

# Prior ties and the limits of peer effects on startup team performance\*

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## Abstract

We conduct a field experiment at an entrepreneurship bootcamp to investigate whether interaction with proximate peers shapes a nascent startup team’s performance. We find that teams whose members lack prior ties to others at the bootcamp experience peer effects that influence the quality of their product prototypes. A one-standard-deviation increase in the performance of proximate teams is related to a two-thirds standard-deviation improvement for a focal team. In contrast, we find that teams whose members have many prior ties interact less frequently with proximate peers, and thus their performance is unaffected by nearby teams. Our findings highlight how prior social connections, which are often a source of knowledge and influence, can limit new interactions and thus the ability of organizations to leverage peer effects to improve the performance of their members.

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# Introduction

Regions from New Delhi, India, to Durham, North Carolina, are attempting to build their own Silicon Valleys—ecosystems propelled by nascent startups developing innovative products (Kenney, 2000; Bresnahan and Gambardella, 2004). Researchers and policymakers have long attributed the success of such ecosystems to the informal networks between entrepreneurs (Saxenian, 1990; Sorenson, 2005) and more recently to the peer learning facilitated by institutions like accelerators and incubators (Hochberg, 2016). Indeed, regions hope that these networks and organizations will spark powerful peer effects that will improve the quality of their startups (Hochberg and Fehder, 2015).

The idea that peer effects drive performance is not new. Such effects have been demonstrated in diverse contexts including retail (Mas and Moretti, 2009; Chan, Li and Pierce, 2014*a,b*), finance (Hwang, Liberti and Sturgess, 2018) and scientific innovation (Azoulay, Graff-Zivin and Wang, 2010; Oettl, 2012; Catalini, 2017). Peers share knowledge, provide help, and set performance expectations (e.g., Mas and Moretti, 2009; Herbst and Mas, 2015; Housman and Minor, 2016). Similarly, much of the value of startup incubators and accelerators is derived from the new social interactions they engineer between nascent entrepreneurs (Hallen, Bingham and Cohen, 2014; Chatterji et al., 2018; Hasan and Koning, 2018). However, when researchers have tried to engineer peer effects in other contexts, they have often failed (Carrell, Sacerdote and West, 2013). Individuals appear to exercise considerable discretion in choosing whom they interact with, preferring to form ties to peers with similar ability (Carrell, Sacerdote and West, 2013), ethnicity (Hjort, 2014) or class (Kato and Shu, 2016). This self-selection of connections *after* peer assignment appears to limit the ability of organizations to design effective social interventions and therefore encourage peer learning.

Research has also suggested that a critical constraint on the formation of new connections is the prior ties of individuals (Granovetter, 1973; Gargiulo and Benassi,

2000; Kim, Oh and Swaminathan, 2006). Within and outside organizations, individuals have ties to existing collaborators, advisers, and friends (Lincoln and Miller, 1979; Krackhardt and Hanson, 1993). While extensive research shows that these ties are rich conduits of information and support (e.g., Podolny and Baron, 1997; Sparrowe et al., 2001), other work suggests that prior ties may reduce how much attention someone devotes to cultivating new interactions with peers—even those who are easily accessible (Kim, Oh and Swaminathan, 2006). Prior connections also entail social commitments and obligations of time and attention (Granovetter, 1973). Given this latter view, we might expect the prior networks and ties of individuals to limit an organization’s ability to engineer peer effects.

We use a field experiment to test if prior ties limit peer effects. We leverage data from a bootcamp for nascent software-product entrepreneurs held in New Delhi, India. During the camp, 112 aspiring entrepreneurs worked in randomly assigned teams that were also randomized to workstations. For a week, teams developed a software prototype for a mobile application. We utilize a team’s randomly assigned location along with naturally occurring variation in the number of prior ties a team’s members have to others at the bootcamp to test our hypothesis. Our empirical approach allows us to estimate cleaner team-level peer effects by accounting for homophily and other sources of endogenous sorting that bias peer effects (Manski, 1993; Hartmann et al., 2008).

We find that teams whose members have few or no prior ties know more of their peers on proximate teams and also seek advice and help from them more frequently. As a consequence, the quality of such teams’ final software prototypes is shaped by the quality of proximate teams. In contrast, the performance of teams whose members have many prior ties to others at the bootcamp is not related to that of their proximate teams. Specifically, we show that the influence of nearby teams declines because members of such teams are unlikely to interact with or seek advice from nearby peers. These findings highlight how the exogenous and endogenous aspects of social interaction at

work—peer effects and prior ties—jointly shape team performance.

One primary contribution of our work is to show that existing social structures fundamentally limit the ability of organizations to design social interventions. Given the recent excitement in both the economics (e.g., Carrell, Sacerdote and West, 2013; Hjort, 2014) and strategy literature (e.g., Catalini, 2017; Puranam, 2018) on the optimal design of social interventions, our findings highlight how salient the constraints of existing social structure are on the ability of organizations to alter the behavior of their members.

Relatedly, our research speaks to the growing literature on the effects of accelerators, incubators, and bootcamps on startup performance (Cohen, 2013; Hallen, Bingham and Cohen, 2014; Hochberg, 2016; Yu, 2018). Many regions are investing in these institutions, expecting them to help forge new connections between entrepreneurs. Our results suggest that these institutions may be ineffective channels for peer learning and advice when members are already embedded in existing networks or have social distinctions that limit their interactions. Thus, accelerators and incubators should ensure that they do not just reinforce existing connections, but also create new ones.

More broadly, our study focuses on peer effects in a novel and economically important context: early-stage software entrepreneurship in India. While India has long been a significant player in software services (Arora et al., 2001; Arora and Athreye, 2002), there is a push to increase the size and quality of India’s software-product industry. We show that, if facilitated well, the connections between nascent entrepreneurs can encourage learning for both startup teams and the ecosystem at large.

# Theory and Hypotheses

## Peer effects at work

Social interactions at work affect individual, team, and firm performance (e.g., Chan, Li and Pierce, 2014*a,b*). Scholars and managers alike believe that workplace peers are an important source of influence in organizations and that this influence can be harnessed to improve performance. An especially important driver of peer effects identified in the literature is spatial proximity to coworkers and peers who may possess diverse knowledge or skills. Substantial literature shows that individuals located near each other are more likely to observe one another, form social ties, and share information (e.g., Allen and Cohen, 1969; Reagans, 2011; Catalini, 2017). Furthermore, research on the link between proximity and interaction has shown the effect of proximate peers exists at various scales—including between individuals in the same room (Boudreau et al., 2017), same buildings (Allen and Cohen, 1969), and the same region (Agrawal, Galasso and Oettl, 2017). While effect sizes do differ across contexts, a general pattern persists: proximate peers, rather than distant ones, are more likely to shape performance.

Researchers have found that a variety of mechanisms drive the observed effect of proximate peers. Increased interaction with proximate peers helps individuals acquire new knowledge, set expectations about appropriate levels of effort, and sparks healthy competition among organizational members. For example, in a well-known study Bandiera, Barankay and Rasul (2010) find that fruit-pickers who work alongside able peers become more productive. These authors argue that having an able peer nearby makes work “enjoyable, generate[s] contagious enthusiasm, [and] generate[s] incentives to compete to be the best in the group.” Chan, Li and Pierce (2014*b*) also find substantial peer effects on the productivity of salespeople at a department store. Peer effects in their setting are driven by employees observing the techniques of their more productive peers, and by these peers directly teaching their techniques to novice

colleagues. In contrast, employees with less-productive peers nearby perform worse because they learn fewer useful techniques.

Scholars have also found peer effects in more complex types of work. Catalini (2017) finds peer effects among co-located French scientists and their laboratories. He finds that co-located labs tend to collaborate more and become similar in their research trajectories. Azoulay, Graff-Zivin and Wang (2010) find evidence that losing a productive peer hurts the performance of scientists, and Oettl (2012) shows that losing a *helpful* and *productive* peer is even worse. In a more recent study, Chatterji et al. (2018) find that management advice from peers helps the founders of startups grow their companies two years after the intervention.

These findings have inspired some scholars in economics and strategy to posit that firms can use peer effects to improve performance. Bandiera, Barankay and Rasul (2010), for instance, state that firms could “exploit social incentives . . . to motivate workers” and Catalini (2017) states that organizations can spark “radically novel opportunities” by “co-locating previously separated teams to encourage serendipitous (and planned) conversations.” This work leads to a natural question: When can organizations engineer peer effects to improve the performance of their members?

### **The limits of peer effects**

Despite the promise of using social interventions to improve performance, recent work suggests that peer effects might not be so easy to engineer. For example, Carrell, Sacerdote and West (2013) attempted to improve the performance of West Point cadets by assigning them to co-located squadrons. They hypothesized that by creating squadrons with a mix of high- and low-ability students, peer learning would improve the performance of lower-ability students but not hurt the high-ability students. Surprisingly, the intervention failed. After the intervention cadets endogenously sorted into homogeneous groups based on their ability, and as a result, the benefits of peer learning

failed to materialize. That is, the self-selection of ties *after* peer assignment limited the effectiveness of their intervention.

A key mechanism underlying the failure to design an effective intervention in Carrell, Sacerdote and West (2013) is the endogenous choices of individuals to interact with or avoid their randomized peers. Indeed, other studies support the idea that the formation of a social tie is a crucial mediator of peer effects. For instance, Mas and Moretti (2009) find that peer effects exist only when individuals interact frequently and can easily observe each others' behaviors. Kato and Shu (2016) also find evidence for peer effects among textile workers in China, but only when proximate peers have similar social origins (i.e., urban or rural) and thus a greater tendency to interact. Finally, Hjort (2014) also finds that ethnic divisions can limit peer effects at work by hindering fruitful coordination. Although the authors of these studies cannot directly observe interaction or the formation of social ties, they conjecture that when peers differ in their backgrounds, they are less likely to interact and learn from each other. Nevertheless, this work suggests that co-location will lead to peer effects, but only when peers interact or form connections.

### **Prior ties and new peers**

Past research suggests that a critical consideration in whether a person forms a new connection is her network of prior ties (Granovetter, 1973; Gargiulo and Benassi, 2000; Kim, Oh and Swaminathan, 2006). Within organizations, individuals have a range of advisers and friends in their social network (Lincoln and Miller, 1979). These ties provide task and strategic advice, as well as social support (Podolny and Baron, 1997). Employees with many existing ties often perform better at work because they can rely on their connections for acquiring new knowledge or help (Castilla, 2005).

However, having many prior ties may also limit how much an individual invests in forming new connections, even with knowledgeable and accessible peers (Kim, Oh and

Swaminathan, 2006). Prior ties may substitute for forming ties to new peers for several reasons. First, prior connections may limit how frequently a person interacts with new coworkers and peers. Connecting with someone new involves many uncertainties that resolve only after repeated interaction (Podolny, 1994). Uncertainty may exist about whether a peer is intelligent, has a good personality, or is trustworthy (Uzzi, 1996). Thus, if a person already has advisers and friends in her network, he may prefer to interact with existing connections over building new ones (Gargiulo and Benassi, 2000; Kim, Oh and Swaminathan, 2006; Dahlander and McFarland, 2013). Second, forming new connections is also socially challenging. Ingram and Morris (2007), for example, found that individuals gravitated toward people they already knew and avoided meeting new people at a social mixer designed to facilitate new connections. Dahlander and McFarland (2013) also find that reconnecting with existing collaborators is easier than finding new coauthors. Finally, existing relationships require commitments of time, effort, and resources. Individuals congregate with members of their network for lunches, coffee, and other informal meetings, which reduces the time they have left over to meet new people.

While these mechanisms suggest that prior ties can limit interaction with new peers, having an extensive network may also indicate that a person is a sociable or friendly (Casciaro and Lobo, 2008; Shi and Moody, 2017). A person with prior ties may have more knowledge, resources, or ability (Podolny and Baron, 1997; Hasan and Bagde, 2015). Thus, if prior connections are primarily capturing these traits, individuals with many existing connections may be those who interact more with their proximate peers.

On balance, although an individual with many prior ties can choose to socialize with her existing friends or colleagues, those with few connections will need to build new relationships. For the latter group, proximate peers are a readily available reservoir of knowledge, social support, and reference points. Thus, we expect those with fewer ties to have a higher propensity to connect with their proximate peers than those with

more.

**Hypothesis 1** *Individuals with prior ties will be less likely to interact with their proximate peers.*

## **Prior ties and team performance**

Considerable literature in management suggests that the external social ties of team members affect not only their own performance but also the outcomes of their team as a whole (e.g., Oh, Chung and Labianca, 2004; Oh, Labianca and Chung, 2006). For instance, work by Oh, Chung and Labianca (2004) and Reagans and McEvily (2003) demonstrate that external social ties provide timely access to new knowledge and insights that individual team members lack. Other related work finds that teams often value this external knowledge more than internally generated ideas, making externally sourced information more influential (Menon and Pfeffer, 2003). Thus, the network connections of individual team members are likely to affect where a team gets outside information, and subsequently how it performs.

A natural source of new information for a team are the members of nearby teams (see, Catalini, 2017). Peers outside of one's team often possess new and relevant knowledge that can help improve performance (Oh, Labianca and Chung, 2006). Proximate peers may know a programming language or have good aesthetic taste, business acumen, or have deep knowledge of a consumer segment. A team member can learn from these interactions and bring this knowledge back into her team to improve performance. Alternatively, peers with weaker skills or knowledge may hurt performance by giving poor or perhaps no advice (Chan, Li and Pierce, 2014*b*). When interaction with members of nearby teams is high, they will increasingly draw on similar knowledge, techniques, and skills. As a result, these interactions will lead the teams' performance to converge.

Another reason to expect the performance of nearby teams to converge is that they

are more likely to see each other as reference points as well as competitors. Prior research shows that peers are referents who anchor expectations about appropriate behaviors and desirable outcomes (Lawrence, 2006). Several studies show that these mechanisms can drive substantial peer effects at work. For example, Cohn et al. (2014) show that social comparison between sales employees affects their effort and performance at work. In a related paper, Kuhnen and Tymula (2012) show that having a high-performing reference group can cause individuals to compete more intensely, and thus increase their performance.

Building on our first hypothesis, in H2 we expect that teams whose members have many prior connections will interact less with nearby peers. A team’s referents, sources of knowledge, and competitors will be those individuals and teams with whom a team’s members already interact. As a result, nearby peers will have less influence on the behavior and outcomes of teams whose members have prior connections.

**Hypothesis 2** *The performance of teams with many prior ties will be less correlated to the performance of proximate teams.*

## Setting and Design

### Setting

We test our hypotheses using data from a startup bootcamp in New Delhi, India. This camp trained 112 aspiring entrepreneurs in idea generation, prototype development, and business model assessment. The program consisted of three week-long modules that combined hands-on practice with instruction from successful Indian entrepreneurs, designers, and venture capitalists. The program required full-time attendance and operated Monday through Saturday.

This article leverages data collected during the first week of the program. During this week, participants were randomly assigned to work in teams. Starting on Tuesday,

teams worked to develop a mobile app prototype. All teams developed a prototype application for the Indian wedding industry to ensure commensurate assessment of performance across the projects.<sup>1</sup> In addition to team randomizations, each team was randomly assigned to a workstation. Each workstation was located within the bootcamp’s large, open office space, as depicted in Figure 1.<sup>2</sup>

[Figure 1 about here.]

On Saturday, which marked the conclusion of the first week, participants completed surveys about their social interactions and also conducted double-blind evaluations of five other teams’ project submissions, including prototypes. These evaluations were done electronically on the bootcamp’s learning management system. Based on the double-blind evaluations, the three highest-rated submissions won prizes totaling 45,000 Indian rupees (INR), that is, about 789.47 USD.<sup>3</sup>

## Participant characteristics

The demographic backgrounds of our participants are similar to bootcamps, incubators, and accelerators in the United States.<sup>4</sup> After the bootcamp, three-quarters of alumni

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<sup>1</sup>We chose the Indian wedding industry for three reasons. First, in conversations with Indian entrepreneurs and venture capitalists, we learned that the Indian wedding industry is large and has significant market potential, with several venture capital firms actively investing in the space. Second, the “Indian wedding” was something that all of the participants had some experience with, but it was an industry in which a subset of individuals was unlikely to have insurmountable knowledge advantages. Third, we chose this industry because, while focused, it is still a relatively diverse space, with problems ranging from finding a spouse to buying wedding dresses to honeymoon selection and even postmarital counseling. This focus helped ensure that the prototypes generated by each team were comparable and could be evaluated against one another.

<sup>2</sup>Additional randomizations were also embedded within the bootcamp during the first and second week of the bootcamp. The third week of the bootcamp was less structured. Please see Appendix Section A1 for further details on the camp and subject recruitment. There are no other papers that rely on the spatial randomization nor the network formation data analyzed in this paper.

<sup>3</sup>The first prize was 20,000 INR, the second was 10,000 INR, and the third was 7,500 INR.

<sup>4</sup>Participant age ranged from 18 to 36, with a mean age of just over 22. The participants all had a college degree or were enrolled in college, 8 had advanced degrees including PhDs in Electrical Engineering, and nearly 70% had degrees in technical fields. Unlike many startup accelerators in the US, the program had a sizable number (25) of female participants.

went on to work in the software and technology industry, with just over 20 percent of alumni founding a startup after graduation. The non-founders were also engaged in the technology ecosystem working for companies like Palantir and Microsoft. Some pursued further education at global institutions such as Georgia Tech and Carnegie Mellon University.

## **Research design**

A difficulty of peer effects research is the empirical challenge of ruling out the effect of self-selection into peer relationships based on unobservable traits of both the focal individual as well as the peer (Manski, 1993). This selection on unobservable variables can potentially introduce an upward bias in peer effects estimates. Our empirical tests overcome this challenge by leveraging two levels of randomization: (1) of participants to teams and (2) of teams to workbenches.

First, by randomizing team assignment, we introduce exogenous variation in how many team members have prior connections to others at the bootcamp. This randomization does not eliminate the possibility that prior connections capture other characteristics of individuals on the team. It does, however, rule out alternative explanations that could result from endogenous team formation. For example, if teams organize based on prior connections, the teams with more connections could also have greater complementarities (e.g., on skills) and thus may need to rely less on peers. The random team assignment reduces the likelihood that this and other team-level explanations drive our results. However, because the selection of prior ties is not randomly assigned, we also account for individual-level heterogeneity in our models by testing whether individual ability and personality explain the effect of prior ties. Furthermore, our randomization of teams to workstations ensures that the distance between any two pairs of teams is unrelated to observed and unobserved differences in their pre-treatment characteristics. These features of our data allow us to test our two key

hypotheses rigorously.

To test Hypothesis 1, we estimate the following model:

$$I_{ij} = \alpha + \beta D_{ij} + \gamma H_i + \delta(D_{ij} \times H_i) + \epsilon_{ij}$$

In this model, our dependent variable  $I_{ij}$  indicates if participant  $i$  interacted with participant  $j$ .  $D_{ij}$  is the randomized distance between  $i$  and  $j$ , and  $H_i$  is a dummy that indicates if  $i$  enters the bootcamp with prior connections or not. If propinquity shapes who interacts with whom, we would expect the coefficient  $\beta > 0$ . If prior connections mitigate the effect of distance, then the magnitude of  $\delta$ , the coefficient on the interaction term, should be  $\approx -\beta$ —i.e., the size of the primary peer effect.

Further, to account for the non-independence of social interactions, we use multi-way clustering at the sender (ego), peer (alter), and sender-receiver (dyad) levels.

To test Hypothesis 2, we estimate a standard peer effects specification where a team  $t$ 's performance  $P_t$  is a function of the average performance of its nearby peers  $N$ .

$$P_t = \alpha + \beta P_{t's\ N\ nearby\ teams} + \gamma H_t + \delta(P_{t's\ N\ nearby\ teams} \times H_t) + \epsilon_t$$

$H_t$  is a measure of the number of prior ties that members of a team have. Using this model, we test for the presence of *any* influence of proximate teams and whether prior ties limit this effect. While recent peer effects research focuses on understanding *what* mechanisms drive peer effects (e.g., advice, peer pressure, or competition), we instead test whether the influence of peers, regardless of channel, is moderated by how many prior ties a team's members have.

Although this specification rules out the effect of homophily and selection, two issues related to the problem of reflection are worth noting. First, one limitation of our model is that it is agnostic to the direction of primary influence, and so we can only estimate the joint effects of peers (Manski, 1993). We cannot determine how much

of the effect is due to team A learning from team B, team B competing harder with team A, or the back-and-forth influence between the two teams. Thus, the joint effect we estimate is likely to be larger than the specific directional effect of one team on another. As a consequence, we limit our interpretation to how prior ties mitigate peer effects in general, but cannot speak conclusively about the direction of influence or the social mechanisms that produce the peer effects between teams.

The second limitation of our model is that because we use contemporaneous performance outcomes, our results might reflect differences in a location’s inherent quality instead of a social mechanism. If some regions of the bootcamp space improve team performance (e.g., are quieter), then proximate teams in that area will perform better, even if the teams do not influence one another. We address this problem of “correlated shocks” in three ways. First, the open floor plan mitigates the variation that exists across teams in terms how much privacy and noise they experience. Second, in Appendix Section A6, we formally test if structural differences in the architecture of the space, over and above distance, affect tie formation. We find no evidence that they do.

Third, we run simulations to check if irregularities in layout lead to a spurious correlation in outcomes between peers. We again find no evidence of such a bias. Specifically, we implement a permutation (randomization) test to account for the non-independence and irregularities inherent in our spatial data (Krackhardt, 1988; Good, 2004; Gerber and Green, 2012). Similar to the bootstrap procedure, permutation tests rely on simulation to estimate an otherwise unknown sampling distribution. Unlike a bootstrap test, permutation tests focus explicitly on testing the experimenter’s (sharp) null hypothesis that assignment status is unrelated to observed outcomes. In our case, we randomly shuffle the performance outcome across all teams in the bootcamp and then use this permuted data to generate a distribution of simulated peer effect coefficients. If the null holds, then outcomes are unrelated to assignment status, and so shuffling outcomes across units—irrespective of their randomized location—should

yield effect sizes similar to the observed effect. If we reject the null, the observed treatment effect should be more extreme than the estimates from the permuted data.

## Data and Variables

Our data come from three sources: (1) A pre-bootcamp survey, (2) measures of the workbench locations taken during the camp, and (3) surveys and evaluations of prototypes submitted at the end of the bootcamp’s first week. We begin by describing variable construction for Hypothesis 1, then Hypothesis 2 separately, since we conduct these analyses at different levels. Hypothesis 1 is tested at the dyad level as it concerns the impact of prior connections on the likelihood that an individual member of a team will interact with peers outside her team, whether near or far. Since our performance measures are at the team level—prototype development happens within a team—we test Hypothesis 2 at this level of analysis. For this test, we aggregate the number of prior connections of individual team members into a team-level measure of prior ties.

## Individual-level variables

### Dependent variables

For the individual analysis, our dependent variables measure whether individual  $i$  has a social interaction with a peer  $j$ . To test H1, we measure three types of social interactions at the bootcamp: (1) if a participant knows, (2) seeks advice from, or (3) digitally communicates with a peer  $j$  outside his or her team.

Prior work highlights the importance of knowing and observation for peer effects driven by social comparison (e.g., Mas and Moretti, 2009). Further, substantial literature in organizational behavior and theory highlights the importance of advice relationships for knowledge spillovers in firms (Lincoln and Miller, 1979; Krackhardt, 1999; Casciaro and Lobo, 2008). Finally, email and social media interaction provide behav-

ioral measures of interaction to complement our survey responses (Aral and Van Alstyne, 2011).

In constructing the variables above, we exclude within-team pairs from our analysis due to the automatic interaction between person  $i$  and  $j$  caused by assignment to the same team.

**Knowing** We measure “knowing” by conducting a roster survey. We provided participants with a list of every other participant and asked them to “select the people you know or know of from the list below.” The roster included the name and a photo of all participants, and respondents could click “yes” on the names of peers they knew.

**Advice** We measured whether a respondent received advice from other participants at the bootcamp. Again, we used a roster method and asked whether a respondent received “feedback and advice about [their] ideas and entrepreneurship” from other participants. This measure captures more intensive interactions between focal participants and peers outside her team.

**Digital Messaging** Finally, we created a behavioral measure using digital communication among participants. For this measure, we captured data from the bootcamp’s Facebook page and participant’s email activity on the bootcamp’s platform. Our digital messaging variable counts the number of emails sent, Facebook likes, and comments from person  $i$  to  $j$ . This measure is meant to provide corroborating behavioral evidence for our *Knowing* and *Advice* measures which are drawn from survey responses.

## Independent variables

**Distance (meters)** We calculate the distance between individuals  $i$  and  $j$  by calculating the shortest walkable route between each workstation in meters. The distance between workstations ranges from just under 1m (3ft) to just over 26m (86ft).<sup>5</sup>

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<sup>5</sup>Allen and Cohen’s (1969) seminal work on micro-geography in R&D labs shows that the effect of distance decays exponentially. Many studies use the log of the distance variable to account for nonlinear distance effects. In our tables, we present the raw distance measure to ease the interpretation, but our results hold when we use the log of distance as well.

Overall, the bootcamp space was free of obstacles that impeded interaction except for a 5-foot boundary wall (see Figure 1). To account for this obstacle, we include this barrier in our distance calculations, by assuming that participants walk around the wall to interact with teams on the other side.<sup>6</sup>

**Has prior ties** Before the bootcamp, all participants completed a roster-based network survey with questions similar to those asked during the bootcamp. We use participant’s responses to the “knowing” survey (e.g., “select the people you know or know of from the list below”) to construct our prior connections measure.<sup>7</sup>

Using the responses to the pre-bootcamp survey, we construct two measures of prior connections at the individual level. The first is an indicator of whether a participant knew *anyone* before the camp. Figure 2 shows the distribution of prior connections. We find that 66 participants knew someone and that 46 did not. We also construct a continuous measure,  $\log(\text{Number of prior ties} + 1)$ , that counts how many prior ties an individual had before the boot-camp started. We take the log since, as can be seen in Figure 2, the number of prior ties is heavily skewed. We use the continuous measure to test whether having more connections reduces the effect of proximity and to check whether our results are an artifact of our dichotomization procedure.

[Figure 2 about here.]

One concern with our prior ties measure is that it may reflect other individual differences beyond variation in prior ties. For instance, our measure may capture personality differences that may lead to more or fewer prior ties. As a result, alternative mechanisms other than our posited ones may have led to our estimated effect. We address this concern by testing, in Appendix Section A2, whether individual-level characteristics predict having more or fewer prior ties. In these robustness tests, we

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<sup>6</sup>In robustness checks in Appendix section A6, we find that teams on either side of the wall do not communicate as if they were seated next to each other.

<sup>7</sup>While the pre-bootcamp survey also asked about advice and friendship ties, these responses were sparse, as expected. Controlling for advice and friendship ties not change our results.

include a participant’s bootcamp admission score, whether she had previously started a company, and measures of self-monitoring and Big Five personality traits (agreeableness, conscientiousness, extroversion, neuroticism, and openness to experience). We find no evidence that these variables predict having prior ties. Furthermore, we believe that our measure of prior ties does not correlate to personality because the number of connections is jointly determined by both whom a participant knew and whether those others were also accepted into the bootcamp.

In Table 1, we provide summary statistics for these variables at the participant-dyad level. In the data, each row represents a potential relationship between participant  $i$  and  $j$ . With 112 participants and 210 within-team dyads, there are a total of  $112 \times 111 - 210 = 12,222$  between-team dyad observations.

[Table 1 about here.]

## Team-level variables

In this section, we describe the construction of the variables used in estimating our team-level peer effects models. In these models, the performance of both focal and proximate teams is measured at the team-level. This level of analysis is because teams, not individuals, produce and submit a single prototype for evaluation. We also aggregate our dyad-level prior ties measure to the team level for all members  $i$  of a given team.

### Dependent variable

**Team performance** Our measure of team performance uses the double-blind evaluations of the team projects submitted on Friday of the first week (for discussion on reducing bias in peer evaluations see Boudreau et al., 2016). Each submission had four components: (1) a one-sentence description of a team’s software app, (2) a one-

paragraph description of a team’s product, (3) a splash page, and (4) a slide deck walkthrough describing how users would interact with the product.

On Saturday morning, each participant conducted a double-blind evaluation of five randomly chosen submissions (excluding that of her team). Participants rated submissions on 13 dimensions, each on a 5-point Likert scale. These dimensions were chosen based on prior literature (see, Girotra, Terwiesch and Ulrich, 2010) and included (1) novelty, (2) insightfulness, (3) empathy with user, (4) predicted demand, (5) purchase intent of the rater, (6) purchase intent of ideal user, (7) feasibility of the product, (8) business potential, (9) prototype quality, (10) splash page quality, (11) paragraph quality, (12) sentence quality, and (13) quality of the startup’s name.<sup>8</sup>

Each of the 40 submissions received 14 double-blinded evaluations. We construct a *Team Performance* measure by averaging across the 14 evaluations, then across the 13 dimensions. To ease interpretation, we standardize this variable to have mean 0 and standard deviation 1. The variation between the top, middle, and bottom teams is statistically significant and was used by bootcamp organizers to select prizewinners for that week.

## Independent variables

To test Hypothesis 2, we create two variables meant to capture proximate team performance and the prior ties of a focal team’s members.

**Peers’ team performance** To estimate our peer effect, we calculate the average *Team performance* of the  $N$  most proximate teams to a focal team, where  $N$  is 4, 8, and 16. For instance, if the 4 most proximate teams had project ratings of  $x_1$ ,  $x_2$ ,  $x_3$ ,  $x_4$ , our peer performance measure is  $(x_1 + x_2 + x_3 + x_4)/4$ .

**Team’s prior ties** Finally, we measure prior connections at the team level in two ways. First, we calculate the total number of prior connections that a team’s members

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<sup>8</sup>Our results hold both in the aggregate and when we look at ratings of the central component of the submission, the prototype.

have. Second, we also compute an indicator of whether a team’s members have an above- or below-median number of prior connections relative to other teams at the bootcamp.

Table 2 provides summary statistics at the team-level for the variables we use to test Hypothesis 2.

[Table 2 about here.]

## Results

### Balance tests of spatial randomization

We begin our analysis by testing for balance on observable variables in our spatial randomization. Specifically, we test whether the distance between persons  $i$  and  $j$  is related to whether they have a prior connection. We also test whether proximity is correlated to other pre-randomization characteristics of individuals.

In Model 1 in Appendix Table A3.1, we show that that distance is unrelated to how far or close a prior connection is. Further, Model 2 finds no evidence that participants with prior ties are nearer to others on average at the bootcamp. Finally, in Model 3 we find that distance is unrelated to our individual difference measures. Together, these tests support the validity of our randomization procedure.

### Prior ties and the effect of proximity on social interaction

To test Hypothesis 1, we begin by graphically examining whether prior connections affect the likelihood of social interaction at the bootcamp.

In the leftmost plot in Figure 3, we present a non-parametric LOWESS curve relating the probability of  $i$  knowing  $j$  as a function of distance. The solid line consists of data from participants with no prior connections. The dashed line represents partic-

ipants with at least one prior connection. The relationship between distance and the probability of interaction is stronger for those without prior connections.<sup>9,10</sup>

The second plot in Figure 3 shows the effect of distance on advice relationships. In this graph, the difference between those with and without prior connections is more striking.

The final plot shows the estimated number of digital messages from  $i$  to  $j$ . Again, we find a similar pattern for those without prior connections and a somewhat upward sloping relationship for those with prior connections.

[Figure 3 about here.]

[Table 3 about here.]

In Table 3 we estimate the magnitude and statistical significance of the effects displayed in Figure 3. To do this, we estimate logistic and quasi-Poisson regressions. In our models, we cluster our errors at the level of an ego’s team, the alter’s team, and at the team-dyad level (Cameron and Miller, 2015). The clustering accounts for the possibility that teams may have correlated networking choices and for the non-independence present in social interaction data.<sup>11</sup> For each type of social interaction, we estimate models with prior ties measured using a dichotomous as well as a continuous variable.

Model 1 in Table 3 tests whether prior connections mitigate the effect of distance on whether  $i$  knows  $j$ . As expected, the baseline effect of distance is negative with more distant peers less likely to know each other ( $-0.023, p = 0.02$ ). Furthermore, participants who have prior ties are also less likely to know others ( $-.309, p = 0.06$ ).

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<sup>9</sup>We do not present confidence intervals in these plots because the observations are non-independent and standard LOWESS uncertainty intervals are therefore inappropriate. In the regression models below, we incorporate the non-independence of observations in our statistical tests.

<sup>10</sup>Because our focus is on how the bootcamp participants interact with new peers, we drop the 304 dyads where  $i$  has a prior tie to  $j$ . Our results remain unchanged when we include and then control for the presence of a prior tie between  $i$  and  $j$  instead of excluding such connections.

<sup>11</sup>Results are substantively unchanged when clustering at the individual ego, alter, and dyad level.

Finally, the coefficient on the interaction of these two variables is  $(0.017, p = 0.07)$ , suggesting that those with prior ties are less likely to know their nearby peers. The magnitude of this coefficient is similar to that of the baseline distance effect.

Overall, the estimated effect size implies meaningful differences in tie formation. The probability that a participant without prior ties knows a peer 1m away is 21%; for a peer with prior ties, this probability is 16.6%. By comparison, for peers across the room (26m away), those without prior ties have a 13% likelihood of connecting with peers; this is compared to a 14.6% likelihood for those with connections.

In Model 3, we replicate this analysis for advice interactions and find similar results. Distance reduces the magnitude of an advice interaction among peers  $(-0.29, p = 0.02)$ . Similar to Model 1, having a prior connection reduces advice seeking in general  $(-0.84, p = 0.002)$ . Finally, we find a positive coefficient on the interaction term of prior ties and distance  $(0.029, P = 0.08)$ .

Finally, Model 5 uses a quasi-Poisson model to test whether distance and prior ties interact to affect digital communications. We find the distance effect is again significant  $(-0.001, p = 0.026)$ , as is the interaction effect  $(0.001, p = 0.06)$ .

Models 2, 4, and 6 replicate Models 1, 3, and 5 using a continuous measure of the number of connections. This specification increases the information in our independent variables but makes interpretation more challenging.

In Model 2, although we find a positive coefficient on the interaction between distance and the number of connections, the statistical significance of the result is weaker at  $p = 0.14$ . However, for advice and digital messaging, we find concordant evidence that participants with prior ties were less likely to interact with their nearby peers. For advice and digital messaging the interaction terms are both positive and significant  $(0.022, p = 0.03; 0.011, p = 0.01)$ .

Finally, we further validate these results in our Appendix. In Section A4, we show that our results hold when estimating linear models instead of nonlinear logit and quasi-

Poisson models (Ai and Norton, 2003). In Section A5, we show that our results hold when we include individual-level fixed effects and estimate within-participant models. In these models, the statistical significance for knowing ties is again weaker, holding at only the 10% level. Furthermore, our findings hold even when we include both fixed effects for individuals and the interactions between distance and individual differences in ability, experience, and personality. Additionally, in Section A6 we show the results are robust to architectural controls, such as whether individuals are seated near a wall or other busy areas of the space.

Across these models, we find consistent evidence that individuals with prior ties were overall less likely to interact with their proximate peers than those who attended the bootcamp having no prior connections.

## **Team-level peer effects and the prior ties of team members**

The results above show that having prior ties reduces the propensity of individual team members to interact with peers on nearby teams. Given that the mechanisms responsible for peer effects—social comparison, learning, and competition—depend on interaction, Hypothesis 2 argues that teams whose members have many prior ties will be less affected by their nearby peers.

[Figure 4 about here.]

[Table 4 about here.]

We test Hypothesis 2 by estimating whether teams whose members have many prior ties are less affected by the performance of nearby teams. Figure 4 plots a focal team’s performance against the average performance of the eight nearest teams. The average distance of the eight nearest teams is 3.7 meters.

The first plot in Figure 4 examines teams with below-median prior ties. We see that performance for these teams is positively correlated to that of their most proximate

teams. The second plot in Figure 4 is for teams with above-median connections. These teams appear unaffected by the performance of nearby teams.

An empirical test of peer effects in this context is challenging because “neighborhoods” across the 40 teams overlap. Thus, we cannot assume independence between observations in our data when estimating statistical uncertainty. As we have discussed above, we address this concern by leveraging models used in the analysis of spatial data. Specifically, we use a permutation (randomization) test to estimate the statistical significance of our peer effects (Good, 2004; Gerber and Green, 2012). In our setting, this involves randomly shuffling the performance across teams without replacement. For each permutation, we then calculate new values of each team’s simulated neighbors and then estimate the model described in Equation 2. We repeat this procedure 1,000 times to produce a distribution of simulated peer effect coefficients that take into account the underlying spatial non-independence of team locations. If there are peer effects, then the estimate from the real data should, on average, be larger than the estimates from our permuted data. To quantify how much larger our estimate is, we calculate pseudo-p-values. A pseudo-p-value is merely the percentage of the simulated estimates that are larger than the estimate from our *real* data.

In Table 4 we report the estimated coefficients from Equation 2 and the corresponding pseudo-p-values for each estimate. We present models with neighborhood sizes of 4, 8, and 16 to provide bounds on how influential a small group of nearby teams is versus a larger group of more distant teams.

Model 1 presents results for a neighborhood size of 4. We find evidence that proximate teams influence team performance. The coefficient on proximate team performance is positive ( $0.72, p = 0.05$ ) and we find that teams with more prior ties (e.g., above the median) appear less influenced by nearby teams ( $-0.97, p = 0.06$ ). Model 2 expands the neighborhood to 8 teams. We again find evidence for our hypothesized effects. The peer effect coefficient has a pseudo-p-value of under 0.01, and the inter-

action term examining the effect of prior ties has a p-value of 0.04. We note that the coefficients on the latter two estimates are similar in absolute magnitude, 1.68 vs.  $-1.80$ . These estimates suggest that prior ties “shut off” peer effects.

Finally, Model 3 expands the neighborhood to the 16 closest teams. In this specification, the peer effect remains statistically significant. However, our estimate of the moderating effect of prior ties appears weaker. Comparing the r-squared values across these models reveals that Model 2 explains roughly twice the amount of the variance as compared with the smaller or larger neighborhood models. Based on our estimates, a focal team’s 8 nearest teams appear to be the most influential. Finally, in all three models, we find little evidence that having more connections improves a team’s performance. This result is consistent with our claim that the number of prior ties is unrelated to ability or experience.

In Models 4 through 6, we replicate our analysis using a continuous measure of prior ties. Again, we find similar results. In Model 5, which uses a neighborhood group of the 8 closest teams, we find the impact of proximate team performance drops for every additional prior connection a team has ( $-0.137, p = 0.06$ ). Our model suggests that a team without any prior ties will experience the most robust peer effects driven by the performance of nearby teams. However, for a team with roughly 13 total prior ties, the equivalent of moving up two standard deviations, the peer effects are nearly zero.

In Appendix Section A7, we test the robustness of our results to alternative measures of a team’s prior ties. These alternative measures yield similar results when we use (1) the average number of connections in a team, (2) the log of the total number of connections, (3) the number of unique connections, and (4) the number of unique connections to other teams. Furthermore, we account for the fact that smaller teams will have fewer total connections by including controls for team size. Again, our results hold with these additional controls.

## Conclusion

In recent years, researchers have argued that organizations might be able to design social interventions—e.g., by encouraging new interactions among members—to increase productivity (Bandiera, Barankay and Rasul, 2010; Catalini, 2017). In this article, we propose that existing social structure—the prior ties of individuals—may limit the power of organizations to facilitate peer effects among individuals or teams. We conduct a field experiment and demonstrate that when a team’s members have many prior ties, that team’s performance is unlikely to be influenced by proximate individuals and teams.

We find that teams whose members have few or no prior ties experience a substantial shift in their performance: a one-standard-deviation increase in the performance of proximate teams leads to a two-thirds standard-deviation improvement in the performance of the focal team.<sup>12</sup> In contrast, we find that individuals with many prior ties are less likely to interact with peers on nearby teams. As a consequence, their teams are unlikely to experience substantial peer effects—i.e., they are unaffected by the performance of nearby teams.

We see several related contributions of our work. First, we contribute to the growing literature in economics (e.g., Carrell, Sacerdote and West, 2013; Hjort, 2014) and strategy (e.g., Housman and Minor, 2016; Puranam, 2018) on the design of social interventions for improving individual and team performance. We highlight how existing social structures might fundamentally limit the ability of organizations to design interventions to encourage new interactions between their members. Our work also contributes to the growing literature on peer effects in strategy (e.g., Oettl, 2012; Chan, Li and Pierce, 2014*a*). To our knowledge, our study is one of the first to highlight how social moderators shape peer effects. Given the richness of social life in most organi-

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<sup>12</sup>For a neighborhood of  $N=8$ , we have an estimated peer effect of 1.67. For this neighborhood, the standard deviation of average performance is 0.39.

zations, the prior networks of individuals should affect the extent to which individuals interact with and are influenced by their workplace peers.

Third, our study is among the first to focus on peer effects in a novel and important context, early-stage startups. Founding an early stage startup is filled with uncertainty. Institutions such as incubators, accelerators, and bootcamps are important sources of new knowledge for individuals and their teams. Peer effects are often a crucial source of influence and learning for young entrepreneurs who attend these startup organizations (Cohen, 2013). If peer effects are limited by the mechanisms we identify here, the impact of such startup ecosystem institutions may be hindered. As countries such as India and regions across the United States work to become the next technological hubs, they must pay careful attention to how their ecosystems design the social interventions they hope will spur interaction among nascent entrepreneurs.

Finally, beyond informing the design of peer effects within an organization, our results also highlight a possibly general social mechanism that could operate in several other contexts. For example, recent organizational research highlights the importance of a new employee's prior ties (i.e., referrals) to existing organizational members. These ties are shown to affect hiring decisions (Fernandez and Weinberg, 1997), productivity (Castilla, 2005), and network formation (Sterling, 2014). Our work suggests a potential trade-off of hiring through referrals. If employees have many ties to organizational members at entry, they may have less incentive to build new and beneficial connections outside their existing network. At the firm level, strategy research has also underlined the value of relational capital (e.g., Uzzi, 1996; Hoetker, 2005; Elfenbein and Zenger, 2014) and more recently has explored how a firm's existing relationships influence its strategic decisions (Mawdsley and Somaya, 2018). A possible generalization of our hypothesis may be that a firm's existing relational capital may limit how much a firm is influenced by proximate firms. We think this also is a promising area for future research.

We also acknowledge several limitations of our approach and method. Although our study is comparable to other field experiments on peer effects, our sample size, owing to the limited number of teams in our sample, limits our ability to test for specific mechanisms and contingencies. While we can show that peer effects “shut off” because of decreased interaction, we cannot distinguish between knowledge sharing, competition, or social-comparison explanations for our main effect (for a review regarding mechanisms in peer effects, see Sacerdote, 2011). Thus, a question that our empirical tests cannot answer is whether prior ties shut off both knowledge sharing and competition, or whether one type of peer effect continues to matter when teams have prior ties.

Furthermore, our experiment studies peer effects over a week and in a large open office. Thus, we cannot test how our theorized effects will unfold in bigger and more enduring organizations. Given prior work on network inertia and embeddedness (Granovetter, 1985; Kim, Oh and Swaminathan, 2006), we posit that entrenched prior ties will perhaps have a stronger negative influence on peer effects in more enduring settings. Moreover, prior work has found that the effects of proximate peers hold at a variety of scales, from small rooms (Allen and Cohen, 1969; Hasan and Bagde, 2013; Boudreau et al., 2017) to cities and regions (Agrawal, Galasso and Oettl, 2017). Thus, we expect that the basic patterns we find here may appear in other contexts as well. Nevertheless, we believe our findings apply more directly to startup ecosystem organizations—e.g., bootcamps, mixers, accelerators, and incubators—where similar short interactions at a similar scale are widespread and consequential (Cohen, 2013).

In sum, we hope that our results encourage new research on when social interventions can be fruitfully deployed by organizations to improve individual and team performance.

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Figure 1: Floor plan of the bootcamp space. Team workstations are indicated by the gray rectangles. The dark black line indicates the location of the the single low wall that prevented interaction between nearby teams.

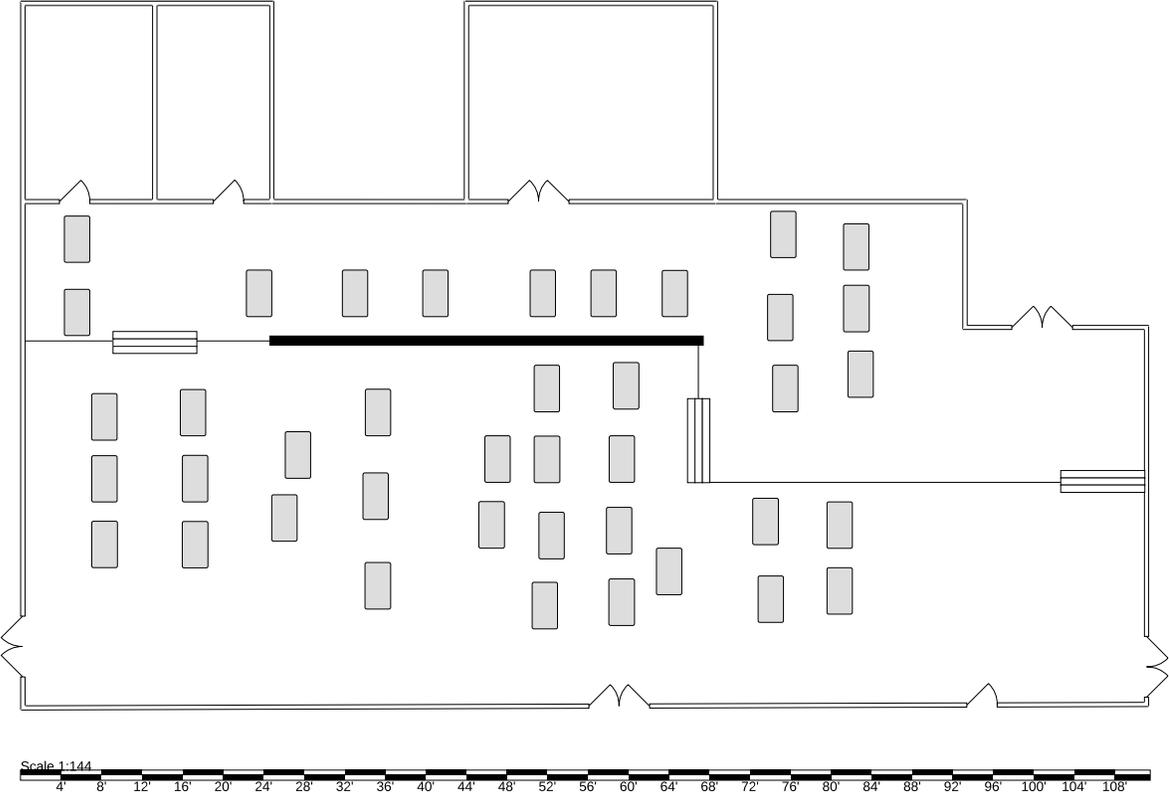


Figure 2: Histogram depicting the number of prior ties for individuals at the entrepreneurship bootcamp.

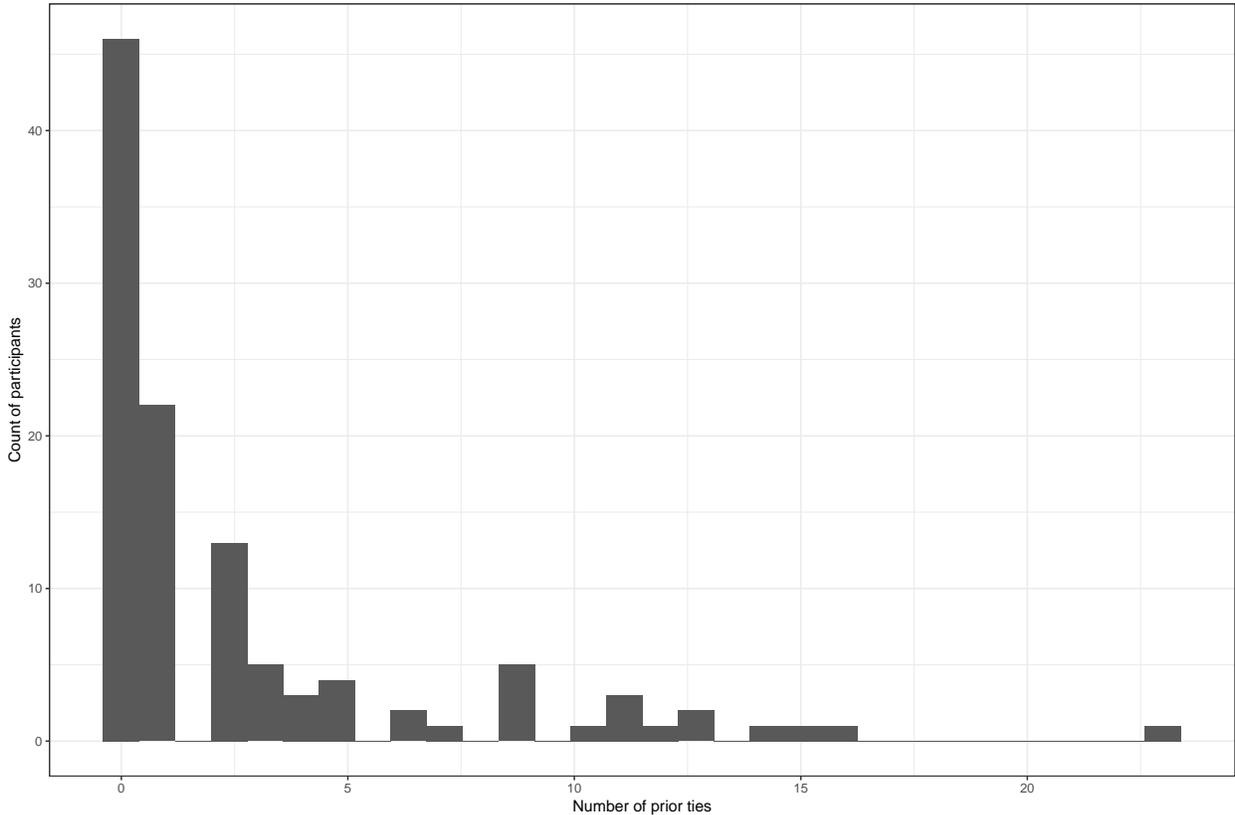


Figure 3: Team members with no prior ties are much more likely to form ties to nearby peers.

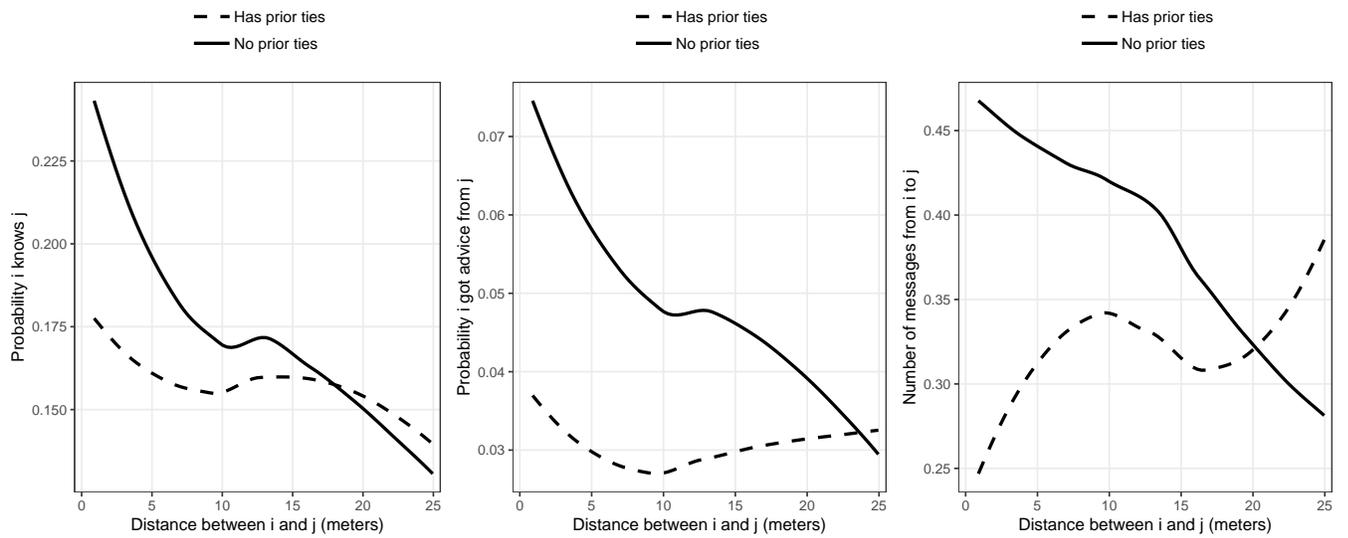


Figure 4: Teams whose members have fewer prior ties perform more similarly to proximate teams than do teams with more prior ties.

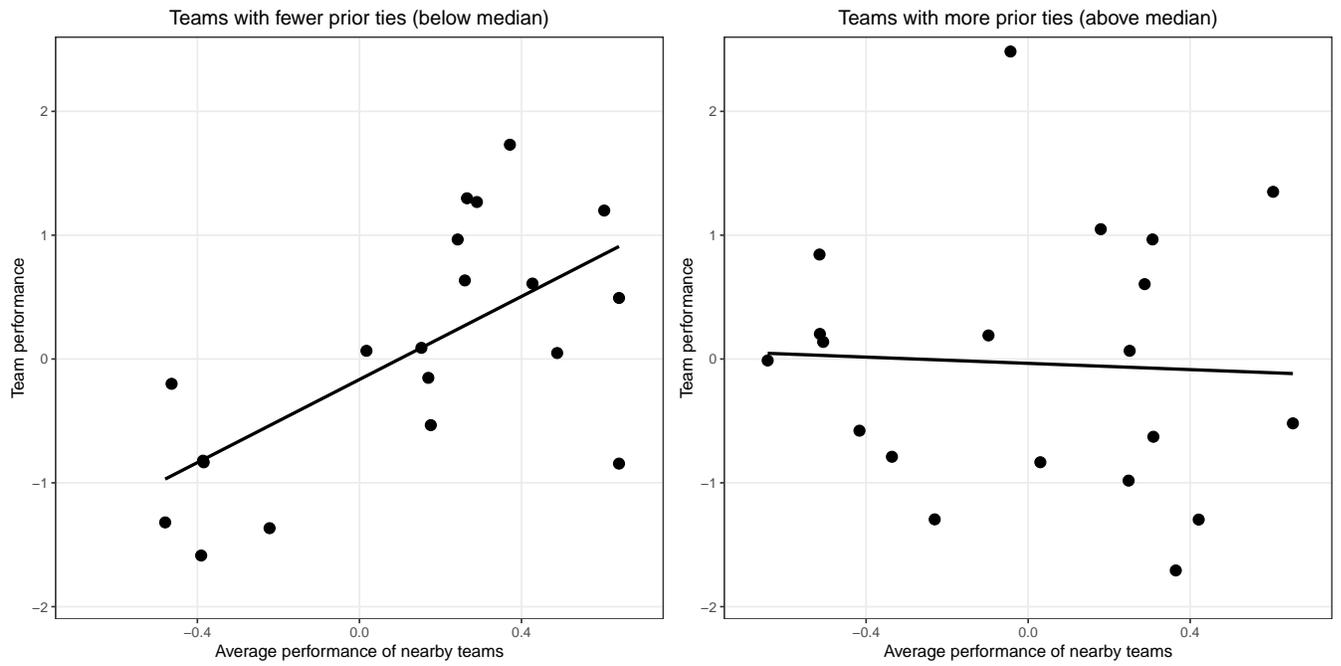


Table 1: Individual-dyad summary statistics

Variable	N	Mean	St. Dev.	Min	Max
$i$ has a prior tie to $j$	12,222	0.02	0.16	0	1
$i$ 's number of prior ties	12,222	2.75	4.33	0	23
$i$ has prior tie	12,222	0.59	0.49	0	1
Distance between $i$ and $j$ (meters)	12,222	13.22	7.24	0.89	26.27
$i$ knows $j$	12,222	0.18	0.38	0	1
$i$ got advice from $j$	12,222	0.05	0.21	0	1
Number of messages from $i$ to $j$	12,222	0.36	1.41	0	35

Table 2: Team-level summary statistics

Variable	N	Mean	St. Dev.	Min	Max
Total number of prior ties	40	7.7	6.5	0	23
Total prior ties above median	40	0.50	0.51	0	1
Team performance	40	0.00	1.00	-1.71	2.48
<i>N</i> = 8					
Performance of nearby teams (mean)	40	0.07	0.39	-0.64	0.65
Distance to nearby teams (mean)	40	3.73	1.14	2.04	7.49
<i>N</i> = 4					
Performance of nearby teams (mean)	40	0.02	0.51	-0.97	0.90
Distance to nearby teams (mean)	40	2.58	0.84	1.50	5.87
<i>N</i> = 16					
Performance of nearby teams (mean)	40	0.14	0.30	-0.37	0.65
Distance to nearby teams (mean)	40	6.22	1.73	3.82	9.59

N is the number of teams treated as nearby peers.

Table 3: Entering the camp with a prior knowing tie mitigates the effect of proximity on peer interaction.

	<i>Dependent variable:</i>					
	Know		Advice		Messages	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance between $i$ and $j$ (meters)	-0.023 p = 0.020	-0.017 p = 0.040	-0.029 p = 0.017	-0.028 p = 0.004	-0.001 p = 0.026	-0.001 p = 0.013
$i$ has prior ties	-0.309 p = 0.057		-0.841 p = 0.002		-0.033 p = 0.004	
Has prior ties $\times$ Distance	0.017 p = 0.068		0.029 p = 0.082		0.001 p = 0.055	
Log( $i$ 's number of prior ties +1)		-0.062 p = 0.519		-0.555 p = 0.003		-0.016 p = 0.001
Log( $\cdot$ ) $\times$ Distance		0.004 p = 0.140		0.022 p = 0.025		0.001 p = 0.011
Constant	-1.299 p = 0.000	-1.428 p = 0.000	-2.647 p = 0.000	-2.708 p = 0.000	0.063 p = 0.000	0.057 p = 0.000
Observations	11,918	11,918	11,918	11,918	11,918	11,918
Log Likelihood	-5,239	-5,243	-1,873	-1,874		

Significance stars (\*) are omitted.

Models 1-4 logistic regression. Model 5-6 quasi-poisson.

Robust standard errors clustered at ego-team, alter-team, and team-dyad levels.

Individual-level between-team dyads.

Dyads where  $i$  knew  $j$  pre-camp are excluded.

Table 4: When does the performance of nearby teams influence a focal team’s performance?

	<i>Dependent variable:</i>					
	Team performance					
	(1)	(2)	(3)	(4)	(5)	(6)
Performance of nearby teams (mean)	0.719 p = 0.050	1.677 p = 0.003	1.500 p = 0.030	0.774 p = 0.051	1.841 p = 0.002	1.838 p = 0.019
Total prior ties above median	-0.045 p = 0.497	0.131 p = 0.507	0.231 p = 0.523			
Performance of nearby teams × Total prior ties above median	-0.967 p = 0.060	-1.803 p = 0.043	-1.706 p = 0.159			
Total prior ties				0.001 p = 0.490	0.008 p = 0.482	0.023 p = 0.539
Performance of nearby teams × Total prior ties				-0.075 p = 0.065	-0.135 p = 0.062	-0.137 p = 0.141
Constant	0.005 p = 0.986	-0.166 p = 0.969	-0.249 p = 0.932	-0.041 p = 0.983	-0.174 p = 0.951	-0.349 p = 0.921
Number of nearby teams	4	8	16	4	8	16
Observations	40	40	40	40	40	40
R <sup>2</sup>	0.069	0.202	0.091	0.078	0.219	0.111

Significance stars (\*) are omitted.

Models 1-6 linear regressions.

We report pseudo-p-values generated from a permutation bootstrap test with 1,000 runs.