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Nominal Wage Rigidity in Village Labor Markets†

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This paper develops a new approach to test for downward wage rigidity by examining transitory shocks to labor demand (i.e., rainfall) across 600 Indian districts. Nominal wages rise during positive shocks but do not fall during droughts. In addition, transitory positive shocks generate ratcheting: after they have dissipated, wages do not adjust back down. Ratcheting reduces employment by 9 percent, indicating that rigidities distort employment levels. Inflation, which is unaffected by local rainfall, enables downward real wage adjustments—offering causal evidence for its labor market effects. Surveys suggest that individuals believe nominal wage cuts are unfair and lead to effort reductions. (*JEL* E24, E31, J23, J31, O15, O18, R23)

This paper empirically examines downward nominal wage rigidity and its employment consequences in a developing country context. As is the case with any price, the wage allocates labor, by far the biggest factor input, especially in developing countries, to production. Adjustments in the wage are therefore what facilitate the labor market response to shocks. Rigidities may prevent wages from adjusting fully to shocks, with potentially important consequences for employment, earnings, and output. A large literature in economics has discussed these implications.[1] For example, if wages do not fall during negative shocks, this may increase layoffs, deepening the impact of recessions and exacerbating business-cycle volatility. In addition, the labor rationing generated by rigidities could give rise to "disguised unemployment" or "forced entrepreneurship," creating a misallocation of labor across firms (Singh, Squire, and Strauss 1986).

Some early work in development argued for the presence of nominal rigidities. For example, Drèze and Mukherjee (1986, p. 240) observe that in casual daily labor markets in Indian villages, "The same standard wage often applies for *prolonged*

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[1] For overviews, see, e.g., Tobin (1972); Greenwald and Stiglitz (1987); Blanchard (1990); Clarida, Galí, and Gertler (1999); Akerlof (2002); and Galí (2008).

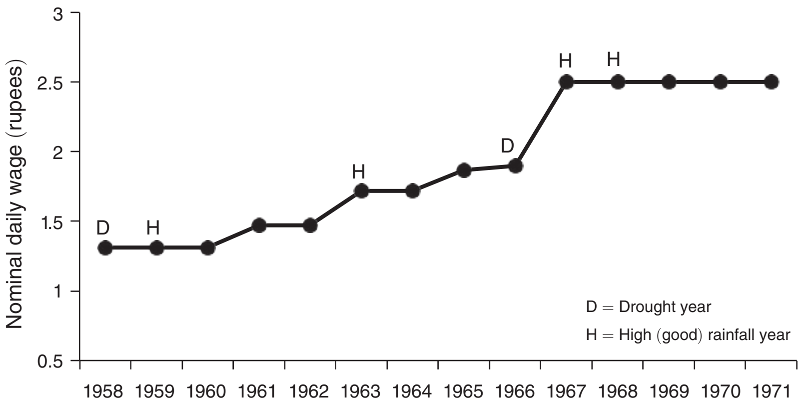

Figure 1. Evolution of the Prevailing Nominal Daily Wage in an Indian Village

Notes: This motivational figure plots the prevailing daily nominal wage for plowing in the Indian village of Tinur, Tamil Nadu during the month of April from 1958–1971. The letters *D* and *H* signify years in which there was a drought (rain below the twentieth percentile of the district's historical rainfall distribution) or very good rainfall (rain above the eightieth percentile of the district's historical rainfall distribution), respectively.

Source: These wages were reported in *Agricultural Wages in India*, published by the Government of India.

periods—from several months to several years … The standard wage (in money terms) … appears to be, more often than not, rigid downward during the slack season." Historical time series data from the Indian village of Tinur, for example, appear consistent with such observations (Figure 1) (Directorate of Economics and Statistics 1971). The prevailing wage follows a step-ladder progression: adjusting upward every few years and with no apparent downward nominal adjustments over a 12-year period, including in drought years.

Looking across a set of 256 districts in India, the distribution of nominal wage changes exhibits a bunching of mass at zero, with a discontinuous drop to the left of zero (Figure 2).[2] These patterns, however, could arise from measurement error such as rounding bias in reported wages. In addition, to the extent that such evidence supports wage rigidity, it does not provide insight on whether rigidities have any real consequences for employment.

These challenges apply more broadly to documenting wage rigidity in any context. The approach in existing work, almost all of which uses data from OECD countries, is based on examining distributions of wage changes, as in Figure 2. This has provided compelling documentation in OECD countries (e.g., Akerlof et al. 1996; Kahn 1997; Card and Hyslop 1997; Dickens et al. 2007; Barattieri, Basu, and Gottschalk 2014; Ehrlich and Montes 2018).[3] However, this approach has made it difficult to directly examine the potential employment effects of rigidities.[4] There

[2] Under a continuous distribution of shocks, one may not expect a large discrete and asymmetric jump at nominal zero changes (McLaughlin 1994, Kahn 1997). In contrast, the distribution of real wage changes in Figure 2 appears continuous and symmetric around zero.

[3] However, more recently, studies have failed to find downward rigidity using this approach. This has led to mixed evidence for downward rigidity in the aftermath of the Great Recession (Fallick, Lettau, and Wascher 2016; Elsby, Shin, and Solon 2016; Verdugo 2016).

[4] This approach typically limits analysis to workers employed by the same firm in consecutive years. This also creates challenges for inference: if workers quit when they anticipate wage cuts, then wage cuts will appear less

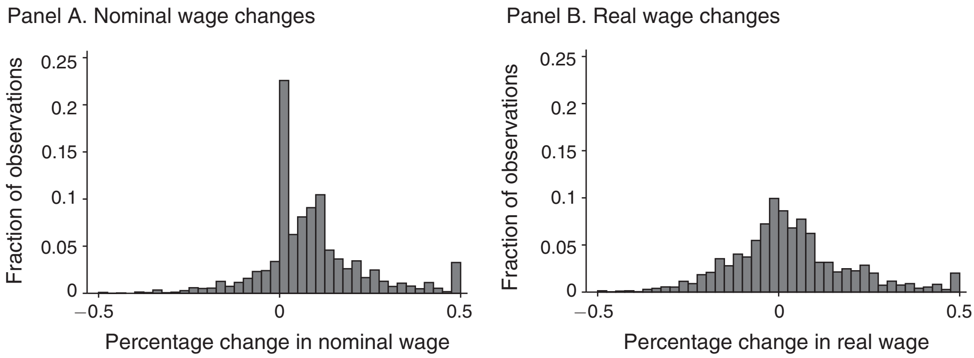

Figure 2. Distributions of Wage Changes

Notes: The figure plots year-to-year percentage changes in agricultural wages (0.5 = 50 percent change). Unit of observation is a district-year. Nominal wage changes are shown for the full sample (7,680 observations). Real wages are computed as the nominal wage divided by the state CPI for agricultural workers, for the years in which state CPI data are available (6,850 observations). Wage changes are top coded at 50 percent and bottom coded at –50 percent.

Source: World Bank Climate and Agriculture dataset (256 districts from 1956–1987)

is little direct evidence that wage rigidity actually affects employment in the labor market in any setting.[5]

In this paper, I develop a different approach to test for wage rigidity: I isolate shocks to the marginal revenue product of labor, and examine wage adjustment and employment effects in response to these shocks.[6] I apply this approach in the context of markets for casual daily agricultural labor, a major source of employment in poor countries. In this setting, local rainfall variation generates transitory labor demand shocks. I investigate responses to these shocks in over 600 Indian districts from 1956 to 2009. My identification strategy relies on the assumption that rainfall shocks are transitory: monsoon rainfall affects total factor productivity (TFP) in the current year, but does not directly affect TFP in future years. I validate this assumption below.

Wage adjustment is consistent with downward rigidities. First, adjustment is asymmetric. Relative to no shock, nominal wages rise in response to positive shocks, but are no lower during negative shocks on average. Second, transitory positive shocks generate ratcheting. When a positive shock in one year is followed by a non-positive shock in the following year, nominal wages do not adjust back

frequent than they actually are. On the other hand, measurement error can make wage cuts appear more frequent than they actually are.

[5] A notable exception is Card (1990), who examines union workers whose nominal wages are explicitly indexed to expected inflation. As a result, real wages cannot adjust to inflation surprises, leading firms to adjust employment. Card and Hyslop (1997) examine whether periods of higher inflation are correlated with smaller impacts of negative shocks on unemployment in labor markets in the United States, and do not find evidence for a relationship. There remains a debate as to whether wage rigidity has any relevance for employment dynamics (e.g., Pissarides 2009, Elsby 2009, Rogerson and Shimer 2011, Schmitt-Grohé and Uribe 2013).

[6] Holzer and Montgomery (1993) perform analysis in this spirit. They assume sales growth reflects demand shifts, and examine correlations of wage and employment growth with sales growth in the United States. They find that wage changes are asymmetric and are small compared to employment changes.

down: they are higher than they would have been in the absence of the lagged transitory positive shock.

Third, particularly consistent with *nominal* rigidity, inflation moderates these wage distortions. In the presence of nominal rigidities, inflation will enable real wages to adjust downward without requiring any nominal wage cuts. Because local rainfall shocks do not affect—and are therefore uncorrelated with—inflation, this enables a causal test of whether inflation affects real wage adjustment. I find that when inflation is higher, negative shocks are more likely to result in lower real wages, and previous transitory positive shocks are less likely to have persistent wage effects. When inflation is above 6 percent, I cannot reject that lagged positive shocks have no impact on current real wages. In contrast, inflation has no differential effect on upward real wage adjustment to current positive shocks, consistent with downward nominal rigidities. These findings support the hypothesis that inflation "greases the wheels" of the labor market.

When rigidities bind, keeping real wages above market clearing levels, this distorts employment. If a district experiences a transitory positive shock (and therefore has a ratcheted wage in the following year), total agricultural employment is 9 percent lower in the following year than if the lagged positive shock had not occurred.[7] In contrast, these shocks have no effect on non-agricultural hiring. Overall, these employment dynamics are consistent with boom and bust cycles in village economies. They also match observations from other contexts that labor markets exhibit relatively large employment volatility and small wage variation.

The brunt of the employment decreases after lagged positive shocks is borne by poorer individuals—the landless and small landholders—who are the primary suppliers of hired agricultural labor. When they are rationed out of the external labor market, small landholders increase labor supply to their own farms. These findings are consistent with the prediction that labor rationing will lead to "disguised unemployment" and separation failures, with smaller farms using labor more intensively in production than larger farms (Singh, Squire, and Strauss 1986; Benjamin 1992).[8]

Could the findings above be explained by factors other than nominal wage rigidity? There are two categories of potential concerns. The first is a violation of the assumption that shocks are transitory. The second is that rainfall affects labor supply or demand through other channels, such as migration or capital accumulation. While such explanations could account for a portion of my findings, I argue that the full pattern of results—wages, employment, and inflation—is most consistent with downward nominal wage rigidity. In addition, in supplementary analyses, I fail to find evidence in support of such alternate explanations.

The results point to the relevance of nominal rigidities in a setting with few of the institutional constraints that have received prominence in the empirical literature on wage rigidity. In villages, minimum wage legislation is largely ignored and formal

[7] Total agricultural employment is total worker-days spent in farm work, whether on one's own land or as hired labor on someone else's land. This effect is driven by a decrease in hired employment.

[8] In the presence of rationing, a household's labor supply decision will not be separable from its decision of how much labor to use on its farm. This is a prominent hypothesis for why smaller farms tend to use more labor per acre and have higher yields per acre than larger farms, a widely documented phenomenon in poor countries (e.g., Bardhan 1973, Udry 1996). These results lend some support to this hypothesis. Behrman (1999) reviews the empirical literature on separation failures.

unions are rare (Rosenzweig 1980, 1988). Wage contracts are typically bilaterally arranged between employers and workers and are of short duration (usually one day), making it potentially easier for contracts to reflect changes in market conditions (Drèze and Mukherjee 1986).

A growing body of evidence argues that nominal wage cuts are perceived as unfair, causing decreases in worker productivity.[9] Following Kahneman, Knetsch, and Thaler (1986), I presented 396 agricultural laborers and employers in 35 villages across 6 districts with scenarios about wage setting behavior, and asked them to rate the behaviors as fair or unfair on a 4-point scale. The results suggest that nominal wage cuts violate fairness norms. For example, the majority of respondents thought it was unfair to cut nominal wages after a surge in unemployment (62 percent) or during a severe drought (64 percent). In contrast, relatively few people thought that a real wage cut is unfair if it is achieved through inflation (9 percent). Respondents also expressed a strong belief that workers decrease effort when fairness norms are violated.[10]

This paper is closely linked to the literature on labor market distortions in poor countries. Early theoretical work in development focused heavily on labor market imperfections.[11] However, there has been no direct empirical documentation of downward wage rigidity in this setting to date. There is a broader empirical literature on the functioning of labor markets in developing countries. Some studies find results consistent with competitive markets exhibiting real wage and employment adjustments to shocks (Rosenzweig 1980; Benjamin 1992; Jayachandran 2006; Mobarak and Rosenzweig 2014; Imbert and Papp 2015; Muralidharan, Niehaus, and Sukhtankar 2017). Other studies find evidence consistent with imperfections such as separation failures (Bardhan 1973; Udry 1996; Behrman, Foster, and Rosenzweig 1997; Barrett, Sherlund, and Adesina 2008; Foster and Rosenzweig 2017; LaFave and Thomas 2016) and rationing (Breza, Kaur, and Shamdasani 2019; Breza, Kaur, and Krishnaswamy 2019).[12] These two strands of evidence should not be viewed as contradictory. The findings in this paper indicate that in this setting, real wages do adjust often in response to market forces and play an allocative role. However, in cases when nominal rigidities bind, thereby distorting real wages, this affects employment, with the potential to contribute to labor market imperfections.

The rest of the paper proceeds as follows. Section I presents a model of nominal wage rigidity. Section II lays out the empirical strategy and Section III presents the results. Section IV evaluates whether explanations other than nominal rigidity are consistent with the results. Section V discusses mechanisms and presents survey

[9] Individual responses to a range of scenarios suggest the relevance of nominal variables (Shafir, Diamond, and Tversky 1997). Employers express perceptions that nominal wage cuts damage worker morale, with potential consequences for labor productivity (Blinder and Choi 1990, Bewley 1999). See Fehr, Goette, and Zehnder (2009) for a broader discussion of the relevance of fairness preferences in labor markets.

[10] Of course, survey responses may not reflect actions under real stakes. To the extent that these responses reflect fairness norms, they do not provide insight on the micro-foundations for these norms.

[11] For example, Lewis (1954); Eckaus (1955); Rosenstein-Rodan (1956); Leibenstein (1957); Shapiro and Stiglitz (1984); Singh, Squire, and Strauss (1986). Many of the early theories for labor rationing have not withstood empirical scrutiny. Rosenzweig (1988) provides an excellent review of the evidence for some of these theories, such as nutrition efficiency wages.

[12] Other recent papers explore other related topics, such as labor supply elasticity (Goldberg 2016), credit and labor allocation (Fink, Jack, and Masiye 2018), and migration (Morten 2019; Bryan, Chowdhury, and Mobarak 2014; McKenzie, Theoharides, and Yang 2014).

evidence for the role of fairness norms in villages. Section VI concludes. The online Appendix contains all Appendix materials, including Appendix figures and tables.

I. Model

I model a small open economy with decentralized wage setting and exogenous product prices. Rigidities arise because workers view nominal wage cuts as unfair, and retaliate to such cuts by decreasing effort.[13] I use this framework to develop testable implications of fairness preferences on labor market outcomes. For simplicity, what follows is a static model of the labor market, in which employers and workers make decisions about the current period, taking the previous period's wages as given. At the end of the section, I discuss implications of a multi-period dynamic setting.

A. *Setup*

The labor force is comprised of a unit mass of potential workers. All workers are equally productive. They are indexed by parameter $\phi_i \sim U[0, \bar{\phi}]$, which equals worker i's cost of supplying 1 unit of effective labor. The worker's payoff from accepting a nominal wage offer of w equals the utility from consuming her real wage minus the disutility of working: $u(w/p) - \phi_i eR(\lambda, w, \bar{w}_{t-1})$, where p is the price level and $R(\cdot)$ captures reference dependence in utility around the previous period's average market wage, $\bar{w}_{t-1}$.[14] Specifically, I assume $R(\lambda, w, \bar{w}_{t-1}) = 1 + ((1-\lambda)/\lambda)\mathbf{1}\{w < \bar{w}_{t-1}\}$. This means that when $w < \bar{w}_{t-1}$, the disutility of work, $\phi_i e$, is scaled up by $(1-\lambda)/\lambda$, where $\lambda \in (0,1]$. The case of $\lambda = 1$ corresponds to the benchmark of no reference dependence. Note that time subscripts are omitted from w, p, and e for simplicity of notation, since all results in the model will pertain to period t (the current period), taking as given $\bar{w}_{t-1}$.

A market-wide fairness norm governs effort behavior. The worker usually exerts a standard amount of effort: $e = 1$. However, when she feels treated unfairly by the firm, she reduces effort to exactly offset the disutility from the fairness violation:

$$e = \begin{cases} 1 & \text{if } w \geq \bar{w}_{t-1} \\ \lambda & \text{if } w < \bar{w}_{t-1} \end{cases}. \tag{1}$$

Consequently, worker i's payoff from accepting wage offer w always reduces to $u(w/p) - \phi_i$. In the model, I take this fairness norm as exogenous.[15] More generally, it can be conceptualized as the reduced form for a strategy in a repeated game. I normalize the payoff from not working as 0. When all firms offer w, aggregate labor supply is: $L^S = (1/\bar{\phi})\, u(w/p)$.

[13] In Section V, I provide support for this modeling assumption using survey evidence.

[14] In Indian villages, at any point in time, there is a gender-specific prevailing wage; any agricultural worker employed in the village is typically paid this wage. Thus, the average market wage in the previous period would also correspond to the individual's own wage in the previous period.

[15] Other fairness norm-based efficiency wage models of wage rigidity (e.g., Akerlof and Yellen 1990; Eliaz and Spiegler 2013; and Benjamin 2015) also assume exogenous rules for effort decreases.

There are J firms (indexed by j), where J is large so that each firm's wage contributes negligibly to the average market wage. Firm j's profits from hiring L_j workers at nominal wage w_j equals

$$\pi_j = p\theta f(eL_j) - w_j L_j, \tag{2}$$

where $f(\cdot)$ is a continuous, increasing, twice-differentiable concave function, and output depends on effective labor, eL_j. I assume θ is a non-negative stochastic productivity parameter whose realization is common to all firms. In the empirical strategy, θ corresponds to the current year's rainfall realization.

All firms simultaneously post a wage. Firms satisfy labor demand in descending order of posted wages. If multiple firms post the same wage, those firms proceed in random order. For simplicity, I assume each firm hires the available workers with the lowest ϕ-values that are willing to work for it.[16]

B. *Benchmark Case: No Rigidity*

In the benchmark case (i.e., when $\lambda = 1$), $e = 1$ for all wage levels. Firm j's profits are therefore $\pi_j = p\theta f(L_j) - w_j L_j$. I focus on the symmetric pure strategy Nash Equilibrium, in which all firms offer the same wage:[17] $w_j = w^*(\theta, p)\ \forall j$, where $w^*(\theta, p)$ will be used to denote the equilibrium wage level in the benchmark case. The firm's first-order condition pins down the optimal choice of labor:

$$p\theta f'(L^*) = w^*. \tag{3}$$

The market clearing condition is

$$JL^* = \frac{1}{\phi} u\left(\frac{w^*}{p}\right). \tag{4}$$

LEMMA 1 (Market Clearing in Benchmark Case): *If workers do not exhibit fairness preferences, the unique pure strategy symmetric Nash Equilibrium will satisfy conditions* (3) *and* (4). *The labor market will clear for all realizations of θ.*

PROOF:

See online Appendix B.1.

Note that (3) and (4) correspond exactly to the conditions in a competitive equilibrium.

[16] Specifying an allocation mechanism by which workers are matched to firms is needed to formalize the impact of off-equilibrium deviations on firm profits in the model proofs. The mechanism described here ensures that the firms offering the highest wage receive priority in hiring. In addition, it maximizes gains from trade in the narrow sense that for a given wage offer, those workers who would benefit the most from employment (the lowest ϕ workers) are the ones who get the job.

[17] Since all employers in a village typically pay the same prevailing wage in this setting, it is reasonable to focus on pure strategy symmetric equilibria.

COROLLARY (Null Hypotheses):

(*i*) *The equilibrium wage will be monotonically increasing in* θ. *If* $\theta' < \theta''$, *then* $w^*(\theta',p) < w^*(\theta'',p)$.

(*ii*) *The equilibrium wage,* $w^*(\theta,p)$, *is not affected by the previous period's wage,* $\bar{w}_{t-1}$.

(*iii*) *The price level has no impact on the real wage. Consequently, for any* $\theta' < \theta''$, $\left(w^*(\theta'',p)/p - w^*(\theta',p)/p\right)$ *is not affected by changes in* p.

Null Hypotheses (i) and (ii) follow directly from Lemma 1. For (iii), it is straightforward to verify from (3) and (4): $\partial w^*(\theta,p)/\partial p = w^*/p$ and $\partial L^*(\theta,p)/\partial p = 0$. If there is a price increase, firms raise nominal wages to keep real wages constant and employment therefore does not change. Consequently, the difference in the equilibrium real wage under two different θ-realizations will also be independent of p.

C. *Downward Rigidity at the Previous Period's Wage*

I now turn to examine the implications of fairness preferences. Equation (2) indicates that for any (w_j, L_j) combination, profits are always weakly lower in the fairness case than the benchmark case.

In the symmetric pure strategy Nash equilibrium: $w_j = \bar{w}(\theta, p, \bar{w}_{t-1})\ \forall j$, where $\bar{w}(\theta, p, \bar{w}_{t-1})$ denotes the equilibrium wage level corresponding to total factor productivity (TFP) θ, price p, and the previous period's wage $\bar{w}_{t-1}$ in the fairness case. All firms demand the same amount of labor, $\bar{L}(\theta, p, \bar{w}_{t-1})$. For a given $\bar{w}$, this is pinned down by the firm's first-order condition, which is discontinuous around $\bar{w}_{t-1}$:

$$(5) \qquad \bar{w} = \begin{cases} p\theta f'(\bar{L}) & \text{if } \bar{w} \geq \bar{w}_{t-1} \\ p\theta\lambda f'(\lambda\bar{L}) & \text{if } \bar{w} < \bar{w}_{t-1} \end{cases}.$$

When $\bar{w} \geq \bar{w}_{t-1}$, this corresponds exactly to the first-order condition in the benchmark case. However, when $\bar{w} < \bar{w}_{t-1}$, retaliation by the firm's workers makes them less productive. I assume $f'(\bar{L}) > \lambda f'(\lambda\bar{L})$ for $\lambda < 1$. This implies that at wages below $\bar{w}_{t-1}$, firms demand less labor than in the benchmark case. Note that this condition holds for many common production functions, such as Cobb-Douglas: $f(eL) = (eL)^\alpha$.

Implicitly define θ_R as

$$(6) \qquad w^*(\theta_R, p) = \bar{w}_{t-1}.$$

In other words, θ_R is the unique value of θ at which $\bar{w}_{t-1}$ would be the market clearing equilibrium wage. Proposition 1 establishes asymmetric wage adjustment around θ_R.

PROPOSITION 1 (Asymmetric Adjustment to Shocks): *In the unique pure strategy symmetric Nash equilibrium:*

(*i*) $\theta < \theta_R$: *For a range of productivity realizations below* θ_R, *there will be no downward wage adjustment. Wages will remain fixed at the previous period's wage and there will be excess supply of labor. Specifically, there exists a* $\tilde{\theta}_R < \theta_R$ *such that for all* $\theta \in (\tilde{\theta}_R, \theta_R)$, $\bar{w}(\theta, p, \bar{w}_{t-1}) = \bar{w}_{t-1} > w^*(\theta, p)$. *In addition*, $\lim_{\lambda \to 0} \tilde{\theta}_R = 0$.

(*ii*) $\theta \geq \theta_R$: *For any productivity realization above* θ_R, *there will be upward wage adjustment. The equilibrium wage will correspond to the benchmark case and the labor market will clear*: $\bar{w}(\theta, p, \bar{w}_{t-1}) = w^*(\theta, p)$.

PROOF:

See online Appendix B.2.

For values of θ above θ_R, firms will increase wages smoothly as θ rises. However, for sufficiently small decreases in θ below θ_R, it will be more profitable to maintain wages at $\bar{w}_{t-1}$ than to cut wages and have effort decreases due to worker retaliation. If θ falls below $\tilde{\theta}_R$, $\bar{w}_{t-1}$ is no longer the unique equilibrium, and wages may fall below $\bar{w}_{t-1}$. Note that $\tilde{\theta}_R$ will be lower for smaller values of λ: as λ approaches 0, firms will never find it profitable to lower wages below $\bar{w}_{t-1}$.

This contradicts Null Hypothesis (i). Proposition 1 predicts that for any two $\theta', \theta'' \in (\tilde{\theta}_R, \theta_R]$, the equilibrium wage will be the same: $\bar{w}(\theta', p, \bar{w}_{t-1}) = \bar{w}(\theta'', p, \bar{w}_{t-1})$.

D. *Impact of Increases in the Previous Period's Wage*

In the benchmark case, previous wages have no impact on period *t* wages. However, this will no longer be true when there is reference dependence around the previous period's wage. Compare the case of two different lagged wage levels: $\bar{w}_{t-1}^{low} < \bar{w}_{t-1}^{high}$. Following equation (6), define θ_R^{high} implicitly as $w^*(\theta_R^{high}, p) = \bar{w}_{t-1}^{high}$.

PROPOSITION 2 (Ratcheting: Effects of a Higher Lagged Wage):

(*i*) *For any* $\theta < \theta_R^{high}$ *and* λ *sufficiently small, the period t wage will be higher and employment will be lower if* $\bar{w}_{t-1} = \bar{w}_{t-1}^{high}$ *than if* $\bar{w}_{t-1} = \bar{w}_{t-1}^{low}$.

(*ii*) *For any* $\theta \geq \theta_R^{high}$, *the period t wage and employment levels will be the same under* $\bar{w}_{t-1}^{high}$ *and* $\bar{w}_{t-1}^{low}$.

PROOF:

See online Appendix B.3.

A higher lagged wage has the potential to exacerbate distortions in the current period through two channels. First, there is a larger range of θ-values at which labor market distortions occur. Second, for any given θ where the rigidity binds, the higher lagged wage will constitute a larger departure from the market clearing level. In contrast, because the rigidity does not bind for $\theta \geq \theta_R^{high}$, the lagged wage, as long as it is weakly less than $\bar{w}_{t-1}^{high}$, is irrelevant. Note that Proposition 2 contradicts Null Hypothesis (ii).

E. *Impact of Inflation*

In the benchmark case, prices are neutral. This is no longer true when workers have fairness preferences over a nominal wage.

PROPOSITION 3 (Inflation Will Mitigate Distortions from Rigidity):

(*i*) *For any fixed $\theta < \theta_R$ where the wage is distorted above the market clearing level so that $\bar{w}(\cdot) = \bar{w}_{t-1}$, an increase in price levels will lower the real wage: $\partial/\partial p(\bar{w}(\cdot)/p) < 0$. With sufficient inflation, $\bar{w}_{t-1}$ will equal the market clearing wage.*

(*ii*) *For any $\theta \geq \theta_R$, an increase in price levels will have no effect on the real wage; nominal wages will rise to keep the real wage constant.*

PROOF:

See online Appendix B.4.

For any $\bar{w}_{t-1}$, a price increase means that the value of θ at which $\bar{w}_{t-1}$ is the market clearing nominal wage will now be lower; i.e., inflation lowers θ_R. Because the rigidity will bind to the left of this lower θ value, distortions will affect a smaller portion of the θ-distribution. Intuitively, inflation enables firms to achieve real wage reductions while keeping the nominal wage fixed at $\bar{w}_{t-1}$, thereby avoiding effort retaliation.

Proposition 3 contradicts Null Hypothesis (iii). Inflation lowers the real wage whenever the wage is distorted at the previous period's wage. This means that inflation undoes the asymmetric adjustment prediction under Proposition 1. Specifically, suppose $\theta' < \theta''$ but $\bar{w}(\theta', p, \bar{w}_{t-1}) = \bar{w}(\theta'', p, \bar{w}_{t-1})$. Then, as shown in the proof in online Appendix B.4, there is a sufficiently high price level, p', where the market clears under both θ' and θ''. Consequently, after a change in prices to p', $\bar{w}(\theta', p', \bar{w}_{t-1}) < \bar{w}(\theta'', p'', \bar{w}_{t-1})$. Similarly, inflation will also mitigate the distortion from high lagged wages in Proposition 2. Regardless of the value of θ, with a sufficient increase in prices, $\bar{w}_{t-1}$ will be less than the market clearing nominal wage in period t, so that the fairness norm becomes irrelevant and the rigidity does not bind.

F. *Discussion*

The model assumes that firms make decisions only taking into account current period payoffs. In a multi-period setting, if there is a high θ-realization, firms would trade off the benefits of raising wages to satisfy labor demand now, versus the expected decrease in future profits from the ratcheting effect. In the model, the former consideration would dominate the latter, producing almost full upward adjustment to positive shocks. This is because each firm gains the full benefit of posting a higher wage this period, but only bears an infinitesimal fraction of the cost since its wage contributes negligibly to the average market wage. In reality, a firm may internalize more of the future costs, e.g., if it has long-term relationships with individual workers or if firms can collude to not raise wages. However, the literature suggests that in the empirical context of this study, this is unlikely.[18] To the extent that this does occur, the core qualitative predictions that distinguish rigidity from the benchmark case above would still remain, but the expected magnitude of the effects would be smaller. This would make it less likely that I would be able to reject the null model in favor of downward nominal rigidity.

In addition, the model assumes the reference point is the previous period's nominal wage. Other formulations, such as the expected wage (Kőszegi and Rabin 2006), would alter some of the specific predictions.[19] Alternately, consistent with Loewenstein and Prelec (1991), workers may demand upward-sloping wage profiles. This could lead the reference wage to be of the form $\bar{w}_{t-1}(1+\varphi)$, reflecting a norm for a φ percentage wage increase in each period. My formulation of the reference point is simple and matches the survey evidence provided in Section V and in Kahneman, Knetsch, and Thaler (1986). While the empirical results below do appear to provide support for some types of reference points as being more likely than others, I take no strong stance on the functional form of the reference point, or on the micro-foundation for rigidity more generally.

II. Empirical Strategy

A. *Context: Rural Labor Markets in India*

Agricultural production in India, as in most developing countries, is largely undertaken on smallholder farms. The median household farm size is about 0.9 acres.[20] The composition of farm employment is often a mix of household and hired labor. Markets for hired labor are active: most households buy and/or sell labor.[21] Labor is typically traded in decentralized markets for casual daily workers. Ninety-eight percent of agricultural wage employment is through casual wage contracts (with regular/salaried workers making up the bulk of the remaining 2 percent). In addition, 67 percent of landless rural workers report casual employment as their primary source of earnings.

Within a village, there is typically a gender-specific prevailing wage for casual daily labor for any given task. This has been documented in earlier development work on India. For example, Drèze and Mukherjee (1986) state, "[I]n normal times a *single* wage rate applies to all adult males in the village for a 'normal' day's work, irrespective of the identity of the partners involved. If the task is of a special

[18] For example, Drèze and Mukherjee (1986) observe, "No explicit collusion exists between either employers or labourers. Individual employers have no monopsonistic power: the pool of employers is large, and re-sorting of partners occurs constantly."

[19] For example, prior positive shocks would not necessarily create ratcheting because the reference point would depend on the expected value of θ. Inflation would not affect real wage adjustment if the reference point is formulated with respect to the real wage.

[20] Unless stated otherwise, the statistics in this subsection are computed from India's National Sample Survey Employment/Unemployment rounds (1983–2009).

[21] See, for example, Rosenzweig (1980), Benjamin (1992), and Bardhan (1973).

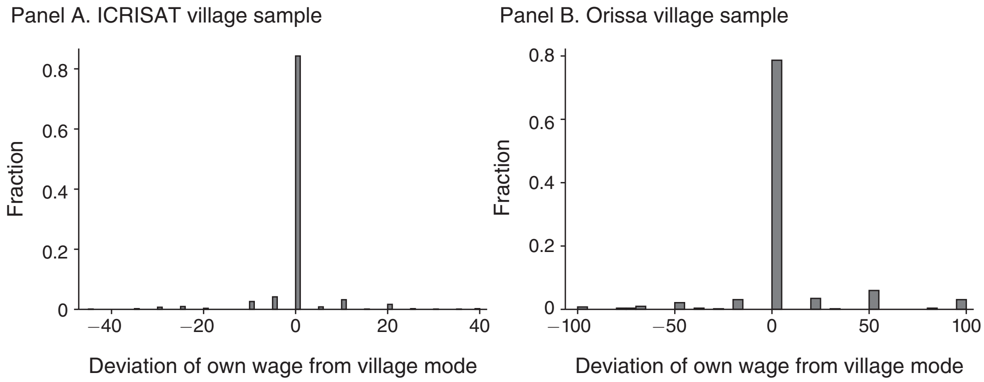

Figure 3. Dispersion of Agricultural Wages

Notes: Each panel displays the distribution of: (wage reported by worker) − (gender-specific mode of wages in village). Panel A uses observations from the 2001–2004 ICRISAT Village Level Studies. Respondents were asked to report their average daily wage in the past year or season. Sample is restricted to "Daily wage earners" who report their work type as agricultural labor. Observations are at the worker-year level. $N = 6$ villages, 259 households. Panel B uses data collected by Breza, Kaur, and Shamdasani (2018). Respondents were asked to list their work activity and wage level for each of the past 10 days. Observations at the worker-day level. $N = 25$ villages, 185 male workers.

nature … some bargaining may take place." Similarly, Bliss and Stern (1982) note, "At any particular time everyone in the village knew what the going rate was. And in nearly every case that wage or something of equivalent value would be paid to every agricultural laborer."

Using more recent data, Figure 3 plots the distribution of casual daily wages reported by agricultural laborers. In the ICRISAT data (panel A), workers were asked their average wage over the past year or season (ICRISAT 2004). Over 80 percent of workers within a village report the same exact wage. In the more detailed data collected by Breza, Kaur, and Shamdasani (2018)—henceforth, BKS—in Orissa (panel B), respondents reported their activity and wage for each day in the past ten days.[22] The reported task-specific daily wage is the same in about 80 percent of worker-day observations within a village. This supports the presumption that there is a salient prevailing wage for a given task at any given point in time. In addition, the prevailing agricultural wage (i.e., mode) stays the same over the one month study period in all villages in the BKS sample.

There are few formal institutional constraints in these markets. Contracts are usually negotiated bilaterally between landowners and laborers in a decentralized manner; unions or other formal labor institutions are rare. Wage contracts are typically of short duration (on the order of 1–3 days).[23] As a result, they can more easily reflect recent changes in market conditions and time worked is more flexible than in other contexts. Minimum wage policies are in practice ignored and there is

[22] These data were collected from laborers who did not participate in the BKS experiment (i.e., those not randomly selected to receive employment in a worksite). Consequently, the survey respondents were engaged in casual daily work in their villages during the study period.

[23] Of course, this does not rule out longer-term informal implicit contracts.

little government intervention in the private wage labor market (Rosenzweig 1980, 1988).

Agricultural production is heavily rainfall-dependent and exhibits considerable seasonality. The major rainfall episode is the yearly monsoon, which accounts for over 80 percent of annual rainfall. The monsoon arrives between May and July in most parts of the country and marks the beginning of the agricultural year. For rice (the major crop) as well as some other crops, planting occurs once the rains begin. Subsequent months involve various activities such as transplanting, fertilizer application, and weeding. Rice harvesting typically occurs between November and January. February to April is the lean season in rain-fed areas; during this time, growing crops usually requires irrigation and the monsoon is a less important determinant of labor demand.

B. *Empirical Tests*

A distinct labor market is defined as an Indian district (an administrative geographic unit). Let θ_{dt} denote the rainfall realization in district d in year t. The empirical implementation will focus on discrete shocks. As discussed in Section IID, in each year, a labor market can experience a negative shock (low rainfall), no shock (usual level of rainfall), or a positive shock (high rainfall): $\theta_{dt} \in \{\theta^{Neg}, \theta^{Zero}, \theta^{Pos}\}$. I assume these shocks are i.i.d.: uncorrelated with any other determinants of the wage and serially uncorrelated across years. In addition, as in the model, I assume the shocks are transitory: rainfall in a given year affects TFP in only that year.[24]

In the absence of rigidities, the following simple model captures the effects of transitory shocks on equilibrium wages:

$$\ln w_{dt} = \alpha_0 + \alpha_1 Pos_{dt} + \alpha_2 Neg_{dt} + \ln p_t + \epsilon_{dt}, \tag{7}$$

where Pos_{dt} and Neg_{dt} are dummies for a positive and negative shock, respectively. Cofficients α_1 and α_2 give the difference in the wage level under these shocks relative to the omitted category of $Zero_{dt}$. Null Hypothesis (i) establishes that $\alpha_1 > 0$ and $\alpha_2 < 0$. In accordance with Null Hypothesis (ii), lagged values do not appear in equation (7) because they are irrelevant. Consistent with Null Hypothesis (iii), prices enter only additively: a price increase raises the nominal wage to keep the real wage constant. This means that, for example, the difference in wages between $Neg_{dt} = 1$ and $Zero_{dt} = 1$ is fixed at α_2; this difference is not affected by inflation. The empirical strategy builds on this basic specification, which is amenable to the fact that much of the analysis relies on data from repeated cross sections over non-consecutive years.[25]

[24] This is a standard assumption in prior work (e.g., Paxson 1992, Rosenzweig and Wolpin 1993, Townsend 1994, Jayachandran 2006). Below, I use the results to directly document a lack of serial correlation in shocks, and to rule out persistent productivity impacts of shocks.

[25] Note that this equilibrium wage model can also be expressed in a first differences framework. It is straightforward to verify that writing equation (7) for $\ln w_{d,t-1}$ and subtracting from (7) gives: $\ln w_{dt} - \ln w_{d,t-1} = \alpha_1(Pos_{dt} - Pos_{d,t-1}) + \alpha_2(Neg_{dt} - Neg_{d,t-1}) + I_t + \xi_{dt}$, where $I_t \equiv \ln p_t - \ln p_{t-1}$ is the inflation level. The α_1 and α_2 coefficients have the same expected value in both specifications. However, equation (7) has the advantage that it does not require data from consecutive years.

These null predictions will not hold in the presence of nominal rigidity. Proposition 1 predicts that in equation (7), $\alpha_2 = 0$ if inflation is sufficiently low. Proposition 2 can be tested by adding lagged values. Specifically, the expected value of $\bar{w}_{t-1}$ will be higher if there was a positive shock in year $t-1$. Proposition 2 (i) therefore implies that if $\theta_{d,t-1} = \theta^{Pos}$, then in year t, wage distortions can occur for any $\theta_{dt} < \theta^{Pos}$ (i.e., for θ^{Neg} and θ^{Zero}). Model (8) expands equation (7) by adding dummies for a lagged positive shock, $Pos_{d,t-1}$, for the cases where $\theta_{dt} = \theta^{Neg}$ and $\theta_{dt} = \theta^{Zero}$. Thus, in order to enable separate tests of Propositions 1 and 2, the following specification breaks up the case of negative shocks into two subcases:

$$(8) \qquad \ln w_{idt} = \beta_0 + \beta_1 Pos_{dt} + \beta_2 NonPos_{d,t-1} Neg_{dt} + \beta_3 Pos_{d,t-1} Neg_{dt} + \beta_4 Pos_{d,t-1} Zero_{dt} + \sum_{k=2}^{K} \phi_k \tilde{Pos}_{d,t-k} + \boldsymbol{\delta}_d + \boldsymbol{\rho}_t + \varepsilon_{idt},$$

where $NonPos_{d,t-1}$ is an indicator for a non-positive shock last year (i.e., $NonPos_{d,t-1} \equiv Zero_{d,t-1} + Neg_{d,t-1}$), $\boldsymbol{\rho}_t$ are year fixed effects (which absorb p_t), and $\boldsymbol{\delta}_d$ are district fixed effects that capture differences in real wage levels across districts.

In principle, positive shocks in even earlier years, such as $t-2$, could distort current period wages; the power to detect these effects will be lower than from a positive shock in period $t-1$ because there is a longer period of time over which inflation can erode the ratcheting effect (see below). However, such earlier positive shocks could still weaken the sharpness of the tests of Propositions 1 and 2. To sharpen the predictions, the $\sum_{k=2}^{K} \tilde{Pos}_{d,t-k}$ covariate vector controls for a longer history of lagged positive shocks from periods $t-2$ to $t-K$. Specifically, $\tilde{Pos}_{d,t-k}$ is a binary indicator that equals 1 if there was a positive shock $t-k$ periods ago and no positive shock since then (i.e., from periods $t-k+1$ to period t), and equals 0 otherwise.[26] With these controls, the omitted shock category in model (8) is no shock this year, and non-positive shocks in the past K years. This means that there have been no upward perturbations in the past wage from high rainfall in earlier years. Consequently, the expected wage associated with the omitted category approximates the market clearing wage under no shock: $w^*(\theta^{Zero}, p)$.[27] In other words, with this specification, θ^{Zero} is a proxy for θ_R, setting up a direct test of the model's predictions. This approach allows me to maximize power for tests by focusing on shocks in periods $t-1$ and t, while creating a "clean" reference value for tests. In the analysis, I show the results with and without these controls.

[26] Specifically, these controls are defined as: $\tilde{Pos}_{d,t-k} \equiv Pos_{d,t-k} \prod_{m=t-k+1}^{t} (1 - Pos_{d,m})$. Under rigidities, prior high rainfall shock will only matter if it is not followed by high rainfall in a more recent year; otherwise the wage would adjust upward later anyway, making the older shock irrelevant. For this reason, I use these $\tilde{Pos}_{d,t-k}$ controls, rather than just dummies for $Pos_{d,t-k}$. This also increases power to detect effects on Pos_{dt} and $Pos_{d,t-1}$ in the specification. In practice, prior positive shocks often dissipate within a couple years. Note that it is not necessary to add similar controls for a longer history of lagged negative shocks; indeed, the inclusion of such controls makes essentially no difference to the results.

[27] Note that the validity of the empirical strategy does not rely on the wage level under the omitted category truly being the market clearing wage. Rather, what is important is that the omitted category captures the counterfactual for the wage under usual rainfall without any ratcheting effects (i.e., no upward distortions) from prior shocks.

Both Null Hypothesis (i) and Proposition 1 predict $\beta_1 > 0$: wages should be higher when there is a positive shock than under no shock (the omitted category). Thus, outcomes under high rainfall states will not distinguish rigidities from full adjustment. Here, β_2 provides a test of asymmetric adjustment. Null Hypothesis (i) predicts $\beta_2 < 0$, while Proposition 1 predicts that $\beta_2 = 0$ (when inflation is sufficiently low). Note that the Null Hypothesis (i) does not necessarily impose the restriction that $\beta_1 = -\beta_2$; this will depend on whether the TFP shock under θ^{Neg} and θ^{Pos} is of equal magnitude, relative to θ^{Zero}. To test for asymmetric adjustment, I therefore test Proposition 1 with the weaker assumption that $\theta^{Neg} < \theta^{Zero} < \theta^{Pos}$. If my weaker test fails, then this implies the more stringent restriction of $\beta_1 = -\beta_2$ will also fail.

The β_3 and β_4 coefficients provide tests of Proposition 2. Null Hypothesis (ii) predicts that $\beta_4 = 0$: this year's TFP is the same as the omitted category and so wages should be the same. However, under downward rigidities, the wage increase from last year's high rainfall would persist into the current year, keeping wages above $w^*\left(\theta^{Zero}, p\right)$. Proposition 2 therefore predicts that $\beta_4 > 0$: nominal wages will be higher due to the ratcheting effect. In addition, as was the case for β_2, under the null, $\beta_3 < 0$. However, Proposition 2 predicts that $\beta_3 > 0$: wages could be *higher* than the omitted category of no shock, even though there is a negative shock in year t.[28]

Finally, note that under the null of full adjustment, model (8) should reduce exactly to model (7): $\beta_1 = \alpha_1 > 0$; $\beta_2 = \beta_3 = \alpha_2 < 0$; and $\beta_4 = 0$. Thus, equation (8) will only have additional explanatory power if there are downward rigidities.

Proposition 3 predicts that, in the presence of rigidities, inflation will move wages closer to market clearing levels. I test this by interacting each of the shock categories with inflation:

$$\begin{aligned}(9) \qquad \ln w_{idt} = {} & \gamma_0 + \gamma_1 Pos_{dt} + \gamma_2 NonPos_{d,t-1} Neg_{dt} \\ & + \gamma_3 Pos_{d,t-1} Neg_{dt} + \gamma_4 Pos_{d,t-1} Zero_{dt} + \psi_1 Pos_{dt} \times I_t \\ & + \psi_2 NonPos_{d,t-1} Neg_{dt} \times I_t + \psi_3 Pos_{d,t-1} Neg_{dt} \times I_t \\ & + \psi_4 Pos_{d,t-1} Zero_{dt} \times I_t \sum_{k=2}^{K} \phi_k \tilde{Pos}_{d,t-k} \\ & + \delta_{\mathbf{d}} + \rho_{\mathbf{t}} + \varepsilon_{idt},\end{aligned}$$

[28] Note that model (8) does not include a separate test for the case of $Pos_{d,t-1} Pos_{dt}$. This is because under both the null and under rigidities (Proposition 2 (ii)), lagged high rainfall levels will not matter if the rainfall this year is also high. This subcase is therefore subsumed under Pos_{dt}. In general, specification (8) expands model (7) to only include those subcases of shocks that can distinguish predictions under rigidity from the null. This keeps the main estimating equation parsimonious. It also helps with statistical power. In the online Appendix, I also show results for the full set of interactions between lagged and current rainfall levels, which constitute $3 \times 3 = 9$ cells.

where I_t is price inflation from $t-1$ to t. In this model, γ_1, γ_2, γ_3, and γ_4 capture the difference between the omitted category and each respective shock category when inflation is zero. They therefore provide a sharper test of Propositions 1–2, which predict $\gamma_1 > 0$, $\gamma_2 = 0$, $\gamma_3 > 0$, and $\gamma_4 > 0$. The coefficients on the interaction terms capture how each of these differences changes with inflation.

First, note that because the omitted category approximates $w^*(\theta^{Zero}, p)$, if price levels rise, the nominal wage in the omitted category will rise accordingly to maintain a constant real wage. The same will be true when $Pos_{dt} = 1$. Consequently, Null Hypothesis (iii) and Proposition 3(ii) both predict that $\psi_1 = 0$: the difference in nominal wages between Pos_{dt} and the omitted category will not change with inflation.

In contrast, inflation will not be neutral in the other shock cases, in which wages are distorted above market clearing levels. In these cases, employers can keep nominal wages fixed, enabling real wage reductions through inflation. Consequently, with inflation, nominal wages will end up being lower under $NonPos_{d,t-1}Neg_{dt}$ than the omitted category: $\psi_2 < 0$. Similarly, inflation will also mitigate the ratcheting effect, so that lagged transitory positive shocks do not cause nominal wages to be higher than the omitted category in year t: $\psi_3 < 0$ and $\psi_4 < 0$. In contrast, under Null Hypothesis (iii), $\psi_2 = \psi_3 = \psi_4 = 0$. This again means that under the null, specification (9) should reduce to specification (7).

In addition to providing a direct test of Proposition 3, specification (9) is helpful for two reasons. Model (8) pools across high and low inflation periods; it will therefore only have power to distinguish rigidities if average inflation across years is sufficiently low. Second and relatedly, in model (8), if $\beta_2 = 0$ but inflation is high (i.e., ρ_t is positive and large), then this could mean that nominal wages are rising in absolute terms despite a negative shock. Under the reference point assumed in Section I, where workers dislike wage cuts, but do not demand consistent wage increases, we would expect this to happen if inflation is high, but not if it is low. For both these reasons, the level effects on the shock covariates in model (9) are important because they isolate wage adjustment in periods of low inflation.

Finally, this empirical strategy allows a test for whether rigidities have real effects on employment. I replace the dependent variable in model (8) with e_{idt}, the employment level of worker i in district d in year t:

$$(10) \qquad e_{idt} = \sigma_0 + \sigma_1 Pos_{dt} + \sigma_2 NonPos_{d,t-1} Neg_{dt} + \sigma_3 Pos_{d,t-1} Neg_{dt} + \sigma_4 Pos_{d,t-1} Zero_{dt} + \sum_{k=2}^{K} \phi_k \tilde{Pos}_{d,t-k} + \delta_{\mathbf{d}} + \rho_{\mathbf{t}} + \varepsilon_{idt}.$$

Under both the Null Hypotheses and Proposition 1, employment should rise with positive shocks and fall under negative shocks: $\sigma_1 > 0$ and $\sigma_2 < 0$.

Testing for employment distortions requires a counterfactual benchmark of what employment would be if wages could adjust downward. Proposition 2 enables such a test using lagged transitory positive shocks. Specifically, in the omitted category, there is no shock in the current year. This therefore serves as a counterfactual for what employment would be if wages could adjust down after the lagged high rainfall in the $Pos_{d,t-1}Zero_{dt}$ case. If the wage distortion from the ratcheting effect lowers employment, then $\sigma_4 < 0$. In contrast, under the null, $\sigma_4 = 0$. Similarly, Proposition 2 predicts that $\sigma_3 < \sigma_2$: $Pos_{d,t-1}Neg_{dt}$ will lead to lower employment than $NonPos_{d,t-1}Neg_{dt}$, because of the additional wage distortion from ratcheting in the former case. In contrast, under the null, $\sigma_3 = \sigma_2$.[29]

It would also be interesting to test whether inflation mitigates employment distortions. However, because employment data are only available for a small number of years, providing little variation in inflation, it is not possible to examine differential employment effects by inflation (see below).

C. *Data*

Wage and employment data are constructed using two primary datasets. The first source is the rural sample of the Employment/Unemployment rounds of the Indian National Sample Survey (NSS), a nationally representative survey of over 600 Indian districts (National Sample Survey Office 2009).[30] Households in each district are sampled on a rolling basis over the agricultural year (July to June). The survey elicits daily employment and wage information for each household member over the 7 days preceding the interview. The surveys were conducted during the 1983, 1987, 1993, 1999, 2003, 2004, 2005, 2007, and 2009 agricultural years.[31] The second source is the World Bank Agriculture and Climate dataset, which provides yearly data on 240 Indian districts in 13 states from 1956–1987 (Sanghi, Kumar, and McKinsey 1994). The unit of observation is a district-year. Rainfall data are taken from *Terrestrial Precipitation: 1900–2008 Gridded Monthly Time Series* (version 2.01), constructed by the Center for Climatic Research, University of Delaware (Willmott and Matsuura 2010). Online Appendix C provides further details on data construction, and online Appendix Table 1 provides summary statistics.

D. *Definition of Shocks*

I focus on rainfall in the first month when the monsoon typically arrives in a district (which ranges from May to July). Focusing on rain in the month of expected arrival reflects the fact that both the level of rain and the timeliness of its arrival are important determinants of productivity. To construct shocks, I compute the rainfall distribution for each district separately for each dataset: for the years 1956–1987 for the World Bank data and the years 1983–2009 for the NSS data. A shock is a deviation in rainfall from a district's usual rainfall level. Specifically, as in Jayachandran (2006), a positive shock is rainfall above the eightieth percentile for the district and a negative shock is rainfall below the twentieth percentile. These discrete cutoffs capture the nonlinear relationship between rainfall and productivity and increase power. This is illustrated in online Appendix Figure 1: rainfall in the upper (lower)

[29] The model also predicts labor rationing under $NonPos_{d,t-1}Neg_{dt}$, but in this case, there is no clear counterfactual for what employment levels would be if wages were flexible.

[30] A district is an administrative unit in India (like counties in the United States). On average, there are 17 districts per state and approximately 2 million residents per district.

[31] Since the monsoon is the rainfall shock used in the analysis, the results will focus on wages and employment between the month of monsoon arrival and the end of harvesting in January.

Table 1—Effect of Rainfall Shocks on Wages

	World Bank data, 1956–1987			National Sample Survey data, 1983–2009		
	All observations (1)	All observations (2)	Non-positive shock this year (3)	All observations (4)	All observations (5)	Non-positive shock this year (6)
Positive shock this year	0.021 (0.009)			0.059 (0.018)		
Negative shock this year	−0.004 (0.010)			0.007 (0.023)		
Positive shock last year		0.017 (0.009)	0.021 (0.010)		0.030 (0.021)	0.050 (0.023)
Negative shock last year		0.007 (0.009)	−0.001 (0.011)		0.005 (0.022)	0.019 (0.023)
Observations: district-years	7,680	7,680	4,806	—	—	—
Observations: individual-years	—	—	—	59,243	59,243	50,158

Notes: Dependent variable is the log of the nominal daily wage for casual agricultural work. A positive (negative) shock is defined as rainfall in the first month of the monsoon above (below) the eightieth (twentieth) percentile of the district's usual distribution. No shock is rainfall between the twentieth and eightieth percentile of the district's usual distribution. Columns 3 and 6 restrict analysis to observations where there was a negative shock or no shock this year. All regressions include district and year fixed effects. Standard errors are clustered by region-year.

tail of the distribution is associated with increased (decreased) yields, while the middle of the rainfall distribution has a relatively flat relationship with yields.

Rainfall is serially uncorrelated across years (online Appendix Table 2). To allow for the possibility of correlated shocks across districts in a given year, standard errors are clustered by region-year in all regressions, using the region definitions from the NSS.[32]

III. Results

A. *Test for Wage Adjustment*

Table 1 provides a preliminary test for wage adjustment (as in model (7)), showing results from the World Bank and NSS datasets side by side. The dependent variable is the log nominal daily wage for agricultural work.[33] In both datasets, relative to no shock, nominal wages adjust up when there are positive shocks, but I cannot reject that they are not lower on average when there is a negative shock (columns 1 and 4).[34] In columns 2 and 5, there is some evidence that a positive shock in one year leads to a persistent increase in wages in the following year.

[32] Online Appendix Table 2 provides some evidence for negative serial correlation in rainfall. Clustering standard errors by region makes a minor difference in the results, and slightly improves precision in some cases. To be conservative, I cluster by region-year.

[33] The World Bank data provide the average daily cash wage in each district-year. In the NSS data, I compute the daily agricultural wage as total (cash plus in-kind) value of paid earnings for casual agricultural work divided by days worked over the past seven days. See online Appendix C for more details.

[34] Below, I show that employment does indeed fall sharply when there are negative shocks.

TABLE 2—TEST FOR WAGE ADJUSTMENT

		World Bank, 1956–1987		National Sample Survey, 1983–2009			
		Full sample (1)	Full sample (2)	Full sample (3)	Full sample (4)	Laborers (5)	Full sample (6)
Last year's shock	*This year's shock*						
None, negative, or positive	Positive	0.026	0.043	0.063	0.072	0.071	0.066
		(0.009)	(0.009)	(0.018)	(0.019)	(0.019)	(0.016)
None or negative	Negative	−0.011	−0.014	0.001	0.001	0.000	−0.000
		(0.010)	(0.010)	(0.024)	(0.023)	(0.024)	(0.022)
Positive	Negative	0.035	0.052	0.046	0.058	0.064	0.054
		(0.020)	(0.021)	(0.042)	(0.041)	(0.039)	(0.039)
Positive	None	0.020	0.037	0.058	0.064	0.060	0.064
		(0.010)	(0.011)	(0.024)	(0.024)	(0.025)	(0.022)
Female worker							−0.211
							(0.028)
Household land size (acres)							0.0000
							(0.0003)
Education							0.0024
							(0.0037)
Prior shock history controls?		No	Yes	No	Yes	Yes	Yes
Observations: district-years		7,680	7,680	—	—	—	—
Observations: individual-years		—	—	59,243	59,243	51,697	59,129
Dependent variable mean		1.21	1.21	3.39	3.39	3.39	3.39

Notes: Dependent variable is the log of the nominal wage for casual daily agricultural work. A positive (negative) shock is defined as rainfall in the first month of the monsoon above (below) the eightieth (twentieth) percentile of the district's usual distribution. No shock (*none*) is rainfall between the twentieth and eightieth percentile of the district's usual distribution. The shock sequences are presented as the shock in the previous year and the shock in the current year. Each of the four shock covariates is an indicator that equals 1 if the sequence of shocks was realized and equals 0 otherwise. The omitted category in each regression is {none or negative} last year and {none} this year. All regressions include district and year fixed effects. Columns 2 and 4–6 add controls for positive shocks 2 years ago and 3 years ago. Column 6 includes fixed effects for quarter of the year. Column 5 limits analysis to individuals whose primary source of earnings is casual daily labor. Standard errors clustered by region-year.

Under rigidities, a lagged positive shock has the potential to distort wages upward particularly if the current year's shock is none or negative. If the current shock is positive, wages would need to adjust up anyway, rendering the prior positive shock irrelevant. Columns 3 and 6 limit analysis to non-positive shocks in the current year; as expected, this increases the magnitude of the coefficients and lagged positive shocks significantly raise current wages (relative to having no shock last year) in both datasets. In contrast, consistent with rigidity in the downward direction, lagged negative shocks have no persistent wage effects.

Table 2 shows the full test corresponding to specification (8). Columns 1–2 examine effects in the World Bank data. In column 2, relative to the counterfactual of no shock this year and no shock last year, wages are 4.3 percent higher if there is positive shock this year (row 1, significant at the 1 percent level). In contrast, consistent with Proposition 1, wages are not significantly lower if there is a negative shock this year: while β_2 has a negative sign, it is small in magnitude and I cannot reject that it is zero (row 2), and β_3 is actually positive (row 3).[35] In addition, consistent with Proposition 2, lagged positive shocks have persistent wage effects (rows 3 and 4). For example, when there is a positive shock last year and no shock this year, wages are 3.7 percent higher on average than if last year's positive shock had not occurred (significant at the 1 percent level). The pattern of findings is similar in the NSS data (columns 3–4). Column 5 limits analysis to individuals whose primary source of earnings is casual daily labor, with similar results. Column 6 adds controls for individual covariates and season of the year. Women earn substantially less than men, but landholdings and education have no predictive power for wages.[36]

I provide a series of robustness checks in the online Appendix tables. The finding of wage rigidity holds separately for each gender (online Appendix Table 5), is robust to limiting analysis to the cash component of the wage (online Appendix Table 6), and appears stronger for flat wages relative to piece rate contracts (though the test is underpowered, online Appendix Table 7). In addition, online Appendix Tables 8–9 show robustness of these and subsequent results to alternative percentile cutoffs for defining positive and negative shocks.

B. *Impact of Inflation on Wage Adjustment*

To test Proposition 3, I use the World Bank data since they cover 32 years, providing substantial variation in inflation. (The NSS rounds are comprised of 9 years of data, with limited variation in inflation). Inflation is computed from the statewise *Consumer Price Index for Agricultural Labourers in India*, published by the Government of India. For each district, I construct inflation as the average of inflation in all states excluding the district's own state. This captures the component of inflation that is nationally determined (by factors outside the district's own state) and therefore unaffected by local idiosyncratic shocks. Online Appendix Table 10 verifies that the district rainfall shocks have no correlation with prices in other states (columns 3–4) or inflation in other states (column 5): the coefficients are small in magnitude and insignificant. The correlation between own state inflation and national inflation is 0.70.

Table 3, columns 1–2 present estimates of model (9), with interactions of each shock category with the continuous inflation rate in other states. Contemporaneous positive shocks increase wages (row 1). Consistent with Proposition 3(ii), there are no differential effects by inflation (row 2). When there are contemporaneous droughts, estimated wages are the same on average as the omitted category when inflation is zero (row 3). However, when there is a positive inflation, nominal (and real) wages are lower under negative shocks than when there is no shock (row 4). Similarly, after lagged positive shocks, wages are ratcheted upward when inflation is low (rows 5 and 7); as inflation rises, such shocks are less likely to have persistent

[35] In addition, I reject that $\beta_1 = -\beta_2$ in both the World Bank data (p-value $= 0.043$) and NSS data (p-value $= 0.011$). However, as discussed in Section IIB, this null hypothesis requires stronger assumptions about the production function.

[36] Online Appendix Table 3 shows the raw wage change patterns for each shock category using the World Bank data. The results on wage change premiums are consistent with the findings in Table 2. Online Appendix Table 4 runs a more detailed version of the main specification in Table 2, with each of the 9 shock sequences estimated separately.

Table 3—Impact of Inflation on Wage Adjustment

		Inflation measure: inflation rate		Inflation measure: Indicator: inflation > 6%	
Last year's shock	This year's shock	(1)	(2)	(3)	(4)
1. Any	Positive	0.027	0.043	0.032	0.047
		(0.009)	(0.010)	(0.010)	(0.011)
2. Interaction with inflation measure		0.002	0.009	−0.016	−0.013
		(0.095)	(0.094)	(0.019)	(0.019)
3. None or negative	Negative	0.005	0.000	0.006	0.001
		(0.012)	(0.012)	(0.014)	(0.013)
4. Interaction with inflation measure		−0.230	−0.184	−0.038	−0.031
		(0.107)	(0.104)	(0.021)	(0.020)
5. Positive	Negative	0.067	0.084	0.069	0.085
		(0.025)	(0.025)	(0.028)	(0.029)
6. Interaction with inflation measure		−0.481	−0.479	−0.083	−0.082
		(0.203)	(0.205)	(0.037)	(0.037)
7. Positive	None	0.041	0.057	0.042	0.057
		(0.014)	(0.014)	(0.015)	(0.015)
8. Interaction with inflation measure		−0.257	−0.248	−0.047	−0.045
		(0.096)	(0.097)	(0.019)	(0.020)
Shock history controls		No	Yes	No	Yes
Observations: district-years		7,680	7,680	7,680	7,680
R^2		0.947	0.948	0.947	0.947
F-test p-value: coefficient 3 + coefficient 4 = 0		—	—	0.043	0.049
F-test p-value: coefficient 5 + coefficient 6 = 0		—	—	0.566	0.891
F-test p-value: coefficient 7 + coefficient 8 = 0		—	—	0.690	0.316

Notes: Dependent variable is the log of the nominal wage for casual daily agricultural work. A positive (negative) shock is defined as rainfall in the first month of the monsoon above (below) the eightieth (twentieth) percentile of the district's usual distribution. No shock (*none*) is rainfall between the twentieth and eightieth percentile of the district's usual distribution. The shock sequences are presented as the shock in the previous year and the shock in the current year. Each of the four shock covariates (rows 1, 3, 5, 7) is an indicator that equals 1 if the sequence of shocks was realized and 0 otherwise. The omitted category in each regression is {none or negative} last year and {none} this year. The remaining covariates (rows 2, 4, 6, 8) are interactions of the shock sequence indicators with a measure of inflation. Inflation is the percentage change in the state CPI for Agricultural Labourers, averaged across all states excluding the district's own state; for 1956 and 1957, the national CPI are used because state CPI data are unavailable. The inflation measure in columns 1–2 is the continuous inflation rate, and in columns 3–4 is a binary indicator for inflation above 6 percent. All regressions include district and year fixed effects. Columns 2 and 4 add controls for positive shocks 2 years ago and 3 years ago. Standard errors are clustered by region-year.

Source: World Bank data

effects on current wages (rows 6 and 8). Overall, the negative coefficients on the interaction terms in rows 4, 6, and 8 violate Null Hypothesis (iii) and are consistent with Proposition 3(i).

In Table 3, columns 3–4, the interaction term is a binary indicator for inflation above 6 percent: about the mean inflation rate in the sample. The pattern of results is similar. Inflation has no differential effects when there are positive shocks, but does enable downward real wage adjustment in the three categories of shocks where rigidity creates distortions. As indicated in the F-test p-values at the bottom of the table, when inflation is above 6 percent: real wages adjust downward when there are negative shocks (significant at the 5 percent level) and I cannot reject that lagged positive shocks have no effect on current wages.

A potential concern is that there could be cotrends in inflation and the impact of rainfall shocks. For example, if inflation and the adoption of irrigation (which makes crops less reliant on rainfall) both trend upward over time, this could create a spurious correlation. In online Appendix Table 11, I conduct two placebo tests to rule out this concern: interactions of the rainfall shocks with a linear time trend (column 2) and with a dummy for whether the year is after 1970 (the sample midpoint and the beginning of India's green revolution, column 3) are small and insignificant, indicating that the inflation results are not driven by cotrends.

C. *Employment Effects*

I test for employment effects on all individuals who comprise the potential agricultural labor force: rural workers for whom casual employment or self-employment (i.e., work on their own farm) is a primary or subsidiary activity. One hundred percent of the individuals in the data who report any positive agricultural work fall within this group. Online Appendix Table 15 verifies that rainfall does not affect the composition of the sample, e.g., through the likelihood of reporting oneself as being in the agricultural labor force (column 1).

Employment in agriculture is the number of worker-days in the last seven days (the interview reference period) in which the individual did any agricultural work: own farm work plus hired work on someone else's farm.[37] Panel A of Table 4 indicates that, on average, a positive shock in the previous year lowers agricultural employment in the current year. The estimated decrease in agricultural activity is 0.153 days/week or 8.8 percent for the average worker (column 2) and 0.193 days/week or 11 percent for landless laborers (column 3); these coefficients are significant at the 1 percent level.

Panel B shows the main specification, equation (10). Contemporaneous positive shocks (row 1) raise average employment by 0.145 days/week or 8.3 percent. Contemporaneous droughts (row 3) decrease employment by 0.094 days/week or 5.4 percent. Consistent with the prediction under rigidity, when a drought is preceded by a positive shock (row 5), employment drops by about 0.254 days/week or 14.6 percent, more than twice the magnitude of the decrease in row 3. This difference is statistically significant at the 10 percent level in column 1 and at the 5 percent level in column 2 (see bottom of table). Similarly, when a year in which there is no shock is preceded by a lagged positive shock (row 7), this lowers employment by 6–7 percent.

In village labor markets, those who own land have the right to use their own labor on their farms before hiring others. As a result, those with little or no land, who are the net suppliers to the casual daily labor market, are the most likely to be rationed when rigidities bind. Consistent with this, employment decreases are concentrated among those with less land (column 3). Finally, there is little evidence that the shocks affect hiring in the non-agricultural sector (column 4).

[37] Only 31 percent of the individuals in the potential agricultural labor force report doing any agricultural work (on their own land or for someone else) in the past week. Among these individuals, 66 percent of worker-days were comprised of work on one's own land, and the remaining were for work on someone else's land. Note that wages can only be measured for this latter group.

TABLE 4—TEST FOR EMPLOYMENT EFFECTS OF RIGIDITY

		Total worker-days in agriculture			Non-agri employment
		(1)	(2)	(3)	(4)
Panel A. Simple specification					
Positive shock last year		−0.117	−0.153	−0.193	−0.014
		(0.051)	(0.051)	(0.059)	(0.027)
Positive shock last year × acres per adult in HH				0.067	0.016
				(0.054)	(0.020)
Panel B. Full specification					
Last year's shock	This year's shock				
1. Any	Positive	0.145	0.100	0.074	−0.033
		(0.063)	(0.068)	(0.080)	(0.027)
2. Interaction with acres per adult in HH				0.053	0.030
				(0.068)	(0.026)
3. None or negative	Negative	−0.094	−0.096	−0.188	0.011
		(0.055)	(0.055)	(0.071)	(0.024)
4. Interaction with acres per adult in HH				0.136	−0.020
				(0.069)	(0.021)
5. Positive	Negative	−0.254	−0.289	−0.416	0.009
		(0.084)	(0.086)	(0.090)	(0.053)
6. Interaction with acres per adult in HH				0.212	0.013
				(0.060)	(0.055)
7. Positive	None	−0.099	−0.130	−0.146	−0.013
		(0.066)	(0.065)	(0.074)	(0.035)
8. Interaction with acres per adult in HH				0.027	0.011
				(0.063)	(0.032)
Acres per adult in HH				0.709	−0.386
				(0.118)	(0.043)
(Acres per adult in HH)^2				−0.201	0.085
				(0.034)	(0.043)
Previous shock history controls?		No	Yes	Yes	Yes
F-test p-value: coefficient 3 $=$ coefficient 5		0.087	0.045	0.027	—
Observations: individual-years		632,324	632,324	632,324	632,324
Dependent variable mean		1.74	1.74	1.74	0.28

Notes: Dependent variable in columns 1–3 is the number of worker-days in the last 7 days in which the worker was employed in agricultural work (own farm work plus hired out work). In column 4 it is the number of worker-days in the last seven days in which the worker was hired for any non-agricultural work. A positive (negative) shock is defined as rainfall in the first month of the monsoon above (below) the eightieth (twentieth) percentile of the district's usual distribution. No shock (*none*) is rainfall between the twentieth and eightieth percentile of the district's usual distribution. In panel A, the shock covariate is a dummy for a positive shock in the previous year. In panel B, each of the four shock covariates (rows 1, 3, 5, 7) is an indicator that equals 1 if the sequence of shocks (presented as the shock in the previous year and the shock in the current year) was realized and equals 0 otherwise. The omitted category is {none or negative} last year and {none} this year. In columns 3–4, each shock covariate is also interacted with the number of acres per adult in the household (rows 2, 4, 6, 8). All regressions include district and year fixed effects. Columns 2 and 4 add controls for positive shocks two years ago and three years ago. Standard errors are clustered by region-year.

Source: NSS data

D. *Separation Failures: Compositional Effects on Employment*

A large theoretical literature has pointed out that labor rationing may affect the allocation of labor across firms (Singh, Squire, and Strauss 1986; Benjamin 1992).

Specifically, a rationed household's decision of how much labor to supply and its decision of how much labor to use in production are no longer separable. Households with smaller landholdings, which are more likely to face a binding rationing constraint since they are more reliant on selling labor in the external market, will supply labor more intensively to their own farms. This will lead to a misallocation of labor, with more labor per acre used in small farms compared to large farms.

In Table 5, I test whether rationing affects the composition of labor supply for agricultural households. I examine effects separately for three groups, defined in terms of acres per adult in the household:[38] the landless, who have no or marginal land (<0.01 acres); below median landholding; and above median landholding. I limit analysis to observations in which there was a non-positive shock in the current year, since this is when lagged positive shocks will be most likely to generate rationing.

The dependent variable in column 1 is total worker-days in agriculture, the same measure as in Table 4. Consistent with the Table 4 results, agricultural employment among the landless drops substantially. On average, there is no effect on households with below median landholdings; however, this masks substantial changes in labor allocation for these small landholders. Column 2 examines effects on hired labor on others' farms. In the year after a positive shock, while the landless experience the largest decrease in wage employment (1.196 days/week), small landholders also experience an estimated decrease of 0.443 days/week or 16 percent (significant at the 5 percent level). Column 3 indicates that, at the same time, small landholders increase the amount of time spent working on their own farms by 0.450 days/week or 33 percent, significant at the 5 percent level: this is the key prediction of the separation failures framework. This magnitude corresponds to having approximately one extra acre of land (the sample median) in a typical year. In contrast, large landowners' labor supply is largely unaffected by lagged positive shocks; this makes sense since these households do not sell much labor externally.

IV. Alternate Explanations

Could the results be explained by reasons other than downward nominal wage rigidity?

First, positive rainfall shocks may have persistent effects on productivity, for example, by improving future soil moisture. However, then future employment should also be higher and inflation should not affect persistence, which contradicts the results.

Second, shocks may affect worker quality. During negative shocks, employers may hire the subset of workers who are better quality, leading to a higher average wage per worker. However, this should not depend on inflation. It also cannot explain why wages do not adjust back down after lagged positive shocks have dissipated. In addition, I find little evidence that the various shocks change the composition of who receives wage employment, in terms of gender, education, age, or wealth (online Appendix Table 14).

[38] Acres per adult proxies for how much "excess" labor the household would traditionally supply off its own farm. This is consistent with traditional tests for separation failures, which examine whether, for a given number of acres, households with more adults tend to use more labor on their own farms (e.g., Benjamin 1992, Shapiro 1990, Udry 1996, LaFave and Thomas 2016). Note that I conduct this analysis at the household-year level to remain consistent with the previous literature.

Table 5—Separation Failures: Compositional Changes in Labor Allocation

	Dependent variable		
	Total worker-days in agriculture (1)	Worker-days as wage laborer (2)	Worker-days on own farm (3)
1 Positive shock last year	−1.727	−1.196	−0.531
	(0.503)	(0.438)	(0.299)
2 Positive shock last year × below median land	1.734	0.753	0.981
	(0.625)	(0.520)	(0.306)
3 Positive shock last year × above median land	1.289	1.348	−0.059
	(0.585)	(0.544)	(0.410)
Below median land	−1.015	−2.105	1.090
	(0.308)	(0.263)	(0.176)
Above median land	−0.618	−4.169	3.552
	(0.374)	(0.358)	(0.234)
F-test p-value: coefficient 1 + coefficient 2 = 0	0.981	0.047	0.046
Observations: household-years	166,002	166,002	166,002
Dependent variable mean: Landless & marginal	5.090	4.963	0.127
Dependent variable mean: Below median land	4.056	2.706	1.350
Dependent variable mean: Above median land	4.826	0.829	3.998

Notes: The table decomposes agricultural employment in the past seven days. The dependent variable in column 2 is the number of worker-days household members worked as hired casual wage laborers for others; in column 3 it is the number of worker-days household members worked on their own land; and column 1 is the total number of worker-days in agriculture (own farm work plus hired out work). A positive shock is defined as rainfall in the first month of the monsoon above the eightieth percentile of the district's usual distribution. The sample is restricted to observations in which there is no positive shock this year. The regressions interact the lagged positive shock covariate with binary indicators for landholding categories (acres per adult in the household). The omitted category is landless and marginal landowners: those with less than 0.01 acres per adult in the household. The median landholding is approximately 0.4 acres per adult in household. All regressions include district and year fixed effects. Standard errors are clustered by region-year.

Source: NSS data

Third, if positive shocks reduce future labor supply, e.g., through out-migration or intertemporal substitution of labor, this could explain why wages rise and employment falls in the following year. However, to explain the lack of downward wage adjustment, this would need to (i) occur both in the year after a positive shock and during a contemporaneous drought and (ii) occur when inflation is low but not when it is high. It is unclear why labor supply shifters would operate in this way. In addition, there is no evidence of increased migration after lagged positive shocks or during contemporaneous negative shocks in the NSS data (online Appendix Table 15) or in the ICRISAT data (online Appendix Table 17).

Fourth, if positive shocks enable credit-constrained small farmers to invest in capital, this could decrease future labor demand. To fit the results in Table 5, capital would need to be complementary with own household labor (to explain the increase in own farm labor supply) and substitutable with hired labor (to explain the large decrease in hired labor). In this case, wages for hired manual labor should be lower after a lagged positive shock, not higher. In addition, it is unclear why these effects would occur only when inflation is low. This explanation also doesn't account for why downward wage adjustment is hindered during negative shocks, again only when inflation is low. Finally, there is little direct evidence that lagged positive shocks lead to an increase in bullocks, tractors, or fertilizer, among the most common and important capital inputs in this setting (online Appendix Table 19).

Fifth, measurement error (e.g., due to rounding) is unlikely to drive the results. It is unclear why respondents would be differentially more likely to round wages during negative shocks and the year after positive shocks. In addition, if the wage results simply reflect reporting errors, we should not observe real employment effects.

Overall, the arguments above are of course suggestive. It is perfectly plausible that rainfall could affect labor supply or demand through a variety of channels. A complete investigation of their role is outside the scope of this paper. The model in this paper delivers a rich set of positive predictions under wage rigidity. The full pattern of results, for wages, employment, and inflation, along with asymmetry in effects for each of these tests, is consistent with these predictions.

Finally, efficiency wage models that do not involve nominal rigidities, such as moral hazard, screening, labor turnover, or nutrition, also generate equilibrium unemployment. However, they do not predict that wages will be rigid in response to shocks. For example, none of these models can account for why wages would rise under a positive shock but then not adjust back down once the shock has dissipated, or why this should be influenced by inflation. Similar arguments apply to search friction models that do not incorporate some nominal rigidity. Other models of unemployment, such as implicit insurance, informal unions, or the fairness efficiency wage model presented in Section I, could be consistent with these results if contracting pertains (at least in part) to the *nominal* wage. In this paper, I do not take a strong stance on the micro-foundation for rigidity, but rather argue that a model would need to incorporate some degree of nominal rigidity to explain the findings above.

V. Mechanisms: Survey Evidence on Fairness Norms

The presence of rigidities in markets for casual daily labor is perhaps especially surprising given the lack of institutional constraints in these markets. This suggests that non-institutional mechanisms discussed in the literature, such as fairness norms against wage cuts, may play a role in maintaining rigid wages. To obtain suggestive evidence on the relevance of fairness considerations, I surveyed 196 agricultural laborers and 200 employers in 35 villages across 6 districts in the Indian states of Orissa and Madhya Pradesh (Kaur 2019a).[39] Following Kahneman, Knetsch, and Thaler (1986), I presented scenarios about wage setting behavior and asked respondents to rate them as “Very Fair,” “Fair,” “Unfair,” or “Very Unfair.” Table 6 presents the scenarios and results.[40] The full questionnaire is presented in online Appendix D, and the data for the full questionnaire are available at Kaur (2019b).

Panel A establishes baseline norms relating to wage cuts in two sets of situations. For example, Question 1 presents a scenario in which a farmer who used to pay Rs. 120/day lowers the wage after a surge in unemployment after a factory (which used to pay Rs. 100/day) shuts down. The majority of respondents believed it was

[39] Orissa is one of India’s poorest states and is dominated by rain-fed paddy. Madhya Pradesh is more affluent, and a large portion of the survey areas is covered by soybeans, a cash crop.

[40] Each respondent was asked one-half of the questions to prevent the survey from becoming tedious, and in the case of paired scenarios (1A/1B, 3A/3C, and 9A/9B), was asked only one version of the scenario.

Table 6—Fairness Norms in Rural Labor Markets
(Proportion of Respondents Saying the Scenario Is "Unfair" or "Very Unfair")

	All	Laborers	Employers
Panel A. Acceptability of wage reductions			
1. A farmer hires a laborer to weed his land for 1 day at a wage of Rs. 120. There is a local factory that pays Rs. 100 per day. One month later, the factory shuts down and many people in the area become unemployed.			
(A) … After this, the farmer decides to do a second weeding and hires the same laborer as before at a wage of Rs. 100.	0.62	0.68	0.57
(B) … After this, the farmer decides to do a second weeding and hires one of the newly unemployed laborers at a wage of Rs. 100.	0.55	0.59	0.52
2. A farmer usually pays laborers Rs. 120 per day. His son becomes sick and the medical bills are very expensive. He lowers the wage to Rs. 110 per day.	0.79	0.71	0.87
Panel B. Money illusion			
3. Last year, the prevailing wage in a village was Rs. 100 per day. This year, the rains were very bad and so crop yields will be lower than usual.			
(A) … There has been no change in the cost of food and clothing. Farmers decrease this year's wage rate from Rs. 100 to Rs. 95 per day.	0.64	0.71	0.58
(B) …. The price of food and clothing has increased so that what used to cost Rs. 100 before now costs Rs. 105. Farmers keep this year's wage rate at Rs. 100.	0.38	0.53	0.23
(C) … The price of food and clothing has increased since last year, so that what used to cost Rs. 100 before now costs Rs. 110. Farmers increase this year's wage rate from Rs. 100 to Rs. 105.	0.09	0.09	0.08
4 A farmer usually pays laborers Rs. 100 per day plus food. There is not much work in the area and many laborers are looking for work. He stops providing food but continues to pay Rs. 100.	0.29	0.33	0.24
Panel C. Market-clearing mechanisms			
5. A farmer needs to hire a laborer to plow his land. There is not much work in the area at that time, and 5 laborers want the job. The farmer asks each of them to state the lowest wage at which they are willing to work, and then hires the laborer who stated the lowest wage.	0.61	0.78	0.44
6. A farmer needs to hire a laborer to plow his land. The prevailing rate in the area is Rs. 120 per day. The farmer knows there is a laborer who needs money to meet a family expense and is having difficulty finding work. The farmer offers the job to that laborer at Rs. 110 per day.	0.53	0.46	0.59
7. It is harvest time and all farmers in a village pay laborers Rs. 120 per day. One large farmer decides to harvest some of his land immediately and needs to hire 10 laborers. To find enough laborers, he pays them Rs. 150 per day for one week. In the following weeks, he decides to harvest the rest of his land, and re-hires 5 of the laborers at Rs. 120 per day.	0.63	0.70	0.57
8. There are 20 landowners in a village. The prevailing wage during plowing time is Rs. 120. Ten landowners want to attract extra laborers, and they increase the wage they pay to Rs. 130. The other 10 landowners don't need much labor and maintain the wage at Rs. 120.	0.45	0.52	0.39

Panel D. Fairness norms and effort

9. A farmer needs a laborer to weed his land. The prevailing wage is Rs. 120. There isn't much work in the area and many want the job. A laborer named Balu has family expenses for which he desperately needs money. The farmer knows Balu's situation, and offers him the job at: (A) Rs. 120 (B) Rs. 100. Given his need for money, Balu accepts the job. How carefully will he weed?

	More carefully than usual	With the normal amount of care	Less carefully than usual
(A) Rs. 120	0.55	0.44	0.01
(B) Rs. 100	0.06	0.54	0.40

Notes: 1. The sample is comprised of 196 casual laborers and 200 landowning farmers (i.e., employers) from 35 villages across 6 districts in the Indian states of Orissa and Madhya Pradesh. Respondents were working males aged 20–80. Interviews were conducted from July to August 2011. 2. Each respondent only received one-half of the scenarios presented in the table. In the case of paired scenarios (questions 1A/1B, 3A/3C, and 9A/9B), each respondent was asked only one of the scenarios in each pair. They were asked to rate each scenario as "Very Fair," "Fair," "Unfair," or "Very Unfair." The table reports the percentage of respondents that selected "Unfair" or "Very Unfair."

unfair if the farmer then rehires a previous employee at Rs. 100 (62 percent) or if he hires one of the newly unemployed factory workers at Rs. 100 (55 percent).[41]

Panel B investigates whether norms are anchored on the nominal wage rather than the real wage. Question 3 presents scenarios that involve a 5 percent real wage

[41] In this setting, it is common for some local factories to hire casual daily laborers from surrounding villages, drawing from the same labor pool as agricultural employers.

cut due to a drought, but vary the level of the nominal wage change; 64 percent of respondents view a 5 percent nominal wage cut as unfair. However, if there is 5 percent inflation and no nominal wage change, 38 percent view it as unfair. If there is 10 percent inflation and a 5 percent nominal wage increase, the percentage viewing this as unfair drops to 9 percent.[42] Note that similar exercises in the United States and Canada have produced similar patterns, with respondents exhibiting some (albeit a lesser) degree of "money illusion" (Kahneman, Knetsch, and Thaler 1986; Shafir, Diamond, and Tversky 1997). Similarly, 29 percent of respondents view a real wage cut as unfair if it is achieved by reducing an in-kind payment of lunch. This is sharply lower than the reactions to a nominal wage cut of smaller magnitude in Scenario 3A.[43]

Panel C indicates that several wage setting behaviors associated with market clearing are at odds with expressed fairness norms. For example, 61 percent of respondents felt it would be unfair if, during a period of high unemployment, a farmer asks workers for their reservation wage and then offers a job to the worker with the lowest reservation wage (Question 5); 63 percent of respondents think it is unfair for an employer to raise the wage during a period of high labor demand to attract enough workers, and then lower the wage to its previous level in later weeks when demand is lower (Question 7).

Finally, panel D investigates whether respondents think worker effort depends on fairness perceptions. Question 9 presents a scenario in which a farmer offers a job to a worker in financial distress. If the job is offered at the prevailing wage (which would uphold fairness norms and possibly also show benevolence given the laborer's distress), 55 percent of respondents say the worker would exert more effort than usual and only 1 percent state he would exert less effort than usual. In sharp contrast, if the wage is below the prevailing rate, only 6 percent of respondents state the worker would exert extra effort, while 40 percent state the worker would exert less effort than usual. Responses to this question were not substantially different between workers and employers.

Of course, survey responses may not reflect the actual actions people take when the stakes are real. The pattern of results in Table 6 simply lends some plausibility to the idea that fairness norms may be a way in which rigid wages are maintained in village labor markets. It is unclear, however, whether such fairness preferences are inherent features of utility or whether they arise endogenously, for example, as a coordinating device among laborers in the presence of incomplete contracting.

Online Appendix Table 20 tabulates responses to supplementary questions about respondents' own experiences and behavior. For example, 100 percent of workers and employers state that in their memory, there has not been a single year during which the prevailing nominal agricultural wage for a given season was lower than that in the previous year (Questions 1 and 6);[44] 74 percent of laborers report having

[42] In the local vernacular, the term "price of food and clothing" is used to describe inflation. Workers and employers say that this is frequently cited by workers when they are negotiating wages.

[43] Based on field interviews, the value of the food, when it is provided, usually exceeds Rs. 10. The responses to Scenario 3A versus 4 are consistent with evidence that there is lower earnings rigidity (and fewer layoffs during recessions) of workers who receive a base salary plus a bonus, presumably because bonuses can be more easily cut during downturns (Kahn 1997).

[44] During informal interviews, respondents stated that wages do vary within a year based on the season/tasks: for example, the transplanting, weeding, harvesting, and lean seasons may each have a distinct wage. They stated that the wage for each season takes as a starting ("reference") point the wage during that season in the previous year.

been involuntarily unemployed in the past (Question 2), and 95 percent of employers claim that they have never hired a worker at a wage below the prevailing wage during the lean season (Question 8).

VI. Conclusion

This paper tests for downward nominal wage rigidity in markets for casual daily agricultural labor. First, there is asymmetric wage adjustment: nominal wages rise in response to positive shocks but do not fall during negative shocks. Second, after transitory positive shocks have dissipated, nominal wages do not return to previous levels: they remain high in future years. Third, inflation moderates these effects: when inflation is higher, real wages are more likely to fall during droughts and after transitory positive shocks. Fourth, wage distortions generate employment distortions, creating boom and bust cycles: employment is 9 percent lower in the year after a transitory positive shock than if the positive shock had not occurred. Fifth, consistent with the misallocation of labor across farms, households with small landholdings increase labor supply to their own farms when they are rationed out of the external labor market.

In addition to its broad implications for unemployment and business-cycle dynamics, wage rigidity has particular relevance for developing country labor markets. One focus of the development literature has been that shocks cause shifts in the production frontier, leading to volatility in income and consumption. In the presence of wage rigidity, volatility has an additional implication: production may often not be at the frontier because labor markets do not adjust fully in each period. As implied by the employment results, this means rigidities may lower the levels and further increase the volatility of output and income. In addition, the evidence indicates that landless and marginal farmers, who are the poorest and most vulnerable workers in this setting, bear the brunt of the labor market effects. The findings in Section IIID suggest that this has not only distributional consequences: it can impact labor (mis)allocation, and consequently is another channel by which rigidities affect aggregate output.

Finding rigidities in casual daily labor markets is perhaps surprising, given the lack of formal institutional constraints in this setting. The survey evidence suggests that agricultural workers and employers view nominal wage cuts as unfair and believe that they cause effort reductions. Fairness preferences against wage cuts have been expressed in a range of contexts, including in richer countries (e.g., Kahneman, Knetsch, and Thaler 1986). While the strength of the norms expressed in Table 6 appears somewhat higher than that expressed in OECD countries, the survey findings suggest the potential for some commonality in the reasons for rigidity across settings.

However, it is unclear whether such fairness preferences are inherent features of utility or whether they arise endogenously. For example, fairness norms may simply be a coordinating device among workers in a setting where formal contracting or unions are difficult. Further exploration of the micro-foundations for fairness norms is necessary to fully assess the efficiency and welfare implications of wage rigidity. This in turn would inform our understanding of why wage rigidity may appear more prevalent in some settings than in others.

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