LABOR RATIONING: A REVEALED PREFERENCE APPROACH FROM HIRING SHOCKS

EMILY BREZA[⋆], SUPREET KAUR[‡], AND YOGITA SHAMDASANI[†]

Abstract. In developing countries, wage employment rates are often low, but whether this reflects high involuntary unemployment is unclear. Economists assess unemployment using survey self-reports, whose reliability is unknown. We develop an approach to obtain the first revealed preference estimates of labor rationing. We generate large transitory hiring shocks in Indian local labor markets — hiring up to 35% of the male labor force for month-long work in external job sites. We find a central role for seasonality. During peak employment seasons, the hiring shocks instantaneously increase local wages and crowd-out local aggregate employment, consistent with textbook predictions. In contrast, we find evidence for severe labor rationing during lean seasons, which account for 6 months of the year. Specifically, "removing" a large portion of workers leads to (i) no change in the local wage and (ii) no change in local aggregate employment levels — due to one-for-one positive employment spillovers on the remaining workers who benefit from decreased competition for job slots. Much of this rationing is disguised in the form of less productive self-employment. We document that traditional government surveys will substantively underestimate labor market slack in this context. Our approach can be extended to obtain revealed preference bounds on rationing in diverse settings.

Date: This Version: June 15, 2020.

We thank Abhijit Banerjee, Pat Kline, and many seminar participants for helpful comments and conversations. We thank Arnesh Chowdhury, Piyush Tank, Silvia Wang, Mohar Dey, Anshuman Bhargava, Vibhuti Bhatt, Asis Thakur, and Anustubh Agnihotri for excellent research assistance.

[⋆]Department of Economics, Harvard University; NBER; JPAL.
[‡]Department of Economics, University of California Berkeley; NBER; JPAL.
[†]Department of Economics, National University of Singapore.

1. Introduction

> [D]istinguishing elements of voluntariness from elements of involuntariness in the unemployment problem is a hopeless endeavour. (Fellner, 1976)

In developing countries, wage employment rates among rural workers often hover around 50%. For example, landless prime age males—whose primary source of earnings is wage labor—work 45.7% of the time in rural India (National Sample Survey 2009). In Bangladesh, the wage employment rate is about 55% during the lean season (Akram et al., 2018). In Sub Saharan Africa, these rates are often even lower (Beegle and Christiaensen 2019).

The interpretation of this empirical regularity is unclear. Do low employment rates reflect extremely high levels of involuntary unemployment, indicating the presence of large distortions? Or are they the outcome of well-functioning labor markets, where workers are simply choosing other activities such as self-employment? These questions are central to the debate about the nature of the labor market equilibrium in poor countries. They inform the way in which labor market analysis should be conducted—for example, whether it is appropriate to compute an elasticity from a wage response. They are also an essential input into social policies—for example, whether workfare programs will have distortionary effects on the private sector, or indeed are even needed in the first place.

Despite the centrality of unemployment as a marker of the economy's health, assessing it has remained notoriously challenging in both rich and poor countries. To date, economists have measured unemployment using survey self-reports, e.g., whether an individual was "looking for a job but could not find one" (Taylor, 2008; Card et al., 2012). The reliability of this approach is unknown, making it difficult to ascertain whether survey measures reflect true labor market slack. For example, if respondents claim they are searching for work but are not, or if they are not employable in the jobs for which they are searching, responses would overestimate the extent of slack.[1] Alternately, if workers who are rationed out of the wage labor market are employed in their own business or domestic duties, traditional survey measures would underestimate the amount of true involuntary unemployment in the wage sector. This is

[1] A worker can only be classified as rationed from a job if, from the employer's perspective, she is qualified for it (see below). However, worker self-reports do not provide insight into such demand-side issues.

especially relevant in developing countries, where self-employment is high (Benjamin, 1992; Adhvaryu et al., 2019).

For concreteness, we define rationing as occurring when a worker: a) would prefer wage employment in a job at the existing market wage over what she is currently doing (i.e. the worker is not on her labor supply curve), and b) the worker is employable at that wage (i.e. from the employer's perspective, her marginal product is weakly above the current market wage for the job).

This paper empirically examines the extent of labor rationing in a developing country context. To tackle the fundamental measurement problem, we develop a simple revealed preference approach. We induce labor market-level transitory hiring shocks, "removing" up to 35% of the labor force of casual male workers for work in external job sites for 2-4 weeks. We then use the local labor market response to diagnose the equilibrium that existed in the absence of our shock. We undertake this exercise across different months of the year—enabling us to examine changes across lean and peak periods.

A benefit of this approach is that it uncovers labor market functioning without direct intervention on the participants of interest. While we exogenously generate hiring shocks, the outcomes we measure are driven completely by the response of existing employers and workers who never interface with the external worksites. We use the phrase "local labor market" to denote all jobs, wages, and employment *excluding the external worksite jobs we create.* If the level of rationing is higher than the size of our shock—e.g., if we remove 25% of the labor force and more than 25% of workers is rationed—then we would expect the following effect in the local labor market: i) no change in wage levels; ii) no effect on aggregate employment; and iii) positive employment spillovers on individual workers, whose employment goes up due to reduced competition for job slots. This constitutes a revealed preference test: workers reveal that they prefer jobs at the market wage over their previous activity (e.g. unemployment or self-employment), and employers reveal that the worker is qualified to be hired for that job at the market wage. If these predictions hold, the size of our hiring shock is a lower bound on the level of rationing in the economy.

A few features of our design are worth noting. First, our shocks are transitory (not affecting permanent income) and very targeted (only available to the participants we randomly "removed"). These two features greatly simplify our analysis of interest—for example, relative to the introduction of a workfare program, which can shift the labor supply curve of participants and non-participants (see discussion below). Second, our test does not require us to take a stance about the labor market equilibrium in the absence of rationing—for example, whether it is fully competitive or subject to monopsony or some other friction. Our test simply diagnoses the level of rationing, regardless of other underlying forces that may be at play. We view this as a strength of our approach, and do not make direct claims regarding potential other frictions. Third, we do not take a stance on the reason for rationing; our test is valid regardless of the microfoundation.[2] Overall, our primary goal is to uncover the extent of labor market slack across different times of the year.

The setting for our test is rural labor markets in Odisha, India, which exhibit many features of developing country village economies—including a reliance on agriculture that generates employment seasonality, which is only partially offset with non-agricultural work. We conduct the experiment using a matched-pair, stratified design in 60 labor markets (i.e. villages) across varying times of the year. We invite workers to sign up for employment at external worksites, drawing signups by a large proportion of the labor force. We then randomly choose which workers are "removed" from among these.[3] On average, we hire 24% of the labor force of male workers in treatment villages.

What is the impact of removing about a quarter of workers on the local labor market? The answer depends crucially on seasonality.

During "peak" months—the 6 months of the year when baseline employment levels are above median levels—the local labor market response to our shock matches the predictions of traditional textbook models of supply and demand. The equilibrium wage rises by 5.3 percent, and aggregate employment (not including our external factory jobs) declines by 3.4 percentage points or 17.1%. In the peak season, each day of work we create in external factories crowds out 0.172 days of local labor market employment. It is worth noting that this wage and employment response is almost instantaneous—occurring within just a week of the start of the transitory hiring shock. This indicates a remarkably high level of labor market responsiveness, and accords with the view that spot labor markets in these settings are capable of quickly reflecting changes in market conditions (Rosenzweig, 1988).

[2]In related work, Breza et al. (2019) offer evidence that implicit collusion among workers helps maintain wage floors in this setting. However, other mechanisms are also possible.

[3]We offer relatively high wages for desirable manufacturing jobs. Since our tests are based on how the remaining local labor market responds, offering a high wage is not problematic. Rather, it is beneficial for our research design, enabling us to draw (random) workers from across the skill distribution in the village.

This stands in sharp contrast to effects during the "lean" season—the half of the year when baseline employment is below median. The lean season response to our shock matches predictions under severe labor rationing. Specifically, removing a quarter of the labor force has no effect on wages or aggregate employment (predictions (i) and (ii) above). These results hold across samples of workers, both those who signed up for the jobs and the full village (including those who did not sign up for the external jobs).[4] In other words, creating external jobs generates no crowd-out in the private labor market in the lean season. Consistent with prediction (iii), there are large positive employment spillovers on the workers remaining in the local labor market; the employment rate for these workers goes up by 5.3 percentage points or 36%. These employment spillovers follow mechanically from the fact that there is no change in aggregate employment: workers who would otherwise have been rationed fill in job slots that are available because many other laborers have become employed outside the village. External employment therefore helps not only those who received our worksite jobs, but also those who did not due to these spillover effects.

These lean season effects are transitory—lasting only as long as the hiring shocks do. Once the shock is ended and all workers are back in the village labor force, the labor market goes back to looking the way it did before the shock. Specifically, 2 weeks after the hiring shock is over, we see no more employment spillovers; there is no detectable difference in employment or wages between the treated and control villages. This is what one would expect under rationing. In contrast, we find evidence for sustained increased wages and decreased employment in treatment villages in peak months, consistent with a ratcheting effect.

We supplement our core findings with two additional exercises linked to the two sets of measurement challenges described above. First, we assess whether self-employment masks involuntary unemployment: what share of rationed workers are self-employed in other tasks? A large body of work in development has highlighted that rationed individuals may turn to less-productive self-employment as a way to generate income — creating "disguised unemployment" or "underemployment" (e.g., Singh et al., 1986; Benjamin, 1992). Self-employment patterns in this data provide initial support for this view: among households that run a self-employment business at baseline, the median number of days a month for which it is their primary activity is 20 in the lean season

[4]In addition, we document that there are no significant differences in worker characteristics among those who receive jobs in treatment vs. control villages. This provides further evidence against an interpretation of these results that is driven by compositional changes in who is getting employed.

and 5 in the peak season. Further, 45% of these households report shutting their business down completely in the peak season. In accordance with this, in response to hiring shocks in lean months, we find that workers switch from self-employment to wage employment when jobs become easier to find. Self employment declines by a large margin: 3.3 percentage points on a base of 0.13. This accounts for an estimated 62% of the employment spillovers.

Second, we assess how traditional survey-based measures of unemployment compare with our revealed preference findings, and compare them with alternate ways of asking about unemployment on surveys. We estimate the impacts of the hiring shock on two measures of self-reported involuntary unemployment. We first measure effects on the traditional measure used in government surveys across a range of settings (e.g. India and the US).[5] This traditional approach only codes workers as involuntarily unemployed if they report no other work activity. Under this measure, we fail to find significant changes on lean season involuntary unemployment as a result of the hiring shocks. Given the importance of disguised unemployment in our setting, this is not surprising — many workers who appear gainfully employed in self-employment are actually involuntarily rationed out of the wage labor market. Next, we propose an alternate measure that allows workers to state whether they would have preferred wage employment at the going wage rather than their activity (unemployment or other work) on a given day. Using this measure, involuntary unemployment falls by 6.7 percentage points in the lean season — roughly the same magnitude as the increased employment from employment spillovers. This suggests that this alternate survey measure better captures the magnitude of the revealed preference response than the current status quo measure used across household surveys.

There are other potential mechanisms through which our hiring shocks could affect wages or employment. For example, wealth effects from the hiring shocks could increase local demand, the composition or quality of workers remaining in the village may change, or there may be inter-temporal substitution in self-employment activities. However, as we discuss in Section 7, such factors cannot explain the full pattern of our results. We also offer direct supplementary tests for concerns that such alternate factors may explain our lean season results even in the absence of rationing. Moreover,

[5]Our elicitation follows the same procedure as the Indian National Sample Survey. The US Labor Bureau also follows the approach where if a worker reports self-employment, they cannot be classified as involuntarily unemployed.

any such explanation would have to reconcile why the effects in lean vs. peak periods are so drastically different.[6]

Of course, the magnitude of our estimates is specific to our particular context of rural Odisha. However, the broad pattern of our results, and their link to survey measures of involuntary unemployment, likely have much wider relevance. In our particular context, two different labor market paradigms are relevant, depending on predictable fluctuations in employment levels. Similar dynamics are likely to prevail in many rural, developing country settings.

This paper advances the literature on labor markets in developing countries. It provides the first direct evidence for severe labor rationing in this context. Some of the earliest work in development, such as the surplus labor hypothesis, was rooted in the belief that there is a large amount of slack in developing country labor markets (Lewis, 1954).[7] Among policymakers, this belief underpins the prevalence of large social programs such as workfare in poor countries today. However, this view is generally not reflected in the modern development literature—particularly in light of wide-ranging evidence that wages and employment adjust readily to labor market shocks (e.g. Rosenzweig, 1988; Jayachandran, 2006; Fink et al., 2018; Imbert and Papp, 2015; Donaldson and Keniston, 2016; Muralidharan et al., 2017; Akram et al., 2018; Breza and Kinnan, 2018).[8] Our findings reconcile these conflicting views. In peak seasons, there is no large rationing; the labor market appears responsive and agile, with almost instantaneous changes in wages and employment. In contrast, in the lean season, the level of rationing is remarkably high; this creates large levels of both unemployment and "disguised unemployment" with rationed laborers turning to self-employment. These patterns are consistent with existing work on wage rigidity (Kaur, 2019) and separation failures (Singh et al., 1986; Benjamin, 1992; Udry, 1996; Foster and Rosenzweig, 2011; LaFave and Thomas, 2016). By providing direct evidence for rationing, our study supports the mechanisms discussed in these papers.

[6] If labor supply is highly elastic, this could could generate our lean season predictions even when there is no rationing. To reconcile our results, the supply elasticity would need to be between 3.9 - 29.2. This is implausibly large, and is inconsistent with the more modest elasticity computed from directly randomizing wages in Breza et al. (2019).

[7] Our approach is inherently related to tests of the surplus labor hypothesis. Notably, Schultz (1964) argued that a large negative supply shock — deaths during an Indian influenza epidemic — decreased agricultural output. See Sen (1967) for a critique.

[8] Of course, one could have rationing despite rapid labor market adjustment, for example, under market power. Labor market analysis in most modern development papers, however, typically does not account for the possibility of severe rationing.

In addition, our paper relates to the labor economics literature on measuring unemployment.[9] We develop an approach to obtain the first revealed preference evidence for labor rationing in any setting. Our approach can be adopted to other settings to check for rationing—by using policy changes and other quasi-random labor market shocks, with some additional assumptions—giving our methodology broader applicability.[10] In addition, we show that the questions typically used in government and household surveys substantively underestimate true labor market slack in our context. We propose and validate a revised survey measure for eliciting involuntary unemployment in the wage sector. This measure will have particular relevance when unemployed workers have the potential to shift their activities to other kinds of work—for example gig economy workers or households with farms.

Finally, our study advances two additional streams of research within development. First, a large body of work emphasizes the importance of seasonality in consumption levels (Bryan et al., 2014; Fink et al., 2018), with correlations in labor market opportunities. Our work expands the literature on seasonality by documenting that the labor market equilibrium is drastically different across lean and peak times. This complements existing studies by suggesting a reason that exacerbates the problem of lean season consumption shortfalls: low labor demand doesn't just depress wages, but rather can make it difficult to find any local work at all due to severe rationing.

Second, a growing body of research studies the general equilibrium impacts of labor market programs such as workfare, with differing effects on wages and crowd-out of jobs (Imbert and Papp, 2015; Muralidharan et al., 2017; Beegle et al., 2017; Zimmermann, 2020). Our intervention is not directly comparable to a workfare policy. Such programs constitute a permanent shock that impacts *all* eligible workers by offering an outside option. This potentially increases reservation wages, shifting the entire labor supply curve to the left, even among workers who don't ever participate in the program. Consequently, such programs are more likely to lead to wage increases than our shock, which was both transitory (not affecting permanent income) and very targeted (only available to the participants we randomly "removed"). However, our study helps

[9]For discussions about challenges surrounding unemployment measures, see, e.g. Bound et al. (2001); Faberman and Rajan (2020).

[10]Crépon et al. (2013) evaluate job placement services by varying treatment intensity across French cities. They find that displacement effects on untreated workers depend on underlying labor market conditions and whether treated workers compete with other types of workers for jobs. With additional information on the size of the shock and elasticities, this approach can be used to diagnose rationing.

provide insight into the likely effects of such programs by highlighting that: (i) crowd-out will depend crucially on the extent to which the policy overshoots the amount of labor market slack, and (ii) these effects are likely to differ at different times of year. Consistent with this intuition, Beegle et al. (2017) find little evidence for crowd-out from a modest workfare program run in the lean periods in Malawi. In contrast, we find that NREGA, which has been shown to have wage impacts in some studies, does operate in Odisha during "peak" months. More fundamentally, our findings provide guidance on how one should model labor markets at different times of year, with broad implications for labor market analysis.

The remainder of the paper is organized as follows. Section 2 describes the context and setting. Section 3 outlines our empirical predictions, alternately, under market clearing and under labor rationing. Section 4 details the experimental design and its implementation. Section 5 describes the data. We present the experimental results in Section 6. Section 7 discusses potential threats to validity. Section 8 provides a discussion.

2. Context

The experiment takes place in villages across five districts in rural Odisha, India. Markets for casual daily labor are extremely active in this setting.[11] Daily-wage workers engage primarily in rainfed agriculture for approximately six months of the year. In the remaining lean months, they typically seek short-term contract employment in non-agriculture, such as manufacturing and construction.

The village constitutes a prominent boundary for the casual labor market — daily-wage workers typically find work in both agriculture and non-agriculture within or close to their own village. For example, among workers in our study sample, 71% and 48% of reported worker-days in agriculture and non-agriculture respectively are for work within the village. For work outside the village, the median distance from own village is 3 kilometers for agricultural employment, and 4 kilometers for non-agricultural employment. Further, hiring is largely employer-directed — employers approach workers for 88% of reported worker-days, while workers approach employers for only 5% of reported worker-days.

[11] Markets for casual daily labor are ubiquitous across poor countries. They constitute an employment channel for hundreds of millions of workers in India alone, and account for 98% of the country's hired agricultural labor (National Sample Survey, 2010). Casual labor markets are characterized by a high degree of decentralization and informality (e.g. Rosenzweig, 1988).

Employment rates in the casual labor market are generally low; the mean daily employment rate, restricting to hired wage employment only, is 19%. Further, employment rates are highly variable. The mean daily employment rate falls to 15% in the lean months, which spans approximately half the year, and rises to 22% in the peak months. The villages in our study thus match a general feature of village economies: large periods of low employment.

3. Hypotheses

3.1. **Definition of Labor Rationing.** Suppose the prevailing wage for one day of work in the casual daily market is w. We define a worker as rationed on a given day when the following two conditions hold: (i) The worker wants to supply labor at wage w, but is unable to find employment; (ii) The worker is qualified for jobs occupied by other villagers.

The first condition essentially states that the worker is not on her labor supply curve. The second condition states that a worker who wants a job but is unqualified for it (in the sense that an employer would never find it profitable to hire her at wage w) is not considered rationed. Note that we take no stance on neither the micro-foundation nor the mechanism for rationing. Our goal is to empirically examine the extent of rationing, and decompose it into involuntary unemployment and disguised unemployment.

3.2. **Predicted Impacts of a Negative Labor Supply Shock.** Our field experiment is designed to exogenously shock village labor supply. This is achieved using an experimental hiring shock, where a subset of workers in the local labor market are hired in external jobs generated outside the village. By removing workers from the village, the shock leads to a reduction in the residual labor supply remaining in the village, while local labor demand remains unchanged. We then examine what happens in the local labor market after the negative supply shock.

In laying out our predictions, we employ the simplest framework to interpret our results: a stylized demand and supply framework. Panel A of Figure 1 shows the effects of a negative labor supply shock under market clearing. Let E denote the level of employment (in terms of worker-days) in the village and w denote the village wage in the absence of our intervention. A negative supply shock (a shift from S to S') should lead to: P1) an increase in local wages from w to w'; and P2) a decrease in aggregate employment among workers who remain in the village (i.e. those who are not hired by us to work in factories), so that total employment after the shock $E' < E$.

Panel B of Figure 1 shows the effects of a negative labor supply shock under a wage floor. As before, E denotes the level of employment in the village and w denote the village wage in the absence of our intervention. Rationing exists in this labor market, with supply E_S exceeding demand E_D at wage w. If the amount of labor rationing is weakly greater than the size of the supply shock, then we predict that a negative supply shock (a shift from S to S') should lead to: L1) no effect on local wages; L2) positive employment spillovers among workers who remain in the village — higher individual employment (due to reduced competition for job slots); and L3) no effect on aggregate employment levels. If predictions L1-L3 hold, workers reveal that they prefer jobs at w over their previous activity (e.g. unemployment or self-employment), and employers reveal that workers are qualified to be hired for the jobs at w — this thus constitutes a revealed preference test for rationing.[12]

Note that predictions L2 and L3 above are inherently related. The amount of rationing equals $E_S - E_D$, in terms of worker-days. With the supply shock, a set of workers in the village achieves full employment in the factories. This generates employment spillovers on the remaining workers (i.e. those who are not hired by us to work in factories), because a larger fraction of them can fill the available job slots (in terms of worker-days) in the local village labor market (prediction L2). However, as long as the amount of labor rationing is weakly greater than the size of the negative supply shock, then total employment (i.e. total worker days hired by employers) in the local village labor market will not change (prediction L3).

To illustrate these predictions, consider the following thought exercise. Suppose 10 workers want to work in the village for wage w, but only 5 job slots are available at this wage. As a result, 5 workers are employed at wage w, while the other 5 workers are rationed (50% employment). Now, suppose we remove 4 workers from the village labor market. This frees up job slots, and a larger portion of the remaining workers can work in the village labor market at wage w. Specifically, there are 6 workers left who want work and 5 available slots (83% employment). In contrast, if the 5 workers who are unemployed did not want work, they would not accept employment at wage w; this provides a test of condition (i) above. In addition, the fact that workers who remain in the village are hired at wage w indicates that employers perceive them as qualified for work at w; this gives a test for condition (ii) above.

[12]Appendix Figure 1 shows the effects of a *positive labor demand* shock under market clearing (panel A) and under a wage floor (panel B). Note that the predictions under a positive demand shock are identical to that of a negative supply shock as described above.

Our predictions above do not require us to take a stance on how the composition of workers — in terms of ability — changes as a result of the supply shock. There may be a distribution of ability levels among rationed workers. However, for us to observe employment spillovers, it must be the case that the rationed workers are qualified for work at wage w.

3.3. **Decomposing Rationing.** If there is labor rationing — supply exceeds demand at wage w — then not all workers who would like to work at wage w will find work. Individuals who are rationed may appear as unemployed or may turn to less-productive self-employment as a way to generate income — creating disguised unemployment or "forced entrepreneurs" (Singh et al., 1986). For these "forced entrepreneurs," self-employment earnings are below w, but above their reservation wage.

We test the extent to which the supply shock induces a subset of self-employed individuals to prefer wage employment at w, thus identifying themselves through revealed preference as "forced entrepreneurs." We predict that under rationing, a negative supply shock will lead to a reduction in worker-days in self-employment for workers who remain in the labor force (i.e. those who are not hired by us to work in external jobs).

4. Experiment: Design and Implementation

4.1. **Experimental Labor Supply Shocks.** We engineer *transitory* aggregate supply shocks in our study villages. In doing so, we exploit an opportunity to recruit casual male workers for full-time employment in low-skill manufacturing jobs for 2-4 weeks. The work takes place in external job sites that are within daily commuting distance from the villages.[13] These contract jobs are attractive to workers — the daily wage paid is weakly higher than the prevailing market wage for casual labor, and there are positive compensating differentials as the work takes place indoors and is not very physically demanding.

Several days prior to the first day of work, jobs at the external job sites are advertised in villages through the distribution of flyers, village meetings, and door-to-door visits. During this time, male workers interested in the job are encouraged to sign up. Hired workers are then drawn randomly from this subset of the village labor force that signs up for the job.

[13]We leverage two separate field projects (Breza et al., 2018; Kaur et al., 2019) that involve hiring workers full-time for low-skill manufacturing jobs. See Breza et al. (2018) for a full description of the employment set up at these factories.

We randomize recruitment at the village (i.e. local labor market) level, so that in treatment villages we hire up to 60% of the male workers who sign up, and in control villages we hire 1-5 male workers who sign up. We thus generate a large hiring shock in treatment villages, and a negligible hiring shock in control villages. By varying the intensity with which we recruit workers across villages, we generate exogenous variation in village-level residual labor supply. We use a matched-pair, stratified research design, so as to achieve balance by local region and time.

To test our predictions, we examine the impact of this external labor supply shock on wages and employment in the local village labor market. We conduct this test in different months of the year, which correspond to different levels of labor market demand and employment. We use employment rates in control villages as our proxy for underlying labor market slack — experimental rounds with above-median employment rates are classified as falling under peak periods of the year, while rounds with below-median employment rates are classified as falling under lean periods of the year.[14]

It is worth noting that our experiment only has power to detect rationing if the village labor market is closed to some extent, so that the removal of workers leads to employment spillovers within the treatment village. If this is not true, our supply shock would appear to be smaller, and it would be harder for us to find employment spillovers.

4.2. **Analysis Samples.** Figure 2 summarizes the analysis samples across control villages (panel A) and treatment villages (panel B). Recall that among workers who sign up for the job, a random subset of them are offered employment at the external factories. The grey shaded areas denote workers who sign up but are *not* offered jobs — these workers remain in the village and constitute our intent-to-treat sample. We refer to these workers hereafter as the spillover group. Note that this group of workers is larger in panel A, since only 1-5 workers are offered jobs in control villages.

To test predictions P1 and L1-L2, we examine how the labor supply shock impacts employment and wages for workers in the spillover sample across treatment and control villages. Given that this group is directly comparable to workers who were removed from the village, we would expect to find the largest spillovers onto them. To test predictions P2 and L3, we examine how the labor supply shock impacts employment for the full village labor force (i.e. all workers who sign up for the external jobs,

[14]We limited our experiment to ten months of the calendar year, omitting the two busiest months in the rice calendar — August (peak planting) and December (harvesting) — from the experiment for ethical reasons.

regardless of whether they are hired) as well as for the full village (i.e. all workers who sign up for the external jobs, as well as workers who do not sign up for the external jobs).

4.3. **Estimation Strategy.** To examine how the experimental hiring shock impacts employment and wages, we first report simple comparisons of outcomes in treatment and control villages, separately for peak and lean months. Our base specification includes round fixed effects, with standard errors clustered at the village level:

$$y_{itvr} = \alpha + \beta HiringShock_v + \gamma HiringShock_v * Peak_r + \rho_r + \epsilon_{itvr} \tag{1}$$

where y_{itvr} is an outcome for worker i on day t in village v and experimental round r. $HiringShock_v$ is an indicator for treatment villages, and $Peak_r$ is an indicator for experimental rounds conducted in peak months. To construct the $Peak_r$ indicator, we calculate the base employment rate in all control villages, and then take averages across experimental rounds to calculate monthly employment rates. Experimental rounds conducted in months with above-median employment rates are classified as falling under peak periods of the year.

We also report specifications that include baseline worker-level mean wage employment and daily total wage levels $\bar{X}^0_{ivr}$ in order to increase precision:

$$y_{itvr} = \alpha + \beta HiringShock_v + \gamma HiringShock_v * Peak_r + \rho_r + \bar{X}^0_{ivr} + \epsilon_{itvr} \tag{2}$$

To test predictions P1 and L1, we estimate Equations 1 and 2 using the spillover sample (i.e. workers who sign up for external jobs but are not offered employment), with y_{ivr} capturing local wages. Prediction P1 hypothesizes that $\beta_w + \gamma_w > 0$, while prediction L1 hypothesizes that $\beta_w = 0$. To test prediction L2, we estimate Equations 1 and 2 using the spillover sample, with y_{itvr} capturing wage employment. Prediction L2 hypothesizes that $\beta_e > 0$. To test predictions P2 and L3, we estimate Equations 1 and 2 using all workers who sign up for the job, as well as the full village labor force (i.e. all workers who sign up for the job and all workers who do not sign up), with y_{itvr} capturing wage employment. Prediction P2 hypothesizes that $\beta_E + \gamma_E < 0$, while prediction L3 hypothesizes that $\beta_E = 0$.

Regressions using the spillover sample are unweighted, while those using all workers who sign up for the job and the full village labor force are weighted by inverse sampling probabilities in order to be representative of the village labor market.

5. Data

We survey all workers who sign up for the job as well as a random sample of workers in the village who do not sign up for the job. We conduct three waves of surveys: at baseline (immediately before workers are hired at the external factories), at endline (during the third week of the month-long hiring shock), and at post-intervention (two weeks after the end of the hiring shock, after all workers are back in the village labor force). In each survey, we collect detailed daily recall data about wages: cash wages, details of in-kind payments (e.g. tea, meals etc., and cash value of in-kind payments), whether the worker was paid on time, etc. In addition, we collect detailed daily recall data about employment (activity, length of breaks, hours worked, location) and self-reports of involuntary unemployment. This provides us with the core data needed to test the predictions outlined in Section 3.2.

Our sample covers 60 villages (labor markets) across 5 districts in Odisha, India. We used a matched-pair randomization design, so we have 30 treatment and 30 control villages. 43% of the experimental rounds were conducted in lean months, and the remaining 57% in peak months. We have survey data for 2,379 workers in total.

Table 1 presents baseline characteristics for workers in the spillover sample, as well as all surveyed workers in the village i.e. workers who signed up for the job and a random sample of workers who did not sign up. Columns 1 and 2 present sample means and standard deviations for a series of characteristics — occupational status, landholdings, ownership of household businesses, wage and self-employment, and wages — for the spillover sample in control and treatment villages respectively. Column 3 presents the p-value for an F-test of the equality of means across the two groups. 78% of respondents in the spillover sample report casual laborer as their primary occupation. This reinforces that workers who express interest in the external jobs in low-skilled manufacturing are part of the casual labor force. 37% of respondents do not own any land. Employment levels are relatively low — on average, respondents are wage-employed for 18% of the recall period, and are self-employed for 10% of the recall period. The treatment and control villages appear to be well-balanced overall. Only the coefficient on self-employment is significantly different across the two groups, at the 10% level.

Columns 4 and 5 of Table 1 present sample means and standard deviations for the same series of characteristics for all surveyed workers in the village in control and treatment villages respectively. Column 6 presents the p-value for an F-test of the equality of means across the two groups. 72% of respondents in the full village sample report casual laborer as their primary occupation. This suggests that the workers who did not sign up for the external jobs in low-skilled manufacturing are less likely to be part of the casual labor force. 34% of respondents do not own any land. Employment levels for this full village sample are similar to the spillover sample, as described above.

6. Results

6.1. **Size of the Shock.** There is substantial interest among workers in the external jobs. On average, 50% of male workers in the village labor force sign up for the job. Up to 60% of workers who sign up in treatment villages are offered the job, while 1-5 workers who sign up in control villages are offered the job.

Figure 3 summarizes the size of the hiring shock in treatment villages, measured as the number of workers hired scaled by the size of the labor force of casual male workers in the village. On average, 24% of the male labor force in treatment villages is hired in the external factories. In one village, take up of the external jobs is zero as harvesting began early. For the remaining villages, the size of the shock ranges from 15-35%. Given that the number of workers hired from each treatment village is similar across experimental rounds, the variation in shock size is driven primarily by variation in the size of the male labor force across villages. Further, the hiring shock is slightly smaller in peak months relative to lean months, though this difference is not significant.

6.2. **Wages.** We study the average impact of the hiring shock on local wages separately for lean and peak months. For all worker-days in the recall period where the worker reports hired employment for a daily wage, we construct two wage measures: (i) cash wages; and (ii) total wages, which is the sum of cash wages and the monetary value of all in-kind wages (e.g. tea, lunch etc.). Figure 4 compares the distributions of total wages for treatment and control villages, limiting the sample to lean season observations (panel A) and to peak season observations (panel B). We cannot reject that the wage distributions in treatment and control villages are equal in the lean months (p-value from a Kolmogorov-Smirnov test is 0.370). In contrast, the wage distribution for treatment villages is shifted to the right relative to the control villages in the peak months (p-value < 0.001), indicating a rise in equilibrium wages.[15]

[15] Appendix Figure 2 compares the distribution of *cash* wages for treatment and control villages, limiting the sample to lean season observations (panel A) and to peak season observations (panel B).

Estimates of Equations 1 and 2 on winsorized wages for the spillover sample are presented in the first three columns of Table 2.[16] Consistent with prediction L1, we find no evidence that wages in the village change in response to an external hiring shock during lean months. However, during "peak" months — the 6 months of the year when baseline employment levels are above median levels — the hiring shock raises equilibrium wages by 5.3% (p-value 0.039) on average, consistent with prediction P1.

In Appendix Table 1, we present estimates of Equation 2 on the full village labor force (i.e. workers who signed up and workers who did not sign up for the external jobs). In Column 2, we find that the predictions hold with the full village sample — there is no detectable change in equilibrium wages in treatment villages in lean months, and an increase in equilibrium wages in treatment villages in peak months. In Column 3, we further interact Equation 2 with an indicator for whether the worker signed up for the external job. We find no differential treatment effect by sign-up status — equilibrium wages in treatment villages increase for both sign-ups (p-value 0.009) and non sign-ups (p-value 0.033) in peak months.

6.3. **Individual-Level Employment Spillovers.** Next, we examine the spillover effects of the hiring shock on employment for individuals remaining in the village. For all worker-days in the recall period, we construct two employment measures: (i) hired employment in the casual labor market (agriculture or non-agriculture); and (ii) hired employment in the casual labor market for a wage.

Estimates of Equations 1 and 2 on employment for the spillover sample are presented in the first three columns of Table 3. Consistent with Prediction L2, we find positive employment spillover effects in response to an external hiring shock in lean months. Specifically, the likelihood of hired wage employment increases by 5.3 percentage points (p-value 0.011) on a base rate of wage employment of 0.145, which implies a 36% increase in employment among workers who remain in the village. This is consistent with our prediction that workers who were previously rationed fill in job slots when their peers are "removed" from the labor market. In contrast, we cannot reject that there are no employment spillovers onto the remaining workers in the peak months (p-value 0.506).

In Column 4 of Tables 2 and 3, instead of using a binary peak indicator, we interact the hiring shock with a continuous measure for labor market demand — the (standardized) employment rate, as measured in control villages for each experimental round. Our

[16] Appendix Table 2 presents similar estimates on non-winsorized wages.

results are robust to this alternate specification — for every one standard deviation increase in the village employment rate, the hiring shock raises equilibrium wages by 0.5 log points and reduces hired wage employment by 68.8 percentage points, on average.

6.4. **Aggregate Employment and Crowd-Out.** Finally, we study the average impact of the hiring shock on aggregate employment levels in the village. To do this, we examine the effects of the shock on hired wage employment (excluding the external jobs generated at the factories) for two groups: (i) all workers who signed up for the external jobs; and (ii) all workers in the village — this consists of those who signed up for the external jobs, as well as those who did not. Estimates of Equation 2 on employment for these two groups are presented in Table 4.

Consistent with prediction L3, there is no significant change in local aggregate employment in response to an external hiring shock in lean months (p-value 0.309). This follows directly from the results above — since wages and local labor demand remain unchanged, rationed workers fill up the job slots, leading to the same level of aggregate employment. In contrast, consistent with prediction P2, an external hiring shock leads to a decline in aggregate employment in peak months. Employment for workers in villages that experienced hiring shocks decreases by 3.4 percentage points (p-value 0.007) on a base rate of wage employment of 0.199, which implies a 17% decline in aggregate employment in the village.

In Appendix Table 3, we run village-day level regressions using the hiring shock as an instrument for employment in the external factories. Column 1 presents the reduced-form result of the hiring shock on village employment, constructed by adding up individual employment across all workers in the village. Consistent with the worker-day level regression results in Table 4, we find no change in local aggregate employment in response to the hiring shock in lean months, and a significant decline in peak months (p-value 0.051). The IV estimates in Column 3 suggest that every day of work that is created in the external factories crowds out 0.172 days of private labor market employment (p-value = 0.0351) in the peak months. In contrast, the estimate for lean months is imprecisely estimated (p-value = 0.417), which implies that generating jobs for up to 35% of workers in the village generates no crowd-out in the private labor market in the lean season.

6.5. **Elasticities.** One concern is that we might be underpowered to detect wage increases in the lean season. To rule this out, we compute the implied labor supply elasticity from our wage and employment spillover results in the lean months. We take the right hand side of the 95% confidence interval of the wage point estimate, 0.022, and the 95% confidence interval of the employment spillover results in the lean season [.012, 0.093], which corresponds to a percentage increase of [8.6%, 64.3%]. The labor supply elasticity required to induce this employment response for the individual worker remaining in village is in the range of 3.9 - 29.2 — this is implausibly high, and is substantively larger than the labor supply elasticity estimates derived by Breza et al. (2019) among casual daily wage workers in the same setting.

6.6. **Decomposing Rationing.** We decompose rationing into involuntary unemployment and disguised unemployment (self-employed workers who would prefer wage labor) by first testing the extent to which the hiring shock induces workers to prefer wage employment over self-employment. A large body of work in development has highlighted that rationed individuals may turn to less-productive self-employment as a way to generate income — creating "disguised unemployment" or "underemployment" (e.g., Singh et al., 1986; Benjamin, 1992; LaFave and Thomas, 2016).

We construct three self-employment measures: (i) total self-employment; (ii) self-employment in non-agriculture; and (iii) self-employment in agriculture. Estimates of Equations 1 and 2 on these three measures for the spillover sample are presented in Table 5. We find that the external hiring shock leads to a 3.3 percentage points decline in total self-employment in lean months (Column 1), on a base of 0.13. This reduction in self-employment accounts for an estimated 62% of the employment spillovers. Workers thus reveal themselves as "forced entrepreneurs" by switching from self-employment to wage employment when jobs are more easily available in their village. In Column 4, we explore heterogeneity in treatment effects by per capita landholdings; we find that the reduction in self-employment is concentrated among workers in households with below median levels of per capita landholdings. Consistent with the literature on separation failures described above, this suggests that the extent of "disguised unemployment" is particularly large for this group of workers.

We next examine the effect of a hiring shock on an indicator for any work activity on that day in Column 1 of Table 6. We find no change in the overall reported work status as a result of the hiring shocks in lean months. This is consistent with disguised unemployment — workers who are rationed out of the wage labor market remain engaged in some form of work activity, such that differences in work status across treatment and control villages as a result of the hiring shocks in lean months appear insignificant.

Further, we examine two sets of self-reported measures of involuntary unemployment in Columns 2 and 3 of Table 6. The first is the traditional measure used in surveys, which lists "would have liked to work but was unable to find any" as one of the options for the activity for that day, along with hired employment or self-employment. The second is an alternate measure that we propose, which asks workers to state whether they would have accepted a job at the prevailing wage that day over whatever else they had been doing (e.g., even if they were self-employed). If there is disguised unemployment, then the first measure may understate rationing, because it will be chosen only when the worker has reported no other activity.

Under the traditional measure (Column 2), we fail to find significant changes in involuntary unemployment as a result of the hiring shocks in lean months. This is unsurprising given the prevalence of disguised unemployment in this setting — workers who appear gainfully employed in self-employment are involuntarily rationed out of the wage labor market. Under the proposed alternate measure (Column 3), we find a 6.7 percentage points (p-value 0.014) decline in involuntary unemployment as a result of the hiring shocks in lean months. This decline corresponds closely to our employment spillover effects in Table 3, which suggests that this alternate measure approximates the magnitude of the revealed preference response.

7. Threats to Validity

There are several potential confounds that could give rise to (a subset of) predictions L1-L3 even if there is no rationing.

Perfectly elastic labor supply. If labor supply is perfectly elastic at the prevailing wage, this could generate our predictions in the absence of rationing. This alternate explanation would require that no workers are willing to accept employment at a wage below the prevailing wage. Appendix Figure 3 summarizes results from a labor supply elasticity estimation exercise which was conducted in similar villages in the same districts as our experimental sample. In this exercise, Breza et al. (2019) partner with agricultural employers in the villages to randomize individual wage offers to workers in the local labor market. When an employer offers a job to workers in his village at the prevailing wage, 26% of otherwise unemployed workers accept the job. At a 10% wage cut, this number drops to 18% – still well above zero. This suggests that labor supply in this setting is far from perfectly elastic.

Wealth effects. The external jobs created under the hiring shocks generate an infusion of wealth in treatment villages. A potential concern is that this may subsequently lead to an expansion of local labor demand, which could counteract the supply shift and subsequently generate no change in aggregate employment (prediction L3). However, if this alternate explanation is true, this should put even more upward pressure on wages, which would be inconsistent with prediction L1. We survey workers two weeks after the hiring shock ends, when all workers are back in the village. With a demand expansion from wealth infusion, we should expect to find continued employment increase. Under rationing, however, the village should return to the way it was prior to the transient hiring shock.

Estimates of Equation 2 on wages, hired wage employment and self-employment for the spillover sample two weeks after the shock ends are presented in Table 7. We find no significant change in wages and employment across treatment and control villages in the lean months, two weeks after the hiring shock ends. In contrast, we do find evidence for sustained increased wages and decreased employment in treatment villages in the peak months, consistent with a ratcheting effect. We also find possible sustained decreases in self-employment, which allows us to rule out inter-temporal substitution in own farm work.

Change in composition of workers. While a substantial share of workers in the village sign up for the external factory jobs, there may be some selection into this group. Note that our design allows for workers in the village to be heterogeneous in ability, and for some workers to be unqualified to work at the market wage rate. We also do not take a stance on the underlying labor allocation mechanism in the presence of rationing, for example, the most productive workers in the village are hired first.

One potential concern is that the quality of workers that are left behind in treatment villages is on average lower than in control villages. If this puts downward pressure on the wage, it could counteract the upward wage pressure from the supply shift, generating no change on average (prediction L1). However, for this alternate explanation to also generate prediction L3, the demand elasticity for these workers would need to be such that employers would still want to hire the exact same number of these workers at w. Further, even if the average quality of workers that are left behind does decline, our estimates would provide a lower bound for the level of rationing. By revealed preference, any worker that receives employment spillovers from the hiring shock must be sufficiently productive to be employed at the market wage rate.

8. Conclusion

Our estimates of rationing are specific to labor markets in rural Odisha, India, during lean months. Since there may be full employment during peak months, we cannot conclude that rationed workers can be completely removed from villages without any decline in agricultural productivity. Our results thus support the idea of "under-utilized labor" but are inconclusive on whether there is "surplus labor" (Lewis, 1954; Leibenstein, 1957).

The prevalence of rationing suggests that there are lean periods in the year where workers are not on their labor supply curve, and subsequently, wages do not play an allocative role. This has important implications for analyses of labor market policies — for example, in the estimation of general equilibrium effects of India's national workfare program, the National Rural Employment Guarantee Scheme (NREGS), that typically provides rural workers with employment during the agricultural lean season. The role of seasonality should therefore be taken seriously as an input in labor market analysis, and subsequently, in the formulation of policies.

Finally, while the magnitudes of our estimates are only relevant for our study context, the measurement problem of involuntary un-and under-employment and the revealed preference methodology we employ have much broader relevance in both poor and rich countries. We find that two different labor market paradigms are relevant in the Odisha context, depending on predictable fluctuations in labor market slack. This implies that the labor market can be fundamentally different in its functioning over the course of the year. Finding evidence for rationing in our context suggests taking seriously the idea that labor markets may not clear in other rural, developing country settings where employment rates are low for some parts of the year. This provides impetus and direction for expanded work on labor market frictions in poor countries.

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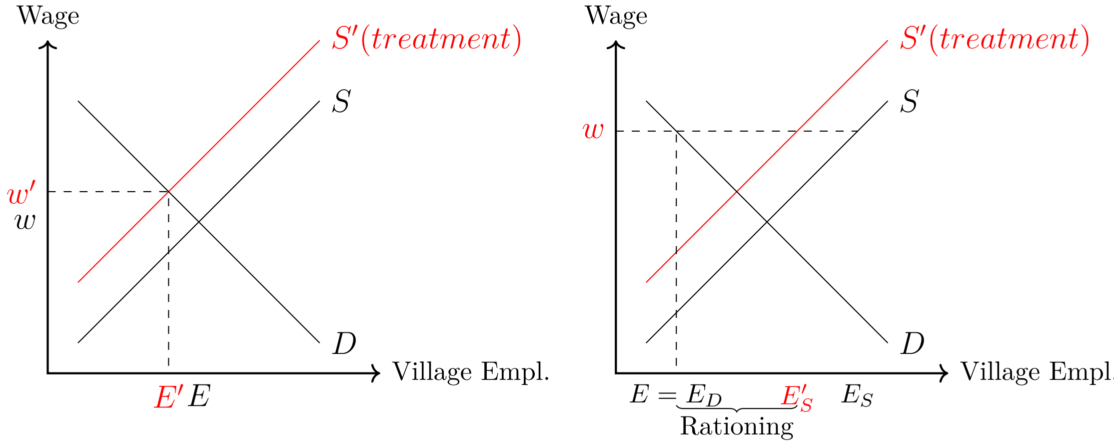
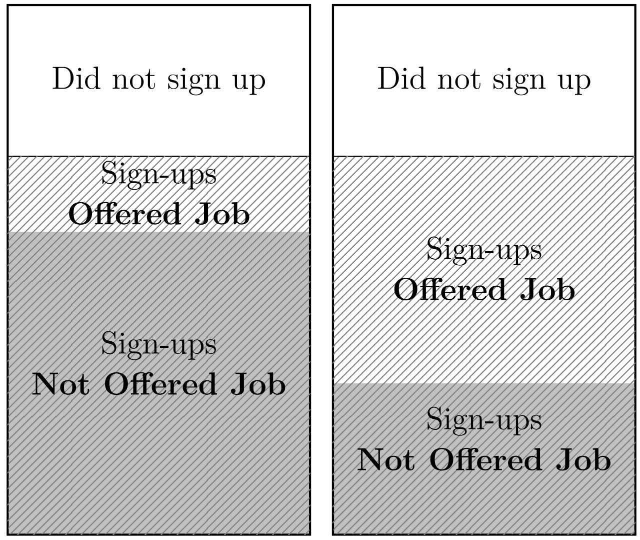
Figures



(A) H_0 : No Rationing ($E_D \geq E_S$) (B) H_1 : Rationing ($E_D < E_S$)

Figure 1. Effects of a Negative Labor Supply Shock

Note: Figure shows the effects of a negative supply shock on employment and wages under market clearing ($E_D \geq E_S$) in panel A, and under rationing ($E_D < E_S$) in panel B.



(A) No hiring shock

(B) Hiring shock

FIGURE 2. Analysis Samples

Note: Figure summarizes the analysis sample in control villages in panel A, and treatment villages in panel B. The grey shaded areas denote workers who signed up but were not offered employment at the external factories — this constitutes the spillover sample.

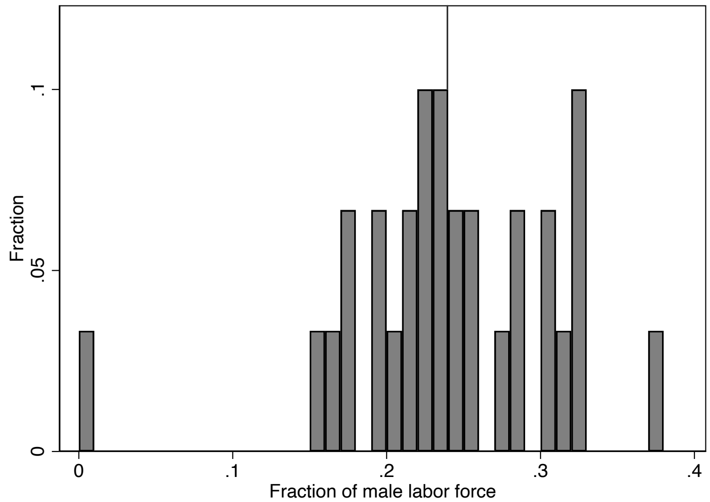
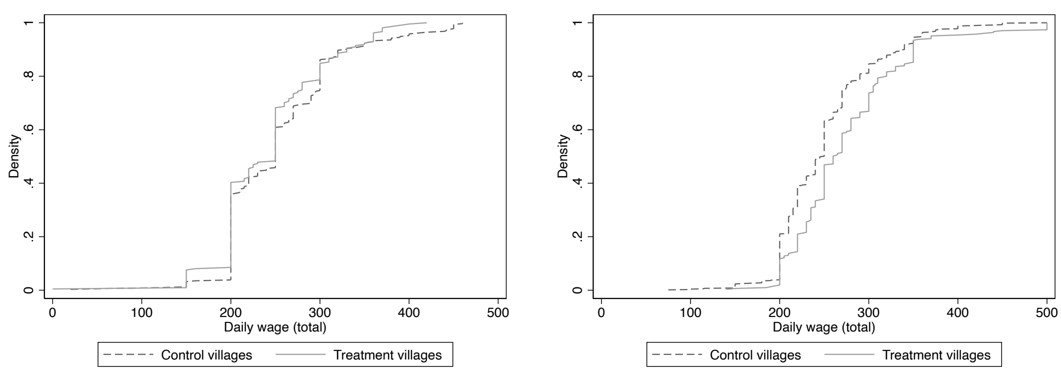


FIGURE 3. Size of the Experimental Hiring Shock

Note: Figure shows the size of the experimental hiring shock in treatment villages. This is measured as number of workers hired scaled by the size of the male labor force in the village. Mean = 0.24.



(A) Lean Season

(B) Peak Season

FIGURE 4. Wage Level Effects (Cash + In-Kind Wages)

Note: Figure compares the distribution of total wages for treatment and control villages, limiting the sample to lean season observations only in panel A, and peak season observations only in panel B. The p-value for the equality of distributions from a Kolmogorov-Smirnov test is 0.370 (panel A), and <0.001 (panel B).

Tables

Table 1. Baseline Characteristics

	Spillover Sample			All Workers		
	(1) No shock	(2) Shock	(3) P-val.	(4) No shock	(5) Shock	(6) P-val.
Occupation: Laborer	0.786	0.761	0.447	0.734	0.686	0.065
	(0.410)	(0.427)		(0.442)	(0.465)	
Landless	0.373	0.365	0.810	0.350	0.334	0.512
	(0.484)	(0.482)		(0.477)	(0.472)	
HH Members (age 12+)	3.965	4.032	0.545	3.931	3.921	0.876
	(1.543)	(1.561)		(1.557)	(1.511)	
Has HH business (non-agri)	0.137	0.150	0.627	0.151	0.190	0.043
	(0.344)	(0.357)		(0.358)	(0.393)	
Has HH business (agri)	0.860	0.859	0.963	0.870	0.852	0.259
	(0.347)	(0.348)		(0.337)	(0.355)	
Hired employment	0.221	0.241	0.439	0.197	0.229	0.028
	(0.318)	(0.346)		(0.302)	(0.320)	
Hired wage employment	0.176	0.190	0.557	0.155	0.167	0.306
	(0.287)	(0.313)		(0.268)	(0.278)	
Log wage (total)	5.485	5.545	0.206	5.519	5.559	0.348
	(0.388)	(0.357)		(0.429)	(0.513)	
Days worked in casual labor market (in past 14 days)	3.006	3.023	0.947	2.741	2.589	0.432
	(3.441)	(3.101)		(3.346)	(3.329)	
Self employment	0.114	0.082	0.073	0.126	0.114	0.440
	(0.243)	(0.199)		(0.261)	(0.250)	

Notes: We restrict to the spillover sample (workers who signed up for external jobs but were not offered employment) in Cols 1-3, and to all workers in Cols 4-6. Cols 1 and 2 present baseline means and standard deviations of characteristics for spillover workers in control and treatment villages respectively. Cols 4 and 5 present baseline means and standard deviations of characteristics for all workers in control and treatment villages respectively. Cols 3 and 6 present p-values from an F-test of the equality of means across the treatment and control villages. Observations are at the worker level. N=992 (spillover sample); N=2379 (all workers)

Table 2. Wage Effects

	(1)	(2)	(3)	(4)
Panel A: Levels	Cash wage	Total wage	Total wage	Total wage
Hiring shock	-5.447	-3.564	-5.488	-21.87*
	(4.363)	(4.489)	(4.088)	(11.822)
Hiring shock * Peak	18.36**	17.29**	18.94**	
	(7.701)	(8.257)	(7.649)	
Hiring Shock * Empl. Level				148.9**
				(59.757)
Test: Shock + Shock*Peak	0.0464	0.0525	0.0390	.
Control mean: lean	239.9	253.7	253.7	253.7
Control mean: peak	232.5	251.5	251.5	251.5
N (worker-days)	1545	1545	1545	1545
Panel B: Logs	Log cash wage	Log total wage	Log total wage	Log total wage
Hiring shock	-0.0202	-0.0111	-0.0179	-0.0684
	(0.021)	(0.022)	(0.020)	(0.055)
Hiring shock * Peak	0.0722**	0.0656**	0.0707**	
	(0.031)	(0.032)	(0.030)	
Hiring Shock * Empl. Level				0.500*
				(0.270)
Sample	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	No	Yes	Yes
Test: Shock + Shock*Peak	0.0252	0.0242	0.0198	.
Control mean: lean	5.458	5.500	5.500	5.500
Control mean: peak	5.428	5.504	5.504	5.504
N (worker-days)	1543	1544	1544	1544

Notes: Cols 1-4 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Total wage = cash + in-kind wages. We winsorize the top one percentile of the cash and total wage distributions. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

Table 3. Employment Spillovers

	(1) Hired employment	(2) Hired wage empl.	(3) Hired wage empl.	(4) Hired wage empl.
Hiring shock	0.0697***	0.0684***	0.0528**	0.137***
	(0.020)	(0.021)	(0.020)	(0.050)
Hiring shock * Peak	-0.0682**	-0.0737**	-0.0685**	
	(0.031)	(0.034)	(0.030)	
Hiring Shock * Empl. Level				-0.688**
				(0.273)
Sample	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	No	Yes	Yes
Test: Shock + Shock*Peak	0.949	0.840	0.506	.
Control mean: lean	0.153	0.145	0.145	0.145
Control mean: peak	0.269	0.216	0.216	0.216
N (worker-days)	8906	8906	8899	8899

Notes: Cols 1-4 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Hired employment = 1{worker hired that day}, and hired wage employment = 1{worker hired that day and paid a wage}. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

Table 4. Aggregate Employment

	(1) Hired wage empl.	(2) Hired wage empl.	(3) Hired wage empl.
Hiring shock	0.0528**	-0.0148	0.0220
	(0.020)	(0.016)	(0.021)
Hiring shock * Peak	-0.0685**	-0.0467**	-0.0561**
	(0.030)	(0.023)	(0.025)
Sample	Spillover	All Sign-Ups	All Workers
Baseline controls	Yes	Yes	Yes
Test: Shock + Shock*Peak	0.506	0.000251	0.00667
Control mean: lean	0.145	0.124	0.129
Control mean: peak	0.216	0.200	0.199
R-squared	0.117	0.106	0.109
N (worker-days)	8899	16613	21071

Notes: Col 1 restricts to only the spillover sample as a reference. Col 2 include all male workers in the village who signed up for the experimental job with appropriate weights. Col 3 includes all male workers in the village with appropriate weights. Hired wage employment = 1{worker hired that day and paid a wage}. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FE. Standard errors are clustered at the village level in parentheses.

Table 5. Self-Employment

	(1) Self employment	(2) Self empl: non-agri	(3) Self empl: agri	(4) Self empl: agri
Hiring shock	-0.0326*	-0.0368**	-0.0309	-0.0852***
	(0.019)	(0.014)	(0.023)	(0.030)
Hiring shock * Above Median Land Per Capita				0.0827*
				(0.047)
Hiring shock * Peak	0.00378	-0.0000130	0.0254	0.133***
	(0.027)	(0.020)	(0.028)	(0.038)
Hiring shock * Peak * Above Median Land Per Capita				-0.194***
				(0.058)
Sample	Spillover	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes	Yes
Test: Shock + Shock*Peak	0.130	0.0181	0.723	0.0484
Control mean: lean	0.139	0.0443	0.149	0.149
Control mean: peak	0.109	0.0441	0.0823	0.0823
N (worker-days)	8381	5007	7513	7513

Notes: Cols 1-4 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Each dependent variable is a binary indicator for whether worker reported each stated activity that day. Controls include worker-level indicators for any self-employment activities at baseline. We restrict the sample to experimental rounds with non-zero self-employment in control villages at endline. Regressions include round (strata) FE. Standard errors are clustered at the village level in parentheses.

Table 6. Measuring Involuntary Unemployment

	(1) Any work	(2) Invol unempl: trad	(3) Invol unempl: alt
Hiring shock	0.0188	-0.0387	-0.0667**
	(0.023)	(0.030)	(0.026)
Hiring shock * Peak	-0.0317	0.0315	0.0653*
	(0.028)	(0.039)	(0.039)
Sample	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes
Test: Shock + Shock*Peak	0.432	0.774	0.961
Control mean: lean	0.342	0.481	0.580
Control mean: peak	0.398	0.395	0.543
N (worker-days)	8899	8899	8899

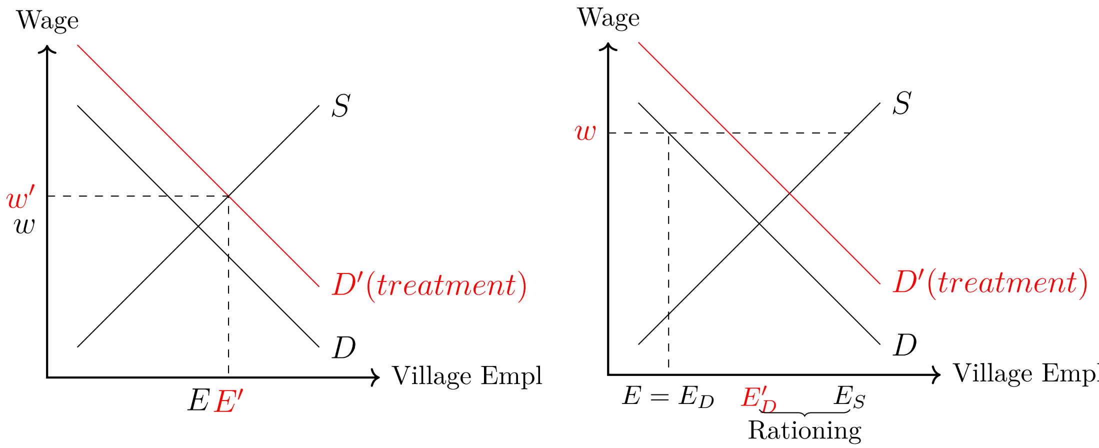
Notes: Cols 1-3 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Any work = 1{worker reports any work that day}. The dependent variable in Column 2 is a binary indicator for whether worker reported "would have liked to work but was unable to find any" as his activity status for that day. The dependent variable in Column 3 is a binary indicator for whether worker stated that they would have accepted a job at the prevailing wage that day over whatever else they had been doing. Controls include worker-level mean employment and wage levels at baseline, as well as indicators for any self-employment activities at baseline. Regressions include round (strata) FE. Standard errors are clustered at the village level in parentheses.

Table 7. Impacts 2 Weeks After End of Hiring Shock

	(1) Log total wage	(2) Hired wage employment	(3) Self employment
Hiring shock	-0.000950	-0.0103	-0.0144
	(0.034)	(0.027)	(0.015)
Hiring shock * Peak	0.0448	-0.0342	-0.0245
	(0.041)	(0.034)	(0.024)
Sample	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes
Test: Shock + Shock*Peak	0.0658	0.0320	0.0404
Control mean: lean	5.520	0.177	0.170
Control mean: peak	5.530	0.210	0.138
N (worker-days)	1328	7616	7623

Notes: Cols 1-3 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Hired wage employment = 1{worker hired that day and paid a wage}. Self employment = 1{worker self-employed that day}. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FE. Standard errors are clustered at the village level in parentheses.

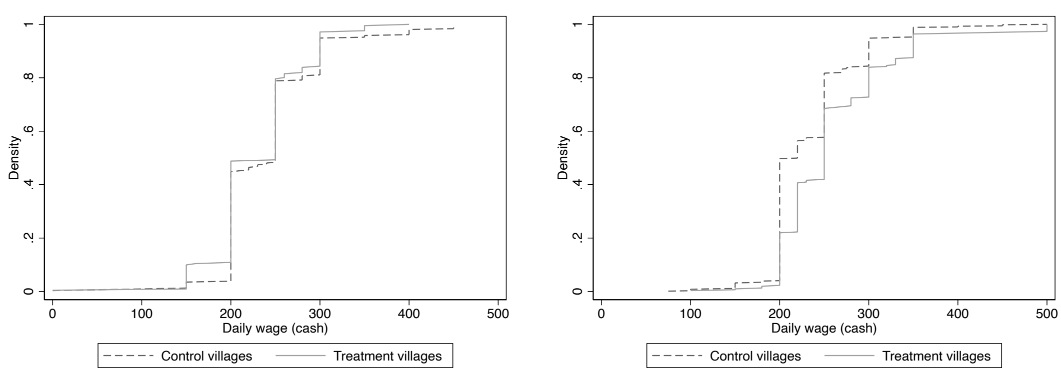
Appendix A. Appendix Figures



(A) H_0 : No Rationing ($E_D \geq E_S$)   (B) H_1 : Rationing ($E_D < E_S$)

Figure 1. Effects of a Negative Labor Demand Shock

Note: Figure shows the effects of a negative demand shock on employment and wages under market clearing ($E_D \geq E_S$) in panel (a), and under rationing ($E_D < E_S$) in panel (b).



(A) Lean Season   (B) Peak Season

Figure 2. Wage Level Effects (Cash Wages)

Note: Figure compares the distribution of cash wages for treatment and control villages, limiting the sample to lean season observations in panel (a), and to peak season observations in panel (b). The p-value for the equality of distributions from a Kolmogorov-Smirnov test is 0.587 (panel a) and <0.001 (panel b).

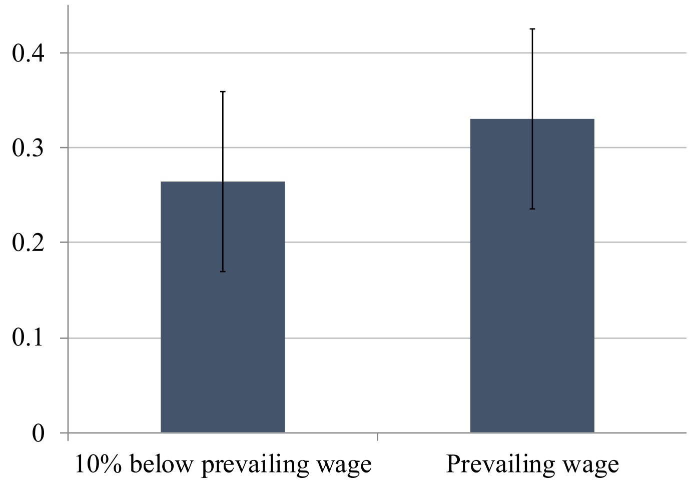


Figure 3. Job take-up: private wage offers

Note: Figure illustrates take up of a job offer at different wage rates among casual workers in villages similar to our study sample, in the same districts of Odisha, India. This data comes from a labor supply estimation exercise conducted by Breza et al. (2019).

Appendix B. Appendix Tables

Table 1. Wage Effects

	(1) Log total wage	(2) Log total wage	(3) Log total wage
Hiring shock	-0.0113	-0.0109	0.000226
	(0.022)	(0.032)	(0.036)
Hiring shock * Peak	0.0676**	0.0775**	0.0587
	(0.032)	(0.037)	(0.042)
Hiring shock * Didn't sign up			-0.0224
			(0.094)
Hiring shock * Peak * Didn't sign up			0.0299
			(0.104)
Sample	Spillover	All Workers	All Workers
Test: Shock + Shock*Peak	0.0227	0.000303	0.00873
Test: Shock + Shock*No Sign Up (NSU)	.	.	0.762
Test: Shock + Shock*Peak + Shock*NSU + Shock*Peak*NSU	.	.	0.0334
Test: Shock*No Sign Up	.	.	0.958
N (worker-days)	1544	2692	2692

Notes: Col 1 restricts to the spillover sample (workers who signed up for external jobs but were not offered employment). Cols 2-3 include all male workers in the village with appropriate weights. Total wage = cash + in-kind wages. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FEs. Standard errors clustered at the village level in parentheses.

Table 2. Wage Effects

Panel A: Levels	(1) Cash wage	(2) Total wage	(3) Total wage	(4) Total wage
Hiring shock	-5.447	-3.659	-5.701	-21.94*
	(4.363)	(4.492)	(4.072)	(11.808)
Hiring shock * Peak	19.22**	18.24**	20.05**	
	(7.868)	(8.457)	(7.789)	
Hiring Shock * Empl. Level				151.8**
				(59.929)
Test: Shock + Shock*Peak	0.0398	0.0465	0.0319	.
Control mean: lean	239.9	253.8	253.8	253.8
Control mean: peak	232.6	251.6	251.6	251.6
N (worker-days)	1545	1545	1545	1545

Panel B: Logs	Log cash wage	Log total wage	Log total wage	Log total wage
Hiring shock	-0.0202	-0.0113	-0.0184	-0.0686
	(0.021)	(0.022)	(0.020)	(0.055)
Hiring shock * Peak	0.0740**	0.0676**	0.0730**	
	(0.031)	(0.032)	(0.030)	
Hiring Shock * Empl. Level				0.506*
				(0.268)
Sample	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	No	Yes	Yes
Test: Shock + Shock*Peak	0.0232	0.0227	0.0176	.
Control mean: lean	5.458	5.500	5.500	5.500
Control mean: peak	5.428	5.504	5.504	5.504
N (worker-days)	1543	1544	1544	1544

Notes: Cols 1-4 restrict to the spillover sample (workers who signed up for external jobs but were not offered employment). Total wage = cash + in-kind wages. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

Table 3. Aggregate Employment Effects and Crowd-Out

	(1) Hired wage empl	(2) Hiring shock empl	(3) Hired wage empl
Hiring shock	0.0250	0.190***	
	(0.031)	(0.010)	
Hiring shock * Peak	-0.0584*	-0.000434	
	(0.032)	(0.019)	
Hiring shock empl			0.130
			(0.160)
Hiring shock empl * Peak			-0.302*
			(0.158)
Specification	RF	FS	IV
Baseline controls	Yes	Yes	Yes
Test: Shock + Shock*Peak	0.0510	1.26e-16	0.0351
N (village-days)	738	738	738

Notes: Cols 1-3 include all male workers in the village with appropriate weights. Controls include mean employment and wages at the village-day level at baseline. Regressions include round (strata) FEs. Standard errors clustered at the village level in parentheses.