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

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PRELIMINARY DRAFT

Abstract

Agency in the workplace – giving workers more influence over organizational decisions – is a commonly cited determinant of employee engagement, productivity, and organizational culture. In this paper, we partner with a large Indian exporter to conduct a firm-level RCT to test whether and how workplace democracy in employee recognition impacts worker and firms, relative to a recognition program where managers decide which workers deserve recognition. We find that workplace democracy increases worker attendance by 4.3 percentage points relative to the control arm (where eligible workers are rewarded randomly with no weight on their conduct), an 8.4 percent increase relative to the control mean, but the managerial arm improves firm productivity. Workplace interactions are also affected, with workers in the manager discretion arm less likely to engage with each other on workplace interactions and helping each other (but no more or less likely to engage in social interactions). Finally, we incorporated a shortlist design to estimate the impacts of winning recognition. Winning recognition has no impact on worker outcomes relative to a control group where rewards are distributed randomly. Finally, despite the presence of drawbacks in the voting arm, workers and managers prefer this mechanism to reward recognition relative to increasing managerial discretion.

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1 Introduction

Agency in the workplace - giving workers more influence over organizational decisions- is a commonly cited determinant of employee engagement, productivity, and organizational culture. 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). Firms implement workplace democracy in a variety of different ways, ranging from 360-degree feedback as an input into compensation schemes, to more formal mechanisms assigning voting rights on specific firm decisions. While several notable case studies point to successes or failures of workplace democracy, it is difficult to discern whether it can meaningfully impact worker and firm outcomes, or whether the adoption of such practices is undertaken by selected firms.[1] Since giving workers agency can be costly for the firm and is potentially difficult to reverse, understanding the returns to such practices is highly policy-relevant.

In this paper, we partner with a large Indian exporter to conduct a firm-level RCT to test whether and how workplace democracy in employee recognition impacts worker and firms, relative to a recognition program where managers decide which workers deserve recognition. Employee recognition programs to reward workers for behaviors valued by their organizations are very common, with nearly a quarter of firms reporting the presence of formal recognition programs (WorldatWork, 2019). Allocating decision-making power to workers via a democratic process vs. managers may lead to different decisions and outcomes due to differences in preferences, information, as well as how much they value decision-making authority. Each of this may then impact worker and firm outcomes, as well as beliefs about what is rewarded in the workplace. Furthermore, using the experimental design, we are also able to estimate the returns to having a recognition program in the firm vs. not, which is a question independently of interest.

Our experiment has three treatment arms, which vary at the firm-level. The firm-level variation ensures that the results are not affected by within-firm discontent amongst control workers not receiving a particular program, since all workers in a firm face the same incentives. In the first treatment arm, managers have discretion to choose which workers receive the recognition award. In the second treatment arm, workers vote to choose which workers receive the recognition award (which is the worker with the most votes). In the control arm, rewards are given out randomly, to ensure that income effects are not impacting the results, as well as ask the policy-relevant question of whether firms could achieve the same effects

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

by giving out the same amount in rewards but without concern for workplace excellence. Everything else, such as number of rewards per firm in a two-week cycle (one reward per ten workers), the amount and type of the reward (10% of baseline salary for the average worker as well as a brief ceremony), the frequency (every two weeks), eligibility in a two-week cycle for the reward (to attend work four days out of twelve working days), and number of reward cycles (six) were constant across the three arms. Furthermore, using a runner-up design, we are able estimate the impacts of winning on workers who received these rewards in the next program cycle after they won, relative to those who were next in line. Each program cycle was two weeks long, and the experiment ran for six program cycles. At the end of each cycles, winners were announced in a small ceremony, and received a bonus roughly equivalent to 10% of baseline wages of workers in that firm. The experiment covered about 2,700 workers in 124 firms, all of whom were suppliers to the implementation partner in addition to supplying some of their output to other buyers or markets.

The recognition program we implemented was similar to common recognition programs in that it sought to reward behaviors which can include, but need not be limited to, only output. The relative emphasis on such behaviors can also of course vary by who is deciding the recipients of such rewards. In the baseline survey we conducted, we asked managers and workers what type of behavior would merit receiving such a reward if they were deciding the winners. There is some agreement, but also significant differences, across managers and workers. The most commonly picked behavior by workers was highest output ("weaves the most knots"), chosen by 41% of workers, followed by good attendance (picked by 33% of workers). The most commonly picked behavior by managers was good attendance (picked by 50% of managers), followed by effort (i.e. the worker "tries hard", picked by 40% of managers), indicating the managers relatively value reliability and effort more. Indeed, what behaviors are rewarded under broad incentives is an outcome of interest.

We collect several sources of data to understand how these treatments shape productivity and culture in the firm, including from daily visits to each of the 125 firms in the experiment to measure worker attendance, output, as well as their workplace interactions (both any and the type of interaction, such as social or helping another worker). Moreover, we conducted two endline surveys to measure survey-based measures of earnings, beliefs about the reward allocation and workplace culture- the first was just before the program ended and the second four-six weeks after the program ended.

We find that workplace democracy increases worker attendance by 4.3 percentage points relative to the control arm (where eligible workers are rewarded randomly with no weight on their conduct), a 8.4 percent increase relative to the control mean. In contrast, the managerial discretion arm does not impact attendance. However, conditional on being at

work, workers in the voting arm are less productive. Indeed, when aggregating productivity to the firm-date level, workers in the managerial discretion arm are more productive relative to the control and the worker arm. Moreover, workplace interactions change as a result of the reward programs, with workers in the manager discretion arm less likely to engage with each other on workplace interactions and helping each other (but no more or less likely to engage in social interactions). This indicates that programs that increase competition amongst workers by using managerial discretion to reward workers can increase total output, but may lower knowledge spillovers in the workplace since they are an externality(Battiston et al., 2021). From the endline survey, we show that winners in the voting arm are more likely to share the reward with co-workers, and when asked why, report that it is because they voted for the winner. This indicates that programs with workplace democracy, while having positive effects on certain outcomes like attendance, may be also accompanied by behaviors like reward-sharing and favor-trading.

Turning to what types of workers get rewarded by the program, we show that while managers are more likely to pick winners with above median attendance and revenue generation relative to winners picked by voting, they are no more or less likely to pick winners who ranked first in any of the performance measures. Both managers and workers value non-weaving work, which is unpaid work that is nonetheless important for productivity - workers who ranked first in performing non-weaving work are about 6 percentage points more likely to win.

Finally, we incorporated a shortlist design to estimate the impacts of winning recognition. Pooling across all arms including the control, we find that winning the reward causes the winner to attend 0.4 more days in the next two weeks relative to the shortlisted candidate, a nearly 5 percent increase relative to the mean. However, there are no impacts on doing non-weaving work, or productivity. Furthermore, the impact of winning a recognition program is not statistically different relative to the control i.e. randomly giving a worker the bonus and small ceremony increased their attendance, but giving them these as a reward for workplace excellence yields no additional benefits. These results are relevant to the literature on impacts of incentive pay and of employee recognition, and show that the reason behind the bonus i.e. whether it is tied to workplace excellence or not, does not differentially impact worker outcomes.

The paper relates to three literatures. The first is the literature on worker agency. In related work, Bandiera et al. (2021) find that worker autonomy over procurement processes 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 on an important firm decision, namely, who to reward.

Second, we relate to the literature on the impacts of managerial discretion in worker compensation. In recent work, Brown (2022) conduct a RCT in Pakistan and show that using managers' (i.e. principals or vice-principals) subjective reports to determine teachers' raises lead to similar improvements in test scores as using objective criteria, but the former also improve pedagogy while the latter do not. 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). 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) and be able to incentivize difficult to observe behaviors (Brown, 2022), increased discretion may also lead to decisions influenced by favoritism (De Janvry et al., 2023) or over-confidence (Hoffman et al., 2018).[2] This paper contributes to the literature in two ways. First, it contrasts outcomes from using discretion against a counterfactual of workplace democracy. Second, it estimates the impact of using managerial discretion in recognition programs, which is a commonly used, tournament-style mechanism to reward a broad set of behaviors important to the firm.

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) and private sector (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)). Even in settings with measurable productivity, workers and managers may value dimensions other than individual output (as mentioned earlier, the most common behaviors managers in our

[2]Also related is the empirical literature on the impacts of managerial autonomy on a broader set of firm decisions, including Aghion et al. (2021) and Kala (2024).

baseline survey wanted to reward were reliability and effort-related, 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 this project contributes to this literature by provide casual returns to such programs. Furthermore, it provides evidence on how the structure of such programs i.e. whether winners are chosen by managers, or by a democratic process involving peer workers, 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 large 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 in particular 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 loom centers located in villages in the area. Obeetee provides loom centers with the design and raw materials, and then pays the centers for the completed carpets. Obeetee monitors the centers to ensure carpets are produced correctly and on time, and that labor laws are adhered to within the centers, but otherwise the management and production process is completely up to the loom centers.

Each center is owned by a household in its village and generally employs 10-20 carpet weavers. The centers may take orders from other large carpet manufacturers in the area in addition to Obeetee. The centers are generally managed by members of the households who own them, though some larger centers hire managers. The weavers mostly come from nearby villages, but some centers employ migrant weavers from other states who live and work on the centers' premises for several months at a time. A large majority of carpet weavers are male as female labor force participation is low in this area, but Obeetee has been working to train and promote employment for female carpet weavers as part of its corporate social responsibility initiatives.[3] In light of local gender norms, the loom centers are gender-segregated, employing either female or male weavers but not both. There are

[3]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; Lowe and McKelway, 2025; McKelway, 2022, 2025*b*,*a*).

wealth and caste gaps between weavers and loom center management, with weavers coming from poorer and often lower caste households. Qualitatively, many weavers voice respect and deference for their center's management, while some voice concerns about underpay or favoritism towards certain weavers.

We work specifically in centers 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 weavers working at once. Threads are strung vertically on the loom and weavers 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 weavers 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. Weavers often work on the same looms and with the same other weavers, but the teams are not totally fixed – some weavers will be 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 weavers working on a single loom on a particular day generally split up the width of the carpet, so that if three weavers were weaving, each would complete only the left third, middle third, or right third of every line before proceeding to the next line. 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 weavers with sufficient experience and skill are assigned complex carpets (meaning teams of weavers are matched on ability). Obeetee pays centers more per square inch for completed carpets that are more complex.

Nominally, weavers are paid based on the number of knots they weave. Loom centers set piece-rates to pay weavers for doing a certain number of knots, generally either 6,000 or 9,000, and rates vary based on the complexity of the carpet. Weavers 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 loom centers to know exactly how many knots a weaver completes on a given day, especially when there are multiple weavers working on a carpet and when those weavers work for different lengths of time. The count is always approximated to some extent, and the degree of precision differs across centers. The cycle on which weavers are paid also varies across centers, with the most

common cycles being weekly or upon the completion of a carpet.

Weaver attendance is quite variable. In the average center in our study, we observed 22 different weavers working at some point over the 12-week intervention period, but only observed 12 present on the average work day. Part of the fluctuation in attendance is due to the volume of work the centers have for weavers on any given day, but much of it is weaver 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 loom centers to meet Obeetee deadlines and for Obeetee to meet deadlines with external retailers.

While the majority of weavers' time is spent tying knots, the centers require some non-weaving work to be done. Much of this 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 weavers are compensated for this work is up to their loom centers.

3 Experimental Design

3.1 Sample Recruitment

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

Obeetee's list included 294 centers from 195 villages. The multiple centers within the same village are very often on the same premises, and owned and managed by the same family. They are considered separate centers for Obeetee's purposes (e.g. because different family members formally own different parts, or because one center was instituted later than another), but we did not want to consider them separate for our experiment given the management and weaver pool is often shared across them. The challenge was that Obeetee's list did not distinguish between affiliated and unaffiliated centers in the same village. Our solution was to randomize the order of centers within a village on Obeetee's list, starting by calling the first one, and moving 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 center that determined eligibility, we asked loom owners to report about the entire loom centers premises, not the center as designated in Obeetee's records.[4] We also asked the

[4]If the loom owner owned looms 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 the owner's income were located.

center if they were aware of any other centers in their village that took Obeetee orders, were on separate premises, and not owned by them; these could be considered separate centers for our experiment. We identified seven centers in this way which we contacted for eligibility screening.

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

We then visited the eligible centers in person to provide the loom owner information about our study and seek their consent for their center to participate. If they consented, we asked them to identify the loom owner or hired manager we should approach for surveys and allocation of the reward (in case the center was assigned the manager discretion arm), recommending they select the person most directly involved in managing the weavers. We refer to this person throughout the text as the manager. Finally, we provided information about the study to weavers in consented centers. In total, we successfully met with 137 of the 144 eligible centers to seek consent, and 125 consented.

3.2 Randomization

We randomly assigned each of the 125 centers to one of three treatment arms: manager discretion, worker vote, or control (lottery). To maximize power, we formed strata that each included three centers, matched on gender and average weaver attendance.[5] The latter came from a question on the weaver baseline survey (detailed further in Section 4.1 below) asking weavers how many of the last seven days they had worked in the center.

The randomization achieved balance on baseline weaver characteristics, measured in the weaver 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, close to, and

[5]The number of female centers (21) was divisible by three, but the number of male centers (104) was not. Hence we formed one male stratum, the one with the highest average attendance centers, with five rather than three centers.

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 centers. Our treatment varied the mechanism for determining which weavers won recognition, but the structure of the program was otherwise identical across the centers. 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 to individual weavers in the centers every two weeks for 12 weeks. The winners of every two-week reward cycle were announced in public ceremonies held in each center the following Monday. The 12-week period occurred between January and April 2025, with surveyors visiting each center prior to the start of the reward period to explain the program and how rewards would be allocated in that center.

Only weavers 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 surveyors; a surveyor visited every center on each work day (work days in this setting are Monday through Saturday, excluding holidays) and recorded which weavers were present. Only weavers observed present on one third of the working days in the two-week period were eligible. In each cycle and each center, one reward was given for every 10 eligible weavers, rounded to the nearest integer.[6] This means that any treatment effects on attendance would not affect the fraction of weavers in a center who could win. On average, 14 weavers were eligible per center in a cycle.

The reward amount to be given in each cycle was fixed over time, but varied across centers based on typical weaver earnings in that center. Earnings vary by center due to differences in weaver compensation rates, attendance, and productivity. Each center's reward amount was 10% of the average monthly weaver earnings in the center, calculated using responses from the weaver baseline survey, and rounded to the nearest 50 rupees. The amount varied between Rs.300 and Rs.1200 across the centers, with an average of Rs.673. This scheme means weavers in all centers faced the same incentives as a proportion of their weaving earnings. Keeping a fixed amount determined by baseline data meant treatment effects on earnings could not affect the return to winning. During the consent process, we told loom

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

centers 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 weaver baseline survey responses in that center to avoid weavers inflating their reported earnings to increase the reward amount. The money for these rewards came from the research project, not the loom centers or Obeetee. 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 weavers in the center who had met the eligibility requirements that cycle. How the rewards were then allocated among the eligible weavers 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 weavers based on who, according to them, had done good work over the past two weeks. The surveyor also asked the manager to select runners-up (for the winning quasi-experiment, detailed below) as well as their reasons for selecting each winner and runner-up. The surveyor recorded all responses in a tablet. 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. Then, after making the announcement to the center listing the eligible weavers, the surveyor said the winners had been 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 names of the runners-up were not revealed, nor were the reasons the managers provided for selecting the winners.

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

Once the voting was complete, the surveyor went inside the center to count the paper ballots. The surveyor gathered the weavers 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 weavers. 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 weavers 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 weavers 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. Note that only the names of the winners were announced to weavers and not the full distribution of votes – it was not revealed who came in second place or what the ordering of the winners was in the cases of multiple winners. We did not reveal the full distribution to mimic the procedure for the manager arm while also making possible the winning quasi-experiment detailed below.

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 weavers selected at random. To make the randomness transparent to weavers, the rewards were allocated through a public lottery. After surveyors announced the eligible weavers, they put a chit into a transparent box for every eligible weaver, with the weaver's name on it. The surveyors then shuffled the box, drew one chit out while looking in another direction, and then read the name on the selected chit. This was repeated as many times as there were rewards to be given in the center, with the weavers whose chits were drawn being the winners. The surveyor recorded whose chits were drawn out in a tablet.

3.4 Winning Quasi-Experiment

We were also interested in how winning affected the winners, and in whether these effects varied based on how the reward had been allocated. Our design generates quasi-experimental variation in winning. This variation is straightforward in the lottery arm; the winners were chosen at random from the eligible weavers, hence we can compare the winners to the rest of the eligible weavers to understand the effect of winning. However, we cannot simply compare winners to other eligible weavers in the manager discretion or weaver vote arms because the winners are likely to differ systematically from the others. For these arms, we use quasi-experimental variation, comparing the winners to the runners-up, with the idea that the runners-up are likely to be very similar to the winners.

In the manager arm, we asked the manager in each cycle to identify the same number of runners up as winners, using the runners up as the comparison group. In the voting arm, both the winners and the runners up are identified based on the records of who voted for whom. Note that the winners as identified in this way vary slightly from the actual winners in the 3% of cases where surveyors mis-counted the votes, but this approach allows us to identify winners and runners up in the same way (surveyors were not asked who the runners up were according to their count) and can be seen as an "intent-to-treat" approach. As detailed above, it was not revealed in either the manager or worker arm who the runners up were.

4 Data and Empirical Specifications

4.1 Data

We conduct several different types of data collection for the project. First, surveyors visited each of the 125 intervention firms every day from the start of the intervention to four weeks after the recognition program had ended. They measured several different worker and manager behaviors. First, using a worker roster, they measured worker attendance, as well as whether the worker was working on weaving carpets (for which they are paid piece-rate) or doing non-weaving work (which is often unpaid, but a key input into paid work). Second, if workers were engaged in weaving work, they measured carpet characteristics if a new carpet had been added to the loom. Third, they measured which loom workers were sitting at, and recorded any interactions such as social and workplace related (helping a co-worker or other workplace related). Fourth, they recorded if the manager was present on the production floor, and if so, what task they were engaged in (e.g. supervising workers, helping workers, engaged in production, etc.). We use this data collection to measure several key outcomes detailed in the Section 5

Second, we collect data using baseline and two endline surveys. The baseline survey was conducted to measure worker characteristics such as tenure, wages, and beliefs about workplace culture. The endline survey measured workers' self-reported attendance in the last week, wages in the last payment cycle, broader questions about workplace culture, as well as several questions about their beliefs about the program. This included their beliefs about the type of worker that was rewarded in the recognition program, as well as whether they received a share in the winners' bonus and why. The surveys also measured workers' and managers' preferred mechanism to reward workers (i.e. workers vote or the manager decides), which is a key outcome.

4.2 Empirical Specifications

4.2.1 Main Specification: Impacts of Recognition Program on Firms and Workers

The main specification for estimating treatment effects is as follows:

$$Y_{ijt} = \alpha + \beta_1 T_{1j} + \beta_2 T_{2j} + \gamma X_i + \varepsilon_{ijt} \tag{1}$$

where Y_{ijt} is an outcome for worker i in firm j on date t. T_{1j} is a dummy variable that takes the value 1 if firm j is assigned to the managerial discretion arm, and 0 otherwise. T_{2j} is a dummy variable that takes the value 1 if firm j is assigned to the workplace voting arm, and 0 otherwise. X_i are strata fixed effects. The two primary coefficients of interest are β_1 and β_2, which is the impact of being assigned to a recognition program where managers decide the winner and where the winner is decided by worker voting, respectively. We also test whether $\beta_1 = \beta_2$ in all regressions.

We include lasso controls from the baseline and screening surveys in each regressions. The level of the regression is, depending on how the outcome was measured, either a panel regression (for instance, at the worker-date level, firm-date level, or worker-fortnight level), or a cross-sectional regression at the firm or worker-level, but the lasso controls and strata fixed effects are included in all specifications. Where the outcome variable is knots woven of a carpet, we additionally include controls for carpet characteristics to control for task difficulty that the worker is assigned to.

4.2.2 Impacts of Winning Recognition Program on Winners

We use two specifications to estimate impacts of winning the recognition program on winners' behavior in the two weeks *after* winning the program i.e. the next program cycle. The first pools all arms including the control group, and compares winners to the runner-up within that cycle, with the following specification:

$$Y_{ijt} = \alpha + \mu \text{Won}_{i,t-1} + \gamma X_i + \varepsilon_{ijt} \tag{2}$$

where Y_{ijt} is an outcome for worker i in firm j for round t, where round is program cycle (ranging from 2 to 6) - for instance, number of days the worker attended, or knots woven. $\text{Won}_{i,t-1}$ is a dummy variable that takes the value 1 if worker i won the reward in the previous round $(t-1)$, and zero if the worker was runner up in that round (this regression is restricted to workers and runners up only). In the control group, the runners up as defined as all other eligible workers who did not win (since all these workers were ex-ante equally likely to win).

In the worker voting arm, these are workers with the next highest number of votes relative to the winner, and in the manager arm, they are the worker that the manager reported they would reward if they were giving an additional reward. As in Equation 1, we include strata fixed effects as well as lasso controls.

The second specification for this analysis tests whether the impacts of winning vary by treatment arm. In particular, this analysis asks two questions. First, does it matter if the worker was rewarded whether they were rewarded for workplace excellence, or solely because of luck regardless of conduct? Second, do the impacts of winning recognition depend on whether the manager is deciding the winners, or winners are chosen by a democratic process within the firm? To answer these questions, we estimate the following specification:

$$\begin{aligned} Y_{ijt} = \alpha + \phi_1 \text{Won}_{i,t-1} + \phi_2 \text{Manager}_j + \phi_3 \text{Won}_{i,t-1} \times \text{Manager}_j + \phi_4 \text{Worker Vote}_j \\ + \phi_5 \text{Won}_{i,t-1} \times \text{Worker Vote}_j + \gamma X_i + \varepsilon_{ijt} \end{aligned} \tag{3}$$

To understand if the manager arm has a statistically significant impact of winning, we present p-values testing $\phi_1 + \phi_3 = 0$, and for the same test for the voting arm, we present p-values testing $\phi_1 + \phi_4 = 0$. We also test whether the treatment effects of winning are the same by treatment arm i.e. $\phi_1 + \phi_3 = \phi_1 + \phi_4$.

5 Outcome Measurement and Results: Main Outcomes

5.1 Attendance and Productivity

Our primary performance-related outcomes are daily attendance and productivity. Both are measured using the daily surveyor visits, where a surveyor marked whether a worker was present at work from a roster, and conditional at being at work, whether the worker was engaged in weaving, or non-weaving work like setting up a loom for a new carpet. The attendance variable is therefore a binary variable at the worker-date level, which takes the value 1 if the worker is found to be present at work on that date, and zero otherwise.

We measure productivity in two different ways - the first is to compute total knots woven by the worker every day, and the second is a revenue-based measure that directly accounts for carpet difficulty. For the former, using the dates a carpet was started and finished, we impute the number of knots a worker did if they are weaving on a particular day. For instance, if the carpet had 9000 knots, took three weaving days to complete, and was worked on by two workers, each worker was interpolated to have woven 1500 knots

per day. If a worker is engaged in non-weaving work, they are assigned zero knots for the day. We consider productivity conditional on being present at work, and present aggregated productivity results at the firm-date level in the appendix.[7] A third measure of worker performance that we measured in the daily visits are a binary variable for whether the worker was engaged in non-weaving work, which largely comprises of removing a finished carpet from the loom or setting up a loom for a new carpet - this type of work is often not directly paid, but an important input into paid work. Willingness to undertake this work was also commonly mentioned as something managers and workers valued in a co-worker, and so it may change as a result of treatment.

We present results on these outcomes in Table 1. Workers in the voting arm are 4.3 percentage points more likely to attend work during the program, an increase of 8.4% relative to the control mean. Th effect is statistically significant relative to the control group and relative to the manager arm, which does not show any impact on attendance relative to the control. Columns 2 and 3 present results on weaving vs. non-weaving work, and show that this increased attendance in the voting arm is largely translating into an increased likelihood of undertaking weaving work, with no impact on undertaking non-weaving work.

Workers in the voting arm are more likely to be present at work, but are they differentially productive conditional on attending? Columns 4 and 5 present results for knots woven (including additional carpet characteristics in the lasso control set) as well as revenue generated by the worker, respectively. Conditional on being at work, workers in the voting arm produce less relative to the control and the manager arm - the effect is about 7 to 10% lower relative to the control mean. This could be driven by a selection effect of who attends work or a treatment effect of the program. We present results on other outcomes as well as firm-level productivity measures to understand how this translates into aggregate firm productivity. However, these results highlight a trade-off that workplace democracy can induce, by improving some behaviors valued by the firm but not others.

How does this translate into firm-level productivity? In Table 2, we present results aggregated to the firm-date level. Column 1 presents results for daily knots woven at the firm-level, and Column 2 for revenue per day at the firm-level. It shows that firms in the manager arm have 18% higher productivity relative to the control mean when considering the

[7]To impute revenue, we fit a linear regression using out implementation partners purchase ledger of completed carpets. The model predicts a carpets total price from three physical characteristics knot density, color count, and area and includes all interactions among them: each characteristic on its own, the three pairwise interactions (densitycolors, densityarea, colorsarea), and the single three-way interaction (densitycolorsarea). We do not use logs or higher-order terms. We apply the fitted model to the loomday data to obtain a predicted price for each observed carpet; for carpets still in progress, we scale the prediction by the carpets recorded completion percentage to obtain revenue observed to date. Our revenue productivity measure is revenue per work day, defined as this scaled revenue divided by work days.

knots woven outcome (an increase of about 23367.6 knots over a control mean of 12,797.6). The estimate on the voting arm is smaller and negative but is noisily measured - we can reject that the impacts on the treatment arms are the same. Results are noisier when considering the revenue-based measure of productivity, but qualitatively similar - an increase of about 11 percent relative to the control mean in the manager arm, and a very small and noisily estimated negative effect in the voting arm.

Next, we estimate impacts on other outcomes on workplace interactions as well as choice of winners to understand why these different mechanisms to determine recognition lead to different outcomes in the workplace.

5.2 Workplace Interactions

In addition to attendance and productivity, workers' interactions with each other or the manager may also 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 during the daily visits, where surveyors noted whether on each loom, workers were engaged in any type of interaction, including the following: whether they were engaged in work-related interaction that was helping each other, a work interaction unrelated to giving or providing help, social interaction (not-work related), discussion of the recognition program, or other interaction.

Results for these outcomes are presented in Table 3, which report the presence of any type of each interaction at the firm-date level.i.e. they are binary variables if a surveyor noted that type of interaction in the firm on that date. Columns 1 and 2 report impacts on work-related interactions unrelated to help and work-related interactions about helping, respectively. We find large reductions in both in the manager arm- the likelihood that a worker is helping another worker falls by 5.2 percentage points, about 37 percent of the control mean, and the likelihood that they are engaged in a work-related interaction other than helping falls by 4.9 percentage points, nearly 50 percent relative to the control mean. Both effects are statistically significant relative to the control as well as the voting arm, and there are no changes in the voting arm on these outcomes. The changes to work-related interactions in the manager arm do not seem to come at the cost of other interactions including social interactions - while the point estimate for this outcome in the manager arm is negative, it is not statistically different from zero. Likewise, discussion about the program are not different across the two treatment arms, though are lower relative to the control. These results show that recognition programs can reduce workplace co-operation when they induce workers to compete for managerial approval, even if these do not translate into productivity impacts.

Thus, if production relies significantly on workplace co-operation or team production, this particular type of incentive structure may not be optimal for the firm.

How do worker interactions change with managers? First, Column 5 shows that while workers are less likely to be talking to the manager in the manager arm during surveyor visits, the effects are not statistically different from the worker arm. Furthermore, Table A.4 presents results for managerial presence on the production floor and their behavior in the workplace, which are also measured during surveyor visits. Managers are not more or less likely to be present on the production floor, nor are they more or less likely to be engaged in behaviors like 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 out this potential mechanism for the impacts of the program.

5.3 Reward Allocation: Who Wins and How Rewards are Allocated

In the previous section, we show that workplace democracy vs. managerial discretion in rewarding recognition have differential impacts on worker-level attendance and productivity, as well as workplace cohesion. In this section, we examine how the program outcomes are themselves different across these treatments: namely, beliefs about what behaviors are rewarded, the actual choice of winners, as well as how rewards are retained by the winners.

5.3.1 What Behaviors are Rewarded?

We begin by testing whether the type of workers who received the recognition awards were different across the treatment arms (relative to the randomly chosen workers in the control arm). We test for differences in attendance, productivity, as well as participation in non-weaving work. In particular, we whether winners are more likely to be those who perform better on these measures relative to the average (eligible) worker as well as whether they were highest on each measure in the fortnight before they won the award.[8]

We present these results, with the sample restricted to only the winners, in Table 4. They indicate that while managers are more likely to pick winners with above median attendance and revenue generation relative to winners picked by voting, they are no more or less likely to pick winners who ranked first in any of the performance measures. Both managers and workers value non-weaving work - workers who ranked first in performing non-weaving work are about 6 percentage points more likely to win. Of course, which type of worker wins

[8]These are also computed relative to eligible workers only.

may also impact the treatment effect of winning on future workplace behaviors, which we examine in Section 6.

5.3.2 Beliefs About What Behaviors are Rewarded

The previous section tested whether winners were different in attendance and productivity by whether they were chosen by managers or worker voting. In addition to winners' differences, *beliefs* about which types of workers were chosen for recognition could also impact the returns to the program for the firms. In the endline survey, we asked the workers what types of workers had generally won the rewards for this program, allowing them to choose several traits. We group these behaviors into five types of possible behaviors. The first was directly productivity-related (the winner wove many knots, worked on complex carpets, made few mistakes, or did non-weaving work well), and the second effort/reliability related (working quickly, meeting deadlines, attendance, trying hard, taking initiative). The third measured helping in the workplace, either for co-workers or the manager. The fourth measured non-productive effort exerted either for co-workers or the manager (if the winner was reported as nice/popular, sharing rewards with workers, or campaigning for votes for the former, and if they were reported to be especially friendly with the manager for the latter.). The fifth measured other unrelated reasons including financial hardship and luck.

We present results for these beliefs in Table 5. Workers in both treatment arms report a much higher likelihood of winners performing better on productivity-related measures, and the point estimate is higher in the manager arm than the voting arm, though we cannot reject that they are the same. On effort and reliability, workers in the manager arm are more likely to report winners as performing better than the voting arm (36 p.p. higher vs. 28 p.p higher relative to the control), and the two coefficients are statistically different. Helping workers is more likely to be chosen as a winning behavior in the voting arm (22 p.p. higher relative to the control; and only 12 p.p. higher relative to control in the worker arm), but helping managers is not differentially more likely in the manager arm than in the voting arm. The latter could be because possibilities to help co-workers were more likely to come up, or that managers relatively valued other traits like reliability and effort.

Turning to non-productive behaviors, the results show that workers in the voting arm are more likely to report winners undertaking non-productive behaviors with co-workers (Column 5), while workers in the manager arm are more likely to report winners undertaking non-productive behaviors with managers (Column 6), which is intuitive. The overall likelihood of reporting any non-productive behavior is higher in the voting arm than the manager arm, and is significant at the 1% level. There are no impacts on beliefs that winners were chosen to help them with financial hardship. Finally, there is a large negative treatment effect on

whether workers thought the winner was chosen by luck, which reflects the randomization in the control group i.e. 74% of the control group chose luck as a reason the winner received the reward, and this is about 63 and 70 p.p lower in the manager and voting arm, respectively.

These results show that there are significant differences in how workers believed winners were chosen, both in how they believed positive workplace behaviors like productivity, reliability, and helping others was rewarded, but also in an increase in unproductive behaviors like lobbying for the reward. In Section 5.3.4, we examine how these differences map on to a preference for worker democracy or preferring managerial intervention.

5.3.3 Reward Sharing

Assigning workers the right to choose can lead them to reward workers who exhibit more valued traits, but also gives workers bargaining power, which they can use to induce the winners to share the rewards of winning in exchange for their support. As we saw in the previous section, workers in voting arm are more likely to report that winners were more likely to engage in behaviors like lobbying and promising to share the reward with co-workers. How does it impact actual sharing behavior?

We measure whether and why rewards are shared with co-workers in the endline surveys. Column 1 of Table 6 presents impacts on a binary variable if the worker reported that a winner had shared the reward with them- the likelihood that a worker reported this is nearly 14 percentage points higher relative to the control in the voting arm, a 20 percent increase relative to the control mean. In the manager arm, it is not statistically different from zero. Thus, rewards are much more likely to be shared in the voting arm. We also asked workers why rewards had been shared with them, with options including that they were friends or family, that they had voted for the winner, helped the winner with work, made them look good to the manager, or because luck should be shared. Workers in the voting arm are 42 p.p. more likely to report that the winner shared with because they voted for the winner, and 6.8 p.p. more likely to report that it is because they helped the winner with their work. Both treatment arms are much less likely to report that the winner shared with them because "luck should be shared", consistent with the control group believing that the winner had received a windfall through luck. Neither group reported making their co-worker look good to the manager as a reason why they had received a share in the reward. These results are consistent with the voting arm giving workers bargaining power to reward workers, which allows them to share in the incentives received by the winners.

5.3.4 Choice of Allocation Mechanism

Workplace democracy for rewarding workers increases attendance, but also leads to lower productivity conditional on being present at work as well as a higher likelihood of unproductive behaviors like lobbying, and a greater likelihood that winners share rewards in exchange for co-workers' support. In contrast, managerial discretion for rewarding workers does not impact attendance but increases firm level productivity, while reducing helping co-workers, but also increases the likelihood that workers report effort and reliability are rewarded rather than unproductive behaviors relative to the voting mechanism. How do these outcomes translate into preferences for how these rewards should be decided? To get at this, we asked workers and managers in the second endline survey which allocation mechanism- workplace voting vs. managerial discretion- they would prefer if we would run the recognition program for two more weeks at their center. To make the responses incentive-compatible, we implemented the chosen allocation of a randomly chosen respondent in their center.

The results are presented in Table 7, and two findings are particularly notable. First, there are strong default effects in which mechanism is preferred - workers in the voting arm are 14.6 p.p. more likely to choose the voting mechanism relative to the control group, while those in the manager arm are 21.5 p.p. more likely to choose it. Second, there is differentially larger support for the worker arm i.e. workers in the voting arm are 75.8 p.p. more likely to pick their own allocation mechanism relative to the control (zero by definition), while those in the worker arm are 67 p.p more likely to pick their own allocation mechanism relative to the control - we can reject that these coefficients are the same.

Turning to managers' preferences, Table A.3 shows similarly that there is also differentially larger support for the worker arm by managers. Workers in the voting arm are 55.4 p.p. more likely to pick their own allocation mechanism relative to the control (zero by definition), while those in the worker arm are 72.6 p.p more likely to pick their own allocation mechanism relative to the control.[9] Thus, both managers and workers are more likely to prefer worker democracy as an allocation mechanism if they have been exposed to it.

6 Treatment Effects of Winning

The different types of recognition programs impact how the returns are allocated within the firm, and there are some differences in the types of workers that win these programs. Each of these may change whether winners' future behaviors are altered by winning the recognition program. Our runner-up design allows us to compare how winning relative to

[9]Interestingly, managers in the voting arm do not exhibit the default effect of preferring their allocation mechanism.

the next person in line for the reward within each firm impacts behavior for two weeks after winning the program (the comparison workers in the control group are all other eligible workers, since they were equally likely to win ex-ante).

Results are presented in Table 8. The odd numbered columns shows the impact of winning pooling across all arms (including the control group where the winner was randomly chosen) for attendance, non-weaving work, knots woven, and revenue generated, respectively. The even-numbered columns report treatment effects for the two treatment arms relative to the control. Column 1 shows that winning the reward causes the winner to attend 0.4 more days in the next two weeks relative to the shortlisted candidate, a nearly 5 percent increase relative to the mean. However, there are no impacts on doing non-weaving work, or productivity (Columns 3,5, and 7, respectively). Furthermore, the even numbered columns show that for any of these outcomes, the impact of winning a recognition program is not statistically different relative to the control i.e. randomly giving a worker the bonus and small ceremony increased their attendance, but giving them these as a reward for workplace excellence yields no additional benefits. These results are relevant to the literature on impacts of incentive pay and of employee recognition, and show that the reason behind the bonus i.e. whether it is tied to workplace excellence or not, does not differentially impact worker outcomes.

7 Other Outcomes

In this section, we present results on secondary outcomes of interest. The first such outcome is defects on the carpet woven. We measure these from the second endline survey, and elicited this from both managers and workers. In the worker survey, we asked workers if there had been a defect on a carpet that they had worked on separately for each of the four intervention months. In the manager survey, we asked managers if there had been a defect on a carpet produced by the firm for each of the four intervention months. Results are presented in Table A.5, and do not show that the recognition program impacted these rates.

8 Conclusion

The structure of rewards and the presence of agency are key determinants of worker sorting, income, retention, and job satisfaction. In this paper, we show that these have nuanced effects on workers and firms, with both managerial discretion and workplace democracy inducing some positive outcomes, at the cost of others. In particular, managerial discretion in rewarding workers can increase productiviey and worker competition but also reduce knowl-

edge spillovers, while workplace democracy can increase attendance but does not improve productivity. We also document that democracy induces bargaining amongst workers, with winners being more likely to share their bonuses in exchange for support. While this could plausibly impact the treatment effects of winning, winning recognition has no impact on worker outcomes relative to a control group where rewards are distributed randomly. Finally, despite the presence of drawbacks in the voting arm, workers and managers prefer this mechanism to reward recognition relative to increasing managerial discretion.

The results also raise additional questions, such as how democratic decisions over other firm decisions may impact firm outcomes. Furthermore, it is possible that different forms of voting rules may lead to different actions being implemented i.e. a different type of worker being rewarded. These, and related questions, remain interesting questions 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.

Table 1: Impacts on Attendance and Productivity

	All Workers			*Weaving Days Only*		*Present Workers*
	Attendance (=1)	Weaving (=1)	Non-weaving (=1)	Knots/day	Revenue/day (Rs.)	Non-weaving (=1)
	(1)	(2)	(3)	(4)	(5)	(6)
Manager	0.007	0.002	-0.000	197.121	-5.475	0.004
	(0.018)	(0.017)	(0.005)	(527.567)	(31.693)	(0.008)
Worker Vote	0.043**	0.033*	0.008	-1288.769**	-50.922*	0.006
	(0.018)	(0.017)	(0.005)	(541.461)	(30.622)	(0.009)
P-Val: M=W	0.076*	0.087*	0.101	0.004***	0.141	0.742
Data Structure	Worker-Date	Worker-Date	Worker-Date	Worker-Date	Worker-Date	Worker-Date
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes
Lasso Carpet Characteristics	No	No	No	Yes	No	No
Control Mean	0.513	0.455	0.057	12868.755	728.168	0.112
N	181660	181660	181660	79963	79953	93482

Notes: Column (1) reports whether the worker was present on a given day. Columns (2) and (3) report whether present workers were engaged in weaving or non-weaving work, respectively. Column (4) is knots tied per day, defined only on weaving days, with values above the 99th percentile replaced at the 99th percentile. Column (5) is revenue earned per day in rupees, defined only on weaving days and winsorized at the 99th percentile. Column (6) is an indicator for non-weaving work, defined among present workers conditional on having a recorded weaving status. Standard errors are clustered at the loom center level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effects on Firm-level Productivity

	Knots/day	Revenue/day (R.)
	(1)	(2)
Manager	23367.579*	743.960
	(13133.857)	(824.809)
Worker Vote	-3428.861	-40.188
	(10980.733)	(682.497)
P-Val: M=W	0.061*	0.370
Data Structure	Firm-Date	Firm-Date
Strata FE	Yes	Yes
Lasso BL Var	Yes	Yes
Lasso Carpet Characteristics	Yes	No
Control Mean	127978.563	6772.093
N	7668	8308

Notes: Columns (1)(2) capture firm-level daily output. Column (1) reports total knots, winsorized at the 99th percentile before aggregation to the firm-date level. Column (2) reports total revenue, likewise winsorized at the 99th percentile. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Interactions in the Workplace (Binary)

	Work-related, not helping (=1) (1)	Work-related, helping (=1) (2)	Social (=1) (3)	Program discussion (=1) (4)	Talking to manager (=1) (5)
Manager	-0.049**	-0.052**	-0.067	-0.050*	-0.010*
	(0.020)	(0.021)	(0.051)	(0.026)	(0.006)
Worker Vote	-0.019	0.010	-0.014	-0.050**	-0.004
	(0.020)	(0.021)	(0.044)	(0.023)	(0.006)
P-Val: M=W	0.070*	0.004***	0.250	0.996	0.337
Data Structure	Firm-Date	Firm-Date	Firm-Date	Firm-Date	Firm-Date
Strata FE	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes
Control Mean	0.104	0.139	0.388	0.127	0.029
N	8308	8308	8308	8308	8308

Notes: Each outcome is a dummy (=1) indicating whether, on a given firm–date observation, at least one weaver was engaged in the listed type of interaction. Column (1) captures work-related interactions that did not involve helping. Column (2) captures work-related interactions involving helping. Column (3) captures social interactions. Column (4) captures discussions related to the research program. Column (5) captures conversations with the manager. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Descriptive Analysis of Winners

	Attendance		Non-Weaving		Knots		Revenue (Rs.)	
	Above-mean (=1)	Top rank (=1)	Above-mean (=1)	Top rank (=1)	Above-mean (=1)	Top rank (=1)	Above-mean (=1)	Top rank (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Manager	0.114***	0.082**	0.037	0.059*	0.006	0.050*	0.069**	0.031
	(0.036)	(0.036)	(0.029)	(0.032)	(0.012)	(0.030)	(0.029)	(0.029)
Worker Vote	0.043	0.057*	-0.007	0.065*	-0.004	0.050*	0.001	0.037
	(0.033)	(0.034)	(0.027)	(0.035)	(0.013)	(0.029)	(0.028)	(0.029)
P-Val: M=W	0.058*	0.568	0.172	0.866	0.509	0.994	0.035**	0.855
Data Structure	Winners-Round	Winners-Round	Winners-Round	Winners-Round	Winners-Round	Winners-Round	Winners-Round	Winners-Round
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.584	0.221	0.384	0.212	0.976	0.119	0.470	0.122
N	1233	1233	1233	1233	1233	1233	1233	1233

Notes: Each outcome is a dummy (=1) defined among winners. Columns (1)(2) capture attendance: Column (1) equals one if a winners attended days in a given loom-center × round exceeded the centerround mean (eligibility: ≥ 4 attended days), while Column (2) equals one if the winner achieved the maximum attendance rank within loom-center × round (ties allowed). Columns (3)(4) capture non-weaving days, defined analogously. Columns (5)(6) capture knots/day, adjusted for carpet characteristics before comparisons are made, with winsorization at the 99th percentile. Columns (7)(8) capture revenue/day, also winsorized at the 99th percentile. Standard errors are clustered at the loom-center level. * p <0.10, ** p < 0.05, *** p < 0.01.

Table 5: Beliefs about Winners Type from Weaver EL1

	Productivity traits (=1)	Effort/attendance traits (=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/ karma (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Manager	0.305***	0.365***	0.123***	0.014*	0.163***	0.148***	0.256***	0.005	-0.624***
	(0.047)	(0.043)	(0.046)	(0.008)	(0.050)	(0.041)	(0.051)	(0.006)	(0.040)
Worker Vote	0.235***	0.282***	0.216***	0.003	0.446***	0.015	0.471***	0.009*	-0.703***
	(0.038)	(0.038)	(0.041)	(0.006)	(0.043)	(0.037)	(0.041)	(0.005)	(0.036)
P-Val: M=W	0.164	0.050**	0.081*	0.282	0.000***	0.004***	0.000***	0.574	0.042**
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.237	0.243	0.126	0.003	0.196	0.049	0.197	0.008	0.738
N	1798	1798	1798	1798	1798	1798	1798	1798	1798

Notes: Each outcome is a dummy (=1) indicating that, in the EL1 beliefs module, the respondent attributed the winners selection to the listed factor. Column (1) includes beliefs about technical productivity (many knots, complex carpets, few mistakes, or doing non-weaving work well). Column (2) includes effort and reliability traits (working quickly, meeting deadlines, attendance, trying hard, taking initiative). Columns (3)(4) capture prosocial behavior: helping other workers or helping the manager through work-related assistance. Columns (5)(7) capture non-productive explanations, divided into peer-related factors (niceness, popularity, sharing rewards with workers, or campaigning for votes) and manager-related favoritism (being nice to, promising rewards to, or being favored by the manager). Column (7) pools all non-productive reasons. Column (8) captures financial or other hardship. Column (9) captures luck, karma, or random chance. Standard errors are clustered at the loom-center level. * p ¡ 0.10, ** p ¡ 0.05, *** p ¡ 0.01. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 6: Effects on Sharing Reward

	EL2										
	Winner Shared with Me (=1) (1)	Amt Received (2)	Friends/ Family (=1) (3)	Voted For Winner (=1) (4)	Helped With Work (=1) (5)	Made Them Look Good to Manager (=1) (6)	Luck Should be Shared (=1) (7)	Shared w/ individuals/groups (=1) (8)	Shared w/ all (=1) (9)	Workplace Amenity (=1) (10)	Party (=1) (11)
Manager	-0.020	49.017**	-0.001	0.049	0.027*	0.004	-0.116***	0.043	0.190***	0.002	-0.263***
	(0.072)	(20.647)	(0.068)	(0.031)	(0.015)	(0.002)	(0.023)	(0.029)	(0.069)	(0.002)	(0.075)
Worker Vote	0.139***	41.839***	-0.138***	0.420***	0.068***	0.001	-0.130***	0.094***	0.255***	-0.001	-0.243***
	(0.053)	(12.652)	(0.047)	(0.036)	(0.019)	(0.001)	(0.026)	(0.030)	(0.071)	(0.001)	(0.073)
P-Val: M=W	0.008***	0.602	0.017**	0.000***	0.043**	0.241	0.487	0.098*	0.353	0.058*	0.788
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.717	22.924	0.465	0.004	0.000	0.000	0.130	0.027	0.195	0.001	0.507
N	2047	2047	2046	2046	2046	2046	2046	2047	2047	2047	2047

Notes: Each outcome is measured in the EL2 sharing module. Column (1) is a dummy (=1) indicating whether the respondent received a share of another winners reward. Column (2) reports the monetary amount received. Columns (3)(7) are dummies for stated reasons for sharing: family or friendship ties (3), voting support (4), help with work (5), making the winner look good to the manager (6), and the belief that luck should be shared (7). Columns (8)(11) are dummies for the form of sharing: with specific individuals or groups (8), with all coworkers (9), as a workplace amenity (10), or as a party (11). Standard errors are clustered at the loom-center level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 7: Weavers' Preference for Worker versus Manager Allocation, EL2

	EL2	
	Picked Workers Vote (=1)	Picked Own Allotted Mechanism (=1)
	(1)	(2)
Manager	-0.215***	0.670***
	(0.046)	(0.037)
Worker Vote	0.146***	0.758***
	(0.038)	(0.032)
P-Val: M=W	0.000***	0.061*
Data Structure	Worker	Worker
Strata FE	Yes	Yes
Lasso BL Var	Yes	Yes
Control Mean	0.591	0.000
N	2045	2045

Notes: Outcomes capture weavers' reward allocation choices in EL2. Column (1) is a dummy (=1) indicating whether the respondent preferred the worker-vote mechanism for allocating rewards. Column (2) is a dummy (=1) indicating whether the respondent chose to allot the reward to themselves through the own-allotted mechanism. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Impact of Winning

	Eligible Weavers (≥ 4 days per round)							
	Attendance (days)	Attendance (days)	Non-weaving (days)	Non-weaving (days)	Knots/day (resid.)	Knots/day (resid.)	Revenue/day (Rs.)	Revenue/day (Rs.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Won	0.402***	0.197*	0.051	0.056	2211.953	2799.986	132.606	193.704
	(0.080)	(0.116)	(0.042)	(0.064)	(2111.081)	(2631.683)	(111.998)	(153.598)
Manager		0.458**		0.116*		11070.808*		518.600
		(0.186)		(0.069)		(5869.206)		(453.943)
Won Manager		-0.116		-0.055		-1440.619		-132.801
		(0.172)		(0.092)		(4664.695)		(272.484)
Worker Vote		0.618***		0.086		4320.620		197.212
		(0.171)		(0.103)		(5595.921)		(344.066)
Won Worker Vote		-0.065		-0.081		-9120.383		-422.915
		(0.219)		(0.119)		(6452.020)		(387.366)
P-Val: Won+WonXManager=0		0.526		0.986		0.721		0.789
P-Val: Won+WonXWorker=0		0.475		0.804		0.278		0.519
P-Val: Won+WonXManager=Won+WonXWorker		0.824		0.828		0.277		0.496
Data Structure	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight	Worker-Fortnight
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	8.576	8.576	0.905	0.905	4927.463	4927.463	5295.644	5295.644
N	3921	3921	3921	3921	3853	3853	3921	3921

Notes: Sample restricted to eligible weavers (attendance ≥ 4 days in the round). Columns (1), (3), (5), and (7) report the main effect of winning the reward. Columns (2), (4), (6), and (8) add interactions with the selection arm. Attendance and non-weaving are totals over the post-ceremony period in the next round; Knots/day is residualized on carpet characteristics before comparison; Revenue/day is winsorized at the 99th percentile and expressed in rupees. Note that Columns (7) and (8) use the same outcome definitions as Table 1, Column (5). Standard errors are clustered at the loom-center level. * 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 Weaver Allocate (=1) (7)	Log(Monthly Earnings) (8)
Manager	-0.021	-0.020	-0.009	0.117	-0.154	0.105*	0.051	0.073
	(0.038)	(0.025)	(0.059)	(1.120)	(0.133)	(0.061)	(0.038)	(0.052)
Worker Vote	0.026	-0.030	0.046	-0.475	-0.117	0.163***	0.046	0.067
	(0.036)	(0.023)	(0.053)	(0.877)	(0.119)	(0.044)	(0.034)	(0.043)
P-Val: M=W	0.210	0.682	0.352	0.578	0.751	0.343	0.904	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	0.524	5.259	0.429	25.063	2.681	0.061	0.421	8.673
N	1555	1547	1472	1555	1553	1555	1530	1539

	Self-Reported Ability (1-10) (9)	Manager Relationship Important (1-4) (10)	Weaving Skill Important (1-4) (11)	Tokens Sent Dictator Game (0-5) (12)	Number Could Ask for Help (13)	Number Close to (14)	Workplace Competitive (1-4) (15)
Manager	0.148	-0.136	-0.060	0.005	-0.177	0.378	0.010
	(0.211)	(0.113)	(0.063)	(0.055)	(0.366)	(0.402)	(0.101)
Worker Vote	0.119	0.121	-0.006	-0.030	-0.132	0.043	0.138
	(0.180)	(0.093)	(0.051)	(0.047)	(0.366)	(0.415)	(0.095)
P-Val: M=W	0.892	0.038**	0.393	0.455	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	2.573	3.650	0.278	2.469	2.341	2.572
N	1546	1539	1544	1555	1555	1555	1547

Notes: Column (1) reports a dummy (=1) for having ever attended school. Column (2) records self-reported attendance in the past week (0–7 days). Column (3) is a dummy (=1) for belonging to one of the most common subcastes, including Chamar, Harijan, Goutam, Bharti, Rahdas, Kori, Bind, Mallah, Kebat, or Nishad. Column (4) records years of weaving experience. Column (5) is self-assessed financial status on a 1–10 scale, where higher values indicate being better-off financially. Column (6) is a dummy (=1) for reporting residence in the loom center premises or in a nearby village. Column (7) is a dummy (=1) if the respondent prefers that work be allocated by the weaver rather than the manager. Column (8) is the log of monthly weaving earnings. Column (9) records self-rated weaving ability on a 1–10 scale, where 1 indicates the least skill and 10 indicates being extremely skilled. Column (10) measures the perceived importance of the manager–worker relationship for rewards on a 1–4 Likert scale (1 = Strongly disagree, 2 = Somewhat disagree, 3 = Somewhat agree, 4 = Strongly agree). Column (11) measures the perceived importance of weaving skill for rewards (1–4 scale, with the same anchors). Column (12) records the number of tokens sent in a dictator game (0–5). Column (13) is the number of coworkers the respondent reports being able to ask for help. Column (14) is the number of coworkers listed as close friends. Column (15) measures the perceived competitiveness of the workplace on a 1–4 scale, with higher values indicating greater competitiveness. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Endline Survey Response Rates

	EL1		EL2	
	Endline Survey Completed (=1) (1)	Endline Survey Completed For Workers Present in the Endline Survey (=1) (2)	Endline Survey Completed (=1) (3)	Endline Survey Completed For Workers Present in the last 4 weeks of Spot Check (=1) (4)
Manager	0.007	0.001	-0.079*	-0.043
	(0.036)	(0.023)	(0.041)	(0.030)
Worker Vote	-0.039	-0.031	-0.072**	-0.014
	(0.033)	(0.022)	(0.035)	(0.024)
P-Val: M=W	0.175	0.229	0.873	0.319
Data Structure	Worker	Worker	Worker	Worker
Strata FE	Yes	Yes	Yes	Yes
Lasso BL Var	No	No	No	No
Control Mean	0.740	0.862	0.818	0.759
N	2545	2047	2693	2234

Notes: Columns (1)(2) report EL1 outcomes, and Columns (3)(4) report EL2 outcomes. Column (1) is a dummy (=1) indicating that an endline survey was completed for the worker. Column (2) is a dummy (=1) indicating that an endline survey was completed for workers who were observed at work at least once during the EL1 survey period. Column (3) is a dummy (=1) indicating that an endline survey was completed for the worker in EL2. Column (4) is a dummy (=1) indicating that an endline survey was completed for workers observed at work at least once in the final four weeks following the main experiment. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Managers Preference for Worker versus Manager Allocation, EL2

	EL2	
	Picked Workers Vote (=1)	Picked Own Allotted Mechanism (=1)
	(1)	(2)
Manager	-0.405***	0.554***
	(0.103)	(0.081)
Worker Vote	-0.014	0.726***
	(0.093)	(0.073)
P-Val: M=W	0.000***	0.060*
Data Structure	Worker	Worker
Strata FE	Yes	Yes
Lasso BL Var	Yes	Yes
Control Mean	0.800	0.000
N	122	122

Notes: Outcomes capture managers' reward allocation choices in EL2. All outcomes are defined identically to those in Table 7. Column (1) is a dummy (=1) indicating whether the respondent preferred the worker-vote mechanism for allocating rewards. Column (2) is a dummy (=1) indicating whether the respondent chose to allot the reward to themselves through the own-allotted mechanism. Standard errors are clustered at the loom-center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Impact on Managerial Presence and Behavior

	Manager Present on Weaving Floor (=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	-0.009	-0.002	-0.002	-0.001	0.001	0.019	-0.025	0.000
	(0.038)	(0.028)	(0.011)	(0.014)	(0.026)	(0.021)	(0.025)	(0.003)
Worker Vote	0.025	0.012	-0.014	0.025	0.025	-0.003	-0.009	-0.004
	(0.037)	(0.029)	(0.010)	(0.018)	(0.029)	(0.018)	(0.027)	(0.003)
P-Val: M=W	0.343	0.651	0.248	0.131	0.394	0.280	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
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.452	0.135	0.051	0.063	0.045	0.075	0.132	0.008
N	8308	8308	8308	8308	8308	8308	8308	8308

Notes: Each outcome is a dummy (=1) indicating that the manager engaged in the listed activity. Column (1) indicates that the manager was present on the weaving floor during the spot check (set to 0 when the firm was closed). 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 preparation work (e.g., taana, chunai, jeri) (6), on break (7), or other (8). Standard errors are clustered at the loom–center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Effects on Defects

	Worker EL2						Manager EL2					
	Any Defect (=1): Jan	Any Defect (=1): Feb	Any Defect (=1): Mar	Any Defect (=1): Apr	Any Defect (=1): May	Any Defect (=1): Jan–Apr	Any Defect (=1): Jan	Any Defect (=1): Feb	Any Defect (=1): Mar	Any Defect (=1): Apr	Any Defect (=1): May	Any Defect (=1): Jan–Apr
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Manager	0.013**	-0.009	0.012*	0.007	0.012	0.012	-0.063	0.177	0.177	0.152	-0.082	0.241
	(0.006)	(0.007)	(0.007)	(0.007)	(0.011)	(0.015)	(0.234)	(0.169)	(0.208)	(0.201)	(0.157)	(0.279)
Worker Vote	-0.004	0.012	0.004	0.004	0.003	0.010	-0.196	0.015	0.174	-0.113	-0.068	-0.099
	(0.004)	(0.007)	(0.006)	(0.007)	(0.008)	(0.014)	(0.208)	(0.171)	(0.220)	(0.181)	(0.158)	(0.282)
P-Val: M=W	0.009***	0.007***	0.226	0.689	0.473	0.872	0.513	0.307	0.987	0.181	0.926	0.232
Data Structure	Worker	Worker	Worker	Worker	Worker	Worker	Manager	Manager	Manager	Manager	Manager	Manager
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso BL Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.009	0.013	0.009	0.023	0.016	0.053	0.500	0.225	0.250	0.400	0.400	0.750
N	2051	2051	2051	2051	2051	2051	122	122	122	122	122	122

Notes: Columns (1)–(6) report weaver EL2 outcomes; Columns (7)–(12) report manager EL2 outcomes. Monthly columns indicate whether any weaving defect was reported in that month (=1). Columns 6 and 12 indicate whether any defect was reported in any of January through April (=1). Standard errors are clustered at the loom–center level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.