The Power (of) Lunch and the Role of Incentives for Fostering Productive Interactions

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November 3, 2018

Abstract

We conducted a field experiment in a sales organization to investigate whether management interventions improve knowledge transmission between coworkers. Encouraging agents to talk about their sales process with a randomly chosen partner during short meetings and over lunch substantially lifted average sales revenue during and after the experiment. Sales increases from these structured meetings are larger and more persistent than those achieved by providing joint incentives tied to partner sales improvement. Joint incentives yielded small incremental sales gains for agents who also received the meetings treatment. Agents in the meetings treatment solved problems together and shared best practices, with the largest gains for agents paired with above-median partners. Given the substantial sales improvement from treatment and the high-powered individual incentives already in place before the experiment, we discuss possible reasons why agents did not realize these gains on their own.

JEL: D8, J3, L2, M5.
Keywords: management practices; organizational design, randomized controlled trial (RCT).
Acknowledgements: We thank Karen Bernhardt-Walther, Nick Bloom, Jen Brown, Ben Golub, James Hines, Ed Lazear, Luis Rayo, John Roberts, Raffaella Sadun, Scott Schaefer, Kathryn Shaw, Orie Shelef, Andrei Shleifer, and Jason Snyder for helpful comments. **RCT registry number:** AEARCTR-0002332.

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1 Introduction

The concentration of economic activity in firms exists, in part, to harness knowledge spillovers (Marshall, 1898; Coase, 1937). While existing evidence points to knowledge spillovers as a source of productivity growth (Glaeser et al., 1992; Barro, 1991), relatively little is known about how knowledge transfers take place between coworkers or how different management practices influence the degree of knowledge spillovers within firms. One of the primary difficulties in studying intra-firm knowledge exchange is the lack of exogenous variation in observational data (Manski, 1993; Glaeser et al., 2003). We conducted a field experiment with the goal of creating this exogenous variation.

Our starting point is the observation that productivity differs tremendously across individuals and firms. This dispersion is even apparent for workers performing identical jobs within the same firm, suggesting that the dissemination of best-practices, or more generally, coworker knowledge exchange may be constrained by frictions.[1] We posit two alternative explanations for the lack of observed knowledge flows. One potentially limiting factor is initiation costs, e.g., the cost of putting work down to coordinate a meeting, social costs that correlate asking for help with perceived weakness, or literal search costs about where to find information.[2] An alternative explanation for the lack of perceived knowledge sharing resides not with knowledge seekers but rather with potential knowledge providers. Specifically, Coase (1937) and Williamson (1981) define contracting costs explicitly as the inability of the knowledge provider to fully capture the value associated with the knowledge exchange.[3]

[1]This dispersion in productivity across workers doing nearly identical tasks is found in many settings, including grocery cashiers (Mas and Moretti, 2009), technology based service workers (Lazear et al., 2015, 2016), and windshield installers (Lazear, 2000).

[2]Earlier work has found evidence of knowledge transfers in settings with relatively low initiation costs. For instance, the growth literature credits the relatively low search costs associated with finding similar firms or people with common interests in cities, as one of the central explanations for urban knowledge spillovers (Jacobs, 1969; Glaeser et al., 1992). More recent work explores the efficacy of lowering initiation costs through managerial processes to accelerate worker learning (Chandrasekhar et al., 2016; Edmondson and Lei, 2014; Cai and Szeidl, 2017).

[3]Many models of person-to-person knowledge transfer begin with the assumption that knowledge sharing or mentoring is difficult to contract over (Morrison and Wilhelm Jr, 2004; Garicano and Rayo, 2017; Fudenberg and Rayo, 2017). Relatedly, Becker (1962) discusses the contracting costs associated with a firm sharing knowledge with employees. Specifically, trainees disproportionately benefit in the long-run while firms pay

The experiment took place in a sales and marketing firm's inbound call center. Agents sell digital services—primarily television, phone, and internet packages from multiple large providers—to customers calling from across the United States. Calls are allocated to agents randomly based on their availability, meaning all agents have the same selling opportunities and agents do not compete with one another for sales leads. The work is individual, and we measure agent productivity throughout the experiment using the firm's focal performance metric, individual agents' revenue-per-call (RPC).[4] The detailed production data, the absence of production spillovers, and the importance of firm-specific human capital within this firm provide us with a unique lens to study knowledge exchange.

Individual performance varies widely across agents in our setting. Agents in the 75th percentile of the productivity distribution have expected revenue on a given call that is over 70 percent higher than agents in the 25th percentile of the distribution. One potential explanation for this dispersion is that the best agents have developed superior techniques that are hard to codify and, therefore, are not easily learned from manuals or traditional training methods. For example, these agents may know how to tailor their sales pitch to deal with anxious customers or may incorporate recommendations reflecting a changing product mix, differences in product availability, or idiosyncratic customer eligibility for discounts. To illustrate, we frequently heard that star agents have techniques to respond when new information arises during a call. For example, if a customer does not qualify for a product because of credit check requirements, a star agent would likely know how to re-direct that customer to other products that fit their needs; less skilled agents may give up on the sale or frustrate the customer by suggesting an infeasible solution.

We experimentally vary initiation and contracting costs through separate treatments. All three treatments paired agents together and took place over a four week treatment period.[5]

an up-front cost, leading to under-provision of general skills training in firms.

[4]Call-centers focused on inside selling represent the fastest growing segment of the 14.5 million sales jobs in the United States. See the BLS website for sales employment: https://www.bls.gov/oes/current/oes410000.htm. See Krogue (2013) in Forbes on the shift to inside sales "What is Inside sales? The Definition of Inside Sales."

[5]We incorporate twelve consecutive weeks of data in our analysis. These data contain the pre-treatment,

The first treatment, labeled *Structured Meetings*, lowered initiation costs by encouraging agent-pairs to meet early in the week to fill out a worksheet that discussed their sales techniques.[6] Agents who filled out a worksheet were encouraged to follow up with their partner over a free lunch toward the end of the week. The second treatment, labeled *Pair Incentives*, reduced contracting costs by providing financial incentives to pairs of agents to increase their joint production. The third treatment, labeled *Combined*, included all the elements of both the *Structured Meetings* and the *Pair Incentives* treatments to measure the effects of simultaneously lowering both initiation and contracting costs. We also include an *Internal Control* group where all agents were paired and knew about the experiment and an off-site *External Control* group, where agents were unaware of the experiment. The primary findings are:

1. The *Structured Meetings* treatment was particularly effective. Relative to the control group, the *Structured Meetings* treatment yielded a 24% increase in revenue-per-call during the treatment period compared to a 13% increase in the *Pair Incentives* treatment. The per-agent cost of implementing the *Structured Meetings* treatment was also approximately 50% lower than that of the *Pair Incentives* treatment, resulting in higher accounting profit margins.[7] Results are similar for every sales measure tracked by the firm, including revenue-per-hour (RPH), and total revenue per week. The return-on-investment (ROI) from the *Structured Meetings* treatment was approximately 500% across the four week treatment period. All other active treatments had positive, albeit smaller, ROIs during the same 4-week period.

treatment, and post-treatment periods. We limit the post-treatment window to four weeks so as not to contaminate our results with a subsequent re-organization that reallocated agents to different teams and brands. This re-organization was announced six weeks after the experiment ended.

[6] One side of the worksheet asked agents to reflect on their performance that week (e.g., their most difficult call and how (in hindsight) it could have been improved), and the other side had the agent solicit the same responses from their partner. This treatment was designed as a human resource management (HRM) process.

[7] While the experiment had two levels of incentive compensation ($0 and a prize worth about $50 per agent), extrapolation based on a linearity assumption suggests it would have taken a prize with a monetary value of about $100 per agent to replicate the *Structured Meetings* results. This extrapolation, however, is not experimentally identified, nor does it distinguish between agent effort and learning.

2. Agents in the *Combined* treatment had roughly similar gains during the treatment period to those in the *Structured Meetings* treatment. Explicit pair incentives had a positive but small effect in addition to the *Structured-Meetings*.[8]

3. Treatments involving *Structured Meetings* induced knowledge transfers between peers, while the *Pair Incentives* treatment did not.

   (a) The *Structured Meetings* and *Combined* treatments yielded persistent performance increases through the four week period after treatment ended.

   (b) Agents in the *Pair Incentives* treatment had post-treatment productivity that was statistically indistinguishable from either control group.

   (c) Heterogeneous effects by partner ability distinguish knowledge transfers from self-improvement and sentiment increases. Agents in the *Structured Meetings* and *Combined* treatments performed better across the treatment and post-treatment periods when paired with stars—agents with pre-treatment productivity above the median. Star agents themselves improved in the *Structured Meetings* and *Combined* treatments only when paired with other stars.

   (d) Survey responses and interviews of participants and managers indicate that the *Structured Meetings* treatment induced agents to share best practices, while the *Pair Incentives* treatment did not.

We further designed the experiment to separate the knowledge transfer channel from other potential mechanisms. First, agents are followed for 4 weeks after the study to test whether the productivity gains are persistent (an indicator of knowledge transfer). Second, we test whether agents randomly paired with a star agent outperform agents not paired with a star. Third, half of the agent pairs in each treatment were randomly assigned to rotate

[8]There exists a large literature in economics and psychology investigating the crowd-out effects of monetary incentives; see Bénabou and Tirole (2006), Ederer and Manso (2013), Titmuss (1970), Frey and Oberholzer-Gee (1997), Ariely et al. (2009a), Ariely et al. (2009b), and Gneezy et al. (2011) and Deci et al. (1999). The evidence suggests that, in this setting, the *Pair Incentives* and *Structured Meetings* treatments are substitutes not complements.

partners each week, allowing us to identify whether exposure to multiple partners or repeated interactions with a single partner deferentially affects post-experiment productivity.[9] Fourth, we examine whether agents that are less socially connected before the study outperform agents that are more socially connected.[10] Finally, we leverage additional survey evidence to provide suggestive evidence on the mechanism. We document that agents believe they can identify star agents and perceive that help from star agents would improve their sales. In fact, the average agent estimates that interacting with a top sales agent would result in a 12% sales lift.

Given the magnitude of the productivity changes that we estimate, an obvious question is: why were these practices not attempted earlier by *the firm*? First, the outcomes were not obvious to the firm's management (nor to the authors). In particular, many leaders on the sales teams believed that providing joint incentives to overcome contracting costs would be sufficient to induce knowledge transfers. Human resource managers were intrigued by the potential efficacy of using non-monetary interventions to increase peer-to-peer interactions. Second, experimentation was necessary to uncover these findings, and large scale experiments had not been attempted within this firm.[11] Based on the favorable outcomes of our experiment, the firm's management has replaced their traditional on-boarding process with a mentoring program to increase knowledge exchange between newly hired sales agents and seasoned workers.[12]

The evidence in this paper has broad implications for human capital spillovers writ large. Our findings suggest that simple, yet deliberate, interventions can successfully foster knowledge exchange between individuals. When designing education interventions, our results

[9] We find positive but imprecisely estimated effects from partner rotations in treatments that included *Structured Meetings*.

[10] Agents who reported fewer co-worker connections had larger treatment effects, suggesting that increasing co-worker interactions is most beneficial for those who begin with a low baseline.

[11] Similarly, Jackson and Schneider (2015) use an experiment and find large, and unanticipated, productivity gains through the introduction of checklists in auto repair shops. The results across numerous studies underscore that field experiments are a useful tool to test new practices prior to firm-wide adoption (Carpenter et al., 2005).

[12] The firm's positive reaction to the experimental intervention is similar to that of the study firm in Bloom et al. (2015).

suggest directly encouraging interaction rather than providing financial incentives will have a larger, and more cost effective, impact. Recent work parallels this finding, including Fryer Jr (2011) that finds financial incentives for academic achievement are ineffective and Shue (2013) that finds alumni reunions contribute to substantial peer effects. Our micro-level evidence complements the findings that wages are higher in urban areas (Glaeser and Mare, 2001) and for those in close proximity to educated workers (Rauch, 1993). Broadly, our experimental evidence supports the findings of large social multipliers in firms, consistent with earlier findings in the contexts of crime, education, and urban design Glaeser et al. (1996); Acemoglu and Angrist (1999); Sacerdote (2001); Glaeser et al. (2003).

2 Productivity Spillovers

Our work relates to several literatures, but we believe ours is among the first to connect management practices (Bloom and van Reenen, 2011; Bloom and Van Reenen, 2011) with their ability to influence "social learning" (Conley and Udry, 2010; Hanna et al., 2014) from peers. A firm's ability to diffuse information among its workers takes center stage in the strategy literature on the knowledge-based theory of the firm (Grant, 1996; Spender, 1996), though testing how organizations can influence knowledge spillovers has proved elusive. Regardless of the root cause–initiation or contracting costs–managment's role is to mitigate any frictions hindering peer learning. Recent work finds large effects from interventions to alleviate social pressure or, initiation costs, suggesting that our study firm is unlikely to be unique (Bursztyn and Jensen, 2017).

The peer knowledge spillover that we document has an obvious connection to the substantial literature on peer effects and mentoring in the workplace (Mas and Moretti, 2009; Lyle and Smith, 2014; Lazear et al., 2015). Much of the focus in the existing literature, however, is on tasks with significant externalities, including: effort externalities (Mas and Moretti, 2009), production coordination (Friebel et al., 2017), internal competition (Chan

et al., 2014), and social spillovers associated with choosing one's coworkers (Bandiera et al., 2005, 2013). Less studied are the potential gains from peer interactions in production settings with minimal interdependencies among co-workers. The closest work is the literature on peer effects in education, with several studies underscoring the need for controlled experiments.[13]

Finally, the spillovers identified in our study relate to the burgeoning work on interfirm interactions—ranging from urban economics to the economics of technology adoption (Cai and Szeidl, 2017; Hasan and Koning, 2017; Catalini, 2017). The channel is distinct from the search barriers preventing scientific collaboration identified in Boudreau et al. (2017); instead, the hurdle to knowledge sharing in our setting appears to stem from the lack of pre-existing lines of communication between agents. This finding aligns with the growing body of evidence on the importance of management practices (Bloom et al., 2016, 2013; Bloom and van Reenen, 2011) and highlights the potential role for *active* management to unlock spillovers inside firms.[14]

3 Experimental Setting

3.1 The Study Firm

The experiment took place in an inbound-sales call-center from July to August of 2017. The firm employs over 730 sales agents across three geographically separate offices.[15] The firm contracts with television, phone, and internet providers to market and sell their goods and services. Sales agents are tasked with fielding inbound calls from potential customers, determining customer needs, and explaining the benefits of premium service packages (up-selling) when appropriate. Such third-party sales contracting is common practice in the

[13]The education literature has long favored RCTs to evaluate alternate policies (e.g. Garlick (2014) and Carrell et al. (2013)), and there are many examples of research where RCTs uncover evidence which would be invisible to observational data (e.g. Nagin et al. (2002); Bertrand and Mullainathan (2004); Fehr and Goette (2007)).

[14]For discussion, see Glaeser and Gottlieb (2009), **?**, and Agrawal, Kapur, and McHale (2008).

[15]The two offices involved in the experiment are within 50 miles of one another, whereas the third office is located over 600 miles away.

United States, especially for nation-wide service providers.

The sales department of the firm consists of six large sales divisions and several smaller sales divisions. Sales divisions are headed by one or two division presidents and are uniquely characterized by the bundles of products, services, and brands offered for sale.[16] Divisions are comprised of multiple groups consisting of approximately ten to fifteen sales agents led by a single sales manager. All agents in the six largest sales divisions working in the firm's two largest offices were eligible for treatment, resulting in 653 workers assigned to a treatment cell. Workers in the third location, 83, were not eligible for assignment to a treatment group, constituting a hold-out control group. Because there is minimal interaction between workers at different locations and because the hold-out location is geographically distant, workers at the third location were unaware of the experiment. Consistent with this lack of awareness, we find no break in productivity for these agents around the experiment.

Sales agents spend about 80% of their workday on the phone or waiting to field a call. When a call arrives, it is assigned to the next available agent in the queue.[17] When not receiving phone calls, agents participate in group- and division-wide meetings as well as in one-on-one discussions with managers. Most agents, 87%, work full-time schedules.[18] The sales floor is predominately male, 68%, and is relatively young, with an average age of 26. In a given week, the average agent takes 62 calls, approximately two calls for every hour available to answer the phone.

[16]For example, one division might only sell internet packages from provider A, while another might sell internet packages from provider B *and* satellite television packages.

[17]Only a small percentage, 3%, of calls are outbound, and most of these arise from agents following up with existing customers or returning a dropped call.

[18]The threshold for full-time employment at the firm is 32 hours per week. The maximum number of weekly hours observed in our data is 46.

3.2 Sales Agent Training, Development Practices, and Productivity Dispersion

When first hired, agents are enrolled in a formal, two-week, on-site sales training regimen. Throughout training, agents are paid to absorb information delivered largely through lectures and by listening in on other agents' phone calls. The trainees then spend up to four weeks in a hands-on training program taking calls under the supervision of a temporary training manager. The training manager oversees all agents as they cut their teeth in sales, both familiarizing agents with the process of selling and educating them on the products being sold. Once trainees reach a threshold level of revenues, they join a permanent group on the sales floor where they continue to sell the same products and services on which they were trained. Sales agents that do not reach the threshold levels of performance within a designated number of weeks after training are usually let go.

There is substantial dispersion in sales across agents. Using data from the 4 weeks preceding treatment, we estimate the overall dispersion in residual sales after controlling for time-by-sales division fixed effects. We decompose this variation further to extract agent fixed effects which proxy for skill differences.[19] Figure 1 shows that differences across agents explain a significant fraction of the total variation in sales. The interquartile range of log Revenue-Per-Call (RPC) residuals due to agent effects suggests that, on a random call, an agent at the 75th percentile of the fixed effects distribution generates about 52 log points (70%) more revenue than an agent at the 25th percentile of the distribution. The agent fixed effects, in this context, capture experience effects and cross-sectional skill or knowledge differences. We explore the extent to which this dispersion changes with tenure by zooming in on highly tenured agents. The productivity dispersion remains significant for agents with greater than 38 weeks of tenure (the median). For these experienced agents, the interquartile range of their fixed effects is about 40 log points. Firm-specific experience reduces some of

[19] We shrink the fixed effects to reduce the influence of sampling error using the procedure in Lazear et al. (2015).

the productivity gap but much remains.

In survey questions designed to assess why agents believe that stars have higher sales than other agents, 32% of survey respondents attribute star agents' success to better social interactions with the customer to learn and respond to their needs. Similarly, 29% of respondents attribute success to a better sales process—knowing when to suggest products, how to overcome objections, and work through specific situations on the back-end computer system. A further 29% of respondents report that superior product knowledge gives star agents an edge. All of these factors point to knowledge and technique differences that drive differences in productivity. A less frequent but still prevalent response focuses on agent effort—where star agents are willing to work harder than other agents when encountering difficult customers.

3.3 Sales Agent and Manager Compensation Structure

Sales agents are compensated in three ways: (1) they receive an hourly wage for the time they are at work (and not on their scheduled breaks). Sales agents start at an hourly wage of approximately 150% of minimum wage and receive small hourly raises for every three months of tenure, with their hourly rate capped at approximately 200% of minimum wage; (2) they receive a weekly commission[20] based on multiple performance measures: the revenue they generate from sales (revenue-per-call is the primary driver of the commission rate, but it also depends on revenue-per-hour worked),[21] their selling efficiency relative to their peers, and the audited quality of their customer service; and (3) they are able to receive bonuses from temporary promotional sales activities.

While agents report that their primary point of contact for solving problems is their direct sales manager, sales managers' incentives do not necessarily reward them for develop-

[20]The average (median) sales agent earns $217.78 ($185.45) per week in commissions.

[21]Partners pay the firm for every sale in accordance with pre-negotiated schedules—some of which vary with the total number of products or services sold by the firm. To insulate agents from the uncertainty surrounding aggregate sales and periodic contractual negotiations, the firm posts relatively fixed "transfer prices" that form the base revenue upon which agents are paid commissions. All use of the term "revenues" in this paper refers to sales priced in accordance with the internal transfer price schedule.

ing under-performing agents. Managers are compensated based on their group's aggregate revenues, and—consistent with Bandiera et al. (2007)—our interviews suggest that these contracts induce managers to focus their efforts on wringing out performance from high-ability workers rather than developing less-able workers. Executives at the firm had previously observed such behavior during promotional periods (e.g., short-run contests between groups reporting to different managers), yet we were told the managerial incentive system in place is too coarse to target managerial attention into developing workers with lower sales.

3.4 Pre-Experiment Collaboration Among Agents

At first glance, two features of the environment potentially reduce agent collaboration. First, peer-learning requires inter-agent communication, yet any time away from the phone results in fewer revenue generating opportunities for an agent which directly decreases their take-home pay (total commissions). However, our observations of agent behavior and the data suggest that there is substantial downtime between calls. The average agent spends about half of their work time on the phone with customers; the lag between calls is predictable, and agents often talk to co-workers between calls. Second, agents' commission rates—i.e., the fraction of their earned revenue paid out as commission—is a weakly decreasing function of their co-workers' success. However, the probability that providing help to a coworker meaningfully shifts one's own compensation is small. [22] Still, we did not observe significant or regular channels for knowledge sharing between agents prior to the experiment.

[22] Commission rates are bucketed into four to five coarse categories that depend on relative performance on revenue-per-call and revenue-per-hour. For the same level of sales revenue, moving from the bottom bucket to the top bucket changes take-home commissions by about ten percent. Employees are fully aware of the incentive structure, but pre-experiment interviews revealed that employees would be willing to collaborate with others if encouraged to do so. This is likely because the probability of changing categories after helping one other person is small. In particular, agents described these categories as if they are relatively fixed.

3.5 Learning about the Firm

Our work with the firm began in June 2016, where we conducted interviews with sales managers and agents. During this time, we learned institutional details about the firm through a process of sharing data analysis around significant organizational changes that occurred later that year. The experiment here post-dates those changes, occurring at an otherwise stable time for the firm.[23]

4 Experimental Design

The design was pre-registered before beginning the experiment.[24] We began by randomly assigning the 52 sales agent groups (based on the identity of a sales manager) into one of four treatment groups. Once groups were assigned to a treatment, agents within each treatment were randomly paired so long as they were in the same division and location and had overlapping schedules. Half of all agent pairs within each cell were then randomly selected to rotate partners each week, whereas the other half had stable partner pairings. The treatments are depicted in the four cells of Figure A (details follow).

	No Meetings Prompted	Meetings Prompted
No Pair-Incentives	*Internal Control* Group	*Structured-Meetings* Treatment
Pair-Incentives	*Pair-Incentives* Treatment	*Combined* Treatment

Figure A: Treatment Assignment Matrix for Agents in Active-Treatment Eligible Locations

[23]The *Pair-Incentives* treatment resulted in a larger sales response with respect to incentives than the response found in our prior work with the firm in Sandvik et al. (2018). We discuss the relationship between these estimates in Section 7.4.

[24]The RCT registry number is AEARCTR-0002332. The IRB at the University of Utah approved the design with approval number IRB 00098156.

4.1 Structured-Meetings Treatment

Pairs in the *Structured-Meetings* treatment were encouraged to complete the following tasks: (1) fill out an individual self-reflective worksheet to prompt discussion prior to meeting with their partner;[25] (2) converse with their partner and record their partner's self-reflective responses on the backside of their own worksheet; and (3) turn in these worksheets to management by Tuesday each week. Completion of these tasks was optional but motivated by the receipt of a free catered lunch on Wednesday or Thursday of the same week (both agents had to be present to receive lunch). During this lunch, agents were provided with high-end, local sandwiches[26] and were asked to discuss several talking-points related to the worksheets while they ate with their partner.[27]

It is important to note that while the *Structured-Meetings* treatment was largely self-guided, agents did have some direction to focus conversations. The distinction is important because relative to the *Pair-Incentives* treatment discussed below, the *Structured-Meetings* treatment required non-trivial, managerial attention; e.g., creating the worksheets, monitoring worksheet completion, answering questions related to the process, coordinating and administrating lunch, etc.

4.2 Pair-Incentives Treatment

Agents in the *Pair-Incentives* treatment were given an incentive valued at approximately \$50 per person to increase their joint production. Agent pairs' probability of achieving the incentive was a function of their joint percentage increase in RPC relative to a pre-treatment baseline. This made sure agents were not disadvantaged by being randomly paired with a less productive sales agent. Further details are provided in Sections 4.5 and 4.7.

[25] Example question: "Think about the least successful sales call you've had in the last week. How could you have done better?" These worksheets were (*ex-ante*) viewed as a necessary step to make the ensuing conversations more salient; points of emphasis on the worksheets were sourced from the firm's leadership to maximize the expected gains from focusing agents' attention. Documentation of this worksheet can be found in Appendix B.

[26] Price for the sandwiches was about \$7.

[27] Documentation of these talking-points can also be found in Appendix B.

To operationalize this incentive, we followed previous literature in distributing the bonus. To ensure pairs knew the bonus criteria was not too strict or too lax, we told pairs they would be grouped with 2 other pairs and the pair with the highest percent increase in joint RPC would win. To buffer against agent pairs giving up, we did not tell pairs who they were paired with until a random drawing at the end of the work week. We created new groups of pairs each week. We test for and find no evidence that agent pairs gave up or that losing in one week had a negative impact in subsequent weeks. To increase the salience of the incentive, we followed the suggestion of management and used prizes such as golf vouchers, on-site massages, and tickets to other extra-curricular activities. These prizes had the advantage of immediacy—allowing us to deliver the prizes every week instead of waiting for the bonus in a pay check. In surveys, agents reported an average valuation for prizes of $40, which equates to an 18% (22%) increase in weekly commission pay for the average (median) agent. The expected prize values were equal to over 8% of the median agent's take-home compensation. Far weaker group incentives have been found to generate meaningful productivity increases, albeit in a setting with complementarity among workers **?**.

While agents in the *Pair-incentives* treatment were not explicitly encouraged to transfer knowledge with their partners, they were free (and able) to do so.

4.3 Combined Treatment

Pairs in the *Combined* treatment were given both the *Structured-Meetings* and *Pair-Incentives* treatments exactly as they were administered separately.[28]

4.4 Control Groups

Agents in the *Internal Control* group learned they were paired with a partner and had their joint change in productivity score published publicly (as all pairs did), but they did not receive: worksheets, guided directions to meet, nor any other monetary incentives. Note,

[28]Their bonus eligibility was only defined based on other groups in the *Combined* Treatment.

however, that this group is an *Internal Control* where agents have knowledge of a pairing but no formal incentives to act on that pairing. When designing the experiment, we expected rank incentives to be minimal, but the design does, in principle, allow us to test for the effect of rank incentives.[29] The *External Control* group, or hold-out group, allows a comparison of each of these cells relative to agents with no pairings, knowledge of the experiment, nor prompts around collaboration. In practice, trends in the *Internal Control* group tracked trends in the *External Control* group throughout the 12-week experimental period.

4.5 Allocation of Agents to Treatments and Implementation Procedures

The experiment is a clustered design, with agents allocated into one of the four cells in Figure A based on the identity of their sales manager. Agents were then randomly paired together so long as they worked in the same location, in the same division, had overlapping schedules, and were in groups that were designated to the same treatment. Pairings were assigned and publicly announced at the beginning of each week.

All three active interventions and the *Internal Control* group shared four common traits. First, all agents were randomly paired with a single partner within the same location, division, and treatment. Second, pairs were randomly designated as stable-pairs or rotating-pairs such that roughly half of all agents had a single partner throughout the entire four-week intervention, and the other half were randomly paired with a new partner each week (repeat assignments permitted).[30] Third, all pairs had their joint performance scores published daily

[29] While other studies have found that the introduction of public rank data (sometimes called rank incentives) may cause deviations from prior productivity (e.g. Bandiera et al. (2013)), rank incentives for individual agents were already present at this firm because commission rates partially depend on relative—albeit private—comparisons of agents. According to the contingency results in Blader et al. (2016), rank displays were in-line with prior practices and therefore may have had minimal effects relative to baseline.

[30] As new agents entered the sample (e.g., newly trained agents or agents moving in from other divisions), they too were randomly paired with a coworker and the pair was randomly designated to remain unchanged or to be repaired in subsequent weeks. Some pairs were dissolved as one or both agents left the sample (e.g., termination of employment, moved to a different division, took a leave of absence, etc.); the partners of these departing agents were paired with a new, randomly chosen, partner.

on TV monitors and on their internal messaging platform.[31] This joint performance score normalized the *change* in the pair's average revenue-per-call (RPC) relative to their RPC in the four weeks immediately preceding the pre-treatment period, allowing for inter-divisional comparisons.[32] Finally, all agents in these four cells were notified that their, and their partners', individual productivity was being shared with a team of university researchers.

Figure B, located at the end of the section, illustrates the allocation of agents to treatments and provides descriptions of the treatments. Table 1 describes the demographic makeup and pre-treatment performance of the participating sales agents. All treatment group characteristics and performance averages in the pre-treatment period are *not* statistically different from one another in these four cells (see the P-values column).[33] The agents in the three active treatment groups appear to be quite homogeneous with each other and to those agents in the *Internal Control* group. Agents in the *External Control* group are, on average, less productive. However, we test for, and find, common pre-period trends in productivity between the agents assigned to different treatment arms and agents in the *External Control* group, giving rise to a difference-in-differences estimator.

Appendix section A.1 describes communication of the experiment and further implementation procedures.

[31] A benefit of publicizing scores on TV monitors is that it kept agents aware of the interventions. However, with dynamic feedback about performance, it is possible some incentivized agents might quit or give up (Brown, 2011). As a result, we randomly assigned three pairs to each competition at the end of each week, meaning all but the bottom two pairs had a chance to win. In addition, agents' individual commissions on each sale limited the attraction of completely minimizing effort. In later analyses we find minimal evidence that agents changed their effort based on past performance.

[32] Management advised us to avoid displaying negative scores, hence scores were normalized around 100, where 100 reflected pre-treatment productivity levels. Employees who joined the sales floor during the treatment period were tagged with the median pre-treatment RPC.

[33] P-values of randomization tests of mean differences in the *Internal Control* and active treatment columns are reported. These tests are computed as the joint-hypothesis test of equality of treatment groups from a regression of the variable of interest on treatment assignment dummies after clustering standard errors based on manager identity (the level of assignment).

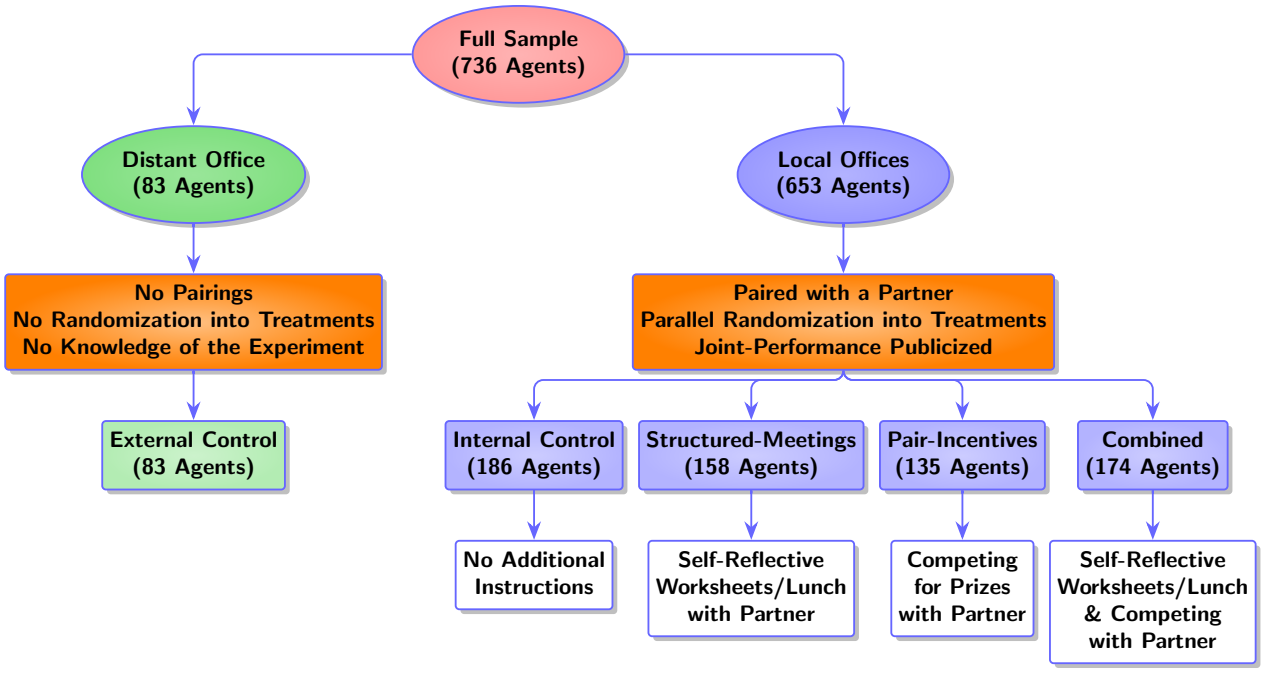

Figure B: Allocation of Agents to Treatments and Treatment Descriptions

5 Theory Development

We provide a parsimonious agency model to capture the nuances of our study firm and to isolate predictions. Suppose agent i has specific human capital $\theta_i \geq 0$. Agents are expected to learn from one another via synchronous interaction and communication, both of which require discrete effort. Accordingly, the agents in our experiment must decide whether to exert positive learning effort $l_i = L > 0$, or whether to abstain from learning altogether $l = 0$; i.e., $l \in \{0, L\}$. In addition to learning effort, the agent must also decide how much sales effort, $e \in \mathcal{R}^+$ to exert. In our context, sales effort captures how attentively agents focus on their callers' needs, how much mental effort they allocate to anticipating and responding to their callers' concerns, and how well they represent the offerings available–all of which can be adjusted throughout the period.

Agent i's learning effort l_i maps to human capital changes via the gain function $\gamma(l_i; \theta_i, \theta_j)$, which depends on i's human capital and the human capital of his partner, j. The function γ captures an agents' human capital gains and is equal to 0 if $\theta_j < \overline{\theta}$ where $\overline{\theta}$ is the average

level of human capital across all agents.[34] That is, we assume agents can only learn from those who are sufficiently productive; e.g. those above the productivity mean. On the other hand, if the agent being asked for help, j is sufficiently productive such that $\theta_j > \overline{\theta}$, then $\gamma(l_i; \theta_i, \theta_j) = l_i$ with probability p, and is 0 otherwise. In particular, even when an agent solicits help from a highly productive agent and directs positive effort towards mastering his more productive colleagues' learnings, the agent's productivity will remain constant with probability $1 - p$–and the agent won't know the observe the realization until the end of the period; i.e. after they have selected their level of sales effort. The change in the human capital stock is then given by: $\theta_{i,t} = \theta_{i,t-1} + \gamma(l_i; \theta_i, \theta_j)$.

We assume further that an agent's extant type is common knowledge, therefore the parameter p identically captures either the rate at which agents make productivity-enhancing discoveries and consequently the level of *disparate* knowledge across all agents, or simply the portability knowledge across agents. Turning to the agents' costs, the agent's dis-utility from effort is given by:

$$c(e, l) = \begin{cases} \frac{e^2}{2} w + W & \text{if } l = L \\ \frac{e^2}{2} & \text{if } l = 0 \end{cases}$$

where $w > 1$ is a marginal tax on sales effort which captures the trade-off faced between costly learning (e.g. experimenting with a new way of working) and (known) productive effort, whereas the fixed initiation cost, W, was explained in the introduction. We model agent i's period t revenue as $Y_{i,t} = e_{i,t}\theta_{i,t} + \varepsilon_{i,t}$ where ε is idiosyncratic noise.

In exchange for generating a revenue of $Y_{i,t}$ in period t, agent i anticipates a commission rate $B_t > 0$ in addition to a minimal hourly wage, which we aggregate into a periodic salary, $\alpha > 0$. To simplify what follows, we only consider learning during the treatment period, albeit the benefits to learning clearly extend into future (un-modeled) periods as well.[35]

[34] Our qualitative results are unchanged if instead, γ is a function of the agents' relative levels of human capital.

[35] The omission of future periods is made without loss of generality, as any future benefit obtained in the

We define the treatment period as period t, all earlier periods are labeled *pre-treatment*, and all subsequent periods are labeled *post-treatment*. Finally, we assume that the pre-treatment and the post-treatment periods share the same incentives (and learning costs); i.e., $B_{t-\tau} = B_{t+\tau}$ and $W_{t-\tau} = W_{t+\tau}$ with $\tau \in \mathcal{N}$. These assumptions are consistent with our empirical setting as only the greenest agents demonstrate a positive sales drift during the pre-treatment periods (which the firm returned to in the post-treatment periods).

When agent i is paired with agent j where $\theta_j > \overline{\theta}$ *and* agent i exerts learning effort $l_i = L$, his period t optimization problem becomes:

$$\max_e B_t(\theta_{i,t-1} + pl_i)e_i - \frac{e_i^2}{2},$$

and the optimal choice of effort is given by: $e^*_{i,t,L} = B_t\theta_{i,t-1}(1+pL)/w$ yielding an expected payoff of $v(\theta_i, L) = \frac{B_t^2\theta_i^2(1+pL)^2}{2w} - W$. The same analysis reveals that an agent exerting $l_i = 0$, will subsequently exert effort $e^*_{i,t,0} = B_t\theta_{i,t-1}$ for an expected payoff of $v(\theta_i, 0) = \frac{Lp(2+pL)B_t^2\theta^2}{2}$. Differencing the intermediate value functions yields the agent's individual rationality constraint to engage in learning:

$$v(\theta_i, L) - v(\theta_i, 0) = \frac{B_t^2\theta_{i,t-1}^2\left((Lp+1)^2 - w\right)}{2w} - W. \tag{1}$$

Mapping treatments to the model, the *Pair-Incentives* treatment operates through an increased commission rate, $B'_t = B_t + \Delta B$.[36] Turning to the other active treatment, the *Structured-Meetings* treatment facilitates interactions and hence lowers initiation costs, $W' = W - \Delta W$, whereas the *Combined Treatment* imposes both changes.

Proposition 1. *The Pair-Incentives treatment always results in larger sales revenue, albeit the larger incentives may be insufficient for the agent to engage in learning effort.*

repeated single-effort setting can be folded into the gain in human capital, L.

[36]This follows Bandiera et al. (2013), who make the same reduced form assumption to capture group tournament incentives. It is also possible that the *Pair-Incentives* treatment changes p, the probability of getting help, but we focus on B_t to keep the analysis simple.

If an agent facing B'_t finds it optimal to engage in learning, then, it must be the case that $(1+Lp)^2 > w$, or equivalently, $B'^2_t\theta^2_{i,t-1}\frac{(1+Lp)^2}{w} > B'^2_t\theta^2_{i,t-1} > B^2_t\theta^2_{i,t-1}$ where the first expression is the agent's effort when he exerts learning effort in the *Paired Incentives* treatment, the second is the same agent's effort were he not to engage in learning, and the last expression is his sales effort in the pre-treatment period. Given the complementary nature of human capital and incentives, the marginal incentives do make learning more attractive to the agent, albeit the (temporarily) increased wages need not be sufficient to overcome the fixed initiation costs, W.

Whereas the *Paired Incentives* treatment indirectly motivates learning with the prospect of temporarily elevated earnings, the *Structured Meetings* treatment targets the extensive margin frictions that impede learning.

Proposition 2. *If the Structured Meetings treatment increases treatment-period sales revenue, any such gains will be persistent.*

The only channel through which the *Structured Meetings* treatment benefits agents is via learning, therefore we assign any associated effects as the consequence of learning. Akin to Proposition 1, the agent only engages in learning if it results in greater treatment-period revenues. Unlike the *Pair Incentives* setting, the agent will not alter his sales effort in the absence of learning. The nature of the *Combined* treatment implies that whatever incentives are present for the agent to exert learning effort under the *Paired Incentives* or *Structured Meetings* treatments, the incentives in the *Combined* treatment are larger. If the agent already exerts positive learning effort in the *Paired Incentives* treatment, than we would anticipate no marginal revenue gains under the *Combined* treatment relative to revenues in the *Paired Incentives* treatment. On the other hand, if the agent exerts no learning effort under the *Structured Meetings* treatment, then our prediction for the *Combined* treatment is identical to that of the *Structured Meetings* treatment from Proposition 2.

The model highlights the potential mechanisms that the *Pair-Incentives*, *Structured-Meetings*, and *Combined* treatments work through. We test empirically for relative magni-

tudes of these effects and evidence of learning.

6 Results

We first present the results by comparing average productivity across treatment groups. Subsequent analyses perform a difference-in-differences estimation with controls including time and manager group fixed effects. These estimates reinforce the findings obtained using simple, unconditional means. We focus our analysis on the firm's main performance measure, revenue-per-call (RPC), which is comparable across agents facing different demand conditions (call frequency).[37] We later evaluate alternate measures including revenue-per-hour (RPH), revenue-per-agent-week (Revenue), and agent turnover, finding qualitatively similar results.[38]

Figure 2 presents a clear treatment effect (later regression-based analyses present standard errors confirming that differences are precisely estimated).[39] During the treatment period, performance is greater in all three treatment groups relative to the control groups. Beginning with a pre-treatment baseline of \$61, RPC climbed by \$11 (17%) for the *Pair-Incentives* treatment, whereas the *Structured-Meetings* and *Combined* treatments yielded a revenue increase of \$15 (23%). Performance stayed relatively constant for the control group.

We then examine possible mechanisms to explain the observed treatment effects. Specifically, we search for evidence of peer-to-peer knowledge exchange by asking whether the treatment effects extend beyond the treatment window. Figure 3 provides graphical evidence of persistent treatment effects by plotting the weekly mean RPC by treatment group (weeks -7 through 8).[40] Each active treatment group has average sales increases during the

[37]The RPC measure also aligns revenue with per-call customer acquisition costs used internally for accounting purposes. The firm did not change their customer acquisition strategy at any point throughout the entire 12-week data collection window, therefore relative RPC increases among treated agents represents incremental profits net of treatment costs (discussed below).

[38]Revenue-per-hour aligns revenue with the labor cost of staffing agents, while total revenue accounts for changes in hours worked or hours off the phone.

[39]Figure 2 displays RPC (normalized to the grand-mean in the pre-treatment period) by treatment group in the 4-week pre-treatment period and the 4-week treatment period. The normalization is for consistency between Figure 2 and Figure 3. Table 1 presents the non-normalized, average log RPC for each group in the pre-treatment period.

[40]Every group in the figure has productivity normalized in Week 0 (prior to the experimental period).

experimental period. Positive effects are present in all three treatments in week 1 and increase for the *Structured-Meetings* and *Combined* groups throughout the treatment period, while the effect dissipates in the pair-incentives group. After the experiment, RPC remains substantially higher than the control for agents in the *Structured Meetings* and *Combined* treatments, whereas it quickly collapsed to the control for the *Pair-Incentives* group.[41] Combined, our results suggest that the productivity gains observed during treatment may have been attained through different channels across the treatments. In particular, the persistent gains observed in both the *Structured Meetings* and *Combined* treatments suggest that agents increased their human capital, whereas those in the *Pair-Incentives* treatment may have simply exerted additional sales effort.

We further corroborate knowledge exchange by examining the interaction of both treatment and post-treatment productivity gains with the identity of one's partner–=in line with our model. Specifically, we define a partner as a star if their performance is above median in the pre-treatment period. Recall that the model predicts that agents can only learn from partners with $\theta_j > \overline{\theta}$. Unsurprisingly, Figure 4 presents overwhelming evidence that the treatment effect in both the *Structured Meetings* and *Combined* treatments is largely determined by whether or not one's partner is a star; there is no such star effect for the *Pair-Incentives* treatment. Figure 5 further shows that the post-treatment effects bifurcate on the basis of whether or not one's partner is a star (or, in the post period, whether the agent was ever paired with a star partner). Put simply, our preliminary analysis suggests that agents in the *Structured Meetings* and *Combined* treatments exchanged information, whereas the gains observed in the *Pair-Incentives* treatment was temporal and thus unlikely to be the product of learning.

[41] Figure 3 also demonstrates parallel productivity trends before the experiment, enabling subsequent analysis with a difference-in-differences estimator. The figure also highlights that neither the *Internal* nor *External Control* groups reacted to the onset of the active treatments.

6.1 Productivity During the Treatment

Table 2 presents analysis of percentage changes in RPC and formal statistical tests of differences between treatments.[42] The sample contains the four weeks of data prior in the pre-treatment period and the four weeks of data concurrent with treatments. The estimating equation is:

$$\begin{aligned}\log(\text{RPC})_{i,t} =& \beta_0 + \beta_1 \text{Structured-Meetings}_i \text{ x Treatment-Period}_t \\ &+ \beta_2 \text{Pair-Incentives}_i \text{ x Treatment-Period}_t \\ &+ \beta_3 \text{Combined}_i \text{ x Treatment-Period}_t + \lambda_t + \theta_g + \varepsilon_{i,t}\end{aligned} \tag{2}$$

where i represents an agent, t represents a week, g represents sales manager group, λ_t and θ_g are week and sales manager fixed effects, respectively, and $\varepsilon_{i,t}$ is an idiosyncratic error term. The variables *Structured-Meetings* x Treatment-Period, Pair-Incentives x Treatment-Period, and Combined x Treatment-Period are set to 1 during the experimental period for those randomly assigned to the treatment and to zero for all agents otherwise. The level effects of the treatment are subsumed by the sales manager fixed effects because all agents reporting to a sales manager are assigned to the same treatment.[43] Week fixed effects remove common time shocks that affect all treatments. Specifications in Columns (1) to (3) are relative to the *Internal Control*; Column (4) replaces the *Internal Control* group with the *External control* as the baseline; Column (5) includes the *Internal Control* and tests for changes in the *Internal Control* relative to the *External Control*.

Consistent with our average results, we observe large increases in productivity for agents in the active treatment groups. Table 2 indicates that agent-pairs provided with the worksheet exercise and catered lunch in the *Structured-Meetings* treatment increased performance

[42] Table A.2 reports similar results using log revenue-per-hour and total revenue per week as dependent variables. Across different measures, results are similar.

[43] The standard errors are clustered at the sales manager level to allow for arbitrary correlation within a sales manager group due to the clustered assignment rule. Standard errors are clustered at the sales manager group level in all subsequent specifications and, for brevity, we do not continue to reiterate this in the text.

by about 24% relative to either control group. Agent pairs who were in the *Pair-Incentives* treatment increased performance by about 13 - 14%. The difference between the productivity gains in the *Structured-Meetings* and *Pair-Incentives* treatments is statistically significant, as indicated by the p-value from the Wald test of equality in the penultimate row. As expected in a randomized experiment, the addition of agent or partner demographic controls across these columns minimally changes the estimates. The results are invariant to the choice of control group.

An important result from Column (5) is that the *Internal Control* group (same locations as the active treatments and partnered with their joint productivity score shared publicly) performed similarly to the (off-site) *External Control* group that was unaware of the experiment. This suggests that the productivity increases observed in the active treatments are not due to Hawthorne effects nor are the results driven by the publication of all treated agent's paired productivity score and relative rank. The difference between the *Internal Control* and the three active treatments shows that merely displaying ranks was not sufficient to improve performance, as agents in the *Internal Control* were aware of the experiment but had no changes in productivity.

Estimates obtained during the treatment period underscore the value of guiding agent interactions as opposed to simply providing incentives for agents to interact. This follows from the improvement in productivity with the *Structured-Meetings* treatment being larger than the increase observed in the *Pair-Incentives* treatment that lacked managerial direction. The relative cost of these two treatments combined with the productivity gains is decision relevant for firm profits. The *Structured-Meetings* treatment encouraged agents to fill out the worksheet in exchange for a \$7 catered lunch (\$14 total). In comparison, the *Pair-Incentives* treatment relied on prizes costing approximately \$50 (valued by the agents at approximately \$40), which were awarded to a third of the participating agents. The cost per six agents was \$42 in the *Structured-Meetings* treatment and \$100 in the *Pair-Incentives* treatment. Nonetheless, the *Structured-Meetings* treatment induced productivity gains that

were 10 percentage points higher than the *Pair-Incentives* treatment while costing only 40% as much.[44]

The differences in productivity gains across the active treatments provide several insights. First, the performance increase among agents in the *Pairs-Incentives* treatment is consistent with previous findings on the effectiveness of group-based incentives to increase effort exertion and productivity (Friebel et al., 2017; Bandiera et al., 2013). Second, the *Structured-Meetings* intervention, meant to test the effectiveness of a management-directed HRM process (Bloom and Van Reenen, 2011), resulted in larger productivity increases than the *Pairs-Incentives* treatment alone. Third, the treatment effect estimates are not due to differential turnover (discussed in Section 7.2.2) and are robust to the inclusion of agent fixed effects (Table A.2).

6.2 Persistence of Productivity Gains in the Post-Treatment Period

Figure 3 shows that the treatment effect for the *Pair-Incentives* treatment treatment collapses once the treatment ends, yet the other treatment groups see persistent results. As shown in our model, persistent gains are indicative of learning or knowledge transfer. Turning to the formal statistical results, we examine the persistence of the observed productivity gains using data from the 4-week pre-treatment period and the 4-week post-treatment period.We restrict attention to the four weeks following treatment rather than a longer time series because a reorganization was initiated six weeks after the treatment ended. This allows us some buffer between the end of the treatment and the re-organization. The reorganization involved reassigning agents to managers with some agents reassigned across divisions (i.e. selling different products and services). Part of the re-assignment appears to have been

[44] A different back-of-the-envelope calculation asks what the *Pair-Incentives* would have to be to induce the same productivity increase as the *Structured-Meetings* group if the treatment effect was linear. Accordingly, if a \$50 incentive resulted in a 14% increase in RPC relative to the control group, then to achieve the same 25% increase realized by agents in the *Structured-Meetings* treatment, the incentives would have to be approximately \$86 $\approx (.24 \times \$50)/.14$ per agent. We postpone a full return-on-investment discussion to section 7.5.

based on lagged productivity. The estimating equation is:

$$\begin{aligned} \log(\text{RPC})_{i,t} \;=\; & \beta_0 + \beta_1 \text{Structured-Meetings}_i \text{ x Post-Period}_t \\ & + \beta_2 \text{Pair-Incentives}_i \text{ x Post-Period}_t + \beta_3 \text{Combined}_i \text{ x Post-Period}_t \\ & + \lambda_t + \theta_g + \varepsilon_{i,t} \end{aligned} \tag{3}$$

where each of the treatment indicators is equal to one in the post-treatment period for agents originally assigned to that treatment cell. The specification mirrors that of Table 2.

The results, reported in Table 3, are consistent with Figure 3. Specifically, agents in the *Structured-Meetings* treatment continue to have productivity gains in excess of 20% higher than agents in either control group after the study. This increase is precisely estimated. In contrast, agents in the *Pair-Incentives* treatment group have changes in post-study productivity that are indistinguishable from either control group. Finally, the *Combined* treatment had positive gains of about 24% after the treatment ended. Although the *Combined* group appears to offer the most persistence, we cannot reject equality of persistence between the *Structured-Meetings* and the *Combined* treatments.

Combined, Table 3 estimates suggest that the *Pair-Incentives* treatment yielded increased effort, but no knowledge transfer. In the *Combined* group, once the firm provided agents with the *Structured-Meetings* framework, the marginal incentives *may* have reinforced learning. The difference between the *Pair-Incentives* treatment and the *Combined* treatment effects suggests that, without managerial guidance, effort was largely directed at exploiting extant human capital in pursuit of short-run gains, as opposed to being applied to peer-learning.

6.3 Partner Quality and Knowledge Transfer

To further delineate learning from alternative channels, we leverage random agent pairings with star partners to assess how heterogeneity in partner knowledge affects treatment gains. For this analysis, we create a binary classifier to sort agents based on their productivity in

the eight weeks preceding treatment (note that this is longer than the 4-week, pre-treatment benchmark used in the regression analysis but helps to guard against classification error). Agents are classified as "stars" if their productivity is above the median in the 8-week run-up to treatment. We estimate how being paired with a star partner affects productivity in the different treatments. We also assess whether there are differences based on an individual agent's own human capital, allowing us to recover the directional nature of who is gaining in a partner pairing.

The estimating equation is

$$\begin{aligned}\log(\text{RPC})_{i,t} \;=\; & \beta_0 + \beta_1 \text{Structured-Meetings}_i \text{ x } \text{T}_t + \beta_2 \text{Pair-Incentives}_i \text{ x } \text{T}_t \\ & + \beta_3 \text{Combined}_i \text{ x } \text{T}_t + \gamma_1 \text{Structured-Meetings}_i \text{ x } \text{T}_t \text{ x Star-Partner}_t \\ & + \gamma_2 \text{Pair-Incentives}_i \text{ x } \text{T}_t \text{ x Star-Partner}_t + \gamma_3 \text{Combined}_i \text{ x } \text{T}_t \text{ x Star-Partner}_t \\ & + \gamma_4 \text{T}_t \text{ x Star-Partner}_t + \gamma_5 \text{Ever-Star-Partner}_i + \lambda_t + \theta_g + \varepsilon_{i,t}\end{aligned} \tag{4}$$

where the variable T_t is interacted with treatments as a placeholder to indicate either the treatment period or the post-treatment period. The parameters of interest are γ_1, γ_2, and γ_3, comparing how star partners affect productivity in different treatments. The parameter γ_4 captures the baseline effect of having a star partner for agents in the *Internal Control* group. A further test of random assignment comes through γ_5, which is an indicator that the agent was ever paired with a star partner.

Table 4 shows that agents randomly paired with a star partner in the *Structured-Meetings* and *Combined* treatments were more productive in both the treatment and post-treatment period, confirming our hypothesis of knowledge transfer. Agents in the *Pair-Incentives* treatment did not respond deferentially to star partners, suggesting an absence of learning. In the first 3 columns, star partners are defined based on the concurrent identity of the partner; e.g. the *Star Partner* dummy variable is applied to agent-weeks. Column (1) is the baseline specification that includes the pre-experimental and treatment periods. While all

active treatments show increases in productivity for agents with non-star partners, agents paired with a star partner increase productivity by an additional 15% in the *Structured-Meetings* treatment and by an additional 14% in the *Combined* treatment.

Not all agents can be paired with star partners, but some assignment rules may fare better than others to realize gains. We ask whether the difference in being paired with a star partner depends on the identity of an agent. Columns (2) and (3) split the sample based on whether the agent is himself or herself a star. Comparing these columns, all treatments at baseline with a non-star partner are more effective for non-star agents (Column 2). These non-star agents have positive star partner interaction effects in treatments involving *Structured-Meetings*. Having a star partner does not raise productivity for non-star agents in the *Pairs-Incentives* treatment. When the agent in question is a star (Column 3), treatments only increase productivity when the partner is also a star. No treatment induces productivity gains when star agents are paired with non-star partners, though stars do not see a decrease in RPC during treatment, suggesting that any concerns over the opportunity cost of helping others may be misplaced. The direction of this result suggests that *star agents provide knowledge to non-stars, while stars only learn from other stars.* Among star agents, however, the *Pair-Incentives* treatment never has statistically distinguishable differences from zero.

Further evidence of knowledge transfer can be found in Column (4) which only examines the post-treatment period. In our post-treatment analysis, the star partner interaction equals 1 for an agent who was ever paired with a star partner. Past pairing with a star partner is responsible for most of the detectable productivity increases from *Structured-Meetings* in the post-experimental period. Taken together, these results further confirm knowledge transfer between more and less skilled agents when management practices dictate a process to minimize peer-interaction frictions.

6.4 Changes in the Distribution of Sales After the Experiment

The evidence on who benefits from treatments suggests that the *Structured-Meetings* and *Combined* treatments disproportionately affected agents at the bottom of the distribution. Figure 7 confirms larger gains for agents in the bottom of the distribution by plotting how quantiles of the sales distribution change for different treatments. To construct the figure, we compute log RPC for each treatment at 100 quantiles in the post-experimental period and the pre-experimental period. For each quantile, we then take the difference in post-experimental log RPC and pre-experimental log RPC. Quantiles 1 through 20 have substantial positive increases in sales through the post-experimental period in the *Structured-Meetings* and *Combined* treatments. Gains are smaller but remain positive across the remainder of the distribution, suggesting that more productive agents either benefited (or at least were not harmed) from these treatments. Quantiles are little changed across the distribution for the *Pair-Incentives* treatment or the *Internal Control*.

7 Discussion

Our primary results compare an incentive change and a management process intervention. While we find evidence suggesting that each approach improves performance via differing mechanisms, several questions remain. This section considers alternative explanations, provides evidence on the mechanism, and discusses to what extent our findings generalize.

7.1 Evidence on the Mechanism

7.1.1 Survey and Worksheet Responses

We observed many examples of specific knowledge being transferred between agents from the worksheets agents filled out in the *Structured-Meetings* treatment. For example, one agent shared with her partner that she was able to help customers overcome their initial skepticism by discussing two unique add-on features of the product. Another agent shared

that her customer wanted a very specific, but unavailable product mix. Using a tip learned from her partner, she was able to explain the problem and, instead, sell the customer a high-quality bundle of products. The worksheets conveyed job-specific knowledge sharing that could immediately be used by the agents' partners to interact with customers more effectively (recall that agents solicited their partner's responses in the treatments involving structured-meetings).

Why was the intervention required to realize these gains? First, agents perceive there are benefits from asking for help, and they themselves estimate positive treatment effects from doing so (see Figure A.1). Agents also know their relative standing compared to top agents (see Figure A.2) and 93% of survey respondents identify three agents in the top 10% of the sales distribution for their division and location.

We thus believe that initiation costs are likely responsible for the lack of observable learning prior to the experiment. Consistent with this interpretation, a sales agent in the *Structured-Meetings* treatment expressed her excitement to us when she learned that she had been paired with a very skilled coworker. Specifically, she said, "I would never have had the courage to approach him for help or advice. But since we are paired together for lunch, I get to learn from one of the best sales agents in the company!" As this anecdote is representative of numerous feedback received, it suggests active management was needed to reduce initiation costs. There is also some suggestive (but imprecisely estimated) evidence that agents who reported initiating fewer work related conversations in the pre-period had larger long-term gains from *Structured Meetings* (see Appendix Table A.6).

Consistent with coordination costs, further survey evidence shows that when asking peers for help, agents turn to those who sit nearby. 25% of survey respondents report that "When I ask other agents for help, I always (100% of the time) look for someone seated besides me." Another 36% of agents report that "When I ask other agents for help, I usually (greater than 75% of the time) look for someone seated beside me." This tendency to ask proximate coworkers for help suggests coordinating with non-proximate agents may limit conversations

or asking for help.

7.1.2 Partner Rotations

We find small, statistically insignificant, positive effects of weekly partner rotations on long-run sales. These results are contained in Appendix Table A.6. Given the effect sizes, we lack power to distinguish between the hypothesis that exposure to varied techniques is valuable and the hypothesis that repeated exposure helps with adoption.

7.2 Alternative Explanations

7.2.1 Did Treatments Detract from Ability to Take Calls?

There is no evidence to suggest that the treatments themselves left the agents with less time to accept calls. Anecdotally, the agents only allowed the treatments to impinge on their slack time at the firm (approximately 20% of their time "on the clock" and then other time waiting for calls). The data confirms that an increase in RPC did not mask a change in time spent selling. Appendix Table A.2 shows that the treatment effects are similar when looking at either log revenue-per-call or log revenue-per-hour. Appendix Table A.3 shows that there was no discernible difference in adherence, defined as the time available to take calls, between the *Internal Control* and other treatments. Appendix Table A.2 also shows that the active treatments increased total revenue in levels.[45] Importantly, while we have no evidence to suggest that our treatments reduced agents' time on the phones, any disruption due to treatments was more than offset by increases in sales revenue.

7.2.2 Are Turnover Differences Responsible for the Results?

To address the possibility that turnover differences drive the results, we add agent fixed effects to the baseline results of Table 2 in Table A.2. The similarity of estimates suggest

[45]The total revenue figure must be imputed for the *External Control* due to reporting differences across establishments.

that the productivity gains are due to within-worker changes rather than differential turnover of unproductive agents across the different treatments.

The propensity for agents of to turnover did not change for those in the *Structured-Meetings* or *Combined* treatments relative to the *Pair-Incentives*, or *Internal Control*. Panel A of Appendix Table A.4 shows no difference in turnover rates through the treatment period. Panel B of Appendix Table A.4 does show increases in turnover for agents in the *Internal Control* group relative to those in the *External Control* group, but these increases in turnover reflect seasonal differences in staffing across locations.[46]

7.2.3 Did Pair-Incentive Discourage Some Agents?

There is a possible trade-off between performance and quits or forfeitures in high-powered contests (Brown, 2011). We observed no evidence from managers or agents to suggest that agents in either the *Pair-Incentives* or *Combined* treatments became discouraged.[47] To test for the possibility of "quitting" in the data, we assess how agents' performance responds to past tournament outcomes. Winners of the tournament in the last week do slightly less well than non-winners in the prior week (see Table A.5). This might be due to mean reversion after a win or possibly to income effects. The excluded category is past tournament losers, so this evidence is inconsistent with agents "quitting" by means of reducing effort in response to tournament cues.

Another possibility is that some agents simply do not engage with the treatment. The *Pair-Incentives* treatment was designed to provide every agent pair with ex-ante equal opportunities to win by filtering pre-treatment productivity. That is, the contest rankings were based on gains relative to pre-treatment performance. It is possible that star performers find it more difficult to improve their own performance in the contest, but our pre-experiment

[46] In particular, locations with active treatments relied more heavily on college students and therefore featured higher natural attrition during the post-treatment period in the fall. There are, however, no differences in turnover between active treatments and the *Internal Control*.

[47] While agents were provided with daily updates of their pair's relative ranking, membership in each three-team contest was randomly determined ex-post.

discussions with management suggested this design would best encourage non-stars to compete. If a star found it difficult to improve on their own, a more productive path would be to help one's partner.

7.3 The Mix of Management Practices: Complements or Substitutes?

When practices are complements, giving agents both treatments together results in gains that are greater than the sum of the individual treatments. Substitute practices means the gains are less than the sum of the individual treatments. The last line in Table 2 presents the p-value from a Wald test of the null hypothesis that the *Combined* treatment effect is larger than the sum of the *Pair-Incentives* and *Structured-Meetings* treatments. The hypothesis that the *Pair-Incentives* and *Structured-Meetings* treatments are complements is rejected in the data, indicating the practices are substitutes.

The fact that *Pair-Incentives* did not reinforce the gains from the *Structured-Meetings* treatment during the experiment has implications for the mix of management practices. Incentives may be necessary to have agents "buy into" a new, non-mandatory process. However, incentives may distort other input choices.[48] Adding the relatively high-powered *Pair-Incentives* to the *Structured-Meetings* did produce marginally higher productivity than the *Structured-Meetings* treatment alone. Because we find the treatments are not complements, the optimal mix of practices requires an assessment of the marginal benefit and cost of the *Pair-Incentives* when the *Structured-Meetings* treatment is in effect. Evaluating the difference in productivity between the *Combined* treatment and the *Structured-Meetings* treatment at the point estimates, while recognizing the sampling variance associated with these estimates, the *Combined* treatment increased productivity by about 1% per call, which would have resulted in about $40.00 per week in additional revenue for each agent. Given

[48] In extreme cases, the "optimal incentive structure may require the elimination or muting of incentives... [to inspire] cooperation and coordination" (Holmstrom and Milgrom, 1994).

that the per-agent cost of the *Pair-Incentives* was about $17 per week, we cannot reject that the practices in the *Combined* treatment were optimal.[49]

7.4 Comparing Pair-Incentives and Estimates of Other Incentive Changes

In Sandvik et al. (2018), we analyze sales changes after one division of the firm reduced agents' effective incentive pay by altering the mapping between sales and commission-eligible revenue. The reduction occurred in the fall/winter of the prior year and was not concurrent with this experiment.

Sandvik et al. (2018) primarily find adjustment on the turnover margin while sales effort responses to commission reductions were muted. The elasticities of effort with respect to incentive changes thus varies across the two studies. What potentially explains the differences? First, the *Pair-Incentives* were transitory and avoided the income effects typically associated with permanent wage changes (Stafford, 2015). Second, Sandvik et al. (2018) focuses on the limited long-run response to the commission reduction, whereas the effort response to the *Pair-Incentives* treatment is short-term (and begins to fade out during the treatment period).[50] Hence, the parameters across the two papers capture different time frames for effort adjustment, with the long-run response including income effects that may reduce the substitution response to changes in incentives. Finally, the *Pair-Incentives* treatment may have had a social component that increased the salience of the group incentives relative to individual incentive changes in Sandvik et al. (2018).

[49]Despite the fact that the interventions are not complements during the treatment period, the gains in the post-treatment period are largest for the *Combined* treatment. However, we believe the right thought experiment to assess complementarity is the effect during the treatment period, as the presumption is that management would keep the practices in place.

[50]Examining the short-term responses in the Sandvik et al. (2018) data, we do observe a limited-term reduction in sales, though the design is insufficiently powered to rule out a sales increase.

7.5 The Firm's Return on Investment

The economic significance of our findings was apparent for the firm. The firm previously relied exclusively on marginal incentives aimed to influence agents' performance. Interviews with management suggest that, while these interventions generally led to increased productivity, all associated gains would evaporate as soon as the incentives were removed. Agent performance is a function of both sales-effort and human capital. The lack of persistent gains suggests that prior incentive changes merely triggered sales-effort. The experiment was designed from the outset to test the efficacy of various incentives at motivating peer-learning as a means of growing agents' human capital.

The *Structured-Meetings* treatment attempts to reduce the cost of knowledge transmission, whereas the *Combined* and *Pair-Incentives* treatment each contained a component of marginal incentives—albeit such incentives were perfectly aligned with a partner agent so as to incentivize agents to train one another for a common goal. Using the estimated treatment effects, we find that the performance improvement of the treated sales agents resulted in over $1M USD of extra-marginal revenue.

At the intervention level, we find that the *Structured-Meetings* treatment resulted in a return on investment (ROI) of over 530%, where the investment base was the cost of having catered meals delivered to the firms' campuses and the top-line value was the sales-margin multiplied by the extra revenue recorded exclusively during the 4-week treatment period (all ROI details can be found in Appendix A.3). We estimate the ROI from the *Pair-Incentives* treatment at approximately 61%, as the estimated profits therein outperformed the *Internal Control* group, but the associated incentives were more costly—at least double those of the *Structured-Meetings* treatment. Finally, the *Combined* treatment had an estimated ROI of nearly 130%. This intervention was the most costly, as both lunches and monetary incentives were provided, but these sales agents did outperform the control group by the largest margin, hence the relatively large ROI.

We stress that the reported ROI values do not impound the persistent, relative revenue

gains identified in the *Structured-Meetings* and *Combined* treatments. While doing-so would obviously cause the reported ROI to increase, the post-treatment observation window is too short to reliably forecast and discount back the gains observed in the post-treatment period.

Taken together, the increased productivity resulting from our relatively low-cost interventions made the randomized control trial a success with management. Pursuant to sharing our results, management has prioritized supporting future randomized control trials for organizational design.

8 Conclusion

In many workplaces, output varies dramatically across individuals. Modern-day managers are quick to credit workplace interactions as a driving force of employee productivity, but careful examination surfaces a host of economic questions. In particular, given the observed gains to peer-learning, which economic costs prevented agents from actively seeking help earlier? Two theorized frictions are fixed initiation costs and contracting difficulties. An extensive literature has studied team incentives to address contracting difficulties. An an adjacent literature in urban economics focuses on overcoming initiation costs in aggregate (Glaeser and Gottlieb, 2009; Glaeser et al., 1992) while a newer literature studies the (micro) social frictions which may burden those seeking help (Chandrasekhar et al., 2016).

Within firms, little evidence exists on the role of management practices to overcome frictions around knowledge sharing. We ran a field experiment that randomly paired more than 650 call center sales agents together and then assigned the pairs to different management practice treatments. One treatment, *Structured-Meetings*, targeted initiation costs by guiding randomly paired agents towards targeted, work-related conversations. A second treatment, *Pair-Incentives*, targeted contracting frictions by tying partners' expected earnings together. A third treatment, *Combined*, simultaneously addressed both frictions.

Although all treatments raised sales relative to the control groups, agents in the *Structured-*

Meetings treatment had persistent performance gains, while the performance gains from the *Pair-Incentives* treatment subsided at the end of the treatment period. A number of additional results suggest that the management-lead approach to breaking down initiation costs resulted in knowledge transfers from skilled agents to others.

These findings add to a small but growing set of studies showing that simple management interventions can dramatically raise productivity (Bloom and van Reenen, 2011; Bloom et al., 2015, 2013; Jackson and Schneider, 2015; Haynes et al., 2009).

Our randomized controlled trial took place in a sales organization featuring relatively autonomous roles. While this setting provides a nearly ideal environment for studying peer interactions, the impediments to peer-learning are likely much more general (Chandrasekhar et al., 2016). Moreover, the gains to peer-learning will likely exceed those reported here when job-roles involve non-zero collaboration. Many settings provide performance incentives and opportunities to interact with coworkers. A fruitful area of future research surrounds how to match workers for these purposes.

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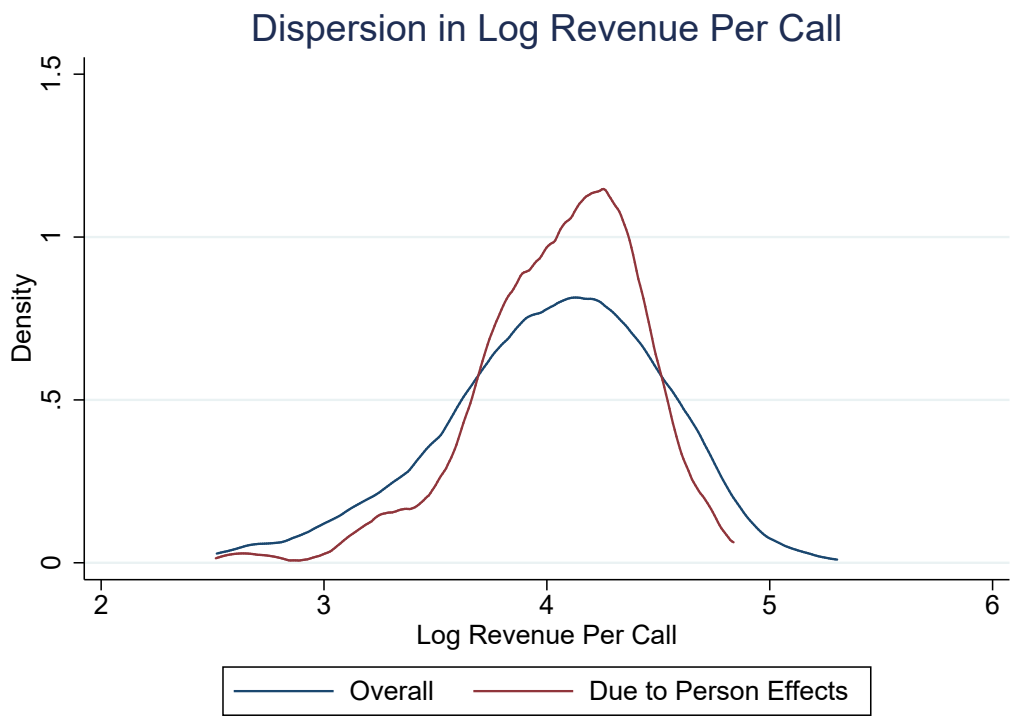
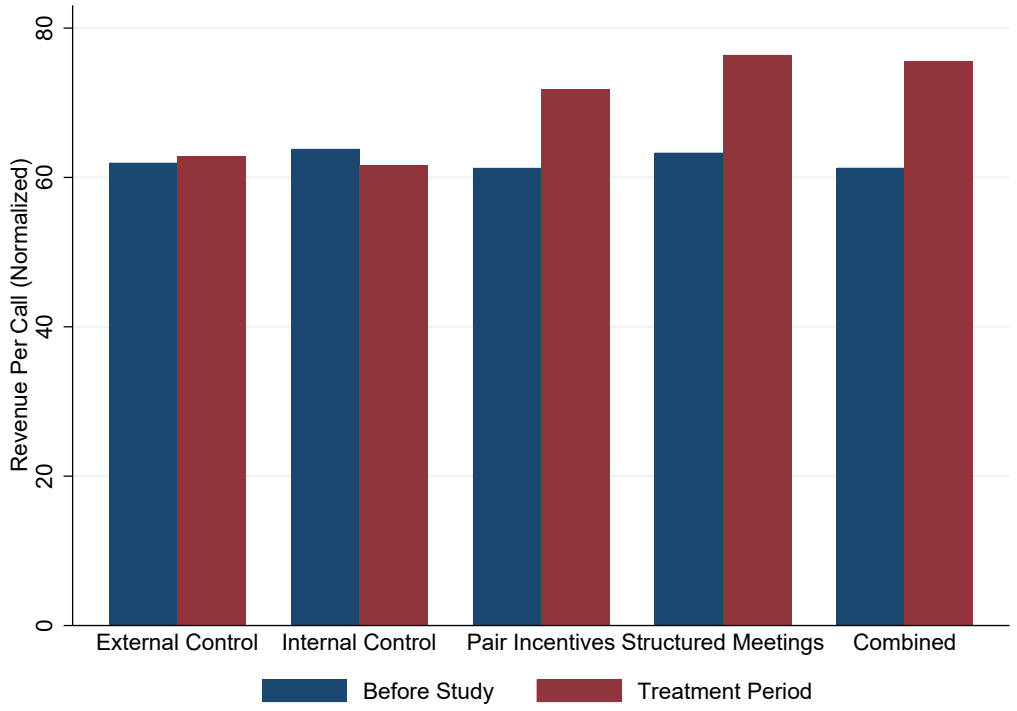
9 Tables and Figures

Figure 1: Dispersion in Residual log Revenue Per Call



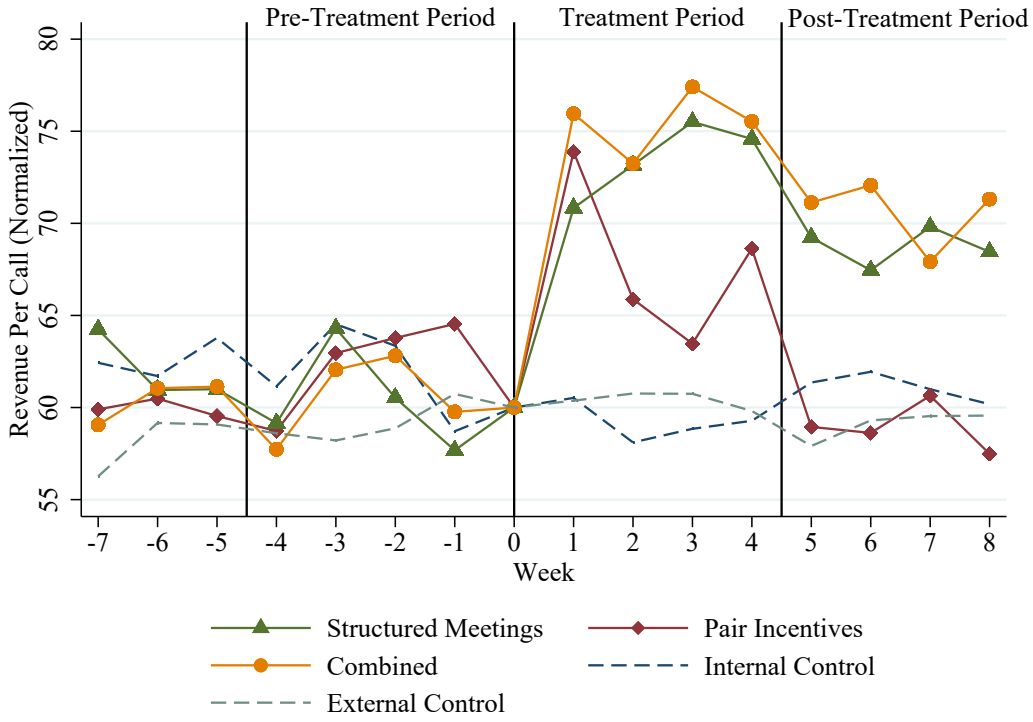
Notes. Figure displays overall residual log RPC and the component attributable to person fixed effects after applying the shrinkage procedure in Lazear et al. (2015). The residuals come from a regression using pre-experiment data and net out sales division, office location, and tenure with the firm. The interquartile range of residual log RPC attributable to person effects is 0.6.

Figure 2: Mean RPC by Treatment During the Pre-Study and Experimental Period



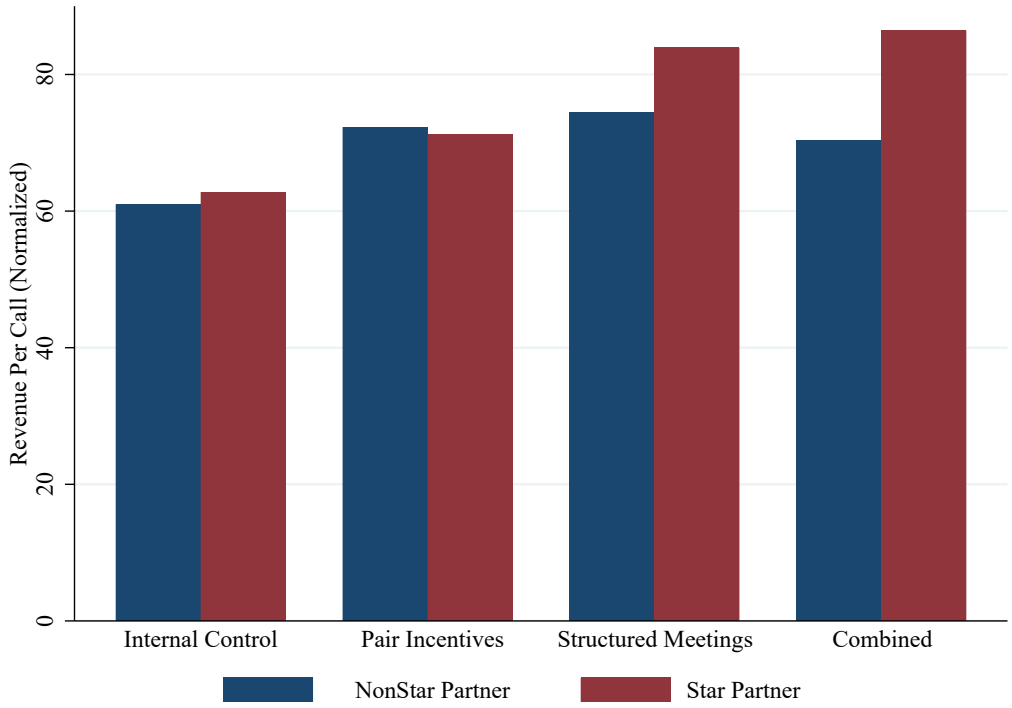
Notes. Average revenue-per-call (RPC) by treatment group. The bars for each treatment are normalized around the grand-mean of RPC in the pre-treatment period.

Figure 3: Treatment Effects by Week



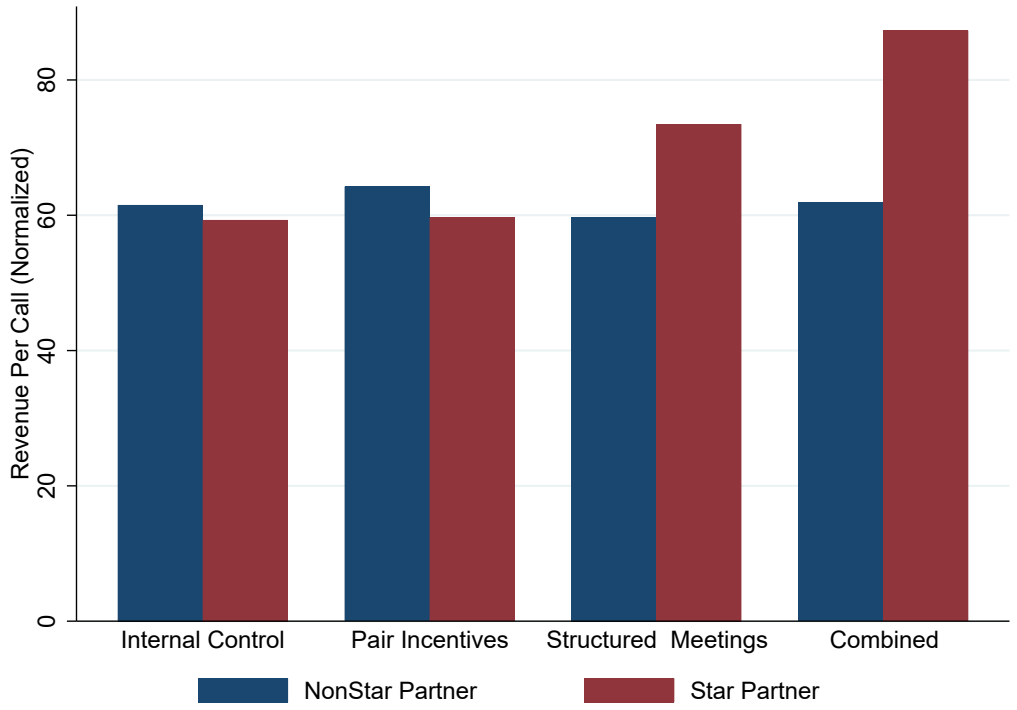
Notes. Average revenue-per-call (RPC) by week and treatment group. Each series is normalized to the grand-mean of RPC in week 0. The experimental intervention begins in week 1 and continues to week 4. After week 10, the firm began a post-summer re-organization that partially depended on lagged productivity, so we restrict analysis to the 8 weeks that follow the start of the experiment.

Figure 4: Mean Productivity During the Experimental Period by Star Partner Assignment



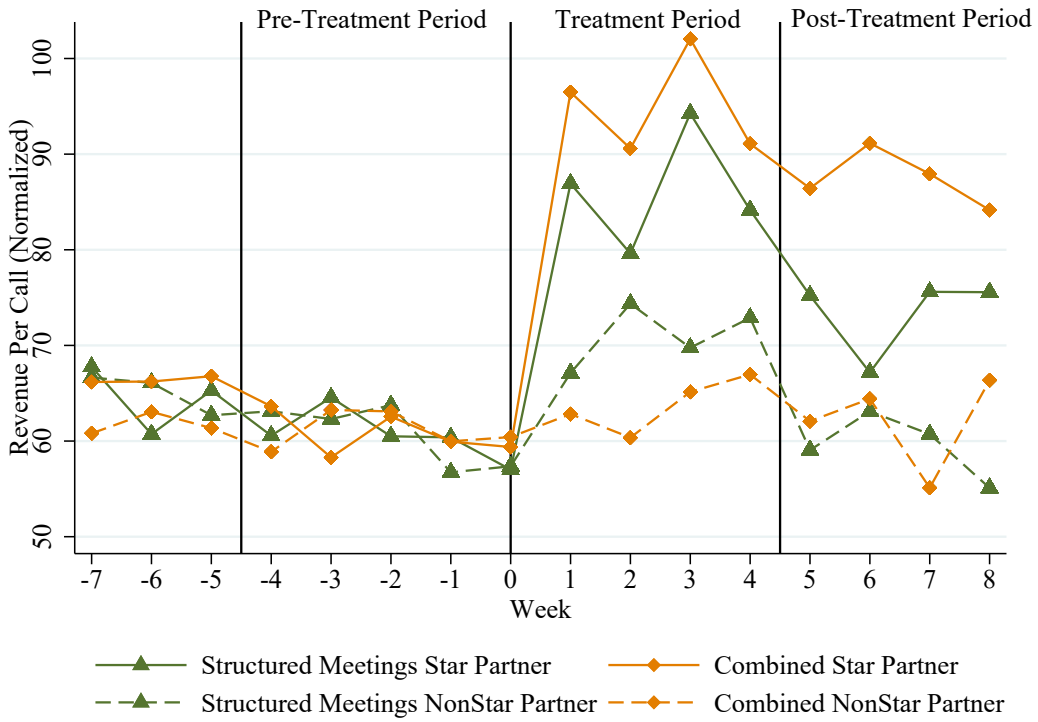
Notes. Average revenue-per-call (RPC) by treatment group during the treatment period based on whether the concurrent partner is a Star agent (defined as above median productivity in the pre-treatment period).

Figure 5: Mean Productivity During the Post-Experimental Period by Star Partner Assignment



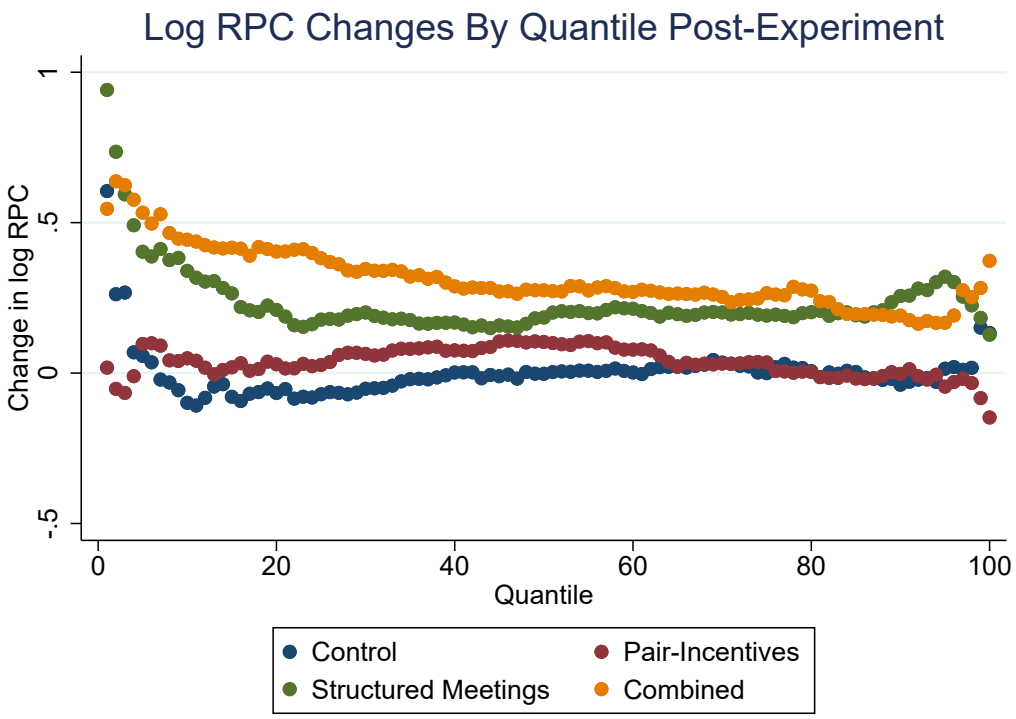
Notes. Average revenue-per-call (RPC) by treatment group during the post-treatment period based on whether the agent was ever partnered with a Star (defined as above median productivity in the pre-treatment period).

Figure 6: Treatment Effects by Week for Star and Non-Star Pairings



Notes: Average revenue-per-call (RPC) by week and treatment group. Agents are classified as stars if they are above median in productivity in the pre-period. Agent are classified as star partner or nonstar partner if they ever were paired with a star or never paired with a star, respectively. The experimental intervention begins in week 1 and continues to week 4. After week 10, the firm began a post-summer re-organization that partially depended on lagged productivity, so we restrict analysis to the 8 weeks that follow the start of the experiment.

Figure 7: Changes Across the Distribution of Sales Productivity in the Post-Treatment Periodthe Lower Tail of Sales Productivity



Notes: To construct this figure, we compute log RPC for 100 quantiles by treatment in the post-experimental period and the pre-experimental period. For each quantile, we then take the difference in post-experimental log RPC and pre-experimental log RPC. The figure plots these differences at each quantile by treatment.

Table 1: **Agent Demographics and Sales Prior to the Experiment**

Sales agent demographic information (age, tenure with the firm, and gender ratio) and weekly performance measures are listed below for the full sample of participating agents and broken out by treatment group. Except for attrition, these numbers are derived from a four-week average immediately before the experiment began. Other than age, tenure, percent female, and attrition, each productivity variable is a weekly measure. A *Phone Hour* is a measure of time an agent is logged into the phone system, and *Adherence* is calculated as the sum of an agent's time available to receive a call and his time on calls, all divided by the total time he is logged into the phone system. *Attrition* is measured as the number of agents that quit during the pre-experimental period, experimental period, and post-experimental period divided by the total number of agents in that treatment. P-values of randomization tests of mean differences in the *Internal Control* and active treatment columns are reported. These tests are computed as the joint-hypothesis test of equality of treatment groups from a regression of the variable of interest on treatment assignment dummies after clustering standard errors based on manager identity (the level of assignment).

	Full Sample	Structured Meetings	Pair Incentives	Combined	Internal Control	External Control	P-Value
Age (yrs.)							
Mean	26.08	25.76	26.61	26.43	25.14	27.19	0.62
Median	23.39	22.51	23.55	24.02	22.97	24.63	
Std Dev.	8.14	8.20	9.61	8.10	6.66	8.41	
Tenure (log days)							
Mean	5.25	5.14	5.38	5.59	5.18	4.67	0.61
Median	5.15	4.62	5.40	5.37	4.62	5.18	
Std Dev.	1.18	1.12	1.07	1.10	1.22	1.24	
Percent Female							
Mean	0.32	0.32	0.31	0.33	0.34	0.25	0.95
Revenue per Call (log)							
Mean	3.92	3.90	4.06	3.94	3.92	3.62	0.69
Median	3.97	4.04	4.09	3.99	3.99	3.69	
Std Dev.	0.49	0.52	0.37	0.47	0.55	0.33	
Revenue per Hour (log)							
Mean	4.51	4.48	4.69	4.56	4.51	4.11	0.54
Median	4.62	4.64	4.78	4.65	4.63	4.18	
Std Dev.	0.60	0.69	0.45	0.59	0.59	0.46	
Commission							
Mean	217.78	202.65	230.41	230.64	202.31		0.75
Median	185.45	168.42	192.28	209.73	169.73		
Std Dev.	155.61	159.99	156.09	157.73	147.70		
Total Calls							
Mean	61.53	57.56	64.16	65.81	58.89		0.33
Median	60.43	57.22	62.41	65.29	58.63		
Std Dev.	21.32	19.16	22.02	20.81	22.43		
Phone Hours							
Mean	32.61	32.52	33.76	33.17	31.22		0.32
Median	34.05	34.08	34.77	33.33	33.75		
Std Dev.	7.36	7.01	6.09	6.74	8.95		
Adherence							
Mean	0.80	0.80	0.84	0.79	0.77		0.19
Median	0.83	0.83	0.85	0.83	0.82		
Std Dev.	0.14	0.11	0.07	0.14	0.21		
Attrition							
Term Rate	0.22	0.22	0.18	0.24	0.24		0.19
N Agents	736	158	135	174	186	83	

Table 2: **Difference-in-differences Estimates of Performance Effects of the Treatments.**

This table reports regression estimates of the log of revenue-per-call using data from the 4 weeks before and 4 weeks during the experiment. The variables *Structured-Meetings*, *Pair-Incentives*, and *Combined* are shorthand for "Structured-Meetings x Treatment-Period," "Pair-Incentives x Treatment-Period," and "Combined x Treatment-Period" and are set to 1 in the experimental period for those randomly assigned to those treatments, and zero otherwise. Using the abbreviated variable names, the baseline estimating equation is:

$$\log(\text{RPC})_{i,t} = \beta_0 + \beta_1 \text{Structured-Meetings}_{i,t} + \beta_2 \text{Pair-Incentives}_{i,t} + \beta_3 \text{Combined}_{i,t} + \lambda_t + \theta_g + \varepsilon_{i,t}$$

where i represents an agent, t represents week, g represents sales manager group, λ_t and θ_g are week and sales manager fixed effects, and $\varepsilon_{i,t}$ is an idiosyncratic error term. Indicators for treatment assignment in the pre-period are absorbed by θ_g as randomization is at the sales manager group level. In Columns (1)–(3) the *Internal Control* (passive pairs) is the omitted category. Column (4) omits the *Internal Control* group and instead uses the *External Control* group (that was not aware of the experiment and had no partner pairing) as the excluded category. Column (5) includes both control groups, with an indicator for the *Internal Control* during the experimental period. Specifications with agent or partner demographics include age, gender, and tenure with the firm. The p-values in the bottom rows report results from Wald tests of two null hypotheses: i) equality of effects between *Pair-Incentives* and *Structured-Meetings* and ii) that the *Combined* group had supermodular gains. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

Control Group:	Internal (Passive Pairs)			External (No Pairs)	Both
	(1)	(2)	(3)	(4)	(5)
Structured-Meetings	0.241***	0.241***	0.241***	0.247***	0.244***
	(0.045)	(0.044)	(0.044)	(0.073)	(0.071)
Pair-Incentives	0.131***	0.136***	0.136***	0.124*	0.134*
	(0.048)	(0.047)	(0.047)	(0.071)	(0.071)
Combined	0.255***	0.258***	0.256***	0.259***	0.259***
	(0.043)	(0.044)	(0.044)	(0.069)	(0.069)
Internal Control					0.004
					(0.065)
Demographics		✓	✓	✓	✓
Partner Demo.			✓		
Manager FE (θ_g)	✓	✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓	✓
Adj. R-Square	0.470	0.482	0.483	0.459	0.476
Observations	3,418	3,418	3,418	2,856	3,821
Agents	653	653	653	580	736
Managers	52	52	52	45	58
P-Values:					
H_0: Meetings = Incent.	0.048	0.051	0.049	0.033	0.050
H_0: Meetings+Incent.≤Comb.	0.049	0.042	0.039	0.110	0.094

Table 3: **Do Some Treatments Result in Greater Persistence of Productivity Gains?**

This table reports regression estimates of the log of revenue-per-call using data from the 4 weeks before the experiment and 4 weeks after the end of the experiment. The variables *Structured-Meetings*, *Pair-Incentives*, and *Combined* are shorthand for "Structured-Meetings x Post-Period", "Pair-Incentives x Post-Period", and "Combined x Post-Period" are set to 1 in the post-experimental period for those randomly assigned to those treatments, and zero otherwise. The baseline estimating equation is the same as in Table 2 except the treatment period is omitted and the post-experimental period is included. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

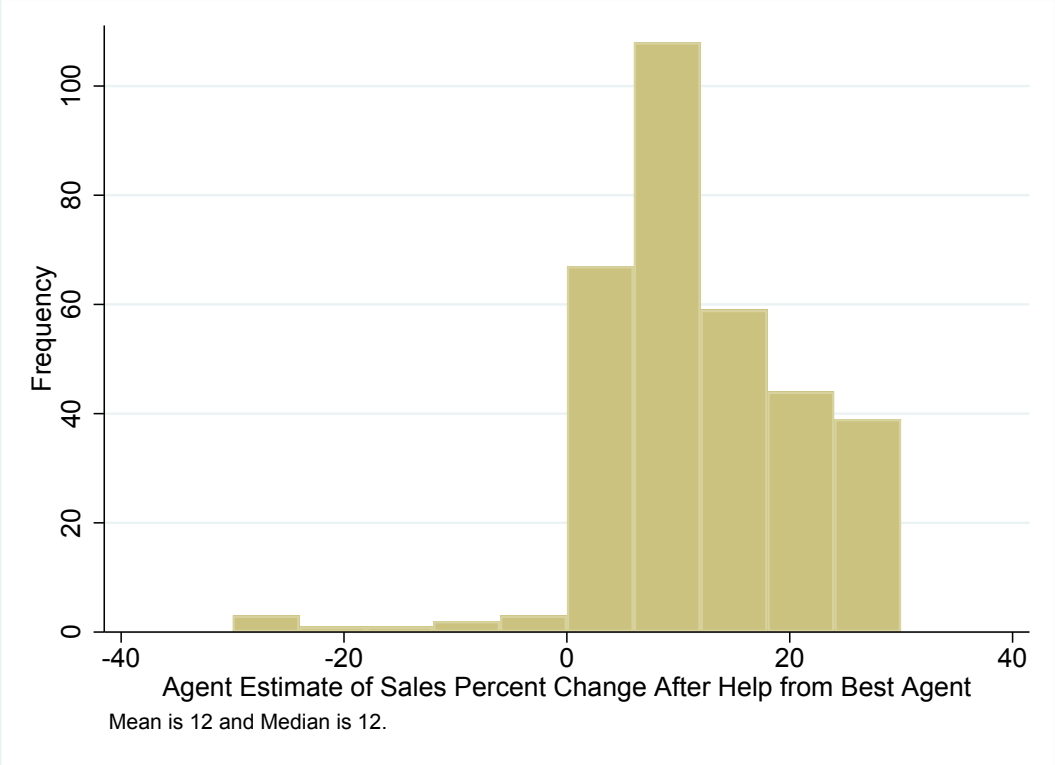
Control Group:	Internal (Passive Pairs)			External (No Pairs)	Both
	(1)	(2)	(3)	(4)	(5)
Structured-Meetings	0.208***	0.204***	0.204***	0.214***	0.214***
	(0.061)	(0.061)	(0.061)	(0.077)	(0.076)
Pair-Incentives	0.030	0.035	0.035	0.028	0.037
	(0.057)	(0.057)	(0.056)	(0.072)	(0.071)
Combined	0.242***	0.246***	0.244***	0.247***	0.247***
	(0.062)	(0.063)	(0.063)	(0.076)	(0.075)
Internal Control					0.005
					(0.070)
Demographics		✓	✓	✓	✓
Partner Demo.			✓		
Manager FE (θ_g)	✓	✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓	✓
Adj. R-Square	0.418	0.429	0.430	0.440	0.448
Observations	3,252	3,252	3,252	3,295	3,627

Table 4: **How do Partner and Agent Baseline Productivity Influence Treatment Outcomes?**

This table examines the effect of star partners on treatment outcomes. An agent is defined as a star if they are an above median performer within their own division 0 to 8 weeks before the study. The *Internal Control* group is the excluded category. The table reports regressions of $log(RPC)$ that add interactions for treatment assignment and being paired with a star partner. Columns (1)—(3) include the pre-period and experimental period while Column (4) includes the pre-period and post-treatment period. During the treatment, star partner assignment is defined based on the concurrent partner. For the post-experimental period, *Star Partner* is defined based on whether the agent was ever paired with a star. The variable "Star Partner x Study/Post" is set to 1 if the agent is paired with a star partner. The variable *Ever Star Partner* is set to one during the entire sample if the agent was ever paired with a star, serving as a test of non-random partner assignment. Columns (2) and (3) condition on the productivity of the agent in question and examine heterogeneous effects of star partnership by agent productivity. Each regression includes time fixed effects and sales manager group fixed effects. Note that observation counts differ due to agents whose assigned partner was not available or was absent that week. These agents are included in prior tables, but based on lack of pair data, are not included in this subsample. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

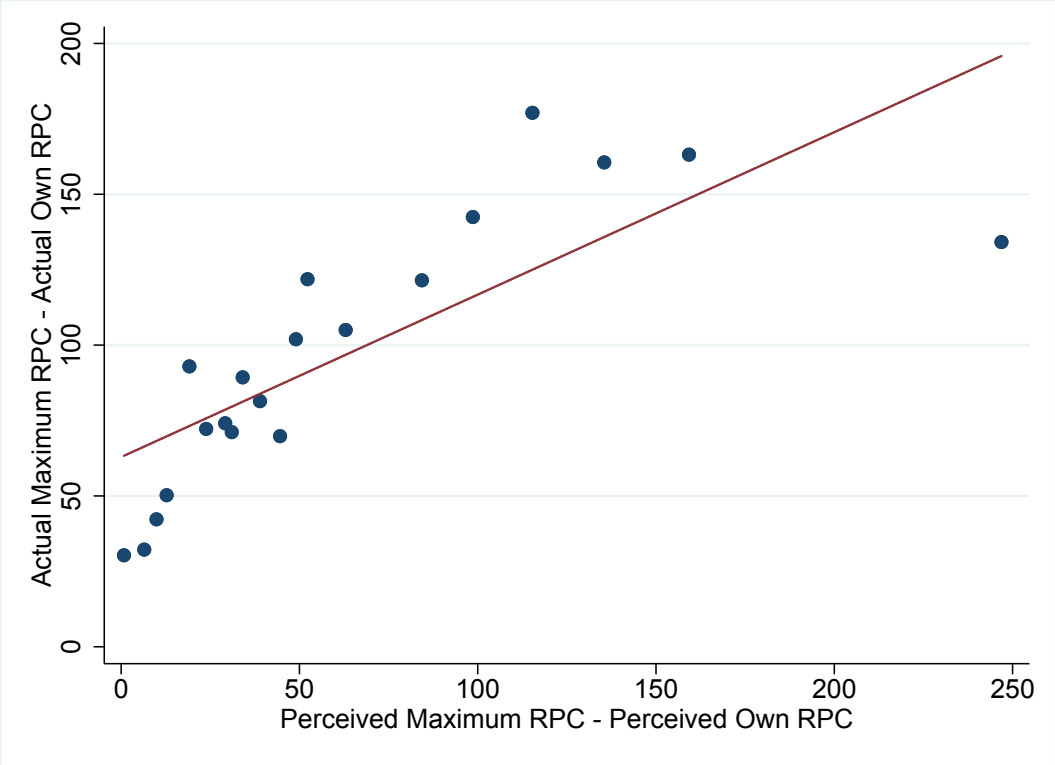
	Baseline	Non-Star Agents	Star Agents	Post-Period
	(1)	(2)	(3)	(4)
Meetings × Star Partner	0.150***	0.210**	0.147*	0.153**
	(0.054)	(0.087)	(0.078)	(0.075)
Incentives × Star Partner	-0.021	-0.031	0.069	-0.104
	(0.067)	(0.088)	(0.085)	(0.081)
Combined × Star Partner	0.136**	0.250***	0.206**	0.232***
	(0.063)	(0.071)	(0.077)	(0.076)
Structured-Meetings	0.222***	0.241***	0.068	0.030
	(0.055)	(0.075)	(0.072)	(0.048)
Pair-Incentives	0.105*	0.290***	-0.012	0.073
	(0.061)	(0.064)	(0.082)	(0.062)
Combined	0.190***	0.372***	0.061	0.058
	(0.057)	(0.064)	(0.062)	(0.049)
Manager FE (θ_g)	✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓
Adj. R-Square	0.488	0.491	0.450	0.441
Observations	3,287	1,465	1,818	3,024

Figure A.1: Agents' Reported Estimates of Treatment Effects after Help from Sales Stars



Notes. Figure plots agent's responses to a survey question asking for their estimated percentage change in RPC if they were to receive help from the top agent on their team. This measure was collected in a followup survey done over a year after the end of treatment. Prior to the experiment, agents responded positively on a Likert scale survey question asking about the effects of receiving help from coworkers.

Figure A.2: Perceived and Actual Differences Between Individual Sales and the Top Agent



Notes. Figure plots the actual deviation between the maximum RPC in a division and location and the agent's own RPC against the agent's reported perceived maximum RPC and their own RPC. These measures were collected in a followup survey done over a year after the end of treatment.

Table A.1: **Pre-Experiment and Post-Experiment Survey Responses for Treatment-Eligible Agents**

Panel A contains answers from the preliminary survey that we administered one week prior to the start of the experiment. The survey was not administered to the *External Control* group. The question wording, as it is displayed in the table, has been adapted from its original form to remove institutionally unique jargon. Agents were provided with a link to the survey and were asked to complete it while at work. Agents were not aware of which treatment they were going to be placed in at the time they took the survey. The question regarding the dollar value of the proposed incentives is the average valuation for the set of prizes offered in the *Pair-Incentives* treatment. Panel B contains answers from a survey given at the end of the treatment period using the same protocol. These questions are meant to assess the overall salience of the treatments.

	Full Sample	Internal Control	Pair-Incentives	Structured-Meetings	Combined
			Panel A: Pre-Experiment Survey		
On a scale of 1-5, how connected do you feel to others within the firm?	3.7	3.7	3.4	3.7	3.8
How many work-related interactions do you initiate in an average work week?	5.8	5.0	5.3	7.1	6.1
On a scale of 1-5, how beneficial are these interactions to you personally?	3.9	3.9	3.9	4.0	4.0
What dollar value would you be willing to spend on the proposed incentives?	\$40.20				
			Panel B: Post-Experiment Survey		
I was aware of the treatment that took place this past month.	82.5%	77.4%	78.3%	84.8%	92.0%
We turned in a completed worksheet each week.				82.6%	88.2%
I spent [ ] minutes with my partner on the worksheet.				6.3	7.3
These interactions with my partner were beneficial.				78.6%	76.0%
N_A (Agents)	378	115	83	105	75

Table A.2: **Results Across Specifications and with Other Revenue Measures**

This table is similar to the specifications in Tables 2 and 3 Column 5 but changes dependent variables or the specification. Columns 1 and 2 add worker fixed effects. Columns 3 and 4 change the dependent variable to the log of revenue-per-hour, log(RPH). Columns 5 and 6 examine total revenue per week in levels. All specifications include week and manager fixed effects. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

Specification:	Log Revenue Per Call		Log Revenue Per Hour		Total Revenue	
	Treatment	Post	Treatment	Post	Treatment	Post
	(1)	(2)	(3)	(4)	(5)	(6)
Structured-Meetings	0.250***	0.246***	0.220**	0.205*	528.10***	877.848**
	(0.052)	(0.071)	(0.090)	(0.112)	(184.33)	(369.59)
Pair-Incentives	0.145***	0.068	0.115	0.041	496.55***	380.396
	(0.052)	(0.060)	(0.074)	(0.097)	(172.52)	(448.09)
Combined	0.269***	0.273***	0.174**	0.171*	704.25***	728.914**
	(0.049)	(0.079)	(0.074)	(0.089)	(221.04)	(342.46)
Internal Control	0.010	0.108*	-0.043	-0.005	-27.78	-12.53
	(0.048)	(0.062)	(0.073)	(0.087)	(239.54)	(189.11)
Individual FE	✓	✓				
Manager FE (θ_g)			✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.485	0.609	0.476	0.442	0.43	0.269
Observations	3,821	3,627	3,821	3,627	3,821	3,627

Table A.3: **Does Ability to Answer Calls Change with Treatments?**

Here we compare the change in scheduled time to take calls, defined as adherence, across the three different treatment groups using data from the 4 weeks before and 4 weeks during the study. The variables *Structured-Meetings*, *Pair-Incentives*, and *Combined* are set to 1 in the study weeks for those randomly assigned to those treatments, and zero otherwise. Each specification contains week and sales manager fixed effects. The *Internal Control* group is the omitted category, as we do not have data on adherence for the *External Control* group. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
Structured-Meetings	-0.041	-0.040	-0.041
	(0.031)	(0.031)	(0.031)
Pair-Incentives	-0.036	-0.036	-0.036
	(0.030)	(0.030)	(0.030)
Combined	-0.019	-0.019	-0.020
	(0.047)	(0.046)	(0.047)
Demographics		✓	✓
Partner Demo.			✓
Manager FE (θ_g)	✓	✓	✓
Week FE (λ_t)	✓	✓	✓
Adj. R-Square	0.114	0.114	0.116
Observations	2,550	2,550	2,550

Table A.4: **Difference-in-differences Estimates of Worker Turnover.**

Panel A of this table reports regression estimates of employee turnover using data from the 4 weeks before and 4 weeks during the experiment. Panel B uses data from the 4 weeks before and 4 weeks after the experiment. The variables *Structured-Meetings*, *Pair-Incentives*, and *Combined* are set to 1 in the treatment/post period for those randomly assigned to those treatments, and zero otherwise. The baseline estimating equation is:

$$\text{Turnover}_{i,t} = \beta_0 + \beta_1 \text{Structured-Meetings}_{i,t} + \beta_2 \text{Pair-Incentives}_{i,t} + \beta_3 \text{Combined}_{i,t} + \lambda_t + \theta_g + \varepsilon_{i,t}$$

where i represents an agent, t represents week, g represents sales manager group, λ_t and θ_g are week and sales manager fixed effects, respectively, and $\varepsilon_{i,t}$ is an idiosyncratic error term. Indicators for treatment assignment in the pre-period are absorbed by θ_g as randomization is at the sales manager group level. In Columns (1)–(3) the *Internal Control* (passive pairs) is the omitted category. Column (4) omits the *Internal Control* group and instead uses the *External Control* group (that was not aware of the experiment and had no partner pairing) as the excluded category. Column (5) includes both control groups, with an indicator for the *Internal Control* during the experimental period. Specifications with agent or partner demographics include age, gender, and tenure with the firm. Standard errors are clustered at the sales manager level and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

Control Group:	Internal (Passive Pairs)			External (No Pairs)	Both
	(1)	(2)	(3)	(4)	(5)
Panel A: Pre-Period and Treatment-Period					
Structured-Meetings	-0.018	-0.018	-0.018	-0.025	-0.023
	(0.021)	(0.021)	(0.021)	(0.041)	(0.041)
Pair-Incentives	-0.011	-0.011	-0.011	-0.017	-0.016
	(0.021)	(0.021)	(0.021)	(0.041)	(0.040)
Combined	0.006	0.006	0.006	0.002	0.002
	(0.023)	(0.023)	(0.023)	(0.041)	(0.041)
Internal Control					-0.005
					(0.045)
Adj. R-Square	0.029	0.029	0.029	0.020	0.027
Observations	3,418	3,418	3,418	2,856	3,821
Panel B: Pre-Period and Post-Period					
Structured-Meetings	-0.014	-0.014	-0.014	0.028**	0.027**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Pair-Incentives	-0.011	-0.012	-0.012	0.029***	0.029***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Combined	-0.010	-0.011	-0.011	0.030**	0.030**
	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)
Internal Control					0.041***
					(0.012)
Demographics		✓	✓	✓	✓
Partner Demo.			✓		
Manager FE (θ_g)	✓	✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓	✓
Adj. R-Square	0.010	0.010	0.010	0.009	0.009
Observations	3,252	3,252	3,252	3,295	3,627

Table A.5: **Do Agents Give-up After Winning or Losing a Prize**

This table reports regressions of log RPC on an indicator that the agent received a prize in the prior week. The estimate is relative to a baseline of agents who did not win in the previous week. The sample contains only those agents in either the *Pair Incentives* or *Combined* treatments during the experimental period.

	Pair-Incentives	Combined	Both
	(1)	(2)	(3)
Won Last Week	-0.035**	-0.027*	-0.033***
	(0.014)	(0.014)	(0.010)
Demographic Controls	✓	✓	✓
Manager FE (θ_g)	✓	✓	✓
Week FE (λ_t)	✓	✓	✓
R-Square	0.114	0.159	0.136
Observations	673	770	1,446

Table A.6: **Do Persistent Effects Differ Based On Rotations and Prior Communication Patterns with Co-workers**

This table reports results by two sample splits: whether the agent reports initiating more or fewer than 5 work-related conversations in a given week in the pre-experiment survey (columns 1 and 2) and whether the agent rotated partners or had stable partners (columns 3 and 4). The dependent variable is log RPC and we consider the post-treatment period relative to the pre-experimental period. That is, these are long-run effects. Errors are clustered by direct manager.

	High Contact	Low Contact	No Rotations	Partner Rotations
	(1)	(2)	(3)	(4)
Structured Meetings	0.083	0.246*	0.142	0.223***
	(0.097)	(0.134)	(0.090)	(0.057)
Pair Incentives	-0.037	-0.057	-0.005	-0.003
	(0.065)	(0.093)	(0.074)	(0.064)
Combined	0.166	0.235**	0.202*	0.248***
	(0.156)	(0.102)	(0.108)	(0.064)
Manager FE (θ_g)	✓	✓	✓	✓
Week FE (λ_t)	✓	✓	✓	✓
R-Square	0.440	0.446	0.445	0.412
Observations	580	1,102	1415	1524

A Appendix

A.1 Implementing the Experiment

To communicate both the disclosures and the intervention guidelines, we did the following: (1) we solicited the help of senior executives who shared the details with division and sales managers; (2) we had a designer create and print posters which we placed in the firm's common areas and on their internal TV monitors during the experiment; (3) we set up an e-mail and phone hot-line to answer questions; (4) we set up a website that explained all aspects of the initiative including daily scores and frequently emailed questions;[51] and (5) a subset of the authors were physically on-site at least three days a week at both locations to answer questions, distribute worksheets, and administer the catered lunches. Table A.1 in the Appendix reports agents' answers to several survey questions that suggest participants (1) knew about the treatments, (2) completed the worksheets meant to facilitate knowledge transfer, (3) interacted with their partners in a meaningful way, and (4) valued the rewards.

A.2 Survey Responses and Robustness Tables

Several survey results are compiled in Table A.1. All surveys were administers through Qualtrics and distributed via email and links on the experiment website. Over 300 agents completed the preliminary survey, answering questions about their social and work-related conversations with coworkers. These results are contained in Panel A of Table A.1. Post-experiment survey results are in Panel B. These questions allow us to get an approximate measure of the effectiveness and salience of the experiment as a whole and of the *Structured-Meetings* treatment specifically. S

A.3 The Study Firm's Return on Investment

This section details the procedure used to estimate the return on investment of each intervention and the total extra-ordinary sales revenue generated by the experiment. The *Internal Control* group revenue-per-call (RPC) during the four week experiment was, on average, \$64.20. If we multiply these by the resultant treatment effects, we get the additional revenue-per-call generated by each intervention.

- *Structured-Meetings*: \$64.20 $\times$ 24.1% $\approx$ \$15.50 extra per call.
- *Pair-Incentives*: \$64.20 $\times$ 13.1% $\approx$ \$8.40 extra per call.
- *Combined*: \$64.20 $\times$ 25.5% $\approx$ \$16.40 extra per call.

We then multiply these numbers by the average number of calls per agent per week within each intervention during the four weeks of the study—58, 64, and 66 calls per week for the *Structured-Meetings*, *Pair-Incentives*, and *Combined* treatment, respectively.

- *Structured-Meetings*: \$15.50 $\times$ 58 = \$899 extra per agent per week.
- *Pair-Incentives*: \$8.40 $\times$ 64 = \$538 extra per agent per week.

[51] The posters pointed employees to the website but the site itself did not explain the treatments until they were live. In particular, agents did not know their treatment cell, nor the details of any treatment until the treatment period began, and within the treatment period, the website *only* surfaced details of an agent's own treatment.

- *Combined*: \$16.40 × 66 = \$1,082 extra per agent per week.

Next we multiply these amounts by the number of agent-weeks in each intervention to get to total amount of extra revenue generated by the four-week intervention.

- *Structured-Meetings*: \$899 × 379 = \$340,721 extra revenue earned across all four weeks.
- *Pair-Incentives*: \$538 × 396 = \$213,048 extra revenue earned across all four weeks.
- *Combined*: \$1,082 × 353 = \$381,946 extra revenue earned across all four weeks.

Now we consider the costs of implementing each intervention.

- *Structured-Meetings*: \$7 was spent on all agents for lunch each week: \$7 × 379 = \$2,700 in treatment costs (rounded up).
- *Pair-Incentives*: 1/3 of the agents won a prized valued at \$50: \$50 × 1/3 × 396 ≈ \$6,600 in treatment costs (rounded up)
- *Combined*: Consider both of these two cost structures for the 353 agent-weeks in this intervention: (\$7 × 353) + (\$50 × 1/3 × 353) ≈ \$8,400 in treatment costs (rounded up)

Finally, we calculate the return on investment of a individual intervention as

$$\text{ROI} = \frac{(\text{Extra Revenue} \times \text{Profit Fraction}) - \text{Treatment Cost}}{\text{Treatment Cost}} \tag{A.1}$$

where *Extra Revenue* equals the extra revenue earned from the given intervention across all four weeks of the experiment, *Profit Fraction* equals 5%,[52] and *Treatment Cost* equals the treatment cost calculated above. Performing computations leads to ROIs of 531%, 61%, and 127% for the *Structured-Meetings*, *Pair-Incentives*, and *Combined* treatments, respectively. Summing up the three extra revenue earned numbers results in a total extra revenue earned of (\$340,721 + \$213,048 + \$381,946) = \$935,715.

[52] This is a conservative estimate that is motivated by conversations had with firm executives.

B Documentation

The following are materials that were provided to participating sales agents and their supervisors in an effort to streamline the communication, increase the salience of the competition, and gather self-reported data. The first two sheets show the front and back sides of the collaboration worksheets completed by agents in the *Structured-Meetings* and *Combined* treatments. The next sheet contains the lunchtime talking points that we encouraged partners to discuss as they ate their free lunch (those in the *Structured-Meetings* and *Combined* treatments only).

[redacted] Sales Representative Collaboration Worksheet
PLEASE PRINT LEGIBLY

Your Full Name: _____ [redacted]: _____

Think about the **most successful** sales call **you** had in the last week. What did you do that made it successful?

Think about the **least successful** sales call **you've** had in the last week. How could you have done better?

Describe the most difficult deal-breaker that **you've** come across **in the last week**; for example: *upgrading callers to a specific new bundle*.

Please write down two goals for **you** to work on for the **rest of this week**; for example: *be braver in suggesting products*.

Goal 1:

Goal 2:

If you did the same exercise **last week**: were you successful in executing your goals? If no, why not?

Goal 1:

Goal 2:

Your Partner's Full name and [redacted]ID: _____

Please TALK to your partner about the questions below and write down their responses.

Ask **your partner** about their **most successful** sales call **last week,** what did they do right?

Ask **your partner** about their **least successful** sales call from the **last week**. What advice did **you** offer your partner?

Was **your partner** successful in accomplishing their goals **last week**? If no, why not?

Goal 1:

Goal 2:

What are **your partner's** two goals for **rest of this week**?

Goal 1:

Goal 2:

Please write down one thing **your partner recommended you** to try:

What day would you and our partner like to pick-up lunch from 12-2? (must hand in with 24 hour notice!): ☐ Thursday ☐ Friday

RXd by Adviser: date/time:

Lunch Talking Points

[redacted] 3 & 4

You do not need to turn this sheet in, put please read through it: is designed to help you make the most of the time with your partner.

1) *Bon Appetit!*

2) Have either of you had an awesome sale since you last met? What made it great?

3) Have either of you had a call go completely sideways? What happened? Does your partner have any advice?

4) Your partner gave you some advice on how to handle difficult stations earlier this week. Did it help?

5) You and your partner each made goals earlier this week, what progress have you each made on those goals?

6) If you have suggestions on how this lunch program could be more productive, please let your adviser know—we greatly appreciate your feedback.

Thank you!