EMPLOYER QUALITY AND INFORMATION FRICTIONS IN INFORMAL LABOR MARKETS*

Vrinda Kapoor†

September 11, 2026

PRELIMINARY AND INCOMPLETE. PLEASE DO NOT CITE OR CIRCULATE.

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. Exploratory 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.

*I am grateful to Heather Schofield, Nancy Chau, and Michèle Belot for their invaluable support and guidance. I also thank Sule Alan, Chris Barrett, Julieta Caunedo, Luisa Cefalà, Brian Dillon, Johannes Haushofer, Mridul Joshi, Ted O'Donoghue, Avantika Prabhakar, Daniela Scur, Nick Swanson, and seminar participants at Cornell University, PacDev, BREW-ESA India Meeting, ESA World Meetings, and the AAEA Annual Meeting for helpful comments and suggestions. Bruno Richardson provided excellent research assistance. This research was funded by the Tata-Cornell Institute, Cornell Center for Social Sciences, Einaudi Center for International Research, Cornell Center for the Study of Inequality, Hatfield Award for the Study of Ethics in Business, IHS Humane Studies Fellowship, and The International Growth Center. Field work was approved by Cornell University's Institutional Review Board under protocol #IRB0149815 and by the Institution for Financial Management and Research, and the study design was pre-registered in the AEA RCT Registry under AEARCTR-0016337. All errors and omissions are my own.

†Dyson School of Applied Economics and Management, Cornell University (vk289@cornell.edu)

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, two conditions likely 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 with employers 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 thirteen dimensions of workplace amenities and employer quality. I use eight of these dimensions, covering both wage-related and non-wage amenities, to construct an aggregate measure of employer quality[1].

[1]The measure includes wage related amenities like timely payment, felt cheated at the workplace, pays correct

I then offer workers incentive-compatible job choices between higher- and lower-rated employers at varying wage rates to measure their valuation of employer quality. 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.

I then ask why poor employer behavior persists if workers value employer quality. 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 on employer conduct. To further examine the conditions under which workers are willing to share information on employer, I randomize whether whether the worker's identity is revealed or not when the information is shared. 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 88.5 percent when the information is negative. When negative information is instead linked to the worker's identity, sharing declines further, to 66.9 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 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 evidence on the remaining through heterogeneity tests. 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 10.3 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 21.6 percentage points. This pattern suggests that workers are particularly reluctant to be personally associated with negative information about employers. Threat of employer retaliation is one plausible explanation for this result.

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

Another channel that could be driving this result is workers' self image concerns. If workers think that the information on employer misconduct reflects their quality as a worker and reveals it to the market, then workers will be more reluctant to share negative information on employer behavior than positive information. To check for this, I ask workers whether being treated badly reveals that they are a bad worker. I examine heterogeneity by this measure and find no significant differences in treatment effects between workers who report self-image concerns and those who do not.

In the next experiment, I test another mechanism: 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. 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. This suggests that workers are more comfortable sharing information when it is based on aggregated worker reports rather than on their own personal experience. 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. In this experiment, I randomize whether the peer receiving the information is described as having recently experienced an economic shock. This is again cross-randomized with whether workers' identity is revealed or not. I find little evidence 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 also 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. Another natural concern is whether these results appear to be explained by general shyness or 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 or shyness, then I would not observe differential sharing patterns for negative and positive information sharing. I also observe high levels of information sharing when aggregated information is shared and worker identity is revealed.

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. Despite these frictions in information sharing, workers in my sample demonstrate some demand for aggregated employer information. 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. This suggests that workers do value third-party aggregation of employer quality information.

Finally, 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 18 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%. 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 literatures. 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 their willingness to pay for employer quality, encompassing both respectful 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 job search 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 towards 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. I study an earlier and necessary stage in this process, that is whether privately observed experiences are shared widely enough to shape employers' reputations. Despite repeated interactions among workers and employers, I find that negative experiences are selectively withheld, particularly when workers can be identified. This result identifies a boundary condition for reputation-based enforcement. Repeated interaction alone need not discipline employers when the information required to reward good conduct and punish misconduct does not reach the market. Thus, information frictions in this scenario weaken employers' incentives to provide better job quality.

The remainder of the paper proceeds as follows. Section 2 describes the context of urban spot labor markets in India. Section 3 details 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 aggregated 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.

[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.

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.

3. Research Design

3.1. Hypotheses

The goal of this study is to understand if the ingredients of reputation building, demand of non-wage amenities and information flow, exist in spot labor markets. First, employer quality must affect workers' labor-supply decisions. Second, information about employer conduct must be shared with other workers. The first two hypotheses test these conditions.

- **H1. Valuation of employer quality.** On average, workers require a positive compensating wage differential to accept a job with a lower-rated employer rather than a higher-rated employer.
- **H2. Asymmetric information sharing.** Workers are less likely to share negative experiences with employer than positive experiences.

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

- **H3. Costs of identifiability.** 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.
  Note that H3. is consistent with anticipated employer retaliation, but the study design does not distinguish retaliation from other costs of being associated with sharing negative information.
- **H4. Individual experiences are random representation of employer behavior.** Workers are less likely to share negative information on employer behavior based on their own experience than employer information aggregated across multiple workers.
- **H5. Concern about recipients' employment opportunities.** 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.

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 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

[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.

employer ratings (on employers on whom I had at least two different sets of ratings) collected during the study. The participants received Rs 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. Appendix section A.3 documents positive and negative experiences shared by workers. Following this, the information sharing experiments are conducted:

H2. Workers are less likely to share negative experiences with employer 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 utilize 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 suggests 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.

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 with employers, 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 11 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, 50% 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 12 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 Non-Wage Amenities

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 a significant premium of an average 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 12 USD), which is striking since wages are low in this market, and workers face

[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.

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 this result. 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.

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

[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.

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:

(1)

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

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 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.

[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.

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 10.2 percentage points, and identity revelation lowers information sharing by 10.3 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 13). This is also true when we only compare the experimental settings where identity is hidden. Thus, participants are less likely to share negative experiences with employers 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.

To alleviate concerns about worker's propensity to give a socially desirable answer, I execute on participant's decision to share information with an allocated peer at the end of all survey activities at a stand. Participants were not told when their experimental choice will be executed to avoid selection in attendance at the stand. 95.6% of participants who agreed to share information with a peer at the stand followed through on their decision [7]. However to address this concern rigorously, I construct a shorter social desirability scale (SDS) based on Crowne and Marlowe (1960), and implement the module during baseline surveys. Table 20 shows heterogeneity results by above median SDS score, and find no meaningful differences by participants who are more likely to give socially desirable answers.

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

[7] This included all experiments under Tables 2, 3, and 4. 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.

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 10.3 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 21.6 percentage points. The predicted sharing rate falls from 98.1 percent in the positive-anonymous condition to approximately 66.3 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:

(2)

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

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 12.7 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, 90.3 percent in the aggregate-non-anonymous condition, 79.5 percent in the personal-negative anonymous condition, and 73.4 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:

$$Y_{is} = \alpha + \beta_1 \text{Shock}_{is} + \beta_2 \text{Negative}_{is} + \beta_3 \left(\text{Shock}_{is} \times \text{Negative}_{is}\right) + X_i'\gamma + \delta_s + \varepsilon_{is} \tag{3}$$

Table 8 provides little support for this explanation. Negative information substantially reduces sharing: in the preferred specification, negative information lowers sharing by 20.9 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, 92.0 percent for shock positive information, 72.8 percent for no-shock negative information, and 71.2 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. One possible explanation is knowledge hoarding, that is workers may withhold information about good employers to preserve their own access to desirable jobs. Table 12 provide little evidence for this mechanism. The indicator for reporting that one would hide information about a good employer is not significantly associated with sharing in the reference condition, and nearly all interactions between this measure and the experimental conditions are statistically insignificant. The only exception is Experiment 1, where the interaction with the negative non-anonymous condition is positive and weakly significant. In the specification with stand fixed effects and demographic controls, the estimated reduction in sharing under negative non-anonymity is 34.6 percentage points among workers who do not report hiding information, while the corresponding interaction is 23.1 percentage points. Thus, workers who report a propensity to hoard information are, if anything, less discouraged from sharing by this condition. Taken together, the results do not support the view that the observed reluctance to share is driven primarily by workers strategically 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 measure are generally small and statistically insignificant. For example, in Experiment 1, the negative non-anonymous condition reduces sharing by 31.1 percentage points among the reference group, and the corresponding interaction is only -2.3 percentage points and is not statistically significant. The interactions with the negative-information conditions are also insignificant in Experiments 2 and 3. There is some weak evidence in Experiment 1 that identity revelation has a smaller effect when the information is positive among respondents who endorse reciprocal behavior, but this pattern does not extend to the negative-information conditions. Overall, the reluctance to share negative information is not concentrated among workers who report that mistreatment would lead them to reciprocate with poor performance.

Anonymity and shyness. A third possibility is that the effect of identity revelation reflects general shyness or 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 shyness, 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.

6. Demand for Aggregated Employer Information

6.1. Demand for Scorecards

In this paper, I identify a significant problem in this complicated urban spot labor market. Next, I attempt to solve this problem with the tools immediately available to me. 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 7). 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 16 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 78% 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 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

[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.

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:

$$Y_{iqt} = \alpha_q + \beta_q \text{Received}_i + \gamma_q \text{Post}_t + \delta_q \left(\text{Received}_i \times \text{Post}_t\right) + \lambda_{s(i),q} + \varepsilon_{iqt}, \tag{4}$$

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 (4) can equivalently be estimated in first differences:

$$\Delta Y_{iq} = \alpha_q + \delta_q \text{Received}_i + \lambda_{s(i),q} + \Delta\varepsilon_{iq}, \qquad q \in \{H, L\}, \tag{5}$$

where

$$\Delta Y_{iq} = Y_{iq,\text{post}} - Y_{iq,\text{pre}}. \tag{6}$$

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

$$\widehat{\delta}_H = \left(\overline{Y}^R_{H,\text{post}} - \overline{Y}^R_{H,\text{pre}}\right) - \left(\overline{Y}^N_{H,\text{post}} - \overline{Y}^N_{H,\text{pre}}\right), \tag{7}$$

while the corresponding effect for lower-rated employers is

$$\widehat{\delta}_L = \left(\overline{Y}^R_{L,\text{post}} - \overline{Y}^R_{L,\text{pre}}\right) - \left(\overline{Y}^N_{L,\text{post}} - \overline{Y}^N_{L,\text{pre}}\right), \tag{8}$$

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. Define the worker-level

difference between higher- and lower-rated employer matches as

$$G_{it} = Y_{iHt} - Y_{iLt}. \tag{9}$$

I then estimate

$$\Delta G_i = \alpha + \delta^{DDD}\text{Received}_i + \lambda_{s(i)} + \Delta\varepsilon_i, \tag{10}$$

where

$$\delta^{DDD} = \delta_H - \delta_L. \tag{11}$$

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. Employers

Finally to understand the other side of this labor market, I also survey 132 employers at the labor stands. 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 8a 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 8b). 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 9a 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 9b 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 10 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.

References

Abebe, Girum, Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn, "Anonymity or distance? job search and labour market exclusion in a growing African city," *Review of Economic Studies*, 2021, *88* (3), 1279–1310.

Alan, Sule, Gozde Corekcioglu, and Matthias Sutter, "Improving Workplace Climate in Large Corporations: A Clustered Randomized Intervention," *Quarterly Journal of Economics*, 2023, *138* (1), 151–203.

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja, "Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets," *American Economic Review*, 2017, *107* (6), 1535–1563.

Annan, Francis, "Misconduct and reputation under imperfect information," *Journal of Political Economy*, 2025, *133* (5), 1460–1496.

Bandiera, Oriana, Vittorio Bassi, Robin Burgess, Imran Rasul, Munshi Sulaiman, and Anna Vitali, "The Search for Good Jobs: Evidence from a Six-Year Field Experiment in Uganda," *Journal of Labor Economics*, 2025, *43* (3), 885–935.

Barbanchon, Thomas Le, Roland Rathelot, and Alexandra Roulet, "Gender Differences in Job Search: Trading off Commute against Wage," *Quarterly Journal of Economics*, 2021, *136* (1), 381–426.

Bassi, Vittorio and Ashish Nansamba, "Screening and Signaling Non-Cognitive Skills: Experimental Evidence from Uganda," 2021.

Beam, Emily A., "Do Job Fairs Matter? Experimental Evidence on the Impact of Job-Fair Attendance," *Journal of Development Economics*, 2016, *120*, 32–40.

Blattman, Christopher and Stefan Dercon, "The Impacts of Industrial and Entrepreneurial Work on Income and Health: Experimental Evidence from Ethiopia," *American Economic Journal: Applied Economics*, 2018, *10* (3), 1–38.

Boudreau, Laura, "Multinational enforcement of labor law: Experimental evidence on strengthening occupational safety and health committees," *Econometrica*, 2024, *92* (4), 1269–1308.

_ , **Rachel Heath, and Tyler H McCormick**, "Migrants, experience, and working conditions in Bangladeshi garment factories," *Journal of Economic Behavior & Organization*, 2024, *219*, 196–213.

Breza, Emily and Supreet Kaur, "Labor Markets in Developing Countries," *Annual Review of Economics*, 2025, *17*.

_ , _ , **and Nandita Krishnaswamy**, "Social norms as a determinant of aggregate labor supply," *Journal of Political Economy*, 2026. Forthcoming.

Buchmann, Nina, Carl Meyer, and Colin Sullivan, "Paternalistic Discrimination," *Econometrica*, 2026. Forthcoming.

Carmichael, H Lorne, "Reputations in the labor market," *The American Economic Review*, 1984, *74* (4), 713–725.

Carranza, Eliana, Robert Garlick, Kate Orkin, and Neil Rankin, "Job Search and Hiring with Limited Information about Workseekers' Skills," *American Economic Review*, 2022, *112* (11), 3547–3583.

Cefala, Luisa, Supreet Kaur, Heather Schofield, and Yogita Shamdasani, "Habit Formation in Labor Supply," 2026.

Chakravorty, Bhaskar, Wiji Arulampalam, Apurav Yash Bhatiya, Clément Imbert, and Roland Rathelot, "Can information about jobs improve the effectiveness of vocational training? Experimental evidence from India," *Journal of Development Economics*, 2024, *169*, 103273.

Chen, M. Keith, Judith A. Chevalier, Peter E. Rossi, and Emily Oehlsen, "The Value of Flexible Work: Evidence from Uber Drivers," *Journal of Political Economy*, 2019, *127* (6), 2735–2794.

Conlon, John J, Laura Pilossoph, Matthew Wiswall, and Basit Zafar, "Labor market search with imperfect information and learning," Technical Report, National Bureau of Economic Research 2018.

Cordes, Simon and Max Müller, "Do Firms Know What Workers Want?," Discussion Paper 739, CRC TR 224 2026.

Crowne, Douglas P and David Marlowe, "A New Scale of Social Desirability Independent of Psychopathology.," *Journal of consulting psychology*, 1960, *24* (4), 349.

Dammert, Ana C., Jose C. Galdo, and Virgilio Galdo, "Digital Labor-Market Intermediation and Job Expectations: Evidence from a Field Experiment," *Economics Letters*, 2013, *120* (1), 112–116.

Dhar, Diva, Tarun Jain, and Seema Jayachandran, "Reshaping Adolescents' Gender Attitudes: Evidence from a School-based Experiment in India," *American Economic Review*, 2022, *112* (3), 899–927.

Dranove, David and Ginger Zhe Jin, "Quality Disclosure and Certification: Theory and Practice," *Journal of Economic Literature*, 2010, *48* (4), 935–963.

Fernando, A. Nilesh and Niharika Singh, "Regulation by Reputation? Intermediaries, Labor Abuses, and International Migration," *Review of Economics and Statistics*, 2026. Forthcoming.

Garlick, Robert, Erica Field, and Kate Vyborny, "Women's Mobility and Labor Supply: Experimental Evidence from Pakistan," 2025. IZA Discussion Paper No. 17883.

Ho, Lisa, Suhani Jalota, and Anahita Karandikar, "Bringing Work Home: Flexible Work Arrangements as Gateway Jobs for Women in West Bengal," 2024. Working paper.

International Labour Organization, "Sectoral Studies on Decent Work in Global Supply Chains," Technical Report 2016.

Jensen, Robert, "Do Labor Market Opportunities Affect Young Women's Work and Family Decisions? Experimental Evidence from India," *Quarterly Journal of Economics*, 2012, *127* (2), 753–792.

Kapoor, Varun, "Wage Theft, Reneging and Liquidity Constraints in Informal Labor Markets," 2025. Working paper.

Karpoff, Jonathan M., "Does Reputation Work to Discipline Corporate Misconduct?," in Timothy G. Pollock and Michael L. Barnett, eds., *Oxford Handbook of Corporate Reputation*, Oxford: Oxford University Press, 2012, pp. 361–382.

Kaur, Supreet, "Nominal Wage Rigidity in Village Labor Markets," *American Economic Review*, 2019, *109* (10), 3585–3616.

Klein, Benjamin and Keith B Leffler, "The Role of Market Forces in Assuring Contractual Performance," *Journal of political Economy*, 1981, *89* (4), 615–641.

Knight Frank and RICS, "Skilled Employment in Construction Sector in India," Technical Report, Knight Frank India and Royal Institution of Chartered Surveyors 2023.

Macchiavello, Rocco and Ameet Morjaria, "The Value of Relationships: Evidence from a Supply Shock to Kenyan Rose Exports," *American Economic Review*, 2015, *105* (9), 2911–2945.

_ and _, "Competition and Relational Contracts in the Rwanda Coffee Chain," *Quarterly Journal of Economics*, 2021, *136* (2), 1089–1143.

Maestas, Nicole, Kathleen J. Mullen, David Powell, Till von Wachter, and Jeffrey B. Wenger, "The Value of Working Conditions in the United States and Implications for the Structure of Wages," *American Economic Review*, 2023, *113* (7), 2007–2047.

Mas, Alexandre, "Non-Wage Amenities," in "Handbook of Labor Economics," Vol. 6, North-Holland, 2025, chapter 5, pp. 373–445.

_ and Amanda Pallais, "Valuing alternative work arrangements," *American Economic Review*, 2017, *107* (12), 3722–3759.

Oster, Emily and Bryce Millett Steinberg, "Do IT Service Centers Promote School Enrollment? Evidence from India," *Journal of Development Economics*, 2013, *104*, 123–135.

Reynolds, William M, "Development of reliable and valid short forms of the Marlowe-Crowne Social Desirability Scale," *Journal of clinical psychology*, 1982, *38* (1), 119–125.

Shapiro, Carl, "Premiums for high quality products as returns to reputations," *Quarterly Journal of Economics*, 1983, *98* (4), 659–679.

Sockin, Jason and Aaron Sojourner, "What's the inside scoop? Challenges in the supply and demand for information on employers," *Journal of Labor Economics*, 2023, *41* (4), 1041–1079.

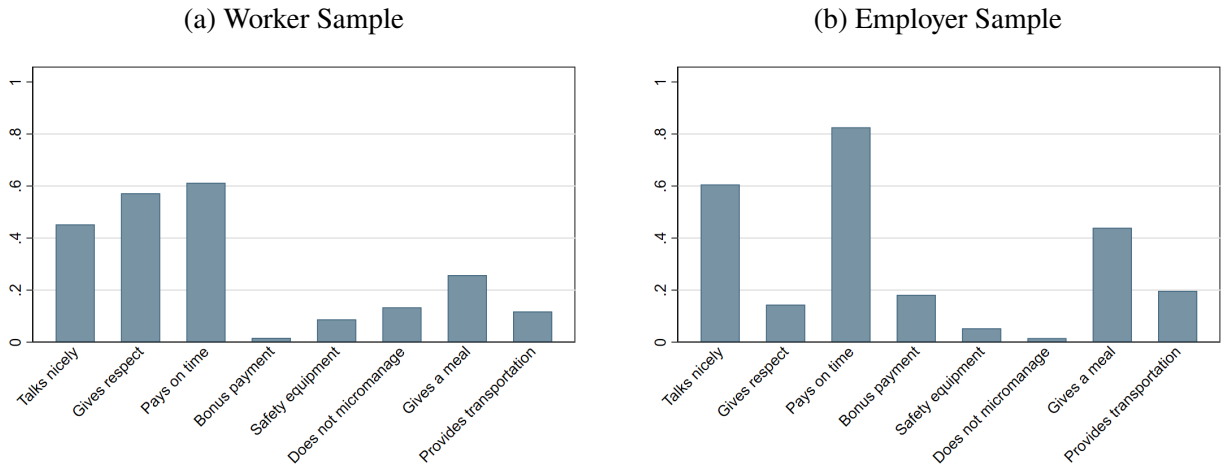
Sorkin, Isaac, "Ranking firms using revealed preference," *Quarterly Journal of Economics*, 2018, *133* (3), 1331–1393.

Tandon, Shreya, "Job Exploration Risk and Spousal Co-working: Evidence from India," 2025. Working paper.

Wiswall, Matthew and Basit Zafar, "Preference for the workplace, investment in human capital, and gender," *Quarterly Journal of Economics*, 2018, *133* (1), 457–507.

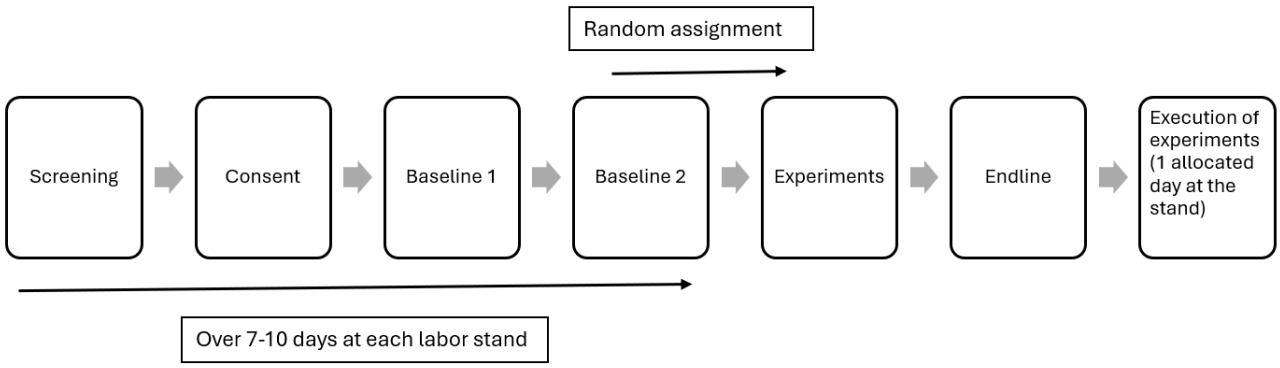
A. Figures

FIGURE 1: Amenities Valued by Workers



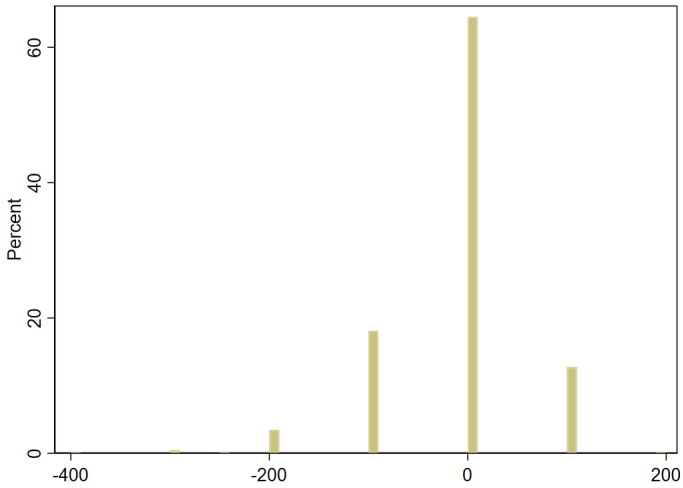
Notes: Panel A reports workers' responses to the question of who is a good employer. Panel B reports employers' beliefs about what workers value in an employer. Responses are not mutually exclusive.

FIGURE 2: Timeline of Survey Activities at Each Stand



Notes: The figure summarizes the sequence of baseline surveys, experimental activities, scorecard distribution, and follow-up surveys conducted at each labor stand.

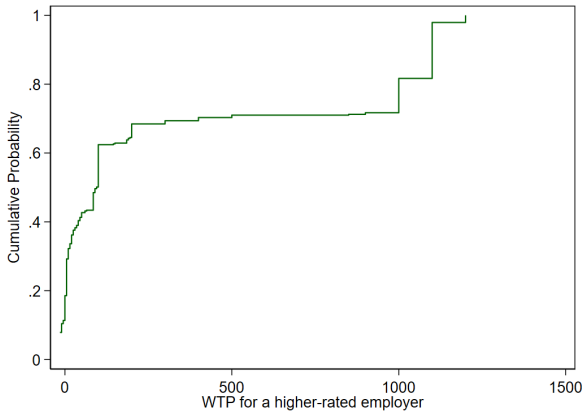
FIGURE 3: Wage Deviation from Stand Mode



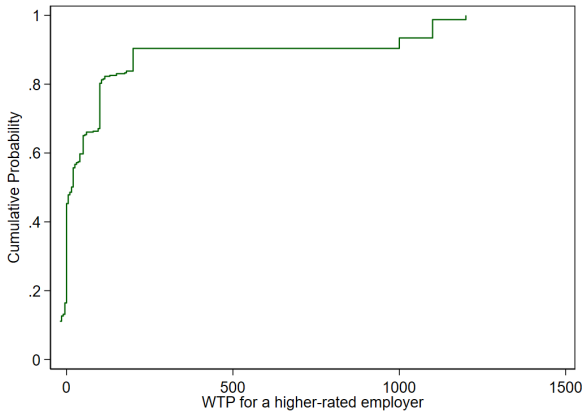
Notes: The figure plots deviations of reported wages from the modal wage at the labor stand. The modal stand wage is used as the benchmark for within-stand wage deviations.

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

(a) Demand a Premium



(b) Go Below the Wage Floor



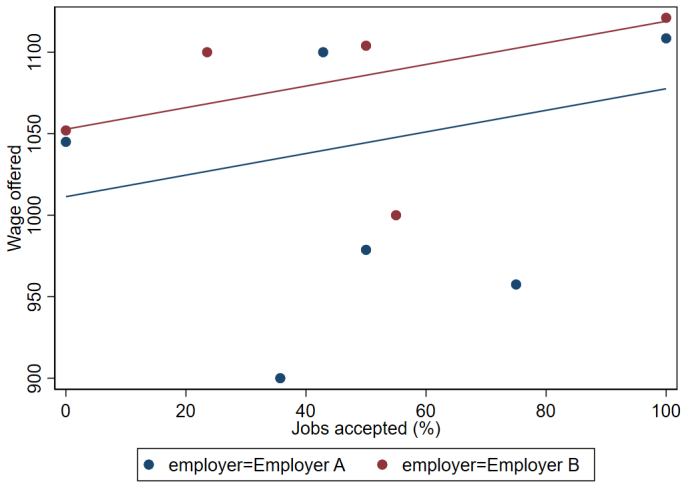
Notes: The figures plot the distribution of workers' willingness to pay for a higher-rated employer across the two job choice experiments. WTP is measured in rupees.

FIGURE 5: Relationship between Residualized Rating Differences and WTP



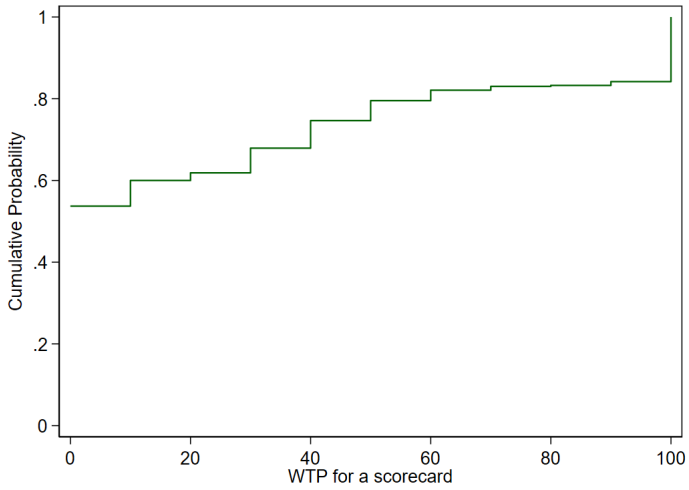
Notes: The figure plots willingness to pay for the higher-rated employer against the difference in employer ratings after residualizing both variables by stand. The fitted line is shown for descriptive purposes.

FIGURE 6: Job Acceptance Rates



Notes: The figure shows job acceptance rates by offered wage for the incentivized exercise. Points represent binned means; fitted lines summarize the relationship between wage offers and acceptance.

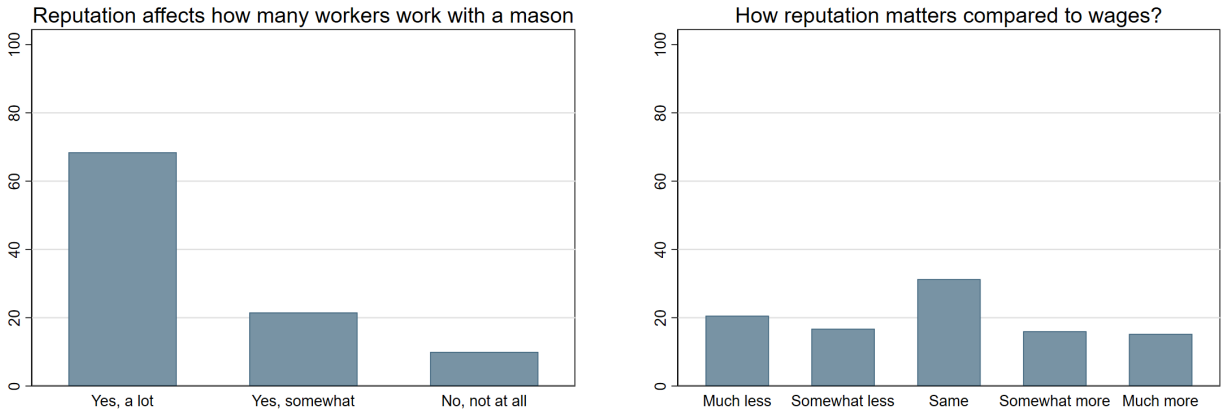
FIGURE 7: Willingness to Pay for a Scorecard



Notes: The figure plots the distribution of willingness to pay for employer scorecards from an INR 100 endowment. Scorecards contain aggregate employer ratings based on worker reports at the labor stand.

FIGURE 8: Importance of reputation for employers

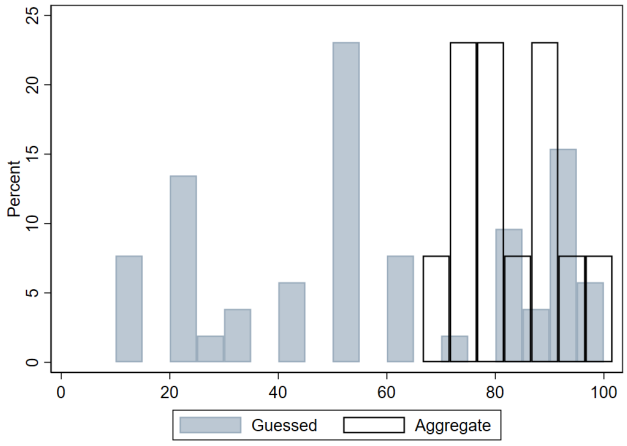
(a) (b)



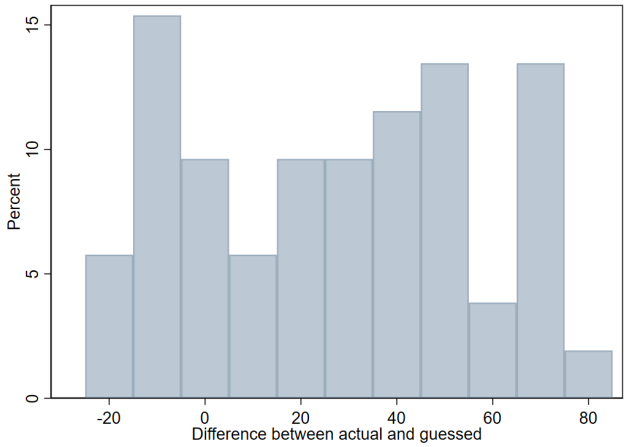
Notes: Panel A reports whether employers believe reputation affects the number of workers willing to work with a mason. Panel B reports employers' beliefs about the importance of reputation relative to wages.

FIGURE 9: Employers' Beliefs about Workers' Willingness to Pay

(a)

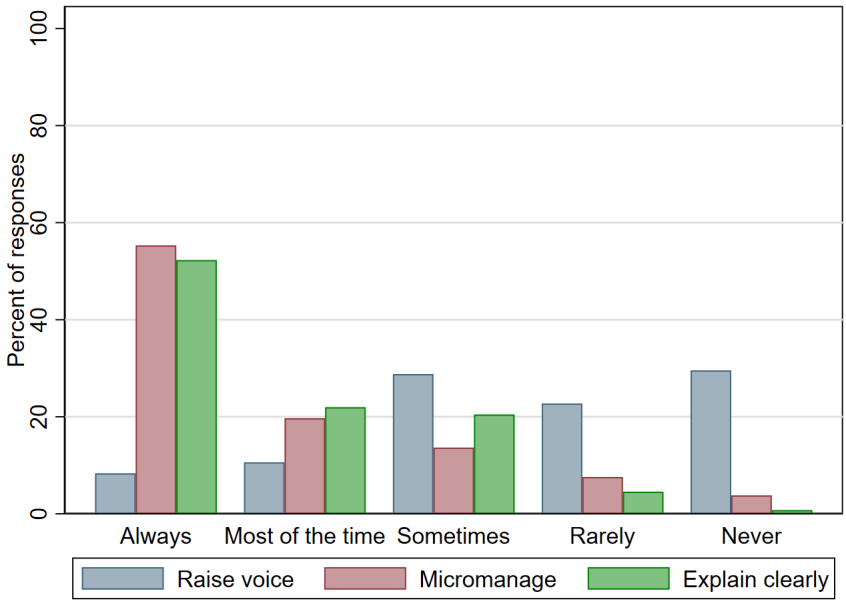


(b)



Notes: Panel A compares employers' incentivized guesses about the share of workers willing to choose a better-rated employer at lower pay with the actual aggregate worker choices. Panel B plots the difference between actual worker choices and employer beliefs.

FIGURE 10: Employer's Beliefs about Productivity Practices



Notes: The figure reports employers' beliefs about whether raising one's voice, micromanaging, and explaining clearly are necessary for worker productivity. Bars show the percent of employer responses within each practice.

B. 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

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 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, 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 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 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, 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 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, 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)
Pos-nonanon	-0.142**	-0.146**	-0.150**
	[0.062]	[0.054]	[0.053]
Pos-nonanon*Bad worker if treated badly	0.081	0.102*	0.101*
	[0.094]	[0.055]	[0.055]
Neg-nonanon	-0.308***	-0.318***	-0.311***
	[0.058]	[0.089]	[0.096]
Neg-nonanon*Bad worker if treated badly	-0.013	-0.007	-0.023
	[0.098]	[0.108]	[0.118]
Neg-anon	-0.084	-0.079	-0.079
	[0.059]	[0.051]	[0.061]
Neg-anon*Bad worker if treated badly	-0.030	-0.047	-0.060
	[0.093]	[0.071]	[0.088]
Bad worker if treated badly	-0.007	-0.018	-0.005
	[0.066]	[0.033]	[0.047]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.000	0.004	0.006
Neg-nonanon=neg-anon	0.000	0.008	0.020
Observations	431	431	431
R-squared	0.105	0.181	0.203
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, 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-nonanon	-0.034	-0.038	-0.019
	[0.065]	[0.044]	[0.052]
Aggregate-nonanon*Bad worker if treated badly	-0.007	0.004	0.010
	[0.106]	[0.082]	[0.097]
Neg-nonanon	-0.230***	-0.227**	-0.205**
	[0.066]	[0.093]	[0.094]
Neg-nonanon*Bad worker if treated badly	0.068	0.056	0.041
	[0.099]	[0.102]	[0.100]
Neg-anon	-0.159**	-0.166***	-0.159**
	[0.065]	[0.052]	[0.065]
Neg-anon*Bad worker if treated badly	0.106	0.128	0.101
	[0.102]	[0.075]	[0.069]
Bad worker if treated badly	0.043	0.022	0.032
	[0.069]	[0.027]	[0.034]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.001	0.014	0.031
Neg-nonanon=agg-nonanon	0.022	0.044	0.095
Observations	431	431	431
R-squared	0.060	0.115	0.152
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, 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-neg	-0.149**	-0.208***	-0.230***
	[0.061]	[0.058]	[0.063]
Noshock-neg*Bad worker if treated badly	-0.032	0.015	0.052
	[0.097]	[0.082]	[0.097]
Shock-pos	0.016	-0.002	-0.004
	[0.060]	[0.050]	[0.050]
Shock-pos*Bad worker if treated badly	-0.044	-0.043	-0.035
	[0.097]	[0.037]	[0.035]
Shock-neg	-0.212***	-0.237***	-0.252***
	[0.061]	[0.044]	[0.052]
Shock-neg*Bad worker if treated badly	0.039	0.034	0.067
	[0.098]	[0.084]	[0.077]
Bad worker if treated badly	0.027	0.024	0.006
	[0.068]	[0.035]	[0.035]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.001	0.000	0.000
Observations	431	431	431
R-squared	0.066	0.113	0.130
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, 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)
Pos-nonanon	-0.088*	-0.081*	-0.082*
	[0.051]	[0.038]	[0.040]
Pos-nonanon*Hides info on good employer	-0.112	-0.115	-0.135
	[0.127]	[0.087]	[0.091]
Neg-nonanon	-0.335***	-0.344***	-0.346***
	[0.050]	[0.087]	[0.089]
Neg-nonanon*Hides info on good employer	0.193	0.204*	0.231*
	[0.129]	[0.113]	[0.127]
Neg-anon	-0.099*	-0.100*	-0.105
	[0.051]	[0.052]	[0.059]
Neg-anon*Hides info on good employer	0.016	0.015	0.022
	[0.115]	[0.085]	[0.093]
Hide information on good employers	0.023	0.028	0.016
	[0.085]	[0.050]	[0.059]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.000	0.003	0.004
Neg-nonanon=neg-anon	0.000	0.004	0.006
Observations	430	430	430
R-squared	0.115	0.189	0.213
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, 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-nonanon	-0.046	-0.041	-0.018
	[0.056]	[0.035]	[0.035]
Aggregate-nonanon*Hides info on good employer	0.012	0.006	0.006
	[0.142]	[0.099]	[0.117]
Neg-nonanon	-0.210***	-0.214**	-0.195**
	[0.054]	[0.071]	[0.073]
Neg-nonanon*Hides info on good employer	0.018	0.040	0.039
	[0.142]	[0.165]	[0.184]
Neg-anon	-0.139**	-0.138*	-0.143*
	[0.054]	[0.068]	[0.072]
Neg-anon*Hides info on good employer	0.061	0.071	0.093
	[0.141]	[0.152]	[0.166]
Hide information on good employers	0.008	-0.001	-0.015
	[0.106]	[0.088]	[0.098]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.000	0.017	0.027
Neg-nonanon=agg-nonanon	0.007	0.004	0.015
Observations	430	430	430
R-squared	0.047	0.106	0.145
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, 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-neg	-0.146***	-0.185***	-0.196***
	[0.052]	[0.052]	[0.053]
Noshock-neg*Hides info on good employer	-0.096	-0.090	-0.068
	[0.122]	[0.094]	[0.084]
Shock-pos	0.013	-0.007	-0.010
	[0.052]	[0.038]	[0.037]
Shock-pos*Hides info on good employer	-0.109	-0.082	-0.065
	[0.125]	[0.129]	[0.114]
Shock-neg	-0.194***	-0.221***	-0.227***
	[0.052]	[0.053]	[0.059]
Shock-neg*Hides info on good employer	-0.028	-0.015	-0.002
	[0.126]	[0.118]	[0.120]
Hide information on good employers	-0.035	-0.034	-0.049
	[0.083]	[0.064]	[0.062]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.000	0.000	0.000
Observations	430	430	430
R-squared	0.074	0.117	0.133
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, 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

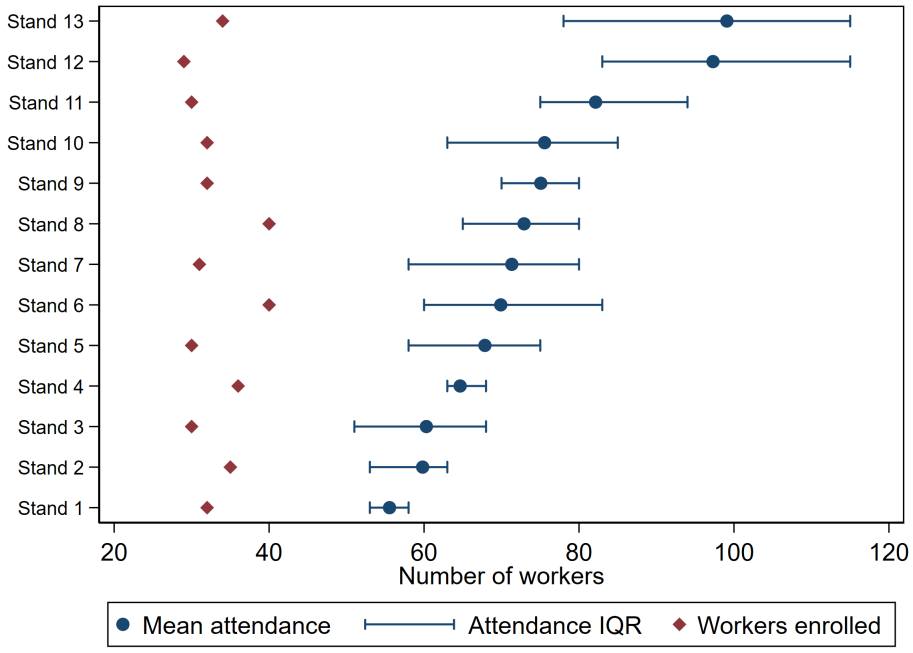
	High-rated matches (1)	Lower-rated matches (2)	High − lower difference (3)
Did not receive scorecard			
Before	0.547	0.554	-0.007
Follow-up	0.149	0.236	-0.088
Change	-0.399	-0.318	-0.081
Received scorecard			
Before	0.626	0.400	0.226
Follow-up	0.200	0.157	0.043
Change	-0.426	-0.243	-0.183
Difference-in-differences	-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

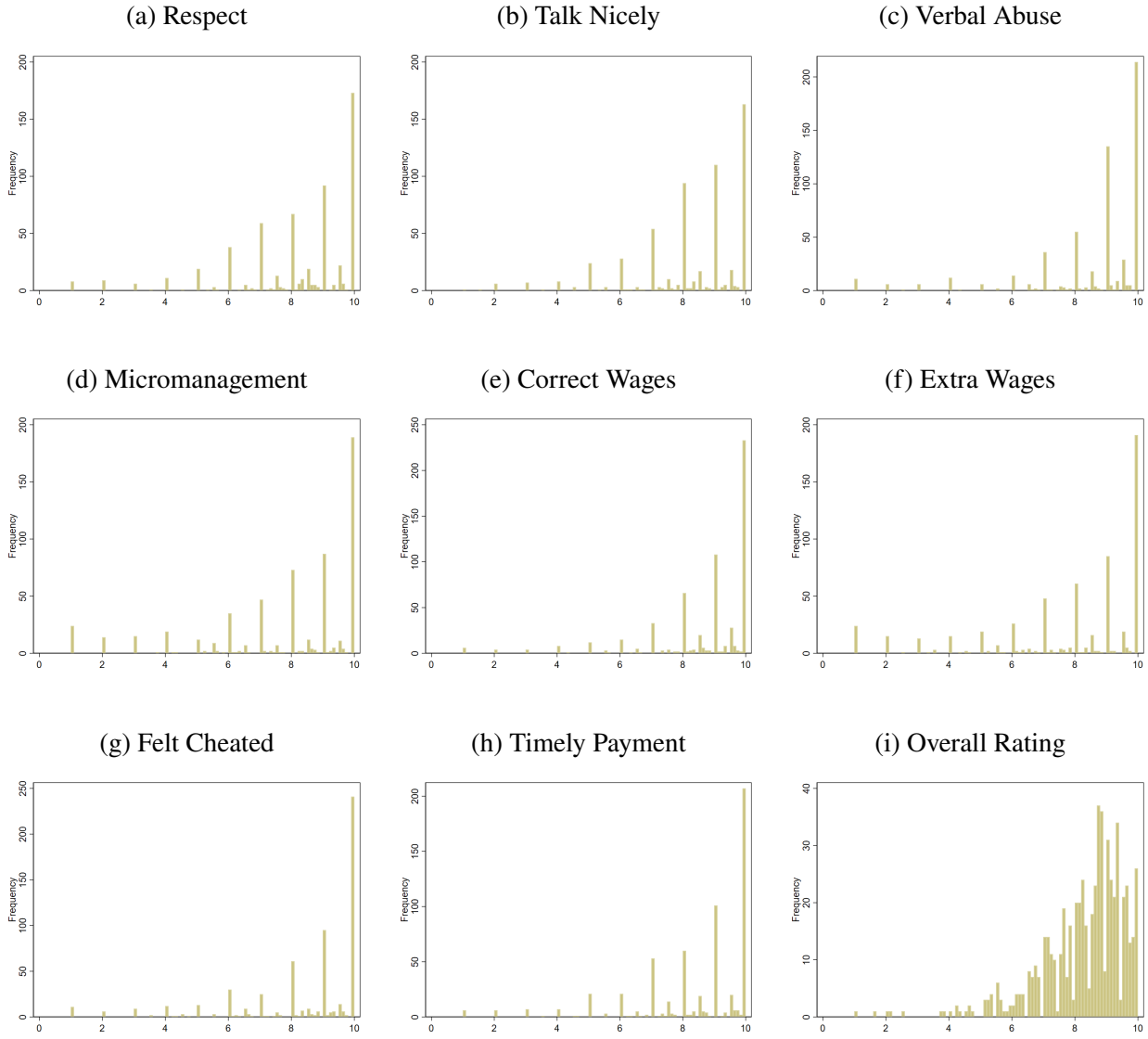
A.1. Figures

FIGURE 11: Worker attendance and study sample across labor stands



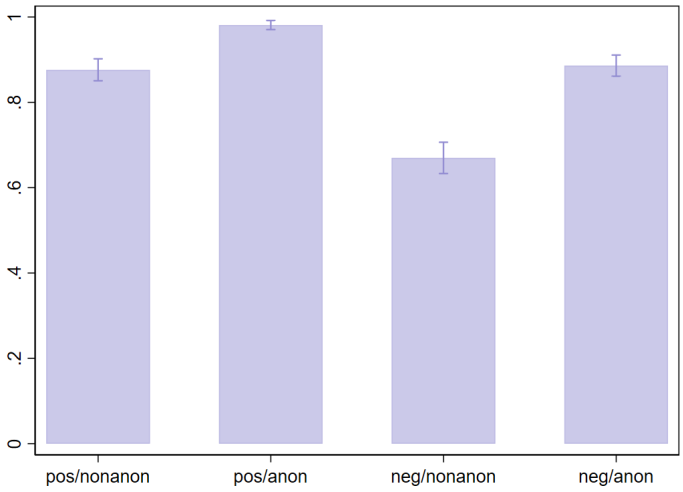
Notes: The figure compares worker attendance and study enrollment across the 13 construction labor stands. Stands are ordered by mean attendance. Blue circles indicate mean daily worker attendance, while horizontal lines show the interquartile range. Red diamonds indicate the number of workers from each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

FIGURE 12: Distribution of Employer Ratings



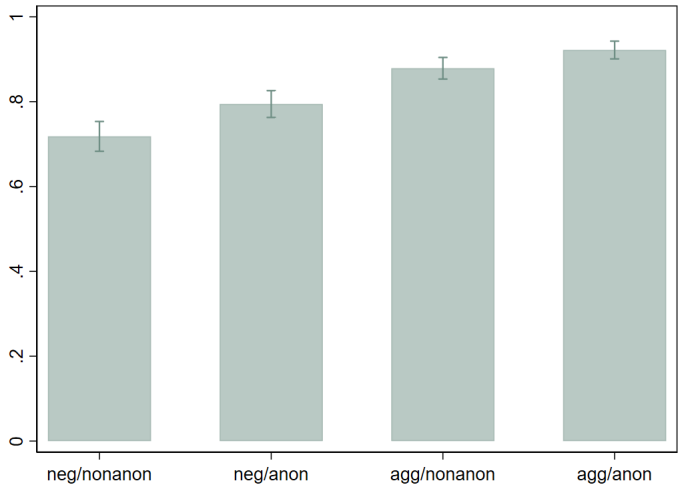
Notes: Each panel plots the distribution of average employer ratings for one dimension of employer quality. I average ratings across workers who report prior experience with a given employer. I code all measures so that higher values indicate better employer quality; I therefore reverse-code micromanagement, 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 13: Asymmetric Information Sharing Experiment



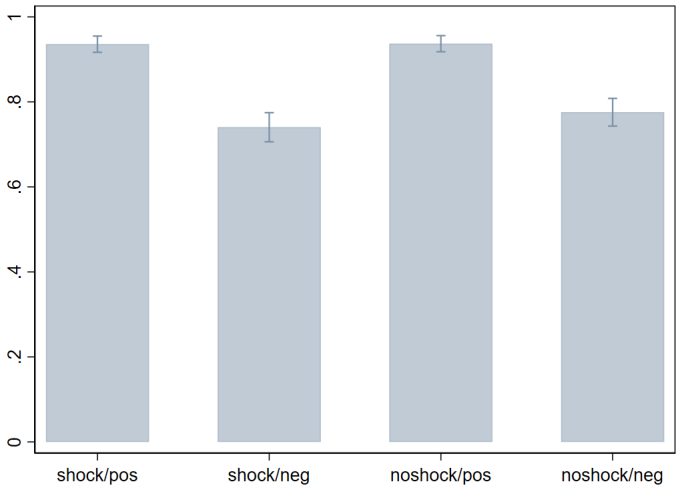
Notes: The figure plots mean willingness to share information in the first information-sharing experiment by information valence and whether the respondent's identity is revealed.

FIGURE 14: Weak Signal Experiment



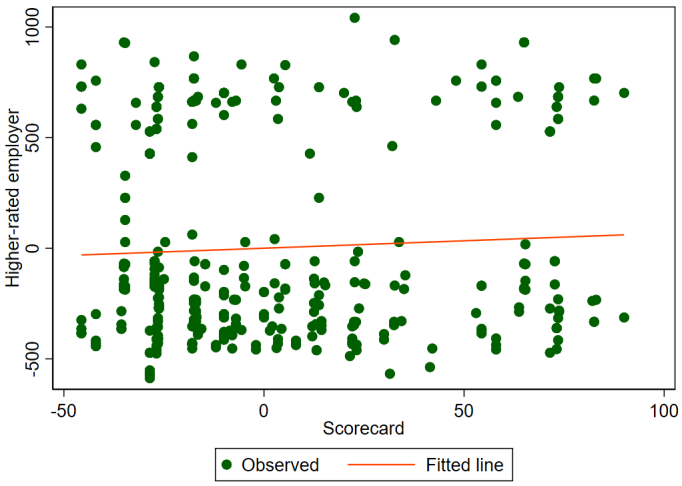
Notes: The figure plots mean willingness to share information in the weak signal experiment by information type and whether the respondent's identity is revealed.

FIGURE 15: Paternalistic Motives Experiment



Notes: The figure plots mean willingness to share information in the peer economic-shock experiment by information valence and recipient shock status.

FIGURE 16: Correlation between WTP for higher-rated employer and scorecard



Notes: The figure plots the relationship between willingness to pay for a higher-rated employer and willingness to pay for an employer scorecard after removing stand-level variation.

A.2. 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 workers who completed the initial screeningsurvey but could not be re-surveyed with workers who were successfullyre-surveyed and included in the final sample. The first two columns report group means. P-values test differences in baseline characteristics by follow-up status. The final column reports standardized mean differences, calculated as the difference between the final-sample and attrited-group means divided by the pooled standard deviation.

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. P-values test equality of baseline characteristics across assigned conditions.

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. P-values test equality of baseline characteristics across assigned conditions.

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. P-values test equality of baseline characteristics across assigned conditions.

Table 20: Asymmetric Information Sharing Experiment

	Share information (1)	Share information (2)	Share information (3)
Pos-nonanon	-0.187***	-0.196***	-0.202***
	[0.066]	[0.061]	[0.063]
Pos-nonanon*Above median SDS score	0.153	0.182**	0.191**
	[0.094]	[0.064]	[0.072]
Neg-nonanon	-0.353***	-0.369***	-0.372***
	[0.064]	[0.078]	[0.076]
Neg-nonanon*Above median SDS score	0.086	0.102	0.113
	[0.093]	[0.091]	[0.090]
Neg-anon	-0.100	-0.115**	-0.120**
	[0.065]	[0.051]	[0.053]
Neg-anon*Above median SDS score	0.007	0.034	0.043
	[0.092]	[0.056]	[0.059]
Above median SDS score	-0.004	-0.028	-0.034
	[0.066]	[0.036]	[0.034]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Pos-nonanon=neg-nonanon	0.000	0.001	0.000
Neg-nonanon=neg-anon	0.000	0.001	0.001
Observations	426	426	426
R-squared	0.115	0.189	0.210
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, 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-nonanon	0.011	0.024	0.057
	[0.071]	[0.050]	[0.053]
Aggregate-nonanon*Above median SDS score	-0.115	-0.133	-0.160*
	[0.102]	[0.086]	[0.086]
Neg-nonanon	-0.291***	-0.294***	-0.275***
	[0.071]	[0.091]	[0.088]
Neg-nonanon*Above median SDS score	0.130	0.136	0.132
	[0.100]	[0.086]	[0.081]
Neg-anon	-0.076	-0.081	-0.071
	[0.069]	[0.079]	[0.093]
Neg-anon*Above median SDS score	-0.108	-0.092	-0.121
	[0.100]	[0.081]	[0.101]
Above median SDS score	0.074	0.064	0.067
	[0.069]	[0.053]	[0.053]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Neg-nonanon=neg-anon	0.002	0.035	0.067
Neg-nonanon=agg-nonanon	0.020	0.050	0.108
Observations	426	426	426
R-squared	0.068	0.125	0.166
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, 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-neg	-0.097	-0.144**	-0.155**
	[0.068]	[0.063]	[0.069]
Noshock-neg*Above median SDS score	-0.132	-0.119	-0.116
	[0.096]	[0.083]	[0.086]
Shock-pos	0.016	-0.003	0.003
	[0.063]	[0.072]	[0.079]
Shock-pos*Above median SDS score	-0.039	-0.032	-0.046
	[0.096]	[0.076]	[0.095]
Shock-neg	-0.134*	-0.178***	-0.164***
	[0.070]	[0.045]	[0.048]
Shock-neg*Above median SDS score	-0.129	-0.095	-0.122
	[0.097]	[0.093]	[0.083]
Above median SDS score	0.078	0.067	0.072
	[0.067]	[0.054]	[0.058]
Stand FEs	No	Yes	Yes
Demographic controls	No	No	Yes
Shock-neg=Noshock-neg	0.047	0.002	0.003
Observations	426	426	426
R-squared	0.070	0.115	0.133
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, 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.

A.3. Workers' accounts of positive and negative employer experiences

Workers were asked to record both positive and negative experiences about employers at the labor stand. They were then randomized into the information sharing experiments. This section describes workers' accounts of employer quality, and characterizes them into different aspects 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 23 provides a breakdown of worker responses into different themes.

Table 23: 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: Percentages are calculated from a total of 431 positive responses and 430 negative responses. Multiple themes may be assigned to each response. The positive response under the 'alcohol' theme describes an employer who does not drink. Pay includes payment timing, amounts, deductions, arrears and overtime compensation. Treatment includes respectful or friendly communication as well as anger, scolding, and 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 24: 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.