Power to the Personnel? The Impacts of Managerial Discretion vs. Worker Democracy in Employee Recognition*

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Abstract

Worker agency – workers' influence over organizational decisions – is a commonly-cited determinant of employee engagement, productivity, and organizational culture. We conducted a firm-level RCT in India, randomizing whether employee recognition and associated bonuses were allocated: based on a worker vote (agency treatment), at the discretion of the manager (managerial discretion treatment), or at random and unrelated to performance (control). We find that workplace democracy increases worker attendance, but managerial discretion improves productivity. There are also implications for firm culture and knowledge spillovers, with the manager arm reducing work-related discussions between workers. Winners in the manager arm are positively selected on attendance and productivity, while those in the democracy arm are positively selected on attendance, social interactions, and likelihood of sharing the reward with co-workers in exchange for votes. These results highlight how what is valued in the workplace impacts worker behavior and firm culture, as well as the potential for informal contracts among workers to interact with workplace incentives. *JEL Codes:* D22, J54, M52, O10

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1 Introduction

Agency in the workplace – influence over organizational decisions – is increasingly viewed as important for organizational performance. Firms have begun to increase the level of agency given to workers in a variety of ways, ranging from mechanisms for worker input into performance evaluations (e.g., 360-degree feedback, peer reviews, or nominations for employee recognition) to worker representation on firm boards and voting rights over specific firm decisions.[1] Employers also view employee agency as critical for productivity, with over 80% of employers in a recent survey identifying increasing agency as important for organizational success (Deloitte, 2023). Similarly, in an incentivized choice, 80% of managers in our control group chose for worker recognition and an associated bonus to be allocated by workers' vote (rather than at the managers' own discretion).

While initiatives to give workers agency are becoming common, the impacts of worker agency on allocating rewards (such as bonuses) in the workplace are conceptually ambiguous. Agency could improve worker engagement, productivity, and firm culture by making workers feel heard, enabling them to build a culture they value, leveraging their information about one another, or alleviating biases – real or perceived – of managers. On the other hand, workers' incentives are unlikely to be aligned with those of the firm; what workers reward may not maximize profit, and could even be unrelated to or counterproductive for profit, for instance, if voting devolves into a popularity contest or if workers reward peers who will share benefits with them rather than generate surplus for the firm. Since giving workers agency could be costly for the firm and is potentially difficult to reverse, understanding the returns to such practices is highly policy-relevant.

We conducted a firm-level RCT in India to study the impacts of increased worker agency relative to increased managerial discretion on worker attendance, productivity, and firm culture. The key decision for which these decision rights were allocated was which employees to reward recognition to. Employee recognition programs – which recognize workers for behaviors valued by their organizations, often providing rewards with monetary value – are very common (WorldatWork, 2019) yet difficult to evaluate given the endogeneity of adopting such programs. Our results provide evidence on the effects of such programs in addition to answering our main research question around the effects of worker agency versus managerial discretion.

We partnered with Obeetee Ltd., India's largest carpet manufacturer. Obeetee outsources production of hand-knotted carpets to many smaller firms, each located in a village and

[1]For instance, the Mondragon Corporation in Spain decides key compensation outcomes through worker voting, and in Semco, a Brazilian manufacturing firm, workers vote on several strategic and compensation-related decisions.

each employing 10-20 workers. Obeetee provides the supplier firms with the designs and raw materials to make carpets, and pays the firms for the carpets upon completion. Workers are paid based on the number of knots they weave and complexity of the carpet, though the details of the compensation system and other management practices are at the discretion of each firm.

We introduced an employee recognition program in 125 of these firms. The recognition program involved giving individual workers monetary rewards, with winners announced in small ceremonies within their firms. Rewards were given every two weeks for 12 weeks. The reward amount was 10% of the average baseline monthly earnings in the firm. To be eligible, workers needed to be present at least one third of the working days in the two-week cycle – as verified through attendance checks done by surveyors – and one reward was given for every 10 eligible workers in each firm per cycle.

Our randomization varied only how the rewards were allocated. In the first arm, rewards were allocated based on a vote of the workers, with each eligible worker casting a vote in private for one other eligible worker, and the rewards being allocated to those with the most votes. In the second arm, the rewards were allocated at the discretion of the manager. This arm is a natural comparison for the worker arm, both because it is closest to status quo, a policy-relevant counterfactual, and because it isolates the effect of worker versus manager control. That said, we were also interested in estimating the impacts of managerial discretion itself and hence we had a third, control arm where rewards were allocated through a public lottery. Providing rewards in this arm allows us to control for income effects, and provides a policy-relevant comparison for the other two arms, asking whether firms could achieve the same results by giving the same amount in rewards but at random and without concern for worker performance.

Our main outcomes are from observations surveyors made on daily visits to each firm during the 12-week reward period. During these "spot checks," surveyors recorded which workers were present and what they were working on; this allows us to estimate impacts on attendance and productivity, where productivity is defined as knots woven per day adjusted for carpet complexity. Surveyors also recorded the workplace interactions they observed – both whether workers were conversing and what about – which provides novel and objective measures of firm culture. We also conducted two waves of endline surveys, one just before the recognition program ended and the second about seven weeks later, which provide a number of supplementary outcomes.

We begin by considering effects of the allocation mechanisms on worker attendance and productivity. The worker agency treatment increased attendance, raising the fraction of workers in a firm observed present on a given day by 4.1 percentage points (p.p.) relative

to the control arm and 5.5 p.p. relative to the manager arm, increases of 8% and 11% relative to the control mean. Attendance did not differ significantly between the manager and control arms. On the other hand, the manager arm increased productivity (i.e. knots woven in the firm on a given day, adjusted for carpet complexity). The productivity effects are 0.30 and 0.41 standard deviations (SDs), or 6% and 8%, relative to the control and worker arms, respectively. There was no significant difference in productivity between the worker and control arms, despite higher attendance in the former. Thus the different allocation mechanisms affect different margins of worker behavior.

We then turn to workplace interactions, as observed by surveyors. The manager arm significantly reduced work-related interactions, compared both to the worker and control arms. It also reduced social interactions, though the effects are not statistically significant. This indicates that while managerial discretion for rewarding workers can increase productivity, it may come at the cost of lower knowledge spillovers between workers.

What behaviors did workers and managers reward? We show that winners in the manager and voting arms were both positively selected on attendance, but those in the manager arm were significantly more so. Winners in the manager arm were also positively selected on productivity. On the other hand, winners in the voting arm were positively selected on social interactions. Using endline survey data, we also show that workers in the voting arm were more likely to receive a share of the reward from winners, and when asked why, most often say it was because they voted for the winner. This indicates that while workplace democracy can have favorable effects on certain outcomes like attendance, it may also produce non-productive behaviors like reward-sharing or favor-trading. More generally, these results show that different behaviors are rewarded by workers and managers.

Our treatment effects could arise from two different mechanisms: (1) workers could have changed their behavior to increase their chances of winning (or of receiving a share of a reward), or (2) winning itself could have affected behavior in ways that vary based on how the reward was allocated. To investigate, we estimate the impacts of winning through a shortlist design. First pooling across all arms including the control, we find that winning a reward had no impact on attendance, productivity, or workplace interactions in the following two weeks. Furthermore, the impact of winning did not differ statistically by how the reward was allocated, with one exception: winning a reward allocated by a manager reduced work-related interactions, and this effect differs significantly from the corresponding effects in the worker and control arms. This could be because other workers were less inclined to collaborate with winners selected by managers out of resentment, or because winning the manager reward made winners feel less need to collaborate with their peers. More broadly, these results indicate that the effects on attendance and productivity are likely driven by

workers responding to incentives to win, whereas the effect on workplace interactions may be driven by winning itself.

Which allocation mechanism do individuals prefer? We elicited incentivized preferences; we asked both workers and managers whether they would prefer worker vote or managerial discretion for an additional round of the reward program, implementing the choice of an anonymous, randomly-chosen respondent. The first result is that individuals in the control group tended to prefer the worker vote, with 59% of workers and 80% of managers selecting this mechanism. Second, the majority of managers in the manager arm chose to retain the right to allocate recognition, consistent with learning and with the average productivity gains in that arm. Managers in the worker vote arm were not more likely to pick the voting mechanism than managers in the control arm. Turning to workers, we find that experience with a particular allocation mechanism through our treatments increased workers' demand for that mechanism. This could be due to workers learning the benefits of their arm, or due to a preference for the status quo and aversion to change.

There are several takeaways from these results. First, our findings highlight how what is valued in the workplace impacts worker behavior and firm culture. Second, our results provide a cautionary tale for the policy question around the value of worker agency; worker voting resulted in increased attendance but not greater output, while also enabling workers to collude and reward those who would share the bonuses with them. That said, managerial discretion is not without trade-offs. While it improved productivity, the reduction in work-related interactions has concerning implications for knowledge spillovers and cohesion in the workplace. Finally, our findings highlight the power of informal contracting among workers. Despite short-run incentives to defect, workers appear to have sustained collusive agreements to share the rewards, with implications for what behaviors were incentivized under workplace democracy.

This paper relates to three literatures. The first is the literature on worker agency. In related work, Bandiera et al. (2021) find that giving procurement agents autonomy over their work in public procurement agencies in Pakistan leads to lower prices with no impact on quality, and Cai and Wang (2022) show that making workers' evaluations a part of managerial compensation improves retention and team output. Boudreau (2024) estimates how worker occupational safety committees in garment factories in Bangladesh impact worker safety, finding small positive effects of this form of representation on safety. Recent work has also examined how formal worker representation on firm boards or work councils for large firms impacts worker and firm outcomes (Harju et al., 2025; Jäger et al., 2021; Kim et al., 2018; Arnold et al., 2020; Blandhol et al., 2020; Gorton and Schmid, 2004; Addison et al., 2010; Fairris and Askenazy, 2010; Freeman and Lazear, 1995; Scholz and Vitols,

2019). We contribute to this literature by providing causal impacts of direct workplace democracy. We focus on an important firm decision – who to reward – and consider two counterfactuals, one where managers retain decision rights, and another where rewards are untied to performance.[2]

Second, we relate to the literature on the impacts of managerial discretion in worker compensation. Using an RCT in Pakistan, Andrabi and Brown (2025) show that using managers' (i.e. principals or vice-principals) subjective reports to determine teachers' raises leads to similar improvements in test scores as using objective criteria based on test scores, but subjective incentives do better for non-test score student outcomes. De Janvry et al. (2023) show that revealing the identity of the person conducting the performance evaluation for junior public employees leads to employees reallocating tasks to those that are important to the evaluator, indicating a shift towards non-productive tasks. Prior work in this literature also estimates the returns to using managers' information in hiring (Hoffman et al., 2018), monitoring (Dal Bó et al., 2021), and promotion decisions (Deserranno et al., 2025; Ho and Huang, 2025). The results of these papers underscore the trade-offs of managerial discretion, namely, that while managers may have informational advantages for some decisions (Dal Bó et al., 2021; Ho and Huang, 2025) and be able to incentivize behaviors that are difficult to observe (Andrabi and Brown, 2025), increased discretion may also lead to decisions influenced by favoritism (De Janvry et al., 2023) or over-confidence (Hoffman et al., 2018). This paper contributes to the literature in two ways. First, it estimates the impact of using managerial discretion in recognition programs, a commonly used, tournament-style mechanism to reward a broad set of behaviors important to firms. Second, it contrasts outcomes from using discretion against a counterfactual of workplace democracy.[3]

Third and most broadly, we relate to the literature on incentive pay, which has estimated the effects of incentive pay on productivity in the public (Leaver et al., 2021; Deserranno et al., 2022; Neal, 2011; Khan et al., 2019; Burgess et al., 2017) and private sectors (the latter literature includes work focused on individual incentive pay (Lazear, 2000; Alexander, 2020; Coviello et al., 2022; Brown and Andrabi, 2020) as well as team incentives (Knez and Simester, 2001; Kuhn and Yu, 2025; Sandvik et al., 2021; Friebel et al., 2017; Bandiera et al., 2013)). In a lab experiment, Bradler et al. (2016) show that when (randomly chosen) groups hired for a three-hour job were given thank-you cards after two-hours, they increased performance. Another related literature focuses specifically on the impacts of tournaments

[2]Lab evidence(Bó et al., 2010) shows that the impact of a policy (fines) on co-operation is larger when subjects choose the policy rather than it being exogenously imposed, which indicates that groups value the ability to choose.

[3]Related empirical literature estimates the impacts of managerial autonomy on a broader set of firm decisions, including labor-related decisions (e.g. Aghion et al. (2021) and Kala (2024)).

on productivity (Bandiera et al., 2013; Delfgaauw et al., 2013; Englmaier et al., 2024; Leuven et al., 2011; Hagenbach and Kranton, 2025). Even in settings with measurable productivity, workers and managers may value dimensions other than individual output (indeed, the most common reasons managers cited for choosing a winner in the manager arm were reliability-related (i.e. attendance), not output-related). This is also the case when externalities such as knowledge spillovers impact firm output. Recognition programs rewarding workers for workplace conduct are common, but may be endogenous to other incentives, and our paper contributes to this literature by estimating casual impacts of such programs.[4] Furthermore, it provides evidence on what behaviors are rewarded and how the structure of such programs, i.e. whether winners are chosen by managers or by a worker vote, impacts the returns to these programs.

2 Background

Our study was conducted in partnership with India's largest carpet manufacturer: Obeetee Ltd. The firm's production, and our study, are based in eastern Uttar Pradesh. Uttar Pradesh is India's most populous state and among its poorest (NITI Aayog, 2018). The carpet industry is largely in the eastern part of the state, with the area where the firm is based often referred to as "the carpet capital of India."

We focus on Obeetee's hand-knotted carpet production, which operates as follows. Designers working at Obeetee create a carpet design and retailers (e.g. Pier 1, Pottery Barn) place an order for a certain number of carpets of that design. Obeetee outsources the production of the carpets to hundreds of supplier firms located in villages in the area. Obeetee provides firms with the design and raw materials, and then pays the firms for the completed carpets. Obeetee monitors the firms to ensure carpets are produced correctly and on time, and that labor laws are adhered to within the firms, but otherwise the management and production process is completely up to the firms.

Each firm is owned by an individual in the village and generally employs 10-20 carpet weavers (our sampling frame includes firms between 4 and 50 workers). The firms may take orders from other carpet manufacturers in the area in addition to Obeetee. The firms are generally managed by members of the households who own them, though some larger firms hire managers. Workers mostly come from nearby villages, but some firms employ migrant workers from other states who live and work on the firms' premises for several months at a

[4]Also related is work that directly incentivizes or encourages behaviors like communication and helping in the workplace (Sandvik et al., 2020; Castro et al., 2025), including with non-monetary rewards such as recognition.

time. A large majority of workers are male as female labor force participation is low in this area, but Obeetee has been working to train and promote employment for women workers as part of its corporate social responsibility initiatives.[5] In light of local gender norms, the firms are gender-segregated, employing either female or male workers but not both. There are often wealth and caste gaps between workers and management, with workers generally coming from poorer or lower caste households. Qualitatively, many workers voiced respect and deference for their firm's management, while some voice concerns about pay or about favoritism towards certain workers.

We work specifically in firms that produce hand-knotted carpets. Such carpets are made of hundred of thousands of knots tied by hand and are generally the highest quality, most expensive carpets on the market. Figure 1 visualizes the production process. Each carpet is woven on a single loom, which can seat 1-4 workers working at once. Threads are strung vertically on the loom and workers tie knots of yarn to those threads as specified by the carpet's design. Weaving is done from the bottom to top of the carpet, with workers completing an individual line of knots spanning the width of the carpet, and then moving on to the line above it. The average carpet produced during our intervention period took 11 days to complete and was 10-by-7 feet in size. Workers often work on the same looms and with the same other workers, but the teams are not totally fixed – some workers are absent on particular days, some may be asked to move to another carpet with an urgent deadline, and sometimes the team is dissolved to work on different looms once its carpet is completed. The workers working on a single loom on a particular day generally split up the width of the carpet, so that if three workers were weaving, each would complete only the left third, middle third, or right third of every line before proceeding to the next line. Thus productivity is largely individual, though some degree of coordination is required and workers may ask for help from co-workers. Errors can be made in the weaving process that vary widely in their severity; the vast majority can be and are corrected, though some result in the finished carpet being rejected by Obeetee.

The carpet design specifies the color of each knot and number of knots per square inch, thus determining carpet complexity. All else equal, carpets that have more knots per square inch, more colors, more colors of a similar shade, and more adjacent knots of different colors are more complex. Only workers with sufficient experience and skill are assigned complex carpets (meaning teams of workers are matched on ability). Obeetee pays firms more per square inch for completed carpets that are more complex.

[5]We have conducted previous projects in partnership with Obeetee that study women's decisions to take-up the weaving training and employment (Kala and McKelway, 2025*b*; Lowe and McKelway, 2025; McKelway, 2022, 2025*b*,*a*).

Firms set piece rates to pay workers for doing a certain number of knots, generally either 6,000 or 9,000, and rates vary based on the complexity of the carpet. Workers can generally complete at least 6,000-9,000 knots within a day, but this varies based on their experience, hours spent working, and carpet complexity. In practice, it is difficult for firms to know exactly how many knots a worker completes on a given day and so this is often somewhat approximate. The cycle on which workers are paid also varies across firms, with the most common cycles being weekly and upon the completion of a carpet.

Worker attendance is quite variable, as is common in firms in developing countries; among control-group firms, an average of 48.3% of workers were present when our surveyors visited for daily spot checks. Part of the fluctuation in attendance is due to the volume of work the firms have for workers on any given day, but much of it is worker absenteeism – workers missing work to do seasonal agricultural work, for festivals/weddings, or for some other personal conflict. This absenteeism is problematic, making it challenging for firms to meet Obeetee deadlines and for Obeetee to meet deadlines with external retailers.

While the majority of workers' time is spent weaving (tying knots), the firms require some non-weaving work to be done (in the spot-check data, conditional on attending, we observe workers doing weaving work about 94% of the time and non-weaving work 6% of the time). Much of the non-weaving work happens around the completion of a carpet, when work is required to take the completed carpet off the loom and set up the loom for the next carpet. Whether and how workers are compensated for this work is up to their firms.

3 Experimental Design

3.1 Sample Recruitment

We recruited firms for the study in November and December 2024 (see Figure 2 for a study timeline). Obeetee shared a list of the hand-knotted weaving firms they contract with, and we began by calling firms on that list and completing a short eligibility screening survey.

Our partner's list included 294 firms from 195 villages. The multiple firms within the same village are very often on the same premises, and owned and managed by the same family. They are considered separate firms for Obeetee's purposes (e.g. because different family members formally own different parts, or because one firm was instituted later than another), but we did not want to consider them separate for our experiment given the management and worker pool is often shared across them. The challenge was that Obeetee's list did not distinguish between affiliated and unaffiliated firms in the same village. Our solution was to randomize the order of firms within a village on Obeetee's list, start by

calling the first one, and move on to the subsequent ones if the earlier ones were unreachable or did not consent for the screening survey. When asking the questions about the firm that determined eligibility, we asked firm owners to report about the entire firm's premises, not the firm as designated in Obeetee's records.[6] We also asked the firm if they were aware of any other firms in their village that took Obeetee orders, were on separate premises, and not owned by them; these could be considered separate firms for our experiment. We identified seven firms in this way which we contacted for eligibility screening.

The screening survey asked whether female or male workers worked in the premises, how many workers of a given gender worked in the premises, and what fraction of the carpets those workers wove were Obeetee carpets. To be eligible, a firm needed to have between 4 and 50 workers of a given gender, and at least 25% of those workers' carpets needed to be Obeetee orders. If both the male and female weaving operations in a firm were eligible, we took the firm with female workers. We did not pool workers of the same gender because they work separately and their productivity tends to be quite different, given male workers work more hours and have more experience. There are far fewer firms with female workers, so picking these when both were eligible maximizes gender variation in our sample. Of the 301 candidate firms (294 plus seven), we successfully conducted screening surveys with 179 and deemed 144 of those eligible.

We then visited the eligible firms in person to provide the firm owner information about our study and seek their consent for their firm to participate. If they consented, we asked them to identify the firm owner or hired manager we should approach for surveys and allocation of the reward (in case the firm was assigned the manager discretion arm), recommending they select the person most directly involved in managing the workers. We refer to this person throughout the paper as the manager. Finally, we provided information about the study to workers in consenting firms. In total, we successfully met with 137 of the 144 eligible firms to seek consent, and 125 consented. The randomization was run and treatment status revealed after firms had agreed to participate. One firm withdrew from the study just after treatment was revealed, which means we conducted the reward program and further data collection in 124 firms. Two more firms shut down during the 12-week intervention period, and we impute zeros for attendance and worker interactions for them post-exit.

[6]If the firm owner owned firms in different premises within the same village, we asked them to respond about the main premises, defined as the premises where the looms that contributed the highest amount to revenue were located.

3.2 Randomization

We randomly assigned each of the 125 firms to one of three treatment arms: worker vote, manager discretion, or control (lottery). To maximize power, we formed strata that each included three firms, matched on gender and average worker attendance.[7] The latter came from a question on the worker baseline survey (detailed further in Section 4.1 below) asking workers how many of the last seven days they had worked in the firm.

The randomization achieved balance on baseline worker characteristics, measured in the worker baseline survey. Table A.1 compares the three treatment arms on 15 baseline characteristics. Of 45 comparisons, three are significant at the 10% level or higher; this is close to, and slightly less than, what we would expect from random chance alone. Note that our analyses include baseline variables selected using post-double-selection (PDS) Lasso (Belloni et al., 2014), which helps address any important chance imbalances.

3.3 Employee Recognition Program

We introduced a 12-week employee recognition program in all firms. Our treatment varied the mechanism for determining which workers won recognition, but the structure of the program was otherwise identical across the firms. We begin this section by detailing the structure of this program common to all treatment arms, and then describe the randomized allocation mechanisms.

The program involved providing monetary rewards through small recognition ceremonies to individual workers in the firms every two weeks for 12 weeks. The 12-week period occurred between January and April 2025, with surveyors visiting each firm prior to the start of the reward period to explain the program and how rewards would be allocated in that firm.

The winners of every two-week reward cycle were announced in public ceremonies held in each firm the following Monday. Only workers who met a minimum attendance criteria over each two-week period were eligible to receive a reward in that reward cycle. Attendance was verified by our spot checks, wherein surveyors visited every firm on each work day (work days in this setting are Monday through Saturday, excluding holidays)[8] and recorded which workers were present. Only workers observed present at least one third of the working days in the two-week period were eligible. Figure 3 illustrates the timeline of a typical reward cycle, including the reward ceremonies and attendance checks.

[7]The number of firms with female workers (21) was divisible by three, but the number with male workers (104) was not. Hence one male stratum, the one with the highest average attendance firms, included five rather than three firms.

[8]Occasionally certain firms were closed on other days (e.g. for a wedding or funeral in the village), and we did not count these as work days when determining reward eligibility.

In each cycle and each firm, one reward was given for every 10 eligible workers, rounded to the nearest integer.[9] This means that any treatment effects on attendance would not affect the fraction of workers in a firm who could win. On average, 14 workers were eligible per firm in a cycle.

The reward amount to be given in each cycle was fixed over time, but varied across firms based on typical worker earnings in that firm. Earnings vary by firm due to differences in worker compensation rates, attendance, and productivity. Each firm's reward amount was 10% of the average monthly worker earnings in the firm, calculated using responses from the worker baseline survey and rounded to the nearest ₹50. The amount varied between ₹300 ($3.32) and ₹1200 ($13.30) across the firms, with an average of ₹673 ($7.46). This implies that all winners within a firm would win the same amount, but workers across firms faced the same incentives as a proportion of their average weaving earnings. Keeping a fixed amount determined by baseline data meant treatment effects on earnings could not affect the return to winning, which is useful in interpreting the treatment effects as the impacts of the assigned allocation mechanism. During the recruitment process, we told firms and weavers that the reward amount would be about 10% of the average salary of a weaver, but we did not detail that the 10% would be calculated based on worker baseline survey responses in that firm to avoid workers inflating their reported earnings to increase the amount. The money for these rewards came from the research project, and the reward amount was given to winners in cash immediately after the reward ceremonies.

Each reward ceremony started with an announcement from surveyors, in which they listed the names of the workers in the firm who had met the eligibility requirements that cycle. How the rewards were then allocated among the eligible workers was determined by treatment status.

In the *manager discretion arm*, the winners were selected by the designated manager. On the day of the reward ceremony and prior to making the announcement, surveyors would ask the manager to select the given number of winners from the list of eligible workers based on who, according to them, had done good work over the past two weeks. Surveyors asked reasons for selecting each winner as well, and entered all responses into tablets. If the manager was not present on the day of the reward ceremony, the surveyor would ask them to make their selections over a phone call, and if that failed, the choice was made by someone the manager designated in advance of the ceremony.[10] Then, after making the announcement to the firm listing the eligible workers, the surveyor said the winners had been

[9] E.g. if 15 workers were eligible, two rewards were given. The exception was if fewer than five workers were eligible – in this case we always gave one reward. No reward was given in the few cases where zero workers were eligible.

[10] This was a rare occurrence, happening only 1.26% of the time.

selected by the manager based on who, according to them, had done good work over the last two weeks, and then proceeded to announce the names of the winners. The reasons the managers provided for selecting the winners were not revealed.

In the *worker vote arm*, a vote by the workers determined who won the reward. After listing the eligible workers, the surveyors said the rewards were to be given to the workers who had done good work over the two preceding weeks, and would be allocated based on a vote of the workers. Surveyors then called eligible workers one-by-one to a private voting booth they had set up outside the firm to cast their votes. Votes were cast on ballot papers, and surveyors assisted any illiterate workers in filling out their ballots. Only eligible workers were included on the ballots and only eligible workers could cast votes. Each person could vote for one person and were not allowed to vote for themselves. Surveyors also asked each worker why they voted for the person they did, recording both the reason and the person the worker voted for on a tablet.

Once the voting was complete, the surveyor went inside the firm to count the paper ballots. The surveyor gathered the workers around for the counting in a way that they could see the counting occur but not see what was selected on individual ballots or the tallies of votes cast for different workers. We had surveyors count the votes and this count determine the winner rather than having the tablets count because we wanted to make it clear to the workers that the votes and the votes alone determined the winners. The surveyors recorded in the tablets who their counts revealed to be the winners, allowing us to check that the surveyor counting was correct the vast majority (97%) of the time. Once the votes were counted, the surveyors announced the winners. If x rewards were to be given, the x workers with the highest number of votes won the reward. More than x winners were selected in the case of ties, with the total amount initially assigned to be distributed across all winners being divided equally among the actual number of winners.[11] Only the names of the winners were announced to workers, not the reasons weavers voted for who they did and not the distribution of votes (i.e. it was not revealed who came in second place or what the ordering of the winners was in the cases of multiple winners).

Finally, in the *control (lottery) arm*, rewards were allocated based on a public lottery. We gave rewards in our control group to hold fixed income effects and interactions with the research team, but allocated the rewards to (eligible) workers selected at random. To make the randomness transparent to workers, the rewards were allocated through a public lottery. After surveyors announced the eligible workers, they put a chit into a transparent box for every eligible worker, with the worker's name on it. The surveyors then shuffled the box,

[11] 16.2% of voting reward ceremonies had cases where at least one recipient had to split the reward with another.

drew one chit out while looking in another direction, and then read the name on the selected chit aloud. This was repeated as many times as there were rewards to be given in the firm, with the workers whose chits were drawn being the winners. The surveyor recorded whose chits were drawn out in a tablet. We show in Table A.6 that a large majority of workers in the control arm (75%) said on the first endline survey that the type of worker who won rewards in their firm was determined by luck/randomness, indicating that workers in this arm indeed thought that this was a lottery.

4 Data and Empirical Specifications

4.1 Data and Outcomes

We collected data from several sources, visualized in the study timeline (Figure 2). Our main source of data is our "spot checks," or surveyor visits to firms. Surveyors visited each of the 124 firms[12] on every work day throughout the 12-week intervention period, recording several different worker and manager behaviors. First, they measured worker attendance, using a dynamic roster which updated as new workers were observed (this is also the data used to determine eligibility for the rewards). Second, they recorded what each worker was working on: whether they were doing weaving or non-weaving work, what loom they were sitting at or if they were doing non-weaving work off loom, and the characteristics of the carpet they were weaving (if they were weaving). Third, they recorded whether the workers on each loom were interacting with each other during the visit, and if so, categorized the topic of their discussion (e.g. social, work-related). Fourth, they recorded if the manager was present, and if so, what they were doing (e.g. supervising workers, helping workers, engaged in production).

We also conducted a baseline survey and two endline surveys in the 124 firms. Both workers and managers were surveyed at each wave of surveys. The baseline surveys gathered information about individuals' demographics, work at the firm, and perceptions of workplace culture. The endline surveys asked about work at the firm, workplace culture, as well as several questions about beliefs and experiences with the reward program. The first endline survey (EL1) was conducted over the final three weeks of the 12-week reward program, and the second (EL2) 5-11 weeks following the end of the 12-week period. For outcomes measured at both endlines, we use data from EL1 when available – since that endline was done during the reward period – but use responses from EL2 for respondents not surveyed at EL1.

Table A.2 presents analyses of endline survey attrition. We were able to complete either

[12]Recall 125 firms consented and were randomized, but one firm subsequently withdrew from the study.

endline survey for 82% of workers in the control group, and this rate is balanced across treatment arms.[13] Among those surveyed at both, there is balance in being surveyed at EL1. These results together imply we have balance in whether the outcomes pooled across the two endlines were observed, and balance in which of the two endlines those outcomes come from. We also see balance in workers' completion of EL1, though there is some imbalance at EL2, with workers in the manager and worker vote arms being less likely than the control to complete that survey. Note, however, that there is no difference in attrition between the manager and worker arms, and we find that our key effects on outcomes observed only at EL2 are robust to Lee bounding (see footnotes 23 and 31). 98% of managers in the control group were surveyed at either endline, and we see balance in all manager endline completion outcomes.

The spot check data is the source of our three main outcomes: attendance, productivity, and workplace interactions. The endline surveys provide a number of supplementary outcomes. We detail all outcomes when presenting effects on them. Our outcomes and analyses closely resemble those listed in our study's pre-registration (Kala and McKelway, 2025*a*), with Appendix B providing additional details on the pre-registration and listing all deviations from it.

4.2 Empirical Specifications

For our main analyses of spot-check outcomes, we run regressions of the form:

$$Y_{jt} = \beta_1 T_{1j} + \beta_2 T_{2j} + \alpha_s + \alpha_r + \gamma X_j + \varepsilon_{jt} \tag{1}$$

where Y_{jt} is an outcome for firm j on date t. T_{1j} and T_{2j} are indicators for firm j being assigned to the manager discretion and worker vote arms, respectively. α_s are fixed effects for strata, and α_r are fixed effects for each of the six rounds of reward cycles. X_j are variables selected via post-double-selection (PDS) Lasso (Belloni et al., 2014) from the screening and baseline surveys. They are either firm-level variables or firm-level averages of worker-level variables. Standard errors are clustered by firm (the level of treatment).

For endline-survey outcomes, we run regressions of the form:

$$Y_{ij} = \beta_1 T_{1j} + \beta_2 T_{2j} + \alpha_s + \gamma X_j + \varepsilon_{ij} \tag{2}$$

where Y_{ij} is the outcome of worker i in firm j, and all other variables are defined as above. We use the same set of firm-level Lasso variables as above because many workers did not

[13]The sample for the worker attrition regressions includes any workers ever observed present in a spot check during the 12-week intervention period.

take the baseline survey, given significant levels of worker churn. As before, standard errors are clustered by firm.

In both specifications, the coefficients of interest are β_1 and β_2 – the impact of the manager discretion and worker vote arms, respectively, relative to the control group. We also test $\beta_1 = \beta_2$ in all regressions to assess the effect of the worker vote arm relative to the manager arm.

5 Effects on Main Outcomes

5.1 Attendance and Productivity

Our primary performance-related outcomes are daily attendance and productivity, both measured using the daily surveyor visits (spot checks) and defined in a balanced firm x date panel covering the 12-week intervention period. Attendance is the fraction of workers present; that is, the number of workers observed present in a firm on a given day, divided by the number of different workers ever observed in that firm. We measure productivity as the number of knots woven in the firm on a given day, adjusted for carpet complexity. This variable is constructed in several steps. Starting in a worker x date panel, we impute the number of knots a worker wove if they were weaving on a particular day, using the dates a carpet was started and finished, the total number of knots in the carpet, and the number of people working on it each day. For instance, if the carpet had 9000 knots, took three weaving days to complete, and was worked on by two workers each day, each worker would be interpolated to have woven 1500 knots on each of these three days. Next, we residualize these knots (winsorised at the 99th percentile) on carpet characteristics such as number of colors and density of knots.[14] Any worker x date observations where the worker was absent or doing non-weaving work on that date are not included in the residualization because carpet controls are not defined. Finally, we sum the residualized knots woven in each firm on each day, using this as our measure of productivity.[15] This outcome is missing when no workers were present and weaving in a firm on a given day, though this is rare (applying to 7.8% of

[14]The full set of carpet controls are number of colors, area, number of knots, density of knots per square yard, dummy variables for three types of carpet requiring different weaving techniques, and a dummy variable for whether the carpet is for the partner firm. We set carpet controls to zero when they are missing (e.g. because the surveyor could not obtain the information) and also include a dummy for any controls being missing. Missingness is rare, with this dummy taking the value of 1 just 0.37% of the time.

[15]As discussed in Section 8, we conducted additional spot checks after the rewards stopped and present effects from those checks in that section. We note here, however, that all data – from both during and after the reward period – are included in the residualization. Residualized knots are higher on average after the rewards, likely due to seasonal effects. This is why residualized knots across the treatment arms average to a negative value in Table 1 and a positive value in Table A.11.

firm x dates) and balanced by treatment (Table A.3).

Results are presented in Table 1. Column 1 shows that the worker voting arm increased the fraction of workers present by 4.1 p.p. (p-value < 0.10) relative to the control arm, an 11% increase relative to the control mean. There is no difference in attendance between the manager and control arms, and we can reject that the two treatment arms have the same effect (p-value < 0.10).[16] Consistent with increased attendance in the voting arm, we also find this treatment increases weavers' likelihood of being eligible for rewards, both relative to the control and to the manager arms (Table A.3). Also in Table A.3, we see no effects on worker entry or exit (as measured by the number of workers who appeared only after and during the first round of bonuses, respectively), indicating that the attendance effect is driven by workers who were attached to the firm attending more often.

On the other hand, in column 2 of Table 1, we find that the manager arm increased productivity, both relative to the control ($p < 0.05$) and worker ($p < 0.01$) arms.[17] We use two approaches to interpret the magnitude of these effects on residualized knots. First, comparing to the standard deviation of the outcome in the control group of 45,639, the manager-versus-control effect is 0.30 SDs and the manager-versus-worker effect is 0.41 SDs. Second, we compute percentage effects relative to the control mean, after adding to the residualized knots variable -1 $\times$ the variable's minimum (we cannot compare to the control mean in Table 1 because residualized knots are often negative and the control mean itself is negative). In Table A.3, we see that the manager-versus-control and manager-versus-worker effects are 6% and 8% of the control mean, respectively. Finally, we note that we cannot reject that residualized knots were the same in the worker and control arms; despite greater attendance, workers in the worker arm produced no more daily output than those in the control arm, and less than those in the manager arm.[18]

A concern is that the increase in residualized knots may have been accompanied by more carpet defects. We measured defects at EL2, asking managers if there had been a defect on a carpet produced by the firm in each of the intervention months. We create a binary outcome for any reported defect during this time. Defects were reported in 65.0% of control firms, but reassuringly, we do not find that the manager arm (or the worker arm) significantly

[16]These results are not driven by attendance on days in which the reward ceremonies were conducted. Dropping the six days on which these ceremonies were scheduled leads to identical results: an attendance increase of 4.1 p.p. Reward ceremonies were largely conducted when scheduled, with the largest deviation in the first round, when four firms' ceremonies were delayed by one day.

[17]Results look similar if we again winsorize residualized knots at the 99th percentile after summing to the firm x date level.

[18]One possibility is that firms did not have enough work for workers to do when attendance increased. However, the median manager surveyed at EL1 said they had never had trouble getting enough carpet orders to keep weavers busy since the start of the employee recognition program, and there are no treatment effects on this.

increased the defect rate (Table A.4).

These results illustrate that the two treatment mechanisms incentivized workers to exert effort along different dimensions valued by firms. However, the effects of the worker arm indicate firms may not be able to translate greater attendance into greater output.

5.2 Workplace Interactions

In addition to attendance and productivity, workers' interactions with each other or the manager may change as a result of the program. This could be due to changes in co-operative or competitive behavior, for instance, or changes to social cohesion in the workplace. We measure workplace interactions using the observations from the daily spot checks described in Section 4.1. We sort the categories of interactions surveyors observed into three groups: any work-related interactions, social interactions, and conversations with the manager. Interactions were recorded at the loom x date level and for analysis we aggregate to the firm x date level, presenting effects on indicators for any interaction of a given type being observed in the firm on that day. We control for the number of workers present at the firm on that date as this may mechanically produce more interactions, but the results are similar if we omit this control variable.

Results for these outcomes are presented in Table 2. Column 1 reports impacts on work-related interactions, where we find large and significant reductions from the manager arm. The likelihood of work-related interactions falls by 9.3 p.p. (p-value < 0.01), about 39% of the control mean. The effect for the worker arm is negative, but smaller (1.8 p.p.) and not significant, and we can reject that the two treatment arms have the same impact (p-value < 0.01). In column 2, we also see a negative effect of the manager arm on social interactions, but the effect is not significant relative to the control (p-value $= 0.172$) or worker (p-value $= 0.268$) arms. These results suggest that recognition programs which give managers discretion could reduce knowledge spillovers, or workplace cohesion. These shifts did not translate into negative productivity impacts overall, though this particular type of incentive structure may not be optimal in firms where production relies significantly on worker co-operation.

How do worker-manager interactions change? Column 3 shows that workers are not more or less likely to be talking to the manager in either arm during surveyor visits. Interactions with managers are also quite uncommon, observed in just 2.8% of firm x dates. Furthermore, Table A.5 presents effects on whether managers were present and what they were doing when surveyors visited for the spot checks. Managers are not more or less likely to be present, nor are they more or less likely to be monitoring workers, helping them, or engaging in

production. These results indicate that managers do not respond to the type of recognition program in their frequency and type of worker interactions, allowing us to rule this out as a potential mechanism for treatment effects.

6 How Were Rewards Allocated?

Next, we examine differences in how rewards were allocated across the treatment arms, considering: what behaviors were rewarded, the extent to which winners were repeated versus rotated, as well as sharing of reward money.

6.1 What Behaviors Were Rewarded?

We begin by testing whether the behavior of workers who received the recognition rewards differed across the treatment arms. We restrict to winners in the manager and worker arms, and workers eligible for rewards in the control arm.[19] We then compare these workers on their within-firm percentiles of attendance, productivity, and participation in work-related and social interactions in the fortnight before they won/were eligible.[20] Attendance is the number of days workers attended, productivity the sum of residualized knots workers wove, and interactions the number of days workers were observed in an interaction of a given type (all in the prior fortnight).[21] We then compute each winner's percentile for each variable, where percentiles are computed relative to all eligible workers in their firm in that round. This allows us to ask: where were winners drawn from in their firm's attendance, productivity, and interaction distributions, and how did this differ by allocation mechanism?

We present these results in Table 3. Column 1 shows that winners in both the manager and worker arms were more positively selected on attendance relative to the control group, though winners in the manager arm were significantly more so. In particular, winners in the manager arm were 10.3 percentiles higher on average than the control group, while those in the worker arm were 6.5 percentiles higher. In column 2, we see that workers in the manager arm were also more positively selected on productivity, though the effect (4.3 percentiles) is smaller in magnitude compared to attendance. The larger effects on attendance are also

[19]The control winners were selected at random from eligible workers so should not differ from the average eligible worker, and including all eligible workers improves power. The winners in the voting arm are identified using information on who voted for whom rather than surveyors counts, though these approaches differ from each other very rarely (3% of the time, see Section 3.3 for details).

[20]We do not include manager interactions as there is very little variation for computing percentiles; as discussed in Section 5.2, manager-worker interactions were observed on just 2.8% of firm x dates.

[21]The construction of firm x date residualized knots is detailed in Section 5.1. To obtain the knots variable considered here, we follow the same process, except that in the final step we sum over residualized knots individual workers wove in the prior fortnight.

consistent with the reward ceremony data, where good attendance was cited by managers and workers as the first and second most common reason for picking a worker as a winner, respectively.

Turning to interactions (columns 3-4), we see small, statistically insignificant differences in interaction percentiles between the manager and control arms. However, winners in the voting arm are more positively selected on social interactions, with voting winners coming from 4.2 higher percentiles on average compared to the control arm (p-value < 0.01). The coefficient on work-related interactions for this arm is also positive, but smaller (2.0 percentiles) and not significant at conventional levels ($p = 0.114$). These findings are also consistent with the reward ceremony data, where the most common reason workers gave for picking a co-worker as a winner was "behaves well with other weavers."

Table A.6 provides effects on workers' beliefs about what was rewarded from an EL1 question asking what types of workers generally won in their firms. Consistent with the results in Table 3, the manager arm increased reports that workers with good attendance, and workers that weave many knots or complex carpets, won the rewards. We also see positive effects on both outcomes from the worker arm that do not differ from the manager arm's effects. Compared to the results in Table 3, this may indicate that workers in the worker arm overestimated the extent to which attendance and productivity were rewarded, though an important caveat is that the worker belief variables do not correspond exactly to the spot check variables.[22]

Two other results from Table A.6 are worth note. First, helping workers is more likely to be chosen as a winning behavior in the voting arm than manager arm, but helping managers is not significantly more likely in the manager arm than in the voting arm. This could be because possibilities to help co-workers arose more often, or because managers valued other traits. Second, the table presents effects on beliefs that non-productive behaviors were rewarded, which we split into co-worker-related (nice to co-workers, promises to share reward with co-workers, most popular, and campaigned for votes) and manager-related (nice to manager, promises to share reward with manager, and manager's favorite/friend/relative). Workers in the voting arm were more likely to report winners undertaking non-productive behaviors related to co-workers, while workers in the manager arm were more likely to report winners undertaking non-productive behaviors related to managers. However, the overall likelihood of reporting any non-productive behavior was higher in the voting arm than the manager arm. This is consistent with the selection-on-social-interaction result from Table 3

[22]In particular, good attendance may differ from the fraction of workers attending daily if the former included, e.g., attendance around firm deadlines. Likewise, knots woven residualized of carpet characteristics is not the same as weaving many knots or weaving complex carpets.

and with the results below on sharing of rewards.

In sum, managers tend to reward workers on dimensions that benefit the firm – attendance and productivity – and workers perceive this. At the same time, there is an increase in perceptions that non-productive traits were rewarded. On the other hand, workers given decision rights reward co-workers on dimensions that could benefit the firm but also on dimensions that are unlikely to, with workers in this arm perceiving non-productive traits as being more likely to be rewarded than in the manager arm. More broadly, which behaviors are rewarded differ based on who is allocating.

6.2 Repeating Versus Rotating Winners

We have seen that the various allocation mechanisms led different behaviors to be rewarded, but did the same individuals with those behaviors win in many reward cycles or were rewards rotated across different individuals? In Table 4, we present effects on the number of workers in a firm who won a reward more than once. Of course the number of repeat winners is partly a function of the number of total winners, which could differ across treatment arms due to ties in the voting arm or to effects on attendance around the eligibility threshold; we therefore also present effects on the total number of workers who ever won.

Around eight weavers in each firm ever won, and this did not differ significantly by treatment arm (column 1). Turning to the number of repeat winners (column 2), two results are notable. First, the number of repeat winners was no different in the worker arm than in the control arm, where rewards were allocated at random. Second, the number of repeat winners was significantly lower in the manager arm compared to both the worker and the control arms (p-value < 0.01 for both). The point estimates from columns 1 and 2 together imply that only 7% of manager-arm winners were repeat winners, versus 22% and 23% in the control and worker arms, respectively.

Thus while workers and managers rewarded certain types of behaviors, there was substantially more rotation in the individual workers managers rewarded. One explanation is that managers were constrained in who they rewarded by perceptions of fairness, intentionally rotating the reward to avoid workers feeling they were picking favorites. We saw above that the manager arm increased reports that non-productive, manager-related behaviors were rewarded, and we will see in Section 8 below an increase in perceptions that personal relationships with managers were important for how workers were rewarded in the firms; these results suggest that concerns about favoritism are real and that the managers were not totally successful in alleviating them.

6.3 Reward Sharing

Assigning workers voting rights can lead them to reward workers who exhibit workplace behaviors they value, but as we have seen, they may also use this power to reward unproductive behaviors. In this section, we examine one particular unproductive behavior: rewarding workers who will share their rewards in exchange for voting for them.

At EL2, we asked whether workers had received a share of a reward from a winner and, if so, why the winner shared with them. Column 1 of Table 5 presents impacts on an indicator for workers reporting a winner had shared with them. It is worth noting that across all arms, the likelihood of reward sharing is high, with 72% of workers in the control group reporting that winners had shared with them. However, the likelihood of receiving a share is 14.2 p.p. higher in the voting arm relative to the control group ($p < 0.01$), a 20% increase from the control mean. Sharing in the manager arm is not statistically different from in the control group but significantly lower than in the voting arm ($p < 0.01$).

We turn to reasons for sharing in columns 2-8, considering the following reasons: they were friends or family, they voted for the winner, they helped the winner with work, they made the winner look good to the manager, they made the winner look good to co-workers, luck should be shared, and other. Workers were only asked about reasons if they reported receiving a share; to make effects interpretable, we code the reasons outcomes as 0 when sharing was not reported. Thus the outcome in, say, column 2 is an indicator for receiving a share and this resulting from being friends/family.

Workers in the voting arm were 37.7 p.p. more likely to report they received a share because they voted for the winner, relative to a control mean of zero ($p < 0.01$). Combined with the rest of the results in the table, this estimate implies voting for the winner was the most common reason for sharing in the voting arm, provided by 44% of respondents who had received a share. There were also positive effects on receiving a share due to helping the winner with work or making the winner look good to co-workers, though these are much smaller in magnitude (6.8 and 0.7 p.p.). Neither treatment impacted receiving a share due to making the winner look good to the manager, but both tended to made workers less likely to report that the winner shared with them because luck should be shared, consistent with luck being seen as more important for determining winners in the control group. There is also evidence that the worker arm reduced sharing due to being friends/family, which was the most common reason for sharing in the other two treatment arms; this reduction suggests movement away from sharing due to social connections and towards sharing in exchange for votes in the worker arm.[23]

[23] Given the imbalances in worker attrition at EL2, we compute Lee bounds for the results in Table 5. Bounds are computed with clustered standard errors, and not adjusted for covariates in light of the fineness

In sum, rewards were more likely to be shared in the worker arm, with voting for the winner being the most common reason for sharing in this arm. More generally, these results speak to the strength of informal contracting among workers and its potential to interact with workplace incentives; workers appear to have been able to sustain collusive agreements to vote for winners in exchange for money, despite short-run incentives for winners to defect.

7 Mechanisms: Incentives to Win Versus Impacts of Winning

What explains the treatment effects on our main outcomes: attendance, productivity, and workplace interactions? Broadly speaking, there are two possible mechanisms. First, workers may have changed their behavior in order to win rewards (or to win a share of a reward). Second, winning itself may have affected winners' behavior in the workplace (e.g. due to reciprocity), and this effect may have differed based on how the rewards were allocated. To investigate, we estimate the impact of winning on our main outcomes using a shortlist design.

7.1 Shortlist Design and Estimation Strategy

We built a shortlist design into our experiment in order to estimate the impact of winning rewards. Estimating the impact of winning is straightforward in the control arm: the winners were chosen at random from the eligible workers, hence we can compare the winners to the rest of the eligible workers following the win to estimate the impact of winning. However, we cannot simply compare winners to other eligible workers in the manager or worker arms because the winners are likely to differ systematically from the others. For these arms, we compare the winners to the runners-up, with the idea that the runners-up are likely to be very similar to the winners. At the same time as managers were asked to identify winners in each

of our strata. The upper bound, lower bound, and associated p-values for the worker coefficients in columns 1-8, respectively, are: [0.186, p=0.004; 0.104, p=0.06], [-0.09, p=0.14; -0.18, p=0.006], [0.445, p=0.00; 0.441, p=0.00], [0.077, p=0.006; 0.077, p=0.006], [0.001, p=0.30; 0.001, p=0.30], [0.006, p=0.11; 0.006, p=0.11], [-0.047, p=0.003; -0.13, p=0.00], and [0.019, p=0.60; -0.06, p=0.18]. Thus the key results in the table – the worker arm increased sharing and increased sharing in exchange for votes – are robust. The rest of the worker-arm effects are largely robust as well, though some bounds for columns 2 and 6 fall just outside conventional levels for statistical significance. While the effects of the manager arm on the outcomes in this table are not a focus of the paper, we present the same results for the manager coefficients for completeness. We do not compute bounds for column 6 as no respondents in the manager or control arms have the value of 1 for that outcome, but the results for columns 1-5 and 7-8, respectively, are: [0.04, p=0.66; -0.075, p=0.35], [0.023, p=0.74; -0.09, p=0.18], [0.083, p=0.009; 0.08, p=0.01], [0.04, p=0.16; 0.04, p=0.16], [0.003, p=0.15; 0.003, p=0.15], [-0.01, p=0.27; -0.13, p=0.004], and [0.12, p=0.00; 0.003, p=0.95].

round, they were also asked to identify runners-up. Specifically, they were asked to share the names of weavers they believed were best after those they had identified as winners. They were asked to identify as many runners-up as winners, and asked for the reason for selecting each. Importantly, only the names of winners were revealed in the reward ceremonies, not the names of runners-up (or the reasons). In the voting arm, runners-up are the weavers with the highest vote counts after the winners.[24] As detailed above in Section 3.3, it was not revealed in the worker arm who the runners-up were.

We use two specifications to estimate impacts of winning on winners' behavior in the two weeks after their win, i.e. in the next reward cycle. The first pools all arms including the control group, comparing winners to runners-up with the following specification:

$$Y_{ijr} = \beta \text{Won}_{ij,r-1} + \alpha_s + \psi Y_{ij,r-1} + \gamma X_j + \varepsilon_{ijr} \tag{3}$$

where Y_{ijr} is an outcome for worker i in firm j in reward-cycle round r - for instance, number of days the worker attended in the cycle, or the total residualized knots they wove. $\text{Won}_{ij,r-1}$ is a dummy variable that takes the value of 1 if worker i won the reward in $r-1$, and zero if the worker was a runner up in $r-1$. The regression is restricted to winners and runners up in $r-1$, and r ranges from 2 to 6. α_s denote fixed effects for the stratification variable implicitly used in the shortlist design: i.e. firm $\times$ round fixed effects, meaning we compare winners and runners up in the same firm and cycle. We control for the value of the outcome in the previous round as well as Lasso controls, selected from the same set of baseline variables as the specifications in Section 4.2.[25] We cluster standard errors at the firm level for consistency with the regression below, where regressors of interest are defined at the firm level.

The second specification tests whether the impacts of winning vary by how the reward was allocated, i.e. at random, by the manager, or by worker vote. This regression takes the form:

$$Y_{ijr} = \beta_1 \text{Won}_{ij,r-1} + \beta_2 \text{Won}_{ij,r-1} \times T_{1j} + \beta_3 \text{Won}_{ij,r-1} \times T_{2j} + \alpha_s + \psi Y_{ij,r-1} + \gamma X_j + \varepsilon_{ijr} \tag{4}$$

[24] Both winners and runners-up are identified based on the records of who voted for whom. The winners as identified in this way vary slightly from the actual winners in the 3% of cases where surveyors mis-counted votes (see Section 3.3 for details), but this approach allows us to identify winners and runners up in the same way (surveyors were not asked for the runners-up according to their count) and can be seen as an "intent-to-treat" approach.

[25] We do not control for the baseline value of the outcome in the regressions in Section 4.2 because we do not have spot check data pre-treatment in our main experiment and many workers were not surveyed on the worker baseline.

where all variables are as defined in Equation 3, and T_{1j} and T_{2j} are indicators for the manager and worker vote treatments, respectively.[26] Standard errors are clustered by firm. We also present p-values for the effects of winning in the manager and worker arms (i.e. tests for $\beta_1 + \beta_2 = 0$ and $\beta_1 + \beta_3 = 0$, respectively), as well as p-values for equal effects of winning in the manager and worker arms (i.e. tests for $\beta_2 = \beta_3$).

7.2 Outcomes and Balance Checks

We consider impacts on: number of days attended, total residualized knots woven, and number of days observed in each of the three interaction types, all totaled across the fortnight following the win.[27]

Before presenting results, we conduct balance tests. We run the worker x round regressions detailed in the previous subsection for the five outcomes, but define the outcomes based on the previous fortnight, i.e. the reward cycle which chose the winner. Looking at the pooled estimates in Table A.7, we see that winners have greater attendance than runners-up, but the difference is small (0.152 days, 2% of the runner-up mean) and there are no significant differences in the other outcomes. Splitting by allocation mechanism in Table A.8, we see winners had higher baseline attendance than runners-up in the manager arm, but again the difference is small. There are no other significant differences within the various treatment arms, nor do any of the differences vary significantly across the treatment arms. In sum, winners and runners-up were similar in outcomes prior to winning, supporting the validity of our shortlist design. Nevertheless, we control for the baseline outcomes in our regressions to adjust for the small baseline differences and to improve power.

7.3 Effects of Winning Results

Tables 6 and 7 present the impacts of winning, estimating equations 3 and 4, respectively. First pooling across all treatment arms, we see no impacts on any of the main outcomes, indicating that winning a reward does not cause workers to increase effort or interact differently with their co-workers. Furthermore, these effects do not differ based on how rewards were allocated, with one exception: work-related interactions in the manager arm. Winning in the manager arm reduces such interactions, and this effect differs significantly from the

[26] Note T_{1j} and T_{2j} on their own, not interacted with $\text{Won}_{ij,r-1}$, are subsumed by α_s (firm $\times$ round fixed effects).

[27] We exclude the day of the reward ceremony itself. Section 5.1 details the construction of residualized knots at the firm x date level; we follow the same process to construct the knots variable here, except that in the final step we sum over residualized knots individual workers wove in the fortnight following the win, excluding the day of the reward ceremony itself.

corresponding effects in the worker and control arms. This could be because other workers resented winners selected by managers and were less likely to collaborate with them as a result, or because workers who won rewards allocated by managers felt less need to collaborate with their peers at work.

These results suggest that our main effects on attendance and productivity from Table 1 are unlikely to be driven by winning and more likely to come from incentives workers faced to win (or to receive a share of a reward). Note that worker voting incentivizing greater attendance is not at odds with the tendency of workers to reward non-productive behaviors; increased attendance may have been driven, at least in part, by workers attending in order to lobby and coordinate around voting and sharing rewards. On the other hand, the results presented in this section imply our main effect on work-related interactions from Table 2 is driven, at least in part, by the effect of winning.

Beyond speaking to mechanisms, the impacts of winning are relevant to the literature on effects of incentive pay and of employee recognition. Our results indicate that winning recognition has few impacts on workplace performance in the short term.

8 Other Outcomes

In this section, we present effects of managerial discretion and worker agency on several additional outcomes of interest, returning to the empirical strategy from Section 4.2. First, we consider specific aspects of firm culture as reported by workers in the two endlines (Table 8). We begin by considering measures of meritocracy, asking how our treatments changed workers' perceptions that effort/skill versus personal relationships determined how workers were rewarded in their firms (columns 1-3). We find that both arms increased perceptions that effort/skill were rewarded, but the effect of the manager arm was larger (0.25 versus 0.15 SD). However, increasing managerial discretion also appears to have increased perceived favoritism; perceptions that personal relationships with the manager are rewarded increased by 0.27 SD in the manager arm relative to the control arm, and by 0.21 SD relative to the worker arm. The manager and worker arms both increase perceptions of co-worker relationships being important, by 0.11 and 0.15 SD, respectively. The effect of the manager arm on this outcome is consistent with the result from Table A.6 that workers perceived managers rewarded helping other workers.

Table 8 also presents effects on two other broad aspects of firm culture. The first is a recognition index, including two questions on the extent to which workers felt managers and other workers appreciated their work. The manager arm significantly increased perceived recognition, but the worker arm did not (column 4). The second is a collegiality index,

comprised of five variables: perceived workplace competitiveness (reverse-coded), reports of giving and receiving help from co-workers, number of co-workers the respondent said they were close to, and number of co-workers the respondent said they would ask for work-related help. We see positive but statistically non-significant effects on this index from both treatment arms (column 5).[28]

Next, we present effects on daily earnings, as reported by the workers at the two endlines and winsorised at the 99th percentile (Table A.10). Daily earnings in the manager arm increase by ₹28.46 relative to the control group (p-value < 0.05), an increase of 12%. This is consistent with increased productivity in the manager arm. Earnings also increase in the worker vote arm, though the point estimate is lower (₹19.36, p-value < 0.10). This could indicate that attendance influences workers' compensation in this setting.

Table A.10 also presents effects on workers' psychology, measured by work locus of control (where questions pertain to jobs in general, not just the current workplace), generalized self-efficacy, self-reported weaving ability, and feelings of nervousness and depression. These outcomes are generally unaffected by our treatments. It is possible that changes in one's workplace can shift these downstream psychological outcomes, but that the changes from our treatments were not large or lasting enough to do so.

We also consider persistence of effects on attendance and productivity beyond the 12-week reward period. We conducted an additional four weeks of daily spot checks starting two weeks after rewards had been removed. Table A.11 presents effects on our main attendance and productivity outcomes (from Table 1), just measured after rewards had been removed. While all point estimates have the same sign as in Table 1, they are smaller in magnitude and none statistically significant; the effects faded once rewards had been removed.

Finally, we present heterogeneous effects on attendance and productivity by several pre-specified dimensions: gender, perceived importance of managerial relationship in rewarding workers, and preference for the worker vote over the manager discretion allocation mechanism.[29] We construct firm-level, baseline measures of these variables, considering heterogeneity by indicators for female firm, above median average importance of manager relationship, and above median share of workers preferring worker vote.

We find limited evidence of heterogeneity by these dimensions (Table A.12), though several findings are notable. First, while the treatment effects do not differ significantly by gender, the effect of the manager arm on productivity is driven by male centers. Second, firms with more workers preferring the worker vote experienced a negative effect on attendance

[28] Table A.9 presents effects on the components of these indices.

[29] As detailed in Appendix B, the second pre-specified dimension was managers reporting being close to workers, but this variable has very little variation so we replace it with the managerial relationship variable.

from the manager arm. Finally, the manager arm was worse for attendance when managerial relationships were more important, though this is also true of the worker arm.

9 Choice of Allocation Mechanism

Which allocation mechanism do individuals prefer? Does first-hand experience with an allocation mechanism affect preferences, i.e. are there treatment effects on preferences?

To get at this, we asked workers and managers at EL2 which allocation mechanism they would prefer if we ran the recognition program for two more weeks at their firm: worker voting or managerial discretion. We explained both options clearly so that all respondents understood what each entailed.[30] To incentivize responses, we implemented the chosen allocation of one randomly selected respondent in one randomly selected firm (and told respondents we would do so). The respondent whose choice was implemented remained anonymous.

The results are presented in Table 9, with column 1 presenting effects on workers' choices and column 2 effects on managers' choices. Two findings are particularly notable. First, the majority of workers and managers in the control group – 59% of workers and a striking 80% of managers – chose worker vote over managerial discretion, indicating a preference to give workers autonomy, at least in the short-run.

Second, there are treatment effects on preferences. The manager arm reduced managers' demand for worker democracy by 40.5 p.p., with a majority of managers in the discretion arm choosing to retain decision rights, while the worker arm had no significant effect on managers' preferences. This pattern of results is consistent with managers being uncertain about the returns to the two allocation mechanisms and those in the manager arm learning about the productivity gains. For workers, we find that experience with an allocation mechanism increased demand for it; the manager arm reduced the fraction of workers who chose the worker vote by 21.6 p.p., while the worker arm increased choice of the worker vote by 13.4 p.p. This could be explained through a learning mechanism like the managers' results, whereby workers learn through first-hand experience about the benefits of the various allocation mechanisms (e.g. increased earnings from the manager arm, or the ability to extract a share

[30]The manager option was explained as: "this option is for the reward to be allocated by your manager, [manager name], based on who, according to them, did good work in the two-week period." (This was revised very slightly in the manager survey to say "you" rather than "your manager, [manager name]" and "them".) The worker option was explained as: "this option is for a weaver vote to determine who gets the reward. A member of my team would call weavers in private one-by-one to vote for a winner. Weavers would be asked to vote for who, according to them, did good work in the two-week period. They would not be allowed to vote for themselves. My team member would then tally the votes and give the reward to the weaver with the most votes."

of rewards in the worker arm). Another plausible explanation is that workers are averse to change, which leads them to prefer the incentive structure they have had.[31]

10 Conclusion

In this paper, we show that managerial discretion and workplace democracy have nuanced effects on workers and firms, with both inducing some positive outcomes at the cost of others. In particular, managerial discretion in rewarding workers can increase productivity, but also reduce workplace interactions. On the other hand, workplace democracy can increase attendance without improving output. Furthermore, workers and managers reward different types of behaviors; winners chosen by managers are positively selected on attendance and productivity, while those selected by workers are positively selected on attendance and social interactions. We also document that democracy induces bargaining amongst workers, with winners being more likely to share their bonuses in exchange for support.

There are several important takeaways from our results. First, our findings underscore that what is valued in the workplace matters. We show differing effects on objectively measured outcomes that are important to firms – attendance, productivity, and worker interactions – based on whether workers or managers allocate rewards.

Second, we provide novel evidence on the effects of increasing worker agency. Firms have begun to increase agency given to workers, with many employers viewing worker agency as important for organizational success (Deloitte, 2023). Indeed, we saw 80% of managers in our control group picked worker agency over managerial discretion in an incentivized choice. Yet there is limited evidence on the returns to worker agency. Our results provide a cautionary tale for firms considering such policies. While our worker democracy treatment increased worker attendance, firms were not able to convert this into greater output. Further, our data indicate that workers colluded to reward those who would split the rewards with them. These results also raise additional questions, such as how democratic processes in other firm decisions may impact outcomes, or whether different forms of voting rules lead to different workers being rewarded; these, and related questions, are interesting topics for future research.

While managerial discretion improved productivity, it did not come without tradeoffs. In particular, the reductions in work-related interactions have concerning implications for

[31] We compute Lee bounds for column 1 since there was imbalance in worker attrition at EL2. Bounds are computed with clustered standard errors, and without covariate adjustments given the fineness of our strata. The upper bound, lower bound, and associated p-values for the manager and worker coefficients, respectively, are: [-0.168, p=0.02; -0.285, p=0.00] and [0.195, p=0.00; 0.116, p=0.03]. Thus both effects are robust to Lee bounding.

knowledge spillovers between workers. Moreover, our results indicate that managerial discretion increased workers' perceptions of favoritism. This is despite the fact that winners in the manager arm were positively selected on attendance and productivity, and the fact that managers generally chose to rotate rewards across workers, with just 7% of manager-arm winners being repeat winners. This suggests worker perceptions of favoritism may be difficult to change when rewards are allocated by managers. An open question, which future research could investigate, is whether the gains in productivity from the manager arm would have been larger had they not been accompanied by perceptions of favoritism.

Finally, the sharing of rewards in exchange for votes in the worker arm highlights the strength of informal contracting between workers and its potential to interact with workplace incentives. Workers appear able to sustain such arrangements despite short-run incentives for winners to renege and keep the full reward, consistent with the presence of repeated interactions or social enforcement in the workplace. Such arrangements can interact with workplace democracy initiatives, potentially weakening the link between rewards and behaviors firms might like to incentivize. More generally, these results underscore that the effectiveness of workplace policies depends not only on their formal structures, but also on how workers collectively respond to and potentially circumvent them. Understanding interactions between formal incentives and informal contracting in the workplace remains an important direction for future research.

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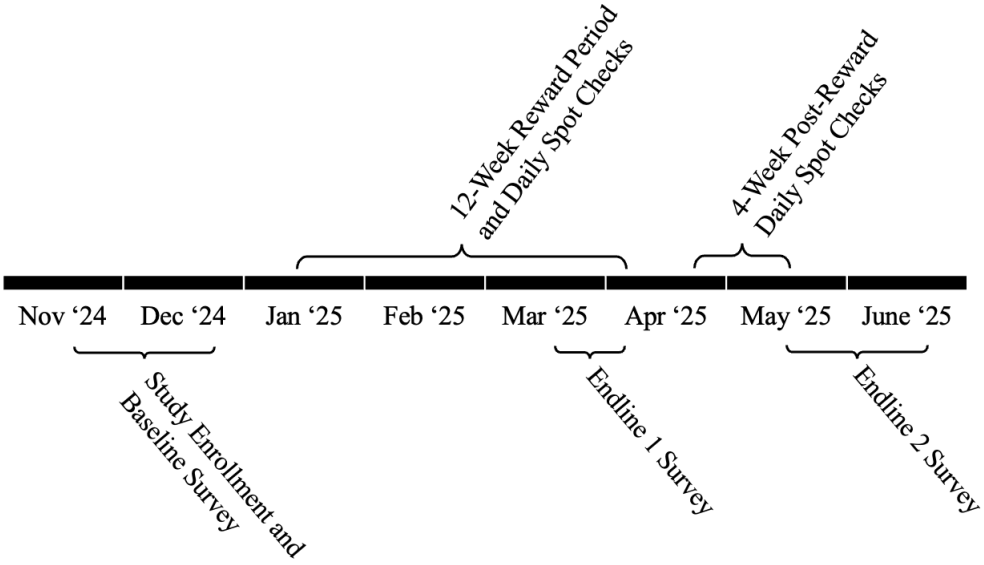
Figures and Tables

Figure 1: Production of a Hand-Knotted Carpet

Notes: This image visualizes the production process for a hand-knotted carpet. In it, three women are seated at a loom and weaving a carpet.

Figure 2: Study Timeline



Notes: This image visualizes the timeline of the study.

Figure 3: Reward Cycle Timeline

M	Attendance check (and reward ceremony for previous cycle)
Tu	Attendance check
W	Attendance check
Th	Attendance check
F	Attendance check
Sa	Attendance check
Su	Off
M	Attendance check
Tu	Attendance check
W	Attendance check
Th	Attendance check
F	Attendance check
Sa	Attendance check
Su	Off
M	Reward ceremony (and attendance check for next cycle)

Notes: This image visualizes the timeline of a typical reward cycle.

Table 1: Effects on Attendance and Productivity

	Fraction of Workers Present	Knots (Res.)
	(1)	(2)
Manager	-0.014	13696.411**
	(0.025)	(6433.085)
Worker Vote	0.041*	-5090.582
	(0.023)	(5697.610)
P-Val: M=W	0.051*	0.005***
Data Structure	Firm-Date	Firm-Date
Strata FE	Yes	Yes
Round FE	Yes	Yes
Lasso BL Var	Yes	Yes
Control Mean	0.483	-8514.794
N	8308	7664

Notes: The outcomes are from the spot check data and are at the firm x date level. The outcome in column (1) is the fraction of workers present. The outcome in column (2) is the total residualized knots woven. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Interactions in the Workplace

	Work-Related Interactions (=1) (1)	Social Interactions (=1) (2)	Talking to Manager (=1) (3)
Manager	-0.093***	-0.070	-0.009
	(0.025)	(0.051)	(0.007)
Worker Vote	-0.018	-0.018	-0.006
	(0.025)	(0.044)	(0.007)
P-Val: M=W	0.001***	0.268	0.698
Data Structure	Firm-Date	Firm-Date	Firm-Date
Strata FE	Yes	Yes	Yes
Round FE	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes
N Workers Present	Yes	Yes	Yes
Control Mean	0.237	0.405	0.033
N	8308	8308	8308

Notes: The outcomes are from the spot check data and are at the firm x date level. Each outcome equals one if at least one interaction of the listed type was observed anywhere in the firm on that date. The outcome in column (1) captures all work-related interactions. The outcome in column (2) captures social interactions. The outcome in column (3) captures conversations with the manager. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. "N Workers Present" denotes the number of workers present in the firm on the given day. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Descriptive Analysis of Winners

	Attendance Percentile	Knots (Res.) Percentile	Work Interact. Percentile	Social Interact. Percentile
	(1)	(2)	(3)	(4)
Manager	0.103***	0.043**	0.012	0.010
	(0.013)	(0.017)	(0.011)	(0.013)
Worker Vote	0.064***	0.009	0.020	0.042***
	(0.012)	(0.015)	(0.012)	(0.013)
P-Val: M=W	0.034**	0.126	0.657	0.071*
Data Structure	Winner-Round	Winner-Round	Winner-Round	Winner-Round
Strata FE	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No
Control Mean	0.533	0.521	0.533	0.533
N	4525	4395	4525	4525

Notes: The outcomes are drawn from the spot check data, and are at the winner-round level. Each is expressed as a within firm-round percentile rank among all eligible workers, calculated by ranking individuals on the relevant measure (ties allowed and higher ranks are better) and then dividing by the total number of eligible workers in that firm-round. Column (1) reports percentiles of the number of days attended. Column (2) reports percentiles of knots per day, winsorized at the 99th percentile and residualized. Column (3) reports the percentile of work-interaction frequency. Column (4) reports the percentile of social interaction frequency. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Number of Winners

	Number of Winners	Number of Repeat Winners
	(1)	(2)
Manager	0.611	-1.118***
	(0.740)	(0.290)
Worker Vote	-0.345	0.004
	(0.631)	(0.286)
P-Val: M=W	0.252	0.000***
Data Structure	Firm	Firm
Strata FE	Yes	Yes
Lasso BL Var	Yes	Yes
Control Mean	7.951	1.732
N	124	124

Notes: The outcomes are from the reward ceremony data and are at the firm-level. The outcome in column (1) is the number of workers in the firm who ever won a reward. The outcome in column (2) is the number of workers in the firm who won more than once. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Robust standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Sharing Behavior and Motivations

	Winner Shared with Me (=1)	Reasons for Sharing						
	(1)	Friends/ Family (=1) (2)	Voted For Winner (=1) (3)	Helped With Work (=1) (4)	Made Them Look Good to Manager (=1) (5)	Made Them Look Good to Co-Workers (=1) (6)	Luck Should be Shared (=1) (7)	Other (8)
Manager	-0.020	-0.001	0.048	0.027*	0.004	0.001	-0.116***	-0.009
	(0.072)	(0.068)	(0.031)	(0.015)	(0.002)	(0.002)	(0.023)	(0.041)
Worker Vote	0.142***	-0.136***	0.377***	0.068***	0.001	0.007**	-0.130***	-0.042
	(0.053)	(0.047)	(0.034)	(0.019)	(0.001)	(0.003)	(0.026)	(0.038)
P-Val: M=W	0.007***	0.017**	0.000***	0.042**	0.241	0.023**	0.484	0.395
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.717	0.465	0.004	0.000	0.000	0.000	0.130	0.192
N	2044	2043	2043	2043	2043	2043	2043	2043

Notes: The outcomes are from the second endline and are at the worker level. The outcome in column (1) is a dummy indicating whether the respondent received a share of another winner's reward. The outcomes in columns (2)-(8) are dummies for receiving a share *and* providing a given reason for having done so: family or friendship ties (2), voted for winner (3), helped with work (4), made winner look good to the manager (5), made winner look good to other workers (6), the belief that luck should be shared (7), and other (8). Respondents could provide multiple reasons. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of Winning on Outcomes in the Next Fortnight

	Attendance (1)	Knots (Res.) (2)	Work-Related Interactions (3)	Social Interactions (4)	Talking to Manager (5)
Winner	0.044	-638.334	0.008	0.050	-0.001
	(0.111)	(1176.860)	(0.024)	(0.052)	(0.009)
Data Structure	Worker x Round	Worker x Round	Worker x Round	Worker x Round	Worker x Round
Firm x Round FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes
Outcome in Previous Fortnight	Yes	Yes	Yes	Yes	Yes
Runner-up Mean	7.076	-10209.909	0.379	1.232	0.047
N	4287	3926	4287	4287	4287

Notes: The outcomes are from the spot check data and are at the worker x round level. Each outcome is measured for a given worker and reflects their total activity over the next fortnight, excluding the day of the reward ceremony. All regressions control for the corresponding outcome measured over the previous two weeks, excluding the day of the reward ceremony. The sample includes only shortlisted workers (winners and runners-up) in each round. The outcome in column (1) reports the total number of days attended. The outcome in column (2) reports the total residualized knots woven. The outcomes in columns (3)-(5) are the total number of days observed in an interaction of a given type: any work-related interaction (3), social interaction (4), and conversation with the manager (5). "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of Winning on Outcomes in the Next Fortnight (Treatment Heterogeneity)

	Attendance (1)	Knots (Res.) (2)	Work-Related Interactions (3)	Social Interactions (4)	Talking to Manager (5)
Winner	0.135	744.930	0.035	0.049	0.007
	(0.165)	(1434.477)	(0.031)	(0.082)	(0.014)
Winner x Manager	-0.310	-2635.400	-0.109**	-0.072	-0.013
	(0.256)	(2835.635)	(0.046)	(0.109)	(0.019)
Winner x Worker Vote	-0.036	-3499.604	0.015	0.110	-0.020
	(0.242)	(3547.192)	(0.073)	(0.132)	(0.021)
P-Val: W+WxM=0	0.370	0.435	0.028**	0.743	0.608
P-Val: W+WxW=0	0.581	0.398	0.448	0.126	0.387
P-Val: WxM=WxW	0.301	0.831	0.093*	0.149	0.734
Data Structure	Worker x Round	Worker x Round	Worker x Round	Worker x Round	Worker x Round
Firm x Round FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes
Outcome in Previous Fortnight	Yes	Yes	Yes	Yes	Yes
Runner-up Mean	7.076	-10209.909	0.379	1.232	0.047
N	4287	3926	4287	4287	4287

Notes: The outcomes are from the spot check data and are at the worker x round level. Each outcome is measured for a given worker aggregated over the next fortnight, excluding the day of the reward ceremony. All regressions control for the corresponding outcome measured over the previous two weeks, excluding the day of the reward ceremony. The sample includes only shortlisted workers (winners and runners-up) in each round. The outcome in column (1) reports the total number of days attended. The outcome in column (2) reports the total residualized knots woven. The outcomes in columns (3)-(5) are the total number of days observed in an interaction of a given type: any work-related interaction (3), social interaction (4), and conversation with the manager (5). "P-Val: W+WxM=0" reports the p-value from a test that the sum of the coefficients on "Winner and Winner x Manager" is equal to zero. "P-Val: W+WxW=0" reports the p-value from a test that the sum of the coefficients on "Winner and Winner x Worker Vote" is equal to zero. "P-Val: WxM=WxW" reports the p-value from a test of equality between the coefficients on "Winner x Manager" and "Winner x Worker Vote". "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Survey Measures of Firm Culture

	Meritocracy Outcomes			Recognition Index	Collegiality Index
	Effort/Skill Important	Managerial Relationship Important	Worker Relationship Important		
	(1)	(2)	(3)	(4)	(5)
Manager	0.253***	0.268***	0.113*	0.116**	0.073
	(0.054)	(0.081)	(0.067)	(0.052)	(0.126)
Worker Vote	0.148**	0.056	0.149*	0.021	0.130
	(0.063)	(0.083)	(0.079)	(0.056)	(0.116)
P-Val: M=W	0.037**	0.011**	0.606	0.066*	0.641
Data Structure	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes
Control Mean	0.000	0.000	0.000	0.000	0.000
N	2272	2271	2268	2274	2274

Notes: Outcomes are from the combined endline surveys and are measured at the worker level. The sample uses first endline survey responses when available; otherwise, it uses the second endline responses for workers not surveyed in the first endline but surveyed in the second. The outcome in columns (1)-(3) report standardized measures constructed from 4-point Likert-scale items (1 = strongly disagree, 4 = strongly agree) based on agreement with the following questions: (1) whether hard work and skill are important for determining how workers are rewarded, (2) whether workers' personal relationship with the owner/manager is important for determining how they are rewarded, and (3) whether workers' personal relationships with each other are important for determining how they are rewarded. The outcome in column (4) reports the Recognition Index, a standardized index combining two 4-point Likert-scale survey questions on whether the respondent feels that the manager and other workers appreciate the work they do in the firm. The outcome in column (5) reports the Collegiality Index, a standardized index combining workplace competitiveness (4-point Likert scale, reverse coded), asking and giving work-related help in the past week (both indicator variables), the number of coworkers the respondent said they were close to, and the number of coworkers the respondent said they would ask for work-related help. Effects on the components of the indices in columns (4) and (5) are reported in Table A.9. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Preference for Worker versus Manager Allocation

	Picked Worker Vote (=1)	Picked Worker Vote (=1)
	(1)	(2)
Manager	-0.216***	-0.405***
	(0.044)	(0.103)
Worker Vote	0.134***	-0.014
	(0.037)	(0.093)
P-Val: M=W	0.000***	0.000***
Data Structure	Worker	Manager
Strata FE	Yes	Yes
Lasso BL Var	Yes	Yes
Control Mean	0.591	0.800
N	2045	122

Notes: The outcomes are from the second endline survey data. Column (1) is at the worker level and column (2) is at the manager level. The outcomes in columns (1) and (2) are dummy variables indicating whether the respondent preferred the worker-vote mechanism for allocating rewards (zero by construction for the control group). "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level in column (1), and robust standard errors are reported in column (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A: Additional Tables and Figures

Table A.1: Baseline Characteristics

	Any Education (=1) (1)	Attendance Last Week (0-7) (2)	Common Subcaste (=1) (3)	Years of Experience (4)	Financial Status (1-10) (5)	Migrant (=1) (6)	Prefers Workers Allocate (=1) (7)	Log(Monthly Earnings) (8)
Manager	-0.020	-0.029	-0.016	0.105	-0.160	0.105*	0.052	0.073
	(0.038)	(0.023)	(0.058)	(1.122)	(0.132)	(0.061)	(0.038)	(0.052)
Worker Vote	0.027	-0.040*	0.031	-0.488	-0.123	0.163***	0.048	0.067
	(0.036)	(0.022)	(0.052)	(0.879)	(0.118)	(0.044)	(0.034)	(0.043)
P-Val: M=W	0.209	0.666	0.432	0.578	0.751	0.343	0.905	0.914
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No	No	No	No	No
Control Mean	0.523	5.269	0.417	25.069	2.684	0.061	0.419	8.673
N	1554	1546	1554	1554	1552	1554	1529	1538

	Self-Reported Ability (1-10) (9)	Manager Relationship Important (1-4) (10)	Effort/Skill Important (1-4) (11)	Tokens Sent in Dictator Game (0-5) (12)	Number Ask for Help From (13)	Number Close to (14)	Workplace Competitive (1-4) (15)
Manager	0.148	-0.140	-0.059	0.008	-0.182	0.372	0.013
	(0.211)	(0.113)	(0.063)	(0.055)	(0.366)	(0.403)	(0.101)
Worker Vote	0.119	0.116	-0.006	-0.027	-0.137	0.036	0.141
	(0.180)	(0.093)	(0.051)	(0.047)	(0.366)	(0.415)	(0.095)
P-Val: M=W	0.892	0.038**	0.393	0.457	0.891	0.373	0.238
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No	No	No	No
Control Mean	7.076	2.576	3.649	0.275	2.473	2.343	2.569
N	1546	1538	1543	1554	1554	1554	1546

Notes: The outcomes are from the worker baseline survey. The outcome in column (1) is a dummy for having ever attended school. The outcome in column (2) is self-reported attendance in the past week (0-7 days). The outcome in column (3) is a dummy for belonging to one of the most common subcastes, including Chamar (Jatav), Harijan, Goutam, Bharti, Rahdas, Kori, Bind, Mallah, Kebat, or Nishad. The outcome in column (4) is years of weaving experience. The outcome in column (5) is self-assessed financial status on a 1-10 scale. The outcome in column (6) is a dummy for reporting residence in the firm premises (which migrant workers often do). The outcome in column (7) is a dummy for the respondent preferring the worker vote to manager discretion allocation mechanism. The outcome in column (8) is the log of monthly weaving earnings. The outcome in column (9) is self-rated weaving ability on a 1-10 scale. The outcomes in columns (10) and (11) are the perceived importance of personal relationships with the manager and hard work/skill, respectively, for determining how workers are rewarded, both on a 4-point Likert scale. The outcome in column (12) is the number of tokens sent in a dictator game (0-5). The outcome in column (13) is the number of coworkers the respondent said they would ask for work-related help. The outcome in column (14) is the number of coworkers the respondent listed as being close to. The outcome in column (15) measures the perceived competitiveness of the workplace on a 1-4 Likert scale. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Endline Survey Response Rates

	Surveyed EL1 (=1) (1)	Surveyed EL2 (=1) (2)	Surveyed Either (=1) (3)	Surveyed EL1, if Surveyed Either (=1) (4)	Surveyed EL1 (=1) (5)	Surveyed EL2 (=1) (6)	Surveyed Either (=1) (7)	Surveyed EL1, if Surveyed Either (=1) (8)
Manager	-0.003	-0.080**	-0.036	0.030	-0.000	0.005	0.025	-0.025
	(0.034)	(0.040)	(0.027)	(0.031)	(0.032)	(0.031)	(0.022)	(0.023)
Worker Vote	-0.021	-0.057*	-0.019	-0.008	0.024	0.029	0.024	-0.001
	(0.033)	(0.034)	(0.026)	(0.028)	(0.026)	(0.025)	(0.022)	(0.014)
P-Val: M=W	0.580	0.586	0.557	0.223	0.362	0.419	0.983	0.275
Data Structure	Worker	Worker	Worker	Worker	Manager	Manager	Manager	Manager
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No	No	No	No	No
Control Mean	0.657	0.771	0.822	0.799	0.976	0.976	0.976	1.000
N	2847	2847	2847	2277	124	124	124	123

Notes: The outcomes are from the first and second endline surveys (EL1 and EL2, respectively). Columns (1)-(4) report worker-level outcomes, and columns (5)-(8) report the same outcomes measured at the manager (firm) level. The sample in columns (1)-(4) includes all workers ever observed during a spot check in the 12-week intervention period. Column (4) additionally restricts to workers surveyed at either endline. The outcome in columns (1) and (4) is a dummy for completing EL1. Column (2)'s outcome is a dummy for completing EL2, while column (3)'s outcome is a dummy for completing either endline. The sample in columns (5)-(8) includes all managers (or, equivalently, all firms), while column (8) additionally restricts to managers surveyed at either endline. The outcomes in columns (5)-(8) mirror the outcomes in columns (1)-(4), but reflect survey completion of managers. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level in columns (1)-(4) and are robust in columns (5)-(8). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Additional Analyses of Attendance and Productivity

	Knots (Res.) Missing (=1) (1)	Late Entrants (post week 2) (2)	Early Exits (post week 2) (3)	Eligible (=1) (4)	Knots (Res.) + -1 × Min (5)
Manager	0.024	0.178	0.062	-0.003	13696.411**
	(0.029)	(0.888)	(0.254)	(0.021)	(6433.085)
Worker Vote	0.016	-0.527	0.006	0.045**	-5090.582
	(0.028)	(0.857)	(0.240)	(0.022)	(5697.610)
P-Val: M=W	0.778	0.469	0.806	0.024**	0.005***
Data Structure	Firm-Date	Firm	Firm	Worker x Round	Firm-Date
Strata FE	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	No	No	Yes	Yes
Lasso BL Var	No	Yes	Yes	Yes	Yes
Control Mean	0.063	5.390	0.732	0.639	221281.631
N	8308	124	124	17082	7664

Notes: The outcomes are from the Spot Check data. Column (1) is at the firm x date level, columns (2)-(3) at the firm level, and column (4) at the worker x round level. The outcome in column (1) is a dummy for the productivity outcome in column (2) of Table 1 being missing. The outcome in column (2) captures the number of workers who entered after the first two weeks of spot checks, meaning they were absent in weeks 1-2 but appeared at least once in weeks 3-12. The outcome in column (3) captures the number of workers who exited after the first two weeks, meaning they were observed in weeks 1-2 but never appeared in weeks 3-12. The outcome in column (4) is a dummy indicating whether a worker was eligible in a given round of rewards. The outcome in column (5) is the residualized knots variable from column (2) of Table 1 plus -1 × the variable's minimum. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level in columns (1) and (4), and are robust in columns (2)-(3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Effects on Defects

	Any Defect (=1): Jan-March
	(1)
Manager	0.038
	(0.279)
Worker Vote	-0.127
	(0.277)
P-Val: M=W	0.528
Data Structure	Manager
Strata FE	Yes
Lasso BL Var	Yes
Control Mean	0.650
N	122

Notes: The outcome is from the second endline with managers. It is a dummy (=1) indicating whether any defect was reported in the firm in any month from January to March. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Impacts on Managerial Presence and Behavior

	Manager Present (=1) (1)	Monitoring/ Supervising (=1) (2)	Helping Worker(s) (=1) (3)	Office Work (=1) (4)	Weaving Carpet (=1) (5)	Non-Weaving Work (=1) (6)	On Break (=1) (7)	Other (=1) (8)
Manager	0.006	-0.002	-0.001	-0.001	0.002	0.011	-0.025	0.000
	(0.032)	(0.028)	(0.011)	(0.014)	(0.026)	(0.019)	(0.025)	(0.003)
Worker Vote	0.014	0.012	-0.012	0.028	0.026	-0.005	-0.009	-0.004
	(0.034)	(0.029)	(0.010)	(0.018)	(0.029)	(0.017)	(0.027)	(0.003)
P-Val: M=W	0.826	0.651	0.301	0.077*	0.395	0.343	0.493	0.138
Data Structure	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.466	0.135	0.051	0.063	0.045	0.075	0.132	0.008
N	8308	8308	8308	8308	8308	8308	8308	8308

Notes: The outcomes are from the Spot Check data and are at the firm x date level. Each outcome is a dummy (=1) indicating that the manager engaged in the listed activity. The outcome in column (1) indicates that the manager was present when the surveyor visited for the spot check. The outcomes in columns (2)-(8) indicate that at least one instance of the activity was observed that day: monitoring/supervising (2), helping workers (3), office work (4), weaving a carpet (5), non-weaving work (e.g., taana, chunai, jeri) (6), on break (7), or other (8). "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Beliefs about Types of Weavers Who Won

	Good Attendance (=1)	Many Knots/ Complex Carpets (=1)	Other Effort/ Productivity (=1)	Helps Other Workers (=1)	Helps Manager (=1)	Non-Productive, Worker-Related (=1)	Non-Productive, Manager-Related (=1)	Any Non-Productive (=1)	Financial/Other Hardship (=1)	Luck/ Random (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Manager	0.256***	0.176***	0.354***	0.115**	0.014*	0.147***	0.138***	0.236***	0.005	-0.628***
	(0.049)	(0.039)	(0.041)	(0.045)	(0.008)	(0.050)	(0.041)	(0.052)	(0.006)	(0.038)
Worker Vote	0.201***	0.169***	0.288***	0.217***	0.003	0.447***	0.015	0.473***	0.009*	-0.700***
	(0.039)	(0.033)	(0.036)	(0.041)	(0.006)	(0.043)	(0.038)	(0.042)	(0.005)	(0.033)
P-Val: M=W	0.265	0.865	0.146	0.052*	0.289	0.000***	0.006***	0.000***	0.537	0.057*
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.196	0.164	0.226	0.126	0.003	0.196	0.049	0.197	0.008	0.749
N	1814	1814	1814	1814	1814	1814	1814	1814	1814	1814

Notes: The outcomes are at the worker level. They are from a question on the first endline asking workers what types of workers generally won the rewards in their firm. Workers could choose multiple responses, and the outcomes are dummies (=1) for providing particular response(s). The outcome in column (1) is an indicator for providing good attendance. The outcome is column (2) is an indicator for providing weaves many knots or weaves complex carpets. The outcome in column (3) includes other effort/productivity-related traits (namely, makes few mistakes, does non-weaving work/does it well, works quickly, meets deadlines, tries hard, and takes initiative). The outcomes in columns (4) and (5) reflect helping other weavers at work and helping the manager at work, respectively. The outcomes in columns (6) and (7) capture non-productive traits, divided into co-worker-related factors (nice to co-workers, promises to share reward with co-workers, most popular, and campaigned for votes) and manager-related factors (nice to manager, promises to share reward with manager, and manager's favorite/friend/relative). The outcome in column (8) pools all non-productive reasons, co-worker- or manager-related. The outcome in column (9) reflects financial or other hardship. The outcome in column (10) includes luck/karma and at random/no discernible pattern. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Differences in Outcomes Between Winners and Runners-Up: Previous Fortnight

	Attendance (1)	Knots (Res.) (2)	Work-Related Interactions (3)	Social Interactions (4)	Talking to Manager (5)
Winner	0.152**	1382.895	-0.006	-0.001	0.009
	(0.075)	(1684.944)	(0.028)	(0.058)	(0.009)
Data Structure	Worker x Round	Worker x Round	Worker x Round	Worker x Round	Worker x Round
Firm x Round FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No	No
Runner-up Mean	7.557	-3663.961	0.503	1.344	0.058
N	4287	4152	4287	4287	4287

Notes: The outcomes are from the spot check data and are at the worker x round level. Each outcome is measured for a given worker and reflects their total activity over the previous fortnight, excluding the day of the reward ceremony. The sample includes only shortlisted workers (winners and runners-up) in each round. The outcome in column (1) reports the total number of days attended. The outcome in column (2) reports the total residualized knots woven. The outcomes in columns (3)-(5) are the total number of days observed in an interaction of a given type: any work-related interaction (3), social interaction (4), and conversation with the manager (5). "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Differences in Outcomes Between Winners and Runners-Up: Previous Fortnight (Treatment Heterogeneity)

	Attendance (1)	Knots (Res.) (2)	Work-Related Interactions (3)	Social Interactions (4)	Talking to Manager (5)
Winner	0.154	930.681	-0.015	-0.002	0.012
	(0.108)	(2055.029)	(0.034)	(0.088)	(0.009)
Winner x Manager	0.120	2969.644	-0.023	-0.065	-0.002
	(0.160)	(3769.292)	(0.058)	(0.107)	(0.018)
Winner x Worker Vote	-0.186	-1901.816	0.076	0.101	-0.015
	(0.201)	(5394.828)	(0.094)	(0.173)	(0.035)
P-Val: W+WxM=0	0.022**	0.219	0.434	0.264	0.516
P-Val: W+WxW=0	0.851	0.846	0.485	0.508	0.929
P-Val: WxM=WxW	0.141	0.411	0.325	0.302	0.724
Data Structure	Worker x Round	Worker x Round	Worker x Round	Worker x Round	Worker x Round
Firm x Round FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No	No
Runner-up Mean	7.557	-3663.961	0.503	1.344	0.058
N	4287	4152	4287	4287	4287

Notes: The outcomes are from the spot check data and are at the worker x round level. Each outcome is measured for a given worker and reflects their total activity over the previous fortnight, excluding the day of the reward ceremony. The sample includes only shortlisted workers (winners and runners-up) in each round. The outcome in column (1) reports the total number of days attended. The outcome in column (2) reports the total residualized knots woven. The outcomes in columns (3)-(5) are the total number of days observed in an interaction of a given type: any work-related interaction (3), social interaction (4), and conversation with the manager (5). "P-Val: W+WxM=0" reports the p-value from a test that the sum of the coefficients on "Winner and Winner x Manager" is equal to zero. "P-Val: W+WxW=0" reports the p-value from a test that the sum of the coefficients on "Winner and Winner x Worker Vote" is equal to zero. "P-Val: WxM=WxW" reports the p-value from a test of equality between the coefficients on "Winner x Manager" and "Winner x Worker Vote". "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Components of Recognition and Collegiality Indices

	Manager Appreciates (1)	Worker Appreciates (2)	Disagree Workplace Competitive (3)	Asked for Help (4)	Gave Help (5)	Number Close to (6)	Number Ask for Help From (7)
Manager	0.089**	0.087	0.087	0.049	0.083	0.043	0.042
	(0.044)	(0.058)	(0.084)	(0.072)	(0.077)	(0.126)	(0.128)
Worker Vote	0.056	-0.007	0.025	-0.008	0.108	0.250**	0.090
	(0.050)	(0.054)	(0.078)	(0.065)	(0.086)	(0.111)	(0.117)
P-Val: M=W	0.442	0.076*	0.391	0.394	0.740	0.109	0.716
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N	2274	2273	2272	2274	2274	2228	2240

Notes: This table presents effects on the components of the recognition and collegiality indices from Table 8. The outcomes in columns (1)-(3) report standardized measures constructed from 4-point Likert-scale items (1 = strongly disagree, 4 = strongly agree): (1) whether the respondent feels that the manager appreciates the work they do, (2) whether the respondent feels that other workers in the firm appreciate them, and (3) whether the respondent feels that workers in the firm are competitive with each other, reverse coded. Columns (4) and (5) report standardized indicators (0/1) for whether, in the past week, the respondent (4) asked another worker for work-related help and (5) provided another worker work-related help. Columns (6) and (7) report standardized counts from roster questions: (6) the number of coworkers the respondent listed as being close to, and (7) the number of coworkers the respondent said they would ask for work-related help if they needed it. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Effects on Worker Earnings and Psychology

	Daily Earnings (1)	Work Locus of Control (2)	GSE (1-4) (3)	Self-Reported Weaving Ability (1-10) (4)	Nervous (1-5) (5)	Depressed (1-5) (6)
Manager	28.456**	-0.004	0.025	0.103	-0.104	-0.009
	(12.799)	(0.053)	(0.037)	(0.185)	(0.067)	(0.088)
Worker Vote	19.363*	0.037	-0.013	0.012	-0.107*	-0.035
	(10.082)	(0.054)	(0.037)	(0.143)	(0.063)	(0.074)
P-Val: M=W	0.436	0.460	0.311	0.597	0.962	0.754
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	239.071	0.007	3.614	7.302	1.797	2.116
N	2238	2274	2272	2273	2274	2273

Notes: Outcomes are from the combined endline surveys and are measured at the worker level. The sample uses first endline survey responses when available; otherwise, it uses the second endline responses for workers not surveyed in the first endline but surveyed in the second. The outcome in column (1) is winsorized daily earnings. Daily earnings are imputed using respondents' reported weekly earnings. The outcome in column (2) is a Work Locus of Control index, including three items from the Spector (1988) scale measuring beliefs about jobs in general, not just the current workplace. The three items, each measured on a 4-point Likert scale from strongly disagree to strongly agree are: (i) to make a lot of money you have to know the right people (reverse coded), (ii) people who perform their jobs well generally get rewarded, and (iii) the main difference between people who make a lot of money and people who make a little is luck (reverse coded). The outcome in column (3) is an item from the Schwarzer and Jerusalem (1995) General Self-Efficacy (GSE) scale that reflects agreement, on a 4-point Likert scale, with: I can solve most problems if I invest the necessary effort. The outcome in column (4) captures self-reported weaving ability on a 10-point scale (1 = lowest, 10 = highest). The outcomes in columns (5) and (6) measure nervousness and depression, respectively, using two questions based on items from the K6 (Kessler et al., 2003) that asked how often the respondent felt nervous and depressed during the past 30 days. Responses were recorded on a 5-point scale ranging from none of the time (1) to all of the time (5). "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Effects on Attendance and Productivity, Post Rewards

	Fraction of Workers Present	Knots (Res.)
	(1)	(2)
Manager	-0.004	2994.947
	(0.023)	(6309.688)
Worker Vote	0.023	-4200.862
	(0.022)	(6620.022)
P-Val: M=W	0.271	0.293
Data Structure	Firm-Date	Firm-Date
Strata FE	Yes	Yes
Round FE	No	No
Lasso BL Var	Yes	Yes
Control Mean	0.399	14185.027
N	2852	2578

Notes: The outcomes are from the spot checks done after the 12-week intervention period and are at the firm x date level. The outcome in column (1) is the fraction of workers present. The outcome in column (2) is the total residualized knots woven. "P-Val: M=W" reports the p-value from a test of equality between the coefficients on the manager arm and the worker-vote arm. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Heterogeneity in Attendance and Productivity Effects

	Fraction of Workers Present	Knots (Res.)	Fraction of Workers Present	Knots (Res.)	Fraction of Workers Present	Knots (Res.)
	(1)	(2)	(3)	(4)	(5)	(6)
Manager	-0.023	15465.439**	0.065	-2423.821	0.033	23163.396*
	(0.027)	(7275.294)	(0.041)	(8146.456)	(0.031)	(12182.350)
Worker Vote	0.037	-7643.966	0.084**	-4024.055	0.189***	-4362.782
	(0.026)	(6149.724)	(0.035)	(9525.711)	(0.035)	(10675.422)
Female Center	0.227	-46432.907***				
	(0.167)	(10944.553)				
Female Center X Manager	0.053	-9396.971				
	(0.067)	(14438.392)				
Female Center X Worker Vote	0.024	14945.555				
	(0.059)	(15679.331)				
Above Median Preferring Worker Vote			0.031	-1767.682		
			(0.039)	(11951.946)		
Above Median Preferring Worker Vote X Manager			-0.148**	26384.474		
			(0.067)	(18689.732)		
Above Median Preferring Worker Vote X Worker Vote			-0.091	-3044.336		
			(0.059)	(13248.457)		
Above Median Manager Relationship Important					0.115***	11510.721
					(0.036)	(10318.326)
Above Median Manager Relationship Important X Manager					-0.094*	-20164.335
					(0.053)	(16826.013)
Above Median Manager Relationship Important X Worker Vote					-0.275***	-2737.288
					(0.055)	(14749.816)
P-Val: M+MxHetVar=0	0.628	0.628	0.047**	0.087*	0.134	0.705
P-Val: W+WxHetVar=0	0.255	0.613	0.875	0.364	0.014**	0.371
Data Structure	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.483	-8514.794	0.483	-8514.794	0.483	-8514.794
N	8308	7664	8308	7664	8308	7664

Notes: The outcomes are from the spot check data and are at the firm x date level. The outcome in columns (1), (3), and (5) is the fraction of workers present. The outcome in columns (2), (4), and (6) is the total residualized knots woven. Heterogeneity is captured using three baseline indicators. "Female Center" equals one for loom centers classified as female at baseline. "Above Median Preferring Worker Vote" equals one if a firm is above the median (across firms) in the share of workers who prefer having weavers vote rather than the manager choosing. "Above Median Manager Relationship Important" equals one if a firm is above the median (across firms) in its average baseline agreement with the statement the manager relationship is important (1-4 Likert scale). "P-Val: M+MxHetVar = 0" reports the p-value from a test that the sum of the coefficients on Manager and Manager times the respective heterogeneity variable equals zero. "P-Val: W+WxHetVar = 0" reports the p-value from a test that the sum of the coefficients on Worker Vote and Worker Vote times the respective heterogeneity variable equals zero. "Lasso BL Var" indicates whether baseline control variables selected via Lasso are included in the specification. Standard errors are clustered at the firm-level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: Pre-Registration Appendix

We pre-registered our experiment on the AEA RCT Registry (Kala and McKelway, 2025*a*). We adhere closely to the outcomes and analyses specified there, with all deviations detailed below. The registration was last updated on February 7, 2025, but as we note in the registration, "this was about a third of the way into our intervention phase but before we estimated any treatment effects."

First, we list each pre-specified outcome, any modifications to it, and where it appears in the paper.

- 1): no modification, in Table 1
- 2): no modification, in Table 1
- 3): no modification, in Table A.10
- 4): no modification, in Table A.6
- 5): we use spot-check but not baseline data for analyses of the actual types of workers who won the bonus (Table 3) since many workers were not surveyed at baseline.
- 6a): we make two modifications to the meritocracy outcome (Table 8). First, we do not index the meritocracy variables because we see strong effects on them in opposite directions, with the manager treatment raising the importance of skill (greater meritocracy) and the importance of managerial relationships (less meritocracy). Second, we add an outcome reflecting the importance of worker relationships given the importance of worker relationships in the worker arm.
- 6b): no modification, in Table 8
- 6c): no modification, in Table 8
- 7a): we revised the collegiality measure observed by surveyors to capture several types of workplace interactions, not just workers helping each other: work-related interactions (helping and non-helping), social interactions, and talking to manager (Table 2). This change gives us a more comprehensive picture of workplace culture from objectively-measured outcomes.
- 7b): we removed the dictator game as the vast majority (over 95%) of workers sent zero tokens at endline.
- 8): no modification, in Table 9

- 9): no modification, in Table A.10
- 10): we faced challenges merging administrative data on defects for carpets from our partner firm, so instead show effects on defects in any carpets as reported on manager endline surveys (Table A.4)

We present effects on the following outcomes which were not pre-specified:

- Number of rewards and repeat winners (Table 4), as these are important for understanding how rewards were allocated
- Sharing reward outcomes (Table 5), as qualitative reports while the experiment was ongoing suggested sharing would be important
- Managerial behavior (Table A.5), as these outcomes help us rule out changes in manager-worker interactions as mechanisms for our effects

Finally, the list below includes all analysis details specified in our registry (under the "Experimental Design" section), noting any modifications.

- Strata controls: we specified a method for pooling strata when firm attrition left strata without observations from each treatment group. Fortunately, we only had one firm attrit, hence we control for the strata as used in randomization, with no pooling.
- Treatment variables, version 1: we specified estimating regressions that pool the two treatment arms together, in addition to the regressions we estimate now (version 2, next bullet). We omit these pooled specifications as several important outcomes are different in the two treatment arms.
- Treatment variables, version 2: no modification
- Baseline LASSO controls: no modification
- Heterogeneity, dimension 1: no modification
- Heterogeneity, dimension 2: we specified heterogeneity by whether managers reported they were close to the worker, but this is true for few workers (less than 5%) so we use perceived importance of managerial relationships instead.
- Heterogeneity, dimension 3: no modification
- Effects of winning analysis: no modification

- Effects of winning outcomes: we study the effects of winning on outcomes from the spot checks but not the endline surveys. This is because winners and runners up changed every fortnight; it is straightforward to construct outcomes from the daily spot checks that match these frequent changes, but not from the endline surveys. Additionally, we see few effects of winning in the spot check data and would only expect the effects of winning to weaken over time.