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INTERFIRM RELATIONSHIPS AND BUSINESS PERFORMANCE

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ABSTRACT

We organized business associations for the owner-managers of randomly selected young Chinese firms to study the effect of business networks on firm performance. We randomized 2,800 firms into small groups whose managers held monthly meetings for one year, and into a "no-meetings" control group. We find that: (1) The meetings increased firm revenue by 8.1 percent, and also significantly increased profit, factors, inputs, the number of partners, borrowing, and a management score; (2) These effects persisted one year after the conclusion of the meetings; and (3) Firms randomized to have better peers exhibited higher growth. We exploit additional interventions to document concrete channels. (4) Managers shared exogenous business-relevant information, particularly when they were not competitors, showing that the meetings facilitated learning from peers. (5) Managers created more business partnerships in the regular than in other one-time meetings, showing that the meetings improved supplier-client matching. (6) Firms whose managers discussed management, partners, or finance improved more in the associated domain, suggesting that the content of conversations shaped the nature of gains.

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1 Introduction

Much research has focused on barriers to firm growth that act at the level of the individual firm, such as limits to borrowing or lack of managerial skills. But firms do not operate in a vacuum: business relationships, which provide information, training, referrals, intermediate inputs and other services, are potentially central. Yet, because of various networking frictions, relationships may not form to maximize the net benefit of these services. A small literature going back to McMillan and Woodruff (1999) has started to explore these issues.[1] But we still know little about the effect of an exogenous increase in business networks on firm performance, about the underlying mechanisms, and about policies that can induce such a change.

We investigate these issues using a large scale field experiment in China, in which we organized experimental business associations for the owner-managers of small and medium enterprises (SMEs). Building on existing approaches to induce variation in business connections—especially those by Fafchamps and Quinn (2016) and Bernard, Moxnes and Saito (2015)—we created networks through regular meetings which had the explicit purpose of fostering business interactions. We also introduced additional interventions to learn about mechanisms. Our main findings are that business meetings substantially and persistently improved firm performance in many domains, and that learning and partnering were active mechanisms. These results suggest that differences in business networks may explain some of the large observed heterogeneity in firm performance (Syverson 2011). And, since SMEs produce a large share of the output in developing countries, they also suggest that organizing business associations can meaningfully contribute to private sector development.

In Section 2 we introduce our experimental design. In the summer of 2013 we invited micro, small and medium enterprises established in the preceding 3 years in Nanchang, China to participate in business associations. From 2,800 firms which expressed interest, we randomly selected 1,480 and randomized their owner-managers into meetings groups with 10 managers each. We informed the remaining 1,320 firms—the control group—that there was no room for them in the meetings.

Managers in each group were encouraged to hold monthly self-organized meetings. These meet-

[1] We review this literature in detail below.

ings were intensive: managers would typically visit the firm of a group member, take a tour, and then spend hours discussing business-relevant issues. The program lasted for one year.[2] We surveyed the firms in 2013 summer before the intervention (baseline), in 2014 summer shortly after the end of the intervention (midline), and in 2015 summer one year after the end of the intervention (endline). In the surveys we collected information on (1) Firm characteristics including sales, employment, borrowing, and other balance sheet variables; (2) Managerial characteristics, including—in the midline and endline surveys—management practices; (3) Firm networks and the type of interaction. We also have brief logs about the topics discussed in each meeting.

We introduced three additional interventions to learn about mechanisms. First, to explore peer effects, we created variation in the composition of groups by sector and size. Second, to document learning, similarly to Duflo and Saez (2003) and Cai, de Janvry and Sadoulet (2015) we provided randomly chosen managers with information about two different financial products.[3] Third, to explore the role of meeting frequency, building on Feigenberg, Field and Pande (2013) we organized one-time cross-group meetings for a random subset of managers.

In Section 3 we present results on the effect of the meetings. We first explore the overall impact of the intervention. Our basic regression is a firm fixed effects specification which effectively compares the within-firm growth rate in the meetings groups to that in the control group. We estimate that by the midline survey the sales of treatment firms increased by a significant 7.8 log points more than that of control firms, corresponding to a treatment effect on sales of 8.1 percent. This effect persisted to the endline survey: the baseline-to-endline change in log sales was 9.8 points higher in treatment than in control firms ($p < 0.05$), corresponding to a long-term treatment effect on sales of 10.3 percent. We also find significant and persistent effects for profits, production factors (employment and fixed assets), and inputs (materials and utility cost). These results show that the meetings had large and persistent positive effects on firm performance.

Turning to intermediate outcomes, we find that the meetings significantly and persistently

[2] As an incentive to participate, after the conclusion of the intervention we gave managers who answered our surveys and attended the meetings a certificate which provided access to certain government services. We also gave the certificate to managers in the control group who answered our surveys.

[3] We also provided the information to random control firms to ensure that the same share of treatment and control firms are directly informed.

increased the number of clients, the number of suppliers, as well as formal and informal borrowing. Using only the midline and endline survey—as data on it was not collected in the baseline—we also find that the meetings significantly and persistently increased management practices. There were also positive effects on innovation. Besides confirming the beneficial effects of the meetings, these results suggest three possible underlying mechanisms. Learning from peers which may have improved management and innovation; better firm-to-firm matching which may have created new partnerships; and improved access to finance. However these results do not yet conclusively identify the mechanisms. It is also possible that the meetings created growth though a different channel, which then led to an increase in intermediate outcomes.

We next explore the role of peer composition. We view this analysis as an internal consistency test that further supports our identification: all plausible mechanisms operating through business networks imply that having better peers should improve performance. We proxy peers' quality with their size (employment) at baseline, and ask whether firms randomized into groups with larger peers grow faster. We find significant and persistent peer effects on most outcomes, including sales, profits, an estimate for total factor productivity, the number of clients, and management practices; but not on employment or the number of suppliers. Overall these findings confirm, using a different source of variation, our basic result that business networks improve firm performance.

We discuss three main issues with identification and interpretation. One concern is that experimenter demand effects may drive the results. Contradicting this explanation, we find essentially no difference between the self-reported and the actual book value of sales. And demand effects are unlikely to explain peer effects, which are identified using only firms in the treatment. A second concern is that the meetings may have improved access to government officials. This logic also cannot easily explain peer effects, or the gains in management and innovation. A third issue is collusion: perhaps firms in the meetings coordinated price increases. But standard models of collusion would predict a reduction in quantity, contradicting the positive effects on factors and inputs; and collusion cannot explain the gains in management.

In Section 4 we study mechanisms. We use our additional interventions to cleanly document two channels for improvement: learning and partnering. And we present suggestive evidence from the meeting logs on a third channel: access to finance. We begin with learning and show that the meetings diffused business relevant information. We do this using the intervention in which we provided information about two different financial products (independently) to randomly chosen managers. For both products, we find that uninformed managers in groups with a higher informed share were much more likely to apply, providing direct evidence on learning as a mechanism. We also show that for the more rival product, a cash grant for the firm—which could help a competitor's business—diffusion was weaker in groups in which firms on average had more competitors. In contrast, for the less rival product, a savings opportunity for the manager, diffusion was not weaker in groups with higher competition. These results suggest that the diffusion of rival information was limited by product market competition. In independent work, Hardy and McCasland (2016) find that the diffusion of a new weaving technique in Ghana was lower in treatments with higher experimentally induced competition. Taken together, their findings and ours highlight a novel friction in technology diffusion: the endogenous (dis)incentive to transmit information.

We document evidence on the second mechanism—improved access to suppliers and clients—using the intervention of one time cross-group meetings. We show that firms established a significant 1.2 more direct partnerships—supplier, client, or joint venture—with their regular group members than with their cross-group members. Firms also got referrals from a significant 2.1 more peers in their regular group than in their cross-group. These results are direct evidence that the meetings reduced the cost of partnering: this is why more new connections came from the regular, and not the cross group. We also find that in hypothetical trust games managers exhibited significantly higher trust towards their regular than their cross-group partners, suggesting that trust built through repeated interactions may have contributed to the improved partnering we observe.

Finally we exploit the meeting logs to document suggestive evidence that the content of conversations affected the domain of improvement. We focus on three domains: management, partnering, and access to finance. For both management and access to finance, we find that firms improved more in a domain if their managers were in groups that discussed that domain. In contrast firms did not improve more in a domain if their managers were in groups that discussed *different* domains. This fact rules out the most plausible alternative explanation that better groups discussed more topics and improved in more domains. We find broadly similar but insignificant effects for partnering. Our results suggest that the learning mechanism may have contributed to the improvements in management, and also that access-to-finance may have been a third mechanism.

In the concluding Section 5 we discuss several implications of the results. We begin with a cost-benefit calculation. We estimate that for the average firm the profit gains from the meetings exceeded the wage cost of attending them by a factor of 5. Thus the intervention was extremely cost effective. A natural question then is why managers did not organize meetings for themselves. There are several possible reasons. There could be significant search costs and trust barriers in organizing meetings; there may be a public good problem if these costs fall on a single organizer; and, paralleling the argument of Bloom, Eifert, Mahajan, McKenzie and Roberts (2013a), managers may have underestimated the gains from business associations.

We then compare our impacts to other interventions. Business training is often estimated to have modest and insignificant effects on firm performance (McKenzie and Woodruff 2014). For intensive and personalized management consulting Bloom et al. (2013a) estimate a productivity gain of 17%. We find smaller effects—an 8 percent sales increase—but our intervention is cheaper and highly cost effective. Their results and ours suggest that intensive interventions may have a higher chance of improving performance, perhaps through a ''demonstration effect" of directly observing superior business practices. And the fact that both their sample and our sample was selected suggests that interventions may have a large effect when participants are interested in improving their business. We conclude that organizing regular business meetings for such firms can be an effective tool for private sector development.[4]

Our work builds on and contributes to three main literatures. Our research questions are most related to the work on firm-to-firm interactions. Theories in this area include Acemoglu, Carvalho, Ozdaglar and Tahbaz-Salehi (2012), Antras and Chor (2013), Oberfield (2013) and Eaton, Kortum and Kramarz (2015), who explore the aggregate and efficiency implications of supply chain networks. Evidence from observational data suggest that business networks can improve several firm outcomes, including access to credit (McMillan and Woodruff 1999, Khwaja, Mian and Qamar 2011,

[4] One example of a somewhat similar policy intervention involving large firms and government agencies is the "Mesas ejecutivas" program in Peru (Ministerio de la Produccion del Peru 2016).

Haselmann, Schoenherr and Vig 2016), managerial compensation policy (Shue 2013), investment performance (Hochberg, Ljungqvist and Lu 2007) and access to business partners (Bernard et al. 2015, Bernstein, Giroud and Townsend 2016).[5] There is almost no experimental evidence on the impact of firm networks, except for the pioneering study by Fafchamps and Quinn (2016) who document the diffusion of some management practices through connections created by joint committee membership. Our contribution to this literature is to experimentally evaluate the impact of business networks on a broad range of firm outcomes, and to identify specific mechanisms.

Our methodology and policy results build on a literature that uses experiments to study private sector development. De Mel, McKenzie and Woodruff (2008) measure the return to capital in microenterprises. Several papers reviewed in McKenzie and Woodruff (2014) study the effects of business training. Bloom et al. (2013a) and Bruhn, Karlan and Schoar (2013) measure the impact of management consulting, and Brooks, Donovan and Johnson (2016) evaluate a business mentoring program. We contribute to this work with a large-scale experiment on the key but understudied segment of SMEs; and with evaluating the new policy intervention of organizing business associations.

Our results on mechanisms relate to a literature on network effects in economics. This includes research on peer effects, information diffusion in networks, network-based referrals, and network-based trust.[6] We contribute to this work by documenting peer effects, referrals, and the role of trust in the new domain of managerial networks; and especially to the work on information diffusion by highlighting—together with Hardy and McCasland (2016)—the new mechanism that competition can limit the transmission of rival information.

[5] Also related is the work about agglomeration effects, reviewed in Duranton and Puga (2004) and Rosenthal and Strange (2004).

[6] See for example Sacerdote (2001) on peer effects, Banerjee, Chandrasekhar, Duflo and Jackson (2013) on information diffusion, Ioannides and Loury (2004) on referrals, and Karlan, Mobius, Rosenblat and Szeidl (2009) on trust. We review these literatures in more detail when we discuss the specific results below.

2 Context, experimental design and data

2.1 Context

Our experimental site was Nanchang, the capital city of Jiangxi province, located in southeastern China. In 2014 the city had a population of around 5 million people, and a GDP of 58 billion dollars, which ranked it as the 19th among the 32 capital cities in China. Nanchang was growing fast before the start of our study, with over 30,000 microenterprises and SMEs established during 2010-2013.

We conducted our intervention in collaboration with the Commission of Industry and Information Technology (CIIT) in Nanchang, one of the main government departments in charge of private sector development.

2.2 Interventions

Basic experiment. In the summer of 2013, through CIIT we invited all microenterprises and SMEs established in the preceding 3 years in Nanchang to participate in business associations. Around 5,400 firms expressed interest. We randomly selected 2,800 firms from this pool as our study sample. Almost all of these firms were owner managed, and from here on we refer to the CEO of a firm simply as the manager. Out of the study sample we randomly selected 1,480 firms—the treatment group—and randomized them into meetings groups with 10 firms each.[7] We then informed the 1,320 control firms that there was no room for them in the meetings.

The managers in each meeting group were expected to meet once a month, every month, for one year. We organized the first meetings, in collaboration with CIIT, in August 2013. For this first meeting only, we offered the managers in each group print material containing business-relevant information. We gave the same material to control firms as well. CIIT chose one of the managers in each meeting group to be the group leader. This person was responsible for planning and scheduling all subsequent monthly meetings. For each meeting, the group leader took notes on the location,

[7] To ensure that the managers of the firms in each meeting group were relatively close to each other, we divided the study area into local regions, and randomized firms into the treatment and control group, and treatment firms into meetings groups, at the local region level.

date, topics discussed, and the main takeaways, and submitted the log to us.

According to informal reports and the meeting logs, in most groups members took turns in hosting the meetings. In a typical meeting, group members toured the firm of the host manager, and then spent hours discussing business relevant issues. Typical meetings lasted for about half a day. Common discussion topics included borrowing, management, suppliers and clients, hiring, recent government policies, and marketing.

To provide incentives to participate, we offered managers who answered our surveys and attended at least 10 out of the 12 monthly meetings a certificate from CIIT. The certificate offered improved access to government services, including government funding and admission to entrepreneur training programs at local universities. In addition the certificate may have been viewed as a potential signal of firm quality. We also offered managers of firms in the control group the same certificate if they answered our surveys. We gave the certificate to the firms after the conclusion of the one-year program.

Additional interventions. To improve identification and explore mechanisms we also introduced three additional interventions. First, to help measure peer effects, we created variation in the composition of groups by size and sector. Almost all of our firms were from two sectors, manufacturing and services. In each region, we created two firm size categories, "small" and "large" by the median employment of our sample firms in that region. We then created four kinds of groups: small firms in the same sector; large firms in the same sector; mixed size firms in the same sector; and mixed size and mixed sector. We randomized treated firms into these groups in each region.

Second, to measure information diffusion, we gave information about two financial products to randomly chosen managers. The first product was a funding opportunity for the firm, the second a savings opportunity for the manager. The firm funding opportunity allowed managers to apply for a government grant of up to RMB 200,0000 (about USD 32,000). Because it could help the business of a competitor's firm, managers may have viewed this product to be rival and may have been unwilling to discuss it with competitors. The private savings opportunity offered an annual return of almost 7%, which was higher than the typical return of available high-yield saving products in the market (about 4%). Because it could not directly help a competitor's business, managers may have viewed this product to be less rival and may have been more willing to discuss it with competitors.[8]

We distributed information about each product via phone calls and text messages to 0%, 50% or 80% of the managers in each meeting group. We randomly assigned about one third of the meeting groups to each of these three treatment intensities.[9] We also distributed the information to 40% of control firms to ensure that the same share of treatment and control firms have the information. We randomized and distributed the information independently for the two products.

Finally, to learn about the role of meeting frequency, we organized one time cross-group meetings. We randomized 439 managers in the meetings treatment into 43 "cross-groups" of around 10 managers each, such that no two managers from the same meetings group were in the same cross-group. Each cross-group met once, in February 2014. And in the 2014 midline survey we asked managers to play hypothetical trust games (with large payoffs) with a randomly selected regular group member as well as with a randomly selected cross-group member.

2.3 Surveys

We conducted a baseline survey before the intervention in 2013 summer, a midline survey after the intervention in 2014 summer, and an endline survey in 2015 summer. Our enumerators conducted the surveys in person with the managers. Because the fiscal year in China ends in June, data in the baseline survey refer to the fiscal year before the intervention; data in the midline survey refer to the fiscal year that almost fully overlaps with the meetings; and data in the endline survey refer to the fiscal year after the conclusion of the meetings.

In the surveys we collected information from both treatment and control firms about the following groups of variables. (1) Firm characteristics. Profits, sales, costs, utility expenses, spending on intermediate inputs, and other balance-sheet variables. For sales we have two measures: besides the self-reported value in the survey we also have the actual book value. To obtain it, at the conclusion of the survey our enumerators asked the accountant of the firm to physically show the value in the firm's book. (2) Managerial characteristics. Demographics, measures of wellbeing, and—in the

[8] Both products were in limited supply.

[9] We stratified this randomization by group type.

midline and endline survey—questions on management. These questions were adapted from the Bloom and Van Reenen (2007) management survey and covered five areas of management: evaluation and communication of employee performance, targets and responsibilities, attracting and incentivizing talent, process documentation and development, and delegation. (3) Firm networks. The number and type of business connections (supplier, buyer, joint venture) both within and outside the group, and information on the nature of any relationship with group members (competitor or some type of partner).[10] (4) Whether managers applied for the funding opportunities about which we had distributed information. (5) Other outcomes measuring product innovation, business with the government, and employee satisfaction. We included questions on these outcomes only in the endline survey.

2.4 Summary statistics and randomization checks

Table 1 shows basic summary statics from the baseline survey. The first three columns report the means for all firms, treatment firms, and control firms; and the final columns reports the difference between treatment and control firms. Panel A on firm characteristics shows that in 2013 average firm age was about 2.3 years, and that 98%of firms were domestic private enterprises.[11] About half of the firms were in manufacturing and 48% in services.[12] Consistent with self-selection of better firms into our sample, in spite of their young age these firms employed on average 36 workers. But the large standard deviation of employment (86) shows that there was much cross-firm heterogeneity.

Panel B presents managerial characteristics. The vast majority of managers were men, and in 2013 they were on average 41 years old. Almost a third of them had a college education. Many managers had government connections: 23% had worked either in government or in state-owned firms, and 20% of them were members of the Communist Party of China. Managers reported to

[10] Because the firms in each group came from a large pool, there were essentially no preexisting in-group partnerships at baseline.

[11] The remaining two percent were either privatized formerly state-owned firms, whose CEOs were appointed by the government; or foreign-owned firms. In both cases the local CEO was responsible for essentially all business-relevant decisions and is the person we label the manager.

[12] Among others, firms in the manufacturing sector included textile, automobile and furniture companies; and firms in the service sector included restaurants, wholesalers, and transportation companies.

Table 1: Summary Statistics: Firm and Manager Characteristics

	All Sample	Treatment	Control	Difference
Number of Observations	*2646*	*1409*	*1237*	
Panel A: Firm Characteristics (2013 Baseline)				
Firm Age	2.34	2.39	2.29	0.1
	(1.75)	(1.72)	(1.77)	(0.068)
Ownership - Domestic Private Firms	0.98	0.98	0.98	0
	(0.15)	(0.15)	(0.15)	(0.006)
Sector - Manufacturing	0.5	0.51	0.48	0.03
	(0.01)	(0.013)	(0.014)	(0.019)
Sector - Service	0.48	0.49	0.47	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
Number of Employees	36.19	36.33	36.01	0.32
	(86.49)	(90.63)	(81.55)	(3.37)
Panel B: Managerial Characteristics (2013 Baseline)				
Gender (1=Male, 0=Female)	0.84	0.846	0.837	0.01
	(0.37)	(0.36)	(0.37)	(0.014)
Age	40.84	41.05	40.59	0.46
	(8.85)	(8.46)	(9.27)	(0.34)
Education - College	0.29	0.288	0.295	-0.007
	(0.45)	(0.45)	(0.46)	(0.018)
Government Working Experience	0.23	0.23	0.22	0.01
	(0.42)	(0.42)	(0.41)	(0.02)
Communist Party Member (1=Yes, 0=No)	0.205	0.207	0.204	0.003
	(0.4)	(0.4)	(0.4)	(0.016)
Working Hours - Weekday	9.62	9.64	9.6	0.04
	(2.81)	(2.81)	(2.79)	(0.12)
Working Hours - Weekend	7.61	7.47	7.77	-0.3
	(4.53)	(4.57)	(4.48)	(0.19)

Note :Standard deviations in parentheses for columns (1)-(3). Column (4) reports the difference in characteristics between treatment and control groups, and standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

have been very busy: they worked on average 9.6 hours during weekdays and 7.6 hours during weekends. That in spite of their intense schedules managers were willing to participate in the meetings suggests that they thought them to be valuable. There are no significant differences between the treatment and control firms in any of the variables in the table, confirming that our randomization is valid.

Table 2 shows summary statistics on firms' business activities. Panels A and B present data on business connections with suppliers, clients, and lenders. The average firm seems to have had a substantial customer and supplier base, with 46 clients and 16 suppliers. About 25% of firms

Table 2: Summary Statistics: Business Activities

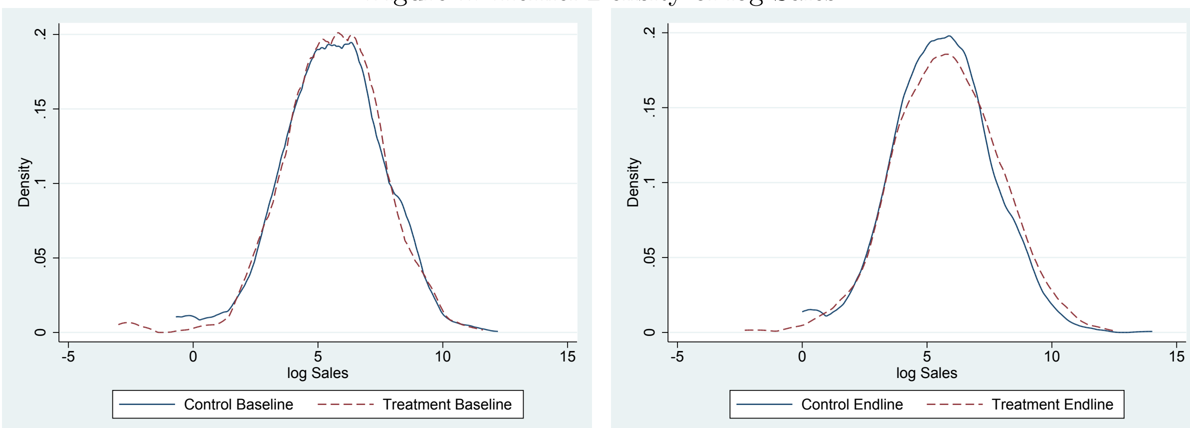
	All Sample	Treatment	Control	Difference
Number of Observations	*2646*	*1409*	*1237*	
Panel A: Partnership (2013 Baseline)				
Number of Clients	45.89	45.58	46.23	-0.65
	(57.37)	(56.16)	(58.74)	(2.24)
Number of Suppliers	16.38	16.7	16.02	0.68
	(19.23)	(20.3)	(17.94)	(0.75)
Panel B: Borrowing (2013 Baseline)				
Bank Loan (1=Yes, 0=No)	0.25	0.25	0.25	0
	(0.43)	(0.44)	(0.43)	(0.017)
Informal Loan (1=Yes, 0=No)	0.12	0.114	0.13	-0.02
	(0.33)	(0.32)	(0.34)	(0.013)
Panel C: Accounting (2013 Baseline)				
Sales (10,000 RMB)	1593.62	1510.7	1686.19	-175.57
	(6475.18)	(5291.86)	(7603.11)	(252.32)
Log Sales	5.59	5.6	5.58	0.02
	(2.01)	(1.99)	(2.02)	(0.08)
Net Profit (10,000 RMB)	79.23	77.26	81.52	4.25
	(205.35)	(199.92)	(211.55)	(8.09)
Panel D: Exit and attrition (2015 Endline)				
Shut Down (%)	10.92	10.86	10.99	-0.13
	(31.11)	(31.12)	(31.29)	(1.2)
Attrition (%)	9.24	9.46	9.00	0.46
	(28.96)	(29.28)	(28.62)	(1.09)

Note :Standard deviations in parentheses for columns (1)-(3). Column (4) reports the difference in characteristics between treatment and control groups, and standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

borrowed from formal banks and 12% borrowed from friends and relatives in the previous year. The relatively large share of informal borrowing suggests frictions in getting formal loans, perhaps because they often require collateral or government guarantors.

Panel C reports data on accounting measures of firm performance. The average net profit was RMB 792,300 (about USD 130,000), but this masks a lot of heterogeneity as indicated by the large standard deviation. A unitless measure of heterogeneity is the coefficient of variation (standard deviation divided by the mean), which for log sales is 0.36, higher than but roughly comparable to the corresponding value of 0.26 in the Banerjee and Duflo (2014) administrative data on mid-sized Indian firms. Consistent with the randomization, there are no significant differences between treatment and control firms in any of these variables.

Figure 1: Kernel Density of log Sales



Finally, Panel D shows that between the baseline and the endline survey about 10.9% of firms in our sample closed down and also that 9.2% disappeared due to attrition. There are no significant differences in either the exit or the attrition rate between treatment and control firms.

The share of firms which did not change their report of sales, the number of suppliers, and the number of clients between the baseline and the endline survey is 1%, 4%, and 10%, respectively. These relatively low values suggest that basic misreporting in the survey was infrequent.

3 Business meetings and firm performance

In this section we measure the impact of the meetings. We show that the meetings improved firm performance in many domains, and also that firms randomized into groups with better peers grew faster. Then in the next section we will study mechanisms.

3.1 Effect of meetings

Graphical evidence. We begin the analysis with graphical evidence that highlights some key patterns in the data. Figure 1 plots the kernel density of log sales for the treatment and the control group, both at baseline and at endline. Given that the surveys were conducted at fiscal year end, the baseline data refer to the twelve-month period before the start, and the endline data refer to the twelve-month period after the end of the one-year meetings intervention. The left panel shows that—consistent with the randomization—before the intervention the distribution of log sales was similar in the treatment and control groups. The right panel shows that one year after the intervention the distribution of log sales for treatment firms was—slightly but visibly—to the right of that for control firms. The shift is present for a large part of the domain, showing that the meetings treatment increased sales for a substantial range of firm sizes. And while the shift seems visually small, this is mainly because the large heterogeneity of log sales demands a wide range on the horizontal axis in the figure.

To quantify the shift, and to explore other outcomes, we now turn to regressions.

Empirical strategy. Our main empirical specification is

$$\begin{aligned} y_{it} = const + \beta_1 \cdot Midline_{it} + \beta_2 \cdot Endline_{it} \\ + \beta_3 \cdot Meetings_{it} \times Midline_{it} + \beta_4 \cdot Meetings_{it} \times Endline_{it} \\ + Firm\ f.\ e. + \varepsilon_{it}. \end{aligned} \quad (1)$$

Here i indexes firms, t indexes years, and y_{it} is an outcome variable such as log sales. $Meetings_{it}$ is an indicator for the treatment, which is time-invariant and equals one if the firm is invited to the meetings. $Midline_{it}$ is an indicator for the midline survey wave and $Endline_{it}$ is an indicator for the endline survey wave. The firm fixed effects take out time-invariant heterogeneity, including whether the firm is in the meetings treatment or in the control group. This specification is analogous to the one used by De Mel et al. (2008).

Our coefficients of interest are β_3 and β_4, which measure—given the fixed effects specification—the differential change over time in the outcome variable in the treatment group relative to that in the control group. Intuitively, β_3 is the treatment-induced additional growth in y between baseline and midline; and β_4 is the treatment-induced additional growth in y between baseline and endline. These coefficients can be compared to β_1 and β_2 which measure the growth in y for the firms in the control group. The key identification assumption is that firms in the treatment group did not have systematically different trajectories from those in the control group for reasons other than the meetings treatment itself. Because the treatment is randomized, any potential omitted variable would have to be a "side-effect" of the treatment itself, such as better access to government officials.

Table 3: Effect of Meetings on Firm Performance

Dependent var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Total Assets	Material Cost	log Utility Cost	log Productivity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Midline	0.004	11.89**	0.018	0.017	0.026	-0.018	0.028***
	(0.019)	(5.402)	(0.017)	(0.019)	(0.022)	(0.021)	(0.010)
Endline	0.013	12.21	0.029	0.040	0.026	0.026	0.012
	(0.029)	(8.278)	(0.024)	(0.035)	(0.03)	(0.027)	(0.016)
Meetings*Midline	0.078**	25.75**	0.052**	0.065**	0.082	0.098***	0.004
	(0.036)	(12.59)	(0.026)	(0.031)	(0.052)	(0.036)	(0.017)
Meetings*Endline	0.098**	32.60*	0.077*	0.116**	0.123***	0.12***	0.036
	(0.049)	(18.52)	(0.044)	(0.047)	(0.040)	(0.046)	(0.025)
Observations	7,857	7,664	7,857	7,851	7791	7676	7851
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.004	0.005	0.004	0.005	0.0007	0.007	0.004

Note: Standard errors clustered at the meeting group level for treated firms and at the firm level for control firms. *** p<0.01, ** p<0.05, * p<0.1.

We will discuss possible omitted variables as we present the results, and also in Section 3.3 below.

Because the treatment can induce correlated errors within a group, for inference we cluster standard errors at the level of the meeting group for treatment firms, and at the level of the firm for control firms. And to control for potential outliers, in specifications in which the dependent variable is neither binary nor a share between zero and one, we winsorize the regressions at 1% in both tails of the distribution.[13]

Results. Table 3 presents results for a range of firm performance measures. Start with column 1 where the outcome is log sales, and consider first the effect at midline, i.e., the fiscal year in which the meetings took place. While log sales in the control group increased, from baseline, by an insignificant 0.004, log sales in the meetings treatment increased by an additional significant 0.078, corresponding to a treatment effect on sales of 8.1%. This effect persisted in the fiscal year after the meetings program ended: the coefficient of the interaction between *Meetings* and *Endline* shows that sales growth between baseline and endline was 9.8 log points higher for treated than for control firms, corresponding to a 10.3% treatment effect on sales. Similarly, column 2 shows that average profits increased by a significant RMB 257,500 (about $36,000) more in the treatment

[13] Not winsorized specifications yield similar results.

group than in the control group by midline, and the difference persisted by endline. These results show large impacts for two key business-relevant outcomes.

The remaining columns look at various components of the production process. Columns 3 and 4 show evidence on factors. We estimate significant and persistent treatment effects on both employment and fixed assets, ranging from 5 to 12 log points. Columns 5 and 6 focus on intermediate inputs. The treatment effect on materials is an insignificant but large 8.2 log points by midline, which increases further to a significant 12.3 log points by endline. The treatment effect on utility cost has a similar magnitude and is highly significant throughout. Finally, column 7 shows the impact on total factor productivity which we inferred using coefficients from estimating a revenue production function in the control group. We find insignificant effects which are small at midline and 3.6 log points at endline. We do not read much into this result, because it is imprecise and subject to the identification problems associated with estimating production functions using revenue data (De Loecker 2011). To avoid those problems, below we focus on particular productivity components which we can directly measure.[14] Overall we conclude that Table 3 shows large and persistent benefits from the meetings.

Table 4 explores intermediate outcomes that may have contributed to firm growth, as well as some alternative explanations. Columns 1 and 2 show highly significant and persistent treatment effects on the number of clients and suppliers, ranging between 8 and 12 log points. Column 3 shows that firms in the meetings treatment were significantly more likely to take out loans following the intervention. For simplicity here we group formal and informal loans into one indicator, but separately estimating treatment effects shows significant gains for both of them. These results can be interpreted in two ways. One possibility is that the meetings helped firms connect with more business partners and raise more capital, which then contributed to firm growth. An alternative is that the meetings generated growth through other mechanisms, which then translated into higher demand for business partners and for capital. In Section 4 we show direct evidence that improved partnering was one benefit of the meetings.

Column 4 shows the treatment effect on innovation, defined as an indicator for whether the firm

[14] Also note that a 3-4 log point productivity gain could generate the observed growth in sales and factors under a demand elasticity of 3 which is well within the ballpark of standard estimates (Hsieh and Klenow 2009).

Table 4: Intermediate Outcomes and Alternative Explanations

Dependent var.:	log Number of Clients	log Number of Suppliers	Bank Loan	Innovation	log Reported - log Book Sales	Tax/Sales
	(1)	(2)	(3)	(4)	(5)	(6)
Midline	0.015	0.027	-0.04***		0.0003	0.0006
	(0.02)	(0.021)	(0.011)		(0.007)	(0.001)
Endline	0.044	0.049*	0.0077		-0.007	0.0019
	(0.029)	(0.029)	(0.014)		(0.006)	(0.001)
Meetings*Midline	0.09***	0.085***	0.091***		0.003	0.0007
	(0.03)	(0.031)	(0.016)		(0.012)	(0.0015)
Meetings*Endline	0.118**	0.09**	0.079***	0.057***	0.0006	-0.002
	(0.046)	(0.041)	(0.019)	(0.020)	(0.009)	(0.002)
Observations	7,841	7,826	7,857	2646	7777	7,849
Firm FE	Yes	Yes	Yes	No	Yes	Yes
R-squared	0.011	0.009	0.014	0.070	0.002	0.001

Note: Standard errors clustered at the meeting group level for treated firms and at the firm level for control firms. *** p<0.01, ** p<0.05, * p<0.1.

introduced new products or services in that fiscal year. Because we asked about innovation only in the endline survey, here we estimate a regression without firm fixed effects

$$y_i = const + \beta_4 \cdot Meetings_{it} \times Endline_{it} + Firm\ controls + \varepsilon_i. \qquad (2)$$

Note, because this regression only uses data from the endline survey, replacing the interaction with the uninteracted $Meetings$ variable would yield the same coefficient β_4. We report it this way purely to make the table easier to read. Because the treatment is randomized, β_4 is identified even in the absence of firms fixed effects; here from comparing the level (not the growth rate) of innovation between the treatment and the control group. The firm controls include indicators for the firm's region, size category (above or below the median employment in the region), sector (manufacturing or services), and all their interactions. Because the purpose of innovation is to increase output given inputs, the significant positive estimate of 5.7 percentage points can be interpreted as evidence for a productivity gain due to the meetings.

Columns 5 and 6 focus on particular alternative explanations. Column 5 reports the treatment effect on the difference between the log of self-reported sales and the log of the book value of sales (which our enumerators took directly from the firm's book). There is no treatment effect on this

Table 5: Effect of Meetings on Firm Management

Dependent var.:	Management Score (Standardized)					
	Overall	Evaluation	Target	Incentive	Operation	Delegation
	(1)	(2)	(3)	(4)	(5)	(6)
Meetings*Midline	0.211***	0.094**	0.034	0.237***	0.159***	0.071*
	(0.051)	(0.046)	(0.043)	(0.047)	(0.05)	(0.041)
Meetings*Endline	0.215***	0.096**	0.021	0.223***	0.179***	0.07
	(0.048)	(0.045)	(0.046)	(0.047)	(0.044)	(0.043)
Observations	5,211	5,211	5,211	5,211	5,211	5,211
Mid/Endline*Firm Demographics	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.011	0.002	0.000	0.013	0.007	0.001

Note: Standard errors clustered at the meeting group level for treated firms and at the firm level for control firms. Column (1) reports the impact of the treatment on the overall management z-score. Columns (2)-(6) report the impact on five components of management: evaluation and communication of employee performance, targets and responsibilities, attracting and incentivizing talent, process documentation and development, and delegation. All regressions include the interaction between indicators for the midline/endline survey and firm demographics (firm size, sector, region, and their interactions). *** p<0.01, ** p<0.05, * p<0.1.

difference, suggesting that experimenter demand effects are unlikely to drive the main results. And column 6 shows that the tax-to-sales ratio of both treatment and control firms was essentially unchanged after the intervention. Thus improvement in tax avoidance is unlikely to have been the channel of the treatment effect.

Finally we turn to the effect of the treatment on management practices. Following Bloom and Van Reenen (2007) we aggregate the responses to management questions into a single index by standardizing, averaging and again standardizing them. Because only the follow-up surveys contain data on management, we estimate an analogous specification to the one we used for innovation, which does not include firm fixed effects (but is still causally identified):

$$y_i = const + \beta_2 \cdot Endline_{it} + \beta_3 \cdot Meetings_{it} \times Midline_{it} + \beta_4 \cdot Meetings_{it} \times Endline_{it} + Firm\ controls + \varepsilon_i. \tag{3}$$

Table 5 reports the results. In column 1, we estimate highly significant treatment effects of 0.21 at both midline and endline, measured in units of the cross-sectional standard deviation of the overall management score. In columns 2-6 we look at the treatment effect on different areas of management. We find that the intervention improved four of the five areas of management we surveyed, the only exception being the transparency of the firm's targets to its employees. Overall, we conclude that the meetings treatment had a large, persistent and highly significant positive effect on management practices. Given the argument in Bloom and Van Reenen (2007) and Bloom et al. (2013a) that management is a component of total factor productivity, this evidence too can be interpreted as a productivity gain from the meetings.

In sum, the results in Tables 3, 4 and 5 show that the meetings treatment substantially improved firm performance on several margins. The results on innovation and management suggest genuine productivity gains. And the effects on intermediate outcomes, taken together, suggest three mechanisms at play: learning from peers which may improved management; improved partnering which may have increased the number of suppliers and clients; and improved access to finance.

3.2 Group composition and peer effects

We turn to estimate peer effects: whether being grouped with better peers at baseline improves a firm's performance. We view this analysis as an internal consistency test, because any mechanism we can imagine that represents genuine network-based gains predicts that the quality of peers should matter. Motivated by models such as Melitz (2003) in which productivity determines firm size, in our basic specification we measure peer quality with peer size (employment) at baseline. Using only the sample of firms in the meetings groups, our starting point is the following specification:

$$\begin{aligned} y_{it} = const &+ \delta_1 \cdot Midline_{it} + \delta_2 \cdot Endline_{it} \\ &+ \delta_3 \cdot Midline_{it} \times log\ Peer\ size_{it} + \delta_4 \cdot Endline_{it} \times log\ Peer\ size_{it} \\ &+ Controls + Firm\ f.\ e. + \varepsilon_{it}. \end{aligned} \quad (4)$$

Here *log Peer size*$_{it}$ is the average of log employment of the other firms in the meeting group of firm i at baseline, i.e., in the year before the intervention. The controls include the interactions of *Midline*$_{it}$ and *Endline*$_{it}$ with a set of firm demographics: indicators for region, sector categories at baseline (manufacturing or services), size categories at baseline (above or below the regional median employment), and all their interactions. As conditional on these firm demographics the

Table 6: Effect of Peer Firm Size on Performance

Dependent var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Productivity	log Number of Clients	log Number of Suppliers	Management
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Midline*log Peer Size	0.118***	36.66***	0.037	0.024**	0.044*	-0.03	0.21***
	(0.032)	(11.47)	(0.023)	(0.009)	(0.024)	(0.03)	(0.035)
Endline*log Peer Size	0.166***	52.25***	0.015	0.03**	0.108***	-0.035	0.111***
	(0.046)	(17.27)	(0.038)	(0.014)	(0.034)	(0.037)	(0.035)
Observations	4,183	4,076	4,183	4179	4,173	4,170	2,774
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Mid/Endline*Firm Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.040	0.040	0.070	0.04	0.068	0.051	0.159

Note: Table only uses data for treated firms. Columns (1)-(6) are based on all three survey rounds while column (7) is based only on the midline and endline surveys since management questions were not included in the baseline survey. Log peer size is the average of log employment of other group members. Firm demographics include firm size, sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

groups were randomized, by including their interactions with $Midline_{it}$ and $Endline_{it}$ we ensure that δ_3 and δ_4 are identified from the exogenous variation in peer size.

Table 6 reports the results for a representative set of final and intermediate outcomes.[15] Column 1 shows that firms randomized into groups with 10 log points larger peers experienced an additional (significant) sales increase of 1.18 log points by midline and 1.66 log points by endline. That is, roughly, having 10% larger peers increased firm sales by more than 1 percent. Column 2 shows that 10 log points larger peers also increased firm profits by a significant RMB 36,660 (about USD 5,000) by the midline, an effect which persisted to the endline. However log employment did not increase significantly. Column 4 suggests that larger peers also improved the firm's productivity.[16] Columns 5 and 6 show the results for suppliers and clients. Firms with 10 log points larger peers experienced an additional (significant) increase in the number of clients of 0.43 log points by the midline and 1.08 log points by the endline. However, the point estimate for the number of suppliers is negative, though small and insignificant.

We next turn to peer effects on management practices. Because the management data is only

[15] In Appendix Table A1 we present the results for all the other outcomes that were included in Tables 3 and 4. Patterns are similar to the estimates reported in Table 6.

[16] As in Table 3, log productivity is obtained as the residual using production function coefficients estimated from a regression in the sample of control firms.

available in the midline survey, here we estimate

$$y_i = const + \delta_3 \cdot Midline_{it} \times log\ Peer\ size_i + \delta_4 \cdot Endline_{it} \times log\ Peer\ size_i + controls + \varepsilon_i. \quad (5)$$

Column 7 in Table 6 reports the results. The coefficient estimate of the midline coefficient (0.21) shows that 10 log points larger peers resulted in a significant 0.02 standard deviations increase in management practices. The effect shrinks by half but remains positive and significant one year after the end of the meetings intervention.

A possible concern with these peer effect regressions is the exclusion bias investigated by Caeyers and Fafchamps (2016), according to which random assignment into groups mechanically creates a slight negative correlation between the ex ante characteristics of individuals and their peers. To address this concern, we re-estimated our peer effect specifications for the control firms—which did not participate in meetings—using artificial groups created by the same procedure that we had used to create groups in the treatment. Because meetings were not held by these groups of control firms we expect no peer effects; but because they were assigned using the same procedure, any exclusion bias would still affect them. Reassuringly the estimates, shown in Appendix Table A2, are insignificant and small in all specifications, indicating that exclusion bias is not a major factor in our specifications.

Why exactly does peer size matter? One possibility is that bigger firms have better management practices, which then diffuse to peers in the meetings. To explore this effect, we need to measure peer quality with management practices. Since we had not included management questions in the baseline survey, we first construct a predicted measure of managerial practices for all firms at baseline. We do this by regressing, in the midline and endline control sample, the management score on log sales, log employment, manager education, age, gender, sector, and region indicators. Using the coefficients we compute a predicted management score for all firms at baseline. Then we estimate peer effect regressions using peers' (predicted) management score as a measure of quality. Table 7 shows that firms randomized into groups with peers that had higher (predicted) managerial skills experienced increases in their sales, profit, productivity, number of clients, and the management score. Although the (predicted) management score may be correlated with several factors, these results are consistent with peers' management skill being one driver of firm growth. A possible

Table 7: Effect of Peer Firm Predicted Management on Performance

Dependent var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Productivity	log Number of Clients	log Number of Suppliers	Management
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Midline*Peer Management	0.147***	51.62***	0.03	0.065***	0.08**	0.0008	0.387***
	(0.048)	(17.66)	(0.035)	(0.024)	(0.036)	(0.0404)	(0.048)
Endline*Peer Management	0.206***	61.47**	0.041	0.073**	0.164***	-0.001	0.144***
	(0.07)	(26.23)	(0.053)	(0.1)	(0.05)	(0.047)	(0.048)
Observations	4,143	4,037	4,143	4139	4,135	4,132	2,748
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Mid/Endline*Firm Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.037	0.039	0.069	0.035	0.070	0.052	0.163

Note: Table only uses data for treated firms. Columns (1)-(6) are based on all three survey rounds while column (7) is based only on the midline and endline surveys since management questions were not included in the baseline survey. Peer management is the average of the predicted baseline management scores of other group members. Firm demographics include firm size, sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

explanation is learning from peers with high management scores. In Section 4 below we present more direct evidence on the learning channel, and also connect it more tightly to improvements in management.

Taken together, our findings show that peers' identity matters: randomly assigned better peers generate faster firm growth in several domains. Beyond providing internal validity to our previous estimates, these results also contribute to the large literature on peer effects by establishing such effects in a new domain, managerial interactions, and showing that they influence several firm-level outcomes.[17]

3.3 Discussion

Our estimates imply that the meetings had a large effect on firm performance. Here we discuss and rule out some potential confounds.

Experimenter demand effects. A natural concern is that managers who participated in the meetings felt that they were expected to perform well, and as a result over-reported their performance

[17] The work on peer effects includes studies about education (Sacerdote 2001, Carrell, Sacerdote and West 2013), worker productivity (Mas and Moretti 2009, Bandiera, Barankay and Rasul 2010), loan repayment (Breza 2016), program participation (Dahl, Lken and Mogstad 2014), as well as neighborhood effects (Chetty, Hendren and Katz 2016), among others. See Jackson (2011) for a review.

in the midline and endline surveys. Several facts suggest that demand effects are unlikely to drive our results. (i) Table 4 shows that the difference between the self-reported and the book value of sales does not vary with the treatment. It is unlikely that managers would have manipulated the sales number in the book—shown to us, without advance notice that we would ask for it, by the firm's accountant—because of experimenter demand effects. (ii) Table 3 shows significant treatment effects on utility costs, which are not an obvious performance measure and as a result are less likely to be manipulated. (iii) Demand effects are unlikely to have driven the results on peer effects which are identified from variation *within* the meetings treatment. Those results constitute strong evidence that the meetings had direct economic impact. (iv) Essentially all treatment effects persisted one year after the meetings had concluded, while experimenter demand effects should weaken over time.

Side-effects of the meetings. Another concern is that the meetings improved firm growth not because of interactions between managers, but because of a "side-effect". One such side-effect is that firms in the meetings may have had better access to the government through CIIT. Because—except for the first meeting—managers met without interference from CIIT or us, there is no obvious forum for regular access to CIIT officials. And since CIIT staff members introduced us to both the treatment and the control firms, it is not clear that treatment firms had better government access than control firms. Thus the circumstances of the design make this effect unlikely. Improved government access also cannot easily explain the positive peer effects: larger peer firms might have actually crowded out the manager from accessing government officials. We also estimated peer effect specifications testing whether firms improved more if a larger share of their peers had prior government working experience, and found no effect (not reported). Finally, it is not fully clear how access to the government would generate gains in management and innovation.

A second possible side-effect is that firms in the meetings could use either the government certificate or the fact of the meetings to signal their quality. This logic cannot work with the formal certificate because it was also given to control firms.[18] And it also cannot explain the positive peer effects.

[18] In the survey we also asked for managers' willingness to pay for the certificate and found no significant difference between treatment and control in any of the waves.

Collusion and business stealing. A third issue is collusion: perhaps firms in the meetings improved performance not because of performance gains but by coordinating price increases. But these firms were small actors in a large market. Also, as emphasized by Duso, Roller and Seldeslachts (2014), standard models of collusion predict that the increase in profit is accompanied by a reduction in quantity, contradicting the positive treatment effect on factors and inputs. And collusion cannot easily explain other gains, such as improved management.

A variant of this concern is that the impacts are the result of business shifting—treatment firms trading with each other at the expense of outsiders—but do not represent aggregate gains. For this argument to work, there has to be a benefit for the firms that shift their business. If this is an economic benefit, then business shifting is just the process of better firms gaining market share through the logic of creative destruction, and should represent aggregate gains. An alternative potential benefit, emphasized by Haselmann et al. (2016), is rent extraction. But in our context it is difficult to see what rents owner-managed firms could obtain by doing business with each other. Indeed, most of the crony lending documented by Haselmann et al. (2016) is driven by state-owned banks, which lack a direct profit motive. And again, this argument does not explain the gains in management or innovation. Overall, we think that in our context any business shifting is most likely due to the reallocation of factors and inputs to more productive firms and hence does represent aggregate gains.

Outliers. A final concern is that some results may be driven by a few large firms, and the impact on the average firm is small. But Figure 1 shows that firms across a range of sizes were impacted. Moreover, we directly address this concern by winsorizing our main regressions at 1%. As the winsorized and the non-winsorized regressions yield similar results, we conclude that obvious outliers are not driving our findings.

Based on this discussion we believe that the most plausible alternative explanations are unlikely to drive our results, and we conclude that the meetings treatment indeed significantly improved firm performance.

4 Mechanisms

In this section we provide direct evidence on the mechanisms underlying the impact of the meetings. We use the additional interventions to cleanly document that learning and partnering were active channels. We then use the meeting logs for suggestive evidence that more fully fleshes out these channels and also indicates that access to finance was a third channel.

4.1 Learning

We show that the meetings facilitated the diffusion of business relevant information using the intervention in which we distributed information about two financial products (independently) to randomly chosen managers. The first product was a firm funding opportunity in the form of a government grant. Because it could be used to improve a competitor's business, we expected that managers would view this product to be rival. The second product was a private savings opportunity: a high yield investment. We expected that managers would view this product to be less rival. We randomized the information about the two products independently, and provided it to the same share of treatment and control firms.

Empirical strategy. We use two main regressions. First, using the full sample of treatment and control firms in the midline, we estimate, separately for each financial product:

$$Applied_i = const + \gamma_1 \cdot Info_i + \gamma_2 \cdot (1 - Info_i) \times Meetings_i + \gamma_3 \cdot Info_t \times Meetings_i + \varepsilon_i. \quad (6)$$

Here the dependent variable is an indicator for whether the manager reported in the midline survey to have applied for the product. The coefficient γ_1 measures whether the information treatment "worked" in increasing the likelihood of application. The coefficient γ_2 measures whether uninformed managers in the meetings treatment were more likely to apply than uninformed managers in the control. A positive γ_2 may indicate information diffusion from peers, some of whom were exogenously informed about the product. But it could also indicate higher demand for funding due to the growth effect of the meetings. And γ_3 measures whether the effect of information on applications was higher in the meetings treatment: whether the meetings complemented the effect of information, perhaps through encouragement from peers.

To get a more precise measure of diffusion, our second regression uses only the sample of *uninformed* managers in the meetings treatment in the year after the intervention:

$$Applied_i = const + \gamma_4 \cdot Groupmember\ informed_i + \gamma_5 \cdot Competition_i + \\ \gamma_6 \cdot Groupmember\ informed_t \times Competition_i + controls + \varepsilon_i. \quad (7)$$

Here $Groupmember\ informed_i$ is an indicator of i having at least one peer in her/his group who had received the information about the product. Given that the information treatment is randomized, γ_4 measures the *causal* effect of having an informed peer on the decision to apply. $Competition_i$ is an indicator for a higher-than-median level of product market competition in the group of i. We define this variable by first computing the average number of (self-reported) in-group competitors of firms in a group; and then splitting the set of groups by the median of this value. Thus γ_5 measures whether average application rates are lower in more competitive groups, and γ_6 the extent to which diffusion is weaker in more competitive groups. Finally, the controls are indicators for region, sector categories at baseline (manufacturing or services), size categories at baseline (above or below the regional median employment), and their interactions. Because the randomization into groups was conditioned on these variables, by including them we isolate the variation in $Competition_i$ which is driven by the random variation in group composition.

Results. Table 8 presents results about the diffusion of the firm funding opportunity. The first two columns show the estimates from regression (6). Column 1, which only includes $Info_i$, shows that being informed increased the likelihood of application by a highly significant 30 percentage points. This confirms that the information treatment worked. Column 2 also includes the interactions with the meetings treatment. Among uninformed managers, being in the meetings treatment increased application rates by a highly significant 20 percentage points. This can come either from information diffusion or from increased demand for funding because of firm growth. More surprisingly, among *informed* managers the meetings treatment also increased the probability of application by a significant 7 percentage points, showing that in our context formal funding and business networks complemented each other. We view this result as an example of a positive interaction between formal and informal institutions (Fafchamps 2016).

Table 8: Diffusion of Information about Funding Opportunity for the Firm

Dependent var.:	Applied for the Firm Funding Product				
	(1)	(2)	(3)	(4)	(5)
Sample:	*All Firms*		*Uninformed Firms in Meetings*		
Info	0.300***	0.370***			
	(0.0208)	(0.0227)			
No Info * Meetings		0.202***			
		(0.0247)			
Info * Meetings		0.0721**			
		(0.0323)			
Having Informed Group Members			0.315***		0.402***
			(0.0340)		(0.0470)
Competition				-0.155***	-0.0715**
				(0.0497)	(0.0344)
Having Informed Group Members					-0.173***
*Competition					(0.0605)
Firm Demographics	No	No	Yes	Yes	Yes
Observations	2,646	2,646	846	846	846

Note: Table uses data from the midline survey. Competition is one for groups in which the average number of competitors (reported by firms) is higher than the median across groups, and is zero otherwise. Firm demogrphics are indicators for firm size (above median employment in region at baseline), sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The remaining columns of the table report estimates of regression (7). The significant coefficient of 0.315 in column 3 shows that having at least one informed group member increased the probability of application by 31.5 percentage points. This is direct causal evidence that the meetings diffused information, i.e., the learning channel. Column 4 suggests that on average competition reduced application rates. And in column 5 the significant and negative interaction effect of -0.173, suggests that competition reduced the strength of information diffusion about the firm funding product. Intuitively, managers may have been less willing to share rival information with their competitors. Overall, these results show that the meetings channeled business relevant information, and also suggest that diffusion was mediated by the extent of competition.[19]

[19] The fact that we find positive diffusion even in the competitive groups ($0.40-0.17 = 0.23 > 0$) suggests—similarly to the model of Stein (2008)—that the benefits of sharing knowledge exceeded the cost of helping competitors in our context.

Table 9: Diffusion of Information about Saving Opportunity for the Manager

Dependent var.:	Applied for the Private Saving Product				
	(1)	(2)	(3)	(4)	(5)
Sample:	*All Firms*		*Uninformed Firms in Meetings*		
Info	0.398***	0.542***			
	(0.0182)	(0.0232)			
No Info * Meetings		0.276***			
		(0.0276)			
Info * Meetings		0.00697			
		(0.0217)			
Having Informed Group Members			0.328***		0.311***
			(0.0310)		(0.0462)
Competition				-0.00781	-0.0224
				(0.0416)	(0.0380)
Having Informed Group Members					0.0456
*Competition					(0.0615)
Firm Demographics	No	No	Yes	Yes	Yes
Observations	2,646	2,646	835	835	835

Note: Table uses data from the midline survey. Competition is one for groups in which the average number of competitors (reported by firms) is higher than the median across groups, and is zero otherwise. Firm demogrphics are indicators for firm size (above median employment in region at baseline), sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In Table 9 we explore the diffusion of information about the private savings opportunity. The structure is identical to that of the previous table. Column 1 shows that the information treatment was very effective for this product as well, while column 2 shows that there was no complementarity between networks and a personal financial product. Column 3 presents direct evidence for information diffusion, while columns 4 and 5 suggest that competition did not affect the strength of diffusion. Consistent with our prior expectation that it is less rival, the estimates suggest that competition did not influence the diffusion of information about this product. The fact that competitive groups had lower diffusion only for the rival product supports the interpretation that it was driven by the unwillingness to share rival information with competitors, not some correlated omitted factor which generally reduced information diffusion.

Taken together, the results provide direct evidence on the learning-from-peers channel. Beyond highlighting a concrete mechanism in our setting, these finding also inform a literature studying information diffusion in social networks.[20] Our contribution to this work is to demonstrate that competition may limit the diffusion of rival information. In combination with Hardy and McCasland (2016) who show in independent work that the diffusion of a new weaving technique in Ghana was lower in treatments with higher experimentally induced competition, these results highlight a novel friction in technology diffusion: the endogenous (dis)incentive to transmit information. This friction may generate a new, as yet unexplored interaction between technology spillovers and product market rivalry (Bloom, Schankerman and Van Reenen 2013b).

4.2 Partnering

We use the cross-group intervention to document evidence for the partnering mechanism. Our approach is to compare the number of new connections—referrals and business partnerships—in the regular groups and in the cross-groups. Because the cross-groups are randomized from the same pool, unless regular meetings reduced the cost of connecting there is no reason for firms to establish more connections in their regular group than in their cross-group.

Table 10 uses the sample of managers in the cross-groups and reports the average number of new connections by group type. Panel A shows the number of referrers—managers who referred suppliers, clients, partners, and lower-ranking managers. On average each manager had 2.13 more peers act as referrers in the regular group than in the cross-group, and this difference is highly significant. Panel B reports the number of direct business partners: suppliers, clients, and firms engaging in other joint business activities such as joint projects. The average manager had a significant 1.15 more direct business partners from the regular group than from the cross group. And Panel C reports average giving in hypothetical trust games played with a randomly chosen member of the regular group and of the cross-group. Managers exhibited significantly more trusting behavior towards their peers in the regular group.[21]

[20] Much of this work has explored the diffusion of technology (Bandiera and Rasul 2006, Conley and Udry 2010), and financial choices (Duflo and Saez 2003, Hong, Kubik and Stein 2004, Banerjee et al. 2013, Cai et al. 2015) in the social networks of individuals. More recent work on the diffusion of business choices in managerial networks includes Cohen, Frazzini and Malloy (2008) who study the diffusion of financial information and Fafchamps and Quinn (2016) who study the diffusion of certain management practices.

[21] We used the following trust question. "Suppose that you are given 100,000 RMB. Out of this, you can choose to give as much as you want for a business project which is controlled by person X. This project is very successful

Table 10: Repeated Interactions and New Partnerships

Panel A	Number of Referrers		Difference
	In Regular Group	In Cross Group	
Mean	2.18	0.06	2.13***
Standard Deviation	(0.083)	(0.62)	(0.079)
Panel B	Number of Direct Partners		Difference
	In Regular Group	In Cross Group	
Mean	1.44	0.29	1.15***
Standard Deviation	(1.49)	(1.52)	(0.07)
Panel C	Choice in Trust game		Difference
	In Regular Group	In Cross Group	
Mean	3.52	0.94	2.58***
Standard Deviation	(0.13)	(0.12)	(0.12)

Note: Referrer is a group member who referred a supplier, client, partner, lower-ranked manager, or worker to a firm. Direct partner is a group member who is doing business with a firm. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

These results imply that the meetings reduced the cost of establishing new partnerships, so that partnering is indeed one of the channels through which they improved firm performance. The result on trust game behavior suggests that these lower partnering costs may have emerged in part because repeated meetings created trust between managers. We conclude that lack of trust is likely to be an important barrier to creating business partnerships in our context.

These results contribute to a literature that studies network-based referrals in the labor market by documenting referrals in a new domain: managers referring business partners.[22] And our result on trust relates to the research about trust in networks. Karlan et al. (2009) show theoretically that networks which embed more trust are more useful for making high-value referrals, while Feigenberg et al. (2013) establish that regular meetings between microfinance borrowers build trust and improve loan performance. Our findings are consistent with these results and highlight the importance of trust in firm-to-firm interactions.

Taken together, our results on learning and partnering suggest that the meetings created some

and triples the money you give. All the proceeds go to person X. Person X can then choose to return to you as much of the money the project earns as he wishes. How much (between 0 and 100K RMB) do you give to person X?"

[22] Calvo-Armengol and Jackson (2004) is a model of network-based job referrals while Ioannides and Loury (2004) document evidence on their role in the labor market.

of the benefits which are commonly associated with business clusters (Porter 1998), but without the firms actually moving near each other.[23]

4.3 Evidence from meeting logs

We use the content of the meeting logs to document suggestive evidence on how conversations mediated the channels of improvement. Our empirical approach is to regress improvements in a particular domain on indicators for the group having discussed various domains. We focus on three domains: management, partnering, and access-to-finance.

Using the meeting logs we create indicators for whether a group discussed a given domain (e.g., management) at least twice. We use a threshold of two because almost all groups discussed each domain at least once. We measure outcomes in the three domains of interest with the management score, the log of the sum of suppliers and clients, and an indicator for having a (bank or private) loan. For the latter two outcomes, which are available in all survey waves, our specification is

$$Outcome\ in\ h\ domain_{it} = const + \sum_{j=1}^{3} \xi_j \cdot Discussed\ j\ domain_i \times Post_t + \kappa \cdot Post_t + Firm\ f.e. + \varepsilon_{it} \quad (8)$$

which we estimate for the sample of treatment firms using all waves of data. The variable *Post* is an indicator for the midline and endline surveys. The coefficient ξ_h measures the "same-domain" effect: whether the *growth* in outcome h was higher in groups which discussed domain h, relative to groups which did not. And the coefficients ξ_j when $j \neq h$ measure "cross-domain" effects: whether the growth in outcome h was higher in groups that discussed domain $j \neq h$ relative to groups that did not. We expect $\xi_h > \xi_j$.

Because the management data is only available in the midline and endline surveys, when management is the outcome we instead estimate

$$Management\ score_{it} = const + \sum_{j=1}^{3} \xi_j \cdot Discussed\ j\ domain_i + Controls + \varepsilon_{it} \quad (9)$$

for the sample of treatment firms using the midline and endline waves. With the convention that domain 1 is management, ξ_1 measures whether the management score was higher in groups which

[23] Recent work on production and entrepreneurial clusters includes Guiso and Schivardi (2007), Martin, Mayer and Mayneris (2011), Martin, Mayer and Mayneris (2013) and Guiso, Pistaferri and Schivardi (2015).

Table 11: Discussion of and Improvement in Various Domains

Dependent var.:	Management Score (Standardized)	log Num of Business Partners	Loan
	(1)	(2)	(3)
Discussion of Management	0.260***		
	(0.060)		
Discussion of Business Partners	0.033		
	(0.051)		
Discussion of Credit	0.004		
	(0.051)		
Discussion of Management*Post		0.066	0.036
		(0.051)	(0.031)
Discussion of Business Partners*Post		0.065	-0.035
		(0.048)	(0.027)
Discussion of Credit*Post		-0.014	0.071**
		(0.038)	(0.028)
Post		-0.018	0.113
		(0.108)	(0.069)
Observations	2,774	4,167	4,183
Firm FE	No	Yes	Yes
Firm Demographics	Yes	No	No
R-squared	0.117	0.065	0.063

Note: Table uses data on treated firms. Column (1) is based only on the midline and endline surveys since management questions were not included in the baseline survey. Columns (2) and (3) are based on all the three survey waves. Discussion of management, discussion of business partners, and discussion of credit are indicators for whether a group discussed the respective topic at least twice. Firm demographics include firm size, sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

discussed it relative to groups which did not, while ξ_2 and ξ_3 measure whether management score was higher in groups which discussed partnering, respectively access to finance, relative to groups which did not. Here too we expect $\xi_1 > \xi_2, \xi_3$.

Table 11 shows the results. Column 1 shows that for management the patterns strongly align with our hypothesis: the coefficient of discussing management is large and significant while those of discussing the other domains are small and insignificant. Column 2 shows that for business partners all coefficients are insignificant, though those of discussing management and partners are larger than that of discussing finance. And column 3 shows that for access-to-finance the results again align with our hypothesis: the same-domain coefficient is large and significant while the cross-domain coefficients are small and insignificant.

The natural interpretation of columns 1 and 3 is that discussion of a domain generated improvements in that domain. The omitted variable concern that good managers talk about multiple domains and also improve in multiple domains is addressed by the small and insignificant cross-domain effects. The patterns for partnering are less clear. The similar-sized coefficients on discussing partnering and management may reflect that both topics helped firms get more partners, but other interpretations are also possible: for example, that for new partnerships trust is an additional input which is not included in this specification.

On the whole we interpret these results as suggestive evidence that conversations mediated the improvements in various domains. This interpretation suggests two new lessons. The management result suggests that the learning-from-peers mechanism may have contributed to the observed improvements in management. And the credit result suggests that beyond the learning and partnering mechanisms access to finance was a third mechanism. Further research—also in other areas of economics—about how conversations shape outcomes, and about the determinants of conversation topics, seems promising to us.

5 Conclusion

In this paper we used a field experiment with experimental business associations to measure the effect of business networks on firm performance. We found significant, robust, large, and persistent effects of the meetings on sales, profits, factors, and inputs, as well as on innovation and management. We also found direct evidence on two mechanisms, learning and partnering, and suggestive evidence on a third, access to finance. We now turn to discuss some implications of these results.

We begin with a cost benefit analysis. Combining publicly available survey and wage growth data we estimate the annual wages of managers in our sample to be RMB 812,300.[24] This value

[24] A survey conducted by the All-China Federation of Industry and Commerce shows the average earning of private business owners to be about RMB 200,000 in 2005. We multiplied this value with wage growth in the private sector between 2005 and 2014 (a factor of 4.06 by the Chinese National Bureau of Statistics) to obtain our estimate. A summary report of the survey is available at http://www.people.com.cn/GB/jingji/42775/3164559.html.

also accords with the range locals reported to us informally. Assuming that managers work 200 days a year and that each meeting takes a full day, we estimate the cost of the meetings to be about RMB 50,000 for our average manager. As Table 3 shows, the average annual profit gain by the midline survey was about RMB 250,000, or about *five times* the estimated wage cost. While there is uncertainty about the precise value of this estimate, it strongly suggests that the meetings were highly cost affective.

Given this result, a natural question is why the managers did not organize meetings for themselves. There are several possible answers. First, there could be significant search costs and trust barriers: managers would need to find others who are willing to form groups with unfamiliar people. Second, there may be a public good problem if the cost of doing this falls on a single organizer. Third, paralleling the argument of Bloom et al. (2013a), managers may have underestimated the gains from business associations or from changing business practices.

We next compare our results to the impacts found in other types of interventions. McKenzie and Woodruff (2014) review several studies evaluating business training and business consulting interventions. For business training they conclude that—perhaps because of limited power—most studies do not find a significant impact on sales or profits (see Table 9 in their paper).[25] In contrast, the high-intensity management consulting intervention evaluated by Bloom et al. (2013a) did create a large productivity increase of 17%. Our 8 percent sales effect is much smaller than this; but our intervention is cheaper, easier to implement, and highly cost effective. Finally, Brooks et al. (2016) show that a one-month business mentoring intervention for Kenyan microenterprises led to a 20% profit effect, which faded during the year after the intervention. The mechanisms they emphasize are similar to the ones we document, but our effects persisted after one year. We conclude that our business meetings intervention had surprisingly large effects in comparison to other interventions that have been evaluated.

Which aspects of the design made the intervention successful? The comparison with the designs of other studies, and the results on mechanisms, highlight regularities that allow us to formulate

[25] Exceptions include Calderon, Cunha and de Giorgi (2013) who find a 20% impact on sales and a 24% impact on profits; and De Mel, McKenzie and Woodruff (2014) who find a 41% increase in sales and a 43% increase in profits for start-up businesses. But these estimates are also quite noisy. Our sales and profit impacts fall within their standard error bands, are more precisely estimated, and are persistent.

some hypotheses. First, similar to the Bloom et al. (2013a) study, but unlike many business training evaluations, our sample of firms was selected. This fact suggests that firm interventions are more likely to succeed when managers themselves are interested in improving their business; and also that a possible way to identify such “gazelles” (Fafchamps and Woodruff 2017) may be to use an explicit recruitment process.[26] Second, also paralleling the Bloom et al. (2013a) study, our intervention was quite intensive. Managers met every month, and combined company visits with several hours of discussion. This intensity may have contributed in multiple ways. Our findings on the effect of meeting frequency suggest that it helped build trust. And observing other firms' operations in practice may have enhanced learning through a “demonstration effect.” Third, our results on management and information diffusion suggest that managers had gaps of knowledge that learning could fill. This could be because the firms were young and did not have access to other sources of business information.

This discussion suggests that the following conditions on the design increase the probability of a successful business meetings policy. (1) Self-selected pool of firms. (2) Regular intensive meetings. (3) Young firm age. The discussion also suggests that meetings are more likely to help in contexts in which the following distortions are important. (4) Contracting problems which increase the value of trust. (5) Relative lack of alternative sources of business information (e.g., MBA programs). We hope that these conditions can help guide future interventions and scale-ups and thus contribute to private sector development.

[26] In the context of our meetings program, recruiting good firms has not only the direct benefit that they may respond to the treatment, but also the indirect benefit that—through peer effects—they generate higher growth for other participants.

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A Peer effects: other outcomes and placebo with control firms

Table A1: Effect of Peer Firm Size on Performance: Other Outcomes

Dependent var.:	log Total Assets	Material Cost	log Utility Cost	Bank Loan	log Reported - log Book	Tax/Sales	Innovation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Midline*log Peer Size	-0.0298	0.114***	0.0962***	0.008	0.0230	-0.00233*	
	(0.0312)	(0.0423)	(0.0360)	(0.0150)	(0.0234)	(0.0012)	
Endline*log Peer Size	-0.0569	0.169***	0.175***	0.0077	0.0128	-0.0009	0.0685***
	(0.0434)	(0.0529)	(0.0412)	(0.0172)	(0.0118)	(0.0011)	(0.0175)
Observations	4,179	4183	4086	4,183	4140	4178	1409
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Mid/Endline*Firm Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.022	0.031	0.042	0.044	0.026	0.028	0.0868

Note: Table only uses data for treated firms. Columns (1)-(6) are based on all three survey rounds while column (7) is based only on the endline survey since innovation questions were not included in the baseline and midline surveys. Log peer size is the average of log employment of other group members. Firm demographics include firm size, sector, region, and their interactions. Standard errors clustered at the meeting group level in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A2: Placebo Effect of Peer Firm Size in Artificial Groups of Control Firms

Dependent var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Productivity	log Number of Clients	log Number of Suppliers	Management
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Midline*log Peer Size	0.0242	3.403	-0.0270	0.0301	0.00871	-0.0415	-0.0013
	(0.0244)	(4.787)	(0.0230)	(0.0273)	(0.0264)	(0.0302)	(0.0345)
Endline*log Peer Size	0.0345	12.78	-0.0163	0.0324	0.0349	-0.0253	0.0261
	(0.0362)	(12.52)	(0.0311)	(0.0435)	(0.0385)	(0.0393)	(0.0379)
Observations	3,671	3,586	3,671	3,505	3,665	3,653	2435
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Mid/Endline*Firm Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.042	0.047	0.071	0.047	0.053	0.039	0.149

Note: Table only uses data for control firms. Groups are artificial groups which were created by the procedure used in the treatment, but which did not meet. Columns (1)-(6) are based on all three survey rounds while column (7) is based only on the midline and endline surveys since management questions were not included in the baseline survey. Log peer size is the average of log employment of other group members. Firm demographics include firm size, sector, region, and their interactions. Standard errors clustered at the group level in parentheses.*** p<0.01, ** p<0.05, * p<0.1.