Networks and Manufacturing Firms in Africa: Initial Results from a Randomised Experiment*

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Abstract

We run a controlled experiment to link managers of African manufacturing firms. The experiment has exogenous link formation, exogenous seeding of information and exogenous assignment to treatment and placebo. We study the impact of the experiment on real firm behaviour outside of the lab. We find that the experiment successfully created substantial new variation in peer networks. As part of the experimental design, we proposed two primary regression specifications to measure peer diffusion. We test both specifications on a range of outcomes and we find only limited evidence of diffusion. We find suggestive evidence of positive diffusion in several activities that may be characterised as relatively low risk and low cost (such as having a bank account or having an overdraft facility). We also find suggestive evidence of negative diffusion in activities that may present relatively higher risks and higher costs (such as exporting and introducing new products).

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1 Introduction

Experimental analysis of networks forms an important emerging area of research. But, to our knowledge, no experimental work has been done on business networks in developing countries. 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), for guiding a firm's policies on executive pay (Shue, 2012) and for learning about promising investment opportunities (Patnam, 2011). 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).

However, apart from the exploratory work of Fafchamps and Söderbom (2012), remarkably little is known about the way that firms in developing economies use business networks. *Do networks really matter for firm performance?* If so, *what kinds of management decisions are affected by the behaviour of its peers? Can researchers and policymakers change a firm's network in order to improve its performance?* Such issues are fundamental for understanding the constraints faced by firms in developing economies — but remain very open questions for academic research.

In this paper, we report initial results from a novel randomised field experiment designed to measure peer effects among manufacturing firms in Africa. We run a 'business ideas competition' in Ethiopia, Tanzania and Zamiba, in which aspiring young entrepreneurs present proposals for new enterprises to managers of established manufacturing firms.[1] By randomly assigning firm managers to different judging committees, we generate exogenous variation in firms' peer networks. This allows us to measure the causal effects of business networks on sub-

[1] The competition was loosely modelled 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.

sequent firm performance. To our knowledge, this is the first experiment to vary exogenously firms' networks of business peers. The experiment has exogenous link formation, 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.

We find only limited evidence of diffusion. We find suggestive evidence of positive diffusion in several activities that may be characterised as relatively low risk and low cost (such as having a bank account or having an overdraft facility). We also find suggestive evidence of negative diffusion in activities that may present relatively higher risks and higher costs (such as exporting and introducing new products).

This study contributes to the literature on the role of peer effects in social networks. First, the paper contributes to research on networks in developing countries. Recent work has emphasised the importance of social networks for risk sharing in poor communities (Fafchamps and Gubert, 2007; Chandrasekhar, Kinnan, and Larreguy, 2012), for assortative matching into community-based organisations (Fafchamps and Arcand, 2012; Zeitlin, 2011), and for adoption of health technology (Oster and Thornton, 2011). This research has considered the issue of diffusion in business networks in developing countries. It finds some evidence of positive spillovers, including for investment decisions (Patnam, 2011), but also indicates that correlation in business practices between peer firms is less than often assumed (Fafchamps and Söderbom, 2012). Our results similarly indicate that social proximity between firms need not cause similar business practices.

Second, this paper contributes to recent work on the use of experimental variation to study network behaviour. Several studies introduced exogenous variation in information to study the relevance of social links for diffusion (see, for example, Möbius, Phan, and Szeidl (2010) 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 behavioural 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, 2012), including one experimental study in a developing country (Duflo, Dupas, and Kremer, 2011). 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*.

Third, the paper contributes to a growing literature concerning econometric strategies for estimating peer effects. Guryan, Kroft, and Notowidigdo (2009) have recently showed that a standard 'linear-in-means' estimation may suffer an omitted variable bias even where peer assignment is random. They argue that this problem may be resolved by including a lagged dependent variable. We propose a alternative simulation-based method for testing for peer effects. This method is broadly similar to the random-matching procedure recently used by Baccara, İmrohoroğlu, Wilson, and Yariv (2012) to test for network effects in a discrete-choice context.

The paper proceeds as follows. Section 2 outlines the experimental protocol; in doing so, it discusses the identification strategy and summarises our simulation-based methodology. This identification strategy comprises the two key estimating equations that we outlined in our original research proposal (submitted to the World Bank in 2010). Section 3 summarises the implementation of the experiment, including a discussion of the firm sample and the covariate balance. In Section 4, we show that the experiment succeeded in creating new peer connections between firms. Section 5 uses our simulation-based methodology to test directly for diffusion of business practices. We conclude in Section 6.

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 ideas 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 our are now being run in several African countries.[2] In our competition, applicants are aspiring entrepreneurs aged between 18 and 25 (inclusive) and recruited through advertising by posters, radio and Facebook.[3] 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.

[2] For example, *Project Inspire Africa* is a reality television competition designed to test and reward young African entrepreneurs in a variety of business-related challenges; the program ran for the first time in 2012, with young entrepreneurs from Kenya, Rwanda, Tanzania and Uganda. *Ruka Juu* was a reality program that ran for 11 weeks in Tanzania in 2011, focusing on six young entrepreneurs. Other competitions encourage a wider range of applicants, beyond the proverbial glare of the television lights — for example, the *Darecha Business Ideas Competition* in Tanzania and the *StartUp Cup* in Zambia.

[3] An example of a promotional poster is included in the appendix.

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.[4] 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.[5] Each committee is responsible for awarding one prize of US$1,000, given to the committee's highest-ranked candidate.

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 held a prize-giving ceremony in each country. These ceremonies were attended by the committee judges and the competition winners. Judges at these ceremonies received free food and drinks, and were 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.

[4] The design is slightly different in Zambia, as we discuss shortly.
[5] Thus, a committee ranks four candidates after its first meeting, eight candidates after its second meeting and 12 candidates after its final meeting.

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.[6] 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.[7] 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 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).

[6] Non-committee judges were seated separately, and completed their work under 'examination conditions'.
[7] We will include 'session dummies' in the subsequent analysis in order to control for any endogeneity arising from this choice.

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.[8]

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…".

2.2 Identification strategy

Creation of network links: We begin our analysis by measuring the creation of network links; that is, by testing whether judges remember being on the same committees, and whether judges have had any discussions since the experiment. Such effects form an important preliminary issue for motivating the subsequent analysis of network spillover effects: one might struggle to accept any claim of network diffusion if judges do not remember each other, or do not admit to having spoken since the experiment.

We measure discussion effects through dyadic regressions. Having asked firm i about firm j, we estimate:

$$y_{ij} = \beta_0 + \beta_1 \cdot P_{ij} + \varepsilon_{ij}, \tag{1}$$

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

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 P_{ij} is a dummy for whether i and j were on the same committee together.[9] We use the two-way clustering method of Cameron, Gelbach, and Miller (2011), as a convenient approximation for the dyadic clustering method of Fafchamps and Gubert (2007).

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?”.[10] 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 $\beta_1 = 1$).

Peer effects: Several papers have studied natural experiments in which peers are randomly matched. Sacerdote (2001) studies the consequences of random assignment of of roommates 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).

One standard method for estimating peer effects is to use a ‘linear-in-means’ specification, in which the outcome of an individual i is estimated as a function of the mean of the baseline characteristics of that individual’s peers (see, for example, Lyle (2007, 2009); Duflo, Dupas,

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

[10] 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.

and Kremer (2011)). We use a similar approach in which we regress a characteristic of individual i on the sum of i's peers having the same characteristic at baseline. This is equivalent to the the linear-in-means approach — up to a rescaling — when all committees are the same size.

Suppose that we wish to test the consequences of baseline peer characteristic $y_{j,t-1}$ on follow-up firm characteristic y_{it}, where y is a binary variable. For example, y_{i1} may refer to whether the ith firm had a bank account at the time of the follow-up survey; y_{j0} would therefore refer to whether the jth firm had a bank account at the time of the baseline. We define $\mathcal{C}_i$ as the set of other firms on the same committee as firm i — that is, the set of i's peers — where, by construction, $i \notin \mathcal{C}_i$. P_{is} refers to a dummy variable for whether the representative of firm i was a committee judge. We estimate the following linear probability model, for firm i in randomisation session s at time $t = 1$:

$$y_{is1} = \beta_1 \cdot \sum_{j \in \mathcal{C}_i} y_{js0} + \beta_2 \cdot P_{is} + \mu_s + \varepsilon_{is}. \tag{2}$$

We also estimate this model in first differences:

$$y_{is1} - y_{is0} = \beta_1 \cdot \sum_{j \in \mathcal{C}_i} y_{js0} + \beta_2 \cdot P_{is} + \mu_s + \varepsilon_{is}. \tag{3}$$

These are the basic specifications that we outlined in the original research proposal.[11] Equations 2 and 3 therefore allow us to test separately two key questions from the experiment. First, we test the effects (if any) of random assignment to being a committee judge. This is tested by $H_0 : \beta_1 = 0$. Second, we test diffusion effects *conditional* on assignment to being a committee judge. This exploits the random variation in peer characteristics, and is tested by $H_0 : \beta_2 = 0$. We include dummy variables for the randomisation sessions (μ_s), and we allow ε_{is} to cluster

[11] There are two differences from that proposal. First, the original proposal suggested the random creation of judging committees, without proposing to assign non-committee judges; for this reason, our original proposal did not include the 'committee judge dummy', P_{is}. Second, the original proposal did not consider the need for a simulation method for inference (discussed shortly).

by judging committee.[12] Non-committee judges are each treated as being in their own cluster.

2.3 A simulation method for inference on peer effects

The estimation of equations 2 and 3 is not straightforward. We expect the 'sum of peers' term, $\sum_{j\in\mathcal{C}_i} y_{js0}$, to be negatively correlated with a firm's own lagged value, y_{is0}. To the extent that y_{ist} is autocorrelated, this lagged value can act as an omitted variable and bias the estimate $\hat{\beta}_1$, even if the randomisation is conducted correctly. This point has been made recently by Guryan, Kroft, and Notowidigdo (2009).[13] For this reason, we expect OLS estimates of equations 2 and 3 to produce biased estimates, and the size of the test to be wrong. Simulation evidence (available on request) suggests that this problem may be particularly severe where the outcome variable measures a behaviour that is either very common or very rare (that is, $\Pr(y_{ist} = 1)$ is close to zero or close to one).

This problem is common to many studies of peer effects. One approach is to include the lag of the dependent variable directly, or the leave-out mean of baseline peer characteristics in the randomisation session (Guryan, Kroft, and Notowidigdo, 2009). However, we are concerned in this paper to estimate the two simple specifications outlined in our original research proposal. To deal with this problem, we therefore introduce a correction of the p-value of $\hat{\beta}_1$ using an approach inspired by permutation-based inference.[14] This approach is easy to implement and is summarised as follows:

[12] As noted earlier, we estimate only on firms that attended a randomisation session and participated in the experiment, either as committee judges or as non-committee judges.

[13] We do not repeat the argument of Guryan, Kroft, and Notowidigdo (2009) here, save to quote briefly the discussion on pages 44 and 45 of their paper: "The problem stems from the fact that an individual cannot be assigned to himself. In a sense, sampling of peers is done without replacement — the individual himself is removed from the 'urn' from which his peers are chosen. As a result, the peers for high-ability individuals are chosen from a group with a slightly lower mean ability than the peers for low-ability individuals." We particularly thank Choon Wang for his discussions on this issue. Guryan, Kroft, and Notowidigdo (2009) consider a linear-in-means model, but the argument extends to our alternative specification.

[14] Permutation methods are commonly used by non-economists to draw inference in network data under the name of 'quadratic assignment procedure', or 'QAP' (see, for instance, Krackardt (1987)).

1. We take the pool of judges assigned to be committee judges in each session; within each pool, we randomly reassign judges to new 'placebo committees'.[15]

2. For each judge, we use the placebo assignment to generate a new 'placebo sum of peers'; that is, we recalculate the term $\sum_{j\in\mathcal{C}_i} y_{js0}$. By design the placebo sum of peers should not affect y_{is1} except for possible correlation with y_{is0}.

3. We estimate equations 2 and 3 using OLS; for each equation, we store the set of estimates for $\hat{\beta}_{1,\text{placebo}}$. The value of $\hat{\beta}_{1,\text{placebo}}$ need not be centered at 0 if $\sum_{j\in\mathcal{C}_i} y_{js0}$ is correlated with y_{is0}.

4. We repeat a large number of times.[16]

For each equation, we then report OLS estimates for $\hat{\beta}_1$ and $\hat{\beta}_2$. We test $H_0 : \beta_2 = 0$ using the t-value from the OLS estimation. We test $H_0 : \beta_1 = 0$ using the set of stored estimates from the simulation; the one-tail p-value is the proportion of simulated cases in which the stored estimates $\hat{\beta}_{1,\text{placebo}}$ are 'more extreme' than the estimate $\hat{\beta}_{1,\text{OLS}}$.[17] By construction, there is no true peer effect in our simulated placebo panels. This allows us to use the resampling method to simulate the distribution of the parameter of interest under the null hypothesis, given the characteristics of the judges that were randomly assigned to judging committees.

The intuition for this approach can be understood through an illustration. In part of the subsequent analysis, we will test for diffusion of labour unionisation; that is, one of our regression specifications will define y as a dummy variable for whether any of a firm's workers are mem-

[15] That is, we treat as fixed both (i) the composition of the randomisation sessions and (ii) the random assignment into committee/non-committee status. We then sample without replacement within the pool of committee judges, within each randomisation session.

[16] In the subsequent estimations, we use 5000 replications.

[17] More specifically, suppose that we have R replications for the simulation, indexed $r \in \{1, \ldots, R\}$. Then, if $\hat{\beta}_1 > 0$, the one-tail p-value is $R^{-1} \cdot \sum_r^R \mathbf{1}\left(\hat{\beta}_{1,\text{placebo}} > \hat{\beta}_{1,\text{OLS}}\right)$, where $\mathbf{1}(\cdot)$ denotes the indicator function. Symmetrically, if $\hat{\beta}_1 < 0$, the one-tail p-value is $R^{-1} \cdot \sum_r^R \mathbf{1}\left(\hat{\beta}_{1,\text{placebo}} < \hat{\beta}_{1,\text{OLS}}\right)$.

bers of a labour union.[18] Figure 1 shows the empirical PDF and empirical CDF for $\hat{\beta}_{1,\text{placebo}}$ for equation 3. Figure 2 shows the distribution of p-values. Together, the figures show how misleading our results would be if we were to rely upon the OLS t-values from equations 2 or 3 to draw inference. Figure 1 shows that, even under the null hypothesis, there is a positive bias in $\hat{\beta}_{1,\text{placebo}}$; indeed, in 5000 replications, not a single simulated parameter lay below the true value of zero. Figure 2 shows the consequence for OLS p-values. Instead of lying on the 45-degree line, the empirical CDF lies far above it; this shows that the p-values from an OLS regression would reject the null hypothesis of no-effect far too often.

< Figure 1 here. >

< Figure 2 here. >

Figure 1 illustrates the problem of relying on OLS t-values for inference. It also shows how our simulation method eliminates this problem. When we estimate equation 3 for whether any workers belong to a labour union, we obtain an estimate of $\hat{\beta}_{1,\text{OLS}} = 0.125$. This is represented by the vertical line in Figure 1. We obtain $\hat{\beta}_{1,\text{OLS}} > 0.125$ in 615 of our 5000 replications under the null; we therefore report a one-tailed p-value of $615/5000 = 0.123$. This is much larger than the one-tailed p-value implied by the OLS estimation, which is 0.008. (These estimates appear in column (2) of Table 14, in the bottom panel.)

[18] We have deliberately chosen this variable for illustrative purposes, because it shows very starkly how the simulated values of $\hat{\beta}_1$ need not follow any known distributional form. However, the same problems exist for any outcome variable.

3 Experiment Implementation

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'.[19] 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.

The sample is designed to cover a combination of small firms (with 1 – 20 permanent employees) and medium firms (21 – 100 permanent employees), with approximately half of sampled firms in each category. Figure 3 shows the distributions of firm size across the three countries.[20]

< Figure 3 here. >

The sample is designed to cover a variety of manufacturing sectors. Specifically, we sought to divide the sample more or less equally between food processing, garment manufacturing, leather products, metal products and wood products. Table 1 records the distribution of manufacturing sector by country.

< Table 1 here. >

Within each firm, we interview someone in a senior management position — in most cases, the firm manager. Table 2 shows the distribution of respondents' management position by country, for the sample participating in the experiment.[21]

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

[20] Note that, for graphical clarity, we have truncated the firm size above at 25; a total of 21 firms had more than 25 permanent employees at baseline.

[21] 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.

< Table 2 here. >

Tables 5 and 6 test balance in baseline covariates. Table 5 compares baseline covariates between committee and non-committee judges. For each variable, the table reports p-values for a t-test of equality in means and a Kolmogorov-Smirnov test for distributional equality. The table shows that the samples are generally well balanced: the only significant differences between groups are in the distribution of baseline permanent employees (though not a significant mean difference), and a significant difference in whether the firm had acquired machinery in the previous year.[22]

< Table 5 here. >

Table 6 compares the same covariates between firms that participated in the experiment (*i.e.* either as committee or non-committee judges) and those that did not (*i.e.* those firms that either refused or were not approached). The table shows that selection into the experiment itself is effectively 'as if random'. The only significant difference is that non-participant firms are slightly larger, on average, at baseline.

< Table 6 here. >

We conducted a follow-up survey in each country between November 2011 and January 2012. This involved resurveying the firms that participated in the experiment and those that did not. This includes an extensive set of dyadic questions, as outlined earlier. Figure 4 illustrates the network of pairwise questions for Ethiopia. Each node represents a different judge; an edge shows that one judge was asked about the other.

< Figure 4 here. >

[22] Of course, these differences could have been eliminated had we randomised after matching on covariates; for example, using the method of Bruhn and McKenzie (2009). However, we decided that the particular challenges of running a socialisation experiment with firm managers weighed in favour of the simpler randomisation device, *i.e.* drawing cards from a bag. There were two main reasons for this. First, we wanted to reassure participants that assignment to committees was done randomly. Second, we wanted to allow the possibility that judges may not arrive at their agreed time; *i.e.* we wanted to randomise the group of judges who actually arrived, rather than those who merely indicated their willingness to do so.

3.1 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.[23]

Table 3 shows the consequent assignments to committee and non-committee judging; Table 4 shows how committee judges were assigned to different committees.[24]

< Table 3 here. >

< Table 4 here. >

4 Results: Creation of network links

We begin by considering the effect of the experiment on the creation of network links. Table 7 shows the results; column (1) uses the pooled sample, and columns (2) to (4) are estimated on each country separately. In each specification, we find a large positive effect that is highly significant. For a pair of judges i and j on the same committee, the probability that i remembers sharing the committee with j is 38.2%. For some pair *not* on the same committee, the probability that i *wrongly* remembers sharing the committee is 2.5%.

< Table 7 here. >

[23] 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'.

[24] Note that two committees in Zambia each comprised only two judges; we drop these four judges from the subsequent analysis.

We then consider whether the judges have spoken since the competition. In Table 8, we define y_{ij} as a dummy for whether judge i agrees that he or she has spoken with judge j. Again, we estimate large and significant positive effects: these range from a point estimate of 10 percentage points in Ethiopia to an estimate of 23.7 percentage points in Zambia. In Table 9, we consider the topics discussed. Column (1) repeats column (1) of Table 8; that is, it considers whether *any* topic was discussed. Columns (2), (3) and (4) respectively consider whether the respondents reported discussing 'export strategies', 'labour management' and 'innovation and business advice'. We estimate positive and significant results for all outcomes; these range from an effect of 3 percentage points for exports to 11.8 percentage points for innovation.

< Table 8 here. >

< Table 9 here. >

Second, we measure the effect of the factsheets. As before, the outcome variable is defined in terms of judge i's recollection of his or her relationship with judge j. However, we augment the earlier estimating relationship by including dummies to record the factsheets that judge j received. In this way, we test for peer effects by considering whether a factsheet given to *judge* j had any effect upon the recollections of *judge* i. Table 11 reports the results; we consider whether judge i remembers judge j (column (1)), whether judge i reported having spoken to judge j since the competition (column (2)), and then the topics that judge i reported having discussed (columns (3) to (5)).

< Table 11 here. >

We find significant effects from three of the factsheets. First, consider the factsheet about CSAE — a factsheet that provided background information on the organisation overseeing the project, but that did not contain any information of relevance to business practices. This factsheet had large and significant positive effects on whether judge j was remembered by judge i, and on whether judge i had spoken to judge j. The factsheet had divergent effects upon discussion topics: a significant positive effect (of almost four percentage points) on the probability of having spoken about innovation and business practices, but a significant negative effect (of about 2.5 percentage points) on the probability of having spoken about export strategies. Second, consider the factsheet about innovation. This had no significant effect on the probability of a judge having been remembered, or of judges having spoken; however, it had positive and significant effects on the probability of discussing business-relevant topics. These include increases of about 5 percentage points in the probability of having discussed labour management and on the probability of having discussed innovation and business advice. Third, consider the factsheet about exports. This had a significant positive effect on the probability of judges having spoken, but no significant effect on discussion of any of the three defined business topics.

5 Results: Diffusion of business practices

We consider a range of outcome variables; these are grouped into the topics 'finance', 'investment and investment-related activities', 'labour management', 'imports and exports' and 'friends and relatives'. In each regression, we define the baseline sum-of-peers term in the same way as the outcome variable — so, for example, if the outcome variable is a dummy for whether the firm has a bank account, we regress on the sum of peers having a bank account at baseline.[25] In each table, we report estimations of 2 in the top panel (*i.e.* estimation of the level, y_{is1}) and estimations of 3 in the bottom panel (*i.e.* estimation of the difference, $y_{is1} - y_{is0}$).

[25] There is one exception: when we test whether the firm plans to begin exporting in 2012, we regress on the *actual* sum of export status.

Table 12 considers measures of firm finance: whether the firm has a bank account, a savings account or an overdraft (columns (1), (2) and (3)), and whether the firm currently owes money (column (4)). We find a positive and significant effect on the first difference of whether the first has a bank account; the equivalent levels estimate is also positive (with $p = 0.187$). Similarly, we find positive and significant diffusion of whether the firm has an overdraft (significant in both levels and first difference). We find no effect on whether the firm has either a savings account or whether the firm currently owes money.

< Table 12 here. >

We consider measures of investment (and other investment-related activities) in Table 13. We test diffusion of whether the firm advertised in the past six months (column (1)), whether the firm purchased machinery or equipment in the past year (column (2)), whether the firm introduced any new products in the past year (column (3)), whether the firm is registered for VAT (column (4)) and whether the firm uses electricity for production (column (5)). We find significant negative diffusion of whether the firm has introduced new products; this is significant in the first difference ($p = 0.006$), and the coefficient is negative in the level ($p = 0.385$). We also find a significant negative effect on whether the firm uses electricity for production; this is also significant in the first difference ($p = 0.051$), and the coefficient is negative and almost significant in the level ($p = 0.101$). In contrast, we find a significant positive diffusion of VAT registration; this is significant in the level ($p = 0.068$), and the coefficient is significant in the difference ($p = 0.195$). We find no significant effects on whether the firm recently advertised, or whether the firm purchased machinery or equipment.

< Table 13 here. >

Table 14 reports measures of labour management. We consider diffusion of whether the firm has multiple managers (column (1)), whether any of the firm's workers is a member of a labour union (column (2)), whether the firm provides meals for its workers (column (3)), whether the firm provides housing for its workers (column (4)), whether the firm provides toilets with running water to any of its manufacturing workers (column (5)) and whether the firm ever hires workers without referral (column (6)). Results here are mixed. We estimate a significant negative diffusion on the level of whether the firm has multiple managers ($p = 0.041$); however, estimating on the first difference produces a positive estimate that is nearly significant ($p = 0.177$). Similarly, we estimate a significant negative diffusion of whether the firm provides meals for workers ($p = 0.099$); but this estimate, too, has the opposite sign in the first difference. When we measure diffusion of providing toilets with running water, we find a significant positive effect in the difference ($p = 0.016$), but no significant estimate in the level (a negative coefficient, with $p = 0.236$). We find no significant effect on either provision of housing or hiring without referrals. We also find no effect on whether the firm has a labour union, though both level and difference estimates are positive and almost significant ($p = 0.184$ in the level and $p = 0.123$ in the difference).

< Table 14 here. >

Table 15 considers entrepreneurs' descriptions of their friends and relatives — we measure whether the respondent has a friend or relative as a bank official (column (1)), whether the respondent has a friend or relative as a party official or an elected official (column (2)), and whether the respondent has any friend or relative working for government (column (3)). We find a significant negative effect for the difference of whether the respondent has a friend as a bank official ($p = 0.039$), but no other significant effects.

< Table 15 here. >

Finally, in Table 16, we consider measures of firm imports or exports. In column (1), we consider diffusion of importing behaviour, and find no significant effect. We consider exporting behaviour in column (2). We find a significant negative effect in the level ($p = 0.080$; the first difference estimate is also negative, and the p-value is small ($p = 0.225$). In column (3), we consider a measure of whether the firm planned to start exporting in 2012; we find a positive point estimate that is almost significant ($p = 0.127$). In column (4), we estimate on the firm's own report of whether it started exporting in the preceding year; this is an alternative measure to the first difference specification in column (2) of the bottom panel. As in column (2) of the bottom panel, we estimate a negative coefficient with a small p-value ($p = 0.134$).

< Table 16 here. >

6 Conclusions

In this paper, we report results from the first field experiment designed to exogenously vary firms' network of peers. We have summarised a novel experimental protocol, and outlined a novel simulation method for testing peer effects in a 'linear-in-means'/'sum-of-means' framework. We have reported estimations on two simple specifications, both of which were outlined in our original research proposal document.

We find little evidence of diffusion. We find significant positive effects on two measures of finance (having a bank account and having an overdraft), on VAT registration, and on provision of toilet facilities to workers. We observe significant negative diffusion for exporting, the introduction of new products, the provision of meals to workers, and using electricity for production.[26] These results should be taken as suggestive given that we have tested multi-

[26] We also found a significant negative coefficient on the level measure of having multiple managers, but the coefficient is almost significant in the opposite direction in the difference.

ple outcomes and have based our discussions of significance upon separate hypothesis tests.[27] Nonetheless, there is some suggestion from these results that peer relationships may create positive diffusion of behaviour that is reasonably low risk and low cost (for example, having a bank account), but negative diffusion of behaviour that is more risky or costly (for example, exporting or innovating).

There may be several reasons that we do not find many significant positive diffusion effects. First, it may be that diffusion of many business practices requires more time than our design allowed: we conducted the follow-up survey between three and five months after the conclusion of the experiment. Second, it may be that the simple 'linear-in-means' model (or, in our case, 'sum-of-means') is too simplistic as a model of peer effects (see, for example, Hurder (2012)). Implicitly, many of our intuitions about peer effects rely upon a notion that information and business practices diffuse through independent adoption decisions. This may be a reasonable approach for the diffusion of technology among firms in highly competitive markets — for example, for the adoption of hybrid corn (Griliches, 1957). But for firms in less competitive markets — for example, African manufacturing firms competing in local markets — peers may have more ambiguous effects. In particular, entrepreneurs may face clear incentives *not* to encourage technology adoption by peers who could then compete away their profit (Foster and Rosenzweig, 1995). Additionally, peer relationships may be a mechanism for the diffusion not only of tales of success, but also of entrepreneurial horror stories — for example, stories of firms that tried and failed at exporting, or at introducing new products. If this interpretation of our results is correct, economists should be cautious in adopting simplistic narratives about the positive value of networks for firm performance.

[27] In the future we will introduce a correction to account for this multiple hypothesis testing — for example, a Bonferronni correction, or a Westfall-Young Stepdown Bootstrap, though this kind of correction may pose a computational challenge in the context of the simulation method that we have used.

Our results suggest several avenues for further analysis, including further analysis of the current experimental data. If the simple 'linear-in-means'/'sum-of-means' approach is a naïve representation of peer diffusion, there may be scope for considering alternative specifications. One obvious candidate is an influence model, in which peers are allowed to have differential effects depending upon their firm's baseline characteristics. For example, small firms may seek to emulate the business practices of larger or more successful firms, even if there is little diffusion in general. These and other questions remain to be explored and will be the focus of future work.

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Figures and Tables

Figure 1: **Simulated distribution of estimates: Diffusion of labour unionisation**

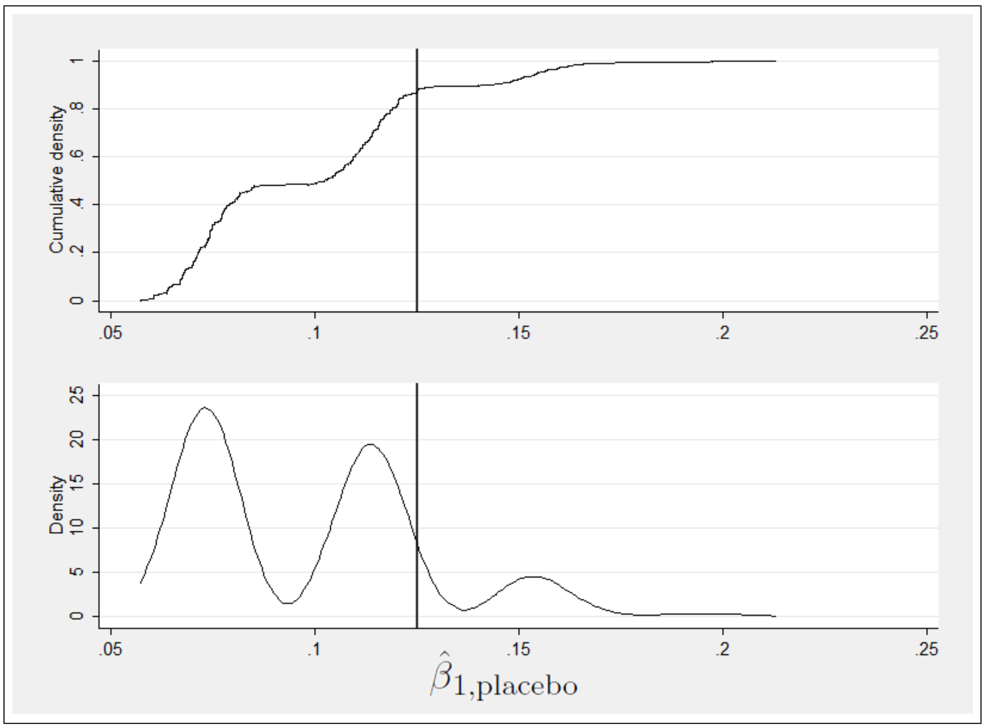


Figure 2: **Simulated distribution of p-values: Diffusion of labour unionisation**

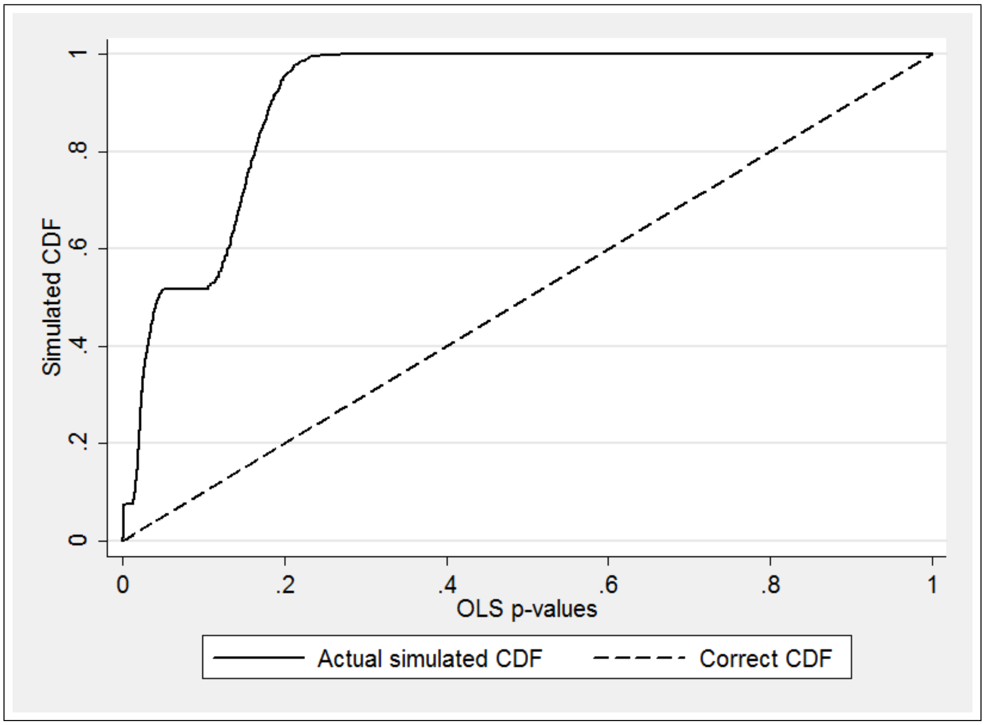


Figure 3: **Size distribution of sampled firms**

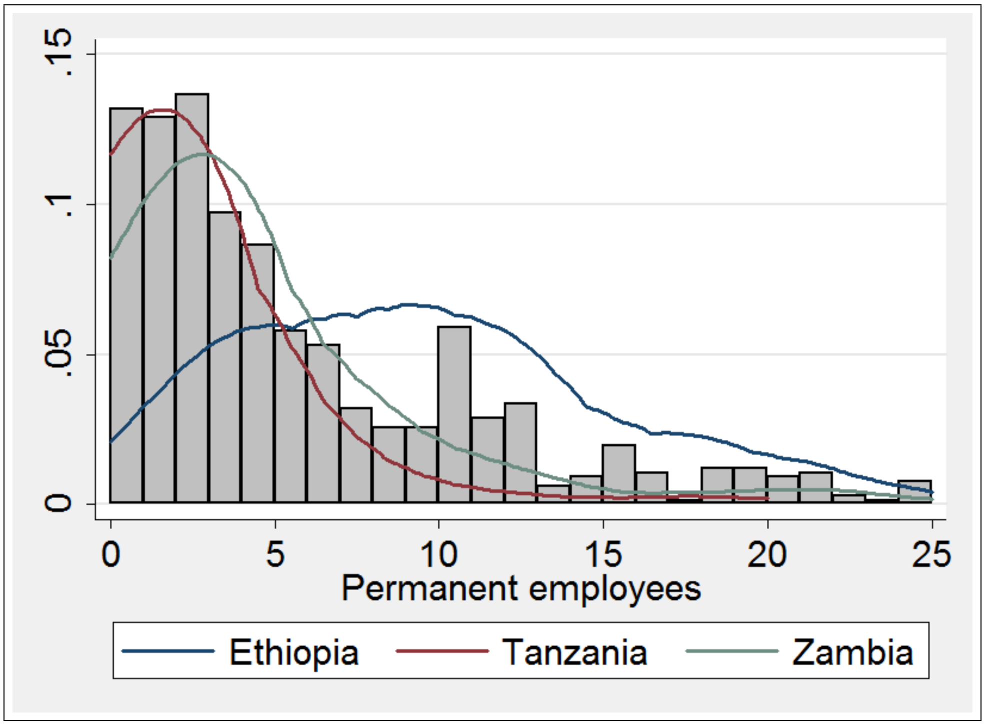


Table 1: **Sector of manufacturing**

	ETHIOPIA		TANZANIA		ZAMBIA	
FOOD PROCESSING	53	**21%**	21	**8%**	38	**14%**
GARMENTS	48	**19%**	58	**22%**	65	**25%**
LEATHER PRODUCTS	49	**20%**	37	**14%**	42	**16%**
METAL PRODUCTS	46	**18%**	50	**19%**	61	**23%**
WOOD PRODUCTS	54	**22%**	96	**37%**	57	**22%**
TOTAL	**250**	100%	**262**	100%	**263**	100%

Table 2: **Management seniority**

	ETHIOPIA	TANZANIA	ZAMBIA	
OWNER AND MANAGER	94	91	109	294
PRESIDENT / MANAGER	93	100	76	269
VICE PRESIDENT / DEPUTY MANAGER	23	0	5	28
DEPARTMENT HEAD	9	37	17	63
ACCOUNTS / FINANCE / ADMINISTRATION	17	0	0	17
OTHER	2	1	4	7
	238	229	211	678

Table 3: **Assignment to treatment: Committee and non-committee judges**

	PANEL JUDGE	NON-PANEL JUDGE	NON-PARTICIPANT
ETHIOPIA	86	40	112
TANZANIA	90	44	124
ZAMBIA	63	22	158
	239	106	394

Table 4: **Assignment to treatment: Judging committees**

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	TOTAL
ETHIOPIA	6	6	6	6	5	5	5	5	6	5	3	6	5	6	5	6	**86**
TANZANIA	6	5	6	6	6	6	5	5	6	6	6	4	6	6	6	5	**90**
ZAMBIA	4	4	4	5	5	6	6	4	4	3	3	3	3	2	2	5	**63**

Table 5: **Covariate balance: Committee judges versus non-committee judges**

	PANEL JUDGE			NON-PANEL JUDGE			Equality (p)	
	N	Mean	Std.Dev	N	Mean	Std.Dev	Mean	Distr.
Total permanent employees	237	5.768	8.560	100	6.950	7.761	**0.235**	**0.037****
Dummy: Owner is female	237	0.169	0.375	98	0.204	0.405	**0.445**	**1.000**
Owner's age (years)	236	38.432	8.737	99	38.000	9.805	**0.691**	**0.925**
Dummy: Firm registered	237	0.502	0.501	100	0.570	0.498	**0.256**	**0.874**
Dummy: Production uses electricity	237	0.793	0.406	100	0.760	0.429	**0.500**	**1.000**
Number of local competitors	232	20.203	49.022	98	18.969	37.242	**0.823**	**0.223**
Number of friends in business	223	11.242	18.151	96	9.635	11.243	**0.423**	**0.988**
Number of regular suppliers	231	4.693	4.734	99	4.677	6.040	**0.980**	**0.854**
Dummy: Firm exports	237	0.051	0.220	100	0.050	0.219	**0.981**	**1.000**
Number of product types	234	6.885	11.415	99	7.646	22.918	**0.686**	**0.852**
Dummy: Acquired machinery	231	0.385	0.488	99	0.515	0.502	**0.029****	**0.178**
Dummy: Bank account	237	0.506	0.501	99	0.515	0.502	**0.883**	**1.000**

*'**Mean equality** (p)' reports the p-value from a two-sample t test with equal variances.*
*'**Distr. equality** (p)' reports the p-value from a two-sample Kolmogorov-Smirnov test (calculated exactly).*
***Confidence:* '$*$': 90%; '$**$': 95%; '$***$': 99%.**

Table 6: **Covariate balance: Participants versus non-participants**

	PARTICIPANT			NON-PARTICIPANT			Equality (p)	
	N	Mean	Std.Dev	N	Mean	Std.Dev	Mean	Distr.
Total permanent employees	337	6.119	8.337	343	7.703	12.611	**0.054***	**0.427**
Dummy: Owner is female	335	0.179	0.384	336	0.149	0.356	**0.290**	**0.996**
Owner's age (years)	335	38.304	9.053	333	39.465	10.651	**0.129**	**0.366**
Dummy: Firm registered	337	0.522	0.500	340	0.535	0.499	**0.734**	**1.000**
Dummy: Production uses electricity	337	0.783	0.413	343	0.764	0.425	**0.544**	**1.000**
Number of local competitors	330	19.836	45.788	330	18.827	45.436	**0.776**	**0.300**
Number of friends in business	319	10.759	16.380	325	9.305	12.190	**0.201**	**0.967**
Number of regular suppliers	330	4.688	5.151	329	5.003	8.555	**0.567**	**0.516**
Dummy: Firm exports	337	0.050	0.219	343	0.070	0.255	**0.286**	**1.000**
Number of product types	333	7.111	15.704	338	5.796	7.454	**0.165**	**0.638**
Dummy: Acquired machinery	330	0.424	0.495	326	0.485	0.501	**0.121**	**0.557**
Dummy: Bank account	336	0.509	0.501	337	0.507	0.501	**0.969**	**1.000**

*'**Mean equality** (p)' reports the p-value from a two-sample t test with equal variances.*
*'**Distr. equality** (p)' reports the p-value from a two-sample Kolmogorov-Smirnov test (calculated exactly).*
***Confidence:* '$*$': 90%; '$**$': 95%; '$***$': 99%.**

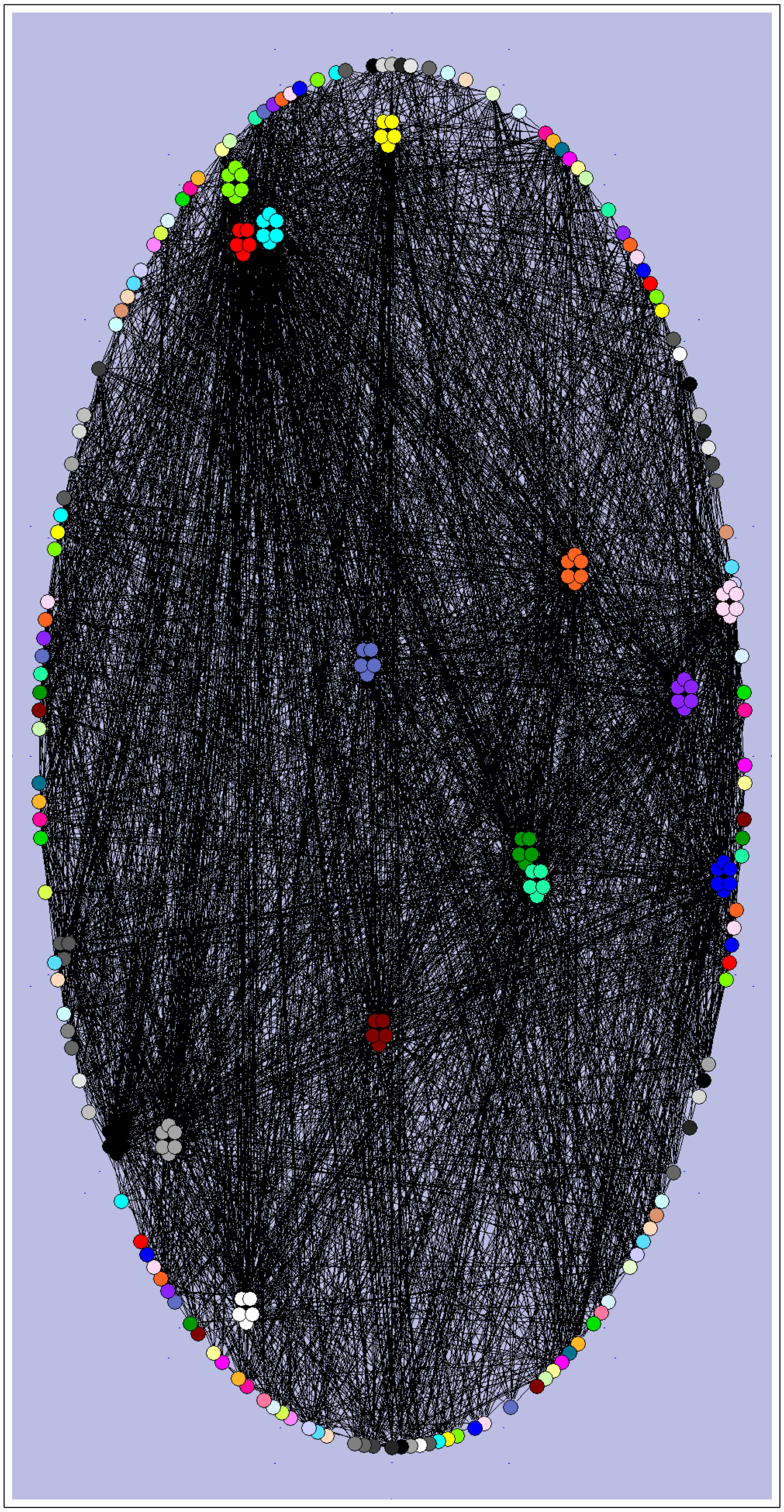


Figure 4: **Network structure of dyadic questions: Ethiopia**

Table 7: **Link creation: 'Were on on a judging panel with this person?'**

	(1) All	(2) Ethiopia	(3) Tanzania	(4) Zambia
Dummy: Same panel	**0.357*****	**0.236*****	**0.405*****	**0.472*****
	(14.80)	(7.73)	(10.17)	(9.76)
Constant	0.025***	0.011***	0.047***	0.016***
	(7.44)	(4.99)	(5.67)	(4.24)
Observations	9617	3407	3311	2899

Confidence: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; t statistics in parentheses.

The unit of observation is a dyadic question.

Table 8: **Link creation: 'Have you spoken to this person since?'**

	(1) All	(2) Ethiopia	(3) Tanzania	(4) Zambia
Dummy: Same panel	**0.161******	**0.100******	**0.176******	**0.237******
	(10.04)	(4.50)	(7.32)	(5.82)
Constant	0.013***	0.008***	0.022***	0.009***
	(8.23)	(3.78)	(6.49)	(4.01)
Observations	9617	3407	3311	2899

Confidence: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; t statistics in parentheses.

The unit of observation is a dyadic question.

Table 9: **Link creation: 'Did you discuss…?'**

	(1) Any	(2) Exports	(3) Labour	(4) Innovation
Dummy: Same panel	**0.161*****	**0.030*****	**0.065*****	**0.118*****
	(10.04)	(4.13)	(6.15)	(8.52)
Constant	0.013***	0.003***	0.006***	0.010***
	(8.23)	(3.27)	(4.92)	(6.43)
Observations	9617	9617	9617	9617

Confidence: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; t statistics in parentheses.

The unit of observation is a dyadic question.

Table 10: **Link creation: Effect of factsheets**

	(1)	(2)	(3)	(4)	(5)
			Discussion topics…		
	Remembers	Spoken	*Exports*	*Labour*	*Innovation*
Same panel × CSAE	**0.084****	**0.045***	**-0.023****	**0.003**	**0.038***
	(2.32)	(1.66)	(-2.03)	(0.20)	(1.66)
Same panel × exports	**0.038**	**0.058****	**0.011**	**0.005**	**0.034**
	(1.05)	(2.20)	(0.79)	(0.31)	(1.45)
Same panel × innovation	**0.046**	**0.029**	**0.021***	**0.050*****	**0.052****
	(1.32)	(1.11)	(1.78)	(3.04)	(2.19)
Same panel × labour	**-0.004**	**-0.022**	**-0.013**	**-0.013**	**-0.027**
	(-0.11)	(-0.89)	(-1.29)	(-0.93)	(-1.32)
Same panel	**0.297*****	**0.120*****	**0.032*****	**0.048*****	**0.082*****
	(8.00)	(5.50)	(2.68)	(3.94)	(4.61)
CSAE	0.000	-0.002	-0.001	-0.002	-0.003
	(0.05)	(-0.68)	(-0.81)	(-0.98)	(-1.18)
Exports	-0.004	0.001	-0.001	0.000	0.003
	(-0.95)	(0.41)	(-0.87)	(0.09)	(1.02)
Innovation	-0.006	0.000	-0.002	-0.001	-0.001
	(-1.56)	(0.13)	(-1.44)	(-0.67)	(-0.56)
Labour	-0.005	-0.003	-0.003**	-0.002	-0.002
	(-1.35)	(-0.91)	(-2.18)	(-1.26)	(-0.70)
Constant	0.030***	0.014***	0.006**	0.007***	0.011***
	(5.95)	(4.20)	(2.15)	(2.85)	(3.52)
Observations	9617	9617	9617	9617	9617

Confidence: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; t statistics in parentheses.
The unit of observation is a dyadic question.

Table 11: **Link creation: Salience of exporting**

	(1)	(2)	(3)	(4)	(5)
				Discussion topics...	
	Remembers	Spoken	*Exports*	*Labour*	*Innovation*
Dummy: Same panel × peer exported	**0.188**	**0.135***	**0.044**	**0.112****	**0.145****
	(1.59)	(1.81)	(1.24)	(2.12)	(2.27)
Dummy: Same panel	0.348***	0.154***	0.028***	0.059***	0.111***
	(14.31)	(9.77)	(3.96)	(5.74)	(8.11)
Dummy: Peer exported	-0.001	0.019**	0.006	0.003	0.015***
	(-0.08)	(2.21)	(1.31)	(0.92)	(2.68)
Constant	0.025***	0.012***	0.003***	0.005***	0.009***
	(7.39)	(7.82)	(3.19)	(4.90)	(6.01)
Observations	9586	9586	9586	9586	9586

Confidence: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; t statistics in parentheses.

The unit of observation is a dyadic question.

Table 12: **Peer effects: Finance**

	(1) *Firm has a bank account*	(2) *Firm has a savings account*	(3) *Firm has an overdraft*	(4) *Firm currently owes money*
Sum of peers	**0.057**	**0.040**	**0.119***	**-0.053**
	(1.42)	(1.17)	(3.03)	(-1.24)
	[0.079]	[0.122]	[0.001]	[0.108]
	[0.187]	**[0.362]**	**[0.098]**	**[0.312]**
Dummy: Committee judge	**-0.069**	**-0.058**	**-0.002**	**0.030**
	(-0.77)	**(-0.87)**	**(-0.09)**	**(0.50)**
Session dummies	✓	✓	✓	✓
Observations	329	326	325	305

	(1) *Firm has a bank account (difference)*	(2) *Firm has a savings account (difference)*	(3) *Firm has an overdraft (difference)*	(4) *Firm currently owes money (difference)*
Sum of peers	**0.172****	**0.106**	**0.172***	**0.013**
	(3.24)	(2.06)	(6.89)	(0.28)
	[0.001]	[0.020]	[0.000]	[0.389]
	[0.021]	**[0.234]**	**[0.098]**	**[0.711]**
Dummy: Committee judge	**-0.311*****	**-0.077**	**-0.008**	**0.045**
	(-2.65)	**(-0.77)**	**(-0.35)**	**(0.70)**
Session dummies	✓	✓	✓	✓
Observations	329	326	325	305

Parentheses show t-statistics from an OLS regression.
The first square brackets show p-values from a one-tailed test, using the t-statistics.
The second square brackets show p-values from a one-tailed test, using simulation (5000 replications).
Confidence: ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$. (For the 'sum of peers', we use p-values reported in the second square brackets.)

Table 13: **Peer effects: Investment and investment-related activities**

	(1) *Firm recently advertised*	(2) *Firm purchased machinery etc.*	(3) *Firm introduced new products*	(4) *Firm is VAT registered*	(5) *Firm produces using electricity*
Sum of peers	**0.022**	**-0.013**	**-0.023**	**0.051***	**-0.025**
	(0.77)	(-0.51)	(-1.01)	(1.01)	(-0.95)
	[0.221]	[0.305]	[0.156]	[0.157]	[0.172]
	[0.352]	**[0.628]**	**[0.385]**	**[0.068]**	**[0.101]**
Dummy: Committee judge	**0.026**	**0.128***	**0.130****	**0.102****	**0.125**
	(0.45)	**(1.77)**	**(2.47)**	**(2.01)**	**(1.35)**
Session dummies	✓	✓	✓	✓	✓
Observations	325	322	324	249	329

	(1) *Firm recently advertised (difference)*	(2) *Firm purchased machinery etc. (difference)*	(3) *Firm introduced new products (difference)*	(4) *Firm is VAT registered (difference)*	(5) *Firm produces using electricity (difference)*
Sum of peers	**0.062**	**0.059**	**-0.053*****	**0.072**	**-0.007***
	(1.55)	(1.12)	(-1.17)	(1.29)	(-0.26)
	[0.062]	[0.133]	[0.121]	[0.100]	[0.396]
	[0.218]	**[0.348]**	**[0.006]**	**[0.195]**	**[0.051]**
Dummy: Committee judge	**0.023**	**0.175**	**0.182****	**0.087**	**0.037**
	(0.28)	**(1.54)**	**(2.01)**	**(1.60)**	**(0.36)**
Session dummies	✓	✓	✓	✓	✓
Observations	325	322	324	249	329

Parentheses show t-statistics from an OLS regression.
The first square brackets show p-values from a one-tailed test, using the t-statistics.
The second square brackets show p-values from a one-tailed test, using simulation (5000 replications).
Confidence: ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$. (For the 'sum of peers', we use p-values reported in the second square brackets.)

Table 14: **Peer effects: Labour management**

	(1) *Firm has multiple managers*	(2) *Firm has unionised labour*	(3) *Firm provides meals*	(4) *Firm provides housing*	(5) *Firm provides flush toilets*	(6) *Firm hires without referral*
Sum of peers	**-0.097****	**0.014**	**-0.054***	**-0.008**	**-0.043**	**0.023**
	(-2.69)	(0.59)	(-1.86)	(-0.20)	(-1.10)	(0.76)
	[0.004]	[0.278]	[0.032]	[0.421]	[0.136]	[0.223]
	[0.041]	**[0.184]**	**[0.099]**	**[0.695]**	**[0.236]**	**[0.474]**
Dummy: Committee judge	**0.144****	**-0.003**	**-0.023**	**0.025**	**0.103**	**-0.039**
	(2.11)	**(-0.13)**	**(-0.36)**	**(0.71)**	**(1.18)**	**(-0.42)**
Session dummies	✓	✓	✓	✓	✓	✓
Observations	325	324	327	326	325	314

	(1) *Firm has multiple managers (difference)*	(2) *Firm has unionised labour (difference)*	(3) *Firm provides meals (difference)*	(4) *Firm provides housing (difference)*	(5) *Firm provides flush toilets (difference)*	(6) *Firm hires without referral (difference)*
Sum of peers	**0.082**	**0.125**	**0.022**	**0.072**	**0.172****	**0.072**
	(1.89)	(2.45)	(0.58)	(1.55)	(2.91)	(1.65)
	[0.031]	[0.008]	[0.283]	[0.062]	[0.002]	[0.051]
	[0.177]	**[0.123]**	**[0.629]**	**[0.392]**	**[0.016]**	**[0.543]**
Dummy: Committee judge	**-0.000**	**-0.008**	**-0.066**	**0.029**	**-0.220***	**-0.167**
	(-0.00)	**(-0.26)**	**(-0.91)**	**(0.65)**	**(-1.76)**	**(-1.26)**
Session dummies	✓	✓	✓	✓	✓	✓
Observations	325	324	327	326	325	314

Parentheses show t-statistics from an OLS regression.
The first square brackets show p-values from a one-tailed test, using the t-statistics.
The second square brackets show p-values from a one-tailed test, using simulation (5000 replications).
Confidence: ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$. (For the 'sum of peers', we use p-values reported in the second square brackets.)

Table 15: **Peer effects: Friends and relatives**

	(1) *Respondent has friend/relative as a bank official*	(2) *Respondent has friend/relative as elected or party official*	(3) *Respondent has friend/relative working for government*
Sum of peers	**0.001**	**-0.036**	**-0.019**
	(0.05)	(-0.96)	(-0.53)
	[0.480]	[0.170]	[0.298]
	[0.273]	**[0.650]**	**[0.706]**
Dummy: Committee judge	**-0.059**	**0.026**	**-0.025**
	(-0.92)	**(0.36)**	**(-0.20)**
Session dummies	✓	✓	✓
Observations	329	329	328

	(1) *Respondent has friend/relative as a bank official (difference)*	(2) *Respondent has friend/relative as elected or party official (difference)*	(3) *Respondent has friend/relative working for government (difference)*
Sum of peers	**-0.021****	**0.039**	**0.034**
	(-0.63)	(0.90)	(0.94)
	[0.266]	[0.185]	[0.175]
	[0.039]	**[0.207]**	**[0.243]**
Dummy: Committee judge	**-0.002**	**-0.041**	**-0.137**
	(-0.03)	**(-0.47)**	**(-1.07)**
Session dummies	✓	✓	✓
Observations	329	329	328

Parentheses show t-statistics from an OLS regression.
The first square brackets show p-values from a one-tailed test, using the t-statistics.
The second square brackets show p-values from a one-tailed test, using simulation (5000 replications).
Confidence: ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$. (For the 'sum of peers', we use p-values reported in the second square brackets.)

Table 16: **Peer effects: Importing and exporting**

	(1) *Firm imports*	(2) *Firm exports*	(3) *Firm plans to start exporting*	(4) *Firm started exporting in last year*
Sum of peers	**-0.002**	**-0.177****	**0.068**	**-0.086**
	(-0.04)	(-3.33)	(0.97)	(-1.86)
	[0.483]	[0.001]	[0.167]	[0.033]
	[0.523]	**[0.080]**	**[0.127]**	**[0.134]**
Dummy: Committee judge	**0.035**	**0.007**	**0.070**	**-0.008**
	(1.59)	**(0.23)**	**(1.45)**	**(-0.29)**
Session dummies	✓	✓	✓	✓
Observations	329	328	326	329

	(1) *Firm imports (difference)*	(2) *Firm exports (difference)*
Sum of peers	**0.020**	**-0.052**
	(0.38)	**(-0.98)**
	[0.354]	[0.164]
	[0.819]	[0.225]
Dummy: Committee judge	**-0.002**	**-0.022**
	(-0.09)	**(-0.65)**
Session dummies	✓	✓
Observations	329	328

Parentheses show t-statistics from an OLS regression.
The first square brackets show p-values from a one-tailed test, using the t-statistics.
The second square brackets show p-values from a one-tailed test, using simulation (5000 replications).
Confidence: ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$. (For the 'sum of peers', we use p-values reported in the second square brackets.)

Appendix: Further details on the experiment protocol

Advertising

Figure 5 shows the poster used in Zambia. This poster was translated into Amharic and Swahili and displayed in public places in Addis Ababa, Dar es Salaam and Lusaka. The content and stye of the poster formed the basis for other advertising run on radio and on Facebook.

In all three countries, applicants were able to apply by submitting a hard copy application form; in Tanzania and Zambia, applicants were also given the option of applying online.

Factsheets

Figures 6 to 9 show the English versions of the four factsheets distributed in each country. As noted, the factsheets relate to the Centre for the Study of African Economies, exporting, innovation and labour management.

Table 17 shows the structure of factsheet assignment. Each committee judge and each non-committee judge was randomly assigned to a row in this table, so that all rows were filled before assigning judges to any new positions. This ensured that, so far as possible, two-thirds of judges received factsheets and one-third did not; it also ensures that, so far as possible, each possible pair of factsheets was assigned the same number of times.

Figure 5: **Advertising for aspiring entrepreneurs: Zambian poster**

ASPIRE

Do you aspire to be a successful entrepreneur?

Do you aspire to start your own business?

Do you have a **business idea** that needs support?

If so, **apply for the chance to win US$1,000** to help you to start your own business!

The Centre for the Study of African Economies (University of Oxford, UK) is interested in learning about the growth of new business ideas in Zambia. We are running a business ideas competition for aspiring young entrepreneurs, and we want you to apply!

Who: Applications are open to any aspiring entrepreneur aged 18 – 25, male or female. (Note that you may be required to provide proof of your age.)
What: In July and August, we will be running a competition to reward aspiring entrepreneurs. You can win the chance to present and explain your idea to a group of Zambian business leaders. Those with the best project win US$1,000!
How: Apply online at **www.csae.ox.ac.uk/aspire/zambia**. There is **no application cost**.
When: It's with immediate effect and applications close on 22 July at 6pm.

TO WIN

US$1,000!!

Figure 6: **Factsheet: The Centre for the Study of African Economies**

The Centre for the Study of African Economies

Did you know...?

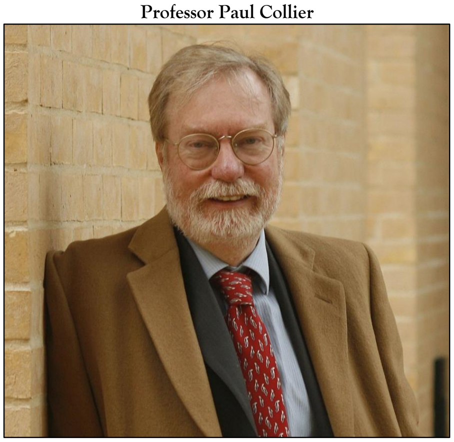
CSAE is celebrating 25 years of studying economic issues in Africa

CSAE was founded at the University of Oxford in 1986. This year, CSAE hosted its 25th Anniversary Conference, on the theme of '**Economic Development in Africa**'. There were **270 presentations** and almost **400 participants**.

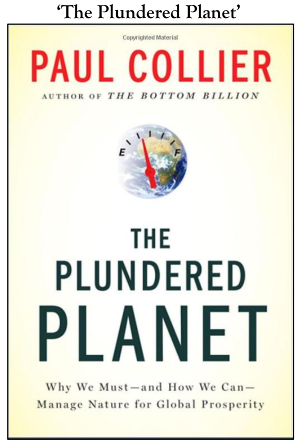
Paul Collier, the CSAE Director, has just published a new book

In his latest book '**The Plundered Planet**', Professor Collier argues that countries can ensure equitable development by using technological innovation, environmental protection and better government regulation. Professor Collier is one of the promoters of the **Natural Resource Charter**, a set of principles for governments and societies to use wisely the development opportunities created by natural resources.

Professor Paul Collier



'The Plundered Planet'



You can learn more about CSAE and our research from our website: www.csae.ox.ac.uk.

Videos from the 25th Anniversary Conference are available at http://www.csae.ox.ac.uk/conferences/.

Marcel Fafchamps
Professor of Development Economics
University of Oxford

Simon Quinn
Post-doctoral researcher
University of Oxford

Figure 7: **Factsheet: Exports**

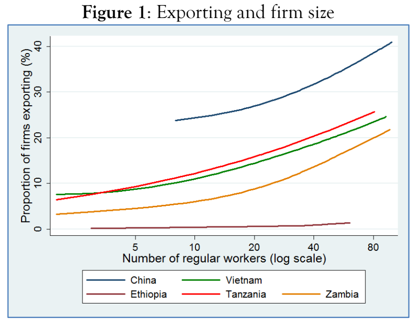
Asia-Africa Study Factsheet

Did you know…?

Fact 1: <u>African firms could export more</u>

Research shows that **Chinese** firms are more likely to export than firms of a similar size in Africa. **Figure 1** illustrates this. This suggests that more African firms could **follow the Chinese example** by exporting.

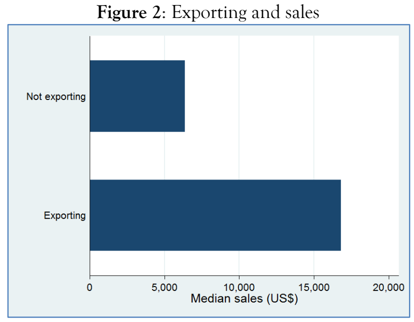
Figure 1: Exporting and firm size



Fact 2: <u>Firms that export have higher sales</u>

Exporting is an important way by which a firm can increase its market. **Figure 2** shows the median sales for African exporters and non-exporters. **On average, exporting firms sell much more.**

Figure 2: Exporting and sales



Here are some steps that a firm can take to start exporting:

- ✓ **Identifying export opportunities** (for example, by learning about foreign markets, or by finding local export agencies);
- ✓ Discussing exporting opportunities with a **bank or other finance organisation**;
- ✓ Obtaining any **necessary export permits** from government authorities;
- ✓ **Discussing exporting strategies with other firms** that export successfully.

We appreciate your participation in the study and we hope that you find this information useful.*

Marcel Fafchamps	**Simon Quinn**
Professor of Development Economics	Post-doctoral researcher
University of Oxford	University of Oxford

* Your firm was surveyed last year by the Centre for the Study of African Economies at the University of Oxford (UK). This was part of a research project to learn about African competitiveness in manufacturing. The study covered China, Vietnam, Ethiopia, Tanzania and Zambia. Many firm managers asked us to pass on results from the study, to help improve their firm's performance.

Figure 8: **Factsheet: Innovation**

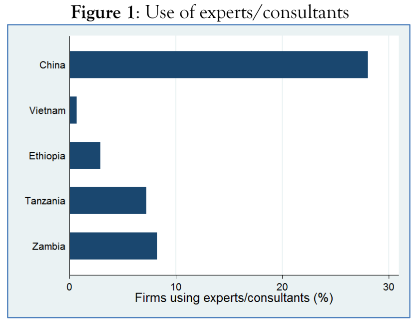
Asia-Africa Study Factsheet

UNIVERSITY OF OXFORD

Did you know…?

Fact 1: <u>African firms could use experts and consultants more</u>

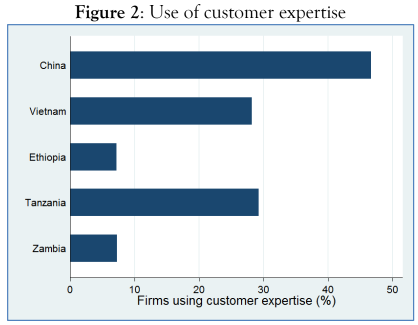
Figure 1: Use of experts/consultants



Research shows that **Chinese** firms are much more likely than firms in Africa to use **experts/consultants** to develop new products and to introduce new production processes. This is illustrated in **Figure 1**. This suggests that more African firms could **follow the Chinese example**.

Fact 2: <u>African firms could use customer expertise more</u>

Figure 2: Use of customer expertise



Customers can be an important source of ideas and technological expertise. **Figure 2** shows that Chinese firms are more likely to use the expertise of their customers for developing new products.

Here are some steps that a firm can take to innovate more successfully:

- ✓ Finding **consulting firms** that can advise on introducing new products or processes;
- ✓ Speaking to **suppliers of machines and equipment** about other firms and their innovations;
- ✓ Discussing potential innovations with **customers**;
- ✓ Joining a **business association**;
- ✓ **Discussing innovation strategies with other firms** that innovate successfully.

We appreciate your participation in the study and we hope that you find this information useful.*

Marcel Fafchamps	**Simon Quinn**
Professor of Development Economics	Post-doctoral researcher
University of Oxford	University of Oxford

* Your firm was surveyed last year by the Centre for the Study of African Economies at the University of Oxford (UK). This was part of a research project to learn about African competitiveness in manufacturing. The study covered China, Vietnam, Ethiopia, Tanzania and Zambia. Many firm managers asked us to pass on results from the study, to help improve their firm's performance.

Figure 9: **Factsheet: Labour management**

CSAE 25 YEARS — CENTRE FOR THE STUDY OF AFRICAN ECONOMIES

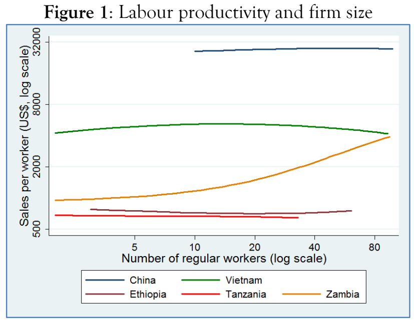
Asia-Africa Study Factsheet

UNIVERSITY OF OXFORD

Did you know…?

Fact 1: <u>Chinese firms produce more per worker than African firms</u>

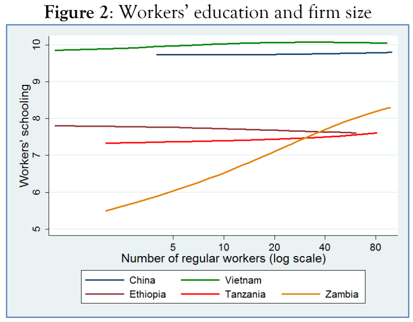
Figure 1: Labour productivity and firm size



Research shows that Chinese and Vietnamese firms produce substantially more per worker than firms in Ethiopia, Tanzania or Zambia.

Fact 2: <u>Asian firms hire more educated production workers</u>

Figure 2: Workers' education and firm size



Chinese and Vietnamese firms have a more highly educated production workforce. **Figure 2** compares the average education of entry-level production workers. This suggests that more African firms could **follow the Chinese example**.

Here are some steps that a firm can take to produce more per worker:

- ✓ Offering **on-the-job training or vocational training**;
- ✓ Relying on more educated workers to **supervise production**;
- ✓ Introducing **double or triple work shifts**;
- ✓ Boosting employee morale by offering **eating areas, private lockers and clean toilets**;
- ✓ **Discussing labour management strategies with other firms**.

We appreciate your participation in the study and we hope that you find this information useful.*

Marcel Fafchamps	Simon Quinn
Professor of Development Economics	Post-doctoral researcher
University of Oxford	University of Oxford

* Your firm was surveyed last year by the Centre for the Study of African Economies at the University of Oxford (UK). This was part of a research project to learn about African competitiveness in manufacturing. The study covered China, Vietnam, Ethiopia, Tanzania and Zambia. Many firm managers asked us to pass on results from the study, to help improve their firm's performance.

Table 17: **Structure of factsheet assignment**

	FACT SHEETS			
	CSAE	**EXPORTS**	**INNOVATION**	**LABOUR**
$\alpha \cdot 1$	✓	✓		
$\alpha \cdot 2$		✓	✓	
$\alpha \cdot 3$			✓	✓
$\alpha \cdot 4$	✓			✓
$\alpha \cdot 5$				
$\alpha \cdot 6$				
$\beta \cdot 1$	✓	✓		
$\beta \cdot 2$		✓		✓
$\beta \cdot 3$			✓	✓
$\beta \cdot 4$	✓		✓	
$\beta \cdot 5$				
$\beta \cdot 6$				
$\gamma \cdot 1$	✓		✓	
$\gamma \cdot 2$		✓	✓	
$\gamma \cdot 3$		✓		✓
$\gamma \cdot 4$	✓			✓
$\gamma \cdot 5$				
$\gamma \cdot 6$				
$\delta \cdot 1$	✓		✓	
$\delta \cdot 2$			✓	✓
$\delta \cdot 3$		✓		✓
$\delta \cdot 4$	✓	✓		
$\delta \cdot 5$				
$\delta \cdot 6$				
$\varepsilon \cdot 1$	✓			✓
$\varepsilon \cdot 2$		✓		✓
$\varepsilon \cdot 3$		✓	✓	
$\varepsilon \cdot 4$	✓		✓	
$\varepsilon \cdot 5$				
$\varepsilon \cdot 6$				
$\zeta \cdot 1$	✓			✓
$\zeta \cdot 2$			✓	✓
$\zeta \cdot 3$		✓	✓	
$\zeta \cdot 4$	✓	✓		
$\zeta \cdot 5$				
$\zeta \cdot 6$				