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FIRING COSTS AND FLEXIBILITY: EVIDENCE FROM FIRMS' EMPLOYMENT RESPONSES TO SHOCKS IN INDIA

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Abstract—A key prediction of dynamic labor demand models is that firing restrictions attenuate firms' employment responses to economic fluctuations. We provide the first direct test of this prediction using data from India. We exploit the fact that rainfall fluctuations, through their effects on agricultural productivity, generate variation in local demand within districts over time. Consistent with the theory, we find that industrial employment is more sensitive to shocks where labor regulation is less restrictive. Our results are robust to controlling for endogenous firm placement and vary across factory size in a pattern consistent with institutional features of Indian labor law.

I. Introduction

AN old insight from labor economics is that firing costs reduce the extent of employment adjustment to economic shocks: during a downturn, firing costs reduce the number of layoffs, while during an upturn, hiring is curbed because of the possibility of having to lay off workers in the future (Oi, 1962; Nickell, 1986; Hamermesh, 1993). Employment inflexibility (from the firm's perspective) and its possible negative effects on average as well as aggregate output, employment, and wages is therefore the price of job security provisions, and this is the basis of a great deal of policy debate surrounding draconian labor laws that have been enacted in many countries (as documented, for example, by Botero et al., 2004).[1]

In this paper we provide the first direct test (to our knowledge) of the prediction that the magnitude of employment responses to shocks should vary negatively with the degree of employment protection. Obtaining a credible test of this prediction is difficult for a number of reasons. First, we require a setting where there is variation across space or time in the extent of employment protection, with the added requirement that this policy variation does not simply reflect variation

in unobserved determinants of employment. Arguably, the latter condition does not obtain in cross-country or even within-country time-series variation in employment protection policies (however, see Heckman & Pages, 2004, for some evidence that labor reforms in Latin America may be considered to have been exogenous).

Being able to credibly attribute differences in outcomes to differences in labor regulation is obviously a general problem for any study of the effects of labor policies. An additional concern for a study such as ours is the identification and measurement of fluctuations. Because the source of fluctuations is typically not observable or directly quantifiable, previous empirical studies have inferred the magnitude of fluctuations from changes in observable quantities such as output or sales. For example, Bentolila and Saint-Paul (1992), in their study of the effects of the introduction of flexible labor contracts in Spanish manufacturing, measure shocks by the change in log sales of a firm, which they then relate to employment responses. Similarly, Abraham and Houseman (1993) relate (aggregate) employment to output in their comparison of employment dynamics in the United States and Germany.

This approach is problematic for at least two reasons. First, fluctuations in aggregate or firm-level output can reflect either unobserved demand or cost shocks (or both), and the corresponding change in employment can be expected to be different in each case. Second, this method cannot satisfactorily distinguish between fluctuations that are foreseeable and those that are inherently unpredictable.[2] The key innovation of this paper is its utilization of a well-defined and measurable source of fluctuations that are strictly unpredictable in nature, exogenous to the labor regime, and comparable across the units of study; this is the precise sense in which we think of our test as being direct. This approach avoids the problems associated with defining fluctuations in terms of endogenously determined variables.

Our setting is rural India, where agriculture exists alongside industry. Differences in employment protection laws across the states of India (and over time) provide variation in firing costs in the industrial sector. To obtain a plausible shock variable, we measure rainfall fluctuations that affect agricultural yield. In this particular context, rainfall shocks are ideal for a number of reasons: (1) they plausibly give rise to labor supply or output demand shifts for local industries through their effect on agricultural yields; (2) they are unpredictable in nature and therefore not likely to induce

[1] The effects of job security provisions on average and aggregate outcomes are, however, theoretically ambiguous. Because firing restrictions reduce hiring as well as firing, the average level of employment (for a given firm) may either increase or decrease (Bentolila & Bertola, 1990, suggest that for realistic parameter values, higher firing costs may actually raise average employment). The effects on aggregate levels of output and employment are also indeterminate once we account for the effects of these restrictions on entry and exit (however, see Hopenhayn & Rogerson, 1993, for calibrations that suggest a negative overall effect of a tax on job destruction). Finally, as Basu, Fields, and Gupta (2008) argue, job security provisions can even result in a lower level of wages, hurting the very constituency they are meant to protect.

[2] The distinction is potentially important because, depending on the structure of adjustment costs, it may be optimal to smooth foreseeable fluctuations in advance, so that the resulting variation in employment is of a different character than in the case of unpredictable shocks. Indeed, the relation between employment and leads and lags of aggregate output is likely to be very different in the two cases.

anticipatory smoothing of employment (which is important for our purposes because our data are not disaggregated enough at the temporal level to identify such anticipatory smoothing); (3) they are temporary and recurring and therefore factor into the forward-looking decisions of firms; (4) they are exogenous to the labor regime, and are not caused by employment changes in the industrial sector or by any other factors that may affect employment; and (5) we are able to provide evidence that the measured rainfall fluctuations represent comparable shocks across labor regimes. The empirical strategy is then to test whether these shocks induce larger factory employment responses in states that have enacted proemployer legislation.

Our results provide a confirmation of the prediction that industrial employment should be more flexible in proemployer regions. We first confirm that rainfall fluctuations do indeed have a impact on local agricultural production and incomes (but not agricultural wages). Importantly for our identification strategy, these effects are not differential across labor regimes. We then document that high (low) rainfall increases (decreases) industrial employment, indicating the operation of a demand effect through agricultural incomes. Furthermore, as predicted by theory, the induced change in employment is indeed significantly greater in proemployer states. We also find that the responses to these local shocks appear to be concentrated among industries that are likely to be dependent on local demand, consistent with our interpretation of the shocks as representing demand fluctuations.

We address the endogeneity of labor regulations in two ways: we verify the robustness of the results to the inclusion of a set of controls that may be plausibly correlated with labor regulation, and we use the fact that labor regulations apply only to factories above a size threshold: if our results reflect the effects of labor regulation, then the responses to rainfall shocks should not vary across labor regimes for unregulated factories, and this is indeed what we find.

Our focus in this paper is primarily on the test of the hypothesis that firing costs reduce employment flexibility, but it is natural to ask how reduced flexibility translates into outputs, profits, and intensity of use of nonlabor inputs. Although we have less confidence in the accuracy of measurement of nonlabor variables in the factory data, there is some weak evidence that the average change in outputs and profits due to shocks is no greater for factories in prolabor regions. Taken at face value, this finding suggests that the latter are able to compensate for the lack of employment flexibility by adjusting along other margins. We are, however, unable to find any differential adjustment of other observable inputs, leaving the possibility that adjustments may take the form of hiring and firing casual (temporary) workers.

Our employment results are a striking confirmation of the hypothesis that job security provisions in India have constrained labor adjustment on the part of firms. Our paper also ties into a wider literature that seeks to understand the workings of the rural economy in India. Whereas the existing literature tends to focus on either the agricultural sector or the industrial sector in isolation, our results highlight the close relation between the two—in particular, our finding of the significance of local demand for the factory sector may be surprising and should be treated as a caveat against thinking of formal sector products as being bought and sold in national rather than regional markets.

The remainder of the paper is organized as follows. Section II describes labor regulations in India, section III describes the data, section IV describes the empirical strategy, section V describes the results, and section VI concludes.

II.   Labor Regulation in India

The basis of labor regulation in India is the Industrial Disputes Act (IDA) of 1947, which sets out the regulations governing employer-worker relations and the legal procedures to be followed in the case of labor disputes in the factory sector. The IDA was passed by the central government, and in its original form, it applied equally to all states. But since India is a federal democracy, with both the central and state governments having jurisdiction over labor legislation, the act has since been amended by state governments. These amendments have caused the states to differ markedly in their labor regulations.

The IDA covers several aspects of industrial disputes, such as unfair labor practices, strikes and lockouts, and layoffs and retrenchments. It calls for the setting up of special bodies (tribunals, boards of conciliation, and labor courts, for example) to arbitrate disputes in the industrial sector, while specifying their composition and extent of authority. Of specific interest for us are sections V-A and V-B of the IDA, which describe the regulations pertaining to layoffs and retrenchments. The regulations in section V-A cover industrial establishments in which more than "fifty workmen on an average per working day have been employed in the preceding calendar month" (section 25-A, chapter V-A, IDA; see Malik, 1997). This section asserts the right of workers who have been laid off or retrenched to adequate compensation. Specifically, workers who have been on the rolls for at least a year are entitled to compensation at 50% of their regular wage for each day that they are laid off (up to a maximum of 45 days). Workers who are to be retrenched are to be given one month's notice and are eligible for compensation from the employer equal to 15 days' average pay for each year of completed service. Section V-A also limits closure of undertakings by requiring notification of the government at least 60 days prior to closure. Furthermore, all workers thereby dispossessed of jobs are to be compensated according to the compensation for retrenched workers.

Section V-B lays out some special provisions that apply only to industrial establishments employing at least 100 workers.[3] This section is more draconian: it requires that

[3] In the original IDA, this section applied only to establishments with more than 300 workers, but this threshold was subsequently revised by the central government in 1982.

TABLE 1.—DATA SOURCES

Source	Years	Variables
Annual Survey of Industries (ASI), conducted by the Central Statistical Organization of India	1988, 1991, 1994	Factory employment, fixed capital, output, raw material and fuel expenditures
State × three-digit industry panel constructed from ASI, from Aghion et al. (2008)	1980–1997	Factory employment
Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series, version 1.02 (Center for Climatic Research, University of Delaware)	1950–1999	Rainfall shock
Besley and Burgess (2004), based on state-level amendments to the Industrial Disputes Act of India	1949–1995	Labor regulation
India Agriculture and Climate Data Set, updated using statistics published by the Directorate of Economics and Statistics, Ministry of Agriculture, India	1980–1997	Agricultural yields
Consumer Expenditure Survey, conducted by the National Sample Survey Organization of India	1987, 1993, 1999	Household per capita expenditure

no workers may be laid off or retrenched without the prior permission of the government. Closure of establishments requires an application to be filed with the government at least ninety days before the proposed closure. The penalty for violating the regulations in V-B includes a prison term of up to a year or a fine of five thousand rupees in the case of illegal closure (or both) and prison term of up to a month and a fine of 1,000 rupees in the case of illegal layoff or retrenchment.

The IDA does not cover temporary or casual workers, so in principle, firms could work around the provisions in V-A and V-B by using casual labor. We do not have any data on the extent of casual labor and are therefore unable to identify whether it is indeed being substituted for formal labor. However, as Fallon and Lucas (1993) note, the vigorous opposition of labor unions, as well as the restrictions imposed on the use of contract labor by the Contract Labor Regulation and Abolition Act of 1970, are likely to significantly curtail this channel of avoidance of labor regulation.

III. Data

We combine a few data sources: data on labor regulation; manufacturing outcomes and district and state-industry levels of aggregation; and agricultural production, agricultural wages, and district per capita expenditure. These sources are summarized in table 1.

A. Labor Regulation

The basis of industrial labor regulation in India is the IDA of 1947, which sets out the legal procedures to be followed in the case of labor disputes such as layoffs, retrenchments, and strikes in a factory. The IDA was passed by the central government, but has since been extensively amended by state governments, causing Indian states to differ markedly in their labor regulations.

Besley and Burgess (2004) read all state-level amendments made to the IDA during 1958 to 1995 in sixteen major Indian states (from Malik, 1997). Each amendment was coded as being either proworker, neutral, or proemployer, depending on whether it lowered, left unchanged, or increased an employer's flexibility in hiring and firing factory workers,

TABLE 2.—PROWORKER, PROEMPLOYER AND NEUTRAL STATES IN ASI DISTRICT-LEVEL DATA

ProWorker	Neutral	ProEmployer
Gujarat	Bihar	Andhra Pradesh
Maharashtra	Haryana	Karnataka[a]
Orissa	Karnataka[a]	Rajasthan
West Bengal	Madhya Pradesh	Tamil Nadu
	Punjab	
	Uttar Pradesh	

[a] Karnataka switches from neutral to proemployer in 1987–1988; Classifications are based on adding the number of proworker laws and subtracting the number of proemployer laws passed in each state; these classifications hold between 1987 and 1994, inclusive. For details, refer to section II.

respectively. A state's labor regulation regime in any year was then obtained as the sum of these scores over all preceding years. Based on this cumulative score, Besley and Burgess (2004) classified four states—Gujarat, Maharashtra, Orissa, and West Bengal—as proworker in 1988. Six states—Andhra Pradesh, Karnataka, Kerala, Madhya Pradesh, Rajasthan, and Tamil Nadu—were categorized as proemployer. Six others—Assam, Bihar, Haryana, Jammu and Kashmir, Punjab and Uttar Pradesh—were classified as neutral with respect to labor laws. These categorizations are summarized in table 2.

We followed this scheme of cumulating the Besley-Burgess scores to categorize the states as proworker, proemployer, or neutral in each year of our study. Since there were few labor law amendments after 1987, this classification remains identical to the original Besley-Burgess classification for 1988 throughout our study period. The only exception is Karnataka, which switched from being neutral to being proemployer between 1987 and 1988.

We also report results using three alternative measures of regulation suggested by Ahsan and Pages (2008). The first measure is based on the critique by Bhattacharjea (2006) of Besley and Burgess's, coding. In addition, Ahsan and Pages distinguish between sections of the IDA that specifically relate to layoffs and sections that address the dispute settlement process between employers and workers. Following Ahsan and Pages, we refer to the former as employment protection legislation (EPL) and the latter as dispute settlement (DS) legislation. All three measures are constructed in a similar way to Besley and Burgess's measure: by coding amendments to the IDA and cumulating the labor score

TABLE 3.—BASELINE SUMMARY STATISTICS BY PROWORKER, NEUTRAL, AND PROEMPLOYER STATES

	District-level Panel		
	Proworker	Neutral	Proemployer
Number of districts (in 1988)	78	186	80
Labor regulation strictness measure[a]	1.79	0	−1.54
	(0.76)	(0)	(0.50)
Rainfall shock	−0.03	−0.12	0.09
	(0.68)	(0.67)	(0.62)
% agrarian employment in district	44.48	45.57	46.04
	(16.31)	(15.66)	(15.53)
% landless in district	22.40	16.67	19.89
	(13.56)	(11.87)	(13.09)
Average capital-to-output ratio in district	1.56	1.60	1.09
	(2.76)	(4.96)	(1.66)
% employment in industries linked to agriculture	43.26	43.82	46.65
	(31.25)	(33.93)	(32.31)

States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. Rainfall shock variable = 1 if annual rainfall is less than the 20th percentile of historical distribution, = 0 if less than the 20th and more than the 80th percentile of historical distribution, and = −1 if more than the 80th percentile of historical distribution. Share of agrarian employment, percent landless, capital-to-output ratios, and share employed in agriculture-related industries are measured in 1988.
[a] This measure is from Besley and Burgess (2004).

over preceding years. In contrast with the remaining measures, there are only proworker and neutral states in the EPL coding.

B. The Industrial Sector

Manufacturing establishments in India are broadly classified as either factories or informal enterprises, where the distinction is based on a cutoff defined in terms of employment. According to the Factory Act, a manufacturing establishment that uses any form of power (such as electricity, steam, or diesel) to drive machinery is a factory if it employs at least ten workers. A manufacturing establishment that does not use power is a factory if it employs at least twenty workers. Since factories alone are subject to industrial entry and labor regulation laws such as those laid out in the IDA, our data set on manufacturing establishments pertains to the factory sector.

The source of our data on factories is the Annual Survey of Industries (ASI), a cross-sectional, national survey and census of factories that is conducted annually by the Central Statistical Organization of India. The ASI has two parts: a census of all factories employing 100 workers or more and a survey that randomly samples about a quarter of all other registered factories. The data are not a panel at the factory level due to the unavailability of factory identifiers, but the combined data from the ASI census and survey sections are fully representative of all factories in India and can be used to estimate industrial sector aggregates at regional levels by weighting the factory-level data by the inverse of the sampling probabilities.

C. District-Level Data Set: Factories, Rainfall Shock, Agricultural Production, and Household Expenditure

The majority of our regressions examine the effects of labor regulation and rainfall shocks on the industrial sector at the spatial level of districts, the primary administrative unit in India. Our district-level data set covers nearly 360 districts,

which constitute the sixteen largest Indian states and account for nearly 95% of India's population. To arrive at district-level estimates of factory sector employment, revenue, input costs, fixed capital, and wages, we used the survey weights to aggregate unit (factory) level data from three rounds of ASI. Our final district data set has 1,042 observations across three years: 1987, 1990, and 1994.[4] Tables 3 and 4 summarize characteristics of the districts in our sample and the industrial sector outcome variables we use, respectively. The summary statistics are grouped by proworker, proemployer, and neutral states.

Our rainfall data are from the Center for Climatic Research at the University of Delaware.[5] The rainfall measure for a latitude-longitude node (on a 0.5° latitude by 0.5° longitude grid) combines data from twenty nearby weather stations using an interpolation algorithm based on the spherical version of Shepard's distance-weighting method. We matched these rainfall data to districts by calculating the grid point nearest to the geographic center of a district.

Previous research on India suggests that while low rainfall hurts agricultural production, excess rainfall helps.[6] Our primary measure of the rainfall shock (Rainshock) is therefore constructed in such a way that higher values indicate lower amounts of rainfall. Rainshock is equal to 1 when the annual district rainfall is less than the 20th percentile of the district's historical average, 0 when it is between the 20th and 80th percentiles, and −1 one when it is above the 80th percentile

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[4] The number of districts in India increased during the study period when new districts were created. To be consistent, we have used the 1988 district definitions throughout the paper, and any new districts created between 1988 and 1994 have been merged back to their parent districts. Going by the 1988 district definitions, we have data on 344 districts in 1987, 347 in 1990, and 351 in 1994. The total number of districts varies by year because the ASI does not stratify sampling by districts and is therefore not guaranteed to cover all districts.

[5] This is the Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series (1950-99), Version 1.02.

[6] Jayachandran (2006) finds similar results for the effects of excess rainfall on agricultural yields.

TABLE 4.—OUTCOME VARIABLES BY PROWORKER, NEUTRAL, AND PROEMPLOYER STATES

	Proworker	Neutral	Proemployer
District-level employment outcomes			
Number of workers	24,406.92	8,806.28	19,560.38
	(44,328.34)	(14,086.25)	(28,465.96)
Man-days (thousands)	10,088.15	3,429.88	7,078.49
	(18,998.96)	(5,836.59)	(9,937.37)
State-industry level employment outcomes			
Number of workers	5,016.81	2,678.27	3,807.37
	(14,090.93)	(7,619.89)	(13,920.15)
Number of employees	6,543.20	3,444.01	4,674.66
	(16,638.14)	(9,349.44)	(14,867.71)
Other district-level outcomes			
Agricultural production	99,917.92	79,458.38	92,412.49
	(72,989.70)	(62,873.04)	(76,355.40)
Monthly per capita expenditures	320.34	308.41	346.62
	(134.76)	(129.99)	(124.58)
Capital stock at close of business year	421.36	135.41	239.09
	(790.44)	(334.22)	(568.89)
Value of materials used in production	813.67	253.60	422.67
	(1,839.71)	(463.95)	(697.14)
Value of electricity used in production	35.58	13.63	22.18
	(59.49)	(30.28)	(28.96)
Value of fuel used in production	80.60	28.27	44.09
	(145.01)	(60.82)	(58.77)
Value of total output	1,274.213	396.03	661.96
	(2,842.30)	(728.12)	(1,058.61)
Value added	243.96	71.51	120.16
	(576.19)	(146.71)	(207.96)
Profits	64.60	21.63	30.62
	(218.02)	(63.19)	(87.78)

States are classified based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. Agricultural production is a weighted sum in which agricultural output for each crop (in kg) is weighted by the crop's average price from 1950 to 1987 (in INR/kg). Per capita expenditures are in 1999 INR. Capital stock, materials, fuel, total output, value added, and profits have been converted to thousands of 2009 U.S. dollars.

(this is identical to Jayachandran's 2006 definition of rainfall shocks).[7]

Before examining the relationship between *Rainshock*, labor laws, and factory employment, we show that *Rainshock* is associated with drops in agricultural production, wages, and district mean per capita expenditure. Our data on agricultural production and wages of agricultural laborers are from an updated version of the district-level India Agriculture and Climate Data Set. This data set was originally compiled for the years 1957/58 to 1986/87 by James Robert E. Evenson and James W. McKinsey Jr. using statistics published by the Directorate of Economics and Statistics (within the Indian Ministry of Agriculture). These data have been updated to 1996 using more recent issues of the same government publications.[8] We measure district annual agricultural production by a constant price-weighted sum of the district output of all major crops, where the individual crop prices are fixed at their average value over 1957 to 1987. We supplement these data

with data on wheat and rice cultivation area by state, also obtained from Ministry of Agriculture publications.

Data on average household per capita expenditure in districts are based on consumption expenditure surveys conducted by India's National Sample Survey Organization (NSSO) in 1987, 1993, and 1999. These cross-sectional, nationally representative household surveys are a standard source of poverty measurement in India. In estimating district-level averages, households were weighted by the inverse of the sampling probabilities.[9]

D. State- and Industry-Level Data Set

In some of our regressions, we use state-industry-level panel data, which are also drawn from multiple rounds of ASI. We are hesitant to attempt to distinguish among industries in the district data set due to the small sample size at the district level. Fortunately, the ASI is designed to estimate manufacturing sector outcomes by industry at the state level, with every state and industry group surveyed as an individual stratum. This stratification by industries is at the three-digit level of the ISIC classification of industries, which yields a high level of disaggregation.

We aggregated our district-level data on rainfall to the state level by taking simple averages of district rainfall within each

[7] This definition of shocks seems appealing because adjusting the number of workers in the face of small fluctuations is an unlikely event in as regulated an environment as we are considering. Nonetheless, we have also experimented with a continuous shock measure, which is the negative of the deviation of annual rainfall from the district's historical average, normalized by the historical standard deviation of rainfall in the district. The results (available on request) are qualitatively similar to the ones we report using the discrete shock measure, but they are less precise.
[8] Yield data updates were compiled by Rohini Pande and Siddharth Sharma.

[9] We are grateful to Rohini Pande and Petia Topalova for sharing with us their district-level estimates based on the NSSO Consumption Expenditure Surveys.

state. Our state-level rainfall shock measure is analogous to the district-level measure and is defined in terms of deviations from the historical state averages of rainfall. There was no need to modify the labor regulation dummies since they were already defined at the state level. We then merged these with state- and industry-level factory data constructed by aggregating unit-level annual ASI data using sampling weights and three-digit industry codes. The resulting data set is an annual panel covering 130 industry groups across thirteen states over seventeen years (1980–1997).[10]

IV.   Empirical Strategy

The basic empirical analysis is derived from a simple model of labor adjustment to exogenous shocks in the face of linear adjustment costs. The model is based on Bertola (1990). Because the theory is standard, we have relegated it to the appendix. The essential intuition is that firing costs factor into the forward-looking employment decisions of firms, creating a wedge between the wage and the marginal product. In the face of positive shocks to the environment, the firm finds that the effective wage is higher than the actual wage and therefore curtails its hiring (relative to the case with no firing costs). When faced with a negative shock, the firm finds the effective wage to be lower than the actual wage, and this curtails its layoffs (relative to the case with no firing costs). Firing costs therefore restrict the firm's adjustments to exogenous shocks. In the context of our empirical analysis, fluctuations represented by rainfall shocks should induce smaller employment adjustments in more regulated environments.[11]

A potential complication in the empirical exercise is that rainfall fluctuations create opposing effects on employment: on the one hand, good (bad) rainfall increases (decreases) agricultural incomes and hence demand for local industrial goods, but on the other hand, good (bad) rainfall may increase (decrease) agricultural demand for labor and represent a negative (positive) labor supply shock for local industry. However, the model clarifies that if rainfall fluctuations create comparable wage and price shocks across labor regimes, the net effect of price and wage changes on employment is magnified in lower firing cost regimes: this is the hypothesis being tested. Key to this test is the comparability of measured fluctuations across space and time, which we establish in the following sections.

[10] The state-industry data on factories were used by Aghion et al. (2008). We are grateful to the authors and the *American Economic Review* for making these data publicly available.

[11] A caveat is that we are not able to deal explicitly with multiestablishment firms. In principle, a multiestablishment firm could avoid firing any workers by moving them from districts facing a negative demand shock to districts with a positive demand shock. This could therefore bias downward the effect of labor regulations on employment flexibility. We think this is an unlikely scenario, given the extremely low rates of geographical mobility of labor in India, the high degree of correlation of rainfall fluctuations within a state, and our sense, given the available information, that there are not many multiestablishment firms in the data. On average, only about 3% of ASI respondents report being part of a multiestablishment firm having another factory in the same state.

A.   District-Level Regressions

Exploiting variation in rainfall across districts over time, we first measure the impact of rainfall shocks by regressing district outcomes on a rainfall shock measure ($Rainshock_{jt}$) for district j and year t. The regressions control for macroshocks with year fixed effects and for time-invariant regional variation with district fixed effects. For outcome x, our base specification is thus

$$x_{jt} = \alpha Rainshock_{jt} + \rho_j + \rho_t + \epsilon_{jt}, \qquad (1)$$

where ρ_j and ρ_t denote district and year fixed effects, respectively. The coefficient α estimates the average effect of the rainfall shock on the district outcome x_{jt}. Since *Rainshock* is constructed to take on higher values the lower the amount of rainfall, a negative estimate of α would mean that low rainfall has a negative effect on x_{jt}.

The theory suggests that the response of the industrial sector to shocks depends on industrial labor regulation. Accordingly, our key regressions estimate how the effect of rainfall shocks varies across districts with different labor regulation regimes by interacting $Rainshock_{jt}$ with the labor law dummies:

$$
\begin{aligned}
x_{jt} = {} & \alpha Rainshock_{jt} + \beta(Rainshock_{jt} \times Proworker_{jt}) \\
& + \delta(Rainshock_{jt} \times Proemployer_{jt}) \\
& + \rho_j + \rho_t + \epsilon_{jt}.
\end{aligned} \qquad (2)
$$

As described earlier, districts are either *Proworker*, *Proemployer*, or *Neutral*, depending on the cumulative value of the Besley-Burgess labor law index in their state. Thus, β and δ measure the effect of rainfall shocks on *Proworker* and *Proemployer* districts, respectively, relative to that in *Neutral* districts. For example, suppose that the average effect of rainfall shocks, as measured by α in equation (1), is negative. Then a negative estimate of δ would imply that the decrease in x_{jt} due to low rainfall is larger in *Proemployer* districts as compared to *Neutral* districts. If α in equation (1) is estimated to be positive, then a negative estimate of δ would imply that relative to *Neutral* districts, the increase in x_{jt} due to low rainfall is lower in *Proemployer* districts.

We estimate equation (2) for several outcome variables. We examine the direct effect of rainfall shocks by looking at how district agricultural production, farm wages, and household per capita expenditures decline when the rains fail. Then our main set of estimations examines the impact of rainfall shocks and labor regulation on employment in the factory sector. Finally, we look at other industrial sector outcomes such as input costs, wages, revenue, and profits.

The coefficients β and δ are intended to capture how responses to rainfall shocks vary across districts with different labor laws, holding all other district characteristics constant. One concern with our interpretation of the coefficients is that labor regulation might be correlated with other factors that, determine rainfall impacts on the local economy or factories responses, to the rainfall shock. For example, since workers

might lobby the government for proworker regulation, states with more nonagricultural employment (and thus presumably a larger blue-collar lobby) may have enacted more proworker legislation. But less agricultural areas are also less likely to be dependent on rainfall. Another possibility is that factories' response to shocks varies by their capital intensity and that labor laws are correlated with the average labor intensity of factories.

Jayachandran (2006) addresses such concerns by including relevant area characteristics and their interactions with rainfall shock as controls. Following a similar strategy, we control for the interaction of $Rainshock_{jt}$ with key baseline characteristics of districts, such as the percent of total employment that is in the agrarian sector and in food-based sectors, and the average capital-to-output ratio in industry. We also interact the rainfall shock variable with the following state-level variables measured in 1980 (before the first year of our data): the number of years that hard-left parties have held a majority in the state legislature (this variable is reported to be an important correlate of manufacturing growth by Besley & Burgess, 2004), the total cultivated area in the state, and the percentages of cultivated area sown with rice and wheat. The cultivation variables are intended to account for differences across states in the agricultural responses to rainfall fluctuations. Rice and wheat are the two major crops grown in India and are also known to have different requirements in terms of rainfall. In addition, wheat cultivation is somewhat concentrated in the neutral states.

B. State and Industry Panel Regressions

We replicate our main results on the differential responses to rainfall shocks across labor regulation regimes using the state-industry panel. The basic specification is now

$$
\begin{aligned}
x_{skt} = {} & \alpha Rainshock_{st} + \beta(Rainshock_{st} \times Proworker_{st}) \\
& + \delta(Rainshock_{st} \times Proemployer_{st}) \\
& + \rho_{sk} + \rho_t + \epsilon_{jt},
\end{aligned} \tag{3}
$$

where ρ_{sk} denotes a fixed effect for industry k in state s. This is analogous to the district-level regressions that measure how the response to rainfall shocks varies by labor law. x_{skt} measures the outcome in state s, three-digit industry group k, and year t. The rainfall shock $Rainshock_{st}$ is measured at the state level by averaging the rainfall in districts within every state and as in the district-level regressions, it is interacted with state labor law dummies. Thus, the interpretation of β and δ is similar to that in the district-level specification. The regressions control for state-industry and year fixed effects.

Clearly, compared to the district data, the local rainfall shock is less precisely measured in these state-level data. But the state-industry panel adds to our analysis in several ways. With data stretching over a period of seventeen years at annual frequency, the state-industry panel offers substantially more variation in rainfall over time. Second, since the ASI is stratified by state and industry, estimates of factory sector

outcomes are more precise in these data. Therefore, one of our first robustness checks is to replicate the main district-level results on the state and industry panel.

C. Robustness Checks Using the District and State-Industry Panels

In Section V, we present the main results on employment responses, as well as a variety of supporting results that demonstrate the consistency and robustness of our empirical findings: (1) exploiting the fact that larger firms (in particular, the IDA specifies two employment size cutoffs) are subject to more draconian firing costs; (2) testing for differential responses to shocks across industry types classified by their a priori susceptibility to local demand; (3) robustness to the inclusion of fixed effects, which control for the potential selection of firms into states based on their level of flexibility in response to shocks; and (4) robustness to the alternative measures of labor regulation described in section CA.

V. Results

A. Effects of Rainfall Shocks on Agricultural Production, Agricultural Wages, and Expenditures

We begin by testing our premise, which is that the factory sector is affected by rainfall shocks through their effects on the local population. To test whether poor rainfall induces a negative shock to local demand, we examine the impact of rainfall shocks on agricultural production and expenditures. The results of these regressions, which control for district and year fixed effects (thus exploiting changes within districts over time), are reported in table 5. Columns 1 and 2 capture the main effects of rainfall shocks on the value of agricultural production and per capita monthly expenditures. We see large declines associated with a rainfall shock in both variables, indicating that the mechanism of rainfall shocks' effects on the factory sector through local demand could be at play.

Next, we test whether rainfall shocks may also induce a labor supply effect: low (high) rainfall, as it reduces (increases) the productivity of agricultural laborers, would drive down (up) the agricultural wage and move workers into (out of) the industrial sector. We test for this mechanism by measuring the impact of rainfall shocks on the agricultural wage, and find, as reported in column 3, that the effect is weak and not statistically significant.[12]

Finally, in columns 4 to 6 of table 5, we verify that the effects of rainfall shocks on agricultural production,

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[12] A decline in agricultural production could also affect the industrial sector through its effect on the price of agricultural outputs, which are often used as intermediate inputs into production in the industrial sector (e.g., cotton sold to textile mills). Because we do not have price data for these inputs, we cannot test for this channel directly. However, when we regress the value of materials used in production on the rainfall shock main effect, we find a small and insignificant negative coefficient (results not shown here), indicating that at least the value of intermediate materials used does not decline in response to rainfall shock.

TABLE 5.—EFFECT OF RAINFALL SHOCK ON AGRICULTURAL PRODUCTION AND HOUSEHOLD EXPENDITURES

	District Panel					
Dependent variables	Agricultural Production (1)	Per Capita Expenditures (2)	Agricultural Wage (3)	Agricultural Production (4)	Per Capita Expenditures (5)	Agricultural Wage (6)
Rainfall shock	−8,462	−14.28	−0.220	−18,393.23	1.66	0.23
	(1,748)***	(3.741)***	(0.159)	(7,183.168)**	(14.660)	(0.462)
Rainfall shock ×						
(β) Proworker state				−1,854.27	0.66	−0.25
				(4,823.374)	(11.422)	(0.359)
(δ) Proemployer state				4,039.27	15.03	−0.18
				(4,025.022)	(11.569)	(0.367)
Fixed effects		District + Year			District + Year	
Ho: δ − β = 0				5,894	0.0752	14.37
				(3,710)	(0.314)	(9.744)
Number of observations	4,398	1,071	4,398	4,398	1,071	4,398

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. Rainfall shock = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution; "Agricultural production" and "Per capita expenditures" are as defined section IIIC. "Agricultural wage" is the daily wage rate in INR. Specifications in columns 4–6 include rice and wheat area per acre and total cultivated area in the state in 1980 interacted with rainfall shock.

TABLE 6.—DISTRICT-LEVEL RESULTS: EFFECT OF RAINFALL SHOCK ON INDUSTRIAL EMPLOYMENT BY LABOR REGULATION STRICTNESS

	District Panel			
Dependent Variables	Number of Workers (1)	Man-Days (thousands) (2)	Number of Workers (3)	Man-Days (thousands) (4)
Rainfall shock	−582.0	−250.0	−15,445.075	−7,635.478
	(403.5)	(145.4)*	(14,778.097)	(5,466.523)
Rainfall shock ×				
(β) Proworker state		−512.621	59.315	
			(1,625.653)	(577.387)
(δ) Proemployer state			−2,851.241	−855.226
			(1,512.609)*	(506.933)*
Fixed effects		District + Year		
Ho: δ − β = 0			−2,339	−914.5
			(1,164)**	(435.5)**
Number of observations	1,042	1,042	1,000	1,000

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution. Specifications in columns 3 and 4 include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1988, factory sector wages in 1988, the ratio of fixed capital to workers in 1988, percent of 1988 employment in food-based sectors and in the agrarian sector, percent of 1988 employment that was landless, the ratio of capital to output in 1988, and cumulative years (in 1988) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock.

farm wages, and household expenditures do not differ systematically across proworker and proemployer states. Such differentials would suggest that our key estimates reflect some spatial variation in the economic shocks induced by rainfall that just happens to be correlated with differences in the labor law regime. Columns 3 to 6 argue against this concern: we cannot reject the hypothesis that the response of agricultural outcomes and per capita expenditures to rainfall shocks is the same across proworker and proemployer states.

B. Effects of Rainfall Shocks on Employment by Labor Regulation Strictness

As described in Section IV, we use both district-level and the state-industry-level panels to test the theoretical prediction that the employment response to shocks should be larger the lower the firing costs. Tables 6 and 7 report our main results on the differential response of industrial employment to shocks across proworker, neutral, and proemployer states.

Table 6 reports results from the district-level panel. First, columns 1 and 2 report the average impact of rainfall shocks on two measures of employment: workers and man-days. The estimated impacts are large (in relation to the district and state-industry means) and negative: for example, moving from the 80th to the 20th percentile of the historical rainfall distribution generates a decrease of 500,000 man-days (column 2).

We then interact the rainfall shock variable with dummies for proworker and proemployer states; the results are reported in columns 3 and 4. In addition to district and year fixed effects, these regressions control for interactions of rainfall shock with a set of baseline characteristics. These include the total cultivated area and its percentage devoted to rice and to wheat in the state in 1980, percentage of 1988 employment in food-based sectors and in the agrarian sector, percentage of 1988 employment that was landless workers, and the ratio of capital to output in factories in 1988.

For both employment outcomes, we can reject the hypothesis that the response to shock is equal across proemployer

TABLE 7.—STATE-INDUSTRY PANEL RESULTS: EFFECT OF RAINFALL SHOCK ON INDUSTRIAL EMPLOYMENT BY LABOR REGULATION STRICTNESS

	State-industry panel					
Dependent Variables	Number of Workers (1)	Number of Employees (2)	Number of Workers (3)	Number of Employees (4)	Number of Workers (5)	Number Employees (6)
Rainfall shock	−151.3	−186.1	−170.925	−218.959	−780.93	−874.456
	(48.92)***	(56.81)***	(141.542)	(170.965)	(464.247)*	(533.400)
Rainfall shock ×						
(β) Proworker state			−33.709	−61.501	−41.188	−77.518
			(108.452)	(127.510)	(110.772)	(130.446)
(δ) Proemployer state			−261.125	−300.622	−271.326	−319.141
			(106.779)**	(125.449)**	(110.563)**	(129.501)**
Fixed effects	State × 3-digit industry code + Year		State × 3-digit industry code + Year		Rainfall shock × 3-digit industry code + State × 3-digit industry code	
Ho: δ − β = 0			**−227.4**	**−239.1**	**−230.1**	**−241.6**
			(116.7)*	**(132.3)***	**(111.3)****	**(126.6)***
Number of observations	24,374	24,374	24,374	24,374	24,374	24,374

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within state × 3-digit industry code. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. Rainfall shock = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution; specifications in columns 3–6 include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1980, factory sector wages in 1980, and cumulative years (in 1980) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock.

and proworker states. Further, the point estimate on the difference between the two interaction coefficients shows that the employment response is larger in proemployer states. In terms of magnitudes, proemployer districts shed about 2,300 more workers than proworker districts do, which translates into a 9 percentage point difference in response.

Columns 1 to 4 in Table 7 report the analogous results using the state-industry panel, the outcomes being the number of employees and workers.[13] The regressions include the interactions of rainfall shock with a set of state and industry baseline characteristics, including the total cultivated area and its percentage devoted to rice and to wheat in the state in 1980 and a dummy for whether an industry was delicensed by 1980. In addition, they include state-by-industry fixed effects. The results are fully consistent with those from the district panel: proemployer state-industry groups shed about 230 more workers than their proworker counterparts, a 7% point difference in response. Together, these results constitute our main test of the theoretical predictions from the canonical labor demand model laid out in the appendix.

A potential concern is that firms' location decisions across states are nonrandom and may be correlated with labor regulation regimes as well as the way in which these firms adjust to shocks. For example, if the least flexible firms—those that require the most labor adjustment in times of shock—locate where there are weak worker lobbies (which often generate proemployer amendments), the differential response across proworker and proemployer states would tend to overstate the effect of labor regulations on the average firm.

Our conjecture is that in part, such inherent differences in flexibility would be determined by the industry to which a factory belongs. Therefore, controlling for differential effects of rainfall shocks by industry should control for the potential selection of flexible firms into proworker states, to the extent that this flexibility is encapsulated by NIC codes for industry

[13] The former includes supervisory and managerial employees.

type. The results are presented in columns 5 and 6 of table 7. Compared to columns 3 and 4, there is virtually no difference in the estimates of the differential impact of rainfall shocks across proemployer and proworker states. This gives us some confidence that our findings are not driven by firm selection into labor regimes.

Finally, in table A1, we report the main employment results using the three alternative measures of labor regulation described in section IIIA. We find that that the estimated effects are qualitatively as well as quantitatively comparable to the results obtained in tables 6 and 7, although the results using Bhattacharjea's coding and the dispute settlement coding are more precise than the results using the employment protection coding.

C. Effect of Shocks on Employment by Factory Size

In this section, we exploit the particulars of the regulation set forth in the IDA related to the extent of firing costs for large factories. As described earlier, the IDA regulation stipulates that larger factories will face higher firing costs. In particular, factories with employment lower than 50 face no firing costs, factories with employment between 50 and 100 must compensate workers who are retrenched, and factories with employment greater than 100 workers must file each layoff with the government, who then has the power to deny the factory the ability to retrench. Accordingly, we partition our data by these size cutoffs before aggregating to the district level, thus creating a panel data set disaggregated by both district and factory size category. This data set has three observations per district-year, corresponding to small (fewer than 50 workers), medium (50–100 workers), and large (more than 100 workers) factories.

In table 8, we use this data set to test if the differential employment response across proworker and proemployer states is largest for large factories, which is to be expected if

TABLE 8.—TRIPLE INTERACTION TESTS OF EFFECT OF RAINFALL SHOCK ON EMPLOYMENT BY FACTORY SIZE

	District Panel	
	Number of Workers	Man-Days (thousands)
Dependent Variables:	(1)	(2)
Rainfall shock ×		
(ζ_1) Proworker state × large factory	887.530	386.890
	(1,036.535)	(392.724)
(ζ_2) Proemployer state × large factory	−1,062.329	−248.158
	(904.782)	(277.437)
(ζ_3) Proworker state × medium factory	786.504	324.94
	(394.378)**	(160.249)**
(ζ_4) Proemployer state × medium factory	205.405	112.081
	(302.347)	(108.586)
(β_1) Proworker state	−538.913	−150.358
	(606.160)	(229.872)
(β_2) Proemployer state	−354.361	−122.865
	(566.411)	(216.420)
Fixed effects	District × size + Year	
Response of small firms across labor regimes [$\beta_2 - \beta_1$]	184.522	27.49
	(494.09)	(195.69)
Response of medium firms across labor regimes [$(\zeta_4 + \beta_2) - (\zeta_3 + \beta_1)$]	−396.55	−185.37
	(343.39)	(122.15)
Response of large firms across labor regimes [$(\zeta_2 + \beta_2) - (\zeta_1 + \beta_1)$]	−1,765.31	−607.56
	(919.59)*	(323.00)*
Diff-in-diff for large firms relative to small [$\zeta_2 - \zeta_1$]	**−1,950**	**−635.0**
	(1163)*	**(412.6)**
Diff-in-diff for medium firms relative to small [$\zeta_4 - \zeta_3$]	**−581.1**	**−212.9**
	(446.0)	**(176.7)**
Number of observations	3,000	3,000

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution. "Large" indicates factories with > 100 workers, "medium" indicates factories with > 50 and < 100 workers, and "small" indicates factories with < 50 workers. The excluded category is "small". Specifications include rainfall shock × size and labor regulation × size interactions. Specifications also include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1988, factory sector wages in 1988, the ratio of fixed capital to workers in 1988, percent of 1988 employment in food-based sectors and in the agrarian sector, percent of 1988 employment that was landless, the ratio of capital to output in 1988, and cumulative years (in 1988) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock. Bold text emphasizes results corresponding to specific hypothesis test of interest.

the estimated differential captures the impact of labor regulations. To do this test, we estimate a model with triple interactions (rainfall shock by labor regulation by size), controlling for district by size rainfall shock by size, and year fixed effects (s indicates size index and r indicates rainfall shock index):

$$x_{jqt} = \zeta_1(Rainshock_{jt} \times Proworker_{jt} \times Large_{jqt})$$
$$+ \zeta_2(Rainshock_{jt} \times Proemployer_{jt} \times Large_{jqt})$$
$$+ \zeta_3(Rainshock_{jt} \times Proworker_{jt} \times Medium_{jqt})$$
$$+ \zeta_4(Rainshock_{jt} \times Proemployer_{jt} \times Medium_{jqt})$$
$$+ \beta_1(Rainshock_{jt} \times Proworker_{jt})$$
$$+ \beta_2(Rainshock_{jt} \times Proemployer_{jt})$$
$$+ \rho_{jq} + \rho_{rq} + \rho_t + \epsilon_{jt},$$

where q indexes the sector (small, medium, or large), ρ_{jq} is a district-sector specific fixed effect, and ρ_{rq} is a rain-shock-sector specific effect. As before, the omitted categories are small factories and neutral states.

The regressions indicate that the employment response to rainfall shocks is larger in proemployer (as compared to proworker) states among medium and large factories but not small factories. This is consistent with the absence of mandated firing costs for small factories. Further, the differential

effect is statistically significant only in large firms. Specifically, the difference in the response of large firms across proemployer and proworker states [$(\zeta_2 + \beta_2) - (\zeta_1 + \beta_1)$] is negative and significant at the 10% level.

Next, we test the null hypotheses that the differences-in-differences relative to the small factories are zero: $\zeta_2 - \zeta_1 = 0$ and $\zeta_4 - \zeta_3 = 0$. In words, the null $\zeta_2 - \zeta_1 = 0$ states that labor regulations moderate the employment response to rainfall shocks in the same way for both large and small factories. We can statistically reject the null for employment in the large versus small comparison (column 1). This difference-in-differences estimate for large versus small is negative, as expected. The same holds for the medium versus small comparison, though the difference-in-differences is not statistically significant.

D. Effect of Shocks on Employment by Industry Type

Next, we use the state-industry panel introduced earlier to test whether the differential employment response to rainfall shocks varies by industry type. In particular, we group industries based on their susceptibility to local demand shocks. If, as the results from table 5 suggest, rainfall shocks affect the factor sector predominantly through their effects on local demand for the goods that factories produce, then industries that are more dependent on local demand should

TABLE 9.—TRIPLE INTERACTION TESTS OF EFFECT OF RAINFALL SHOCK ON EMPLOYMENT BY INDUSTRY TYPE

	State-Industry Panel			
Dependent Variables:	Number of Workers (1)	Number of Employees (2)	Number of Workers (3)	Number of Employees (4)
Rainfall shock	−285.1 (114.7)**	−387.5 (138.5)***	−189.976 (137.696)	−245.168 (167.316)
Rainfall shock ×				
(ζ_1) Proworker state × Dummy for NIC code in 200–299			18.757 (137.216)	67.147 (168.642)
(ζ_2) Proemployer state x Dummy for NIC code in 200–299			−390.469 (264.746)	−387.504 (294.352)
(ζ_3) Proworker state			−45.336 (106.501)	−96.510 (136.068)
(ζ_4) Proemployer state			−79.633 (120.268)	−120.829 (141.052)
Dummy for NIC code in 200–299	−51.02 (87.60)	−10.40 (99.96)	51.502 (59.395)	67.701 (75.405)
Fixed effects	State × 3-digit industry code + Year			
Response of 300–400 NIC industries across labor regimes **[$\zeta_4 - \zeta_3$]**			**−34.397** **(104.683)**	**−24.319** **(136.150)**
Response of 200–300 NIC industries across labor regimes **[$(\zeta_2 + \zeta_4) - (\zeta_1 + \zeta_3)$]**			**−443.523** **(244.668)***	**−478.970** **(266.424)***
Diff-in-diff for 200–300 NIC industries relative to 300-400 NIC **[$\zeta_2 - \zeta_1$]**			**−409.2** **(286.6)**	**−454.7** **(322.0)**
Number of observations	24,374	24,374	24,374	24,374

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within state × 3-digit industry code. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution. Specifications in columns 3–6 include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1980, factory-sector wages in 1980, and cumulative years (in 1980) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock. Bold text emphasizes results corresponding to specific hypothesis tests of interest.

exhibit a larger differential response across proworker and proemployer states.

Based on the NIC code, we split industries into two groups: between NIC codes 200 and 299, inclusive, and between NIC codes 300 and 399, inclusive. NIC codes 200 to 299 correspond to industries whose focus is agricultural and natural industrial products, such as food products and beverages, textiles, paper, wood, and leather products. NIC codes 300 to 399 describe the more technological and heavy industries like chemicals and pharmaceutical, metal products, machinery, and electronics. This is an admittedly rough categorization. Although we expect the first group to be more dependent on local demand, both industry groups are likely to contain traded and nontraded goods industries. Another caveat is that rainfall shocks might also affect factories through higher prices of locally produced raw materials. Since NIC groups 200 to 299 are more likely to be tied to the local supply of agricultural raw materials, both the demand and raw material shocks are expected to be stronger for this group.

The results are reported in table 9. The differential response across proworker and proemployer states is more sizable for industries tied to local demand (NIC 200 to 299). Specifically, the difference between proemployer and proworker responses is estimated to be −443.5 for NIC groups 200 to 299 and only −34.9 for NIC groups 300 to 399, and only the former is statistically significant. These results are consistent with the demand or input channel interpretations of the effects of rainfall shocks on the factory sector, as well as with our basic premise that shocks induce differential responses across

labor regulation regimes. However, a triple interactions test (rainfall shock by labor regulation by NIC code grouping) cannot reject the null that this difference across the two industry groups is zero.

E. Effects of Shocks on Output and Profits

Finally, we examine whether firms in proworker states were able to adjust their output to the same extent as those in proemployer states in response to shock and whether the constraints imposed by firing costs have on impact on proworker firms' profits more than proemployer firms. To test these hypotheses, we again employ the district-level panel and run regressions of the form described in equation (2), which include district and year fixed effects.

The results are reported in table 10, with the dependent variables being the value of total output, value added, and profits. They suggest that there is no differential change across proworker and proemployer states in these outcomes. We should note, however, that the coefficients on the interaction terms in table 10 are large relative to the mean values of these variables in table 4. Hence, the evidence for no differential change is weak; that is, while we cannot reject that the differentials between proworker and proemployer are 0, we also cannot reject that they are very large.

Taken at face value, the results on total value added and output are consistent with our evidence on the equal effects of rainfall shocks across proworker and proemployer states on agricultural production and household expenditures: we find large declines in these (consistent with rainfall shocks'

Table 10.—Effect of Rainfall Shock on Output and Profits by Labor Regulation Strictness

	District-Level Panel					
Dependent Variables:	Total Output (1)	Value Added (2)	Profits (3)	Total Output (4)	Value Added (5)	Profits (6)
Rainfall shock	−38.86 (36.02)	−14.23 (7.958)*	−8.495 (5.895)	−597.533 (1,080.098)	−238.728 (232.376)	−137.519 (145.620)
Rainfall shock ×						
(β) Proworker state				−34.791 (125.066)	10.651 (27.377)	9.540 (21.007)
(δ) Proemployer state				13.173 (100.574)	8.072 (23.064)	11.046 (18.599)
Fixed effects		District + Year			District + Year	
Ho: δ − β = 0				**47.96** (92.73)	**−2.579** (22.24)	**1.506** (14.95)
Number of observations	1,042	1,042	1,042	1,000	1,000	1,000

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of proemployer amendments passed after Indian independence in 1947. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution. Specifications in columns 4–6 include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1988, factory-sector wages in 1988, the ratio of fixed capital to workers in 1988, percent of 1988 employment in food-based sectors and in the agrarian sector, percent of 1988 employment that was landless, the ratio of capital to output in 1988, and cumulative years (in 1988) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock. "Value added" is the pecuniary value of total output minus intermediate inputs; capital stock, materials, fuel, total output, value added and profits have been converted to thousands of 2009 U.S. dollars. Bold text emphasizes results corresponding to specific hypothesis tests of interest.

Table 11.—Effect of Rainfall Shock on Nonlabor Inputs, Wages and Labor Intensity by Labor Regulation Strictness

	District-Level Panel							
Dependent variables:	Capital (1)	Materials (2)	Fuel (3)	Electricity (4)	Number of Factories (5)	Wage per worker (6)	Man-Days per worker (thousands) (7)	Value of Output per Worker (8)
Rainfall shock	−542.315 (697.882)	−176.485 (749.014)	33.557 (86.060)	60.413 (41.867)	−1,378.69 (518.152)***	0.004 (0.002)*	0.170 (0.122)	0.089 (0.053)*
Rainfall shock ×								
(β) Proworker state	−16.060 (93.969)	−63.414 (84.223)	−7.668 (8.898)	−4.917 (5.748)	−9.394 (53.123)	−0.000 (0.000)	0.003 (0.017)	−0.008 (0.007)
(δ) Proemployer state	−28.481 (67.904)	−19.850 (65.353)	3.376 (7.843)	−1.528 (4.222)	−67.577 (43.873)	0.000 (0.000)	−0.003 (0.017)	−0.004 (0.006)
Fixed effects				District + Year				
Ho: δ − β = 0	**−12.42** (61.31)	**43.56** (61.01)	**11.04** (9.840)	**3.388** (5.641)	**−58.18** (39.06)	**0.000227** (0.000152)	**−0.00614** (0.00953)	**0.00370** (0.00382)
Number of observations	1,000	1,000	1,000	1,000	1,000	1,000	1,000	1,000

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts. States are classified as proworker, proemployer, or neutral based on adding the number of proworker amendments and subtracting the number of pro-employer amendments passed after Indian independence in 1947. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution; specifications in columns 4–6 include rice and wheat area per acre and total cultivated area in the state in 1980, a dummy for whether the three-digit industry was delicensed by 1988, factory-sector wages in 1988, the ratio of fixed capital to workers in 1988, percent of 1988 employment in food-based sectors and in the agrarian sector, percent of 1988 employment that was landless, the ratio of capital to output in 1988, and cumulative years (in 1988) since 1957 hard-left parties majority state legislature, all interacted with rainfall shock. "Capital" is the value of fixed capital stock at close of business year. "Materials," "Fuel", and "Electricity" are annual values of intermediate inputs used. "Wage per worker" is the wage bill (total amount paid to workers in 2009 USD) divided by number of workers. "Man-days per worker" is the number of man-days divided by the number of workers; "Value of output per worker" is value of total output in thousands of 2009 USD divided by number of workers. Bold text emphasizes results corresponding to specific hypothesis tests of interest.

effects through a demand channel) but no differential decline across labor regulation regimes.[14]

The finding of no differential change in profits is surprising in the sense that we might expect there to be a larger profit decline in proworker states, which were constrained by firing costs from adjusting the level of employment optimally. Several potential arguments could explain the results on profits.

We might expect that the constraints imposed on firing costs by labor regulation could generate adjustment along other margins of inputs. If this were the case, we might see that, for example, capital or intermediate inputs would adjust more intensively in proworker states than proemployer states.

If firms were able to adjust along these margins well enough, we might not measure an effect on profits.

To test this hypothesis, we use as dependent variables the value of capital and intermediate inputs—in particular, materials, fuel, and electricity. The results are reported in columns 1 to 4 of table 11. For all four outcomes, we find no differential adjustment across proworker and proemployer states in response to shock. These results indicate that nonlabor inputs are not declining more intensively in proworker states to mitigate the impact of employment adjustment constraints.[15]

The second hypothesis is related to differential attrition of factories across labor regulation regimes. We might find no effects on profits if the firms with the largest negative profits

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[14] We also verify that the results presented in table 10 are robust to the using the alternative measures of labor regulation. These results are available on request.

[15] We note that as is the case with output and profits, the evidence for no differential change in intermediate inputs is weak: the coefficients on intermediate inputs in table 11 are large compared to the mean values of these variable in table 4.

are going out of business (dropping out of the sample) more intensively in proworker states in response to the shock. To test this hypothesis, we look directly at responses in the number of factories in the district to rainfall shock. Column 5 of table 11 reports the results. We find no evidence of differential declines in the number of factories across proemployer and proworker states in response to shock.

The third hypothesis we examine for the lack of differential declines in profits is that the industrial wage declined more intensively in proworker states than in proemployer states.[16] If this were the case, we would expect that in proworker states, firms would see a greater reduction in the wages per worker than that seen by firms in proemployer states. To test this hypothesis, we examine wages per worker. The results are reported in column 6 of table 11. The results show that wages per worker do not decline differentially across proworker and proemployer states.

The fourth hypothesis related to the nonresult on profits is that since firms in proworker states are constrained from adjusting the number of workers to the optimal extent, they may choose instead to adjust the labor intensity of their current workforce, for example, the number of hours per day or the number of days per month each worker puts forth in labor. If this were the case, we should observe that proworker states use workers differentially more intensively in times of shock. We use two outcomes to test this hypothesis: man-days per worker and value of total output per worker (the second of which is used under the assumption that a worker more intensively utilized will produce more output). The results of these regressions are reported in columns 7 and 8 of table 11. Again, we find no evidence of differential adjustment across proworker and proemployer states.

Finally, there are two hypotheses for which, due to data constraints, we have no test, which might explain the results on profits. First, the prices of nonlabor inputs might be adjusting differentially across proworker and proemployer states. If we saw a larger reduction in the price of nonlabor inputs in proworker states, profits fluctuations due to shocks may equalize across labor regulation regimes. We believe this explanation is unlikely to be the only one, given that if the prices of nonlabor inputs changed differentially across proworker and proemployer states, we would likely see a differential change in the use of those inputs as well. The results from columns 1 to 4 of table 11 seem to refute this claim.

Second, if proworker firms were more intensively laying off casual laborers (part-time laborers who are not accounted for in the data) during periods of shocks, these firms might be able to achieve a commensurate reduction in "effective" employment as the reduction seen for firms in proemployer states. This explanation would also be consistent with the fact

(from column 6 of table 11) that reductions in the wage bill are equal across proemployer and proworker states.

VI. Conclusion

Job security provisions, although politically popular, have been the focus of intense academic debate. The job security they confer needs to be weighed against reduced flexibility in hiring and firing, which has been found to have negative impacts on aggregate outcomes. In this paper, we have devised a novel test of the fundamental hypothesis that employment protection laws attenuate the employment responses of firms to external shocks. We exploit a setting that exhibits variation in labor regulation as well as a measurable source of unpredictable shocks. Our setting is rural India, where rainfall fluctuations create demand and wage shocks for local industries and labor regulation varies temporally as well as spatially. Our results provide a striking confirmation of the theory: rainfall shocks change industrial employment by shifting the demand for industrial products, and the employment adjustment is more pronounced in regions where labor regulations are less restrictive. We also examine the responses of factories that were exempt from the regulation and find that there is no differential adjustment across labor regimes, consistent with our interpretation that the differential responses for nonexempt factories are indeed attributable to labor regulation.

Because we are looking at employment adjustment in the formal manufacturing sector (the only part of the economy subject to the labor laws in question), this is only one part of the full picture. To understand the overall effects of labor laws on employment and job security requires more comprehensive data. Reduced job creation and destruction rates may seem to imply longer unemployment spells, and possibly disproportionately so for certain segments of the labor force. We believe this is a promising line of inquiry for future research. In the Chilean context, Montenegro and Pages (2004) use household survey data and find that job security provisions and minimum wage requirements confer positive benefits on older and skilled workers, as well as male workers, but that these benefits are achieved at the expense of young, unskilled, or female workers. These costs of labor regulation are likely to be magnified when labor is not very mobile. Jayachandran (2006) shows that agricultural productivity shocks in rural India create large changes in the wage when labor is immobile and incomes are near subsistence level, a finding that may be related to the inability of the manufacturing sector to absorb workers.

Although we have not touched on the issue in this paper, one may wonder how employment adjustment plays out in an economy in which there is a large, unregulated informal sector coexisting with a smaller, regulated (but more productive) formal sector. In fact it has been conjectured that labor regulations, in as much as they apply only to the formal sector, tend to encourage informality. In the Indian context, this could account for the preponderance of small firms. In fact, the vast

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[16] In fact, we might suspect the opposite if we believe that worker lobbies in proworker states are better able to bargain for wage stability. In this case, we would expect the wage to fall more intensively in response to shock in proemployer states.

majority of nonagricultural workers in India are employed in the informal sector. An interesting hypothesis, and one that we propose to test in the future, is that the informal sector serves as a buffer for the formal sector: when the latter sheds workers, the informal sector soaks up the extra workers. In this case, the employment adjustment in the informal sector would be the mirror image of employment adjustment in the formal sector.

There is a sizable literature on another aspect of job security provisions, namely their effect on aggregate employment and output. Fallon and Lucas (1993) estimated labor demand to show that the increased stringency of job security provisions in India after 1982 resulted in a large reduction in employment. Similar findings are reported in Besley and Burgess (2004), based on comparing employment and output across labor regimes in India. Aghion, Burgess, Redding, and Zilibotti (2008) have extended the analysis to show that the effect of labor regulations on aggregate employment and output has been greater in more regulated product markets. Overall, the negative effects of firing restrictions are many, and need to be weighed against the employment stability that they confer.

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APPENDIX

Model

We outline a partial-equilibrium model based on Bertola (1990) that formalizes the key intuition of the paper. To keep the model simple, we do not directly introduce an agricultural sector or specify a labor supply equation. Instead, we consider the labor demand of a price-taking firm that is subject to exogenous shocks to the wage and output price, the shocks being assumed to flow from productivity shocks to agriculture.

The model is set in continuous time. Consider an infinitely lived price-taking firm that uses only labor to produce its output according to an increasing, concave production function $f(L)$. The firm discounts future profits at the constant rate r. There are two possible states of the world, denoted by G (good, or high, rainfall) and B (bad, or low, rainfall). The associated prices and wages in these states are given by p_G, w_G, p_B, and w_B respectively.

Suppose that the state is currently B at time t. The transition to the G state follows a Poisson process with constant rate of arrival θ_B. Similarly the transition from state G to state B is a Poisson process with constant arrival rate θ_G. We model employment protection in terms of a simple firing cost: hiring workers is frictionless, but firing workers is assumed to entail a cost of c per worker. This linear specification of adjustment costs is convenient for our purposes but not strictly necessary. However, because our data cannot be used to distinguish between different adjustment cost specifications, we stay with the linear specification here while remaining agnostic about the exact form.

In what follows, we will consider a stationary policy for the firm such that the firm employs L_G workers whenever the state is G and L_B workers whenever the state is B.[17] We will assume that $p_G > p_B$, corresponding to the assumption that high rainfall tends to raise demand for the industrial good. The wage rates in the two states are unrestricted, although we may assume without loss of generality that $w_B < w_G$, reflecting the possibility that poor rainfall reduces the labor demand in agriculture and thereby increases the labor supply to industry. For concreteness, we will assume that the price of output and the wage in the different states are such that $L_G > L_B$ (i.e., the demand effect outweighs the wage effect, as will turn out to be true in the data).

The choice of L is analogous to investment in an asset whose return is stochastic. Since the policy is stationary, we need only define the value of the asset in the two states of the world, G and B. Let V_G and V_B denote these two values. Given the assumptions on the transition probabilities, we can use the standard asset equation to write:

$$rV_G = p_G f(L_G) - w_G L_G + \theta_G[V_B - V_G - c(L_G - L_B)], \qquad (A1)$$

$$rV_B = p_B f(L_B) - w_B L_B + \theta_B[V_G - V_B]. \qquad (A2)$$

Upon transitioning to state B from state G, the firm chooses L_B to solve

$$\max V_B - c(L_G - L_B). \qquad (A3)$$

The first-order condition is simply $\frac{\partial V_B}{\partial L_B} = -c$.

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[17] The profit function of the firm is bounded, continuous, and concave. The stationarity of the shocks and the presence of discounting can then be used to establish the optimality of a stationary policy. For a simple proof in the discrete-time case, see Adda and Cooper (2003).

TABLE A1.—MAIN RESULTS USING ALTERNATIVE LABOR REGULATION MEASURES

	District Panel			State-Industry Panel		
Labor regulation measure used:	Bhattacharea's Recoding of Besley & Burgess (2004) (1)	Employment Protection Legislation (Ahsan & Pages, 2008) (2)	Dispute settlement (Ahsan & Pages, 2008) (3)	Bhattacharjea's Recoding of Besley & Burgess (2004) (4)	Employment Protection Legislation (Ahsan & Pages, 2008) (5)	Dispute Settlement (Ahsan & Pages, 2008) (6)
Rainfall shock	$-14,602.59$ (14,976.48)	$-9,110$ (11,840)	$-20,034.91$ (18,954.27)	-216.55 (111.88)	-304.32 (132.41)**	-169.16 (101.18)*
Rainfall shock ×						
(β) Proworker state	-667.45 (1,442.68)	400.3 (834.3)	1532.05 (2,198.52)	-0.001 (0.004)	113.58 (95.23)	-93.15 (94.27)
(δ) Proemployer state	$-2,662.96$ (1,054.48)***		$-1,538.87$ (638.71)**	-215.46 (106.76)***		-190.41 (95.54)**
Fixed effects	District + Year			State × 3-digit industry code + Year		
Ho: δ − β = 0	**−1,995.51** **(1,588.09)**	**−400.3** **(834.3)**	**−3,070.91** **(2335.90)**	**−215.46** **(106.76)** **	**−113.58** **(95.23)**	**−97.26** **(113.58)**
Number of observations	1,000	1,000	1,000	24,374	24,374	24,374

The dependent variable in all regressions is the number of workers. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Robust standard errors are reported in parentheses below the coefficient estimates and allow for correlation in the error term within districts for specifications in columns 1–3, and within state × 3-digit industry code for specifications in columns 4–6. "Rainfall shock" = 1 if annual rainfall < 20th percentile of historical distribution, = 0 if > 20th and < 80th percentile of historical distribution, and = −1 if > 80th percentile of historical distribution; please refer to the Appendix for definitions of the alternate labor regulation definitions used; for controls used in specifications reported in columns 1–3, please refer to footnotes in table 4, for controls used in specifications reported in columns 4–6, refer to footnotes in table 7. Bold text emphasizes results corresponding to specific hypothesis tests of interest.

On transitioning to state G from state B the firm chooses L_G to solve

$$\max \quad V_G. \tag{A4}$$

The first-order condition is $\frac{\partial V_G}{\partial L_G} = 0$. These first-order conditions, along with equations (A1) and (A2), imply that $\frac{\partial V_B}{\partial L_G} = \frac{\partial V_G}{\partial L_B} = 0$.

Using the asset-pricing equations, we also have

$$\frac{\partial V_B}{\partial L_B} = \frac{1}{r + \theta_B} \left[p_B f'(L_B) - w_B + \theta_B \frac{\partial V_G}{\partial L_B} \right],$$

$$\frac{\partial V_G}{\partial L_G} = \frac{1}{r + \theta_G} \left[p_G f'(L_G) - w_G - c\theta_G + \theta_G \frac{\partial V_B}{\partial L_G} \right].$$

The first-order conditions, together with the fact that $\frac{\partial V_B}{\partial L_G} = \frac{\partial V_G}{\partial L_B} = 0$, then imply

$$p_B f'(L_B) = w_B - (r + \theta_B)c,$$
$$p_G f'(L_G) = w_G + c\theta_G.$$

These equations capture the intuition that adjustment costs create a wedge between the firm's marginal revenue product and the wage. The effective wage is therefore higher than the actual wage during good times and lower during bad times. It is easy to see that an increase in the firing cost c reduces employment in the high-rainfall state G and increases employment in the low-rainfall state B. Put differently, fluctuations represented by rainfall shocks will induce smaller employment adjustments in more regulated environments. This is the hypothesis we will proceed to test.

As we noted in the text, shocks represented by rainfall fluctuations plausibly create opposing effects on industrial labor demand, through the demand and labor supply channels. The model outlined here clarifies that it is the net effect on labor demand of these wage and price shocks that is magnified in lower firing cost (i.e., more flexible) regimes. That is, if the net effect of good rainfall is to increase (decrease) industrial employment, then we should expect to observe a greater increase (decrease) in employment in regions where labor regulations are less stringent. This conclusion is valid as long as rainfall shocks represent identical demand and labor supply fluctuations across different labor regimes.