match (e.g. Jovanovic and Rob, 1989; Lucas and Moll, 2014).

1.1 Related Literature

This paper is most closely related to the growing literature focused on understanding constraints to managerial capital in small firms, and in particular its relation to business practices. In-class training has been subject to a number of recent studies, and McKenzie and Woodruff (2014) provide an excellent and comprehensive review. 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), though they point out that this could be due in part to small sample sizes. Confirming these results, we find an immediate change in accounting practices and little change in profit in our class treatment. Others have focused on varying delivery methods for microenterprise training outside of formal classroom training. Drexler et al. (2014) compare in-class financial literacy training to simpler rule-of-thumb training. They find that the latter improves practices and some evidence that rule-of-thumb training helps businesses better manage negative shocks, similar to the finding of Karlan and Valdivia (2011). Neither, however, find an average impact on profit. Calderon et al. (2013), one of the few training studies that finds a positive impact on profit, also points out the potential importance of supplier differentiation between control and treatment, as we find here. Closer to our work are recent studies by Bruhn et al. (2013) and Karlan et al. (2014), who provide individual-level consulting services to enterprises in Mexico and Ghana. We instead provide business advice from inside the community, which provide more local advice and are much cheaper to implement.

Second, learning from other businesses or individuals has been considered in other work. Foster and Rosenzweig (1995), Munshi (2004), Bandiera and Rasul (2006), and Conley and Udry (2010) all carefully document the existence and importance of social learning in various contexts. BenYishay and Mobarak (2014) and Beaman et al. (2015) leverage existing social networks to study diffusion and targeting of new technological information. Our point is distinct, but complimentary to this literature. While the aforementioned papers focus on how information flows through existing networks – and in the latter cases, exogenously introduce new information – we introduce a new node to microenterprise owners' networks (successful business owners) and show that these mentors generate profitable changes to mentee businesses.

Lastly, understanding the constraints to managerial ability are important from an aggregate prospective as well. Bhattacharya et al. (2013) and Da-Rocha et al. (2014) extend the policy distortion models developed in Restuccia and Rogerson (2008) and Hsieh and Klenow (2009) to allow for endogenous managerial skill, and show the aggregate quantitative importance of barriers to skill investment. We show that eliminating barriers to information embodied in other business owners can potentially play a large role. We therefore provide some micro evidence in support of theories that use the interaction of economic agents to explicitly model the link from information transmission to economic development (Lucas, 2009; Lucas and Moll, 2014; Perla and Tonetti, 2014). More specifically, we show the importance of improving the distribution from which businesses draw matches, which can take many other forms than the one considered here. Buera and Oberfield (2015), for example, study the effects of learning from high-quality exporters in response to a change in trade barriers. Furthermore, these models assume that information (broadly defined) flows in one direction from the more to less productive member of the match. We test this with a regression discontinuity design, and confirm the the observable benefits of the match accrue solely to mentees.

The rest of this paper proceeds as follows. In Section 2 we use our baseline survey of 3,290 businesses to provide background on the business climate in Dandora, Kenya. Section 3 lays out the design of our experiment, including the mentor selection procedure. Section 4 provides the empirical results, and Section 5 studies the business impact of being chosen on as a mentor. Finally, Section 6 concludes.

2 Business 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. Though not the focus of this study, Dandora borders the Dandora Municipal Dump Site and is the only waste site for the entire city of Nairobi, resulting in some of the highest levels of pollution in the world.

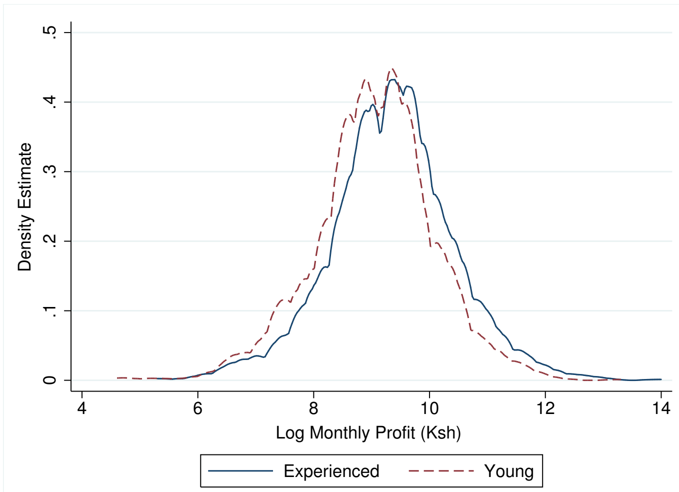
To assess the business characteristics in Dandora, we conducted a street-level survey of 3,290 randomly selected business.[2] Table 1 provides summary statistics for business. Column

[2] The procedure worked as follows. We generated 200 points randomly throughout the city, and then gave each enumerator a list of randomly selected numbers. Starting from a randomly selected point, they were instructed to count businesses until

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 average business in our survey has profit of 16,899 Ksh (167 USD) in the previous month. This is approximately 72 percent above GDP per capita in Kenya. However, while the average young owner earns 14266 Ksh, the average experienced (i.e. not "young") owner earns nearly 42 percent more profit per month or 20168 Ksh. Figure 1 plots the distribution of log profit for young and experienced enterprises.

Figure 1: Log profit distribution for young and experienced enterprises



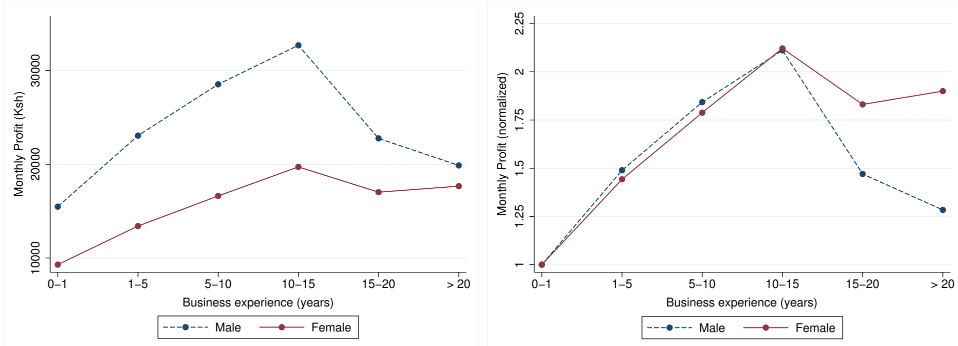
The profit of established businesses is clearly shifted to the right. Of course, Figure 1 suffers from survivor bias. Yet despite the substantial difference in profit, there is not much difference in observable business practices. They are equally likely to offer credit to costumers, have a bank account, have taken a loan at some point in the past, or engage in formal accounting or advertising. Moreover, they are roughly equally educated. To the extent that we believe proper business practices are driving business success, Table 1 suggests that business practices of young businesses are roughly the same as more established enterprises.

We further focus on female microenterprise owners, as they make up 71 percent of inexperienced owners. As Figure 3a shows, they are unambiguously less profitable than their male counterparts at every business experience level. Interestingly, however, this does not seem to be an issue of catch-up over the lifecycle as the average profit growth rate is relatively similar between men and women over the first 15 years of experience (Figure 2b).

the reached a number on their list, and survey the business owner of that establishment.

Figure 2: Gender differences over the lifecycle

(a) Average lifecycle profit (b) Average lifecycle profit growth



2.1 Self-Taught vs. Learned Business Owners

To motivate our study of learning, we asked individuals in the baseline about where they learned to operate a business - watching either family or non-family operate a similar business, working for someone else, in school, or through trial and error on their own. Table 2 describes the fraction of individuals that claim each learning technique.[3] There is no difference between young firms and the population average. However, when the sample is divided between those who are exclusively self-taught and those that are not, those who learn from others run more successful businesses on average, which is in Table 3.

Those who are self-taught make substantially less profit and operate at a smaller scale. The profit ratio is almost identical among young (15,907 versus 12,778 Ksh) and experienced firms (21,972 versus 18,055 Ksh). Those numbers do hide some catch-up of the self-taught however. Figure 3 plots thre measures of business scale over the lifecycle.[4] First, Figure 3a shows that the self-taught do seem to catch up to learned enterprises over time, especially over the first few years of existence, though this may be driven by differential exit rates. At their closest (5-10 years), the self-taught are still 10 percent less profitable than the learned businesses. Other measures show similar patterns of self-taught operating at a lower scale than those who learned from others. Figure 3b shows that the self-taught are less likely to have employees and pay a smaller total wage bill.

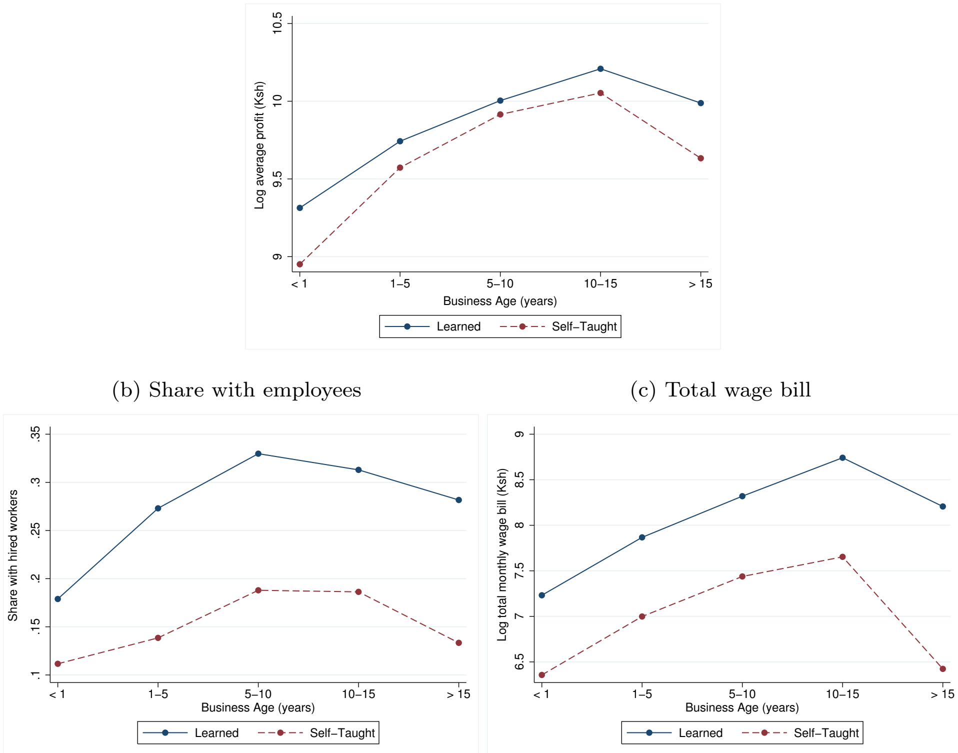
[3]The answers do not add to one because individuals were able to choose more than one option.
[4]Total employment looks extremely similar to Figure 3b given that those that do have workers have so few.

Figure 3: Business scale differences over the lifecycle

(a) Average profit

(b) Share with employees

(c) Total wage bill



3 Experimental Design

Our sample is derived from the baseline survey. 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 included 1094 business owners, 787 of whom were women. Since 72 percent were women, we further restricted attention to female-run businesses as to limit heterogeneity in the sample. Out of these 787 women, we contacted 723 to participate in the study after dropping some with particularly severe missing baseline data or extreme outliers in the baseline. 538 (68%) accepted our invitation to participate in the program. We set up relatively strict participation requirements due to the numerous follow-up surveys expected, and in particular required attendance of an in-person orientation. Of the 538 individuals, 378 showed up at orientation (70% of 538, or 52% of the original 723). Randomization took place among these 538 individuals, but no one was given any indication of their assigned group until arrival at orientation. The control group received a cash payment of 4800 Ksh (48 USD) to encourage participation, which is equal to approximately two weeks of average profit. The class group received an identical cash payment along with one month of business classes. The mentor group received the cash payment in addition to a mentor drawn from local successful business owners. Of the original 378 individuals contacted, 369 business owners answered at least one post-treatment survey.

The business classes were conducted by faculty from Strathmore University, a leading university in Kenya that is located in Nairobi. The classes were not designed specifically for this project, but have been used as part of a small and medium size business outreach program by the Strathmore University School of Management and Commerce. The curriculum was therefore based on what they believed to be the best available topics and information to cover. Moreover, all of the instructors had taught the class numerous times before, and were therefore well prepared and comfortable with the curriculum. The treatment consisted of four two hour classes that broadly covered marketing, accounting, cost structure and inventory management, and the creation and development of business plans. These topics are similar to programs used in other studies.[5] Classes were offered at a local hall in Dandora, and were offered at multiple days and times throughout the week to accommodate individual schedules. While each of the four class topics had a separate instructor, the same instructor presided over all sections of each class topic.

Individuals selected into the mentor treatment were matched with a mentor drawn from a set of successful local business owners (mentor selection is detailed in the next section). Once the pool of mentors was chosen, mentees were matched based on narrowly defined business sectors. For example, we match perishable food sellers with other perishable food sellers, tailors with other other tailors, and so on. Conditional on business sectors matching, mentors were randomly assigned. Mentees were asked to meet with the mentor each week at the mentor's business. This was designed to minimize the cost to the mentors, and also to match the fact that the class treatment required time away from the business. For further comparison with the class treatment, they were asked to meet weekly for four weeks. The meetings, however, were relatively unstructured. We put no constraint on minimum meeting time nor the topics that must necessarily be discussed. However, they were given prompts, including "What were some of the challenges the mentee faced this week?" and "What should the mentee change this week?"

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

[5] Anticipating the results somewhat, we find similar results to previous formal training research using other training programs, suggesting that there is nothing specific about our class design that generates our results.

middle of December 2014 (preceding the Christmas holiday) and then in the last week of January, February, March, June, and November of 2015. 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 $t \in \{1, 2, 3, 4, 7, 12\}$ will reference December, January, February, March, June, and November.

3.1 Mentor Selection

The pool of mentors was selected from our baseline survey. We first constrained our search to female business owners who were over 35 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

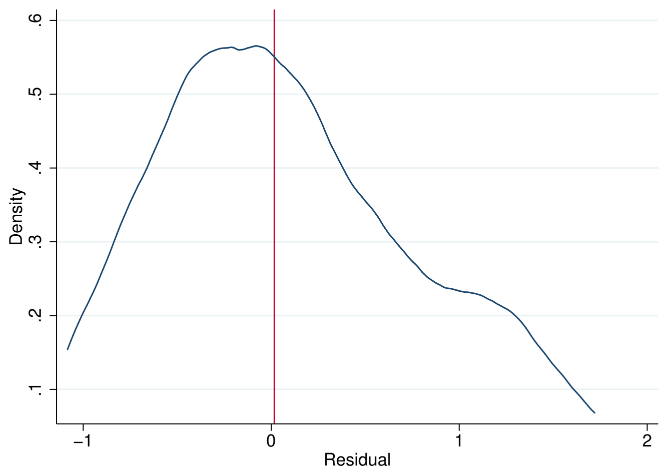
$$\log(\pi_i) = \alpha + \beta Sector_i + \gamma \log(age)_i + \varepsilon_i \tag{3.1}$$

where π_i is baseline profit for individual i, $Sector_i$ 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 estimated error terms $\widehat{\varepsilon}_i$. That is, once we account for sectoral and age differences, these are the female business owners that have the highest residual profit. These sector-specific estimates turn out to be small and statistically insignificant, as the correlation between log baseline profit and $\widehat{\varepsilon}_i$ is 0.98. From there, we simply count up the number of business owners until we have enough to link each mentee to a mentor that is in the same tightly defined business sector. Figure 4 plots the distribution of $\widehat{\varepsilon}$ along with the cut-off.
As expected, Table 4 shows that mentors run substantially more successful businesses, earning about 4 times higher profit than those not chosen as mentors. Moreover, their businesses have been in operation for almost twice as long, and they are nearly three times as likely to have employees.

3.2 Sample Size and Balance across Survey Waves

Followup surveys were conducted over the phone, and therefore not all individuals answered every survey. Of the 378 individuals who agreed to participate, 372 (98%) answered at least one followup. The response rates by wave were 352 (93% of 378), 318 (84%), 319 (84%), 323 (85%), 325 (86%), so that after the first followup the response rate leveled off at 85

Figure 4: Distribution of $\widehat{\varepsilon}$ and cut-off



percent. In terms of number of followups completed, 4 individuals completed exactly one followup, 6 completed two, 21 completed three, 41 completed four, and 108 completed five, and 194 completed all six. All told, 81% completed at least five of six follow up surveys. In Appendix B 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 27 of Appendix B. A few observables are correlated with answering surveys at the 5 percent level, though none at 1 percent. However, the differences are small. Manufacturing business owners answer 5.8 surveys on average, compared to 5.2 for the rest. Restaurants answer 5.0 surveys, compared to 5.3 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 5.1 surveys on average, compared to 5.3 in the top 25 percent of the age distribution. Later results show that there is little difference in estimation results with or without controlling for baseline factors.

3.3 Take Up of Treatments

Attendance at the business class was encouraged, but not mandatory to receive payment. Consistent with other training studies, attendance was therefore not perfect. However, 72 percent of the class treatment attended at least three of the four 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. Note that these numbers refer only to the extensive margin without any claim on the intensity of use, as we put no restrictions on meeting time or length.

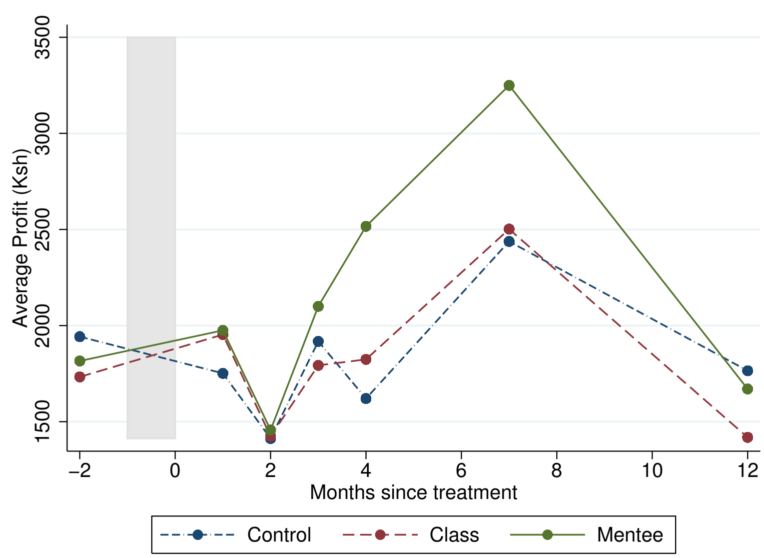
4 Empirical Results

We begin by considering business profitability and scale in Section 4.1. We find that mentorship increases average profit relative to control, while in-class training has no statistical effect. Moreover, we can reject the hypothesis that the class effect is weakly higher than the mentorship effect. In Section 4.3 we investigate the underlying channels driving this result, and find that the key difference lies in inventory and cost management. Mentees are more likely to switch suppliers, have lower production costs, and spend more on inventory. In Section 4.4 we focus on business practices that were discussed in the class setting, and indeed find that some business practices change among the class treatment.

4.1 Profitability

We begin by looking at the effect on the previous week's profit. Figure 5 plots the time series of average weekly profit by treatment arm.

Figure 5: Profit time series



The control group mimics the class group closely throughout the study period, and actually sees a slight decrease relative to control by month 12.[6] The mentee group, however, sees a substantial growth in profit relative to both the control and class that we first pick up in month four and lasts through month seven. However, this effect fades out by our twelve month followup. To assess the impact of our interventions more seriously, we run a series of regressions. First, we pool the data and run the following regression to measure the average treatment effect

$$y_{it} = \alpha + \beta M_{it} + \gamma C_{it} + \nu X_i + \theta_t + \epsilon_{it}. \tag{4.1}$$

Here, y_{it} is the outcome for individual i at time $t \in \{1, 2, 3, 4, 7, 12\}$ months since the treatment. $M_{it} = 1$ if i is in the mentor group at time t, and $C_{it} = 1$ if i is in the class group at time t. X_i is a vector of baseline fixed effects including secondary education, log age, and business sector fixed effects, and 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 run wave-by-wave regressions

$$y_{it} = \alpha_t + \beta_t M_{it} + \gamma_t C_{it} + \nu_t X_i + \varepsilon_{it} \quad for\ t \geq 1 \tag{4.2}$$

Table 6 begins by considering the impact on business profit. On average during this time period, their profit is 419 Ksh higher than the control group, which is nearly 25 percent of baseline mean. The result is robust to including controls. The class group, on the other hand, is nearly identical to the control group and cannot be statistically distinguished from the control. Furthermore, the one tailed t-test shows that the effect of mentorship larger than that of the in-class training. Looking at the time series of profit across the three groups, the average results are clearly driven by a large increase that begins 4 months post-treatment. Looking back on Figure 5, this is the rebound after January 2015. In March 2015 (4 months post-treatment), profit of the mentees is 911 Ksh more than control compared to 94 Ksh more in the class treatment. This result remains into July 2015 (7 months post-treatment), as profit is 32 percent higher (811.96 Ksh) among mentees. A one tailed t-test again implies that in both March and July, we can reject that the class effect is weakly larger than the the mentorship effect.[7] 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

[6] 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.

[7] It is important to note that these results are certainly not the last statement on in-class training, as we are subject to the criticism levied in McKenzie and Woodruff (2014) on power requirements in training experiments. Hence, we wish to emphasize the importance of mentorship *relative* to in-class training.

in profitability in any period. However, the effect fades over time, and we return to this theme in Section 4.2.

In addition to increasing the mean of profit, mentorship also increases variance, so that some individuals gain more than others. One possibility that arises in models of ability diffusion is that those with better mentors should see a larger treatment impact, as is the case in work by Jovanovic and Rob (1989) and Lucas (2009) among others. We test this with the regression

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

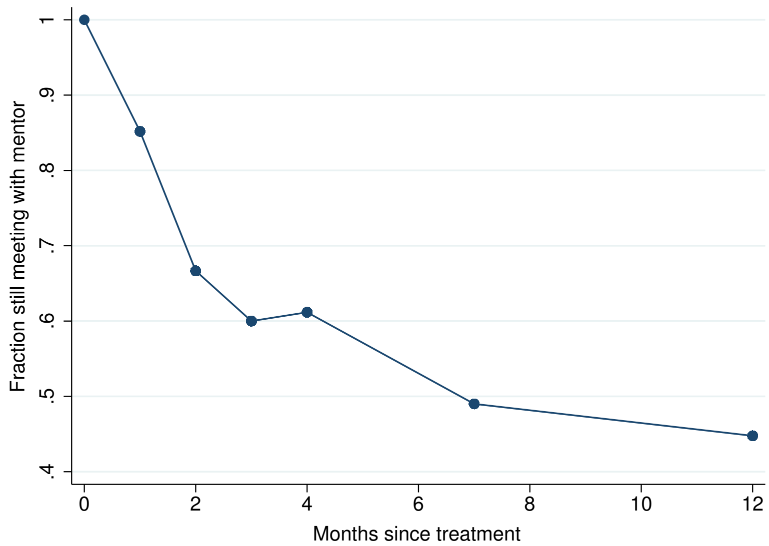
where $M_{1it} = 1$ if i has a mentor from the bottom 25 percent of the baseline mentor profit distribution, $M_{2it} = 1$ if i's mentor is in the 25th to 75th percentile, and $M_{3it} = 1$ if i's mentor is in the top 25 percent. The results are in Table 7. On average, having a mentor from the top 25 percent implies 11 percent higher profit relative to the bottom 25 percent. The coefficient estimates are increasing, consistent with the importance of the mentor's profit, but cannot be statistically distinguished from either other. Focusing on the periods in which mentorship has a positive average impact, having a highly profitable mentor consistently implies a larger treatment impact, though again, the estimates are too imprecise to distinguish with a t-test.

4.2 Understanding the Dynamics of Profitability

Figure 5 shows that the average treatment effect is only positive in the short run, and one year after the treatment all three groups have similar average profit realizations. We next turn to understanding the dynamics of the mentorship effect. Figure 6 begins by plotting the fraction of mentees that were meeting with their mentor over the course of the study. As mentioned previously, everyone met with their mentor in the official treatment month. While this fraction declines over time, 45 percent were still meeting after twelve months despite the fact that we provided no incentives to continue the relationship.

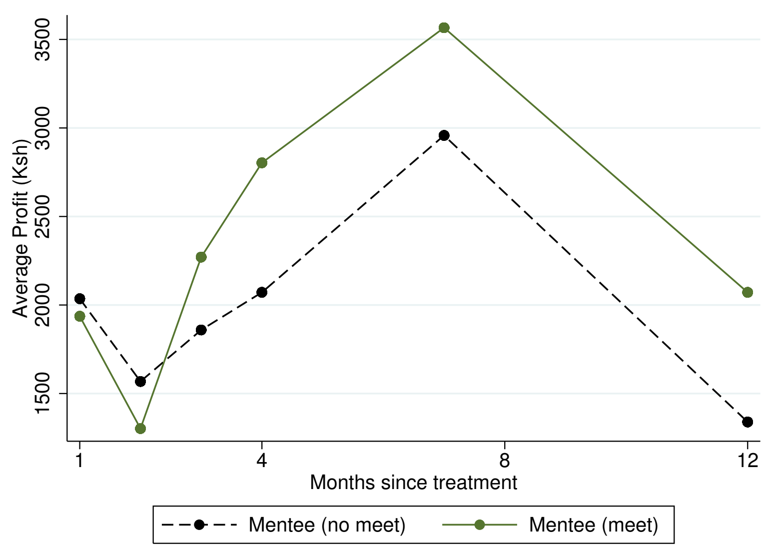
A revealed preference argument then implies that at least the 45 percent of mentees still meeting were receiving some benefit from the mentorship program. The data support this hypothesis. Twelve months after the treatment, average profit for those still meeting with their mentor is 2071.38 compared to 1339.47 among those not meeting - a difference of 55 percent (and statistically significant at 0.05). This result is not specific to the final wave as can be seen in Figure 7, though it is largest in that wave. Four months after the treatment, profit is 35 percent higher ($p = 0.16$) for those still meeting with their mentor, and is 22

Figure 6: Fraction of mentees still meeting with mentor



percent higher seven months after ($p = 0.23$), though it is worth emphasizing that the results are not precisely estimated enough to statistically distinguish the difference from zero in the relatively small sample.

Figure 7: Average Profit for Mentees



There are at least two possible explanations for the profit gap between these two groups that have different implications for the underlying cause of the difference. The first is that mentees begin the mentorship program and then end the relationship when the benefits are sufficiently low. The second is that mentors end the relationship despite the fact that there still are potential benefits to the mentees. The evidence points toward the latter. First, in the last survey round, we asked mentees directly why they were no longer meeting with their mentors. As shown in Table 8, nearly 70 percent of those not meeting claimed it was due to the mentor ending the relationship. Second, if mentees are experimenting with mentorship and mentors are willing to continue the relationship, we should see likelihood of meeting respond to profit realizations. We therefore ask whether changes in profitability affect the likelihood of meeting with a mentor in the future with the regressions

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

run on just the mentees. The variable $Meet_t = 1$ if the mentee is still meeting with her mentor and $\Delta Meet_t = Meet_t - Meet_{t-1}$, and $\Delta \pi_{i,t-1} = \pi_{i,t-1} - \pi_{i,t-2}$. If mentees are responding to changes in profit, we would expect to see $\widehat{\beta} > 0$. The results are presented in Table 9, and we find no evidence that meeting likelihood is responding to mentee profit realizations. 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.

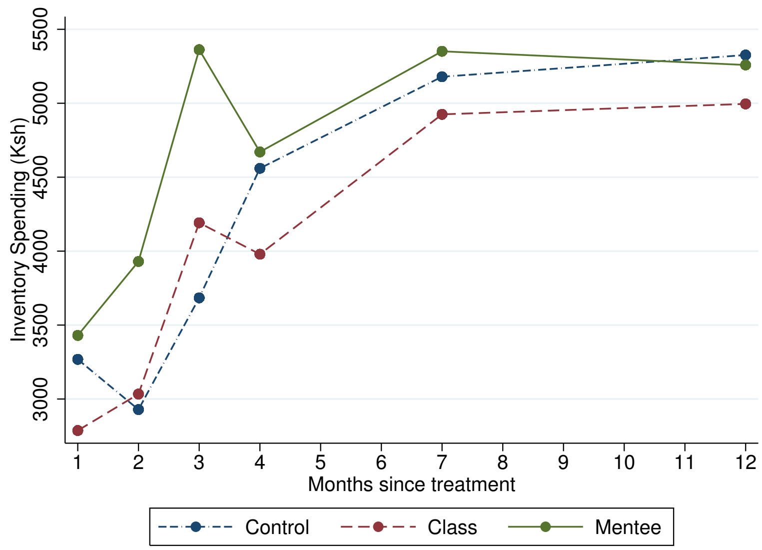
4.3 Why Does Profitability Change? Mentorship and Local Knowledge

Since profit increases, we next turn to understanding why. Table 10 uses regressions (4.1) and (4.2) to study inventory spending across the three groups. On average over the year, mentees spend 15 to 20 percent more (depending on controls in the regression) on inventory than the control, while the class spends a (statistically insignificant) 2 percent more. We can again reject the hypothesis that the class group spends weakly more than the mentorship group. Howoever, the average effect masks interesting timing of the change relative to the change in profit, as can be seen in Figure 8.[8]

Inventory spikes for mentees at $t = 2$ and $t = 3$. Put differently, the largest treatment effects are immediately preceding the increase in profit seen in Figure 5 (and Table 6). Two months after the treatment, mentees spend 1028.62 Ksh (or 35 percent) more on inventory relative to the control, compared to a statistically insignificant 267.11 Ksh (9 percent) more

[8] In followup surveys, we asked about inventory spending in the previous week. In the baseline survey, we asked about inventory spending the last time owners went to the market. Hence, we exclude the pre-treatment values in Figure 8.

Figure 8: Inventory spending time series



in the class treatment. This continues into the next month, in which mentees spend 1742.91 (47 percent) more on inventory. The class treatment spends 20 percent more on inventory this month, but cannot be statistically distinguished from the control. Regardless, in both months we can reject the hypothesis that the class group spends weakly more on inventory in these months, so that the relative effect is still stronger among the mentees.

To get a more complete understanding of changes in scale, we asked more detailed questions in our 7 and 12 month post-treatment surveys. The results from those regressions are presented in Table 11. We find no evidence of any business scale changes other than the value of inventory stock. After 7 months, the point estimates imply that value of inventory stock is 39 percent higher among mentees (13,356 Ksh compared to 9,617 in the control), and only 0.5 percent higher among in-class trainees, consistent with our evidence on inventory spending over the post-treatment period. While we cannot reject equality to the control for either treatment (though the mentorship impact just barely misses), we can reject the hypothesis that the average impact among trainees is weakly higher than the average impact of mentorship. The other columns of Table 11 show no other differences in scale across any other metric or time period. Hours of operation and employment are nearly identical across all three groups. We find some evidence of higher wage bills among mentees at $t = 7$, but the results are imprecise with only 24 non-zero estimates.

So far, we have shown that inventory spending and profit increase among the mentees, with little other change in business scale. The obvious next question then is to understand why inventory increases. To do so, we assess underlying changes in suppliers and prices. In the July 2015 survey ($t = 7$), we asked whether individuals had switched suppliers at any point since the start of the study. We also asked about the sale price for their main product (in January and in July) and the total cost paid to suppliers to create that good or service. Table 12 shows the differential treatment impact across these pricing and supplier channels.

The first result is that mentees are much more likely to have switched suppliers. While there is high churn in suppliers among all groups (57 percent in the control), 89 percent of mentees changed suppliers post-treatment, compared to 61 percent of those receiving in-class training. Switching suppliers comes with the ability to lower production costs in their businesses. One month after the treatment, the mentees spend only half of what the control group requires to produce its main product. In-class trainees see a large drop in cost as well, though it cannot be distinguished statistically from the control group. Seven months after the treatment, however, the mentees still pay roughly half of the control. This we can distinguish from the class treatment through a one tailed t-test. Interestingly, however, there is no passthrough to consumers. Sale prices of the main product are similar across all three groups, despite the fact that cost decreases most strongly among the mentees. The mentees therefore are able to find new suppliers who provide lower costs, and thus allow profit to increase.

4.4 "Generic" Business Practices

So far, the argument put forward is that mentorship provides access to local information that is not available in training classes. However, when we focus on behavior that was taught in training classes, we do see some change in behavior among the trainees.

In every survey, we asked about accounting and advertising practices. Table 13 is accounting and marketing time series. The accounting variable is equal to one if the individual reports keeping track of sales, costs, or inventory. The marketing outcome is equal to one if the owner reports doing any advertising or marketing to attract new business. In Panel A, two results emerge. First, marketing practices do not change relative to the control for either treatment. In general, Table 13 finds small point estimates that cannot be distinguished statistically from the control. However, accounting practices do exhibit changes across the treatments. First, the fraction of business owners who do some sort of formal record keeping is significantly larger than either the control or mentees on average. 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 et al., 2014), and implies that the in-class training does in fact change behavior without changing business outcomes. We also see some evidence the mentees change their record keeping practices, though the effect is weaker. Table 14 further breaks down the results by mentees whose mentors do some sort of formal record keeping and those who do not. All of the mentee treatment effects seen in Table 13 are driven by mentees whose mentors use formal bookkeeping methods, suggesting that mentors are indeed transmitting their their own information and experience to their mentees.[9]

To better understand business practices in finer detail, 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 et al. (2014) and, as shown in McKenzie and Woodruff (2015) correlate with profit in a number of countries. Table 15 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 when disaggregating each score. These three components are further broken down into detailed business practices in Tables 16, 17, and 18.

We find an increase in our measure of business practices at $t = 7$ but not at $t = 12$, consistent with the results on profitability in Section 4.1. Breaking the results down into its underlying components, Table 16 shows that if anything, the class treatment is actually do less marketing than the control at $t = 7$, those these results flip at $t = 12$. The point estimates of practices underlying the marketing score and significantly smaller across all groups, and cannot be distinguished from the control in any case.

Table 18 decomposes the record keeping score. In terms of recording sales and consulting records, neither treatment has a substantial impact at $t = 7$. However, we do find that the mentorship group is 14 percentage points more likely to budget costs, compared to a 4 percentage point increase in the class. This again highlights the role of cost and supplier management for understanding the impact of mentorship. At $t = 12$, the mentorship group actually is *less* likely than the control to budget costs, consistent with the lack of profit

[9] A similar exercise for marketing and advertising (Panel B in Table 13) finds no difference between mentors who advertise and those who do not. Results are available upon request.

impact as well.

In terms of *Stock score*, both treatments see a large decrease in their likelihood of running out of inventory at $t = 7$. This could in part be due to better record management found in previous periods (Table 13). This again points to the fact that in-class training does have positive impact on business practices and some outcomes, but the transmission to profit is relatively small. Furthermore, we find that mentees are more likely to haggle with suppliers and compare suppliers relative to the control, though we cannot statistically distinguish the impact of the two treatments along these margins. 76 percent of mentees are likely to compare suppliers, compared to 61 percent in the control and 71 in the class treatment. Again, the results are consistent with the idea that mentorship primarily generates benefits through supplier and cost management, and not necessarily through marketing or tracking sales. Again, none of these margins are active at $t = 12$.

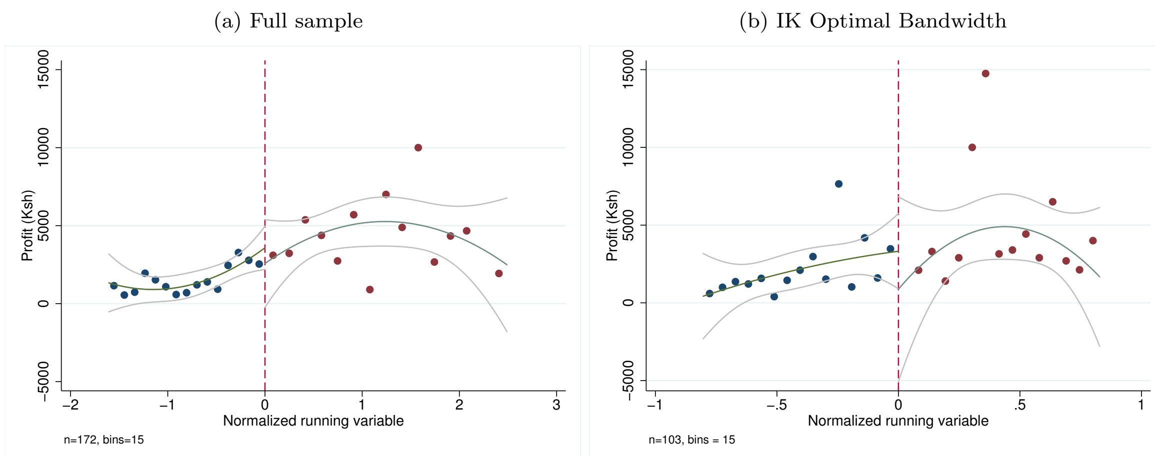
In summary, we find some changes in behavior and outcomes among those who receive in-class training, but little change in profit. We also see changes in behavior among the mentees, and they are broadly consistent with a focus on cost and supplier prices.

5 Impact on Mentors

In the previous section we provided evidence that mentors use their knowledge of business to change the practices of their mentees, thus suggesting that information is being transferred from mentors to mentees. A second question is whether the reverse is true - does having a mentee impact mentors? This is key to understanding whether the intervention generates surplus, or whether it simply generates reallocation across production units. Most recent theoretical and quantitative work assumes the former. That is, information (broadly defined) flows from those with higher to lower ability, but has no effect on the higher ability individual. For example, Jovanovic and Rob (1989), Lucas (2009), Lucas and Moll (2014), and Buera and Oberfield (2015) all assume learning functions with this underlying assumption.

In our context, this suggests that mentors should see no impact of being chosen as a mentor, despite the impact on the mentees. However, we choose mentors because of their underlying entrepreneurial talent, which eliminates a direct comparison between mentors and non-mentors. We overcome this issue with a regression discontinuity design that exploits our mentor selection procedure. In particular, we utilize the cut-off in Figure 4 and survey a number of individuals just to the leftof the mentor cutoff, a procedure helped by the fact that the cutoff occurs near the mean of the distribution. Four months after the treatment, we resurveyed all mentors, along with 150 female business owners to the right of the cutoff. Ninety five of these 150 agreed to answer the survey. We then assess the impact of being chosen as a mentor on profit. For preliminary evidence that mentorship has no impact on the mentors, Figure 9 plots profit along with a fitted quadratic and its 95 percent confidence interval. Figure 9a uses the entire sample, while Figure 9b uses the Imbens and Kalyanaraman (2012) procedure to choose the optimal bandwidth. Both use 15 bins on either side of the cutoff.

Figure 9: Profit for mentors and non-mentors



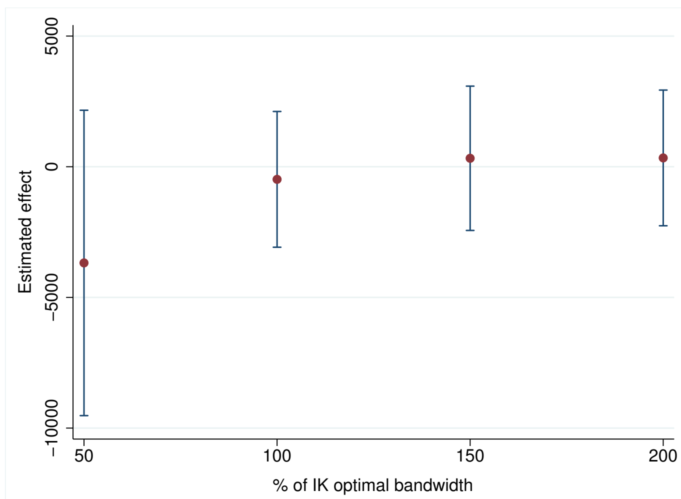
While Figure 9 suggests no discontinuity around the cutoff, we next assess this more formally. In particular, letting $\bar{\varepsilon}$ be the cut-off value for mentors derived from regression (3.1), we run the regression

$$\pi_i = \alpha + \tau D_i + f(N_i) + \nu_i \tag{5.1}$$

where π_i is profit, $D_i = 1$ if individual i was chosen as a mentor ($\widehat{\varepsilon}_i \geq \bar{\varepsilon}$ in regression 3.1), $f(N_i)$ 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. The function $f(\cdot)$ is allowed to vary on both sides of the cutoff, and we vary the functional form to limit concerns of sensitivity to assumed functional forms. In particular, we assume f is linear and second order polynomial, as Gelman and Imbens (2014) argue against using higher order polynomials in regression discontinuity designs. We also use local linear regressions, and vary the bandwidth. Table 19 shows the estimated values of τ for weekly profit (trimmed the top and bottom one percent) four months after the treatment under different choices of bandwidth for linear and quadratic forms of f.

At reasonable bandwidth choices, none of the estimates are statistical different from zero. Next, we use local linear regressions to estimate the same treatment effects, the results of which are in Table 20. Again, there is no evidence that mentors benefit from being mentors. Figure 10 graphically shows the point estimate of the treatment effect and the 95 percent confidence interval at 50, 100, 150, and 200% of the IK optimal bandwidth. As when f was assumed linear, an overly restrictive bandwidth predicts a large negative treatment impact, though insignificant here. However, this immediately disappears at reasonable bandwidth choices.

Figure 10: RD treatment estimates with local linear regressions



We further consider whether being chosen as a mentor has an effect on inventory spending, marketing or record keeping. Results from the RD with local linear regressions are in Table 20. There is no change in marketing or record keeping practices. We do see 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.

6 Conclusion

We conduct a randomized controlled trial in which we assess the impact of learning from a successful local business owner. Our results show that mentorship generates a persistent 30 percent increase in profit. Mentees increase inventory spending, are more likely to switch suppliers, and have lower costs of production than the control, while the class treatment looks statistically similar to the control along these dimensions. Taken together, this points to the importance of local information about suppliers and cost that mentors have from years of successful experience in the same local market. This also implies a rationale for the lack of success of formal training classes (at least in terms of higher profit). Training is designed to be replicable, and therefore does not focus on the specifics of information we have shown to be important. This intervention is also extremely cost-effective, due both to a larger treatment impact on profit and also the relatively low cost of operating such an intervention. The expensive aspects of in-class training (instructors and space) are replaced with mentors and their business, respectively.

Our results also provide some micro evidence in support of a recent class of macroeconomic models, pioneered by Jovanovic and Rob (1989) and recently utilized by Lucas (2009), Lucas and Moll (2014), and Buera and Oberfield (2015), in which economic growth is a result of information diffusion among economic agents. We show that indeed this type of learning process has potential to generate firm growth, albeit in a specialized setting. We further find that two features of the model have at least some support - mentor profits are positively related to the treatment effect, and mentors see no effect (positive or negative) in response to their interaction with mentees. The experimental design has the ability to bring substantial structure to the parameters that govern dynamics and aggregate implications in these models. We plan to pursue this avenue in future research.

Lastly, the work presented here presents a number of potential extensions. We discuss two here. While recent work (BenYishay and Mobarak, 2014; Beaman et al., 2015) shows that technology adoption can be generated through existing networks, we show that there is profitable information outside these the existing networks of young business owners. In particular, optimal policy might entail deciding not just how to deliver a treatment to key players in a network, but also actively decide which links to form or what kind of link to promote. Moreover, our results point to the importance of continued interaction with profitable business owners. Taken together, this research implies that understand the dynamics of network formation is an important next step. Second, our experiment purposely restricts attention to a small portion of the experience profile of businesses. To the extent that information can be learned over time, the effect will be smaller among more experienced business owners (abstracting from spillover or equilibrium effects). Moreover, the relative importance of local knowledge may potentially differ across observable characteristics of businesses. Bloom et al. (2013), for example, finds that consulting services increase productivity among larger textile firms in India. We find no evidence of such effects here, but a more robust analysis of these ideas across a wider cross section of businesses would allow a more comprehensive understanding of business-to-business learning.

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Appendices

A Main Tables

Table 1: Baseline Characteristics

	Overall (3290)	Young Firms (1405)
Firm Scale:		
Profit (last month)	16,899	14,226
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: Baseline Business Knowledge

	Overall (3292)	Young Firms (1405)
Watching family members	0.26	0.25
Watching non-family members	0.11	0.09
In school	0.10	0.09
Worked for business or apprenticed	0.12	0.12
Self-taught	0.55	0.58

Table 3: Differences among the Self-Taught

	Overall (learned)	Overall (self-taught)	Young firms (learned)	Young firms (self-taught)
Firm Scale:				
Profit (last month)	18,803	14,963	15,907	12,778
Firm Age	6.3	4.9	2.28	1.93
Has Employees?	0.27	0.14	0.24	0.12
Number of Emp (if $n > 0$)	2.1	1.5	1.5	1.4
Business Practices:				
Offer credit	0.65	0.69	0.66	0.72
Have bank account	0.41	0.30	0.10	0.14
Taken loan	0.23	0.19	0.17	0.14
Practice accounting	0.03	0.01	0.02	0.01
Advertise	0.11	0.08	0.10	0.09
Owner:				
Age	33.8	34.1	28.8	28.9
Female	0.58	0.74	0.64	0.78
Secondary Education	0.62	0.54	0.62	0.54

Table 4: Mentor vs. non-mentor baseline characteristics

	Mentors (182)	Non-mentors (184)
Firm Scale:		
Profit (last month)	20,205	5,967
Firm Age	23.5	13.0
Has Employees?	0.30	0.11
Number of Emp (if $n > 0$)	1.9	1.4
Business Practices:		
Offer credit	0.65	0.74
Have bank account	0.48	0.32
Taken loan	0.45	0.26
Practice accounting	0.11	0.10
Advertise	0.08	0.08
Owner:		
Age	42.8	43.6
Secondary Education	0.58	0.49

Table 5: Initial Balance Test

	Control (119)	Class (135)	Mentor (124)
Firm Scale:			
Profit (last month)	11403	11821	10416
Firm Age	2.5	2.6	2.4
Has Employees?	0.13	0.15	0.18
Number of Emp (if $n > 0$)	1.3	1.9	1.4
Business Practices:			
Offer credit	0.72	0.73	0.74
Have bank account	0.31	0.29	0.30
Taken loan	0.16	0.11	0.10
Practice accounting	0.11	0.08	0.10
Advertise	0.12	0.07	0.12
Sector:			
Manufacturing	0.04	0.06	0.02
Retail	0.63	0.52	0.62
Restaurant	0.15	0.19	0.14
Other services	0.23	0.26	0.23
Owner Characteristics:			
Age	28.7	29.6	28.7
Secondary Education	0.55	0.51	0.55
Self-taught	0.48	0.48	0.50

Table 6: Profit

Panel A: No controls		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	339.45	223.55	44.02	182.34	895.99	811.96	-94.60
	(133.10)**	(204.06)	(207.78)	(277.02)	(277.11)***	(331.86)**	(216.82)
Class	3.89	201.51	11.59	-124.82	203.73	63.91	-346.43
	(143.72)	(199.85)	(202.05)	(267.02)	(271.11)	(325.75)	(213.81)
Constant	1783.57	1751.54	1412.42	1917.86	1620.28	2473.84	1764.84
	(109.06)***	(143.97)***	(145.08)***	(193.94)***	(194.32)***	(236.44)***	(152.58)***
One tailed t-test p value	0.013	0.465	0.437	0.128	0.034	0.011	0.121
Obs.	1927	350	315	317	320	305	320
R^2	0.052	0.004	0.000	0.005	0.006	0.024	0.009
Controls	N	N	N	N	N	N	N
Panel B: Include controls		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	357.56	209.60	34.08	203.08	933.68	879.93	-66.43
	(136.50)***	(205.19)	(211.20)	(276.13)	(278.87)***	(336.33)***	(216.87)
Class	35.48	170.50	53.86	-22.14	255.19	103.42	-282.65
	(147.34)	(201.85)	(205.24)	(267.41)	(274.60)	(329.26)	(216.39)
One tailed t-test p value	0.022	0.424	0.538	0.204	0.008	0.009	0.162
Obs.	1923	349	314	316	319	305	320
R^2	0.063	0.033	0.016	0.051	0.063	0.045	0.042
Controls	Y	Y	Y	Y	Y	Y	Y

Table notes: Standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. Controls in panel B include secondary education, log age of owner, and sector fixed effects. 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, ***. One person did not answer for age, so she is dropped in panel B.

Table 7: Heterogeneous Mentor Effects for Profit

		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee: mentor in $(0, 25)$	289.73	270.00	-87.74	18.26	662.54	915.76	-38.69
	(175.21)	(257.56)	(259.04)	(341.65)	(353.46)*	(405.71)**	(273.13)
Mentee: mentor in $(25, 75)$	337.12	304.24	101.07	522.68	983.29	469.76	-325.55
	(180.56)*	(257.57)	(276.15)	(377.33)	(365.01)***	(454.80)	(286.14)
Mentee: mentor in $(75, 100)$	521.77	-154.35	380.91	-167.86	1401.15	1322.88	406.59
	(259.24)**	(410.90)	(452.13)	(579.45)	(569.28)***	(666.26)**	(446.32)
Class	3.89	201.51	11.59	-124.82	203.73	63.91	-346.43
	(143.72)	(199.85)	(202.05)	(267.02)	(271.11)	(325.75)	(213.81)
Constant	1783.57	1751.54	1412.42	1917.86	1620.28	2473.84	1764.84
	(109.06)***	(143.97)***	(145.08)***	(193.94)***	(194.32)***	(236.44)***	(152.58)***
One tailed t-test p value ($\text{H} \leq \text{L}$)	0.210	0.832	0.164	0.619	0.113	0.282	0.176
One tailed t-test p value ($\text{H} \leq \text{M}$)	0.263	0.847	0.283	0.861	0.250	0.123	0.066
Obs.	1927	350	315	317	318	305	320
R^2	0.052	0.007	0.003	0.010	0.040	0.029	0.016
Controls	N	N	N	N	N	N	N

Table notes: Standard errors are in parentheses. Standard errors for pooled regressions are clustered at individual level and include wave fixed effects. 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 one tailed t-test here on the null that the highest mentor group (row 3) is weakly less than the lowest (row 1). The second null is whether the highest group (row 3) is weakly less than the middle (row 2).

Table 8: Reason for Ending the Relationship

Reason	Number	Pct. of total
Mentor ended the relationship	40	31.0
Mentee ended the relationship	18	69.0
Total	58	100.0

Table 9: Meeeting and Previous Profit Realizations

Panel A: Meet$_t$				
	Meet$_t$	Meet$_t$	Meet$_t$	Meet$_t$
$\log \pi_{t-1} - \log \pi_{t-2}$	0.011	0.008	–	–
	(0.022)	(0.023)		
$\log \pi_{t-1} - \log \pi_0$	–	–	0.029	0.033
			(0.021)	(0.022)
Constant	0.564	0.648	0.559	0.643
	(0.024)***	(0.06)***	(0.025)***	(0.063)***
Obs.	373	373	383	383
R^2	0.001	0.014	0.004	0.023
Wave F.E.	N	Y	N	Y

Panel B: Meet$_t$ - Meet$_{t-1}$				
	Meet$_t$ - Meet$_{t-1}$	Meet$_t$ - Meet$_{t-1}$	Meet$_t$ - Meet$_{t-1}$	Meet$_t$ - Meet$_{t-1}$
$\log \pi_{t-1} - \log \pi_{t-2}$	0.027	0.029	–	–
	(0.031)	(0.033)		
$\log \pi_{t-1} - \log \pi_0$	–	–	0.016	0.018
			(0.021)	(0.021)
Constant	-0.055	-0.210	-0.061	-0.208
	(0.022)**	(0.084)**	(0.021)***	(0.083)***
Obs.	328	328	338	312
R^2	0.002	0.014	0.001	0.016
Wave F.E.	N	Y	N	Y

Table notes: Standard errors are in parentheses, and are clustered at individual level. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Profit is trimmed at 1% before taking differences. The variable $Meet_t = 1$ if an individual has met with their mentor in period t.

Table 10: Inventory Spending

Panel A: No controls		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	498.04	162.04	1001.47	1678.11	110.85	171.99	-67.38
	(393.22)	(483.82)	(565.56)*	(787.20)**	(770.07)	(910.11)	(1131.65)
Class	-180.45	-481.17	104.81	507.42	-580.61	-254.31	-339.53
	(427.21)	(473.89)	(594.83)	(760.46)	(746.50)	(887.55)	(1113.16)
Constant	3053.33	3268.11	2928.63	3684.22	4559.86	5179.31	5326.17
	(295.30)***	(342.12)***	(396.92)***	(553.46)***	(533.85)***	(650.15)***	(792.54)***
One tailed t-test p value	0.063	0.088	0.053	0.064	0.182	0.314	0.408
Obs.	1918	349	312	315	318	304	320
R^2	0.022	0.006	0.012	0.015	0.003	0.001	0.003
Controls	N	N	N	N	N	N	N
Panel B: Include controls		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	657.97	248.66	1028.62	1742.91	133.46	635.80	165.42
	(386.58)*	(485.84)	(563.780)*	(788.05)**	(762.50)	(893.46)	(1116.26)
Class	43.05	-429.64	267.11	720.06	-542.87	200.26	166.40
	(409.47)	(478.77)	(548.43)	(766.53)	(745.38)	(865.88)	(1111.35)
One tailed t-test p value	0.073	0.080	0.087	0.094	0.187	0.307	0.500
Obs.	1918	349	312	315	318	304	320
R^2	0.059	0.036	0.061	0.052	0.062	0.088	0.061
Controls	Y	Y	Y	Y	Y	Y	Y

Table notes: Standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Controls include secondary education, log age of owner, and sector fixed effects. 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, ***.

Table 11: Business scale

Panel A: $t = 7$	Stock of inventory (Ksh)	Any employees?	Number of employees	Total wage bill (Ksh)	Hours open (last week)
Mentee	3738.98	-0.00	0.02	555.48	0.20
	(2336.21)	(0.03)	(0.06)	(413.80)	(3.17)
Class	52.38	0.01	-0.02	284.65	-0.90
	(1931.79)	(0.03)	(0.08)	(295.85)	(2.87)
Constant	9617.02	0.05	0.08	309.90	52.13
	(1327.21)***	(0.02)**	(0.04)**	(165.18)*	(2.01)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.061	0.671	0.248	0.275	0.365
Obs.	303	308	307	315	304
R^2	0.012	0.000	0.001	0.001	0.001

Panel B: $t = 12$	Stock of inventory (Ksh)	Any employees?	Number of employees	Total wage bill (Ksh)	Hours open (last week)
Mentee	-1887.85	-0.07	-0.05	-19.88	4.24
	(3811.39)	(0.05)	(0.05)	(216.60)	(3.03)
Class	-2398.64	-0.06	-0.03	-97.36	1.34
	(3201.14)	(0.05)	(0.05)	(200.74)	(3.16)
Constant	12439.55	0.20	0.11	393.52	47.05
	(3075.83)***	(0.04)***	(0.04)***	(132.55)***	(2.18)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.432	0.370	0.312	0.633	.824
Obs.	323	325	321	322	324
R^2	0.001	0.016	0.003	0.001	0.006

Table notes: Standard errors are in parentheses. Results are presented without controls, but are nearly identical when controls are added. The top and bottom one percent of dependent variables are trimmed for all dependent variables except the 0-1 employee indicator, though results are robust to other (or no) trimming procedures. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 12: Costs and Suppliers

Panel A: No controls	Switch supplier	Sale Price (months since treatment) (1)	(7)	Cost from Suppliers (months since treatment) (1)	(7)
Mentee	0.21†††	-18.46	5.63	-380.91	-318.041†
	(0.06)***	(76.82)	(71.78)	(185.13)**	(174.19)*
Class	0.04	-42.38	-24.75	-261.26	-102.99
	(0.07)	(71.53)	(63.57)	(184.03)	(193.41)
Constant	0.57	249.22	23.42	764.91	687.70
	(0.05)***	(60.84)***	(51.32)***	(158.25)***	(153.15)***
One tailed t-test p value	0.003	0.654	0.686	0.187	0.069
Obs.	315	315	315	315	315
R^2	0.038	0.002	0.001	0.019	0.012
Controls	N	N	N	N	N

Panel B: Include controls	Switch supplier	Sale Price (months since treatment) (1)	(7)	Cost from Suppliers (months since treatment) (1)	(7)
Mentee	0.21†††	-36.07	-10.32	-391.72	-334.41†
	(0.06)***	(73.76)	(69.26)	(185.14)**	(173.46)*
Class	0.04	-52.05	-33.27	-252.39	-104.47
	(0.07)	(73.35)	(65.71)	(181.27)	(190.83)
One tailed t-test p value	0.003	0.608	0.646	0.154	0.055
Obs.	315	315	315	315	315
R^2	0.059	0.080	0.082	0.038	0.033
Controls	Y	Y	Y	Y	Y

Table notes: Robust standard errors are in parentheses. Controls include secondary education, log age of owner, and sector fixed effects. 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, ***. †, ††, and † † † indicate the ability to reject the hypothesis that the mentee effect is weakly smaller (larger) than the class effect at 0.10, 0.05, 0.01 using a one tailed t test for switching suppliers (sale and inventory prices).

Table 13: Business Practice Time Series

Panel A: Record Keeping		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	0.05	-0.01	0.11	0.06	0.13	-0.02	0.10
	(0.03)*	(0.06)	(0.06)*	(0.06)	(0.07)*	(0.07)	(0.06)
Class	0.14	0.19	0.17	0.10	0.30	-0.06	0.07
	(0.03)***	(0.05)***	(0.06)***	(0.06)*	(0.06)***	(0.07)	(0.06)
Constant	0.72	0.72	0.68	0.70	0.57	0.64	0.64
	(0.03)***	(0.04)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.999	1.00	0.870	0.737	0.999	0.287	0.350
Obs.	1945	338	315	315	320	305	325
R^2	0.037	0.053	0.027	0.009	0.077	0.002	0.008

Panel B: Advertising		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	-0.02	-0.00	-0.03	0.07	-0.015	-0.02	-0.07
	(0.02)	(0.05)	(0.05)	(0.05)	(0.03)	(0.05)	(0.05)
Class	-0.03	0.00	-0.07	-0.01	0.00	-0.00	-0.09
	(0.02)	(0.20)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Constant	0.21	0.20	0.16	0.10	0.07	0.19	0.18
	(0.02)***	(0.04)***	(0.04)***	(0.03)***	(0.03)***	(0.04)***	(0.03)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.306	0.522	0.212	0.059	0.719	0.601	0.305
Obs.	1945	338	315	315	320	305	325
R^2	0.016	0.00	0.007	0.010	0.001	0.000	0.013

Table notes: Standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Results are presented without controls, but treatment impacts are nearly identical when they are included. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 14: Accounting and Mentor Effects

Panel A: Record Keeping		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee (formal)	0.08	0.03	0.13	0.07	0.21	-0.04	0.07
	(0.03)**	(0.06)	(0.07)*	(0.07)	(0.07)***	(0.08)	(0.08)
Mentee (no formal)	0.05	-0.06	0.05	0.10	0.05	0.02	0.12
	(0.04)	(0.07)	(0.07)	(0.07)	(0.08)	(0.09)	(0.08)
Class	0.13	0.18	0.17	0.10	0.30	-0.05	0.07
	(0.03)***	(0.05)***	(0.06)***	(0.06)*	(0.06)***	(0.07)	(0.06)
Constant	0.71	0.72	0.68	0.69	0.56	0.63	0.64
	(0.03)***	(0.04)***	(0.05)***	(0.04)***	(0.04)***	(0.05)***	(0.04)***
One tailed t-test p value ($H_0 : M_F \leq M_{NF}$)	0.209	0.131	0.190	0.627	0.028	0.744	0.713
Obs.	1945	352	318	319	323	308	325
R^2	0.039	0.052	0.029	0.011	0.084	0.003	0.009

Table notes: Standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Results are presented without controls, but treatment impacts are nearly identical when they are included. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 15: Business Practice Aggregate Measures

Panel A: $t = 7$		Score Components		
	Aggregate z-score	Marketing z-score	Stock z-score	Record keeping z-score
Mentee	0.38	0.17	0.58	0.16
	(0.15)**	(0.16)	(0.13)***	(0.14)
Class	0.06	-0.24	0.50	0.03
	(0.17)	(0.14)*	(0.13)***	(0.14)
One tailed t-test p value (H_0 : M $\leq$ C)	0.011	0.004	0.258	0.162
Control σ	2.31	1.51	1.04	1.75
Obs.	306	306	306	306
R^2	0.015	0.025	0.072	0.003
Controls	N	N	N	N

Panel B: $t = 12$		Score Components		
	Aggregate z-score	Marketing z-score	Stock z-score	Record keeping z-score
Mentee	-0.18	-0.21	-0.13	-0.02
	(0.14)	(0.14)	(0.14)	(0.14)
Class	0.06	0.11	-0.05	0.01
	(0.14)	(0.14)	(0.14)	(0.14)
One tailed t-test p value (H_0 : M $\leq$ C)	0.968	0.990	0.703	0.593
Control σ	2.33	1.48	0.91	1.05
Obs.	320	320	320	320
R^2	0.015	0.025	0.072	0.003
Controls	N	N	N	N

Table notes: Robust standard errors are in parentheses. Results are presented without controls, but treatment impacts are nearly identical when they are included. Scores are computed as z-scores, so mean control is zero for each measure. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 16: Marketing Practices Decomposed

Panel A: $t = 7$		Marketing Score Components				
	Marketing Score	Check competitor price	Check competitor products	Have sales	Upsell	Advertise
Mentee	0.21	0.03	0.08	0.07	0.09	-0.08
	(0.20)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)
Class	-0.29	-0.06	-0.03	-0.03	-0.07	-0.10
	(0.17)*	(0.05)	(0.05)	(0.06)	(0.07)	(0.06)
Constant	1.51	0.21	0.19	0.29	0.55	0.28
	(0.13)***	(0.04)***	(0.04)***	(0.05)***	(0.05)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.004	0.042	0.021	0.067	0.007	0.356
Obs.	306	306	306	306	306	306
R^2	0.025	0.010	0.015	0.008	0.019	0.001
Panel B: $t = 12$		Marketing Score Components				
	Marketing Score	Check competitor price	Check competitor products	Have sales	Upsell	Advertise
Mentee	-0.31	-0.14	-0.10	-0.06	0.03	-0.03
	(0.20)	(0.07)**	(0.07)	(0.06)	(0.06)	(0.06)
Class	0.16	0.08	0.08	0.00	0.11	-0.12
	(0.21)	(0.07)	(0.07)	(0.06)	(0.06)*	(0.04)**
Constant	1.55	0.41	0.43	0.25	0.21	0.22
	(0.15)***	(0.05)***	(0.05)***	(0.04)***	(0.04)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.990	0.999	0.996	0.840	0.912	0.030
Obs.	306	306	306	306	306	306
R^2	0.018	0.010	0.015	0.008	0.019	0.001

Table notes: Robust standard errors are in parentheses. Results are presented without controls, but treatment impacts are nearly identical when they are included. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Marketing score is computed by summing all its components.

Table 17: Stock Practices Decomposed

Panel A: $t = 7$		Stock Score Components		
	Stock Score	Haggle with suppliers	Compare suppliers	Run out of stock
Mentee	0.51	0.13	0.15	-0.22
	(0.12)***	(0.06)**	(0.07)**	(0.05)***
Class	0.44	0.10	0.11	-0.23
	(0.12)***	(0.06)*	(0.07)	(0.05)***
Constant	1.04	0.71	0.61	0.27
	(0.09)***	(0.05)***	(0.05)***	(0.05)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.290	0.279	0.246	0.439
Obs.	306	306	306	306
R^2	0.072	0.019	0.018	0.105
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)
Constant	0.92	0.65	0.44	0.19
	(0.09)***	(0.0.05)***	(0.05)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.702	0.439	0.837	0.430
Obs.	306	306	306	306
R^2	0.002	0.001	0.003	0.001

Table notes: Standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Results are presented without controls, but treatment impacts are nearly identical when they are included. 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 18: Record Keeping Practices Decomposed

Panel A: $t = 7$		Record Keeping Score Components		
	Record Keeping Score	Record every sale	Consult records	Budget costs
Mentee	0.21	0.04	0.03	0.14
	(0.19)	(0.07)	(0.07)	(0.07)**
Class	0.03	-0.04	0.03	0.04
	(0.18)	(0.07)	(0.07)	(0.07)
Constant	1.74	0.61	0.57	0.57
	(0.14)***	(0.05)***	(0.05)***	(0.05)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.162	0.126	0.500	0.062
Obs.	306	306	306	306
R^2	0.004	0.004	0.001	0.015

Panel B: $t = 12$		Record Keeping Score Components		
	Record Keeping Score	Record every sale	Consult records	Budget costs
Mentee	-0.02	0.10	0.03	-0.15
	(0.14)	(0.05)*	(0.07)	(0.06)**
Class	0.01	-0.01	0.00	0.01
	(0.15)	(0.06)	(0.07)	(0.07)
Constant	1.53	0.77	0.38	0.38
	(0.10)***	(0.04)***	(0.05)***	(0.05)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.593	0.126	0.334	0.994
Obs.	306	306	306	306
R^2	0.000	0.013	0.001	0.015

Table notes: Standard errors are in parentheses. Pooled regression are clustered at individual level and include wave fixed effects. Results are presented without controls, but treatment impacts are nearly identical when they are included. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Record keeping score is computed by summing all its components.

Table 19: Profit RD Treatment Effect

% of IK optimal bandwidth	Linear	Quadratic Polynomial
50	-7042.91*	10776.79
	(3585.36)	(8379.39)
100	533.12	-2439.26
	(1652.36)	(2827.28)
200	24.27	1033.65
	(1069.45)	(1744.92)
Treatment Average	4387.34	4387.34
Control Average	1791.94	1791.94

Table notes: Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***.

Table 20: RD treatment effect with local linear regressions

% of IK	Scale		Practices	
optimal bandwidth	Profit	Inventory	Marketing	Record keeping
50	-3680.61	-813.82	0.16	-0.02
	(2981.00)	(3733.72)	(0.15)	(0.25)
100	-482.61	-1526.83	0.01	0.02
	(1325.07)	(2296.83)	(0.11)	(0.18)
150	313.67	-943.97	0.01	0.07
	(1408.75)	(2028.38)	(0.09)	(0.14)
200	329.92	-148.09	0.01	0.10
	(1324.69)	(1734.28)	(0.07)	(0.13)
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, but results are robust to other (or no) procedures.

B Further Balance Tests and Attrition

Table 21: 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 22: 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 23: 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 24: 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 25: 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 26: 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 27: Correlation of baseline observables with number of surveys completed

Variable	Correlation coefficient
Firm Scale:	
Profit (last month)	0.031
Firm Age	0.121**
Has Employees?	-0.051
Number of Emp (if $n > 0$)	0.041
Business Practices:	
Offer credit	0.081
Have bank account	0.067
Taken loan	-0.047
Practice accounting	-0.020
Advertise	-0.053
Sector:	
Manufacturing	0.101**
Retail	-0.006
Restaurant	-0.089*
Other services	0.003
Owner Characteristics:	
Age	0.077**
Secondary Education	0.073

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