KELLOGG INSTITUTE
FOR INTERNATIONAL STUDIES

exploring DEMOCRACY and HUMAN DEVELOPMENT

working paper

THE DYNAMICS OF INTER-FIRM SKILL TRANSMISSION AMONG KENYAN MICROENTERPRISES

WYATT BROOKS, KEVIN DONOVAN and TERENCE R. JOHNSON

414

November 2016

exploring DEMOCRACY and HUMAN DEVELOPMENT

The Kellogg Institute for International Studies
University of Notre Dame
130 Hesburgh Center for International Studies
Notre Dame, IN 46556-5677
Phone: 574/631-6580
Web: kellogg.nd.edu

THE DYNAMICS OF INTER-FIRM SKILL TRANSMISSION AMONG KENYAN MICROENTERPRISES*

Wyatt Brooks, Kevin Donovan and Terence R. Johnson

Working paper #414 – November 2016

Wyatt Brooks is assistant professor of economics at the University of Notre Dame, where he is a concurrent faculty member of the Keough School of Global Affairs, and a Kellogg Institute for International Studies faculty fellow. His research focuses on international trade, macroeconomics, and development economics. A large part of his research studies the effects of international trade taking into account country characteristics. He has also done fieldwork in Nicaragua and Kenya. He holds a PhD from the University of Minnesota.

Kevin Donovan is assistant professor of economics, a concurrent faculty member of the Keough School of Global Affairs, and a Kellogg Institute for International Studies faculty fellow at the University of Notre Dame. His research interests lie broadly in growth and development. Recent work focuses on constraints to both rural and urban firm growth, including the aggregate consequences of agricultural risk and the impact of inter-firm knowledge transfer among urban microenterprises. He holds a PhD from Arizona State University.

Terence Johnson is the Joe and Deborah Loughrey Assistant Professor of Economics and Human Development and a Kellogg Institute for International Studies faculty fellow at the University of Notre Dame. Johnson investigates the informational and institutional inefficiencies that hamper markets in developing countries and uses tools from mechanism design and industrial organization economics to design and test solutions. Current studies focus on Burkina Faso, Ghana, and Kenya. His scholarly work appears in the *Journal of Economic Theory* and the *Oxford Handbook of Market Design*. He holds a PhD from the University of Maryland, College Park.

*Thanks to conference and seminar participants at Arizona State, Notre Dame, York University, the Chicago Fed M&M Development Workshop, Development Day (Michigan), the Econometric Society NASM (Penn), IGC Growth Week at LSE, and the World Bank ABCDE for comments and insights, especially Paulo Bastos, Nick Bloom, Paco Buera, Bill Evans, Joe Kaboski, Molly Lipscomb, Ezra Oberfield, and Chris Woodruff. We thank the Ford Family Program in Human Development Studies and Solidarity and the Kellogg Institute for International Studies for financial support, including thanks to Bob Dowd and Dennis Haraszko for their help coordinating the project and to Lawrence Itela, Jackline Aridi, and Maurice Sikenyi for their excellent work managing the project in Dandora.

ABSTRACT

We provide empirical evidence in favor of inter-firm productivity transmission among Kenyan microenterprises. We do so with a field experiment in which a random subset of young firms are matched one-to-one with an older, successful local firm, then use a seven-round survey over the course of a year to study the dynamics of the response. Profit is on average 20 percent higher among these young firms than in the control group. Treated firms are more likely to switch suppliers and have lower inventory costs, highlighting the importance of cost and supplier information. The benefits are driven by the flow of information rather than a permanent change in practices, as we show that the effect fades most among those whose matches dissolve earliest. We exploit our selection procedure with a regression discontinuity design to show that no observable benefits extend to the more successful, older firm in the match. Finally, we benchmark this treatment against a standard microenterprise training program. Training generates changes in business practices covered in the classes but no change in profit, consistent with previous work.

RESUMEN

Proveemos evidencia empírica a favor de la transmisión de productividad entre microempresas de Kenia. Realizamos un experimento de campo en el cual un subconjunto aleatorio de las empresas jóvenes es emparejado a razón de uno a uno con una empresa local más exitosa y con más antigüedad. Luego utilizamos una serie de siete encuestas en el curso de un año para estudiar la dinámica de los efectos del experimento**.** La ganancia de las empresas jóvenes emparejadas es en promedio un 20 por ciento mayor a las empresas que están en el grupo control. Las empresas tratadas son más propensas a cambiar de proveedor y tienen costos de inventario menores, resaltando la importancia de la información sobre los costos y proveedores. Los beneficios son impulsados por el flujo de información y no por un cambio permanente en las prácticas, tal como se muestra que el efecto se desvanece más entre aquellos cuyo emparejamiento se disuelven más temprana. Explotamos nuestro procedimiento de selección con un diseño de regresión discontinua para mostrar que no hay beneficios observables que se extienden a la empresa más exitosa y con más antigüedad. Finalmente, comparamos este tratamiento a un programa de formación de microempresas estándar. Formación genera cambios en las prácticas comerciales tratados en las clases de formación, pero ningún cambio en la ganancia que es consistente con el trabajo previo.

1 Introduction

Microenterprise operation accounts for a large share of employment in many developing countries. Despite the size of this sector, these businesses face many challenges including poor access to capital and products, high failure rates, and stiff competition. It is therefore no surprise that in many countries, microenterprises underperform other business ventures on factors such as productivity and profitability. One possible explanation for these outcomes is that microenterprise owners lack what Bloom and Van Reenen (2007) and Bruhn et al. (2010) refer to as managerial capital, which is the skill or know-how to run a business. In response, policy makers and academics have tried to increase managerial capital through business training, spending over a billion dollars per year pursuing various forms of training (Blattman and Ralston, 2015). Despite this effort, training has had limited success increasing business profit or operational scale (McKenzie and Woodruff, 2014).

Of course some small businesses are successful, even in the most adverse business conditions. These business owners are more likely to be equipped with the skills and knowledge to successfully operate a business in their local economy. We assess whether interaction between a randomly chosen successful business owner and a young, inexperienced microenterprise owner facilitates the transfer of managerial skill or information, and in turn, increases profit. We do so with a field experiment in the dense, urban slum of Dandora, Kenya, located on the outskirts of Nairobi. We randomize 372 young, female-operated businesses into one of three groups: access to a local, successful female business owner (a "mentor"), free entrance into a standard business training program, and a control group that received neither. Mentors–who are randomly assigned to microenterprise owners 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 than the average Dandoran business. The treatment was the pairing of the two business owners. With small cash payments, we incentivized four meetings over the course of a month with no continued support for additional interaction. This treatment was sufficient to generate continued meetings: half were still meeting 17 months after the official treatment period ended. As a comparison to this treatment and to previous literature on training, we also randomly assigned another group of microenterprise owners to receive formal classroom business training, a common form of microenterprise support by both academics and policy institutions (see McKenzie and Woodruff, 2014, for a review). 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 new interaction with a successful firm, 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 owner is an effective means to increase profit among microenterprises. Over the twelve months following treatment, those receiving the mentor treatment have weekly profits that are 20% higher on average compared to those in the control group. The mentors themselves are participating in the study, and might also experience gains or losses as a consequence of participation. In particular, mentor profit might increase if there are economies of scale or scope between a mentor and mentee, or decrease if mentors expend time or other valuable resources participating in the study. We exploit our mentor selection procedure with a regression discontinuity design to identify the treatment effect on mentor profit and business practices.[1] After resurveying the mentors and 95 business owners just below the cutoff used to select mentors, we find that mentors who just make it into the selected group have no higher profits than those just below. This implies one-way flows of skill and knowledge to the mentee, and is consistent with models of knowledge transfer or diffusion, 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, 2009; Lucas and Moll, 2014; Buera and Oberfield, 2015).

The results from the formal business training are in line with previous literature. This treatment generates a statistically insignificant 1% increase in profit relative to the control, which is unsurprising given previous studies summarized in McKenzie and Woodruff (2014) and Blattman and Ralston (2015). While profit does not change, we do find short-run changes in business practices. The 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.

Our evidence on how mentoring changes business practices suggests that interacting with a more successful business allows microenterprise owners to overcome frictions associ-

[1] As mentioned previously, mentors were incentivized with small cash payments. To facilitate comparability, our experimental design delivered identical cash transfers to those we selected into RD but did not choose to act as mentors. This is discussed in detail in Section 4.2.

ated with finding low cost suppliers. Mentees are nearly 40% more likely to have switched suppliers in the aftermath of the treatment, which generates lower inventory costs and more inventory spending. Average inventory costs in the mentor treatment are on average 18% lower than control, despite increases in inventory quantity purchased over this time period. We find little evidence of changes in firm quality (customer relations, marketing, production quality, etc.) among the mentor treatment, which are potentially less profitable practices for microenterprises facing relatively low demand. Interventions that do find these changes either concurrently increase demand (Atkin et al., 2016; Hardy and McCasland, 2016) or focus on larger firms (Bloom et al., 2013). Instead, our results highlight the importance of local, market-specific information on suppliers. This is in contrast to formal training, which is designed to provide benefits regardless of the market in which it is employed. Indeed, among firms in the formal training arm of the experiment, we find behavior changes along these margins covered in the class, yet we find no change in profit. Our results therefore provide one potential explanation for the relatively small profit effect of training found in the literature: the information provided in classes is too general in nature. These small businesses benefit from local, context-specific information.

While these average effects highlight the key channels, we also use the panel dimension of our data to study how these effects vary over time and find important heterogeneity. Average profit in the mentor treatment is 30% higher than control after four months and remains approximately the same up to seven months post-treatment,[2] but after twelve months, there is no significant profit difference across any of the three groups. The evidence suggests that this result is due to a combination of substantial churn in suppliers and the dissolution of matches over time. First, note that while all mentor-mentee pairs during the treatment period, that fraction drops to 60% after four months and 45% after twelve months. If mentors provide the link to profitable information about suppliers, more than half lose access to this link by the later part of the study. How this impacts the dynamics of profit depends on the persistence of the benefits derived from this information. Consistent with the profit time series, we find that nearly 60% of the control switches suppliers during the study period, supporting the notion that the profitable information transferred as part of the treatment has a short half-life. Moreover, the average treatment effects on inventory expenditures and unit costs are concentrated in the early periods post-treatment, when most were still

[2]This large point estimate is in part driven by the low baseline profit of the young firms we focus on. 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.

meeting with their mentors. While average inventory cost in the mentorship treatment are 30% lower than control in the first four months after the treatment, there is no difference across treatment arms twelve months later. Thus the effect on both profit and the underlying inventory savings fades as matches dissolve.

For further evidence in support of this explanation, we exploit variation in outcomes between mentees who continue to meet with mentors and those who do not. Despite nearly identical average profits at baseline, those who continue to meet with their mentors twelve months after the official end of the treatment period are on average 55% more profitable than those who are no longer meeting. However, the result could be simply survivor bias if matches with future benefits are the only ones that continue. We show that this is not the case, making it unlikely that survivor bias is driving the results. Moreover, nearly 70% of matches were ended by the mentor as opposed to the mentee, limiting the effect of mentee selection out of matches. The evidence therefore suggests that the benefits are driven by the flow of information from mentor to mentee rather than a permanent change in practices.

1.1 Related Literature

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), despite billions of dollars spent by governments and international organizations. Our training intervention is consistent with these results. The mentorship intervention, however, overcomes two issues in this literature. First, in-class training requires diagnosing what skills the entrepreneurs are missing, and then designing a curriculum to effectively address those deficiencies. This has proved difficult, in part because of the large number of different constraints facing microenterprises (Bruhn et al., 2013; Karlan et al., 2014). Our intervention allows each mentor-mentee pair to diagnose and discuss information or skills they believe to be most useful. Second, building evidence on the constraints facing businesses is expensive. Classroom training usually can cost over $100 per student (Blattman and Ralston, 2015), while more personalized consultants can cost as much as $11,000, as in the case of Bruhn et al. (2013). Mentors were paid $9.83 for attending four meetings, yet 45% were still meeting 17 months after the official treatment period. Our training class, on the other hand, cost $40 per student enrolled.

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

Lastly, our results highlight a particular micro-foundation for distortions to managerial capital accumulation, albeit in a relatively specialized setting. Bhattacharya et al. (2013) and Da-Rocha et al. (2014) show that frictions limiting managerial capital accumulation can have an important aggregate effect in the context of policy distortion models developed in Restuccia and Rogerson (2008) and Hsieh and Klenow (2009). 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 spillovers from other firms. Our empirical results support the mechanisms of these models. First, firms are capable of increasing profit through interaction with other firms, even in the case of exogenous matching. Second, interaction with a more profitable firm has a larger effect. Third, these benefits only accrue to the less profitable member of the match.

2 Microenterprise Characteristics in Dandora, Kenya

Dandora is a dense, urban slum to the northeast of Nairobi. It is approximately four square kilometers, and as of the 2009 census, contained 151,046 residents. To assess the business characteristics in the area, we conducted a street-level survey of 3,290 randomly selected business.[3] Table[4] 1 provides summary statistics for business. Column three also includes the same information for "young" firms with owners under 35 years old and less than 5 years

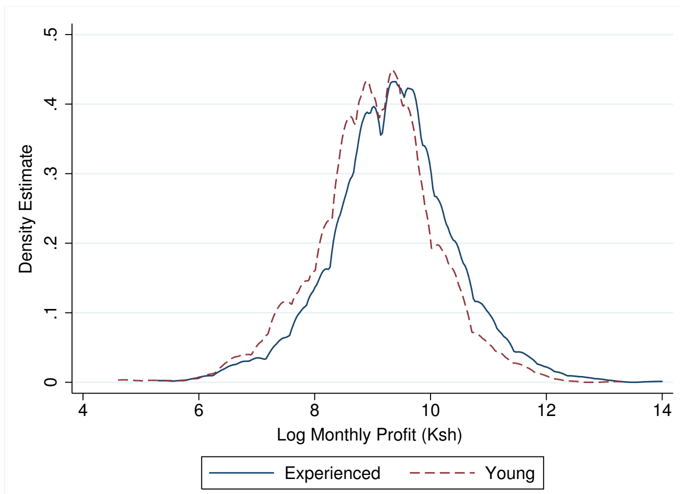
[3] Our sampling procedure was intended to reduce convenience sampling, and worked as follows. We generated 200 points randomly throughout the city, and then gave each enumerator a list of randomly selected numbers. Starting from a randomly selected point, they were instructed to count businesses until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

[4] All tables are collected in Appendix A.

of experience, as we eventually draw our sample from this group. These businesses make up 43% of all businesses surveyed. The rest of the businesses–those with owners older than 40 or more than 5 years of experience–we classify as "experienced."

The average business in our survey has profit of 16,899 KES ($167) in the previous month. This is approximately 72% above GDP per capita in Kenya. However, while the average young owner earns 14,266 KES, the average experienced owner earns nearly 42% more profit per month or 20,168 KES. Figure 1 plots the distribution of log profit for young and experienced enterprises.

Figure 1: Log profit distribution for young and experienced enterprises



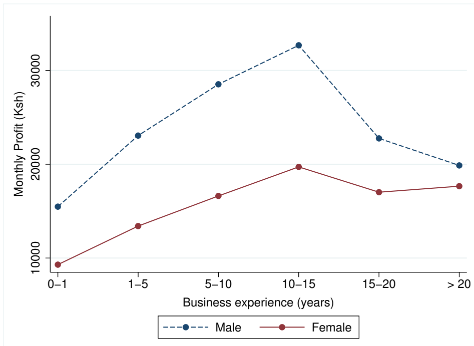
This result is driven by selection. However, despite the substantial difference in profit, there is not much difference in observable business practices. They are equally likely to offer credit to customers, have a bank account, have taken a loan at some point in the past, or engage in formal accounting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

We focus on female microenterprise owners, as they make up 71% of inexperienced owners. As Figure 2a shows, they are 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).

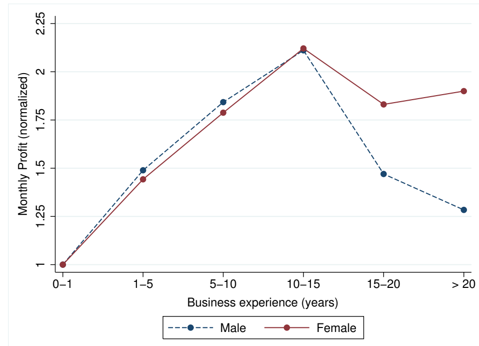
2.1 Self-Taught vs. Business Owners who Learn from Others

To motivate our study, we assess how profitability varies in the cross-section based on self-reported learning methods. Fifty-five percent of all firms claimed they were self-taught, where the rest claimed to learn either from another business operator, in school, or through

Figure 2: Gender differences over the lifecycle



(a) Average lifecycle profit



(b) Average lifecycle profit growth

an apprenticeship. We then consider various measures of business success across these two groups, and find that those who are self-taught make substantially less profit and operate at a smaller scale. Figure 3 plots three measures of business scale over the lifecycle.[5] First, Figure 3a shows that the self-taught earn less profit at any point over the lifecycle. The average self-taught firm has profit that is 82% of firms that learn from others. Other measures show similar patterns of self-taught operating at a lower scale than those who learned from others. Figures 3b and **??** show that the self-taught are less likely to have employees and pay a smaller total wage bill.

The results provide suggestive evidence that learning from others plays an important role in the profitability of business, though not all businesses have access to it. However, confounding factors (e.g. selection) limit our ability to say much more than this, and we therefore design an experiment to more formally investigate this idea.

3 Experimental Design

We use the baseline survey discussed in Section 2 to construct our experimental sample. We restricted our sample to business owners who are under 40 years old and have been running a business for less than 5 years. This 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 a high fraction of missing baseline data or extreme outliers in the baseline. Of these, 538 (68%) accepted our invitation to participate in the program. We set up relatively strict participation requirements due to the numerous follow-

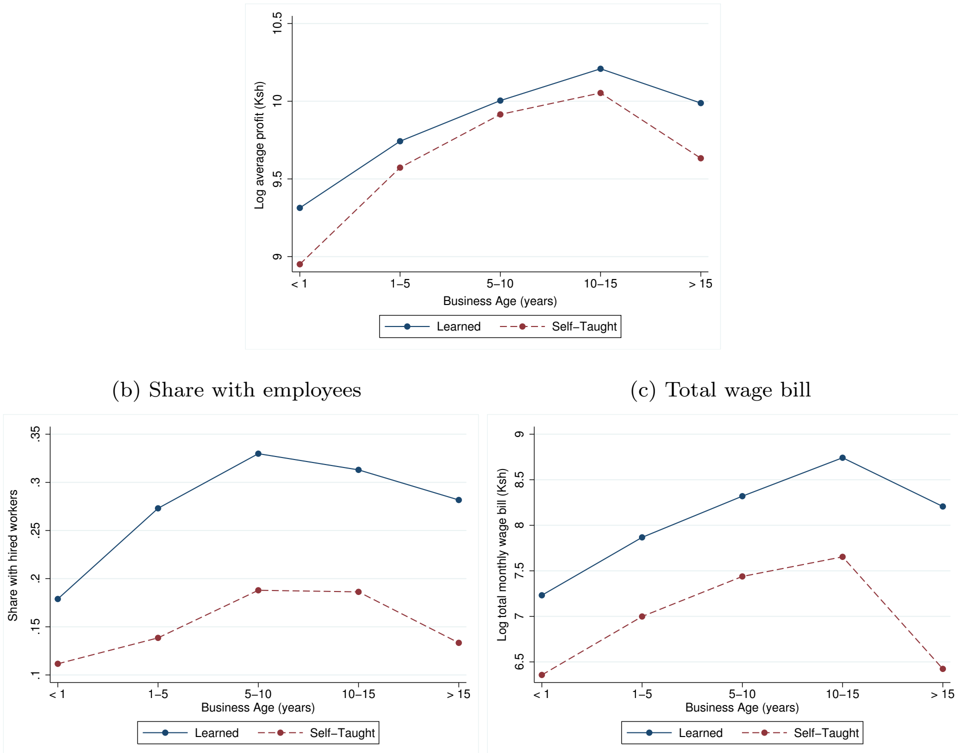
[5]Total employment looks quite similar to Figure 3b given there are so few firms that have more than one worker.

Figure 3: Business scale differences over the lifecycle

(a) Average profit

(b) Share with employees

(c) Total wage bill



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 KES ($48) to encourage participation, which is equal to approximately two weeks of average profit. The classroom treatment 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 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.[6] Classes were offered at a local hall in Dandora, and were offered six times over the course of 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. We refer to participants in this arm of the experiment as the classroom treatment.

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 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 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 follow-ups 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 $t \in \{1, 2, 3, 4, 7, 12\}$ and will reference December, January, February, March, June, and November, respectively.[7]

[6] 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.

[7] We conducted a much shorter survey 17 months after the treatment to assess the seasonality of the treatment effect (see Section 6). It is not included in the main analysis because it covered few variables of interest, but its inclusion does not change any of the results presented below.

3.1 Mentor Selection

To select the pool of mentors, we first constrained our sample 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

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

where π_i is baseline profit for individual i, δ_i is a sector fixed effect (manufacturing, retail, restaurant, other services) and age_i is age in years. Our mentors are chosen based on having the highest 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.

Mentors were offered 1000 KES to participate, worth approximately two days of profit for these most successful firms. We also told both mentors and mentees that meetings would take place at the mentor's business to minimize inconvenience to the mentor and maintain some similarity with the travel requirement of the class treatment.[8] These incentives were sufficient to generate high take-up, as 95% accepted our invitation to take part in the program. Figure 4 plots the distribution of $\widehat{\varepsilon}$ along with the cut-off.

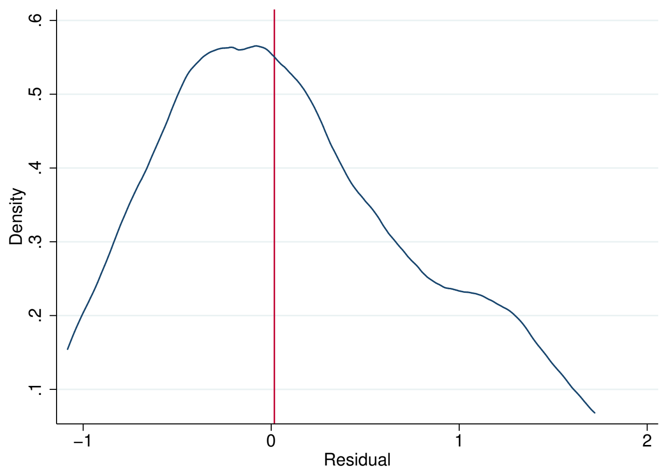
As expected, those selected to be mentors run substantially more successful businesses than those not selected by all metrics. Compared to the entire population at baseline, profit is about 4 times higher among mentors (20,205 KES to 5,967 KES), they have been in business eleven more years (24 to 13 years), and they are nearly three times as likely to have employees.

3.2 Sample Size and Balance across Survey Waves

Follow-up 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 follow-up. The response rates by wave were 352 (95% of 372), 318 (85%), 319 (86%),

[8] This meeting location requirement was not enforced, and a variety of different meeting procedures formed among different matches.

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



323 (87%), 325 (87%), so that after the first follow-up the response rate leveled off at 85%. In terms of number of follow-ups completed, 4 individuals completed exactly one follow-up, 6 completed two, 21 completed three, 41 completed four, and 108 completed five, and 194 completed all six. In Appendix 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 18 of Appendix B. A few observables are correlated with answering surveys at the 5% level, though none at 1%. However, the differences are small and we find little difference in estimation results with or without controlling for baseline factors.[9]

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% attended one of four classes, 17% attended two, 32% attended three, and 40% 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% had met with their mentor within the past week.

[9]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% of the age distribution answers 5.1 surveys on average, compared to 5.3 in the top 25% of the age distribution.

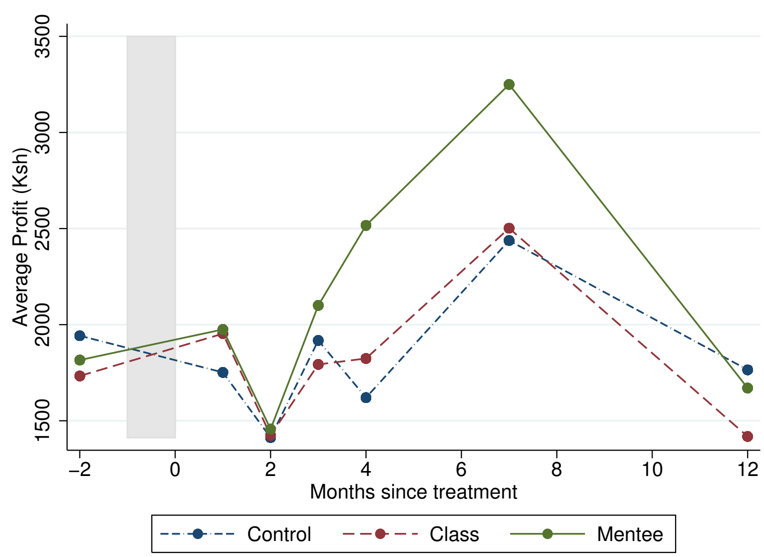
4 Empirical Results: Profitability

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.

Figure 5: Profit time series



The classroom treatment mimics the control group closely throughout the study period, and is actually lower than control in month 12.[10] The mentor treatment, however, sees a substantial growth in profit relative to both the control and class that we first pick up in month four and that lasts through month seven. However, this effect fades out by our twelve month follow-up, 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

[10]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.

run the following regression to measure the average treatment effect

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

Here, y_{it} is the outcome for individual i at time $t \in \{1, 2, 3, 4, 7, 12\}$ 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 classroom treatment at time t. X_i is a vector of baseline controls including secondary education, log age, and business sector fixed effects, and θ_t is a time fixed effect. All pooled regressions have standard errors clustered at the individual level. To understand the dynamics of the response, we run wave-by-wave regressions

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

Table 3 begins by considering the impact on business profit. On average during this time period, mentee profit is 339 KES higher than the control group, which is nearly 25% of baseline mean. The result is robust to including controls. The classroom treatment, 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 four months post-treatment. Looking back on Figure 5, this follows the general drop in demand following the Christmas holiday. In March 2015 (four months post-treatment), profit of the mentees is 896 KES more than control compared to 203 KES more in the class treatment. This result remains into July 2015 (7 months post-treatment), as profit is 812 KES 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.[11] 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

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

[11] 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.

where $M_{1it} = 1$ if i has a mentor from the bottom 25% 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%. The results are in Table 4. On average, having a mentor from the top 25% implies 11% higher profit relative to the bottom 25%. The coefficient estimates are increasing, consistent with the importance of the mentor's profit, but cannot be statistically distinguished from each 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

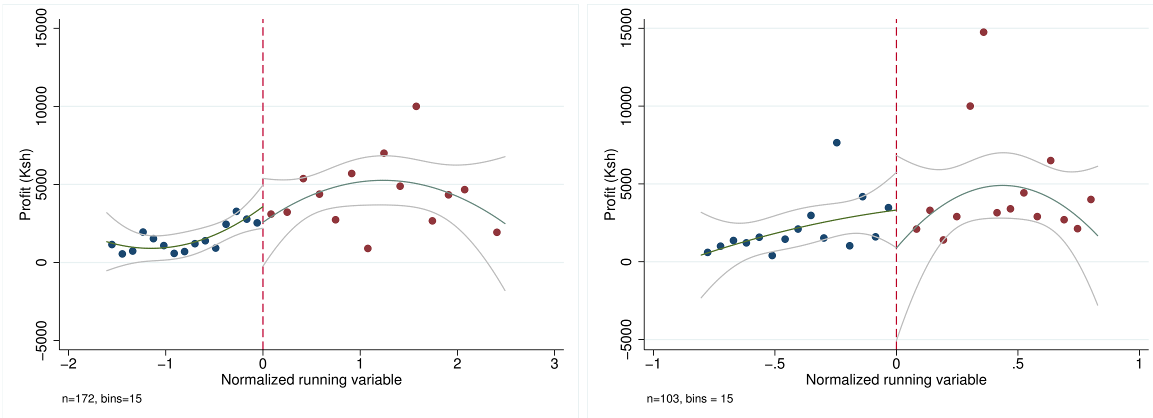
Since mentee profit increases on average, a second question is if there is any impact on the mentor. One possibility is that the mentor is an experienced agent who can impart knowledge to a less experienced business without receiving any return benefit from the interaction, an assumption made in most recent theoretical and quantitative work (e.g. Jovanovic and Rob, 1989; Lucas, 2009; Lucas and Moll, 2014; Buera and Oberfield, 2015). Alternatively, the mentor-mentee relationship may be better described as a collaboration or business group, where both sides gain from interacting with the other (e.g. Cai and Szeidl, 2015). We therefore ask whether the interaction implies any gains to the mentors themselves. However, we chose mentors because of their profitability, 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, 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% 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.

While Figure 6 suggests no discontinuity around the cutoff, we next assess this more

Figure 6: Profit for mentors and non-mentors

(a) Full sample (b) IK Optimal Bandwidth



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

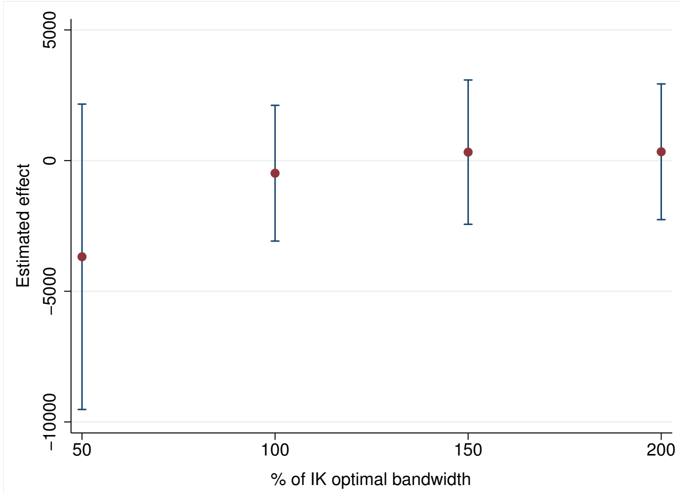
$$\pi_i = \alpha + \tau D_i + f(N_i) + \nu_i \tag{4.4}$$

where π_i is profit, $D_i = 1$ if individual i was chosen as a mentor ($\widehat{\varepsilon}_i \geq \bar{\varepsilon}$ in regression 3.1), $f(N_i)$ is a flexible function of the normalized running variable $N_i = (\widehat{\varepsilon}_i - \bar{\varepsilon})/\sigma_\varepsilon$, and ν_i is the error term. The parameter τ captures the causal impact of being chosen as a mentor. We use local linear regressions to estimate the treatment effects on profit and inventory, along with business practices of record keeping and marketing. The results are in Table 5, and there is no evidence that mentors benefit from being mentors. Figure 7 graphically shows the point estimate of the treatment effect on profit and the 95% confidence interval at 50, 100, 150, and 200% of the IK optimal bandwidth. An overly-restrictive bandwidth predicts a large but imprecisely estimated negative treatment effect, though it immediately disappears at reasonable bandwidth choices.[12]

Moreover, 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 the observable gains are driven by less productive member of the match, and moreover, mentee

[12] We also vary the functional form of $f(\cdot)$ to limit concerns about the assumed form, and it has no effect on the results.

Figure 7: RD treatment estimates with local linear regressions



gains do not come at a cost to mentors.

5 Understanding the Treatment Impact: Channels and Dynamics

Section 4 shows two key results. In the short run, mentorship generates a substantial increase in profit while the class does not. Neither treatment, however, generates any long-run change in profit. In this section we turn to understanding these results: we investigate the underlying changes that generate the profit results, and find 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.[13]

"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 and those who are 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 KES to 100 KES after making this change, while she kept her sale price exactly the same as before. She still meets with her mentor weekly.

[13] 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.

"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 from 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 Prudence, her mark-up has increased, as she has not changed her sale price in response to the lower cost.

These anecdotes point to an important difference between mentorship and classroom training. While topics in standard training classes are designed to span across markets, 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 use regressions (4.1) and (4.2) to more systematically analyze the validity of the hypotheses suggested 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. Lastly, in Section 5.3, we return to business skills covered in classroom training.

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. First, 62% of the the control switch suppliers during the treatment, implying substantial supplier churn in the economy independent of any intervention, and a point we return to in the next section when discussing the time series of profit changes. When assessing differences more formally using regression (4.2), we find a point estimate of 0.203 ($p = 0.002$) for mentees and 0.001 ($p = 0.987$) on the class treatment. Therefore, the mentorship treatment induced substantial changes in suppliers, as over 80% switched suppliers during the treatment.[14]

To test whether the supplier changes are beneficial, we first compute the time series of unit cost as

$$\text{Unit Cost} = \frac{\text{Revenue - Profit}}{\text{Number of Sales}}$$

[14] This result is independent of any controls being included, though the numbers quoted in the text include no controls.

so that we have a standard measure across all sectors in which firms operate. These results are presented in Table 7.[15] The estimates imply that mentees have a unit cost that is approximately 65% of the control in the early periods after the treatment, but the effect fades. The classes, on the other hand, have a similar trend to the control across all time periods. Therefore, changing suppliers lowers costs for mentees. Note, however, that like the effect on profit, cost savings fade as well: seven months post-treatment, both the mentorship and class treatment have a similar unit cost. Put differently, the lower cost faced by mentees wanes just before the effect on profit. Taken together, the results imply that having a mentor allows microenterprise owners to more easily seek out lower cost suppliers.

Consistent with the importance of suppliers, we also see an increase in inventory expenditures only among mentees. The regression results are presented in Table 6. On average over the year, mentees spend 15 to 20% more (depending on controls in the regression) on inventory than the control, while the class spends (a statistically insignificant) 2% more. We can again reject the hypothesis that the classroom treatment spends weakly more than the mentorship group. Figure 8 plots the time series of inventory spending, and shows that it is consistent with the evidence presented so far.[16] The main increase in inventory expenditures occur in months two and three, in which inventory spending is approximately 40% higher among mentees, while this effect disappears by month four. Put differently, the effect is concentrated in months immediately following the treatment, a time in which unit costs were lower for mentees but profit was still identical across the three treatment arms.

The changes related to suppliers and inventory highlighted above seem to be the only margin on which business scale changes. Regressing the treatments on other measures of scale, we show in Table 8 that there is no evidence of changes in employment, wage bill, or hours.

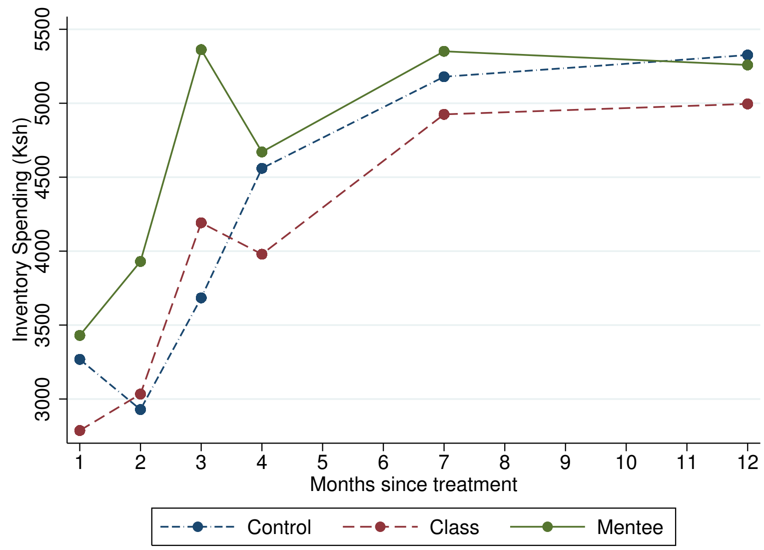
5.2 Match Persistence

We have shown that having a mentor increases profit, and primarily works through the ability to find lower cost suppliers. We lastly turn to assess why the effects fade over time. This is due to the fact that matches dissolve over time, coupled with the substantial churn

[15] We did not ask about quantity of sales in the first two rounds of the survey, so cannot compute cost for the first two waves post-treatment. At $t = 7$, we included a retrospective question about cost to create one unit of their most common product at $t = 1$. This is included in Table 7 for completeness. Though they are consistent with the results, the estimates are quite a bit different and higher than usual. This is most likely in part because of the difficulty in defining unit cost (especially retrospectively) outside of retail. We therefore do not include them in any other results, including the pooled regression.

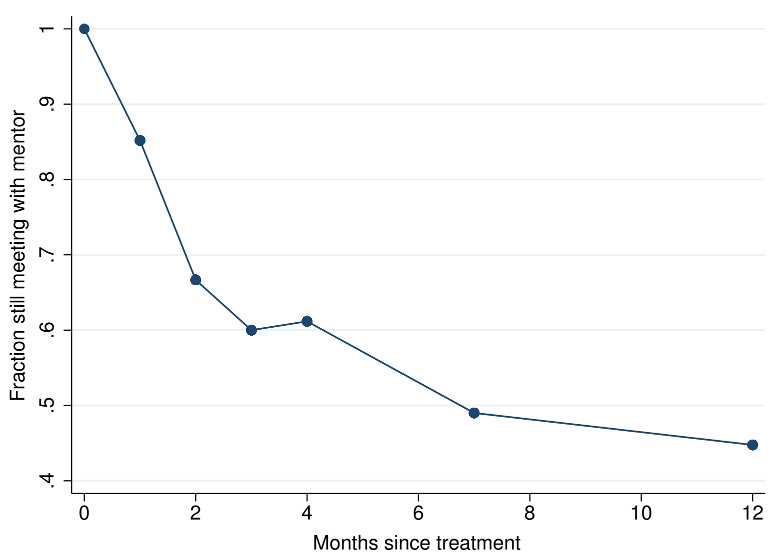
[16] In follow-up surveys, we asked about inventory spending in the previous week. In the baseline survey, we asked about inventory spending the last time owners went to the market. Hence, we exclude the pre-treatment values in Figure 8.

Figure 8: Inventory spending time series



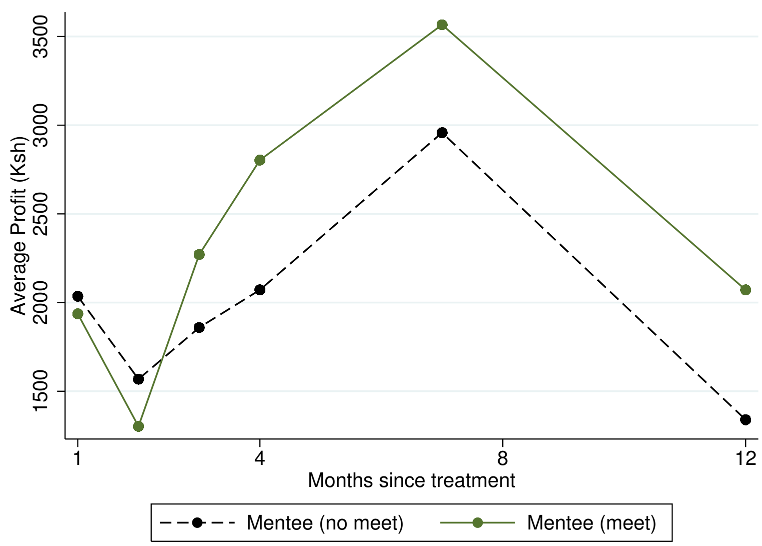
in suppliers. That is, mentees lose their access to the information provided by mentors. To see this, first, Figure 9 plots the fraction of mentees still meeting with their mentor over the course of the study. As mentioned previously, everyone met with their mentor in the official treatment month. This fraction declines over time, though 45% were still meeting after twelve months despite the fact that we provided no incentives to continue the relationship.

Figure 9: Fraction of mentees still meeting with mentor



As highlighted in the previous section, nearly two-thirds of the control group switch suppliers following the treatment, suggesting that the value of being able to find the proper supplier is short lived. To the extent that the mentor is the link to this information, mentees that continue meeting should see higher profit. The data is consistent with this. Twelve months after the treatment, average profit for those still meeting with their mentor is 2071.38 compared to 1339.47 among those not meeting–a difference of 55% (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 10, though it is largest in that wave. Four months after the treatment, profit is 35% higher ($p = 0.16$) for those still meeting with their mentor, and is 22% 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 each other in the relatively small sample.

Figure 10: Average profit for mentees



However, Figure 10 is not sufficient to imply that mentors continually deliver profitable information to mentees. That result requires identifying a positive counterfactual effect of continuing meetings after the meetings have already ceased in reality, which is of course an endogenous outcome. Alternatively, if Figure 10 is driven by selection, the counterfactual effect is close to zero. That is, mentees end the relationship with their mentors when meetings are no longer profitable. This would generate a time series like Figure 10, but imply no continued benefits if those mentees (counterfactually) continued to meet with their mentors.

To test this idea, we first asked mentees directly why they no longer met with their mentors. Nearly 70% claimed it was due to the mentor ending the relationship, suggesting selection is not the key issue.

We also run an additional test not subject to the biases that potentially entered into mentee answers (though we were careful to explain that the program had ended, and any answer would have no positive or negative effects on anything in the future). The idea of this test is the following. If a mentee ends her match when all benefits have expired, we should see profit decreasing over time leading up to the match dissolution. That is, if a mentee has high profit at t_1 and low profit at t_2, then the selection explanation implies the match should end. Therefore, the likelihood of meeting with her mentor at t_3 should be positively related to the change in profits $\pi_{t_2} - \pi_{t_1}$: those who continue to generate profit growth through meetings continue to meet, while those who do not are less likely to meet. Alternatively, if matches end for reasons unrelated to mentee profitability (e.g. the mentor tires of time away from her own business, the mentor moves to different city) then there should be no relationship between meeting and profit changes. We therefore ask whether changes in profitability affect the likelihood of meeting with a mentor in the future with the regressions

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

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 as in the selection case, we would expect to see $\widehat{\beta} > 0$. The results are presented in Table 9, and we find no evidence that meeting likelihood is responding to mentee profit realizations. Combined with the previous evidence, this suggests that the cause of the decline in average effect is driven by the dissolution of matches by mentors, not necessarily by a decrease in the impact of continued mentorship.

5.3 Skills Taught in Class

Lastly, we turn back to studying the business practices covered in the training classes. We show that–consistent with the existing literature–the classes had an impact on treated firms' behavior, but this did not translate into a statistically significant increase in profits.

In every survey, we asked about accounting and advertising practices, and Table 10 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% of the control does some sort of record keeping, compared 86% of those who receive in-class training (19% increase) and 77% of the mentees (7% 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.

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 that these questions are positively correlated with profit in a number of countries. Table 11 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 classroom treatment, 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 did not translate into increased profit. Moreover, they demonstrate that the mentorship treatment impact is not simply driven by an initial absolute lack of knowledge relative to places previously studied, but that mentorship is effective even in circumstances where classroom study has the same effects as found previously.

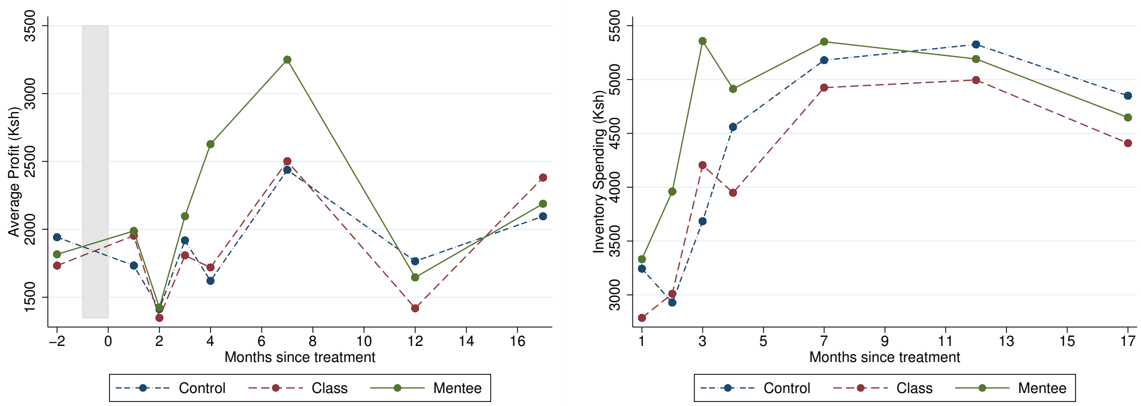
6 Ruling Out Alternative Explanations

6.1 Seasonality

An 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 appears 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 11 plots the profit and inventory time series including the $t = 17$ data, and shows that our results are not driven by seasonality or cyclicality.[17] While there is a cyclical component to the economy, it does not differentially affect the treatment groups.

Figure 11: Profit and inventory (including $t = 17$)

(a) Profit (b) Inventory



6.2 Transfers between Businesses

Another possibility is that mentors provided monetary help to the mentees. While interesting in its own right, it suggests constraints other than local information driving the results. Qualitatively, when asked about the most important benefit of having a mentor, only 6% mentioned anything related to financial help or transfers. In fact, only 2% reported receiving any financial help from their mentor. Financial transfers do not seem to be critical in increasing mentee profit.

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% 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

[17] 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 interpretation 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.

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.

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 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 deciding which links to form or what kinds of links to promote. Moreover, our results point to the importance of continued interaction with profitable business owners. Taken together, this research implies that understanding 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.

References

D. Atkin, A. Khandelwal, and A. Osman. Exporting and Firm Performance: Evidence from a Randomized Trial, July 2016. MIT Working Paper.

O. Bandiera and I. Rasul. Social Networks and Technology Adoption in Northern Mozambique. *Economic Journal*, 116(514):869–902, 2006.

L. Beaman, A. BenYishay, J. Magruder, and A. Mobarak. Can Network Theory-Based

Targeting Increase Technology Adoption?, June 2015. Northwestern University Working Paper.

A. BenYishay and A. Mobarak. Social Learning and Incentives for Experimentation and Communication. *Review of Economic Studies*, 2015. Forthcoming.

D. Bhattacharya, N. Guner, and G. Ventura. Distortions, Endogenous Managerial Skills and Productivity Differences. *Review of Economic Dynamics*, 16(1):11–25, 2013.

C. Blattman and L. Ralston. Generating Employment in Poor and Fragile States: Evidence from Labor Market and Entrepreneurship Programs, June 2015. Columbia University Working Paper.

N. Bloom and J. Van Reenen. Measuring and Explaining Management Practices Across Firms and Countries. *Quarterly Journal of Economics*, 122(4):1351–1408, 2007.

N. Bloom, B. Eifert, A. Mahajan, D. McKenzie, and J. Roberts. Does Management Matter? Evidence from India. *Quarterly Journal of Economics*, 128(1):1–51, 2013.

M. Bruhn and B. Zia. Stimulating Managerial Capital in Emerging Markets: the Impact of Business Training for Young Entrepreneurs. *Journal of Development Effectiveness*, 5(2): 232–266, 2013.

M. Bruhn, D. Karlan, and A. Schoar. What Capital Is Missing in Developing Countries? *American Economic Review Papers and Proceedings*, 100(2):629–633, 2010.

M. Bruhn, D. Karlan, and A. Schoar. The Impact of Consulting Services and Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico, June 2013. World Bank Policy Research Working Paper 6508.

F. J. Buera and E. Oberfield. The Global Diffusion of Ideas, June 2015. Chicago Fed Working Paper.

J. Cai and A. Szeidl. Interfirm Relationships and Business Performance, December 2015. University of Michigan Working Paper.

T. Conley and C. Udry. Learning about a New Technology: Pineapple in Ghana. *American Economic Review*, 100(1):35–69, 2010.

J. Da-Rocha, M. Mendes Tavares, and D. Restuccia. Policy Distortions and Aggregate Productivityh with Endogenous Establishment-Level Productivity, November 2014. University of Toronto Working Paper.

S. de Mel, D. McKenzie, and C. Woodruff. Business Training and Female Enterprise Start-Up, Growth, and Dynamics: Experimental Evidence from Sri Lanka. *Journal of Development Economics*, 106(1):199–210, 2014.

A. Foster and M. Rosenzweig. Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture. *Journal of Political Economy*, 103(6):1176–1209, 1995.

X. Giné and G. Mansuri. Money or Ideas? A Field Experiment on Constraints to Entreprenuership in Rural Pakistan, June 2014. World Bank Policy Research Working Paper 6959.

M. Hardy and J. McCasland. It Takes Two: Experimental Evidence on the Determinants of Technology Diffusion, July 2016. University of British Colombia Working Paper.

C. Hsieh and P. Klenow. Misallocation and Manufacturing TFP in China and India. *Quarterly Journal of Economics*, 124(4):1403–1448, 2009.

G. Imbens and K. Kalyanaraman. Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *Review of Economic Studies*, 79(3):933–959, 2012.

B. Jovanovic and R. Rob. The Growth and Diffusion of Knowledge. *Review of Economic Studies*, 56(4):569–582, 1989.

D. Karlan, R. Knight, and C. Udry. Consulting and Capital Experiments with Microenterprise Tailors in Ghana. *Journal of Economic Behavior and Organization*, 2014. Forthcoming.

R. E. Lucas. Ideas and Growth. *Economica*, 76(301):1–19, 2009.

R. E. Lucas and B. Moll. Knowledge Growth and the Allocation of Time. *Journal of Political Economy*, 122(1):1–51, 2014.

D. McKenzie and C. Woodruff. What Are We Learning from Business Training and Entrepreneurship Evaluations around the Developing World? *World Bank Research Observer*, 29(1):48–82, 2014.

D. McKenzie and C. Woodruff. Business Practices in Small Firms in Developing Countries. *Management Science*, 2016. Forthcoming.

K. Munshi. Social Learning in a Heterogeneous Population: Technology Diffusion in the Indian Green Revolution. *Journal of Development Economics*, 73(1):185–213, 2004.

J. Perla and C. Tonetti. Equilibrium Imitation and Growth. *Journal of Political Economy*, 122(1):52–76, 2014.

D. Restuccia and R. Rogerson. Policy Distortions and Aggregate Productivity with Heterogeneous Establishments. *Review of Economic Dynamics*, 11(4):707–720, 2008.

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% of answers. 3171 establishments answered about profit.

Table 2: Balancing Test at Baseline

	Control Mean (1)	Class - Control (2)	Mentor - Control (3)
Firm Scale:			
Profit (last month)	10,054	-360.95	-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 3: OLS Estimates of Profit Equation at Different Time Periods

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 4: Heterogeneous Mentor Effects from OLS Profit Regression

	Pooled	Months since treatment (1)	(2)	(3)	(4)	(7)	(12)
Mentee: mentor in $(0, 25)$ pctile	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 $(25, 75)$ pctile	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 $(75, 100)$ pctile	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, 75-100, "high") is weakly less than the lowest (row 1, 0-25, "low"). The second is whether the highest group (row 3, 75-100) is weakly less than the middle (row 2, 25-75, "middle").

Table 5: Regression Discontinuity Results for Mentor Treatment Effect

% of IK optimal bandwidth	Scale: Profit	Scale: Inventory	Practices: Marketing	Practices: 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%, but results are robust to other (or no) procedures.

Table 6: OLS Treatment Effects on Inventory Spending at Different Time Periods

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 regressions 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 7: OLS Treatment Effect on Average Cost in Different Time Periods

Panel A: No controls		Months since treatment				
	Pooled	(1)	(3)	(4)	(7)	(12)
Mentee	-30.35	-428.98	-38.00	-75.95	-9.87	-0.25
	(23.47)	(175.74)**	(19.89)*	(31.48)**	(48.74)	(55.20)
Class	15.01	-249.01	29.09	-38.23	49.63	19.91
	(42.27)	(185.46)	(29.16)	(73.92)	(77.21)	(55.65)
Constant	104.81	764.17	100.74	211.43	193.95	162.48
	(20.60)***	(158.33)***	(17.68)***	(27.30)***	(41.85)***	(48.07)***
One tailed t-test p value ($H_0 : M \geq C$)	0.130	0.072	0.004	0.296	0.196	0.303
Obs.	1163	350	317	303	305	320
R^2	0.011	0.004	0.005	0.00	0.003	0.001
Controls	N	N	N	N	N	N

Panel B: Include controls		Months since treatment				
	Pooled	(1)	(3)	(4)	(7)	(12)
Mentee	-29.04	-426.83	-43.99	-78.47	-14.08	15.25
	(22.23)	(174.30)**	(21.53)**	(33.28)**	(47.20)	(48.67)
Class	5.83	-242.89	26.57	-44.97	42.59	1.50
	(41.06)	(183.44)	(28.50)	(73.55)	(78.23)	(57.19)
One tailed t-test p value ($H_0 : M \geq C$)	0.179	0.073	0.005	0.312	0.205	0.633
Obs.	1163	350	317	303	305	320
R^2	0.039	0.046	0.062	0.036	0.042	0.107
Controls	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, ***. Note that month 1 estimates are derived from self-reported recall of unit cost, asked at $t = 7$. This is included for completeness of the results. The pooled results include only months 3-12.

Table 8: OLS Treatment Effects on Business Scale Measures

Panel A: $t = 7$	Stock of inventory (KES)	Any employees?	Number of employees	Total wage bill (KES)	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 (KES)	Any employees?	Number of employees	Total wage bill (KES)	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 9: Relationship between Meeting with Mentor and Previous Profit Realizations

Panel A: Meet$_t$

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

Panel B: Meet$_t$ - Meet$_{t-1}$

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

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

Table 10: OLS Treatment Effects for Business Practices at Different Time Periods

Panel A: Record Keeping		Months since treatment					
	Pooled	(1)	(2)	(3)	(4)	(7)	(12)
Mentee	0.05	-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 regressions 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 11: OLS Treatment Effects on Aggregated Business Practice Measures used in McKenzie and Woodruff (2016)

Panel A: $t = 7$		Score Components		
	Aggregate z-score	Marketing z-score	Stock z-score	Record keeping z-score
Mentee	0.38	0.17	0.58	0.16
	(0.15)**	(0.16)	(0.13)***	(0.14)
Class	0.06	-0.24	0.50	0.03
	(0.17)	(0.14)*	(0.13)***	(0.14)
One tailed t-test p value (H_0 : M $\leq$ C)	0.011	0.004	0.258	0.162
Control σ	2.31	1.51	1.04	1.75
Obs.	306	306	306	306
R^2	0.015	0.025	0.072	0.003
Controls	N	N	N	N
Panel B: $t = 12$		Score Components		
	Aggregate z-score	Marketing z-score	Stock z-score	Record keeping z-score
Mentee	-0.18	-0.21	-0.13	-0.02
	(0.14)	(0.14)	(0.14)	(0.14)
Class	0.06	0.11	-0.05	0.01
	(0.14)	(0.14)	(0.14)	(0.14)
One tailed t-test p value (H_0 : M $\leq$ C)	0.968	0.990	0.703	0.593
Control σ	2.33	1.48	0.91	1.05
Obs.	320	320	320	320
R^2	0.015	0.025	0.072	0.003
Controls	N	N	N	N

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

B Further Balance Tests and Attrition

Table 12: 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 13: 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 14: 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 15: 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 16: 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 17: 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 18: 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 19: 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 20: Stock Practices Decomposed

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

Panel B: $t = 12$		Stock Score Components		
	Stock Score	Haggle with suppliers	Compare suppliers	Run out of stock
Mentee	-0.12	-0.03	-0.05	0.02
	(0.13)	(0.07)	(0.07)	(0.06)
Class	-0.05	-0.04	0.02	0.01
	(0.13)	(0.07)	(0.07)	(0.06)
Constant	0.92	0.65	0.44	0.19
	(0.09)***	(0.0.05)***	(0.05)***	(0.04)***
One tailed t-test p value (H_0 : M $\leq$ C)	0.702	0.439	0.837	0.430
Obs.	306	306	306	306
R^2	0.002	0.001	0.003	0.001

Table notes: Standard errors are in parentheses. Pooled regressions 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 21: 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 regressions 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.