Local Knowledge and Managerial Capital: Evidence from Kenyan Microenterprises*

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Abstract

We conduct a randomized controlled trial in which we assess different techniques for increasing business knowledge among young, female-run microenterprises in a Kenyan slum. Business owners are randomly assigned to receive classroom business training, a mentor drawn from a set of successful local business owners, or neither. We find that mentorship generates a sustained 25 percent increase in profit relative to control while no such change occurs among the class treatment. The key channel is cost and supplier management. 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. Taken together, this suggests mentorship provides local information on suppliers that training classes do not. 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 individual 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 at a larger scale. In response, in-class 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 training. 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 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 382 young, female-run businesses to receive a mentor drawn from the most successful business owners in Dandora, attend in-class training, or a control group. Mentors 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 at the 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

[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.

mentors and mentees jointly decide on the relevant topics. 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, and the evidence suggests that mentors provide local information not available in business classes. Mentees have 30 percent higher profit than the control group after four months, compared to a (statistically insignificant) 5 percent increase for the in-class training group. That effect is persistent, as the differences across groups remain roughly the same seven months after the treatment. We then show that 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, economy-specific 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.

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 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 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 who just missed the cut, 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).

From a policy perspective, this mentorship program was extremely cost effective. In addition to the aforementioned impact on profit, the cost is significantly lower than standard in-class training programs. While training can range from $1000 to $2000 per person (Blattman and Ralston, 2015) due to instructor and space requirements, our mentorship program minimized both of these major cost requirements. Mentors were paid 1000 Ksh (less than $10) to participate in the program and all meetings took place at the mentor's business. We further find that nearly half of all mentor-mentee pairs were still meeting seven months after the treatment ended, suggesting that the initial cost is sufficient to generate a large number of long-lasting relationship. All told, each dollar spent on the treatment generated an annual return of $1.78, well above not only other training programs but also large compared to most other microenterprise interventions as well.

1.1 Related Literature

This paper relates to a number of different literatures. Most closely related to this paper is 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, Karlan and Valdivia, 2011; 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 based training and finds the latter improves practices. 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 consulting services from inside the community, as they have more experience operating in these markets, and are also presumably much cheaper to implement.

Second, our focus on local information relates to work on social learning and production. 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 growth (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 (2014), 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. Section briefly discusses the relationship between our study and other studies of training and also the cost effectiveness of mentorship. Finally, Section 7 concludes.

2 Business Characteristics of Dandora, Kenya

Dandora is a dense, urban slum to the northeast of Nairobi. It is approximately four square kilometers, and as of the most recent 2009 census, contained 151,046 residents. This makes it approximately two-thirds as dense as the borough of Manhattan with almost no buildings larger than two stories. Perhaps most famously – though not the focus of this study – the Dandora Municipal Dump site borders Dandora and is the only waste site for the entire city of Nairobi, thus making Dandora widely regarded as one of the most polluted places in the world.

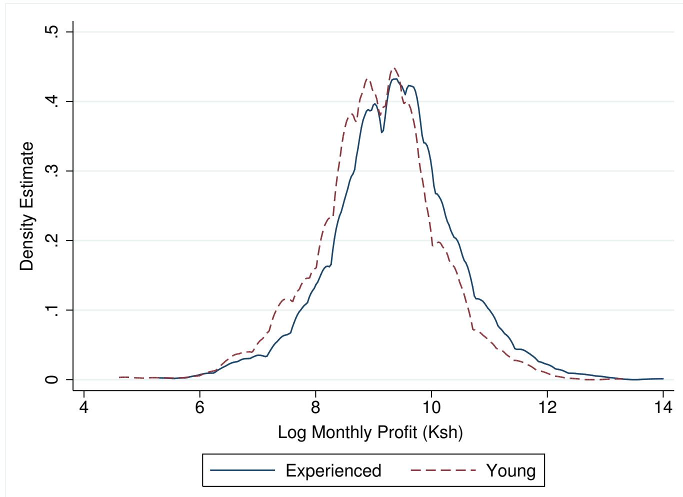
To assess the business characteristics in Dandora, we conducted a street-level survey of 3,290 randomly selected business.[2] Table 1 provides summary statistics for business. Column three also includes the same information for "young" firms with owners under 40 years old and less than 5 years of experience, 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.

The profit of established businesses is clearly shifted to the right. Of course, Figure

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

Figure 1: Log profit distribution for young and experienced enterprises

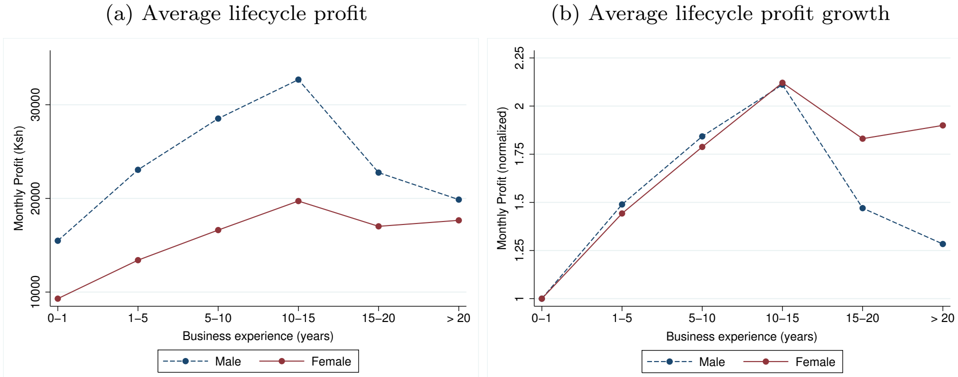


1 suffers from survivor bias. Yet despite the substantial difference in profit, there is not much difference in observable business practices. They are equally likely to offer credit to costumers, have a bank account, have taken a loan at some point in the past, or engage in formal accounting or advertising. Moreover, they are roughly equally educated. To the extent that we believe proper business practices are driving business success, Table 1 suggests young businesses are practicing at roughly the same rate 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 productive than their male counterparts at any 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).

Figure 2: Gender differences over the lifecycle

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



2.1 Learning and Business Scale in Dandora

To more explicitly motivate our study of local information, we again utilize our baseline survey. We asked individuals 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 self-teaching. 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 Experimental Design

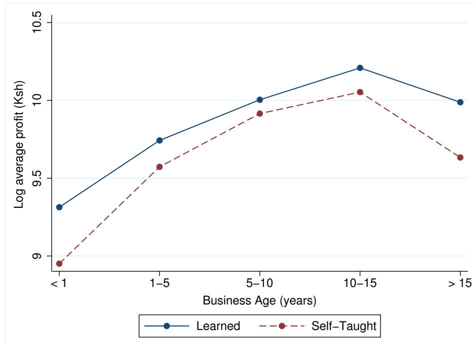
Our sample is derived from the baseline survey. We restricted our sample to business owners who are under 40 years old and have been running a business for less than 5 years. This included 1094 business owners, 787 of whom were women. Since 72 percent were women, we further restricted attention to female-run 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. 588 (81%) 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 588 individuals, 387 showed up at orientation (64% of 588, or 52% of the original 723). Randomization was conducted on this group of

[3] The answers do not add to one because individuals were able to choose more than one option.

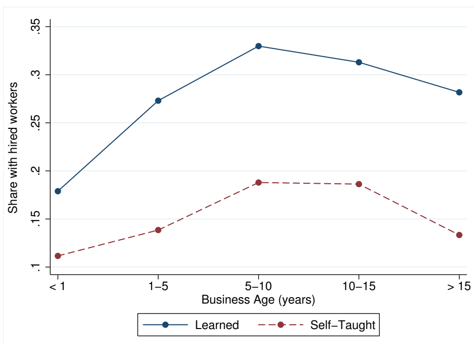
[4] Total employment looks extremely similar to Figure 3b given that those that do have workers have so few.

Figure 3: Business scale differences over the lifecycle

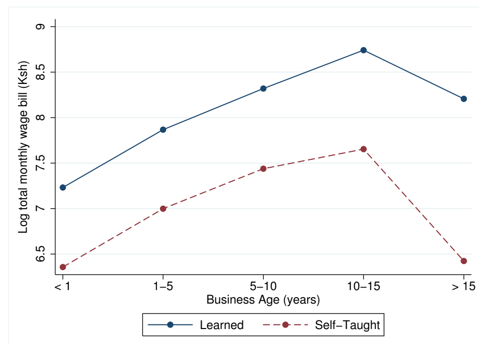
(a) Average profit



(b) Share with employees



(c) Total wage bill



387, and they were divided into one of three groups.[5] 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 and 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

[5] This may cause some concern about external validity. The large number of follow-up surveys (6 in a year) required trading off external validity issues for internal validity issues from sample attrition over time. As we discuss in Section 3.2, the restrictions on participation worked well in limiting attrition.

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.[6] 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 were 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?" Mentees were asked about the implementation of previous mentor advice in followup surveys.

The treatment was completed at the end of November 2014. To understand the dynamics of the response across different treatment arms, we conducted four follow up surveys in the middle of December 2014 (preceding the Christmas holiday) and then in the last week of January, February, and March of 2015. A longer followup was conducted in June 2015. Throughout the rest of the paper these surveys will be numbered by months since treatment, so that the surveys will be numbered $t \in \{1, 2, 3, 4, 7\}$ will reference December, January, February, March, and June.

3.1 Mentor Selection

The pool of mentors was selected from our large baseline survey in Dandora. 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

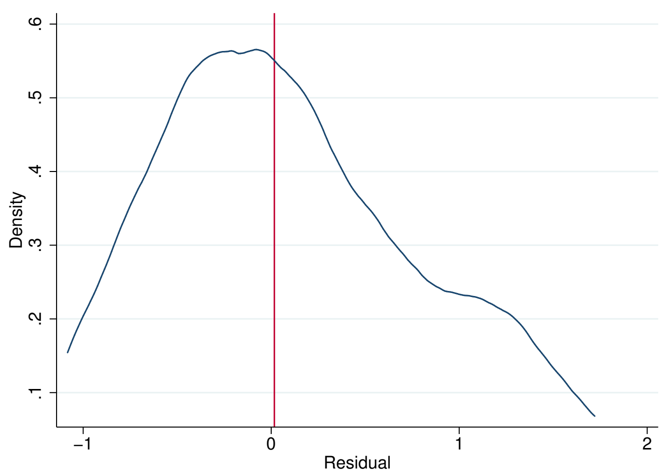
[6] Anticipating the results somewhat, we find similar results to previous formal training research using other training programs, suggesting that there is nothing specific about our class design that generates our results.

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 effects turn out to be unimportant, 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.

Figure 4: Distribution of $\widehat{\varepsilon}$ and 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 Final Sample Size and Balance

Initially 378 individuals who met our constraints (female, younger than 40, business operation of less than 5 years) agreed to participate in the study. The randomization was conducted on these 378 individuals. Table 5 provides the initial balance test.

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%), so that after the first followup, the response ready leveled out at around 85%. In terms of number of followups completed, 6 individuals completed exactly one followup (0.3% of 372), 18 completed two (5%), 32 completed three (9%), and 112 completed four (30%), and 204 completed all five (55%). All told, 85% completed at least 4 of the 5 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 25 of Appendix B. We find little evidence suggesting that response is associated with baseline observables. Manufacturing businesses are slightly more likely to answer more surveys, which is the only correlation coefficient significant at 10 percent. Even so, the difference is not large. Manufacturing business owners answer 4.8 surveys on average, compared to 4.3 for the rest. Anticipating the results slightly, we also find 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. We put no restrictions on meeting time, place, or length. Interestingly, 48 percent had met at some point at least six months after the treatment had been completed, suggesting that mentees did receive some value from the treatment.

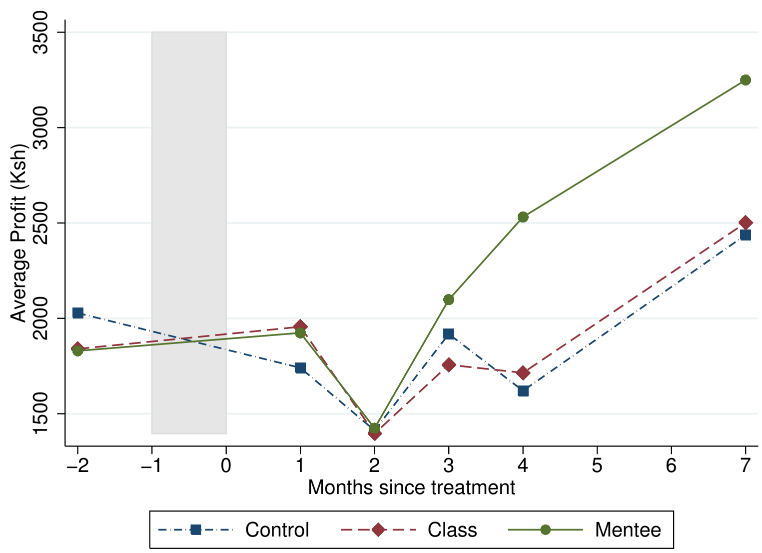
4 Results

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 statistically reject the hypothesis that the class effect is weakly higher than the mentorship effect. In Section 4.2 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. Lastly, in Section 4.3 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.[7] The mentee group, however, sees a substantial growth in profit relative to both the control and class group, providing evidence on the relative effectiveness of the mentorship treatment. To assess the impact of our interventions more seriously, we run a series of regressions. First, to get some notion of the average treatment effect, we pool the data and run

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

Here, y_{it} is the outcome for individual i at time $t \in \{1, 2, 3, 4, 7\}$ 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.

[7]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.

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 robust 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, nearly a 25 percent increase. 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, the mentorship effect is larger than the class effect.[8] Overall, the mentorship program generates a large increase in profit relative to the control, while the in-class training program delivers almost no change in profitability.

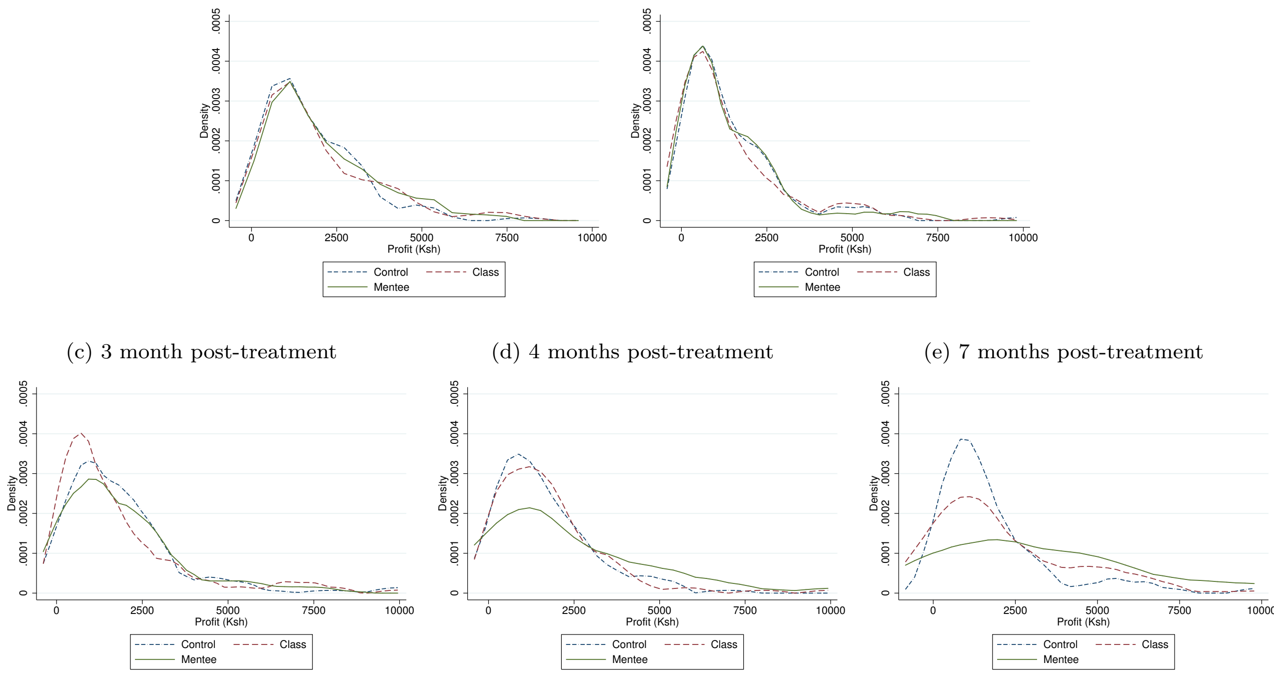
Figure 6 suggests that mentorship increases variance along with the mean, so that some individuals gain more than others. One possibility that arises in models of ability diffusion is that those with better mentors should see a larger treatment impact. To fix ideas, imagine the evolution of mentee productivity z takes the form

$$z_1^{\text{mentee}} = \max\{z_0^{\text{mentee}}, z_0^{\text{mentor}}\} \tag{4.3}$$

which is a simple version of the process assumed in recent work such as Jovanovic and Rob (1989) and Lucas (2009), among others cited in the introduction. By our experimental design, a natural assumption here is $z_0^{\text{mentor}} > z_0^{\text{mentee}}$, so (4.3) implies $\Delta z^{\text{mentee}} = z_0^{\text{mentor}} - z_0^{\text{mentee}}$. Guided by this idea, we next investigate heterogeneity in the impact based on both own

[8] 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 *relative* importance of mentorship relative to in-class training.

Figure 6: Distribution of profit over time



(a) 1 month post-treatment (b) 2 month post-treatment (c) 3 month post-treatment (d) 4 months post-treatment (e) 7 months post-treatment

baseline profit and mentor baseline profit. We run the regression

$$y_{it} = \alpha + \beta_1 M_{1it} + \beta_2 M_{2it} + \beta_1 M_{3it} + \gamma C_{it} + \nu X_i + \theta_t + \epsilon_{it}. \tag{4.4}$$

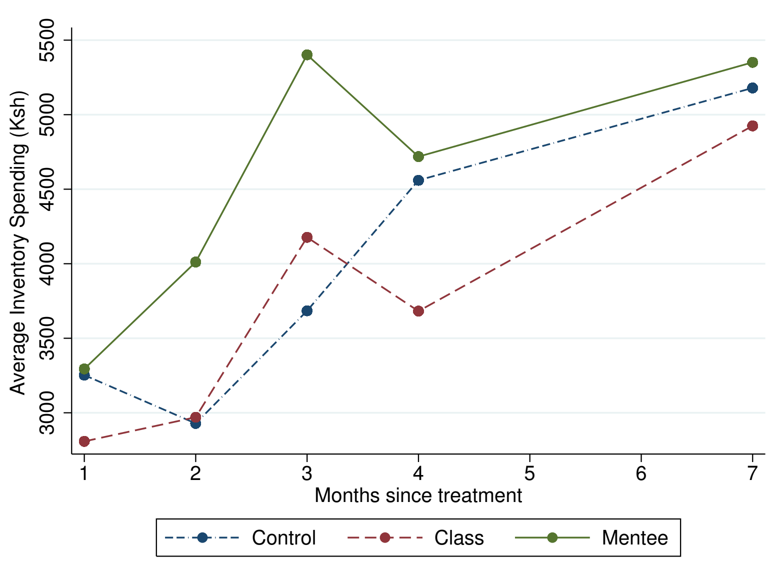
where $M_{1it} = 1$ if i has a mentor from the bottom 25 percent of the baseline mentor profit distribution, $M_{2it} = 1$ if i's mentor is in the 25th to 75th percentile, and $M_{3it} = 1$ if i's mentor is in the top 25 percent. The results are in Table 7. On average, having a mentor from the top 25 percent generates a 10 percent larger treatment impact than a mentor drawn from the bottom 25 percent. Focusing on the periods in which mentorship has a positive average impact, that same metric implies a 32 percent larger impact at $t = 4$ and 40 percent larger impact at $t = 7$. Consistent with theoretical models of knowledge diffusion, interaction with better business owners generates a larger treatment impact. Alternatively, the impact of baseline mentee profit less clear. We re-estimate (4.4) except replacing mentor profit with mentee profit and present the results in Table 8. All three groups show a significant increase in profit after the treatment, but it is strongest among the poorest and the richest mentees. The pooled effect does mask some underlying timing differences across groups, as the richest mentees have a delay in the impact.

Overall, the results imply that having a better mentor generates a larger treatment impact, which is consistent with theoretical models of knowledge diffusion. The impact of the mentee's own profit is less clear. There is, however, an important caveat in order. The standard deviation of mentor profit is nearly ten times that of mentee profit, as we specifically targeted less experienced businesses owners who are much more homogenous in profit than their mentors. Allowing for more heterogeneity among treated businesses could provide more robust results along this dimension.

4.2 Why Does Scale Change? Mentorship and Local Knowledge

Since profit increases, we next turn to understanding why. Table 9 uses regressions (4.1) and (4.2) to study inventory spending across the three groups. On average over the course of the study, mentees spend around 25 percent more 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, giving further evidence as to the relative importance of mentorship. The time series, however, reveals a different pattern than the profit time series, as can be seen in Figure 7.[9]

Figure 7: Inventory spending time series



Inventory spikes for mentees two to three months following the treatment or put differently, immediately preceding the increase in profit seen in Figure 5 and Table 6. Two months

[9]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 7.

after the treatment, mentees spend 1082.62 Ksh (or 37 percent) more on inventory relative to the control, compared to 41.48 Ksh (1 percent) more in the class treatment. This continues into the next month, in which mentees spend 171.52 (47 percent) more on inventory. The class treatment actually spends 13 percent more on inventory this month, but cannot be statistically distinguished from the control. Regardless, we can again reject the hypothesis that the class group spends weakly more on inventory in these months, so that the mentee group is spending relatively more on inventory.

To get a more complete understanding of changes in scale, we asked more detailed questions in our 7 month post-treatment survey. The results from those regressions are presented in Table 10. We find no evidence of any business scale changes other than the value of inventory stock. 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 10 show no other differences in scale. Hours of operation and employment are nearly identical across all three groups. We find some evidence of higher wage bills among mentees, 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, 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 11 shows the differential treatment impact across these pricing and supplier channels.

The first point 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.

4.3 "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 12 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 12 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 claims to do 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 suggests 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 13 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 12 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.[10]

To better understand the details of business practices, in our $t = 7$ survey we asked a

[10] A similar exercise for marketing and advertising (Panel B in Table 12) finds no difference between mentors who advertise and those who do not. Results are available upon request.

much more detailed 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 including Kenya. We find at least some evidence of business practices changes in both treatment groups. However, the most robust evidence that mentorship changes business practices comes when we focus on supplier-related practices. Table 14 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.

Consistent with previous evidence, Table 15 shows that more detailed questions do not change the fact that mentees look similar to the control in terms of marketing practices. If anything, we find evidence that in-class training actually decreases the extent of marketing, as the marketing score decreases from 1.51 in the control to 1.22 in the class treatment. That said, 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.

The other two broad topics, stock management and record keeping show more interesting results. In terms of record keeping, we find similar results to the time series comparison when considering sales recording and record consulting. After seven months, neither treatment has much impact. Interestingly, however, we do find evidence that the mentees are more likely to use a budget to plan future costs. 57 percent of the control claim they budget future costs, compared to 61 percent in the class treatment (a 7 percent statistically insignificant increase) and 71 percent (25 percent increase) among mentees. This is consistent with our evidence in Section 4.2 that mentors primarily provide benefit through a better focus on cost and supplier management.

Further evidence of the focus on cost and suppliers can be found in Table 16, which decomposes the *Stock score*. First, both treatments see a large decrease in their likelihood of running out of inventory. This could in part be due to better record management found in previous periods (Table 12). 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.

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 particularly important in light of standard models of learning in which information (broadly defined) flows from those with higher ability to lower ability. Our simple learning function (4.3) implies that

$$\Delta z^{\text{mentor}} = \max\{z_0^{\text{mentee}} - z_0^{\text{mentor}}, 0\} = 0$$

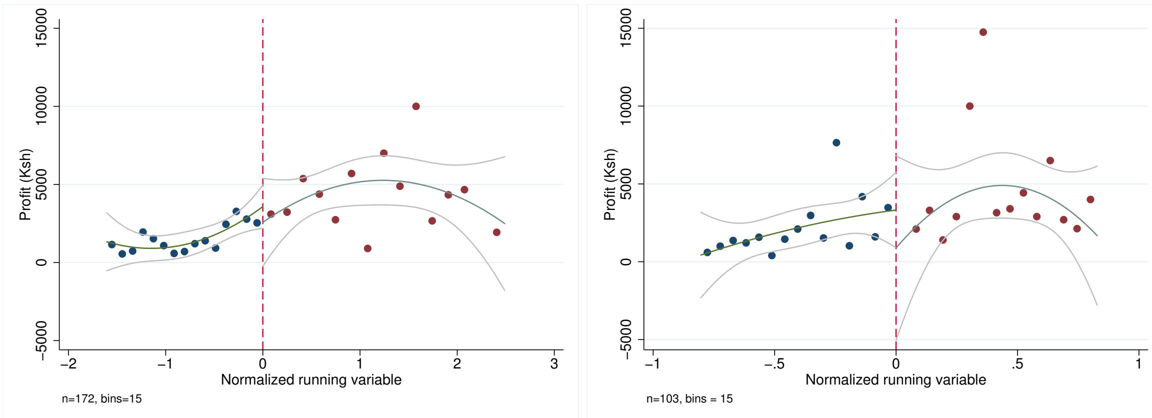
where the final equality follows from our assumption that $z_0^{\text{mentor}} > z_0^{\text{mentee}}$. In our context, this suggests that mentors should see no impact of being chosen as a mentor, despite the impact on the mentees. Choosing mentors because of their underlying entrepreneurial talent eliminates 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 right of 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 survey. 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 8 plots profit along with a fitted quadratic and its 95 percent confidence interval. Figure 8a uses the entire sample, while Figure 8b uses the Imbens and Kalyanaraman (2012) procedure to choose the optimal bandwidth. Both use 15 bins on either side of the cutoff.

While Figure 8 suggests no discontinuity around the cutoff, we next assess this more

Figure 8: Profit for mentors and non-mentors

(a) Full sample

(b) IK Optimal Bandwidth



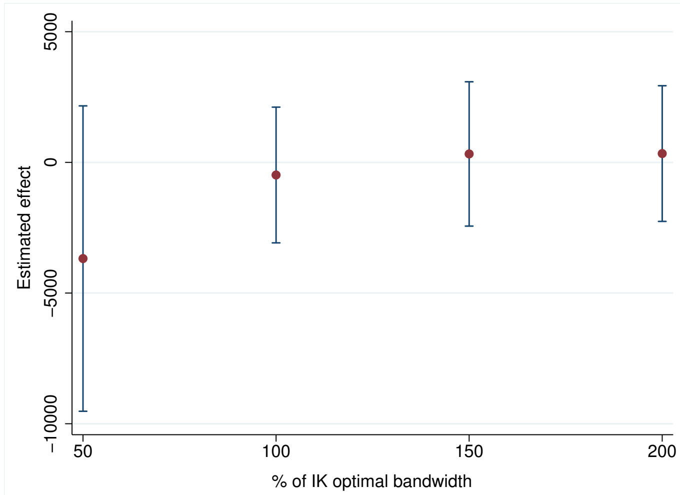
formally. In particular, letting $\overline{\varepsilon}$ be the cut-off value for mentors derived from regression (3.1), we run the regression

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

where π_i is profit, $D_i = 1$ if individual i was chosen as a mentor ($\widehat{\varepsilon}_i \geq \overline{\varepsilon}$ in regression 3.1), $f(N_i)$ is a flexible function of the normalized running variable $N_i = (\widehat{\varepsilon}_i - \overline{\varepsilon})/\sigma_\varepsilon$, 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 18 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.

The only treatment estimate that is statistically different from zero is restricting the sample to 50 percent of the optimal bandwidth and assuming f is linear which also restricts the same size to 51. This immediately disappears once quadratic terms are added. Next, we use local linear regressions to estimate the same treatment effects, the results of which are in Table 19. Again, there is no evidence that mentors benefit from being mentors. Figure 9 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 9: 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 19. 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 Discussion: Cost-Effectiveness and Relation to Other Microenterprise Interventions

As pointed out in McKenzie and Woodruff (2014), in-class training has been subject to numerous evaluations. Most have found little evidence of effects, but lack of power plays some role here. The in-class training treatment in this paper is subject to the same criticism. After 7 months, the 95 percent confidence interval for in-class training is $(-24, +29)$ percent increase relative to control which is similar to other studies. Giné and Mansuri (2014), for example, have a 95 percent confidence interval of (-33, +17) percent relative to control, while Karlan and Valdivia (2011) estimate (-29, +59) percent. Our point estimate implies a 2.6 percent increase in profit, which is obviously quite small, but as in other works the imprecision in the estimate makes it difficult to draw any deep conclusions about the impact of training directly from the result.

The mentorship treatment, however, warrants some discussion. We can reject that the class treatment generates a weakly higher effect on profit. We therefore have more confidence in the *relative* lack of in-class training impact than any absolute lack of impact. Moreover, the mentorship program was cheap to implement, especially relative to standard training. Including the 3,290 business baseline survey, our treatments cost 11,074 Ksh ($109), 15,036 Ksh ($148), 12,192 Ksh ($120) per participant for control, class, and mentorship respectively. We include the cost of the baseline survey since we used it to define potential mentors, but exclude the direct labor cost of followup surveys as it is unrelated to treatment. Using the average return over the course of the study in Table 6, the annual return to the program is 2176 Ksh (41.86×52 weeks) for the class treatment and 21,792 for the mentorship treatment. This estimate is conservative, as it weights earlier waves with no mentorship impact equally to later waves. Taken together, our mentorship treatment generated $1.78 per dollar spent, with a 95 percent confidence interval of $(0.82, 2.75)$. Our class treatment generated a return of $0.18 per dollar spent with confidence interval (-0.59, 0.94).

The low cost of the mentorship program presumably makes it more cost effective than other training programs, which require the cost of hiring qualified instructors and renting space. Our training course cost $4000 which is approximately $37 per student, but not include the cost of space rental. This is toward the low end of other studies on training McKenzie and Woodruff (2014). Regardless, the mentorship treatment required neither formal instructors or space rental, as meetings took place at the mentor's place of business. Even at the relatively low cost of our training treatment, the mentorship program is significantly more cost-effective.

This measure of cost-effectiveness also compares favorably to cash grant programs as well. Using the same procedure, we compute the return to two successful unconditional cash transfers. de Mel et al. (2008) find that the monthly effect of a 10,000 LKR transfer in Sri Lanka is 1421 LKR. Only counting the cost of the transfer, this implies a return of 0.65 per dollar spent with a 95 percent confidence interval of (0.16, 1.14). They find a lower return for women as well. Blattman et al. (2015) provide a suite of services to women in Uganda, including a $150 cash transfer. Again, using only the transfer amount (and forgoing the cost of training), they find an annual return of 0.65 per dollar, with a confidence interval of (0.40, 0.90). Fafchamps and Quinn (2015) target businesses with a business plan competition and then deliver $1000 cash prizes. They find an 80 percent return on cost. Of course, this is not a statement about overall cost effectiveness, as the persistence of treatment impact may differ. However, using our admittedly crude metric, even the lower bound on the return to mentorship is near the upper bound on these asset grant programs.

Overall, even toward the low end of the confidence interval, our intervention provides an extremely cost-effective method to transfer managerial skills to microenterprises. This is perhaps surprising in light of the recent suggestions that training is generally too costly to pass simple a cost-benefit test (Blattman and Ralston, 2015). Here, the cost-effectiveness comes from both the increased impact relative to in-class training and also the relatively lower cost. On an even broader scale, mentorship provides larger – or at least competitive – effect with the most successful microenterprise treatments without requiring large cash grants to generate the effect, as is the case in programs discussed above.

7 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 (2014), 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. The experimental design, however, has the ability to bring substantial structure to model parameters that govern the welfare impact of knowledge diffusion. We plan to pursue this avenue in future research.

Lastly, the work presented here presents a number of potential extensions. We discuss two here. We leave unanswered why individuals do not seek out lower cost suppliers on their own. That is, we leave unanswered the *cause* of this information barrier. 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 networks. Moreover, nearly half our mentee-mentor pairs were still meeting seven months after the treatment, suggesting that the these matches can be persistent. An important next step is to better understand the how these networks are formed, and the extent to which they can be extended. Second, our findings are almost certainly an upper bound on impact (net of spillover effects), as we specifically targeted young business owners. To the extent that this information can be learned over time, the effect will be smaller among more experienced business owners. We find no evidence of such an effect here, but we restrict attention to only a small portion of the entire experience profile. A more robust analysis of this idea 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)
Mentee	419.09†††	183.41	11.50	180.30	911.00†††	811.96††
	(116.16)***	(208.32)	(207.78)	(275.90)	(261.84)***	(331.86)**
Class	41.86	215.55	-16.12	-161.23	94.04	63.91
	(112.84)	(200.95)	(201.44)	(266.35)	(255.73)	(325.75)
Constant	1729.60	1740.49	1412.42	1917.86	1620.28	2473.84
	(120.77)***	(144.21)***	(145.08)***	(192.65)***	(182.90)***	(236.44)***
One tailed t-test p value	0.000	0.562	0.446	0.103	0.001	0.011
Obs.	1586	337	312	314	318	305
R^2	0.057	0.003	0.000	0.005	0.044	0.024
Controls	N	N	N	N	N	N

Panel B: Include controls		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee	439.17†††	164.98	2.11	208.68	964.65†††	875.88††
	(116.59)***	(209.47)	(210.48)	(275.05)	(262.33)***	(335.59)**
Class	71.26	204.57	9.56	-99.17	134.59	109.76
	(113.13)	(201.91)	(202.82)	(265.74)	(257.02)	(328.20)
One tailed t-test p value	0.000	0.576	0.514	0.128	0.001	0.010
Obs.	1586	337	312	314	318	305
R^2	0.070	0.0255	0.016	0.045	0.073	0.045
Controls	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, ***. †, ††, and † † † indicate the ability to reject the hypothesis that the mentee effect is weakly smaller than the class effect at 0.10, 0.05, 0.01 using a one tailed t test.

Table 7: Heterogeneous Mentor Effects for Profit

		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee: mentor in $(0, 25)$	311.18	4.97	-235.96	377.14	616.56	698.70
	(199.65)	(357.30)	(386.14)	(478.77)	(469.07)	(515.584)
Mentee: mentor in $(25, 75)$	400.80	394.81	128.41	25.79	780.89	713.98
	$(142.53)^{***}$	(258.65)	(247.82)	(333.31)	$(320.87)^{**}$	$(424.34)^{*}$
Mentee: mentor in $(75, 100)$	528.19	-37.74	-66.50	337.91	1305.52	1053.83
	$(174.43)^{***}$	(310.87)	(318.91)	(430.02)	$(384.44)^{***}$	$(487.72)^{**}$
Class	41.86	215.55	-16.12	-161.23	94.04	63.91
	(112.84)	(200.95)	(201.44)	(266.35)	(255.73)	(325.75)
Control mean	1729.60	1740.49	1412.42	1917.86	1620.28	2473.84
Obs.	1586	337	312	314	318	305
R^2	0.057	0.006	0.000	0.005	0.044	0.026
Controls	N	N	N	N	N	N

Table 8: Heterogeneous Mentee Effects for Profit

		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee: own profit in $(0, 25)$	734.51	805.73	112.60	841.67	1771.52	206.50
	$(234.48)^{***}$	(420.47)	(414.63)	(598.39)	$(527.36)^{***}$	(642.26)
Mentee: own profit in $(25, 75)$	266.48	-148.05	-63.03	152.90	777.70	652.82
	$(151.57)^{*}$	(267.44)	(-63.03)	(357.72)	$(345.60)^{**}$	(433.28)
Mentee: own profit in $(75, 100)$	504.76	386.56	87.11	-227.39	720.38	1589.74
	$(191.54)^{***}$	(344.73)	(346.56)	(445.35)	(437.96)	$(544.29)^{***}$
Class	61.38	252.34	-23.61	-135.51	104.20	98.48
	(111.94)	(196.97)	(201.47)	(264.55)	(254.28)	(324.37)
Control mean	1729.60	1740.49	1412.42	1917.86	1620.28	2473.84
Obs.	1586	337	312	314	318	305
R^2	0.078	0.057	0.015	0.036	0.068	0.056
Controls	N	N	N	N	N	N

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. In the "No Controls" panel, we still include profit-bin 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 than the class effect at 0.10, 0.05, 0.01 using a one tailed t test.

Table 9: Inventory Spending

Panel A: No controls		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee	622.42†††	41.65	1082.62††	1717.52†	158.51†	171.99
	(315.42)**	(487.06)	(569.64)*	(790.32)**	(705.32)	(910.11)
Class	-214.64	-444.45	41.48	492.99	-878.09	-254.31
	(305.51)	(469.92)	(550.65)	(764.71)	(705.32)	(887.55)
Constant	2990.80	3252.64	2928.63	3684.22	4559.86	5179.31
	(327.28)***	(337.96)***	(396.65)***	(553.22)***	(503.29)***	(650.15)***
One tailed t-test p value	0.003	0.156	0.032	0.057	0.076	0.314
Obs.	1576	336	308	312	316	304
R^2	0.027	0.003	0.015	0.016	0.008	0.001
Controls	N	N	N	N	N	N

Panel B: Include controls		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee	742.87†††	96.30	1107.73†	1773.82†	196.07†	612.73
	(312.15)**	(498.27)	(565.69)*	(788.93)**	(720.57)	(891.49)**
Class	-6.46	-365.31	206.86	661.76	-758.71	219.07
	(302.02)	(472.07)	(544.84)	(764.79)	(700.75)	(863.34)
One tailed t-test p value	0.001	0.170	0.053	0.076	0.092	0.319
Obs.	1576	336	308	312	316	304
R^2	0.066	0.027	0.065	0.052	0.062	0.088
Controls	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, ***. †, ††, and † † † indicate the ability to reject the hypothesis that the mentee effect is weakly smaller than the class effect at 0.10, 0.05, 0.01 using a one tailed t test.

Table 10: Business scale 7 months post-treatment

Panel A: No controls	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	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
Controls	N	N	N	N	N

Panel B: Include controls	Stock of inventory (Ksh)	Any employees?	Number of employees	Total wage bill (Ksh)	Hours open (last week)
Mentee	3622.02†	-0.01	0.00	412.23	-0.96
	(2412.91)	(0.03)	(0.05)	(371.55)	(3.13)
Class	428.06	0.00	-0.03	162.37	-1.00
	(1938.14)	(0.03)	(0.05)	(283.14)	(2.79)
One tailed t-test p value	0.093	0.699	0.246	0.282	0.495
Obs.	303	308	307	315	304
R^2	0.084	0.064	0.072	0.084	0.087
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 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, ***. †, ††, and † † † indicate the ability to reject the hypothesis that the mentee effect is weakly smaller than the class effect at 0.10, 0.05, 0.01 using a one tailed t test.

Table 11: 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	$0.21^{\dagger\dagger\dagger}$	-18.46	5.63	-380.91	$-318.041^{\dagger}$
	$(0.06)^{***}$	(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	$0.21^{\dagger\dagger\dagger}$	-36.07	-10.32	-391.72	$-334.41^{\dagger}$
	$(0.06)^{***}$	(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 12: Business Practice Time Series

Panel A: Record Keeping		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee	0.05	-0.01	0.11	0.06	0.13	-0.02
	(0.03)*	(0.06)	(0.06)*	(0.06)	(0.07)*	(0.07)
Class	0.14	0.19	0.17	0.10	0.30	-0.06
	(0.03)***	(0.05)***	(0.06)***	(0.06)*	(0.06)***	(0.07)
Constant	0.72	0.72	0.68	0.70	0.57	0.64
	(0.03)***	(0.04)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.999	1.00	0.870	0.737	0.999	0.287
One tailed t-test p value (H_0 : M $\geq$ C)	0.001	0.00	0.130	0.263	0.001	0.713
Obs.	1593	338	315	315	320	305
R^2	0.037	0.053	0.027	0.009	0.077	0.002
Controls	N	N	N	N	N	N

Panel B: Advertising		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee	-0.00	-0.00	-0.03	0.07	-0.015	-0.02
	(0.02)	(0.05)	(0.05)	(0.05)	(0.03)	(0.05)
Class	-0.01	0.00	-0.07	-0.01	0.00	-0.00
	(0.02)	(0.20)	(0.05)	(0.04)	(0.04)	(0.05)
Constant	0.21	0.20	0.16	0.10	0.07	0.19
	(0.02)***	(0.04)***	(0.04)***	(0.03)***	(0.03)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.290	0.522	0.212	0.059	0.719	0.601
One tailed t-test p value (H_0 : M $\geq$ C)	0.710	0.478	0.780	0.941	0.281	0.399
Obs.	1593	338	315	315	320	305
R^2	0.019	0.00	0.007	0.010	0.001	0.000
Controls	N	N	N	N	N	N

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 13: Accounting and Mentor Effects

Panel A: Record Keeping		Months since treatment				
	Pooled	(1)	(2)	(3)	(4)	(7)
Mentee (formal)	0.08	0.05	0.14	0.04	0.21	-0.05
	(0.03)**	(0.07)	(0.07)**	(0.08)	(0.08)***	(0.08)
Mentee (no formal)	0.03	-0.08	0.07	0.09	0.04	0.01
	(0.04)	(0.08)	(0.086)	(0.07)	(0.09)	(0.09)
Class	0.14	0.19	0.17	0.10	0.30	-0.06
	(0.03)***	(0.05)***	(0.06)***	(0.06)*	(0.06)***	(0.07)
Constant	0.72	0.72	0.68	0.70	0.57	0.64
	(0.03)***	(0.04)***	(0.05)***	(0.05)***	(0.05)***	(0.05)***
One tailed t-test p value ($H_0 : M_F \leq M_{NF}$)	0.105	0.085	0.187	0.687	0.033	0.711
One tailed t-test p value ($H_0 : M_F \geq M_{NF}$)	0.895	0.043	0.813	0.313	0.0967	0.289
Obs.	1593	338	315	315	320	305
R^2	0.039	0.060	0.030	0.010	0.077	0.004
Controls	N	N	N	N	N	N

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: Business Practice Aggregate Measures

		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
	(0.31)	(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
One tailed t-test p value (H_0 : M $\geq$ C)	0.989	0.996	0.742	0.838
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

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 15: Marketing Practices Decomposed

Marketing		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
One tailed t-test p value (H_0 : M $\geq$ C)	0.996	0.958	0.979	0.933	0.993	0.664
Obs.	306	306	306	306	306	306
R^2	0.025	0.010	0.015	0.008	0.019	0.001
Controls	N	N	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. Statistical significance at 0.10, 0.05, and 0.01 is denoted by *, **, and, ***. Marketing score is computed by summing all its components.

Table 16: Stock Practices Decomposed

Stock		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
One tailed t-test p value (H_0 : M $\geq$ C)	0.710	0.721	0.754	0.561
Obs.	306	306	306	306
R^2	0.072	0.019	0.018	0.105
Controls	N	N	N	N

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 17: Record Keeping Practices Decomposed

Record Keeping		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
One tailed t-test p value (H_0 : M $\geq$ C)	0.834	0.874	0.500	0.938
Obs.	306	306	306	306
R^2	0.004	0.004	0.001	0.015
Controls	N	N	N	N

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 18: 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 19: 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 20: 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
Self-taught	0.52	0.50	0.50

Table 21: 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
Self-taught	0.52	0.50	0.51

Table 22: 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
Self-taught	0.52	0.51	0.51

Table 23: 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
Self-taught	0.52	0.50	0.49

Table 24: 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
Self-taught	0.52	0.54	0.48

Table 25: Correlation of baseline observables with number of surveys completed

Variable	Correlation coefficient
Firm Scale:	
Profit (last month)	0.028
Firm Age	0.042
Has Employees?	-0.025
Number of Emp (if $n > 0$)	0.040
Business Practices:	
Offer credit	0.056
Have bank account	0.077
Taken loan	-0.023
Practice accounting	-0.012
Advertise	-0.071
Sector:	
Manufacturing	0.101*
Retail	0.011
Restaurant	-0.075
Other services	-0.031
Owner Characteristics:	
Age	0.077
Secondary Education	0.053
Self-taught	-0.051

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