EMPLOYER QUALITY AND INFORMATION FRICTIONS IN INFORMAL LABOR MARKETS*

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Abstract: Informal labor markets in developing countries are infamous for poor working conditions, ranging from delayed payments to verbal abuse. Why do poor quality employers persist in informal labor markets? This paper studies the role of information frictions in sustaining low quality employment relationships in spot labor markets in India. Using field experiments with 431 casual construction workers in Chennai, I first show that workers place substantial value on employer quality, including respectful treatment, timely payment, and not being cheated. Workers demand an average premium of INR 352, approximately 32 percent of daily wages, to work for a lower-rated employer, despite facing a meaningful risk of not finding work on a given day. I then demonstrate that information about employer misconduct diffuses poorly among workers in this tightly knit labor market where workers and employers meet everyday. I find that negative employer experiences are significantly less likely to be shared than positive ones, particularly when the identity of the person sharing information is revealed. Mechanism experiments offer suggestive evidence that fear of employer retaliation and concerns about the signal strength of individual experiences may play a role in limiting the transmission of negative information. These findings demonstrate the limitation of reputation as a disciplining mechanism for employer behavior even when formal enforcement is absent.

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1. Introduction

Informal work in developing countries is notorious for poor working conditions for workers (Blattman and Dercon, 2018). Workers in this sector often experience delayed payments, verbal abuse, and disrespectful treatment from employers (International Labour Organization, 2016; Breza and Kaur, 2025; Kapoor, 2025). Unlike formal employment where relationships are governed by written contracts, legal protections, and regulatory oversight, workers in informal markets have limited recourse when employers behave poorly. However, repeated interactions between employers and workers could potentially discipline employer misconduct through the threat of reputational gains or losses, even in the absence of formal, written contracts (Klein and Leffler, 1981; Shapiro, 1983; Karpoff, 2012).

How does employer reputation develop in informal labor markets in developing countries? For employer reputation to discipline bad behavior in the labor market, at least two conditions need to hold. First, workers must value employer quality enough to act on differences across employers, for example by refusing work or requiring higher wages to work for a lower-quality employer. Second, sufficiently credible information about employer conduct must circulate among workers. If workers do not value employer quality, or if information about misconduct does not reach potential employees, poor treatment will impose few reputational costs on employers (Annan, 2025).

The goal of this paper is to understand employer reputation building in the context of a large, informal labor market: urban spot labor markets in India. To do this, I ask three questions. First, are workers in low-income environments willing to forgo wages for better workplace experiences such as respect, timely payment, and fair treatment? This question speaks to whether non-wage amenities matter enough for workers that employer reputation could carry market consequences. Next, if this is true, I ask if asymmetric information sharing inhibits reputation building. Specifically, I ask if workers are less likely to share their own personal, negative experiences about employer behavior than positive experiences, creating systematic biases in the information that reaches the market. Third, what mechanisms drive these asymmetries in information sharing?

To answer these questions, I conduct a field experiment with 431 male construction workers across 13 informal labor markets in India where employers and workers meet for spot contracts. At baseline, workers rate employers with whom they regularly match at the stand across eight dimensions of employer quality covering both wage-related and non-wage amenities. These are used to construct an aggregate measure of employer quality[1], and it captures workplace experiences such as being treated

[1]The wage-related items are timely payment, feeling cheated at the workplace, payment of the correct wage, and

with respect and being paid on time. To measure their valuation of employer quality, workers are then offered incentive-compatible job choices between higher- and lower-rated employers at varying wage rates. Workers demand INR 352, roughly 32 percent of daily wages, as a premium to work with a lower-rated employer. This is substantial in a setting where workers face meaningful risk of not finding work on a given day. However, workers do not demand systematically higher premia as the rating gap between employers widens, suggesting that the valuation may be driven more by avoiding worse employers than by valuing marginal improvements in ratings.

If workers place substantial value on employer quality, then why does poor employer behavior persist? One possibility is that information about employer quality does not circulate efficiently in this market. To test this, I conduct an information sharing experiment in which workers are asked whether they are willing to share information about an employer with a peer at their stand. To understand whether information about employer conduct flows differently when the information is positive or negative, I randomize the valence of employer conduct. This is cross-randomized with whether the worker's identity is revealed or not when the information is shared to further examine the conditions under which workers are willing to share information about an employer. Nearly all workers (98.1 percent) agree to share positive information when their identity is concealed. Under the same anonymity condition, willingness to share falls to 88 percent when the information is negative. When negative information is instead linked to the worker's identity, sharing declines further, to 66.8 percent. Thus, negative experiences circulate less even under anonymity, and workers become especially reluctant to share them when the negative information can be attributed to them personally. This asymmetry limits the information available to other workers in the labor market and weakens the reputational consequences employers face for poor treatment.

There are a number of reasons why this could happen in a labor market, and I test a few channels that could drive this asymmetric flow of information. I test three mechanisms through experiments, and provide suggestive evidence on the remaining mechanisms. The first mechanism I explore is the threat of employer retaliation. In the above experiment, I observe that identity revelation reduces information sharing even when the information is positive, lowering the probability of sharing by 10.1 percentage points. However, identity revelation has a substantially larger effect when the information is negative. The combined estimates imply that revealing a worker's identity reduces the probability of sharing negative information by 21.2 percentage points. Although not definitive, this pattern is consistent with worry about retaliation given the reluctance to be personally associated with negative information about employers.

extra pay for overtime work. The non-wage items are respectful communication, micromanagement, feeling respected at the workplace, and ever being verbally abused.

In the next experiment, I test whether workers are reluctant to share negative information because they view their own experience as a noisy or subjective signal of employer quality. To do this, I compare workers' willingness to share negative information about their own experience with their willingness to share aggregate information about an employer. Again, this is cross-randomized with whether workers' identity is revealed or not. Workers are significantly less willing to share personal negative information than aggregate employer information. In contrast, identity revelation has a small and statistically insignificant effect for aggregate information. Although the experiment cannot disentangle concerns about signal credibility from the threat of employer retaliation, the results suggest that aggregating information across workers may facilitate disclosure by reducing the extent to which negative information on employer behavior is attributed to any one worker.

Preliminary exploratory and qualitative work suggested another potential mechanism: workers may withhold negative information because they do not want to discourage economically vulnerable peers from accepting available work at the stand. Though counter-intuitive, I decided to test for this in the next experiment. Here I randomize whether the peer receiving the information is described as having recently experienced an economic shock. This is cross-randomized with workers' own experiences with a good or bad employer at the stand. I find little support for this mechanism. Negative information substantially reduces information sharing, but this effect is essentially unchanged when the peer is described as having experienced a shock. Thus, workers are not more or less likely to share negative information when the recipient may especially benefit from avoiding a bad employer.

I then examine alternative explanations. First, asymmetric information sharing does not appear to be driven by simple knowledge hoarding on good employers by workers. I find that workers who report hiding information about good employers are not systematically less willing to share information across all three experiments. Nor does it appear to reflect self-image concerns, since treatment effects do not differ between workers who say that being treated badly signals that they are a bad worker and those who do not. Another natural concern is whether these results are explained by general discomfort with being observed, since identity revelation has a much larger effect for negative than for positive information. I argue that if these results were capturing general discomfort, then I would not observe differential sharing of negative and positive information. I also observe high levels of information sharing when aggregated information is shared and worker identity is revealed. Finally, the reluctance to share negative information is just as large among workers who score high on a social desirability scale, who should be most inclined to give the answer they think is expected.

To summarize, my evidence primarily points to two main frictions that restrict workers' willingness

to share negative information on employer behavior: concerns about employer retaliation when negative reports are identifiable, and reluctance to share personal negative experiences that workers may perceive as weak or noisy signals of employer quality. Both suggest a role for third-party aggregation of employer quality, and workers do value it. When offered the opportunity to purchase employer "scorecards" that aggregate ratings from multiple workers at a stand, the average worker is willing to pay INR 27 out of an INR 100 endowment.

Next I turn to employers to understand the other side of this labor market. Survey evidence from 132 employers at the sample labor stands provides context for why employer quality may not be fully reflected in wages or behavior. Employers appear to understand that reputation matters: most report that reputation affects how many workers are willing to work with a mason. However, employers may misperceive what workers value. Workers' responses indicate that a good employer is defined not only by timely payment but also by respectful treatment: approximately 60 percent identify both being paid on time and being treated with respect as important. Employers place markedly different weights on these attributes. While 80 percent report that workers value timely payment, only 14 percent identify respect as important to workers. This contrast suggests that employers recognize workers' concerns about pay but substantially underestimate the value workers place on interpersonal, non-wage dimensions of job quality. To further examine employers' beliefs, I conduct a small exercise with an additional sample of 52 employers. Employers are asked to guess how many out of 100 workers would choose a better-rated employer even if that employer offered lower pay. Responses were incentivized for accuracy. Employers' beliefs are highly dispersed, ranging from 10% to 100%, with many guessing that only about half of workers would make this choice. In contrast, the actual distribution of workers' willingness to forgo wages for a better-rated employer is concentrated at higher values, i.e., between 65 to 100 percent. This suggests that employers may underestimate the extent to which workers value employer quality relative to wages.

Finally, improving the quality of workplace interactions may not be costless for employers. Employers generally do not report that raising one's voice is necessary to increase worker productivity. However, many report that close supervision and clear communication are important for ensuring that workers are productive. This distinction suggests that employers do not view all dimensions of workplace treatment in the same way. Some forms of poor treatment, such as verbal abuse, may be seen as unnecessary, while other management practices may be viewed as involving productivity tradeoffs.

This paper contributes to three sets of literature. First, it adds to the vast literature on characterizing willingness to pay for non-wage amenities at the workplace (Sorkin, 2018; Maestas et al., 2023;

Mas, 2025). Prior work in developed economies has shown that workers hold a willingness to pay to avoid employer-set schedules (Mas and Pallais, 2017), receive higher earnings growth (Wiswall and Zafar, 2018), and have a flexible work arrangement (Chen et al., 2019). In developing economies, there is evidence on non-wage attributes valued by women like co-working with a spouse (Tandon, 2025), need for flexible jobs (Ho et al., 2024), safety concerns (Buchmann et al., 2026), and commuting with a spouse or friends (Le Barbanchon et al., 2021; Garlick et al., 2025). I contribute to this literature by identifying and valuing an understudied dimension of job quality: how workers are treated by their employers. Existing research has largely focused on more observable job attributes, such as scheduling, flexibility, and commuting. It is not obvious ex ante that interpersonal aspects of employment, like respectful communication and fair treatment, would be similarly salient, particularly in labor markets where earnings are low and uncertain. I examine these attributes and elicit workers' willingness to pay for employer quality, where ratings capture both interpersonal treatment and reliable wage practices. I find that workers are willing to forgo substantial earnings to work for a better-rated employer. To my knowledge, this is the first incentive-compatible evidence from a low-income informal labor market that workers attach substantial monetary value to interpersonal treatment at work. The findings therefore broaden the conventional conception of job quality: even under severe economic constraints, workers evaluate employment relationships along dimensions that extend well beyond wages.

Second, I contribute to research on information frictions in labor markets in developing countries. Prior work documents frictions in the diffusion of information about job opportunities (Jensen, 2012; Oster and Steinberg, 2013; Dammert et al., 2013; Beam, 2016), difficulties employers face in learning about worker quality (Abebe et al., 2021; Bassi and Nansamba, 2021; Carranza et al., 2022), and inefficient search arising from workers' biased beliefs about job finding prospects and match quality (Conlon et al., 2018; Bandiera et al., 2025; Chakravorty et al., 2024). I study a distinct friction in the production of labor market information: workers may possess valuable information about employer quality but choose not to share it with others, particularly when that information is negative. This selective transmission can create a persistent rather than temporary information problem. Experiences that are never shared cannot enter other workers' beliefs, forcing them to learn about employer quality through their own potentially costly employment experiences. Consequently, repeated interaction need not cause the market to converge toward full information, even within a close-knit labor market. This mechanism complements Sockin and Sojourner (2023), who show that negative employer information is particularly difficult to elicit even when highly valued, and Boudreau et al. (2024), who show that inexperienced migrants are poorly informed about working conditions but learn through industry experience. My findings show that experience

alone may not resolve information frictions when workers are reluctant to share what they learn. I further identify two barriers to this information sharing – threat of retaliation through identity disclosure and concerns that individual experiences provide weak signals of employer quality. Without institutions that protect workers or credibly aggregate their experiences, these frictions may persist, producing costly employer-worker matches.

Third, I contribute to research on reputation as a self-disciplining mechanism. Work on relational contracting shows how the value of future business can sustain cooperation when formal contracts are weak or unenforceable (Macchiavello and Morjaria, 2015, 2021). A related literature studies how the disclosure of quality information affects market behavior, showing that credible ratings can alter consumer choices and induce providers or intermediaries to adjust prices, quality, and conduct (Dranove and Jin, 2010; Andrabi et al., 2017; Fernando and Singh, 2026). Research on misconduct similarly demonstrates that firms may face substantial penalties when their behavior becomes known, although the strength of these reputational consequences depends on the setting and on whether misconduct harms market counterparties (Karpoff, 2012; Annan, 2025). These studies largely examine how market participants respond once information becomes available. A smaller literature studies the supply of such information, mostly on online platforms (Bolton et al., 2013; Sockin and Sojourner, 2023). I study both stages in an informal labor market with no rating infrastructure. Workers value employer quality, but negative experiences are shared less than positive ones particularly when the person sharing information can be identified. This suggests a limit of reputation-based enforcement: repeated interaction need not discipline employers if the information needed to penalize misconduct does not circulate among workers.

The remainder of the paper proceeds as follows. Section 2 describes urban spot labor markets in India. Section 3 presents a simple conceptual framework and the experimental design, and Section 4 describes the data. Section 5.1 presents results on worker preferences for employer quality. Section 5.2 examines information frictions and their mechanisms. Section 6 presents evidence on worker demand for and use of aggregated employer information, and Section 7 documents the employer side of the market. Section 8 concludes.

2. Context

Labor stands are public spaces where workers can meet employers for short-term contractual work, and are ubiquitous in developing countries. These labor stands employ a large number of the world's informal workers. In this study's context, labor stands largely engage Tamil-speaking men in the

construction industry. This industry currently employs 57.7 million unskilled workers in India (Knight Frank and RICS, 2023). Workers show up at the stands early in the morning until 10 a.m., which allows the 'helpers' (employee) to work with the 'masons' (employer) on mostly short-term contracts. Most contracts in this market are one day long.

There exists a prevailing wage rate in the stands, which increases annually. Although there is some variation in the wages experienced by workers, there is a consensus among the workers on the prevailing wage at each stand (Figure 3). Every morning, workers arrive at the labor stand and wait for an employer to show up with a job. Employers show up at the stand with a job offer at a geographical location. Workers and employers commit to a verbal contract after agreeing on wages, work expectations, amenities, and expected work hours[2].

Repeated interaction is an important feature of this market. Workers in the study were eligible only if they regularly attended the labor stand and expected to continue attending it over the following month, which was necessary because study activities were conducted over approximately 30 days at each stand. Employer ratings were likewise restricted to employers who regularly visited the respondent's stand and with whom workers had prior experience. Thus, although most employment contracts last only one day, workers and employers participate repeatedly in the same geographically concentrated market and may encounter one another across multiple days.

These labor stands also provide a setting where it is fairly easy to observe workers' willingness to supply labor, employers' demand for labor, real time daily labor market matches, wages, and other contract details and provision of job amenities. Importantly, this is also a context in which respectful treatment and being paid on time are valued (Figure 1a). When asked who is a 'good' employer, 50% of the participants said that the employer should talk nicely to them, and 60% want to be treated with respect at their workplace. To put this in comparison with other workplace amenities, despite poor workplace safety in this setting, only 10% wanted more safety practices at their workplace. These features make labor stands a useful setting in which to study reputation based enforcement. Repeated participation creates the possibility that employer conduct will affect future hiring, while limited union involvement in routine contracts makes the circulation of information among workers particularly consequential.

[2]Worker unions are present at most labor stands, but their role in governing everyday employment relationships is limited. In particular, unions generally do not bargain over daily wages or intervene in the terms of individual day-labor contracts. 30% of workers reported that unions helped secure compensation from employers following workplace accidents; and 40% reported receiving no assistance from unions at all.

3. Research Design

3.1. Conceptual Framework and Hypotheses

This subsection lays out a simple framework under which we can understand the hypotheses, experiments, and results of this paper. The goal here is to not write a new theory of reputation or misconduct in labor markets. I begin by stating the conditions that should hold true if reputation can discipline employer behavior. First, workers must value employer quality, so that information about an employer changes their willingness to work for him. Second, workers' experiences must be informative about underlying quality. Third, these experiences must reach other workers.

Decision to work. In period 1, workers i and employers j meet at a labor stand. An employer is either good or bad, measured by $q \in \{0, 1\}$. A worker is offered the prevailing wage w by an employer he believes is bad with probability p. He accepts the jobs if

$$w - \theta p \geq \bar{u}, \tag{1}$$

where $\theta \geq 0$ is the cost to the worker of a day with a bad employer, like being spoken to badly, paid late, or cheated, and $\bar{u}$ is his outside option. Workers may differ in their valuation of θ. The lowest wage the worker will accept, $\bar{u} + \theta p$, rises with the probability that the employer is bad, and the premium an employer believed bad with probability p^L must pay relative to one believed bad with probability $p^H < p^L$ is

$$\Delta w = \theta\,(p^L - p^H). \tag{2}$$

Learning about employers. After a day of work the worker has a good or bad experience, $s \in \{G, B\}$. The experience is an imperfect signal of the employer: a good employer produces a good experience, and a bad employer a bad one. Starting from a belief p that the employer is bad, a bad experience raises the belief to $p_B > p$ and a good experience lowers it to $p_G < p$, and both posteriors move further from p.

Decision to share. The worker now decides whether to tell a peer at the stand about his experience.

He shares if the benefit of sharing information exceeds the cost of information sharing,

$$b_s \geq c_s + \varepsilon, \tag{3}$$

where $\varepsilon \geq 0$ is distributed across workers with c.d.f. F, so that the fraction of workers who share an experience of type s is

$$\tau_s = F(b_s - c_s). \tag{4}$$

The benefit b_s is the value of the report to the peer. The cost c_s has various components like social or self-image costs, employer retaliation, and increasing the competition for good employers. Some of these costs arise only when the information can be traced back to the worker. Therefore, I try to separate out the costs associated with negative information and when the worker can be associated with that information through experiments. Whether negative information is shared less than positive information under anonymity is not predicted by the model, rather it depends on whether the cost of negative information, $c_B - c_G$, exceeds the extra benefit of sharing that information, $b_B - b_G$. I check for these conditions next.

Hypotheses

The framework yields the following hypotheses, which organize the experiments in Section 3 and the results in Section 5. The first two concern the conditions for reputation to operate; the remaining three concern the mechanisms behind the information friction.

H1. Valuation of employer quality. On average, workers demand a positive compensating wage differential to accept a job with a lower-rated rather than a higher-rated employer: $\mathbb{E}[\Delta w_i] > 0$ in Equation (2).

H2. Asymmetric information sharing. Workers are less likely to share negative than positive experiences about employer behavior.

H3. Costs of identifiability. Revealing the identity of the worker reduces the sharing of negative experiences by more than it reduces the sharing of positive experiences.

H4. Individual experiences as weak signals. Workers are less likely to share negative information based on their own experience than information aggregated across workers.

H5. Concern for recipients' employment. Relative to positive information, workers are less likely to share negative information with a peer who has recently experienced an economic shock.

3.2. Implementation and Survey Protocols

Data were collected through a multi-stage field protocol implemented at 13 urban construction labor stands in Chennai, India. Each stand was visited for approximately 30 days during which workers were recruited, surveyed, randomized into different experimental tasks, and finally experiments were executed. Figure 2 summarizes the timeline of data collection at each stand.

Enumerators recruited workers directly at labor stands where workers typically wait to be hired for the day. Interested workers were then screened for eligibility for participating in the study. To participate, workers had to: (i) be between 18 and 60 years old, (ii) be fluent in Tamil, (iii) work as a helper on construction sites (unskilled laborer) [3], (iv) intend to regularly attend the stand for the next 30 days, and (v) report no physical disability that would prevent participation.

I then conducted two rounds of baseline surveys. The first baseline survey collected detailed demographic and socioeconomic information, including age, education, household composition, dependents, spousal employment, debt, expenses, financial stress, and banking access. The survey also included measures related to social desirability (Reynolds, 1982; Dhar et al., 2022), motivation, and grit. The second baseline survey focused on labor market histories and employer characteristics. This survey asked detailed information on previous employment relationships, daily wages, union presence, social networks on the stand, and employer ratings with whom they had previously worked. Workers were asked to rate regular employers at the stand on thirteen dimensions namely: timely payment, respectful communication, provision of meals, provision of transportation, feelings of respect at the workplace, safety provisions, payment of correct wages, provision of a cooler at the job site, provision of a pitcher of water at the job site, overtime compensation, micromanagement, ever verbally abused, and feelings of being cheated at the workplace. A long list of non-wage amenities was furnished to parse out different dimensions of employer quality, and finally eight amenities were included in the aggregate employer rating.

After completion of the second baseline survey, workers were randomly assigned within labor stand to different experimental treatment arms (as explained in the next section). Randomization occurred

[3] A helper is a standard occupational category in Chennai's construction labor market, distinct from skilled work such as masonry, carpentry, and plumbing. Helpers in this market perform general manual tasks, including carrying materials, mixing cement, clearing work areas, and assisting skilled workers. Restricting the sample to helpers increases comparability in job responsibilities and prevailing wages across participants.

at the individual level, and was stratified at the stand level. Soon after, workers participated in an endline survey that collected information on recent employment outcomes, beliefs, and attitudes. At this stage, workers were also offered the opportunity to purchase an employer scorecard aggregating employer ratings (on employers on whom I had at least two different sets of ratings) collected during the study. The participants received INR 100 (1.2 USD) in cash or the employer scorecard, depending on their choices in the valuation task.

To ensure incentive compatibility, participants were informed that a randomly selected decision could be implemented during a subsequent execution phase. Enumerators returned to each labor stand for an additional 2 days to implement the experiments. For the job choice experiment, a random subset of the sample was offered jobs with employers at a wage rate that was randomly selected from their choice set. For the information sharing experiment, each worker was randomly selected to execute on one experiment. Workers who failed to comply with the assigned information sharing task did not receive their participation compensation of INR 100 for that survey round.

3.3. Research Design and Predictions

H1. On average, workers require a positive compensating wage differential to accept a job with a lower-rated employer rather than a higher-rated employer.

During baseline surveys, I ask workers to rate employers they have worked with the past on eight dimensions. These were a mix of wage related amenities like timely payment, felt cheated at the workplace, pays correct wages, and pays extra wages for overtime work, and non-wage related amenities like respectful communication, micromanagement, feelings of respect at the workplace, and ever verbally abused. I then create an aggregate score for employers on whom I have at least two sets of ratings. I then offer unemployed workers at the stand a menu of contracts where they choose to work with relatively higher-rated and lower-rated employers with different wage rates. These are two employers that the worker has not worked with in the past, and I do not reveal the names of the employers at this stage.

In the first experiment, I keep the wage of the higher-rated employer constant, and gradually increase the wage of the lower-rated employer. (Table 1)

Table 1: Job Choice Experiment (Version 1)

	'higher-rated' employer	'lower-rated' employer
Option 1	1000	1000
Option 2	1000	1020
Option 3	1000	1040
...	...	...

Notes: This table illustrates the first job choice experiment. Workers choose between a higher-rated and a lower-rated employer while the wage offered by the lower-rated employer increases across choices. Employer names are not revealed at the time of choice.

I also include a second experiment where I keep the wage of the lower-rated employer fixed, and gradually decrease the wage of the higher-rated employer. I randomize the order in which the participant receives these two job choice experiments.

Once worker preferences are established, I examine the flow of information about employer quality within labor stands. Before participants were asked to participate in the next set of experiments, they were asked to do two activities. First, they were asked to participate in a demonstration of the experiment where they makd a choice and were asked to follow through on the choice. Participants were asked to select a friend at the stand (if they could not identify a friend at the stand, they were matched with any worker in the vicinity by the enumerator), and were asked whether they would be willing to share an attribute they do not like about living in Chennai. I then inform them if their name is picked in a lottery, they will have to execute on their choice. Otherwise, they lose 50% of their survey compensation to ensure real stakes were involved. 80.4% of participants agreed to share this information. I execute the choice with almost all participants (to avoid deception), and 97.8% of participants who previously responded yes to sharing information followed through on their choice.

Second, all workers are asked to recall both a positive and a negative experience with an employer at the stand, and this information is noted by the enumerator. Following this, the information sharing experiments are conducted:

H2. Workers are less likely to share negative experiences about employer behavior than positive experiences.

To test this hypothesis, workers were randomly assigned to share a positive or negative experience with an employer with a peer outside of their social group at the labor stand, stratified at the stand

level. For respondents who say no, I ask them to indicate their willingness to accept payment to share this information. The experimental setting was cross-randomized along another dimension: first the personal information of the worker is kept anonymous, and second the personal information of the worker is revealed (Table 2). I posit that negative employer experiences will be less frequently shared than positive employer experiences, i.e., $D < C$.

Table 2: Asymmetric Information Sharing Experiment

	Positive Information	Negative Information
Anonymous	A	B
Non-anonymous	C	D

Notes: This table shows the 2x2 design for the first information-sharing experiment. Workers are randomized into positive versus negative information and anonymous versus non-anonymous sharing conditions.

Next, I test the mechanisms behind asymmetric flow of information at the stand:

H3. Revealing the identity of the worker providing information reduces the sharing of negative employer experiences more than it reduces the sharing of positive employer experiences.

This hypothesis examines the role of anonymity in information sharing under the experiment under H2. I use the cross-randomization with anonymity of the disclosing worker to test this hypothesis (see Table 2 for the full randomization design). If workers are reluctant to share negative experiences when their identity is revealed, it could suggest a fear of employer retaliation. Comparisons of willingness to share information under anonymous and non-anonymous conditions will provide evidence for this channel. Therefore, I hypothesize that information sharing under condition D will be less than information sharing under condition B, while observing no difference in information sharing under conditions A and B.

The design does not distinguish anticipated retaliation from other costs of being personally associated with a negative report about an employer, but it does rule out explanations in which identity matters only through a generic reluctance to be observed, since those predict a zero interaction.

H4. Workers are less likely to share negative information on employer behavior based on their own experience than employer information aggregated across multiple workers.

To explore this channel, I ask workers to share either share their own negative experiences with an employer, or aggregated information that I collect on an employer at the stand level. I conduct this

experiment under both anonymous and non-anonymous settings (see Table 3 for full randomization detail), and stratify the randomization at the stand level. I believe that if workers worry about their own signal being misinterpreted then they would be more willing to share aggregated information about an employer and not their own personal experiences ($C < D$).

Table 3: Weak Signal Experiment

	Negative information about person's own experience	Aggregate information about an employer
Anonymous	A	B
Non-anonymous	C	D

Notes: This table shows the 2x2 design for the weak signal experiment. Workers are randomized into sharing personal negative information versus aggregate employer information, crossed with anonymous versus non-anonymous sharing.

H5. Relative to positive information, workers are less likely to share negative employer information with a peer who has recently experienced an economic shock than with a peer who has not experienced such a shock.

To test this hypothesis, I randomize worker's positive versus negative experiences about employer behavior, and further vary whether the worker receiving the information has received an economic shock in the past one month (see Table 4 for the full randomization design). All conditions under this experiment are conducted under full disclosure of the participant's identity. The hypothesis predicts that 'paternalistic' motives may prevent workers from sharing negative information if it could discourage peers, particularly those in vulnerable situations, from pursuing available jobs.

Table 4: Paternalistic Motives Experiment

	Positive Information	Negative Information
Peer with economic shock	A	B
Peer with no information on economic shock	C	D

Notes: This table shows the 2x2 design for the peer economic shock experiment. The respondent's identity is revealed in all conditions; the experiment varies information valence and whether the recipient peer is described as having experienced an economic shock.

4. Data

4.1. Labor Stands

The study was conducted at 13 construction labor stands across Chennai, and the analysis sample comprises 431 male workers. The stands vary in size. On average, approximately 55 to 100 workers attend each stand everyday. Figure 14 shows mean attendance, the degree of daily variation at each study stand, and the number of workers from each stand included in the analysis sample. Recruitment targeted approximately equal numbers of workers across stands, ensuring adequate representation of smaller stands and preventing larger stands from dominating the sample.

4.2. Sample Balance and Covariates

I present summary statistics of the worker sample in Table 5. The average worker has been attending the stand for about 10 years. Only 7% of participants visit other labor stands in the city. 4% had taken debt from someone at the stand implying that participants do have financial relationships with other workers or employers at the stand. Lastly, 51% of participants own a bank account, and 74% own a mobile phone.

Balance. Tables 17–19 compare baseline characteristics across the four treatment arms of each information-sharing experiment. To test for balance across treatment arms, I regress the covariate on treatment indicators with stand fixed effects, and conduct a Wald test of joint significance of the regression coefficients.

I achieve balance across treatment arms in the first experiment: none of the baseline characteristics differs significantly across treatment arms at the 5 percent level. In the second experiment, the share of workers who recently experienced an economic shock differs across treatment arms ($p = 0.002$), while the remaining characteristics are generally balanced. In the third experiment, I find significant differences in birthplace outside Chennai, bank-account ownership, and alcohol consumption across treatment arms.

Because I randomly assigned participants to treatment, I interpret these as natural sampling variation. However, to test this rigorously, I report specifications both with and without stand fixed effects and baseline covariates. The treatment estimates remain similar across these specifications, indicating that the observed covariate imbalances do not drive the main results.

Attrition. Of the 638 workers surveyed during the initial screening, 431 were successfully re-

surveyed and included in the final sample, yielding a follow-up rate of 67.6 percent. The remaining 207 workers could not be located or re-surveyed. Table 16 compares baseline characteristics of workers who attrited with those retained in the final sample. Attrited and workers included in the final sample are broadly similar across the characteristics observed at screening.

Employer Ratings. At baseline, workers provided ratings of employers[4] they had worked with during the previous two weeks. Workers reported recall on an average of 1.10 employers during the previous two weeks, and conditional on reporting at least one employer, the average employer recall was 1.61 per worker. Figure 15 plots the distribution of average employer ratings across the eight dimensions included in the employer quality index, together with the distribution of the overall index. I code each measure so that higher values indicate better employer quality. Thus, higher ratings indicate that an employer treats workers with respect, speaks to them politely, avoids verbal abuse and excessive micromanagement, pays the agreed wage correctly and on time, compensates overtime work, and does not make workers feel cheated.

Workers rate most employers favorably. However, the distributions contain a meaningful lower tail, with some employers receiving markedly lower ratings for both payment practices and interpersonal treatment. Ratings for overtime compensation and micromanagement appear somewhat more dispersed than the other components. The overall employer quality index is a simple average of its eight components. Although the index remains concentrated toward the upper end of the scale, it spans nearly the full range and distinguishes employers within the relatively large group that workers generally rate favorably. This variation allows me to construct higher- and lower-rated employer pairs for the job-choice experiment.

5. Results

5.1. Demand for Employer Quality

The primary outcome of interest is the workers' willingness to accept a job at the given wage rate with an employer with a given rating. I calculate this in two ways. First, I increase the wage offered by the lower-rated employer while keep the wage of the higher-rated employer constant. Figure 4a shows that workers demand an average premium of INR 352 on their daily wages to accept work with lower-rated employers. This is about 32 percent of the median daily wage of INR 1100 (around

[4]Worker recall is restricted to regular employers at the stand, and with whom workers work with at the job site. This eliminates recruiters or hiring contractors at the stand.

12 USD), which is striking since wages are low in this market, and workers face daily income risk [5]. Yet even in this high-stakes environment, the value placed by workers on being treated respectfully and paid timely is substantial.

Second, I conduct a test of willingness to accept job offers with a a higher-rated employer at a wage below the wage floor. Figure 4b shows workers are willing go INR 139 below the prevailing wage rate at the stand, which is 12.6% of the median daily income. Several explanations could drive these two results. First, downward nominal wage rigidity in informal labor markets could explain workers' hesitancy to accept wages below the prevailing wage rate at the stand (Kaur, 2019). At the same time, workers could hold private beliefs of reservation wage below the wage floor, but do not publicly reveal it for fear of social sanctions or future bargaining motives. Breza et al. (2026) find that when labor supply among agricultural workers increases by 93% when job offers with a wage cut are offered in private versus when they are offered in public. Their results are driven by fear of social sanctions by other workers in the local labor market. In this study's setting, job offers were made privately, however workers were still surveyed at the labor stand with other peers in the vicinity. This could explain why workers' willingness to accept job offers with higher-rated employers at below the wage rate is lower than the premium demanded for working with lower-rated employers, but is still non-zero.

Workers' stated wage premia are weakly positively related to differences in employer ratings. Figure 5 shows the relationship between the premium demanded to work for the lower-rated employer and the difference in the average ratings between higher- and lower-rated employers, after accounting for stand-level variation. The relationship is positive and directionally consistent, but very weak with a correlation of 0.03. Therefore, workers demand higher premia when rating gaps exist (extensive margin), this premia does not go up when ratings gaps are larger (intensive margin).

A natural concern that then emerges is whether workers believed or understood the aggregated employer ratings. Before offering job offers, workers were asked whether they believe the ratings provided to them. 61% of workers said that they completely believed the rating, 36.6% of workers said that they 'mostly' or 'somewhat' believe this rating. Only 2 workers reported not believing this rating at all. Secondly, I ask workers what they believe the aggregate ratings represent. 78% of workers could accurately explain the meaning of an average rating. The surveyor team corrected the beliefs of the remaining participants who answered this question incorrectly. Therefore, if workers map ratings into beliefs through a threshold, for example, an employer is either "bad" or

[5] Cefala et al. (2026) report the job finding probability if a worker arrives by 8 a.m. at a labor stand is 77% and plummets significantly by 9.30 a.m.

"acceptable", then premia will respond to the existence of a rating gap but not to its size.

Lastly, I execute job offers with 113 workers who reported not having a job the morning of the survey date. Job offers were randomized with lower- and higher- rated employers with randomized wage offers compatible with workers' reported willingness to pay in the experiment. 60% of workers accepted the job offered to them. I argue that this job acceptance rate is reasonable in my setting [6]. Workers' probability to accept a job on a particular day depends on factors like their beliefs about outside options, or whether they are willing to work with an employer unknown to them. Therefore, I only capture the 'residual' labor supply to an employer, and not the total labor supplied at any given wage (Breza et al., 2026).

Finally, Figure 6 presents a binscatter plot for the job acceptance rate for different employer types and at different wage rates. It shows a separation in the labor supply curves for higher- and lower-rated employers. At a given job acceptance rate, workers demand a higher wage for lower-rated than higher-rated employers, however, the slope remains the same. I provide this graph as suggestive evidence on the premium demanded to work with a lower-rated employer, as I am underpowered to say anything meaningful beyond that.

5.2. Information Flow on Employer Quality

There could be a number of reasons why poor employer-worker interactions still exist in this market, given workers' valuation of employer quality. I investigate one such reason: asymmetric information flow on employer quality among workers in this market. To do this, I run an experiment to test Hypothesis 2 which is laid out in Table 2.

Table 6 estimates the following regression:

(5)

$$Y_{is} = \alpha + \beta_1 \text{Identity Revelation}_{is} + \beta_2 \text{Negative}_{is} + \beta_3 \left(\text{Identity Revelation}_{is} \times \text{Negative}_{is}\right) + X_i'\gamma + \delta_s + \varepsilon_{is}$$

where dependent variable Y_i is an indicator for whether respondent i shares information or not with a peer at stand s. Identity Revelation$_{is}$ is an indicator equal to one if the respondent's identity is revealed and zero if the participant's identity is kept anonymous. Negative$_i$ is an indicator equal to one if the information is negative and zero if it is positive. The coefficient β_1 measures the effect of identity revelation when the information is positive, while β_2 measures the effect of negative information under anonymity. The interaction coefficient β_3 captures whether the effect of negative

[6] Breza et al. (2026) offer jobs to agricultural workers, and report that on average 33% of workers accept jobs at prevailing rates publicly. This drops to 26% when all potential workers in a village are considered.

information differs when identity is revealed. The vector X_i' includes respondent demographic controls, and δ_s denotes stand-level fixed effects. The omitted category is the positive-information condition under anonymity, in which 98.1 percent of respondents agree to share information.

Across specifications in Table 6, both identity revelation and being asked to share negative information reduce information sharing with peers. Column 3 notes the preferred specification with stand fixed effects and demographic controls, where negative information lowers information sharing by 10.1 percentage points, and identity revelation lowers information sharing by 10.1 percentage points.

The interaction between identity revelation and negative information is also negative and statistically significant, implying that negative information is less likely to be shared when identity of the respondent is revealed. Comparing only the conditions where identity is revealed, the predicted sharing rate in the negative information condition is approximately 67 percent, compared with 87.6 percent in the positive condition (Figure 18). This is also true when we only compare the experimental settings where identity is hidden. Thus, participants are less likely to share negative experiences about employer behavior than positive ones, even when their identity is hidden.

One might worry that workers might have differential recall rates of negative and positive experiences at the workplace. To address this concern, I ask everyone to recall positive and negative experiences before their randomization is revealed by the surveyor. Appendix section A.4 provides details on positive and negative experiences shared by workers.

Incentivized elicitation. To make the information sharing decisions consequential, participants were informed that their decisions could be implemented with an assigned peer after the completion of survey activities at the stand. Participants were not informed of the exact implementation date, in order to limit selective attendance at the stand on that date. Overall, 95.6% of participants who agreed to share information with a peer at the stand followed through on their decision [7].

Making the sharing decisions consequential does not, by itself, eliminate the possibility that participants agreed to share because they wished to provide a socially desirable response or satisfy perceived experimenter expectations. As a complementary robustness check, I constructed a shortened social desirability scale (SDS) based on Crowne and Marlowe (1960), and implement the

[7] This included all experiments under Tables 2, 3, and 4. One experiment, chosen at random, was executed per participant. Participants who had agreed to share but subsequently declined to implement their selected decision received a deduction in their financial payout for the survey round. Participants who were randomized into the anonymous arms and agreed to share information, a member of the survey team shared their employer experience on their behalf, and the participant could verify later if needed. Participants who were randomized into the non-anonymous arm, they shared information with their allocated peer themselves while a member of survey team listened in.

module during baseline survey. Table 20 examines heterogeneity in the treatment effects by whether a participant's social desirability score is above the sample median. While the main results hold, workers who score high on the social desirability score thus readily share positive information under their own name, but are no more willing than others to put their name to negative information.

These results have two implications. First, the reluctance to share negative information is not confined to workers with low social desirability scores. It is large among workers with high scores, who should be the most inclined to give the answer they think is expected. Second, workers who score higher on the SDS score share positive information more freely when identified, so part of the identity effect for positive information may reflect a wish to appear cooperative. The same applies to part of the gap between positive and negative sharing in the non-anonymous arms. The comparison of negative and positive information under anonymity does not vary with social desirability and is not subject to this concern. Given the number of interactions tested and the small number of clusters, I treat this with caution.

5.3. Mechanisms

Next, I examine mechanisms that may explain why information about employer quality does not circulate fully in the market. I focus on three primary explanations which I test through experimental data and then discuss alternative explanations. First, workers may fear retaliation from employers if negative information can be traced back to them. Second, workers may be reluctant to share their own negative experiences because these experiences feel weak or noisy. Third, workers may avoid sharing negative information because they do not want to discourage economically vulnerable peers from accepting work.

5.3.1. Fear of Employer Retaliation

Table 6 also tries to isolate the retaliation channel. The experiment varies both whether the respondent's identity is revealed and whether the information is positive or negative. In the preferred specification, identity revelation reduces sharing of positive information by 10.1 percentage points. Negative information also reduces sharing under anonymity by 10.2 percentage points. The interaction between identity revelation and negative information is negative and statistically significant, indicating that identity revelation matters especially when the information is negative.

Combining the main effect of identity revelation with the interaction term implies that revealing identity reduces sharing of negative information by approximately 21.2 percentage points. The

predicted sharing rate falls from 98.1 percent in the positive-anonymous condition to approximately 66.8 percent in the negative-non-anonymous condition. Thus, workers are less willing to share information when they are identifiable, and this reluctance is substantially stronger for negative information. This pattern is consistent with workers fearing employer retaliation, reputational costs, or discomfort from being personally associated with negative reports about employers.

5.3.2. Weak Signal Concerns

A second possible mechanism is that workers hesitate to share negative information because their own experience may feel subjective or noisy. To examine this, I compare willingness to share negative information about one's own experience with willingness to share aggregate information about an employer. To do so, I estimate the following regression equation:

(6)

$$Y_{is} = \alpha + \beta_1 \text{Identity Revelation}_{is} + \beta_2 \text{PersonalNegative}_{is} + \beta_3 \left(\text{Identity Revelation}_{is} \times \text{PersonalNegative}_{is}\right) + X_i'\gamma + \delta_s + \varepsilon_{is}$$

Table 7 shows that respondents are less willing to share personal negative information than aggregate employer information. In the preferred specification, negative personal information reduces sharing by 11.9 percentage points relative to aggregate information under anonymity.

By contrast, identity revelation has a small and statistically insignificant effect for aggregate information. The interaction between identity revelation and personal negative information is also statistically insignificant. The predicted sharing rate is 92.2 percent in the aggregate-anonymous condition, 90.9 percent in the aggregate-non-anonymous condition, 80.3 percent in the personal-negative anonymous condition, and 73.8 percent in the personal-negative non-anonymous condition.

These results suggest that workers are more willing to transmit aggregate employer information than negative information based on their own experience. One interpretation is that workers view aggregate information as a stronger or less noisy signal of employer quality. However, this evidence is suggestive rather than definitive: aggregate information may also feel less personally accusatory or less socially costly to share than one's own negative experience.

5.3.3. 'Looking Out' for Fellow Workers

A third possibility is that workers withhold negative information because they do not want to discourage peers from taking work, especially when those peers are economically vulnerable. To test this mechanism, I vary whether the worker receiving information is described as having recently experienced an economic shock. I estimate the following equation:

$$Y_{is} = \alpha + \beta_1 \text{Shock}_{is} + \beta_2 \text{Negative}_{is} + \beta_3 \left(\text{Shock}_{is} \times \text{Negative}_{is}\right) + X_i'\gamma + \delta_s + \varepsilon_{is} \tag{7}$$

Table 8 provides little support for this explanation. Negative information substantially reduces sharing: in the preferred specification, negative information lowers sharing by 20 percentage points. However, information that the peer experienced an economic shock has no meaningful effect on sharing, and the interaction between peer shock and negative information is close to zero and statistically insignificant. The predicted sharing rates are very similar across the shock and no-shock conditions: 93.7 percent for no-shock positive information, 92.4 percent for shock positive information, 73.7 percent for no-shock negative information, and 71.7 percent for shock negative information.

Thus, workers are not more or less likely to share negative information when the recipient may especially benefit from avoiding a bad employer. This suggests that the information-sharing friction is not primarily driven by workers trying to protect economically vulnerable peers from being discouraged from work.

5.3.4. Alternative Explanations

Knowledge hoarding. A possible explanation is knowledge hoarding: workers may keep information about good employers to themselves to protect their own access to scarce jobs. Hoarding predicts less sharing of positive information, not of negative information, so on its own it would narrow the gap between positive and negative sharing rather than create it. It could still affect the identity results if hoarders are reluctant to be seen recommending an employer. To test this, I use an indicator for workers who report that they would not tell other workers about a good employer and interact it with the treatment arms (Tables 12 – 14).

The evidence does not support this explanation. Workers who report hiding information are no less willing than others to share positive information. The coefficient on the hiding indicator, which

measures this difference in the omitted positive-information arm, is 0.024 in Experiment 1 and −0.055 in Experiment 3, and neither is statistically significant. Its interactions with the remaining positive-information arms are negative but also insignificant: −0.144 for positive, non-anonymous information in Experiment 1 and −0.057 for positive information shared with a peer undergone a shock in Experiment 3. In the preferred specification, none of the interactions between the 'hides info' indicator and the treatment arms is significant in any of the three experiments. The largest is in Experiment 1. There, sharing negative information non-anonymously lowers sharing by 33.7 percentage points among workers who do not report hiding information, and by about 13 points among those who do. This interaction is significant at the 10 percent level only without demographic controls (column 2). The main effects of valence and identity revelation remain large and significant throughout. Because the interactions are imprecisely estimated, I cannot rule out moderate differences between the two groups. However, the reluctance to share negative information does not appear to be driven by workers protecting information about desirable employers.

Self-Image and Reciprocity Concerns. A second possibility is that workers avoid sharing negative information because doing so conflicts with their self-image as a worker. I examine heterogeneity using an indicator for whether respondents report that they think they are a bad worker if treated badly by an employer. The main effects of the negative-information conditions remain large and statistically significant, whereas the interactions with this indicator are generally small and statistically insignificant. For example, in Experiment 1, the negative non-anonymous condition reduces sharing by 29.9 percentage points among the reference group, and the corresponding interaction is not statistically significant. The interactions with the negative-information conditions are also insignificant in Experiments 2 and 3. Overall, the reluctance to share negative information is not concentrated among workers who report that mistreatment would lead them to reciprocate with poor performance.

Preference for Anonymity. A third possibility is that the effect of identity revelation reflects general discomfort with being observed rather than a specific concern about revealing negative information. The results are not fully consistent with this interpretation. If identity revelation captured only general discomfort, its effect should be similar regardless of whether the information is positive or negative. Instead, Table 6 shows that revealing the worker's identity reduces sharing by 10.1 percentage points when the information is positive, with an additional 11.1 percentage point reduction when the information is negative. The total effect of identity revelation is therefore approximately 21.2 percentage points for negative information, which is more than twice its effect for positive information. This differential response suggests that general discomfort with being observed may account for part of the anonymity effect, but not all of it. Additionally, I also do

not see a decline in information sharing when the information in aggregated and worker identity is revealed, nor when I conduct a placebo information sharing activity.

5.4. Threat of Unemployment

The threat of unemployment may also shape the effectiveness of reputation-based enforcement. Relational contracts are self-enforcing when the loss of future surplus is sufficiently large to deter short-run deviations (MacLeod and Malcomson, 1989; Levin, 2003). In labor markets with sometimes scarce employment opportunities, this continuation surplus may be distributed asymmetrically. Because losing access to employment is costly, workers may have incentives to preserve their relationships with employers, consistent with the idea of unemployment as a worker-discipline device (Shapiro and Stiglitz, 1984).

The framework in Section 3.1 gives this discussion some structure. The cost of sharing a negative experience, c_B in equation (3), includes the risk that an employer who learns of the report refuses to hire the worker again. How much that risk costs may also depend on the scarcity of work. When a worker who is passed over on a given morning is unlikely to find another job that day, losing one of the few employers who hire him regularly is expensive, and c_B is high; when alternative employers are easy to find, exclusion by one of them matters little. Through equation (4), a higher c_B lowers the share of workers who transmit bad experiences, τ_B. Since the reputational penalty an employer faces for misconduct is proportional to τ_B, scarce employment weakens reputational discipline through workers' reluctance to speak, even holding fixed how much workers value employer quality and how informative their experiences are.

The two sides of the relationship are not symmetric. Declining to rehire one worker costs the employer little, since many others wait at the stand each morning, so the threat of exclusion is cheap to carry out. Losing one regular employer costs the worker a great deal, so the threat is effective. Repeated interaction can therefore discipline worker behavior without symmetrically disciplining employer behavior. Information sharing could in principle correct this imbalance by turning one worker's experience into a cost the employer bears with every prospective worker, but the same scarcity that makes exclusion costly for the worker is what keeps him from sharing.

Evidence from daily variation in labor supply. I test this mechanism using day-to-day variation in competition for work at each stand. Throughout the study, surveyors counted the workers present at each stand on each survey day every half hour between 6am and 10am. Labor supply typically peaks around 8:30am and then falls as workers are hired. For each stand-day, I take the number of workers at the peak hour, and divide it by the stand's average peak over the previous seven days.

This captures whether more workers than usual were competing for jobs that morning, relative to the stand's own recent average. Workers surveyed on days when this ratio is above the sample median are classified as facing "more workers than usual". Because the measure is defined relative to the stand's own recent history, it varies across days within stands. The survey team could not get a reliable estimate of the number of employers, so the measure captures labor supply rather than market slack: a crowded morning could in principle also be one with more jobs on offer.

I find two results. First, workers are less willing to pay for a better-rated employer on crowded days. Figure 17 plots the distribution of WTP by labor supply. On days with more workers than usual, mean WTP in the first experiment is INR 313, against INR 399 on other days, and the distribution is shifted toward zero at every point: 66 percent of workers have a WTP of INR 100 or less, compared with 58 percent on other days. However, the difference is not statistically significant (Kolmogorov–Smirnov $p = 0.24$), and it is smaller for the wage-floor experiment.

Second, workers are more reluctant share negative information when is identity is revealed on crowded days. Table 23 interacts the treatment arms of Experiment 1 with the labor supply indicator. On days with fewer workers than usual, revealing identity when the information is negative lowers sharing by 23 percentage points relative to the anonymous-positive benchmark. On days with more workers than usual, the penalty is 18 to 19 percentage points larger, roughly 42 percentage points in total, with the interaction significant at the 10 percent level with the preferred specification (column 3, $p = 0.09$).

These estimates are suggestive rather than definitive. Identification comes from the day a worker happened to be surveyed, which was not randomized. Moreover, I construct a measure of labor supply and not slack in the labor market. Nonetheless, I see a similar pattern in the two results. Higher than usual labor supply might indicate that competition for work intensifies, and workers accept worse employers and would be reluctant to share negative information about employers. Thus, unemployment and information frictions may reinforce one another: involuntary unemployment raises the cost of worker voice, and limited worker voice reduces the reputational cost of employer misconduct.

6. Demand for Aggregated Employer Information

6.1. Demand for Scorecards

Having established that information frictions limit worker's ability to learn, I next generate a simple potential solution. I use the aggregate employer ratings to test whether workers value aggregated information about employer quality. I offered workers the opportunity to purchase employer "scorecards" at endline. The scorecards summarized employer ratings collected from workers at the same labor stand. Workers stated their willingness to pay for access to this information out of an endowment of INR 100. The survey team distributed the employer scorecards a few days later at the labor stand.

Workers exhibit meaningful demand for employer quality information (Figure 10). On average, they are willing to pay 27% of the endowment for access to the scorecards. This suggests that workers perceive aggregated information about employer behavior as valuable, even in a setting where job matches are short-term and wages are relatively rigid. The distribution of willingness to pay also indicates heterogeneity in demand: while many workers report low or zero willingness to pay, a non-trivial share of workers are willing to give up a substantial portion of the endowment for employer ratings. Further, Figure 21 shows weak positive correlation between willingness to pay for a higher-rated employer and the aggregate employer scorecards after removing stand level variations.

6.2. Scorecards Usage

After 2-5 months of scorecard distribution[8], I visited the labor stands again to assess if and how scorecards were used by workers. Even though scorecard usage is not exogenous to demand for scorecards, this follow up data collection activity could still give us some meaningful insights on whether workers found such information useful. I successfully re-surveyed 328 participants, which is roughly 76% of the experimental sample.

Recall. I first examine whether workers who received scorecards remembered and used them after the intervention. Among workers who were given a scorecard, only 35% reported receiving it at follow-up. This low rate of recall of the scorecard should be interpreted in light of the 2-5 month

[8]I conducted endline surveys at labor stands between September 2025 and January 2026, and returned to collect recall data in March and April 2026. As a result, the lag between the endline survey and recall data collection varies across labor stands.

lag between scorecard disbursement and recall.

Reported scorecard usage was also limited. Approximately 30% of workers said they used the scorecard to work with an employer, while most reported not using it. Consistent with this, recall of the specific employers listed on the scorecard was also low: most participants correctly identified only a small share of the employers that appeared on their scorecard. However, among the employers workers did report as being on the scorecard, a sizable share were correct, suggesting that workers may remember selected employers rather than the full set.

Together, these results suggest a gap between ex ante demand for employer-quality information and later usage or recall. Workers appear willing to pay for aggregated employer ratings, but the information may not remain salient enough, or may not be operationally useful enough, to meaningfully affect subsequent matching decisions. On a given day at the labor stands, workers and employers looking to match with one another may not show up at the same time or may not show up at all.Additionally, workers might be employed in a job elsewhere or employers may not have a job for that particular day.

Labor market matching. I next examine whether receiving a scorecard changed workers' subsequent matches with scorecard-listed employers. Overall, I find little evidence that scorecard receipt increased matching with these employers. In both the scorecard and no-scorecard groups, the average number of matches with listed employers declined between the baseline and follow-up periods, and this decline did not differ between the two groups.

In Table 15, I examine whether receiving an employer scorecard changed workers' subsequent matches with higher- and lower-rated employers. Let Y_{iqt} denote the number of distinct scorecard-listed employers of type q with whom worker i matched in period t, where $q \in \{H, L\}$ indexes higher-rated and lower-rated employers, respectively. The pre-scorecard period combines matches observed before scorecard distribution, while the post-scorecard period corresponds to the follow-up survey. I estimate the following difference-in-differences specification separately for each employer type:

$$Y_{iqt} = \alpha_q + \beta_q \text{Received}_i + \gamma_q \text{Post}_t + \delta_q \left(\text{Received}_i \times \text{Post}_t\right) + \lambda_{s(i),q} + \varepsilon_{iqt}, \tag{8}$$

where Received_i is an indicator equal to one if worker i received an employer scorecard, Post_t is an indicator equal to one for the follow-up period, and $\lambda_{s(i),q}$ denotes stand fixed effects. Standard errors are clustered at the stand level. The coefficient of interest, δ_q, measures the change in matches among scorecard recipients relative to the contemporaneous change among workers who did not

receive a scorecard.

Because there are two periods, Equation (8) can equivalently be estimated in first differences:

$$\Delta Y_{iq} = \alpha_q + \delta_q \text{Received}_i + \lambda_{s(i),q} + \Delta\varepsilon_{iq}, \qquad q \in \{H, L\}, \tag{9}$$

where

$$\Delta Y_{iq} = Y_{iq,\text{post}} - Y_{iq,\text{pre}}. \tag{10}$$

This first-difference specification corresponds to the regressions reported in Columns (1) and (2) of Table 15. In particular, the estimated effect for higher-rated employers is

$$\widehat{\delta}_H = \left(\overline{Y}^R_{H,\text{post}} - \overline{Y}^R_{H,\text{pre}}\right) - \left(\overline{Y}^N_{H,\text{post}} - \overline{Y}^N_{H,\text{pre}}\right), \tag{11}$$

while the corresponding effect for lower-rated employers is

$$\widehat{\delta}_L = \left(\overline{Y}^R_{L,\text{post}} - \overline{Y}^R_{L,\text{pre}}\right) - \left(\overline{Y}^N_{L,\text{post}} - \overline{Y}^N_{L,\text{pre}}\right), \tag{12}$$

where the superscripts R and N denote scorecard recipients and non-recipients, respectively.

To test whether scorecard receipt shifted workers toward higher-rated employers relative to lower-rated employers, I additionally estimate a triple-difference specification. I define the worker-level difference between higher- and lower-rated employer matches as

$$G_{it} = Y_{iHt} - Y_{iLt}. \tag{13}$$

I then estimate

$$\Delta G_i = \alpha + \delta^{DDD} \text{Received}_i + \lambda_{s(i)} + \Delta\varepsilon_i, \tag{14}$$

where

$$\delta^{DDD} = \delta_H - \delta_L. \tag{15}$$

The triple-difference coefficient reported in Column (3) therefore measures whether scorecard receipt caused a larger change in matches with higher-rated employers than in matches with lower-rated employers. A positive value would indicate that receipt shifted matching toward higher-rated

employers, whereas a negative value would indicate a relative shift toward lower-rated employers.

The estimates provide little evidence that scorecard receipt changed subsequent matching behavior. Relative to workers who did not receive a scorecard, recipients experienced a 0.031 larger decline in matches with higher-rated employers and a 0.074 smaller decline in matches with lower-rated employers. Neither difference-in-differences estimate is statistically significant. The resulting triple-difference estimate is $\widehat{\delta}^{DDD} = -0.105$, with a standard error of 0.106. Thus, I cannot reject the null hypothesis that scorecard receipt had the same effect on matches with higher- and lower-rated employers. More generally, the results provide no statistically significant evidence that receiving a scorecard shifted workers toward employers classified as higher rated or away from employers classified as lower rated during the follow-up period.

One possible explanation is that the aggregate ratings presented on the scorecards did not align closely with individual workers' assessments of employer quality. The eight components of the employer-quality measure exhibit good internal consistency (Cronbach's $\alpha = 0.83$), indicating that they capture a coherent underlying concept within workers. However, agreement across workers about which employers are high quality is limited. The employer-level intraclass correlation is 0.11, and an individual worker's rating correlates only 0.10 with the average rating assigned by other workers to the same employer. Agreement is somewhat greater for wage-related amenities than for non-wage treatment, although it remains modest in absolute terms. The intraclass correlation is 0.09 for the wage-related amenities and approximately zero for the non-wage amenities. Agreement is highest for whether employers pay the correct wage (ICC = 0.20) and whether workers feel cheated (ICC = 0.12), while there is essentially no agreement regarding overtime pay, micromanagement, or verbal abuse. Both domain scales nevertheless exhibit reasonable internal consistency (Cronbach's $\alpha = 0.73$).

These findings suggest that workers interpret the dimensions of employer quality coherently but disagree about how particular employers perform on those dimensions, especially with respect to non-wage treatment. Consequently, an employer classified as higher rated on the scorecard may not have been viewed as preferable by every recipient. This limited consensus may have weakened workers' incentives to act on the rankings.

A broader implication of these results is that collective action around workplace amenities may be difficult to sustain in spot labor markets. For an employer's treatment of workers to affect its reputation, workers must form sufficiently similar assessments and collectively reward well-rated employers or avoid poorly rated ones. When experiences and evaluations differ across workers, information about employer quality may not generate a common ranking around which workers

can coordinate their matching decisions. This problem may be particularly acute for non-wage amenities, which are less readily observed and appear to be evaluated more heterogeneously than wage-related practices. The fragmented and short-term nature of spot labor markets further limits communication among workers and repeated opportunities to act on shared information. Limited employer availability, restricted worker choice, and the immediate need for employment may also make it costly for an individual worker to avoid a poorly rated employer unless others do the same. Thus, the scorecards appear to generate interest ex ante, but I find little evidence that they shifted subsequent matches toward higher-rated employers or away from lower-rated employers. The results suggest that providing information alone may be insufficient to produce reputational incentives when workers lack both a shared assessment of employer quality and the ability to coordinate their responses.

7. Why Is This Amenity Not Provided?

A natural question that emerges is if workers value employer quality and good behavior, then why is this amenity not provided? To explore this question, I survey 132 employers at the labor stands. Overall, the employer survey provides suggestive evidence on three possible explanations: whether employers believe that their reputation among workers matters, whether employers know which amenities workers value, and whether improving these workplace amenities is costless.

First, employers appear to understand that reputation is important. Figure 11a notes that nearly 70% of employers report that reputation affects how many workers are willing to work with a mason. When asked how reputation matters relative to wages, employers give more mixed responses, but a substantial share report that reputation matters at least as much as wages (Figure 11b). This suggests that the absence of clear wage differentials is unlikely to be driven simply by employers not being aware that reputation affects labor supply.

Second, employers may misperceive what workers value. Workers' own responses indicate that a good employer is not only defined by timely payments, but also by respectful treatment, including being spoken to nicely and respect. Employers, however, predict that workers place especially high value on timely payment and comparatively less value on respect-related amenities. This contrast suggests that employers understand the importance of pay-related concerns, but may underestimate workers' valuation of respect and other softer non-wage amenities. To better understand this misperception, I conduct a small exercise with a sample of 52 employers in the same stands. I tell employers about the job choice experiment I conducted with workers at their stand (Table 1),

along with the eight components of the aggregated employer ratings. I then ask them if a worker is offered two jobs – one with a higher-rated employer at the prevailing stand wage and one with a lower-rated employer at a higher pay – out of 100 workers at your stand, how many do you think would choose the better-rated employer despite the lower pay? I also incentivize employers for accuracy. Figure 12a shows the distribution of employer's beliefs about workers' willingness to pay to work for a higher-rated employer. Employers are likely to say 50% of the workers would prefer working for better-rated employer for lower pay, which could imply that they are merely guessing. Overall, the employers' guesses are dispersed suggesting imperfect knowledge of how important employer quality is for workers. Figure 12b plots the difference between actual willingness to pay and employers' beliefs.

Third, improving the quality of worker-employer interaction may not be costless from the employer's perspective. Figure 13 shows employers' beliefs about whether different practices are necessary for worker productivity. Employers generally do not report that raising one's voice at workers is necessary to increase worker productivity. However, many employers report that micromanagement and clear communication are important in ensuring that workers are productive. This distinction is useful: some dimensions of poor treatment, such as verbal abuse, may be viewed as unnecessary, but other dimensions of workplace management may involve perceived productivity tradeoffs. As a result, employers may not view all amenities valued by workers as necessary to provide.

Thus, the employer-side evidence suggests that the lack of wage differentials is not simply due to employers ignoring reputation. Employers know that reputation matters, and workers appear willing to give up wages for higher-rated employers. Instead, the persistence of poor employer quality may reflect a combination of imperfect employer beliefs about what workers value, perceived costs of changing management practices, and information frictions that prevent worker preferences from being fully reflected in spot-market wages. Thus, reputation may matter, but there exist barriers for it to translate cleanly into wage premia or employer behavior in this market.

8. Conclusion

This paper studies why poor employer quality can persist in an informal labor market even when workers care substantially about how they are treated. I conclude with discussions on the generality of this paper's results, and directions for future research.

For reputation to discipline employers, workers must value employer quality and information about employer conduct must reach other workers. The evidence from construction labor stands in

Chennai shows that the first condition holds, but that the second condition is substantially weakened by communication frictions. These results may also extend beyond construction labor stands to other labor markets characterized by short-term employment, decentralized hiring, weak contract enforcement, and limited communication among workers. Some examples include agricultural day labor, domestic work, and migrant worker recruitment through labor contractors. Although some of these institutional settings differ, workers in these environments often have limited access to reliable information about employers before accepting a job, while current and former workers may face social, economic, or retaliation costs from sharing negative experiences. The results are most likely to generalize where employment relationships are difficult to verify formally and workers cannot easily communicate with one another.

However, these results can also hold true in richer environments. Sockin and Sojourner (2023) identify a similar information demand and supply problem with online employer reviews on the platform 'Glassdoor'. The negative information on employers that is particularly valuable to prospective workers may also be the most costly for current or former workers to provide. My results show that this tension is not confined to formal employment or online platforms, but might have adverse effects on worker well-being in low-income environments given the precarious nature of work.

The mechanism experiments shed some light on the reasons behind asymmetric sharing of information in this market. Employer retaliation and credible signals of employer behavior matter. Workers are more willing to share aggregate information about an employer than negative information based on their own experience. One interpretation is that an individual experience is perceived as a weak or subjective signal, whereas an aggregate rating provides social validation and reduces the personal responsibility associated with making a negative claim. However, aggregating information does not remove frictions to access to credible employer information. Institutions that help with reputation building and credible flow of information could be the way forward (Carmichael, 1984).

Another promising direction for future research is to study firm-level interventions designed to strengthen worker voice and improve workplace quality. The employer-side evidence points to an additional friction: although employers generally recognize that their reputations affect their ability to recruit workers, they appear to have imperfect knowledge of which job attributes workers value and how much they value them. This finding complements Cordes and Müller (2026), who document a similar information gap in Germany. Existing research suggests that workplace interventions can help address some of these problems. Alan et al. (2023) show that improving workplace climate and leadership behavior positively affects worker well-being, while Boudreau (2024) shows that

strengthening worker voice through occupational safety and health (OSH) committees can increase worker satisfaction. Future research could examine whether such interventions also help employers learn about workers' preferences and whether this learning produces sustained improvements in job quality.

This paper shows that the persistence of poor working conditions need not imply that workers place little value on dignity at work. Workers may care deeply about employer quality while lacking the information, protection, or effective choice necessary to reward better employers and discipline worse ones. Understanding these constraints is therefore central to understanding both worker welfare and the persistence of poor-quality employment relationships in informal labor markets.

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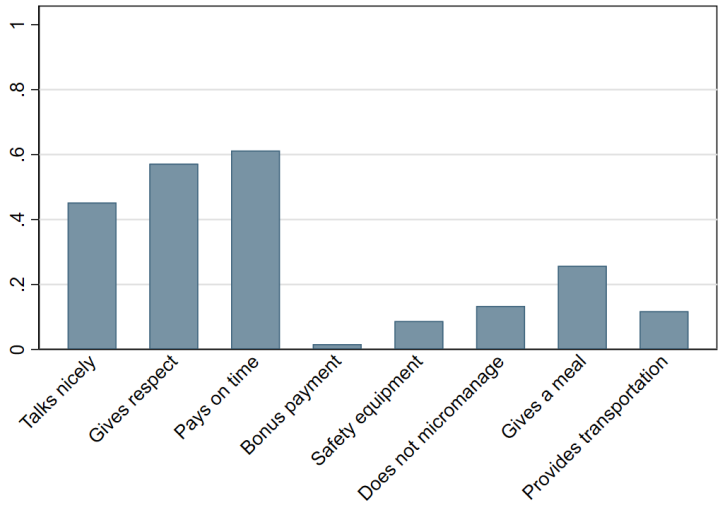
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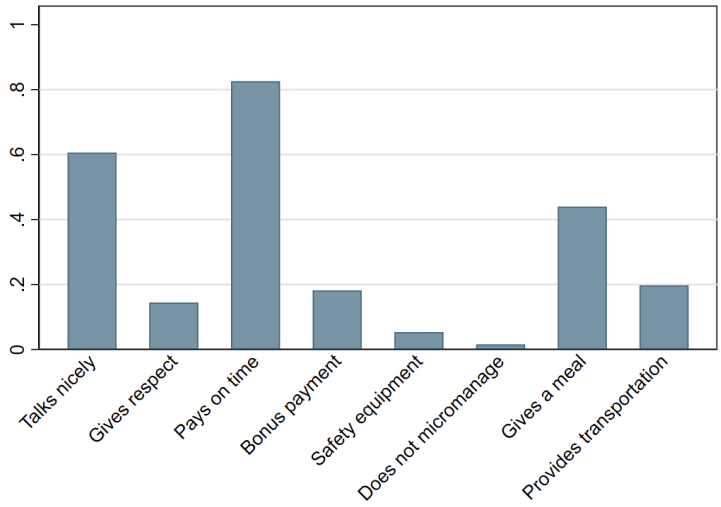
9. Figures

FIGURE 1: Amenities Valued by Workers

(a) Worker Sample

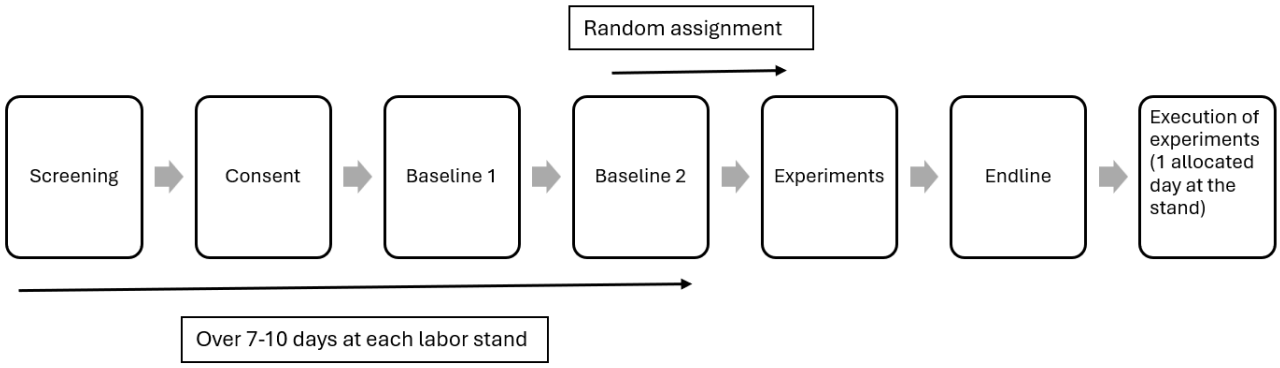


(b) Employer Sample



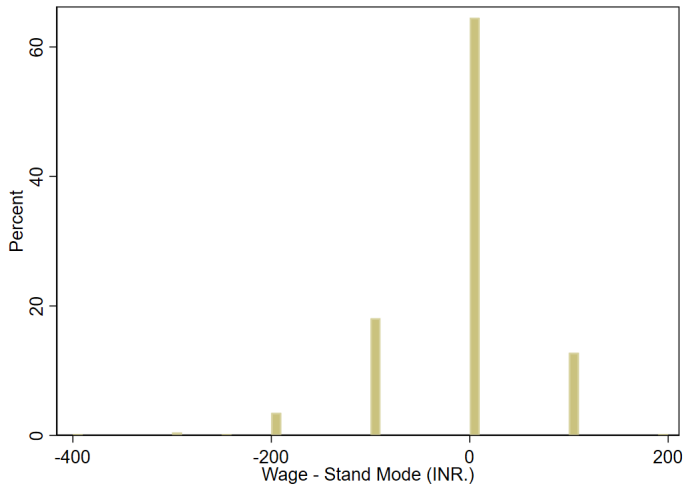
Notes: Panel (a) reports the share of workers ($N = 493$) who name each attribute when asked what makes a good employer. Panel (b) reports the share of employers ($N = 132$) who name each attribute when asked what workers value in an employer. The question was open-ended, and answers were coded into the categories shown. Respondents could give more than one answer, so shares do not sum to one.

FIGURE 2: Timeline of Survey Activities at Each Stand



Notes: The figure shows the order of study activities at each of the 13 labor stands. Screening through Baseline 2 took 7–10 days per stand. Treatment arms were assigned after Baseline 2, at the individual level and stratified by stand. The endline survey included the scorecard valuation task, and scorecards were handed out a few days later. Decisions from the experiments were implemented over 1-2 days at each stand; workers were not told the date in advance. A follow-up survey on scorecard use took place in March–April 2026, roughly 2–5 months after the endline survey (not shown).

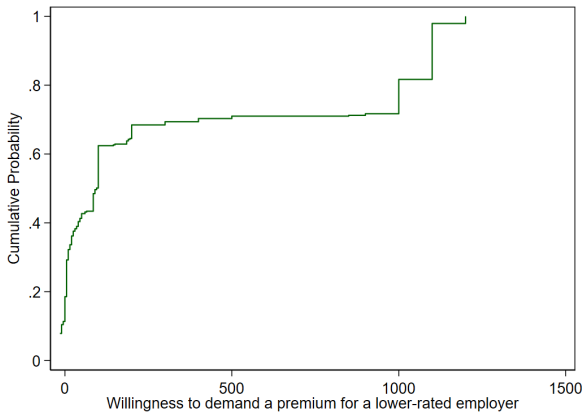
FIGURE 3: Wage Deviation from Stand Mode



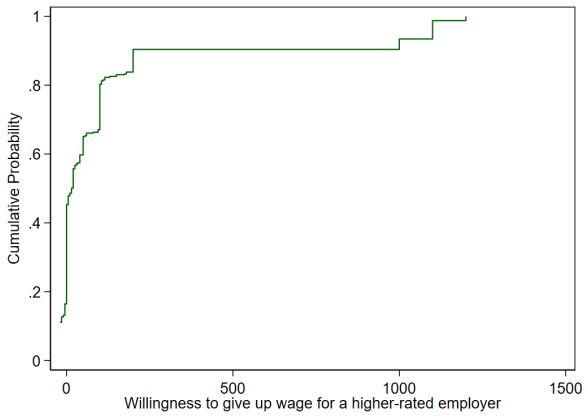
Notes: The figure shows the distribution, across workers, of the difference between each worker's reported daily wage and the modal wage reported at his stand, in INR. The reported wage is is the wage on their most recent 'helper' job at the stand. Reported wages are multiples of INR 100. About 64% of workers report exactly the stand mode, and about 95% report a wage within INR 100 of it.

FIGURE 4: Willingness to Pay for a Better-Rated Employer

(a) Demand a Premium

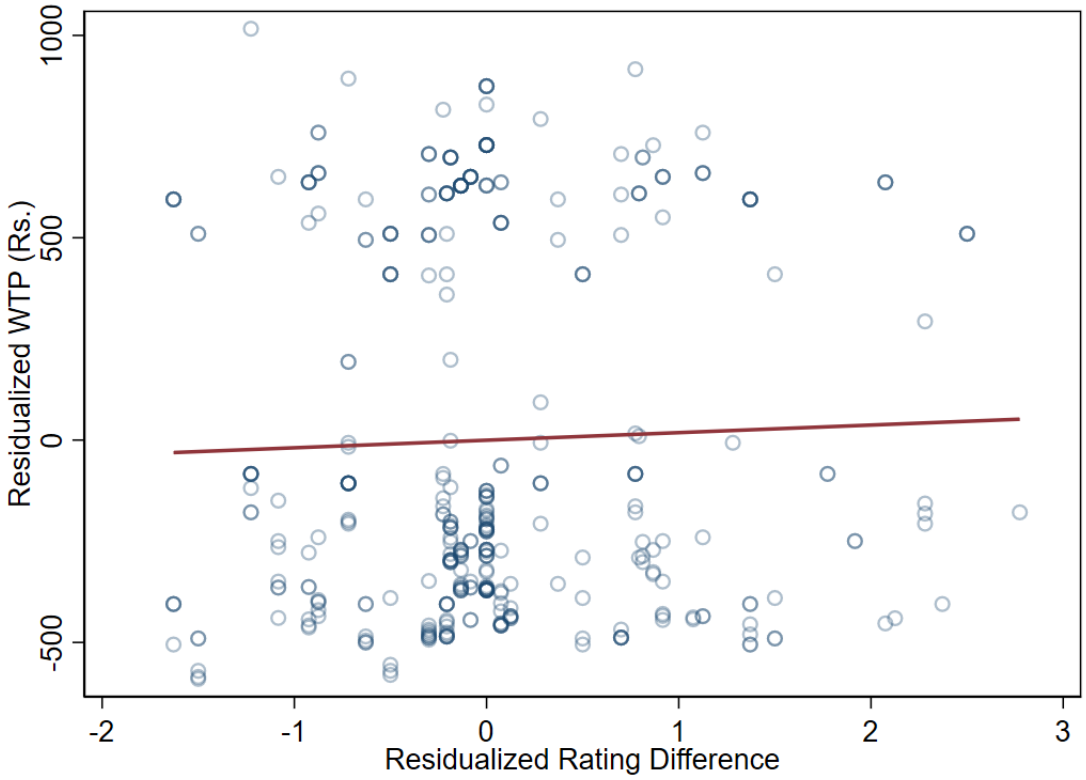


(b) Go Below the Wage Floor



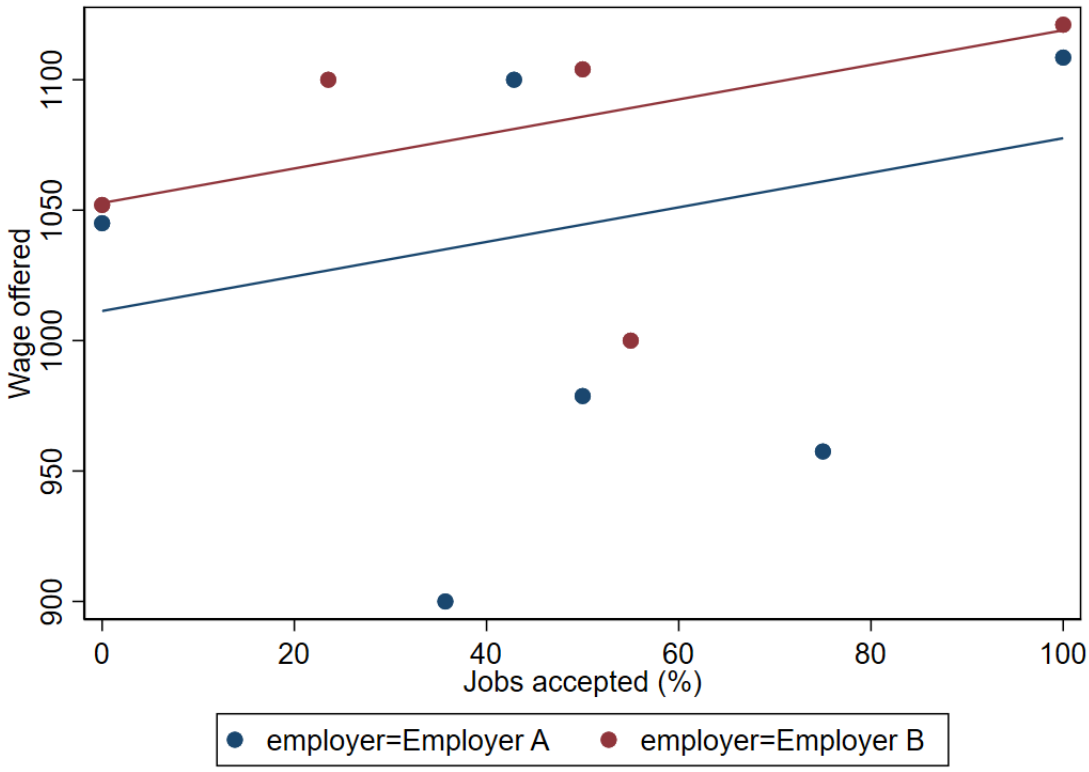
Notes: The figure plots cumulative distributions of willingness to pay (WTP), in INR, to work for the higher-rated rather than the lower-rated employer in a pair. WTP was elicited with two multiple price lists, completed in random order. In panel (a), the higher-rated employer's wage is fixed at the stand's prevailing wage, and the lower-rated employer's wage rises in steps of INR 20; WTP is the smallest premium at which the worker switches to the lower-rated employer. In panel (b), the lower-rated employer's wage is fixed, and the higher-rated employer's wage falls in steps of INR 20; WTP is the largest wage cut the worker accepts to stay with the higher-rated employer. Workers who never switch are coded at to give up their entire wage. Mean (median) WTP is INR 352 in panel (a) and INR 139 in panel (b). In each pair, both employers were regular employers at the worker's stand for whom he had not worked and who had been rated by at least two other workers. The worker saw their aggregate ratings but not their names. A random subset of choices was implemented by the survey team.

FIGURE 5: Relationship between Residualized Rating Differences and WTP



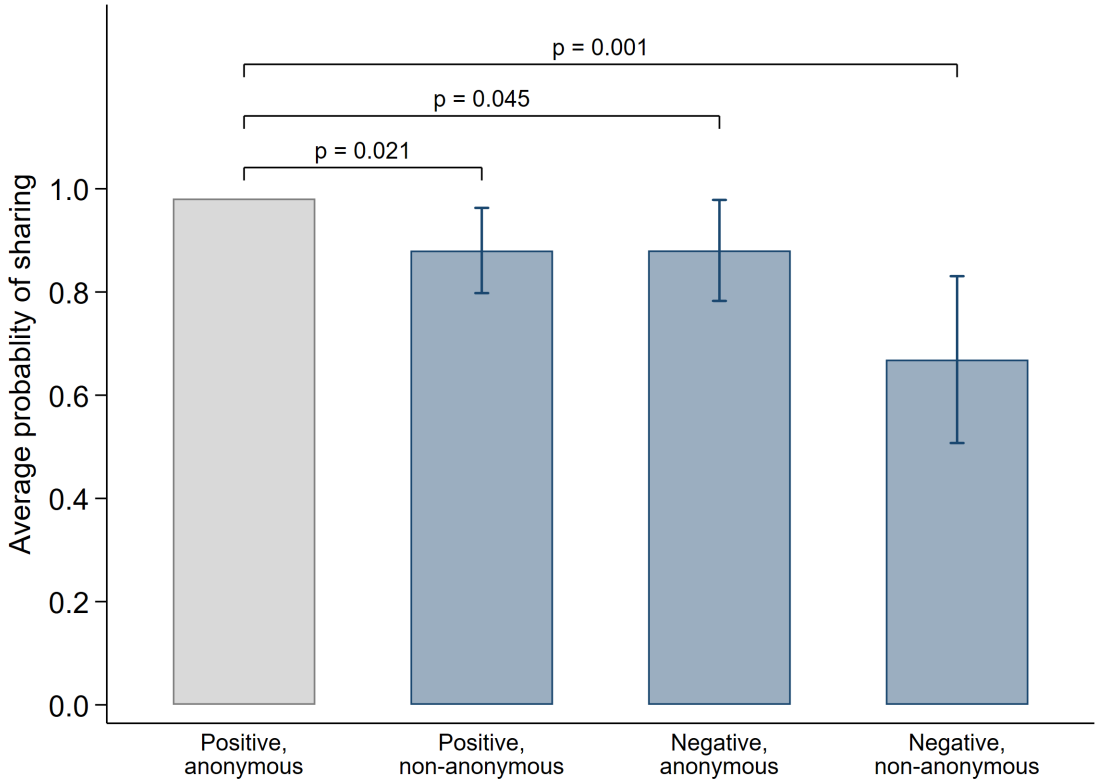
Notes: Each point is a worker. The vertical axis is the WTP of Figure 4a in INR. The horizontal axis is the difference in the aggregate employer-quality index (Figure 15) between the higher- and the lower-rated employer the worker was shown. Both variables are residualized on stand fixed effects. The line is the OLS fit (correlation = 0.03).

FIGURE 6: Job Acceptance Rates



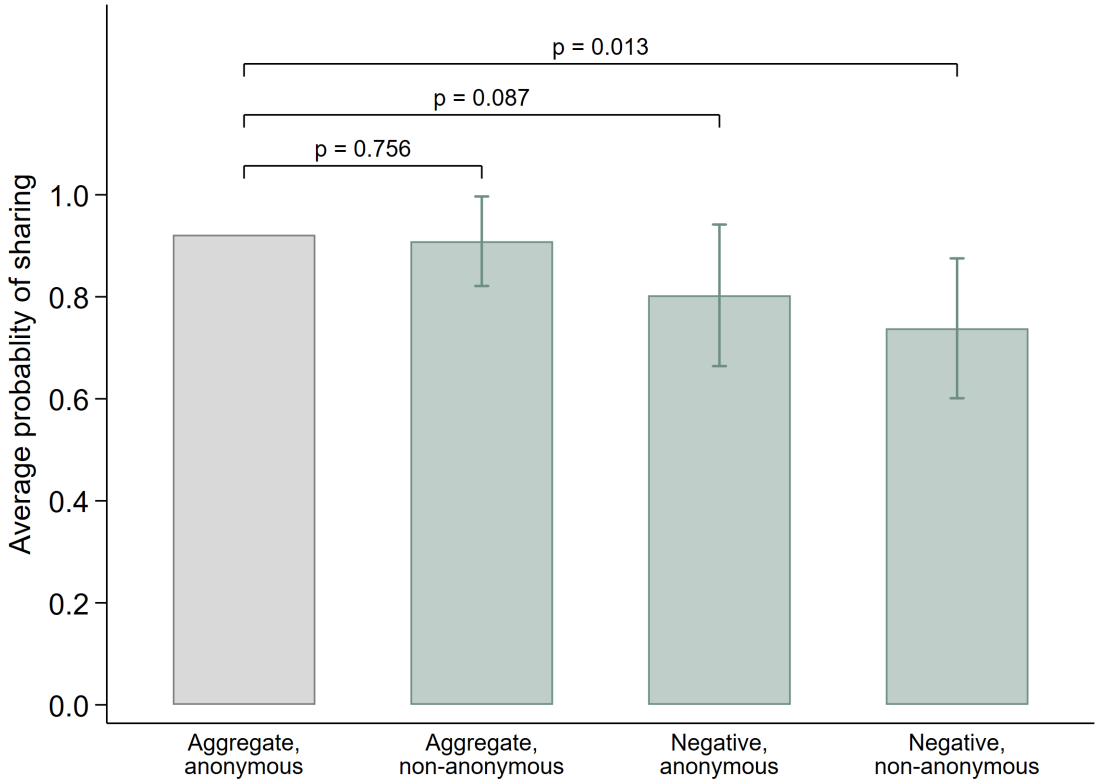
Notes: The figure uses the incentivized job offers made to the 113 workers who had not found work on the morning of the survey; 60% of workers accepted the job offer. Each offer came from a higher- (Employer A) or a lower-rated employer (Employer B), at a wage drawn from the range consistent with the worker's choices in the job-choice experiment. Each point is the share of workers who accepted an offer at a given wage from a given employer type. Lines are OLS fits of the offered wage on the acceptance rate, estimated. Because the wage is on the vertical axis, each line traces an inverse labor supply curve: a higher line means a higher wage is needed for the same acceptance rate. Wages are in INR per day.

FIGURE 7



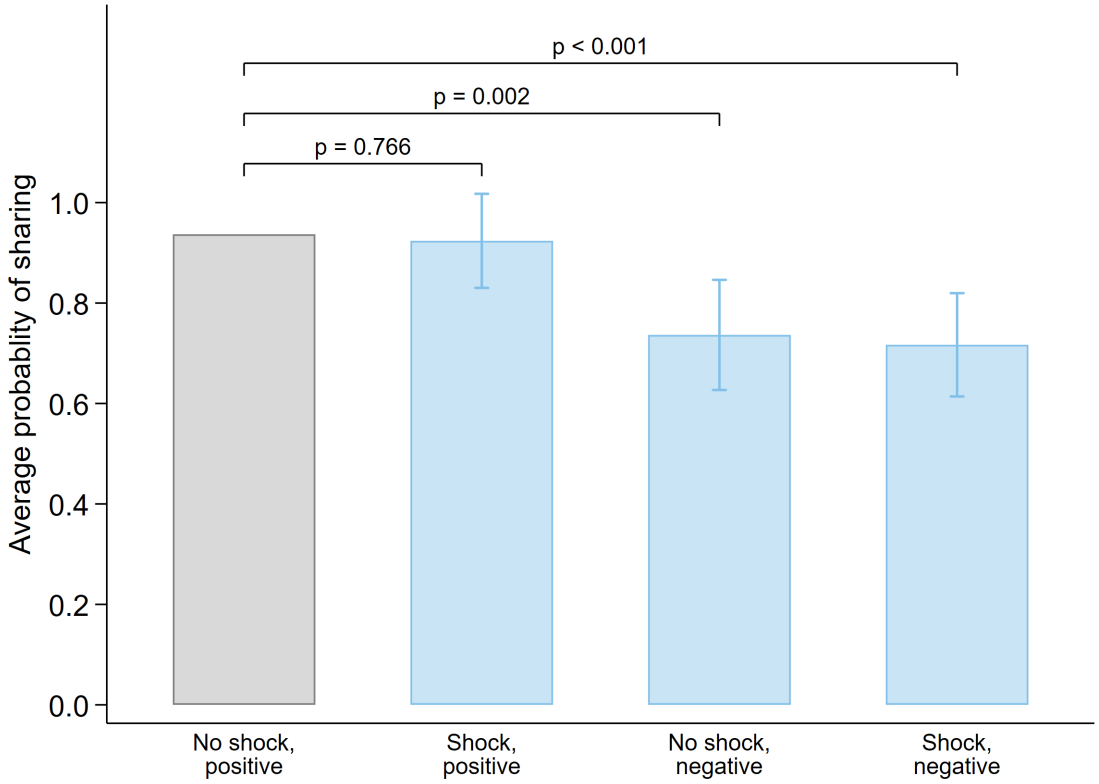
Notes: The figure shows the share of workers who agreed to share an employer experience with an assigned peer at the stand, by arm of Experiment 1 (Table 2). The gray bar is the raw mean in the benchmark arm, positive information shared anonymously (0.981). Each other bar is the benchmark mean plus that arm's coefficient from equation 5, estimated with demographic controls and stand fixed effects (Table 6, column 3). Capped lines are 95% confidence intervals for the coefficient. Brackets report p-values using standard errors clustered by stand. In anonymous arms, a member of the survey team passed on the information without naming the worker; in non-anonymous arms, the worker told the peer himself while a team member listened. Demographic controls are age, religion, caste, years at the stand, education, own economic shock in the past month, number of children, bank account ownership, daily wage, debt at the stand, number of friends at the stand, and alcohol consumption.

FIGURE 8



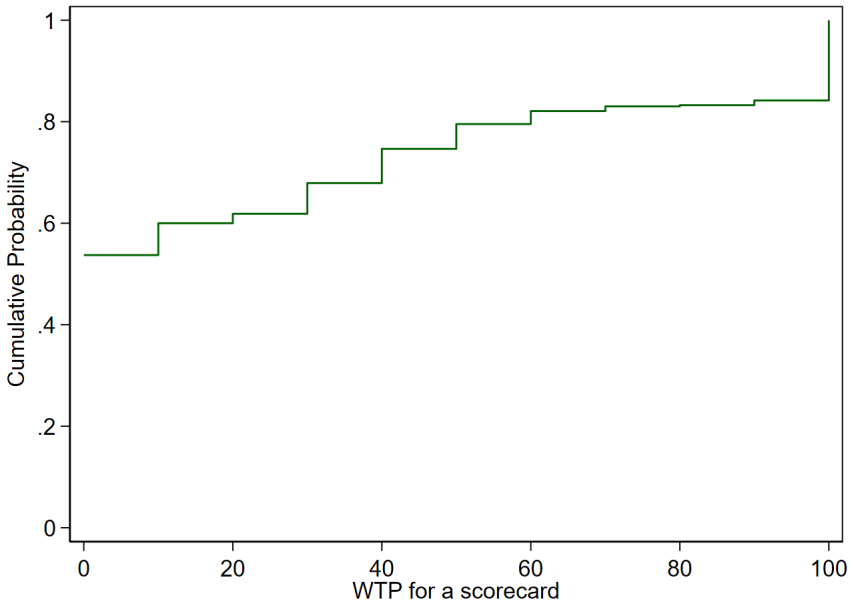
Notes: The figure shows the share of workers who agreed to share information about an employer with an assigned peer at the stand, by arm of Experiment 2 (Table 3). Personal information is the negative experience the worker recalled earlier in the survey. Aggregate information is the aggregate rating of an employer, compiled by the research team from at least two workers' reports at the stand. The gray bar is the raw mean in the benchmark arm, aggregate information shared anonymously (0.922).Each other bar is the benchmark mean plus that arm's coefficient from equation 6, estimated with demographic controls and stand fixed effects (Table 7, column 3). Capped lines are 95% confidence intervals for the coefficient. Brackets report p-values using standard errors clustered by stand. In anonymous arms, a member of the survey team passed on the information without naming the worker; in non-anonymous arms, the worker told the peer himself while a team member listened. Demographic controls are age, religion, caste, years at the stand, education, own economic shock in the past month, number of children, bank account ownership, daily wage, debt at the stand, number of friends at the stand, and alcohol consumption.

FIGURE 9



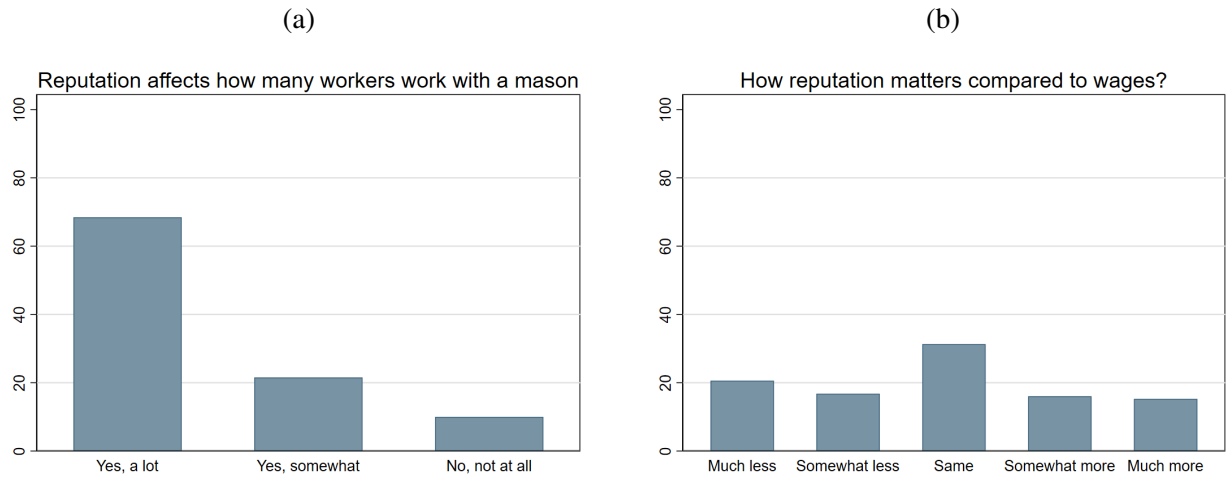
Notes: The figure shows the share of workers who agreed to share an employer experience with an assigned peer at the stand, by arm of Experiment 3 (Table 4). In the shock arms, the peer was described as having experienced an economic shock in the past month; in the other arms, the peer's circumstances were not mentioned. The worker's identity was revealed in all arms. The gray bar is the raw mean in the benchmark arm, positive information shared with a peer without a shock (0.937). Each other bar is the benchmark mean plus that arm's coefficient from equation 7, estimated with demographic controls and stand fixed effects (Table 8, column 3). Capped lines are 95% confidence intervals for the coefficient. Brackets report p-values using standard errors clustered by stand. Demographic controls are age, religion, caste, years at the stand, education, own economic shock in the past month, number of children, bank account ownership, daily wage, debt at the stand, number of friends at the stand, and alcohol consumption.

FIGURE 10: Willingness to Pay for a Scorecard



Notes: The figure plots the cumulative distribution of WTP for an employer scorecard, elicited at endline from an endowment of INR 100. The scorecard listed the aggregate quality rating of every employer at the worker's stand who had been rated by at least two workers. Mean WTP is INR 27. About 54% of workers bid zero and about 16% bid the full endowment.

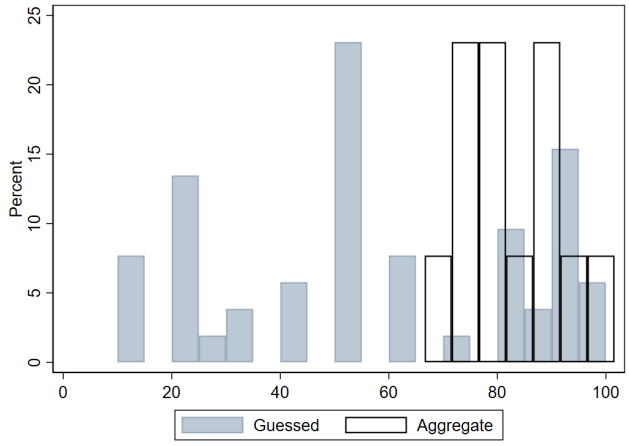
FIGURE 11: Importance of reputation for employers



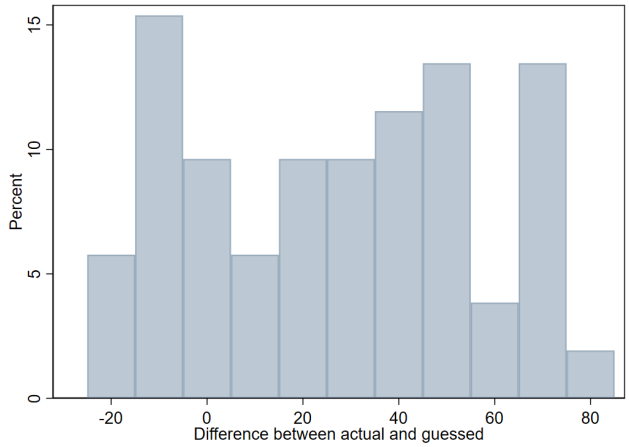
Notes: Sample is 132 masons who hire at the study stands. Panel (a) shows responses to "Do you think reputation at the stand affects how many workers agree to work with a mason?" Panel (b) shows responses to "Compared to wages, how important do you think reputation is for workers in deciding to accept a job?" Bars give the percent of employers choosing each response and sum to 100 within each panel.

FIGURE 12: Employers' Beliefs about Workers' Willingness to Pay

(a)

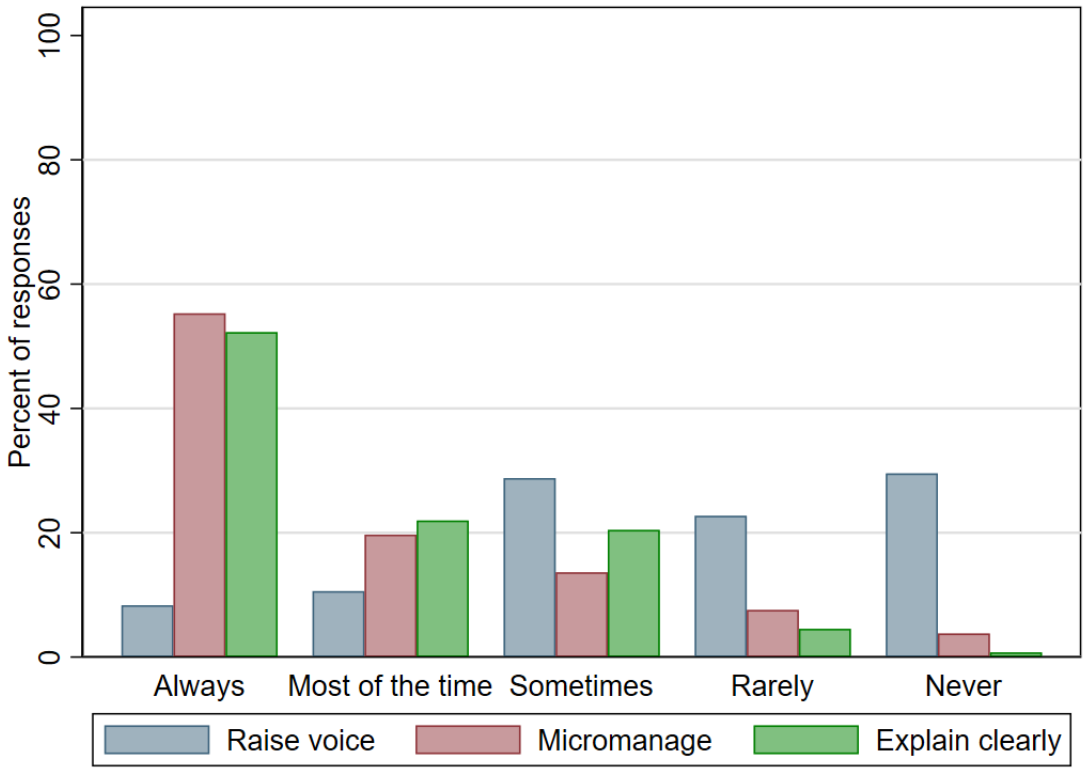


(b)



Notes: Panel (a) compares two distributions. Shaded bars show the guesses of 52 employers, surveyed separately from the main employer sample. Each guessed how many of 100 workers at his stand would choose a higher-rated employer paying the prevailing stand wage over a lower-rated employer paying any positive amount. Guesses were incentivized for accuracy. Hollow bars show the actual share of workers at each of the 13 stands who chose the higher-rated employer at that wage gap in the job-choice experiment, one observation per stand. Panel (b) plots, for each employer, the actual share at his stand minus his guess, in percentage points. Positive values mean the employer underestimated how many workers would give up pay for a better-rated employer.

FIGURE 13: Employer's Beliefs about Productivity Practices



Notes: Employer survey, $N = 132$. Employers were asked how often it is necessary to raise one's voice at workers, closely supervise (micromanage) workers, and explain tasks clearly for workers to be productive. Bars give the percent of employers choosing each response and sum to 100 within each practice.

10. Tables

Table 5: Sample

	Mean	Min	Max
Obervations	431		
Age	45.58	20.00	60.00
Hindu	0.89	0.00	1.00
Birthplace outside of Chennai	0.82	0.00	1.00
Visit other stands	0.07	0.00	1.00
Lives in own house	0.13	0.00	1.00
Has own phone	0.74	0.00	1.00
Caste: General	0.00	0.00	1.00
Caste: BC	0.14	0.00	1.00
Caste: MBC	0.37	0.00	1.00
Caste: OBC	0.03	0.00	1.00
Caste: SC	0.30	0.00	1.00
Caste: ST	0.04	0.00	1.00
Years at the stand	9.95	0.00	45.00
Any schooling	0.84	0.00	1.00
Education: Primary	0.32	0.00	1.00
Education: Secondary	0.44	0.00	1.00
Education: High school	0.03	0.00	1.00
Education: Any college	0.03	0.00	1.00
Had an economic shock	0.50	0.00	1.00
Number of children	2.17	0.00	6.00
Has bank account	0.51	0.00	1.00
Current daily wage	1068.10	800.00	1200.00
Taken debt at the stand	0.04	0.00	1.00
Number of friends at the stand	6.41	1.00	50.00
Alcohol consumption (days per week)	2.45	0.00	7.00
Motivation Index	0.48	0.00	1.00
Social Desirability Score	0.85	0.20	1.00

Notes: The table reports summary statistics for the 431 workers in the final experimental sample. For indicator variables, the mean represents the proportion of workers for whom the indicated characteristic equals one. Current daily wages refers to the prevailing wage rate, and is reported in Indian rupees. "Years at the stand" measures the number of years the respondent has attended the labor stand. "Had an economic shock" indicates that the respondent experienced an economic shock during the past month. BC, MBC, OBC, SC, and ST denote Backward Class, Most Backward Class, Other Backward Class, Scheduled Caste, and Scheduled Tribe, respectively.

Table 6: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Identity is revealed	-0.105**	-0.100**	-0.101**
	[0.046]	[0.035]	[0.038]
Information is negative	-0.095**	-0.096**	-0.101**
	[0.045]	[0.042]	[0.045]
Identity is revealed*Information is negative	-0.111*	-0.122**	-0.111**
	[0.065]	[0.048]	[0.045]
Depvar mean [Pos-Anon]	0.981	0.981	0.981
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Observations	431	431	431
R-squared	0.102	0.176	0.207
Clusters	0	13	13

Notes: The dependent variable equals one if the worker agrees to share an employer experience with an assigned peer at the stand. *Identity is revealed* equals one in the non-anonymous arms. *Information is negative* equals one if the worker was asked to share his negative rather than his positive experience. The omitted category is positive information shared anonymously (mean of the dependent variable = 0.981). Demographic controls are age, caste, years at the stand, education, own economic shock in the past month, number of children, bank account ownership, daily wage, debt at the stand, alcohol consumption, and number of friends at the stand. Column (1) reports heteroskedasticity-robust standard errors; columns (2) and (3) report standard errors clustered by stand (13 clusters). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Weak Signal Experiment

	Share information (1)	Share information (2)	Share information (3)
Identity is revealed	-0.043	-0.040	-0.013
	[0.051]	[0.040]	[0.040]
Information is negative	-0.127**	-0.125*	-0.119*
	[0.050]	[0.057]	[0.064]
Identity is revealed*Information is negative	-0.033	-0.040	-0.052
	[0.071]	[0.070]	[0.073]
Depvar mean [Aggregate-Anon]	0.922	0.922	0.922
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Observations	431	431	431
R-squared	0.044	0.104	0.155
Clusters	0	13	13

Notes: The dependent variable equals one if the worker agrees to share information about an employer with an assigned peer at the stand. *Personal negative information* equals one if the worker was asked to share his own negative experience rather than aggregate information about an employer (definition as in Table 3). *Identity is revealed* equals one in the non-anonymous arms. The omitted category is aggregate information shared anonymously (mean of the dependent variable = 0.922). Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Paternalistic Motives Experiment

	Share information (1)	Share information (2)	Share information (3)
Peer endured economic shock	-0.001	-0.018	-0.013
	[0.047]	[0.041]	[0.043]
Information is negative	-0.161***	-0.202***	-0.200***
	[0.047]	[0.048]	[0.050]
Peer endured economic shock*Information is negative	-0.034	-0.003	-0.007
	[0.067]	[0.084]	[0.093]
Depvar mean [Noshock-Pos]	0.937	0.937	0.937
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Observations	431	431	431
R-squared	0.063	0.110	0.129
Clusters	0	13	13

Notes: The dependent variable equals one if the worker agrees to share an employer experience with an assigned peer at the stand. *Peer endured economic shock* equals one if the peer was described as having experienced an economic shock in the past month. *Information is negative* equals one if the worker was asked to share his negative rather than his positive experience. The worker's identity was revealed in all arms. The omitted category is positive information shared with a peer whose circumstances were not described (mean of the dependent variable = 0.937). Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Positive-Nonanonymous	-0.142**	-0.146**	-0.143**
	[0.062]	[0.054]	[0.057]
Positive-Nonanonymous*Bad worker if treated badly	0.081	0.102*	0.091
	[0.094]	[0.055]	[0.057]
Negative-Nonanonymous	-0.308***	-0.318***	-0.299***
	[0.058]	[0.089]	[0.095]
Negative-Nonanonymous*Bad worker if treated badly	-0.013	-0.007	-0.040
	[0.098]	[0.108]	[0.119]
Negative-Anonymous	-0.084	-0.079	-0.072
	[0.059]	[0.051]	[0.061]
Negative-Anonymous*Bad worker if treated badly	-0.030	-0.047	-0.074
	[0.093]	[0.071]	[0.085]
Bad worker if treated badly	-0.007	-0.018	-0.000
	[0.066]	[0.033]	[0.046]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.005	0.031	0.052
Neg-nonanon=neg-anon	0.000	0.008	0.005
Observations	431	431	431
R-squared	0.105	0.181	0.214
Clusters	0	13	13

Notes: The omitted category is positive information shared anonymously, for which the mean of the dependent variable is 0.981. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Weak Signal Experiment

	Share information (1)	Share information (2)	Share information (3)
Aggregate-Nonanonymous	-0.034	-0.038	-0.013
	[0.065]	[0.044]	[0.050]
Aggregate-Nonanonymous*Bad worker if treated badly	-0.007	0.004	0.008
	[0.106]	[0.082]	[0.092]
Negative-Nonanonymous	-0.230***	-0.227**	-0.197*
	[0.066]	[0.093]	[0.093]
Negative-Nonanonymous*Bad worker if treated badly	0.068	0.056	0.032
	[0.099]	[0.102]	[0.100]
Negative-Anonymous	-0.159**	-0.166***	-0.147**
	[0.065]	[0.052]	[0.067]
Negative-Anonymous*Bad worker if treated badly	0.106	0.128	0.090
	[0.102]	[0.075]	[0.073]
Bad worker if treated badly	0.043	0.022	0.032
	[0.069]	[0.027]	[0.038]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.267	0.364	0.426
Neg-nonanon=agg-nonanon	0.002	0.048	0.059
Observations	431	431	431
R-squared	0.060	0.115	0.163
Clusters	0	13	13

Notes: The omitted category is aggregate information shared anonymously, for which the mean of the dependent variable is 0.922. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 11: Paternalistic Motives Experiment

	Share information (1)	Share information (2)	Share information (3)
NoShock-Negative	-0.149**	-0.208***	-0.221***
	[0.061]	[0.058]	[0.059]
NoShock-Negative*Bad worker if treated badly	-0.032	0.015	0.050
	[0.097]	[0.082]	[0.097]
Shock-Positive	0.016	-0.002	-0.002
	[0.060]	[0.050]	[0.051]
Shock-Positive*Bad worker if treated badly	-0.044	-0.043	-0.031
	[0.097]	[0.037]	[0.033]
Shock-Negative	-0.212***	-0.237***	-0.247***
	[0.061]	[0.044]	[0.052]
Shock-Negative*Bad worker if treated badly	0.039	0.034	0.066
	[0.098]	[0.084]	[0.077]
Bad worker if treated badly	0.027	0.024	0.004
	[0.068]	[0.035]	[0.036]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.308	0.714	0.756
Observations	431	431	431
R-squared	0.066	0.113	0.132
Clusters	0	13	13

Notes: The omitted category is positive information shared with a peer with no information on economic shock, for which the mean of the dependent variable is 0.937. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 12: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Positive-Nonanonymous	-0.088*	-0.081*	-0.078*
	[0.051]	[0.038]	[0.043]
Positive-Nonanonymous*Hides info on good employer	-0.112	-0.115	-0.144
	[0.127]	[0.087]	[0.100]
Negative-Nonanonymous	-0.335***	-0.344***	-0.337***
	[0.050]	[0.087]	[0.089]
Negative-Nonanonymous*Hides info on good employer	0.193	0.204*	0.208
	[0.129]	[0.113]	[0.128]
Negative-Anonymous	-0.099*	-0.100*	-0.099
	[0.051]	[0.052]	[0.060]
Negative-Anonymous*Hides info on good employer	0.016	0.015	0.000
	[0.115]	[0.085]	[0.095]
Hide information on good employers	0.023	0.028	0.024
	[0.085]	[0.050]	[0.058]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.000	0.002	0.002
Neg-nonanon=neg-anon	0.000	0.005	0.004
Observations	430	430	430
R-squared	0.115	0.189	0.221
Clusters	0	13	13

Notes: The omitted category is positive information shared anonymously, for which the mean of the dependent variable is 0.981. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 13: Weak Signal Experiment

	Share information (1)	Share information (2)	Share information (3)
Aggregate-Nonanonymous	-0.046	-0.041	-0.017
	[0.056]	[0.035]	[0.036]
Aggregate-Nonanonymous*Hides info on good employer	0.012	0.006	0.030
	[0.142]	[0.099]	[0.105]
Negative-Nonanonymous	-0.210***	-0.214**	-0.185**
	[0.054]	[0.071]	[0.071]
Negative-Nonanonymous*Hides info on good employer	0.018	0.040	0.011
	[0.142]	[0.165]	[0.183]
Negative-Anonymous	-0.139**	-0.138*	-0.134*
	[0.054]	[0.068]	[0.073]
Negative-Anonymous*Hides info on good employer	0.061	0.071	0.090
	[0.141]	[0.152]	[0.159]
Hide information on good employers	0.008	-0.001	-0.021
	[0.106]	[0.088]	[0.095]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.208	0.221	0.358
Neg-nonanon=agg-nonanon	0.004	0.066	0.077
Observations	430	430	430
R-squared	0.047	0.106	0.157
Clusters	0	13	13

Notes: The omitted category is aggregate information shared anonymously, for which the mean of the dependent variable is 0.922. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 14: Paternalistic Motives Experiment

	Share information (1)	Share information (2)	Share information (3)
NoShock-Negative	-0.146***	-0.185***	-0.186***
	[0.052]	[0.052]	[0.052]
NoShock-Negative*Hides info on good employer	-0.096	-0.090	-0.071
	[0.122]	[0.094]	[0.083]
Shock-Positive	0.013	-0.007	-0.007
	[0.052]	[0.038]	[0.039]
Shock-Positive*Hides info on good employer	-0.109	-0.082	-0.057
	[0.125]	[0.129]	[0.106]
Shock-Negative	-0.194***	-0.221***	-0.222***
	[0.052]	[0.053]	[0.057]
Shock-Negative*Hides info on good employer	-0.028	-0.015	0.009
	[0.126]	[0.118]	[0.123]
Hide information on good employers	-0.035	-0.034	-0.055
	[0.083]	[0.064]	[0.063]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.362	0.599	0.642
Observations	430	430	430
R-squared	0.074	0.117	0.136
Clusters	0	13	13

Notes: The omitted category is positive information shared with a peer with no information on economic shock, for which the mean of the dependent variable is 0.937. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 15: Effect of Scorecard Receipt on Matches with Higher- and Lower-Rated Employers

	High-rated matches (1)	Lower-rated matches (2)	High − lower difference (3)
Did not receive scorecard			
Before	0.547	0.554	-0.007
Follow-up	0.149	0.236	-0.088
Change	-0.399	-0.318	-0.081
Received scorecard			
Before	0.626	0.400	0.226
Follow-up	0.200	0.157	0.043
Change	-0.426	-0.243	-0.183
Difference-in-differences	-0.031	0.074	-0.105
	(0.080)	(0.080)	(0.106)
Observations	263	263	263
Stand fixed effects	Yes	Yes	Yes

Notes: This table compares the number of distinct higher- and lower-rated scorecard employers matched by workers before and after scorecard distribution, separately for workers who did and did not receive a scorecard. The difference-in-differences estimates compare the before-to-follow-up change among scorecard recipients with the corresponding change among non-recipients. Column (3) reports the difference between the higher- and lower-rated employer estimates. All regressions include stand fixed effects. Standard errors clustered at the stand level are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

A. Appendix

A.1. Amenities Worksheet



1. Timely payment  
2. Respectful Communication 
3. Provision of meals 
4. Provision of transportation 
5. Feelings of respect at the workplace 
6. Safety Provisions  
7. Pays correct wages 
8. Provided a cooler at the job site 
9. Provided a pitcher at the job site 
10. Pays extra wages for over time 



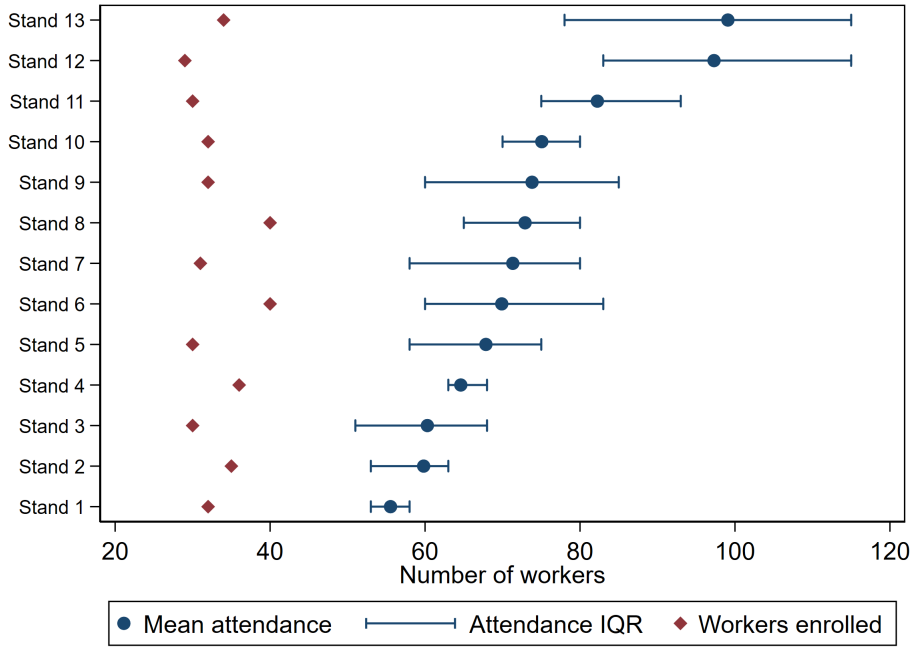
11. Micromanagement 

12. Verbally abused at the workplace 

13.Felt cheated at the workplace 

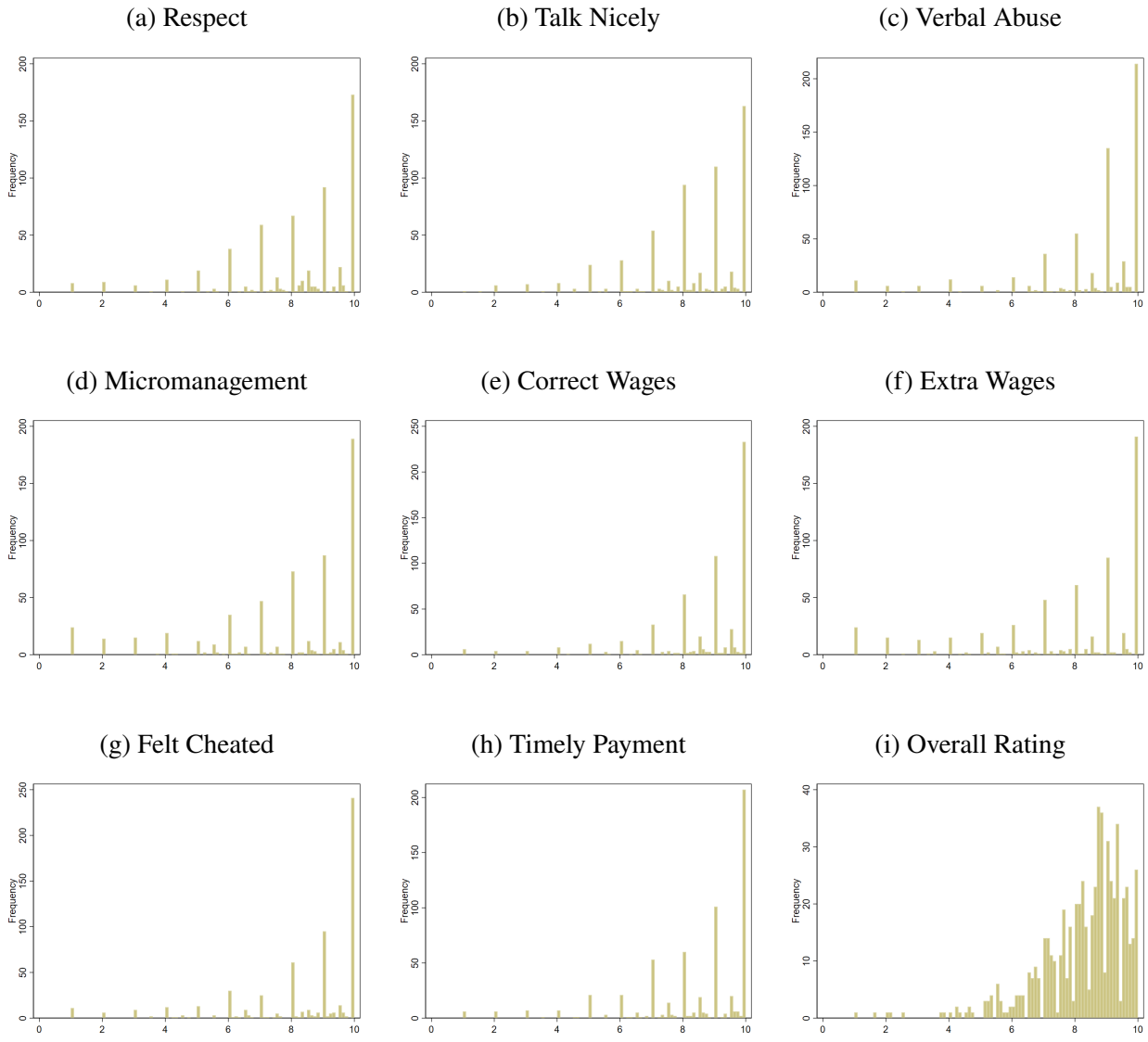
A.2. Figures

FIGURE 14: Worker attendance and study sample across labor stands



Notes: The figure compares worker attendance and study enrollment across the 13 construction labor stands. Stands are ordered by mean attendance. Blue circles indicate mean daily worker attendance, while horizontal lines show the interquartile range. Red diamonds indicate the number of workers from each stand included in the analysis sample.

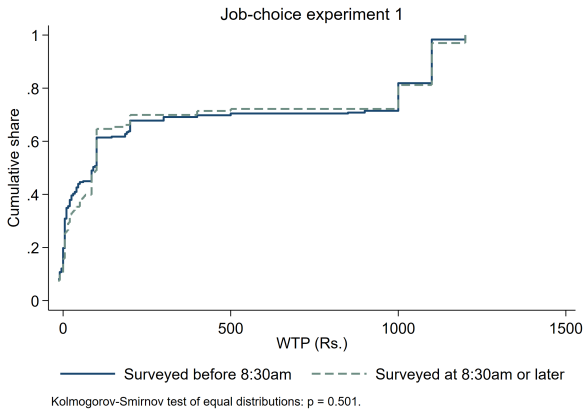
FIGURE 15: Distribution of Employer Ratings



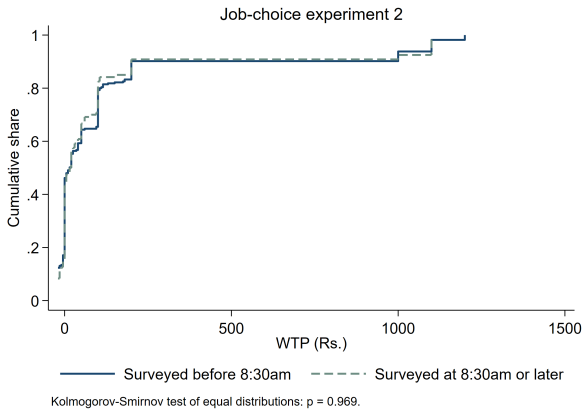
Notes: Each panel plots the distribution of average employer ratings for one dimension of employer quality. I average ratings across workers who report prior experience with a given employer. I code all measures so that higher values indicate better employer quality; I therefore reverse-code micromanagement, verbal abuse, and feeling cheated. The final panel plots the overall employer-quality index, constructed by averaging the eight component ratings. Ratings range from 0 to 10.

FIGURE 16: Willingness to Pay for a Better-Rated Employer

(a) Demand a Premium



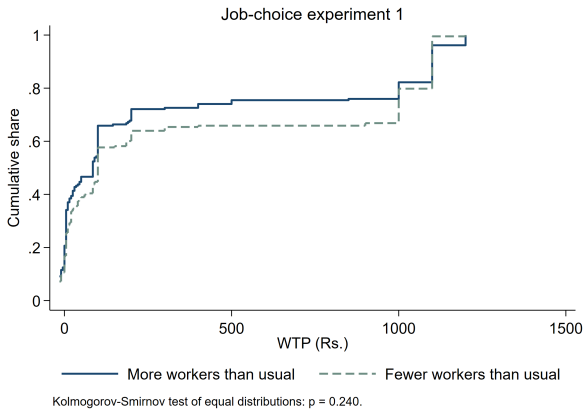
(b) Go Below the Wage Floor



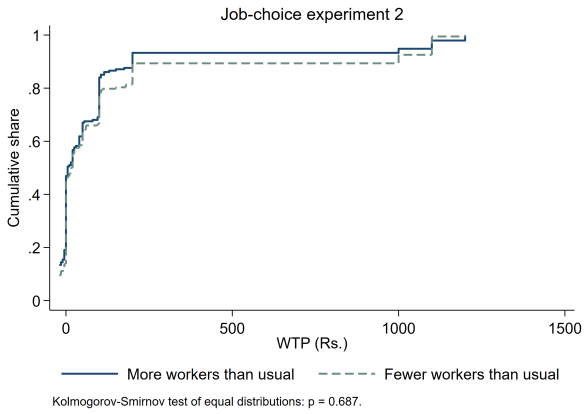
Notes: The figure plots cumulative distributions of willingness to pay (WTP), in INR, to work for the higher-rated rather than the lower-rated employer in a pair. WTP was elicited with two multiple price lists, completed in random order. In panel (a), the higher-rated employer's wage is fixed at the stand's prevailing wage, and the lower-rated employer's wage rises in steps of INR 20; WTP is the smallest premium at which the worker switches to the lower-rated employer. In panel (b), the lower-rated employer's wage is fixed, and the higher-rated employer's wage falls in steps of INR 20; WTP is the largest wage cut the worker accepts to stay with the higher-rated employer. Workers who never switch are coded at to give up their entire wage. Mean (median) WTP is INR 352 in panel (a) and INR 139 in panel (b). In each pair, both employers were regular employers at the worker's stand for whom he had not worked and who had been rated by at least two other workers. The worker saw their aggregate ratings but not their names. A random subset of choices was implemented by the survey team. Distributions are shown separately for workers whose survey began before 8:30am (solid) and at 8:30am or later (dashed); the worker count at the stand peaks at 8:30am on most days. The p-value is from a Kolmogorov–Smirnov test of equal distributions.

FIGURE 17: Willingness to Pay for a Better-Rated Employer

(a) Demand a Premium

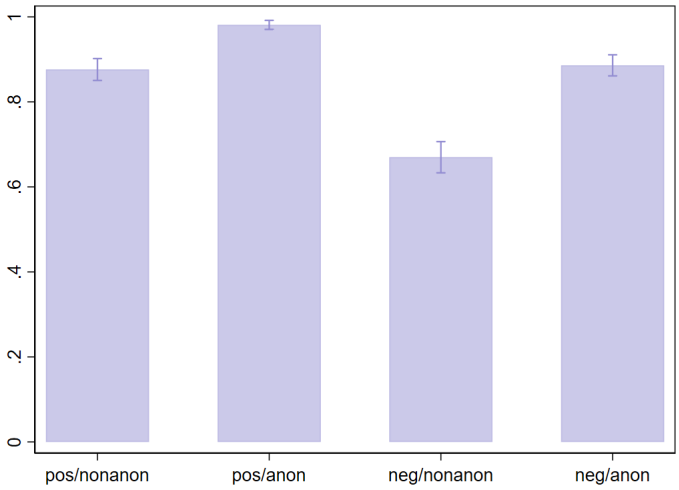


(b) Go Below the Wage Floor



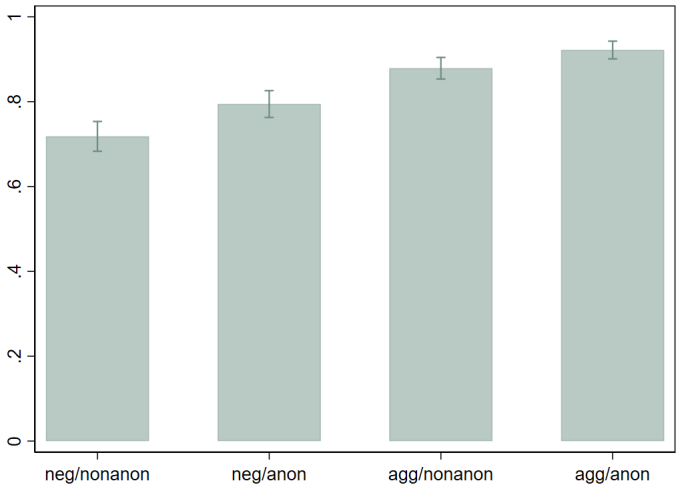
Notes: The figure plots cumulative distributions of willingness to pay (WTP), in INR, to work for the higher-rated rather than the lower-rated employer in a pair. WTP was elicited with two multiple price lists, completed in random order. In panel (a), the higher-rated employer's wage is fixed at the stand's prevailing wage, and the lower-rated employer's wage rises in steps of INR 20; WTP is the smallest premium at which the worker switches to the lower-rated employer. In panel (b), the lower-rated employer's wage is fixed, and the higher-rated employer's wage falls in steps of INR 20; WTP is the largest wage cut the worker accepts to stay with the higher-rated employer. Workers who never switch are coded at to give up their entire wage. Mean WTP is INR 352 in panel (a) and INR 139 in panel (b). In each pair, both employers were regular employers at the worker's stand for whom he had not worked and who had been rated by at least two other workers. The worker saw their aggregate ratings but not their names. A random subset of choices was implemented by the survey team. Distributions are shown separately by labor supply at the stand on the survey day: the day's peak half-hour worker count divided by the stand's mean peak over the previous seven days, split at the stand median. "More workers than usual" (solid) is above the median. The p-value is from a Kolmogorov–Smirnov test of equal distributions.

FIGURE 18: Asymmetric Information Sharing Experiment



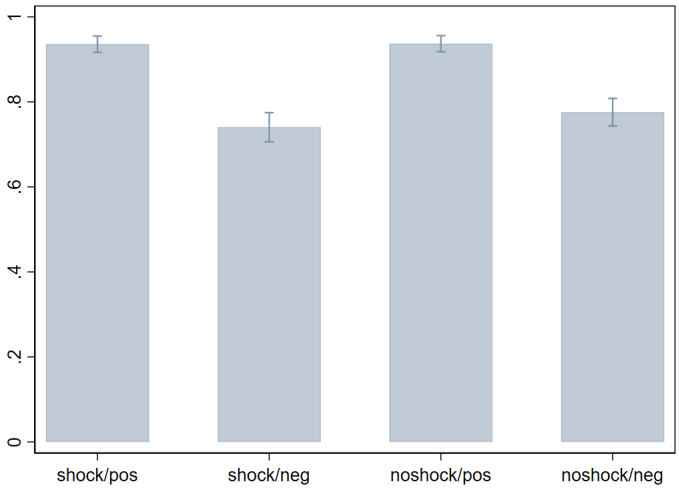
Notes: Bars show the share of workers who agreed to share an employer experience with an assigned peer at the stand, by arm of Experiment 1 (Table 6). In anonymous arms, a member of the survey team passed on the information without naming the worker. In non-anonymous arms, the worker told the peer himself while a team member listened. Bars are raw means. Error bars show 95% confidence intervals / ±1 standard error. Decisions were incentivized: one decision per worker could be implemented, and 95.6% of workers who had agreed to share followed through.

FIGURE 19: Weak Signal Experiment



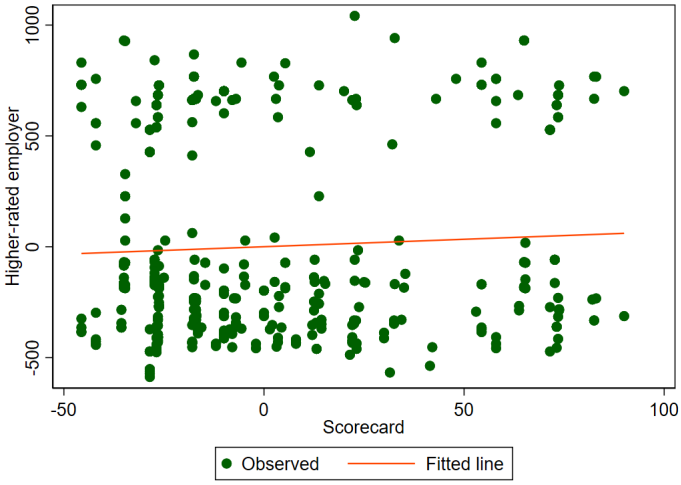
Notes: Bars show the share of workers who agreed to share information about an employer with an assigned peer at the stand, by arm of Experiment 2 (Table 7). Personal information is the negative experience the worker recalled earlier in the survey. Aggregate information is the aggregate rating of an employer, compiled by the research team from at least two workers' reports at the stand.

FIGURE 20: Paternalistic Motives Experiment



Notes: Bars show the share of workers who agreed to share an employer experience with an assigned peer at the stand, by arm of Experiment 3 (Table 8). In the shock arms, the peer was described as having experienced an economic shock in the past month. In the other arms, the peer's circumstances were not mentioned. The worker's identity was revealed in all arms.

FIGURE 21: Correlation between WTP for higher-rated employer and scorecard



Notes: Each point is a worker. The vertical axis is WTP for the higher-rated employer, and the horizontal axis is WTP for an employer scorecard. Both are in INR and residualized on stand fixed effects. The line is the OLS fit.

A.3. Tables

Table 16: Attrition

	Screened	Final Sample	P-value	Std diff.
Age	44.338	45.575	0.101	0.127
Birthplace outside of Chennai	0.816	0.817	0.993	0.003
Visit other stands	0.087	0.067	0.374	-0.071
Lives in own house	0.145	0.132	0.663	-0.037
Has own phone	0.686	0.735	0.193	0.105
Alcohol consumption (days per week)	2.227	2.450	0.294	0.096
Daily wage reported at screening	1027.053	1036.543	0.339	0.104
Observations	207	431		

Notes: The table compares the 207 workers who completed the screening survey but could not be re-surveyed with the 431 workers in the final sample. Columns (1) and (2) report means. Column (3) reports p-values from two-sided t-tests of equal means. Column (4) reports standardized differences: the final-sample mean minus the non-retained mean, divided by the pooled standard deviation. The daily wage is the wage reported at screening, in INR.

Table 17: Balance for Information Sharing Experiment 1

	Pos-NonAnonymous	Pos-Anonymous	Neg-NonAnonymous	Neg-Anonymous	Joint Sig F-test
Age	46.371	45.198	44.972	45.754	0.598
Hindu	0.914	0.877	0.887	0.886	0.832
Birthplace outside of Chennai	0.838	0.802	0.849	0.781	0.556
Visit other stands	0.057	0.057	0.075	0.079	0.850
Lives in own house	0.105	0.160	0.142	0.123	0.705
Has own phone	0.676	0.745	0.802	0.719	0.214
Caste: General	0.019	0.000	0.000	0.000	0.106
Caste: BC	0.133	0.179	0.104	0.149	0.387
Caste: MBC	0.324	0.349	0.472	0.325	0.064
Caste: OBC	0.038	0.028	0.028	0.018	0.851
Caste: SC	0.333	0.264	0.274	0.333	0.607
Caste: ST	0.048	0.047	0.009	0.053	0.315
Years at the stand	9.200	9.736	9.321	11.412	0.204
Any schooling	0.808	0.856	0.811	0.884	0.362
Education: Primary	0.314	0.274	0.330	0.368	0.511
Education: Secondary	0.438	0.509	0.396	0.430	0.382
Education: High school	0.029	0.028	0.047	0.026	0.753
Education: Any college	0.029	0.028	0.047	0.026	0.753
Had an economic shock	0.462	0.578	0.443	0.530	0.114
Number of children	2.250	2.075	2.038	2.296	0.117
Has bank account	0.491	0.501	0.519	0.536	0.879
Current daily wage	1067.619	1059.434	1072.642	1072.368	0.362
Taken debt at the stand	0.058	0.000	0.047	0.044	0.104
Number of friends at the stand	6.133	5.736	6.802	6.939	0.412
Alcohol consumption (days per week)	2.419	2.566	2.283	2.526	0.898
Observations	105	106	106	114	

Notes: The table reports baseline balance across treatment arms in the first information-sharing experiment. The last column reports the p-value from regressing the covariate on treatment indicators with stand fixed effects, and conducting a Wald test of joint significance of the regression coefficients. Missing covariate values are replaced with the sample mean.

Table 18: Balance for Information Sharing Experiment 2

	Neg-NonAnonymous	Neg-Anonymous	Aggregate-NonAnonymous	Aggregate-Anonymous	Joint Sig F-test
Age	46.200	46.131	45.343	44.661	0.354
Hindu	0.836	0.935	0.899	0.896	0.133
Birthplace outside of Chennai	0.827	0.832	0.838	0.774	0.498
Visit other stands	0.082	0.047	0.111	0.035	0.102
Lives in own house	0.145	0.112	0.111	0.157	0.648
Has own phone	0.736	0.729	0.747	0.730	0.975
Caste: General	0.000	0.000	0.010	0.009	0.569
Caste: BC	0.145	0.140	0.101	0.174	0.488
Caste: MBC	0.400	0.383	0.364	0.322	0.545
Caste: OBC	0.036	0.019	0.020	0.035	0.796
Caste: SC	0.273	0.243	0.333	0.357	0.190
Caste: ST	0.027	0.047	0.061	0.026	0.423
Years at the stand	9.818	9.785	11.000	9.313	0.574
Any schooling	0.841	0.888	0.796	0.833	0.395
Education: Primary	0.391	0.327	0.303	0.270	0.254
Education: Secondary	0.391	0.495	0.394	0.487	0.272
Education: High school	0.018	0.056	0.030	0.026	0.502
Education: Any college	0.018	0.056	0.030	0.026	0.502
Had an economic shock	0.655	0.486	0.400	0.465	0.002
Number of children	2.147	2.290	2.096	2.132	0.493
Has bank account	0.460	0.523	0.501	0.561	0.537
Current daily wage	1063.182	1071.963	1065.657	1071.304	0.730
Taken debt at the stand	0.064	0.028	0.041	0.018	0.324
Number of friends at the stand	6.964	6.075	6.000	6.557	0.662
Alcohol consumption (days per week)	2.473	2.252	2.384	2.670	0.603
Observations	110	107	99	115	

Notes: The table reports baseline balance across treatment arms in the weak signal experiment. The last column reports the p-value from regressing the covariate on treatment indicators with stand fixed effects, and conducting a Wald test of joint significance of the regression coefficients. Missing covariate values are replaced with the sample mean.

Table 19: Balance for Information Sharing Experiment 3

	Shock-Pos	Shock-Neg	NoShock-Pos	NoShock-Neg	Joint Sig F-test
Age	45.853	44.423	45.568	46.421	0.337
Hindu	0.890	0.885	0.874	0.916	0.884
Birthplace outside of Chennai	0.752	0.788	0.856	0.869	0.036
Visit other stands	0.064	0.077	0.072	0.056	0.969
Lives in own house	0.193	0.096	0.090	0.150	0.123
Has own phone	0.734	0.712	0.721	0.776	0.825
Caste: General	0.000	0.010	0.009	0.000	0.453
Caste: BC	0.128	0.115	0.135	0.187	0.615
Caste: MBC	0.394	0.404	0.324	0.346	0.646
Caste: OBC	0.046	0.000	0.054	0.009	0.052
Caste: SC	0.257	0.298	0.351	0.299	0.439
Caste: ST	0.028	0.038	0.027	0.065	0.482
Years at the stand	10.532	10.077	9.441	9.748	0.786
Any schooling	0.841	0.854	0.791	0.877	0.194
Education: Primary	0.330	0.365	0.234	0.364	0.135
Education: Secondary	0.413	0.452	0.505	0.402	0.414
Education: High school	0.046	0.010	0.027	0.047	0.315
Education: Any college	0.046	0.010	0.027	0.047	0.315
Had an economic shock	0.509	0.505	0.521	0.480	0.998
Number of children	2.098	2.012	2.200	2.352	0.061
Has bank account	0.505	0.409	0.545	0.585	0.043
Current daily wage	1072.477	1064.423	1060.811	1074.766	0.473
Taken debt at the stand	0.019	0.049	0.036	0.047	0.683
Number of friends at the stand	7.330	6.038	5.270	7.028	0.124
Alcohol consumption (days per week)	2.541	2.365	2.811	2.065	0.019
Observations	109	104	111	107	

Notes: The table reports baseline balance across treatment arms in the peer economic-shock experiment. The last column reports the p-value from regressing the covariate on treatment indicators with stand fixed effects, and conducting a Wald test of joint significance of the regression coefficients. Missing covariate values are replaced with the sample mean.

Table 20: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Positive-Nonanonymous	-0.187***	-0.196***	-0.198**
	[0.066]	[0.061]	[0.066]
Positive-Nonanonymous*Above median SDS score	0.153	0.182**	0.188**
	[0.094]	[0.064]	[0.072]
Negative-Nonanonymous	-0.353***	-0.369***	-0.372***
	[0.064]	[0.078]	[0.080]
Negative-Nonanonymous*Above median SDS score	0.086	0.102	0.125
	[0.093]	[0.091]	[0.091]
Negative-Anonymous	-0.100	-0.115**	-0.118**
	[0.065]	[0.051]	[0.054]
Negative-Anonymous*Above median SDS score	0.007	0.034	0.042
	[0.092]	[0.056]	[0.062]
Above-Median SDS	-0.004	-0.028	-0.031
	[0.066]	[0.036]	[0.035]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.013	0.028	0.031
Neg-nonanon=neg-anon	0.000	0.008	0.007
Observations	426	426	426
R-squared	0.115	0.189	0.221
Clusters	0	13	13

Notes: The omitted category is positive information shared anonymously, for which the mean of the dependent variable is 0.981. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 21: Weak Signal Experiment

	Share information (1)	Share information (2)	Share information (3)
Aggregate-Nonanonymous	0.011	0.024	0.062
	[0.071]	[0.050]	[0.053]
Aggregate-Nonanonymous*Above median SDS score	-0.115	-0.133	-0.159*
	[0.102]	[0.086]	[0.079]
Negative-Nonanonymous	-0.291***	-0.294***	-0.264***
	[0.071]	[0.091]	[0.086]
Negative-Nonanonymous*Above median SDS score	0.130	0.136	0.116
	[0.100]	[0.086]	[0.077]
Negative-Anonymous	-0.076	-0.081	-0.058
	[0.069]	[0.079]	[0.091]
Negative-Anonymous*Above median SDS score	-0.108	-0.092	-0.132
	[0.100]	[0.081]	[0.097]
Above-Median SDS	0.074	0.064	0.080
	[0.069]	[0.053]	[0.052]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.004	0.006	0.004
Neg-nonanon=agg-nonanon	0.000	0.002	0.001
Observations	426	426	426
R-squared	0.068	0.125	0.178
Clusters	0	13	13

Notes: The omitted category is aggregate information shared anonymously, for which the mean of the dependent variable is 0.922. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 22: Paternalistic Motives Experiment

	Share information (1)	Share information (2)	Share information (3)
NoShock-Negative	-0.097	-0.144**	-0.144*
	[0.068]	[0.063]	[0.072]
NoShock-Negative*Above median SDS score	-0.132	-0.119	-0.120
	[0.096]	[0.083]	[0.089]
Shock-Positive	0.016	-0.003	0.010
	[0.063]	[0.072]	[0.084]
Shock-Positive*Above median SDS score	-0.039	-0.032	-0.051
	[0.096]	[0.076]	[0.098]
Shock-Negative	-0.134*	-0.178***	-0.158***
	[0.070]	[0.045]	[0.046]
Shock-Negative*Above median SDS score	-0.129	-0.095	-0.123
	[0.097]	[0.093]	[0.083]
Above-Median SDS	0.078	0.067	0.076
	[0.067]	[0.054]	[0.060]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.619	0.640	0.868
Observations	426	426	426
R-squared	0.070	0.115	0.135
Clusters	0	13	13

Notes: The omitted category is positive information shared with a peer with no information on economic shock, for which the mean of the dependent variable is 0.937. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors are clustered at the stand level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 23: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Positive-Nonanonymous	-0.118*	-0.115*	-0.114*
	[0.068]	[0.057]	[0.061]
Positive-Nonanonymous*More workers than usual	0.034	0.021	0.015
	[0.095]	[0.079]	[0.085]
Negative-Nonanonymous	-0.232***	-0.239***	-0.232***
	[0.065]	[0.071]	[0.070]
Negative-Nonanonymous*More workers than usual	-0.178*	-0.183	-0.191*
	[0.094]	[0.105]	[0.095]
Negative-Anonymous	-0.087	-0.089**	-0.091*
	[0.064]	[0.039]	[0.045]
Negative-Anonymous*More workers than usual	-0.024	-0.026	-0.033
	[0.093]	[0.070]	[0.076]
More workers than usual	-0.003	0.075*	0.077
	[0.067]	[0.040]	[0.045]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.100	0.068	0.060
Neg-nonanon=neg-anon	0.025	0.048	0.049
Observations	416	416	416
R-squared	0.124	0.190	0.221
Clusters	0	13	13

Notes: The dependent variable is an indicator for whether the worker agreed to share the information. The omitted category is positive information shared anonymously, for which the mean of the dependent variable is 0.981. "More workers than usual" equals one if the worker was surveyed on a day when the number of workers at his stand was high relative to the previous week. For each stand-day, I take the largest half-hourly worker count between 6am and 10am and divide it by the stand's average over the previous seven days. Workers with a ratio above the sample median are coded as facing more workers than usual. Demographic controls include respondent's age, caste, tenure at the stand, education, incidence of an economic shock in the past one month, number of children, ownership of bank account, wage, debt, alcohol consumption, and number of friends at the stand. Standard errors in columns (2) and (3) are clustered at the stand level (13 clusters). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

A.4. Workers' accounts of positive and negative employer experiences

All participants were asked to record both positive and negative experiences about employers at the labor stand before being randomized into different information sharing treatment arms to limit recall bias. This section describes workers' accounts of employer quality, and characterizes them into different dimensions of the employment relationship like payment reliability, interpersonal treatment, working hours, fair pay, and other amenities.

Out of a sample of 431 workers, I identify 426 positive accounts and 411 substantive negative accounts from workers. The remaining entries comprise unclear fragments, unspecified 'No' or 'Nil' answers, four statements of no bad experience, and one refusal to answer. All responses were read and assigned thematic codes through AI-assisted interpretation of the original text. Coding was based on meaning in context rather than keyword frequency. Each response could receive several codes, but a theme was counted at most once within a response.

I distinguish between pay, interpersonal treatment, food, water and basic amenities, workload and hours, micromanagement, job access, practical or financial help, and transportation among others Pay includes payment timing, amounts, deductions, arrears and overtime compensation. Treatment includes respectful or friendly communication as well as anger, scolding, and insults. Table 24 provides a breakdown of worker responses into different themes.

Table 24: Thematic Breakdown of Positive and Negative Experiences

	Positive		Negative	
Theme	*N*	%	*N*	%
Pay and wage reliability	276	64.0	240	55.8
Interpersonal treatment	175	40.6	124	28.8
Food, water, and amenities	104	24.1	18	4.2
Workload and working hours	17	3.9	61	14.2
Autonomy and micromanagement	32	7.4	42	9.8
Continuous job provision	35	8.1	16	3.7
Practical or financial help	51	11.8	2	0.5
Transport and travel costs	25	5.8	5	1.2
Mentoring and skill development	15	3.5	1	0.2
Unfair pay	0	0.0	59	13.7
Alcohol or smoking	1	0.2	20	4.7

Notes: Workers' open-ended accounts of one positive and one negative experience with an employer at their stand. Accounts were recorded in Tamil before random assignment to the information-sharing arms. Percentages use all 431 positive and 430 negative responses as denominators. These include 5 positive and 19 negative entries that were unclear, blank, or reported no bad experience; one refusal is excluded. Responses were coded into themes with AI assistance, based on meaning rather than keywords. A response can receive several themes, but each theme is counted at most once per response. *Pay and wage reliability* covers payment timing, amounts, deductions, arrears, and overtime pay. *Unfair pay* covers paying less than the agreed wage; *Interpersonal treatment* covers both respectful or friendly communication and anger, scolding, or insults.

Pay is the most frequently mentioned theme in both sets of accounts: 276 positive responses (64.0 percent) and 240 negative responses (55.8 percent). Positive experiences commonly describe correct or timely payment. Negative experiences mention delayed wages, unpaid balances, commission deductions, payment below the amount promised, and uncompensated overtime. Because phrases such as 'correct salary' do not distinguish timing from adequacy, the broad pay category should not be interpreted as a measure of wage levels alone. Interpersonal treatment appears in 175 positive responses (40.6 percent) and 124 negative responses (28.8 percent). Treatment is mentioned without

pay in 66 positive and 92 negative experiences; the two themes co-occur in 109 positive and 32 negative experiences. Workers' description of experiences thus include an interpersonal dimension beyond references to wage compensation.

Positive experiences also describe food, water and amenities (24.1 percent), practical or financial help (11.8 percent), and access to work (8.1 percent). Negative accounts more often mention demanding work or hours (14.2 percent), broken verbal contracts (13.7 percent), and micromanagement (9.8 percent).

Next, I provide some excerpts of these recorded experiences by workers. Employer names are omitted. The examples illustrate the content of the themes and were not randomly selected.

Table 25: Examples of Workers' Experiences about Employers

Positive Information	Negative Information
"Provides tea, food, and water, speaks kindly, and treats me like a younger brother."	"Takes me to work saying the wage is INR 1,100, but gives only INR 800."
"Has helped me financially, gives salary advances, and provides regular work."	"Makes me work extra time but does not pay extra."
"Taught me the trade in six months."	"Calls me for work and then says there is no work."
"Helps us at work, does not pressure us, pays for overtime, and even buys medicine when we have a fever."	"Scolds using abusive words and speaks disrespectfully."

Notes: Excerpts are contextual English translations of responses recorded in transliterated Tamil. Employer names have been omitted.

A.5. Survey Instruments

Experiment 1: Jobs with different employers

We want to ask you a few questions regarding employers at the stand. Recall that we spoke with you last week about all the employers you have worked with, and asked you to rate them on different amenities offered by them. We did the same exercise with other workers at the stand and received information on employers' aggregate behavior from them.

Now here are two employers (that you have not worked with in the past but they visit the stand regularly), and here are the aggregate ratings. If given a choice to work with either of the two for different wages, could you tell us what Would you choose?

After this exercise, we will do a lottery at the stand. If your name is selected in the lottery, then you will be offered a job with this employer. We will not give you the names of the employers, but these will be chosen from a list of employers you have not worked with at the stand.

Employer 1: He is an employer who regularly visits the stand with a rating X

Employer 2: He is an employer who regularly visits the stand with a rating Y

(where X<<Y)

The rating that we have is aggregated on the following criteria:

1. Timely payment (1= Always late, 10= Always on time)

2. Respectful communication (1= never 10=always)

6. Feelings of respect at the workplace (1=Never, 10=Always)

9. Pays correct wages (1=Never, 10=Always)

12. Pays extra wages for over time (1= Never, 10= Always)

3. Micromanagement (1 = none, 10 = a lot)

8. Verbally abused at the workplace (1 = never, 10 = always)

8. Felt cheated at the workplace (1 = never, 10 = always)

Again, you do not know either of the two employers.

- Do you believe this rating?
    - If no, why not?
- What does an aggregate rating represent?
    - a) One person's opinion
    - b) The average of multiple users' ratings
    - c) The most recent rating only

- d) The highest rating received

(correct the participant if they answer incorrectly)

- For employer A, on a scale of 'worst (1)' to 'best (10)' employer that you have worked with in the past, where Would you place this person?
- For employer B, on a scale of 'worst (1)' to 'best (10)' employer that you have worked with in the past, where Would you place this person?

OPTION 1:

	Employer A (X/10)	Employer B (Y/10)
Choice 1:	1000	1000
Choice 2:	1000	1020
Choice 3:	1000	1040
Choice 4:	1000	1060
		…

OPTION 2:

	Employer A (X/10)	Employer B (Y/10)
Choice 1:	1000	1000
Choice 2:	980	1000
Choice 3:	960	1000
Choice 4:	940	1000
		…

(X>>Y)

Stop when the participant chooses Employer B

Rating an employer they have not worked with:

1. Have you ever worked with 'employer name' at the stand?
   - Yes
   - No
2. We collected information on all employers at the stand. The average employer at the stand has a rating of XX, and 1 out of Y people get a rating of 8 and above

   ${employer_name} He is an employer who comes regularly to the stand. Do you think you can guess what other people at the stand have rated him on all the above criteria?
   Note: If you do not know, then you can make a guess.

   - Integer between 1 and 10

Information sharing experiments

A. Demo round to make the participant comfortable with the tablet:

First, we are going to show you a demo on how to answer the next round of questions.

We will present a few situations and Would like to know how you Would respond to each one. We will read the question aloud, and then show you a screen with two options. One will be green which is "Yes" and the other will be Red which is "No". Please do not say your answer out loud. Once you make your selection, the survey will automatically move to the next question. We will not be able to see your responses, and there is no way to go back. So please feel free to answer honestly based on what you truly think

For the surveyor: Please move to the next slide, display the options to the participant, select any option and move the slide using your finger (Give them a quick demo of how it works)

Demo question: Do you like to eat rice?

B. Placebo round with a penalty:

Can you identify a friend of yours at the stand?

Now we are going to ask you to go up to your friend and share something you do not like about living in Chennai. Soon after we will run a lottery to see if you will be asked to do it in real life. If your name is selected in the lottery, you will be asked to share this information. If you fail to do so, then we will deduct Rs. 15 from today's final survey payment (50% of the total amount). Do you think you can go up to your friend and share information?

Congratulations! Your name is selected! Can you please share this information with your friend?

[For the surveyor] Did the participant speak with his friend? Yes/No

C. Instructions for the main experiments

For the next set of questions, we have matched you with another participant from the stand who is not in your friend group. You do not know this person.

There is no wrong or right answer here. People behave differently under different situations and we just want to understand how you would actually behave at the stand on a daily basis.

We are offering this service, but it's not something we endorse or feel like something that you should do. It's just an option.

After this exercise, we will do another lottery at the stand, and we will ask you to do one of the three questions we ask you. Which task you do, depends on the lottery. If you do not do what we ask you to do, then we will deduct payment from the final Rs. 200 we pay at the end of the study (exactly like we did in the placebo round)

Before the next set of questions, we will ask you do two sets of exercises:

Can you recall a 'positive' experience with an employer who regularly visits the stand?

Do you think this is common knowledge at the stand/ Do other people at the stand know about this employer's behavior? Yes/NoFor the surveyor:

1. Is the respondent very far from what we want them to think about?
2. Only give an example if they cannot think of an experience

Can you recall a 'negative' experience with an employer who regularly visits the stand?

Do you think this is common knowledge at the stand/ Do other people at the stand know about this employer's behavior? Yes/No

For the surveyor:

1. Is the respondent very far from what we want them to think about?
2. Only give an example if they cannot think of an experience

Experiment 2: Information sharing

[Participants will be randomized into one of four groups:

Group 1: Positive information and non-anonymous setting

Group 2: Positive information and anonymous setting

Group 3: Negative information and non-anonymous setting

Group 4: Negative information and anonymous setting

For the next set of questions, we have matched you with another participant from the stand who you do not know

[Each participant will be given only one text]

Text 1:

Would you like to share information on the 'positive' experience with the participant you have matched with? You will have to go up to the person and share this information yourself. We will disclose your name to the person to whom you are matched

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 2:

Would you like to share information on the 'positive' experience with the participant you have matched with? We will NOT disclose your name to the person to whom you are matched. We will facilitate this exchange and if you like you can go and verify this information too.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no , how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 3:

Would you like to share information on the 'negative' experience with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 4:

Would you like to share information on the 'negative' experience with the participant you have matched with? We will NOT disclose your name to the person to whom you are matched. We will facilitate this exchange and if you like you can go and verify this information too.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Experiment 3: Information sharing with aggregate experiences

For the next set of questions, we have matched you with another participant from the stand who you do not know. You do not know this person.

[Each participant will be given only one text]

[Participants will be randomized into one of four groups:

Group 1: Personal negative experience and non-anonymous setting

Group 2: Personal negative experience and anonymous setting

Group 3: Aggregate information about an employer and non-anonymous setting

Group 4: Aggregate information about an employer and anonymous setting

After this exercise, we will do a lottery at the stand. If your name is selected in the lottery, then we will do exactly what you answered in the survey

Text 1:

Would you like to share information on the 'negative' experience with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 2:

Would you like to share information on the 'negative' experience with the participant you have matched with? We will NOT disclose your name to the person to whom you are matched. We will facilitate this exchange and if you like you can go and verify this information too.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 3:

Now I am going to tell you about an employer who we collected data on and their aggregate rating from all workers at the stand.

Would you like to share this information with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 4:

Now I am going to tell you about an employer who we collected data on and their aggregate rating from all workers at the stand

Would you like to share this information with the participant you have matched with? We will NOT disclose your name to the person to whom you are matched. We will facilitate this exchange and if you like you can go and verify this information too.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Experiment 4: Information sharing with peer information

For the next question, we have matched you with another person at the stand. You do not know this person.

[Participants will be randomized into one of four groups

Group 1: Information on peer economic shock and sharing positive information

Group 2: Information on peer economic shock and sharing negative information

Group 3: No information on peer economic shock and sharing positive information

Group 4: No information on peer economic shock and sharing negative information

[Each participant will be given only one text]

Text 1:

Now we have matched you with another person at the stand and they have recently undergone a major economic shock in their life where they lost a sufficient amount of their income or savings.

Would you like to share information on the 'positive' experience with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 2:

Now we have matched you with another person at the stand and they have recently undergone a major economic shock in their life where they lost a sufficient amount of their income or savings.

Would you like to share information on the 'negative' experience with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 3:

We have matched you with a person whose favourite color is blue. Would you like to share information on the 'positive' experience with the participant you have matched with? We will disclose your name to

the person to whom you are matched. You will have to go up to the person and share this information yourself.

-
- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment

Text 4:

We have matched you with a person whose favourite color is blue. Would you like to share information on the 'positive' experience with with the participant you have matched with? We will disclose your name to the person to whom you are matched. You will have to go up to the person and share this information yourself.

- Would you be willing to accept Rs. 20 as payment to share this information?
- If no, how about Rs. 40? [Keep increasing by Rs. 20]
- Stop, when the participant accepts the payment