Networks and Manufacturing Firms in Africa: Results from a Randomized Field Experiment*

Marcel Fafchamps† and Simon Quinn‡

September 7, 2016

Abstract

We run a novel field experiment to link managers of African manufacturing firms. The experiment resembles the many forms of interaction that business and community organizations offer to their members. The design features exogenous link formation, exogenous seeding of information, and exogenous assignment to treatment and placebo. We study the impact of the experiment on firm business practices outside of the lab. We find that the experiment successfully created new variation in social networks. We find significant diffusion of business practices in terms of VAT registration and having a bank current account. This diffusion is a combination of diffusion of innovation and simple imitation. At the time of our experiment, all three studied countries were undergoing large changes in their VAT legislation.

JEL codes: **D22, L26, O33**.

*Data collection and experimental implementation were funded by the World Bank; we thank Hinh Dinh for his constant support and encouragement. We thank Sourovi De, Simon Franklin, Anja Grujovic and Jono Lain for excellent research assistance on this project. We have appreciated the generous assistance of partner organisations throughout the research process: Economic Development Initiatives in Dar es Salaam, the Ethiopian Development Research Institute in Addis Ababa and RuralNet Associates in Zambia. We thank seminar audiences at CERGE-EI, CORE (Université Catholique de Louvain), the Ethiopian Development Research Institute, the Georgetown Public Policy Institute, Monash University, the 2012 NEUDC Conference (Dartmouth College), the 2013 Annual Conference of the Royal Economic Society, the University of Gothenburg, the University of Kiel, the University of Oxford, and the University of Sydney.

†**Freeman Spogli Institute for International Studies, Stanford University**; fafchamp@stanford.edu.

‡**Centre for the Study of African Economies ('CSAE') and Department of Economics, University of Oxford**; simon.quinn@economics.ox.ac.uk.

1 Introduction: A novel field experiment

A growing body of applied research finds that management practices differ substantially across firms — even firms of similar size in the same sector and country: Bloom and Van Reenen (2007, 2010). This is particularly true in developing economies, where the distribution of management quality appears — relative to the United States — to have a 'far larger left tail' (Bloom, Sadun, and Van Reenen, 2015; Bloom, Eifert, Mahajan, McKenzie, and Roberts, 2013). Such heterogeneity is one important correlate to 'persistent performance differences among seemingly similar enterprises' (Gibbons and Henderson, 2012; Syverson, 2011; Hsieh and Klenow, 2009).

This kind of heterogeneity — in both management practices and firm performance — presents a mystery: why don't best management practices diffuse from firm to firm? One possible explanation is lack of awareness due to inexperience and unfamiliarity. This may be more prevalent in countries with a short history of modern manufacturing: knowledge about best practices may remain isolated in closed social groups, and the segmentation of social networks may impede their diffusion. If this is the case, inciting entrepreneurs and managers to socialize and exchange views about business practices should be sufficient to trigger the diffusion of best practices. Our experiment is designed to test this simple idea.

Many business interventions aim to create an environment conducive to the diffusion of information among entrepreneurs. Business associations typically organize opportunities for entrepreneurs to socialize and share views. Churches, sports club, and other organizations (*e.g.*, Rotary or Lions Club) can serve a similar purpose, also on the basis of occasional socialization. Yet another example is when a head of state on an official visit abroad brings along a bevy of entrepreneurs to be introduced to others in the visited country. All these activities are costly, and yet they are undertaken with the hope that they will diffuse new business practices — *e.g.*, encourage innovation, exports, and the exploration of new markets. The treatment that we introduce mimics such activities, *i.e.*, we bring entrepreneurs together around a common interest task that encourages conversation about business practices.

There are several reasons why interventions of this type may fail. First, many best practices are industry-specific: their usefulness varies across firms and sectors (Bloom, Schankerman, and Van Reenen, 2013). Furthermore, competition among similar firms may become a barrier to technology diffusion along social networks (*e.g.*, Hardy and McCasland (2015)). To obviate these issues, we enlist subjects from

different sectors and we focus our attention on general-purpose practices such as having a bank account, advertising, and introducing new products. Second, even if a practice is relevant, adopting it may not be profitable for a particular firm (*e.g.*, Fafchamps and Söderbom (2014)). This possibility is probably more common in less developed countries, where less educated entrepreneurs are often less capable of benefiting from certain innovations. For this reason, we omit microenterprises from our experiment and focus instead on small and medium firms with permanent employees. It is also conceivable that competition is needed to pressure firms into changing their management practices, and such competition may be less intense in developing economies (Bloom, Sadun, and Van Reenen, 2015). To minimize this possibility, we concentrate on light manufacturing in a large urban center, where markets are thick and competition is present.

Many economists view networking as a valuable business strategy — for sharing information about customers or suppliers (McMillan and Woodruff, 1999; Greif, 1993), for meeting potential business partners (Casella and Rauch, 2002), for improving a firm's access to production technologies (Parente and Prescott, 1994; Conley and Udry, 2001, 2010), for guiding a firm's policies on executive pay (Shue, 2013) and for learning about promising investment opportunities (Patnam, 2013). This may be particularly true in developing economies, where business networks can often form an attractive substitute to the relatively high transaction costs required to use the market (Rauch and Casella, 2003).

Research on social interactions has often found evidence of large diffusion effects among peers. This particularly true of studies on peer effects on adolescent health behaviors (Bifulco, Fletcher, and Ross, 2011; Fletcher, 2010; Oster and Thornton, 2012), and on academic performance (Duflo, Dupas, and Kremer, 2011; Sacerdote, 2001; Carrell, Sacerdote, and West, 2013; Fafchamps and Mo, 2015). There is also a broader literature about adoption via social networks among adults in developing countries, documenting diffusion, for instance, in financial decisions (Bursztyn, Ederer, Ferman, and Yuchtman, 2014; Beshears, Choi, Laibson, Madrian, and Milkman, 2015); (Duflo and Saez, 2003; Banerjee, Chandrasekhar, Duflo, and Jackson, 2013); job information (Beaman, Keleher, and Magruder, 2015; Beaman and Magruder, 2012); and health inputs (Godlonton and Thornton, 2012; Oster and Thornton, 2012; Miller and Mobarak, 2014; Kremer and Miguel, 2007). In many of these situations, however, information is not used to secure a competitive edge. Things may be different between firms. If, for example, competitive pressures favor firms with better management techniques Bloom, Sadun, and Van Reenen (2015), we should expect firm managers to be reluctant to share business wisdom with their peers (*e.g.*,

Hardy and McCasland (2015)).

For these reasons, business networks form a pressing area for empirical research: such networks are fundamental to understanding heterogeneity in firm performance, and cannot be understood through analogies to peer effects in other contexts. However, apart from the exploratory work of Fafchamps and Söderbom (2014), remarkably little is known about diffusion of management practices along entrepreneurial networks. *Do management practices diffuse along such networks?* If so, *what kinds of management practices are affected by the behavior of an entrepreneur's peers? Can researchers and policymakers change a firm's network in order to encourage the diffusion of best management practices?*

In this paper, we report results from a novel randomized field experiment designed to measure peer effects among manufacturing firms in Africa. We run a business plan competition in Ethiopia, Tanzania and Zambia, in which aspiring young entrepreneurs present proposals for new enterprizes to managers of established manufacturing firms.[1] This intervention, which took place over several weeks, resembles the many forms of interaction that business associations and community organizations offer to their members – e.g., selecting new members, awarding prizes, deciding on activities and budgets, and other committee work. Business competitions are also becoming increasingly popular in Africa and elsewhere, often with entrepreneurs and managers serving as judges. Our intervention mimics this, using the same competition protocol in three different countries at the same time.

By randomly assigning firm managers to different judging committees, we sucessfully generate exogenous variation in firms' peer networks. To our knowledge, this is the first field experiment to exogeneously create new links between firms' managers. This exogeneously induced variation in business acquaintances makes it possible to study the diffusion of management practices through causal peer effects. The experiment also includes exogenous seeding of information and exogenous assignment to treatment and placebo, and we study the impact of the experiment on real firm behaviour outside of the lab.

Our experiment succeeded in creating new business links — in the sense that participants remembered the peers to whom they had randomly been introduced, and spoke to some of them after the experiment. Given the short time frame of the study and the limited magnitude of the treatment, it would probably be foolish to expect massive diffusion effects. We nonetheless find diffusion of VAT registration and of

having a bank current account, even after correcting for multiple hypothesis testing. We then run a series of heterogeneity tests to explore the mechanisms by which this diffusion occurs.

Our study makes two primary contributions. Our first contribution is to provide rigorous empirical evidence on business networks and the diffusion of business practices among firms. Diffusion processes have been widely studied, but experimental methods have seldom been used to study link formation and diffusion among firms. Results obtained in other contexts (e.g., diffusion of smoking or petty crime among teenagers) need not transfer to firms.

Second, the paper provides a methodological contribution on the use of experimental variation to study network behavior. Several studies have introduced exogenous variation in information to study the relevance of social links for diffusion (see, for example, Möbius, Phan, and Szeidl (2015) and Aral and Walker (2011)). But very few studies have experimentally varied network connections to measure the effect of peer relationships themselves. Centola (2010, 2011) shows how online networks may be created artificially to study behavioral diffusion in an experimental context (namely, registration for an internet health forum and participation in an internet-based diet diary). Similarly, several studies have considered the consequences of random student assignment to peer groups (Sacerdote, 2001; Zimmerman, 2003; Lyle, 2007, 2009; Shue, 2013), including two experimental studies in a developing country (Duflo, Dupas, and Kremer, 2011; Fafchamps and Mo, 2015). To our knowledge, our experiment is the first to take a similar approach with firm managers, using a novel experimental protocol that had large and significant effects on the creation of entrepreneurial linkages. In this way, our work shows that field experiments can be used not merely to study effects *within* firms or *between* firms (Bandiera, Barankay, and Rasul, 2011), but also effects through firm *peer relationships*. This observation has not been lost on Cai and Szeidl (2016), who have reproduced our experimental design in China, and also find some evidence of diffusion along experimentally created social links.[2]

The paper proceeds as follows. Section 2 outlines our experimental design, including our identification strategy. Section 3 describes our implementation of the design, in Ethiopia, Tanzania and Zambia. Section 4 summarises our results, and Section 5 concludes.

2 The experiment

2.1 Experiment protocol

The competition: To measure the effect of peer relationships on firm performance, we design an experiment in which managers of manufacturing firms are randomly matched to work together on a task. The task is related to the challenges of firm management and entrepreneurship — in order to create an environment that encourages participants to share experiences and opinions on management strategies. The task relates to real and large payoffs to encourage participants to take the task seriously, and it requires managers to interact on multiple separate occasions to give several opportunities for personal relationships to develop.

To devise a task that satisfies all these requirements, we organise a business plan competition in which aspiring young entrepreneurs pitch new business ideas to experienced firm managers, who act as judges and are our experimental subjects. Competitions such as ours are now being run in several African countries.[3]

In our competition, applicants are aspiring entrepreneurs aged between 18 and 25 (inclusive) and recruited through advertising by posters, radio and Facebook.[4] As part of the application process, aspiring entrepreneurs are required to complete a detailed questionnaire about their business proposal, and to submit a three-page written business plan. Competition judges assess these questionnaires and business plans, along with oral presentations. Judges were drawn exclusively among managers of African manufacturing firms.

Committee judges: Candidates are judged in two ways: by judging committees, and by 'non-committee judges'. Most judging committees comprise five or six judges, who work together to assess candidates. Each judging committee assesses 12 applicants.[5] This involves holding three meetings, each assessing four applicants. These meetings follow a clear protocol. Applicants enter the room one at a time. Each applicant speaks for about 10 minutes, then answers questions from committee judges for an additional 10 minutes. Judges then complete separate mark sheets, assessing different aspects of the applicant's performance and business idea. Committee members then discuss the applicant for a few minutes, before calling the next applicant. At the end of each meeting, the committee is required to reach a joint ranking of all of the candidates whom the committee has judged up to that point.[6] Each

committee is responsible for awarding one prize of US$1,000, given to the committee's highest-ranked candidate.[7]

We wish to ensure that committee members interact in as natural a manner as possible, with suggestions and interjections flowing in a natural group conversation. For this reason, we prescribe no specific protocol by which committee members are to discuss candidates or to reach their decision. As with a criminal jury, we require only that each committee chooses a chair and reaches a final consensus ranking at the end of each meeting (which every committee did). Each committee judge then receives about US$25 for each session.

At the conclusion of the competition, we hold a prize-giving ceremony in each country. These ceremonies are attended by the committee judges and the competition winners. Judges at these ceremonies receive free food and drinks, and are seated with their other committee members. These ceremonies are designed to thank participants and congratulate the successful aspiring entrepreneurs — and to provide an opportunity for informal social engagement between committee members so as to reinforce the treatment.

Non-committee judges: Candidates are also assessed by 'non-committee judges'. These judges assess the submitted business plans individually, assigning scores without seeing the applicants' oral presentations, and without conferring with other judges.[8] Each non-committee judge attends only once, and receives about US$25. The role of the non-committee judge is therefore designed to act as a placebo to the committee judges: non-committee judges were randomised from the same pool of firm managers as the committee judges and were exposed to the same pool of new business proposals. We will estimate only on firms that participated in the experiment; that is, firms whose representatives were either committee judges or non-committee judges.

Assignment of judges: Judges are assigned to their tasks randomly. Each judge attends the competition venue at an agreed time. To maximise participation, judges are allowed to choose their preferred competition session.[9] Having arrived at this session, judges are then randomly assigned either to act as a non-committee judge, or to join a specified judging committee. This assignment is done by having participants draw cards from a bag. The use of a 'physical randomisation device' is intended to reassure participants that assignment is random (Harrison, Humphrey, and Verschoor, 2010).

Distribution of factsheets: At the conclusion of the prize-giving ceremonies, we distribute factsheets to both committee and non-committee judges. Three of the factsheets summarise descriptive results from the baseline survey. These results are grouped into topics of 'labour', 'innovation' and 'exporting'. A fourth factsheet relates to the implementing research group (the Centre for the Study of African Economies at the University of Oxford). The distribution of factsheets is designed to introduce random variation in information between participants, to provide a further basis for testing information diffusion. The factsheet assignment — that is, random distribution of descriptive information from an earlier survey — is loosely styled on the work of Jensen (2010).

Two-thirds of the judges each receive two factsheets; the other one-third receive none. The assignment of factsheets to judges is randomised, such that each possible pairing of factsheets is equally likely. In appendix we provide further details of the randomisation and show the English-language versions of the factsheets.[10]

Dyadic data: Our follow-up survey (discussed shortly) includes a set of dyadic questions, that is, questions in which respondent i is asked directly about respondent j. For committee judges, we ask about (i) all other judges who served on the same committee, (ii) a random sample of other committee judges who participated in the competition, and (iii) a random sample of non-committee judges who participated in the competition. For non-committee judges and entrepreneurs who did not participated, we ask about a random sample of committee judges and a random sample of non-committee judges. We ask each respondent about 10 committee judges in total, and five non-committee judges. Judges are identified to respondents by name and firm – for example, "I will now ask about Mary Smith, from Alpha Manufacturing…". For each of the 10 listed names, the respondent is asked whether the recall this person and whether they talked to this person since the Aspire competition ended. If they recall talking to this person, we ask the respondent whether they discussed three specific topics: export strategies; labour management; and innovation and business advice.

2.2 Identification strategy

Creation of network links: We begin our analysis by measuring the effect of the experiment on network formation. We do this by testing whether judges remember being on the same committees, and whether judges have had any discussions since the experiment. We use a very simple dyadic regression

structure; having asked firm i about firm j, we estimate:

$$y_{ij} = \alpha_0 + \alpha_1 \cdot S_{ij} + \varepsilon_{ij}, \tag{1}$$

where y_{ij} is some outcome of interest (for example, a dummy for whether the representative of firm i said that (s)he had spoken to the representative of firm j), and S_{ij} is a dummy for whether i and j were on the same committee together.[11] We use the dyadic clustering method of Fafchamps and Gubert (2007).[12]

We begin by considering whether respondents remember having been on the same judging committee, defining y_{ij} as a dummy for whether judge i answers in the affirmative to the question, "Were you on a judging panel with this person?".[13] We expect that judges on the same committee will be much more likely to answer 'yes' (indeed, if all respondents had perfect recall, we would have $\beta_0 = 0$ and $\beta_1 = 1$). We go on to estimate whether judge i spoke to judge j, and then consider topics of discussion (namely, whether the judges discussed 'export strategies', 'labour management' and 'innovation and business advice').

Diffusion of business practices: Several papers have studied natural experiments in which peers are randomly matched. Sacerdote (2001) studies the consequences of random assignment of of room-mates and dormmates at Dartmouth College; he argues that matched peers exhibit significant positive correlation in academic results and joining of social groups. However, even peer groups formed by random assignment are susceptible to common shocks; for this reason, positive correlations between peers' outcome variables need not imply network diffusion. This has been emphasised by Lyle (2007, 2009) in studying academic peer effects among cadets at West Point. Lyle argues that researchers should estimate network diffusion by considering the effects of peers' *pre*-assignment characteristics (see also Zimmerman (2003)). This approach has been adopted in several subsequent papers, including by Duflo, Dupas, and Kremer (2011). This is the approach we take. To measure diffusion, we use a 'linear-in-sum' specification, in which we explain a firm's management practices at follow-up by the number of its peers having adopted particular management practices at baseline. The management practices that we consider are each represented by dummy variables; we therefore nest the linear-in-sum specification within a probit model. (This follows directly the general approach of Banerjee, Chandrasekhar, Duflo, and Jackson (2013), who nest a linear-in-means specification within a logit model.)

Specifically, for firm i in randomization session s at time $t = 1$, we estimate:

$$\Pr\left(y_{is1} = 1 \,\middle|\, \{y_{js0} : j \in \mathcal{C}_i\}, y_{is0}, \sum_{k \in \mathcal{S}_i} y_{ks0}, n_s\right)$$
$$= \Phi\left(\beta_0 + \beta_1^p \cdot \sum_{j \in \mathcal{C}_i} y_{js0} + \beta_1^n \cdot \sum_{j \in \mathcal{C}_i} (1 - y_{js0}) + \beta_2 \cdot y_{is0} + \beta_3 \cdot \sum_{k \in \mathcal{S}_i} y_{ks0} + \beta_4 \cdot n_s\right), \quad (2)$$

where y_{is1} is a dummy for whether the firm follows a particular management practice, $\mathcal{S}_i$ is the set of firms in the same randomization session as firm i (with cardinality n_s) and $\mathcal{C}_i$ is the set of other firms on the same committee as firm i (defined as an empty set for non-committee judges). Therefore, the term $\sum_{j \in \mathcal{C}_i} y_{js0}$ is the sum of firm i's committee peers who had adopted the same management practice at the time of the baseline survey. β_1^p is our main parameter of interest; if firm i is more likely to adopt a management practice because it had more peers who had adopted by baseline, we will estimate $\beta_1^p > 0$.

We also include the sum of peers *not* adopting at baseline, $\sum_{j \in \mathcal{C}_i} (1 - y_{js0})$.[14] This allows us to test between two alternative mechanisms for diffusion. If $\beta_1^p = -\beta_1^n$, firms are merely imitating their peers: they are more likely to adopt a management practice if more of their peers have done so, and *less* likely to adopt if *fewer* of their peers have adopted. But if $\beta_1^p > 0$ and $\beta_1^n = 0$, we have an asymmetric process: a firm is more likely to adopt if it had more peers who had adopted, but the firm's decision to adopt is unaffected by the number of peers not adopting. This asymmetric process is similar to Rogers's (1962) famous notion of 'diffusion of innovations', and to 'infection' models of diffusion (Kermark and McKendrick, 1927; Banerjee, Chandrasekhar, Duflo, and Jackson, 2013).

To these terms we add several controls. First, we add the lagged dependent variable, y_{is0}; this is because, even where groups are formed randomly, y_{is0} correlates with $\mathcal{C}_i y_{js0}$, so its omission creates an endogeneity problem (see Guryan, Kroft, and Notowidigdo (2009) and Caeyers and Fafchamps (2016)). Second, we add the sum of adopters in the randomization session, and the size of that session; this controls for possible endogeneity by self-selection into the randomization session.[15] We continue to cluster observations by judging committee (where, as before, non-committee judges are defined, for clustering purposes, as each comprising a single-judge committee).

Inference with multiple outcomes: Our experiment is designed to test for diffusion across a wide range of different business practices. We use two methods for inference in this multiple-hypothesis con-

text; we use these methods both for estimating the perceptions of business networks and for estimating the diffusion of business practices. Our primary method of dealing with multiple outcomes is the 'sharpened q value' approach of Benjamini, Krieger, and Yekutieli (2006). This requires us to group outcomes into related families; the q value then controls for each family the False Discovery Rate ('FDR'), 'the expected proportion of rejections that are type I errors' (Anderson, 2008) (see also Casey, Glennerster, and Miguel (2012)). We also report standard p-values for each estimation separately; this is the appropriate measure for a reader interested in diffusion of some particular business practice, ignoring the fact that we tested multiple outcomes (for example, if a reader is interested specifically in whether VAT registration diffuses through networks).

3 Experiment implementation

3.1 Sample

We ran this experiment in 2011 in Ethiopia, Tanzania and Zambia. Participating manufacturing firms were initially surveyed between November 2010 and January 2011, as part of a World Bank study on 'African Competitiveness in Light, Simple Manufactured Goods'.[16] In each country, a sampling frame was constructed from firm lists obtained from the Bureau of Statistics, Chambers of Commerce and other similar organisations. These sources do not provide sufficient coverage of small and informal firms, so the sampling frame is complemented by firms selected in geographical areas with a concentration of informal firms.

Our sample was divided approximately equally between five main manufacturing sectors: food process, garments, leather, metal and wood. Within each firm, we interview someone in a senior management position — in most cases, the firm manager.[17] On average, our firms have about six permanent employees, with 20% being owned by women. The Online Appendix describes our sample in more detail, and shows that our randomisation between committee and non-committee status was balanced. We conducted a follow-up survey in each country between November 2011 and January 2012.

3.2 Running the experiment

The Aspire Business Ideas Competition was run simultaneously in Addis Ababa, Dar es Salaam and Lusaka in July and August 2011. 192 competitors participated in Ethiopia. In Tanzania, the number was

179. In Zambia, where we received fewer applications, we had only 90 competitors. We distributed a total of 40 prizes, each of US$1,000: 16 prizes in each of Ethiopia and Tanzania, and eight prizes in Zambia.[18] A random sample of baseline firms were invited to participate in the Aspire project, and 337 agreed to participate. In practice, this was achieved by randomly sorting all baseline firms and then inviting them sequentially, until we reached the needed number of committee and non-committee judges. Tables A3 and A4 (in the Online Appendix) shows a comparison of firm characteristics between participants and non-participants. Table 1 shows how committee judges were assigned to different committees.[19] In total, we assigned 239 participants to committees, leaving 106 as non-committee judges.

< Table 1 here. >

We test for diffusion of a variety of 20 different business practices, which we group into four headings: 'formalisation', 'labour management', 'relations with clients and suppliers' and 'innovation'. In Table 2, we summarise the baseline adoption of each practice. This is useful for two purposes. First, in the third column of Table 2, we test whether baseline adoption is balanced between committee and non-committee judges; we find that almost all practices are well balanced.[20] (The only exception is whether the firm imports — where a very small difference in percentage proportions is significant because we have such a small proportion of firms importing.) Second, in the fourth, fifth and sixth columns, Table 2 shows the proportion of committees that, at baseline, had zero, one or more participants having adopted each practice. We see that, across the committees in the experiment, there is substantial variation in baseline adoption practices; this is useful for understanding intuitively the source of identification.

< Table 2 here. >

4 Results

4.1 Creation of network links

We begin by considering the probability of creating network links (equation 1). Table 3 describes a series of outcome variables and explanatory variables from the dyadic data. Table 4 reports results from the resulting dyadic estimation. The table shows that being on the same panel had a large and highly significant effect on the probability of creating a relevant network link. Column 1 shows that there is a 5.6% probability that judge i erroneously claims to have been on a judging committee with judge j if the judges were not, in fact, on a committee together. For judges on a committee together, the probability increases to 38.2%. This number is well below 100%, which implies that a large proportion of committee

judges do not recall all the individuals on their committee. In column 2 the dependent variable for dyad ij equals 1 if i remembers speaking with j since the conclusion of the Aspire Competition. This is our main measure of the creation of new social links. The regression shows a highly significant effect of being in the same committee on the probability of having spoken. The magnitude of the estimated effect is 15%, implying that, on average, each judge formed a new social link with approximately one of his or her committee peers. While respectable, this proportion is likely to be underestimated because of under-reporting, the presence of which is expected to be large given Column 1.[21] Sharing a committee also increased the probability of having discussed specific management practices specifically mentioned in our survey; we find significant positive effects on the probability of having discussed export strategies (column 3), labor management (column 4) and innovation (column 5).

< Table 3 here. >

< Table 4 here. >

4.2 Diffusion of business practices

We now test directly for diffusion in management practices, by estimating equation 2. To do this, we group our measures of business practices into four families: (i) formalisation, (ii) labour management, (iii) relations with clients and suppliers and (iv) innovation. Table 5 reports our diffusion tests for four business practices relating to firm formalisation. We find significant positive diffusion for two outcomes: being registered for VAT (column 1), and for having a bank current account (column 3). We estimate that having a committee peer with VAT registration at baseline increased the probability of VAT registration at follow-up by about 7 percentage points, and that having a committee peer with a bank current account at baseline increased the probability of having a bank current account by about 4 percentage points at follow-up.[22] We find no significant evidence of diffusion in our other measures of management practices (*i.e.* the other outcomes listed in Table 2). Specifically, we find no evidence of diffusion on labour management (namely, on whether firms provide housing for employees, subsidise meals for any production workers, provide toilet facilities with running water for production workers, ever hire production workers without recommendation/referral, whether the average production worker has more than seven years' education, and whether entry-level production workers receive more than a month's training). We find no evidence of diffusion in our measures of relations with clients and suppliers (namely, whether the firm has advertised in the past six months, whether the firm pays any purchases before delivery, whether the firm pays any purchases after delivery, whether the firm has any sales paid before delivery, whether the

firm has any sales paid after delivery, whether the firm imports, and whether the firm exports). Finally, we find no evidence of diffusion on our measures of innovation (namely, whether the firm introduced new products in the past year, whether the firm has changed production processes in the past year, and whether the firm has changed delivery methods in the past year). For completeness, we report these estimates in the Online Appendix. It is worth noting that our result on the diffusion of VAT registration remains significant even if we correct for multiple hypothesis testing across all of these separate outcomes as a single family.

< Table 5 here. >

It is notable that all three study countries were undergoing important VAT reforms at the time of our experiment. In Ethiopia, legislation was enacted in December 2008 to widen substantially the powers of the Ethiopian Revenues and Customs Authority in enforcing VAT compliance.[23] This reform was making headlines in the Ethiopian business community around the time of our experiment; for example, in one prominent case, four businessmen were jailed in 2010 after employees of the Ethiopian Revenues and Customs Authority disguised themselves as customers and purchased a small plastic product for 24 Ethiopian Birr (approximately US$1.50) (Sebsibe, 2010).[24] In Tanzania, legislation was enacted in May 2010 to require the use of Electronic Fiscal Devices; this substantially expanded the ability of the Tanzania Revenue Authority to determine which traders would be required to register for VAT, and to demand compliance.[25] This led to an increase in VAT revenues of approximately 40% between the 2010/11 financial year and the 2011/12 financial year — that is, exactly at the time that our experiment was running — which the Tanzania Revenue Authority directly attributed to the introduction of Electronic Fiscal Devices (Chiwango, 2012). Similarly, in Zambia, legislation entered into force in January 2010 to increase penalties and powers against unregistered firms; the changes imposed tax on supplies made by unregistered taxable suppliers and made unregistered suppliers liable for back payments they should have made (including interest).[26] At the time, the Zambia Revenue Authority described this amendment as 'introduced to curb the incidence of non-registration for VAT…' (Zambia Revenue Authority, 2009).

Of course, we cannot claim a direct causal link between these reforms and our experimental results; it is possible that we would have found the same effect on VAT registration even if the regulatory environment were much more settled. Nonetheless, this is clearly an important and interesting aspect of the institutional context: it suggests that firms were looking to learn from peers' experience with a business

practice that was, at the time, attracting substantial attention and concern among the business community.

4.3 Discussion and Robustness

It is important to acknowledge an important caveat in the interpretation of our results: we cannot identify the channel by which peers influence occurs. To illustrate the issue, imagine that firms are more likely to have a bank account at endline when peers have a bank account at baseline. Further, imagine that we observe a similar correlation for VAT as well. This constitutes evidence of diffusion of VAT registration and bank accounts, but we do not know the mechanism by which diffusion takes place. One possibility is a direct peer effect, whereby observing other firms having a bank account makes me want to have a bank account. Another possibility is that the effect is indirect: observing other firms registering for VAT makes me want to register for VAT, and this registration then leads me to also open a bank account (e.g., because I need an account to collect VAT refunds from the tax authority). Since VAT registered firms at baseline are also more likely to have had an account, regressing my having a bank account at endline on whether my peers had a bank account at baseline yields a positive and significant coefficient. This example illustrates an important caveat: while our reduced-form estimation identifies diffusing practices, it does not identify whether the channel of causation is direct or indirect. This is not unusual — in fact, it is a feature of most if not all estimated peer effect models. We explore this caveat through a series of additional regressions, reported in Tables A10 and A11 of the Online Appendix.

4.4 Mechanisms for diffusion

We have found some evidence, though limited, of diffusion of management practices related to firm formalisation. So how does such diffusion occur? In this section, we run a series of complementary tests, to explore the potential mechanisms that might explain the limited diffusion that we do observe.

Imitation, or diffusion of innovation? Our identification strategy allows us to test between two alternative mechanisms for diffusion: (i) an 'imitation' model, in which firms copy their peers both in adopting and in not adopting ($\beta_1^p = -\beta_1^n$), and (ii) an asymmetric 'diffusion of innovation' process, in which firms copy peers in adopting, but are not affected by non-adoption ($\beta_1^p > 0$ and $\beta_1^n = 0$). Table 5 tests the null hypothesis of innovation. We strongly reject this null in the case of VAT registration: firms are more likely to register if they have peers who have done so, but are no less likely to register if they have more peers who have not done so ($p = 0.005$). Similarly, we find suggestive evidence of an asymmetry for the case of having a bank current account: firms are 4 percentage points more likely to

have a bank account for each peer that does so (significant), and 2 percentage points less likely to have a bank account for each peer that does not (not significant). However, we cannot reject a null hypothesis that this is a simple imitation process ($p = 0.367$).

Sharing a committee, or speaking? One possibility is that our experiment facilitated diffusion simply by having placed entrepreneurs on a committee together: entrepreneurs then choose with whom they wish to speak, and diffusion occurs through having spoken. Alternatively, it may be that diffusion needed more — that it required not merely for us to place judges on a committee together, but also to prompt judges to speak with each other. In some sense, the former mechanism is more complex: it suggests that managers *choose* optimally with whom they will speak, on the basis of some characteristics observable to each other. In contrast, the latter mechanism is more deterministic: it suggests that diffusion occurred through the relationships that we created, rather than through the relationships that judges chose.

To distinguish between these two mechanisms, we return to the dyadic data — and, for the first time in the paper, we exploit the random distribution of factsheets. Table 6 uses a dyadic Linear Probability Model (analogous to equation 1). It tests how the factsheets influenced the probability of judge i having spoken to judge j since the Aspire Competition, conditional on having been on the same committee.[27] We estimate this probability as a function of (i) the factsheets that judge j received (which we expect to have little effect, if any), and (ii) whether judges i and j received the same factsheet. We find that having randomly received the same factsheet increased significantly the probability of having spoken since the competition, with a magnitude of almost seven percentage points.

< Table 6 here. >

Our random distribution of factsheets therefore generated random variation in the probability of having spoken — above the variation we generated by the formation of the judging committees. We can exploit this random variation to distinguish between our two hypothesised mechanisms for diffusion. To do this, we generate a predicted probability of having spoken from Table 6. We now run a diffusion estimation in which we interact baseline peer characteristics with (i) the predicted probability of having spoken (which we denote $\widehat{\text{spoken}}$) and (ii) a dummy for whether judge i reports having spoken to judge j, *less* the predicted probability $\widehat{\text{spoken}}$. If diffusion occurs via self-selected conversations, we should expect this second term to be non-zero. In contrast, if diffusion occurs through conversations that we induced by the distribution of factsheets, we expect the first term to be non-zero.[28] We estimate on the two outcomes

for which, in the primary specifications, we found significant diffusion. We calculate the p-values using a wild bootstrap procedure, in which we repeat both the dyadic first stage and the probit second stage.[29]

The results are reported in Table 7. In both cases, we find that it is the interaction with the predicted measure of having spoken — rather than the interaction with the 'residual' — that is larger, and that has the smaller p-value. (Indeed, in each case, the interaction with the predicted measure of having spoken is reasonably close to being significant: $p = 0.162$ and $p = 0.105$.) The diffusion that we observed is explained more through variation that we induced in the probability of having spoken, rather than by variation caused by managers' own decision to seek out peers whose expertise might benefit their firms.

< Table 7 here. >

Firm size effects: Next, we test for heterogeneity by firm size. To do this, we bifurcate our sample at the median firm size (four permanent employees); we denote firms with more than four employees as 'large' and firms with four or fewer as 'small'. We repeat our earlier estimations on VAT registration and having a bank account, but allow effects to differ by firm size. Table 8 reports the results. In each case, we test for whether firms have different reactions to small peers as to large; we also test for whether small and large firms behave differently.

We find interesting differences between the diffusion of VAT registration and the diffusion of having a bank current account. For VAT registration, firm size seems to matter little. Smaller firms and larger firms react in very similar ways to their peers' registration, and firms react in a similar way to smaller peers as to larger peers. For having a bank account, larger firms react significantly more than smaller firms. Indeed, we estimate that a large firm is about nine percentage points more likely to adopt a bank account if a large peer has done so at baseline (significant at the 1% level); the estimate is almost identical (7 percentage points) for a large firm reacting to a small firm having a bank account.

< Table 8 here. >

Sectoral effects: The previous results suggest that the characteristics of firms' peers are not particularly important for diffusion: firms react similarly as to large peers as to small. To extend this analysis, we allow for heterogeneity by whether peers are in the same sector. If diffusion occurs because firms seek to imitate others in the same sector — for example, in an attempt to catch up with competitors —

then a firm should react to the business practices only of other firms in the same sector. Table 9 tests this formally. We find no additional diffusion of business practices from firms in the same sector.

< Table 9 here. >

5 Conclusions

We report results from a field experiment designed to exogenously vary peer networks. To our knowledge, our paper is the first to design an externally valid experiment to create new business acquaintances, and to document the ensuing diffusion of some business practices among firm managers and entrepreneurs. The field experiment is a business idea competition open to young prospective entrepreneurs. This competition took place entirely outside of the lab, was organized simultaneously in three African countries, and lasted for a period of several weeks. Furthermore, the form of the intervention was carefully selected so as to closely resemble the kind of activities that business and community organizations offer to their members (e.g., selection of new members, decisions on activities and budgets). It may be possible to generate stronger links when people spend more time together, *e.g.*, at school or in the military. By the time individuals are old enough to create or manage a firm, they cannot really afford to leave their business for long.[30] For this reason, the type of intervention that we tested is precisely the type of format that is most likely to be implemented in practice. This ensures the external validity of our findings.

Our experiment induced a large and highly significant change in link formation between managers, thereby demonstrating that it is possible to induce exogenous variation in business acquaintances. Earlier studies of network diffusion — in particular, studies of adolescent health and of student academic performance – have provided evidence of rapid diffusion of knowledge and behaviors along peer networks. We do not find such strong results among firms, perhaps because changing a firm's business practice is a much more costly and time consuming process than changing one's smoking habits. Even so, and in spite of the short time frame of our experiment, we do find some evidence of diffusion in the positive diffusion of VAT registration and of having a bank current account. Diffusion appears to be a combination of 'diffusion of innovation' (for the relatively novel business practice: VAT registration) and simple imitation (for the long-standing business practice: having a bank account). At the time of our experiment, all three studied countries were undergoing large changes in their VAT legislation (e.g., increased penalization), which suggests that diffusion of business practices may be more likely when

changing conditions affect the costs and benefits of adoption.

The type of diffusion brought to light in our study does not appear to be particularly purposive or strategic. Three separate heterogeneity tests support this conclusion. First, observed diffusion appears to be driven by exogenous variation in the probability of having spoken, rather than by managers seeking out specific peers susceptible to be a source of original information. Second, firms do not react more to the business practices of peers of the same approximate size. Third, firms do not react more to peers in the same sector. These results paint a less sophisticated — *i.e.*, more serendipitous — picture of the diffusion of business practices than economists sometimes assume.

There may be other reasons why we do not find more evidence of diffusion. As mentioned earlier, changing business practices is much more costly for firms than for individuals. Moreover, changing the way a firm does business raises coordination problems both internal and external to the firm. Some managers may feel sufficiently set in their existing practices — or sufficiently wary of experimentation — not to see a need to learn from other managers' experiences (Callander and Matouschek, 2014). For these reasons, it is natural to expect that, unless the gain from changing a practive is particularly large, many firms would continue using the same business practices for a while, even if they know them to be suboptimal. Diffusion among firms is therefore likely to take more time – and may never reach firms for which improving their practices is too difficult or challenging.

There may also be strategic reasons why the formation of business links need not lead to the circulation of information conducive to the spread of better business practices. First, entrepreneurs may face incentives *not* to encourage adoption of better practices by peers who could then compete away their profit (Foster and Rosenzweig, 1995). Second, contact among peers may diffuse not only of tales of success, but also of entrepreneur horror stories — for example, stories of firms that tried and failed at exporting, or at introducing new products. For all of these reasons, business networks need not provide a sufficient basis for reducing the heterogeneity of either management practices or productivity outcomes between competing firms. These findings contribute to our understanding how inferior business practices and productivity differentials among firms may persist over time (Bloom and Van Reenen, 2007).

Notes

[1]The competition is loosely modeled on several popular reality television shows — for example, the program *Shark Tank* in the United States, and the program *Dragon's Den* in the United Kingdom and Canada.

[2]The authors use a larger sample and a much longer socialisation treatment than we do, and find significant positive effects on sales, profits, employment, productivity and the number of business partners.

[3]For example, TechnoServe is currently running the *ENGINE* business plan competition in Ghana, with substantial support from the UK Department for International Development (DFID). The same general format has been used recently for the *African Innovation Prize* (in Burundi, Rwanda and Sierra Leone), the Enablis Entrepreneurial Network's *Business Plan Competition* (in Ghana), the *Darecha Business Ideas Competition* (in Tanzania), the *SEED Awards* (in Ethiopia, Kenya, Malawi, Morocco, Mozambique, Namibia, South Africa, Tanzania and Uganda), the *StartUp Cup* (in Cameroon, Ghana, Kenya, Rwanda and Zambia) and the *YouWiN!* competition (in Nigeria).

[4]An example of a promotional poster is included in the Online Appendix.

[5]The design is slightly different in Zambia, as we discuss shortly.

[6]Thus, a committee ranks four candidates after its first meeting, eight candidates after its second meeting and 12 candidates after its final meeting.

[7]Fafchamps and Quinn (2015) reports the effects of these prizes on successful candidates.

[8]Non-committee judges were seated separately, and completed their work under 'examination conditions'.

[9]Our identification strategy — described shortly — will control for any possible endogeneity arising from this choice.

[10]The factsheets were distributed in English in Zambia, in Amharic in Ethiopia, and in Swahili in Tanzania.

[11]That is, C_{ij} is defined from our official records of committee membership.

[12]We thank Bruno Caprettini for providing very useful code for dyadic regressions with an incomplete adjacency matrix. Note that, because our network adjacency matrix is sparse, the dyadic method is almost identical here to the two-way clustering method of Cameron, Gelbach, and Miller (2011).

[13]That is, we are estimating equation 1 as a Linear Probability Model. Since P_{ij} is binary, we would obtain identical estimates if we were to use marginal effects from a probit or logit model.

[14]If we only had data on committee judges we could not separately identify the coefficients of $\sum_{j\in\mathcal{C}_i} y_{js0}$ and $\sum_{j\in\mathcal{C}_i} (1-y_{js0})$ in equation 2 because they would be collinear. The presence of non-committee judges is what mathematically identifies the two coefficients because, for non-committee judges, both variables are zero by construction.

[15]An alternative approach would be simply to include session fixed effects. This is not our preferred approach; given that we have a non-linear estimator and a relatively small sample, this causes a substantial loss of efficiency in this context. Nonetheless, in the Online Appendix, we repeat all of the primary estimations using session fixed effects; we show that this has no meaningful effect on our point estimates.

[16]This project is summarised at `http://econ.worldbank.org/africamanufacturing`, and the main report has been published as Dinh, Palmade, Chandra, and Cossar (2012).

[17]In Tanzania and Zambia, our original sample also includes a number of respondents holding relatively junior roles in their firms; for example, respondents who described themselves as 'technicians'. In those two countries, we deliberately favoured more senior respondents for participation in the experiment. Where we needed to use more junior respondents to fill judging committees, we then exclude them from the analysis.

[18]In Zambia, we had 16 committees — but, because of the smaller number of applicants, awarded only eight prizes. We chose the eight prize winners from the 16 highest-ranked applicants by randomly matching committees in pairs. Within each pair, we awarded the prize to the committee winner with the better average scores from the 'non-committee judges'.

[19]Note that two committees in Zambia each comprised only two judges (shown in square brackets); we drop these four judges from the subsequent analysis.

[20]We do this by regressing baseline adoption on a dummy for being a committee judge, and cluster by judging committee (where, as in the main estimations, we treat non-committee judges as each comprising a single-judge committee for clustering purposes).

[21]To get an idea of the bias, we use the methodology proposed by Comola and Fafchamps (forthcoming). The approach is based on noting that when i remembers talking to j, j does not always remember talking to i. This fact can be used to obtain an estimate of under-reporting as follows. Formally, let τ be 1 if i and j spoke to each other and let $\lambda = \Pr(\tau = 1)$. This is the statistic that we wish to recover from the data. What we observe are sample equivalents of $\Pr(i = 1, j = 0) = \Pr(j = 1, i = 0)$, which we denote P_1, and $\Pr(i = 1, j = 1)$ which we denote P_2. Here $i = 1$ is shorthand for i having reported talking to j, and so on. Further let $\theta = \Pr(i = 1|\tau = 1)$, *i.e.*, the probability that i remembers talking to j if this actually happened, that is, if $\tau = 1$; $1 - \theta$ is the extent of under-reporting. Adapting the formulas presented by Comola and Fafchamps to our setting, it is easy to show that $P_1 = \lambda\theta(1 - \theta)$ while $P_2 = \lambda\theta^2$, which gives $\theta = \frac{P_2}{P_1+P_2}$ and $\lambda = \frac{(P_1+P_2)^2}{P_2}$. Applying these formulas to the data, the probability θ of reporting having spoken to another judge conditional on having spoken to that person is 31.8% – implying 68.2% under-reporting. Correcting for that, the likelihood λ of talking to another judge rises from 17.4% to 54.6%, suggesting that the actual treatment effect on the creation of new social links is larger than the point estimate reported in Column 2.

[22]It is possible to approximate the LATE by combining these figures with the information from Table 4. From table 5 we see that the ITT is 6.6% for VAT and 4.4% for bank account. The effect of treatment on social links was estimated to 15.5% in Table 4, but after correcting for under-reporting we have shown in footnote 20 that the effect is probably closer to 54.6%. It follows that the LATE on VAT and bank account are approximately 12% and 8%, respectively.

[23]Proclamation No. 609/2008 was enacted on 25 December 2008. Among other changes, it amends Article 30 of the Value Added Tax Proclamation No. 285/2002. It provides that the Ethiopian Revenues and Customs Authority may, without court order, seize 'any illegal vouchers, documents or books of account they encounter', and that these shall be admissible evidence in court. It also provides that police force may be used to do this, and that the police shall be required to cooperate with the Authority to seize such documents.

[24]The businessmen were released from jail after serving one month of a two year sentence, following a ruling of the Federal Supreme Court: Sebsibe (2010).

[25]The relevant legislation is the Value Added Tax (Electronic Fiscal Devices) Regulations 2010, which were enacted and entered into force on 28 May 2010. Supporting amendments to the Value Added Tax Act were made by Part XII of the Finance Act 2010, which was enacted

on 15 June 2010 and received assent on 23 June 2010.

[26]The relevant legislation is the Value Added Tax (Amendment) Act No. 29 of 2009, which came into effect on 1 January 2010.

[27]That is, we run this estimation only for dyads where judge i and judge j were on the same committee.

[28]For this section, we are therefore making the simplifying assumption that diffusion from firm j to firm i only occurs if the manager of i reports having spoken to the manager of firm j. For two managers who were not on the same committee, we therefore define $\widehat{\text{spoken}} \equiv 0$.

[29]For this algorithm, we use the sample cluster definition as in the earlier specifications — namely, we cluster by committee and, for clustering purposes, treat non-committee judges as each forming their own committee.

[30]The only relevant exception is joining an MBA program, which may very well create stronger network effects. But for our participants this would require either that they leave their current managerial position, or that they close down the firm they own.

References

ANDERSON, M. L. (2008): "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 103(484), 1481–1495.

ARAL, S., AND D. WALKER (2011): "Creating Social Contagion Through Viral Product Design: A Randomized Trial of Peer Influence in Networks," *Management Science*, 57(9), 1623–1639.

BANDIERA, O., I. BARANKAY, AND I. RASUL (2011): "Field Experiments with Firms," *Journal of Economic Perspectives*, 25(3), 63–82.

BANERJEE, A., A. G. CHANDRASEKHAR, E. DUFLO, AND M. O. JACKSON (2013): "The diffusion of microfinance," *Science*, 341(6144), 1236498.

BEAMAN, L., N. KELEHER, AND J. MAGRUDER (2015): "Do Job Networks Disadvantage Women? Evidence from a Recruitment Experiment in Malawi," *Working Paper*.

BEAMAN, L., AND J. MAGRUDER (2012): "Who Gets the Job Referral? Evidence from a Social Networks Experiment," *The American Economic Review*, 102(7), 3574–3593.

BENJAMINI, Y., A. M. KRIEGER, AND D. YEKUTIELI (2006): "Adaptive Linear Step-up Procedures That Control the False Discovery Rate," *Biometrika*, 93(3), 491–507.

BESHEARS, J., J. J. CHOI, D. LAIBSON, B. C. MADRIAN, AND K. L. MILKMAN (2015): "The Effect of Providing Peer Information on Retirement Savings Decisions," *The Journal of finance*, 70(3), 1161–1201.

BIFULCO, R., J. M. FLETCHER, AND S. L. ROSS (2011): "The Effect of Classmate Characteristics on Post-Secondary Outcomes: Evidence from the Add Health," *American Economic Journal: Economic Policy*, 3(1), 25–53.

BLOOM, N., B. EIFERT, A. MAHAJAN, D. MCKENZIE, AND J. ROBERTS (2013): "Does Management Matter? Evidence from India," *The Quarterly Journal of Economics*, 128(1), 1–51.

BLOOM, N., R. SADUN, AND J. VAN REENEN (2015): "Management as a Technology?," *Working Paper*.

BLOOM, N., M. SCHANKERMAN, AND J. VAN REENEN (2013): "Identifying Technology Spillovers and Product Market Rivalry," *Econometrica*, 81(4), 1347–1393.

BLOOM, N., AND J. VAN REENEN (2007): "Measuring and Explaining Management Practices Across Firms and Countries," *The Quarterly Journal of Economics*, 122(4), 1351–1408.

——— (2010): "Why do Management Practices Differ Across Firms and Countries?," *The Journal of Economic Perspectives*, 24(1), 203–224.

BURSZTYN, L., F. EDERER, B. FERMAN, AND N. YUCHTMAN (2014): "Understanding Mechanisms Underlying Peer Effects: Evidence From a Field Experiment on Financial Decisions," *Econometrica*, 82(4), 1273–1301.

CAEYERS, B., AND M. FAFCHAMPS (2016): "Exclusion Bias in the Estimation of Peer Effects," *Working Paper*.

CAI, J., AND A. SZEIDL (2016): "Interfirm Relationships and Business Performance," *Working Paper*.

CALLANDER, S., AND N. MATOUSCHEK (2014): "Managing on Rugged Landscapes," *Working paper*.

CAMERON, A., J. GELBACH, AND D. MILLER (2011): "Robust Inference with Multiway Clustering," *Journal of Business and Economic Statistics*, 29(2), 238–249.

CARRELL, S. E., B. I. SACERDOTE, AND J. E. WEST (2013): "From natural variation to optimal policy? The importance of endogenous peer group formation," *Econometrica*, 81(3), 855–882.

CASELLA, A., AND J. RAUCH (2002): "Anonymous Market and Group Ties in International Trade," *Journal of International Economics*, 58(1), 19–47.

CASEY, K., R. GLENNERSTER, AND E. MIGUEL (2012): "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan*," *The Quarterly Journal of Economics*, 127(4), 1755–1812.

CENTOLA, D. (2010): "The Spread of Behavior in an Online Social Network Experiment," *science*, 329(5996), 1194–1197.

——— (2011): "An Experimental Study of Homophily in the Adoption of Health Behavior," *Science*, 334(6060), 1269–1272.

CHIWANGO, A. (2012): "TRA: Electronic Fiscal Devices Already Making a Difference," *Business Times*, 8 December 2012.

COMOLA, M., AND M. FAFCHAMPS (forthcoming): "The Missing Transfers: Estimating Mis-Reporting in Dyadic Data," *Economic Development and Cultural Change*.

CONLEY, T., AND C. UDRY (2001): "Social Learning through Networks: The Adoption of New Agricultural Technologies in Ghana," *American Journal of Agricultural Economics*, 83(3), 668–673.

CONLEY, T. G., AND C. R. UDRY (2010): "Learning About a New Technology: Pineapple in Ghana," *The American Economic Review*, pp. 35–69.

DINH, H., V. PALMADE, V. CHANDRA, AND F. COSSAR (2012): *Light Manufacturing in Africa: Targeted Policies to Enhance Private Investment and Create Jobs*. World Bank Publications.

DUFLO, E., P. DUPAS, AND M. KREMER (2011): "Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya," *The American Economic Review*, 101(5), 1739–1774.

DUFLO, E., AND E. SAEZ (2003): "The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment," *The Quarterly Journal of Economics*, 118(3), 815–842.

FAFCHAMPS, M., AND F. GUBERT (2007): "The Formation of Risk Sharing Networks," *Journal of Development Economics*, 83(2), 326–350.

FAFCHAMPS, M., AND D. MO (2015): "Peer Effect Heterogeneity in Computer Assisted Learning: Evidence from a Randomized Experiment," *Working paper (Stanford University)*.

FAFCHAMPS, M., AND S. QUINN (2015): "Aspire," *CSAE Working Paper WPS/2014-34*.

FAFCHAMPS, M., AND M. SÖDERBOM (2014): "Network Proximity and Business Practices in African Manufacturing," *The World Bank Economic Review*, 28(1), 99–129.

FLETCHER, J. M. (2010): "Social Interactions and Smoking: Evidence using Multiple Student Cohorts, Instrumental Variables, and School Fixed Effects," *Health Economics*, 19(4), 466–484.

FOSTER, A., AND M. ROSENZWEIG (1995): "Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture," *Journal of Political Economy*, pp. 1176–1209.

GIBBONS, R., AND R. HENDERSON (2012): "What Do Managers Do? Exploring Persistent Performance Differences Among Seemingly Similar Enterprises," in *The Handbook of Organizational Economics*, ed. by R. Gibbons, and J. Roberts. Princeton University Press.

GODLONTON, S., AND R. THORNTON (2012): "Peer Effects in Learning HIV Results," *Journal of Development Economics*, 97(1), 118–129.

GREIF, A. (1993): "Contract Enforceability and Economic Institutions in Early Trade: The Maghribi Traders' Coalition," *The American Economic Review*, pp. 525–548.

GURYAN, J., K. KROFT, AND M. NOTOWIDIGDO (2009): "Peer Effects in the Workplace: Evidence from Random Groupings in Professional Golf Tournaments," *American Economic Journal: Applied Economics*, 1(4), 34–68.

HARDY, M., AND J. MCCASLAND (2015): "Are Small Firms Labor Constrained? Experimental Evidence from Ghana," *Working Paper*.

HARRISON, G., S. HUMPHREY, AND A. VERSCHOOR (2010): "Choice Under Uncertainty: Evidence from Ethiopia, India and Uganda," *The Economic Journal*, 120(543), 80–104.

HSIEH, C.-T., AND P. J. KLENOW (2009): "Misallocation and Manufacturing TFP in China and India," *The Quarterly Journal of Economics*, 124(4), 1403–1448.

JENSEN, R. (2010): "The (Perceived) Returns to Education and the Demand for Schooling," *The Quarterly Journal of Economics*, 125(2), 515–548.

KERMARK, M., AND A. MCKENDRICK (1927): "Contributions to the Mathematical Theory of Epidemics. Part I," in *Proc. R. Soc. A*, vol. 115, pp. 700–721.

KREMER, M., AND E. MIGUEL (2007): "The Illusion of Sustainabiltiy," *Quarterly Journal of Economics*, 122(3).

LYLE, D. (2007): "Estimating and Interpreting Peer and Role Model Effects from Randomly Assigned Social Groups at West Point," *The Review of Economics and Statistics*, 89(2), 289–299.

——— (2009): "The Effects of Peer Group Heterogeneity on the Production of Human Capital at West Point," *American Economic Journal: Applied Economics*, pp. 69–84.

MCMILLAN, J., AND C. WOODRUFF (1999): "Interfirm Relationships and Informal Credit in Vietnam," *The Quarterly Journal of Economics*, 114(4), 1285–1320.

MILLER, G., AND A. M. MOBARAK (2014): "Learning About New Technologies Through Social Networks: Experimental Evidence on Nontraditional Stoves in Bangladesh," *Marketing Science*, 34(4), 480–499.

MÖBIUS, M., T. PHAN, AND A. SZEIDL (2015): "Treasure Hunt: Social Learning in the Field," *Working Paper*.

OSTER, E., AND R. THORNTON (2012): "Determinants Of Technology Adoption: Peer Effects In Menstrual Cup Take-Up," *Journal of the European Economic Association*, 10(6), 1263–1293.

PARENTE, S., AND E. PRESCOTT (1994): "Barriers to Technology Adoption and Development," *Journal of Political Economy*, pp. 298–321.

PATNAM, M. (2013): "Corporate Networks and Peer Effects in Firm Policies," *Working Paper*.

RAUCH, J., AND A. CASELLA (2003): "Overcoming Informational Barriers to International Resource Allocation: Prices and Ties," *The Economic Journal*, 113(484), 21–42.

ROGERS, E. M. (1962): *Diffusion of Innovations*. Glencoe: Free Press.

SACERDOTE, B. (2001): "Peer Effects with Random Assignment: Results for Dartmouth Roommates," *The Quarterly Journal of Economics*, 116(2), 681–704.

SEBSIBE, M. (2010): "Tena Kebede Out of Prison," *Addis Fortune*, 1 August 2010.

SHUE, K. (2013): "Executive networks and firm policies: Evidence from the random assignment of MBA peers," *Review of Financial Studies*, 26(6), 1401–1442.

SYVERSON, C. (2011): "What Determines Productivity?," *Journal of Economic Literature*, 49(2), 326–365.

ZAMBIA REVENUE AUTHORITY (2009): "Practice Note No. 1/2010," *Domestic Taxes Division*.

ZIMMERMAN, D. (2003): "Peer Effects in Academic Outcomes: Evidence from a Natural Experiment," *Review of Economics and Statistics*, 85(1), 9–23.

Table 1: **Assignment to treatment: Judging committees**

	COMMITTEE																NON-COMMITTEE	
	A	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**	**K**	**L**	**M**	**N**	**O**	**P**	**TOTAL**	**TOTAL**
ETHIOPIA	6	6	6	6	5	5	5	5	6	5	3	6	5	6	5	6	**86**	**40**
TANZANIA	6	5	6	6	6	6	5	5	6	6	6	4	6	6	6	5	**90**	**44**
ZAMBIA	4	4	4	5	5	6	6	4	4	3	3	3	3	5	[2]	[2]	**63**	**22**
																	239	106

Table 2: **Description of baseline adoption**

	Baseline sample means		Balance	Committees with baseline adoption of...		
	non-committee	committee	(p-value)	0	1	≥ 2
Formalisation:						
Registered for VAT	0.08	0.08	0.96	70%	24%	7%
Uses an external auditor	0.15	0.17	0.75	40%	40%	20%
Has a bank current account	0.40	0.42	0.68	7%	35%	59%
Has a savings account	0.37	0.30	0.27	37%	24%	39%
Labour management:						
Provides housing for any employees	0.11	0.08	0.43	63%	33%	4%
Subsidises meals for any production workers	0.37	0.36	0.83	17%	37%	46%
Provides toilets with running water for any production workers	0.45	0.41	0.45	2%	33%	65%
Hires production workers without recommendation/referral	0.56	0.56	0.99	7%	18%	75%
Average production worker has $>$ 7 years' education	0.44	0.43	0.92	19%	23%	58%
Entry-level production workers receive $>$ 1 month's training	0.31	0.35	0.49	20%	33%	48%
Relations with clients and suppliers:						
Has advertised in the past 6 months	0.39	0.32	0.24	30%	22%	48%
Pays any purchases before delivery	0.11	0.13	0.61	56%	30%	14%
Pays any purchases after delivery	0.26	0.24	0.70	29%	42%	29%
Has any sales paid before delivery	0.58	0.68	0.10	4%	9%	87%
Has any sales paid after delivery	0.33	0.41	0.18	11%	33%	57%
Firm imports	0.00	0.04	0.00	83%	13%	4%
Firm exports	0.05	0.05	0.95	76%	22%	2%
Innovation:						
Has introduced new products in the past year	0.41	0.42	0.91	13%	27%	60%
Has changed production process in the past year	0.22	0.29	0.20	17%	50%	33%
Has changed delivery methods in the past year	0.15	0.21	0.24	39%	35%	26%
Observations:	100	233				

Table 3: **Description of dyadic data**

	Sample means
Outcome variables:	
Dummy: 'Were you on a judging panel with this person?'	0.123
Dummy: 'Have you spoken to this person since the Aspire Competition awards ceremony?'	0.051
Dummy: 'Did you discuss export strategies?'	0.011
Dummy: 'Did you discuss labor management?'	0.021
Dummy: 'Did you discuss innovation and business advice?'	0.038
Explanatory variables:	
Dummy: Judges i and j were on the same panel	0.205
Dummy: Judges i and j received the same factsheet	0.369
Dummy: Judge j received the CSAE factsheet	0.370
Dummy: Judge j received the exports factsheet	0.359
Dummy: Judge j received the innovation factsheet	0.354
Dummy: Judge j received the labor factsheet	0.412
Observations:	4788

Table 4: **Results: Creation of network links**

	(1)	(2)	(3)	(4)	(5)
			discussion topics		
	remembers	spoken since	exports	labor	innovation
Dummy: Same panel	**0.326**	**0.154**	**0.027**	**0.062**	**0.112**
	(0.024)***	(0.016)***	(0.007)***	(0.010)***	(0.014)***
Constant	0.056	0.020	0.006	0.008	0.015
	(0.007)***	(0.003)***	(0.001)***	(0.002)***	(0.002)***
Observations	4788	4788	4788	4788	4788

The unit of observation is a dyadic response. Parentheses show standard errors, allowing for dyadic clustering.

'Remembers' is a dummy for 'Were you on a judging panel with this person?'.

'Spoken since' is a dummy for 'Have you spoken to this person since the Aspire Competition awards ceremony?'.

'Exports' is a dummy for 'Did you discuss export strategies?'.

'Labor' is a dummy for 'Did you discuss labor management?'.

'Innovation' is a dummy for 'Did you discuss innovation and business advice?'.

Significance: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 5: **Diffusion results: Formalisation**

	(1)	(2)	(3)	(4)
Sum of peers adopting	**0.066**	**0.016**	**0.044**	**0.029**
	[0.013]**	**[0.255]**	**[0.078]***	**[0.255]**
	(0.003)***	(0.406)	(0.048)**	(0.334)
Sum of peers <u>not</u> adopting	**0.003**	**0.008**	**-0.022**	**-0.022**
	[0.673]	**[0.673]**	**[0.673]**	**[0.673]**
	(0.757)	(0.359)	(0.201)	(0.110)
Controls	✓	✓	✓	✓
Observations	333	326	329	326
H_0: Imitation (p)	0.005***	0.277	0.367	0.826
Baseline adoption	8.1%	16.3%	42.2%	32.8%

Outcome variables:

1: Whether the firm is registered for VAT ('missing' = 'no')

2: Whether the firm's financial statements are certified by external auditor.

3: Whether the firm has a bank current account.

4: Whether the firm has a savings account.

Coefficients show the estimated mean marginal effect.

'Controls' means (i) the lagged outcome variable, (ii) the sum of session adoption at baseline, and (iii) the size of the baseline session.

'[ ]' show the 'sharpened' False Discovery Rate adjusted q-values.

'( )' show standard p-values, allowing for clustering by committee.

Significance: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 6: **Results: Creation of network links: Effect of receiving the same factsheet**

Dependent variable: 'Have you spoken to this person since the Aspire Competition awards ceremony?'	
Dummy: Received the same sheet	**0.067****
	(0.033)
CSAE sheet	0.029
	(0.026)
Exports sheet	0.043*
	(0.023)
Innovation sheet	0.015
	(0.028)
Labor sheet	-0.038
	(0.024)
Constant	0.134***
	(0.020)
Observations	976

The unit of observation is a dyadic response. Parentheses show standard errors, allowing for dyadic clustering.
The estimation is run only for dyads where both judges were on the same panel.
Significance: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 7: **Results: Diffusion and the probability of having spoken**

	(1)	(2)
Σ **(peer adopting** $\times$ $\widehat{\textbf{spoken}}$**)**	**0.379**	**0.213**
	(0.162)	(0.105)
Σ **[peer adopting** $\times$ **(spoken** $-\widehat{\textbf{spoken}}$**)]**	0.003	0.034
	(0.988)	(0.493)
Controls	✓	✓
Observations	333	329

Outcome variables:
1: Whether the firm is registered for VAT ('missing' = 'no')
2: Whether the firm has a bank current account

'( )' show standard p-values, allowing for clustering by committee.
Significance: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table 8: **Diffusion results and firm size**

	(1)	(2)	(3)	(4)
	Registered for VAT		**Has a bank current account**	
	Small firms	*Large firms*	*Small firms*	*Large firms*
Sum of small peers adopting	**0.069**	**0.103**	**0.033**	**0.074**
	(0.069)*	(0.291)	(0.231)	(0.275)
Sum of small peers not adopting	**-0.008**	**0.026**	**0.036**	**-0.073**
	(0.513)	(0.121)	(0.126)	(0.019)**
Sum of large peers adopting	**0.038**	**0.119**	**0.006**	**0.086**
	(0.089)*	(0.030)**	(0.885)	(0.007)***
Sum of large peers not adopting	**0.025**	**0.007**	**-0.078**	**-0.072**
	(0.368)	(0.761)	(0.200)	(0.299)
Controls	✓	✓	✓	✓
Observations	197	136	195	134
H_0: Common reaction to large and small peers (p)	0.329	0.825	0.137	0.986
H_0: Common parameters between large and small firms (p)	0.317		0.019**	

Coefficients show the estimated mean marginal effect.
'Controls' means (i) the lagged outcome variable, (ii) the sum of session adoption at baseline, and (iii) the size of the baseline session.
'( )' show standard p-values, allowing for clustering by committee.

Table 9: **Diffusion results and sector**

	(1)	(2)
Sum of peers adopting	**0.083**	**0.044**
	(0.006)***	(0.065)*
Sum of peers not adopting	**0.005**	**-0.016**
	(0.596)	(0.420)
Sum of peers adopting (same sector)	**-0.079**	**0.003**
	(0.389)	(0.946)
Sum of peers not adopting (same sector)	**-0.013**	**-0.020**
	(0.451)	(0.511)
Controls	✓	✓
Observations	333	329

Outcome variables:
1: Whether the firm is registered for VAT ('missing' = 'no')
2: Whether the firm has a bank current account.

Coefficients show the estimated mean marginal effect.
'Controls' means (i) the lagged outcome variable, (ii) the sum of session adoption at baseline, and (iii) the size of the baseline session.
'( )' show standard p-values, allowing for clustering by committee.
Significance: *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.