# The Dynamics of Inter-Firm Skill Transmission among Kenyan Microenterprises*


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**Abstract**

We provide empirical evidence in favor of inter-firm productivity transmission using a randomized controlled trial with Kenyan microenterprises. We do so by randomly matching young firms owners with older, successful local firms and use a seven-round survey over the course of a year to study the dynamic response of matched firms. Profit is on average 20 percent higher among treated firms than in the control, though the effect fades as matches dissolve. These young firms are 40 percent more likely to switch suppliers and spend 16 percent more on inventory, highlighting the importance cost and supplier information. We exploit our selection procedure with a regression discontinuity design to show that none of these benefits extend to the more successful firm in the match, consistent with models of diffusion. To compare our results, we also include a second treatment arm in which firms are given access to a free business training program. Training generates changes in business practices covered in the classes but no change in profit, consistent with previous work.



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# 1 Introduction

Microenterprises account for a large share of businesses in many developing countries, despite their low profit. 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.

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. Recent theoretical and quantitative work has highlighted the transfer of these skills across businesses as an important component of development (Lucas and Moll, 2014; Perla and Tonetti, 2014; Buera and Oberfield, 2015). Motivated in part by this idea, we design a randomized controlled trial to answer two questions. First, can successful business owners transfer profitable information to young, inexperienced business owners? Second, if so, what are the underlying channels this information changes?

We study these questions in the Kenyan slum of Dandora. We select a random subset of 372 young businesses to receive access to a successful business owner in Dandora, which we refer to as "mentors." These mentors – randomly assigned conditional on matching business sector – are on average twice as profitable, twice as likely to have employees, and have been in business an average of ten years longer. The treatment consisted of four mentee-mentor meetings over one month. As we highlight later, however, nearly half were still meeting 12 months after this treatment period. As a comparison to this treatment, we also randomly assign another subset to receive formal business training. This group received classes taught in Dandora with a well-established microenterprise training curriculum, covering 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 formal training allows us to assess the relative importance of interaction with a local business to more formal training, which potentially provides a different set of information or skills.

We find that this interaction with a successful business is an effective means to increase

[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.

business profit among microenterprises. Over the twelve months following treatment, weekly profit is on average 20 percent higher among mentees than the control. However, this result is not immediately indicative that the interaction increased total match profit, as there could be changes in profit for mentors as well. We therefore exploit our mentor selection procedure with a regression discontinuity design to see if mentorship affects profit or business practices. After resurveying the mentors and 95 female business owners just below the cutoff, we find no impact on profit or business practices. This implies that the matches created by our intervention generated profit, and were not simply a transfer between businesses entered into the match. Moreover, the results are consistent with models of knowledge transfer or diffusion, which typically assume that the gains form an interaction between two firms accrue solely to the less productive member of the match (e.g. Jovanovic and Rob, 1989; Lucas, 2009; Lucas and Moll, 2014; Buera and Oberfield, 2015).

In contrast, the results from the formal business training are in line with previous studies of similar programs. This treatment generates a statistically insignificant 1 percent increase in profit relative to the control. This is unsurprising given previous studies on the topic, summarized in reviews by McKenzie and Woodruff (2014) and Blattman and Ralston (2015). While profit does not change, we do find short run changes in business practices. This lack of profit increase coupled with a short run change in business practices is consistent with previous work on microenterprise training, including Bruhn and Zia (2013) and Giné and Mansuri (2014). These results demonstrate that the mentorship treatment impact is not simply driven by an initial absolute lack of knowledge relative to places previously studied, but that mentorship is effective even in circumstances where classroom study has the same effects as found previously.

We then turn to understanding how this profit increases varies over time. First, we use the panel dimension of our data to show that the average profit increase among mentees masks important underlying heterogeneity across time. After four months, the average mentee treatment impact is 30 percent and remains approximately the same up to seven months post-treatment. At its highest point, the average mentee profit is at the sixty-first percentile of the baseline profit distribution, compared to the fifty-first percentile at baseline. After twelve months, however, there is no significant difference across any of the three groups, and profit is similar to baseline levels. The average impact of mentorship, therefore, fades over time.

The key underlying change generating this effect is that mentees are nearly 40 percent more likely to have switched suppliers in the aftermath of the treatment, which generates lower inventory costs and more inventory spending. On their main product, mentees have a unit cost that is half of both the treatment and control, while also spending 16 percent more on inventory over this time period. Again, however, variation in the effect over time contains important information. In the months immediately following treatment (a time when profit is the same across the three groups), mentees spend approximately 35 percent more on inventory than the control or class groups, while there is no difference across groups in the later months. Taken together, this evidence implies that local, market-specific information is effective at increasing profitability. This is in contrast to formal training, which is designed to provide benefits regardless of the market in which it is employed. Indeed, we find behavior changes along these margins in the class, yet we find no change in profit. This provides one potential explanation for the relatively small profit effect of training found in the literature (McKenzie and Woodruff, 2014; Blattman and Ralston, 2015): the information provided in classes is too general in nature. These small businesses benefit from local, context-specific information.

In addition to the large effect of mentorship on supplier changes, there was substantial changes in suppliers irrespective of our intervention. Nearly sixty percent of the control group switches suppliers during the study period. Profitable information about suppliers, therefore, seems to have a short half-life. This, coupled with the dissolution of matches over time, is key to understanding the profit effect over time. As matches end, mentees lose their link to profitable supplier information, and thus the effect on profit fades. The data is consistent with this interpretation. Mentees who continue to meet with their mentors 12 months after the official end of the treatment period are on average 55 percent more profitable than those who are no longer meeting, despite similar average profit at baseline. Of course, this is a necessary but not sufficient to imply benefits from continued meeting. If meetings ended when matches no longer generated any benefit, then this difference 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 mentors provide continued benefits while meeting.

### 1.1 Related Literature

This paper relates most closely to recent work emphasizing the transmission of skill or knowledge among individuals. 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. Guiso et al. (2015) focus on the role of learning among young Italian entrepreneurs, and finds that they adopt better management practices late in life. BenYishay and Mobarak (2015) and Beaman et al. (2015) leverage existing social networks to study diffusion and targeting of new technological information. We highlight a complimentary point in this paper: there exists profitable information outside these existing networks, highlighting the importance of constraints to information diffusion among microenterprises.

In contrast to our focus on constraints to profitable information within the local economy, the literature on increasing managerial capital in micro and small firms has overwhelmingly focused on in-class training. McKenzie and Woodruff (2014) provide an excellent and comprehensive review of previous studies. The overriding theme of this research is that business practices do change, but translate into little impact on revenue and profit (for example, Bruhn and Zia, 2013; Giné and Mansuri, 2014), though they point out that this could be due in part to small sample sizes. We find similar results 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. Bloom et al. (2013) finds a substantial impact from consulting services, though they focus on much larger businesses. We instead provide business advice from inside the community, which provide more local advice and is cheaper to implement. Indeed, mentors were paid 1000 KES (9.83 USD) for the one month of meetings. Despite the fact that they received no additional payment, 45 percent were still meeting 12 months after the treatment ended. In contrast, Bruhn et al. (2013) report that a formal consultant cost 11,856 USD in Mexico.

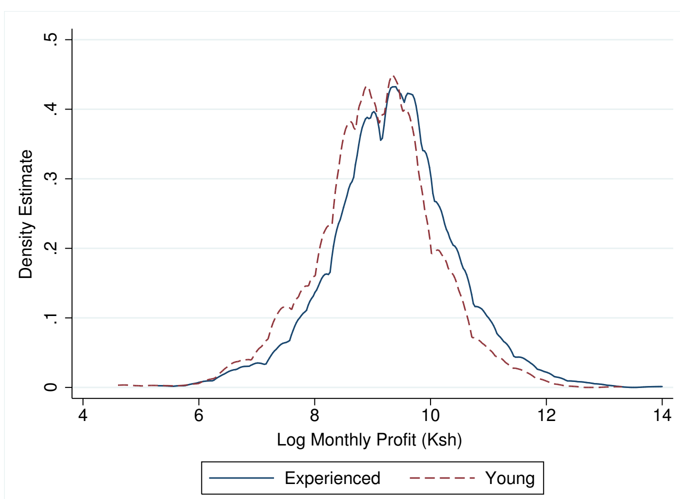
Finally, our results highlight a particular micro-foundation for distortions to managerial capital accumulation. Bhattacharya et al. (2013) and Da-Rocha et al. (2014) show these distortions can have an important aggregate effect in the context of policy distortion models developed in Restuccia and Rogerson (2008) and Hsieh and Klenow (2009). We draw a link between these distortion models and recent work by Lucas (2009), Lucas and Moll (2014), and Perla and Tonetti (2014) in which growth is generated through interaction with other firms. We highlight the benefit of improving the distribution from which businesses draw matches. Furthermore, these models assume that information (broadly defined) flows in one direction from the more to less productive member of the match. Our results from the regression discontinuity design confirm that the observable benefits of the match accrue solely to the less productive member of the match.

## 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. To assess the business characteristics in the area, 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 14,266 Ksh, the average experienced (i.e. not "young") owner earns nearly 42 percent more profit per month or 20,168 Ksh. Figure 1 plots the distribution of log profit for young and experienced enterprises.

Figure 1: Log profit distribution for young and experienced enterprises




Despite the substantial difference in profit, there is not much difference in observable

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

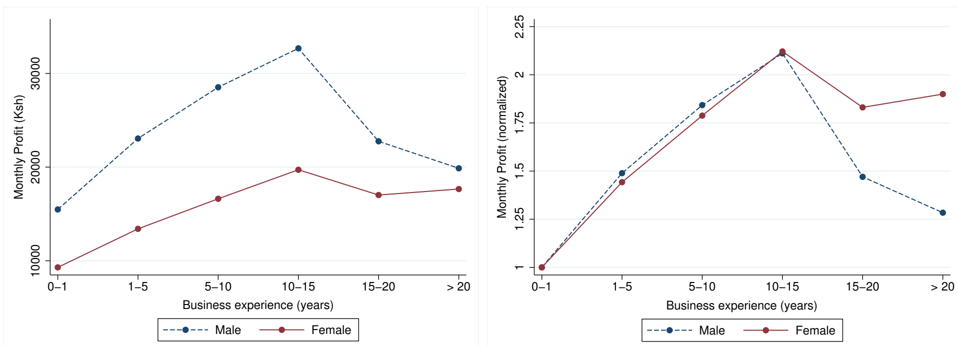
business practices. They are equally likely to offer credit to customers, have a bank account, have taken a loan at some point in the past, or engage in formal accounting or advertising. Moreover, they are roughly equally educated.

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 percentage difference in profits is roughly constant over the first fifteen years of the firms' operating lives (Figure 2b).

Figure 2: Gender differences over the lifecycle

(a) Average lifecycle profit

(b) Average lifecycle profit growth




## 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 their business. Fifty-five percent of all firms claimed they were self-taught, where the rest claimed to be to lear either from another business operator, in school, or through an apprenticeship. 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, as shown in Table 2.

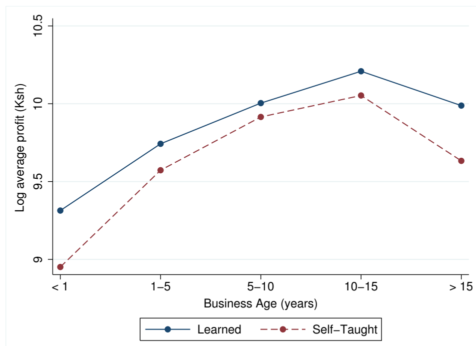
Those who are self-taught make substantially less profit and operate at a smaller scale. The profit ratio is almost identical among young (15,907 versus 12,778 Ksh) and experienced firms (21,972 versus 18,055 Ksh). Those numbers do hide some catch-up of the self-taught however. Figure 3 plots three measures of business scale over the lifecycle.[3] 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

[3]Total employment looks extremely similar to Figure 3b given there are so few firms that have workers.

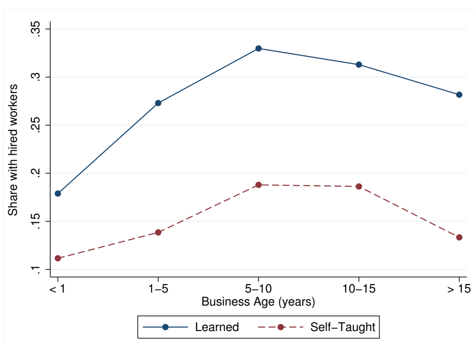
their closest (5-10 years), the self-taught are still 10 percent less profitable than the learned businesses. Other measures show similar patterns of self-taught operating at a lower scale than those who learned from others. Figure 3b shows that the self-taught are less likely to have employees and pay a smaller total wage bill.

Figure 3: Business scale differences over the lifeycle

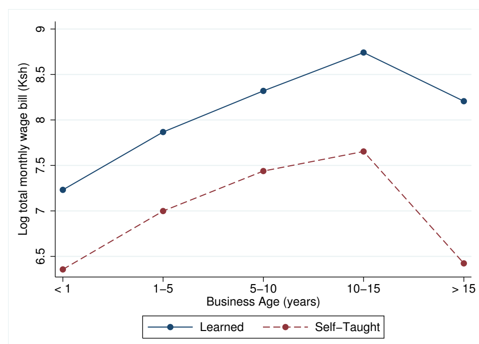
(a) Average profit




(b) Share with employees




(c) Total wage bill




All of this suggests that non-formal, local information may play an important role in the profitability of business. Of course, confounding factors limit our ability do say much more, and we therefore design an experiment to more formally test this idea.

# 3 Experimental Design

We use the baseline survey discussed in Section 2 to construct our sample. We restricted our sample to business owners who are under 40 years old and have been running a business for less than 5 years. This included 1094 business owners, 787 of whom were female-operated businesses. 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, 372 attended orientation (69% of 538, or 51% of the original 723). Randomization took place among these 538 individuals, and no one was given any indication of their assigned group until arrival at orientation. 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 372 individuals at orientation, 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 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.[4] 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 conducted all sections of each class topic.

Individuals assigned to 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

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

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 be discussed. To facilitate discussion, they were given optional 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 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

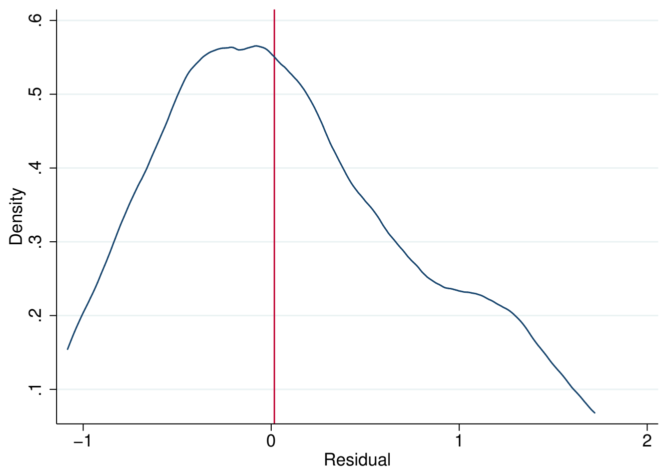
The pool of mentors was selected from our baseline survey. We first constrained our search to female business owners who were over 35 years old and had been operating the same business for at least 5 years. This left 366 individuals. We then ran a simple regression to control for age and sector-specific differences

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

where $\pi_i$ 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 sorted potential mentors by residual profit, then, starting with the most profitable, we recruited mentors until we have enough to link each mentee to a mentor that is in the same tightly defined business sector. Take-up was high, as 95 percent accepted our invitation to take part in the program. Figure 4 plots the distribution of $\widehat{\varepsilon}$ along with the cut-off.

As expected, Table 3 shows that mentors run substantially more successful businesses, earning about 4 times higher profit than those not chosen as mentors. Moreover, their businesses

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




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 372 individuals who attended orientation, 369 (99%) answered at least one followup. The response rates by wave were 352 (95% of 372), 318 (85%), 319 (86%), 323 (87%), 325 (87%), so that after the first followup the response rate leveled off at 85 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. 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 22 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 and we find little difference in estimation results with or without controlling for baseline factors.[5]

[5]To give some examples, 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.

### 3.3 Take Up of Treatments

Attendance at the business class was encouraged, but not mandatory to receive payment. 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: Profitability

We begin by considering the impact of our various treatments on 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. In Section 4.2 we use a regression discontinuity design to show that there is no change among mentors. Taken together, the results imply that our treatment generates match surplus, and is not just a reallocation of profitability across production units.

### 4.1 Treatment Impact

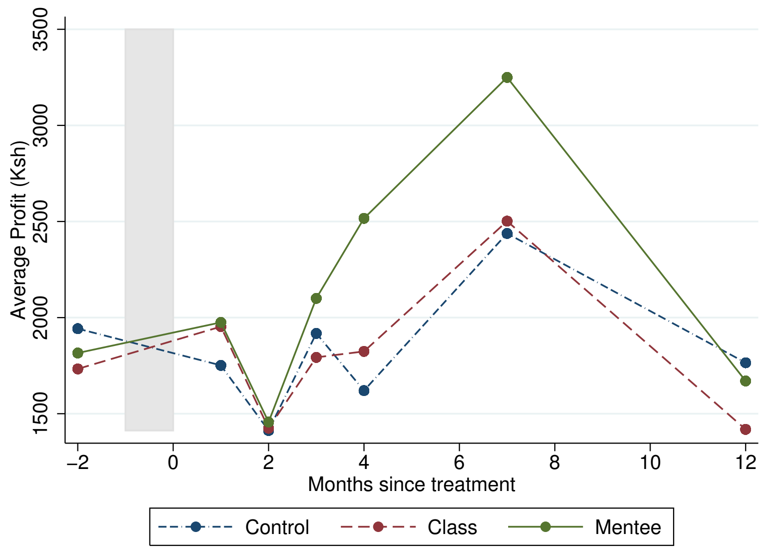
We begin by looking at the effect on the previous week's profit among the treatment groups. Figure 5 plots the time series of average weekly profit by treatment arm.

The class group mimics the control 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, which is discussed in detail in the next section. To measure 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

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

[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.

Figure 5: Profit time series




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 controls including secondary education, log age, and business sector fixed effects, and $\theta_t$ 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

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

Table 5 begins by considering the impact on business profit. On average during this time period, mentee profit is 339 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 it. Furthermore, the one tailed t-test shows that the effect of mentorship is 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 follows the general drop in demand following the Christmas holiday. In March 2015 (4 months post-treatment), profit of the mentees is 896 Ksh more than control compared to 203 Ksh more in the class treatment. This result remains into July 2015 (7 months post-treatment), as profit is 812 Ksh higher among mentees. A one tailed t-test again implies that in both March and July, the mentorship effect is larger than the class 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 in profitability. However, the effect fades over time, and we return to this theme in Section 5.

If mentors are utilizing their own skill or ability to increase mentee profit, better mentors should generate a larger treatment effect. 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 6. 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 Impact on Mentors

Since mentee profit increases on average, a second question is if there is any impact on the mentor. This is key to understanding if the intervention generates profit, or whether it simply generates reallocation across production units. Most recent theoretical and quantitative work assumes the former, including Jovanovic and Rob (1989), Lucas (2009), Lucas and Moll (2014), and Buera and Oberfield (2015). That is, interaction generates benefits only to the less productive member of the match. We therefore ask whether the interaction implies any gains to the mentors themselves. 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.

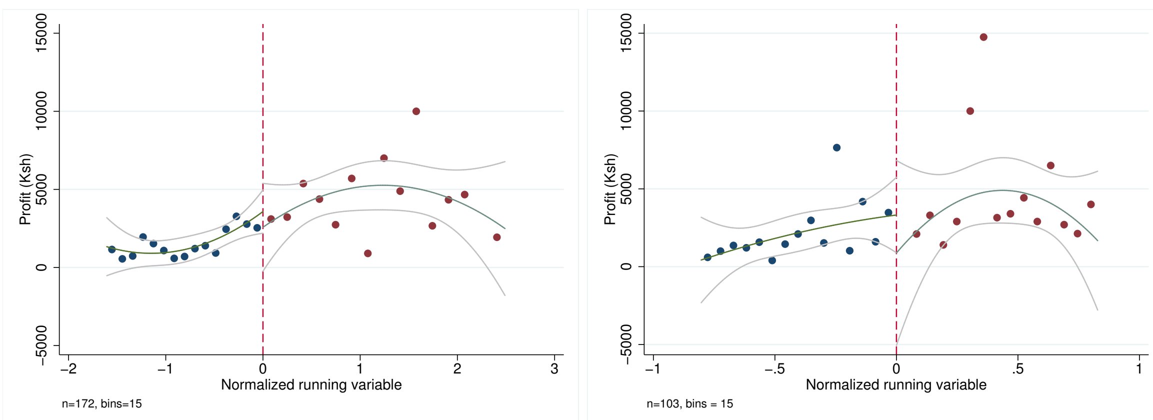
As mentioned above, mentors were paid 1000 KES for their participation. Therefore, when selecting mentors at the beginning of the program, we also contacted the 150 female business owners closest to the cutoff to participate as well. Ninety five agreed to participate,

[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.

and they were also paid 1000 KES at the start of the program, to facilitate our ability to compare the two groups. Four months after the treatment – a period with a significant profit increase among mentees – we resurveyed all mentors, along with these 95 additional business owners. 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 6 plots profit along with a fitted quadratic and its 95 percent confidence interval. Figure 6a uses the entire sample, while Figure 6b uses the Imbens and Kalyanaraman (2012) procedure to choose the optimal bandwidth. Both use 15 bins on either side of the cutoff.

Figure 6: Profit for mentors and non-mentors

(a) Full sample　　(b) IK Optimal Bandwidth



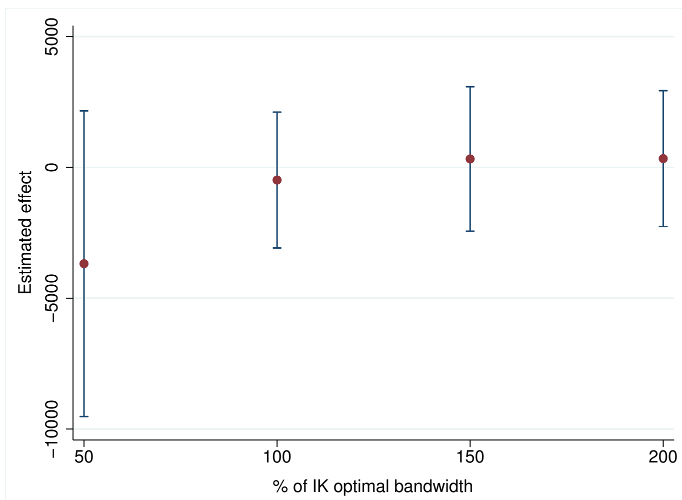

While Figure 6 suggests no discontinuity around the cutoff, we next assess this more formally. In particular, letting $\bar{\varepsilon}$ be the cut-off value for mentors derived from regression (3.1), we run the regression

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

where $\pi_i$ 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 $\nu_i$ is the error term. The parameter $\tau$ 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 7 shows the estimated values of $\tau$ 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 8. Again, there is no evidence that mentors benefit from being mentors. Figure 7 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 7: RD treatment estimates with local linear regressions




We further consider whether being chosen as a mentor has an effect on underlying business practices, such as inventory spending, marketing or record keeping. Results from the RD with local linear regressions are in Table 8. 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. When combined with the effect on mentees, this implies that our mentorship intervention generates profit, but also provides no observable benefits to mentors' businesses. That is, all of the gains are from the less productive member of the match.

## 5 Understanding the Treatment Impact: Channels and Dynamics

Section 4 show two key results. In the short run, mentorship generates a substantial increase in profit while the class does not. Neither treatment, however, sees any long-run change in profit. In this section we turn to understanding these results. Our main finding is that the changes among the mentorship group primarily relate to market-specific information, not the more general business skills covered in the class. Some examples may be helpful to provide some context.[8]

"Prudence" opened a women's clothing shop four months before the baseline survey. She purchased her inventory at the Gikomba Market, about a 15 kilometer trip from Dandora. At this market, there are two types of sellers: those at stalls deep into the market, or those who are more mobile. The mobile sellers approach you immediately as you enter the market. Prudence originally purchased her inventory from these suppliers, as she thought it was the most cost-effective use of her time, and that these suppliers tended to have good inventory. Her mentor, however, told her to go deeper into the market and compare prices before purchasing anything. The cost of her average woman's top dropped from 250 Ksh to 100 Ksh after making this change, while she kept her sale price exactly the same as before. She still meets with her mentor weekly.

"Margaret" – a belt retailer – had a similar experience. Her mentor used to sell belts before branching out into other clothing items as well. This mentor showed Margaret a new market in which to buy belts. While the mentor has grown out of that market and now buys from wholesalers, it was the market used by the mentor when her business was similar to Margaret's current scale. Margaret saves 5 Ksh per belt purchased, but has purchased hundreds of belts from this new market, thus making a sizable savings overall from this change. Like Sarah, her markup has increased, as she has not changed her sale price in response to the lower cost.

These examples at least anecdotally point to an important difference between mentorship and classroom training. While topics in standard training classes are designed to be orthogonal to the market in which these skills are employed, these examples suggest that mentorship provides information that is specific to Dandora. Margaret's mentor would certainly not be able to provide the same advice to a mentee located in another city, for example, as her advice requires a relatively deep understanding of available suppliers in the local economy. We

[8] In March 2016, we followed up with 25 mentor-mentee pairs to collect more detailed narratives of their experience with the program, and to provide some additional context for the quantitative results we found. These examples are derived from those narratives. They are available upon request, but note that nearly all were conducted in Swahili.

use regressions (4.1) and (4.2) to more systematically analyze the validity of the hypotheses implied by these examples, by looking at the impact of treatments on supplier choice and inventory. We then relate this to the short-run nature of the treatment effect by considering these changes in relation to the dissolution of matches over time.

### 5.1 Suppliers and Inventory Expenditures

First, we show that the key channel is supplier and inventory expenditures, consistent with the examples above. 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 9 shows the differential treatment impact across these pricing and supplier channels.

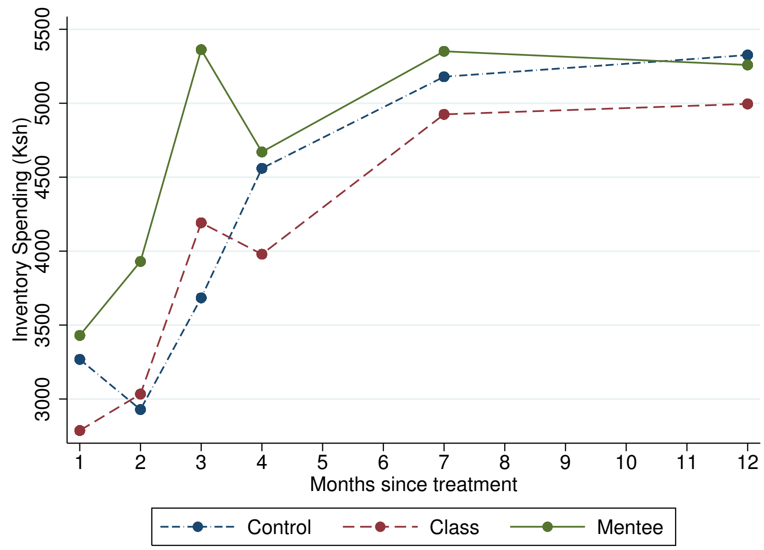
Mentees are much more likely to have switched suppliers. 89 percent of mentees changed suppliers in the first 7 months post-treatment, compared to 61 percent of those receiving in-class training, and 57 percent in the control. Note, however, that 57 percent of the control group switched suppliers in the first 7 months, implying substantial supplier change in economy independent of any interventions.

Switching suppliers comes with the ability to lower costs for their businesses. One month after the treatment, the mentees spend only half of what the control group requires to sell its main product. Seven months after the treatment, the mentees still pay roughly half of the control, and can be distinguished 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.

Consistent with these results, we also see an increase in inventory expenditures among mentees with no change among the class treatment. The regression results are presented in Table 10. 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. Figure 8 plots the time series of inventory spending.[9]

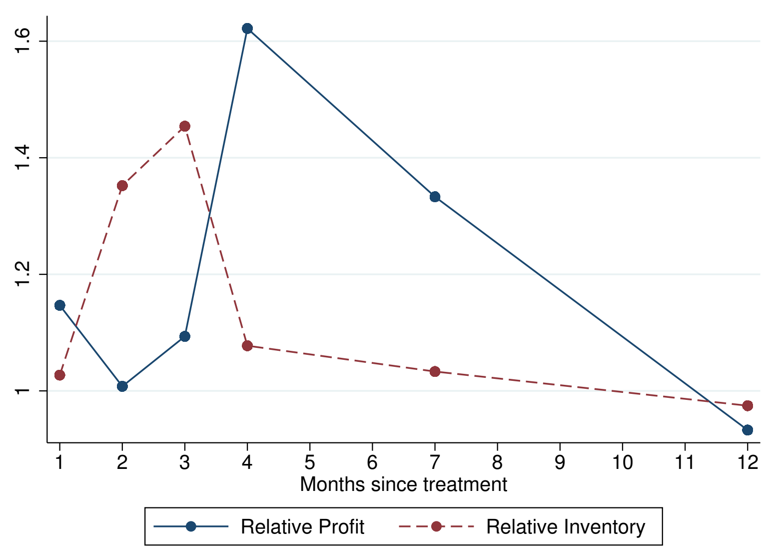
[9] In followup surveys, we asked about inventory spending in the previous week. In the baseline survey, we asked about inventory spending the last time owners went to the market. Hence, we exclude the pre-treatment values in Figure 8.

Figure 8: Inventory spending time series



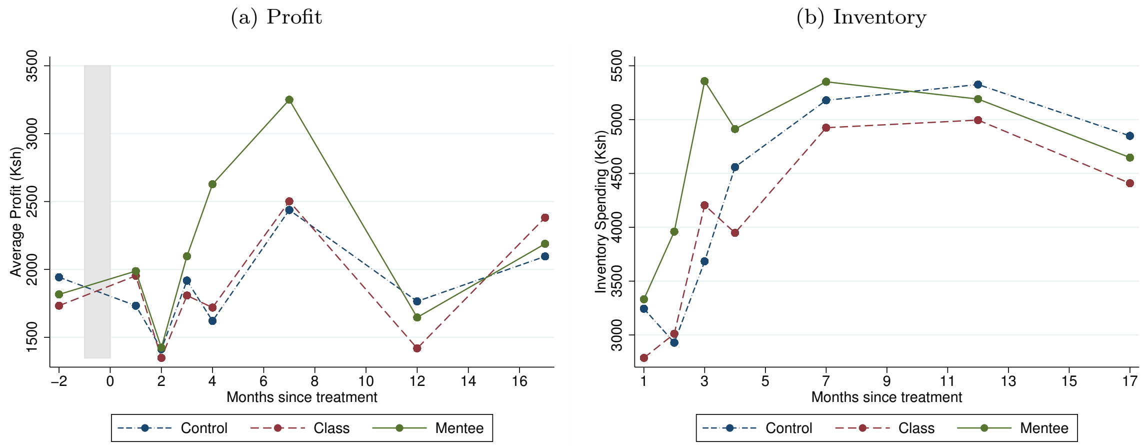

The average treatment effect on inventory masks important heterogeneity over time. The main increase in inventory expenditures occurs in the months preceding the increase in profit, implying a build up in inventory followed by an increase in profit. Graphically this can be seen in Figure 9, which plots the time series of inventory expenditures and profit for mentee relative to the control group. While inventory expenditures increase significantly in months two and three, the profit increases are concentrated between months four and seven.

Figure 9: Timing of Inventory and Profit Changes among Mentees



One alternative explanation is that the effect is seasonal. For example, perhaps mentors instructed mentees to do major inventory purchases in the beginning of the year. Then a potential long-term benefit only looks temporary within the context of one year. To answer this question we conducted a short survey 17 months after the treatment month asking only about profit and inventory. If the effect is seasonal, we should see an increase in profit for mentees. Figure 10 plots the profit and inventory time series including the $t = 17$ data, and shows that our results are not driven by seasonality or cyclicality.[10]

Figure 10: Profit and Inventory (including $t = 17$)




## 5.2 Match Persistence

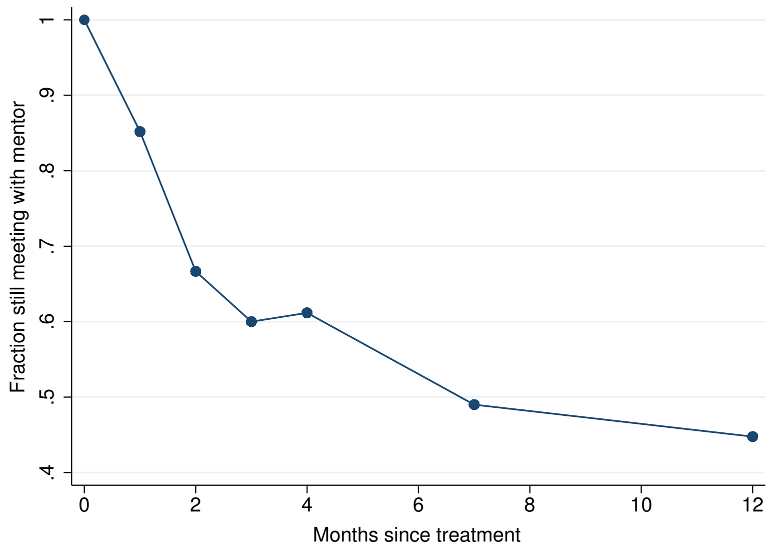
Despite the changes in inventory and suppliers, there is no long run effect on profit. This is due to the fact that matches dissolve over time, coupled with the substantial churn in suppliers. That is, mentees lose their access to the information provided by mentors.

To see this, first, Figure 11 plots the fraction of mentees still meeting with their mentor over the course of the study. As mentioned previously, everyone met with their mentor in the official treatment month. This fraction declines over time, though 45 percent were still meeting after twelve months despite the fact that we provided no incentives to continue the relationship.

If mentors are providing profitable information, then those mentees who continue to meet should see higher profit. The data is consistent with this. Twelve months after the

[10]We do not include these results in the broader analysis here because of the short nature of the survey. Their inclusion does not affect the nature of the results for either profit or inventory. All estimates are still significant, and there are only small changes in the pooled point estimates. They are available upon request.

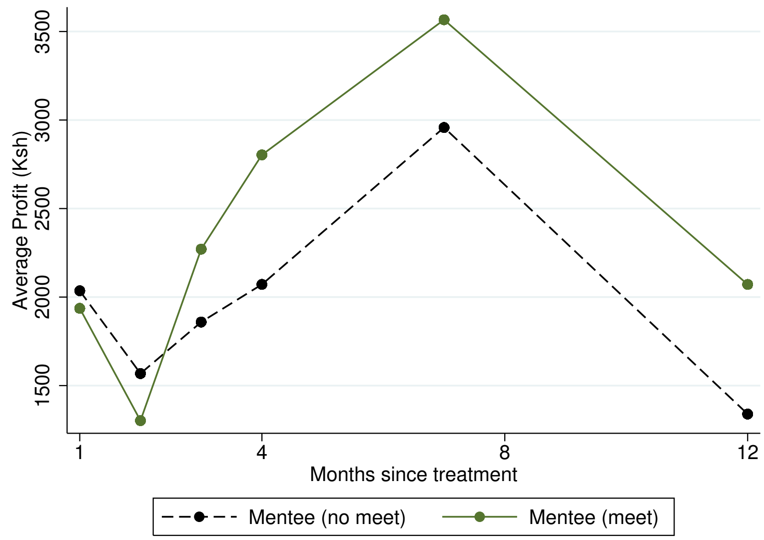
Figure 11: Fraction of mentees still meeting with mentor




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 holds despite the fact that the two groups on average have nearly identical baseline profit levels. Moreover, this result is not specific to the final wave, which can be seen in Figure 12, 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 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 12 is necessary but not sufficient to imply mentors continually deliver profitable information to mentees. An alternative explanation for Figure 12 is simply that mentees end the relationship with their mentor when all of the information has been extracted, and Figure 12 is due to selection by mentees. Distinguishing between these two explanations is critical to understanding the dynamics of the treatment effect. One way to do this is by testing whether previous profit realizations affect the likelihood of continued meeting. If mentees end the relationship when the benefit of the match has expired, changes in profit realizations should be negatively related to the likelihood of meeting. On the other hand, if the matches end despite the potential for continued mentee benefits, there should be no relationship between profit and meetings. We therefore ask whether changes in profitability

Figure 12: Average Profit for Mentees




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 12, 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. For further evidence, we asked mentees directly why they were no longer meeting with their mentors. Nearly 70 percent of those not meeting claimed it was due to the mentor ending the relationship, consistent the regression results in Table 12. This suggests that the cause of the decline in average effect is driven by the dissolution of matches by mentors coupled with the relatively short half-life of profitable supplier information.

# 6 "Generic" Skills from the Class

An alternative explanation for our results is that the businesses classes operated in the study simply transferred no knowledge or skill. We refute that argument here, and show that our results are consistent with previous work on training classes. That is, we see changes in underlying business practices, but they do not translate into profit changes.

In every survey, we asked about accounting and advertising practices, and Table 13 provides the time series of estimates using regressions (4.1) and (4.2). Marketing practices do not change relative to the control for either treatment. Accounting practices do change across treatments, and in fact we find a significantly larger impact among the class than the mentees. On average, 74 percent of the control does some sort of record keeping, compared 86 percent of those who receive in-class training (19 percent increase) and 77 percent of the mentees (7 percent increase). However, this effect is only present in the first four months following the treatment for the class treatment. This is consistent with short run changes in behavior found in other studies as well (e.g. Karlan et al., 2014), and implies that the in-class training does in fact change behavior without changing business outcomes.

We use this information to ask a final question about mentorship. We divide the mentee group into those whose mentor kept formal records and those that did not. The results are in Table 14. 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.

For further evidence that classes change business practices, in our $t = 7$ and $t = 12$ surveys we asked a much longer battery of business practice questions. The questions are primarily drawn from the survey instrument first used in de Mel et al. (2014) and McKenzie and Woodruff (2016) show a positive correlation between these practices and 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 in Appendix C for disaggregated categories. Again, we see short run business practices among the class group, both a decrease in the marketing score and an increase in the stock score. These results highlight the fact that the class was indeed successful in generating changes in behavior, but that they did not translate into increased profit. Moreover, they demonstrate that the mentorship treatment impact is not simply driven by an initial absolute lack of knowledge relative to places previously studied, but that mentorship is effective even in circumstances where classroom study has the same effects as found previously.

# 7 Conclusion

We conduct a randomized controlled trial in which we assess different methods of learning among microenterprises in Dandora, Kenya. Our results show that interacting with a successful, local business owner generates a 20 percent increase in profit, but that the effect fades over time. We use the dynamics of the response to show that the result is driven by the dissolution of matches over time, as those that still meet earn higher profit. 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 local information we have shown to be important.

Our results also provide some micro evidence in support of a recent class of macroeconomic models – albeit in a relatively specialized setting – 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 suggests a number of potential extensions. We discuss two here. While recent work (BenYishay and Mobarak, 2015; 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: 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 3: 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 4: Initial Balance Test

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

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

Table 5: 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 6: 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 (H $\leq$ L) | 0.210 | 0.832 | 0.164 | 0.619 | 0.113 | 0.282 | 0.176 |
| One tailed t-test p value (H $\leq$ 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 7: 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 8: RD treatment effect with local linear regressions

| % of IK optimal bandwidth | Scale | | Practices | |
|---|---|---|---|---|
| | 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.

Table 9: 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 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: 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 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 $\sigma$ | 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 $\sigma$ | 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, ***.

# B Further Balance Tests and Attrition

Table 16: 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 17: 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 18: 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 19: 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 20: 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 21: 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 22: 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 ***.

## C Decomposition of Business Scores

Table 23: 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 24: 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 ($H_0 : \text{M} \leq \text{C}$) | 0.290 | 0.279 | 0.246 | 0.439 |
| Obs. | 306 | 306 | 306 | 306 |
| $\text{R}^2$ | 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 ($H_0 : \text{M} \leq \text{C}$) | 0.702 | 0.439 | 0.837 | 0.430 |
| Obs. | 306 | 306 | 306 | 306 |
| $\text{R}^2$ | 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 25: 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.