

Labor Rationing[†]

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This paper measures excess labor supply in equilibrium. We induce hiring shocks—which employ 24 percent of the labor force in external month-long jobs—in Indian local labor markets. In peak months, wages increase instantaneously and local aggregate employment declines. In lean months, consistent with severe labor rationing, wages and aggregate employment are unchanged, with positive employment spillovers on remaining workers, indicating that over a quarter of labor supply is rationed. At least 24 percent of lean self-employment among casual workers occurs because they cannot find jobs. Consequently, traditional survey approaches mismeasure labor market slack. Rationing has broad implications for labor market analysis. (JEL E24, J22, J23, J31, J64, O15, R23)

[D]istinguishing elements of voluntariness from elements of involuntariness in the unemployment problem is a hopeless endeavor.

(Fellner 1976)

In developing countries, wage employment rates among rural workers often hover around 50 percent. For example, Indian landless prime age males—whose primary source of earnings is wage labor—work 45.7 percent of the time (National Sample Survey 2009). In Bangladesh, wage employment rates for landless males are about 55 percent in lean months (Akram, Chowdhury, and Mobarak 2017). In Sub-Saharan Africa, these rates are typically even lower (Beegle and Christiaensen 2019).

The interpretation of this empirical regularity is unclear, and has long been a source of debate. Research going back to Lewis (1954) argues that low employment reflects extremely high involuntary unemployment, indicating large distortions. However, other work has shown that these labor markets adjust rapidly to changing market conditions (e.g., Rosenzweig 1988). Thus, low wage work might

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instead be the outcome of reasonably well-functioning labor markets, where workers are simply choosing other activities such as self-employment. These two possibilities have drastically different implications for the labor market equilibrium. Moreover, while the development labor literature has documented frictions—giving rise to separation failures, labor misallocation, and downward wage rigidity (e.g., LaFave and Thomas 2016; Bryan, Chowdhury, and Mobarak 2014; Kaur 2019)—knowing that these exist does not tell us about the extent of aggregate distortions in equilibrium. In other words, existing work cannot explain how one should interpret observed employment patterns. Specifically, whether there is a large amount of labor rationing in this setting still remains unknown.

This paper empirically assesses the extent to which labor supply exceeds labor demand in equilibrium. For concreteness, we define a worker as rationed if (i) she would prefer wage employment at the current market wage over what she is doing (i.e., the worker is not on her labor supply curve), and (ii) the worker is employable at that wage (i.e., from the employer's perspective, her marginal product is weakly above the current wage). A rationed worker may be involuntarily unemployed, or engaged in another activity such as self-employment.

Determining the amount of rationing presents challenges under current approaches. To measure involuntary unemployment, economists use survey self-reports—whether an individual was looking for a job but could not find one. However, the validity of this approach has been questioned, and remains unknown (e.g., Bound, Brown, and Mathiowetz 2001; Taylor 2008). These challenges are exacerbated in developing countries, where self-employment is prevalent and can absorb workers who cannot find jobs. While previous work discusses the presence of such “disguised” or “hidden” unemployment, we currently have no estimates of whether a sizable fraction of self-employment is due to rationing in the wage labor market.

We tackle these fundamental measurement problems by developing a simple revealed preference approach to test for excess labor supply (i.e., labor rationing). We induce transitory hiring shocks in Indian local labor markets—giving jobs to on average 24 percent of the labor force of casual male workers in external jobsites for two to four weeks. This shock substantively reduces how many workers remain in the local economy, without changing local labor demand.¹ We then use the local labor market response to learn about the equilibrium that existed in the absence of our shock. We take seriously the idea that seasonality in labor demand may be consequential for labor market functioning (Leibenstein 1957), with relevance for understanding seasonal fluctuations in employment levels (Bryan, Chowdhury, and Mobarak 2014; Fink, Jack, and Masiye 2020). Consequently, we undertake this exercise across the year—enabling us to test for excess supply separately across lean and peak months.

This approach uncovers labor market functioning without direct intervention on the participants of interest. While we exogenously generate shocks, the outcomes are driven completely by the response of existing employers and workers who never interface with the external jobsites. We use the phrase “local labor market” to denote

¹ We discuss the possibility of multiplier effects from demand below.

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 percent of the labor force and more than 25 percent of worker-days are rationed—then we would expect the following local labor market impacts: (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 less competition for jobs. This constitutes a revealed preference test of rationing driven by a failure of wage adjustment. Specifically, workers reveal they prefer jobs at the market wage over their previous activity (e.g., unemployment or self-employment), and employers reveal the worker is qualified for a job at the market wage. If these predictions hold, the size of our hiring shock is a lower bound on the level of excess labor supply in the economy.

This design diagnoses rationing without assumptions about the equilibrium in the absence of rationing—for example, whether it is fully competitive or subject to monopsony or some other friction. We view this as a strength of our approach, and do not make direct claims regarding potential other frictions.

The setting for our test is rural labor markets in Odisha, India, which exhibit many features of developing country village economies, including low wage employment and high seasonality. 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.² We randomize the intensity of hiring across villages: in treated villages, we provide jobs to on average 24 percent of the labor force of male workers; in control villages, we hire one to five workers only. In each case, we randomly choose which workers are “removed” from among the signups.

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—when employment is above median—the equilibrium wage rises by 5.0 percent. In addition, local aggregate employment declines by 4.2 percentage points or 22 percent. In the peak season, each day of work created in external jobsites generates 0.718 days of new work for laborers in the economy overall (i.e., crowds out 0.282 days in the local labor market). 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 can quickly reflect changes in market conditions (Rosenzweig 1988).

In sharp contrast, effects during “lean” months—when employment is below median—match predictions under severe labor rationing. Removing a quarter of the labor force has no effect on wages or aggregate employment (predictions (i) and (ii) above). In other words, creating external jobs for a quarter of workers 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

² We offer relatively high wages for desirable manufacturing jobs. This is beneficial for our design, enabling us to draw (random) workers from across the skill distribution in the village.

goes up by 5.4 percentage points (38 percent). This follows 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 others have become employed outside the village. External employment therefore helps not only those who received our worksite jobs, but also those who did not. Overall, our results indicate that on average, at least a quarter of male labor supply is rationed out of the wage labor market in lean months.

These lean season spillover effects are transitory, lasting only as long as the hiring shocks do. This is what one would expect if the response to our hiring shock was due to excess labor supply. It also helps rule out, for example, a wealth effect explanation for our results.³

A large portion of excess labor supply is disguised as self-employment. In lean months, we see a 3.4 percentage point, or 24 percent, decline in self-employment among those who sign up for worksite jobs. By revealed preference, these business owners prefer wage labor at the prevailing wage to working in their businesses. This implies that in our sample, in the lean season, at least 24 percent of self-employment stems from workers being rationed out of wage labor. Among farm households, these effects are entirely concentrated among those with below median per capita landholdings, the group we would expect to be most affected by a ration in the labor market. Across the village as a whole, small landholders reduce labor supply to their farms by 10.5 percentage points (64 percent), indicating that on small farms in the village, more than half of self-employment in lean months is driven by rationing. Like the employment spillovers, these effects are also transitory. Once the shock ends, self-employment levels between treatment and control villages are indistinguishable—ruling out the concern that business owners simply intertemporally substitute by increasing self-employment in the future.

Finally, we examine traditional survey-based unemployment measures. Despite the sizable impacts on wage employment, overall work status does not move in lean months, precisely due to disguised unemployment. In addition, if we mimic the approach for measuring involuntary unemployment used in government surveys (including India's National Sample Survey), we would conclude that our shocks had no impact on unemployment in lean months. We evaluate an alternate survey measure, and find its movement matches the revealed preference magnitudes. However, our findings suggest that, in our setting, traditional survey questions will mismeasure the extent to which individuals are unable to find wage work.

We argue that other potential mechanisms through which the shocks could affect wages or employment—such as a local demand expansion from increased wealth, or a change in worker composition—cannot explain our pattern of results. We also offer direct tests for such potential confounds, and rule out perfectly elastic labor supply. In addition, we evaluate potential microfoundations for excess labor supply—such as efficiency wages, dynamic contracting, implicit insurance, and worker collective action—in light of our findings.

Overall, the patterns we document are consistent with the hypothesis of “under-utilized labor” proposed by Leibenstein (1957). If the labor market, and

³ Also consistent with rationing, the wage increase in peak months persists after the shock ends, leading to lower employment—in line with a ratcheting effect from downward wage rigidity (Kaur 2019).

particularly the wage, does not adjust fully to seasonal reductions in labor demand, this can generate rationing in lean periods. While our estimates are of course specific to our particular context, the pattern of differences in labor market functioning across peak and lean times is likely more general. Similar dynamics plausibly prevail in many rural, developing country settings.

Our results have implications for labor market policies, such as workfare—implying that crowd-out will depend crucially on the level of slack. In a suggestive exercise, we examine the phased-in roll-out of India’s workfare program, the National Rural Employment Guarantee Act (NREGA) (Imbert and Papp 2015). When slack is higher (i.e., baseline district-month employment is lower), NREGA has no detectable impact on wages or private employment. However, when the labor market is tighter, wages rise and agricultural employment falls by almost 20 percent, indicating substantial crowd-out. These patterns match our experimental findings. Note that we don’t rule out the possibility of lean season wage increases more generally: if a shock is larger than the number of rationed workers or changes reservation wages, wages could rise even in lean times.

This paper provides novel evidence on the functioning of labor markets in poor countries. It offers the first direct evidence that labor supply substantively exceeds labor demand in equilibrium. The earliest work in development, such as the surplus labor hypothesis, was premised on the view that rural economies have large slack (Lewis 1954). The empirical relevance of this view has been unclear, especially in modern times. We document considerable slack, indicating that at least a quarter of labor supply is rationed during lean months. These findings suggest that moving rural workers to other sectors—where their labor may be more readily absorbed—could increase aggregate output not simply due to total factor productivity (TFP) differences between sectors (Gollin, Lagakos, and Waugh 2014; McMillan and Rodrik 2011), but also by enabling rationed workers to be employed.⁴

We also provide the first estimates of how much of self-employment is actually “disguised unemployment” (i.e., “forced entrepreneurship”). A striking share of self-employment in our sample (24 percent on average and 64 percent among smallholder farms) would not occur if business owners could find work at the prevailing wage in lean months. This helps further our understanding of a prominent empirical fact: why self-employment is so high in poor countries (e.g., Banerjee and Duflo 2007; de Mel, McKenzie, and Woodruff 2010).⁵

Relatedly, we advance the literature on labor market frictions in poor countries. A large body of work has examined separation failures (Singh, Squire, and Strauss 1986; Benjamin 1992; Fafchamps 1993; Udry 1996; LaFave and Thomas 2016; Dillon, Brummund, and Mwabu 2019; Jones et al. 2020; LaFave, Peet, and Thomas 2020). These studies do not take a stance on the exact distortion that generates the

⁴Our design relates to tests of surplus labor (Schultz 1964, Sen 1967, Donaldson and Keniston 2016). While other work has documented low employment levels during lean months, our study is the first to link this to rationing, rather than simply market clearing with low employment demand. Note that the presence of rationing complicates the interpretation of analyses that rely on wage differences across sectors to infer TFP differences.

⁵Adhvaryu, Kala, and Nyshadham (2019) document that coffee farmers increase nonagricultural self-employment when farming becomes less profitable, though this does not require any market frictions such as rationing.

failure—rationing or some other friction.⁶ They also do not quantify how many firms are affected.⁷ Our findings complement this work by tracing the first direct link from labor rationing to separation failures, and showing that rationing increases self-employment on the majority of smallholder farms. More broadly, while previous studies have documented frictions—including separation failures, wage rigidity (Kaur 2019), and worker collusion (Breza, Kaur, and Krishnaswamy 2019)—they are not designed to tell us whether the magnitudes are ultimately consequential for labor market functioning. Our design is built solely to address this question, offering the first evidence that the labor market is, at times, severely distorted.

At the same time, our finding of instantaneous adjustment in peak months supports the view that spot labor markets can be quite flexible—consistent with studies showing robust labor market adjustment to shocks (e.g., Rosenzweig 1988; Jayachandran 2006; Imbert and Papp 2015; Donaldson and Keniston 2016; Akram, Chowdhury, and Mobarak 2017; Breza and Kinnan 2018; Muralidharan, Niehaus, and Sukhtankar 2020; Egger et al. 2019). Our results therefore reconcile these studies with those above, providing a more complete characterization of labor market functioning.

Our methodological approach also contributes to the literature on measuring unemployment and labor market slack. To date, economists have measured involuntary unemployment using survey self-reports, whose reliability is difficult to ascertain (e.g., Bound, Brown, and Mathiowetz 2001; Taylor 2008; Card 2011; Faberman and Rajan 2020). We stack survey responses against our revealed preference findings, documenting the unreliability of such measures. We highlight this can be particularly problematic when rationed workers are likely to switch to other activities, such as self-employment or gig economy jobs. In addition, we build on previous work, which has documented heterogeneity in employment crowd-out from shocks (e.g., Crépon et al. 2013, Gautier et al. 2018). Through precise information on the shock size and by sampling outcomes for the entire labor force, we provide the first revealed preference estimates for the extent of rationing in the economy in any setting.

Finally, our research design provides a cleaner test of excess labor supply relative to other potential approaches, such as offering jobs to the unemployed (e.g., Breza, Kaur, and Krishnaswamy 2019). For example, this would lower hiring costs by solving the search and matching problem. Consequently, in the absence of external intervention, a worker who accepts a job may not have preferred it, or an employer may not consider the worker as qualified. In addition, our design enables a fuller picture of excess supply and its implications, including the essential role of seasonality, effects on self-employment, and private-sector crowd-out—which requires examining the interaction of labor supply and demand in equilibrium.

⁶See Behrman (1999) for a review. Some studies examine specific microfoundations: for example, Pitt and Rosenzweig (1986) test for substitutability of own versus hired labor; Foster and Rosenzweig (2017) analyze the implications of transaction costs in hiring; and Kaur (2019) examines the link between labor rationing and own farm work.

⁷For example, the comparative static of whether household size affects farm labor use tests for a failure of separability, but does not shed light on whether the associated labor supply distortion affects 1 percent or 50 percent of workers.

Our findings have broad implications for labor market analysis and policy. First, the absence of lean-season market clearing implies that the wage does not always play an allocative role. Thus, analyses that use wages to infer the marginal product of labor, for example, may be misleading. Second, our findings are relevant for predicting the effects of labor market programs, such as workfare (Imbert and Papp 2015; Beegle, Galasso, and Goldberg 2017; Bertrand et al. 2017; Zimmermann 2020; Muralidharan, Niehaus, and Sukhtankar 2020). Specifically, heterogeneity in slack may help explain why some programs have substantive labor market impacts while others have virtually none.⁸ Finally, our finding of large quantities of “forced entrepreneurship” can help explain why policies that direct resources to small firms have only had limited success for the average firm (e.g., McKenzie and Woodruff 2014; Banerjee, Karlan, and Zinman, 2015).

The remainder of the paper is organized as follows. Section I describes the context. Section II outlines our hypotheses and predictions. Section III details the implementation and Section IV describes the data. We present the results in Sections V and VI. Section VII discusses microfoundations and potential threats to validity. Section VIII offers suggestive extensions, such as heterogeneity in the impact of India’s workfare program. Section IX concludes.

I. Context

Our field experiment takes place in villages across five districts in rural Odisha, India. Markets for casual daily labor are extremely active in our setting specifically, and constitute an employment channel for hundreds of millions of workers in India more generally (National Sample Survey 2010). The village constitutes a prominent boundary for the casual labor market: daily wage workers typically find jobs in both agriculture and nonagriculture within or close to their own village. This local nature of hiring is a necessary condition for our experimental hiring shocks to have a meaningful impact on the aggregate labor supply facing firms and on the labor market equilibrium.

The casual labor markets in the study areas are characterized by the same decentralization and informality as much of India (e.g., Rosenzweig 1988; Drèze, Leruth, and Mukherjee 1986). Contracting is usually bilaterally arranged between employers and workers a few days before the start of work. Moreover, the typical job lasts one to three days. This short duration of employment contracts creates the potential for transitory labor supply shocks to quickly impact wages. Labor demand is typically infrequent and variable. As in much of India, agricultural labor demand is seasonal, and both agricultural and nonagricultural jobs tend to be task-based (Foster and Rosenzweig 2017). For example, an employer might hire a different group of five workers to weed his paddy fields once or twice in a season, or a skilled roof-thatcher may hire a different assistant each time he is hired to re-thatch a roof in the village.

Hiring is also employer-directed: 98 percent of agricultural employers report typically approaching workers, by physically going to the neighborhoods where

⁸For example, Beegle, Galasso, and Goldberg (2017) find no workfare crowd-out in Malawi, and hypothesize this may be due to slack.

workers live, to fill hiring needs. Given the intermittent nature of an individual employer's labor demand, a typical agricultural employer in our sample only hires workers for six days per month. Thus, workers work for many different employers, and each employer works with many different workers. Because rural villages tend to be relatively small and engage in labor relationships every year, employers and workers within a village tend to know one another. For example, when asked to rate the ability of workers in the village, surveyed employers reported not knowing a randomly chosen worker only 27.9 percent of the time. In addition, nonagricultural employers also recruit by driving into the village and picking up workers who are available at the time in trucks.⁹

In our context, labor markets are not fully integrated spatially. For example, employers must pay higher wages to workers who are hired from outside their village, in part due to transportation costs and commuting time. In our data, this amounts to a 12 percent wage premium. In addition, workers believe their village-level collective labor supply decisions affect village wages (Breza, Kaur, and Krishnaswamy 2019). This would not be possible under fully integrated markets.

Consistent with the seasonal, project-based nature of labor demand, employment rates in the casual labor market are highly variable, higher in peak months and lower in lean months. Wage employment rates fall by more than 40 percent from peak to lean months. The villages in our study match a general feature of village economies: large periods of low employment (Muralidharan, Niehaus, and Sukhtankar 2020; Drèze, Leruth, and Mukherjee 1986).

Self-employment is extremely common in our setting. Among the workers who sign up for the external jobs that form the basis of our hiring shock, 63 percent own land, while 89 percent have some kind of household business. Common household businesses reported in our context include grocery store owners, street vendors, vegetable sellers, and firewood collectors. However, businesses tend to have low levels of capital and small landholdings, especially for those cultivators who are also engaged in casual labor.

We also note that while India has a well-known workfare program, NREGA, it is not very well implemented in Odisha. In principle, NREGA offers 100 days of employment in public works annually, but the average employment rate in public works in our sample is 0.5 percent.¹⁰

Finally, in Table 1, we use data from the 2011 Indian census to show how our study villages compare to the average village in the five districts in Odisha where the experiment was run. The study villages are slightly smaller than average, with 148 households per village in the sample, compared to 176 households overall (p -value = 0.299). However, our sample villages are comparable on most dimensions. For example, 67.8 percent of residents are literate in our sample compared to 66.1 percent overall (p -value = 0.428), and 74.5 percent of males participate in the

⁹Workers also obtain nonagricultural work by commuting to district towns, though this is less common in our specific context.

¹⁰In their analysis of NREGA, Imbert and Papp (2015) discuss the vast heterogeneity in program implementation across India; Odisha is notably absent from their list of high-performing states. Moreover, in the National Sample Survey data used by Imbert and Papp (2015), respondents in our five study districts (two of which were early program districts), reported zero days of public works employment. Our more recent survey data indicates that the state's implementation issues have persisted.

TABLE 1—REPRESENTATIVENESS OF STUDY VILLAGES

	Study villages (1)	All villages (2)	<i>p</i> -value (3)
Total population	648.5 (335.075)	773.3 (850.876)	0.286
Total households	147.9 (80.252)	175.5 (193.128)	0.299
Total scheduled caste/scheduled tribe pop.	279.4 (237.695)	262.9 (347.685)	0.729
Male population share	0.513 (0.019)	0.511 (0.038)	0.650
Literacy rate	0.678 (0.117)	0.661 (0.155)	0.428
Worker share	0.431 (0.133)	0.402 (0.124)	0.083
Male worker share	0.745 (0.176)	0.769 (0.172)	0.306
Main worker share	0.629 (0.221)	0.642 (0.273)	0.730
Agricultural labor share	0.164 (0.175)	0.166 (0.191)	0.940
Cultivator share	0.228 (0.149)	0.216 (0.195)	0.637
Nonfarm self-employment share	0.397 (0.226)	0.380 (0.271)	0.650
Other workers share	0.211 (0.161)	0.239 (0.207)	0.332

Notes: Observations in column 1 are from villages in this study, which span five districts across Odisha. We were able to match 88 percent of our study villages to the 2011 census village directory ($N = 53$). Observations in column 2 are from all other census villages in the five study districts ($N = 8,442$). Column 3 reports *p*-values from a comparison of means across columns 1 and 2. Data on village characteristics come from the 2011 Indian Population Census. A worker is a person who participated in any economically productive activity in the last year; a main worker worked for more than six months in the last year. Worker share = total workers/total population. The four mutually exclusive employment categories (agricultural labor, cultivator, nonfarm self-employment and other work) are based on workers' primary occupation status over the past year. Details on the construction of each variable can be found in online Appendix D.

labor market in our sample compared to 76.9 percent across all villages (*p*-value = 0.306).

II. Hypotheses

A. Definition of Labor Rationing

Suppose the prevailing wage for one day of work in the casual daily market is w .¹¹ 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.

¹¹ Our results hold if we use hourly wages as the basis for our definition instead.

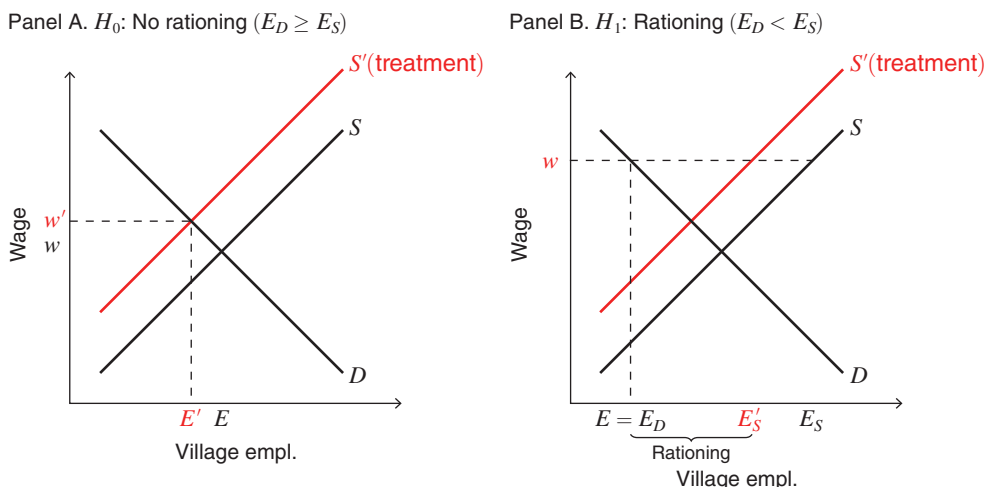


FIGURE 1. EFFECTS OF A NEGATIVE LABOR SUPPLY SHOCK

Note: Figure shows the effects of a negative supply shock on employment and wages under no rationing ($E_D \geq E_S$) in panel A, and under rationing ($E_D < E_S$) in panel B.

The first condition 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 this takes no stance on the microfoundation for rationing.

B. Predicted Impacts of a Hiring Shock

The core of our design is an experimental hiring shock, through which a (random) subset of workers in the local labor market is hired in external jobs outside the village. To diagnose rationing, we examine the effects of this shock on *local* wages and employment—i.e., wages and employment in the local labor market, *excluding the external worksite jobs we create*.

We model the experimental hiring shock as a supply shock to the local labor market. Through our intervention, some workers are “removed” and placed in external jobsites. This constitutes a reduction in the residual labor supply available to employers in the village; in other words, the (residual) village labor supply curve shifts to the left. However, this modeling choice is not consequential for our test. As we discuss below, we could model this as a demand shock instead and would arrive at the exact same predictions for local wages and employment under rationing.¹²

To lay 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

¹²Under the typical definition of a demand shock, we would have posted the jobs, and then, as price takers, hired workers at whatever equilibrium wage prevailed after posting. Instead, we purposely choose which workers to “remove” at a predetermined (high) wage. This parallels, e.g., workfare programs, which are also typically modeled as supply shocks to the local private sector.

of local employment (in terms of worker-days) in the village and w denote the village wage in the absence of our intervention. A supply shock (a shift from S to S') will:

- P1) Increase local wages from w to w' ;
- P2) Decrease aggregate employment among workers who remain in the village (i.e., those who are not hired by us to work in jobsites), so that total employment after the shock E' is less than E .

Panel B of Figure 1 shows the effects of a negative labor supply shock when there is excess labor supply. As before, E denotes the level of employment in the village and w denotes 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 . Employment levels are therefore determined by the labor demand curve. If the amount of labor rationing is weakly greater than the size of the supply shock, then we predict that the shock (a shift from S to S') will have:

- L1) No effect on local wages (the wage remains w);
- L2) No effect on aggregate employment levels (aggregate employment remains E);
- L3) Positive employment spillovers on individual workers, whose employment goes up due to reduced competition for job slots.

Note that these predictions are not sensitive to whether S' is a parallel shift of the supply curve; they hold even if the supply elasticity changes (i.e., due to a nonparallel shift). If predictions L1–L3 hold, the size of our hiring shock is a lower bound on the level of rationing in the economy.

Note that predictions L2 and L3 above are inherently related. Under rationing, workers who would have otherwise been rationed fill in job slots that are now available because many other laborers competing for the same jobs have become employed outside the village. The resultant increase in the proportion of days employed among remaining workers is why aggregate employment remains unchanged. Online Appendix Figure A.1 validates that predictions L1–L3 hold under rationing if we model the hiring shock as a positive demand shock instead.¹³

This constitutes a revealed preference test for excess labor supply: 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 .¹⁴

¹³ As online Appendix Figure A.1 makes clear, we would expect: (i) no change in the wage, and (ii) aggregate employment (including the external hiring shock jobs) would increase one-for-one with the size of the hiring shock—implying no change in aggregate local village employment (i.e., excluding the hiring shock). This corresponds exactly to predictions L1 and L2, respectively, and L3 follows mechanically from L2.

¹⁴ To illustrate these predictions, consider the following thought exercise. Suppose ten workers want work in the village at wage w , but only five job slots are available. As a result, five workers are employed at w , while the other five workers are rationed: the employment rate is 50 percent. Now, suppose we remove four workers from the village labor market. This frees up job slots, and a larger portion of the remaining workers can now work in the village at wage w . Specifically, there are six workers left who want work and still five available slots: the employment rate

To enrich our understanding of labor market functioning, we undertake the above exercise across different months of the year. This is motivated by earlier work in the development labor literature, which argued that seasonality in labor demand is consequential for shaping labor market equilibrium at different times of the year (e.g., Leibenstein 1957, Drèze and Mukherjee 1989). Consistent with this work, we hypothesize that the peak season effects of hiring shocks will be closer to those in panel A of Figure 1, whereas the lean season effects will match those in panel B.

C. Self-Employment and Disguised Unemployment

If there is excess supply, rationed individuals may appear as unemployed or may turn to less-productive self-employment as a way to generate income—creating “disguised unemployment” or “forced entrepreneurship” (Singh, Squire, and Strauss 1986). For this group, self-employment earnings are below w , but above their reservation wage.

Consequently, under rationing, when job slots open up through the hiring shock, a fraction of both unemployed and/or self-employed workers will reveal that they prefer jobs at the prevailing wage w over what they were previously doing. Among business owners, the magnitude of this shift will provide a lower bound on the fraction of self-employed workers who are disguised unemployed.¹⁵

D. Discussion of Predictions

Predictions L1–L3 diagnose rationing, with minimal other assumptions about the labor market. For example, our test does not require assumptions about the equilibrium in the absence of rationing—whether it is fully competitive ($E_D = E_S$) or subject to monopsony or some other friction ($E_D > E_S$). For example, if there were monopsony in the labor market but no rationing, then our hiring shock would necessarily lead to an increase in wages—contradicting prediction L1 (see online Appendix Figure A.2).¹⁶

In addition, we do not take a specific stance on the reason for rationing; our test is valid for a range of microfoundations. Overall, our test is chiefly powered to detect rationing that is generated by some failure of wage adjustment to seasonal reductions in labor demand. Rationing from effort efficiency wages (Shapiro and Stiglitz 1984), for example, would not generate the patterns we hypothesize. There, wages would respond to a reduction in labor supply: because the hiring shock would

is now 83 percent. In contrast, if the five workers who are unemployed did not want work, they would not accept employment at wage w ; this provides a test of condition (i) in the rationing definition 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 provides a test of condition (ii) above.

¹⁵Since we do not take a stance on the rationing mechanism, we do not have an ex ante prediction on whether unemployed or self-employed workers are more likely to be hired into empty job slots first. In addition, if there are fixed costs of stopping and then going back to one's business, then this is another reason why the estimates from this exercise will be a lower bound on disguised unemployment.

¹⁶If our hiring shock simply shifts the labor supply curve to the left, then the predictions under monopsony are unambiguous—wages should rise and employment should fall. However, if the shock also changes the labor supply elasticity facing the employer, then wages would still increase, but the employment effect is ambiguous. One example is the canonical case of an increase in the minimum wage. See chapter 2 of Manning (2013) for a detailed discussion.

lower the cost of unemployment, wages would need to increase to restore incentives. Section VIIA discusses microfoundations in light of our results.

We also do not need to specify the rationing mechanism, i.e., how jobs are allocated across workers. There may be a distribution of ability levels among rationed workers, and this distribution could even change with the shock. However, if we observe predictions L1 and L3, it must be the case that there are workers in the village who prefer wage jobs and employers are willing to hire them for those jobs at the prevailing wage w , satisfying the two criteria in the definition for rationing.

Note that a key feature of our research design is that we can precisely quantify what fraction of the labor force is “removed” through the hiring shock because our shocks are targeted, available only to some workers who are offered the external jobs (see below). This stands in contrast to other labor market shocks such as workfare programs, which are permanent and impact *all* eligible workers by offering an outside option. This potentially increases reservation wages and shifts the labor supply curve even among workers who do not ever participate directly, making it difficult to know what fraction of workers is affected. Consequently, it would be difficult to use a shock such as a workfare policy to quantify rationing. Our transitory targeted shocks greatly simplify analysis, enabling us to bound the level of rationing without needing to impose assumptions about labor supply responses.

III. Experiment: Design and Implementation

We engineer *transitory* hiring shocks in study villages. We exploit an opportunity to recruit casual male workers for full-time manufacturing jobs for two to four weeks.¹⁷ The work occurs in external jobsites within daily commuting distance from study villages.¹⁸ Such temporary one-time contract jobs are a common source of nonagricultural employment for men in the region. The external jobs are attractive: the daily wage is weakly higher than the prevailing market wage for casual labor, and there are positive compensating differentials (e.g., the work occurs indoors and is not very physically demanding). This offers an advantage because it draws interest from many workers in the village, enabling us to (randomly) draw from a sizable swath of the labor force.

The external jobs are advertised in villages through flyers, village meetings, and door-to-door visits to male adults. Hired workers are then drawn randomly from the subset of the village labor force that signs up for the job. See online Appendix C for more details about the recruitment protocols and external jobs.

We randomize recruitment intensity at the village (i.e., local labor market) level, so that in treatment villages we hire up to 60 percent of sign ups, and in control villages we hire one to five workers only.¹⁹ We thus generate a large hiring shock in

¹⁷ In this cultural context, women are unwilling or unable to travel outside of the village for work.

¹⁸ We leverage two separate field projects (Breza, Kaur, and Shamdasani 2018; Kaur et al. 2019) that involve hiring workers for low-skill manufacturing jobs. See online Appendix C for a full description of these jobs.

¹⁹ We set this limit of 60 percent in order to ensure there were enough workers left over in the village to comprise a substantive spillover sample (on whom we could observe treatment effects), and to avoid the possibility that the shock ended up being larger than the amount of rationing in lean seasons. In addition, we hired no more than approximately 30 workers per village due to space constraints at the external worksites. Note that workers were told in advance that the number of job slots would be determined based on contractor needs, so that *ex ante* beliefs about hiring probability were the same across treatment and control villages.

treatment villages, and a negligible shock in control villages. We use a matched-pair, stratified research design, so as to achieve balance by local region and time.

We implement these shocks in different months of the year, which correspond to different levels of labor demand and employment. We limited our experiment to ten months of the calendar year, omitting the two busiest months (August—peak planting and December—harvesting) so as to not affect labor supply during important work periods. Thus, our experiment does not run in the pure peak season, but rather in the lean and semi-peak (or shoulder) months.

We use employment rates in control villages as our proxy for underlying labor market slack. Months with above-median employment rates are classified as semi-peak periods, while months with below-median employment rates are classified as lean periods of the year.

We run the experiment in 60 villages (labor markets) across five districts in Odisha, India, between the years 2014 to 2018. We used a matched-pair randomization design, so we have 30 treatment and 30 control villages. Note, 43 percent of the experimental rounds were conducted in lean months, and the remaining 57 percent in semi-peak months. We have survey data for 2,379 workers in total.²⁰

Our experiment only has power to detect rationing if the labor market is not fully integrated across villages, so that removing workers in one village constitutes a meaningful local supply shock from employers' perspective. Under full labor market integration, we would not expect to find wage adjustment from our hiring shock in either the lean or semi-peak season. Moreover, even in the presence of rationing, we would not detect employment spillovers.

The lack of full integration is a reasonable assumption in our context. Agricultural hiring occurs primarily within the village, as described in Section I above. In addition, even for nonagricultural jobs, it is common for contractors to recruit by coming to a village and loading (a predetermined number of) workers onto a truck—providing scope for employment spillovers even for jobs conducted outside the village.

IV. Empirical Strategy and Data

A. Analysis Samples

Figure 2 summarizes the analysis samples across control villages (panel A) and treatment villages (panel B). The dark gray areas denote workers who sign up for the jobs and are “removed” to work in the external jobsites. The light gray 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* sample. Note that the spillover sample is larger in control villages (panel A), since only one to five workers are offered jobs in control villages.

For the analysis, we examine effects on three sets of samples. The first is the spillover sample, which is directly comparable to the workers who were removed from the village, and therefore would be most likely to benefit from employment

²⁰We were unable to complete end line data collection in two experimental rounds due to operational issues (see online Appendix C). These rounds are excluded from the main analysis. In online Appendix Table B.13, we show robustness to including the partial data from these rounds.

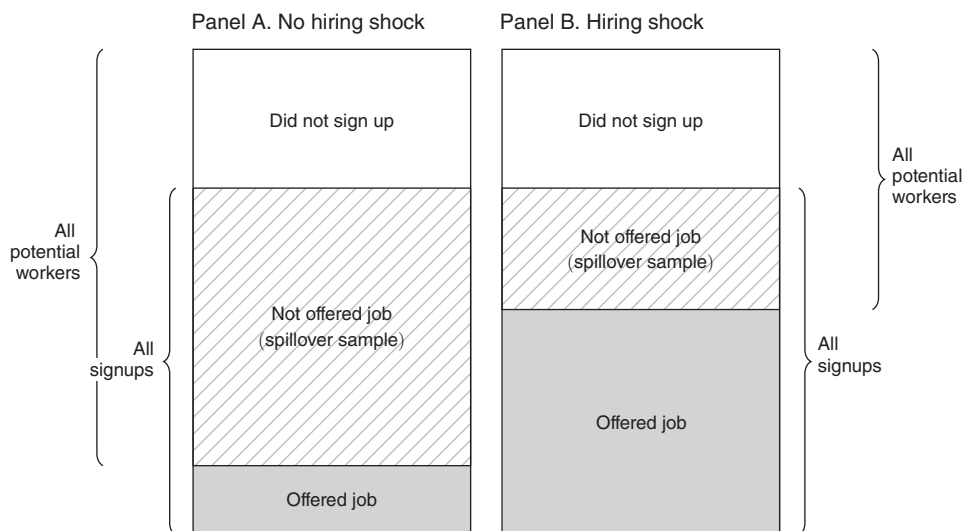


FIGURE 2. ANALYSIS SAMPLES

Notes: Figure summarizes the analysis samples in control villages in panel A, and treatment villages in panel B. The dark gray area denotes workers who sign up and are offered employment at the external job sites (i.e., “removed” from the village). The light gray shaded area denotes workers who sign up but are not offered jobs—this constitutes the spillover sample (the main analysis sample). The sum across all three areas denotes the full village sample.

spillovers. The second is all potential workers in the village—regardless of whether they signed up for our jobs. The third is the full village sample, in order to accurately assess effects on aggregate employment levels.

B. Estimation Strategy

To test how the experimental hiring shock impacts employment and wages, we compare outcomes in treatment and control villages, separately for semi-peak and lean months. Our base specification is

$$(1) \quad y_{itvr} = \alpha + \beta \text{HiringShock}_v + \gamma \text{HiringShock}_v \times \text{SemiPeak}_r + \rho_r + \bar{X}_{itvr}^0 + \epsilon_{itvr},$$

where y_{itvr} is an outcome for worker i on day t in village v and experimental round r . The variable HiringShock_v is an indicator for treatment villages, and SemiPeak_r is an indicator for experimental rounds conducted in semi-peak months. We include the worker’s baseline mean employment rate and mean daily wage levels \bar{X}_{itvr}^0 in order to increase precision (and we also report estimates without baseline controls). Regressions always include round (strata) fixed effects (ρ_r), and cluster standard errors by village.

To construct the SemiPeak_r indicator, we calculate the mean month-wise employment rate in control villages for each month in our sample, averaging across all rounds that started in a given month (see online Appendix Figure A.3). We then take the median of this variable across rounds in the sample. Months with above-median values are classified as semi-peak periods of the year. In addition, in the analysis,

we show robustness to replacing the *SemiPeak*, binary variable with the continuous month-wise employment rate in the interaction with *HiringShock_v*.

Regressions using the spillover sample are unweighted because we survey 100 percent of spillover workers. Regressions examining effects for all potential workers and for the full village sample are weighted by inverse sampling probabilities in order to be representative of the full labor force.

C. Data

We survey all workers who sign up for the external job (i.e., the spillover sample). In addition, we survey a random sample of non-signups: prime age males who work in any capacity—the casual labor market, self-employment, or salaried work—since ex ante we cannot distinguish who among these may potentially be interested in local wage work. We refer to these non-signups plus those who signed up as the “full village sample,” and use this sample when testing how the shock affected the entire village labor market. We conduct three waves of surveys: at baseline (immediately before workers are hired at the external jobsites), at endline (during the last two weeks of the 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).

Each survey includes a detailed daily employment grid, in which workers describe their employment activity separately for each day over a recall period, which was either 7, 10, or 14 days (see online Appendix C). This includes rich data about wages: cash wages, details of in-kind payments (e.g., tea, meals, and cash value of in-kind payments), whether the worker was paid on time, etc. In addition, it includes detailed characteristics about employment status (activity, length of breaks, hours worked, location) and self-reports of involuntary unemployment for each day. This provides us with the core data needed to test the predictions outlined in Section IIB.

In addition to the worker surveys, we survey a subset of agricultural employers in the village at endline.²¹ We discuss details in online Appendix C.

D. Descriptive Statistics

Table 2 presents descriptive statistics and tests of balance across treatment and control villages. Panel A presents characteristics for workers in the main analysis sample (i.e., the spillover sample) drawn from the baseline survey. Panel B presents village-level information. We show means and standard deviations for each covariate in control villages and coefficients and standard errors from a comparison of means across treatment and control villages, obtained using a simple univariate regression with round (strata) fixed effects. As expected, given randomization, the groups are well-balanced on covariates.

Consistent with the characterization of the empirical context in Section I, wage employment rates are low, and self-employment is common among workers in the

²¹ In each village, we consulted a village resident to obtain a (partial) list of employers, and surveyed these in random order until we reached 20 employers. While this sample is not necessarily representative of all employers, it can be used as supplementary data to check qualitative patterns, as well as what happens to wages, given strong norms wherein employers pay workers the same wage within the village (Breza, Kaur, and Shamdasani 2018).

TABLE 2—BASELINE CHARACTERISTICS

	No hiring shock (1)	Difference (2)
<i>Panel A. Worker-level</i>		
Landless	0.370 (0.483)	−0.020 (0.039)
Household members (age 12+)	3.965 (1.543)	−0.039 (0.104)
Any activity	0.400 (0.364)	0.044 (0.031)
Employment rate: hired wage employment	0.178 (0.288)	0.034 (0.027)
Total wage (Rs.)	251.968 (60.904)	−1.863 (9.040)
Public works employment	0.005 (0.058)	0.003 (0.004)
Has household business	0.890 (0.314)	0.003 (0.021)
Self-employment	0.115 (0.244)	−0.017 (0.021)
Days would like to work in labor market (in next 30 days)	18.217 (8.496)	−0.216 (0.406)
<i>Panel B. Village-level</i>		
Fraction of signups	0.423 (0.104)	0.00967 (0.021)
Total households	145.1 (92.731)	21.67 (16.276)
Total scheduled caste/scheduled tribe population	319.0 (266.111)	−71.62 (46.122)
Literacy rate	0.679 (0.126)	−0.00186 (0.023)
Worker share	0.408 (0.131)	0.0382 (0.032)
Main worker share	0.647 (0.221)	−0.0282 (0.066)

Notes: Panel A presents baseline characteristics for the spillover sample (workers who signed up for external jobs but were not offered employment) at the worker level ($N = 992$). Panel B presents baseline characteristics at the village level using data from the experiment ($N = 60$) as well as data from the 2011 Indian Population Census ($N = 53$; 88 percent match rate between our study villages and 2011 census villages). A worker is a person who participated in any economically productive activity in the last year; worker share = total workers/total population. A main worker is a person who worked for more than six months in the last year; main worker share = total main workers/total workers. Details on the construction of the census variables can be found in online Appendix D. Column 1 presents baseline means and standard deviations of characteristics in control villages. Column 2 reports coefficients from regressing the covariate in each row on a dummy for treatment with round (strata) fixed effects and standard errors clustered at the village level.

spillover sample. On average, respondents report some work on 40 percent of the recall period of ten days—including wage employment on 18 percent of the days, and self-employment on 12 percent of days. However, respondents report wanting wage work on only 18 days per month (i.e., 60 percent of days) on average—a proxy for full employment in our sample based on worker preferences. This suggests that, scaling by this number, workers are employed in wage employment $0.18/0.60 = 30\%$ of the time that they would like. Our employment rate estimates match those in other parts of India. For example, Muralidharan, Niehaus, and

Sukhtankar (2020) report an average employment rate in any private work (wage employment + self-employment) of 7.1 days per month, or 24 percent, among their respondents in Andhra Pradesh in June. In the Odishan villages surveyed in the Rural Economic and Demographic Survey, prime age males with small landholdings are employed in wage labor 22.9 percent of the time in the lean season.²²

Online Appendix Table B.1 presents descriptive statistics comparing those who signed up for the external jobs with those who did not. As expected, those who did not sign up are less likely to be in the labor force: they are 12–19 percentage points less likely to ever participate in the casual labor market and have lower desired labor supply for wage labor. Consistent with this, non-signups are likely to be wealthier; for example, they are less likely to be landless.

At endline, we manage to survey 90 percent of spillover sample workers. In online Appendix Tables B.2 and B.3, we test for differential survey completion among workers in the main spillover and non-signup samples, respectively. We find no evidence of differential survey attrition rates by treatment assignment. Finally, in online Appendix Table B.4, we present descriptive statistics and balance for the employer survey.

V. Results I: Test for Rationing

A. Size of the Shock

On average, 42 percent of potential workers in the village sign up for our external jobs (Table 2, panel B). Even among workers who actively participate in the casual labor market, one would not expect a 100 percent sign up rate for the external jobs. Over 85 percent have a household business, which may not be feasible to leave for a one month job. In addition, a job that requires regular attendance and hours may be a disamenity for some workers (e.g., Blattman and Dercon 2018).

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 percent of the male labor force in treatment villages is hired in the external jobs. In one village, take-up is zero as harvesting began early. For the remaining villages, the size of the shock ranges from 15 to 38 percent. 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. The average shock size in control villages is 3 percent.

Further, the size of the hiring shock is indistinguishable in lean and semi-peak months (p -value = 0.652). This greatly simplifies the interpretation of our analysis, which compares the effects of the hiring shocks across lean and semi-peak months.²³

²²Specifically, we consider individuals with less than 1.56 acres, which represents the ninetieth percentile of landholdings in the spillover sample.

²³See Table 6, column 4. One might expect the shock size to be weakly smaller in semi-peak months, due to fewer signups when work is locally available. The fact that the worksite jobs were desirable led to ample signups across months. This enabled us to hire enough workers for worksite jobs from among these signups to maintain robust shock sizes across months. Note that if the shock size had been smaller in peak months, this would make it more difficult to find the pattern of our expected results in the reduced form: no wage or aggregate employment effects in lean months, but meaningful effects in peak months.

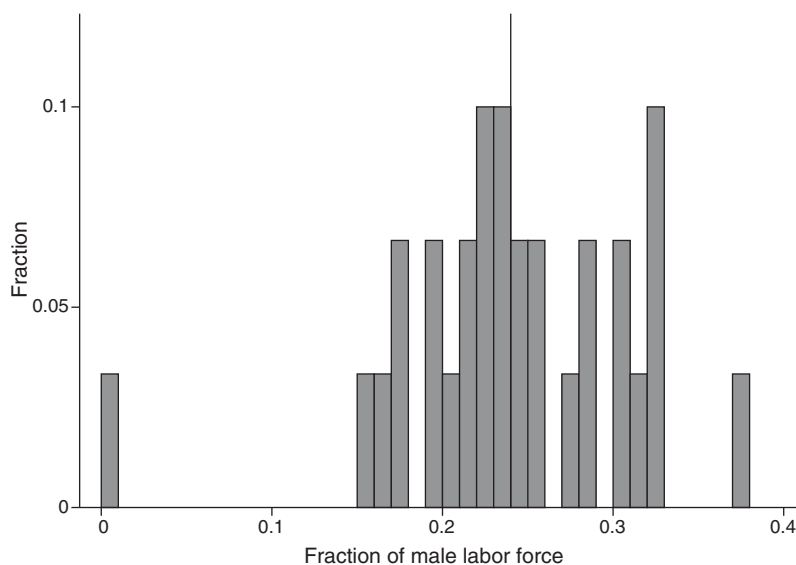


FIGURE 3. SIZE OF THE EXPERIMENTAL HIRING SHOCK

Notes: Figure shows the size of the experimental hiring shock in treatment villages. This is measured as number of workers hired divided by the size of the male labor force in the village. The mean shock size (indicated by the vertical line) is 0.24.

B. Wages

We study the impact of the hiring shock on wages in the local labor market (i.e. excluding our external worksite jobs). For each worker-day 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).

Figure 4 compares the distributions of total wages for treatment and control villages, limiting the sample to lean season observations (panel 1A) and to semi-peak season observations (panel 1B). 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 semi-peak months (p -value < 0.001), indicating a rise in equilibrium wages.

Table 3 presents regression estimates for the spillover sample on log cash wages (column 1) and log total wages (columns 2 and 3).²⁴ At the bottom of the table, we report the p -value for the F -test of whether the total treatment effect on wages in semi-peak months is significantly different from zero (i.e., $\beta + \gamma = 0$ in equation (1)). Note from the control means that wages paid in lean and semi-peak periods are extremely similar. This is consistent with a failure of wage adjustment in the presence of seasonal changes to labor demand.

²⁴Wages are winsorized. Online Appendix Table B.5 documents similar estimates using non-winsorized wages.

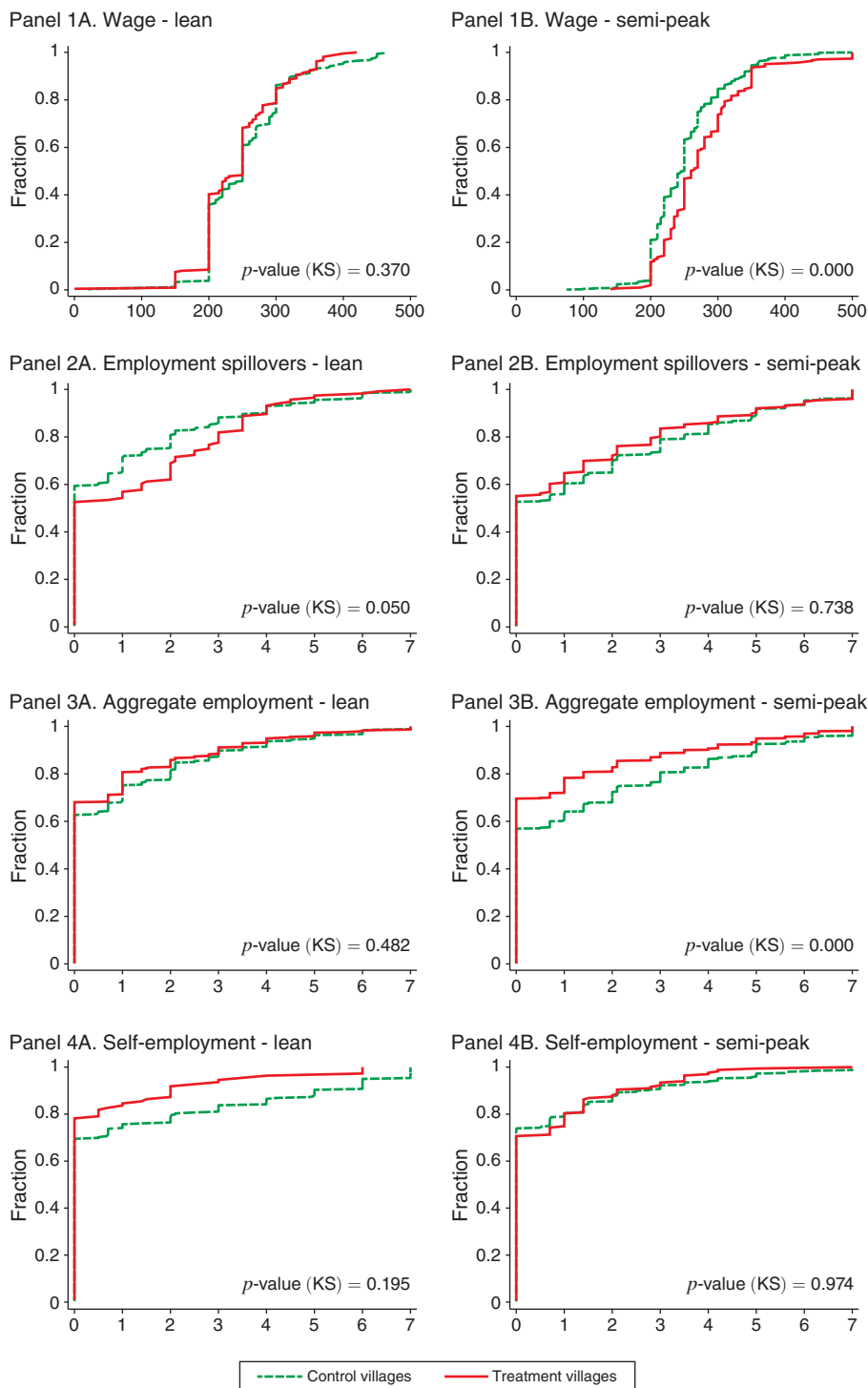


FIGURE 4. IMPACTS OF HIRING SHOCK

Notes: Figure compares the distribution of wages, employment spillovers, aggregate employment, and self-employment for treatment and control villages, limiting the sample to lean month observations only in panel A, and semi-peak month observations only in panel B. We report the p -value for the equality of distributions from a Kolmogorov-Smirnov test in the bottom right corner of each graph.

TABLE 3—WAGE EFFECTS

	log cash wage (1)	log total wage (2)	log total wage (3)	Total wage (4)	log total wage (5)	log total wage (6)	log total wage (7)	log total wage (8)
Hiring shock	−0.0202 (0.021)	−0.0113 (0.022)	−0.0183 (0.019)	−5.632 (3.925)	−0.0620 (0.050)	−0.0214 (0.041)	−0.00728 (0.040)	0.000439 (0.032)
Hiring shock × semi-peak	0.0733 (0.031)	0.0676 (0.032)	0.0684 (0.029)	18.57 (7.595)		0.0797 (0.047)	0.0798 (0.048)	0.112 (0.044)
Hiring shock × empl. level					0.457 (0.240)			
Hiring shock × non-signups (NSU)							−0.0238 (0.091)	
Hiring shock × semi-peak × NSU							0.00499 (0.100)	
Semi-peak × NSU							0.00597 (0.080)	
NSU							−0.00478 (0.074)	
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	All potential workers Yes	All potential workers Yes	Village employers No
Baseline controls	No	No	Yes	Yes	Yes			
<i>p</i> -value: shock + shock × semi-peak	0.0239	0.0227	0.0256	0.0472	.	0.0100	0.00837	0.000619
SE: shock + shock × semi-peak	0.0229	0.0241	0.0219	6.379	.	0.0219	0.0266	0.0306
<i>p</i> -value: shock + shock × NSU	0.680	.
<i>p</i> -value: shock + shock × semi-peak + shock × NSU + shock × semi-peak × NSU	0.0958	.
<i>p</i> -value: shock × NSU + shock × semi-peak × NSU	0.654	.
Control mean: lean	5.458	5.500	5.500	253.8	5.500	5.532	5.532	5.355
Control mean: semi-peak	5.428	5.504	5.504	251.6	5.504	5.511	5.511	5.422
Observations	1,543	1,544	1,544	1,545	1,544	2,249	2,249	346
Level of observations	worker- days	worker- days	worker- days	worker- days	worker- days	worker- days	worker- days	employer- activity

Notes: Observations in columns 1–5 are from the spillover sample (workers who signed up for external jobs but were not offered employment). Observations in columns 6 and 7 are from all potential workers in the village with appropriate weights, and observations in column 8 are from a sample of employers surveyed in the village. Total wage = cash + in-kind wages. We winsorize the top one percentile of the cash and total wage distributions. Controls in columns 1–7 include worker-level mean employment and wage levels at baseline. Regressions include round (strata) fixed effects. Standard errors clustered at the village level in parentheses.

Consistent with prediction L1, we find no evidence that wages increase in response to the external hiring shock during lean months. However, during semi-peak months, the hiring shock raises equilibrium wages by 5.0 percent (column 3, *p*-value = 0.026) on average, consistent with prediction P1. The results are similar if we examine effects on wage levels rather than log wages (column 4, *p*-value = 0.047).

In column 5, we document that the pattern of these findings is similar if we interact the hiring shock treatment dummy with a continuous measure of the base employment rate rather than the semi-peak dummy. Note that the negative coefficient on *Hiring shock* in column 5 does not have a clear interpretation, because there is no month in our data with an employment rate of zero.²⁵ Note however, we

²⁵The mean lean season employment rate in control villages is 0.145 (reported in Table 5 below). Using the results in Table 3, column 5, this corresponds to an estimated wage effect of 0.0043 (SE = 0.0204, *p*-value = 0.834)—consistent with no wage increase in the lean season.

would expect the effect on wages to be nonlinear in the shock size when there is rationing. Under rationing, there would be no change in the wage until the size of the supply shock is greater than the amount of rationing, after which the wage would start increasing in the size of the supply shock. Consequently, we would expect the coefficient on the continuous linear specification to be attenuated relative to the true semi-peak wage effect. While we show the continuous specification for completeness, we primarily focus on the binary specification in the analysis.

In columns 6 and 7 of Table 3, we present estimates of equation (1) on the full potential village labor force. In column 6, we find that the predictions hold with the sample of all potential workers: there is no detectable change in equilibrium wages in treatment villages in lean months (p -value = 0.606), and an increase in equilibrium wages in treatment villages in semi-peak months (p -value = 0.010).²⁶ In column 7, we further interact equation (1) with an indicator for whether the worker signed up for the external job. In treatment villages, equilibrium wages increase in semi-peak months for both signups (p -value = 0.008) and non-signups (p -value = 0.096), and do not change for either group in lean months.²⁷ This indicates that the wage results are not driven simply by a compositional change in the labor force. In column 8, we run a similar analysis using information about total wages reported by a sample of employers. We find quite comparable effects, both qualitatively and quantitatively.

Our primary wage measure is in terms of the daily wage, since this is the most common form of wage contract in these labor markets. In Table 4, we verify that our results are robust to measuring impacts on the hourly wage (rather than the daily wage). In addition, we find little evidence of shifts in other aspects of the wage contract or compensating differentials; for example, the number of hours per workday, or expectation of receiving future benefits from the employer such as work in the future. This helps assuage concerns that the effective wage did increase in the lean season, but through non-price amenities. Moreover, such a story would need to explain why wage levels adjust in semi-peak months but not in lean times.

C. Individual-Level Employment Spillovers

To test for positive employment spillovers, we measure effects on the spillover sample. These workers are exactly comparable to those who were “removed,” and therefore should benefit from the decreased competition for job slots. We measure effects on all village residents when we examine effects on aggregate employment below.

Table 5 provides estimates of equation (1). The dependent variable is a dummy for whether worker i was hired to work for someone else on day t in the local labor market for a wage.

Consistent with Prediction L3, in lean months, wage employment increases by 5.44 percentage points (p -value = 0.005) on a base rate of 0.145 (column 2)—implying a 38 percent increase in employment among workers who remain in the

²⁶ Because we sampled a small share of workers who did not sign up for the outside jobs, the weighted full village regression has larger standard errors than those in column 3.

²⁷ Note that these results are not powered to detect whether the wage effects on non-signups are different from signups, given the standard errors on the interaction terms. Our goal in this table is to test whether overall wages for non-signups also went up in each season.

TABLE 4—ALTERNATE MEASURES OF WAGES AND WAGE CONTRACT

	Hourly total wage (1)	Hours per day (2)	Expected days of work (3)	Expect loan (4)	Paid immediately (5)	Paid an advance (6)
Hiring shock	−0.653 (1.131)	0.0655 (0.152)	−0.179 (4.426)	−0.0943 (0.065)	0.0906 (0.048)	−0.0663 (0.040)
Hiring shock × semi-peak	4.422 (2.168)	0.0414 (0.245)	−0.125 (5.500)	0.198 (0.106)	−0.112 (0.096)	0.0840 (0.073)
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover
<i>p</i> -value: shock + shock × semi-peak	0.0464	0.580	0.927	0.229	0.802	0.774
SE: shock + shock × semi-peak	1.849	0.192	3.265	0.0835	0.0827	0.0608
Control mean: lean	42.76	6.354	14.18	0.342	0.849	0.0959
Control mean: semi-peak	44.05	5.910	13.68	0.343	0.806	0.104
Observations	1,469	1,469	223	262	262	262
Level of observations	worker-day	worker-day	worker	worker	worker	worker

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Hourly total wage = total (cash + in-kind) wages/total hours worked that day. Hours per day = total hours worked that day. For a subset of experimental rounds, workers were asked a series of questions about all the employers they had worked for in the past ten days; these responses are summarized in columns 3 to 6. Expected days of work = total number of days of work till the next harvest they expect from these employers. Expect Loan = $\mathbf{1}\{\text{expect a loan from these employers}\}$. Paid immediately = $\mathbf{1}\{\text{paid immediately for work by these employers}\}$. Paid an advance = $\mathbf{1}\{\text{paid an advance by these employers}\}$. Regressions include round (strata) fixed effects. Standard errors are clustered at the village level in parentheses.

TABLE 5—EMPLOYMENT SPILLOVERS

	Hired wage empl. (1)	Hired wage empl. (2)	Hired wage empl. (3)
Hiring shock	0.0684 (0.021)	0.0544 (0.019)	0.138 (0.045)
Hiring shock × semi-peak	−0.0737 (0.034)	−0.0735 (0.030)	
Hiring shock × empl. level			−0.706 (0.254)
Sample	Spillover	Spillover	Spillover
Baseline controls	No	Yes	Yes
<i>p</i> -value: shock + shock × semi-peak	0.840	0.427	.
SE: shock + shock × semi-peak	0.0265	0.0239	.
Control mean: lean	0.145	0.145	0.145
Control mean: semi-peak	0.216	0.216	0.216
Observations (worker-days)	8,906	8,906	8,906

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Hired wage employment = $\mathbf{1}\{\text{worker hired that day and paid a wage}\}$. Controls comprised of worker-level mean employment and wage levels at baseline. Regressions include round (strata) fixed effects. Standard errors are clustered at the village level in parentheses.

village. This is consistent with our prediction that workers who were previously rationed fill in available job slots. In contrast, we cannot reject that there are no employment spillovers in semi-peak months (*p*-value = 0.427). These patterns are robust to using the continuous employment rate rather than the semi-peak dummy (column 3).²⁸ Panels 2A and 2B of Figure 4 verify these patterns visually.

²⁸ As discussed above, under rationing, employment effects will be nonlinear in the size of the hiring shock. One would expect positive employment spillovers in the lean season as long as the shock size is less than the rationing

D. Aggregate Employment

To test impacts on aggregate employment levels (Predictions L2 and P2), we must measure employment for the entire potential labor force—including those who did not sign up for our jobs. We undertake this analysis in Table 6. We consider the full village sample, which includes our spillover sample, the individuals that were randomly selected to receive external jobs, and a random sample of all other village residents.²⁹ The dependent variable is the same as in Table 5: a dummy for whether worker i was hired for paid wage work on day t in the local labor market (i.e., all employment excluding our external worksite jobs).

Consistent with prediction L3, there is no detectable change in local aggregate employment in response to an external hiring shock in lean months (column 1, p -value = 0.483). This follows directly from the results above. Because wages and local labor demand remain unchanged, rationed workers fill up the job slots, leading to the same level of aggregate employment. In contrast, in semi-peak months, the hiring shock reduces aggregate employment, consistent with prediction P2. Local employment among all workers declines by 4.2 percentage points overall (p -value = 0.005) on a base rate of wage employment of 0.195 in semi-peak months. This corresponds to a 22 percent decline in aggregate employment. Moreover, this decrease is detectably different from the null result in lean months. Column 2 shows that these effects are robust to using the continuous employment rate.

This pattern of results is similar if we run the analysis at the village-day level instead (summing up all employment within the village on each day) (column 3). Finally, in panels 3A and 3B of Figure 4, we plot the cumulative distribution functions showing aggregate employment effects in semi-peak versus lean times, providing visual verification of these patterns.

E. Crowd-Out

The findings in columns 1 and 2 of Table 6 help us understand the extent to which an external hiring shock crowds out private wage employment. In the lean season, giving full-time jobs to a quarter of workers generates no crowd-out in the private labor market.

To quantify the crowd-out in the semi-peak season, we must scale the employment estimates by the number of worker-days of external work created through our hiring shock. In columns 3 to 5 of Table 6, we run village-day level regressions using the hiring shock as an instrument for employment in the external jobsites. Column 3 presents the reduced-form result of the hiring shock on village employment, constructed by adding up individual employment across all potential workers in the village. Consistent with the worker-day level regression results in

level. Once the shock size exceeds this, and wages begin to rise, the employment effect should be smaller, possibly even becoming negative: a potential nonmonotonic effect in shock size.

²⁹For those offered the external jobs, they could have worked in the local labor market on days they were not at the external jobsites, such as weekends, absences, or if they quit the worksite job. Note that if some of the employment spillovers accrue to those who did not sign up for jobs, the aggregate employment effect among only the signups could be negative in the lean season, even in the presence of rationing. We consequently include all workers, including non-signups, in this test.

TABLE 6—AGGREGATE EMPLOYMENT EFFECTS AND CROWD-OUT

	Hired wage empl. (1)	Hired wage empl. (2)	Hired wage empl. (3)	Hiring shock empl. (4)	Hired wage empl. (5)
Hiring shock	0.0141 (0.020)	0.0760 (0.041)	−0.00558 (0.020)	0.187 (0.012)	
Hiring shock × semi-peak	−0.0559 (0.024)		−0.0496 (0.023)	0.00785 (0.019)	
Hiring shock × empl. level		−0.533 (0.207)			
Hiring shock empl.					−0.0301 (0.102)
Hiring shock empl. × semi-peak					−0.252 (0.115)
Sample	Full village sample	Full village sample	Full village sample	Full village sample	Full village sample
Baseline controls	Yes	Yes	Yes	Yes	Yes
<i>p</i> -value: shock + shock × semi-peak	0.00477	.	0.00102	1.57e-19	0.0000838
SE: shock + shock × semi-peak	0.0143	.	0.0160	0.0146	0.0717
Control mean: lean	0.135	0.135	0.169	0.0357	0.169
Control mean: semi-peak	0.195	0.195	0.205	0.0233	0.205
Specification			RF	FS	IV
Observations	21,085	21,085	738	738	738
Level of observations	worker-days	worker-days	village-days	village-days	village-days

Notes: Observations are from the full village sample. Each observation in columns 1 and 2 is at the worker-day level, with appropriate weights. Controls in columns 1 and 2 comprised of worker-level and village-level mean employment and wage levels at baseline. Each observation in columns 3 to 5 is an average at the village-day level. Column 3 shows the reduced-form (RF) impact of the hiring shock on aggregate employment (excluding the labor performed in the external jobs), column 4 shows the first-stage (FS) impacts of the hiring shock on employment in the external jobs, and column 5 is the instrumental variable (IV) impact of hiring shock employment on hired wage employment. Controls in columns 3 to 5 comprised of mean employment and wages at the village-day level at baseline, averaging both across the whole village sample and the subset of individuals with an employment observation on that day. Regressions in columns 3 to 5 use analytical weights to weigh by the number of endline observations present in the village on each day. Regressions include round (strata) fixed effects. Standard errors are clustered at the village level in parentheses.

column 1 of Table 6, we find no change in local aggregate employment in response to the hiring shock in lean months, and a significant decline in semi-peak months. Column 4 presents the first stage, and shows that a substantial fraction of workers were “removed” into the external job, with no detectable differences in lean versus semi-peak months. The IV estimates in column 5 suggest that, in semi-peak months, each day of work that is created in the external jobsites crowds out 0.282 days of private labor market employment (*p*-value < 0.001).³⁰

F. Persistence after the End of the Shock

We survey workers two weeks after the hiring shock ends, when all workers are back in the village, to measure the persistence of the shock in both lean and semi-peak periods. Table 7 presents estimates of equation (1) for our main wage

³⁰In contrast, the estimate for lean months is much smaller in magnitude and noisily estimated (*p*-value = 0.768), implying that external jobs generate no detectable crowd-out in the private labor market in the lean season. This is consistent with the reduced-form results in the first two columns of Table 6.

TABLE 7—IMPACTS TWO WEEKS AFTER END OF HIRING SHOCK

	log total wage (1)	log total wage (2)	Hired wage employment (3)	Self- employment (4)
Hiring shock	−0.0241 (0.038)	0.00410 (0.035)	−0.00846 (0.027)	−0.0177 (0.015)
Hiring shock × semi-peak	0.0673 (0.044)	0.0362 (0.042)	−0.0378 (0.034)	−0.0193 (0.024)
Sample	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	Yes	Yes	Yes
<i>p</i> -value: shock + shock × semi-peak	0.0537	0.109	0.0255	0.0531
SE: shock + shock × semi-peak	0.0219	0.0247	0.0201	0.0187
Control mean: lean	5.529	5.529	0.177	0.162
Control mean: semi-peak	5.532	5.532	0.211	0.134
Observations (worker-days)	1,328	1,328	7,623	7,623

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Total wage = cash + in-kind wages. Hired wage employment = $\mathbf{1}\{\text{worker hired that day and paid a wage}\}$. Self-employment = $\mathbf{1}\{\text{worker self-employed that day}\}$. Controls in columns 2–3 comprised of worker-level mean employment and wage levels at baseline. Controls in column 4 include worker-level indicators for any self-employment activities at baseline. Regressions include round (strata) fixed effects. Standard errors clustered at the village level in parentheses.

and employment outcomes using this post-shock data. Columns 1–3 document that the lean season spillover effects are transitory—lasting only as long as the hiring shocks do. Once the shock ends and all workers are back in the village labor force, we see no more employment spillovers and there remains no detectable difference in treatment and control village wages. This is what one would expect if the initial response to our hiring shock was due to rationing.³¹ It also rules out, for example, a wealth or aggregate demand effect explanation for our lean season results (see Section VII below).

In contrast, also consistent with excess labor supply, the semi-peak season hiring shocks do have persistent effects. After wages go up, they do not adjust back down after the transitory shock ends, leading to a drop in employment, consistent with a ratcheting effect from downward wage rigidity (Kaur 2019). These results point to a dynamic inefficiency in labor market adjustment.

VI. Results II: Rationing Implications

A. Self-Employment and Separation Failures

A worker who is rationed out of wage labor may remain involuntarily unemployed or engage in a productive activity such as self-employment to earn at least some money. In our setting, recall that 89 percent of workers in the spillover sample report

³¹In some earlier rounds, follow-up surveys were implemented with longer lags. The results in Table 7 are robust to restricting to rounds where follow-up surveys were within a month of the end of the hiring shock. In addition, note that changes in mean control group employment rates between the endline and follow-up surveys are not statistically different from each other.

some form of self-employment at baseline, ranging from farming to nonagricultural activities such as food preparation or selling firewood.

During the lean season, when job slots open up under the hiring shock, workers have the option to switch from self-employment to wage employment at the prevailing wage. Such a switch would indicate that these self-employed workers were rationed: they prefer wage employment over what they were previously doing. Switching between self- and wage employment is plausible in our context because the vast majority of individuals with a business participate actively in the casual labor market. In fact, 72 percent of those with a household business in the spillover sample report casual labor as their primary occupation and an additional 24 percent report casual labor as their secondary occupation.

We test for this pattern in Table 8. In lean months, the hiring shock leads to a 3.4 percentage point (24 percent) decline in self-employment days (column 1, p -value = 0.088).³² This implies that in the spillover sample, at least 24 percent of lean season self-employment stems from workers being rationed out of wage work. This is likely a lower bound, both because our test is constructed as such, and additionally because there may be fixed costs of switching from self-employment to wage employment for a short duration of time. This reduction in self-employment accounts for 62 percent of the employment spillovers documented in Table 5 above. Note that there is no clear prediction on what should happen to self-employment in semi-peak months, since the wage has gone up.³³

If workers switch to wage work, other family members may substitute into working on the family enterprise. While this in itself would not undermine our interpretation of rationing, in online Appendix Table B.8, we empirically examine the effect of the hiring shock on total household self-employment across all adult members. Consistent with the results in column 1 of Table 8, total household self-employment declines by 34 percent in lean months (column 1, p -value = 0.006).

Note that similar to the employment spillovers, the lean season effects on self-employment are also transitory. Once the hiring shock ends, self-employment levels between treatment and control villages are indistinguishable (documented above in Table 7, column 4). This rules out the concern that our self-employment results simply reflect intertemporal substitution; under this explanation, there should be an increase in self-employment in the two-week follow-up after the shock ends. If anything, there is a persistent decline in self-employment, potentially reflecting fixed costs.

In Table 8, columns 2 and 3 decompose the self-employment effects during the hiring shock. Nonagricultural self-employment declines by 3.3 percentage points, corresponding to a stark 50 percent decrease relative to the control group mean for small landholders (column 2). This reflects a sharp decline in nonagricultural self-employment across the village as a whole, with a 3.5 percentage point

³²This effect is more precise when we restrict to a smaller recall period of seven days, corresponding to a 31 percent decline (p -value = 0.004), consistent with less measurement error (see online Appendix Table B.6). In addition, in online Appendix Table B.7, we document that if we examine effects on hours of self-employment rather than days, results are similar although noisier, consistent with difficulty in recall of self-employment hours (Arthi et al. 2018).

³³This increases the opportunity cost of self-employment, and so could decrease own business work. Alternatively, this could also lead to an increase in self-employment—either to equate the shadow wage of self-employment with external wage work, or if smallholder farm households are liquidity constrained in their ability to pay for wage labor. The results indicate that on average, self-employment also declines in semi-peak months.

TABLE 8—SELF-EMPLOYMENT

	Self-empl. (1)	Self: nonagri (2)	Self: agri (3)	Self: agri (4)	Self: nonagri (5)	Self: agri (6)	Total farm labor (7)
Hiring shock	−0.0336 (0.019)	−0.0333 (0.011)	−0.0300 (0.023)	−0.0713 (0.027)	−0.0351 (0.011)	−0.105 (0.051)	−3.656 (1.377)
Hiring shock × above median land per capita				0.0648 (0.050)		0.196 0.080	4.705 (1.972)
Hiring shock × semi-peak	0.00289 (0.027)	−0.00337 (0.019)	0.0207 (0.027)	0.117 (0.035)	0.00636 (0.017)	1.172 (0.059)	4.799 (1.476)
Hiring shock × semi-peak × above median land per capita				−0.173 (0.059)		−0.265 (0.093)	−5.494 (2.060)
Sample	Spillover	Spillover	Spillover	Spillover	All potential workers	All potential workers	All potential workers
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>p</i> -value: shock + shock × semi-peak	0.118	0.0213	0.548	0.0605	0.0587	0.0179	0.0295
SE: shock + shock × semi-peak	0.0193	0.0151	0.0154	0.0239	0.0146	0.0273	0.504
Control mean: lean	0.139	0.0443	0.149	0.149	0.0312	0.163	6.248
Control mean: semi-peak	0.109	0.0441	0.0823	0.0823	1.406	0.0825	1.541
Observations	8,381	5,007	7,513	7,513	7,371	11,355	1,187
Level of observations	worker-days	worker-days	worker-days	worker-days	worker-days	worker-days	HHs

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment) in columns 1–4, and from all potential workers in the village with appropriate weights in columns 5–7. Self employment = 1 {worker self-employed that day}. Self employment: nonagri = 1 {worker self-employed in nonagriculture that day}. Self employment: agri = 1 {worker self-employed in agriculture that day}. Total farm labor = total number of worker-days (own family labor + hired labor) used on the farm over the past week. We restrict the sample to experimental rounds with non-zero self-employment at endline in column 1, non-zero non-agricultural self-employment in control villages at endline in columns 2 and 5, and non-zero agricultural self-employment at endline in columns 3, 4, 6, and 7. Controls include worker-level indicators for any self-employment activities at baseline. Regressions include round (strata) fixed effects. Standard errors clustered at the village level in parentheses.

decrease across all potential workers (column 5, p -value = 0.003). This is consistent with the fact that most nonagricultural businesses in our sample have no fixed assets, and a large fraction shut down in semi-peak months. Similarly, agricultural self-employment goes down by 3.0 percentage points, but this average effect is not statistically significant (column 3, p -value = 0.193).³⁴

Among farm households, a ration in the labor market is most likely to distort employment when landholdings are small (relative to the number of adult workers in the household) because the household's own farm will not be able to absorb all its labor.³⁵ Consistent with this, we find a sizable decline in agricultural self-employment among households with below median levels of per capita landholdings (Table 8, column 4). Among these small farms, there is a 7.2 percentage point reduction in agricultural self-employment during the lean season (off a base of 11.3 percentage

³⁴The analysis on agricultural work restricts to rounds where at least one person in the control village reports working at least one day on his farm. This drops five rounds from the estimates. Having zero agricultural work among all respondents indicates that the region is one where agriculture is nonexistent, or that the lean season production function is such that there is literally nothing to do on the land.

³⁵This is closely related to the ubiquitous observation that smaller farms tend to use more labor per acre than larger farms (e.g., Sen 1962, Bardhan 1973, Barrett 1996, Foster and Rosenzweig 2017). We find this relationship in our baseline data as well (online Appendix Figure A.4). See Gollin and Udry (2021) for a discussion of the role of measurement error in interpreting this relationship.

points in control villages). Similarly, across all potential workers, smallholder farmers decrease own-farm labor by 10.5 percentage points in lean months (column 6, p -value = 0.045), whereas larger farms show no change in own-farm labor supply. Our findings imply that among small farms in lean months, 64 percent of self-employment is driven by rationing. These workers would prefer to divert the majority of their farm-work time to wage labor at the prevailing wage if jobs were available. In column 7, we verify that this translates into a shift in total labor use on the farm: own family labor plus hired labor. Consistent with separation failures, in small farms, total labor use on the farm declines by 61 percent (p -value = 0.012) when rationing goes down. Among households with above-median land-holdings, there is no detectable decline in total farm labor; the differential impact of the hiring shock for this group fully offsets the decline for small farms (p -value = 0.022). This traces a direct link from labor rationing to separation failures.

B. Measuring Involuntary Unemployment in Surveys

In Table 9, we examine survey-based measures of unemployment status. In columns 1 and 2, we first begin by testing the effect of the hiring shocks on an indicator for whether worker i did any private sector work on day t in the local labor market (wage employment or self-employment). There is no detectable change in overall reported work status. This is consistent with “disguised unemployment”—because rationed workers had switched to other work activities, the hiring shocks in lean months appear inconsequential.

Next, we assess the traditional measure for involuntary unemployment used in surveys. This lists “would have liked to work but was unable to find any” as one of the options for the activity for that day. Workers can choose this option if they do not report having work in some other activity. This is how involuntary unemployment is measured in virtually all surveys, from India’s National Sample Survey to Labor Bureau surveys in the United States. However, when there is disguised unemployment—such that self-employment masks rationing—these measures would not accurately reflect labor market slack. Consistent with this, in columns 3 and 4, we cannot detect an effect of the hiring shocks on lean month involuntary unemployment: the coefficient is negative but insignificant (p -value = 0.173 and p -value = 0.242 respectively).

To address this challenge, we wrote an alternate survey question that asks workers to state whether they would have accepted a job at the prevailing wage that day over whatever else they had been doing on any days they did not have wage employment (i.e., even if they were self-employed). Consequently, it should be less sensitive to the presence of disguised unemployment. The exact phrasing of this question was, “Suppose someone offered you work at the prevailing wage in your village on this day. Would you have accepted the work?” To denote the “prevailing wage,” we used the phrase in the local language (Odiya) that denotes the standard going rate for a day of agricultural work in the village. Using this measure, the hiring shocks reduce involuntary unemployment by 7.0 percentage points in the lean season (column 5, p -value = 0.008). This magnitude closely matches the size of the revealed preference response from our employment spillover effects (see Table 5). Moreover, the lean season estimate in column 5 is statistically different from that in column 3 (p -value = 0.067).

TABLE 9—MEASURING INVOLUNTARY UNEMPLOYMENT

	Any work (1)	Any work (2)	Invol. unempl. (traditional) (3)	Invol. unempl. (traditional) (4)	Invol. unempl. (alternate) (5)	Invol. unempl. (alternate) (6)
Hiring shock	0.0222 (0.028)	0.0153 (0.023)	−0.0385 (0.028)	−0.0322 (0.027)	−0.0699 (0.026)	−0.0611 (0.026)
Hiring shock × semi-peak	−0.0264 (0.035)	−0.0282 (0.029)	0.0296 (0.039)	0.0300 (0.038)	0.0638 (0.039)	0.0647 (0.038)
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	Yes	No	Yes	No	Yes
<i>p</i> -value: shock + shock × semi-peak	0.842	0.477	0.741	0.932	0.837	0.900
SE: shock + shock × semi-peak	0.0207	0.0181	0.0267	0.0260	0.0297	0.0285
Control mean: lean	0.341	0.341	0.482	0.482	0.580	0.580
Control mean: semi-peak	0.398	0.398	0.395	0.395	0.540	0.540
Observations (worker-days)	8,906	8,906	8,906	8,906	8,906	8,906

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Any work = $\mathbf{1}\{\text{worker reports any work that day}\}$. Involuntary unemployment (traditional) = $\mathbf{1}\{\text{worker reports "would have liked to work but was unable to find any" as his activity status for that day}\}$. Involuntary unemployment (alternate) = $\mathbf{1}\{\text{worker states that they would have accepted a job at the prevailing wage over whatever else they had been doing that day}\}$ and equals zero if a worker had a wage job that day. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) fixed effects. Standard errors clustered at the village level in parentheses. Test comparing the hiring shock coefficients in columns 3 versus 5: *p*-value = 0.0674. Test comparing the hiring shock coefficients in columns 4 versus 6: *p*-value = 0.1147.

While this alternate question offers benefits over the traditional measure, and its movement contains signal about labor market conditions, it may suffer from its own issues. For example, it could overstate involuntary unemployment if business owners intertemporally substitute self-employment across days. As with any self-reported measure, it could also overestimate slack if workers are hesitant to admit that they are voluntarily unemployed, or searching for an unattainable job. Consistent with such concerns, the means of this variable are unrealistically high. For example, the sum of involuntary unemployment plus wage employment days is greater than workers' self-reported preferred "full-employment" rate of 60 percent (see Table 2). Overall, this highlights challenges with using self-reported survey measures.

C. Effects on Employers

If there is severe rationing in lean months, then removing a quarter of workers may not have any negative consequences on employers in the lean season. We examine this idea using proxies from an employer survey conducted at endline in online Appendix Table B.9. In lean months, the hiring shocks do not negatively disrupt agricultural work (columns 1 and 2), do not make it harder to find workers (columns 3 and 4), and do not change the perceived ability of workers (column 5).

In contrast, in semi-peak months, the hiring shocks appear to negatively impact employers using these proxies. Employers are 6.6 percentage points more likely to have to resort to hiring workers from outside the village, an 180 percent increase relative to the control mean (panel B column 3, *p*-value < 0.001). The results also suggest that employers did not undertake as many cultivation activities as they would have liked, or as early as they would have liked (panel B columns 1–2, *p*-value = 0.060

and p -value = 0.082, respectively). Interestingly, there is some indication that in the semi-peak season, the wage increase enables employers to attract more able workers, as evidenced by employer's ratings of worker ability (panel A, column 5). These results should only be taken as suggestive given that we did not survey a random sample of employers (see footnote 21).

Overall, these patterns are consistent with the idea of “surplus labor” in lean months only.³⁶ They match Leibenstein's predictions of “underutilized labor.” In lean times, there exist workers who can be removed from the labor market with little apparent impact. However, in semi-peak times, the marginal product of the marginal worker is meaningfully large.

D. Allocation Mechanism

In the presence of rationing, the wage no longer plays an allocative role—raising the question of the rationing mechanism: are higher ability workers hired first, or are job slots randomly assigned? In our setting, queuing by ability is possible for hiring within the village for agricultural work (since farmers know the workers) but less likely for casual work in the nonagricultural sector (where contractors come to villages to load workers onto trucks in a more arms-length fashion) (see Section I). If higher ability workers are hired first, then the employment spillovers we document should accrue to less able workers, who would be next in line for jobs.

We empirically investigate this in online Appendix Table B.10 by examining heterogeneous treatment effects by commonly used proxies for worker ability. In column 1, we find little evidence that in lean months, spillover effects accrue to workers who receive worse baseline ability ratings from employers in the village. In column 2, we do see suggestive evidence that those with higher baseline employment are less likely to benefit from employment spillovers: a one standard deviation increase in baseline employment rate is associated with a 4.41 percentage point reduction in the hiring shock treatment effect in the lean season, but the coefficient is insignificant (p -value = 0.149). In column 3, we find no detectable differences in employment spillovers by baseline wages. However, in our context, wages are unlikely to convey information about worker ability because casual wages are compressed, limiting the scope for any differential impacts (e.g., Breza, Kaur, and Shamdasani 2018). Overall, these findings do not offer conclusive evidence on whether there is queuing by ability—an interesting topic for future research.

The allocation mechanism is potentially consequential for employer surplus. Removing workers from the labor force will have no effect on employer profits under random rationing, but may decrease profits under queuing if enough high ability workers are removed. Note that this does not alter the interpretation of the amount of rationing detected by our hiring shocks: in both cases unemployed workers' marginal product is above the prevailing wage, in line with the definition of rationing (see Section VII below). In addition, if workers are heterogeneous in ability, this would imply that one can remove the lowest ability workers (whose marginal

³⁶ A crisper test of the surplus labor hypothesis would entail measuring impacts on yields or profits. However, since our shocks lasted as little as two to three weeks in some rounds, this would be an underpowered test.

product may still be above the prevailing wage) in lean months without any change in output—a more nuanced interpretation of the idea of “underutilized labor.”

VII. Discussion: Potential Microfoundations and Threats to Validity

We next turn to two important discussions. First, we consider possible microfoundations that could give rise to excess labor supply. This is important because many questions of efficiency and welfare depend on understanding why the labor market looks this way. Second, we consider several potential threats to validity that could, in principle, confound the interpretation of our results.

A. Potential Microfoundations

We examine possible microfoundations for rationing in light of our results. The canonical moral hazard efficiency wage model (Shapiro and Stiglitz 1984) produces involuntary unemployment in equilibrium. However, this model would predict that our hiring shocks would increase wages to maintain equilibrium unemployment to restore incentives.³⁷ It is also unclear why rationing would exist differentially in lean months relative to semi-peak ones. Nutrition efficiency wages can also generate involuntary unemployment (Dasgupta and Ray 1986). However, in semi-peak months, our finding of a ratcheting effect—wages remain higher even after the transitory hiring shock ends—is inconsistent with a nutrition efficiency wage, since wages should be able to fall back down to their preshock levels if the wage floor was based on the cost of purchasing a certain number of calories. Thus, both of these efficiency wage models are inconsistent with our findings.

In addition, dynamic contracting or implicit insurance models (e.g., in which employers smooth wages for their more risk averse workers across seasons) could generate rationing in some demand states (e.g., Azariadis 1975, Rosen 1985).³⁸ The most straightforward versions of these models should not generate a ratcheting effect on wages, leading to a drop in employment—contradictory to our results. However, our results do not definitively rule out the potential relevance of dynamic contracting between workers and employers.

Two other microfoundations are potentially more compatible with our results. First, a feature of workers' preferences may lead them to resist wage cuts (e.g., Keynes 1937; Akerlof and Yellen 1990; Fehr, Goette, and Zehnder 2009). Second, monopoly power on the part of workers could also generate rationing. In a similar setting, Breza, Kaur, and Krishnaswamy (2019) find evidence that workers behave as an informal union to prop up wages.³⁹

³⁷ As we discuss in Section IIB, our test is not powered to detect rationing from this class of models.

³⁸ Some versions of these models would imply that while there is rationing, it is not inefficient—the wage is simply not playing an allocative role. Our ratcheting effect suggests that rationing can lead to inefficiencies in employment levels.

³⁹ Work in the development labor literature has documented other frictions that can generate inefficiencies in employment levels, such as information frictions where the employer does not know if the worker is qualified for the job (e.g., Foster and Rosenzweig 1996, Beaman and Magruder 2012). The primary microfoundations we consider in this section are based on our definition of rationing. In our test, employers reveal that they consider the worker to be employable at the current wage, suggesting they knew that rationed workers are qualified.

Our findings only allow a suggestive glimpse into microfoundations. Ultimately, which microfoundations are responsible for rationing will determine its welfare and efficiency consequences. This provides impetus for future work on microfoundations.

B. 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. Any alternate explanation would need to explain all three of our lean month findings, and also explain why this confound would only operate in lean months but not peak months (during which we observe drastically different results).

Change in Composition of Workers.—There may be some selection into which workers sign up for the external worksite jobs, potentially changing the composition of workers left behind in treatment villages. If this decreases the average ability of workers in lean months, it could put downward pressure on the wage, counteracting the upward wage pressure from the supply shift, generating no change on average (confounding prediction L1). Note that for this to also generate prediction L3, the demand elasticity would need to be such that employers would still want to hire the same number of workers at w , even though worker ability has changed. More directly, in Section VIC, we show that employer perceptions of worker ability are no different in treatment versus control villages during lean months (online Appendix Table B.9 column 5). In addition, recall from Section VB that our wage results look very similar among those who signed up for the external job (Table 3, columns 1–5), and those who did not (Table 3, columns 6 and 7).

Note that our test allows for workers to be heterogeneous in ability and for some workers to be unqualified to work at the market wage. Even if the average ability of workers that are left behind did change, this would not confound our test 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. In other words, what matters for rationing is workers' absolute marginal product level being above w .

Wealth and Aggregate Demand 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 (confounding prediction L3). However, if this alternate explanation were true, this should put even more upward pressure on wages, which would be inconsistent with prediction L1.

Results from the two-week follow-up survey (Table 7, Section VF) also help rule out this confound. The laborers hired in the external jobs received their final wage payment on the last day of work. If households began consuming more, we would expect that immediately after the transitory hiring shock ends, there should still be a demand expansion. However, we find that as soon as the transitory shock ends, the labor market returns exactly to where it was before the shock, indicating that any wealth impacts on employment are minimal.

Substitution in Work Activities across Household Members.—In online Appendix Table B.11, we show impacts of the hiring shock on family members of spillover sample workers.⁴⁰ If other household members are also participants in the casual wage market, we should expect their employment to mirror that of our spillover sample: positive employment spillovers and reduction in self-employment in lean months. In column 2, we detect positive lean-season hiring spillovers onto the family members that participate in the casual labor market, providing robustness to our main findings. In column 5, we find a negative and statistically significant impact of the lean-season hiring shock on self-employment, implying that total work days in household enterprises fall when labor market opportunities improve. This indicates that substitution in activities across household members is not driving our results.

Integrated Labor Markets.—If the labor market is integrated across villages, our hiring shock would only constitute a small supply shock to employers, potentially explaining the lack of wage effects in lean months. However, in this case, we should also see no employment spillovers in the lean season. This could also not explain why wages rise in semi-peak months.

Perfectly Elastic Labor Supply.—If labor supply is perfectly elastic at the market wage, this could generate predictions L1–L3 even when there is no rationing. Online Appendix Figure A.5 summarizes results from a labor supply elicitation exercise, conducted in the same districts as our experimental sample. Breza, Kaur, and Krishnaswamy (2019) partner with agricultural employers to randomize individual wage offers to workers in the local labor market. When the wage offer is reduced from the prevailing wage to a 10 percent cut, take-up of the job declines from 26 percent to 18 percent.⁴¹ This suggests that labor supply in this setting is far from perfectly elastic.

Power to Detect Wage Increases.—Because there is a confidence interval around our wage effect estimates, one potential concern is that in the lean season, there may have been a meaningful wage increase (contradicting L1), but we are underpowered to detect it precisely. We examine this concern by taking the confidence intervals around the wage and employment effects seriously. Suppose the wage increased by the magnitude corresponding to the right side of the wage effect confidence interval: 1.92 percent, and that aggregate employment does not change. The 95 percent confidence interval around the employment spillover effect in lean months is [0.018, 0.091], corresponding to a percentage increase of [12.4 percent, 62.6 percent]. From the perspective of an individual (atomistic, price-taking) worker, this would indicate that seeing a wage increase of 1.92 percent led him to increase his labor supply by 12.4–62.6 percent. The labor supply elasticity required to induce this employment response for an individual worker remaining in the village is in the range of

⁴⁰ Each spillover sample respondent reported the number of days in the prior week that other adult household members worked in wage employment and self-employment.

⁴¹ These estimates use the take-up of jobs under the “private” condition. We view this as reflecting true residual labor supply and offering the best estimate of the residual supply elasticity under a wage increase—since that is the relevant direction for wage changes under our negative supply shock.

6.47–32.59.⁴² This is implausibly high and substantively larger than the residual labor supply elasticity estimates derived by Breza, Kaur, and Krishnaswamy (2019) and Goldberg (2016). The aggregate semi-peak labor supply elasticity implied by our experimental results is also much smaller (see online Appendix E). Such an overly large individual supply elasticity estimate is exactly what one would expect under excess labor supply.

Repeated Contracts and Implicit Insurance.—Because workers and employers participate in the same local labor market, there is potential scope for repeated or reciprocal relationships (e.g., Bardhan 1983; Mukherjee and Ray 1995; Anderson, Francois, and Kotwal 2015; Rosen 1985).

One might worry that repeat relationships might mask wage increases in lean months because employers could compensate workers through other channels such as promises of future work. Such concerns amount to potential mismeasurement of the wage (contradicting L1). However, in Table 4, workers do not report differential expectations of future work between treatment and control villages in either season. Moreover, if these unpriced favors are costly to the employers, then we should also expect aggregate village employment to decrease in treated villages in lean months, which we do not observe empirically.

It might also be the case that workers and employers insure one another against unanticipated labor market shocks, such as the experimentally induced shock studied here. While possible in principle, such an explanation would involve poorer workers providing insurance to the wealthier employers, whereas the literature normally posits insurance flowing the opposite way. In addition, our semi-peak findings would indicate that insurance provision by workers only occurs in lean times, when workers themselves are poorest.

Finally, as we discuss in Section VIIA above, there is a class of dynamic contracting or implicit insurance models that could generate labor rationing. However, in this case, these would not be a confounding explanation but rather a potential microfoundation for our findings.

VIII. Extension: Labor Market Shocks and Labor Market Slack

Our experimental results highlight how the same labor supply shock can have dramatically different impacts in periods of high versus low labor market slack. This finding has implications for labor market policies. In online Appendix Figure A.6, we document high levels of variation in employment rates across districts in India (panel A) and across the months of the year (panel B). The interquartile range of cross-district employment levels spans 0.76 to 2.07 days per week. Moreover, the median state has more than a 100 percent employment increase between the month with the lowest versus highest employment rate within the year. If baseline employment rates correlate with labor market slack, then this spatial and temporal variation

⁴²For this exercise, we allow for correlation between the wage and employment spillover regressions when calculating the confidence intervals using seemingly unrelated regression. If instead, we use the confidence intervals directly from the regressions in Tables 3 and 5 (assuming independence), the implied labor supply range elasticity is 4.82–25.68.

suggests that the same policy implemented nationwide and yearlong might have heterogeneous impacts, absorbing rationed workers in some cases and crowding out private sector employment in others.

In a suggestive exercise, we analyze India's workfare program, the National Rural Employment Guarantee Act (NREGA), through this lens. Like many workfare programs around the world, NREGA guarantees work in public works at a government-mandated wage. While such programs primarily operate during agricultural lean seasons, NREGA does occur throughout the year (online Appendix Figure A.7).

We note that our experimental hiring shock 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 and shifts the entire labor supply curve, even among those who never participate in the program. Consequently, such programs are more likely to lead to wage increases than our shock, which was both transitory and available only to the participants we randomly selected.

We examine the phased rollout of NREGA across districts in India, following Imbert and Papp (2015). Further, we consider differences in the program's impacts by baseline employment levels. We use worker-level employment data from the National Sample Survey, rounds 61 (July 2004–June 2005) and 64 (July 2007–June 2008). The NREGA phase-in began in 2006, and by April 2007, 330 of India's 610 districts had received the program. We estimate the following triple-differences regression:

$$(2) \quad y_{idmt} = \beta NREGA_{dt} + \gamma NREGA_{dt} \times High_{dm} \\ + \delta High_{dm} + \rho_d + \rho_{mt} + \lambda X_{idmt} + \varepsilon_{idmt},$$

where i indexes the individual, d indexes the district, m is calendar month and t is year. The variable $NREGA_{dt}$ is the program indicator, equal to one in districts that had already received the program in round 64; $High_{dm}$ is a binary indicator for whether a district \times month pair had above-median casual employment (low slack) prior to NREGA, averaging across households surveyed in rounds 60 and 61.⁴³ Note that this captures both cross-district spatial variation and within-district seasonal variation in casual employment. The terms ρ_d and ρ_{mt} denote district and month-by-year fixed effects. The term X_{idmt} is a vector of controls, including worker characteristics by round (gender, education, age), baseline agricultural yields by round, and district-by-month baseline means of casual employment, wages, and self-employment. We predict that when there is less slack in the labor market, labor market shocks such as NREGA should put more upward pressure on wages, leading to relatively more crowd-out of private sector employment, $\gamma_w > 0$ and $\gamma_e < 0$.⁴⁴

⁴³In some cases, the survey months for a district in rounds 60 and 61 do not overlap with the survey months in round 64. We thus control for whether the heterogeneity measure is not well defined for a round 64 observation. We code such cases as not having high baseline employment.

⁴⁴We do not have strong predictions on β_w and β_e given the size of the NREGA shock and its relevance for the wages of *all* workers in the labor market.

We view this exercise as merely suggestive because of one important caveat. While the national program phase-in can be taken as quasi-exogenous under the assumptions laid out in Imbert and Papp (2015), the size of the NREGA program in a given district-month is endogenous. Importantly, the demand and supply of NREGA jobs, conditional on having the program, may be directly related to labor market slack at the time. Consequently, we cannot use findings from this analysis to make any strong causal claims.

Online Appendix Table B.12 presents the results. There is a strong first stage: districts that received the program by 2007 have more employment in public works (column 1). If anything, there are more public works in settings with higher baseline employment, though the coefficient is not statistically significant.

Consistent with our predictions, we find that labor market slack mediates NREGA's reduced-form impacts. In column 2, we find no evidence for NREGA-driven wage increases in places with below-median employment rates. However, the daily wage rises in places with above-median employment rates (p -value = 0.055). In columns 3 and 4, we find no evidence of private sector employment crowd-out in district-months with below-median baseline employment. However, NREGA does put downward pressure on private sector employment in district-months with above-median employment. The program crowds out agricultural employment by almost 20 percent (p -value = 0.031) when the labor market appears tighter. Finally, the program decreases self-employment when there is more slack (column 5); consistent with Section VIA, we observe people switching out of self-employment, despite no change in the wage.

This suggests that programs like NREGA can be implemented with little crowd-out in places and times of year where slack is high. However, when slack is low, the story is different. If the policy goal is to provide work opportunities to rationed workers, then this could be accomplished at substantially lower cost (and reduced private sector crowd-out) by improved geographical and temporal targeting.

IX. Conclusion

We document the coexistence of two markedly different labor market paradigms. During lean periods, over a quarter of labor supply is rationed, while during semi-peak periods, the labor market adjusts rapidly to shocks. These patterns of seasonality match Leibenstein's predictions of "underutilized labor." Moreover, during lean periods, we find no evidence of adverse impacts on employers in treated labor markets. Taken at face value, this suggests that workers can be "removed" from the labor market, but only during slack periods. Because semi-peak season aggregate employment does decrease in response to a labor supply shock, permanently "removing" workers through long-term out-migration, for example, would likely have negative productivity impacts.

Our results have important implications for rural labor market analysis. The prevalence of rationing in our context suggests that there are periods when workers are not on their labor supply curve, and subsequently, wages do not play an allocative role. Therefore, in these settings, prices are uninformative (de Janvry, Fafchamps, and Sadoulet 1991)—the wage does not reflect the marginal product of labor. Moreover, analyses that aggregate labor market data to an annual horizon, averaging

across lean and peak periods, will likely produce misleading estimates. This underscores the need to incorporate seasonality into labor market analysis.

As discussed above, these findings also have implications for the design of labor market policies. We provide suggestive evidence that private sector crowd-out from workfare programs is mediated by underlying labor market conditions. Our results also have implications for designing incentives for temporary migration (Bryan, Chowdhury, and Mobarak 2014; Akram, Chowdhury, and Mobarak 2017). However, we also acknowledge that some policy decisions require a better understanding of the specific microfoundations that give rise to labor rationing. That we find so much excess labor supply should be an impetus for more research into understanding its causes.

Finally, our lean season results highlight how high rates of self-employment in our context make it difficult to diagnose rationing. We show that in lean seasons, so-called “disguised” unemployment (Singh, Squire, and Strauss 1986) explains 24 percent of the labor workers supply to household enterprises in lean months, with this number rising to 64 percent for smallholder farms. This also sheds light on the nature of many small businesses in developing countries. If at least a quarter of self-employment among workers is quickly abandoned in favor of wage work, then perhaps it is not surprising that the average developing country business shows low growth and low returns to blanket interventions such as credit or skills training (Banerjee, Karlan, and Zinman 2015; McKenzie and Woodruff 2014). This view is supported by de Mel, McKenzie, and Woodruff (2010); Schoar (2010) and Adhvaryu, Kala, and Nyshadham (2019); who relate low-productivity entrepreneurship by workers to poor labor market prospects. This creates a rationale for targeting interventions like subsidized capital drops or skills training on “true,” rather than “forced” entrepreneurs (e.g., Banerjee et al. 2019; Hussam, Rigol, and Roth 2020).

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