SCABS: THE SOCIAL SUPPRESSION OF LABOR SUPPLY

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Abstract. Social norms have the potential to shape aggregate behavior in economic markets. We test whether norms alter the aggregate labor supply curve by preventing workers from supplying labor at wage cuts—leading decentralized individuals to implicitly behave as a cartel to maintain wage floors in their local labor markets. We partner with 183 existing employers, who offer jobs to 502 workers in informal spot labor markets in India. Unemployed workers are privately willing to accept jobs below the prevailing wage, but rarely do so when this choice is observable to other workers. In contrast, social observability does not affect labor supply at the prevailing wage. Workers give up 49% of average weekly earnings to avoid being seen as breaking the social norm. In addition, workers pay to punish anonymous laborers who have accepted wage cuts—indicating that cartel behavior is reinforced through the threat of social sanctions. Punishment occurs for laborers in one's own labor market and for those in distant regions, suggesting the internalization of norms in moral terms. Finally, consistent with the idea that social conformity could have aggregate implications, measures of social cohesion correlate with downward wage rigidity and business cycle volatility across India.

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1 Introduction

In this paper, we study the potential for social norms to shape aggregate behavior in economic markets. Broad literatures in economics, psychology, political science, and sociology document that norms embody a shared understanding of acceptable actions (e.g. Fehr and Schurtenberger, 2018; Cialdini and Goldstein, 2004; Crawford and Ostrom, 1995; Bendor and Swistak, 2001). This implies norms can serve as an equilibrium coordination device—enabling collective behaviors among a large decentralized group of individuals, even in the absence of any explicit coordination or formal institutions.

We examine this idea within the context of the labor market. Specifically, we document the presence of implicit collusion among workers to maintain wage floors in their local labor markets—altering aggregate labor supply without any formal labor organization. Consistent with theories of how norms are sustained in equilibrium, we provide positive evidence for the role of social sanctions, which are used to deter self-interested deviations by workers.[1]

This mechanism has potential applicability in any setting with meaningful social interaction. This makes the labor market a particularly relevant economic domain: most labor arrangements involve repeated inter-personal interaction among workers. Consistent with this, collective behaviors—mass walkouts with foregone wages, punishment of rate busters, and productivity compression—have been documented across history and contexts, even without formal unions. More broadly, the presence of cartel-like behavior among decentralized individuals has been qualitatively observed in a range of markets, from NASDAQ traders and real estate agents in the US (Christie and Schultz, 1994; Hsieh and Moretti, 2003), to taxi drivers and vegetable vendors in poor countries. We posit that social norms can sustain such behavior—enabling agents to maintain market power in equilibrium. Moreover, such forces may be particularly relevant in poor countries, where the communal nature of village economies and high levels of social capital in networks may make it more likely that norms arise and can be sustained effectively through punishment.

We test whether norms shape aggregate labor supply in informal markets for casual daily labor in India. Such markets are ubiquitous in poor countries, serving as an

[1]A nearly universal observation in social psychology is that individuals are prone to engage in punishment or social disapprobation when they see norm violations (e.g. Henrich et al., 2006; Cialdini and Goldstein, 2004). This is consistent with a theoretical literature in economics that models norms as a coordination device, sustained through social punishment (Kandori, 1992; Ellison, 1994; Jackson et al., 2012).

employment channel for hundreds of million workers in India alone (National Sample Survey 2010). Casual labor markets are decentralized and informal: daily contracts arranged bilaterally between individual workers and employers, and there are no unions or formal labor organization (e.g. Rosenzweig, 1988). Despite this, they exhibit features consistent with potential distortions, such as downwardly rigid wages and high unemployment rates in the agricultural lean season (e.g. Dreze and Mukherjee, 1989; Breza et al., 2018; Kaur, 2018).

We hypothesize that, in our setting, there is a norm against accepting jobs below the prevailing wage, and this norm is consequential for workers' labor supply during times of unemployment. Figure 1 provides motivational evidence for the relevance of such forces in our setting. About 80% of workers state that it is "unacceptable" or "very unacceptable" for an unemployed worker to offer to work at a wage cut. In addition, about 80% of workers also state that workers would become angry at a laborer who accepts a job at a rate below the prevailing wage—indicating the potential role of sanctions in deterring deviations.

The main part of our study is comprised of two experimental exercises. First, we use a field experiment to document that although underlying labor supply is positive below the prevailing wage, it is suppressed due to extrinsic concerns: workers will not accept jobs below the prevailing wage because they do not want others to learn they have done so. Second, we provide evidence for sanctions: workers are willing to pay to punish those who have accepted wage cuts.

For the field experiment, we partner with existing agricultural employers, who make job offers to workers in their respective local labor markets. Employers follow the typical process for agricultural hiring: offering jobs by approaching workers at their homes. In this setting, there is a clear prevailing daily wage for each type of agricultural task in the village—providing a clear benchmark wage for job offers.[2]

To test for effects on labor supply, we induce two types of cross-cutting variation during employers' hiring process. (i) First, the job is offered at a random wage level: at the prevailing wage, or 10% below the prevailing wage. (ii) Second, we vary the extent to which the wage level is publicly observable: whether the job is offered inside the worker's home or outside on the street where neighbors (who are typically other workers) can overhear the offer.

A natural concern about our observability treatments is whether they may also change worker beliefs about aspects of the job in other ways as well, confounding their

[2]We provide direct evidence for the presence of a prevailing wage that is common knowledge.

interpretation. We use the prevailing wage job offers as a placebo test against this concern. Under our hypothesis, observability will only matter under norm violations (i.e. under wage cuts); it should not matter when jobs are offered at the prevailing wage. Finding this prediction in our results helps assuage such concerns.

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. Aside from inducing variation in 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 labor markets (i.e. villages) with 183 partnering employers (one in each village), with jobs extended to 502 workers.

At the prevailing wage, the average take-up rate of jobs is 26%, with no detectable differences across the observability conditions. Given a 42% baseline unemployment rate, this reflects a robust level of take-up.[3]

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, the willingness to accept wage cuts falls by 13.6 percentage points (78%) when offers are observable (significant at 1% level). When restricting the sample to workers who are actually in the agricultural labor force—those who consider agricultural labor as their primary or secondary occupation—these results become even starker. Only 1.8% of agricultural workers accept wage cuts in public (relative to an estimated 26% in private).

Overall, this pattern indicates that there is substantial underlying labor supply below the prevailing wage, but workers will not actually accept such jobs due to social concerns. This distortion on individual labor supply is economically meaningful. Workers forego 26-49% of their weekly agricultural wage earnings in order to avoid being seen as breaking the social norm.[4]

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

[4]These estimates are based on exit surveys conducted 1 week after the day of work, in which we ask workers to recount the employment on each day over the past week.

We provide two supplemental exercises to further characterize our results. First, we analyze our effects separately for villages with above- and below-median levels of involuntary unemployment (elicited in surveys with untreated households). In places with more involuntary unemployment, there is more scope for residual labor supply below the prevailing wage. Therefore, there is more scope for social observability to erode labor supply. It is also possible that more involuntary unemployment is indicative of stronger wage norms and more effective collective action. However, on the other hand, when involuntary unemployment is high the value from accepting a wage cut is higher, and some individuals might be willing to pay any social costs of deviating from the norm in order to realize that value. We find that our results are indeed strongest for villages with high levels of involuntary unemployment. In places with low unemployment, we find low levels of residual labor supply below the prevailing wage and no evidence of negative impacts of social observability on take-up.

Second, we explore how village network characteristics interact with the treatments. Previous work shows that network structure is central to spreading information and reputation (e.g. Alatas et al., 2016; Breza and Chandrasekhar, 2019) Consistent with this literature, we find evidence that social observability is more consequential in places with higher diffuseiveness.

One alternative interpretation of our labor supply results might be that workers fear revealing their personal reservation wage to the employers in the village. Here, social observability might matter if the information is likely to spread to other employers. We show that there is no detectable labor supply response when the wage offer is observable to the employer but not other laborers in the village. We also show that this pattern holds even when the employer has a pre-existing employment relationship with the worker.

We posit that the observability effects stem from an attempt to avoid social disapprobation. However, other reputational mechanisms could produce the above pattern—for example, shame from being seen as financially desperate enough to accept a wage cut. In the second part of the paper, we provide positive evidence for social sanctions. We first use a survey to document that 80% of workers state that accepting a job below the prevailing wage will result in sanctions from others—ranging from a decrease in referrals to exclusion from social activities like drinking after work.

To provide revealed preference evidence on sanctions, we design a costly punishment game with a random subset of laborers ("players") who were *not* offered jobs. Each player is paired with an anonymous worker. Using a 2x2 design, the player is

told whether his paired worker: (i) lives in the player's own village or a village that is far away, and (ii) accepted a job at the prevailing wage or at 10% below the prevailing wage. The player can give up some of his endowment to reduce the endowment of his paired worker, providing a mechanism to punish the worker at a cost to oneself.

As expected, there is no punishment of workers who accept jobs at the prevailing wage. In contrast, those who accepted a wage cut are punished 37% of the time. Conditional on punishment, the magnitude corresponds to 37% of average daily labor market earnings (requiring players to give up 7.4% of typical daily earnings in order to undertake the sanction). Moreover, these patterns are similar if the paired worker is from a distant village—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). The willingness to punish those in other labor markets is particularly consistent with individuals viewing norm violations in moral or general terms.

If social norms enable workers to resist downward competitive pressures on wages, this could help us understand equilibrium wage adjustment. In the third part of the paper, we undertake a supplementary suggestive exercise using observational data across India. Specifically, the proposed mechanism that drives our results above is that the lack of labor supply below the prevailing wage stems from social pressure, and is maintained through the threat of social sanctions. One potential implication of this mechanism is that wage floors may be more likely to arise in places with higher social cohesion. This is because in settings with higher social capital, there is potentially more scope for norm enforcement—for example, information flows better through the network (making violations more public) and it is easier to levy sanctions (Jackson et al., 2012; Breza and Chandrasekhar, 2019). We exploit the fact that in India, caste homogeneity is a strong proxy for social cohesion (Munshi and Rosenzweig, 2006, 2016; Mazzocco and Saini, 2012). While in our field experiment, workers within a village are fully homogenous by caste, this is not true for India as a whole.

Using the wage rigidity test from Kaur (2018), we examine whether districts with higher cohesion—proxied by caste homogeneity among agricultural laborers—exhibit more rigidities. We find areas with higher social cohesion exhibit substantively more downward wage rigidity in response to labor demand shocks; they also show correspondingly higher levels of employment reductions in instances where downward wage adjustment is needed, leading to larger boom and bust cycles in employment.

These patterns are consistent with the idea that social conformity can have aggregate implications for markets. Of course, this correlation with wage rigidity is only suggestive—it does not necessarily denote a causal relationship.

Despite these implications, maintaining wage floors could be optimal for workers if it enables them to increase surplus from employers. A simple back of the envelope exercise suggests that this is likely the case: worker surplus is 39% higher relative to the competitive equilibrium. However, we cannot make any direct claims about efficiency. For example, worker collusion could be efficiency enhancing if there is monopsony in the local labor market.

Our findings advance the economics literature on norms and social influence. First, we provide evidence for the idea that because norms enable coordination on the same action, they can have aggregate economic implications. Specifically, we document that norms can shape the equilibrium in economic markets—in our case, by altering the aggregate labor supply curve. Second, our work provides field evidence for social sanctions as a force that helps sustain norms in equilibrium. This augments a rich body of laboratory observations that when individuals are seen as violating norms, others will sanction them—even in the absence of direct benefit of instrumental value to themselves (e.g. Gachter and Fehr, 1999; Henrich et al., 2006; Charness and Rabin, 2002). This helps us understand why norms can be long-lived in societies, and accords with economic theories of how decentralized group behavior can be sustained (Kandori, 1992; Ellison, 1994; MacLeod, 2007). Third, our work relates to the social observability literature. A growing number of studies document that individuals alter their behavior when it is observable to others in a range of settings—including charitable giving, voting, education, and effort in the workplace (Bandiera et al., 2005; Mas and Moretti, 2009; DellaVigna et al., 2012; Bursztyn and Jensen, 2015, 2017; DellaVigna et al., 2016; Bursztyn et al., 2017).[5] We tie such behavior explicitly to the presence of norms, and show that an underlying mechanism that drives social conformity (at least in our setting) is fear of sanctions.

In addition, 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 idea that downwardly rigid wages contribute to high involuntary unemployment levels (e.g. Lewis, 1954; Eckhaus, 1955). Recent empirical evidence points to the continued relevance of these ideas today (Kaur, 2018; Breza et al., 2018).

[5]In related work, Bursztyn et al. (2018) document that altering perceptions of norms can affect labor force participation. See Bursztyn and Jensen (2017) for a review of the literature on social observability.

However, the presence of rigidities in this setting has been a long-standing puzzle in development. Micro-foundations proposed in the previous literature, 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. Our study offers the first piece of positive evidence in support of any micro-foundation for this phenomenon.[6]

Finally, our study has bearing on the labor literature on unions (see Farber and Saks, 1980; Dickens et al., 2007). While the prevalence of formal unions has declined over time, our study indicates that informal versions of these forces have potential relevance for the labor market. As we argue in the Discussion section, 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.

The paper proceeds as follows. In Section 2, we describe the setting and experimental design. We present the results of the main field experiment in Section 5, and the costly punishment game in Section 6. Section **??** discusses potential threats to validity. Section 7 provides suggestive evidence that correlates of social cohesion are predictive of levels of wage rigidity across India. Section 8 uses a back-of-the-envelope calculation to estimate worker surplus from maintaining wage floors. Section 9 concludes with a discussion of the potential generality of the mechanism we document to other settings.

2 Context

The core field experiment takes place in 183 villages in four districts of rural Odisha, India and involves hiring for casual agricultural jobs. Markets for casual daily labor represent a primary employment channel for hundreds of millions of workers in India alone and account for the 98% of the country's hired agricultural labor (NSS, 2010). In our setting, where paddy is the predominant crop, over 70% of survey respondents are primarily engaged in agriculture, with 53% listing daily-wage agricultural labor as their main occupation. 84% of all respondents engage in daily-wage agricultural labor.

Our setting shares many market features common across Indian agricultural labor markets, notably a high degree of decentralization and informality (e.g. Rosenzweig, 1988). Contracts tend to be bilaterally arranged by individual employers and are also short in duration, typically lasting only 1-3 days. The intermittent nature of

[6]Our findings support the ideas developed by Osmani (1990), which offers a model based on informal worker collusion.

any individual employer's labor demand, along with the relatively large number of workers and employers in a village, lead to frequent resorting of hiring relationships. Moreover, minimum wage laws are ignored, and formal unions or other formal labor market institutions are virtually non-existent.

Many of the observations of Dreze and Mukherjee (1989) also resonate in our setting and suggest possible labor market distortions. There are high levels of unemployment, especially during the so-called "lean" seasons of the cropping calendar, wages appear downwardly rigid (also see Kaur (2018)), and the market is largely segmented by gender. During the lean season, when we conduct all of our field activities, employment rates are low, with workers in our study reporting a daily employment rate below 50%. Further, workers report not being able to find work an average of 12 out of 30 days. These high rates of unemployment create the potential for substantial willingness to supply labor below the market wage during the lean season.

A clear prevailing wage for agricultural labor exists in our study areas, again echoing Dreze and Mukherjee (1989). Notably, there is little variation in the wages paid to workers of the same gender within a village, and almost all workers agree about the value of the prevailing wage for a given task. In Figure 2, we use data collected by Breza et al. (2016) in similar Odishan villages to show that almost 80% of male agricultural worker days for a given task inside the village are compensated with the modal village wage. In Figure 3, we plot the deviations between an individual's report of the prevailing wage for men and the employer-verified prevailing wage in our study villages, using responses from an untreated holdout sample of workers.[7] Approximately 80% of respondents agree about the value of the prevailing wage. These two facts are consistent with the presence of a clear wage norm that can be easily followed by laborers when making labor supply decisions. In our experiment, the prevailing wage serves as a clear benchmark for wage job offers.

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, 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.[8] In addition, workers within a village (i.e. those whose primary source of earnings is wage labor) tend to live the same densely-populated area (referred to as the "labor colony"). Such an environment, where workers typically share

[7]See Appendix A.1.2 for a discussion of how we elicited and verified the prevailing wage.
[8]Anecdotal evidence also indicates that employers are required to pay a premium when hiring laborers from other nearby villages.

a caste identity and are embedded in the same risk-sharing and information networks, may help to enable worker collusion and sanctions.

The process through which agricultural workers are typically hired looks similar across our study villages. The employer typically travels to the the "labor colony" one or two evenings before the intended work will take place. He offers jobs to workers who may or may not have a prior job history with the employer. Moreover, the offers may be made in public (in the street or in a central square of the village) or at the worker's house. In addition to the prevailing wage, the timings of the work and breaks, and the inclusion of any meals are commonly understood (and expected) by the workers.

3 Research Design

3.1 Hypotheses We denote the prevailing village wage as W. If worker collusion contributes to a wage floor 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.) Social pressure prevents workers from supplying labor below W. When other workers can observe an individual's job take-up decision, workers will be less likely to accept work below W.

H3.) Violations of the norm result in sanctions. That is, a mass of workers will sanction others who have accepted work below W.

Note that H2 is inconsistent with a preference-based wage norm (e.g. Bénabou and Tirole, 2006). If intrisic altruism fully sustained the norm, social observability should not affect take-up.

We next turn to our core labor supply experiment, which we designed to test Hypotheses 1 and 2. In Section 6, we present survey evidence and our social punishment lab-in-the-field experiment to investigate Hypothesis 3.

4 Experimental Design: Labor Supply

Our core field experiment elicits the willingness for workers to take up one day of agricultural labor under different conditions. In order to test H1 and H2, we vary

both the wage at which the job is offered along with the social observability of that offer. To do this, we partnered with 183 employers (one per study village), who made job offers to a total of 502 workers. Given the gender segmentation of the local markets, all of the workers in the study were males. In this section, we outline the protocols with a focus on the experimental treatments. Appendix A.1.8 contains a more detailed description of the experimental protocols.

Our experimental design involves following the usual procedures for making job offers wherever possible. The hiring employer approaches the worker at his home in the labor colony to offer him one day of employment. The employers are known to the workers, and hire for tasks that they need done on their own land and that are familiar to the workers. The hiring takes place two days before the work day.[9] On the day of work, the employer supervises the workers and gives the standard package of meals, breaks, start times, and end times. The employers in our study are compensated with a lump sum payment for working with us, and they are blind to treatment status.

Figure 4 details the cross-cutting 2x3 treatment design, randomized at the village level. The first dimension of variation, the wage offer, is straightforward. We randomize the wage rate for the take-it-or-leave-it job offer between the prevailing wage, W, and a 10% cut to the prevailing wage, W-10%. The second dimension of experimental variation is the degree of social observability and takes one of three conditions – Public, Employer Only, and Private.

We hold as many aspects of the treatment implementation fixed as possible. In all cases, the employer, accompanied by two JPAL-South Asia enumerators, approaches the pre-selected worker at home and describes the job task, location and timings.[10] The enumerator then conveys the wage level to the worker. The worker then accepts or declines the offer, and the work takes place on specified date.

In the *Public* condition, the job offer is simply made in the street *in front* of the worker's home. Therefore, the employer and any passers-by are able to overhear the job offer. Given how the villages in our study area are organized, the passers-by are largely other villagers who live in the labor colony. In the *Employer Only* condition, the employer and JPAL enumerators follow the same exact protocols but step *inside* of the worker's home. In this case, the employer overhears the wage offer, but other members of the community do not.

[9]This is consistent with the modal gap between hiring and workday for agricultural jobs in the market.

[10]See Subsection 4.1, below, for details of how we selected our sample frame.

To estimate the effect of making the W-10% wage offer observable to others in the community, we can therefore compare take-up in cell D of Figure 4 to that of cell E. However, from an ex ante perspective, this test might be too strong. After all, the employer is also potentially a member of the village information network and might himself spread the wage information to members of the community. This could lead to depressed take-up relative to the worker's true reservation wage, biasing the tests of both H1 and H2 toward zero.

Thus, the best conceptual test of H1 and H2 is a treatment where only the worker knows the wage offer. This motivates our *Private* condition. This treatment is very similar to the *Employer Only* condition, but after the employer describes the task, timings, location, and in-kind benefits, the wage offer is conveyed to the worker by a JPAL enumerator, out of earshot of the employer. In all of the treatments, the JPAL enumerator is the person who tells the wage to the worker. The only difference here is that the employer is not present for this part of the conversation.[11]

To test H1, that a mass of individuals is willing to work at W-10%, we ask whether take-up in treatment cell F is statistically distinguishable from zero. Note that the take-up in treatment cell E, for reasons described above, likely provides a lower bound on the private willingness to work (H1). In order to test H2, whether social observability decrease the take up of work at W-10%, we can ask whether take up in cell F-D is positive and statistically distinguishable from zero. Again, E-D likely provides a stronger test and a lower bound for the difference in take-up between private and public.

One natural concern is that the different social observability conditions may inadvertantly convey other information, changing the beliefs of the workers across treatment cells. We note that our fully saturated research design allows us to net out any level shifters caused by the different social observabilty treatments. That is, we can use the prevailing wage treatments as a placebo. We can also use a differences-in-differences regression specification to absorb such shifters across the observability treatments, comparing (F-D)-(C-A) in our test of H2, for example. Again, in our

[11]In all treatment arms, the JPAL enumerator tells the worker that he is from a research institute with an office in Bhubaneswar, the state capital, and that he is interested in learning about the labor markets for agricultural production in the respondent's village. At the end of the job offer in all treatments, the JPAL enumerator asks a few survey questions about local agricultural practices. In the *Private* treatment, the employer simply is led of the room before the wage is announced.

Public and *Employer Only* treatment, the wage offers look almost identical to common practice in the village, and we can use the results from these two observability conditions alone to bound the results.

4.1 Treatment Implementation We next provide an overview our treatments implementation from village scouting through the day of work. A more detailed description is available in our Protocols Appendix A.

Our experiment took place during non-peak (lean) production periods between 2015 and 2017. The 183 study villages in our sample frame were chosen to satisfy four criteria. First, the laborers in the village needed to be primarily engaged in agricultural activities. Second, the agricultural labor market needed to be closed. That is, the workers all needed to work for the same set of agricultural employers. This is the most common way for labor to be organized in our study areas. Third, the size of the labor colony needed to be between 30 and 100 households (the average labor colony in our sample has 46 households (Table 1)). The lower bound ensured that by making 2-3 job offers per village, we were not communicating directly with a large fraction of the village. The upper bound on village size ensured a level of comparability across study villages. Finally, there needed to be a clear notion of the prevailing wage at the time we came to the village.[12] We provide more details on each of these criteria in Appendix A.1.2.

Once the village was determined to meet our criteria, we returned to recruit an employer who was interested in hiring workers on their land in the following two weeks. The employers were all regular hirers in the agricultural daily wage markets. In order to avoid working with an employer with undue influence, we cut the right tail of land-holdings from the set of potential employers. We also did not work with employers who held political or leadership positions in the village. More details are provided in Appendix A.1.3.

At the same time, we also conducted a quick listing of households in the labor colony.[13] Using this list, we pre-randomized households to approach for the job offer and the order in which they were approached. We did this without having any baseline information about the workers. In each village, we selected a maximum of six households for the treatment, and we made a maximum of 2-3 job offers (in order to minimize the total amount of information injected into any village).

[12]In a small number of villages we considered for the study, the wage was in transition from the previous year's value to the current year's value. We excluded villages in which there was dispersion in the wages paid for a given task done in the two weeks prior.

[13]See the Protocols Appendix A.1.4 for more information about how this was done.

We randomized treatment status at the village level[14]. In every village, we offered jobs in the morning two days prior to the day of work (the modal time lag between offers and the day of work among the employers whom we surveyed). The employer, with two JPAL enumerators, approached each selected household, spoke with the head of the household[15], and told the participant the date, nature, and location of the work. He then allowed one enumerator to take over with a "survey", during which the enumerator revealed the wage rate to the participant. This was common across treatments.

In each treatment cell, aside from the wage level and the observability condition, we attempted to make the protocols as similar as possible. In order to confirm that the *Public* treatment was indeed public, one of the JPAL enumerators recorded the number of people within earshot of the participant at the time the offer was made. Table 1 shows that an average of four individuals observed the hiring across the public treatments.[16] Moreover, we show that the number of onlookers is balanced across the W and W-10% cells (Table 1).

Work happened two days after recruitment. Enumerators conducted a spot check on the work in the morning, collected the employer's contribution to the workers' wages, and, at the end of the scheduled work day, returned to the field to give each worker his daily wage in an envelope (to keep the amount private from the employer). All work day procedures were identical across treatment arms. More details are provided in Appendix A.1.9.

4.2 Data Our primary outcome is whether the approached laborer did indeed work for the employer. This information comes directly from the enumerator's observation of the workers in the employer's fields.

The enumerators also conducted exit surveys in the three days after the day of work. For the main study participants (the approached workers), the survey contained some basic demographic traits, the laborer's primary and secondary occupation, and a full time-use listing of all employment activities for the previous 7 days. The surveys also contained measures of all wages, in-kind transfers, and other cash payments

[14]This helped to preserve consistency in our public treatments and to abstract from concerns about relative pay comparisons.

[15]In the event that the head of the household was not present or not a participant in the agricultural labor market, enumerators randomly selected among workers who were present in the home at the time of hiring. We provide several robustness checks to compare these workers with the overall population. Appendix A.1.7 provides more details

[16]Only four public hiring interactions in our main sample were conducted with no onlookers. This information was recorded for each hiring interaction by one of our enumerators.

received by the laborer for his work. Surveys were also administered to a hold-out, untreated sample of laborers. Given our randomization procedure, these laborers are comparable to the main study participants. Finally, the enumerators also conducted exit surveys with the partner employer. These surveys include information about any in-kind and cash transfers made to the workers (aside from the cash wage) along with information about worker quality and effort. More details about all of the exit surveys are provided in Appendix A.1.10.

4.3 Summary Statistics and Covariate Balance

4.3.1 Balance We present summary statistics of important covariates in Table 1. To test for balance across the treatment arms, we regress the covariate on indicators for each treatment arm, and use a Wald test of joint significance of the coefficients on the treatments.

Of participants who were offered the job at $w-10\%$ in private, 94% of participants participate in the casual daily labor market as their primary or secondary occupation, with 84% reporting casual labor in agriculture to be their primary or secondary occupation. 59% of participants in the group do not own land, highlighting the importance of the casual daily-labor market for their livelihood. About a third of the participants belong to the scheduled tribe group. These descriptive statistics are all balanced across treatment arms.

Employers are not differentially selected across treatment arms; we find no difference across treatments in indicator variables for experience working for the employer ever and within the past year (average in the private wage-cut group of 32% and 24% respectively), and in a scale-rating of employer influence.

Job offers that are made outside the home are equally public across the two wage levels; At below the prevailing wage each hiring has, on average, 4 onlookers (statistically indistinguishable from the number of onlookers for job offers made at the prevailing wage).

We also present information on NREGA as reported by the untreated holdout sample. While holdout sample individuals report that there has ever been NREGA work in 91% of villages, the mean proportion of NREGA workdays reported in the recall grid is only 0.5%. These variables appear balanced across treatment arms.

We achieve balance across treatments for almost all variables, with two exceptions. Participants with low individual involuntary unemployment (defined as below-median number of days in the past 30 that the individual wanted work but was unable to

find it) and those who engage in non-agricultural daily labor are less likely to be in the private $w - 10\%$ treatment group, compared to one of the prevailing wage treatments, or the public $w - 10\%$ treatment group. If these are correlated with lower private reservation wages for the job, they could increase takeup in private at below the prevailing wage. However, we show in Sections 5.1 and 5.3 that our hypotheses hold even for the subset of workers who engage primarily in agricultural labor, and those with low involuntary unemployment.

4.3.2 Attrition Given that our main experimental outcome is directly observed by our enumerators for all workers, we do not need to worry about differential attrition in our core take-up regressions. However, we do use responses to our exit surveys in several places. Recall that we attempted to complete an exit survey for all participants, whether or not they chose to work, and aimed to complete five surveys per village with an untreated holdout sample of individuals. Our survey completion rate for main experimental sample participants offered jobs in private at below the prevailing wage was 88%. In Tables 1 and 2, we find no differential attrition in completing the exit survey, assuaging any concern that participants who turned down the job or those who were offered jobs at below the prevailing wage may have been less likely to respond to our exit survey.

We completed an average of 5.4 untreated holdout sample surveys per village in places where participants were offered jobs in private at below the prevailing wage, and we find no statistically significant differences between this treatment arm and others.

5 Evidence: Labor Supply

5.1 Take-up of the Job Our primary tests of Hypotheses 1 and 2 are presented graphically in Figure 5. Panel A of Figure 5 includes the full experimental sample, while Panel B restricts the sample to the 81% of workers who report agriculture wage work as a primary or secondary occupation (henceforce, "agricultural workers"). Table 3 presents the results in regression form along with all of the relevant statistical tests. Cols. (1)-(2) report OLS regression results for the main experimental sample, where Fully private: $w - 10\%$ is the omitted category. Col. (3) restricts the sample to the agricultural workers. Columns (2) and (3) include task fixed effects and year X month fixed effects. All regressions are clustered at the village-level. In these tests, our dependent variable is job take-up, measured as an indicator for a worker reporting to the job on the day of work.

We begin by presenting the take-up rates of the job offers made at the prevailing wage w. Panel A of Figure 5 shows that, on average across the three conditions, 26% of workers accept the job at w. This magnitude of takeup is reasonable when compared with our exit survey, which finds that on 42% of worker-days, workers desired a job but could not find one[17]. When we compare take-up rates across the three observability conditions at w, we find no statistically significant differences. This supports the claim that there is no detectable effect of the social observability protocols on job take-up when the prevailing wage norm is not being violated (for example, through changing workers' beliefs), although the estimates are somewhat imprecise. We find slightly higher levels of job take-up at w in the subsample of agricultural workers (30% job take-up). But again, we see no detectable differences across the observability treatments. Given the insensitivity of take-up at w to social observability, we pool the three treatments for power in some of our subsequent analyses.

Next, we turn to H1, our hypothesis that workers are privately willing to supply labor at below the prevailing wage. To test this, we consider the take-up rate for the job when it is offered in private at $w-10\%$. Both Figure 5 and Table 3 show that 18% of all workers offered the job under the *Private* treatment accept and complete the work, indicating robust labor supply below the prevailing wage. It is also suggestive of a positive private residual labor supply elasticity, as we would anticipate, though the difference in take-up is not quite significant at conventional levels (p-value = 0.118). Among agricultural workers, the take-up rate for the job in private at below the prevailing wage is also positive, at 21%.

Recall that a stronger test of H1 is whether there is any take-up below the prevailing wage in the *Employer Only* condition. We again find that take-up is robustly positive at $w-10\%$, even when the employer is present. While take-up is 5 pp lower, relative to the fully private setting, the difference is statistically insignificant (p-value = 0.42). The employer's knowledge (alone) of the wage rate does not appear to shift workers' labor supply in quantitatively meaningful ways. The same pattern holds for agricultural workers.

We provide evidence for H2 by considering the effect of social observability on take-up rates for jobs offered at below the prevailing wage. Our first test of this compares take-up rates for work offered at a wage rate of $w-10\%$ in public and in private. Conceptually, this compares a scenario in which only the worker knows his own wage

[17]Based on exit survey recall grids for our untreated holdout sample of workers.

rate with one in which his co-villagers and the employer know the wage rate. We find that participants are 13.6 pp less likely to take up the job when it is offered in public relative to when it is offered in a fully private setting. This is 78% lower than the 18% take-up rate in the private condition (at $w - 10\%$). This difference is even more stark for agricultural workers, who reduce their labor supply by 24.6 pp. Increased social observability (i.e. the *Public* condition), which had no impact on takeup at the prevailing wage, significantly reduces labor supply only for jobs offered at below the prevailing wage.

We can also run a similar test of H2 using a differences-in-differences specification with the w treatments. This allows us to net out any potential level-shifters between the *Employer Only* and *Public* observability conditions. Unsurprisingly, the results are quite similar. In the full sample, we estimate that social observability at $w - 10\%$ causes a decrease in take-up of 13pp (p-value = 0.0481). In the agricultural worker subsample, this difference grows to 18pp (p-value = 0.0086).

Again, recall that we can also test H2 by comparing the job take-up rates at $w-10\%$ between the *Employer Only* and *Public* treatment arms. Given that the employer himself might communicate information about norm violations to other workers, this is an even stronger test. We do find that the increased social observability of the *Public* treatment causes a statistically significant decline in take-up of $w - 10\%$ jobs of 8.4pp (p-value=0.0865), relative to the *Employer Only* treatment. This decline grows to 17pp (p-value=0.0107) for agricultural workers. These findings also suggest that social observability of accepting below-market wage offers only matters in front of other workers. Given these results, in many of the subsequent empirical exercises, we pool the $w - 10\%$ treatments across *Private* and *Employer Only* for power.

We provide several robustness checks of these fresults. First, in Appendix Table A.1, we use randomization inference to check our main empirical conclusions. The results are very similar to those presented in Table 3.

Second, recall that we could only make job offers to households where a male laborer was home at the time of our visit. Appendix Table A.2 presents additional robustness in light of this implementation feature. In Cols. (1)-(2), we restrict our analysis to the first household and first two households where job offers were made. While results are noisier due to the reduced sample size, the results are qualitatively similar. In Col. (3) we code any "doorlock" household as having zero take-up and run an intent-to-treat regression. 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 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 conditions.

5.2 Earnings We next quantify the implications for worker earnings from decreased takeup of the job in public at below the prevailing wage. We hypothesize that supression of labor supply due to the village norm (which can be enforced when job offers are made in public) will result in a lower probability of working for a wage and lower earnings for participants on the day of work. However, if all participants who decided to turn down the job at $w - 10\%$ in public (who were willing to work at the prevailing wage) were able to find alternate employment, then we should observe no differences in total employment and earnings for participants offered the job at $w - 10\%$ in public and in private.

We present our findings in Table 4. Here, every observation is a respondent-day. In Cols. (1) and (2), we focus on the probability of working for a wage and earnings on the day of work as reported in the employment recall module of the exit survey. We find that participants offered the job in *Public* at a wage of $w - 10\%$ are 16pp less likely to work for a wage on the day of work than their counterparts in the *Private* $w - 10\%$ treatment. This corresponds closely to the gap in take-up rate for the job from our "administrative" records. Accordingly, they earn Rs. 32 less for the day, on average. For a sense of magnitude, the Rs. 32 loss is 71% of the mean earnings on the day of work in the Private $w - 10\%$ group. As a proportion of the mean earnings of the control group (not offered the job), the magnitude of earnings losses is even larger.

However, we might think that inter-temporal substitution over a longer time period can mitigate earnings losses from the one-day offer. In Cols. (3) and (4), we confirm that inter-temporal substitution of work does not mitigate the earnings losses from the day of work with the partner employer. Specifically, we find no significant change in earnings or in the probability of working in a 7-day window around (but excluding) the day of work, indicating that there is no detectable spillover effect on other days in the same week.

We then test the average impact of the job offer on earnings for the week in Cols. (5)-(6) by including the day of work in this window [18]. We find that, in that 7-day

[18]We weight the observations to account for missing days in the worker grid due to the timing of the exit survey.

window (including the day of work), participants in the *Public* $w - 10\%$ treatment are on average 6 pp less likely to work on any given day than those in the Private $w - 10\%$ treatment, and earn Rs. 11.82 less per day.

We can use the results in Table 4 to scale the earnings loss from social observability. The coefficients in Cols. (5)-(6) translate to a 49% loss in average weekly earnings for the public $w-10\%$ group relative to the private $w-10\%$ group. In a more conservative calculation, in which we assume there is no decrease in the probability of employment on any given day other than our single day of work,[19] those who are offered jobs at below the prevailing wage in public lose 26% of average weekly earnings, relative to those offered the same jobs in private. It is important to note that while informative, earnings are not a direct measure of welfare (surplus). We discuss worker welfare in Section 8.

5.3 Involuntary Unemployment There are two reasons for why we might expect our results to be especially strong in environments with high levels of involuntary unemployement. First, our design is only powered against the null in environments where there exists positive residual labor supply *below* the prevailing wage. If all agents willing to supply their labor at $w - 10\%$ were able to find jobs at w, we would not expect observe robust take-up in any of the $w-10\%$ treatments. Second, informal union-like behavior may itself lead to rationing, and therefore generating a part of the observed level of involuntary unemployment. We would expect, therefore, that our treatment effects increase in magnitude in the level of unemployment.

In the employment recall grids in our exit surveys, we asked all respondents to report whether they would have preferred wage employment at the prevailing wage to the activity that they reported doing each day. We indicate that a village has high involuntary unemployment if the mean response to this question in the untreated holdout sample is above the median across all study villages. We can therefore compare job takeup rates under the various treatment arms in villages with below- and above-median levels of involuntary unemployment. We present the results in Figure 7 and in Appendix Table 5.

In Panel A of Figure 7 and Col. (1) of Appendix Table 5, we note that the takeup rate for the job offer is 33.3 pp at $w - 10\%$ in private when levels of involuntary unemployment in the village are high (above median). In contrast, it is significantly (16.6 pp, p-value=0.0187) lower in villages with with below-median involuntary unemployment. This effect is even stronger among agricultural workers, with a 23.1 pp

[19] due to the noise in the point estimate in Col(3)

lower takeup rate in low involuntary unemployment areas (p-value=0.009), relative to a mean of 39.3 pp in high invountary unemployment areas (Panel B of Figure 7 and Col. (2) of Table 5). We find similar effects in Cols. (3) through (6) for individual- and village-level involuntary unemployment.[20]

Indeed we also find stronger support for H2 in villages with above-median levels of involuntary unemployment. Namely, in the full sample, take-up of the job falls by 19.6pp (p-value = 0.00354) in high unemployment villages in the *Public* relative to *Private* treatment. This treatment effect of social observability is 16pp smaller in magnitude in low unemployment villages (p-value = 0.0808). The resulting total drop in take-up between the *Public* and *Private* wage cut treatments is only 3.6pp and is statistically indistinguishable from zero. The patterns are even more pronounced in the subsample of agricultural workers. Social observability in high involuntary unemployment villages leads to a drop in take-up of 37.2pp (p-value ¡ 0.001). Again, this large treatment effect is almost completely offset in low unemployment villages (coefficient = 0.355, p-value = 0.00252). Again, we test separately for heterogeneous treatments effects using individual- rather than village-level involuntary unemployment in Cols. (3)-(4), and jointly using both village- and individual-level unemployment in Cols. (5)-(6). We find evidence of similar patterns in both cases.

An alternative prediction might have been that when facing high levels of involuntary unemployment, individuals might be more desperate, and thus be willing to suffer some level of social sanctioning in order to find any wage employment. The findings suggest that this is not the driving force behind our results. Indeed, the foregone earnings consequences of social observability are the highest in villages with high levels of involuntary unemployment.

5.4 Information Flow Our view of the mechanism underlying our main labor supply 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 more diffusive villages - where more individuals are likely to learn of worker take-up decisions - we hypothesize that the main treatment effects will be larger.

This prediction echoes findings in the social networks literature. Breza and Chandrasekhar (2019) argue that individuals behave better in the context of achieving self-set savings goals when information about their savings is likely to spread more

[20]Work the main sample workers, individual-level unemployment is constructed in a similar way to village-level unemployment, but only using days in the recall grid excluding the day of work. High involuntary unemployment is an indicator for above-median involuntary unemployment in the main worker sample.

widely in the village network. Alatas et al. (2016) show that more communities with more diffusive networks have better information about the characteristics of community members. Finally, in their theory of social enforcement, Jackson et al. (2012) show that networks with denser structures are better suited to maintaining cooperation.[21]

In order to look for supporting evidence, we asked two questions about village diffusiveness in our untreated holdout sample. Specifically, we asked respondents a) the extent to which laborers learned about the wages at which others accept agricultural work and b) how many others would find out if a worker accepted an agricultural job at below the prevailing wage.[22] We 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 smaller for these low-diffusiveness villages than for the high-diffusiveness villages.

In Table 6 we present the results. To economize on power, we pool all of the social observability treatments for wage w, and we pool the *Employer* and *Private* treatments for wage $w - 10\%$. In Col. (1), we consider the diffusiveness of wage information in general and find that publicizing a low wage offer in highly diffusive villages leads to an approximately 20 pp decline in take-up rates (p-value=0.00344). However, in low diffusiveness villages, this large treatment effect is almost completely offset (coeff=17.0 pp, p-value=0.0701), leading to no measurable differences in take-up rates between public and private low-wage offers. The same pattern holds when, in Col. (2), we instead use the second measure of diffusiveness that more directly addresses norm violations. Finally, in Col. (3), we show that the magnitudes of both the decline in take-up rates (coeff= -30.8 pp, p-value=0.0745) and the offsetting effect in low diffusiveness villages (coeff=21.4 pp, p-value=0.0614) are larger for the subset of agricultural workers.

These findings are consistent with our proposed mechanism. First, more diffusive villages may better publicize norm violations, which may lead to greater social sanctions. Second, more diffusive villages may be able to sustain greater levels of collective action, again creating stronger incentives to follow the norm.

5.5 Threats to Validity and Interpretation Next, we discuss several potential threats to the interpretaion of our main results.

[21]Specifically, networks with high levels of triadic closure.

[22]Unfortunately, measuring network structure in each study village was beyond the scope of our field activities.

Employer Bargaining Our preferred explanation for our findings is that workers behave collectively to maintain village wage norms. However, one different mechansim that might generate predictions similar to H1 and H2 is employer bargaining. Under this theory, workers may have heterogeneous reservation wages, such that those with reservation wage below w may prefer to hide that fact from the employers in the village in order to extract more private surplus. One implication is that workers only worry about others in the village observing their acceptance of wage cuts insofar as that information will be transmitted to the village employers.

The fact that the presence of the employer does not appear to lead to a meaningful reduction in labor supply at $w - 10\%$ is already seemingly at odds with the employer bargaining mechanism.[23] In Appendix Table A.3, we examine whether the treatment effects look different when the employer is more important for the worker. If the employer bargaining story were true, then we should expect large drops in job take-up rates moving from *Private* to *Employer* when the worker has a pre-existing relationship with the employer. We find no evidence of this pattern. In fact, the point estimates are positive rather than negative, though noisy. Thus, employer bargaining does not appear to be a primary driver of our results. Furthermore, it is unclear why such a mechanism would trigger social sanctions if agents only worried about revealing their own individual reservation wages.

Side Payments and Adjustment on Other Margins Our hypotheses are constructured under the assumption that working with the partner employer at a wage of $w-10\%$ is indeed less valuable than working at w. Using our exit surveys, we can check whether employers tried to compensate workers for the low offer wage by making cash side-payments, by shortening the work day, by increasing the value of in-kind perks, or by allowing workers to shirk. The results are presented in Appendix Table A.4, pooling across all social observability treatments.

We first consider whether workers in the $w - 10\%$ treatments indeed received lower wages than the prevailing wage. Overall, we find that only 4 out of the 74 workers report a wage different than what was offered at the time of hiring. Moreover, the difference in recieved wage is small and statistically indistinguishable from zero, comparing the $w - 10\%$ treatments to th w treatments. Similarly, we find no strong evidence that the work day was shorter for the workers earning lower wages or that the $w - 10\%$ group was compensated with more meals.

[23]However, this test is admittedly imperfect. If workers worried about information spreading to other employers, it is conceivable that other laborers might still transmit that information to them.

Turning to worker selection and quality, we find that if anything, the workers in the $w-10\%$ treatments are less likely to have worked for the employer in that past, though the coefficient is far from significant at conventional levels.[24] We also find very small, though imprecisely estimated, coefficients on average worker quality.[25] Finally, in Col (6), we restrict the sample of $w-10\%$ workers to only the *Private* treatment, where the employer made his assessment not knowing the wage rate of the workers. Again, we find a small positive, though noisy, point estimate.

Furthermore, even if there were compensating differentials that partially closed the wage gap between w and $w-10\%$, then they would most likely cause an increase in take-up across all $w-10\%$ treatment cells. Moreover, if the employer were compensating workers through other channels, it's unclear why taking the job would be viewed as a norm violation worthy of social punishment. Thus, we do not think that compensating differentials can fully explain our findings.

Threats to Implementation Finally, we consider two other concerns that might impact the interpretation of our findings.

One might worry that the private wage offers did not remain private. As mentioned above, we limited the number of job offers to a maximum of three per 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.

An additional potential concern with our design is that the public treatments may have provided workers with information about the prevailing wage—e.g., through potential comments from onlookers. This information, in turn, could have depressed take-up of public jobs below the prevailing wage. However, this is inconsistent with this setting: the prevailing wage is general knowledge, as validated in our exit survey (Figure 3). As further support of this idea, in Appendix Table A.5, we document that among workers who were approached for job offers, reports of the prevailing wage are not systematically different across treatment cells.

[24]Even if the set of workers willing to accept an offer at $w-10\%$ is somewhat negatively selected, in these markets all workers have been employed at the prevailing wage and are thus capable of satisfying the basic job requirements.

[25]If lower wages select for a lower quality worker (e.g., if the outside option of on-farm employment is more valuable for higher ability workers), then the small positive coefficient on the work day rating is not consistent with high levels of moral hazard (perhaps stemming from negative gift exchange or efficiency wages).

6 Evidence: Sanctions

We posit that the observability effects stem from an attempt to avoid social disapprobation. However, other reputational mechanisms could produce patterns consistent with H1 and H2. Namely, shame from being seen as financially desperate enough to accept a wage cut is one such alternative. Rather than attempt to fully rule out all other potential mechanisms, we take on the more modest aim of providing positive evidence for H3 - that individuals are willing to sanction norm violators.

6.1 Survey Responses We first use survey evidence to show that wage-setting happens in a decentralized manner, which highlights the role of social norms (and the sanctions that preserve the norm) as an equilibrium coordination device. In follow-up surveys with control group workers (Figure A.1), 89% of workers agree that there is no village-level meeting for all or most laborers in the village to discuss the wage, and 97% state that there is no meeting between laborers and landowners to bargain over the wage for the season.

We then tabulate 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 8.

59% 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. 56% of respondents said that a laborer who accepted the wage cut would not be included in such an opportunity. In addition,17% 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). 66% 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?" 87% 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.

6.2 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 6-8 workers 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.[26] The player's payoff is determined by a random roll of the dice, in which one of his four rounds is implemented.[27]

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 9 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 7 presents these results in regression form. Col. (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). Col. (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. Cols. (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, Col. (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).

[26]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.

[27]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.

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.

7 Correlation with Wage Rigidity

In this section, we explore whether the mechanism documented in our field experiment has potential relevance for wage rigidity. To motivate this link, we first document that workers believe that acceptance of a wage cut by one individual can affect the prevailing wage–generating a potential externality from an individual's labor supply onto the equilibrium wage for all workers. Appendix Figure A.2 documents worker beliefs, using data collected by Kaur (2018) in a different set of Indian villages that span 6 districts in two states.

Workers recognize that if an individual worker agrees to work below the prevailing wage, he likely increases his own individual chances of employment (Question 1). 84% of workers in this broader sample also believe that other workers would get angry with such behavior, suggesting the relevance of our mechanism more broadly within India. In addition, 74% of workers believe that such behavior could lead other employers to try to pay lower wages for future work (Question 3). This suggests that, according to worker beliefs, a sufficient number of deviations from the social norm could undermine the wage floor in the village.

Our experimental results indicate that social pressure, and specifically the threat of social sanctions, is what prevents such deviations from occurring. In areas with less social cohesion, it may be harder to levy meaningful social sanctions: workers will be less socially integrated, potentially less reliant on each other (e.g., for leisure, marriage networks, job networks), and information will flow less well through the

network (making it harder to learn about deviations and enforce them across the network).[28]Consequently, a potential implication of our hypothesis is that, in areas with less social cohesion, wage floors will be more strongly maintained.

We use observational data from across India to test one potential implication of this idea: whether stronger social cohesion leads to more downward wage rigidity.[29] In India, caste is a strong proxy for in-group and social cohesion (Munshi and Rosenzweig, 2006, 2016; Mazzocco and Saini, 2012). In our experiment, the composition of laborers is extremely cohesive by caste: all agricultural workers in a given village belong to the same caste (Scheduled Caste or Scheduled Tribe), and the median number of subcaste groups within a village is 1. This indicates a high level of social cohesion in our sample—helping explain the strength of our experimental results, with virtually no agricultural workers willing to accept wage cuts in public.

We exploit the fact that across India, the level of caste cohesion varies substantively. We use the National Sample Survey (NSS) household data (all employment rounds from 1983-2009, covering all of the 600+ districts in India). We measure caste heterogeneity by constructing a Herfindahl index of the caste composition of agricultural workers.[30]

To test whether social cohesion correlates with wage rigidity, we use the wage rigidity test developed by Kaur (2018). This paper tests how wages and employment respond to transitory labor demand shocks (generated exogenously by rainfall). The core result in the paper is that lagged positive shocks generate ratcheting in the labor market. Specifically, wages adjust upward in response to positive rainfall shocks.

[28]Munshi and Rosenzweig (2006) and Rosenzweig and Stark (1989) link caste networks to investments in human capital, job choice and, marriage markets. Munshi and Rosenzweig (2016) argue that caste groups provide high levels of social insurance largely because of their ability to self-monitor and enforce collective punishments.

[29]In order to maximize their surplus, workers could distort wages above market clearing levels, but the wages themselves could still adjust to shocks. However, as we describe above, the norm that operationalizes union behavior is that individuals should not accept jobs below the prevailing wage (e.g. Figure 1). This rule effectively creates downward wage rigidity. This highlights a way in which norms are a crude technology, which must be operationalized through simple decision rules that are observable. In contrast, formalized groups can write state contingent rules for their workers, enabling more optimal outcomes. This point has been made, for example, in Elinor Ostrom's work on key ingredients through which informally organized groups provide public goods (e.g. Ostrom et al., 1992).

[30]The NSS measures four caste categories: Scheduled Caste, Scheduled Tribe, Other Backward Caste, and General Caste. Prior work has shown that subcaste in particular is a dominant indicator (Mazzocco and Saini, 2012). Since the NSS only captures these caste categories, our analysis relies on the assumption that members of the same caste show stronger in-group cohesion than those of different castes.

However, in the following year, when the positive shock has dissipated and rainfall is back at its normal levels, wages do not adjust back down—they remain ratcheted up. Because of this distortion on the wage, agricultural employment falls: it is lower than it would have been in absence of the lagged positive shock.[31]

Panel A, Col. (1) of Table 8 replicates the core result for wages from Kaur (2018):

$$\ln w_{idt} = \alpha_0 + \alpha_1 Pos_{dt} + \alpha_2 Pos_{d,t-1} NonPos_{dt} + \delta_d + \rho_t + \varepsilon_{idt},$$

where w_{idt} is worker i's average nominal daily wage in district d in year t, Pos_{dt} is a binary indicator for having a positive shock (rainfall above the 80th percentile of the district's usual rainfall distribution), and $Pos_{d,t-1}NonPos_{dt}$ is a binary indicator for having a positive shock in the previous year and no positive shock this year. The omitted category in the regression is no positive shock this year or last year.

Relative to having no shock, wages rise robustly by 6.3% in response to having a positive shock this year. In addition, consistent with rigidities, lagged positive shocks also positively predict current wages: wages are 5.3% higher if there was a positive shock last year than if there had been no lagged shock.

We examine whether these wage rigidity effects are mediated by the level of social cohesion, proxied by caste heterogeneity. For this test, we add interactions of caste heterogeneity to the shock covariates. Such analysis is, of course, only suggestive. As with any heterogeneous treatment effects, our social cohesion proxies may be correlated with other factors, and may themselves be endogenously determined. We consequently view this as a descriptive exercise, not a causal one.

In Col. (2), we proxy for social cohesion by constructing a Herfindahl index of caste heterogeneity among those who are observed as doing any agricultural wage labor in the district. We interact each shock covariate with a dummy for a below median value of the index—indicating a diversity of castes among agricultural wage earners. In areas with high social cohesion, there is strong wage rigidity: lagged positive shocks lead to a 10% increase in current wages. However, in areas with low cohesion, the interaction term of -0.0826 offsets the level effect (significant at the 10% level), and we cannot reject that lagged shocks have no predictive power for future wages. In contrast, we do not see a strong interaction effect by social cohesion

[31]The paper also examines downward wage adjustment in response to negative shocks. However, there is no clean test for employment effects for negative shocks; the paper focuses on lagged positive shocks to look for employment effects. Consequently, in the below, we focus on lagged positive shocks as it is the core test to look at both wage and employment effects. In addition, pooling to look at current and lagged positive shocks increases power in the analysis relative to the 6 cell specification in the original paper.

for current positive shocks; this serves as a placebo test, and suggests that places with high vs. low caste cohesion do not simply have different agricultural production functions. In Col. (3), we use an alternate definition for the cohesion proxy measure: the Herfindahl index of caste heterogeneity among all individuals who state that their primary or secondary occupation is agricultural wage labor. The results are similar to those in Col. (2).

In Panel B, we examine whether this correlation tracks the employment effects of rigidity. Col. (1) replicates the basic employment test. Employment rises in response to current positive shocks. However, the following year, when wages are ratcheted above market clearing levels, employment is lower than it would have been in the absence of the lagged positive shock—consistent with boom and bust cycles. We add interactions with the proxy for social cohesion among agricultural wage earners in Col. (2). In areas with high social cohesion, lagged positive shocks lead to a decrease in weekly employment of 0.234 days or 13%. However, in areas with low cohesion, we cannot reject that there is no employment effect of lagged shocks: the interaction term of 0.189 (p-value of 0.03) almost fully offsets the level effect. This is consistent with the fact that there is no lasting ratcheting effect on the wage from lagged shocks in Panel A. As before, there is no significant interaction effect of social cohesion with current positive shocks.

Appendix Figure A.4 shows the underlying distribution of each caste Herfindahl index across Indian districts. Appendix Table A.6 shows robustness of these results to alternate specifications, such as the linear Herfindahl and linear in ranks in the Herfindahl.

These descriptive findings indicate that areas with low social cohesion exhibit larger levels of downward wage adjustment in response to labor market conditions. Consequently, areas with high levels of social cohesion exhibit not only more wage rigidity, but also higher levels of business cycle volatility. A causal analysis of such forces is beyond the scope of our paper. While only suggestive, the results in Table 8 are consistent with the view that social cohesion, and its resultant ability to lead to stronger social norms, could have aggregate implications by leading large numbers of workers to coordinate on the same strategy.

8 Surplus Estimation

One key question is whether workers benefit from adhering to the wage norm. On one hand, the norm might help the laborers to behave as a single monopolist,

extracting surplus from the employers. On the other hand, it is possible that the norm originated under different labor market conditions and could actually make the workers worse off. After all, while wage floors increase wages for the average worker, they also raise the possibility of involuntary unemployment.

We conduct a simple back-of-the-envelope exercise to estimate the counterfactual market-clearing level of wages and employment in the absence of the wage floor and estimate the change in worker surplus from moving to the wage floor equilibrium. Any such exercise requires a number of strong assumptions. We consider a static, 1-sector environment, where following Lee and Saez (2012), the workers with the highest reservation wages are rationed first under the wage floor. We also assume that employers do not behave monopsonistically.

Figure 10(a) illustrates the distortionless competitive equilibrium (L^*, W^*), and Figure 10(b) illustrates the distorted wage floor equilibrium (L^F, W^F). Workers are better off under a wage floor if the increase in average wages for those who remain employed is large enough to offset the portion of the worker surplus that becomes deadweight loss.

We proceed by estimating the demand and supply curves and assume that both are linear in the neighborhood of the observed and counterfactual wage and employment levels. To estimate the labor supply curve, we use the data directly from our field experiment, namely the levels of take-up and wages from the private $w - 10\%$ and the pooled w treatments. To estimate labor demand, we observe that the equilibrium level of employment under a wage floor (L^F, W^F) is determined by the demand curve and estimate those quantities using the employment levels and earned wages reported in our untreated hold-out sample. We also use the labor demand elasticity estimated in Kaur (2018).

We present our calculations in Table 9. From our full sample of participants (Col. (1)), we find that the counterfactual equilibrium wage in the absence of distortions is 7% lower than the observed wage, and employment is 7% higher. We do estimate that workers benefit from the wage floor, with an increase in workers' surplus of 64% relative to the competitive equilibrium. We should also note that 96% of the gains to the workers come at the expense of the employer surplus and only 4% from deadweight loss. In Col (2), gains for agricultural workers are qualitatively very similar, though the gains are more modest. Our calculations, albeit crude, indicate that the ability to set a wage floor helps workers extract more total surplus.

We then examine our key assumptions. The labor supply elasticity estimate of 3.89 from our experiment is a *residual* labor supply elasticity from workers who were unemployed at the time of the job offer. This elasticity is therefore an upper bound on the true labor supply elasticity, which is consistent with its large magnitude relative to other measures in similar contexts (e.g., **????**).Similarly, the labor demand elasticity measure is based on data from the peak agricultural season, and may be inelastic relative to the true labor demand elasticity in our lean season experiment. We therefore analyze the sensitivity of our main estimates to a range of possible labor supply and demand elasticities between 0.5 and 4 (Table 10). Within these ranges of demand and supply elasticities, there are gains to producer surplus from the wage floor under efficient rationing of between 8% and 98%.[32] As with Lee and Saez (2012), workers gain more when labor supply is relatively elastic and labor demand is relatively inelastic.

Next, we relax our assumption that rationing is efficient. We present a version of our results in which workers with the highest surplus (lowest reservation wages) from the job are rationed first under the wage floor, which we term "inefficient rationing". Logically, this implies a greater loss in worker surplus from the rationing of limited jobs than in the base case. We illustrate this scenario in Figure 5(b), and provide estimates in Table A.7 . Even under this conservative assumption on the rationing of jobs, worker surplus increases up to 72% when labor demand is relatively inelastic ($\eta < 2$).

Finally we note that, contrary to our assumption, if the employers did exert monopsony power, then this would lead to even greater gains in surplus for workers under the norm. This makes our main estimates a lower-bound on workers' potential surplus gains. Moreover, in that case, acting like an informal union might even be efficiency-improving.

9 Conclusion: Potential Generality of Mechanism

We find evidence that workers would privately like to supply labor below the prevailing wage, but do not do so when their take-up decisions are 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

[32]With the exception of the case in which labor demand is extremely elastic ($\eta = 4$) and labor supply is extremely inelastic ($\nu = 0.5$).

cuts distort labor supply behavior, with large impacts on the foregone earnings of unemployed workers.

The forces we document in this paper have potential generality beyond our specific setting. Our proposed mechanism hinges on two features: a clearly defined norm (violations over which are observable) and a mechanism to impose social sanctions (in order to prevent individual deviations). These features can arise naturally in many settings with repeated interpersonal interaction.

This makes the labor market an especially relevant economic domain. Collective behaviors among workers—e.g., mass walkouts with foregone wages, the coordinated restriction of output, punishment of rate busters—have been documented across history and contexts, in settings with no formal unions (Clark, 1984, 1987). In the US labor market, norms have the potential to arise at the establishment level, where social interaction is high: workers see their peers for eight hours per day every day. In addition, peers have the ability to levy sanctions that are meaningful for success at work (e.g., help with tasks, undermining a peer with the manager) and also meaningful for experienced utility (e.g., eating lunch together, chatting at the coffee machine, warmth in routine exchanges). Consistent with this view, in *The Labor Market as a Social Institution*, Robert Solow (1990) argues that the social norms that naturally arise in the workplace are inherent to what distinguishes labor from other commodity markets. He posits that the resultant implicit social pressures on workers—such as pressure to not undercut co-worker wages by accepting wage cuts, or to not exceed co-workers' output—are important for understanding outcomes such as wage rigidity, equilibrium unemployment, and productivity compression.

More broadly, the presence of cartel-like behavior among decentralized individuals has been qualitatively observed in a range of markets. For example, NASDAQ traders historically only quoted buy and sell prices in denominations of even-eighths (i.e. 2/8, 4/8, etc)—avoiding odd-eights increased their income by widening bid-ask spreads (Christie and Schultz, 1994). However, in its formal investigation of this behavior, the US Department of Justice was unable to make a case for explicit collusion or coordination. Rather, qualitative interviews revealed that this pricing behavior reflected a norm: traders implicitly understood that this pricing behavior should be followed. Moreover, traders felt that deviations would be punished through social sanctions by fellow traders—e.g., exclusion from social events like after-work drinks, and awkwardness or hostility when facing a trader on the other side of a transaction.[33]

[33]We thank Doug Bernheim for pointing us to this example.

Similarly, in many US cities, all real estate agents charged a 6% commission on housing sales, regardless of circumstance (Hsieh and Moretti, 2003). In this setting as well, agents have to interact with each other on each deal, and are also dependent on agent networks for referrals—making social disapprobation potentially both socially and financially costly.

The impact of social norms on markets may be particularly relevant in poor countries. Given the small scale of most enterprises, the "firm" is often a single individual—for example, a vendor in a market, whose stall is surrounded by that of other vendors who he sees each day. This may help explain price collusion or rigidity in product markets—from taxi drivers to vegetable vendors to agricultural traders (e.g. Bergquist, 2018). Similarly, the communal nature of village economies may help explain price homogeneity in a range of factor markets—from bullock and tractor rentals to sharecropping (e.g. Shaban, 1987). More broadly, the high levels of social capital in networks in poor countries may make it more likely that norms arise and can be sustained effectively through punishment.

While only speculative, one implication of this idea is that, through the process of development, the anonymity that arises in markets involving firms rather than individuals has the potential to generate more competitive outcomes. However, given that a substantive component of economic exchange—for example, in the labor market—must invariably involve repeat interpersonal interaction, our findings suggest that social norms are a potentially relevant force for market equilibria in a variety of contexts.

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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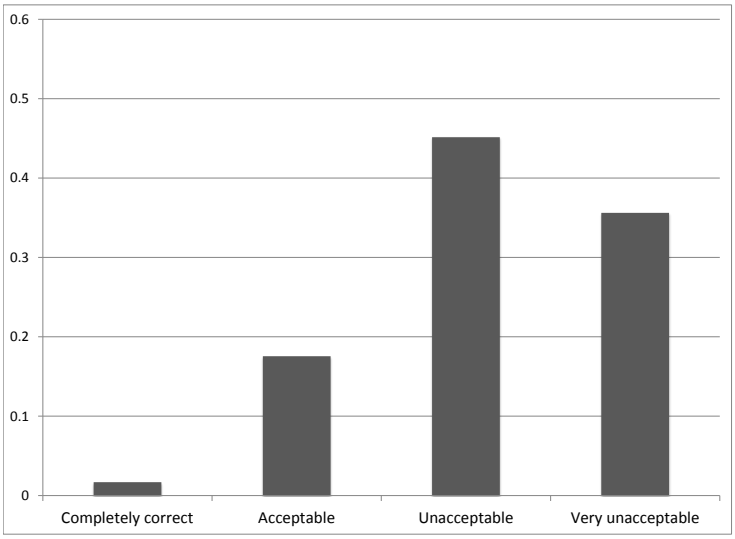
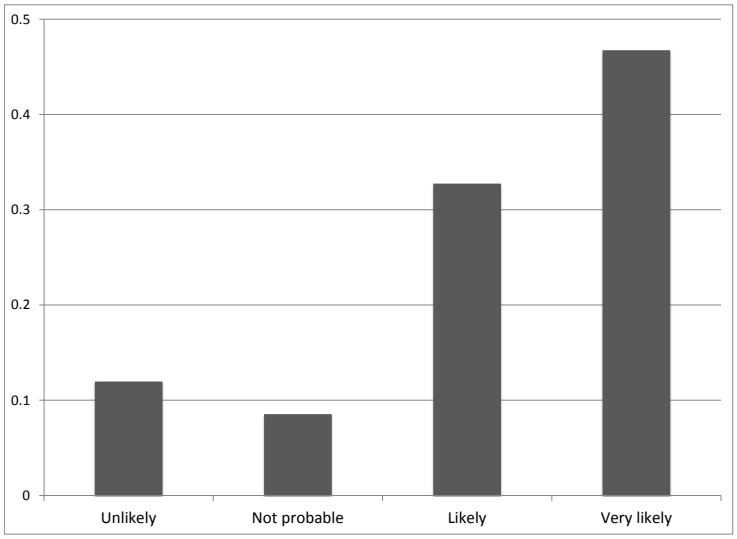
Figures



(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

Note: These figures graph the exit survey responses from(N= 370) participants to questions about the acceptability of wage cuts and about other workers' responses to a worker taking a wage cut. We restrict the sample to participants from villages in which the participating employers offered jobs at prevailing wages.

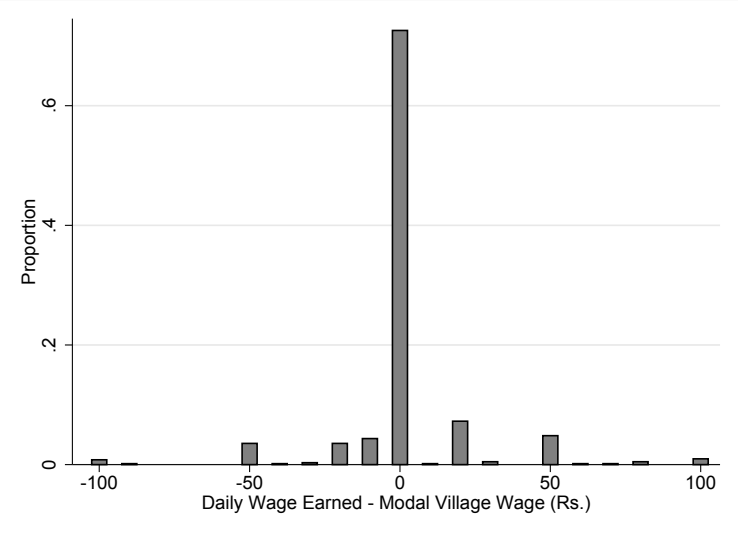


FIGURE 2. Distribution of Wages Inside the Village. *Source:* Breza, Kaur, and Shamdasani 2018.

Note: This figure graphs the dispersion from the task-specific modal prevailing wage earned for agricultural activities carried out within a village. Wage reports are taken from exit surveys conducted in similar villages in Odisha among an untreated holdout sample in Breza, Kaur, and Shamdasani 2018.

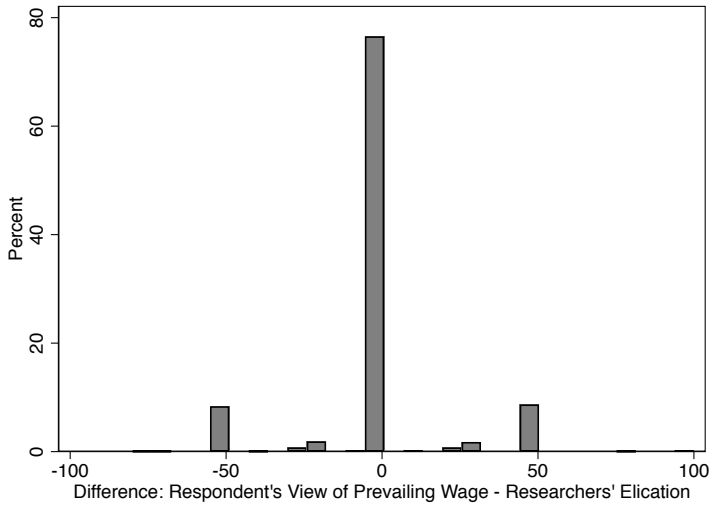
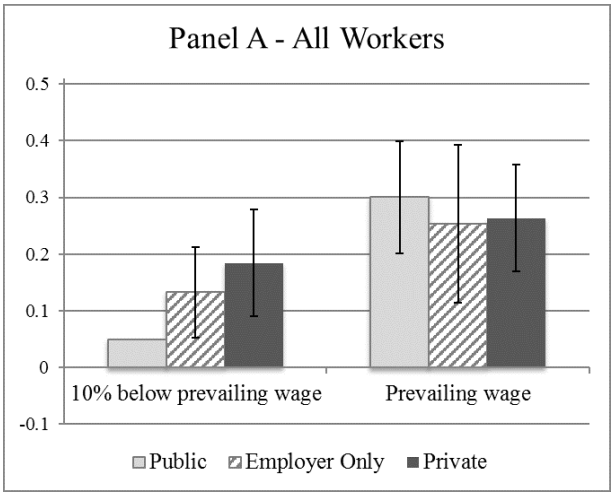


FIGURE 3. Untreated Holdout Group Reports of Prevailing Wage, Normalized by Informant Report.

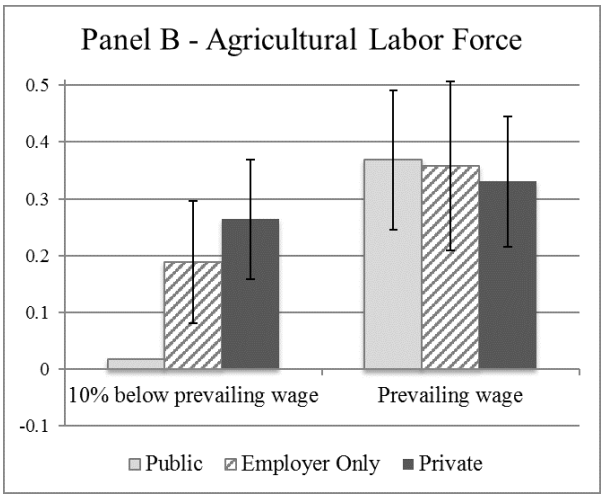
Note: This figure graphs the distribution of the difference between the prevailing wage reported by untreated holdout households in a village and the prevailing wage reported by the informant in that village.

Social Observability	Wage Level: w	Wage Level: w-10%	
Public	A	D	Job offer made on street in front of worker's home
Employer only	B	E	Job offer made inside worker's home
Private	C	F	Job offer made inside worker's home: employer out of earshot for wage

FIGURE 4. Experimental Design



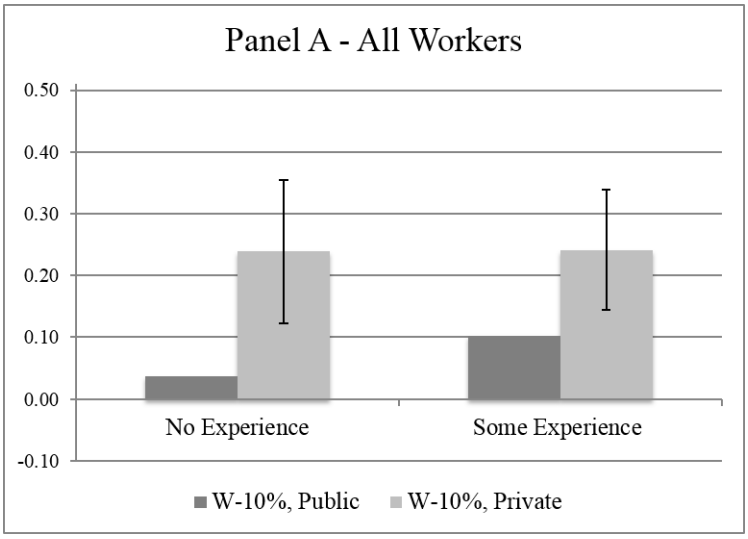
(a) All Workers



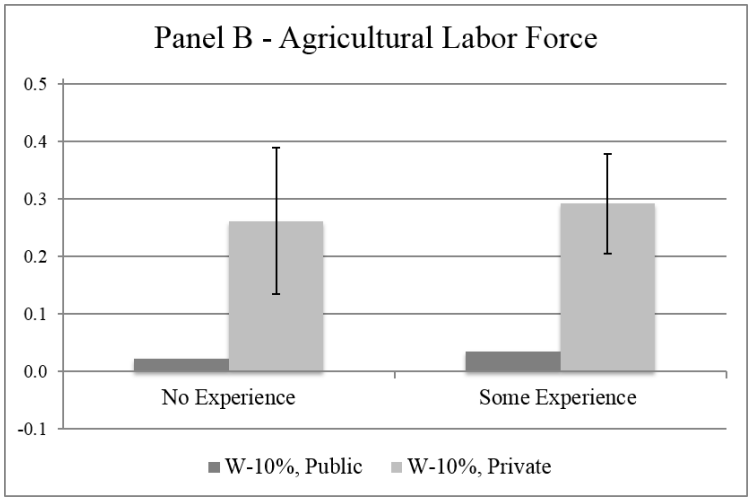
(b) Agricultural Workers Only

FIGURE 5. Job Take-Up by Treatment

Note: These figures graph the take-up rates for the job offer under different treatment arms. Job offers in each village are made either in public, with only the employer present, or in private, and are offered either at the prevailing wage, or at 10% below the prevailing wage. Each bar represents the take-up rate for the job as defined by attendance on the day of work. Panel A uses the entire sample (N=502 participants) while Panel B restricts the sample to casual daily wage laborers (N=363 participants), who report their primary or secondary occupation to be agriculture. All robust 90% CIs are constructed using standard errors from a test of the difference between the take-up rate for that treatment arm and the take-up rate for the public job offers at 10% below the prevailing wage. These results are also presented in the form of regressions in Col. (1) of Table 3. In Table 3, we include task and yearXmonth fixed effects in Col. (2) with the full sample and in Col. (3) with the sample restricted to casual daily wage laborers only.



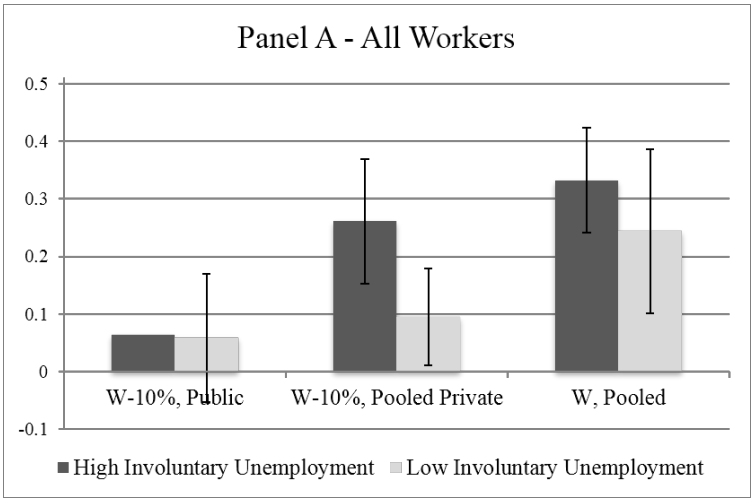
(a) All Workers



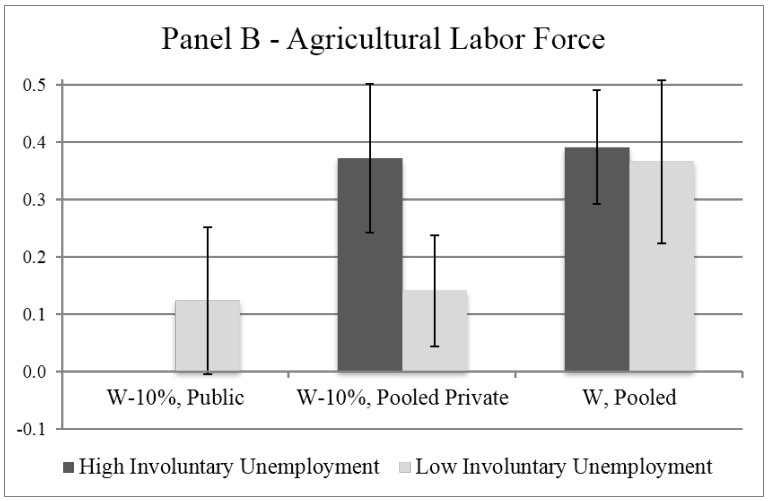
(b) Agricultural Workers Only

FIGURE 6. Job Take-Up by Level of Prior Experience With Employer

Note: These figures present heterogeneous treatment effects by high and low prior experience working forthe participating employer. High employer experience is measured by an indicator for having worked for the participating employer in the past. Job offers in each village are made either in public, with only the employer present, or in private, and are offered either at the prevailing wage, or at 10% below the prevailing wage. Each bar represents the take-up rate for the job as defined by attendance on the day of work. Panel A uses the entire sample (N=426 participants) while Panel B restricts the sample to casual daily wage laborers (N=350 participants), who report their primary or secondary occupation to be agriculture. All robust 90% CIs in each panel are constructed using standard errors from a test of the difference between the take-up rate for that treatment arm and the take-up rate for the public job offers at 10% below the prevailing wage. These results are also presented in the form of regressions in Cols. (2) and (3) of Table A.3. Here and in Table A.3, we include task and yearXmonth fixed effects.



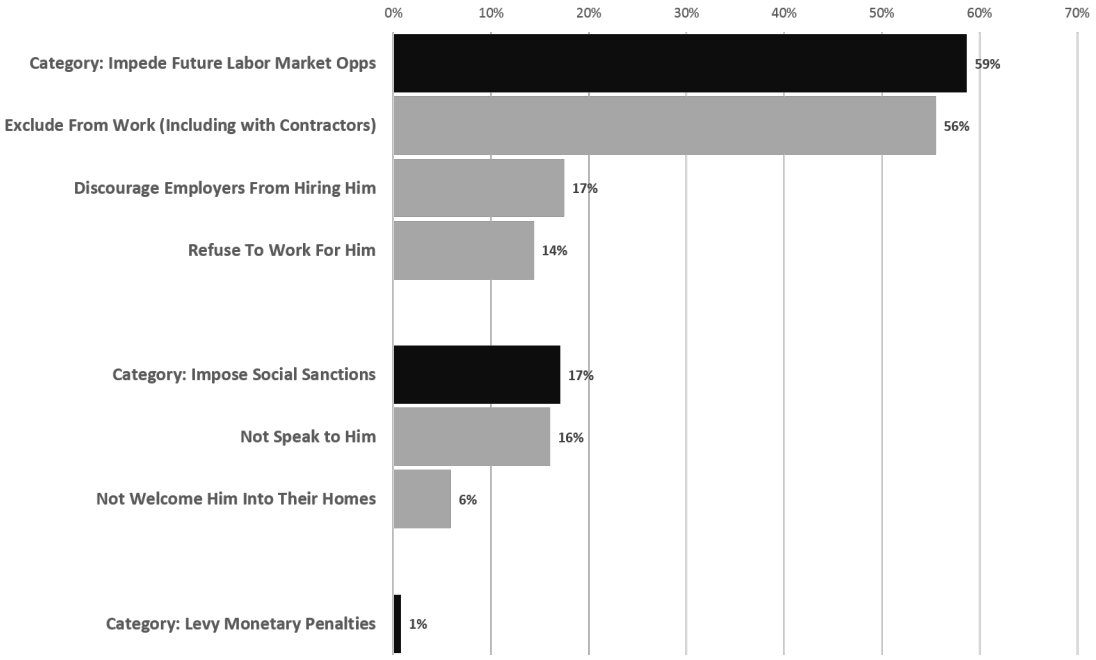
(a) All Workers



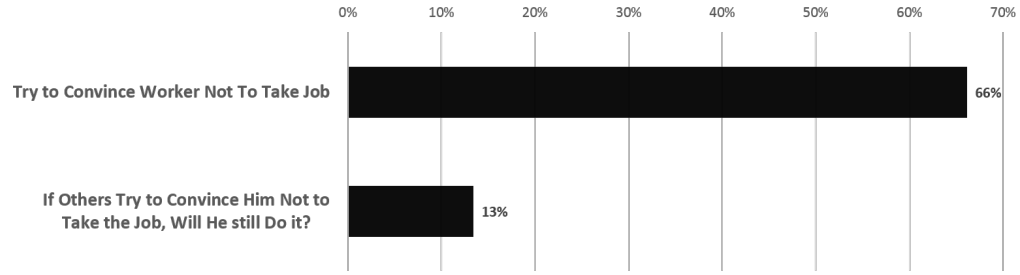
(b) Agricultural Workers Only

FIGURE 7. Job Take-Up by Level of Involuntary Unemployment

Note: These figures present heterogeneous treatment effects by below- and above-median involuntary village unemployment. Involuntary village unemployment is measured by the mean number of days in the past 10 days that untreated holdout sample respondents would have preferred a prevailing-wage job to their actual timeuse, as reported in the untreated holdout sample survey. Job offers in each village are made either in public, with only the employer present, or in private, and are offered either at the prevailing wage, or at 10% below the prevailing wage. Each bar represents the take-up rate for the job as defined by attendance on the day of work. Panel A uses the entire sample (N=493 participants) while Panel B restricts the sample to casual daily wage laborers (N=363 participants), who report their primary or secondary occupation to be agriculture. All robust 90% CIs are constructed using standard errors from a test of the difference between the take-up rate for that treatment arm and the take-up rate for the public job offers at 10% below the prevailing wage in villages with above-median involuntary unemployment. These results are also presented in the form of regressions in Cols. (1) and (2) of Table 5. Here and in Table 5, we include task and yearXmonth fixed effects.



(a) Sanctions



(b) Social Pressure

FIGURE 8. Survey Evidence - Sanctions for Accepting Wage Cuts

Note: This figure graphs exit survey responses from 1,448 untreated holdout sample households (who did not participate in our experiment) to the question: "Suppose a laborer accepts work at a rate lower than the prevailing wage. What will be the reaction of other workers?" Respondents were able to select as many responses as were applicable, and had the option of providing their own response. Responses were then aggregated into the categories shown in (a). In (b), we graph the proportion of respondents who answered 'Yes' to the question "If others try to convince him not to take the job, will he still do it?" to elicit workers' anticipation of the effectiveness of the verbal sanctions.

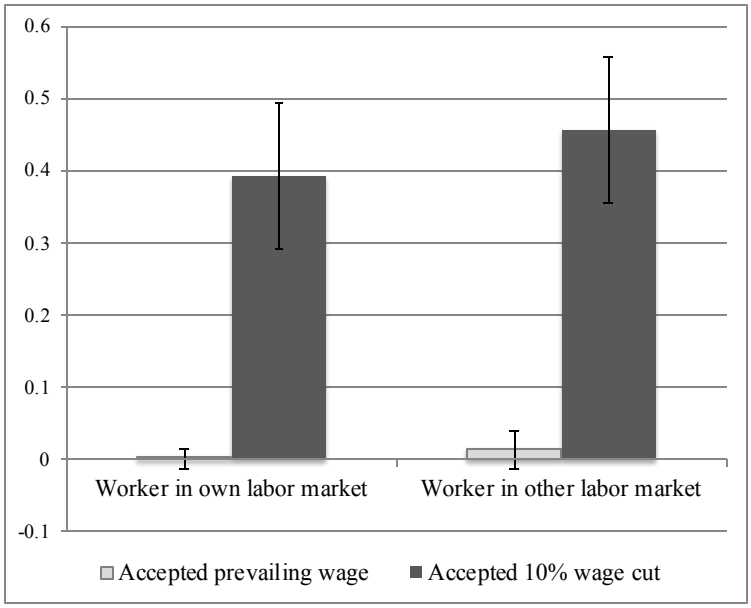
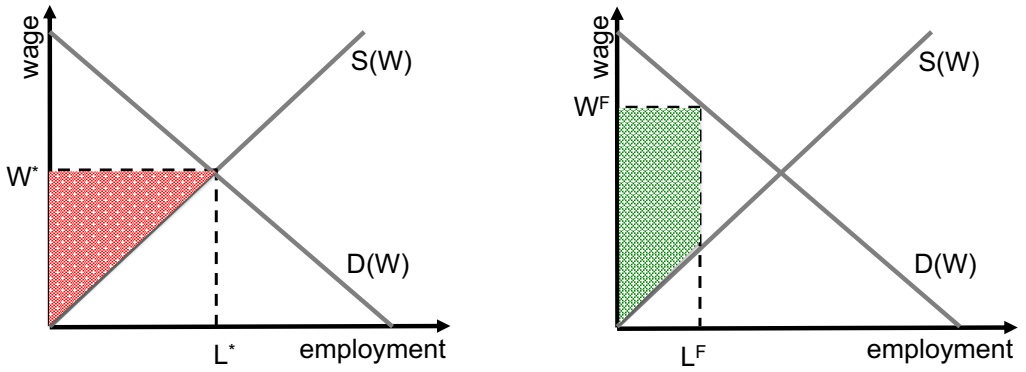


FIGURE 9. Sanctions: Costly Punishment Game

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 figure plots 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 7.



(a) Competitive Equilibrium (b) Wage Floor

FIGURE 10. Equilibrium wages and employment under different market structures

Tables

TABLE 1. Covariate Balance

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Treatments	Wage Cut Private	Wage Cut Employer	Wage Cut Public	Prevailing All	Joint Sig F-Stat	Obs
INDIVIDUAL-LEVEL						
Age	44.337	2.890	1.893	0.881	0.180	442
	(12.639)	(2.032)	(1.911)	(1.806)		
		[0.157]	[0.323]	[0.626]		
Caste: Scheduled Tribe	0.333	-0.0179	-0.106	-0.0259	0.842	444
	(0.474)	(0.0827)	(0.0671)	(0.0755)		
		[0.829]	[0.116]	[0.731]		
Casual Laborer	0.944	0.0417	0.0604	0.0486	0.177	446
	(0.23)	(0.0346)	(0.0269)	(0.0267)		
		[0.229]	[0.0260]	[0.0700]		
Casual Laborer - Agriculture	0.844	-0.0691	-0.0398	-0.00587	0.840	446
	(0.364)	(0.0662)	(0.0620)	(0.0530)		
		[0.298]	[0.522]	[0.912]		
Casual Laborer - Non-Agriculture	0.398	0.225	0.161	0.210	0.001	502
	(0.492)	(0.0734)	(0.0614)	(0.0660)		
		[0.00300]	[0.00900]	[0.00200]		
Not in Agricultural Labor Market	0.136	0.0672	0.0435	0.00699	0.375	502
	(0.344)	(0.0603)	(0.0577)	(0.0481)		
		[0.266]	[0.453]	[0.885]		
Doesn't Own Land	0.592	-0.0835	-0.0502	-0.0260	0.976	502
	(0.494)	(0.0851)	(0.0752)	(0.0694)		
		[0.328]	[0.505]	[0.708]		
Individual Employment	8.841	-0.582	0.994	0.0841	0.054	427
	(6.172)	(0.978)	(1.030)	(0.0699)		
		[0.553]	[0.336]	[0.231]		
Low Individual Unemployment	0.523	-0.0858	0.156	0.114	0.011	427
	(0.502)	(0.0851)	(0.0733)	(0.0871)		
		[0.314]	[0.0340]	[0.194]		
Prop Days NREGA Work in Recall	0	0.0104	-0.00158	0.0645	0.331	936
	(0)	(0.00910)	(0.00314)	(0.0845)		
		[0.257]	[0.614]	[0.446]		
Employer Experience	0.318	0.188	0.0346	-0.0716	0.248	426
	(0.468)	(0.0987)	(0.0999)	(0.0769)		
		[0.0590]	[0.730]	[0.353]		
Employer Experience - Past Year	0.239	0.162	0.0978	1.563	0.318	426
	(0.429)	(0.0947)	(0.0966)	(0.944)		
		[0.0900]	[0.313]	[0.100]		
Considers Employer Not Influential	0.593	-0.0790	-0.0875	0.00187	0.713	383
	(0.494)	(0.102)	(0.0867)	(0.00363)		
		[0.439]	[0.314]	[0.606]		
Number of Onlookers	.	.	4.099	3.651	.	189
	.	.	(0.472)	(0.472)		
		.	.	[0.458]		
Took Exit Survey	0.874	0.0178	0.0284	-0.00678	0.389	502
	(.334)	(0.0495)	(0.0458)	(0.0499)		
		[0.719]	[0.537]	[0.892]		
VILLAGE-LEVEL						
Number of Households in Labor Colony	45.639	-3.283	-3.880	1.366	0.195	172
	(13.135)	(3.603)	(3.266)	(3.057)		
		[0.364]	[0.237]	[0.656]		
Low Village Unemployment	0.577	-0.0966	-0.124	-0.123	0.766	180
	(0.501)	(0.144)	(0.132)	(0.126)		
		[0.504]	[0.346]	[0.327]		
Low Information Spread Village	0.541	0.0676	-0.0773	-0.0871	0.242	182
	(0.505)	(0.134)	(0.123)	(0.114)		
		[0.616]	[0.530]	[0.446]		
NREGA ever in village	0.914	-0.109	-0.0487	0.0457	0.132	180
	(0.284)	(0.0737)	(0.0678)	(0.0643)		
		[0.140]	[0.474]	[0.478]		
Number of Villages	37	34	40	72		
Number of Observations	103	88	108	203		

Notes: Table reports results from a regression of covariates on treatments, restricting to main experimental sample. Omitted category is households offered the wage cut (W-10%) in private. Means and standard deviations of each dep var for this omitted group are provided in Col (1). Cols (2)-(4) report coeffs for each treatment arm relative to the omitted group. Standard errors relative to the omitted group are clustered at the village level and are reported below each coeff in parentheses. P-values of tests of significance of coefficients in Cols (2)-(4) relative to this omitted category are reported below each coeff in brackets. P-value from Wald test of joint significance of all treatment arms (relative to the omitted category) reported in Col (5). Variation in sample sizes (Col (6)) comes from non-response in the exit survey and slightly different questions being asked across experimental rounds. Regs with full sample of 502 individuals impute 0 for non-response where necessary. Last two rows of the table report the number of villages and observations in each treatment arm. Regressions include yearxmonth and task fixed effects. In the first panel, coeffs are from regressions of individual-level characteristics from exit survey: respondent age, indicators for scheduled caste status, for participating in the casual labor market, for participating in the casual agricultural daily labor market, for not participating in the casual agricultural daily labor market, for not owning land, for below-median proportion of days in the prior 30 that participant wanted paid work but could not find it, for previous experience with the employer ever, and in the past year, rating of the employer as 1, 2 or 3 on a 4-point influence scale, number of days in the prior 30 in which the worker earned a positive wage, proportion of days in the past ten control group individuals reported NREGA work, number of onlookers during hiring, and an indicator for completing the exit survey. Observations weighted by the number of individuals in each village. In the second panel, coeffs are from regressions at the village-level: the number of households in the village, an indicator for below-median information flow as measured in the mop-up survey, an indicator for below-median village average of number of days the untreated holdout sample individuals would have preferred to work at the prevailing wage instead of their actual timeuse, and whether anyone in the untreated holdout sample reported NREGA work in the village ever. Number of onlookers variable is recorded only for public treatments. The coeff for the number of onlookers for hiring conducted at W in public is shown in Column (4). The test statistic in brackets for the number of onlookers in Col (4) is from a test of the equality of the number of observers in the two public treatments.

TABLE 2. Survey Attrition and Untreated Holdout Sample Composition

VARIABLES	(1) Has Exit Survey	(2) Num Untreated Holdout Surveys in Village
Wage cut: Public	0.0342	0.407
	(0.0514)	(0.37))
Wage cut: Employer	0.0124	0.104
	(0.0525)	(0.355)
Prevailing wage: Private	0.0383	-0.154
	(0.0525)	(0.397)
Preailing wage: Public	-0.0857	0.214
	(0.0662)	(0.433)
Prevailing wage: Employer	0.0696	0.834
	(0.0554)	(0.486)
Observations	502	502
Task and Year x Month FE	✓	✓
Sample	Main	Main
Depvar Mean (Wage cut: Private)	0.879	5.364

Notes: This table reports survey attrition and untreated holdout sample composition by treatment arm. Col. (1) reports the likelihood of successfully completing an exit survey with an member of the main experimental sample, by treatment. The outcome variable in Col. (2) is the number of untreated holdout sample surveys conducted in the experimental household's village. In all columns, the omitted category is the Wage cut: Private 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 3. Main Results

VARIABLES	(1) Worked	(2) Worked	(3) Worked
Wage cut: Public	-0.122	-0.136	-0.246
	(0.0564)	(0.0573)	(0.0644)
Wage cut: Employer	-0.0657	-0.0516	-0.0758
	(0.0611)	(0.0633)	(0.0788)
Prevailing wage: Private	0.0609	0.0791	0.0663
	(0.0703)	(0.0659)	(0.0819)
Preailing wage: Public	0.119	0.116	0.104
	(0.0808)	(0.0713)	(0.0856)
Prevailing wage: Employer	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	Ag. laborers
Depvar Mean (Wage cut: Private)	0.175	0.175	0.211
Test Wage cut: Private = Wage cut: Public	0.0316	0.0188	0.000181
Test Prevailing wage: Private = Prevailing wage: Public	0.460	0.589	0.658
Test Wage cut: Private - Public = Prev. wage: Private - Public	0.0629	0.0481	0.00858
Test Wage cut: Employer = Wage cut: Public	0.143	0.0865	0.0107
Test Wage cut: Private = Prevailing wage: Private	0.387	0.232	0.419
Test Prev. wage: Private = Employer = Public	0.609	0.816	0.904
Test Prevailing wage: Employer = Prevailing wage: Public	0.331	0.608	0.918
Test Prevailing wage: All = Wage cut: Private	0.196	0.118	0.228

Notes: This table presents the effect of each treatment on our main outcome of interest, the take-up rate for the job offer. In all specifications, the dependent variable is an indicator for whether the laborer accepted the job and worked for the employer. In all columns, the omitted category is the Wage cut: Private treatment. Cols. (1) and (2) include the full sample. Col. (3) restricts the sample to workers who answered the exit survey 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 4. Earnings Results

VARIABLES	(1) Wage work	(2) Wage earnings	(3) Wage work	(4) Wage earnings	(5) Wage work	(6) Wage earnings
Wage cut: Public	-0.161	-32.42	-0.0376	-6.794	-0.0646	-11.82
	(0.0510)	(11.13)	(0.0278)	(7.019)	(0.0249)	(6.942)
Prevailing wage (pooled)	0.0937	27.97	0.0170	3.747	0.0230	6.690
	(0.0515)	(13.07)	(0.0247)	(6.167)	(0.0252)	(6.399)
	[0.0706]	[0.0338]	[0.491]	[0.544]	[0.363]	[0.297]
Observations	428	428	1,303	1,303	1,731	1,731
Period	Work day	Work day	Ex work day	Ex work day	Full Week Weighted	Full Week Weighted
Sample	Endline recall	Endline recall	Endline recall	Endline recall	Endline recall	Endline recall
Task and Year x Month FE	✓	✓	✓	✓	✓	✓
Depvar Mean (Wage cut: Private)	0.222	45.49	0.0781	17.96	0.110	24.09
Test Wage cut: Private = Wage cut: Public	0.00190	0.00405	0.177	0.334	0.0102	0.0903

Notes: This table presents the effects of our job offers on individual earnings, derived from the employment recall grid performed in the exit survey. Each observation represents a day of recall. In Cols. (1), (3), and (5), the dependent variable is an indicator for whether the respondent worked that day for a wage. In Cols. (2), (4), and (6), the dependent variable is the total wage (cash + in kind) earned on that day in agricultural work. Cols (1)-(2) only consider responses for the day on which work was completed for the experiment in the village. Cols (3)-(4) consider the day before the work day and up to five days following the day of work, excluding the day of work. Cols.(5)-(6) include the day before work occurred in the village, the day work occurred in the village, and up to five days after the day of work. Non-workday observations are weighted to account for missing grid days in the worker exit survey (due only to the timing of the survey). Variation across respondents comes from the timing of when the exit surveys were conducted across households and villages. 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. Heterogeneous Treatment Effects: Individual Unemployment History

VARIABLES	(1) Worked	(2) Worked	(3) Worked	(4) Worked	(5) Worked	(6) Worked
Wage cut: Public	-0.196	-0.372	-0.213	-0.287	-0.298	-0.436
	(0.0664)	(0.0792)	(0.0742)	(0.0693)	(0.0880)	(0.0839)
	[0.00354]	[5.24e-06]	[0.00463]	[5.36e-05]	[0.000877]	[5.84e-07]
Prevailing wage (pooled)	0.0712	0.0192	0.133	0.124	0.0735	0.0313
	(0.0645)	(0.0773)	(0.0693)	(0.0798)	(0.0761)	(0.0889)
	[0.271]	[0.804]	[0.0575]	[0.123]	[0.336]	[0.725]
Low Village Unemployment	-0.166	-0.231			-0.192	-0.238
	(0.0699)	(0.0875)			(0.0733)	(0.0850)
	[0.0187]	[0.00895]			[0.00950]	[0.00576]
Wage cut: Public x Low Village Unemployment	0.160	0.355			0.200	0.358
	(0.0909)	(0.116)			(0.0989)	(0.114)
	[0.0808]	[0.00252]			[0.0449]	[0.00201]
Prevailing wage (pooled) x Low Village Unemployment	0.0780	0.205			0.134	0.198
	(0.0948)	(0.124)			(0.104)	(0.125)
	[0.412]	[0.101]			[0.201]	[0.114]
Low Individual Unemployment			-0.0901	-0.0988	-0.0822	-0.0892
			(0.0500)	(0.0605)	(0.0484)	(0.0557)
			[0.0734]	[0.105]	[0.0912]	[0.111]
Wage cut: Public x Low Individual Unemployment			0.132	0.154	0.119	0.140
			(0.0711)	(0.0685)	(0.0706)	(0.0687)
			[0.0648]	[0.0262]	[0.0927]	[0.0426]
Prevailing wage (pooled) x Low Individual Unemployment			-0.00102	0.0310	-0.0144	0.0193
			(0.0752)	(0.0877)	(0.0753)	(0.0846)
			[0.989]	[0.724]	[0.849]	[0.819]
Observations	493	363	427	350	427	350
Sample	Main	Ag. Lab.	Main	Ag. laborers	Main	Ag. laborers
Task and Year x Month FE	✓	✓	✓	✓	✓	✓
Depvar Mean (Wage cut: Private, High unempl.)	0.333	0.393	0.262	0.282	0.262	0.282
Take-up Wage cut: Public, High unempl.	0.0611	0	0.0882	0.0370		
Take-up Wage cut: Public, Low unempl.	0.0308	0.0444	0.0500	0.0217		
Test Wage cut: Public, High - Low unempl.	0.928	0.114	0.474	0.165		

Notes: This table presents heterogeneous treatment effects by individual and village unemployment. Low village unemployment (Cols. (1), (2), (5), and (6)) is measured as below-median village average of days in the past 10 that the untreated holdout sample group reports preferring working for a prevailing wage to their timeuse on that day, aggregated to the village-level from untreated holdout sample surveys. Low individual unemployment (Cols. (3)-(6)) is defined as below-median days in the past 30 that the worker reports wanting work but is unable to find any, as measured in the worker exit survey. Cols. (2), (4), and (6) restrict the sample to workers who indicated in the exit survey that they engage in agricultural labor as a primary or secondary occupation. In all columns, the omitted category is the Wage cut: Private treatment at $W - 10\%$ for the workers with high (individual or village) unemployment. 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 6. Heterogeneous Treatment Effects: Village Information Spread

VARIABLES	(1) Worked	(2) Worked	(3) Worked
Wage cut: Public	-0.200	-0.186	-0.308
	(0.0675)	(0.0646)	(0.0745)
	[0.00344]	[0.00442]	[5.50e-05]
Prevailing wage (pooled)	0.0794	0.0564	0.0467
	(0.0717)	(0.0567)	(0.0717)
	[0.269]	[0.321]	[0.515]
Wage cut: Public x Low info spread village	0.170	0.150	0.214
	(0.0932)	(0.0921)	(0.114)
	[0.0701]	[0.105]	[0.0614]
Prevailing wage (pooled) x Low info spread village	0.0521	0.115	0.146
	(0.0913)	(0.0844)	(0.106)
	[0.569]	[0.175]	[0.171]
Low info spread village	-0.0732	-0.0380	-0.0263
	(0.0667)	(0.0621)	(0.0796)
	[0.274]	[0.541]	[0.742]
Observations	499	499	361
Task and Year x Month FE	✓	✓	✓
Low info definition	Wage info	Norm violation	Norm violation
Sample	Main	Main	Ag. laborers
Depvar Mean (Omitted)	0.204	0.200	0.214

Notes: This table presents heterogeneous treatment effects by village-level diffusiveness, as measured in the mop-up survey. In Col. (1), the heterogeneous variable of interest is an indicator for below-median knowledge of the wages of others. In Cols. (2)-(3), 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. Col. (3) restricts the sample to only agricultural laborers. In this table, we pool Wage cut: Private and Wage cut: Employer. We also pool all of the Prevailing wage treatments together. In all columns, the omitted category is the Wage cut: Private pooled treatment for high info spread villages 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 7. 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: This table presents results from our costly punishment lab game exercise from N=131 participants (i.e. agricultural laborers) in 31 villages (villages are different from those in the main experimental sample). 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.

TABLE 8. Wage Rigidity: Correlation with Social Cohesion

		Proxy for Low Worker Cohesion	
		Wage Labor: Caste Herfindahl (Below Median)	Agri Labor Force: Caste Herfindahl (Below Median)
	(1)	(2)	(3)
Panel A - Dependent variable: Log Agricultural Wage			
Positive shock last year	0.0532	0.102	0.0971
	(0.022)	(0.042)	(0.033)
Positive shock last year		-0.0826	-0.0899
x Low worker cohesion		(0.050)	(0.038)
Positive shock this year	0.0633	0.0800	0.0751
	(0.018)	(0.038)	(0.039)
Positive shock this year		-0.0242	-0.0181
x Low worker cohesion		(0.042)	(0.043)
Observations (worker-days)	59243	59243	59243
Panel B - Dependent variable: Agricultural Employment			
Positive shock last year	-0.135	-0.234	-0.172
	(0.055)	(0.078)	(0.080)
Positive shock last year		0.189	0.0716
x Low worker cohesion		(0.088)	(0.107)
Positive shock this year	0.157	0.133	0.131
	(0.062)	(0.083)	(0.091)
Positive shock this year		0.0394	0.0469
x Low worker cohesion		(0.114)	(0.123)
Observations (workers)	632324	623861	631909

Notes: This table presents the effect of current and lagged productivity shocks on wages and employment (testing the ability of wages to fall after a positive shock has dissipated, a test for wage rigidity) and examines the heterogeneity of the effect by two measures of worker cohesion. The analysis uses National Sample Survey data (1986-2007). Low worker cohesion is defined as a) a below-median Herfindahl index of caste for all workers who engage in daily-wage labor within the village, indicating higher caste heterogeneity (Col. (1)), and b) a below-median Herfindahl index of caste for all workers who report agriculture as their primary or secondary occupation, indicating higher caste heterogeneity within the agricultural work force (Col. (2)). The dependent variable in Panel A is the log of the daily agricultural wage, and in Panel B is the number of days of agricultural employment (in wage labor or on one's own farm) in the past week. Positive shock is defined as rainfall above the 80th percentile of the district's usual rain distribution. Positive shock this year is a dummy for a positive shock in the current year. Positive shock last year is a dummy that equals one if the district had a positive shock last year and did not have a positive shock in the current year. The interaction terms are the binary variables defined at the top of each column. Regressions include year and district fixed effects. Standard errors clustered by region-year.

TABLE 9. Surplus Estimation

	(1) **All Workers**	(2) **Agricultural Workers**
Labor Supply - W	0.29	0.32
Labor Supply - W-10%	0.17	0.21
Labor Demand - W (Breza et al. 2019)	0.20	0.30
Labor Supply Elasticity	3.89	3.41
Labor Demand Elasticity (Kaur 2018)	1	1
L*	0.21	0.30
W*	186.90	197.22
Percent Difference between Prevailing Wage W and W*	7.01%	1.41%
Percent Difference between Labor Supply at W and (L*,W*)	-6.55%	-1.39%
Worker Surplus, Competitive Equilibrium	4.08	8.51
Worker Surplus, Wage Floor	6.69	9.34
Increase in WS compared to Comp Eq.	2.61	0.83
Percent Increase in WS compared to Comp Eq.	63.81%	9.79%
Employer Surplus, Competitive Equillibrium	22.71	30.84
Deadweight Loss	0.10	0.0074

Notes: This table presents various outcomes of interest in our surplus calculations based on labor supply elasticity estimates from the main experiment. The outcomes of interest include labor supply and wage under the competitive equilibrium, deadweight loss under the wage floor equilibrium, and employer and worker surplus under both equilibria. Col. (2) restricts the sample to only agricultural laborers. The level of labor demand at the prevailing wage is based on survey evidence collected as part of a labor market rationing experiment by Breza et al. (2018), set in other villages in the same districts of Odisha in which this experiment was conducted. The labor demand elasticity measure we use in our baseline calculations is derived in Kaur (2018) using National Sample Survey data. The level of labor supply at various wage rates comes from take-up rates for one day of agricultural work in our main experiment. The prevailing wage, W, is taken to be Rs. 200, the modal prevailing wage among villages in our experiment.

TABLE 10. Surplus Estimation Sensitivity Analysis

Labor Supply in the Competitive Equilibrium, L*				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	0.24	0.26	0.28	0.30
1	0.23	0.24	0.26	0.28
2	0.22	0.23	0.24	0.26
4	0.21	0.22	0.23	0.24

Wage in the Competitive Equilibrium, W*				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	110.26	135.34	158.52	175.84
1	144.33	155.13	167.67	179.26
2	168.36	172.17	177.56	183.83
4	183.02	184.18	186.08	188.78

Total Surplus in Competitive Equilibrium				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	97.95	79.40	70.74	66.74
1	68.35	48.97	39.70	35.37
2	53.95	34.17	24.49	19.85
4	46.91	26.98	17.09	12.24

Deadweight Loss in Surplus Under Wage Floor Equilibrium, W				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	3.29	4.74	6.08	7.08
1	1.02	1.64	2.37	3.04
2	0.29	0.51	0.82	1.18
4	0.08	0.14	0.26	0.41

Percent Increase in Worker Surplus Under Wage Floor Equilibrium, W				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	43.89%	23.17%	7.76%	-1.94%
1	66.22%	43.89%	23.17%	7.76%
2	85.24%	66.22%	43.89%	23.17%
4	98.49%	85.24%	66.22%	43.89%

Notes: This table presents various outcomes of interest in our surplus calculations for a range of labor demand and supply elasticities. The outcomes of interest include labor supply and wage under the competitive equilibrium, deadweight loss under the wage floor equilibrium, and worker and employer surplus under both equilibria.

Appendix Figures

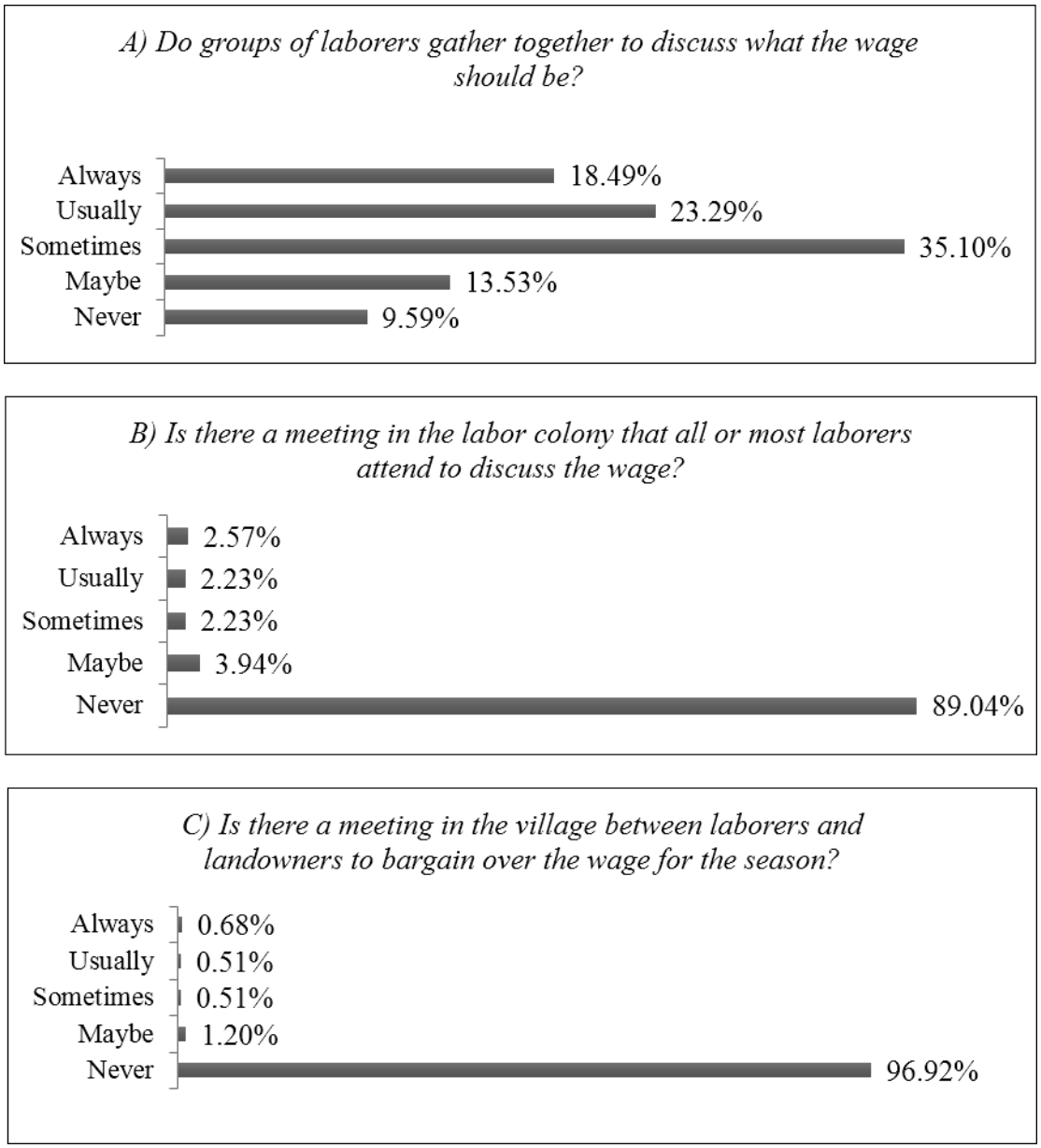


Figure A.1. Collective Action: Wage Setting in the Village

Notes: This figure shows responses to two survey questions about collective action in wage-setting within the village. Data are from the sample of untreated holdout sample households from experiment villages surveyed following the completion of the experiment. N = 584 male casual laborers in 183 villages.

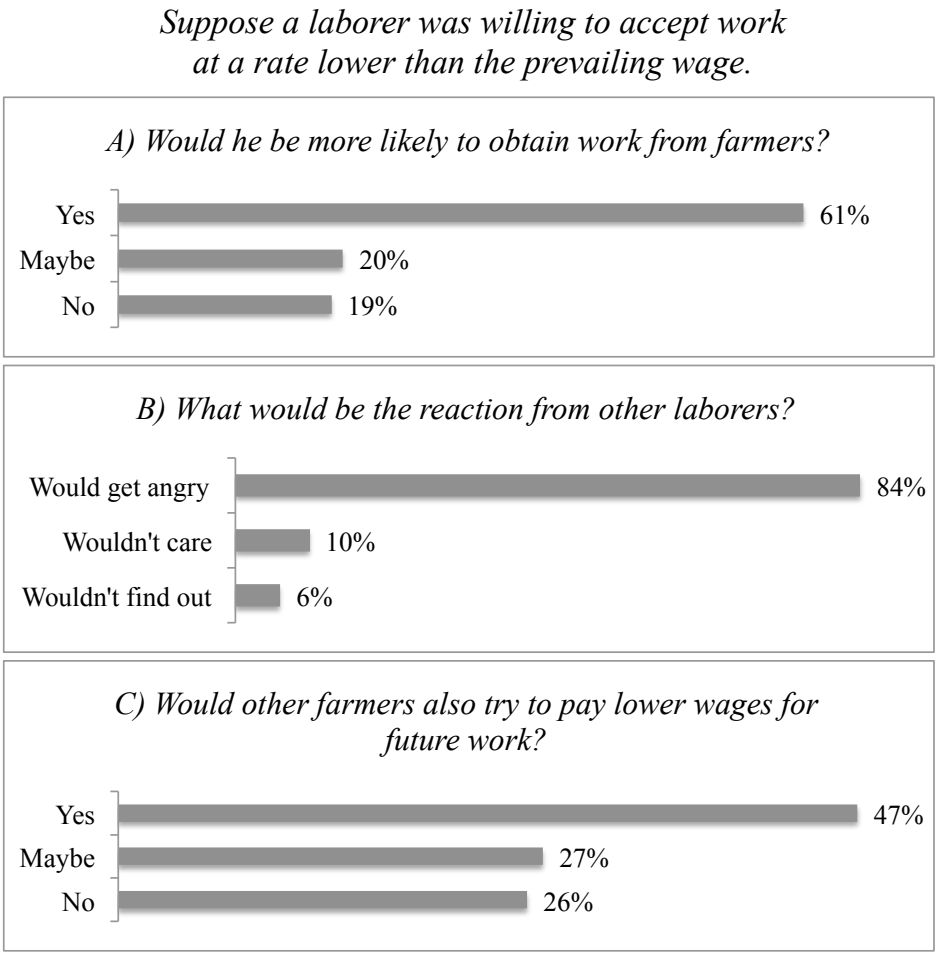
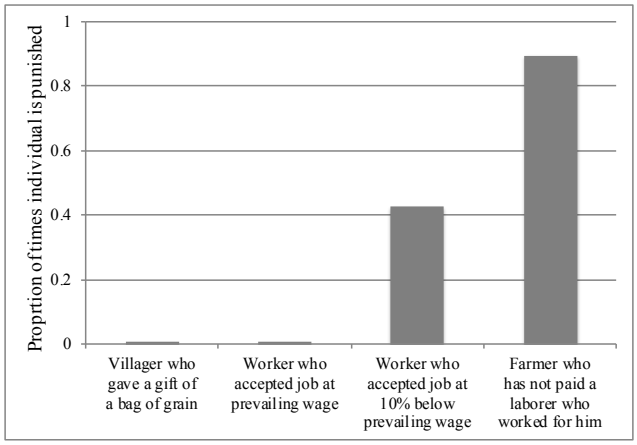
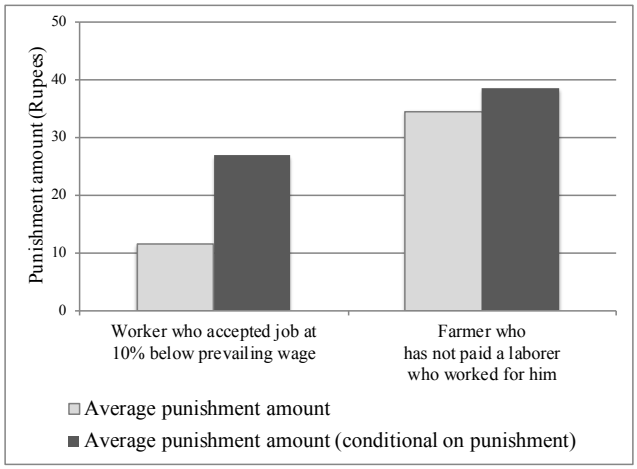


FIGURE A.2. Worker Beliefs: Impacts of Accepting Wage Cuts

Notes: This figure shows responses to three survey questions about employer and worker responses to a worker accepting a job at below the prevailing wage. Data from Kaur (2018). N = 196 male casual laborers in 34 villages across 6 districts in the states of Odisha and Madhya Pradesh.



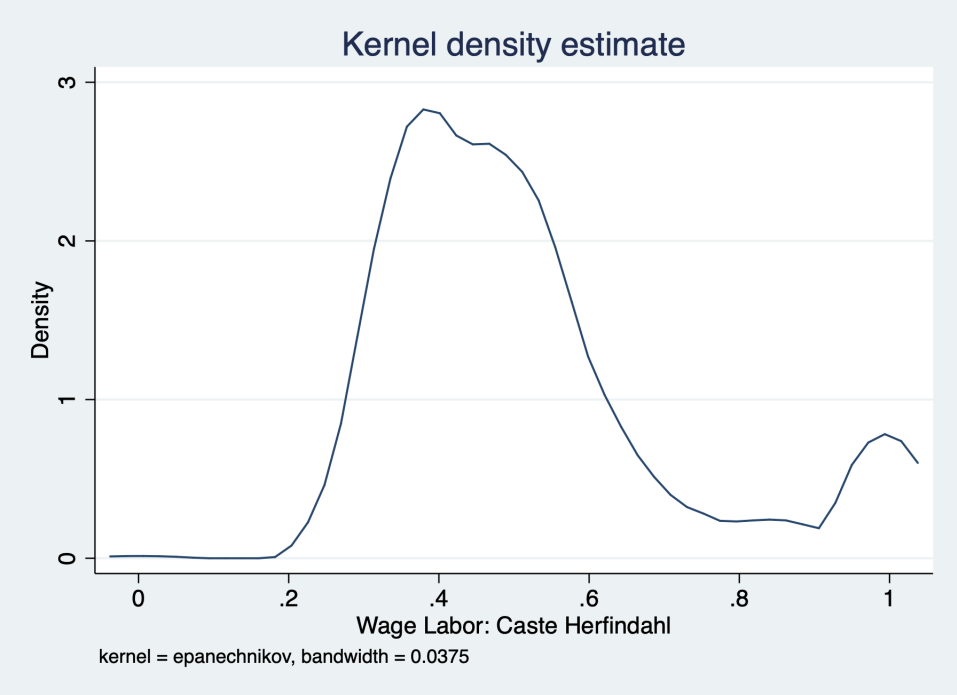
(a) Punishment across Scenarios



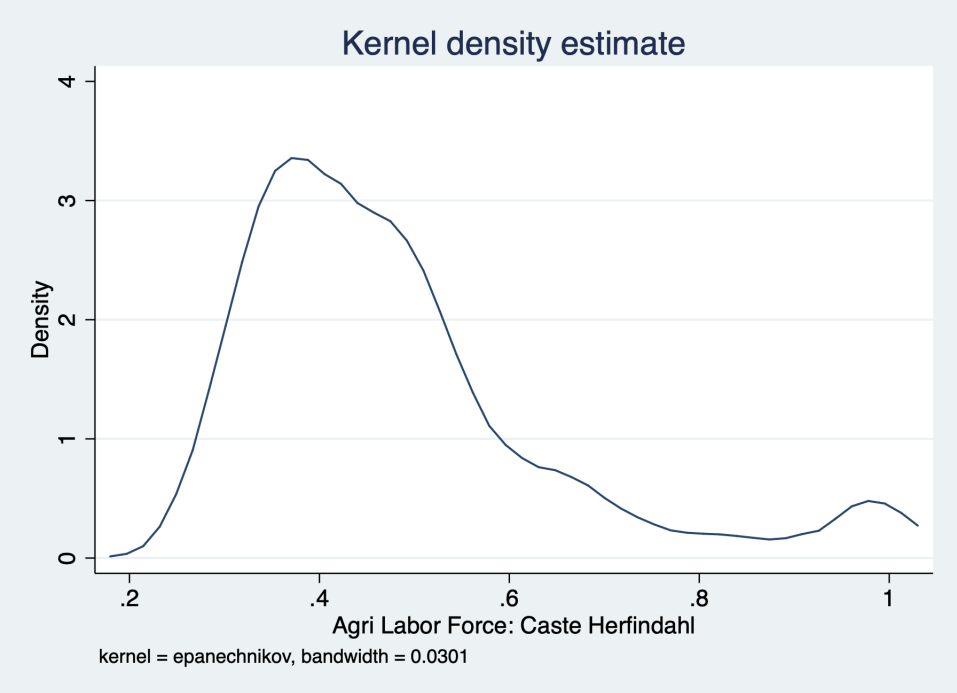
(b) Punishment Amounts

FIGURE A.3. Laboratory Games: All Scenarios

Notes: This figure shows punishment responses by N=131 lab game participants (players) to various scenarios about the behavior of the anonyous partner. 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, out of a maximum possible of Rs. 100) deducted from the partner's payoff under scenarios (iii) and (iv), unconditional and conditional on punishment.



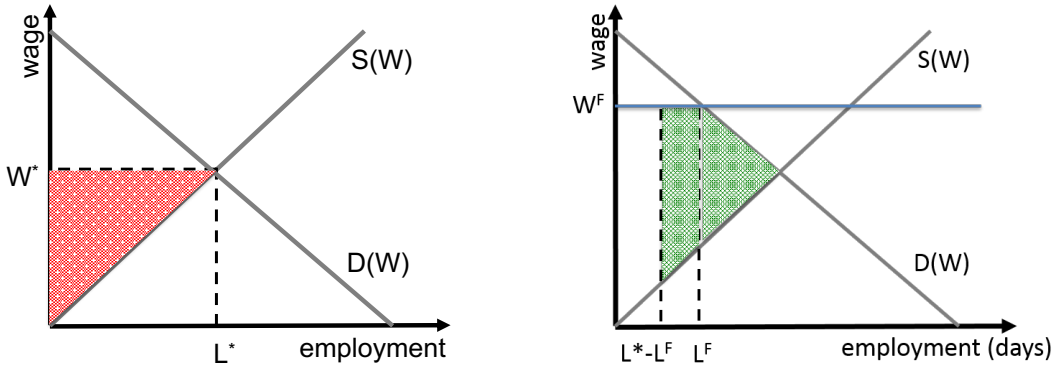
(a) Wage Labor: Caste Herfindahl



(b) Agri Wage Labor: Caste Herfindahl

FIGURE A.4. Kernel Density of Caste Herfindahl Measures

Notes: This figure uses National Sample Survey data (1986-2007).This figure shows the distribution of the Herfindahl index of caste across districts for a) all workers who engage in daily-wage labor within the village, and b) for all workers who report agriculture as their primary or secondary occupation.



(a) Competitive Equilibrium (b) Wage Floor, Inefficient Rationing

FIGURE A.5. Equilibrium wages and employment under the competitive equilibrium and under inefficient rationing.

Appendix Tables

Table A.1. Main Results With Randomization Inference

VARIABLES	(1) Worked	(2) Worked	(3) Worked
Wage cut: Public	-0.122	-0.136	-0.246
	[0.035]	[0.032]	[0]
Wage cut: Employer	-0.0657	-0.0516	-0.0758
	[0.346]	[0.448]	[0.349]
Prevailing wage: Private	0.0609	0.0791	0.0663
	[0.414]	[0.334]	[0.413]
Preailing wage: Public	0.119	0.116	0.104
	[0.157]	[0.191]	[0.272]
Prevailing wage: Employer	0.0364	0.0690	0.0935
	[0.687]	[0.538]	[0.309]
Observations	502	502	363
Task and Year x Month FE		✓	✓
Sample	Main	Main	Ag. laborers
Depvar Mean (Wage cut: Private)	0.175	0.175	0.211
Test Prevailing wage: Private = Prevailing wage: Public	0.505	0.637	0.621
Test Wage cut: Employer = Wage cut: Public	0.124	0.107	0.012
Test Prevailing wage: Employer = Prevailing wage: Public	0.439	0.646	0.930
Test Prevailing wage: All = Wage cut: Private	0.195	0.137	0.244

Notes: This table presents the effect of each treatment on our main outcome of interest, the take-up rate for the job offer. In all specifications, the dependent variable is an indicator for whether the laborer accepted the job and worked for the employer. In all columns, the omitted category is the Wage cut: Private treatment. Cols. (1) and (2) include the full sample. Col. (3) restricts the sample to workers who answered the exit survey 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. Randomization inference p-values are reported in square brackets below each coefficient, and at the bottom of the table for relevant tests. Inference for the coefficients was carried out with 1000 permutations of treatments (at the village level), permuting over the treatment of interest and the omitted treatment, the Wage Cut: Private category.

Table A.2. Main Results: Sample Robustness

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

Notes: This table presents results from our primary specification, restricted to various samples as a robustness check. In Col. (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 Col. (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 Wage cut: Private 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.3. Heterogeneous Treatment Effects: Experience with the Hiring Employer

VARIABLES	(1) Worked	(2) Worked
Wage cut: Public	-0.202	-0.239
	(0.0708)	(0.0778)
Wage cut: Employer	-0.110	-0.103
	(0.0790)	(0.0953)
Prevailing wage (pooled)	0.0232	0.0376
	(0.0794)	(0.0880)
Wage cut: Public x Employer Experience	0.0636	-0.0184
	(0.111)	(0.118)
Wage cut: Employer x Employer Experience	0.0494	0.00161
	(0.119)	(0.143)
Prevailing wage (pooled) x Employer Experience	0.140	0.107
	(0.123)	(0.135)
High Employer Experience	-0.0206	0.0193
	(0.0966)	(0.108)
Observations	426	350
Task and Year x Month FE	✓	✓
Sample	Main	Ag. laborers
Test Wage cut: Public + Public x Experience = 0	0.163	0.0105
Test Wage cut: Employer + Employer x Experience = 0	0.557	0.395
Test Wage cut: Pub. + Pub. x Exp. = Wage cut: Empl. + Empl. x Exp.	0.316	0.0725
Depvar Mean (Omitted)	0.183	0.188

Notes: This table presents heterogeneous treatment effects by previous work experience with the participating employer, as measured in the worker exit survey. The specifications use an indicator for the worker having ever worked for the hiring employer in the past. Col.(2) restricts the sample to workers who indicated in the exit survey that they engage in agricultural labor as a primary or secondary occupation. In all columns, the omitted category is the Wage cut: Private pooled treatment for the low employer experience group 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 A.4. Wages, amenities, and worker quality and selection on the day of work

VARIABLES	(1) Received vs. Offered Cash Wage (%)	(2) Length of work (mins)	(3) Number of meals included	(4) Hired before	(5) Work day rating	(6) Work day rating
Wage Cut	0.0243	-2.863	-0.280	-0.207	0.0206	0.0469
	(0.0259)	(16.91)	(0.291)	(0.195)	(0.297)	(0.402)
	70	74	74	77	74	61
Constant	0	750	-0	1	2	2
		(5.76e-06)				
Depvar Mean (Omitted)	0	313.3	0.690	0.652	1.178	1.178

Notes: This table presents statistics on job amenities and worker selection and quality on the day of work. The sample is restricted to all main sample individuals who came to the job on the day of work. The omitted category is Prevailing wage (Pooled). Cols (1)-(5) consider the full sample of workers, while in Col (6) we only consider the *Private* $w - 10\%$ treatment compared against all of the w treatments. The outcome variable in Col (1) is the percent difference in total cash wage received (including any side transfers) versus the offered wage. Total cash wage received, length of the work day, and meals received from them employer are measured in the exit survey. Workers' prior experience with employer is measured by an indicator for having been hired before by the employer. Worker quality is reported for the day of work on a rating scale of 1-4 by the employer. All specifications include month x year and task fixed effects. Standard errors are clustered at the village level and reported in parentheses. N=74.

Table A.5. Exit Survey Reports of Village Prevailing Wage

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

Notes: This table presents statistics on the accuracy of the informant's report of the prevailing wage, relative to respondents' reports in the exit survey. Sample restricted to all experimental subjects who responded to our exit survey. In Col. (1), the dependent variable is an indicator for whether the respondent reports the same prevailing wage in the exit survey as the village informant reported prior to the intervention. In Col. (2), the dependent variable is the difference between the respondent's view of the prevailing wage and the informant's report. In Col. (3), the dependent variable is the absolute value of this difference. In all columns, the omitted category is the Wage cut: Private treatment. Standard errors are clustered at the village level and are reported in parentheses. P-values are reported in brackets.

TABLE A.6. Robustness of Wage Rigidity Results to Definition of Low Worker Cohesion

	Proxy for Low Worker Cohesion									
	Wage Labor: Caste Herfindahl					*Agri Labor Force: Caste Herfindahl*				
	Below median	Below median	Linear	Linear in ranks	Bottom tercile	Below median	Below median	Linear	Linear in ranks	Bottom tercile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A - Dependent variable: Log Agricultural Wage										
Positive shock last year	0.102	0.110	0.0580	0.0601	0.0480	0.0971	0.106	0.0662	0.0565	0.0583
	(0.042)	(0.042)	(0.023)	(0.023)	(0.037)	(0.033)	(0.033)	(0.027)	(0.022)	(0.032)
Positive shock last year	-0.0826	-0.0821	-0.208	-0.112	-0.0319	-0.0899	-0.0898	-0.183	-0.110	-0.0443
x Low worker cohesion	(0.050)	(0.050)	(0.141)	(0.071)	(0.047)	(0.038)	(0.038)	(0.145)	(0.066)	(0.042)
Positive shock this year	0.0800	0.0910	0.0569	0.0639	0.0540	0.0751	0.0870	0.0531	0.0653	0.0503
	(0.038)	(0.038)	(0.021)	(0.024)	(0.063)	(0.039)	(0.039)	(0.028)	(0.022)	(0.054)
Positive shock this year	-0.0242	-0.0273	0.138	-0.00253	0.0195	-0.0181	-0.0221	0.107	-0.0155	-0.000865
x Low worker cohesion	(0.042)	(0.041)	(0.169)	(0.075)	(0.068)	(0.043)	(0.043)	(0.181)	(0.072)	(0.056)
Observations (worker-days)	59243	59243	59243	59243	59243	59243	59243	59243	59243	59243
Panel B - Dependent variable: Agricultural Employment										
Positive shock last year	-0.234	-0.269	-0.133	-0.142	-0.183	-0.172	-0.206	-0.160	-0.143	-0.223
	(0.078)	(0.079)	(0.055)	(0.056)	(0.114)	(0.080)	(0.083)	(0.063)	(0.055)	(0.087)
Positive shock last year	0.189	0.193	0.429	0.387	0.259	0.0716	0.0766	0.358	0.311	0.180
x Low worker cohesion	(0.088)	(0.089)	(0.331)	(0.183)	(0.135)	(0.107)	(0.109)	(0.448)	(0.201)	(0.122)
Positive shock this year	0.133	0.0860	0.158	0.147	0.235	0.131	0.0853	0.127	0.145	0.124
	(0.083)	(0.089)	(0.062)	(0.062)	(0.129)	(0.091)	(0.098)	(0.075)	(0.061)	(0.122)
Positive shock this year	0.0394	0.0420	-0.142	0.135	0.0675	0.0469	0.0503	0.394	0.246	0.137
x Low worker cohesion	(0.114)	(0.115)	(0.496)	(0.231)	(0.162)	(0.123)	(0.123)	(0.600)	(0.243)	(0.162)
Observations (workers)	623861	623861	623861	623861	623861	631909	631909	631909	631909	631909

Notes: National Sample Survey data (1986-2007). Positive shock is an indicator for rainfall above the 80th percentile of the district's usual distribution. All regressions contain year and district fixed effects. Standard errors clustered by region-year.

TABLE A.7. Surplus Estimation Sensitivity Under Inefficient Rationing

Labor Supply in the Competitive Equilibrium, L* (Identical to Efficient Rationing)				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	0.24	0.26	0.28	0.30
1	0.23	0.24	0.26	0.28
2	0.22	0.23	0.24	0.26
4	0.21	0.22	0.23	0.24

Wage in the Competitive Equilibrium, W* (Identical to Efficient Rationing)				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	110.26	135.34	158.52	175.84
1	144.33	155.13	167.67	179.26
2	168.36	172.17	177.56	183.83
4	183.02	184.18	186.08	188.78

Total Surplus in Competitive Equilibrium (Identical to Efficient Rationing)				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	97.95	79.40	70.74	66.74
1	68.35	48.97	39.70	35.37
2	53.95	34.17	24.49	19.85
4	46.91	26.98	17.09	12.24

Deadweight Loss in Surplus Under Wage Floor Equilibrium, W, and Inefficient Rationing				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	10.17	14.66	18.81	21.91
1	4.55	7.33	10.56	13.55
2	2.08	3.66	5.91	8.51
4	0.98	1.83	3.22	5.20

Percent Increase in Worker Surplus Under Wage Floor Equilibrium, W				
LS Elasticity (Rows)/LD Elasticity(Columns)	**0.5**	**1**	**2**	**4**
0.5	25.77%	0.82%	-17.33%	-28.54%
1	44.77%	13.96%	-13.75%	-33.69%
2	60.93%	27.84%	-9.67%	-42.90%
4	72.18%	39.49%	-6.01%	-56.92%

Notes: This table presents various outcomes of interest in our surplus calculations for a range of labor demand and supply elasticities. The outcomes of interest include labor supply and wage under the competitive equilibrium, deadweight loss under the wage floor equilibrium, and worker and employer surplus under both equilibria. Here, we assume "inefficient rationing" of jobs in the following sense: workers with the highest potential surplus from the jobs are rationed out of the market in the wage floor equilibrium.

A Protocol Appendix

A.1 Main Experiment Protocols

A.1.1 Village Setup Villages in our study region are typically split into two or more neighborhoods, or 'colonies'. The landowners, who tend to be of upper castes, live in

the 'employer colonies' and have net demand for hired labor. The casual daily wage laborers tend to live in the 'labor colonies' and typically belong to the Scheduled Castes and Scheduled Tribes (SC/ST).

A.1.2 Village Selection We employ local enumerators from Odisha, India, whose native language is Odiya, which is spoken throughout the study region. Our enumerators identify an initial sample of study villages by visiting every employer colony in a given block (administrative region), and speaking with any sampling of employers whom they encounter in the colony.

Enumerators then visit each labor colony, and ask one individual in that labor colony about the number of households in the colony. Enumerators minimize the amount of time spent in the labor colony to prevent information diffusion and preconceptions about our work in the village. This process continues until our enumerators visit every village in the block.

The information from the scouting process is used in village selection. The experiment is conducted only in villages with at least 30 households that have at least one daily-wage agricultural worker, with the potential for some agricultural work to be done at the time of the experiment. We exclude villages where there is high incidence of external work through brick kilns, stone quarries, and other similar industries, to restrict our sample to primarily agricultural villages. We also exclude villages in which the prevailing wage has changed within the past two weeks to abstract from the time taken to coordinate around a new village prevailing wage.

We conduct the main experiment in a total of 183 villages.

A.1.3 Employer Selection and Compensation Once a village meets the criteria for our study, our enumerators return to the employer colony of the village to recruit the participating employer.

The participating employer is chosen to satisfy four criteria: he should be a midsized landowner (between 2 and 5 acres of land), he should not have undue influence in the village (the three power proxies used are detailed in the Employer Recruitment Script), he should typically pay the regular wage rate in the village, and he should have enough work for three people for a full day within the next two weeks. The Employer Recruitment Script (see A.3.1) details this protocol. The selected participating employer is informed about the nature and purpose of the study and the possible treatment arms, but does not know to which treatment arm his village will be assigned.

The participating employers are involved in providing work, accompanying enumerators to identify treatment households and to make job offers, and overseeing work (see further details in Section A.1.9). They pay up to Rs. 50 (USD 0.71) of the wage of each participant, and the research team provides the rest of the wage payment[34]. They are also given a bonus of Rs. 500 (USD 7.07) for adhering to the timeline of the experiment and accompanying our enumerators during the various phases of the project.
The script for the enumerators' interaction with the participating employer' is provided in Section A.3.1.

A.1.4 Worker Enumeration The next step is to obtain the sampling frame for the main hiring experiment. Two enumerators visit the labor colony in which the experiment will happen a few days before hiring for the employer's work is scheduled. They identify a "worker census participant", who must be a daily-wage agricultural laborer who resides in that village and is able to list the households in the village. This individual works with our enumerators in private to create a census list of households in the labor colony. Care is taken to include every household in the village, and to ask the worker census participant to identify the head of household. The script for the enumerators' interaction with the worker census participant is provided in Section A.3.2.

A.1.5 Treatment Randomization The unit of randomization for the study is the village. At this stage, each village is assigned to a privacy level ("public", "private", or "employer only") and a wage rate (the prevailing wage or 10% below the prevailing wage). To do this, we use a computer program that carries out *ex-ante* block randomization of the sequence of treatments, stratified by block (administrative unit) and village size. At the time that we are about to make job offers in a particular village, that village is assigned to the next treatment on the list for its block and size category.

A.1.6 Household Randomization Within the village, a minimum of six and maximum of eight households with daily-wage agricultural workers (depending on village size) are randomly selected (with coded names) from the sampling frame provided by the "worker census participant." The household ID numbers are randomly selected

[34]Each participant who works for this employer is paid the entire wage payment in a single lump sum in an envelope at the end of the day by our enumerators, in every treatment arm.

using a statistical program. Selected households form the group of main study participants. Participants in each study household within the village are offered the job under the exact same treatment conditions (determined at the village-level).

A.1.7 Hiring Procedure Hiring takes place in the morning, two days before the scheduled day of work (this is the modal gap between the day of hiring and the day of work in our scouting survey). Two enumerators, together with the participating employer, arrive at the village at 7am, a time when many laborers have not yet left for their day's work, and are therefore still present at home.

Enumerators, together with the participating employer, approach the first household on the randomized list, ask to speak with the head of the household, deliver the hiring script (see Section A.3.3), and record the response.

The enumerators and the participating employer proceed from one house to the next until they have delivered the hiring script to at most three different individuals from different households, or have covered all of the 6-8 randomly-selected households, whichever threshold is reached first.

If the head of the household is not present, enumerators ask to speak with any adult male in the household (ages 18-65) who engages in daily-wage agricultural work. That adult, if he provides verbal consent, is given the hiring script, and becomes a main study participant. In total, we offer the job to one individual in each of 502 households.

This hiring is done in different ways depending on the treatment arm, as described in the next section.

A.1.8 Treatment Implementation The experiment varies whether the wage is made at or below the prevailing village wage and the observability of the offer. The experimental design includes six variants of the hiring procedures. All job offers within the same village are made under the same treatment conditions. In this section, we describe the specific elements of hiring procedures that change across treatment arms, and highlight some important ones that are identical across villages.

Common Across Treatments: The participating employer and our enumerators approach the head of the household (or another adult in the household who engages in daily-wage agricultural labor, if the head of the household is not available or does not participate in agricultural work). The employer conveys the nature and details of the task and the date of work. He then hands the conversation over to the enumerators

for a brief survey, during which they convey the wage level for the task.

Public: Employment offers are made on the street in front of the worker's home. The participating employer and any other passers-by can hear the terms of the wage offer and the participant's response. In public-treatment villages, one of our enumerators unobstrusively records the number of onlookers.

Employer Only: Employment offers are made in private, inside the worker's home. However, the participating employer remains in the worker's home throughout the disclosure of the wage, and can hear the participant's response to the job offer.

Private: Employment offers are made in private, inside the worker's home. The participating employer is led out of the main study participant's home before the wage rate is disclosed. The research team never informs the employer of the wage, and the participant knows this. Wage payments are disbursed by the enumerators, so there is no need for the participating employer to ever know the wage.
In our fully-private and employer treatments, we take additional care to ensure that there is a low likelihood that the job offer and the worker's response can be overheard by anyone else in the village. There are always two enumerators present during the hiring process. One enumerator will undertake the exchange with the participant, while the other's role is to ensure no one approaches within hearing distance of the exchange. Since the exchange happens within the participant's house, this second enumerator stands outside the house and engages any approaching people in casual conversation until the exchange within the home is over.

Once the job offers are made, the participant is asked whether or not he would like to accept the job, and the enumerator makes a note of his response. The script for the job offers is provided in Section A.3.3.

A.1.9 Workday Logistics Two days after hiring, our enumerators reach the village in the morning. By this time, the participating employer has visited the home of each participant who indicated at the time of hiring that he would like to work. These workers may opt out of working at this time[35]. The participants who choose to work, together with the participating employer, proceed to the designated field for work.

[35]Signing up to work and then deciding not to go to work the next day is extremely common in the casual agricultural markets in the study area.

Our enumerators conduct a spot check that work has begun and make a note of which participants have chosen to work.
Work is completed according to the general work hours in the village (e.g. 7.30am-1:30pm). The participating employer provides whatever in-kind benefits he usually does (e.g. a snack, tea, etc.).
At the end of the workday, enumerators proceed to the designated field. The employer gives the enumerators his portion of the wage payment for each worker - Rs. 50. Each participant is then given an envelope containing the wage for the day by the enumerators. The participating employer does not hand cash wages directly to the workers. This is common across treatments and vital for the confidentiality of the wage rate in the fully private treatment.

A.1.10 Endline Survey All our main study participants complete an endline survey in the 2-3 days following the day of work. This survey includes demographic information, together with information on their perception of the participating employer, a ten-day recall grid of employment, and questions about fairness norms. The participating employer also completes an endline survey within this timeframe, which includes demographic information, together with information on worker quality , a specific employer rating of worker effort on the day of work, and information about wages in kind provided to the participants.

A.1.11 Untreated Holdout Sample Selection We select five households who suit the selection criteria for the main treatment sample frame, but are not approached and offered the job. The five household ID numbers are randomly selected using a statistical program.
These untreated holdout households complete an endline survey which includes demographic information, together with information on their perception of the participating employer, a ten-day recall grid of employment, and questions about information flow and fairness norms.

A.1.12 Timeline for Each Village
Pre-experiment: Scouting and Employer Selection
Day 0: Worker enumeration
Day 1: Hiring
Day 3: Day of Work
Days 4-6: Endline surveys with main study participants, the participating employer, and the untreated holdout sample households.

A.2 Lab Game Protocols

A.2.1 Village Selection Similar to Main Experiment Protocols. In addition, during the scouting process, enumerators ensure there is a private location (such as a village meeting hall) within which they can set up private lab game stations in which participants can take part in the lab games without being overheard.

A.2.2 Employer Selection and Compensation Similar to Main Experiment Protocols.

A.2.3 Worker Enumeration Similar to Main Experiment Protocols.

A.2.4 Household Randomization Similar to the Main Experiment Protocols, except that randomization is used to select a minimum of six households to whom agricultural job offers will be made, and 12 other households who are recruited as lab game participants.

A.2.5 Treatment Randomization The costly punishment game presents each participant with two of the following four scenarios: a co-villager accepted a job at w or $w - 10\%$, or a villager from a distant village accepted a job at w or $w - 10\%$. The choice and order of the scenarios are randomized at the individual level using a statistical program.

A.2.6 Hiring Procedure In this section, we describe the recruitment of two types of individuals - the lab game participants, and their anonymous partners who receive real job offers.

Agricultural Job Offers: Job offers are made using precisely the same procedure as in the main experiment, with two exceptions.

First, we make job offers to at least six randomly-selected people in each village (to preserve confidentiality of partners' identity in the costly punishment game). Second, enumerators elicit a complete strategy response from each participant - that is, they would ask first if the worker is willling to accept the job at Rs. *w*, and if the worker says yes, then to ask if he would be willling to take the job at Rs. *w-10%*.

All job offers are made under the fully private protocols described in Section A.1.8. The complete script for recruiting for the agricultural job is provided in Section A.3.4.

Lab Game Participants: That same evening, from the remainder of the sampling frame, 12 lab game participants are selected using a statistical program. Enumerators visit their homes to recruit participants for the half-day lab games. The complete script for recruiting for the lab games is provided in Section A.3.5.

A.2.7 Lab Game Procedures Enumerators first conduct a series of ice-breaker questions, to diffuse knowledge about the precise goal of the lab games. Following this, they introduce the costly punishment game to the group of participants and fully role-play two examples - one about a person in the village who has stolen a bicycle, and another about a person who has gifted the participant a box of sweets. Neither example is related to social norms or prevailing wages in the village. Next, participants are each taken to a private area within the lab games location, with one enumerator. Then, the enumerator confirms their knowledge of the game and introduces two more examples (that the participant himself responds to) - one about a co-villager who gifts the participant a bag of grain when the participant really needs it, and another who has not paid wages to another villager whom he employed. Finally, enumerators introduce the two main lab game examples (with the treatments and treatment order randomized).

The complete script for the lab games is provided in Section A.3.6.

A.2.8 Workday Logistics Similar to Main Experiment Protocols.

A.2.9 Timeline for Each Village
Pre-experiment: Scouting and Employer Selection
Day 0: Worker enumeration
Day 1: Hiring For Agricultural Work and Hiring for Lab Games
Day 2: Lab Games
Day 3: Day of Agricultural Work

A.3 Scripts

A.3.1 Employer Recruitment Script

Hello. We are from an academic organization called JPAL/IFMR and we are conducting research on the role of social norms in labor markets. As part of this research, we would like to ask a few questions about your role as an employer. To ensure your privacy, any responses you give will be kept strictly confidential and will not be told to anyone else.

You may choose to end this conversation at any time, or you may choose not to answer any questions that you do not wish to. Can we proceed with some questions?

Confirm the following points with the employer in a colloquial conversational manner:
a. He and his close relatives are not currently holding leadership roles in the village's

panchayat and have not held the position in the recent past.
b. He and his close relatives are not currently holding leadership roles in the village's paani-panchayat (water distribution board) and have not held the position in the recent past.
c. He and his close relatives are not currently holding leadership roles as the village's ward member representative and have not held the position in the recent past.
d. How much land he owns

If any of the first three points is true for the employer or if he owns anything other than 2-5 acres of land, do not proceed with the remaining script. Please thank him for his time and end the survey.

We are doing a study to understand more about the social norms in labor markets in villages in the area. In particular, we are interested in how laborers' acceptance and rejection of job offers is different when offered jobs at different wages in public and in private. We would like to offer some male daily wage laborers in this village employment for a day in agricultural work on your land. We would just like to assess your willingness to participate in our study. These laborers who will be approached and offered the job are among the daily wage laborers in this village.

For our study, we would like to help you hire three daily-wage workers from your village to do whatever work is there on your land for a day in the next week. You will only pay Rs. _____(50) to each worker for the day of work and we will pay the remaining amount of their wage.

Their total wage will be very close to (or at) the prevailing wage. It could be slightly below or at the usual wage rate in the village. We might offer these jobs either in public, where others in the labor colony might be able to hear, or in private, where we will make every effort to ensure no one else in the labor colony overhears the job offer. You must also allow us to choose whom we hire (everyone that we hire will be from your village). We are doing this to achieve the objectives of our study.

In return, you must provide all customary wage-in-kind provisions for the laborers on the day of work (e.g. food, tea). You cannot provide anything more than the usual amounts and types of in-kind provisions.

We would like you (or your appointed assistants) to monitor the work as you regularly do. You must also keep the details of the study and of this arrangement to yourself for the time period that we are working with you.

So far, are you interested in this proposal?

If you are, then great. We will plan to be here to hire on _____ (date, day). Are you available to accompany us for hiring on that day? *If no, then ask about another available day for hiring. Once a hiring day is decided, continue with the below paragraph:*

The work will happen on _____ (day of hiring + 1 day). Are you available to supervise work on that day?

If no, then continue with other available date options for hiring and recalculate the confirmation and work day dates and repeat the above paragraph. Once you have agreed on a timeline, write down the same on a slip of paper with pen (do not carry typed paper) and hand it over to the employer. Then continue with the below instructions.

Once you finish this whole process without changing the agreed upon timeline and renegotiating on the requirements regarding your presence on the day of hiring, and conduct work on the agreed upon day, you will be given Rs. 500 as a gift from us. This is over and above the laborer's wage contribution that we will be providing.

Let us go over considerations: First, the benefits are the following:

- You will only pay Rs. _____(50) to each of three workers worker for the day of work and we will pay the remaining amount of their wage.
- Once you finish this whole process without changing the agreed upon timeline and renegotiating on the requirements regarding your presence on the day of hiring and confirmation, and conduct work on the agreed upon day, you will be given Rs. 500 as a gift from us. This is over and above the laborer's wage contribution that we will be providing.

Other considerations are:

(1) We will select all three laborers. These will all be daily-wage male laborers from your village's labor colony.
(2) The wage rate may be at or below the usual wage rate in the village. The jobs may be offered either in earshot of others in the labor colony, or in private.
(3) You will have to accompany us on the day of hiring and say a few words of introduction between us and the laborer we are hiring.
(4) You will provide all customary wage-in-kind provisions for the laborers on the day of work, and monitor work as you usually do on that day.
(5) You must also keep the details of the study and of this arrangement to yourself for the time period that we are working with you.

Are you willing to work with us under such an arrangement? *If he says no, thank him for his time and end the script here.*

Please let us know if you plans change in any way.

Contact details: _____

A.3.2 Worker Enumeration Script

Hello, we are here from a research organization called JPAL/IFMR. We are interested in studying the role of social norms in labor markets. We would like to obtain information about laborers in this village, including a listing of which laborers participate in agricultural labor. Would you be willing to provide us with this information?

We may return later to conduct research with participants in this village to understand their work and labor supply decisions. We will not disclose your participation in this study to anyone. If you would prefer not to participate, that is perfectly fine. You can stop at any time by just telling me.

Q1. What work is going on in the village at the moment? What work will happen in the next 1-2 weeks?
Check all that apply.
Ploughing (1) [ ]
Field preparation (land leveling etc.) (2) [ ]
Sowing (3) [ ]
Transplanting (4) [ ]
Besooning (5) [ ]
Weeding (6) [ ]
Harvesting (7) [ ]
Threshing (8) [ ]
Other Farm Crop Work (9)[ ](Please specify: _____)
Other Non-Crop Farm Work (10) [ ] (Please specify:_____)
Other (-98)[ ] (Please specify:_____)

Q2. Now, we would like to ask you some questions about wage rates for agricultural work in this village.

(1)	(2)	(3)
Task *(Use code from above)*	Wage rates for male laborers this year	Daily wage or piece rate?

Q3. What is the total number of labourer groups in your hamlet that work in brick kilns? _____

Now, we will just go house by house and ask you some basic information about the families in this village. If you don't know some information, it is no problem - please tell us that you don't know and we can move on to the next household in the village.

(1)	(2)	(3)	(4)	(5)	(6)
Household ID	Name	Nickname	Age	Primary Occupation	Are they currently living in the village or have they migrated?

Thank you very much for your time.

A.3.3 Main Sample Participant Hiring Script

Enumerators and Employer approach a household and speak to anyone present:
Hello, we are from an educational institute and we are here to learn about agriculture in the village. Can we speak to «Name of household head» please?

[If he is not present:] That is fine. Does any other adult male in the household do agricultural work for a daily wage?
[If yes:] Is «Name» at home? *[If there is at least one adult male daily-wage agricultural laborer in the house, proceed to deliver the remainder of the script to him. If there are multiple adult male daily-wage agricultural laborers, select one person at random to whom they will deliver the remainder of the script.]*

Then, if fully private treatment, the group should move indoors by asking "May we step inside to speak with you for a few minutes?" Employer initiates the dialogue by giving an introduction of the field enumerators:
Employer: Is your name _____? These people have come from an educational institute to study agricultural labor markets. Could they speak with you for five minutes about this?
[If yes, then proceed. If no, thank the participant for his time and end the survey.]

Enumerators: [Oral Consent (with incomplete disclosure)] As _____ «Employer name» just said, we are here from JPAL/IFMR, a research institute in Bhubaneswar. We are studying how agricultural labor markets work. Can we please ask you some questions? We will use your responses for our research. We will not share your responses with anybody outside the research team. But of course, there may be people who can overhear what you are telling us.

Do you agree to proceed?

Do you do agricultural work on others' land for a daily wage? *[If yes, then proceed. If no, thank the participant for his time and end the survey.]*
[If there is any doubt that the respondent is above 18 years of age:] What is your age? *[If under the age of 18 or above the age of 65, thank the participant for his time and end the survey]*

Employer: I have some work on my field for which I'd like to hire you for one day. The work is _____ (insert specific task for this village) on my field for one day on _____ (insert the date for the work). The other details will be given to you by them *(indicates for the enumerator to take over the dialogue. In the case of the private treatment, he steps away with the other enuemrator out of earshot).*

Enumerators: You will get _____ (insert wage here) for this work. The employer needs three people for this task on _____«Employer name»'s field on ___ _____«scheduled day». We are offering the job to you and two others in this hamlet for this task. We have to get this task completed so if we require more people, we will arrange for them.

(For private treatment only) We will not inform anyone else how much you have been offered for this job.

Do you understand? Whether or not you want to take this job, is fine. Would you like to take the job for «Rs. _____(wage)»?

If laborer rejects: Why? *[After the respondent has given a reason, skip to the questions]*
If laborer accepts: Be ready to work in the morning. We will come and fetch you at _____ AM, in the morning i.e. half an hour before the work starts. The work will happen at _____ *[insert location of the employer's farm/field].*

We have a few questions about agriculture to ask you related to our study. Would you be able to answer them?
1. For one acre land, what is the optimal number of laborers required for land levelling?

2. Post crop cutting, what is the total number of paddy straw bundles one laborer can get in one day?
3. How many paddy shoot bundles can one laborer pick in one day (contractual piece rate labor)?
4. When you go for daily wage labor, does the employer provide you with the farming tools or you carry them along?

[For enumerator: Afterwards, record on a scale of 1-3 whether the person seemed engaged and knowledgeable in answering questions, with 1=not engaged/knowledgeble, and 3=very engaged/knowledge.]

Thank you for your time.

A.3.4 Lab Game - Agricultural Job Hiring Script

Enumerators and Employer approach a household and speak to anyone present:
Hello, we are from an educational institute and we are here to learn about agriculture in the village. Can we speak to _____ «Name of household head» please?

[If he is not present:] That is fine. Does any other adult male in the household do agricultural work for a daily wage?
[If yes:] Is _____«Name» at home? *[If there is at least one adult male daily-wage agricultural laborer in the house, proceed to deliver the remainder of the script to him. If there are multiple adult male daily-wage agricultural laborers, select one person at random to whom they will deliver the remainder of the script.]*

Then, the group should move indoors for the fully private treatment by asking "May we step inside to speak with you for a few minutes?" Employer initiates the dialogue by giving an introduction of the enumerators:
Employer: Is your name _____? These people have come from an educational institute to study agricultural labor markets. Could they speak with you for five minutes about this?
[If yes, then proceed. If no, thank the participant for his time and end the survey.]

Enumerator: [Oral Consent (with incomplete disclosure)] As _____«Employer name» just said, we are here from JPAL/IFMR, a research institute in Bhubaneswar. We are studying how agricultural labor markets work. Can we please ask you some

questions? We will use your responses for our research. We will not share your responses with anybody outside the research team. But of course, there may be people who can overhear what you are telling us.
Do you agree to proceed?

Do you do agricultural work on others' land for a daily wage? *[If yes, then proceed. If no, thank the participant for his time and end the survey.]*
[If there is any doubt that the respondent is above 18 years of age:] What is your age? *[If under the age of 18 or above the age of 65, thank the participant for his time and end the survey]*

Employer: I have some work on my field for which I'd like to hire you for one day. The work is _____ (insert specific task for this village) on my field for one day on _____ (insert the date for the work). The other details will be given to you by them *(indicates for the enumerator to take over the dialogue. He steps away with the other enumerator out of earshot).*

Enumerator: The employer needs three people for this task on _____ «Employer name»'s field on _____«scheduled day». We are offering the job to you and two others in this hamlet for this task. We have to get this task completed so if we require more people, we will arrange for them.

Do you understand? Whether or not you want to take this job, is fine. Would you like to take the job for Rs. _____ *[insert w-10% wage here]*?

If laborer rejects: Would you be willing to take this job for _____ *[insert w wage here]*? *[If no, skip to the questions]*
If laborer accepts: We will come back to you to confirm the job. If we confirm, we will come and fetch you at ____ AM, in the morning i.e. half an hour before the work starts. The work will happen at _____ *[insert location of the employer's farm/field].*

[For enumerators: Afterwards, record on a scale of 1-3 whether the person seemed engaged and knowledgeable in answering questions, with 1=not engaged/knowledgeble, and 3=very engaged/knowledge.]

Thank you for your time.

A.3.5 Lab Game - Participant Hiring Script

Hello, my name is _____. We are here on behalf of a research organization called JPAL/IFMR. We wish to study and learn about agriculture and work in this village, and better understand the financial lives of the villagers. We will be conducting a small research activity with the laborers from your village tomorrow.

Are you free tomorrow morning, from ____am to ____am?

0[ ] No. *End Survey*

1[ ] Yes. *Proceed with the next question*

To see if you are eligible to participate, may I ask you a few questions?

0[ ] No. *End Survey*

1[ ] Yes. *Proceed with the next question*

Q1. What is your name? _____

Q2. How old are you? [ ] *If below 25 or above 55, End Survey.*

Q3. What is your highest level of education? (Use code from Code list-2) [ ]

Q4. What is your caste?

1 [ ] Scheduled Tribe

2[ ] Scheduled Caste

3 [ ]Other Backward Class (OBC)

4[ ] General

-98 [ ]Don't know

-97[ ] Not applicable

Q5. How long have you been living in this village?

1 [ ] my whole life

2 [ ] less than one year

3 [ ] 1-3 years

4 [ ] 3-9 years

5 [ ] more than 10 years

Q6. Are you fluent in Odiya?

a) Read

0. [ ] No. 1. [ ] Yes.

b) Write

0. [ ] No. 1. [ ] Yes.

Q7. Are you the primary breadwinner of your family?

0. [ ] No. 1. [ ] Yes.

Q8. What is your occupation? (Use code from Code list-1)

a. Primary occupation [___] b. Secondary occupation [___]

Q9. Do you work as a casual laborer in agriculture for a paid wage?

0[ ] No. *End Survey*

1[ ] Yes. *Proceed with the next question*

Q10. Have you worked as a casual agricultural laborer for a paid wage in your village in the past year?

0. [ ] No. 1. [ ] Yes.

Q11. Approximately how many days have you worked as a casual agricultural laborer in the past 60 days?

_____days

Q12. What is the prevailing wage rate for casual agricultural work in your village?

Rs. [____] per day

Q13. Do you cultivate land?

0.[ ] No. *Go to Q17.*

1.[ ] Yes.

Q14. How much land do you cultivate (including your own land and land that does not belong to you is cultivated by you)?

1. Acres [____] 2. Guntas [____]

Q15. Do you hire laborer for your own agricultural work?

0.[ ] No. *Go to Q17.*

1.[ ] Yes.

Q16. How many laborers did you hire during the current agricultural season? [____] *Instruction to surveyor: Please calculate number of man-days (no of laborers hired x no of days worked).*

Q17. Are there any other work opportunities available in this village, such as mining, stone breaking, in brick kilns, etc.?

0.[ ] No 1. [ ]Yes Specify: _____

Q18. Do you participate in this activity?

0. [ ]No

1. [ ]Yes Specify number of days in last 30 days: _____

[If eligible]

We will be conducting a small research activity from _____ to _____ tomorrow morning in your village, for which we would like to have your participation. The activity will take 3 hours, and you will receive an amount equivalent to your daily wage rate for participating. Please note this is not a job offer, and the activity will only be tomorrow for 3 hours, from _____-_____ am. Will you be free to attend the research activity?

0. [ ]No. [Thank you for giving us time for this survey.]
1. [ ]Yes. [Great. We will come tomorrow morning at ____ am to call you.]

A.3.6 Lab Game Script

Introduction: Group Script

Hello, thank you for coming here today and taking part in this research study. We are here from a research organization called JPAL/IFMR in Bhubaneswar. We are here to learn about agricultural activities and work opportunities in your village, and also understand more about your lives. We will be conducting a small activity with you today, in which you have the chance to earn some money for yourself and others in your village. For participating in today's activity, you will get a fixed wage of Rs. 120. Over and above that, you can earn more depending upon your decisions and performance in the activity. However, we assure you that you will get not less than your daily wage, and even have the chance to earn more. We will explain more as we move ahead. Is that clear? What is the base wage for being here today? Whatever discussion we have here, there are no negative repercussions to anything you say. Please remember, we are here only for today and there are no added benefits or long term gains from being here. If you wish to leave at any time you may, but you will lose the chance to earn any money.

Icebreaker Activity
We want to start with a small group activity. We'll make a few statements and ask a few questions about conditions in your village, and we'd like you to just call out your responses.
"What are the kinds of crops you grow?"
"What task are you doing at this time on the field?"
"What is the primary festival celebrated in your village?"
"Did you know, we were in your village this morning with an employer, to hire laborers for his task?"
"What is the wage rate for agricultural work this year?"
"Do all employers typically pay this wage rate, or do some employers pay less and others pay more?"
"How is the wage rate decided? Is it decided as a group?"

Thank you. Now let's start today's research activity.

Costly Punishment Let us now try and understand what this research is.

For conducting this research, we have some money with us that we want to give to people in your village. However to decide how to allocate this money, we will need your help. Please note that this money is not for any job offer or work in the future, it is only for the research we are doing today. The process in which this works is this:

There are two trays in front of you. The white tray belongs to you (point) and the red tray (point) belongs to another person in your village, whose identity neither you nor I know. There is Rs.100 kept in your tray and Rs.100 kept in the tray of the other person. This money in the white tray is what you can earn today, in addition to the base wage, and the money in the red tray is what the person in your village can earn today. How much money is in the trays?

In order to decide how much money is to be given, we will be presenting various situations to you about different people. Based on what you think of the situation, and how you feel about that person -if what they did is right or wrong, good or bad- we want you to decide how much of this Rs.100 they should get. In every situation, the red tray will belong to a different person. Please note, there are no wrong or right answers, we just want to know your opinion. So suppose, you think that in a situation, the person should get less than Rs.100. If you wish to reduce Rs.5 from him, you will need to give up Re.1 of your own. If you wish to reduce Rs.10, you will need to give up Rs.2 of your own. Hence a reduction of each of his 5 rupees is

equal to a reduction of 1 of yours. If you think he should get Rs.100, then even you will get Rs.100. It is important to remember that this money that you take from him and lose of your own, is not given to anyone, it is kept aside by us. Nobody gets that money. Understood?

To understand this activity properly, let us see some examples. After this, we will have the final activity.

Let us see an example.

Example 1

Surveyor 1: Let's suppose the other person *[show the red tray]* in this example is a person from your village who stole your bike. That person has his own land, owns a bike yet decides to come and steal your bike.

To the audience: How would you feel? Would you like to punish that person?

Audience responds

The white tray is your tray and the red tray is that other person's tray from your village.

To the audience: How many coins are there in each tray?

Audience responds

To the audience: What is the total amount in each tray?

Audience responds

Surveyor 1 to Surveyor 2: Based on how you feel about that person -if what he did is right or wrong, good or bad, You have the opportunity to decide and reduce some money from his tray if you wish. Please tell us how much money you would like him to get? We will then use the coins to show you how much you get and how much he gets.

For each Rs 5 that you take away from the red tray person, we will remove 1 Re from your tray.

To the audience: If we take away Rs. 5 from the red tray, how much money would you lose?

Audience responds

Surveyor 1: Do you want to cut any money from this person? *Surveyor 2:* Yes

Surveyor 1: How many rupees would you like to take away from the red tray guy to punish him? This answer need not be final and you can always change your mind later or go back to your original choice. This is just for you to see how much you keep and how much you take away from him.

Surveyor 2: I would like to take away Rs. 50 from the Rs. 100 of this person. He therefore gets Rs. 50. To do that, I have to give up Rs. 10 of my own allocated money.

Surveyor 1: Why did you take away Rs. 50 from his tray?

Surveyor 2: Because he stole my bike which is unethical so I would like to punish him.

To the audience: How much would you take away from the white tray, if we take away Rs. 50 from the red tray?

Audience responds Help the audience respond, if they are unable to figure out the math.

Surveyor 1: Who gets the money which has been removed from the red tray or your tray? *Surveyor 2:* **Nobody gets that money, it is just kept aside.**

Surveyor 1 to Surveyor 2: Now, he has Rs. 50 *[show the red tray]* and you have Rs. 90, as shown by the coins on this tray. Are you satisfied with your answer? Would you like to think again? There's no hurry to make your decision, you can always change your mind or go back to an earlier choice.

Surveyor 1 to audience: We always ask if you are satisfied with your choice, no matter how you choose. It is so that people take their time in making this decision for themselves. There is no right or wrong answer, so it is perfectly acceptable to stick to the decision you have originally made. If you want to change your decision at this point, that is absolutely fine too.

Surveyor 2: No, I think I would instead like to take away Rs. 75 from his payment.

Surveyor 1: Okay, that is fine. To take away Rs. 75 from your partner, you need to give up Rs. 15 of your payment. That leaves him with Rs.25 and you with Rs. 85. [Moves coins to match] Is this what you would like to do? Take your time.

Surveyor 2: Yes, this is what I want to do.

Now, let us take another example. In this, the red tray belongs to some other person in your village; it is not connected to the first example.

Example 2

Surveyor 1: Let's suppose the other person *[show the red tray]* in this example is a guy from your village who gifted you a box of sweets.

How would you feel about him?

Looks at the audience to see if they havequestions or comments

Surveyor 2: I will be happy with him.

To the audience: What is the total amount in your white tray? *To the audience:* What is the total amount in the red tray of your partner?

Audience responds

Surveyor 1 to Surveyor 2: Based on how you feel about that person -if what he did is right or wrong, good or bad, You have the opportunity to reduce some money from his tray if you wish. Please tell us how much money you think he should get. We will then use the coins to show you how much you get and how much he gets. Would you like to reduce amount from his tray?

For each Rs 5 that you take away from the red tray person, we will remove 1 Re from your tray.

To the audience: If we take away Rs. 5 from the red tray, how much money would you lose?

Audience responds

Surveyor 2: I would like to take away nothing from this person, because he has been nice to me.

Surveyor 1 to Surveyor 2: That would just mean that you get to keep Rs. 100 of your own share and the other person also gets to keep Rs.100.

Are you satisfied with your answer?

Surveyor 2: Yes

To the audience: Why do you think this person took away nothing from the red tray? If you were in his place, what would you have done?

Audience responds

Are you all understanding how this activity works? Do you understand the examples and how the process of the activity?

Great. So we can begin. We will do this activity with each of you. Every person in the group will get to participate. Each time we play with a new participant, the red tray will belong to a new person. However, we will not know his identity. Do you have any questions? Can we begin?

[Surveyors take the participants one at a time to the private station.]

Lab Game: Private Script

Let us quickly revise.

[Perform concept check and mark score card]

Comprehension Check Questions:

1. Who does the red tray belong to? Who does the white tray belong to?
2. How much money does each of the trays have?
3. If we remove Rs.5 from the red tray, how much is removed from the white tray?
4. How much is left in the red tray now? How much is left in the white tray?
5. Who gets the money that is removed from the red tray? And that is removed from the white tray?

Now, let us start. We will do this activity four times. Every time, the red tray will belong to a different person that we will tell you about. After the four rounds are over, we will do a lottery to determine which round's earnings you are to get. Whichever round's chit you pick, you will get the amount that was in the white tray for that round.

Example 1

In this round, let us say that the red tray belongs to someone who gifted you a bag of grain when you really needed it. Based on how you feel about that person -if what he did is right or wrong, good or bad, you have the opportunity to reduce some money from his tray if you wish. Please tell us how much money you think he should get. We will then use the coins to show you how much you get and how much he gets. Would you like to reduce amount from his tray?

Wait for the participant to respond According to your decision, you get Rs. ____ and the person with red tray gets Rs. ____. Are you happy with this decision? Do you want to change it?

Wait for respondent

According to your decision, you get Rs. _____ and the person with red tray gets Rs. ____. Are you happy with this decision? Would you like to change it? Why did you choose to give this amount? We are just asking because we want to know your opinion, there are no wrong or right answers.

Example 2

Let us go to the next round. Please remember, this is about another person, not the one from the previous round. In this round, there is a farmer who hired labor to work on his land two months ago. The laborer was not his relative or a close friend. Although the laborer worked two months ago, the farmer still hasn't paid him the wage. In this example, the red tray belongs to that farmer who has still not paid the wage to the laborer.

Based on how you feel about that person -if what he did is right or wrong, good or bad, you have the opportunity to reduce some money from his tray if you wish. Please tell us how much money you think he should get. We will then use the coins to show you how much you get and how much he gets. Would you like to reduce amount from his tray?

Wait for the participant to respond According to your decision, you get Rs. ____ and the person with red tray gets Rs. ____. Are you happy with this decision? Do you want to change it?

Wait for respondent

According to your decision, you get Rs. _____ and the person with red tray gets Rs. ____. Why did you choose to give this amount? We are just asking because we want to know your opinion, there are no wrong or right answers.

[Note: After the examples, the concept check for understanding of rounds should be marked by the surveyor.]

Okay, now we are done with the first two rounds. Do you have any questions? Great. Let us start our final round.

Main Round

We will now give you two more scenarios about two different potential laborers. Please listen carefully, and then decide your response. You are doing Round 3 of this research. Please remember, this is about another person, not the one from the previous round.

Before we begin, please tell us what is the prevailing wage in your village? **Wait for respondent to give the correct answer**

[If the design is about hypothetical hiring in a neighboring village:] Farmers in a neighboring village, 15-20 km away from your village, need to hire labor for work. Hiring was done yesterday, and people were asked if they would work for landowners at different wage rates. A farmer in that village wanted to offer a job to a laborer in his village. The prevailing wage in that village is the same as that in your village, Rs. _____. The laborer has a decent lifestyle and does not have major problems in life. He is not a relative or close friend of the farmer. The farmer offered him the job of _____. The work would be from _____ to _____ time, at the wage rate of Rs. _____. The laborer accepted it. In this round, this red tray belongs to that laborer in your neighboring village who accepted the wage rate of _____ for the work of _____.

[If the design is with hiring in own village:] There are landowners in your village that have to hire labor. Hiring was done yesterday, and people were asked if they would work for landowners at different wage rates. The task offered for the job is ___ __. This red tray belongs to one of the laborers who was offered the job. The laborer the job was offered to has a decent lifestyle and does not have major problems in life. He is not a relative or close friend of the employer. The employer offered him the job of _____ from _____ to _____ time. He offered him a wage of Rs. _____, and the laborer accepted it. Hence in this round, the red tray belongs to a laborer in your own village who accepted the wage rate of _____ for the work of _____ __.

Check. Who does the red tray belong to? What job did he get? What were the timings? At what wage? Did he accept it? How much money does this tray have? How do you feel about what he did? Based on how you feel about that person - if what he did is right or wrong, good or bad, you have the opportunity to reduce some money from his tray if you wish. According to you, how much money should this laborer get? Would you like to reduce some of his money from the red tray based on his actions?
For every Rs. 5 that you take away from him, you will also have to give up Rs. 1 from your own Rs. 100.

Wait for respondent

[Surveyor moves coins according to the participant's response] Now, are you satisfied with this choice? Why did you choose to give this amount? We are just asking because we want to know your opinion. There are no wrong or right answers. After the completion of third round you have Rs. _____ in your tray *[Surveyor shows him the chit*].

Repeat for Round 4.

[Note: After the rounds, the concept check for understanding of rounds should be marked by the surveyor.] Thank you. Now that we have finished the four rounds, we need to decide for which round you will receive the payment. We will do a lottery. Each of these chits in this folder has the number of a round written on it. The number you pick out will determine which round's payment you get. You will get the amount in the white tray for that round. Any questions?

[Pick chit. Announce lottery round number, and the payment associated with it]

Thank you. According to the amount you decided the person in the red tray should get, we will pay you the amount that is there in the white tray, along with your base

wage. Our surveyor will now escort you back to the common area with the other participants, where you will wait. Thank you for your participation.

[After all participants have finished playing the game and received payment.] We will be doing a lottery to see which of the laborers that had been approached for the job will get the job.

[Enumerator picks three chits from the bag] Please note, that all of these laborers will be working at Rs.200, i.e. the prevailing wage. NONE of the laborers will be working below the prevailing wage. Thank you for your participation today.