SCABS: THE SOCIAL SUPPRESSION OF LABOR SUPPLY

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Preliminary and Incomplete

ABSTRACT. A distinguishing feature of the labor market is social interaction among co-workers—providing the ingredients for social norms to develop and constrain behavior. We use a field experiment to test whether social norms against accepting wage cuts distort workers' labor supply during periods of unemployment. We undertake our test in informal spot markets for casual daily labor in India. We partner with 183 existing employers, who offer jobs to 502 randomly-selected laborers in their respective local labor markets. The job offers vary: (i) the wage level and (ii) the extent to which the offer is observable to other workers. We document that unemployed workers are privately willing to accept work at wages below the prevailing wage, but rarely do so when this choice is observable to other workers. In contrast, observability plays no role in affecting take-up of jobs at the prevailing wage. The consequences of this behavior are substantial: workers are giving up 38% of average weekly earnings in order to avoid being seen as breaking the social norm. In a supplementary exercise, we document that workers are willing to pay to punish anonymous laborers who have accepted a wage cut. Costly punishment occurs both for workers in one's own labor market, and for workers in distant other labor markets—suggesting the internalization of norms in moral terms. Our findings support the presumption that, even in the absence of formal labor institutions, collusive norms can constrain labor supply behavior at economically meaningful magnitudes.

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1. Introduction

Traditional theories of the labor market presume that workers are atomistic individual agents, whose labor supply curve is pinned down solely by individual preferences and alternate sources of income. However, a long tradition of work in social science posits that worker behavior shows a tendency to deviate from this individualistic approach—for example, through phenomena such as coordinated restriction of output, strikes, and other collective behaviors. The labor economics literature has explored these outcomes within the context of formal unions (Farber, 1986). A broader set of literatures—spanning economics, political science, sociology, and psychology—suggests that such collective behaviors emerge even in the absence of any formal labor organization. This view has focused on the role of norms as a coordinating device that serves to constrain individual worker behavior.[1] For example, in *The Labor Market as a Social Institution*, Robert Solow argues that the social norms that arise in the workplace are inherent to what distinguishes labor markets from other commodity markets, and are important for understanding labor market equilibria.

This paper empirically examines community-wide norms against accepting wage cuts in a developing country context. Specifically, we test whether such implicit collusion distorts labor supply—preventing workers from accepting jobs at wages below the prevailing wage during times of unemployment. The setting for our study is informal markets for casual daily labor in India. Such markets are ubiquitous in poor countries, serving as the primary channel for hired employment for hundreds of million workers in India alone (National Sample Survey 2010). They are characterized by a high degree of decentralization and informality: employment is bilaterally arranged between individual employers and laborers in spot contracts (typically lasting 1-3 days), with frequent resorting between employers and workers. Unions or other formal organizations are virtually nonexistent, and minimum wage laws are largely ignored (Rosenzweig 1980, 1988; Dreze and Mukherjee 1986). However, observational evidence points to a lack of wage flexibility in these markets—both across workers and across time (Dreze et al., 1986; Kaur, 2018; Breza et al., 2018).

Figure 1 provides initial suggestive support for the presence of norms against accepting wage cuts in this setting, using the approach developed in Kahneman et al.

[1]We are agnostic about whether norms are the result of innate human preferences, for example for fairness, or are simply a way to describe a set of equilibrium strategies. A rich body of theoretical work models how, in the absence of any formal organization, collective behaviors can be sustained in equilibrium through sanctions or the internalization of norms into utility (Kandori, 1992; Ellison, 1994; MacLeod, 2007).

(1986). In a survey conducted with Indian agricultural laborers, over 80% of respondents said it was "unacceptable" or "very unacceptable" for an unemployed worker to offer to work at a rate below the prevailing wage (Panel A). In addition, about 80% of respondents stated that other workers in the village would become angry with a villager who accepted work below the prevailing wage (Panel B). While only speculative, these responses suggest that laborers view working for a lower wage, even when unemployed, as a violation which could result in sanctions.

In this paper, we use a field experiment to test whether these forces actually constrain workers' labor supply decisions. In doing so, we do not take a strong stance on why the norms arise. For example, they may stem from or be supported through primitives in utility. Alternately, they may simply describe an equilibrium strategy that enables decentralized workers to behave as a union (Kandori, 1992; Ellison, 1994). Moreover, these two views are not necessarily contradictory (e.g. MacLeod, 2007). Below, we discuss possible interpretations in light of our empirical results. However, our primary focus is on documenting the existence of these social forces and their effects on labor supply.

Specifically, we hypothesize that during times of unemployment, (at least some) workers find it privately optimal to take up jobs at wages lower than the prevailing wage, but are less likely to do so because this would be perceived as a norm violation, resulting in sanctions from co-villagers. To test this hypothesis, we proceed in two steps. The main part of our study is a field experiment with existing employers in which we vary two aspects of job offers: (i) the level of the wage and (ii) the extent to which the job offer is observable to other workers. In addition, using a supplementary exercise, we document workers' willingness to destroy surplus in order to sanction those who have accepted wage cuts. We describe each of these components in turn below.

In this context, agricultural employers hire laborers in their village in one-day spot contracts to work on their land and perform a given cultivation activity (e.g. weeding). To implement the field experiment, we partner with such employers. We induce two types of variation during their hiring process. (i) First, the job is offered at a random wage level: at the prevailing wage, or 10% below the prevailing wage.[2] (ii) Second, we vary the extent to which the wage level is publicly observable: whether the job

[2]In this setting, there is a prevailing daily wage for each type of agricultural task. We provide direct evidence for this below.

offer is made inside the worker's home or outside on the street where neighbors (who are typically other workers) can overhear the offer.

All offered jobs correspond to actual employment opportunities on the employer's land—so that our data reflects real employment decisions by workers. Treatment randomization is at the village level, so that all workers within a given village receive the same wageXobservability condition. In addition, the workers in our experiment (i.e. those who are offered jobs) are sampled randomly from the village population of laborers.[3] Aside from hiring, we are not involved in any other aspect of the employment relationship: employers supervise workers as usual, provide them food, etc. The experiment is conducted across 183 villages (i.e. labor markets) with 183 distinct partnering employers (one in each village), with jobs extended to 502 workers.

We predict that, when assured privacy, at least some unemployed workers will choose to accept jobs below the prevailing wage. However, if other villagers can observe their decision, this will dampen their willingness to accept a job at this lower wage. In contrast, we predict that observability will not decrease take-up of jobs at the prevailing wage—since taking up these jobs does not constitute a norm violation.

Our results are consistent with these predictions. At the prevailing wage, the average take-up rate of jobs is 26%, and we cannot reject that this take-up rate is the same regardless of whether the job offer is publicly observable by others.[4] In contrast, when a worker is offered a job below the prevailing wage, take-up depends crucially on whether his decision is publicly observable. When the lower wage is offered in private, take-up remains a robust 18%. However, this falls by 13.6 percentage points when low-wage offers are observable (significant at 1% level). When restricting the sample to workers who are in the agricultural labor market—defined as those who consider agricultural labor as their primary or secondary occupation—these results become even starker: the acceptance of wage cuts is 20.6% in private vs. 1.8% in public.

This distortion on individual labor supply is economically meaningful. The experiment was conducted during the lean season, when workers typically only find a few days of employment in a week. Consequently, passing up one day of work at a 10%

[3]As is typical, hiring is done by employers who approach laborers at their homes to make job offers. We randomly select among workers who are home at the time of hiring. We provide several robustness checks to compare these workers with the overall population.

[4]Note that, even under the prevailing wage, we would not expect take-up to be 100%. Recall that we sampled randomly from the labor force in each village when making job offers. Workers may decline the job because they have another work activity already lined up, or because their reservation wage is higher than the prevailing wage.

wage cut is equivalent to foregoing 38% of average weekly earnings in our sample. This is a large magnitude, especially given that workers report skipping meals and struggling for cash during this time. Our experimental results suggest that, in our sample, 13.8% of workers (i.e. those who would have accepted the job in private but do not do so in public) choose to forego these earnings in order to avoid being seen as violating the village norm.

To provide positive evidence on the willingness to sanction those who violate labor supply norms, we use a supplementary exercise. In another set of villages—drawn from the same population as our study villages—we partner with employers to make private job offers to a random subset of laborers ("workers") at varying wage rates within each village. We then play a costly punishment game with another random subset of laborers ("players") in each village who were *not* offered jobs. Each player is paired with an anonymous worker and told that the worker is either in the player's own village, or in a village that is geographically far away. To implement the game, the player is told that his paired worker accepted a job at either (a) the prevailing wage or (b) 10% below the prevailing wage. The player can then give up some of his endowment to reduce the endowment of his paired worker.

As expected, we find that there is no punishment of workers who accept jobs at the prevailing wage. In contrast, when paired with a worker who accepted a wage cut, players punish the worker 37% of the time. When players do punish, the amount of money they deduct from those who violated the norm corresponds to 37.2% of average daily labor market earnings in our sample. In order to impose this punishment on their partner, the amount that players forego from their own endowment corresponds to 7.4% of typical daily earnings. Finally, we find that the desire to punish norm violations is not limited to workers in one's own village. Players also punish workers from distant villages who accepted a wage cut—even though that worker's action has no scope to affect the player's own labor market.

These results are consistent with the literature on social preferences, which indicates that individuals will be willing to destroy their own surplus to punish those who have engaged in norm violations (Charness and Rabin, 2002). Our findings are also consistent with contagious punishment models (Ellison, 1994), in which norms are an equilibrium strategy that is enforced through decentralized sanctions. We should note, however, that the willingness to punish those in other labor markets—where the deviating party's actions have no scope to affect one's own payoffs—is particularly consistent with villagers viewing norm violations in moral or general terms.

Our findings relate to the literature on wage adjustment and labor market distortions in poor countries. Early work in development economics focused heavily on the observation that wages in poor countries appear downwardly rigid, potentially contributing to high levels of involuntary unemployment (Lewis, 1954; Eckaus, 1955; Leibenstein, 1957). Recent empirical evidence documents that downward nominal wage rigidity continues to be relevant in village labor markets today, with consequences for unemployment levels (Kaur, 2018). The presence of rigidities in this setting has been a long-standing puzzle in the development literature. A substantial body of theoretical work has proposed various micro-foundations for rigid wages (Shapiro and Stiglitz, 1984; Dasgupta and Ray, 1986). However, many of these proposed micro-foundations, such as nutrition efficiency wages, have not withstood empirical scrutiny (Rosenzweig, 1988). To date, there is scant empirical evidence supporting any micro-foundation for why wage floors should arise in this setting. Osmani (1990) offers a model based on informal worker collusion that theoretically reconciles the different stylized facts about wage adjustment in this setting. Our study provides the first empirical test of this mechanism.

Our study also has bearing on the labor literature on formal and informal unions. While a large literature has sought to understand formal unions in developed countries (see Farber and Saks, 1980; Dickens et al., 2007), there has been less work on such forces in developing countries and also limited work in economics documenting the role of informal unions more broadly. Casual labor markets in poor countries display many of the same characteristics that are often rationalized by unions in developed markets: wage rigidity and wage compression (Kaur, 2018; Breza et al., 2016; Dreze et al., 1986). Consequently, documenting the presence of informal unions in our setting suggests that some of the considerations historically attached to formal unions may apply more broadly in the labor market.

Finally, our project relates to the literature on fairness norms and markets. Recent work in behavioral economics—building on psychology, sociology, and organizational behavior—has advanced the notion that fairness norms potentially affect wage and employment behavior (Akerlof and Yellen, 1990; Fehr et al., 2009; Kahneman et al., 1986; Card et al., 2012; Kaur, 2018; Breza et al., 2016). This paper directly documents the presence of a community-wide norm against accepting wage cuts, and shows that this norm has a distortionary effect on labor supply in a high-stakes field setting: employment in real jobs. Our finding that workers are willing to give up a large amount of earnings in order to avoid being seen as violating the norm, coupled with

costly punishment of deviators, supports the view that such norms have relevance for labor market behaviors.

While our design enables us to understand whether workers' labor supply is affected by collusive pressure, our evidence does not allow us to make predictions about what wage levels would exist in equilibrium in the absence of such pressure. Such predictions would require understanding the demand side of the market, which is outside the scope of our study. In addition, our findings will of course be specific to the five Indian districts (and 183 villages) in which our study was conducted. However, the features of our setting, such as wage rigidity and low employment rates, are mirrored across India and in other developing countries (Kaur, 2018; Beegle et al., 2015). Providing the first piece of evidence for a potential micro-foundation for wage rigidity in such a setting would advance the literature and suggest an exploration of this micro-foundation in other locations as well.

The paper proceeds as follows. In Section 2, we describe the setting, research hypotheses, and experimental design. We present the results of the main experiment in Section 3, and the costly punishment game in Section 4. Section 5 discusses potential threats to validity. Section 6 concludes.

2. Setting and Experimental Design

2.1. **Overview of Setting.** The study takes place rural Odisha, one of India's most underdeveloped states. Markets for casual daily wage labor are extremely active, and provide the primary source of wage labor earnings for residents in the area. A large proportion of workers in construction, unskilled manufacturing, and other factories are hired through these labor markets. In the data collected by Breza et al. (2016), there appears to be a wage floor. This floor coincides with the prevailing wage in agriculture. Concretely, denote as W the prevailing wage in agriculture in a village. When workers from that village work in the non-agricultural sector – largely in jobs that take place outside the village – the wage they earn is at or above W in 98% of cases. This is despite the fact that unemployment levels appear high. Employment rates (in terms of total worker-days across all sectors) are below 50%. A striking 80% of workers report being involuntarily unemployed at least one day in the past two weeks. Understanding the source of the wage floor inside the village therefore has potential bearing on understanding determinants of the wage in the labor market as whole.

2.2. **Research Hypotheses.** As before, we denote the prevailing village wage as W. If worker collusion contributes to downward wage rigidity at W, then we hypothesize that during times of high unemployment, (at least some) workers would find it privately optimal to take up jobs at wages lower than the prevailing wage, but do not do so because this would result in sanctions from co-villagers. Specifically, we predict:

H1.) The true private opportunity cost of working for a subset of individuals is less than W – i.e., workers will be privately willing to accept work at wages below W.

H2.) When other workers can observe an individual's job take-up decision, workers will be less likely to accept work below W.

H3.) Workers will sanction others who have accepted work below W.[5]

We construct a design to test these hypotheses, and rule out confounding factors.

2.3. **Experimental Design.** Our experiment takes place in five rural districts in Orissa, India. In each study village, we partner with a local agricultural employer (i.e. landowner in that village). We induce experimental variation in the wage rate offered by the employers, and in the observability of these offers to other laborers in the community. Partner employers typically hire daily-wage laborers for tasks like weeding and field preparation. Our experiment involves measuring the job take-up of each worker approached by the employer under the different treatment conditions below. Importantly, workers in the experiment make decisions about real jobs, working for an actual local employer who is typically familiar to the workers.

The research design requires that we have full experimental control over offered wages and the observability of those offers. In exchange for this control, we subsidize the cost of the labor for the employers. The size of the employer's contribution is the same regardless of the size of the wage offer. This allows us to make employment offers in some treatment arms, described below, without the employer knowing the wage.[6] Note that because we care about the labor supply side only, internal validity is not affected by the fact that the employer is being compensated for his cooperation (see Section VII).

[5]We are agnostic as to how the community sustains the costly punishment of deviations by workers. Punishment may be a community norm or may be enforced through mechanisms similar to Ellison (1994) and Kandori (1992).

[6]Following the completion of work, wage payments are made to workers by members of the research staff. The employer pays his contribution directly to the research field team and never makes any payments directly to the workers. For ethical reasons, the employer in each village is aware of the possibility that we offer a wage below the prevailing wage.

Our full experimental design is presented in Figure 2. The core experiment follows a 2x2 design (treatment cells A-D). We incorporate 2 supplemental treatment cells (D and E) to allow for additional tests. Note that randomization is at the village (i.e. labor market) level, so that only one treatment cell will be implemented in any given village.

The first dimension of exogenous variation in the core 2x2 design sets the wage offer at either W or W-10%. The second dimension changes the observability of the wage offer. In the core design, there are two observability conditions:

i.) Fully Private: Employment offers are made in private, inside the worker's home. The employer does not enter the worker's home, and the research team never informs the employer of the wage.

ii.) Fully Public: Employment offers are made in public, on the street in front of the worker's home. The employer and any other passers-by can hear the terms of the wage offer.[7]

Treatment cells A and B give rise to basic tests of H1. First, the take-up rate in cell B measures whether there is any willingness to work below the prevailing wage when the offer is made in private. Under H1, take-up in cell B should be strictly positive. Second, a comparison of job take-up rates in treatment cells A versus B measures the fall in willingness to work attributable to a 10% lower wage, when job offers are made in private. Note that if workers do not believe that the information will be kept completely private, then this will result in a downwardly-biased estimate of an individual's true willingness to work below the prevailing wage, making it harder to validate H1. We return to this in our discussion of threats to validity in Section **??**.

The difference in take-up rates between treatment cells B and D offers a basic test of hypothesis H2. This comparison identifies how much an individual's willingness to work below the prevailing wage falls when the take-up decision is made observable to the community.

By examining the impact of observability on take-up at the prevailing wage—i.e. the differences between cells A and C—we can validate whether observability itself has any impact on job take-up even when no community norm is being violated. We

[7]Villages in the study districts are typically quite compact, with small dwellings that share adjacent walls and no real doors. When an employer visits the home of a worker, it is not uncommon for curious neighbors to overhear the wage offer. It is important to note that in the study villages, laborers and large employers live in distinct neighborhoods. Thus in most cases, all passers-by who overhear the wage offers will be individuals whose primary occupation is also wage labor and not landowners (i.e. employers).

predict observability will have no impact in this case (i.e. A and C will have the same take-up rate). This helps us rule out a story where changes between B and D are not due to community pressure around wage cuts, but some other level shifter. Similarly, we can perform our test of H2 in a differences-in-differences framework by examining (D-B) - (C-A) — thereby partialling out any level shifters in from observability itself.

Note that in moving from fully private to fully public, there is a change both in whether community members at-large learn about the wage rate and also whether the employer himself learns about the wage rate. Consequently, one interpretation of any differential take-up between cells B and D could stem from a desire to avoid having the employer learn the worker's reservation wage, which may affect future bargaining dynamics with employers. This story would not necessarily rely on co-worker sanctions. To help distinguish between these two interpretations, we introduce a third source of variation in the observability of the wage offer.

iii.) Partially Private: Employment offers are made in private, inside the worker's home. However, the employer does enter the worker's home and overhears the wage offer.

The difference (B-F) captures the aversion to taking a wage below the prevailing rate in front of an employer, while the difference (F-D) captures the aversion to accepting a wage below the prevailing rate in front of other laborers. Of course, if an employer is aware of the wage, then information transmission through the village may lead workers to learn it as well. Consequently, to the extent that we observe a take-up difference between B and F, we cannot disentangle whether it results from employer knowledge only, or the indirect channel of employer knowledge spreading to workers. We acknowledge that, to the extent that the purpose of collusive behavior is to enforce a collective bargaining outcome with employers, the difference between D and F is not necessarily well defined. Regardless, we view this as a useful additional source of variation.

2.4. **Context and Protocols.**

Context. This experiment takes place in 183 villages in four districts of Odisha, India. Agricultural production in these districts focuses mainly on paddy, which is both seasonal and labor-intensive. Over 70% of survey respondents are primarily engaged in agriculture, with 53% listing daily-wage agricultural labor as their main occupation. 91% of all respondents engage in daily-wage agricultural labor. There is strong baseline evidence of wage rigidity and wage compression in this area. In the survey

conducted by Kaur (2018) in this area of Odisha, 100% of laborers and employers reported that they could not recall a year when the prevailing nominal wage in the village was lower than the wage in the previous year. This is consistent with the distribution of wage changes across India as a whole (Figure 3).[8] In addition, the baseline survey evidence collected by Breza et al. (2016) indicates that there tends to be very little variation in wages within a village (Figure 4). Over 80% of agricultural workers in a village receive the modal village wage. This is consistent with the presence of a clear wage norm that can be easily followed by laborers when making labor supply decisions.

In our setting, the village constitutes a prominent boundary for the labor market. Agricultural employers hire daily-wage laborers solely from within or close to their village. For example, in our pilot surveys, laborers report that 70% of worker-days in agriculture involve work within the village, and 97% of agricultural work-days take place within 5 kilometers of the village. In addition, workers within a village (i.e. those whose primary source of earnings is wage labor) tend to live the same tightly packed area of the village (referred to as the "labor colony"). This is common in India as laborers within a village generally stem from low-caste groups, and live in designated areas. Such an environment may be expected to make worker collusion and sanctions easier. Indeed, prior work suggests that the presence of strong within-village ties, risk-sharing, and job search networks allow sanctions to have a significant clout against those who violate a village norm.[9]

We take advantage of a few distinct features of production in our study area. First, as is typical in subsistence farming, paddy production has lean periods in which employment declines (particularly between February and June, and again between September and November), which allows us to document that informal unions hold even during the eight months of the year when the opportunity cost of turning down a job is potentially high.[10] Second, the labor-intensive nature of paddy production in

[8]The figure shows some areas where nominal wage cuts may occur. This could be driven by measurement error and compositional changes (see Kaur (2018)). More generally, the occurrence of wage cuts is not inconsistent with the presence of wage rigidity.

[9]See, for example, Townsend (1994), Chandrasekhar, Kinnan, and Larreguy (2014), Karlan, Mobius, and Szeidl (2014), and Dhillon, Iverson, and Torsvik (2013).

[10]We pause the experiment during the four months of peak demand in the agricultural labor market. The labor market is most likely to clear at this time, and workers' alternative to taking up a job with us is taking a job with another employer at the prevailing wage with very high certainty. At these times of the year, a worker's reservation wage of employment is likely to be the prevailing wage, leading us to expect zero takeup at below the prevailing wage in both public and private. While conducting the experiment during these times would be fascinating, for cost and sample size reasons, we stick to the remaining eight months of the year for our study.

Odisha results in the ubiquitous use of casual daily-wage labor. Third, because of the uniformity of the crop produced in the region, we can work with local employers to offer nearly-identical agricultural jobs in different villages. We can work with everyone who participates in the agricultural daily-wage labor market, without selecting for people who have special skills or knowledge in a particular type of production or crop, which is helpful for the external validity of our results.

These features of village agricultural labor markets in Odisha (and, indeed, in many places elsewhere in India and the rest of the developing world)—siloed labor markets, a clear and consistent prevailing wage, and a relatively homogeneous skill and knowledge base among those who participate in the agricultural labor market—make this an effective context in which to study the micro-foundations of the wage rigidity and wage compression we observe.

Protocols. Village Selection: We sample 183 villages in rural areas (i.e. at least 20 km from a town) across four districts in Odisha, India. We limited the sample to villages that have forty or more households in the labor colony.[11] The sample means for key village and individual-level variables are presented in Table 1, along with balance across treatments.

Employer Selection: Once a village has been selected, we conduct a preliminary visit in which we ask an informant to list 20 employers in the village and tell us how much land they own and cultivate. After using this information to understand the distribution of land size in the village, we then recruit a mid-sized employer willing to hire up to three workers to work on his land within the next week.

Employers are told that the wage rate offered for a job on their land may be above or below the prevailing wage. They are not given any information about the level of observability of the job offers. Their contribution to the wage that will be paid to each worker is always Rs. 100 (approximately US$.1.6) per worker hired. This ensures that their incentives to hire are not changed differentially across treatments, and enables us to keep wage offers blind to the employer in the fully private treatments. Our field staff accompanies the employer during hiring, as discussed below. After that point, we are not involved in the employment arrangement and the employer supervises the worker on his land as usual etc.

[11]We use a floor on the size of the village to ensure heterogeneity across villages in the level of information spread, particularly in private treatments. In smaller villages, information may consistently be transmitted to all households.

Treatment Assignment: Once an employer has been selected in the village, we assign the village to one of six treatment cells, in accordance with the sampling weights assigned to each treatment.[12] Our unit of randomization is the village. Thus within a village, all workers are recruited under the same wage and observability condition.

Participant Selection and Hiring: We select one informant from the labor colony of the village to create a comprehensive census of daily-wage agricultural workers. We then partner with the employer to offer jobs to two-three workers (depending on the task, which was specified before treatment assignment and based on the area). We approached a random subset of workers in the labor colony with job offers, moving on to the next randomly selection household in case the worker was not home.

In accordance with local practice, job offers are made two days in advance, and at dusk, when the majority of workers are home. In our survey of employers in the area, employers in 60% of villages typically hire two days in advance of when they would like to complete the work, and less than two days in advance in 85% of villages. The norm in every village is to hire less than four days before the day of work.

In all hiring, the employer informed the worker he wanted to hire laborers to work on his land. The employer then introduces one of our field staff, saying "this person is here with me, and will give you some more details". Across all treatments, the field staff person accompanying the employer then relays the wage level to the worker. This enables us to keep which information is being conveyed constant across all the observability treatments.

Hiring - Public observability: Job offers are made outside the participant's home, which generally lead to others in the labor colony observing the job offer and take-up decision. Field staff do not interact with or provide the onlookers with any information directly. As we document below, on average there were 5 onlookers present when public hirings happened; these would typically have been other residents of the labor colony, i.e. laborers.

Hiring - Private observability: Job offers are made in the participant's home. After his initial conversation with the worker, the employer wanders away with a staff

[12]Specifically, we generated a random treatment assignment order in accordance with our desired sampling weights. Villages in the sample were then sequentially assigned to the next treatment assignment on this list as we moved through the study areas. In the second half of the sample, we also stratified by whether the labor colony population size in the village is above or below median for the block (a geographical subunit of a district). We did not perform this stratification for the first half of the sample due to an oversight.

member out of earshot, while a second staff member continues the conversation with the worker and informs him of the exact wage level.

Hiring - Partially private observability: Job offers are made in the participant's home, but the employer remains present for the entire conversation.

Confirmation and Day of Work: On the day before work is scheduled, the employer and field staff confirm the work with those who accepted the job. This is the common practice by employers in our study area. On the day of work, the employer meets the workers in his fields. The work itself proceeds as it would normally: the employer supervises the work, provides in-kind benefits like tea and lunch, without our staff present. Members of the research team do verify that the workers who agreed to the job actually work a full day. They also deliver the physical wage payments to workers at the end of the day across all treatments. This enables us to hold total wages confidential from the employer.

Surveys. After the workday is complete, we conduct a variety of surveys to provide further support for our hypotheses, including surveys with the employer, approached workers, and randomly chosen workers from the labor colony who were not approached for job offers.

3. Results

3.1. **Take-up of the Job.** Panel A of Figure 6 presents the raw job take-up rates in the public and private treatments across wage offers made at the prevailing wage and at a 10% discount to the prevailing wage. In all the analysis, the outcome variable for job take-up equals 1 if the worker accepted the job: i.e. showed up to and completed the work. The figure shows that when the wage is set below the prevailing rate, take-up falls substantially when the job offers are made in public instead of private. However, when the wage is set at the prevailing rate, if anything, public offers lead to a weak increase in take-up rates. Table 2 presents the results in regression form across treatment cells. Cols. (1)-(2) report OLS regression results for the main experimental sample, where Fully private: Prevailing wage - 10% is the omitted category.

For job offers at a 10% wage cut, take-up falls by 13.6 percentage points when an offer is public versus fully private (Col. 2). The results are similar with and without controls, with p-values of this difference ranging from 0.019-0.032 in Cols. (1)-(2) (p-values reported at bottom of table).

In contrast, for jobs made at the prevailing wage, take-up rates are positive in sign and statistically indistinguishable under public and private. This is consistent

with no role for observability when the social norm is not being violated (i.e. under the prevailing wage), but an important role for social observation when workers are contemplating whether they will take up jobs below the prevailing wage.

In addition, our design enables us to gain some suggestive evidence on whether the difference between Fully Private versus Public is driven by the presence of the employer rather than presence of other workers. Our results suggest that in the presence of the employer (under Partially Private (Employer)), take-up of low wage offers declines—the difference with Fully Private is negative but insignificant—but remains substantively higher than take-up under Public offers. In Col. (2), we can reject that (Partially Private W-10% = Public W-10%) at the 10% level (p-values reported at the bottom of the table). This test has limited statistical power due to the smaller sample size in the Partially Private treatment cells. As discussed above, it is also not perfectly interpretable as the incremental impact of co-worker observability, since the employer could also indirectly spread information to others in the village. However, these results support our presumption that pressure from other workers plays an important role in depressing labor supply below the prevailing wage.

Finally, note that the absolute magnitude of difference between Fully Private at the prevailing wage and Fully private at a 10% cut is 6-8 percentage points, but statistically not different than zero in any column. This is consistent with an underlying labor supply curve that is likely upward sloping, but not highly elastic around the prevailing wage. However, in the presence of social pressure (i.e. under the Public treatments), observed labor supply drops substantially below the prevailing wage—behavior that can reinforce a wage floor at the prevailing wage.

As explored in detail below, we hypothesize that the collective behavior of the informal union is stronger for "insiders" in the village union. We define an "insider" to be a worker who engages regularly in the agricultural wage market and show our main results for only this group of individuals. Panel B of Figure 6 shows the raw take-up rates for the subset of workers who self-identify in the endline survey as working in wage agricultural labor as a primary or secondary occupation. Interestingly, take-up rates in private look very similar to those in the full sample at both wage rates. However, we observe that when the below-prevailing wage offer is made public, take-up falls almost to zero. Col (3) of Table 2 shows the results of the OLS regression specification, restricting the sample to this same group of "insiders." Indeed, the key treatment effect of public wage offers holding fixed the below-prevailing wage rate increases in magnitude to 24.6% (significant at the 1% level). Moreover, for

this group of insiders, the treatment effect is much larger when the low wage offer is made in front of other workers in comparison to when the offer is made only in front of the employer (25.6% versus 7.58%). This difference is also significant at the 1% level. These results provide preliminary evidence that workers who are members of the informal union are especially concerned with violating the village wage norms in front of other workers.

A potential concern with our design is that when public offers begin in the village, information may spread about the wage rate at which offers are being made. This, in turn, could affect which workers are available to be approached by the employer for a job offer (for the subsequent second and third offers made in the village). Note that information spread affecting the take-up decision is not a problem by itself. However, it is important that the treatments do not induce a type of differential selection into receiving a job offer. To ensure that this is not a problem, in columns 1-2 of Appendix Table A.1, we restrict our analysis to the first household and first two households, respectively, approached in our randomized list. While results are noisier due to the reduced sample size, the results are qualitatively similar.

Recall that during hiring, if a household that was approached for a job was not home, the employer moved to another household—approaching no more than six households total (regardless of how many people were home). The process of approaching three households in each village was sufficiently quick that we do not think any aspect of the wage level would have led to differential door locks. However, as a robustness check against this concern, in column 3 of Appendix Table A.1, we code any household that was not home as zero take-up. While this mechanically dampens the observed take-up levels across all treatments, and consequently predictably decreases statistical power, the results remain qualitatively similar. For job offers below the prevailing rate, the take-up difference between public and fully private offers is statistically significant at the 10% level. In addition, for jobs at the prevailing wage, take-up levels are similar across the observability levels.

3.2. **Heterogeneous Treatment Effects.** We next turn to two tests for heterogeneous treatment effects. First, we ask whether the treatment effects are any different statistically between insiders in the informal union and outsiders. We hypothesize that insiders, who rely on casual agricultural labor markets as an important income source, have more to lose from violating the village wage norm in front of other workers. We look for support for this hypothesis in Table 3. Here we define outsider in two different ways using each worker's endline survey responses: first, as above, an

outsider is any individual who does not participate in the agricultural labor market as a primary or secondary occupation; second, we define an outsider as an individual who participates in the non-agricultural labor market as a primary or secondary occupation. Because we undersampled the partially private treatment cells, we pool Fully Private and Partially Private together for each wage. Cols. (1)-(2) present the main treatment effects, but using the pooled specification. Col. (3) presents heterogeneous treatment effect estimates using the first outsider definition, and Col. (4) does the same for the second definition. We find that insiders respond to a public low-wage offer with an 18-22 percentage point decrease in job take-up rates (both significant at the 1% level). Moreover, this treatment effect is detectably smaller for outsiders, supporting our insider vs. outsider hypothesis.

Our view of the mechanism underlying our main results is that workers reduce their take-up of below-prevailing wage jobs when they worry that their decisions are observable to others in the village. Therefore, in villages where more individuals are likely to learn of worker take-up decisions, we hypothesize that the main treatment effects will be larger. Following the completion of hiring in all villages, we returned to all but one of them to capture village-level characteristics that were not recorded in our initial endline. In this survey exercise we asked approximately five randomly-chosen workers per village a series of questions, some of which pertained to information flow in the village. We asked each individual the extent to which laborers learned about the wages at which others accept agricultural work. W aggregate responses at the village level and create an indicator for whether a village has below-median information flow. We predict that the magnitude of the treatment effect will be relatively smaller for these low-diffusiveness villages.

In Table 4 we explore heterogeneous treatment effects based on this measure of low information flow. Again, we augment the pooled average treatment effects specification. Publicizing a low wage offer in highly diffusive villages leads to an approximately 20 percentage point decline in take-up rates (significant at the 1% level). However, in low diffusiveness villages, this large treatment effect is completely offset, leading to no measurable differences in take-up rates between public and private low-wage offers. These findings are consistent with our proposed mechanism.

4. Evidence for Sanctions - Costly Punishment

In this section, we provide positive evidence that accepting wage cuts results in sanctions. This helps distinguish the mechanism behind the observability effects from other types of reputational concerns, such as shame.

4.1. **Survey responses.** We begin by tabulating worker perceptions of sanctions. In a survey with agricultural workers who did not participate in the experiment (i.e. were not offered jobs), we elicited beliefs about the consequences of accepting wage cuts. Respondents were asked "Suppose a laborer accepts work at a rate lower than the prevailing wage. What will be the reaction of other workers?" Respondents could agree with as many options as they wanted, or could give their own. We compile these responses into categories in Figure 7.

48% of respondents state that others would impede that worker's future labor market opportunities. For example, a common source of non-agricultural employment is contractors, who come into the village and deputize a worker to round up a larger number of workers for an outside job. In such a scenario, the laborer who accepted the wage cut would not be included in such an opportunity. Because referrals play an important role in this market, respondents cited this as a way to punish behavior. In addition, 20% of respondents said that a worker who accepts wage cuts would be excluded from social activities, such as drinking together. In contrast, only 1% of respondents rarely agreed with the notion that accepting wage cuts results in financial punishments—for example, refusal to help a laborer with a financial emergency in the future.

Workers also expressed a belief that social pressure is generally successful in preventing such actions to begin with (panel (b)). 63% of workers stated that others would try to convince the worker not to accept a job at a wage cut. In addition, we asked all workers "If others try to convince such a worker not to take the job, will he still do it?" 85% of workers said "No", indicating their view that a worker would not go against group pressure.

Of course, such survey evidence is only suggestive. To obtain more direct revealed preference evidence on sanctions, we use a costly punishment game in a supplementary lab-in-the-field exercise.

4.2. **Costly punishment exercise.** In another set of 13 villages—drawn from the same population as our study villages—we again partner with employers to make job offers to a random subset of workers at varying wage rates within each village. These

offers are always made in private. Each worker is first offered a job at 10% below the prevailing wage, and if he says no, is asked if he would be willing to work for the employer at the prevailing wage. We typically approached 10-15 workers with job offers in each village (with the number of workers per village decided ex ante). This larger number of offers guaranteed that in each village, at least some workers have accepted a wage cut. This sets up the backdrop for the costly punishment exercise.

Specifically, we then recruit another (random) subset of 8-12 laborers in each village who were *not* offered jobs. These other laborers, who we will refer to as "players", are the ones who actually participate in the costly punishment game. Each player is paired with an anonymous worker (the "partner") who received a job offer. The player and his anonymous partner are both given an endowment of Rs. 100. The player can "punish" his partner, reducing his endowment, by giving up some of his own endowment. Specifically, for every Rs. 5 that is removed from the partner's endowment, the player must give up Rs. 1 of his own endowment. To make visualization easy, we implement this by placing 2 trays in front of the player, with Rs. 100 on each tray. The player than removes money from his tray and his partner's tray, in accordance with the above proportion, until he is satisfied with the final allocations.

To conduct our test, we randomize two features of the partner's characteristics. First, we randomly vary whether the player is partnered with a worker in the player's own village, or is partnered with a worker in a village that is geographically far away. Note that in this latter case, the worker's job acceptance decision has no direct consequences for the player, since the partner's actions take place in a different labor market. Second, the player is told that his paired worker accepted a job at either (a) the prevailing wage or (b) 10% below the prevailing wage. The sample is weighted so that there is an equal number of observations in each of the 2x2=4 cells.

Furthermore, in order to obfuscate the reason for the exercise, we add in two "placebo" rounds of the game, which are played by the player before the above conditions.[13] The player's payoff is determined by a random roll of the dice, in which one of his four rounds is implemented.[14]

[13]In each round, the player's paired partner is a different individual. In each of these earlier rounds, the paired worker undertakes a positive, negative, or neutral action: giving a gift of a bag of grainbaking someone a cake, stealing someone's bike, and traveling to the city for work.

[14]Note that the costly punishment game is played in the evening after job offers are made, but before the day of employment occurs. After the game is played, we announce that those laborers who do get jobs will receive the full prevailing wage (regardless of their initial response at the time of the wage offer). This enables us to fully preserve the anonymity of workers' take up decisions and prevent any sanctions outside the game.

If accepting a wage cut violates the social norm, then the literature on social preferences indicates that individuals may be willing to destroy their own surplus to punish those who have engaged in norm violations. In contrast, we do not expect to see punishment among workers who accept work at the prevailing wage—providing a helpful benchmark.

Figure 8 shows the estimated level of punishment under each scenario. As expected, there is virtually no punishment of workers who accept jobs at the prevailing wage. In contrast, when paired with a worker who accepted a wage cut from their own labor market, players punish the worker about 40% of the time. In addition, the desire to punish norm violations is not limited to actions in one's own village. Players also punish workers from distant villages in similar frequencies—even though that worker's action has no scope to affect the player's own labor market.

Table 5 presents these results in regression form. Column 1 shows that, on average, the punishment probability increases by 42 percentage points when the "partner" accepts a wage lower than the prevailing level (statistically significant at the 1% level). Column 2 shows that this effect size is of very similar magnitude and is statistically indistinguishable when the "partner" lives in a different labor market versus the player's own labor market. Columns 3-4 show that these results are robust to village fixed effects and to considering only the first experimental round pertaining to the "partner's" labor supply decisions. Finally, Column 5 shows that "partners" who accept a job below the prevailing wage from the same labor market receive payoffs that are about Rs. 15 smaller (on a base of Rs. 100).

When players do punish, the amount of money they deduct corresponds to 42.8% of average daily labor market earnings in our sample. In order to impose this punishment on their partner, the amount that players forego from their own endowment, conditional on punishment, corresponds to 8.6% of typical daily earnings.

These results are consistent with the literature on social preferences, which indicates that individuals will be willing to destroy their own surplus to punish those who have engaged in norm violations (Charness and Rabin, 2002). Our findings are also consistent with contagious punishment models (Ellison, 1994), in which norms are an equilibrium strategy that is enforced through decentralized sanctions. We should note, however, that the willingness to punish those in other labor markets—where the deviating party's actions have no scope for equilibrium effects on one's own payoffs—is particularly consistent with villagers viewing norm violations in moral or general terms.

5. Threats to Validity

5.1. **Internal Validity.** We discuss some potential confounds that could contaminate the interpretation of the results of the main take-up field experiment.

Information Spread: One might worry that the private wage offers do not remain private given that we make job offers to multiple workers. This is a valid concern, a priori, for several reasons. First, if the number of employment offers is high relative to village size, then even in the private offer condition, the wage offers will essentially become public. This would likely bias take-up at W-10% in private toward zero. As mentioned above, we limit the number of job offers to a small number in each village. The fact that we observe robust take-up in the private wage cut treatment (as opposed to close to zero take-up in the public wage cut treatment) validates our premise that at least a portion of workers believed that confidentiality would be maintained in the private treatment. To the extent that workers did not believe their take-up decision would remain confidential, this suggests our take-up estimates are a lower bound.

Number of onlookers: Our design rests on the idea that the presence of onlookers during public job offers will affect take-up behavior, because it directly enables observability by other laborers in the labor colony. In addition, in Appendix Table A.3, we validate that the number of onlookers was similar under the Public treatments under the two different wage rates (prevailing wage and 10% cut).

Information about the prevailing wage: One potential concern with our design is that the public treatments provide workers with information about the prevailing wage—e.g., through potential comments from onlookers. This information, in turn, could depress take-up of public jobs below the prevailing wage. This is not consistent with this setting: the prevailing wage is general knowledge, as validated in our endline survey (Figure 5). As further evidence in support of this idea, in Appendix Table A.4, we document that among workers who were approached for job offers, reports of the prevailing wage are not systematically different across treatment cells. Importantly, there is no evidence that knowledge of the prevailing wage is different among Public and Fully Private treatments.

Signaling Poverty: It is possible that in some villages, only the poorest households might absolutely need to take a job below the prevailing wage. Thus, other households that might prefer to take such a job in private might worry that doing so in public might send a signal about their wealth to the community. If individuals experience disutility from being classified as very poor, such a mechanism could explain a fall in take-up at W-10% when the wage offer is public. The difference between cells A versus

C, and G versus I, respectively, provides a possible suggestive test against such an explanation. If projecting status and wealth is desirable, workers should be marginally more likely to reject job offers in public versus private in these conditions as well, providing a helpful, albeit imperfect, placebo test. In addition, in endline surveys, the majority of workers state that accepting job offers below the prevailing wage would result in anger and sanctions from others—consistent with our hypothesized mechanism. Finally, our costly punishment game results provide positive support for sanctions. If accepting a wage cut is only costly because it is a sign of financial destitution, then it is unclear why workers would punish such individuals by taking money away from them.

Side payments: In our endline surveys we checked whether employers tried to compensate workers for the low offer wage by making side transfers, and do not find evidence for this. Furthermore, if such behavior were to exist, then it would most likely cause an increase in take-up across all W-10% treatment cells. Thus side payments can't rationalize our hypotheses.

5.2. **External Validity.** The magnitudes of our estimates are, of course, specific to labor markets in the study districts in Orissa, India, during the agricultural lean season. Our primary goal is to provide evidence that in our setting, villagers belong to informal unions and use social sanctions to enforce adherence to a village wage. While the shape and form of informal unions may vary across settings, we have reason to believe that the phenomenon of interest is not limited to rural Orissa. Several papers provide descriptive evidence consistent with worker co-ordination in setting and enforcing wages across rural labor markets in South Asia (Kaur, 2018; Osmani, 1990; Dreze et al., 1986). There is also reason to believe that similar informal institutions may exist in other settings. For example, Prothero (1912) points to a similar phenomenon in the early stages of industrialization in England. Further, the implications of informal unions – such as wage rigidity and low employment rates – are phenomena that are observed in many developing country contexts even outside of South Asia (Beegle et al., 2015).

It is also important to reiterate that in our study, we are interested in understanding the labor supply consequences of informal unions. Given that we subsidize employment, we cannot make any claims about the demand side of the market. We leave this to future work.

6. Conclusion

We find evidence that workers privately would like to supply labor below the prevailing wage, but do not do so when their take-up decision is publicly observable. This supports the hypothesis that collective pressure dampens labor supply below the prevailing wage, supporting the presence of wage floors in village labor markets. Our findings provide documentation of a way in which norms against accepting wage cuts distort labor supply behavior, with large impacts on the foregone earnings of unemployed workers.

Finding evidence that co-worker pressure dampens labor supply below the prevailing wage—even during times of high unemployment—provides impetus for exploring this mechanism in other settings. If this mechanism is indeed more generally applicable, then this can inform our understanding of the role of norms in shaping labor market outcomes, such as wage rigidity and wage compression.

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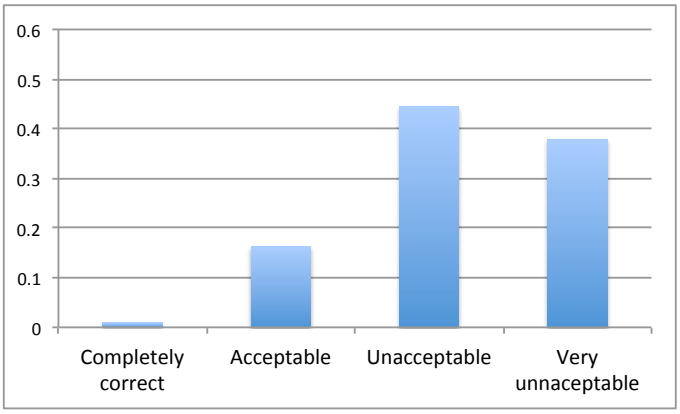
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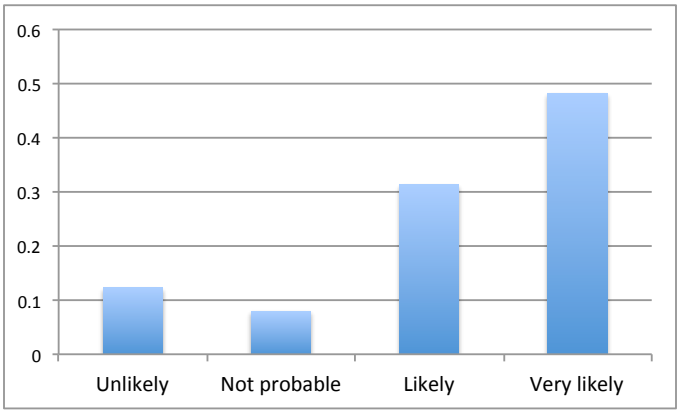
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Figures



(a) Acceptability of Taking a Wage Cut. *Suppose it is the lean season. The prevailing wage is Rs. 200. To increase his chance of finding work, a laborer tells farmers that he would be willing to work any day that week at Rs. 180. Is the laborer's behavior acceptable?*



(b) Sanctions for Accepting Wage Cuts. *If a laborer accepts work at a rate lower than the prevailing wage, how likely is it that the other laborers in the village become angry?*

Figure 1. Survey Evidence

Observability of Wage	Wage: W	Wage: W-10%
Fully Private	A	B
Fully Public	C	D
Partially Private (Employer Observes Offer)	E	F

FIGURE 2. Experimental Design

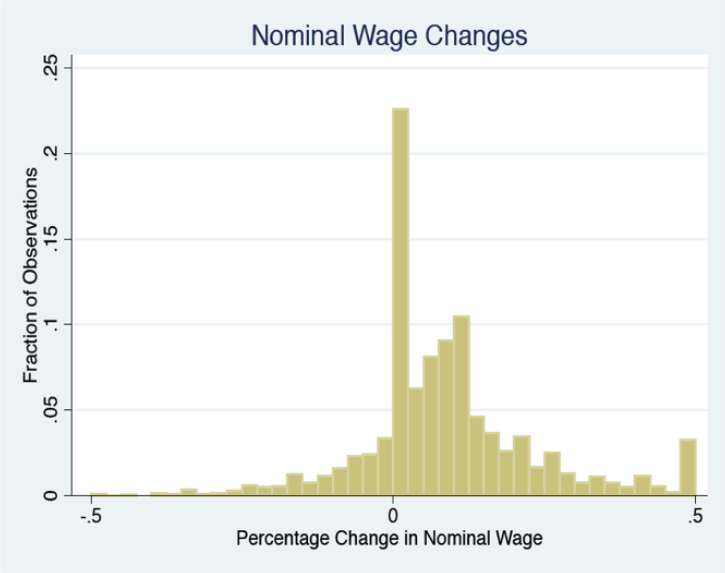


FIGURE 3. Nominal Wage Changes. *Source:* Kaur (2015), World Bank Climate & Agricultural Data (256 districts).



FIGURE 4. Distribution of Wages Inside the Village. *Source:* Breza, Kaur, and Shamdasani 2017.

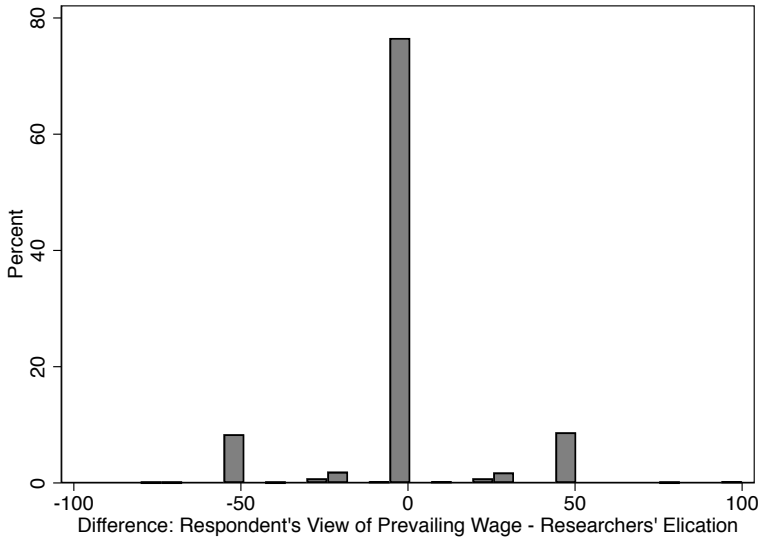
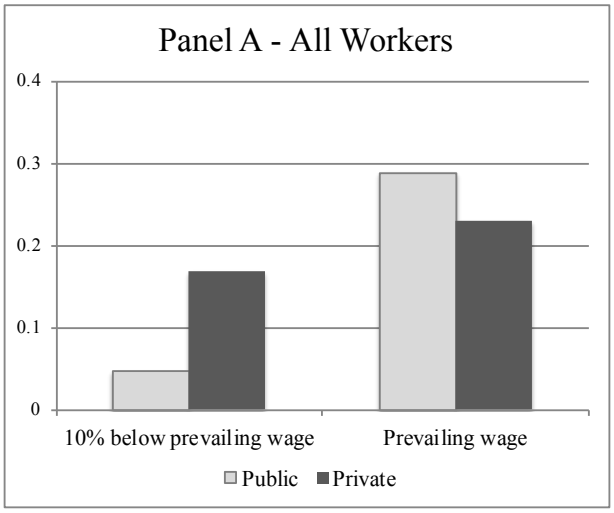
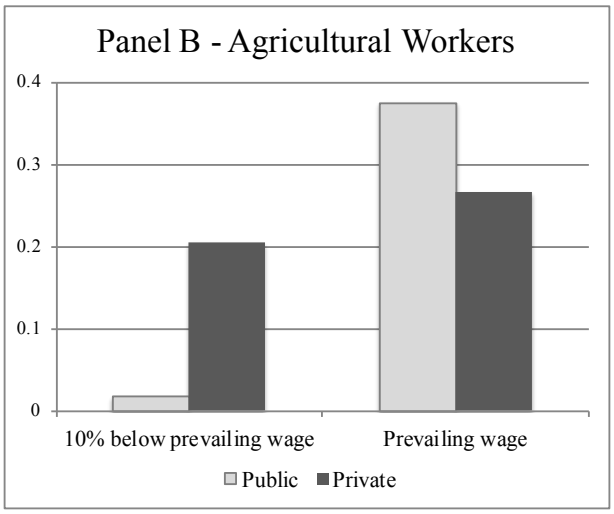


FIGURE 5. Control Group Reports of Prevailing Wage, Normalized by Informant Report.

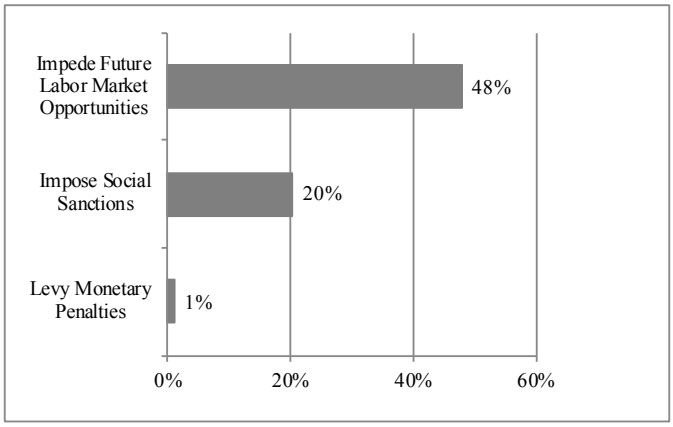


(a) All Workers

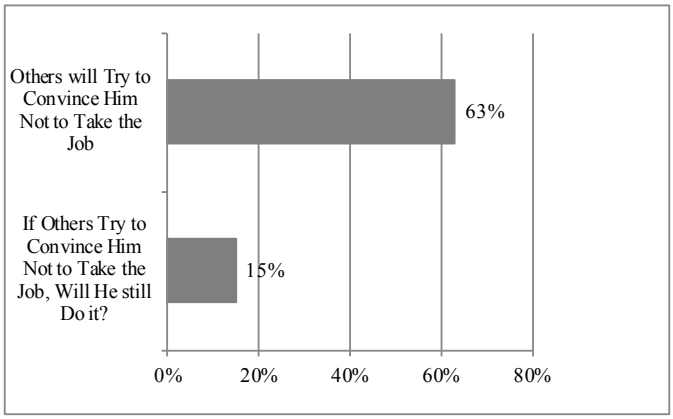


(b) Agricultural Workers Only (Insiders)

FIGURE 6. Job Take-Up by Treatment: Raw Data



(a) Sanctions



(b) Social Pressure

FIGURE 7. Sanctions and Social Pressure for Accepting Work Below the Prevailing Wage

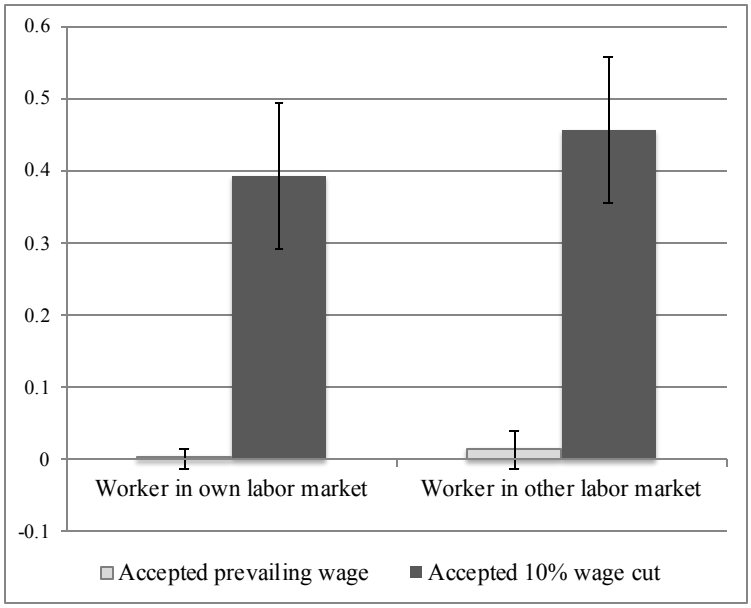


FIGURE 8. Laboratory Games: Fraction of Respondents Punishing

Note: This figure graphs the results of the costly punishment game. Each participant (player) was anonymously paired with a worker in his own village or in a distant village, and given various scenarios about his paired worker. The table reports the proportion of times players punished their paired worker under the 2 employment scenarios: (i) the worker accepted a job at the prevailing wage, or (ii) the worker accepted a job at a wage 10% below the prevailing wage. N=131 participants in 31 villages (villages are different from those in the main experimental sample). The plotted coefficients correspond to Col. (3) of Table 5.

Tables

Table 1. Covariate Balance

VARIABLES	(1) Number HHs	(2) Field Prep	(3) Weeding	(4) Compost Crush.	(5) Age	(6) Sched Tribe	(7) Has Worked Empl.	(8) Empl. Influence	(9) Days Paid Wage 30	(10) Not Ag. Laborer
Public: Prevailing Wage - 10%	-2.227	0.0708	-0.0736	-0.0144	0.561	-0.0264	0.0897	-0.0419	0.984	0.0370
	(3.642)	(0.108)	(0.0798)	(0.0567)	(1.826)	(0.109)	(0.0887)	(0.183)	(1.063)	(0.0634)
Partially Private (Employer): Prevailing Wage - 10%	-1.826	0.0724	-0.108	-0.0628	1.940	-0.00439	0.191	-0.143	-0.912	0.0810
	(3.722)	(0.116)	(0.0745)	(0.0476)	(1.820)	(0.118)	(0.0911)	(0.226)	(0.995)	(0.0700)
Fully Private: Prevailing Wage	5.316	0.0653	-0.00345	-0.0601	1.285	0.0103	0.0511	0.315	1.379	-0.00601
	(4.146)	(0.124)	(0.0966)	(0.0486)	(2.224)	(0.126)	(0.0901)	(0.193)	(1.179)	(0.0604)
Public: Prevailing Wage	-2.477	0.0206	0.0425	-0.0443	-1.492	0.194	0.197	-0.0324	1.336	-0.0251
	(4.091)	(0.112)	(0.117)	(0.0565)	(2.116)	(0.132)	(0.105)	(0.191)	(1.058)	(0.0650)
Partially Private (Employer): Prevailing Wage	4.454	0.101	-0.0831	0.0170	3.619	-0.0167	-0.0205	-0.000504	1.149	0.0503
	(4.979)	(0.149)	(0.0981)	(0.0915)	(2.374)	(0.149)	(0.103)	(0.186)	(1.012)	(0.0856)
Observations	502	502	502	502	442	444	426	383	427	446
Task and Year x Month FE										
Sample	Main	Main	Main	Main	Main	Main	Main	Main	Main	Main
Depvar Mean (Private: Prevailing Wage - 10%)	46.58	0.214	0.165	0.126	44.34	0.333	0.318	3.333	8.841	0.156
Test: Full Private W-10% = Public W-10%	0.542	0.513	0.358	0.799	0.759	0.809	0.313	0.819	0.356	0.560
Test: Full Private W = Public W	0.104	0.731	0.709	0.729	0.215	0.197	0.194	0.0817	0.974	0.768
Test: Full Private - Public, W-10% = Full Private - Public, W	0.355	0.495	0.416	0.678	0.249	0.242	0.693	0.260	0.538	0.536
Test: Partial Private W-10% = Public W-10%	0.920	0.989	0.579	0.283	0.355	0.845	0.305	0.652	0.0911	0.544
Test: Full Private W-10% = Full Private W	0.201	0.599	0.972	0.218	0.564	0.935	0.572	0.106	0.244	0.921

Notes: Table reports means and balance across treatments, restricting to the main experimental sample. The reported tests match those of interest for our main results, reported in Table 2. Columns 1-4 report village-level characteristics: number of households in the village, an indicator for the task being field preparation, an indicator for the task being weeding, and an indicator for the task being compost crushing. The three tasks reported here together represent more than 75% of the experimental rounds. Columns 5-10 report (time invariant) individual-level characteristics collected in the endline survey. These variables include: respondent age, scheduled caste status, an indicator for having previous experience with the employer, the fraction of individuals in the village rating the employer as 1, 2 or 3 on a 4-point influence scale, days in the prior 30, where the worker earned a positive wage, and an indicator for a non-participant in the casual agricultural daily labor market. Variation in sample sizes comes from non-response in the endline survey and slightly different questions being asked in the endline survey across experimental rounds. Standard errors are clustered at the village level and are reported in parentheses. Observations are weighted by the number of individuals in each village.

TABLE 2. Main Results

VARIABLES	(1) Accepted Offer	(2) Accepted Offer	(3) Accepted Offer
Public: Prevailing Wage - 10%	-0.122	-0.136	-0.246
	(0.0564)	(0.0573)	(0.0644)
Partially Private (Employer): Prevailing Wage - 10%	-0.0657	-0.0516	-0.0758
	(0.0611)	(0.0633)	(0.0788)
Fully Private: Prevailing Wage	0.0609	0.0791	0.0663
	(0.0703)	(0.0659)	(0.0819)
Public: Prevailing Wage	0.119	0.116	0.104
	(0.0808)	(0.0713)	(0.0856)
Partially Private (Employer): Prevailing Wage	0.0364	0.0690	0.0935
	(0.0775)	(0.0886)	(0.0992)
Observations	502	502	363
Task and Year x Month FE		✓	✓
Sample	Main	Main	Agricultural HHs
Depvar Mean (Private: Prevailing Wage - 10%)	0.175	0.175	0.211
Test: Full Private W-10% = Public W-10%	0.0316	0.0188	0.000181
Test: Full Private W = Public W	0.460	0.589	0.658
Test: Full Private - Public, W-10% = Full Private - Public, W	0.0629	0.0481	0.00858
Test: Partial Private W-10% = Public W-10%	0.143	0.0865	0.0107
Test: Full Private W-10% = Full Private W	0.387	0.232	0.419

Notes: In all specifications, the dependent variable is an indicator for whether the worker signed up for the job and showed up for work. In all columns, the omitted category is the Fully Private: Prevailing Wage - 10% treatment. Columns 1 and 2 include the full sample. Column 3 restricts the sample to workers who answered the endline questionnaire and who indicated that they engage in agricultural labor as a primary or secondary occupation. Observations are weighted by the number of experimental subjects in each village. Standard errors are clustered at the village level and are reported in parentheses.

Table 3. Heterogeneous Treatment Effects: Insiders vs. Outsiders

VARIABLES	(1) Accepted Offer	(2) Accepted Offer	(3) Accepted Offer	(4) Accepted Offer
Public: Prevailing Wage - 10%	-0.113	-0.142	-0.182	-0.216
	(0.0439)	(0.0487)	(0.0447)	(0.0539)
Fully or Partially Private: Prevailing Wage	0.100	0.0984	0.0590	0.0190
	(0.0478)	(0.0516)	(0.0595)	(0.101)
Public: Prevailing Wage	0.141	0.148	0.139	0.0839
	(0.0599)	(0.0655)	(0.0803)	(0.0988)
Public: Prevailing Wage - 10% x Outsider			0.297	0.167
			(0.103)	(0.0689)
Fully or Partially Private: Prevailing Wage x Outsider			-0.0629	0.0626
			(0.108)	(0.124)
Public: Prevailing Wage x Outsider			-0.105	0.0793
			(0.131)	(0.133)
Outsider			-0.0793	-0.0721
			(0.0503)	(0.0532)
Observations	502	446	446	446
Task and Year x Month FE	✓	✓	✓	✓
Sample	Main	Individual Endline	Individual Endline	Individual Endline
Depvar Mean (Omitted)	0.147	0.160	0.211	0.245
Outsider Definition			Not ag. laborer	Non-ag. laborer

Notes: This table presents heterogeneous treatment effects by insider versus outsider status using the responses of experimental subjects in the worker endline. In all columns, we pool Fully Private: Prevailing Wage - 10% and Partially Private (Employer): Prevailing Wage - 10%. In columns 1 and 2, we present the pooled version of the main results for the full sample and endline survey sample, respectively. In column 3, outsider is defined as an individual who does not claim agricultural labor as a primary or secondary occupation. In column 4, outsider is defined as an individual who works in non-agricultural labor as a primary or secondary occupation. In all specifications, the dependent variable is an indicator for whether the worker signed up for the job and showed up for work. In columns 1 and 2, the omitted category is the pooled Fully or Partially Private: Prevailing Wage - 10% treatment. In columns 3 and 4, the omitted category is the Fully or Partially Private: Prevailing Wage - 10% treatment for insiders only. Observations are weighted by the number of experimental subjects in each village. Standard errors are clustered at the village level and are reported in parentheses.

TABLE 4. Heterogeneous Treatment Effects: Village Information Spread

VARIABLES	(1) Accepted Offer
Public: Prevailing Wage - 10%	-0.199
	(0.0677)
Fully or Partially Private: Prevailing Wage	0.0565
	(0.0750)
Public: Prevailing Wage	0.111
	(0.0972)
Public: Prevailing Wage - 10% x Low Wage Info Spread	0.170
	(0.0934)
Fully or Partially Private: Prevailing Wage x Low Wage Info Spread	0.0613
	(0.0996)
Public: Prevailing Wage x Low Wage Info Spread	0.0335
	(0.130)
Low Wage Info Spread	-0.0731
	(0.0671)
Observations	499
Task and Year x Month FE	✓
Depvar Mean (Omitted)	0.204

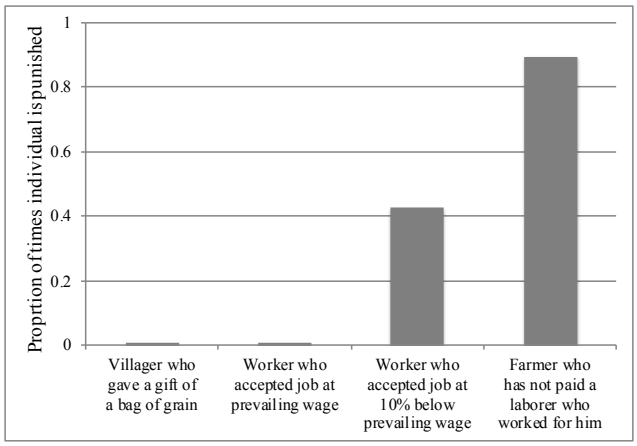
Notes: This table presents heterogeneous treatment effects by village-level diffusiveness, as measured in the mop-up survey. In column 1, the heterogeneous variable of interest is an indicator for below-median knowledge of the wages of others. In column 2, we use an indicator for below-median spread of information about other workers accepting a job below the prevailing wage. In all specifications, the dependent variable is an indicator for whether the worker signed up for the job and showed up for work. In this table, we pool Fully Private: Prevailing Wage - 10% and Partially Private (Employer): Prevailing Wage - 10%. In all columns, the omitted category is the pooled Fully or Partially Private: Prevailing Wage - 10% treatment. Observations are weighted by the number of experimental subjects in each village. Standard errors are clustered at the village level and are reported in parentheses.

Table 5. Costly Punishment Game

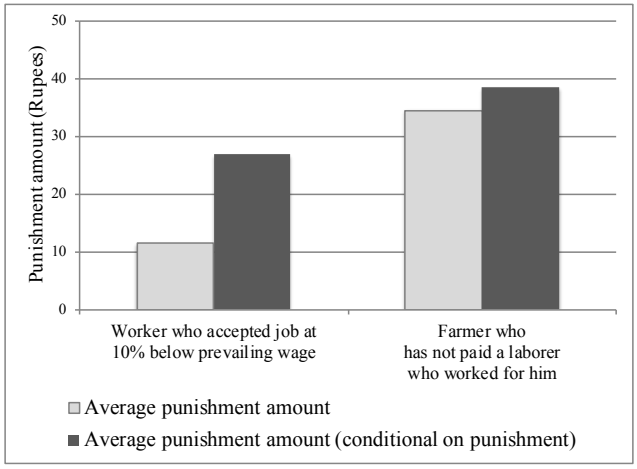
VARIABLES	(1) Any Punishment	(2) Any Punishment	(3) Any Punishment	(4) Any Punishment	(5) Partner's Payoff
Partner Accepts a Job Below Prevailing Wage	0.420	0.393	0.393	0.436	-14.57
	(0.0447)	(0.0632)	(0.0647)	(0.103)	(4.425)
Partner Accepts a Job Below Prevailing Wage x Different Village		0.0494	0.0494	-0.00310	5.569
		(0.0894)	(0.0916)	(0.137)	(4.551)
Partner lives in Different Village		0.0143	0.0133	0.00737	-0.701
		(0.0143)	(0.0185)	(0.0294)	(1.259)
Observations	262	262	262	131	131
Village FE			✓	✓	✓
First Round Only				✓	✓
Depvar Mean: Partner Accepts Job at Prevailing Wage	0.00763	0.00763	0.00763	0	100

Notes: Each participant ("player") was anonymously paired with either another worker in his village or in a distant village, and given various scenarios about his paired worker. A player could take away money from his paired worker's endowment by giving up money from his own endowment. The table reports results under the 2 employment scenarios: (i) the worker accepted a job at the prevailing wage, or (ii) the worker accepted a job at a wage 10% below the prevailing wage. OLS regressions. The dependent variable in Cols. (1)-(4) is a dummy for whether the player punished the other worker at all; in Col. (5) it is the payoff of the anonymous partner (his initial endowment minus the amount deducted by the participant). Each player plays these two scenarios in random order; Cols. (4)-(5) report results only from the first of these two rounds. Standard errors clustered by player. N=131 participants (i.e. agricultural laborers) in 31 villages (villages are different from those in the main experimental sample).

Appendix Figures



(a) Punishment across Scenarios



(b) Punishment Amounts

Figure A.1. Laboratory Games: All Scenarios

Notes: Panel A shows the proportion of times players punished their anonymous partners under 4 different scenarios about partner behavior: (i) A villager who gave a gift of a bag of grain when it was needed; (ii) A worker who accepted a job at the prevailing wage (pooled across partners in own and other villages); (iii) A worker who accepted a job at 10% below the prevailing wage (pooled again across own and other villages); (iv) A farmer who hired a worker two months ago but still has not paid him. Panel B shows the amount (in rupees) deduced from the partner's payoff under scenarios (iii) and (iv), unconditional and conditional on punishment. N=131 participants (players).

Appendix Tables

Table A.1. Main Results: Sample Robustness

VARIABLES	(1) Accepted Offer	(2) Accepted Offer	(3) Accepted Offer
Public: Prevailing Wage - 10%	-0.126	-0.122	-0.0817
	(0.0820)	(0.0645)	(0.0474)
Partially Private (Employer): Prevailing Wage - 10%	0.0260	-0.0374	-0.0377
	(0.0911)	(0.0702)	(0.0493)
Fully Private: Prevailing Wage	0.0664	0.0788	0.0598
	(0.100)	(0.0754)	(0.0598)
Public: Prevailing Wage	0.136	0.0966	0.0793
	(0.102)	(0.0776)	(0.0514)
Partially Private (Employer): Prevailing Wage	0.126	0.137	0.0629
	(0.131)	(0.105)	(0.0746)
Observations	188	359	545
Sample Restriction	First HH	First Two HHs	Intended Sample
Task and Year x Month FE	✓	✓	✓
Depvar Mean (Private: Prevailing Wage - 10%)	0.158	0.173	0.213
Test: Full Private W-10% = Public W-10%	0.127	0.0611	0.0869
Test: Full Private W = Public W	0.506	0.824	0.725
Test: Full Private - Public, W-10% = Full Private - Public, W	0.139	0.171	0.170
Test: Partial Private W-10% = Public W-10%	0.0628	0.161	0.241
Test: Full Private W-10% = Full Private W	0.508	0.297	0.318

Notes: In column 1, sample restricted to the first household approached in each village, and in column 2, sample restricted to the first two households approached in each village. In column 3, sample restricted to the intended main sample households in the village, including households where no respondent was home. In these cases, we code the outcome variable "Accepted Job" as 0 (job refusal). In all specifications, the dependent variable is an indicator for whether the worker signed up for the job and showed up for work. In all columns, the omitted category is the Fully Private: Prevailing Wage - 10% treatment. Standard errors are clustered at the village level and are reported in parentheses. Observations are weighted by the number of experimental subjects in each village.

Table A.2. Survey Attrition and Control Sample Composition

VARIABLES	(1) Has Endline Survey	(2) Num Control in Village
Public: Prevailing Wage - 10%	0.0342	0.407
	(0.0514)	(0.376)
Partially Private (Employer): Prevailing Wage - 10%	0.0124	0.104
	(0.0525)	(0.355)
Fully Private: Prevailing Wage	0.0383	-0.154
	(0.0525)	(0.397)
Public: Prevailing Wage	-0.0857	0.214
	(0.0662)	(0.433)
Partially Private (Employer): Prevailing Wage	0.0696	0.834
	(0.0554)	(0.486)
Observations	502	502
Task and Year x Month FE	✓	✓
Sample	Main	Main
Depvar Mean (Private: Prevailing Wage - 10%)	0.879	5.364

Notes: Column 1 reports the likelihood of successfully completing an endline survey with an member of the main experimental sample, by treatment. The outcome variable in column 2 is the number of control endline surveys conducted in the experimental household's village. In all columns, the omitted category is the Fully Private: Prevailing Wage - 10% treatment. Standard errors are clustered at the village level and are reported in parentheses. Observations are weighted by the number of subjects in each village.

Table A.3. Number of Onlookers in the Public Treatments

VARIABLES	(1) Number of Onlookers	(2) Number of Onlookers
Public: Prevailing Wage - 10%	0.100	0.520
	(0.568)	(0.614)
	[0.860]	[0.400]
Observations	160	160
Task and Year x Month FE		✓
Depvar Mean (Public: Prevailing Wage)	5.294	5.294

Notes: Sample restricted to the Public Rs. 180 and Rs. 200 wage treatments. In all specifications, the dependent variable counts the number of onlookers present at the hiring. In all columns, the omitted category is the Public: Prevailing Wage treatment. Standard errors are clustered at the village level and are reported in parentheses. P-values are reported in brackets. Sample restricted to the experimental subjects in the village.

TABLE A.4. Endline Reports of Village Prevailing Wage

VARIABLES	(1) 1(Agree)	(2) Difference	(3) Abs. Difference
Public: Prevailing Wage - 10%	0.0442	-1.126	-1.291
	(0.0713)	(3.246)	(3.051)
	0.536	0.729	0.673
Partially Private (Employer): Prevailing Wage - 10%	0.0333	-1.900	-1.266
	(0.0841)	(4.011)	(3.557)
	0.692	0.636	0.722
Fully Private: Prevailing Wage	0.123	-1.598	-2.557
	(0.0771)	(4.109)	(3.718)
	0.112	0.698	0.493
Public: Prevailing Wage	0.0579	2.640	-0.109
	(0.0856)	(4.505)	(4.084)
	0.499	0.559	0.979
Partially Private (Employer): Prevailing Wage	0.122	-0.675	-3.194
	(0.0918)	(6.082)	(4.805)
	0.185	0.912	0.507
Observations	431	431	431
Sample	Salient	Salient	Salient
Task and Year x Month FE	✓	✓	✓
Depvar Mean	0.800	5.650	8.875
Test: Full Private W-10% = Public W-10%	0.536	0.729	0.673
Test: Full Private W = Public W	0.399	0.369	0.561

Notes: Sample restricted to all experimental subjects who responded to our endline survey. In column 1, the dependent variable is an indicator for whether the respondent reports the same prevailing wage at endline as the village informants reported prior to the intervention. In column 2, the dependent variable is the difference between the respondent's view of the prevailing wage and the informant's report. In column 3, the dependent variable is the absolute value of this difference. In all columns, the omitted category is the Fully Private: Prevailing Wage - 10% treatment. Standard errors are clustered at the village level and are reported in parentheses.