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# THE MORALE EFFECTS OF PAY INEQUALITY*

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Abstract. Relative pay concerns have potentially broad labor market implications. In a month-long experiment with Indian manufacturing workers, we randomize whether co-workers within production units receive the same flat daily wage or differential wages according to their (baseline) productivity ranks. When co-workers' productivity is difficult to observe, pay inequality reduces output by 0.45 standard deviations and attendance by 18 percentage points. It also lowers co-workers' ability to cooperate in their own self-interest. However, when workers can clearly perceive that their higher paid peers are more productive than themselves, pay disparity has no discernible effect on output, attendance, or group cohesion.

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## 1. Introduction

In traditional agency models, workers care about only their own wage levels when making labor supply decisions. However, a long tradition in economic thought—as well as in psychology, sociology, and organizational behavior—has advanced the notion that individuals also care about their pay *relative* to that of their co-workers.[1] This paper tests the empirical validity of this view. In addition, it examines workers' underlying notion of equity: specifically, whether reactions to pay inequality depend on whether it appears justified.

Relative pay comparisons between workers have the potential to influence various labor market features (Fehr et al. 2009). For example, they could help explain why wage compression—when wages vary less than the marginal product of labor—appears prevalent in both poor and rich countries (Frank 1984, Dreze and Mukherjee 1989, Charness and Kuhn 2007). They have also been proposed as a micro-foundation for wage rigidity (Akerlof and Yellen 1990). In addition, relative pay concerns could affect how workers sort into firms—for example, leading some firms to specialize by worker ability, or mediating how productivity dispersion maps to earnings inequality (Frank 1984, Song et al. 2015, Card et al. 2013). Relatedly, they could influence the decision to contract labor within or across firm boundaries (e.g., Nickerson and Zenger 2008). Moreover, understanding the circumstances under which workers' notions of equity are violated is important for predicting when these labor market implications may arise.

The existing behavioral economics literature has focused on the idea that agents' own internal preferences over relative pay can affect their utility. This idea has two related implications for worker behavior. First, relative pay can act as a compensating differential—affecting the willingness to accept employment at a given absolute wage. Second, such utility effects could impact effort, for example, in the presence of reciprocity.[2] Work in social psychology and sociology suggests that this exclusive focus on internal preferences may provide an incomplete view. These literatures emphasize that pay disparity can foster "resentment, lack of cooperation, sabotage, and lack of team potency" (Shaw 2015). These forces have the potential to create social conflict, amplifying effects on labor supply and output across all workers. For example, even if higher-paid workers individually derive positive utility from being paid more than others, their outcomes could be worsened due to discontent or hostility from their co-workers—who, mechanically, must be paid relatively less (Schmitt and Marwell 1972, Deutsch 1986, Lazear 1989, Levine 1991, Duffy et al. 2012).

[1]In economics, see, e.g., Marshall (1890), Veblen and Almy (1899), Hicks (1963), Hamermesh (1975). In psychology, see Festinger (1954) and Adams (1963). For reviews of work in social psychology, sociology, and organizational behavior, see Cook and Hegtvedt (1983) and Shaw (2014).

[2]Behavioral agency theory predicts that workers may reduce effort in response to fairness violations, especially under incomplete contracting (Kőszegi 2014). Fehr et al. (2009) argue that because most jobs contain some incomplete contracting, maintaining morale is essential for effort provision. This view is espoused by Bewley (1999), who documents managers' perceptions that relative pay is important for worker motivation.

Predicting when pay inequality could generate such adverse effects hinges on understanding how workers conceptualize equity. In a world with heterogeneous productivity, are fairness norms violated if pay levels are unequal? Or does fairness require that pay differences reflect productivity differences across workers (e.g., Adams 1963, Thibaut and Kelley 1959, Baron and Kreps 2013)?

We use a field experiment to test whether workers care about relative pay. We construct a design that enables comparisons of workers who earn the same absolute wage, but differ in their co-workers' wages. To explore workers' notions of fairness, we incorporate variation in the perceived justification for pay differences. We examine the effects of relative pay on labor supply: the extensive margin decision to accept work (attendance) and effort provision (output). We also explicitly test for impacts on group cohesion among co-workers.

To enable the experiment, we set up factory workshops in Odisha, India to employ 378 workers full-time for one month in seasonal manufacturing jobs—a prominent source of local employment. All output is sold in wholesale market by partnering contractors, who set production standards. The workshops are organized into distinct production units, with 3 workers per unit. All unit members produce the same exact product (e.g. rope), while every unit within a workshop produces a different product (e.g. rope, brooms, incense sticks). Each unit sits together in a separate physical space, both during work and lunch breaks. Thus, the workers in one's unit constitute a natural and salient reference group for pay comparisons. Note that production is an individual activity, with no joint production of any kind.

All workers are paid a flat daily wage for attendance, in accordance with the typical pay structure in the area. The decision to come to work is therefore incentivized: being absent results in a loss of earnings. However, there is incomplete contracting on effort: workers have some latitude to select effort levels, which are reflected in output. To obtain measures of worker output, we bear substantial overhead costs—in the form of extra staff and record keeping—to quantify each worker's daily individual production.

We induce exogenous variation in co-worker pay by randomly assigning units to one of four different pay structures. In the *Pay disparity* condition, each unit member is paid a different wage—$w_{High}$, $w_{Med}$, or $w_{Low}$—in accordance with his respective productivity rank within the unit (determined by baseline productivity levels). These pay differences are fairly modest: the difference between each wage level is less than 5%. In the three *Compressed* pay conditions, all three unit members are paid the exact same wage, which we randomly assign to be $w_{High}$, $w_{Med}$, or $w_{Low}$.[3] This allows us to compare, for example, workers with the same average baseline productivity level who both earn an absolute wage of $w_{Low}$, but differ in whether they are paid less than their peers (under the *Pay disparity* treatment) or

[3]At the beginning of the baseline (i.e. "training") period, workers are told that they will receive a wage increase on a pre-specified date, and that the size of this increase may depend on their baseline productivity. Once they are randomized into their wage treatment on this date, no additional future wage changes are possible. This shuts down dynamic incentive effects; see below for a discussion of this.

the same as their peers (under the *Compressed Low* wage treatment). Note that managers maintain pay secrecy; any learning about peer wages is through self-disclosure.

To test whether perceived justifications mediate morale effects, we incorporate two additional sources of variation into our design. First, while wage levels are fixed, underlying baseline productivity is continuous. Thus, by randomizing workers to units, we induce variation in the extent to which pay differences among co-workers overstate productivity differences. Second, co-worker output is far more observable for some tasks relative to others. By randomizing units to production tasks, we generate variation in the observability of co-worker productivity.[4]

Even though managers maintain pay secrecy, workers learn about co-worker wages. At the end of the month, 86.6% of workers can accurately report the wages of both co-workers in their product units. In contrast, they know the wages of those in other product units only 7.2% of the time. This is consistent with the presumption that workers primarily compare their pay with that of co-workers in their own unit.

For a given absolute pay level, output declines by 0.33 standard deviations (22%) on average when a worker is paid less than both his co-workers. This is accompanied by a 12 percentage point decrease in attendance.[5] In addition, holding fixed the level of absolute pay, we find little evidence that performance improves if a worker is paid more than his peers. In fact, in units where there is *Pay disparity*, the highest and median wage workers also have substantively lower attendance than their counterparts on *Compressed* units (10 and 13 percentage points, respectively). We also see no evidence of positive impacts on output for these workers. Overall, we estimate that workers give up 9.3% of their earnings to avoid a workplace where they are paid differently than their peers. The negative effects of *Pay disparity* on labor supply persist over the duration of the employment period and appear to strengthen in later weeks.

Perceived justifications mediate these morale effects. Each of our two sources of variation in perceived justifications yields the same pattern of effects. First, when co-workers' baseline productivity levels are farther apart—so that differences in productivity swamp differences in wages— *Pay disparity* unit workers have the same attendance and output as their counterparts on *Compressed* units. Second, in production tasks where workers can easily see that their higher paid peers are more productive than themselves, we also find no negative effect of pay disparity. These findings indicate that fairness violations are only triggered when the rationale for pay differences is not extremely clear to workers.

If these morale effects operate through emotions such as resentment or envy of co-workers, this could generate hostility and reduce social cohesion among unit members. This could

[4]We quantified the observability of each of the ten tasks ex ante in a pilot. A different sample of workers—all of whom were paid *Compressed* wages—were asked after three weeks of work to rank their output relative to that of their unit-mates. We use the mean accuracy of responses for each task to proxy for task observability.
[5]Attendance accounts for about half of the output decline.

help explain, for example, why workers in *Pay disparity* give up substantial earnings to avoid coming to work—even those who are paid relatively more than their peers. We designed two cooperative endline games, conducted on the last day of work, to investigate effects on group cohesion. The games have no benefit for the firm, ruling out motives such as reciprocity or beliefs about the firm. In both games, workers are paid group piece rates for performance. This enables us to test whether *Pay disparity* induces a group dynamic that subsequently affects co-workers' ability to cooperate in their own self-interest.

In the first game, workers are organized into their product units, and build towers out of raw materials. Each unit is paid a piece rate based on the height of its tower. *Pay disparity* units build towers that are 17% shorter on average than those with *Compressed* wages. However, in units where pay differences are justified—based on baseline productivity differences or task observability—*Pay disparity* units perform as well as *Compressed* units.

In the second set of activities, workers play cooperative puzzle games in pairs. Workers are randomly paired with someone from either their own product unit or from another unit. *Compressed* unit workers perform better on these games when paired with someone from their own product unit than when paired with a "stranger" (someone from another unit). In stark contrast, *Pay disparity* workers perform 28% worse when they are paired with someone from their own unit than with a stranger. In addition, when in mixed pairs, there is no evidence that *Pay disparity* workers perform worse than *Compressed* pay workers—their decrease in performance arises only when paired with someone from their own unit. Finally, when pay disparity is clearly justified, we cannot reject that *Pay disparity* workers perform similarly to *Compressed* workers—regardless of with whom they are paired.

Data from an endline survey also suggests a decrease in social cohesion. Specifically, after over a month of working together, *Pay disparity* workers report fewer social network connections—defined as a willingness to borrow or lend, seek or give advice, or visit one-another's homes—than *Compressed* workers. Survey responses also provide suggestive evidence about fairness perceptions. Relatively lower paid workers in *Pay disparity* units are more likely to state that their wages were set unfairly in relation to their unit-mates than low rank workers in *Compressed* units. In contrast, *Pay disparity* workers with relatively higher wages do not believe their wages are unfair; however, they are substantially less likely to report being happy at endline. While only suggestive, this is consistent with a decrease in attendance for higher-paid workers being driven by resentment or hostility in the work environment.

This study builds on the literature on relative pay in the workplace. In a recent field experiment, Card et al. (2012) document that University of California employees report higher job dissatisfaction on surveys when they find out they are paid less than their co-workers. In addition, Cohn et al. (2012) show that random relative pay cuts matter more than absolute pay cuts for effort; these effects persist over a six-hour period. Our results

are consistent with those of both studies.[6] More generally, our work relates to the broader literature on fairness preferences and effort provision, such as gift-exchange (Akerlof 1982).[7]

Our study advances this literature. We document conditions under which relative pay comparisons can substantially impact output and labor supply, with workers giving up earnings to avoid a workplace with ("unjustified") pay disparity. We also document deleterious impacts on group cohesion and cooperation. This dimension has been emphasized in social psychology and sociology—albeit with limited identified evidence—but largely ignored in the empirical behavioral economics literature to date. In addition, in our experiment, relative wages are not arbitrary, but rather reflect productivity differences; this is important if justifications can undo fairness violations (Falk et al. 2008, Bracha et al. 2015) and matches why pay disparity may arise in the labor market. We provide direct field evidence that perceived justifications play an essential role in mediating morale effects.[8] This has bearing on understanding, for example, why wage compression may arise in some settings or occupations and not in others. Finally, workers make decisions for a job from which they derive their primary source of income over the one-month study period. Given low baseline levels of income and employment, the decision to give up earnings is therefore meaningful. The time horizon also indicates that impacts do not disappear once the novelty of wage changes wears off (Gneezy and List 2006, Levitt and List 2007).

We find evidence that relative pay enters workers' utility function, with the potential for sizable negative impacts on labor supply and group cohesion. However, our findings indicate that pay inequality in itself is not necessarily problematic—at least not if it is clearly justified in the worker's eyes.

Despite the magnitude of the results in our setting, one cannot draw conclusions about optimal pay structure. One potential benefit to firms of differential pay is dynamic incentives: workers know that if they work hard now, it could lead to higher pay in the future. Similarly, a dynamic salary history may generate incentive effects by creating reference dependence in one's previous pay level. Our design intentionally shuts down such channels

[6]A number of other field studies are also consistent with a relationship between relative pay and worker utility (Rege and Solli 2013, Dube et al. 2015, Cullen and Pakzad-Hurson 2015, Levine 1993, Pfeffer and Langton 1993, Clark and Oswald 1996, Hamermesh 2001). Earlier work has used gift exchange games in the laboratory to examine the effects of different pay levels (Charness and Kuhn 2007, Gatcher and Thoni 2010, Bartling and von Siemens 2011, Bracha et al. 2015) and ranks (Brown et al. 2008, Clark et al. 2010, Kuziemko et al. 2014), but with mixed results. It is possible that in our setting—where co-workers are not anonymous and repeatedly interact over a month—there is potentially larger scope for peer wages to matter for the reference point relative to in the lab.

[7]In contrast to our focus on relative pay, gift exchange posits reference dependence in a worker's own absolute wage (past or expected). A large body of work finds gift exchange in the lab (e.g., Fehr et al. 1993, Charness and Kuhn 2011) but much more limited evidence in the field (Gneezy and List 2006, Kube et al. 2013, Esteves-Sorenson and Macera 2015, DellaVigna et al. 2016, List 2009).

[8]Consistent with this, Pfeffer and Langton (1993) document that wage dispersion within academic departments is negatively correlated with faculty satisfaction and co-authorships. While not focused on relative pay, Chen and Horton (2016) also provide evidence that justifications increase worker willingness to accept an absolute pay cut in an online labor market.

because after the baseline period, there is no further chance of wage changes (or even of future employment). This is important for our goal: cleanly isolating whether workers care about relative pay. It is also a realistic feature of seasonal and other contract jobs—a common form of employment among the workers in our study. However, in choosing the optimal pay structure, a firm would weigh any potential costs of differential pay (e.g. morale effects) against the potential benefits (e.g. dynamic incentive or selection effects). Our findings indicate that workers' relative pay concerns, especially in situations where pay differences are difficult to clearly justify, could affect this calculus.

Section 2 below presents a brief framework. We lay out the experiment design in Section 3. Section 4 outlines the empirical strategy and Section 5 presents the results. In Section 6, we discuss potential alternate explanations and threats to validity. Section 7 concludes. All appendix figures and tables, as well as detailed experimental protocols, are available in the Online Appendix.

## 2. Framework

We adapt the framework of DellaVigna et al. (2016), in which workers' social preferences affect effort provision. We modify their approach to allow peer wages to affect morale.

We assume that a worker $i$ receives a wage offer $w_i$ from the firm and makes two decisions: a) whether to work, $s_i \in \{0, 1\}$; and b) if $s_i = 1$, how much effort $e_i \geq 0$ to exert. Effort is not contractible by the firm. If the worker chooses not to work (i.e., $s_i = 0$), then he receives a stochastic outside option, $R_i = R + \varepsilon_i$. His payoff from working is:

$$V\left(w_i, w_R, e_i; \theta_i\right) = w_i - c\left(e_i; \theta_i\right) + M\left(w_i, w_R\right) e_i,$$

where $c\left(e_i; \theta_i\right)$ is a convex effort cost, $\theta_i$ is a worker-specific productivity parameter, and $M\left(\cdot\right)$ is a morale effect term that depends on the worker's own wage and a reference value, $w_R$. The worker chooses to supply his labor if the value from working is greater than his outside option, that is: $s_i = 1\left(\arg\max_e V\left(w_i, w_R, e_i; \theta_i\right) \geq R_i\right)$. Note that this formulation leads to potential effects on both effort $e_i$ and attendance $s_i$. A decrease in M(·) leads to lower benefits from exerting effort and will decrease $e_i$. Given the restriction $e_i \geq 0$, if $M\left(\cdot\right)$ falls far enough, the participation constraint will no longer be satisfied, leading to $s_i = 0$.

We conceptualize relative pay concerns as reference-dependence in utility, where the worker's reference value is a function of co-worker pay (and productivity):

$$w_R = r\left(\mathbf{w_{-i}}, \theta_i, \theta_{\mathbf{-i}}\right).$$

We incorporate this into the worker's morale effect term:

$$M\left(w_i, w_R\right) = \alpha f\left(w_i - w_R | w_i < w_R\right) + \beta f\left(w_i - w_R | w_i > w_R\right) + g\left(w_i\right).$$

This formulation captures the idea that morale is at least partially determined by relative wage differences. The function $f(\cdot)$ is monotonically increasing in the gap between the

worker's wage and his reference value, and $f(0) = 0$. We allow for asymmetric effects of differences between own and reference wages. Specifically, if the worker is paid less than the reference wage, $w_i < w_R$, the morale component of his utility increases by $\alpha f'(w_i - w_R)$ per unit of effort, relative to the case where $w_i = w_R$. Consequently, the direction of the effect on both $e_i$ and $s_i$ is pinned down by the sign of $\alpha$. Similarly, if $w_i > w_R$, the direction of the effect on $e_i$ and $s_i$ (relative to when $w_i = w_R$) is pinned down by the sign of $\beta$. Finally, $g(w_i)$ captures peer-independent contributors toward morale, such as gift exchange.[9]

Note that this is a reduced form specification of worker utility. The parameters $\alpha$ and $\beta$ capture both innate preferences and social dynamics in the workplace. Prior work conceptualizing relative pay comparisons predicts that $\alpha < 0$, that is, individuals dislike being paid less than their peers (i.e., Adams 1963, Akerlof and Yellen 1990). The prediction on the sign and relative magnitude of $\beta$, however, varies. Preferences for status or advantageous inequality generate $\beta > 0$. However, if workers have inequity aversion (e.g., Fehr and Schmidt 1999), or if pay disparity generates strife or a breakdown in social relations in the workplace, then this could generate $\beta < 0$ (e.g., Shaw 2014).

In this simple framework, changes in $1(w_i < w_r)$ and $1(w_i > w_r)$, holding fixed $w_i$, will affect both the probability of accepting work and effort if $\alpha$ and $\beta$ are non-zero. In the experiment, we generate variation in $\mathbf{w_{-i}}$ (i.e. co-worker wages). This enables us to construct test cases where the value of $1(w_i < w_r)$ or $1(w_i > w_r)$ is clear. For example, we posit that $w_i < w_r$ if a worker is paid strictly less than his co-workers, but not when he is paid the same as all his co-workers. We infer the sign of $\alpha$ and $\beta$ by examining the impact on labor supply of changes in $1(w_i < w_r)$ and $1(w_i > w_r)$, respectively.

It is important to note that the reference wage is not directly observable to the researcher. To make progress, we use experimental manipulations to test whether workers care only about wage differences in levels (i.e. $w_R = r(\mathbf{w_{-i}})$), or whether relative productivity also enters into workers' assessments of pay equity ($w_R = r(\mathbf{w_{-i}}, \theta_i, \theta_{-\mathbf{i}})$). It is also plausible that workers might form their reference wages using subjective assessments $\hat{\theta}_i, \hat{\theta}_{-\mathbf{i}}$ of own and co-worker productivity ($w_R = r\left(\mathbf{w_{-i}}, \hat{\theta}_i, \hat{\theta}_{-\mathbf{i}}\right)$). In our experiment, we vary the composition of co-workers, in terms of their productivity. Conceptually, this enables us to change $\theta_{-\mathbf{i}}$ (and $\hat{\theta}_{-\mathbf{i}}$), holding fixed own productivity, generating orthogonal variation with respect to $w_i$ and $\mathbf{w_{-i}}$. We also use random variation in task observability stemming from the randomization of units to tasks, which leads to differences in how easy it is a for a worker to infer the productivity of his co-workers. These sources of variation can provide insight into workers' underlying notions of equity.

[9]Alternately, $g(w_i)$ could capture other drivers of effort such as monitoring or norms for effort provision.

## 3. Experimental Design and Protocols

3.1. **Experimental Design.** We explore relative pay concerns using a field experiment with manufacturing workers. In our setting, workers are paid a flat daily wage for attendance. Consequently, as in the framework above, extensive margin changes in labor supply (i.e. attendance) affect earnings, whereas there is incomplete contracting on effort.

3.1.1. *Reference group.* We must first be able to isolate, for each worker, a clear reference group of peers for pay comparisons. In the factories, we organize workers into production units of 3 workers each. All workers within a unit produce the *same* exact product, while every unit in a factory produces a *different* product. Production is strictly an individual activity. Because each worker's two unit members are the only other people at the factory making the same product, they constitute the most salient reference group for wage comparisons. We empirically validate this intuition in Section 5.1.[10]

3.1.2. *Wage treatments.* We construct wage treatments to generate variation in co-worker pay, holding own absolute pay fixed. Using baseline productivity data (see below), we rank each worker as the lowest, medium, or highest productivity worker within his respective unit. Each unit is then randomized into one of four wage structures, as shown in Table 1:

- *Pay disparity*: Each worker is paid according to his baseline productivity rank within the unit. In each unit, the lowest rank worker receives $w_L$, the middle rank receives $w_M$, and the highest rank worker receives $w_H$. Thus, all three co-workers in the unit always have a different wage from each other.
- *Compressed Low*: All unit members are paid the same daily wage of $w_L$.
- *Compressed Medium*: All unit members are paid the same daily wage of $w_M$.
- *Compressed High*: All unit members are paid the same daily wage of $w_H$.

The difference between each of the three wage levels is fairly modest: less than 5%.

This design enables us to compare workers who have the same expected productivity and are paid the same absolute wage, but differ in their wage relative to their co-workers. For example, low rank workers in *Compressed Low* are paid $w_L$—the same wage as the other workers in their unit. The low rank workers in *Pay disparity* also receive an absolute wage of $w_L$, but earn strictly less than their co-workers. If being on a unit where one is paid less than one's peers reduces morale, low rank workers in *Pay disparity* units will have worse performance than their counterparts in *Compressed Low* units. The design generates three such pair-wise comparisons, illustrated in each respective row of Table 1.

3.1.3. *Perceived justifications.* We incorporate two sources of variation in the perceived justification for pay differences. First, while wage differences are fixed at discrete intervals, underlying baseline productivity differences among co-workers are continuous.This generates

[10]This view is consistent with Card et al. (2012), who find that University of California workers are most likely to compare pay with those in the same department.

variation in the ratio of {wage difference}/{productivity difference} within and across wage treatments. Because workers are randomly organized into their product units, these baseline productivity differences are exogenously determined.

Second, we exploit the extent to which workers can observe co-worker productivity. Ten different products are manufactured in the factories. These products differ substantively in how easy it is to observe the output of one's unit-mates—for example, whether piles of output build up quickly, clearly highlighting production differences across co-workers. To ex ante quantify the observability of each production task, we used a pilot that was conducted with a different sample of workers before the experiment began. These pilot participants were paid equal wages and asked to rank their productivity relative to their co-workers at the end of three weeks. We use the accuracy of responses to this exercise as a proxy for output observability for each product. We stratify wage treatments by production task, enabling us to test for the effects of observability within and across wage treatments.

We examine whether variation in perceived justifications mediates the effects of *Pay disparity*. This provides insight into workers' notions of what constitutes fairness in wages (e.g. Adams 1963). Figure 2 summarizes the sources of variation in the experiment design.

### 3.2. **Context, Protocols, and Social Cohesion Tests.**

3.2.1. *Context.* We conduct our study in a developing country setting, among rural Indian workers. Drawing on the methodology developed by Kahneman et al. (1986), in Figure 1, we document perceptions about the consequences of pay inequality among this population. Respondents predict that, for a given absolute wage level, a worker who is paid less than his peers would be less likely to accept employment (Question 1) and would reduce effort (Question 2). In addition, 94% of respondents state that pay disparity will lead to conflict among co-workers (Question 3). While only suggestive, these perceptions match those of respondents in other contexts such as the U.S. (e.g., Bewley 1999).

To conduct the experiment, we set up three workshop sites in India, located in semi-rural areas surrounding the city of Bhubaneswar, Odisha. Workers are employed in low skill manufacturing of local products such as rope, brooms, incense sticks, candle wicks, disposable plates, and floor mats. Using a partnership with local contractors—who provide expertise in production, set quality and training standards, and sell all output in the local wholesale market—we manage the operational implementation of the experiment.

In this area, laborers work in agriculture during peak planting and harvesting, which comprise about four months of the year. In the remaining lean agricultural months, they typically seek short-term contract employment in non-agricultural jobs, such as manufacturing and construction. In our experiment, workers are employed in such seasonal contract jobs full-time for one month. During the period of employment, the job constitutes the primary source of household earnings. These earnings are particularly valuable because of

high unemployment in the agricultural lean season. For example, before recruitment, the average worker reports involuntarily unemployment for 15 of the past 30 days (Table 2).

In accordance with the typical pay structure in the area, workers are paid a flat daily wage for each day they come to work.[11] There are no strict production minimums, but workers can be fired for excessive absences (more than three days in a row) or disruptive behavior. All workers have experience with flat daily wage pay. In addition, they are familiar with the concept of performance pay—for example, because piece rates are often used during harvesting—and about 45% have worked under piece rates themselves. In addition, factories will sometimes pay workers differential flat wages based on experience. All workers in the sample are adult males; see Table 2 for demographic descriptives.

3.2.2. *Timeline & Protocols.* The experiment was run in 14 one-month rounds across three workshop sites. Figure 3 details the within-round timeline. For each round, workers are recruited from a different local labor market (set of villages) within a 15 km radius of the workshop. The position is advertised as a one-time contract job. Each round requires hiring 30 workers; if more than 30 workers apply for the job, we randomly select among applicants.[12]

On the first day of work, workers are randomly assigned into one of the ten product units, with three workers per unit. Each worker's unit (and therefore production task) remains the same throughout their employment. The workers within a unit sit together in a designated area—they work together and also eat lunch together in their production area. Each unit is physically separated from other units—often in separate rooms—due to space requirements for each production activity and for storing inputs nearby.

Production is strictly an individual activity—all workers produce their product from beginning to end on their own, with no joint production within units. We hire numerous additional staff, who sit in the workshops and count each worker's individual production of goods of salable quality (e.g. number of individual incense sticks or paper bags) in a separate back storage room. This enables us to obtain daily measures of worker output.

Workers initially undergo a "training period" on how to produce their assigned product. Typically after day four, output has reached a level of quality that can be sold in the market. However, we elongate the training period to two weeks to obtain stable baseline productivity measures for each worker once experience trends begin to flatten out. During training, all workers are paid a daily wage that corresponds to the prevailing daily wage in the area (e.g. Rs. 250/day or ~$4/day). On the first day of work, they are told that their post-training wage may depend on their productivity during the training period. In

[11]In general, firms in the area pay both flat daily wages and piece rates for low skill manufacturing—likely based on their preferences for quality and their monitoring technology.

[12]Four of the rounds involved fewer than 30 workers (15, 18, 21, and 24 workers); the remaining rounds had 30 workers exactly.

addition, on Day 10, each worker is given private feedback on his own productivity rank within his unit (as part of a routine check-in with the manager).[13]

On Day 14, each product unit is randomized into one of the four wage treatments in Table 1.[14] For the wage assignments, each worker's productivity rank within his unit is defined based on performance in the final three days of the training period (Days 11-13).[15] All workers get a pay raise relative to the training wage—with $w_L$, $w_M$, and $w_H$ corresponding to a roughly 4%, 8%, and 12% pay increase, respectively (with wage levels of e.g., Rs. 260, Rs. 270, and Rs. 280). Consequently, even $w_L$ constitutes a wage premium above workers' outside option. On Day 14, each worker is told his own post-training wage privately by his manager. As is typical in the setting, managers maintain pay secrecy, so that workers are not informed of co-workers' wages. Consequently, to the extent workers learn about relative wages, it is through self-disclosure among co-workers. We return to this in Section 5.1.

After wage treatments are implemented, workers continue to work in their assigned units for an additional 20 days—allowing us to observe subsequent output and attendance. On the final day (Day 35), we conduct tests for social cohesion and administer an endline survey.[16]

To ensure understanding of the timeline, each worker is given an individualized calendar on Day 1. The calendar highlights the date that training will end (and wages will increase) as well as the last day of the work contract. On Day 1, the training wage amount (Rs. 250) is filled in on the calendar for each training day, with the remainder of the days left blank. On Day 14, the worker's post-training wage is written on the calendar for each of the remaining work days. Each worker sees his individual calendar each day upon arrival to work. This makes it clear to workers that: there will be a wage increase on Day 14 (which may depend on baseline productivity); the post-training wage is fixed, with no further wage changes; and the employment period ends on a pre-set date. In the Online Appendix, we provide a detailed account of scripts and protocols, including how we fixed worker beliefs and the steps we took to make the environment as natural as possible.

3.2.3. *Endline Tests for Social Cohesion.* As discussed above, the impact of pay disparity can operate through both individual preferences over relative pay as well as group-level

[13]This is based on each worker's production in the previous three workdays. This helps underscore to workers that we are paying attention to productivity. It also helps ensure our subsequent wage treatment effects are not confounded by information revelation about relative ranks.

[14]An implication of our design is that within a workshop, different units have differing pay structures and average pay levels. This is not odd since every unit within a workshop produces a unique product and is associated with a distinct contractor.

[15]Results are robust to dropping the small number of units where ranks are different within this window relative to the full training period.

[16]The average round lasted approximately 22 work days and 35 calendar days. At times, workers dropped out early in the pre-period. These workers were replaced, so long as the dropouts occurred early enough (i.e., no later than three days before the scheduled wage treatment day). Note, this creates heterogeneity in the number of workdays across workers within a round.

dynamics such as social cohesion and conflict. To the extent that pay disparity generates feelings such as envy or resentment, these two channels are inherently related.

To explicitly test for effects on social cohesion, we design a set of cooperative games on the last day of employment, as part of a "fun farewell". In these games, workers are paid piece rates for group-level performance. There is no benefit to the firm from the games—ruling out motives such as retaliation against the firm in driving behavior.

We implement two types of activities, which we describe in detail in Section 5.6. In the first activity, workers play in their assigned product units, enabling us to examine general effects on team cooperation. In the second activity, we induce variation in whether workers are paired with someone from their own product unit versus a "stranger" (someone from another unit). This enables us to distinguish general changes in worker disgruntlement from dynamics that are specific to working with one's co-unit members. These activities allow us to test whether *Pay disparity* affects workers' ability to cooperate in their own self-interest.

Finally, we use an endline survey to obtain additional suggestive evidence on mechanisms. We ask workers about social ties at endline—for example, whether, after working together for over a month, they would visit the home of or borrow money from their co-workers. We also collect self-reports on the perceived fairness of wages and worker happiness.

## 4. Empirical Strategy

4.1. **Outcomes.** Attendance is a binary variable capturing whether worker $i$ is present on day $t$. We also record the continuous, total output of salable products by each worker $i$ on each day $t$. We code raw production as zero when a worker is absent. We standardize output within each task—using the mean and standard deviation for that task in the final three days of the training period.[17] This enables us to pool across tasks and measure output using consistent units: standard deviations from the mean.

Table 2 presents baseline values of attendance and standardized production in the full training period. Attendance is approximately 95%. Both baseline attendance and productivity are balanced across the *Compressed* and *Pay disparity* units. Appendix Figure 5 documents, within *Compressed* units, both the stability of relative productivity ranks over time as well as the stability of output levels even in the post period.

4.2. **Empirical Specification.** To test our key predictions, we compare outcomes between individuals in the *Pay disparity* and *Compressed* units, holding fixed a worker's production rank and wage. Recall from Table 1 that the most direct comparisons are between the Low rank *Pay disparity* worker with the Low rank *Compressed Low* worker, the Medium rank *Pay disparity* worker with the Medium rank *Compressed Medium* worker, and the High rank *Pay disparity* worker with the High rank *Compressed High* worker. We refer to this set of six rank-treatment cells, which are circled in Table 1, as the "relevant group". To

[17]Recall this corresponds to the days used to compute productivity ranks. Standardized output over the full training period is 0.2 standard deviations lower on average than during these final three days (Table 2).

use all of the variation in our experimental data, we use a differences-in-differences strategy that incorporates the pre-period production information.

The most basic differences-in-differences approach restricts the sample to the "relevant" group of six rank-treatment cells:

$$\begin{aligned} y_{it} \;=\; & \alpha_1 \left[Post_t \times PayDisp_i \times Low_i\right] \;+\; \alpha_2 \left[Post_t \times PayDisp_i \times Med_i\right] \\ & + \alpha_3 \left[Post_t \times PayDisp_i \times High_i\right] \;+\; \alpha_4 \left[Post_t \times Low_i\right] \;+\; \alpha_5 \left[Post_t \times Med_i\right] \\ & + \alpha_6 \left[Post_t \times High_i\right] \;+\; \lambda_{\mathbf{i}} \;+\; \tau_{\mathbf{t}} \;+\; \eta_1 x_{kt} \;+\; \eta_2 x_{kt}^2 \;+\; \varepsilon_{it}. \end{aligned} \tag{4.1}$$

In all specifications, $i$ indexes the worker, $k$ indexes the task, and $t$ indexes the day-round. $Post_t$ is a binary indicator that equals 1 on the days after the wage treatment takes effect within the round. $PayDisp_i$ is an indicator for being a member of a *Pay Disparity* unit. The variables $Low_i$, $Med_i$ and $High_i$ are binary indicators for productivity rank, as defined in Section 3.2.2. Any time-invariant unit or worker characteristics are absorbed by the worker fixed effects, $\lambda_{\mathbf{i}}$, while any time trends are captured by the day-by-round fixed effects, $\tau_{\mathbf{t}}$. Finally, $x_{kt}$ and $x_{kt}^2$ allow for task-specific quadratic experience trends.

The key treatment effects of interest are captured by $\alpha_1$, $\alpha_2$, and $\alpha_3$. The coefficient $\alpha_1$ measures the average change in outcomes in the post period (relative to the training period) for Low rank workers in *Pay disparity* units relative to those in *Compressed L* units. Similarly, $\alpha_2$ captures the average treatment effect for Medium rank workers in *Pay disparity* vs. *Compressed M* units. $\alpha_3$ captures the average treatment effect for High rank workers in *Pay disparity* vs. *Compressed H* units.

Note that by restricting the sample to the "relevant" workers, the above specification ignores observations from the "irrelevant" workers (the complement of the "relevant" group). These additional observations can be used to help estimate the controls: round-by-day fixed effects and experience trends. To improve precision, we augment Equation (4.1):

$$\begin{aligned} y_{it} \;=\; & \alpha_1 \left[Post_t \times PayDisp_i \times Low_i\right] \;+\; \alpha_2 \left[Post_t \times PayDisp_i \times Med_i\right] \\ & + \alpha_3 \left[Post_t \times PayDisp_i \times High_i\right] \;+\; \alpha_4 \left[Post_t \times Low_i\right] \;+\; \alpha_5 \left[Post_t \times Med_i\right] \\ & + \alpha_6 \left[Post_t \times High_i\right] \;+\; \bar{Irrel}_{it}'\theta \;+\; \bar{Neigh}_{it}'\gamma + \lambda_{\mathbf{i}} + \tau_{\mathbf{t}} + \eta_1 x_{kt} + \eta_2 x_{kt}^2 + \varepsilon_{it}. \end{aligned} \tag{4.2}$$

We define $Irrel_i$ as an indicator for whether a worker is in the "irrelevant" group (not among the six "relevant" rank-treatment cells). In Specification (4.2), $\bar{Irrel}_{it}'$ is a vector of interactions of $Irrel_i$ with $Post_t$ and each rank. These interactions fully absorb the change in outcomes for "irrelevant" workers in the post period; the main coefficients of interest—$\alpha_1$, $\alpha_2$, and $\alpha_3$—are therefore still estimated off of only the "relevant" group. Consequently, this improves statistical power without affecting identification.

In addition, we define $Neigh_i$ as an indicator for whether a unit is located in the same physical room or production space as another unit, facilitating conversation across units.[18]

[18]Usually, units were in separate physical rooms so this wasn't possible. Seating charts available on request. Due to random assignment, $Neigh_i$ is exogenous to both treatment status and worker characteristics.

In Equation (4.2), the vector $\bar{Neigh}_{it}{}'$ contains interactions of $Neigh_i$ with $Post_t$, the treatment and rank of worker $i$, and the treatment of $Neigh_i$. Thus, the coefficients $\alpha_1$, $\alpha_2$, and $\alpha_3$ are identified off of units with no neighbors. In neighborless units, it is extremely clear that one's two product unit co-workers comprise the relevant reference group.[19]

To maximize statistical power, we use Equation (4.2) as our main specification. Our results are similar if we exclude "irrelevant" workers or omit the $\bar{Neigh}'_{it}$ controls. Appendix Table 2 documents robustness to these and other changes to our estimating equation.

We also examine the pooled effect of *Pay disparity* across Low, Medium, and High ranks:

$$y_{it} \quad = \quad \alpha\left[Post_t \times PayDisp_i\right] + \bar{Irrel}'_{it}\theta + \bar{Neigh}'_{it}\gamma + \lambda_i + \tau_t + \eta_1 x_{kt} + \eta_2 x_{kt}^2 + \varepsilon_{it}. \quad (4.3)$$

The main coefficient of interest, $\alpha$, is still identified off of comparisons within the "relevant" cells. It therefore captures the average treatment effect of *Pay disparity* compared to *Compressed* pay (holding fixed absolute pay) across all worker ranks.

Finally, we test whether perceived justifications mediate the effects of pay inequality. As described in Section 3.1, we built two sources of heterogeneity in perceived justifications into the design: productivity differentials and output observability. To examine their role, we fully interact the variables encoding treatment status, rank, and $Post_t$ in Equations (4.2) and (4.3) with each of the justification measures.

## 5. Results

5.1. **Knowledge of Co-worker Wages.** Given that managers maintain pay secrecy throughout the experiment, a necessary condition for the wage treatments to affect productivity is that workers actually learn co-workers' wages. We verify this using the endline survey. Panel A of Table 3 reports that among *Compressed* unit workers, 95.8% correctly report the wage of at least one co-worker, and 90.9% correctly report the wages of both co-workers. Workers from *Pay disparity* units are less accurate—with 87.1% and 74.2% of respondents reporting the correct wage of at least one or both co-workers, respectively. Note that these differences are not mechanically driven by workers simply guessing that their co-workers' earn the same wage as themselves; in only four instances do *Pay disparity* unit workers believe that both their co-workers earn the same wage.[20] These findings indicate that overall, there is substantial learning within production units. In addition, *Compressed* unit workers have statistically significantly more accurate beliefs. This suggests that it may be awkward for co-workers to discuss pay under pay dispersion.

[19]Our experiment isn't powered to investigate whether workers respond to the wage levels of neighboring units; such a question would require sufficient variation in the number of instances of neighboring units and in their wages. We leave this for future work.

[20]To see this more formally: Suppose that in these four instances, we count these as "correct" responses. This increases the proportion of correct responses among *Pay disparity* units in row 1 to 0.785. This is still substantively lower than the proportion correct in the *Compressed* units of 0.909, and this difference is statistically significant (p-value of 0.018).

For the treatments to have power, it is also important that workers consider their product unit as their reference group. In the endline survey, each worker was also asked to list the wages of each member of the incense stick, candle wick, floor mat, and paper bag production units—each of which could be assigned to either *Pay disparity* or *Compressed* pay, depending on the round. In Panel B of Table 3, we examine how well individuals learn about the wages of other units. Strikingly, the majority of workers have no opinion about the wages of any of the workers on these other units. Approximately 87% of respondents are not willing to wager a guess about the wages on a given *Compressed* production unit, and only 8.6% of respondents can accurately list all of the wages of that unit. It appears even harder to learn about the wages on *Pay disparity* units, with only 1.9% of respondents able to accurately list the wages of all workers on a given such unit. These findings support our assumption that workers primarily compared their wages with their two other unit-mates.

Of course, while our field checks indicate that managers followed pay secrecy protocols, we cannot guarantee that in no circumstance did managers disclosure relative pay information. However, the above indicates the pattern of worker knowledge corresponds to what we expected to achieve with our design.

5.2. **Average Effects of Pay Disparity.** Figure 4 provides an overview of the patterns in the underlying production data. It plots average standardized production on each day for each of the 3 sets of "relevant" pairwise comparisons. Among Low rank workers in the baseline period, production in the *Compressed Low* and *Pay disparity* units shows a common trend as workers gain experience. The treatment ("post") period begins on day 0, when each worker is told his post-training wage. Within about 5 days (i.e. by the first payday following the wage change), differences in output start to emerge, with workers on *Pay disparity* units (who are paid less than their peers) reducing output relative to the *Compressed L* units. This delay in the onset of treatment effects is consistent with non-immediate diffusion of pay information among unit members. In addition, there is no evidence of positive effects of being paid relatively more than one's peers: High rank workers in *Pay disparity* units appear to perform worse than those in *Compressed H* units. This pattern also generally holds for Medium rank workers in *Pay disparity* versus *Compressed M* units.

Panel B of Table 4 presents regression estimates of treatment effects separately for each rank. Low wage workers in *Pay disparity* units reduce output by 0.385 standard deviations relative to those in Compressed Low units (significant at the 1% level). This effect is robust to the inclusion of individual fixed effects (Col. 2, our preferred specification corresponding to Equation (4.2)), indicating that $\alpha_1 < 0$. The 0.332 standard deviation decrease in Col. 2 is equivalent to a 22% reduction in output relative to the *Compressed Low* (control) treatment mean. In addition, we find little evidence that performance improves when workers are paid more than their peers. In fact, there are large, but statistically insignificant decreases in production for High rank workers in *Pay disparity* units relative to their counterparts

on *Compressed High* units. We find similar results for the Medium rank workers on the *Pay disparity* units relative to those in *Compressed Medium* units. In contrast, we see little evidence for changes in quality (Appendix Table 3).[21]

In addition, *Pay disparity* reduces attendance for all ranks. Treatment effects for Low, Medium, and High wage earners are -12, -12.9, and -10.4 percentage points, respectively (all significant at 5% level, Col. 4), off a base of 94% attendance among *Compressed* units.

Because we code a worker's raw production as zero when he is absent, treatment effects on standardized production combine the extensive margin attendance effects with any intensive margin effort effects. We attempt to decompose these effects in two different ways. First, we regress output conditional on positive attendance in Col. 5. Conditional on coming to work, Low rank workers in *Pay disparity* units produce 0.2 standard deviations less than those in *Compressed Low* units (p-value 0.077). Because this is a selected sample, this result must be interpreted with caution. However, if the most aggrieved workers are those who decide to skip work, this coefficient is likely to underestimate the true intensive margin effect. Second, we use a back-of-the-envelope calculation. Mean output conditional on attendance for Low rank workers in *Compressed Low* units is 1.64. The attendance effect of *Pay disparity* for the Low rank is -12 percentage points. If the full effect on production were coming through attendance, we would predict an output decrease of $-0.12 \times 1.64 = -0.197$ standard deviations. This corresponds to about half (59%) of the total effect on output. While both these decomposition approaches have their limitations, they suggest that in our setting, being paid less than one's peers likely negatively affects effort as well as attendance.

In contrast, the effects for the Medium and High wage earners appear to operate primarily through attendance. For these workers, there is little evidence of conditional intensive margin effort effects in Col. 5. Furthermore, the back-of-the-envelope decomposition suggests that the attendance effects can fully account for the (statistically insignificant) coefficients on standardized output. This suggests that Medium and High wage earners are less likely to show up to work, but there is no evidence that they reduce effort while at the workplace. While we did not have strong ex ante predictions for these workers, these effects may indicate a reduced desire to work alongside disgruntled co-workers—consistent with resentment or hostility at work. We further explore this idea below.

Given that the treatment effects of *Pay disparity* are negative in sign for all three ranks, the pooled specification of Equation 4.3 may provide improved statistical power. These results are presented in Panel A. Overall, individuals in *Pay disparity* units decrease production by 0.311 standard deviations and reduce attendance by 11.5 percentage points relative to individuals of the same rank and absolute wage level in *Compressed* units. We also report pooled regression results for all of the analyses that follow.

[21]It is difficult to quantify quality in our setting—one of the reasons for the prevalence of flat daily pay. For a subset of rounds, tasks and days, we had management rate the quality of each worker's output for that day on a scale of 1-5. We do not see evidence for a change in these subjective quality ratings. It is possible, however, that this subjective measure is too crude and noisy to enable us to examine such effects.

Finally, in Appendix Table 2, we show that the results in Table 4 are robust to a number of alternative specifications, including dropping "irrelevant" workers, omitting controls for the presence of other units in close proximity, and estimating treatment effects using only post-treatment observations. We also provide suggestive evidence in Appendix Figure 2 that attendance effects are driven both by increases in the number of days missed by the chronically absent and in the likelihood of workers missing only a few days of work.

*Earnings Consequences of Attendance Effects.* Because employees are not compensated when absent, the attendance effects indicate that workers are willing to give up full-time wage earnings to avoid a workplace where they are paid differently than their peers. However, the foregone factory earnings may overstate this value if workers can find outside casual employment when absent. To bound this value, we use survey data from individuals who applied for the factory jobs, but were not hired in the randomization protocol. These respondents report finding paid work on only 23.7% of days. Their average total cash plus in-kind wage, conditional on working, is Rs. 271. Thus, in the experiment, workers are paid an average daily wage premium of Rs. 39.

If absentee workers find outside jobs at similar rates as the non-selected workers, then the attendance treatment effect of *Pay disparity* causes workers to forego an average of Rs. 403 in earnings.[22] This corresponds to 9.3% of potential earnings in the factory in the post period. Furthermore, under the extremely conservative assumption that absentees find work at the prevailing wage every day they are absent, they are still giving up the average wage premium of Rs. 39 each day. These foregone earnings amount to 7.1% of the mean wage of Rs. 899 earned over the same period by individuals who were not selected for the factory jobs. While this exercise provides a sense for the size of the compensating differential, we note that it does not capture the welfare loss, as our calculations do not include the value of leisure or home production.

*Effects Across Time.* Table 5 examines the evolution of treatment effects over time. We separately estimate Equation 4.2 over three parts of the post period—the days before the first payday, between the first and second payday, and the days after the second payday—after wage treatments took effect. The full pre-treatment period is included in all regressions. Focusing on the pooled regressions in Panel A, the effects of *Pay disparity* are only evident after the first pay day, post wage change. This is consistent with non-immediate diffusion of wage information. It also matches supervisors' informal observations that workers were more likely to discuss wages on paydays, which occurred each Friday. Furthermore, we find little evidence that the effects wane over time.

[22]The wage premium is calculated using the average factory cash wage plus an in-kind wage of Rs. 40 (the monetary value of lunch), which is similar to the in-kind wage paid in the market. We assume a wage of 0 on days that workers cannot find work; on days they can find paid work, they only forego the wage premium. We use the pooled treatment effect on attendance of 11.7 percentage points and calculate that *Pay disparity* causes workers to miss 1.64 extra days of work (based on the modal number of workdays in the post period).

5.3. **Perceived Justifications.** We use the two (pre-registered) sources of heterogeneity built into our design to investigate if perceived justifications mediate morale effects.

5.3.1. *Baseline Productivity Differences.* We first examine treatment effects when workers' higher-ranked peers are substantially more productive than themselves. For each worker, we compute average baseline output conditional on attendance; this corresponds to what is observed by workers before wages change.[23] We then compare this baseline productivity average with that of the worker's next-higher ranked co-worker (i.e. for each low rank worker, the difference with his medium-ranked co-worker; for each medium rank worker, the difference with his high-ranked co-worker). Note that this variable is not defined for high rank workers. Recall that because workers are randomly assigned to units, baseline productivity differentials are exogenously determined.[24]

We define the productivity difference between a worker and his higher-ranked peer to be "large" when it is above 0.37 standard deviations, the mean difference among the "relevant" *Compressed* workers. In Columns 1-2 of Table 6, we interact this binary variable with treatment status. Panel A displays pooled treatment effects for Low and Medium ranks combined, while Panel B decomposes the effects by rank. There are large negative impacts of *Pay disparity* when productivity differences are small: Low and Medium rank workers decrease output by 0.358 standard deviations and attendance by 16.7 percentage points (both significant at 1% level). However, large baseline productivity differences almost fully mitigate these negative effects. When a worker is substantially less productive than his higher-paid peer, the total treatment effect of *Pay disparity* is indistinguishable from zero. Note that the $Post \times Perceived\,justification$ term in Panel A reveals no detectable changes in the behavior of *Compressed* units when productivity differences are large. These heterogeneous effects appear (though under-powered) for each rank separately in Panel B.

In Appendix Table 4, we show that the results are robust to alternate specifications, including adding interactions of the worker's own baseline productivity level with an indicator for post treatment.[25] Appendix Table 5 explores a range of productivity thresholds, defined as the percent difference in baseline output between a worker and his next higher-rank co-worker.[26] Productivity differences need to be quite large (on the order of 20%) for us to

[23]Specifically, we use the final six days of training—which are days used to compute the ranks that are conveyed to workers on day 10 and to compute ranks for the wage treatments on day 14.

[24]Baseline output differences among co-workers could also reflect team dynamics or culture. This potentially broadens the interpretation of the heterogeneous treatment effects presented here. The fact that baseline productivity ranks have stable predictive power in other domains, such as the group cohesion games in Section 5.6, suggests that these ranks do capture some inherent differences among co-workers.

[25]One potential concern is that when a Low rank worker has a large baseline difference with his Medium rank peer, this may mean his absolute productivity level is extremely low—leaving him little room to fall in response to *Pay disparity*, or indicating something else about his "type". In Cols. 3-4 of Appendix Table 4, we show robustness to adding controls so that the bottom decile of Low rank workers are not used to estimate effects. Cols. 5-6 add full interactions with own baseline productivity; here, the heterogenous effects are estimated off differences with one's higher-rank peer, holding fixed own productivity.

[26]Note that this scaling of productivity differentials is highly correlated with that used in the main analysis.

detect a significant offsetting response to *Pay disparity*. While this is substantially larger than the underlying difference in wages, it may simply reflect observability: it is difficult for workers to recognize that their peers are more productive unless differences are big. Regardless, this highlights why it may be difficult for firms to pay unequal wages. Even though managers told workers their productivity rank during the training period, this is not sufficient to justify differential pay—workers must observe these differences themselves.

5.3.2. *Output Observability.* We next check for mediating effects of whether co-worker output is observable. We quantify observability using data from 3-week pilot rounds with a different sample of workers (conducted before the start of this experiment). These workers were all paid *Compressed* wages, and were never told their relative productivity ranks. On the last day of the pilots, we asked workers to rank their co-workers by productivity.[27] We use the mean accuracy of these responses for a given task as a proxy for output observability. There is substantial variation across tasks in accuracy rates—from essentially 0 to 0.88 (Figure 5). We define tasks with accuracy rates above the sample median of 0.5 as "observable".

Columns 3-4 of Table 6 present heterogeneous treatment effects by task observability. The results mirror those in Columns 1-2. Panel A shows pooled effects across all three ranks. When co-worker output is difficult to observe, *Pay disparity* sharply lowers production and attendance. In contrast, there are no detectable total effects when co-worker output is easy to observe; the triple interactions are large and statistically significant. We also find no evidence that task observability differentially affects the performance of *Compressed* units post wage change. The same patterns emerge in Panel B, when we estimate heterogeneous effects separately by rank. We show in Appendix Table 6 that the results are robust to sequentially dropping each of the ten tasks.

As with any heterogeneous treatment effects analysis, one might be concerned that observability is correlated with other task characteristics. Appendix Figure 3 plots the distribution of output for each production task separately and shows that there is no discernible correlation between, for example, a task's observability value and output dispersion. More generally, for other correlated task characteristics to pose a problem, they would need to generate our pattern of results through a different mechanism. One possible trait might be the amount of social cohesion naturally developed during the training period. In Appendix Table 7, however, we find that observability is not correlated with turnover or attendance in the training period. Further, the fact that the pattern of results for task observability matches that in the productivity differences regressions bolsters our interpretation.

Note that observability and productivity differences derive from independent sources of variation – productivity differences from the randomization of individuals to units and observability differences from the randomization of units to tasks. Unsurprisingly, Appendix

[27]Two production tasks were added after piloting was completed; we followed a similar procedure with a separate sample of workers to quantify observability for these two additional tasks.

Table 8 verifies that there is substantial non-overlap among these two measures. Finally, we find no evidence to suggest that observable tasks simply have more natural productivity dispersion, making it easier to learn about productivity differences. In Col. (4) of Appendix Table 7, we show that observability is uncorrelated with the steepness of the learning curve.

5.3.3. *Aggregate Perceived Justifications.* Finally, for parsimony and power, we define an aggregate perceived justifications indicator. This binary indicator equals 1 if any justification is present—i.e., if an individual has a large productivity difference with his next-higher ranked peer, or he is on an observable production task.[28] Not surprisingly, heterogeneous treatment effects using this indicator are similar to those above (Table 6, Columns 5-6).[29]

5.4. **Effects of Pay Disparity at the Production Unit-Level.** Our key empirical tests are based on comparing workers who earn the same absolute wage, but differ in co-worker wages. However, our design also enables us to examine the effects of *Pay disparity* at the production unit level. Note that this exercise uses a different source of variation than the analyses presented above; all workers in each production unit are used for identification. Table 7 presents the results.[30] Panel A reports comparisons between *Pay disparity* units and all *Compressed* units combined. Panel B compares *Pay disparity* units with only *Compressed Low* units. This latter test is strong given that all workers on *Pay disparity* units are paid weakly more than those on *Compressed Low* units. Columns 1-2 present average treatment effects, while Columns 3-8 present heterogeneous treatment effects.[31]

On average, *Pay disparity* units have lower rates of attendance relative to all *Compressed* units pooled and to *Compressed Low* units. The average effects on production are also negative, but not statistically different from zero. When output differences are not large or co-worker output is not observable, compared to *Compressed Low* units, *Pay disparity* units have 0.306 standard deviations lower output and 8 percentage points lower attendance on average (Panel B, Cols. 7-8). However, when pay differences are clearly justified, no negative treatment effects are detectable. We again show that *Compressed* units behave no differently in the post period when there are large differences in productivity or when output is easily observable.

These results suggest that in our setting, *Pay disparity* is indeed detrimental to unit-level outcomes—even compared to *Compressed Low* units which have weakly lower absolute wages for all workers—when perceived justifications are not present. This indicates that, for the harder to observe tasks, firm output will be higher if the firm pays lower, compressed

[28]For high rank workers, this indicator always takes the value of task observability.
[29]Appendix Table 8 verifies that the productivity difference results and observability results are identified off of different sources of variation. There is substantial non-overlap among these two measures in the sample.
[30]This table uses the same specification as Equation 4.3, but eliminates all controls for "irrelevant" workers.
[31]Recall that observability is already defined at the production unit level. We define large productivity differentials at the unit level as an indicator for whether both the Low and Medium rank workers have large productivity differentials with the worker of next higher rank.

wages rather than productivity-based wages. However, it is impossible to pin down the impact on profits. For example, workers are not paid for absences, but absenteeism is nonetheless costly due to fixed costs of production. We focus on examining the relevance of relative pay for worker utility. The resultant optimal pay policy, however, will depend on a variety of context-specific factors.

5.5. **Effects of Absolute Pay Differences (Gift Exchange).** While not our primary goal, we can use wage variation across *Compressed* units to explore whether absolute pay affects performance. We examine this using differences-in-differences regressions in Table 8. If units reciprocate higher wages with higher productivity, then we should expect output to be increasing in pay. Further, any positive peer effects from harder working co-workers should amplify such effects. However, consistent with the previous field literature, we find no evidence of positive gift exchange on productivity at any time horizon—in the days immediately following wage changes or in the longer-run. Relative to *Compressed Low* units, *Compressed Medium* and *Compressed High* units do increase attendance in the short run—consistent with a simple substitution effect on labor supply. However, even these effects appear to dampen over time; we cannot reject that the attendance effect is zero in the full sample period. Note in our setting, even the *Compressed Low* wage is 4% above the prevailing wage in the area (i.e. workers' previous outside option) and workers were all expecting some wage increase after training, making their ex ante absolute reference wage unclear. Consequently, our design does not provide not a good test for gift exchange, and the lack of an effect could just stem from these contextual features of our setting.

5.6. **Relative Pay Effects on Group Cohesion and Dynamics.** The above results confirm our initial hypothesis that workers dislike disadvantageous inequality in pay: workers who are paid less than their peers reduce output and attendance. Further, we find striking evidence that even workers in a position of advantageous inequality forego wages to avoid the workplace. Recall that workers spend the entire day with their unit co-workers—sitting in close proximity and also eating lunch together at their stations. If *Pay disparity* fosters resentment between workers, then it might create a negative or even hostile work environment that workers would pay to avoid. Emotions such as acrimony or envy may also erode the ability of peers to cooperate even when it is in their own self-interest. Such forces are of course impossible to examine with just the output and attendance data alone. Consequently, we designed tests to enable us to directly examine social cohesion and cooperation.

5.6.1. *Teamwork Games.* We developed two sets of collaborative games that require teamwork. Workers played these games at endline on the last day of work, as part of a "fun farewell" day. Workers were paid their full wage plus piece rates based on performance in

one randomly-chosen game.[32] Importantly, there was clearly no benefit to the firm from worker effort on the games—ruling out reciprocity as a motive for performance.

In the first game, workers were asked to build a tower with the other members of their assigned product units. Each unit was given the same set of raw materials (e.g., cardboard, pens, rubber bands, playing cards) and was asked to build as tall a tower as possible within a 25 minute time limit. The payment schedule was a linear piece rate for the tower's height (in cm), paid equally to each unit-member.

*Pay disparity units* build towers that are 9.376 cm (17.5%) shorter than those of *Compressed units* on average (Table 9, Col. 1). However, as before, these negative effects are concentrated in cases where *Pay disparity* is not clearly justified (Cols. 2-4). When it is, we detect no difference between the performance of *Pay disparity* and *Compressed* units.

Do these results indicate general disgruntlement among *Pay disparity* workers, or are they driven by a breakdown in within-unit cooperation? To understand this, we constructed a second set of games in which workers solved two types of cooperative puzzles in pairs of two. In the "Spot the Difference" game, each person in the pair received a printed sheet with nearly-identical pictures on each sheet. The workers had to compare and circle any difference in the pictures on the two sheets (Appendix Figure 4, Panel A). In the "Symbol Matching" game, each pair member was given a sheet with a grid of symbols. Workers had to match symbols—circling all instances where the same symbol appeared in the same grid position in both of their respective sheets (Appendix Figure 4, Panel B).[33] In both games, both members of the pair received the same payment, a piece rate for every correct answer that was circled on *both* workers' respective sheets. For these partnered games, we constructed pairs by reshuffling workers across product units. Each worker played four iterations each of Spot the Difference and Symbol Matching. We randomized pair construction so that in 50% of cases, a worker played with someone from his own product unit, and 50% with someone from another product unit for each of the games.

Table 10 presents effects on the cooperative pairwise puzzles. If both members of a pair are from the same unit, they score 1.105 points (27%) lower if that unit had *Pay disparity* versus *Compressed* pay (Col. 1). The F-test p-value of 0.0536 in Column 1 suggests that *Pay disparity* workers actually perform better playing with a stranger (i.e. someone from another product unit) than playing with a unit co-worker. In addition, note that *Pay disparity* workers do not perform worse than *Compressed* workers in general—the point estimate for at least one *Pay disparity* worker in pair is actually positive (though insignificant). Rather, these workers only perform worse when they are paired with a unit co-worker.[34] Finally,

[32]Note that these endline games were only played in the final 8 rounds of the experiment (80 product units only).

[33]We thank Heather Schofield for providing us with the Symbol Matching game grids (see Schofield 2014.)

[34]Because the endline games were conducted on the last day of work—when all workers received their final pay for the contract job, attendance was high. In the tower game, units just played with whichever workers were present. In the cooperative puzzles, if a worker was absent, then the person who had been paired with

Columns 3-4 provide evidence that treatment effects are larger in the absence of perceived justifications, and perceived justifications mitigate the negative treatment effect. However, the mitigating effects of justifications are not statistically distinguishable from zero.

It is worth noting that the worker productivity rankings in the main experiment have predictive power in these endline games. On average, a pair with a Low or Medium rank worker scores 15.8% and 13.8% less, respectively, than a pair with a High rank worker (Col. 2). This suggests that the baseline rankings capture, in part, some stable differences in ability or effort across workers.

Overall, these findings indicate a decrease in *Pay disparity* workers' ability to cooperate in their own self-interest. This does not appear to result from general disgruntlement: low performance only arises when they must work with the other people in their own product units. While in our study, workers engage in individual production, the results suggest that the effects of pay inequality may be exacerbated in settings with team production.

5.6.2. *Network Formation.* Tension within *Pay disparity* groups might impede the formation of relationships outside of work, and could represent an additional manifestation of a lack of group cohesion. In the endline survey, we asked each employee whether he would engage in the following with each co-worker in his unit: seek or dispense advice; socialize; and borrow or lend. We code a worker as having a network link with a co-worker if he states that he and his co-worker would engage in any of these activities.

In Table 11, the outcome variable in Columns 1-2 measures the number of co-workers with whom the worker reports a link, with possible values of 0, 1, or 2. The outcome in Columns 3-4 indicates whether the worker has a link with both of his unit co-workers. Consistent with the results of the team cohesion games, workers in *Pay disparity* units appear substantially less likely to interact with one another outside of work. On average, *Pay disparity* workers report .23 fewer friends from the production unit (a 30% decrease, Col. 1) and are 12.7 percentage points less likely to be friends with both co-workers (a 48% decrease, Col. 3). These effects are larger and statistically significant in magnitude when no justification is present; as before, there are no detectable treatment effects when disparity is clearly justified Cols. 2 and 4).

5.6.3. *Fairness and Happiness.* In the endline survey, we asked workers to assess how fair their wages were in relation to those of their peers. Panel A of Table 12 presents the results. Overall, *Pay disparity* units are no more likely to believe that their wages were set unfairly in relation to their unit co-workers than the *Compressed* units. However, this pooled result

that worker for a pair-game sat out during that round and their score for that pair-game is coded as 0. Appendix Table 10 verifies that the endline game results are not driven by differential absence across units. First, as expected on the last day, treatment status did not affect worker attendance (Cols. 1-2). Cols. 3-4 replicate results for the tower game restricting the sample to units where all three workers were present. Cols. 5-6 replicate results for only those pair-games where both members of a pair were present. The results are similar to those in Tables 9 and 10.

hides substantial heterogeneity. Compared to their *Compressed* unit counterparts, *Pay disparity* Low rank workers are significantly more likely to state that their wages were set unfairly in relation to their co-workers (Col. 4). Moreover, *Pay disparity* High wage workers are significantly less likely to state that their wages were set unfairly (Col. 2). Taken seriously, the survey responses of the High rank workers suggest that inequity aversion does not play a very meaningful role in explaining the main attendance results, as no fairness violation appears to be triggered by the higher levels of pay.

In addition, we also asked respondents to report their happiness on a ten-step ladder (taken from the World Values Survey). Panel B of 12 presents the results on this proxy of subjective well-being. Overall, *Pay disparity* units report being less happy than their *Compressed* unit counterparts (Col. 1). Despite believing that their wages are fair, High rank workers on *Pay disparity* units report significant reductions in happiness (Col. 2). Note that we cannot reject that the happiness response is the same across ranks.

5.7. **Discussion.** We find that unjustified *Pay disparity* causes all workers in our setting to decrease attendance and forego earnings, regardless of whether they are paid more or less than their co-workers. Moreover, the results paint a consistent picture of a drop in social cohesion within the production unit. *Pay disparity* can undermine cooperation between workers on the same unit, causes a decrease in social ties outside of work, and leads to reports of more unhappiness at endline. These findings are consistent with the view in the social psychology and sociology literature that inequality in the workplace can lead to a break-down in social cohesion and an increase in social conflict.

This evidence also helps to shed light on why some *Pay disparity* workers decrease attendance even when they are paid more than their co-workers. Our endline results suggest that while on average, high rank workers consider their wages fair, the condition of *Pay disparity*, nonetheless, makes them less happy. This could reflect the fact that working in close proximity to aggrieved workers (i.e., those earning less than their unit-mates) could be unpleasant due to hostility, resentment, or social awkwardness. Our finding that co-workers in *Pay disparity* units are less likely to discuss their wages (Table 3) suggests that *Pay disparity* may also give rise to awkwardness that all workers in a unit might seek to avoid.

We provide one final piece of suggestive evidence in Appendix Table 11, where we consider differences in the effects of Pay Disparity when unit coworkers are from the same village. This is the third and last dimension of heterogeneity that we pre-registered. Columns (1) and (2) consider heterogeneous treatment effects on production and attendance, respectively, for production teams where all workers are from the same village. Columns (3) and (4) explore the treatment effects by whether a given worker has any co-villagers in the same production unit, while Columns (5) and (6) do so by the number of co-villagers in the production unit. All three specifications provide similar results. While there are no detectable differential treatment effects for Low or Medium rank workers, the results for the

High rank workers are quite striking. Consistent with there being social awkwardness from earning more than an aggrieved peer, we find that the High rank worker has a significantly more negative average response to *Pay disparity* when workers from his village are on his team. This could be because co-villagers are more likely to have pre-existing relationships or because co-villagers are more likely to run into one another in the future.

## 6. Threats to Validity

6.1. **Internal validity concerns.** We now consider whether an explanation other than relative pay concerns could produce our findings.

*Peer effects in attendance.* Recall that in the endline games, *Pay disparity* workers perform worse with their own co-workers than with someone from another unit, with whom they have not worked at all. This implies that the decreased performance of *Pay disparity* units in the endline games does not arise from the fact that they have had less time to work together (due to lower attendance) than the *Compressed* units. However, could the main attendance results still be partly driven by traditional peer effects in attendance, leading us to overstate the treatment effect of relative pay? We explore this in Appendix Table 12, taking advantage of the randomization of workers to units. Because some villages have much higher attendance rates than others, the villages of one's co-workers strongly predicts co-worker attendance (Col. 1). This is due to factors including distance to the workshop, social events, and local labor demand shocks. If there were a causal attendance peer effect, then the villages of one's co-workers should also predict one's *own* attendance. While the estimates are quite imprecise, we find no strong evidence that co-worker attendance—identified using the random composition of co-workers' villages—predicts one's own attendance (Cols. 2-3).

*Career concerns.* We stress to workers that this is a one-time contract job—a common form of employment in this setting. Despite this, when a worker in *Pay disparity* observes he is paid less than his co-workers, he may believe the firm is less likely to re-hire him and therefore may decrease effort. However, such beliefs should be more likely when it is clear that the worker is much less productive than his peers. In contrast, we find decreased performance when workers perceive that they are close in productivity to their higher-paid peers. In addition, a basic career concerns story would not predict that workers would give up earnings while they have the job, or that higher-paid workers would also reduce attendance. It also cannot explain the social cohesion results.

*Learning about one's outside option.* It is also unlikely that *Pay disparity* causes workers to infer that they could find higher wages at another firm. Even the lowest post-treatment wage, $w_L$, is above workers' outside options. For example, Appendix Figure 1 documents that only 1.7% of workers state the prevailing wage in their local labor market to be above our training wage of Rs. 250. In addition, given that employment rates are low in the lean season (Table 2), it would be surprising for workers to be absent to search for alternate work, rather than waiting for the current contract to end. Finally, this hypothesis cannot

explain why the higher-paid workers also attend less or why *Pay disparity* units do not cooperate well in the social cohesion games.

*Discouragement effects / self-signaling.* Our results are not consistent with discouragement effects from the *Pay disparity* wages revealing a worker's type (i.e., productivity). Similar arguments to those above make this explanation difficult to reconcile with our results. Namely, we find no effects of *Pay disparity* in the cases with perceived justifications, when it's easiest for a worker to learn about his low productivity. In addition, we disclose information about relative productivity ranks to workers on Day 10—before treatment. We find little evidence that this disclosure has a differential impact on those who learn they are relatively less productive than their coworkers (Appendix Table 13).

*Belief formation about employer.* Finally, it is unlikely that *Pay disparity* affected performance because workers found differential pay unusual. In our setting, all workers are aware of performance pay regimes such as piece rates for harvesting, and about half of workers have worked under piece rates themselves. Indeed, there are no differential effects of *Pay disparity* for workers who have worked on piece rates versus those who have not. In addition, among local employers that pay flat daily wages, we observe differential pay based on experience or worker skill-levels. Finally, such an explanation could not explain the impact of *Pay disparity* on workers' ability to cooperate in the endline games, which had nothing to do with the employer.

6.2. **External validity concerns.** *Pay structure.* Because output in our production tasks is in principle measurable, performance incentives like piece rates are potentially possible. However, the feasibility of this depends on the cost of monitoring. In the experiment, we bear considerable expenses to hire extra staff to measure each worker's output daily (e.g. count every single incense stick). Indeed, different local firms produce the same retail good under piece rates or under flat wages—with implications for the quality grade sold by the firm.[35]

*Dynamic incentives and pay policy.* One potential benefit to firms of differential pay is dynamic incentives: workers know that if they work hard now, it could lead to higher pay in the future. Our study design explicitly shuts down this channel because after the

[35]For example, the contractors with whom we worked sold the output produced as part of the experiment at a relatively high quality grade (and therefore price premium). As another example, some employers pay piece rates while others in the same village pay fixed daily wages to harvest a given crop—again, with implications for quality. There is a large literature on such multitasking problems.

training period, there is no possibility of a wage change.[36] The objective of our study is not to identify the optimal pay policy for firms, but rather, to isolate whether workers care about relative pay. Optimal pay policy would depend on weighing the potential costs of differential pay (e.g. morale reductions) against the potential benefits (e.g. selection or increased effort). Our findings indicate that relative pay concerns could affect this calculus.

*Magnitude of effects and context.* Of course, as with any empirical study, the effect sizes we document are specific to our setting. While we cannot predict the magnitude of effects in other populations, a growing body of work in economics lends credence to the view that concerns about relative pay matter in a range of field settings including the US and Europe (Card et al. 2012, Cohn et al. 2012, Rege and Solli 2013, Dube et al. 2015).

## 7. Conclusion

The pattern of our findings broadly supports the view of pay disparity articulated in the psychology, sociology, and organizational economics literatures. These literatures posit that the workplace is a social organism, where the relational aspects of pay cannot be divorced from its economic value. They predict that being paid less than one's peers is a disamenity; this accords with our finding that relatively lower paid workers substantially reduce output and attendance on average. These literatures also predict that discontent among some workers can break down social cohesion and cooperation, fostering social conflict and altering the dynamics of the group more broadly. This is consistent with our findings that *Pay disparity* workers are less able to cooperate at endline—doing worse when they work with co-workers from their own unit than with strangers. The fact that even relatively higher-paid workers decrease attendance—coupled with their happiness responses on the endline survey—also provides some suggestive evidence in support of this view. If lower-paid workers were discontent or resentful, then working and eating lunch alongside them may have been socially awkward or unpleasant for their relatively higher paid peers, dampening their desire to go to work. Such externalities are in accordance with Frank (1984)'s observation that "Status is, like Coase's social costs, a reciprocal phenomenon...[O]ne person's gain in status can occur only at the expense of a loss in status for others." This underscores the reason why our empirical results do not allow us to isolate workers' individual internal preferences for relative pay. The effects we document are a reduced form combination of internal preferences and group-level dynamics.

[36]Note that fixed wage setting based on prior (or expected) productivity is not uncommon in many settings. As discussed above, workers in our study are routinely employed in one-time seasonal contract jobs, where the pay is fixed for the duration of the contract. In other contexts, firms often set the pay of short-term consultants based on expected productivity. Even for salaried workers, pay is usually based on ex-ante expectations, with stickiness throughout a worker's tenure at the firm (e.g. Fehr et al. 2009)—this is not adjusted with new information on ex-post performance, but rather re-negotiated at infrequent intervals. More generally, explicit incentives like piece rates based on ex-post output are not very common in our setting (e.g. Dreze and Mukherjee 1989, Kaur 2015) or in the US (MacLeod and Parent 1999).

However, pay inequality need not have any adverse consequences. We find no negative morale effects when productivity differences are large and observable. In summary, workers appear to accept homogenous wages as fair even when there are productivity differences, but similarly have no issue with clearly justified heterogeneous wages. This indicates that workers have no problem with pay inequality per se. Rather, they may resent what they perceive as arbitrary behavior from an employer.

At the same time, our study also highlights why employers may find it difficult to avoid perceptions of "arbitrariness". Recall that in the experiment, managers told workers their productivity rank during the training period. That in itself was apparently not sufficient justification. In settings where there is imperfect trust in management, workers may be inclined to suspect favoritism unless they can observe justifications with their own eyes. In addition, Appendix Table 5 suggests that productivity differences have to far outpace the 4% earnings differences before workers accept pay inequality.[37] It is unclear whether this simply reflects observability or stems from self-serving bias, in which workers are unwilling to believe they are worse than their peers unless the evidence is hard to deny (Fang and Moscarini (2005)). In many workplaces where multi-tasking is common, co-worker productivity may be quite difficult to perfectly observe. Consequently, in practice, it may be difficult for employers to implement pay differences that correspond to workers' marginal products without risking some backlash.

The perceived justifications results help advance our understanding of when pay compression is more likely to arise in the labor market. Specifically, in settings where it is difficult to quantify individual productivity, it will be challenging to justify pay differences. This may help explain why pay compression appears especially common among flat (e.g. hourly or daily) wage workers, but not among performance pay workers. Performance pay, such as piece rates or commissions, occurs when important dimensions of output are precisely quantifiable. Performance pay structures embed a clear and justifiable mapping between output and pay; consequently, pay differences across workers are less likely to violate fairness norms. In contrast, flat wages tend to arise in those settings where important dimensions of output are hard to quantify. Thus, even though managers may have a sense of which workers are more productive than others on average, setting pay according to expected output can be difficult to justify from co-workers' perspective. This suggests pay compression may be optimal in flat wage occupations—such as tollbooth attendants, supermarket cashiers, or agricultural day laborers—even when signals of differential productivity are available to employers (Frank 1984, Dreze and Mukherjee 1989). Furthermore, to the extent that wage compression plays a role in generating wage rigidity (e.g., Akerlof and Yellen 1990), our findings may have some relevance for understanding why wages appear to be more rigid among flat hourly workers relative to salaried workers with bonuses (e.g., Kahn 1997).

[37]We caution that confidence intervals in Appendix Table 5 are too large to precisely pin down how big productivity differences must be in order for the 4% pay difference to be justified.

In addition, our findings suggest that firms may have several potential tools at their disposal to manage morale in the presence of pay dispersion. For example, increased transparency in output could more easily allow firms to motivate workers through disparate wages—generating aggregate output benefits not just through a reduction in moral hazard, but also through improved morale. Firms could also potentially alter the organizational structure of the workplace itself—through job titles, physical co-location of similar workers, or the construction of teams or "units" (as we did in the experiment)—to affect who a worker views as being in her reference group. Furthermore, social norms or stereotypes—for example, around caste, class, race, or gender—could also constitute a source of perceived justification. This could perpetuate pay inequality among subgroups even in settings where within-group wages are compressed.

Our findings indicate that relative pay concerns and their perceived justification could have potentially large implications for worker behavior. While speculative, the above discussion suggests that incorporating these concerns could enrich our understanding of the broader functioning of the labor market.

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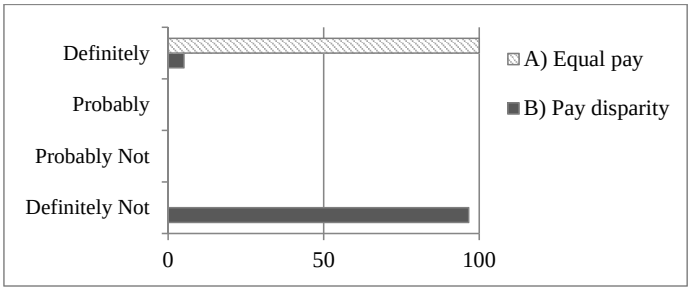
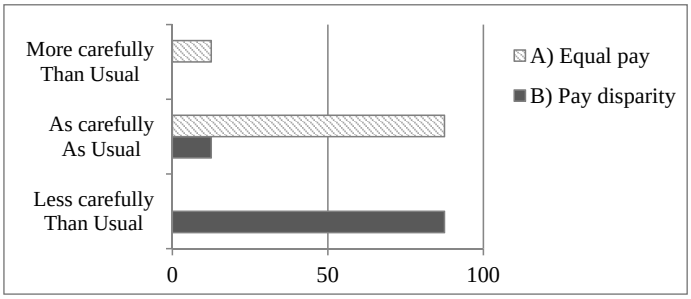
# Figures

## Figure 1. Perceived Impact of Pay Disparity on Labor Supply and Cohesion

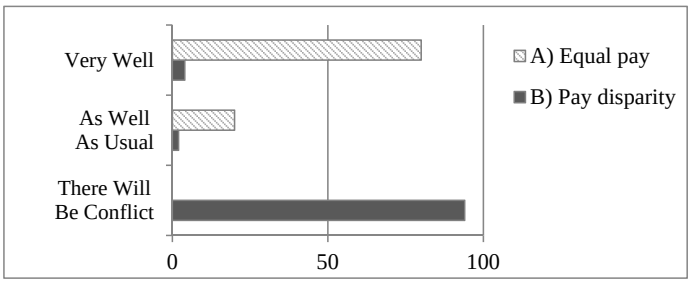
1) A laborer accepts a job from an external contractor at Rs. 240/day. The work begins the next day. At night, he learns that 2 other workers in the village have been offered the same job at:
   A) Rs. 240/day.
   B) Rs. 270/day.
   Do you think the worker would show up to work the next day?




2) There is a local soda factory that hires workers to load bottles onto trucks. Sunil accepts a contract to work at the factory for Rs. 240/day. The next morning, he arrives at work. He learns that other workers in his neighborhood have been hired for the same job at:
   A) Rs. 240/day.
   B) Rs. 260/day.
   How carefully will Sunil do the work relative to his usual level of effort?

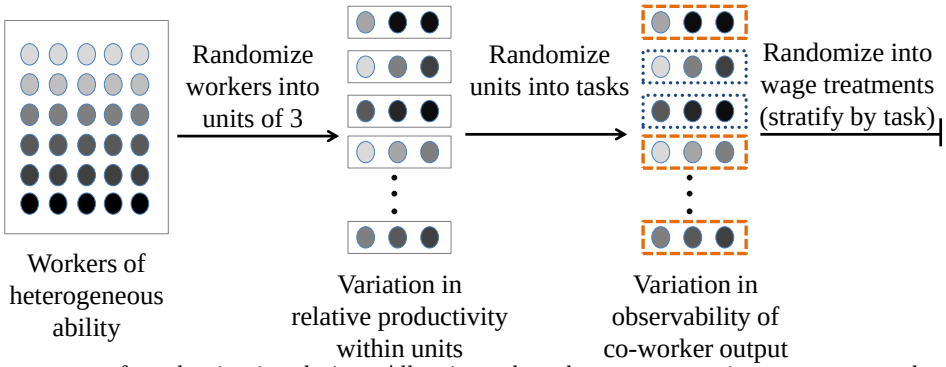



3) 3 people from a village get hired to work on a construction site together. The prevailing wage is Rs. 250. The contractor pays them:
   A) Rs. 250/day.
   B) Different wages based on their quality: Rs. 250/day, Rs. 270/day, and Rs. 290/day.
   How well will they work together?



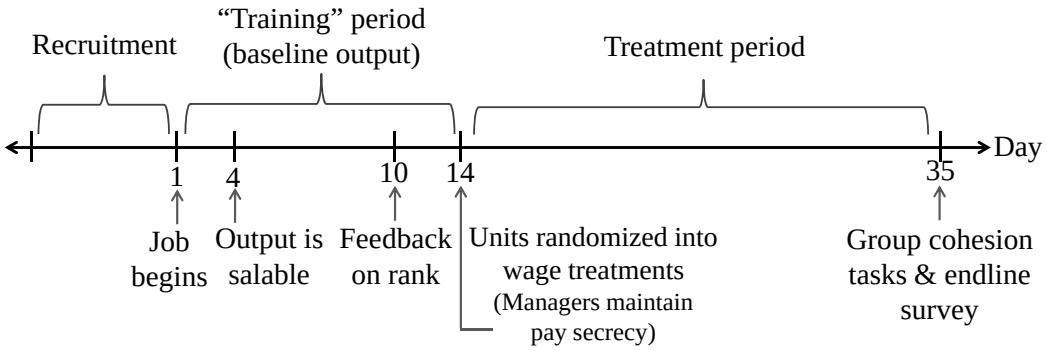

*Notes:* Surveys conducted with 200 laborers drawn from the same population as the experiment participants. Each respondent was asked 1 question out of each pair above. The x-axis in each chart shows the percentage of respondents choosing a given answer.

## Figure 2. Randomization Design



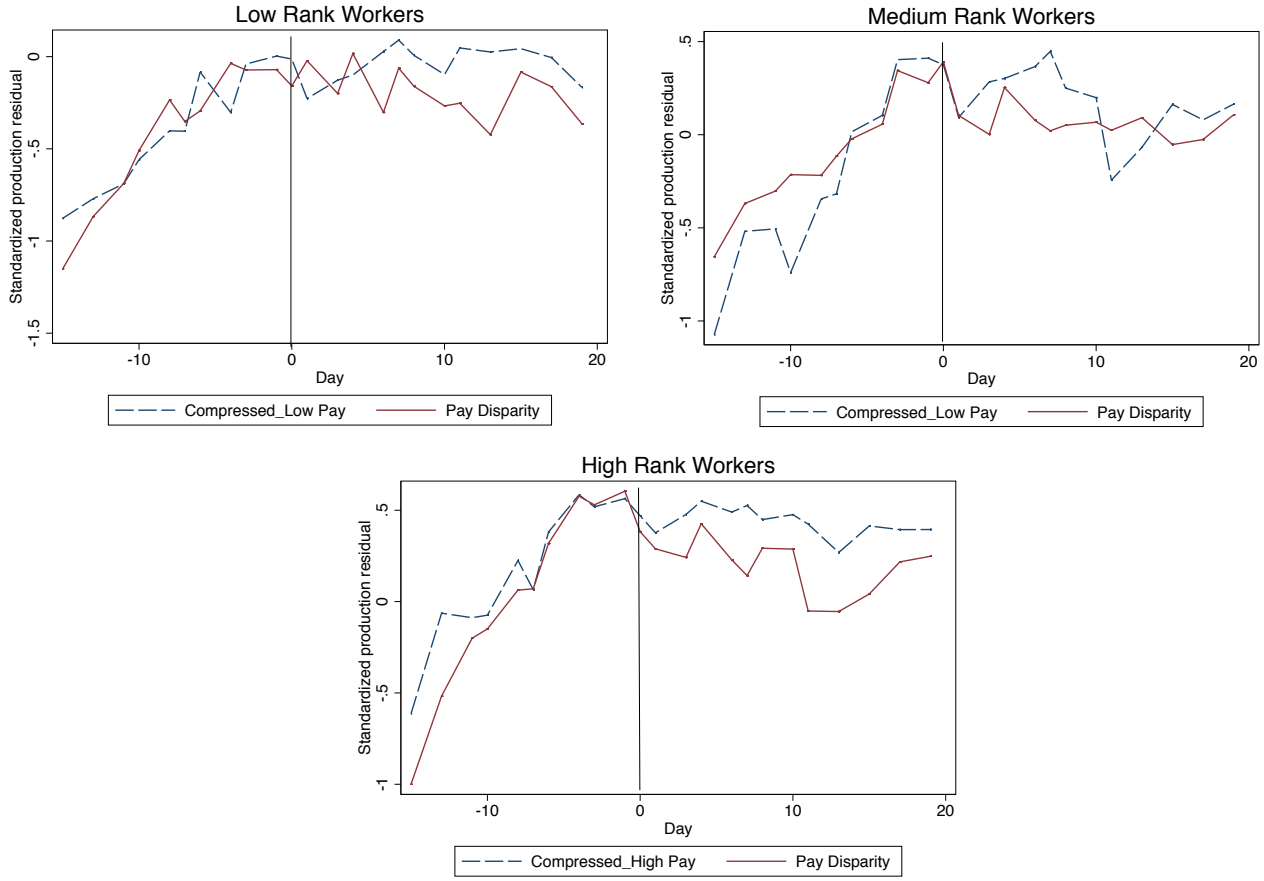

*Notes:* Three steps of randomization design. All unit, task and treatment assignments are made on day one of the experiment by the researchers. Factory management is not informed of treatment status until the day of the wage change.

FIGURE 3. Time Line



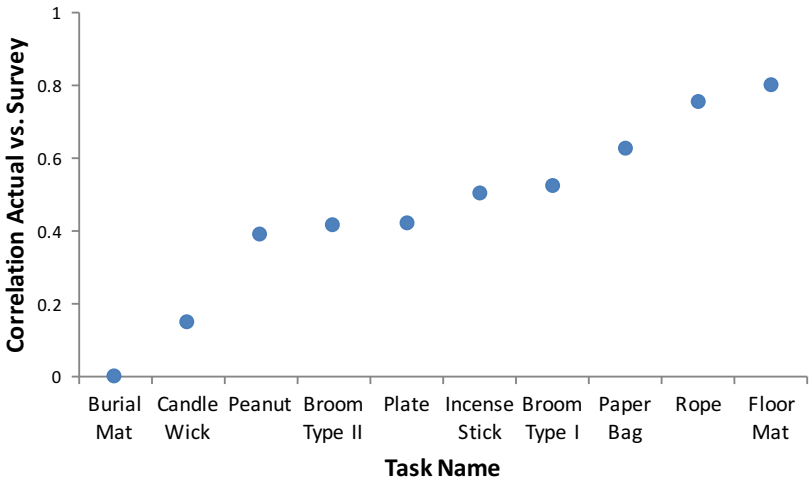

*Notes:* This figure shows the timeline of activities in each of the 14 production rounds.

FIGURE 4. Effects of Pay Disparity on Worker Output




*Notes:* Y-axis shows residuals of standardized output after removing individual fixed effects in the pre-period and dummies for festivals. Each plot compares workers of same rank who earn the same wage, but are in *Compressed* vs. *Pay disparity* units. Day=0 is when wage treatments took effect.

FIGURE 5. Task Observability: Actual vs. Survey Correlations




*Notes:* Figure plots the correlation between actual productivity rankings and perceived rankings by the workers (reported after 3 weeks of work). Note that this data comes from four pilot rounds, using a separate sample of workers, where all workers were paid the same wage and were not informed about their production rankings. In our analysis, we split the tasks at the median level of observability (0.5 correlation).

# Tables

## Table 1. Wage Treatments and Relevant Comparisons

| Worker Rank | *Pay Disparity* | *Compressed Low* | *Compressed Medium* | *Compressed High* |
|---|---|---|---|---|
| Low productivity | $w_{Low}$ | $w_{Low}$ | $w_{Medium}$ | $w_{High}$ |
| Medium productivity | $w_{Medium}$ | $w_{Low}$ | $w_{Medium}$ | $w_{High}$ |
| High productivity | $w_{High}$ | $w_{Low}$ | $w_{Medium}$ | $w_{High}$ |

*Notes:* Table presents randomization design. The key comparison groups in each row are highlighted. These six cells combined form the "relevant" group of workers.

## Table 2. Summary Statistics

| | Pay Disparity (1) | Compressed (2) | Compressed: Relevant (3) | P-value of Difference (1) vs. (2) (4) | P-value of Difference (1) vs. (3) (5) |
|---|---|---|---|---|---|
| Married | 0.800 | 0.738 | 0.724 | 0.341 | 0.305 |
| | (0.402) | (0.440) | (0.45) | | |
| Number of Children | 1.698 | 1.531 | 1.437 | 0.418 | 0.283 |
| | (1.502) | (1.490) | (1.55) | | |
| Owns land | 0.461 | 0.522 | 0.467 | 0.381 | 0.934 |
| | (0.501) | (0.500) | (0.5) | | |
| Sharecrops land | 0.490 | 0.565 | 0.522 | 0.270 | 0.687 |
| | (0.502) | (0.497) | (0.5) | | |
| Has missed at least one meal in last 30 days | 0.105 | 0.112 | 0.138 | 0.881 | 0.525 |
| | (0.309) | (0.315) | (0.35) | | |
| Number of days can't find work in last 30 days | 15.260 | 14.612 | 14.724 | 0.474 | 0.610 |
| | (6.972) | (6.838) | (6.34) | | |
| Number of days worked in past 10 days | 2.441 | 2.707 | 2.489 | 0.461 | 0.908 |
| | (2.585) | (2.736) | (2.67) | | |
| Number of days worked inside village in past 10 days | 1.843 | 1.938 | 1.761 | 0.757 | 0.816 |
| | (2.512) | (2.436) | (2.35) | | |
| Total wage earnings over past 10 days | 438.559 | 543.768 | 506.804 | 0.164 | 0.487 |
| | (587.394) | (708.571) | (745.86) | | |
| Typical wage in village during work period | 211.505 | 216.464 | 218.791 | 0.383 | 0.220 |
| | (35.138) | (33.190) | (30.87) | | |
| Has experience working with piece rates | 0.473 | 0.413 | 0.429 | 0.391 | 0.580 |
| | (0.502) | (0.493) | (0.5) | | |
| Baseline production (full training period) | -0.199 | -0.218 | -0.253 | 0.870 | 0.637 |
| | (0.635) | (0.688) | (0.66) | | |
| Baseline attendance (full training period) | 0.960 | 0.950 | 0.948 | 0.319 | 0.304 |
| | (0.074) | (0.082) | (0.08) | | |
| F-test of Joint Significance: | | | | 0.519 | 0.894 |

*Notes:* Responses taken from baseline surveys conducted on first day of work. Means and standard deviations are shown in Cols (1), (2) and (3) for Pay Disparity, Compressed and Compressed: Relevant workers, respectively. Compressed: Relevant workers are the Low rank workers in *Compressed Low* units, Medium rank workers in *Compressed Medium* units and High rank workers in *Compressed High* units. Col (4) displays the p-values of the comparisons of means across Pay Disparity and Compressed production units obtained from a simple univariate regression, where standard errors are clustered by production unit. Col (5) displays the p-values of the comparisons of means across Pay Disparity and Compressed: Relevant production units. In the last row, we report the p-values from F-tests of joint significance. We regress an indicator for Pay Disparity on each of the controls and report the p-values from the F test of joint significance. N=378.

TABLE 3. Knowledge of Co-worker Wages

| | Compressed (1) | Pay Disparity (2) | P-value of Difference (3) |
|---|---|---|---|
| *Panel A — Own Unit* | | | |
| Indicator for knows correct wage of both co-workers | 0.909 | 0.742 | 0.003*** |
| | (0.288) | (0.440) | |
| Indicator for knows correct wage of at least one co-worker | 0.958 | 0.871 | 0.031** |
| | (0.200) | (0.337) | |
| Indicator for has an opinion of wage of both co-workers | 0.921 | 0.871 | 0.203 |
| | (0.271) | (0.337) | |
| Indicator for has an opinion of wages of at least one co-worker | 0.966 | 0.923 | 0.206 |
| | (0.181) | (0.265) | |
| *Panel B — Other Units* | | | |
| Indicator for knows correct wage of all unit members | 0.086 | 0.019 | 0.000*** |
| | (0.280) | (0.138) | |
| Indicator for has an opinion of wages of all unit members | 0.117 | 0.143 | 0.237 |
| | (0.321) | (0.351) | |
| Indicator for has an opinion of wages of some unit members | 0.134 | 0.174 | 0.070* |
| | (0.341) | (0.380) | |

*Notes:* Responses taken from endline surveys in which workers were asked to list the wages of the members of their own production units and also the members of a set of four fixed product units. Means and standard deviations are shown in Cols (1) and (2) for Compressed and Pay Disparity units, respectively. Col (3) displays the p-values of the comparisons of means across Compressed and Pay Disparity units obtained from a simple univariate regression, where standard errors are clustered by production unit. Panel A describes beliefs about a worker's own production unit. N=358. Panel B summarizes beliefs about the wages of individuals in four other units: incense stick, candle wick, floor mat, and paper bag. Responses by individuals on those units are omitted from this analysis. The dataset is a panel of worker x unit observations. Knowledge about the wages of Compressed units is reported in Col (1), and knowledge about the wages of Pay Disparity units is reported in Col (2). Observations in Panel B only include instances of individuals evaluating the wages of units other than their own. N=1252.

TABLE 4. Effects of Pay Disparity

| | Output (std dev.) (1) | Output (std dev.) (2) | Attendance (3) | Attendance (4) | Output \| Attendance (5) |
|---|---|---|---|---|---|
| *Panel A — Pooled Treatment Effects* | | | | | |
| Post x Pay disparity | -0.311*** | -0.242** | -0.115*** | -0.117*** | -0.0893 |
| | (0.110) | (0.097) | (0.026) | (0.025) | (0.094) |
| *Panel B — Treatment Effects Separately by Rank* | | | | | |
| Post x Pay disparity x Low wage | -0.385*** | -0.332** | -0.113** | -0.120** | -0.204* |
| | (0.134) | (0.128) | (0.055) | (0.053) | (0.114) |
| Post x Pay disparity x Med wage | -0.262 | -0.226 | -0.126** | -0.129** | -0.0608 |
| | (0.201) | (0.187) | (0.056) | (0.060) | (0.138) |
| Post x Pay disparity x High wage | -0.288 | -0.172 | -0.106** | -0.104** | -0.00901 |
| | (0.199) | (0.181) | (0.053) | (0.052) | (0.152) |
| Individual fixed effects? | No | Yes | No | Yes | No |
| Post-treatment Compressed mean | -0.099 | -0.099 | 0.939 | 0.939 | 0.015 |
| N | 8375 | 8375 | 8375 | 8375 | 7678 |

*Notes* : Difference in differences regressions. Panel A pools the treatment effects across the low, medium, and high rank workers, while Panel B shows the treatment effects separately by rank. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. Col (5) limits to observations where the worker was present. Regressions include day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. All coefficients are identified off comparisons of workers who earn the same absolute wage and have the same productivity rank within their production unit (see regression specification in text). Standard errors clustered by production unit.

TABLE 5. Effects Over Time

| | Output (std dev.) | | | Attendance | | |
|---|---|---|---|---|---|---|
| | Before first payday in post period (1) | Between first and second paydays in post period (2) | After second payday in post period (3) | Before first payday in post period (4) | Between first and second paydays in post period (5) | After second payday in post period (6) |
| *Panel A — Pooled Treatment Effects* | | | | | | |
| Post x Pay disparity | -0.0530 | -0.309*** | -0.294** | -0.0260 | -0.133*** | -0.149*** |
| | (0.123) | (0.114) | (0.133) | (0.037) | (0.029) | (0.042) |
| *Panel B — Treatment Effects Separately by Rank* | | | | | | |
| Post x Pay disparity x Low wage | -0.0607 | -0.325** | -0.461*** | 0.0493 | -0.103* | -0.224*** |
| | (0.187) | (0.151) | (0.174) | (0.087) | (0.062) | (0.079) |
| Post x Pay disparity x Med wage | 0.0541 | -0.343* | -0.231 | -0.0400 | -0.153** | -0.126 |
| | (0.169) | (0.195) | (0.283) | (0.049) | (0.063) | (0.089) |
| Post x Pay disparity x High wage | -0.144 | -0.259 | -0.199 | -0.0753 | -0.144** | -0.101 |
| | (0.235) | (0.248) | (0.203) | (0.069) | (0.066) | (0.069) |
| Individual fixed effects? | Yes | Yes | Yes | Yes | Yes | Yes |
| Post-treatment Compressed Mean | 0.081 | -0.028 | -0.264 | 0.973 | 0.945 | 0.916 |
| N | 4565 | 4670 | 5654 | 4565 | 4670 | 5654 |

*Notes*: Difference in differences regressions. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. All specifications include all of the pre-wage change observations. Col (1) and (4) only include post observations before the first payday. Col (2) and (5) only include post observations after the first but before the second payday. Col (3) and (6) only include post observations after the second payday. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. All coefficients are identified off comparisons of workers who earn the same absolute wage and have the same productivity rank within their production unit (see regression specification in text). Standard errors clustered by production unit.

TABLE 6. Mediating Effects of Perceived Justifications

| | Definition of Perceived Justification Indicator | | | | | |
|---|---|---|---|---|---|---|
| | Baseline output difference between a worker and his higher ranked peer is large | | Co-worker output is highly observable | | Baseline differences are large or co-worker output is highly observable | |
| Dependent variable | Output | Attendance | Output | Attendance | Output | Attendance |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A — Pooled Treatment Effects* | | | | | | |
| Post x Pay disparity | -0.358*** | -0.167*** | -0.384*** | -0.153*** | -0.445*** | -0.181*** |
| | (0.133) | (0.039) | (0.131) | (0.031) | (0.147) | (0.037) |
| Post x Pay disparity x Perceived justification | 0.292* | 0.159** | 0.395** | 0.0996** | 0.429*** | 0.134*** |
| | (0.173) | (0.061) | (0.161) | (0.046) | (0.154) | (0.045) |
| Post x Perceived justification | 0.0483 | -0.0500 | -0.0518 | 0.0332 | -0.0342 | 0.0160 |
| | (0.107) | (0.032) | (0.103) | (0.028) | (0.094) | (0.025) |
| *Panel B — Treatment Effects Separately by Rank* | | | | | | |
| Post x Pay disparity x Low wage | -0.448*** | -0.168*** | -0.513*** | -0.158** | -0.718*** | -0.239*** |
| | (0.147) | (0.061) | (0.160) | (0.071) | (0.191) | (0.091) |
| Post x Pay disparity x Low wage x Perceived justification | 0.467** | 0.181** | 0.512** | 0.121 | 0.712*** | 0.225** |
| | (0.231) | (0.087) | (0.220) | (0.077) | (0.230) | (0.096) |
| Post x Pay disparity x Med wage | -0.270 | -0.170** | -0.248 | -0.157** | -0.283 | -0.187** |
| | (0.224) | (0.075) | (0.227) | (0.068) | (0.280) | (0.088) |
| Post x Pay disparity x Med wage x Perceived justification | 0.127 | 0.150 | 0.0890 | 0.0876 | 0.0829 | 0.104 |
| | (0.267) | (0.094) | (0.293) | (0.118) | (0.311) | (0.115) |
| Post x Pay disparity x High wage | | | -0.386 | -0.139* | -0.372 | -0.137* |
| | | | (0.242) | (0.071) | (0.242) | (0.072) |
| Post x Pay disparity x High wage x Perceived justification | | | 0.582** | 0.0884 | 0.564** | 0.0845 |
| | | | (0.269) | (0.078) | (0.269) | (0.082) |
| R-squared | 0.436 | 0.171 | 0.436 | 0.172 | 0.437 | 0.173 |
| Number of observations (worker-days) | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 |

*Notes:* Panels A and B show comparisons of each worker in Pay disparity production units with the relevant worker (who has the same rank and absolute earnings level) in the Compressed production units. In Cols. (1)-(2), the Perceived justification indicator equals 1 if the baseline productivity difference between a worker and his higher-ranked co-worker (for low and medium rank workers) is above the Compressed group mean. In Cols. (3)-(4), this indicator equals 1 if the observability correlation for the worker's production task (computed using a separate baseline sample) is above the mean. In Cols. (5)-(6), this indicator equals 1 if the baseline productivity difference between a worker and his higher-ranked co-worker (for low and medium rank workers) is above the Compressed group mean or if the observability correlation for the worker's production task is above the mean. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 7. Effects of Pay Disparity: Unit Level Variation

| | | | *Definition of Perceived Justification Indicator* | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | Co-worker output is highly observable | | Large baseline output difference between co-workers | | Co-worker output is highly observable or baseline differences are large | |
| Dependent variable | Output | Attendance | Output | Attendance | Output | Attendance | Output | Attendance |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A — Comparison with all Compressed Units* | | | | | | | | |
| Post x Pay disparity | -0.134 | -0.0735*** | -0.273** | -0.116*** | -0.125 | -0.0797*** | -0.273** | -0.111*** |
| | (0.092) | (0.026) | (0.122) | (0.032) | (0.120) | (0.027) | (0.125) | (0.033) |
| Post x Pay disparity x Perceived justification | | | 0.383** | 0.119** | 0.241 | 0.0961* | 0.379** | 0.111** |
| | | | (0.159) | (0.047) | (0.177) | (0.051) | (0.160) | (0.047) |
| Post x Perceived justification | | | -0.0375 | 0.0140 | 0.0345 | 0.0413 | -0.0241 | 0.0289 |
| | | | (0.100) | (0.028) | (0.100) | (0.030) | (0.085) | (0.024) |
| *Panel B — Comparison with Compressed Low Units Only* | | | | | | | | |
| Post x Pay disparity | -0.148 | -0.0597* | -0.317** | -0.0845** | -0.136 | -0.0605 | -0.306** | -0.0800* |
| | (0.108) | (0.035) | (0.139) | (0.042) | (0.122) | (0.039) | (0.144) | (0.045) |
| Post x Pay disparity x Perceived justification | | | 0.465*** | 0.0892* | 0.127 | 0.106 | 0.432** | 0.0848 |
| | | | (0.163) | (0.051) | (0.219) | (0.070) | (0.169) | (0.053) |
| Post x Perceived justification | | | -0.122 | 0.0465 | 0.147 | 0.0371 | -0.0784 | 0.0577 |
| | | | (0.125) | (0.035) | (0.157) | (0.052) | (0.124) | (0.035) |
| R-squared | 0.432 | 0.165 | 0.433 | 0.167 | 0.432 | 0.166 | 0.433 | 0.167 |
| Number of observations (worker-days) | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 |

*Notes:* Differences in differences regressions examining unit-level performance across wage treatments. Specifications are similar to the pooled individual regressions, but use variation from all workers, not only the "relevant" ones. Panel A examines the average performance of all workers on Pay disparity units relative to all workers in Compressed units. In Panel B, Pay disparity units are compared to the Compressed Low units only; these regressions include a dummy for being on a Compressed Medium or Compressed High unit, and an interaction of the dummy with the Perceived justification indicator (so the omitted category is Compressed Low). In Cols (3)-(8), the Pereceived justification indicator takes the same value for all workers in a unit. In Cols (2)-(3), the Perceived justification indicator equals 1 if the worker was assigned to a production task where co-worker output is highly observable. In Cols (5)-(6), this indicator equals 1 if the baseline productivity difference between each worker and his higher-ranked co-worker (for *both* the low and medium rank workers) is above the Compressed group mean. In Cols (7)-(8), this indicator equals 1 if either the observability or large baseline productivity difference indicator equals 1. All regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 8. Effects of Higher Absolute Pay among Compressed Units

| | Output (std dev.) | | | Attendance | | |
|---|---|---|---|---|---|---|
| | First two days post wage change | First week post wage change | Full sample | First two days post wage change | First week post wage change | Full sample |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post x Compressed Medium | 0.00218 | 0.0875 | 0.0184 | 0.0411 | 0.0570* | 0.0225 |
| | (0.144) | (0.129) | (0.138) | (0.044) | (0.033) | (0.039) |
| Post x Compressed High | -0.00447 | 0.0274 | -0.00973 | 0.0691* | 0.0768** | 0.0321 |
| | (0.109) | (0.099) | (0.099) | (0.037) | (0.031) | (0.034) |
| Post-treatment Compressed Low Mean | -0.212 | -0.237 | -0.292 | 0.891 | 0.894 | 0.899 |
| N | 2807 | 3620 | 6107 | 2807 | 3620 | 6107 |

*Notes*: Difference in differences regressions restricting the sample to only Compressed teams. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. Regressions use unit-level variation, so all Compressed wage unit workers are included. Omitted category is Compressed low wage units. All specifications include the full training period. Cols (1) and (4) include only the first two post wage change days. Cols (2) and (5) include only the first work week post wage change. Cols (3) and (6) include the full sample. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 9. Effects on Group Cohesion: Endline Games 1

*Dependent variable: Tower height*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Pay disparity | -9.376*** | -18.89** | -20.49*** | -25.72*** |
| | (3.487) | (8.068) | (5.532) | (6.923) |
| Pay disparity x Observable task | | 17.81* | | |
| | | (9.472) | | |
| Pay disparity x Large productivity difference | | | 17.11** | |
| | | | (6.815) | |
| Pay disparity x Perceived justification (aggregate) | | | | 19.66*** |
| | | | | (7.445) |
| Dependent variable mean | 53.97 | 53.97 | 53.97 | 53.97 |
| R-squared | 0.291 | 0.397 | 0.397 | 0.410 |

*Notes:* Observable task equals 1 if the observability correlation for the unit's production task (computed using a separate baseline sample) is above the mean. Large productivity difference equals 1 if the difference between a worker and his higher paid peer is above average (>0.375 standard deviations) for at least one member of the unit. Perceived justification (aggregate) is a boolean that equals 1 if either of the perceived justification measure dummies -- Observable task or Large productivity difference -- equals 1. All regressions include round fixed effects. N=80 production units. Games were only run in later rounds of the experiment. Robust standard errors.

TABLE 10. Effects on Group Cohesion: Endline Games 2

*Dependent variable: Number correct*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Both workers from same unit x Pay disparity | -1.105** | -1.082** | -1.480** | -1.467** |
| | (0.498) | (0.499) | (0.617) | (0.616) |
| Both workers from same unit | 0.350 | 0.498 | 0.502* | 0.502 |
| | (0.295) | (0.300) | (0.300) | (0.319) |
| At least one worker on Pay disparity unit | 0.234 | 0.222 | 0.203 | 0.185 |
| | (0.413) | (0.406) | (0.504) | (0.504) |
| Both workers from same unit x Pay disparity x Perceived justification | | | 0.598 | 0.613 |
| | | | (0.558) | (0.561) |
| At least one worker on Pay disparity unit x Perceived justification | | | 0.0305 | 0.0221 |
| | | | (0.489) | (0.484) |
| At least one low rank worker in pair | | -0.645** | -0.659** | |
| | | (0.261) | (0.260) | |
| At least one medium rank worker in pair | | -0.564* | -0.587** | |
| | | (0.286) | (0.286) | |
| F-test p-value: Sum of first two coefficients | 0.0536 | 0.135 | 0.0593 | 0.0639 |
| Fixed effects for all rank combinations? | No | No | No | Yes |
| Dependent variable mean | 4.085 | 4.085 | 4.085 | 4.085 |
| R-squared | 0.123 | 0.129 | 0.130 | 0.132 |

*Notes:* Dependent variable is the number of correct matches made by the pair. Perceived justification equals 1 if a worker on a pay disparity unit was assigned to an observable production task, or was substantially less productive than his higher paid peer, and equals 0 otherwise. This variable always equals 0 for workers in Compressed units. Regressions include fixed effects for the order in which a game was played during the day and the game station (location in worksite), as well as education controls (a dummy for whether both workers in the pair completed primary school, and a dummy for whether both can write a sentence in the local language). N=1904 pair-game observations. Games were only run in later rounds of the experiment. Standard errors are clustered by production unit.

TABLE 11. Social Cohesion Outside Work: Network Formation

| Dependent variable | Number of within-unit network links | Number of within-unit network links | Indicator for network link with both co-workers in unit | Indicator for network link with both co-workers in unit |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Pay disparity | -0.236* | -0.349** | -0.127* | -0.173** |
| | (0.124) | (0.169) | (0.070) | (0.085) |
| Pay disparity x Perceived justification | | 0.208 | | 0.0845 |
| | | (0.245) | | (0.137) |
| Compressed mean | 0.779 | 0.779 | 0.265 | 0.265 |

*Notes:* Network links are measured from endline surveys. We define a link to take the value 1 if the worker reports that he would go to the co-worker or the co-worker would come to him for: advice, borrowing money, or visiting one another's houses. In Cols (2) and (4), the perceived justification indicator equals 1 if the observability correlation for the worker's production task is above the mean or if any worker in the unit has a large productivity difference with his higher-ranked co-worker. Regressions include an indicator for having unit co-workers who are relatives. Standard errors clustered by production unit. N=358.

Table 12. Endline Survey: Fairness and Happiness

| Sample restriction | All workers (1) | High rank only (2) | Medium rank only (3) | Low rank only (4) |
|---|---|---|---|---|
| *Panel A — Believes wage set fairly relative to co-workers* | | | | |
| Pay disparity | -0.0143 | 0.363*** | -0.163 | -0.587*** |
| | (0.091) | (0.120) | (0.170) | (0.186) |
| Pay disparity x Perceived justification | 0.0154 | -0.108 | 0.111 | 0.470* |
| | (0.118) | (0.190) | (0.228) | (0.252) |
| R-squared | 0.0840 | 0.244 | 0.203 | 0.239 |
| *Panel B — Above-median happiness (World Values Survey)* | | | | |
| Pay disparity | -0.292*** | -0.448*** | -0.241 | -0.146 |
| | (0.111) | (0.163) | (0.172) | (0.168) |
| Pay disparity x Perceived justification | 0.298** | 0.349 | 0.307 | 0.241 |
| | (0.133) | (0.225) | (0.220) | (0.205) |
| R-squared | 0.190 | 0.315 | 0.292 | 0.278 |
| N | 358 | 121 | 119 | 118 |

*Notes*: OLS regressions using endline survey responses. Panel A dependent variable is whether the worker viewed his wages as “fair” or “very fair” (relative to a 5 point scale). Panel B dependent variable is an indicator for above-median happiness (World Values Survey). Perceived justification indicator equals 1 if the observability correlation for the worker's production task is above the mean or if the baseline productivity difference between a worker and his higher-ranked co-worker (for low and medium rank workers) is above the Compressed group mean. All specifications include task and round fixed effects. Col (1) includes all observations. Cols (2) - (4) restrict the samples to the high, medium, and low rank workers. Standard errors clustered by production unit in Col (1). Robust standard errors are used in the single-rank specifications.

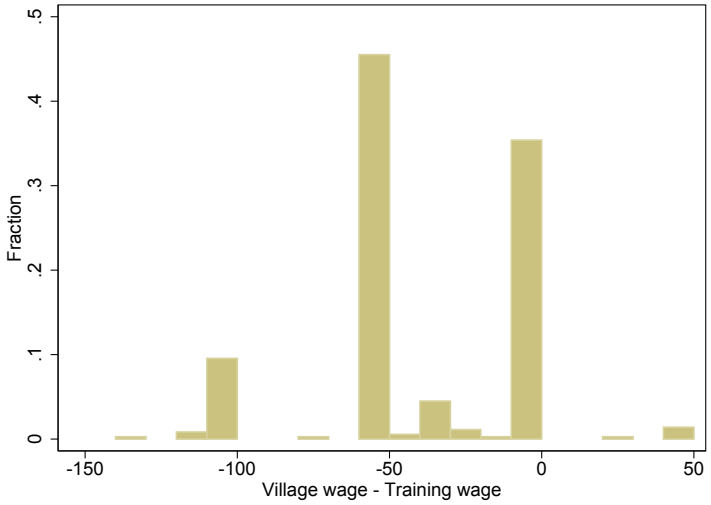
# Online Appendix

## The Morale Effects of Pay Inequality

Emily Breza, Supreet Kaur, and Yogita Shamdasani

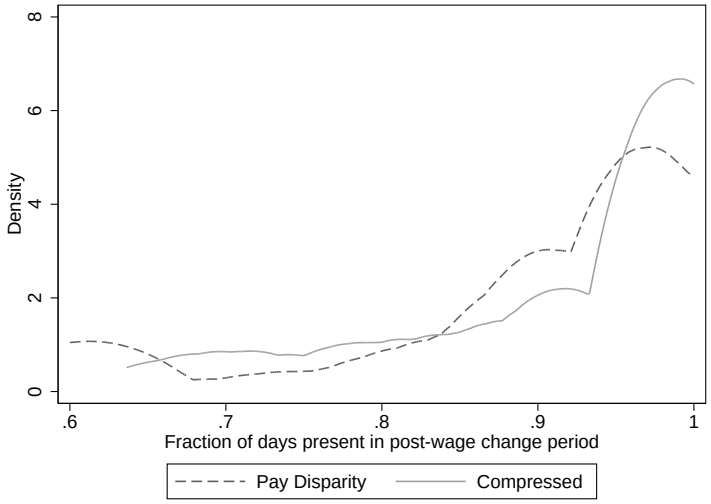
## Appendix A. Supplemental Figures and Tables

Figure 1. Distribution of Village Prevailing Wages: Endline Survey Responses



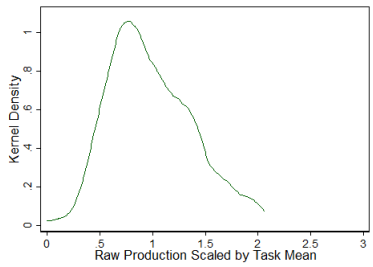

*Notes:* Histogram plots distribution of the prevailing wage, as reported by workers.. Responses from endline survey responses of 356 individuals from 39 villages. Note only 1.7% of workers indicate a prevailing wage above Rs. 250, which is the training wage in all rounds. The modal village wage is always $\leq Rs.250$.

Figure 2. Total Attendance on Pay Disparity and Compressed Units

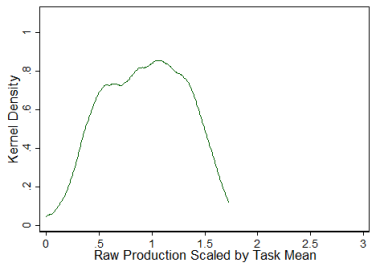



*Notes:* Kernel density plots of worker attendance rates during the post-wage change period among "relevant" workers, separately for Pay disparity and Compressed production units. Attendance rate is measured as the fraction of days worker was present in the post-wage change period.

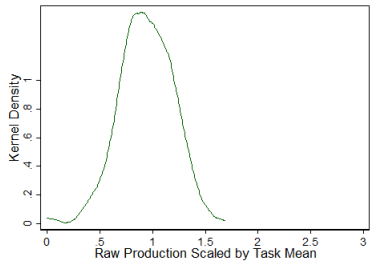
FIGURE 3. Density of Raw Production by Task, Scaled by Mean



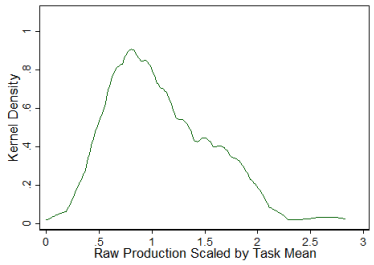

(A) Correlation = 0



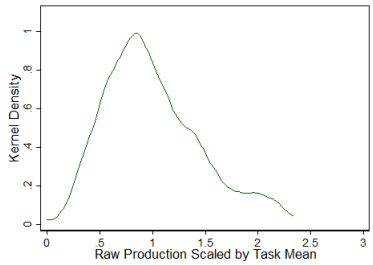

(B) Correlation = 0.15



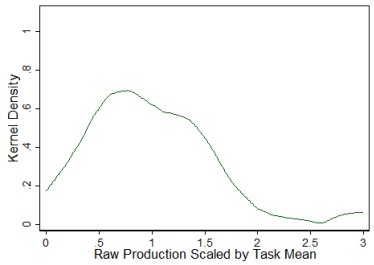

(C) Correlation = 0.39



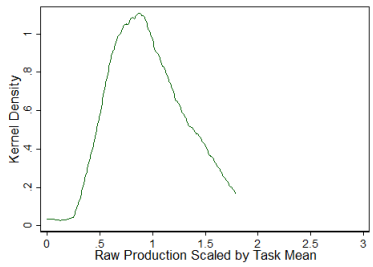

(D) Correlation = 0.41



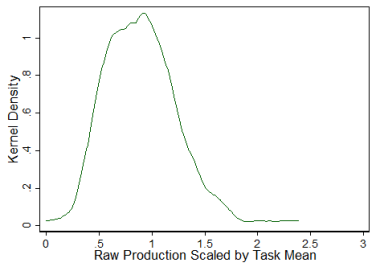

(E) Correlation = 0.42



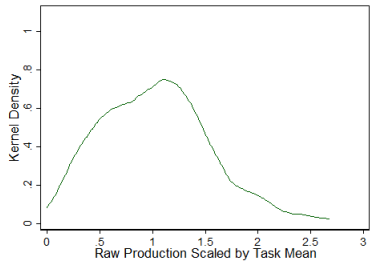

(F) Correlation = 0.50



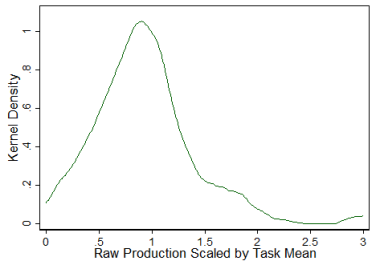

(G) Correlation = 0.52




(H) Correlation = 0.62




(I) Correlation = 0.75



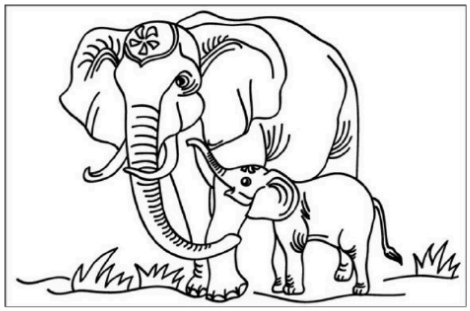

(J) Correlation = 0.88

*Notes:* Density plots of raw production, scaled by mean. Subfigures ordered by task observability.

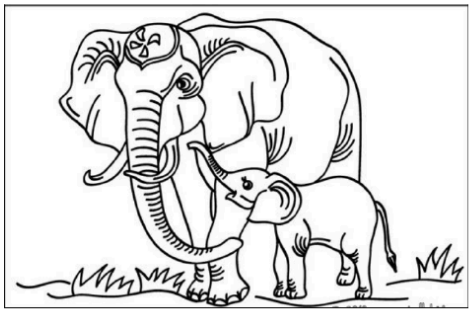
FIGURE 4. Cooperative Puzzle Games - Examples

Panel A: Spot the Difference - Example

Sheet 1, Player 1

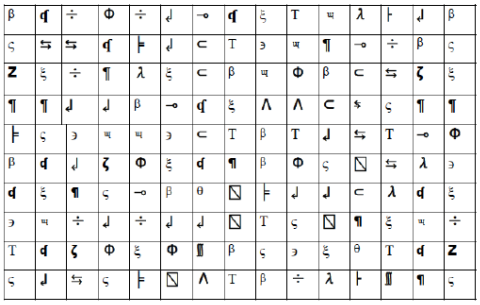


Sheet 2, Player 2

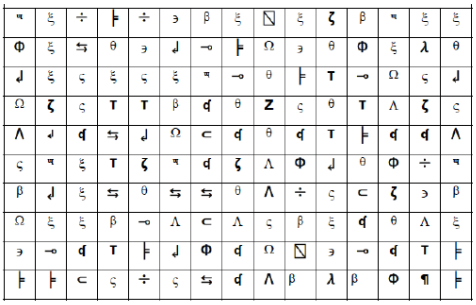


Panel B: Symbol Matching - Example

Sheet 1, Player 1

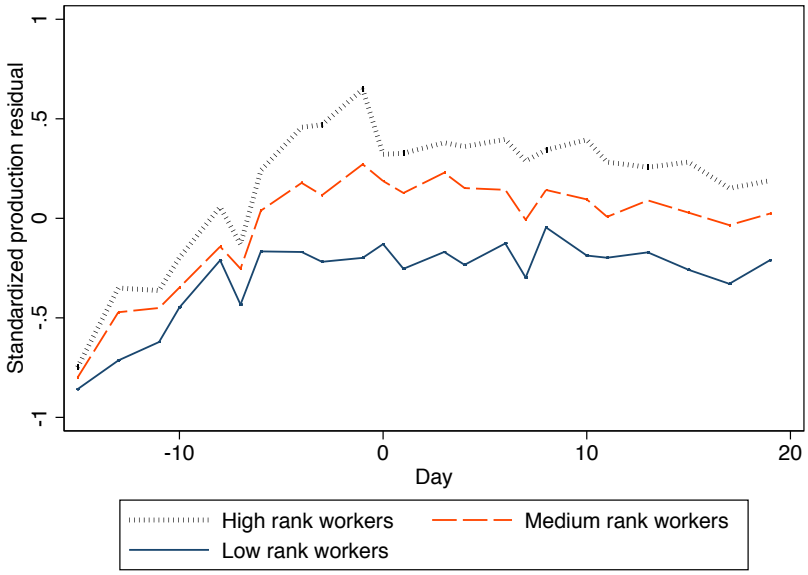


Sheet 2, Player 2



*Notes:* Examples of the cooperative pair games. Each worker in a pair would receive one of the sheets. Workers had to compare their respective sheets, and circle items that were different (Spot the Difference) or matched (Symbol Matching) on both their sheets.

FIGURE 5. Stability of Relative Productivity Ranks Across Time




*Notes:* The y-axis shows the residual of standardized output after removing individual fixed effects in the pre-period (before day 0) and dummies for festival days. The figures plot, for each day of the experiment, the average of the residuals for each group of workers restricting to members of the Compressed production units. Day=0 is the day wage treatments took effect (i.e. when workers were told their post-training wage).

TABLE 1. Summary Statistics by Rank

| | Low Rank | | | Medium Rank | | | High Rank | | |
|---|---|---|---|---|---|---|---|---|---|
| | Pay Disparity | Compressed: Relevant | P-value of Difference | Pay Disparity | Compressed: Relevant | P-value of Difference | Pay Disparity | Compressed: Relevant | P-value of Difference |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Married | 0.781 | 0.667 | 0.322 | 0.871 | 0.821 | 0.607 | 0.750 | 0.821 | 0.608 |
| | (0.42) | (0.48) | | (0.34) | (0.39) | | (0.44) | (0.39) | |
| Number of Children | 1.656 | 1.433 | 0.561 | 1.750 | 1.571 | 0.652 | 1.688 | 1.571 | 0.366 |
| | (1.47) | (1.52) | | (1.37) | (1.64) | | (1.69) | (1.64) | |
| Owns land | 0.529 | 0.500 | 0.815 | 0.500 | 0.533 | 0.794 | 0.353 | 0.533 | 0.911 |
| | (0.51) | (0.51) | | (0.51) | (0.51) | | (0.49) | (0.51) | |
| Sharecrops land | 0.529 | 0.594 | 0.605 | 0.441 | 0.500 | 0.644 | 0.500 | 0.500 | 0.794 |
| | (0.51) | (0.5) | | (0.5) | (0.51) | | (0.51) | (0.51) | |
| Has missed at least one meal in last 30 days | 0.065 | 0.100 | 0.622 | 0.125 | 0.250 | 0.227 | 0.125 | 0.250 | 0.466 |
| | (0.25) | (0.31) | | (0.34) | (0.44) | | (0.34) | (0.44) | |
| Number of days can't find work in last 30 days | 14.656 | 13.333 | 0.473 | 16.406 | 15.464 | 0.567 | 14.719 | 15.464 | 0.656 |
| | (7.41) | (7.03) | | (7.01) | (5.67) | | (6.55) | (5.67) | |
| Number of days worked in past 10 days | 2.353 | 1.969 | 0.518 | 2.618 | 2.933 | 0.665 | 2.353 | 2.933 | 0.708 |
| | (2.63) | (2.16) | | (2.61) | (3.13) | | (2.59) | (3.13) | |
| Number of days worked inside village in past 10 days | 1.706 | 1.438 | 0.629 | 2.059 | 1.933 | 0.843 | 1.765 | 1.933 | 0.795 |
| | (2.6) | (1.85) | | (2.52) | (2.52) | | (2.47) | (2.52) | |
| Total wage earnings over past 10 days | 368.971 | 361.094 | 0.951 | 528.765 | 665.133 | 0.528 | 417.941 | 665.133 | 0.578 |
| | (545.15) | (487.69) | | (602.48) | (1033.89) | | (618.08) | (1033.89) | |
| Typical wage in village during work period | 212.000 | 221.875 | 0.265 | 216.250 | 215.172 | 0.891 | 206.129 | 215.172 | 0.149 |
| | (36.24) | (32.67) | | (31.5) | (29.84) | | (37.92) | (29.84) | |
| Has experience working with piece rates | 0.500 | 0.344 | 0.220 | 0.469 | 0.448 | 0.875 | 0.452 | 0.448 | 0.711 |
| | (0.51) | (0.48) | | (0.51) | (0.51) | | (0.51) | (0.51) | |
| Baseline production (full training period) | -0.477 | -0.563 | 0.572 | -0.183 | -0.117 | 0.676 | 0.061 | -0.117 | 0.422 |
| | (0.55) | (0.67) | | (0.6) | (0.65) | | (0.65) | (0.65) | |
| Baseline attendance (full training period) | 0.955 | 0.944 | 0.603 | 0.949 | 0.948 | 0.959 | 0.977 | 0.948 | 0.124 |
| | (0.07) | (0.1) | | (0.09) | (0.07) | | (0.06) | (0.07) | |
| F-test of Joint Significance: | | | 0.888 | | | 0.899 | | | 0.904 |

*Notes:* Responses taken from baseline surveys conducted on first day of work. Means and standard deviations are shown for Low, Medium, and High rank workers in Cols (1)-(2), (4)-(5), and (7)-(8), respectively. We show means separately for Pay Disparity and Compressed: Relevant workers. Compressed: Relevant workers are the Low rank workers in *Compressed Low* units, Medium rank workers in *Compressed Medium* units and High rank workers in *Compressed High* units. Cols (3), (6), and (9) display the p-values of the comparisons of means across Pay Disparity and Compressed: Relevant production units obtained from a simple univariate regression, where standard errors are clustered by production unit. In the last row, we report the p-values from F-tests of joint significance. We regress an indicator for Pay Disparity on each of the controls and report the P-values from the F test of joint significance. N=194.

TABLE 2. Effects of Pay Disparity: Robustness to Alternate Specifications

| | Output (std dev.) (1) | Attendance (2) | Output (std dev.) (3) | Attendance (4) | Output (std dev.) (5) | Attendance (6) | Output (std dev.) (7) | Attendance (8) |
|---|---|---|---|---|---|---|---|---|
| *Panel A — Pooled Treatment Effects* | | | | | | | | |
| Post x Pay disparity | -0.222*** | -0.0961*** | -0.272*** | -0.124*** | -0.242** | -0.117*** | -0.272** | -0.115*** |
| | (0.070) | (0.019) | (0.086) | (0.022) | (0.097) | (0.025) | (0.111) | (0.028) |
| *Panel B — Treatment Effects Separately by Rank* | | | | | | | | |
| Post x Pay disparity x Low wage | -0.279** | -0.100** | -0.400*** | -0.115** | -0.332** | -0.120** | -0.292** | -0.0987* |
| | (0.110) | (0.043) | (0.120) | (0.049) | (0.128) | (0.053) | (0.134) | (0.054) |
| Post x Pay disparity x Med wage | -0.180 | -0.0995** | -0.278 | -0.148** | -0.226 | -0.129** | -0.291 | -0.146** |
| | (0.127) | (0.045) | (0.194) | (0.065) | (0.187) | (0.060) | (0.198) | (0.062) |
| Post x Pay disparity x High wage | -0.205 | -0.0882** | -0.145 | -0.111** | -0.172 | -0.104** | -0.238 | -0.103** |
| | (0.140) | (0.037) | (0.174) | (0.052) | (0.181) | (0.052) | (0.202) | (0.052) |
| Sample | Relevant | Relevant | Relevant | Relevant | Full | Full | Full | Full |
| Include Pre-treatment observations? | Yes | Yes | Yes | Yes | Yes | Yes | No | No |
| Include "Irrelevant" workers? | No | No | No | No | Yes | Yes | Yes | Yes |
| Neighbor Controls? | No | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects? | Yes | Yes | Yes | Yes | Yes | Yes | No | No |
| Post-treatment Compressed Mean | -0.099 | 0.939 | -0.099 | 0.939 | -0.099 | 0.939 | -0.099 | 0.939 |
| N | 4307 | 4307 | 4307 | 4307 | 8375 | 8375 | 5283 | 5283 |

*Notes*: Difference in differences regressions are presented in Cols (1)-(6). Cols (7)-(8) only include post-training, post-treatment observations. Panel A pools the treatment effects across the low, medium, and high rank workers, while Panel B shows the treatment effects separately by rank. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. Regressions include day*round fixed effects, and task-specific quadratic experience trends. Cols (3)-(8) include controls for neighboring production units. All coefficients are identified off comparisons of workers who earn the same absolute wage and have the same productivity rank within their production unit (see regression specification in text), i.e., from the so-called "relevant" workers. Standard errors clustered by production unit.

TABLE 3. Effects on Output Quality

| | Quality rating (1) | High quality rating (2) | Proportion rejected (3) |
|---|---|---|---|
| *Panel A — Pooled* | | | |
| Post x Pay disparity | -0.0128 | -0.0907 | -0.0123 |
| | (0.026) | (0.092) | (0.017) |
| *Panel B — Separately by Rank* | | | |
| Post x Pay disparity x Low wage | 0.00705 | -0.0471 | 0.0187 |
| | (0.050) | (0.173) | (0.021) |
| Post x Pay disparity x Med wage | -0.0361 | -0.183 | -0.0396 |
| | (0.046) | (0.164) | (0.039) |
| Post x Pay disparity x High wage | -0.0182 | -0.109 | -0.00563 |
| | (0.026) | (0.110) | (0.018) |
| Post-treatment Compressed mean | 0.823 | 0.842 | 0.012 |
| N | 3868 | 3868 | 1669 |

*Notes* : Difference in differences regressions. The sample is restricted to days when a worker was present. Cols. (1)-(2) show effects on supervisors' subjective daily assessment of the quality of each worker's output (collected in only a subset of days and rounds). Quality rating is a proportion: the rating score divided by the maximum possible rating (either 3 or 5). High quality rating is a boolean for whether the worker's quality rating on that day was above 0.8. The dependent variable in Col. (3) is the proportion of output that was rejected due to substandard quality; this is only measured for the two production tasks where quality standards were quantifiable (candle wicks and incense sticks). Panel A pools effects across the low, medium, and high rank workers, while Panel B shows effects separately by rank. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring units. Note that caution must be used when interpreting these coefficients, since the sample conditions on attendance. Standard errors clustered by production unit.

TABLE 4. Perceived Justifications: Robustness of Relative Productivity Results

| | Controls for own baseline productivity x post x rank | | Controls for baseline productivity lowest 10% x post x treatment | | Controls for own baseline productivity x post x treatment x rank | |
|---|---|---|---|---|---|---|
| Dependent variable | Output | Attendance | Output | Attendance | Output | Attendance |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post x Pay disparity | -0.353** | -0.158*** | -0.413*** | -0.179*** | -0.456*** | -0.178*** |
| | (0.137) | (0.042) | (0.134) | (0.042) | (0.144) | (0.042) |
| Post x Pay disparity x Perceived justification | 0.299* | 0.152** | 0.292* | 0.162*** | 0.221 | 0.129* |
| | (0.177) | (0.063) | (0.171) | (0.061) | (0.178) | (0.068) |
| R-squared | 0.436 | 0.173 | 0.438 | 0.172 | 0.439 | 0.175 |
| Number of observations (worker-days) | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 |

*Notes:* Table shows comparisons of each worker in Pay disparity teams with the relevant worker (who has the same rank and absolute earnings level) in the Compressed teams. The Perceived justification indicator equals 1 if the baseline productivity difference between a worker and his higher-ranked co-worker (for low and medium rank workers) is above the Compressed group mean. Cols (1)-(2) include time- and rank- varying controls for baseline productivity. Cols (3)-(4) include time- and treatment- varying controls for whether the worker's baseline productivity was in the lowest 10% of the distribution (for Low workers). Cols (5)-(6) add time-, treatment-, and rank- varying controls for the worker's baseline productivity. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 5. Perceived Justifications: Robustness to Alternative Cutoffs for Relative Productivity Thresholds

| | Definition of Covariate for Relative Productivity Fraction | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | >10% difference | | >15% difference | | >20% difference | | >25% difference | | Continuous Linear | |
| Dependent variable | Output | Attendance | Output | Attendance | Output | Attendance | Output | Attendance | Output | Attendance |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Post x Pay disparity | -0.270 | -0.122** | -0.321** | -0.156*** | -0.355*** | -0.177*** | -0.351** | -0.173*** | -0.407*** | -0.170*** |
| | (0.170) | (0.052) | (0.145) | (0.040) | (0.135) | (0.037) | (0.138) | (0.039) | (0.135) | (0.038) |
| Post x Pay disparity x Perceived justification | 0.00679 | -0.00936 | 0.151 | 0.0790 | 0.332** | 0.178*** | 0.313* | 0.171** | 0.781*** | 0.260*** |
| | (0.187) | (0.076) | (0.164) | (0.064) | (0.166) | (0.060) | (0.175) | (0.067) | (0.264) | (0.090) |
| R-squared | 0.434 | 0.170 | 0.435 | 0.170 | 0.436 | 0.172 | 0.436 | 0.172 | 0.436 | 0.170 |
| Number of observations (worker-days) | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 |

*Notes:* Regressions show comparisons of each worker in Pay disparity teams with the relevant worker (who has the same rank and absolute earnings level) in the Compressed production units, heterogeneously by Perceived justifications. Results are pooled across the Low and Medium rank workers. In Cols (1) - (8), Perceived justifications indicator equals 1 if the difference between a worker and his higher-ranked peer (for low and medium rank workers) is greater than X%, where X is stated at the top of each column. Cols (9) - (10) use a continuous measure of relative productivity. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 6. Perceived Justifications: Task Observability Robustness

| Sample restriction: drop tasks | Task 1 | Task 2 | Task 3 | Task 4 | Task 5 | Task 6 | Task 7 | Task 8 | Task 9 | Task 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A — Output (std. dev)* | | | | | | | | | | |
| Pay disparity | -0.359*** | -0.381*** | -0.423*** | -0.385*** | -0.391*** | -0.115 | -0.399*** | -0.533*** | -0.516*** | -0.378** |
| | (0.125) | (0.130) | (0.137) | (0.129) | (0.135) | (0.121) | (0.129) | (0.145) | (0.149) | (0.151) |
| Pay disparity x Perceived justification | 0.393** | 0.386** | 0.453*** | 0.373** | 0.430** | 0.177 | 0.313* | 0.563*** | 0.513*** | 0.351* |
| | (0.180) | (0.164) | (0.171) | (0.167) | (0.183) | (0.156) | (0.160) | (0.166) | (0.177) | (0.184) |
| *Panel B — Attendance* | | | | | | | | | | |
| Pay disparity | -0.143*** | -0.152*** | -0.151*** | -0.152*** | -0.153*** | -0.144*** | -0.156*** | -0.205*** | -0.139*** | -0.125*** |
| | (0.032) | (0.034) | (0.035) | (0.032) | (0.034) | (0.036) | (0.033) | (0.029) | (0.041) | (0.029) |
| Pay disparity x Perceived justification | 0.141*** | 0.101** | 0.0901* | 0.103** | 0.105* | 0.0871* | 0.0672 | 0.155*** | 0.0885* | 0.0719 |
| | (0.049) | (0.049) | (0.049) | (0.047) | (0.056) | (0.047) | (0.055) | (0.046) | (0.053) | (0.047) |
| N | 7459 | 7638 | 7453 | 7638 | 7511 | 7500 | 7482 | 7573 | 7565 | 7556 |

*Notes:* Table shows specification for main observability regressions, but sequentially drops each production task from the regression. Comparisons are between each worker in Pay disparity production units with the relevant worker (who has the same rank and absolute earnings level) in the Compressed production units. Treatment effects are pooled across the Low, Medium and high Rank workers. The Perceived justification indicator equals 1 if the observability correlation for the worker's production task (computed using a separate baseline sample) is above the mean. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 7. Absence of Correlation Between Task Observability Measure and Training Outcomes

| | Attendance (1) | Attendance (2) | Did not complete training (3) | Output growth (std dev.) (4) |
|---|---|---|---|---|
| Observable task | -0.0107 | -0.0103 | -0.00476 | -0.0125 |
| | (0.011) | (0.008) | (0.011) | (0.151) |
| Sample | All hired workers | Training completors | All hired workers | Training completors |
| Frequency of data | Daily | Daily | Worker-level | Worker-level |
| Pre-treatment Mean | 0.914 | 0.954 | 0.019 | 0.233 |
| N | 3557 | 3092 | 415 | 378 |

*Notes*: OLS regressions are presented in Cols (1)-(4) to measure pre-treatment correlates with observability. The regressor in all specifications is an indicator for whether the production task has above-median observability. Cols (2) and (4) restrict the analysis to only workers who completed the training period and were thus available to be randomized into treatments. Cols (1)-(2) use daily attendance as the dependent variable. Col (3) captures whether a worker terminated his employment before the end of the training period. Col (4) measures output growth in the training period by taking the difference between standardized output on days 3 and 8. This captures the steepness of the learning curve for each worker. Regressions include round fixed effects. Standard errors clustered by production unit.

TABLE 8. Sample Overlap Between Perceived Justification Measures

| | Below mean observability (Total 51.59%) | Above mean observability (Total 48.41%) |
|---|---|---|
| Below mean production difference (Total 69.44%) | 34.52% | 34.92% |
| Above mean production difference (Total 30.56%) | 17.06% | 13.49% |

*Notes:* Tabulations of overlap between the two sources of variation for perceived justifications: baseline productivity differences between a worker and his higher ranked peer, and the observability of co-worker output. The binary splits for each measure shown here are the same as those used in the tables in the analysis. The table shows the percentage of observations in each cell.

TABLE 9. Effects of Higher Absolute Pay by Rank

| | Output (std dev.) (1) | Attendance (2) |
|---|---|---|
| Post x Low Rank x Compressed Wage Ordinal | -0.134 | -0.0604* |
| | (0.089) | (0.033) |
| Post x Medium Rank x Compressed Wage Ordinal | 0.0668 | 0.0764* |
| | (0.104) | (0.040) |
| Post x High Rank x Compressed Wage Ordinal | 0.0380 | 0.0223 |
| | (0.073) | (0.017) |
| Post x Medium Rank | -0.186 | -0.124* |
| | (0.165) | (0.069) |
| Post x High Rank | -0.145 | -0.0112 |
| | (0.142) | (0.033) |
| Post-treatment, Low Rank, Compressed Low Mean | -0.123 | 0.918 |
| N | 6107 | 6107 |

*Notes*: Difference in differences regressions restricting the sample to only Compressed teams. Post is an indicator that equals 1 if the day is after workers have been randomized into wage treatments, and 0 during the baseline training period. Compressed Wage Ordinal takes the value 0 if the unit is Compressed Low, 1 if the unit is Compressed Medium, and 2 if the unit is Compressed High. Omitted category is Low rank workers on Compressed low wage units. All specifications include the full training period. Cols (1) and (4) include only the first two post wage change days. Cols (2) and (5) include only the first work week post wage change. Cols (3) and (6) include the full sample. Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 10. Robustness: Effects on Group Cohesion - Conditional on Attendance

| | | | Game: Tower building in teams | | Game: Cooperative puzzles in pairs | |
|---|---|---|---|---|---|---|
| Dependent variable | Attendance | Attendance | Tower height | Tower height | Number correct | Number correct |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pay disparity | -0.0106 | -0.00889 | -10.18** | -12.98** | | |
| | (0.031) | (0.034) | (4.354) | (6.241) | | |
| Compressed_Medium pay | | 0.0198 | | -7.400 | | |
| | | (0.044) | | (6.429) | | |
| Compressed_High pay | | -0.0150 | | -0.936 | | |
| | | (0.053) | | (8.362) | | |
| Both workers from same unit x Pay disparity | | | | | -0.939** | |
| | | | | | (0.431) | |
| Both workers from same unit | | | | | 0.404* | |
| | | | | | (0.227) | |
| At least one worker in pair was on Pay disparity unit | | | | | 0.203 | |
| | | | | | (0.325) | |
| At least one low rank worker in pair | | | | | -0.604** | -0.714*** |
| | | | | | (0.236) | (0.252) |
| At least one medium rank worker in pair | | | | | -0.387* | -0.398* |
| | | | | | (0.219) | (0.216) |
| Dependent variable mean | 0.921 | 0.921 | 54.17 | 54.17 | 4.695 | 4.695 |
| Observations | 240 | 240 | 65 | 65 | 1632 | 1632 |
| R-squared | 0.170 | 0.172 | 0.318 | 0.338 | 0.203 | 0.105 |

*Notes*: This table replicates results for cooperative games, conditional on attendance. Cols (1)-(2) include the full sample of workers who participated in endline games (run only in the later rounds of the experiment). Cols (3)-(4) limit analysis to units where all 3 unit members were present on the day of endline games. Cols (1)-(4) include round fixed effects. Cols (5)-(6) limit analysis to pair-games where both workers in an assigned pair were present the day of endline games. Col (5) includes fixed effects for the order in which a game was played during the day and the game station (location in worksite) as well as education controls. Standard errors are clustered by production unit.

Table 11. Effects of Pay Disparity by Village Ties

| | Definition of Village Coworkers Interaction | | | | | |
|---|---|---|---|---|---|---|
| | All workers in unit from same village | | Indicator for any coworker in unit from worker's village | | Number of coworkers in unit from worker's village | |
| Dependent variable | Output | Attendance | Output | Attendance | Output | Attendance |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A — Pooled Treatment Effects* | | | | | | |
| Post x Pay disparity | -0.139 | -0.123*** | -0.0845 | -0.130** | -0.0609 | -0.135*** |
| | (0.118) | (0.030) | (0.188) | (0.061) | (0.166) | (0.047) |
| Post x Pay disparity x Village coworkers | -0.199 | 0.00620 | -0.159 | 0.0300 | -0.124 | 0.0193 |
| | (0.158) | (0.045) | (0.191) | (0.070) | (0.107) | (0.036) |
| *Panel B — Treatment Effects Separately by Rank* | | | | | | |
| Post x Pay disparity x Low wage | -0.231 | -0.142** | -0.539* | -0.276*** | -0.277 | -0.205** |
| | (0.158) | (0.065) | (0.295) | (0.100) | (0.243) | (0.087) |
| Post x Pay disparity x Low wage x Village coworkers | -0.145 | 0.0348 | 0.239 | 0.181 | -0.0230 | 0.0622 |
| | (0.267) | (0.100) | (0.323) | (0.111) | (0.179) | (0.064) |
| Post x Pay disparity x Med wage | -0.301 | -0.146* | -0.0352 | -0.174 | -0.291 | -0.181* |
| | (0.233) | (0.075) | (0.310) | (0.122) | (0.314) | (0.106) |
| Post x Pay disparity x Med wage x Village coworkers | 0.239 | 0.0457 | -0.151 | 0.103 | 0.102 | 0.0608 |
| | (0.225) | (0.085) | (0.317) | (0.120) | (0.176) | (0.061) |
| Post x Pay disparity x High wage | 0.132 | -0.0796 | 0.214 | 0.0257 | 0.398* | -0.0176 |
| | (0.183) | (0.061) | (0.286) | (0.072) | (0.235) | (0.069) |
| Post x Pay disparity x High wage x Village coworkers | -0.763*** | -0.0727 | -0.456 | -0.148* | -0.462*** | -0.0703** |
| | (0.278) | (0.058) | (0.310) | (0.076) | (0.163) | (0.035) |
| R-squared | 0.436 | 0.171 | 0.436 | 0.174 | 0.436 | 0.172 |
| Number of observations (worker-days) | 8375 | 8375 | 8375 | 8375 | 8375 | 8375 |

*Notes:* Panels A and B show comparisons of each worker in Pay disparity production units with the relevant worker (who has the same rank and absolute earnings level) in the Compressed production units. In Cols. (1)-(2), the Village coworkers variable is equal to one if all unit members are from the same village. In Cols. (3)-(4), this variable is an indicator for whether any of the worker's unit coworkers are from his village. In Cols. (5)-(6), this variable is equal to the number of unit coworkers that are from the worker's village (can be 0, 1, or 2). Regressions include individual fixed effects, day*round fixed effects, task-specific quadratic experience trends, and controls for neighboring production units. Standard errors clustered by production unit.

TABLE 12. Lack of Evidence of Attendance Peer Effects

| | First Stage | Reduced Form | IV (2SLS) |
|---|---|---|---|
| Dependent variable | Co-worker attendance | Own Attendance | Own Attendance |
| | (1) | (2) | (3) |
| Predicted attendance of co-workers | 0.600** | 0.0035 | |
| | (0.278) | (0.169) | |
| Actual attendance of co-workers (instrumented) | | | 0.0063 |
| | | | (0.279) |
| F-stat: First stage excluded instrument | 4.65 | | |
| Number of observations (worker-days) | 6020 | 6020 | 6020 |

*Notes:* Table shows peer effects regressions instrumenting actual peer attendance with predicted peer attendance. Predicted attendance is calculated using the average attendance of all workers from the same village excluding the worker and others from his production unit (i.e., a jack-knife or leave-one-out strategy). Observations limited to workers on Compressed pay units. Column 1 presents the first stage regression of actual peer attendance on predicted peer attendance. Column 2 presents the reduced form regression of own attendance on predicted peer attendence. Column 3 presents the IV estimation of the effects of peer attendance on own attendance. All specifications include fixed effects for round, calendar month, and day of round, and indicators for being assigned to a unit with one or more co-workers from the same village. Standard errors clustered at the production unit level.

TABLE 13. Response to Disclosure of Productivity Ranks in Training Period

| | Output (std dev.) | Output (std dev.) | Output (std dev.) | Attendance |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Post rank disclosure x Low rank (pre disclosure) | 0.00346 | 0.00801 | -0.000635 | 0.00146 |
| | (0.089) | (0.093) | (0.094) | (0.024) |
| Post rank disclosure x Medium rank (pre disclosure) | 0.00828 | -0.00984 | -0.00891 | 0.00303 |
| | (0.058) | (0.059) | (0.060) | (0.020) |
| Experience x task controls? | No | Quadratic trends | Fixed effects | Fixed effects |
| Mean of dependent variable: 3 days prior to ranking treatment | -0.322 | -0.322 | -0.322 | 0.969 |
| F-test p-value: Post ranking x Low = Post ranking x Medium | 0.943 | 0.814 | 0.911 | 0.935 |
| N | 3060 | 3060 | 3060 | 3060 |

*Notes:* Difference in differences regressions presented to test whether workers of different ranks respond differently to the information about their relative productivity that was revealed four days (on average) before the end of the training period. Here, rank is calculated based on production during the three days prior to the release of the ranking information. Regressions only contain data from the training period, and Post rank disclosure is an indicator for the four days (on average) following disclosure before the end of the training period. All regressions include individual fixed effects as well as worksite x day fixed effects. The coefficients are all relative to the output and attendance of the person with the highest rank. The specifications that include experience x task fixed effects are identified using variation in the lengths of the training periods across rounds. Standard errors clustered by production unit.

Table 14. Supplemental Survey: Fairness of Different Wage Regimes

Outcome: Percentage of respondents saying the scenario is "Unfair" or "Very Unfair"

| | | Pay type | |
|---|---|---|---|
| | | A) flat daily wage | B) piece rate |
| | | (1) | (2) |
| 1 | A brick factory in an area pays its workers a:<br>A) flat daily wage<br>B) piece-rate<br>for laying bricks. Balu and Mohit are both laborers who have worked in the factory for 6 months. Balu earns Rs. 20 more per day. | 98% | 0% |
| 2 | A brick factory in an area pays its workers a:<br>A) flat daily wage<br>B) piece-rate<br>for laying bricks. Balu and Mohit are both laborers who have worked in the factory for 6 months. Balu, *who makes more bricks than Mohit*, earns Rs. 20 more per day. | 16% | 4% |

*Notes*: Survey conducted with workers who were not a part of the experiment, but drawn from a similar population in the area where the experiment was conducted. N=200 workers. Each worker was only asked one of the 4 questions shown in the table. Respondents were given one of the 4 scenarios, and asked to rate them as "Completely fair", "Acceptable", "Unfair", or "Very unfair".

## Appendix B. Protocols Appendix

### B.1. **Protocols.**

B.1.1. *Production Consultants.* We partner with local contractors to help us with the initial setup of the worksites and the training of the management staff. These contractors typically live in one of the two large cities in the area (Bhubaneswar and Cuttack) and manage the production and sale of the same goods produced in our worksites. They train the worksite management staff in the production of the goods, teach them how to determine quality, and help them to set up the production supply chain. They also instruct management on how to select inputs for production and also how to store those inputs. They also either sell all output in the local market or help us to find partnering contractors to buy the final goods.

B.1.2. *Worksite Management.* We employ enumerators from the local regions, and train them for the roles of worksite supervisors and managers. Their local background and expertise are essential in ensuring that they come across to workers as credible and realistic employers. Worksite supervisors manage the day-to-day operations at the worksite; for example, taking attendance, conducting training, providing workers with feedback, and delivering scripts to workers. There are multiple worksite supervisors in one worksite; each

supervisor is assigned to oversee several production units. Worksite managers play a more senior role; they circulate across different worksites to do spot checks and to ensure protocols are being followed.

B.1.3. *Worksite Setup.* We set up worksites in semirural areas surrounding the city of Bhubaneswar, Odisha. Each worksite is organized into 10 distinct areas; usually, each distinct area is in a separate room. On Day 1, production units are assigned to one of the 10 distinct areas within the worksite. The workers within a production unit sit in their designated area, where they work and eat lunch together for the duration of employment. Inputs required for the production activity are often stored within the same designated area. This results in each production unit being physically separated from other units. This seems natural to the workers because there are space requirements for each production activity and for storing task-specific inputs nearby.

B.1.4. *Production Monitoring.* We hire additional staff whose job is to monitor production. They collect, count and assess the quality of each worker's output. While the workers are working, each individual's output is kept separately from their unit co-workers. Periodically throughout the day, these finished goods are removed from the production areas and are taken to a back storage room, which is not accessible to the workers. There, the production monitors count and record the raw quantities produced and also separately indicate the quantity produced of salable quality (e.g. number of individual incense sticks or paper bags). The worksite supervisor performs random backchecks to count and evaluate the quality of a subsample of production. Once the backcheck is completed, the production monitors bundle and package the high-quality products for pickup by partnering contractors.

B.1.5. *Worksite Timeline.*
Day 1. A worksite manager delivers a welcome script to all workers. Workers are told that they are being hired for a month-long one-time "training" contract, with no possibility of future employment. By labeling it a "training" contract, this enables workers to accept and understand that this is a one-time job, because the employer's policy strictly dictates that each person only gets the benefit of the skills training once. It is made clear to workers that their output is being sold to the external market by contractors, so from the workers' perspective, it is a job with an employer that would benefit financially from increases in their effort. Their likely perception is that they are working for an altruistically minded "social enterprise" type employer, who is willing to train them on how to make a product.

Workers are told that they would first undergo a training period during which their production would be monitored, and everyone's wages would increase after the training period ended. The rationale behind the training period is that most workers are doing this kind of work for the first time and need several days to learn and practice. The "training" contract helps justify a clear training period at baseline, after which wages would go up for everyone. Workers are told that the wage increase may depend on their productivity. In

this setting, employers typically do not give workers written contracts with fixed terms, and some amount of fluidity in what will happen in the future is not unusual given the casual and short-term nature of work. Further, while monitoring of worker activity is normal in any workplace, a "training" contract helps justify some additional features – such as individualized calendars or feedback on performance (including rank) during the training period.

Workers are then assigned to their production units, and are instructed to sit in the area designated to their unit. The relevant worksite supervisor then delivers a welcome script to the 3 workers in the unit. Workers are given information on the specific production task that they have been assigned to, and are told to focus their attention on their task, and not on tasks that other are engaged in.

To ensure understanding of the timeline, each worker is given an individualized calendar. The calendar highlights key dates, including when the initial training period will end (and wages will increase) as well as the last day of employment. On Day 1, the training wage amount is filled in on the calendar for each of the training days, with the remainder of the days left blank. Upon arrival to work each day, the worker is shown his calendar and the worksite supervisor delivers a daily attendance script. This serves as a reminder to workers that there will be a wage increase at the end of the initial training period, and that the employment period ends on a pre-set date.

We also put numerous protocols in place in order to control workers' up-front beliefs. For each round, we recruit from a new local labor market (i.e., a geographically separate pair of villages), so each crop of workers come in without having communicated with any of the previously employed workers. In baseline surveys that we conduct on Day 1, we only ask a few questions, and we limit these questions to things that an employer may reasonably be interested in: education, literacy, work history, etc. We sacrifice some data collection needs (e.g. eliciting social network data among workers at baseline, collecting baseline happiness data) here in order to avoid asking for information that would seem odd at the beginning of the job.

Day 10. During a routine private check-in with each worker, the worksite manager delivers the ranking treatment script. Workers are told their ranks relative to the other workers in their unit and are reminded that there are 3 days remaining in the training period, during which their production would be monitored.

Day 14. Workers are told that they will have a one-on-one check-in with the worksite manager. During this private meeting, the worksite manager delivers the wage treatment script. Workers are told their new wage, which is then written in their individual calendar for each of the remaining days until the end of employment. This serves as a reminder to workers that there will be no further wage changes. It is very important to correctly fix beliefs on this day that there are no opportunities for future wage increases. In a dynamic environment, the predictions of how a worker might respond to pay inequality are unsigned, as

low wage workers may actually work harder to try to obtain a higher wage in the future. Workers are also told that pay secrecy would be maintained, and that the employer has no intention of revealing wages to co-workers. The calendars are not shared with other unit coworkers.

Day 34. The worksite manager delivers a short script to all workers in order to remind them that the "training" contract is ending, and to encourage them to show up on the last day of employment.

Day 35. Endline games and surveys are conducted on this last day, after all the revealed preference output data has been collected. Workers are not told about these endline activities in advance. The worksite manager leads the endline games, with worksite supervisors supporting the logistics.

### B.2. **Scripts.**

**Day 1 of Employment: Welcome Script (all workers).** *This script should be delivered by the worksite manager to all new workers on the first day of employment, once all the workers have arrived at the worksite.*

Welcome to the worksite.

This is a training program that will teach you how to make one of ten types of products. You will each learn how to make one of the following products: broom / rope / leaf mat / paper bag / incense stick / peanuts / cotton wick / grass mat / leaf plate / stick broom.

The goal of this program is to train you on how to make this product. You will be able to participate in this training program for <*insert number*> days. We will end on <*insert day and date*>. After this day, there will be no more work here. No person can work here for more than <*insert number*> days. Our policy is to train each person only once, and only in one product.

*To ensure that workers understand, manager should prompt workers with the following questions: So how many days is this work for? What are the different types of products you may be learning to make? How many days will this training program be? When is the last day of work here?*

As we announced during recruitment, you will be paid <*insert daily wage*> a day. All wage payments will be made at the end of the week on Friday. There will be no work on Saturdays and Sundays. You must arrive at the worksite at 9am, and work will end at 5pm. When you arrive, you should report to your supervisor, sign in on the attendance sheet and sit in the area that has been designated to you. We will provide you with lunch at 1pm. At the end of the day, you should let your supervisor know before you leave the worksite.

If you arrive after 10am or leave before 4pm, it will be considered a half-day and you will be paid half the daily wage. If you leave before lunch or arrive after lunch, you will be

marked as absent and you will not be paid for the day. Further, if you arrive after 10am, we will not be able to provide you with lunch as we place the order for lunch before 10am.

*Pause and ask if there are any questions. Manager should prompt workers with the following questions: What time are you expected to arrive every morning? When will work end each day? What is the wage that you will be paid?*

Finally, you should note that we will be monitoring how much you produce throughout this training. For example, we will count the number of brooms / incense stick / paper bags made, measure the length of leaf mat made, weigh the de-shelled peanuts etc.

The first 10 days will be a practice period, where you will learn and practice how to make the product that is assigned to you. Once the practice period is over, we will increase your daily wage, and this increase might depend on the amount that you have produced.

We would like to remind you that this is a training program and it is our policy to train each person only once, and only in one product. Our goal is to train as many people as possible, so after this training program is over, you will not be able to work for us again.

*Pause and ask if there is any questions. At this point it should be made clear to all workers that this is a training program with no possibility of future employment.*

Now we will introduce you to your product unit and the supervisor who will teach you how to make this product. Do you have any questions before we begin?

**Day 1 of Employment: Welcome Script (Product Unit).** *This script should be delivered by the unit supervisor to the 3 members of a product unit, once the workers have been put into their respective units.*

Before we begin, I will give you a brief introduction to the product that has been assigned to your unit, as well as explain your responsibilities. Feel free to ask any questions at any point.

The three of you will be learning how to make <*insert product name and description of product*>. The workers in the other units in the worksite will be making something different. You should not be concerned with what the other units are doing. You should only focus on your product and what you have to learn.

First, we will teach you how to make <*insert product name*> and allow you to practice for 10 days. It is important that you are present on all days during the practice period. If you are absent for two consecutive days during the practice period, we will replace you and give your spot in this training program to someone else. During this practice period, I will be monitoring your progress and giving you feedback at regular intervals. You will be producing <*insert product name*>, and we will be measuring the <*insert unit*> of <*insert product name*> you make each day. By the end of the practice period, we expect that the quality of the <*insert product name*> made to be of market quality. Once the practice period is over, we will commence full production until the last day of the training program <*insert final day and date of employment*>.

As the worksite manager explained earlier, you will be paid a daily wage of Rs. 250. At the end of the practice period, we will change your wage on <*insert day and date of wage treatment*>. Your new wage might depend on how much you produce during the practice period. After this, there will be no further change in your wage. You will continue to receive this wage until the last day of work on <*insert final day and date of employment*>.

*Pause and ask if there are any questions.*

To recap what the worksite manager explained earlier, your work here will start at 9am and end at 5pm, from Monday to Friday. You will be paid at the end of each week, on Friday. When you arrive each morning, you should report to me and sign in the attendance sheet. We will provide lunch at 1pm. At the end of the day, you should let me know before leaving the worksite. You must sit with your unit <*insert designated area*> every day.

If you arrive after 10am or leave before 4pm, you will be paid half the daily wage as it will be considered a half-day. If you leave before lunch or arrive after lunch, you will be marked as absent. Further, if you arrive after 10am, we will not be able to provide you with lunch, as we place the order for lunch before 10am.

This training program will last for <*insert number*> days. We will end on <*insert day and date*>. Our goal is to provide training to as many people as possible, so after this date, we will move on to training other people. There will be no opportunities for future work for you once this training program is completed. Do you have any questions before we begin?

**Daily Attendance Script.** *This script should be delivered by the unit supervisor to each worker individually. The unit supervisor should have the relevant worker's calendar in hand when delivering the script. On the calendar, the day of the wage treatment and the final day of worksite employment should be clearly circled in red. This script should be delivered daily once the worker arrives at the worksite. Note that there are two versions of this script - one for the initial practice period, and one for after the practice period concludes.*

*During the practice period (pre wage treatment):*

Please cross out today's date in your calendar. You have <*insert number of days*> days remaining in the practice period to learn and practice making this product. On <*insert day and date of wage treatment*>, the practice period will be over. After the practice period, you will commence full production until <*insert last day and date*>.

*The supervisor should point out the marked last day of the practice period on the worker's calendar.*

Your wage today is <*insert daily wage*>.

*The supervisor should write the wage in the box for this day.*

Remember, this training program will end on <*insert last day and date*>. After this there will be no more work here. You will not get a second chance to work here because our policy is to train everyone only once. Do you have any questions?

*The supervisor should point out the marked last day of employment on the worker's calendar.*

*After the practice period has concluded (post wage treatment):*

Please cross out today's date in your calendar. You have <*insert number of days*> days remaining in the training program. Your daily wage will remain fixed until <*insert last day and date*>.

*The supervisor should point out the wage that has been pre-filled for the remaining days of employment.*

Remember, this training program will end on <*insert last day and date*>. After this there will be no more work here. You will not get a second chance to work here because our policy is to train everyone only once. Do you have any questions?

*The supervisor should point out the marked last day of employment on the worker's calendar.*

**Day 10 of Employment: Ranking Treatment Script.** *This script should be delivered by the worksite manager to each worker individually. The three workers in each product unit should receive this script consecutively, with the highest productivity worker receiving the script first.*

Today is the seventh day of this training program. Over the past six days, we have taught you how to make <*insert product*>. After today, there will only be <*insert number of days*> more days of work. Remember that this is a training program, and it is our policy to give this training only once, so you will not be able to participate in this training program again after <*insert day and date*>.

*To ensure that the worker understands, manager should prompt the worker with the following question: After today, how many days of work are left?*

As we mentioned on the first day, we have been closely monitoring the number of <*insert product*> that you have been making. From what we have measured, compared to the other two workers in your unit, <*you are producing the most/one worker is producing more than you and one worker is producing less than you/you are producing the least*>.

*To ensure that the worker understands, manager should prompt the worker with the following question: In terms of making <insert product>, what is your position in the unit?*

After today, there will be three more days remaining in the practice period, after which, based on what you have learnt, you will commence full production. We will continue to monitor how much you are making, and we will announce your new wage on <*insert day and date*>. After that day, there will be no further change in wages. Do you have any questions?

**Day 14 of Employment: Wage Treatment Script.** *This script should be delivered by the worksite manager to each worker individually.*

*For the high and medium productivity worker:*

We are going to discuss the training program so far with you. For the last 10 days, we have taught you and your two co-workers how to <*insert product*>, and given you time to learn and practice this product. The <*insert product*> that you make can now be sold in the market. The practice period is over, and we will begin full production starting today. Starting from today, your new wage will be <*insert daily wage*>. This new wage is going to be fixed from today till <*insert day and date*>. There will be no changes to your wage after today—it will not go up or down after this. Just like before, we will make payments at the end of the week, on Friday.

*To ensure that the worker understands, manager should prompt the worker with the following question: What is your new wage? Will it change after today?*

On <*insert day and date*>, this training program is going to end. Recall that our goal is to train as many people as possible, so after <*insert day*>, there will be no more work here for you. What you have learnt here, you can use it to do work from home, but you will not get a second chance to work here because you have already been trained by us. We have a strict policy of only training everyone once. Do you have any questions?

*To ensure that the worker understands, manager should prompt the worker with the following question: How many days do you have left in this training program with us?*

*For the low productivity worker:*

We are going to discuss the training program so far with you. For the last 10 days, we have taught you and your two co-workers how to <*insert product*>, and given you time to learn and practice this product. The <*insert product*> that you make can now be sold in the market. The practice period is over, and we will begin full production starting today. Starting from today, your new wage will be <*insert daily wage*>. This new wage is going to be fixed from today till <*insert day and date*>. There will be no changes to your wage after today—it will not go up or down after this. Just like before, we will make payments at the end of the week, on Friday.

Our policy is to keep wages confidential. We will make wage payments to you privately so this will remain between us. We will not tell anyone else what your new wage is.

*To ensure that the worker understands, manager should prompt the worker with the following question: What is your new wage? Will it change after today?*

On <*insert day and date*>, this training program is going to end. Recall that our goal is to train as many people as possible, so after <*insert day*>, there will be no more work here for you. What you have learnt here, you can use it to do work from home, but you will not get a second chance to work here because you have already been trained by us. We have a strict policy of only training everyone once. Do you have any questions?

*To ensure that the worker understands, manager should prompt the worker with the following question: How many days do you have left in this training program with us?*

**Post Wage Treatment: Weekly Reminder Script.** *This script should be delivered by the worksite manager to all workers once a week, after the wage treatment is implemented. The worksite manager should walk around the worksite, and stop by each product unit's designated area to talk to the workers. This announcement should be made informally to each group of workers.*

Keep in mind that this is a training program which will end on <*insert day and date*>. After <*insert day*>, there will be no more work here. We would like to give this training to as many people as possible, so our policy is to train each person only once. Your wage is also final, and will remain the same till the end of the training program. Do you have any questions?

**Day 34 of employment: Pre-lab games Script.** *This script should be delivered by the worksite manager to all workers on the day prior to the last day of employment at the worksite.*

Tomorrow is the last day of this training program. As you know, after tomorrow, you will not get an opportunity to work with us again. Tomorrow, in addition to your daily wages, you will have an opportunity to earn some extra money at piece rates, so it is very important for you to come and complete your training as well as to collect your final wage payments.

**Day 35 of employment: Lab Games Script.** *This script should be delivered by the worksite manager to all workers on the last day of employment at the worksite.*

Today is the last day of the training program, so we have decided to pause work. Instead, we want you to complete three new activities this morning. In completing these activities, you will have the chance to earn extra money in addition to your daily wage. We will tell you how much extra you have earned at the end of the day.

The three different activities are called 1) Picture Game, 2) Symbols Game, and 3) Tower Building Game.

You will play a total of 9 rounds across the different activities. From the 9 rounds, we will randomly pick 1 round, and we will only pay you the amount that you earned for that particular round. We will not tell you which round we will pick. We will tell you how much you earned at the end of the day when making the final payment.

We will first explain how each activity is done. We will then give you some time to practice before we start. All of you will be given a unique ID. Using these ID numbers, we will pair you with another worker. The person you are paired with will be your practice partner for the first 2 activities. When you practice an activity, you should be in your assigned seat with your practice partner. After the practice round, we will pair you with a different worker with whom you will do the actual activity.

*Supervisors should hand out Lab ID cards to workers and show them to their assigned practice seats.*

*Anytime workers are being given instructions, they should be in their assigned seats, next to their practice partner. All practice exercises will be completed in these assigned seats. If there is an odd number of workers, a unit supervisor should complete the practice exercises with the worker who does not have a partner.*

*Game 1: Pictures*

*Hand out the example sheets to the practice pairs. Each member of the pair should have a different sheet in the set.*

We will start by explaining the picture game, which you will play in pairs. We will first teach you how this game is played. You will work with your practice partner who is seated next to you. In this game, you and your partner will receive two pictures. The two pictures are almost the same, but there are some small differences. The goal for you and your partner is to find anything that is different between the two pictures, and to indicate those differences on your sheets. You should circle the part of the picture that is different. It is important to only circle parts that are different across the two sheets – any other parts that are circled incorrectly will lead to a lower payment at the end of the game.

Each of you should circle the part that is different on your own sheet. You cannot circle items on your partner's sheet, and neither can he circle items on your sheet. If you and your partner are found to be doing that, you will both be penalized. Do you have any questions?

I will now give you 2 minutes to work with your partner to circle all of the differences in the practice pictures. Please begin now.

*Workers should work on the practice example in their assigned practice pairs. Supervisors should rotate between the pairs and provide guidance whenever needed.*

Now let's talk about the differences that you and your partner found.

*The manager should lead an interactive discussion with the workers in order to ensure that everyone has understood the game. Supervisors should rotate and verify that both members of each pair have circled the differences on their respective sheets.*

You will play 4 rounds of this game. Each round will be played with a different partner. If any of these rounds are selected for payment, you will earn Rs. 2 for each correct difference that is circled on both partners' sheets, and you will lose Rs. 2 for both incorrect answers and for answers that are only circled on one of the two sheets.

You will have 3 minutes per round. You will work on one set of pictures for the first 1.5 minutes and another set of pictures for the next 1.5 minutes.

We will now assign you to the partners with whom you will play the first round. We have labeled 15 different areas of the worksite. Each pair will work in one of these areas. For each area, I will call out the ID numbers of the workers who are assigned there.

If your partner is absent, you will not play this round. Instead, we will give you a fixed compensation of Rs. 10 if this round is selected for payment.

*The manager announces the first pairing. Once all pairs are seated in the designated seating areas, supervisors begin to hand out the first set of pictures. They should place the*

*sheets face down in the masking tape box and instruct workers not to turn the sheets over until they are told to do so. They should also be told that after 3 minutes, time will be called and they should return to their assigned practice seats, leaving the completed sheets in the masking tape box.*

*After the first round concludes, the manager announces the second pairing. During this time, unit supervisors should collect the completed first set of sheets and store them in a folder. Repeat. Once four rounds are completed, workers return to their assigned practice seats for Game 2.*

*Game 2: Symbols*

*Hand out example 1 sheets to the practice pairs. Each member of the pair should have a different sheet in the set.*

We will now explain the symbols game, which you will play in pairs. In this game, you and your partner will receive two sheets. On each sheet, there is a grid with different symbols. The sheet that you have received is different from your partner's sheet. What this means is that in some boxes within the grid, the symbol on your sheet and your partner's sheet is the same, whereas in other boxes within the grid, the symbol on your sheet and your partner's sheet is different.

The goal for you and your partner is to circle all the symbols that are in exactly the same position within the grids in both your sheets. Whenever a common symbol is found in your grids, both you and your partner should circle the symbol on your sheets. It is important to only circle the symbols that are exactly the same across the two sheets – any other symbols that are circled incorrectly will lead to a lower payment at the end of the game.

Each of you should circle the common symbol on your own sheet. You cannot circle items on your partner's sheet, and neither can he circle items on your sheet. If you and your partner are found to be doing that, you will both be penalized. Do you have any questions?

I will now give you 2 minutes to work with your partner to circle all the identical symbols in the practice sheets. Please begin now.

*Workers should work on the practice example in the assigned practice pairs. Supervisors should rotate between the pairs and provide guidance whenever needed.*

Now let's talk about the similar symbols that you and your partner found.

*The manager should lead an interactive discussion with the workers in order to ensure that everyone has understood the game. Supervisors should rotate and verify that both members of each pair have circled the similar symbols on their respective sheets.*

Now let's do another practice example.

*Hand out example 2 sheets to the practice pairs. Each member of the pair should have a different sheet in the set.*

I will now give you 3 minutes to work with your partner to circle all the identical symbols in the practice sheets. Please begin now.

*Workers should work on the practice example in the assigned practice pairs. Supervisors should rotate between the pairs and provide guidance whenever needed.*

Now let's talk about the similar symbols that you and your partner found.

*The manager should lead an interactive discussion with the workers in order to ensure that everyone has understood the game. Supervisors should rotate and verify that both members of each pair have circled the similar symbols on their respective sheets.*

Like before, you will play 4 rounds of this exercise, each with a different partner. If any of these rounds are selected for payment, you will earn Rs. 2 for each correct symbol that is circled on both partners' sheets, and you will lose Rs. 2 for both incorrect answers and for answers that are only circled on one of the two sheets.

You will have 6 minutes per round. You will work on the first set of symbols for the first 3 minutes, and another set of symbols for the next 3 minutes.

We will now assign you to the partners with whom you will play the first round. Just like before, I will call out the ID numbers of the workers who are assigned to each area.

If your partner is absent, you will not play this round. Instead, we will give you a fixed compensation of Rs. 10 if this round is selected for payment.

*The manager announces the first pairing. Once all pairs are seated in the designated seating areas, supervisors begin to hand out the first set of symbols. They should place the sheets face down in the masking tape box and instruct workers not to turn the sheets over until they are told to do so. They should also be told that after 6 minutes, time will be called and they should return to their assigned practice seats, leaving the completed sheets in the masking tape box.*

*After the first round concludes, the manager announces the second pairing. During this time, unit supervisors should collect the completed first set of sheets and store them in a folder. Repeat. Once four rounds are completed, workers return to their assigned practice seats for Game 3.*

*Game 3: Tower Building*

*Supervisors should set up 10 stations with the building kit. The workers are instructed to sit with their co-workers from their original product units.*

This is the last game that we are going to play this morning. You will play this game with the two co-workers from your product unit.

In this game, you will be given some materials e.g. cups, pencils, rubber bands and cards, which you can use to build a tower. The goal is for you and your co-workers to build a tower as tall as possible. You should work together with your co-workers to build this tower. You will have 25 minutes to complete this task. You are free to stop working on the tower at any point, and should let us know when your team is done. Once your team is done, we will measure the height of your tower. If this game is selected for payment, you will each earn Rs. 15 for every foot.

## B.3. **Figures.**

FIGURE 6. Recruitment Flyer

**Laborers Needed**

- Production of incense sticks, papad, leaf brooms, paper bags etc.
- No experience required
- Guaranteed employment for 1 month
- Work will be from Monday to Friday, from 9 am to 5 pm, at _____
- Daily wage of Rs.250 per day, with lunch
- Daily wage will increase after a training period

FIGURE 7. Individual Calendar

| **Participant Name:** | | | **Production Unit:** | | | |
|---|---|---|---|---|---|---|
| **Supervisor Name:** | | | | | | |
| **Monday** | **Tuesday** | **Wednesday** | **Thursday** | **Friday** | **Saturday** | **Sunday** |
| **Day 1**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 2**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 3**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 4**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 5**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | | |
| **Day 6**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 7**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 8**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 9**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | **Day 10**<br>Pre-fill Calendar Date<br>Pre-fill Training Wage | | |
| **Day 11**<br>**End of Training Period**<br>Pre-fill Calendar Date | **Day 12**<br>Pre-fill Calendar Date | **Day 13**<br>Pre-fill Calendar Date | **Day 14**<br>Pre-fill Calendar Date | **Day 15**<br>Pre-fill Calendar Date | | |
| **Day 16**<br>Pre-fill Calendar Date | **Day 17**<br>Pre-fill Calendar Date | **Day 18**<br>Pre-fill Calendar Date | **Day 19**<br>Pre-fill Calendar Date | **Day 20**<br>Pre-fill Calendar Date | | |
| **Day 21**<br>Pre-fill Calendar Date | **Day 22**<br>Pre-fill Calendar Date | **Day 23**<br>Pre-fill Calendar Date | **Day 24**<br>Pre-fill Calendar Date | **Day 25**<br>**End of Training Program**<br>Pre-fill Calendar Date | | |