Public Works Program, Labor Supply, and Monopsony*

Tsenguunjav Byambasuren, Nancy H. Chau, and
Vidhya Soundararajan†

May 20, 2025

Abstract

We develop a monopsony model where a firm's wage markdown, a measure of monopsony power based on the gap between wage and MRPL, is a weighted average of markdowns over different workers to study the impact of changes in the workforce composition on the average markdowns. We empirically illustrate the theory using India's National Rural Employment Guarantee Act (NREGA), which generates non-manufacturing jobs in rural villages. Consistent with the model predictions, at manufacturing firms, we find that the program crowds out mobile workers, effectively increases the share of immobile workers with low labor supply elasticity, and increases average markdowns.

Keywords: Workfare programs, NREGA, Indirect effect, Labor market power, Markdown, India

JEL Codes: I38, J42, H53, J38, J68

*We are grateful to Emily Beam for insightful discussion. We also thank Joseph Kaboski and participants at the North East Universities Development Consortium (NEUDC) at Northeastern University for useful comments and helpful conversations. The usual disclaimer applies.

†Byambasuren: Dyson School of Applied Economics and Management, Cornell University (email: tb497@cornell.edu); Chau: Dyson School of Applied Economics and Management, Cornell University and Institute for the Study of Labor (IZA) (email: hyc3@cornell.edu); Soundararajan: IIT Bombay, Centre for Advanced Financial Research and Learning (CAFRAL), and Institute for the Study of Labor (IZA) (email: vidhya.srajan@cafral.org.in).

1 Introduction

Policy interventions in developing countries significantly affect groups ineligible for treatment through several channels. Cash and in-kind transfers indirectly affect nonparticipants' consumption and welfare by changing the price and income gains of suppliers and workers (Coate et al., 1994; Cunha et al., 2019; Egger et al., 2022) and through risk-sharing (Townsend, 1994; Angelucci and De Giorgi, 2009).[1] Such non-labor market policy interventions also impact non-targeted groups' labor market outcomes by changing the labor supply and wages of participants who work for non-participants (Bandiera et al., 2017) and via enabling ineligible individuals related to the eligibles to search for better jobs (Ardington et al., 2009).

The spillover effects of labor market policies are relatively understudied; however, some studies investigate the impact of the world's largest public workfare and India's flagship antipoverty program, the National Rural Employment Guarantee Act (NREGA) on non-participants' labor market outcomes via general equilibrium effects (Muralidharan et al., 2023) and crowding out effects (Imbert and Papp, 2015; Zimmermann, 2024). These few studies, estimating the spillover effects of the NREGA program that generates public work focus on private-sector labor markets. Additionally, Franklin et al. (2024) show that the urban public works program in Addis Ababa, Ethiopia crowds out private employment and increases private wages. However, another potentially important but largely overlooked aspect of this policy, which guarantees employment in projects mainly in agriculture, is the indirect or crowding-out effect on labor markets in manufacturing, especially monopsony power. In this paper, we thus fill this gap in the literature by providing the first evidence on the effect of the NREGA on manufacturing firms' labor market power and other labor market outcomes, such as wage and employment.

We first develop a model of imperfect competition in the labor market that features heterogeneous workers and NREGA to provide a systematic framework for studying the effect of the program on monopsony power and guide our empirical analysis. For our empirical analysis, we first quantify establishment-level[2] wage markdown—the wedge between the marginal revenue product of labor (MRPL) and the wage—as a measure of monopsony power in India's manufacturing industry based on the production function approach. We also investigate how the aggregate markdowns evolve in the country and examine to what extent various types of heterogeneous workers are exposed to different degrees of monopsony power. Then, we estimate the indirect impacts of the NREGA program on manufacturing firms' labor market outcomes, emphasizing the markdown effects.

The production approach to estimate the monopsony power requires firm-level production data, and we intend to examine the indirect effects of the NREGA on manufacturing firms. We thus use

[1] There is also evidence against the risk-sharing effect of cash transfer policies via network mechanisms, such as Haushofer and Shapiro (2016) who show that unconditional cash transfers in Kenya had a negative spillover effect on ineligible households within the community.

[2] We interchangeably use the terms firm, plant, establishment, and factory throughout the paper.

the nationally representative firm-level panel data of manufacturing establishments from the Annual Survey of Industries (ASI) between 1998 and 2008. The longitudinal version of the ASI data allows us to use the semi-structural control function method to estimate the production function, which relies on lagged information for identification and control for fixed effects at the granular level, e.g., firm fixed effects for quantifying the causal impacts of the NREGA using reduced-form regressions. Several other unique features of the ASI data make the survey ideal for our analysis. For instance, the firm-level data provides detailed employment information, e.g., workdays and headcounts, and wage bills for heterogeneous workers, such as production and non-production workers. This feature is critical for this study because the policy intervention we examine intends to generate temporary jobs over 100 days in a given financial year for low-skilled manual workers. Thus, the intervention likely has highly heterogeneous impacts on production and non-production workers. Additionally, the data reports information for contract workers, enabling us to provide the first estimates on markdowns for workers who differ by employment contracts and analyze the NREGA's impacts on labor market conditions for regular and contract workers.

The key identification challenge for estimating the causal impact of the NREGA is endogeneity due to a selection bias as the program rolled out in three phases, starting with the poorer districts. To account for this policy endogeneity, we use a difference-in-differences (DID) design exploiting the staggered rollout of the program. Recent advances in DID literature point out estimating heterogeneous treatment effects of staggered policy interventions using DID design and two-way fixed effects (TWFE) model yield biased estimates (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Fortunately, some studies proposed several potential solutions to the problems of staggered DID design and distinguish the settings that introduce bias from those that provide credible estimates (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).[3] The identification problems and the suggested methods under staggered DID design are especially relevant when there is no never-treated control group. Although the NREGA is a nationwide intervention, the program rollouts in the initial years investigated in this paper allow us to have never-treated districts, i.e., phase-3 districts, that received the treatment after our study period. So, our estimates are not subject to the biases described in the recent DID literature. Three main identification assumptions must be satisfied in our setting to interpret our estimates as causal, including the parallel trends, no anticipation effects, and stable assignment assumption. We show that these assumptions are plausible in our context using various approaches, such as event-study analysis.

In this paper, we find that manufacturing firms in India operate in a monopsonistic environment, and workers in an average manufacturer earn 0.72 rupees for each rupee generated. The aggregate markdown has been growing during our study period between 1999 and 2008.[4] The

[3]One can review Baker et al. (2022) who summarize the potential issues with the staggered DID design and alternative solutions proposed in the current literature and Freedman et al. (2023) who provide some practical guidance for applying the newly proposed methods.

[4]We lose the first year in our sample, 1998, as the production function uses the lagged information as an instrument, and production parameters and markdowns are not estimated for the first period.

monopsony power over contract and non-production workers (managers or high-skilled workers) that are not protected by India's employment protection law is higher than regular and production workers (non-managers or low-skilled workers) in Indian manufacturing, respectively. In our regression analysis, we fail to find significant impacts of the NREGA on employment, wages, and markdowns for manufacturing firms on the baseline, even for heterogeneous workers. However, we found heterogeneous effects in low-paying and labor-intensive manufacturing firms with low labor productivity, those in districts with high labor mobility, and whose initial average wage was below minimum wage. Incorporating imperfect enforcement, we also find that the heterogeneous impacts of the public works program around minimum wage are more significant in higher enforcement regimes. The impacts are mainly driven by production and regular or non-contract workers and are concentrated in the leading industries and among rural firms. These empirical results are in line with the theoretical predictions from our model.

We contribute to several strands of literature. First, we make theoretical and empirical contributions to the literature examining the public employment programs, particularly India's public employment program. Several studies investigate the program's consequences on various non-labor market outcomes. However, we contribute to a strand of the literature looking at the labor market impacts, which can be divided into two groups that estimate the direct and indirect impacts. The first line of works analyzes the direct effect of NREGA, which hires rural adults on rural public works during the agricultural off-season, on public employment (Azam, 2012; Imbert and Papp, 2020b), agricultural wages (Berg et al., 2018), and incomes of the rural poor (Muralidharan et al., 2023). The second strand of the literature on the indirect effects of the NREGA studies its spillover impacts on child labor (Islam and Sivasankaran, 2015; Li and Sekhri, 2020), private works (Imbert and Papp, 2015; Muralidharan et al., 2023; Zimmermann, 2024), and urban labor markets (Imbert and Papp, 2020b). However, an indirect effect of the program on manufacturing labor markets is understudied, while Agarwal et al. (2021) provide employment and wage impacts in manufacturing. Our findings on employment and wage effects are strongly consistent with Agarwal et al. (2021), whose focus was to examine whether manufacturing firms react to employment reduction by investing in capital and technology. However, our focus in this paper is to explain what happens to workers' bargaining power in response to NREGA and subsequent changes in employment and wages in manufacturing. Theoretically, we contribute to the literature by developing a wage-setting model featuring NREGA and heterogeneous workers who vary in origin and skill level, an extension of a firm-specific labor supply setup in Card et al. (2018) and Manning (2021). Empirically, this paper is the first to investigate the spillover effect of NREGA on monopsony power in the manufacturing industry. To the best of our knowledge, no study yet empirically quantifies the impact of the NREGA on manufacturing firms' monopsony power.

Second, we add to the growing literature on measuring monopsony power by providing markdown estimates from developing countries in the context of India's manufacturing. The monopsony literature suggests that labor market power exists by estimating wage markdowns in different labor markets from developed and developing countries. For India, Brooks et al. (2021a,b) estimated markdowns using the ASI data and found that the markdown is 1.01-1.13, which suggests that the labor market in India's manufacturing is almost perfectly competitive. The average markdown estimates imply that a worker receives 88-99 cents on each dollar[5] generated, which is higher than estimates found in other developing countries, for example, 47 cents in Vietnam (Hoang et al., 2023), 50 cents in Brazil (Felix, 2022), and 71 cents in Colombia (Amodio and Roux, 2024) and even those in developed countries, for example, 65-80 cents in the U.S. (Yeh et al., 2022)[6] and 79 cents in Germany (Byambasuren, 2025). The median markdown estimates for India from Brooks et al. (2021a,b) are below unity and around 0.5, implying that workers have market power in most manufacturing firms.[7] However, other studies such as Muralidharan et al. (2023) suggest considerable monopsony power in the state of Andhra Pradesh, India, quantifying an implied markdown of 4, i.e., workers receive 0.25 rupees on the marginal rupee. Given these mixed findings on labor market power in India, we estimate the markdown to contribute to this debate based on the "production approach" proposed in the recent monopsony literature (Morlacco, 2019; Mertens, 2020; Brooks et al., 2021b; Yeh et al., 2022; Delabastita and Rubens, 2023) by closely following Yeh et al. (2022). We also provide the first markdown estimates for heterogeneous workers who differ by employment contracts and skills since the NREGA program that generates low-skilled manual jobs might have affected such workers differently.[8]

Third, our work adds to the literature on monopsony power that actively explores the sources of a firm's wage-setting power. This fast-growing literature finds that trade (Mertens, 2020; Kondo et al., 2022; Felix, 2022; Hoang et al., 2023; Kusaka, 2023), infrastructure investments (Brooks et al., 2021a; Pérez et al., 2022), foreign direct investment (Lu et al., 2020), wage-setting collusion between firms (Delabastita and Rubens, 2023), displacement threat from automation (Byambasuren, 2025) contribute to labor market power. However, evidence on the drivers of monopsony power is still needed (Card, 2022). Among studies on NREGA, Muralidharan et al. (2023) find that an experiment of the public employment program randomized at the sub-district level in Andhra Pradesh improved workers' bargaining power at private enterprises in non-agricultural industries by enhancing their outside options in the treated areas in 2013, one year after the experiment. They

[5]The currency in which the marginal revenue product of labor (MRPL) and the wages are expressed is irrelevant because these estimates are based on the MRPL-to-wage ratio.

[6]Other studies also suggest that labor market power exists in the U.S. labor market (Berger et al., 2022; Lamadon et al., 2022).

[7]Another study suggesting perfectly competitive labor markets in developing countries is Kondo et al. (2022) for China, where the median markdown estimate is below unity.

[8]A growing literature on heterogeneity in monopsony power shows that monopsony power differs by worker characteristics, for example, gender (Hirsch et al., 2010; Caldwell and Oehlsen, 2023; Sharma, 2023), distaste for commuting (Datta, 2024), job tasks being performed by the worker (Bachmann et al., 2022; Byambasuren, 2025), and for production and non-production workers (Yeh et al., 2022) using administrative and experimental data. Also, some studies examine the heterogeneity of monopsony power by labor market tightness (Hirsch et al., 2022) and industries (Bachmann and Frings, 2017). These studies on heterogeneity in labor market power mainly estimate labor supply elasticities for different workers as a measure of monopsony power, except for Yeh et al. (2022) and Byambasuren (2025) who estimate markdowns across heterogeneous labor.

also show heterogeneous employment effect by employer power, suggesting that the changes in market employment covary with employer power and NREGA led to an increase in private-market employment in villages with greater land concentration. However, in this paper, we exploit the early phases of the nationwide program that mainly generates non-manufacturing employment as a source of variation in manufacturing labor supply and examine it as a source of monopsony power in India's manufacturing.[9]

The rest of the paper is structured as follows. Section 2 provides background on the NREGA program, focusing on its aspects related to the study. Section 3 presents the model of labor supply, labor hiring, and the NREGA. Section 4 describes the data, and Section 5 discusses the method used for estimating the markdowns and the estimated markdowns for India's manufacturing firms. Section 6 presents the empirical strategy employed to identify the causal effects. Section 7 discusses the empirical results, and Section 8 presents the robustness checks. Finally, Section 9 concludes.

2 India's Public Work Guarantee Program

The National Rural Employment Guarantee Act (NREGA), or Mahatma Gandhi NREGA (MGNREGA), was passed in September 2005 and is the world's largest workfare program. The objective was "to provide at least 100 days of guaranteed wage employment in a financial year to every rural household whose adult members volunteer to do unskilled manual work." The NREGA program (sometimes called the National Rural Employment Guarantee Scheme—NREGS)[10] was implemented in a staggered rollout over three phases and started in 200 poorest districts in the first phase in February 2006. In the second phase, the program was extended in April 2007, with another 130 districts placed next in the income distribution. The policy was implemented in all remaining rural districts in the third phase that started in April 2008 (Ministry of Rural Development 2010). The NREGA has been a nationwide policy operating in all rural districts, around 99 percent of all India's districts until now, and urban districts were excluded from the program. Figure 1 shows the distribution of India's districts where the NREGA has been implemented in three phases.

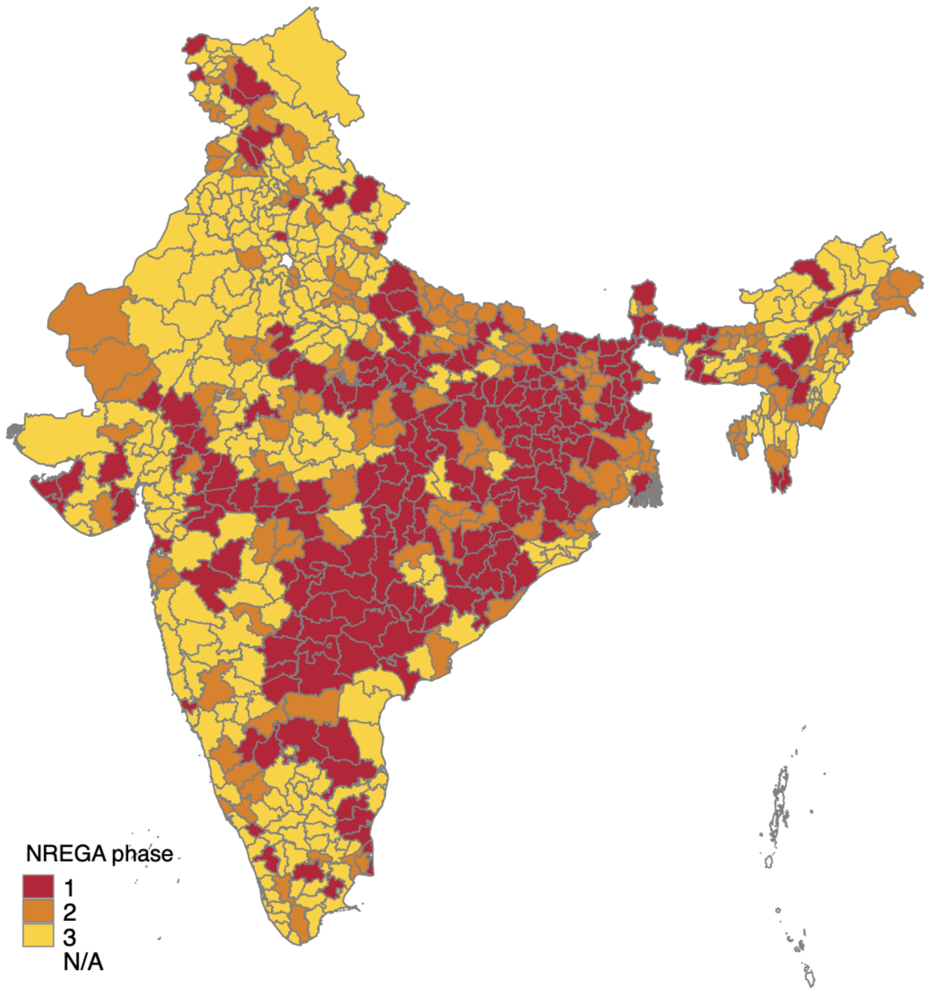
The program primarily focuses on creating jobs in agriculture, and the projects include, for example, the construction of drought-proofing, flood prevention, and irrigation infrastructures (Berg et al., 2018; Taraz, 2023). The Ministry of Rural Development, Government of India, outlines the complete list of works permissible under the MGNREGA.[11] As mentioned above, the implementa-

[9]This paper deviates from Muralidharan et al. (2023) in several ways. First, they exploit a randomized control trial (RCT) of NREGA in Andhra Pradesh in 2012. However, we use the first two stages of the nationwide NREGA program in 2006 and 2007 to provide nationally representative estimates that satisfy external validity. Second, we measure the employer power using wage markdowns, whereas they use labor supply elasticity to measure the workers' bargaining power. Third, our focus is on manufacturing plants while they examine the overall impact on private firms in all non-agricultural sectors, including manufacturing and construction, among others.

[10]In this paper, we use these terms interchangeably.

[11]https://megsres.nic.in/sites/default/files/mgnrega-permissible-work-list.pdf

Figure 1: NREGA Phases



Notes: The figure presents the three phases in which the NREGA program has been implemented in India at the district level.

tion rollout was based on an income distribution of districts, and the government prioritized poorer districts in the early stages. For prioritizing the poorer districts first, the authority used a "backwardness index" developed by the Planning Commission of India (Planning Commission, 2003) using mid-1990s district-level data on agricultural wages, agricultural productivity, and the share of scheduled caste individuals. The existing studies suggest that the NREGA increased agricultural wages (e.g., Berg et al., 2018) and agricultural production, e.g., aggregate yields and crop production (Taraz, 2023). Imbert and Papp (2015) also argue that well-implemented projects might increase agricultural productivity in the medium run. The intervention that we investigate in this paper attracts workers into agriculture.

Although the stated mandate of the policy was to create jobs, it ultimately intends to reduce poverty via employment generation, especially for low-skilled individuals. According to the act, machines are banned from worksites, and expenses on material, capital, and high-skilled workers are restricted to 40 percent of total expenditures. The Gram Panchayats (GPs) were responsible for implementing the program. The state government independently sets the wages of the NREGA jobs at the state-level minimum wage level. However, wages are paid entirely by the central government,[12] and states cover 25 percent of the expenditures other than low-skilled workers, including materials, capital, and high-skilled workers. So, the act encourages generating short-term employment for low-skilled workers.

The remuneration structure of NREGA workers is based on a piece rate or a fixed daily wage, and wages must be paid within 15 days. Workers are compensated according to the work they completed in the piece-rate system, a more prevalent payment system. As discussed above, the daily wage received by the worker is usually lower than the declared wage rates set at the state level. However, the NREGA jobs still attract workers due to (i) no search cost and (ii) the average daily earnings for these public works are relatively higher than the wages for casual workers. Results from existing studies suggest hardly an impact of the intervention in the short run. For example, Zimmermann (2024) finds that the second phase of the NREGA, ignoring the other phases, did not crowd out workers out of private sector jobs. World Bank (2011) also fails to find an improvement in productivity in the early years. However, Imbert and Papp (2015) show a crowding out effect of workers out of the private sector to the public sector and an increase in private sector wages in early districts (i.e., districts received the program in the first two phases) relative to late districts (i.e., districts in the third phase) over the period from July 2007 to June 2008. In this paper, we also focus on these initial years.

3 A Model of Labor Supply, Labor Hiring and the NREGA

We take two features of the NREGA program as building blocks of the model. First, only workers who are residents of rural settlements within a district are eligible to apply for NREGA employment. Second, NREGA employment entitles workers effectively to part-time unskilled manual work. These features lend themselves well to a model featuring heterogeneity (i) in worker (urban/rural) origin, which dictates program entitlement, (ii) in worker skill, which changes the ability or willingness to engage in or benefit from the job insurance features of the NREGA, and (iii) in firm (urban/rural) location, which can change the composition of urban/rural labor supply depending on skill level and worker mobility.

[12] There were some fraudulent activities by state officials to over-report the NREGA workers rather than the actual employment to request extra funding from the central government and receive the salaries of those "artificial" or "ghost" workers. For instance, Niehaus and Sukhtankar (2013) show that only about half of the individuals registered in the government administrative data as NREGA workers existed and worked in Orissa. The wages paid to some workers were lower than the promised state minimum wage level. This behavior could be due to the payment structure, i.e., the funding source is the central government. However, there is no guarantee that no theft will occur even if the local government funds the program.

We will focus on assessing the potentially heterogeneous markdown, wage, productivity, and employment impacts of an NREGA program for skilled and unskilled workers from rural and urban settlements employed in both urban and rural firms in the presence of inter-regional mobility. How does an NREGA program, which takes rural unskilled workers as intended beneficiaries, impact labor supply and hiring patterns in urban and rural establishments?

3.1 Labor Supply in the Presence of NREGA

We adapt the firm-specific labor supply setup in Card et al. (2018) and Manning (2021) and incorporate heterogeneous workers with origins $o = \{u, r\}$ for urban and rural, and skills $s = \{H, L\}$ for high and low skilled. $\mathcal{I} = \mathcal{I}_U \cup \mathcal{I}_R$ is a set of firms where $\mathcal{I}_U = \{1, ..., n\}$ index urban firms in a district, and $\mathcal{I}_R = n + 1, ..., N$, index rural firms in the same district. Also, let N denote an employment state that relies only on NREGA benefits. The number of workers from origin o and skill s in a district, L^{os}, is exogenously given.

Guided by the need to provide an employment safety net to benefit workers of rural origin, the NREGA program offers up to 100 days of employment to any worker who can provide proof of rural residency and who is willing to undertake manual labor, including those who may be employed part of the year. Thus, while the NREGA can clearly benefit the otherwise rural unemployed by providing subsistence employment, an employed individual may also perceive expected benefits by leveraging the NREGA as a supplemental source of income in times of need, or unexpected job turnover. The size of the perceived benefit from NREGA can also depend on the location of the job held by an individual. For example, a rural migrant who landed an urban job can find NREGA benefits difficult to access because of transportation-related return-migration costs.

Thus, let the utility of a worker of type os, henceforth worker os, in firm i be given by

$$u_i^{os} = \beta^s(w_i^{os} + \tau_i^{os} + a_i^{os}) + \eta_i^{os}$$

where w_i^{os} is the log wage $\log W_i^{os}$ of worker-type os in firm i, a_i^{os} is a non-wage employment amenities shifter adjusted for effort and commuting costs, and η_i^{os} is a type 1 extreme value distributed preference shifter.

To capture firm-specific NREGA benefits, τ_i^{os} is a log wage adjustment term indicating the perceived NREGA benefits facing a worker in firm i. Since NREGA targets rural residents,

$$\tau_i^{us} = 0, \quad \text{for} \quad s = H, L$$

and since NREGA work involves unskilled manual labor, and if high-skilled workers are not able to or not willing to take jobs requiring manual labor

$$\tau_i^{rH} = 0.$$

Finally, the perceived NREGA benefit for a worker rL in firm i is

$$\tau_i^{rL} \geq 0.$$

Note that τ_i^{rL} is specific to firm location and nature of work, indexed by i.

Accounting for the wage, amenities, and NREGA benefits of work, the likelihood that worker os will choose employer i over others in $\mathcal{I}^{os}$ is given by the familiar multinomial form:

$$L_i^{os}(w_i^{os}) = \frac{\exp(\beta^s(w_i^{os} + \tau_i^{os} + a_i^{os}))L^{os}}{\sum_{i' \in \mathcal{I}^{os}} \exp(\beta^s(w_{i'}^{os} + \tau_{i'}^{os} + a_{i'}^{os}))} \tag{1}$$

where $\mathcal{I}^{rs} = \mathcal{I}$, and $\mathcal{I}^{us} = \mathcal{I} \setminus N$. To develop intuitions, it is helpful to start with a log-linear approximation of (1) (Manning 2021). Denote ℓ_i^{os} as $\log L_i^{os}$:

$$\ell_i^{os}(w_i^{os}) \approx \beta^s \left[(w_i^{os} - \bar{w}^{os}) + (a_i^{os} - \bar{a}^{os}) + (\tau_i^{os} - \bar{\tau}^{os})\right] + \ell^{os}, \tag{2}$$

where $\ell^{os} \equiv \log L^{os}$, and

$$\bar{w}^{os} \equiv \sum_{i' \in \mathcal{I}^{os}} s_{i'}^{os} w_{i'}^{os}, \quad \bar{a}^{os} \equiv \sum_{i' \in \mathcal{I}^{os}} s_{i'}^{os} a_{i'}^{os}, \quad \bar{\tau}^{os} \equiv \sum_{i' \in \mathcal{I}^{os}} s_{i'}^{os} \tau_{i'}^{os}.$$

s_i^{os} is the employment share of firm i, L_i^{os}/L^{os}, $\bar{w}^{os}$, $\bar{a}^{os}$ and $\bar{\tau}^{os}$ are the weighted average log wage, amenities and job insurance benefits from the NREGA program in $\mathcal{I}^{os}$.

Equation (2) is the firm-specific labor supply schedule of i. The log-linearization makes it plain that the NREGA is a variable labor supply shifter at the firm level. In particular, since $\tau_i^{us} = 0$ for urban workers, and $\tau_i^{oH} = 0$ for high skilled workers. It follows that the NREGA program does not directly affect these two lists of labor supply schedules:

$$\begin{aligned} \ell_i^{us}(w_i^{us}) &= \beta^s \left[(w_i^{us} - \bar{w}^{us}) + (a_i^{us} - \bar{a}^{us})\right] + \ell^{us}, \quad s = H, L, \\ \ell_i^{oH}(w_i^{oH}) &= \beta^H \left[(w_i^{oH} - \bar{w}^{oH}) + (a_i^{oH} - \bar{a}^{oH})\right] + \ell^{oH}, \quad o = u, r. \end{aligned} \tag{3}$$

This leaves urban and rural firms employing low-skilled workers of rural origin. The NREGA presents itself as a negative (positive) labor supply shock to firm i if and only if

$$\tau_i^{rL} - \bar{\tau}^{rL} < (\geq)\ 0.$$

Furthermore, the NREGA is also a variable labor supply elasticity shifter. From equation (1), let the own-wage elasticity of labor supply ϵ_i^{os} be:

$$\epsilon_i^{os} \equiv \frac{d\ell_i^{os}}{dw_i^{os}} = \beta^s(1 - s_i^{os}).$$

Thus the elasticity of labor supply brings together worker type-specific (β^s) and firm-specific effects (s_i^{os}) – the larger the share, the less elastic the labor supply will be. This dependency on relative employment shares suggests that labor supply elasticity can change due to the NREGA program. From (2), at constant wages w_i^{os} (to be endogenized in the sequel) and amenities a_i^{os} for all i and worker type os, labor supply elasticity ϵ_i^{os} rises when the employment share falls, or equivalently when

$$\tau_i^{rL} - \bar{\tau}^{rL} < (\geq)\ 0. \tag{4}$$

Henceforth, we will say that the NREGS has a *pro-competitive effect* (*pro-monopsony effect*) on firm i's employment – by shifting back (out) and flattening (steepening) a firm's labor supply – when the inequality above in (4) is (not) satisfied.[13]

The labor supply elasticity, which depends on employment shares s_i^{os}, can also be sensitive to a worker's urban/rural origin relative to the firm's location. It occurs when, for example, amenities preferences or commuting costs depend on a worker's residential origin. In particular, if

$$a_i^{us} - \bar{a}^{us} \gg 0 \ \text{ and } \ a_i^{rs} - \bar{a}^{rs} \ll 0$$

for $i \in \mathcal{I}^u$ and

$$a_i^{us} - \bar{a}^{us} \ll 0 \ \text{ and } \ a_i^{rs} - \bar{a}^{rs} \gg 0$$

for $i \in \mathcal{I}^r$, then $s_i^{us} > s_i^{rs}$, for $i \in \mathcal{I}_U$, and $s_i^{rs} > s_i^{us}$ for $i \in \mathcal{I}_R$, or equivalently

$$\epsilon_i^{us} < \epsilon_i^{rs}, \ \ i =\in \mathcal{I}_U, \ \ \epsilon_i^{rs} < \epsilon_i^{us}, \ \ i =\in \mathcal{I}_R.$$

Summarizing,

Proposition 1. *An NREGA shifts labor supply ℓ_i^{os} backwards and raises labor supply elasticity ϵ_i^{os}, if and only if it is pro-competitive:*

$$\tau_i^{os} < \bar{\tau}^{os}.$$

With rural-urban mobility, the labor supply elasticities of urban workers in urban firms are less than the labor supply elasticities of rural workers in urban firms, all else equal if and only if:

$$a_i^{us} - \bar{a}^{us} > 0, \ \ a_i^{rs} - \bar{a}^{rs} < 0 \ \text{ for } \ i \in \mathcal{I}_U \ \text{ and } \ a_i^{us} - \bar{a}^{us} < 0, \ \ a_i^{rs} - \bar{a}^{rs} > 0 \ \text{ for } \ i \in \mathcal{I}_R.$$

[13]This is consistent with Basu et al. (2009), for example, which demonstrates in a setting where $\tau_i^{os} = 0$ for any $i \neq N+1$, that an NREGA program introduces contestability in the labor market – effectively flattening the labor supply facing a firm. Similarly Muralidharan et al. (2023) makes a similar assessment about the pro-competitive effects of the NREGS program.

3.2 Labor Hiring in the Presence of NREGA

We formulate the hiring problem in an AKM setup (Abowd et al., 1999), where wage formation is jointly a function of a firm and a worker-type fixed effects. Specifically, firm i may hire only high-skilled workers, only low-skilled workers, or both (L_i^{os}) to produce an output that yields revenue y_i:

$$y_i = \sum_{o=u,r} \sum_{s=H,L} A_i^s \rho^{os} L_i^{os}.$$

Each firm takes the vector of wage decisions of all other firms W_{-i}^{os} as given, and maximizes profit by choice of W_i^{os}:

$$\max_{W_i^{os}} \sum_{o=u,r} \sum_{s=H,L} (A_i^s \rho^{os} - W_i^{os}) L_i^{os},$$

where labor supply schedules L_i^{os} are given by (1), while $A_i^s \geq 0$ and $\rho^{os} \geq 0$ are the firm- and worker-type specific productivity parameters. With positive employment, the associated first-order conditions imply a markdown formula for worker os in firm i:

$$\mu_i^{os} = \frac{A_i^s \rho^{os} - W_i^{os}}{W_i^{os}} = \frac{1}{\beta^s (1 - s_i^{os})} = \frac{1}{\epsilon_i^{os}}. \tag{5}$$

The log wage of worker os in firm i is:

$$w_i^{os} = log A_i^s + \log \rho^{os} + \log \left(1 + \frac{1}{\beta^s (1 - s_i^{os})}\right). \tag{6}$$

From (2), (5), and (6),

Proposition 2. *An NREGA raises the wage w_i^{os}, lowers employment ℓ_i^{os}, and suppresses the markdown μ_i^{os} if the NREGA is pro-competitive in firm i hiring worker os, or $\tau_i^{os} < \bar{\tau}^{os}$.*

Intuitively, when the NREGA is pro-competitive, firms raise compensation to compete for workers who find other jobs made more desirable by the NREGA to be more attractive. The opposite wage, employment, and markdown effects arise with a pro-monopsonistic NREGA program when the NREGA benefits render a firm more desirable thanks to the NREGS job safety net, or $\tau_i^{os} > \bar{\tau}^{os}$. Of course, an NREGA may also have neutral effects on labor supply, and thus wages. This occurs when $\tau_i^{os} = \bar{\tau}^{os}$ when the relative desirability of firm i and the average firm in worker os's choice set is unchanged by NREGA benefits.

Since the NREGA only targets unskilled workers of rural origin, the average markdown among employers of unskilled workers from both urban and rural settlements is:

$$\bar{\mu}_i^{os} = (1 - \theta_i^{rL}) \mu_i^{uL} + \theta_i^{rL} \mu_i^{rL} \tag{7}$$

where the employment share θ_i^{rL} is given by:

$$\theta_i^{rL} = \frac{L_i^{rL}}{L_i^{uL} + L_i^{rL}}.$$

From Proposition 2, the NREGA program changes the markdown applied to rural workers through μ_i^{rL}, as well as the composition of workers in the firm through θ_i^{rL}. Consider, therefore, an average markdown effect of a pro-competitive NREGA. From Proposition (2), μ_i^{rl} falls, but a composition effect works through a reduction in θ^{rL_i} suggests that the influence of rural workers falls as well. Indeed, the composition effect can more than fully offset the pro-competitive effect if workers whose share in firm i is rising confront higher markup, or if $\mu_i^{uL} > \mu_i^{rL}$. Equation (7) reminds us that the markdown effect of the NREGA program is nuanced, particularly in cases where the urban or rural origin of workers, and thus the composition of these worker types in a firm, are unknown to the researcher.

Turning to the average wage in these firms, it is expressed as

$$(1 - \theta_i^{rL})W_i^{uL} + \theta_i^{rL} W_i^{rL}.$$

The effect of NREGS is generally ambiguous here as well since the employment and hiring effects are of opposite signs in any firm i that hires rural unskilled workers from Proposition 2.

Finally, the marginal product of labor in these firms also changes with worker composition. In particular, the marginal product of labor averaged across the two groups of workers is:

$$(1 - \theta_i^{rL})A_i^L \rho^{uL} + \theta_i^{rL} A_i^L \rho^{rL}.$$

Again using the case of a pro-competitive NREGA as a case in point with $\tau_i^{os} - \bar{\tau}^{os} < (\geq) 0$, the worker composition changes as a result of the NREGA program can give rise the appearance of an increase in labor productivity (through a reduction in hiring θ_i^{rL} of rural unskilled workers) if and only if urban workers are more productive than rural workers in firm i.

To apply these findings to help organize our empirical results, we need to distinguish between high and low-skilled employment. In particular, high-skilled employment, as shown above, should be immune to any direct NREGA effects.[14] We also need to distinguish between firms where the NREGA program is more likely to be pro-competitive. To reiterate, the NREGA is pro-competitive in firms where workers find it hard to access NREGA benefits.

Since we do not directly observe skill-specific labor market outcomes, we will divide firms into two productivity groups – above and below median productivity – as high-skilled workers are often more likely to be hired in higher-productivity firms (e.g., Haltiwanger et al., 2018). It al-

[14]There may be general equilibrium-type effects. We explore these in Section 7.3 using a production function that allows for complementarities between skilled and unskilled workers within a firm.

lows us to check whether the impact of the NREGA on high and low-productivity firms fits with the model's prediction about the labor market impacts of NREGA for high and low-skilled workers.

Finally, we do not directly observe workers' urban/rural origin. Nonetheless, to unpack the differential image of the NREGA on worker origin, we will divide firms into urban and rural firms, as reported in the ASI. Since urban firms are more likely to be farther away from the NREGA job sites, we check whether urban firms are more likely to face the pro-competitive effects of NREGA, as shown in the model. We can also check whether some rural firms face pro-competitive, neutral, or pro-monopsonistic effects, depending on whether:

$$\tau_i^{rL} < \ (=,\ \geq)\ \bar{\tau}^{rL}, \quad \text{for} \ \ i \in \mathcal{I}_R.$$

4 Data

4.1 NREGA Data

The data on policy change, NREGA, that we investigate in this paper is based on Imbert and Papp (2015), who mapped the three phases of the national program covering the entire country. However, the data producing the map was not available in their paper. So, we generated the data using their NREGA map and India's district-level boundary information. The NREGA data is thus at the district level.[15] Our generated data does not cover seven union territories (UTs), including Andaman and Nicobar Islands, Chandigarh, Dadra and Nagar Haveli, Daman & Diu, Lakshadweep, Delhi, and Puducherry. These excluded areas are all UTs and major settlements based in the city or town (i.e., urban areas), consistent with the fact that NREGA aims at rural areas.

4.2 Firm-Level Data

The firm-level data used in our empirical analysis is the panel version of the Annual Survey of Industries (ASI), conducted by the Ministry of Statistics and Programme Implementation (MoSPI), Government of India. The ASI is a nationally representative survey of all factories registered under The Factories Act: factories employing at least 10 workers and do not use electricity or employing at least 20 workers independent of the electricity use status. Even in the panel version, the ASI sample consists of firms from the Census and Sample sectors. The firms in the Census Sector include establishments employing at least 100 workers and are in a longitudinal structure. The firms in the Sample Sector are randomly sampled via a systematic circular sampling method from each State$\times$4-digit NIC Industry stratum and thus are not necessarily panel.

[15] Several studies provide data on NREGA districts, such as Berg et al. (2018) and Zimmermann (2024); however, these datasets do not cover all districts, given their specific research questions and empirical methods. For instance, Zimmermann (2024) uses data for only 17 states because the data for constructing the running variable for regression discontinuity design (RDD), an empirical method used in their paper, were only available for those states.

Our ASI data spans from 1998-1999 to 2007-2008, where the reference year is the financial year of the factory, which starts in April and ends in March. For example, the latest financial year in our dataset is between 1 April 2007 and 31 March 2008. Our sample covers the pre- and post-treatment periods, allowing us to examine the impact of the policy initiated on 2 February 2006 on manufacturing firms. Since we have multiple pre-treatment periods, we can credibly test the parallel pre-trends assumption. Our estimated effects, however, are likely the short-run effects since we only have limited post-treatment years. However, a few post-treatment years favor causal identification by providing a never-treated control group. In the next section, we discuss our empirical strategy and identification assumption.

This data offers several unique features that are particularly suited for this study. First, the ASI offers nationally representative data for manufacturing firms, enabling us to provide country-level estimates. The sample size is also large. Second, it records detailed information on inputs and output of production necessary to provide unbiased estimates of markdowns using the production approach, such as labor headcount[16] and sales. Although the data does not report the quantity of goods produced and sold at the product level, the available information is sufficient for estimating unbiased markdowns.[17] Third, the ASI data contains detailed information for various types of heterogeneous workers, enabling us to estimate markdowns and the effects of the policy change on labor market outcomes for workers who differ by their, e.g., skills and employment contracts. For example, heterogeneous workers relevant to this study are production (non-managers or low-skilled workers) and non-production (managers or high-skilled workers) workers as the NREGA guarantees temporary jobs in rural areas, mostly in agriculture and construction industries where production or non-manager workers dominate the workforce.

We merge the firm-level data with the NREGA data at the district level as (i) the policy changes are at the district level and (ii) the most granular spatial information in the ASI data is the district.

4.3 Additional Data

We also collect information on rainfall, worker mobility or migration, and minimum wages to supplement our analysis. First, we measure annual average rainfall at the district level using satellite

[16] The ASI data also reports total mandays and mandays for manufacturing and non-manufacturing, and we use these employment measures to check the robustness of our results on markdown estimates and the effect of NREGA on markdowns and other labor market outcomes. The information on mandays worked and paid is available; however, we focus on mandays worked because only about two-thirds of the mandays worked were paid. For the measure of mandays worked, we concentrate on total and manufacturing mandays worked as most of the mandays were only manufacturing, i.e., approximately 90 percent of the firms had zero non-manufacturing mandays worked from 1999-2008.

[17] As shown in Yeh et al. (2022), outputs do not have to be measured by physical product but can be measured by revenue (either whether or not deflated by some aggregate price) to provide an unbiased estimate of wage markdowns using the production function approach. However, the price markups should be interpreted cautiously as markup estimates are biased towards zero, i.e., should be interpreted as lower bounds, when physical outputs are proxied by revenue even though deflated by industry-level prices (Klette and Griliches, 1996; Bond et al., 2021).

data on daily precipitation (thousand mm/d) between 1981 and 2022. This environmental factor can affect the production, employment, and wages of the agricultural industry, which would also affect the labor market conditions of the manufacturing industry.[18] We merge the rainfall data with the firm-level data at the district-year level.

Second, we leverage the 2001 Census tables on migrant stocks to measure worker mobility at the district level (https://censusindia.gov.in/census.website/data/census-tables). We use the total number of people living in a particular district whose previous residence was anywhere but in the same district (Census Table D-02) and express it as a share of the population. Using this information, we split districts into groups, such as those with high (low) worker mobility or a share of migrants above (below) the national median. We classify the districts based on the pre-treatment level of worker mobility by 2001 because the program could have affected the migration pattern. The number of migrants with different durations of residence has been used for robustness checks, such as those with a duration of residence less than a year, 1-4 years, or 5-9 years.

Third, we collect State$\times$Two-digit NIC Industry$\times$Year level information on minimum wages as state governments set the minimum wages that vary across industries in India. The minimum wage data is obtained from the annual reports on the working of the Minimum Wages Act (1948), compiled by the Ministry of Labour and Employment, Government of India, between 2001-2011 and covers eight two-digit NIC industries, including tobacco, food, leather, printing, chemicals, wood, plastic, and automobile. Although our minimum wage data is limited to only eight broad industries, we merge the minimum wage data with the firm-level data at the state-industry-year level to briefly examine the impact of the NREGA around minimum wage as the wages paid for NREGA works are set at the state's minimum wage level.

The labor market effects of NREGA around minimum wage could also vary depending on minimum wage enforcement. Relatedly, the literature on minimum wage theoretically shows that the labor market effects of the minimum wage are different under perfect (e.g., Stigler, 1946) and imperfect (e.g., Basu et al., 2010) enforcement. Studies on NREGA have not incorporated imperfect enforcement, but we allow imperfect enforcement of minimum wages. The crowding-out effect of NREGA on employment in manufacturing is likely to be stronger in states where the minimum wage is highly enforced and whose average compensation is below the minimum wage. Fourth, we

[18] Several studies study the combined impacts of NREGA and rainfall shocks on labor markets. For instance, Maitra and Tagat (2024) estimate the effect of rainfall shocks on time allocation of individual members to different activities and show that the NREGA can dampen the impact of shocks. Both men and women increase their participation in the NREGA program when faced with rainfall shocks. Exploiting the NREGA rollout and random weather fluctuations based on nationwide panel data, Taraz (2023) finds that NREGA makes crop yields more sensitive to low rainfall shocks. These results are consistent with a labor market channel, by which NREGA increases non-farm labor supply in low rainfall years, and an income channel, by which NREGA leads to riskier agricultural practices. Using a regression discontinuity design, Zimmermann (2024) also shows that, after the NREGS rollout, private sector wages increase substantially for women but not men, and these effects are concentrated during the main agricultural season. Additionally, Johnson (2009) suggests that households in a village that suffers from bad weather may compensate for the loss of income by increasing their participation in the NREGA program in Andhra Pradesh if the workfare program participation is sufficiently flexible.

thus leverage the number of inspections per worker at the state level to measure minimum wage enforcement following Soundararajan (2019), who shows that minimum wage effects are heterogeneous by enforcement regimes in India. The data on inspections conducted at the state level over time is obtained from the same source as minimum wages, annual reports on the Working of the Minimum Wages Act (1948) between 1998 and 2010.

Additionally, we obtained the wholesale price index (WPI) from the Ministry of Commerce and Industry, Government of India (https://eaindustry.nic.in/) to deflate the firm's sales revenue. The annual WPI is defined at the two-digit NIC industry level and spans between 1994 and 2020, with 1993-1994 as the base year. The WPI is available for only manufacturing industries, excluding recycling, which yields 22 two-digit industries.

4.4 Descriptive Statistics

Our baseline outcomes include employment, wage, markdown, and marginal revenue product of labor (MRPL). In our baseline empirical analysis, we use the sample on which markdown has been estimated, which is a sub-sample of the full ASI sample. So, Table 1 provides the descriptive statistics of the employment and wage on the markdown sample we used for our regression analysis and the full sample to show that the two samples are comparable. The markdown was estimated for about 30% of the firms in the sample. However, the employment and wages are consistent across the two datasets. The mean log employment ranges from 3.2 to 3.9, while the mean log wage per worker ranges from 4.6 to 4.7 across the markdown and the whole sample. The following section presents the method used for estimating markdowns, the results from the markdown estimation, and the calculation of MRPL in detail.

Table 1: Summary Statistics of Employment and Wages

	Full sample		Markdown sample	
	(1) Mean	(2) SD	(3) Mean	(4) SD
$\ln L_{it}$	3.205	1.208	3.989	1.433
$\ln W_{it}$	4.602	0.525	4.727	0.580
N	289,385		86,449	

Notes: The table presents the summary statistics (mean and SD values) for the outcome variables, including employment (L_{it}, number of workers) and wage (W_{it}). Columns (1) and (2) report statistics based on all firms in the full sample of ASI data between 1999 and 2008; Columns (3) and (4) report the statistics based on the the sample of firms for which markdown was estimated between 2000 and 2008. The statistics are calculated using sampling weights provided in the data.

5 Estimating Markdowns

5.1 Estimation Method

We estimate the plant-level markdowns using the "production" approach following Yeh et al. (2022). The duality of a firm's profit maximization and cost minimization problem yields the following expression for wage markdown,

$$\nu_{it} = \frac{\theta_{it}^L}{\alpha_{it}^L} \cdot \mu_{it}^{-1}, \tag{8}$$

where ν_{it} is the markdown for firm i in year t, θ_{it}^L is the output elasticity of labor, α_{it}^L is the firm's share of labor cost in revenue, and μ_{it} is the firm's markup in the product market. According to the definition of wage markdowns, the MRPL is defined as $MRPL_{it} = \nu_{it} W_{it}$ where W_{it} is the average wage of workers at firm i in year t. To measure the output elasticity of labor (θ_{it}^L) and price markup (μ_{it}), we estimate the production function using a "proxy variable" method (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015). We measure markup in the spirit of De Loecker and Warzynski (2012), who show that

$$\mu_{it} = \frac{\theta_{it}^M}{\alpha_{it}^M}, \tag{9}$$

where θ_{it}^M is the output elasticity of a variable input M_{it} other than labor, e.g., material inputs, and α_{it}^M is the share of expenditures on input M_{it} in total sales or revenue. We can calculate the share of labor cost in revenue (α_{it}^L) and the share of material cost in revenue (α_{it}^M) directly from the data by taking the ratio of labor payments and costs on materials to the firm's sales revenue, respectively. The output elasticities of labor and materials are computed as follows, respectively, under the translog production function, which is our preferred production technology:

$$\begin{aligned} \theta_{it}^L &= \hat{\beta}_l + \hat{\beta}_{kl} k_{it} + \hat{\beta}_{lm} m_{it} + 2\hat{\beta}_{ll} l_{it}, \\ \theta_{it}^M &= \hat{\beta}_m + \hat{\beta}_{km} k_{it} + \hat{\beta}_{lm} l_{it} + 2\hat{\beta}_{mm} m_{it}, \end{aligned} \tag{10}$$

where the β are the production parameters, and l_{it}, k_{it} and m_{it} are log labor, log capital, and log intermediate material inputs, respectively. It is worth noting that output elasticities are firm-specific and time-varying, given the translog form. Under Cobb-Douglas production form, however, the output elasticities are defined common across firms and over time, i.e., $\theta_{it}^L = \hat{\beta}_l$ and $\theta_{it}^M = \hat{\beta}_m$.

To estimate the production function and identify parameters β, we follow Yeh et al. (2022) who also closely followed De Loecker and Warzynski (2012) and perform IV-GMM[19] estimation,

[19] The intuition of estimating production parameters using the proxy variable method can be thought through the IV logic (Wooldridge, 2009; Yeh et al., 2022) because the current use of production inputs are instrumented by one-period lagged values of every polynomial term in $f(\mathbf{x}_{it}; \beta)$ including l_{it} and m_{it} but capital at the current period k_{it}.

relying on the refined control function approach proposed by Ackerberg et al. (2015). The main identification assumption of the method is that firms dynamically optimize their decisions in discrete times, and intermediate material is fully flexible, i.e., the materials market is competitive. In general, we estimate the following production function

$$\begin{aligned} y_{it} &= f(\mathbf{x}_{it}; \beta) + \omega_{it} + \varepsilon_{it} \\ &= f(\mathbf{v}_{it}, \mathbf{k}_{it}; \beta) + \omega_{it} + \varepsilon_{it}, \end{aligned} \tag{11}$$

where y_{it} is the log output, $\mathbf{x}_{it}$ is the vector of log inputs, including fully flexible inputs such as intermediate materials ($\mathbf{v}_{it} = m_{it}$) and not fully flexible inputs such as labor and capital ($\mathbf{k}_{it} = (l_{it}, k_{it})'$), and ω_{it} is unobserved productivity of the firm. We proxy the unobserved productivity with $g_t(m_{it}; \mathbf{k}_{it}, \mathbf{c}_{it})$, an inverse function of demand for intermediate materials. The vector $\mathbf{c}_{it}$ includes any additional factors that affect the firm's demand for material inputs, such as input prices.

5.2 Estimated Markdowns in India's Manufacturing Plants

We first present the plant-level markdowns for India's manufacturing and characterize the estimates against the firm's idiosyncratic factors. Second, we discuss the evolution of aggregate markdowns over time during our study period between 2000 and 2008. We then consider worker heterogeneity and present the markdowns estimated for heterogeneous workers.

Plant-level markdowns. Table 2 shows the baseline plant-level markdowns. We find that the median markdown is close to unity, suggesting that the labor market in manufacturing is almost perfectly competitive. However, the average markdown is 1.387, implying that workers at average manufacturing receive 0.72 rupees for each rupee generated. Thus, fewer firms that charge high markdowns drive the average markdown higher than unity. The labor market power significantly varies across industries within manufacturing, and industries such as basic metals, petroleum, and plastic products posit the highest labor market power.

Our estimates on average and median values for wage markdowns are higher than the previous estimates by Brooks et al. (2021a,b), who argue that there is no labor market power in India's manufacturing, mainly due to a difference between Yeh et al.'s (2022) and their methods.[20] However, our estimates are generally consistent with Muralidharan et al. (2023), who show considerable labor market power in Andhra Pradesh, India. We estimated an average markdown of 1.301 for Andhra Pradesh, which indicates imperfect competition in its labor market. We conducted several robustness checks and validation exercises for our markdown estimates. First, we use various employment measures as alternatives to our baseline employment measure of worker headcounts, including total mandays worked, manufacturing mandays worked, and each of the three considered labor measure

[20] Specifically, Yeh et al. (2022) estimate the total wedge between wage and MRPL, while Brooks et al. (2021a,b) make further adjustments on the estimated wedge by conditioning that small firms with negligible market share to have no labor market power or a zero markdown.

Table 2: Estimated Plant-Level Markdowns in India's Manufacturing

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Basic metals	2.546	2.973	2.703	1.818	5396
Coke, refined petroleum products and nuclear fuel	2.481	2.753	2.293	1.666	1083
Rubber and plastics products	1.872	2.202	1.639	1.377	3278
Electrical machinery and apparatus	1.573	1.941	1.428	1.310	4149
Machinery and equipment	1.280	1.594	1.336	1.207	7348
Wood and of products of wood and cork, except furniture	1.275	1.458	0.909	0.871	1717
Fabricated metal products, except machinery and equipment	1.274	1.470	1.049	0.936	3894
Leather and related products	1.203	1.516	1.100	1.118	1971
Office, accounting, and computing machinery	1.187	1.664	1.820	1.640	212
Publishing, printing, and reproduction of recorded media	1.062	1.345	1.028	1.053	1440
Other transport equipment	1.045	1.500	1.515	1.373	2194
Textiles	1.009	1.349	1.103	1.180	10594
Paper and paper products	1.003	1.085	0.549	0.513	2690
Motor vehicles, trailers, and semi-trailers	0.894	1.038	0.614	0.610	3224
Furniture	0.858	1.062	0.709	0.762	2565
Chemicals and chemical products	0.852	1.070	0.773	0.842	10759
Radio, television, and communication equipment and apparatus	0.826	1.265	1.269	1.260	1512
Other non-metallic mineral products	0.821	1.032	0.757	0.748	9311
Medical, precision, and optical instruments, watches and clocks	0.744	1.124	0.991	1.110	2019
Food products and beverages	0.736	0.949	0.777	0.869	13731
Tobacco products	0.504	1.062	1.398	1.282	2047
Wearing apparel	0.245	0.555	0.545	0.850	1835
Whole sample	**1.024**	**1.387**	**1.135**	**1.211**	**92969**

Notes: Markdowns are estimated for 34,575 unique manufacturing establishments using the ASI data from 2000-2008 under the assumption of a translog specification for gross output, where 2000 is the financial year between 1 April 1999 and 31 March 2000. The labor inputs are measured by headcount in the production function, estimated separately for each two-digit industry group. Each industry group in manufacturing corresponds to the manufacturing categorization of the National Industry Classification (NIC-1998) at the two-digit level. The distributional statistics are calculated using sampling weights provided in the data.

plus one to avoid losing observations when taking natural logs. Table 3 reports the estimated markdown, suggesting that it is robust to alternative employment measures, and the alternative estimates are even higher than our baseline estimate. Second, our baseline markdown estimate is based on the assumption of a translog production function, and we check the robustness of our baseline estimates by using the Cobb-Douglas production function as an alternative functional form. As shown in Appendix A, markdowns estimated under the Cobb-Douglas production function are higher than unity and even those quantified under the translog production function both at the median and mean (see Table A.1). Third, we aggregate the plant-level markdowns at the state level and estimate the relationship between aggregate markdowns and the friendliness of labor market regulations, which we discuss further below.

We then document the heterogeneity of plant-level markdowns by plant characteristics that likely determine labor supply elasticity and labor share, including size, age, and productivity. We first investigate the plant size as a potential determinant of markdowns, and Figure 2a shows that larger firms charge significantly lower markdowns in India's manufacturing industry. This contra-

Table 3: Estimated Markdowns using Different Measures of Labor Input

	(1) Median	(2) Mean	(3) $IQR_{75\text{-}25}$	(4) SD	(5) N
	Panel A. Labor input				
Headcount	1.024	1.387	1.135	1.211	92969
Total mandays worked	1.296	1.684	1.399	1.347	96512
Manufacturing mandays worked	1.215	1.575	1.263	1.272	91622
	Panel B. Labor input + 1				
Headcount	1.086	1.469	1.210	1.285	93861
Total mandays worked	1.254	1.603	1.304	1.256	96088
Manufacturing mandays worked	1.390	2.254	2.022	2.589	93824

Notes: Panel A presents plant-level markdowns estimated using worker headcounts, total mandays worked, or manufacturing mandays worked as a measure of labor input in production function estimation with the assumption of a translog specification. Panel B shows plant-level markdowns estimated using the labor input plus one in the production function estimation. The ASI data on headcount and total mandays worked are available from 2000-2008, while information on manufacturing mandays worked is only available from 2001-2008. Thus, the number of observations is slightly smaller when manufacturing mandays are used than when the total mandays are used. The distributional statistics are calculated using sampling weights provided in the data.

dicts the previous findings from the U.S., where plant-level markdowns are associated with larger firms (Yeh et al., 2022).[21] Second, as shown in Figure 2b, firm age is also negatively correlated to markdowns in India. However, for an advanced economy in the U.S., Yeh et al. (2022) suggest that the age-markdown relationship is positive but only weakly significant over the age distribution. Third, Figure 2c illustrates that plants with higher total factor productivity of revenue (TRPR) charge higher markdowns, especially those in the top 90th percentile.[22] The TFPR-markdown relationship in India's manufacturing is also different from that in U.S. manufacturing, where the correlation between TFPR and markdown is mostly negative along the TFPR percentile and usually not significantly different from zero. Overall, these relationships for India's manufacturing differ from those for U.S. manufacturing.

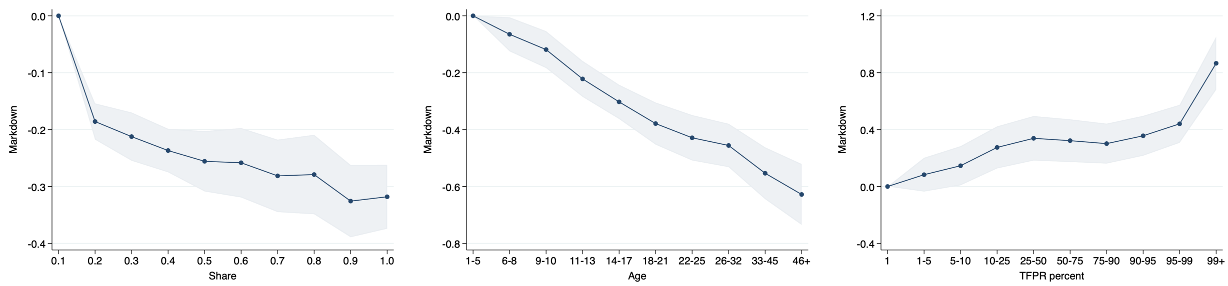
Aggregate markdowns. Now we discuss the aggregate markdowns—the weighted harmonic means of plant-level markdowns—constructed following Yeh et al. (2022). Figure 3 illustrates the trend of aggregate markdowns, showing an upward trend between 2000 and 2008. This pattern is consistent with an upward-sloping trend of markdown from Brooks et al. (2021a) over the same period. The trend of aggregate markdowns under the Cobb-Douglas production function also depicts an upward-sloping pattern similar to the baseline trend. It lends credence to the credibility of

[21]This descriptive finding is one of the reasons that we prefer to use Yeh et al.'s (2022) approach in this paper as it does not impose an assumption that larger firms have lower markdowns, which is opposite in our context of India's manufacturing.

[22]Although we did not report the results, this relationship remains the same when we use the TFPR percentiles specific across local labor markets.

Figure 2: Relationship between Markdown and Firm Characteristics

(a) Markdown-Size (b) Markdown-Age (c) Markdown-TFPR



Notes: Based on the ASI data from 2000-2008, and 2000 is the financial year between 1 April 1999 and 31 March 2000. Panel (a) illustrates the point estimates and 95% confidence intervals from estimating plant-level markdowns on size (measured by employment share) indicators. In the production function estimated separately for each two-digit industry group, labor inputs are measured by headcount. The regression controls for indicators for plant age and industry, district, and year fixed effects. The smallest size indicator is omitted, and thus coefficients reflect deviations relative to this reference group. The reference group labeled "0.1" includes plants with employment shares $s \in (0, 0.1]$. Other indicator variables are similarly defined. Panel (b) shows the point estimates and 95% confidence intervals from estimating plant-level markdowns on indicators of age deciles. The regression controls for indicators for plant size and industry, district, and year fixed effects. The first age decile is omitted; thus, coefficients reflect deviations relative to this reference group. Firm ages included in each decile are shown on a horizontal axis. Panel (c) shows the point estimates and 95% confidence intervals from estimating plant-level markdowns on productivity. The regression controls for industry, district, and year fixed effects. The first percentile of productivity is omitted; thus, coefficients reflect deviations relative to this reference group. Standard errors (SEs) are clustered by 4-digit NIC-1998 industries. The qualitative results remain the same when the SEs are clustered at the state level (31 clusters).

our baseline measure and suggests that the markdown estimate is not strongly specific to our choice of functional form. Appendix B analyzes the relationship between the state-level labor market reforms and markdowns aggregated at the state level to further validate our estimated markdown as an indicator of labor market power. The result shows that our markdown estimates are reasonable. In Appendix A.2, we discuss the trend of aggregate markups. Figure E.1 also presents the trends in other components of markdowns. The labor share presents a downward trend, and the labor output elasticity has been stable.

Figure 3: Time Evolution of the Aggregate Markdown

Notes: The plant-level markdowns are constructed using the ASI data from 2000-2008 under the assumption of translog production where labor inputs are measured by headcount. The plant-level markdowns are aggregated at the year level using employment shares of the labor market (combination of 4-digit NIC-1998 industry and states).

Markdowns for heterogeneous workers. We consider two sets of worker heterogeneity. First, we allow workers to differ by their skills or roles in the production line: production (low-skilled or non-managers) and non-production (high-skilled or managers) workers. To test for heterogeneity in markdowns across these two types of workers, we estimate the production function by treating them as separate inputs. Table 4 presents the estimated plant-level markdowns for production and non-production workers. Using different measures of labor inputs, we find that non-production workers are subject to higher markdown than production workers. The markdown heterogeneity for this group of workers in India's manufacturing is consistent with other studies, such as Bachmann et al. (2022) and Byambasuren (2025), who respectively estimated labor supply elasticity and wage markdowns for high- and low-skilled workers in a German context. The potential reason could be that production workers' outside employment options are better, and such workers are more flexible and mobile than non-production workers. Another context-specific reason that could explain the higher markdown for managers is the Industrial Disputes Act 1947 (IDA), India's key employment protection legislation for payroll workers. The distinction between workers and those in management, supervisory, or administrative positions (or managers) is one of the main concepts of labor regulations in India. Most regulations cover only those employees who qualify as workmen under Indian law.

Table 4: Estimated Plant-Level Markdowns for Production and Non-Production Workers in India's Manufacturing

	(1) Median	(2) Mean	(3) $IQR_{75\text{-}25}$	(4) SD	(5) N
	Panel A. Production workers (non-managers)				
Headcount	1.109	1.529	1.392	1.375	77378
Headcount + 1	1.119	1.542	1.379	1.390	79835
Total mandays worked	1.173	1.572	1.367	1.338	78265
Total mandays worked + 1	1.151	1.462	1.265	1.144	81517
Manufacturing mandays worked	1.134	1.517	1.340	1.309	75340
Manufacturing mandays worked + 1	1.164	1.526	1.295	1.26	78523
	Panel B. Non-production workers (managers)				
Headcount	2.954	5.005	4.569	5.780	77378
Headcount + 1	3.831	6.670	5.991	8.010	79835
Total mandays worked	2.971	4.933	4.482	5.610	78265
Total mandays worked + 1	3.029	5.692	5.206	7.347	81517
Manufacturing mandays worked	2.888	4.621	4.111	5.120	75340
Manufacturing mandays worked + 1	3.110	5.762	5.148	7.510	78523

Notes: The table presents plant-level markdowns estimated under the assumption of a translog specification for gross output with production (non-managers or low-skilled) and non-production (managers or high-skilled) workers. Panel A and B show markdowns for production and non-production workers, respectively, estimated using different labor input measures. The ASI data on headcount and total mandays worked are available from 2000-2008, while information on manufacturing mandays worked is only available from 2001-2008. Thus, the number of observations is slightly smaller when manufacturing mandays are used than when the total mandays are used. The distributional statistics are calculated using sampling weights provided in the data.

Second, we estimate the markdowns for workers with different employment contracts: regular workers who are directly employed and contract workers hired through contractors. Table 5 reports the estimation results and shows that markdowns over contract workers are relatively higher than markdowns over regular workers, independent of employment measures. Regular workers are usually protected by labor laws and have better job security. So, contract workers could be exploited more by their employers. However, contract workers are likely more mobile across firms and less tied to their current employers than regular workers. In this sense, contract or temporary workers are less constrained and have better outside options, so they are less exploited than permanent workers. In the context of India's manufacturing, we find that the former force dominates as we find that contract workers are exploited more than regular workers.[23]

Table 5: Estimated Plant-Level Markdowns for Regular and Contract Workers in India's Manufacturing

	(1) Median	(2) Mean	(3) $IQR_{75\text{-}25}$	(4) SD	(5) N
	Panel A. Regular workers				
Headcount	1.506	1.962	1.624	1.583	23981
Headcount + 1	1.600	2.172	1.946	1.847	29926
Total mandays worked	1.488	1.991	1.583	1.683	24548
Total mandays worked + 1	1.435	1.884	1.625	1.546	32780
Manufacturing mandays worked	1.545	2.050	1.674	1.720	24346
Manufacturing mandays worked + 1	1.332	1.819	1.606	1.531	31391
	Panel B. Contract workers				
Headcount	1.787	3.340	3.628	4.412	23981
Headcount + 1	2.050	3.944	4.611	4.906	29926
Total mandays worked	1.901	3.238	3.416	3.881	24548
Total mandays worked + 1	2.213	5.152	5.272	7.756	32780
Manufacturing mandays worked	1.945	3.129	3.458	3.398	24346
Manufacturing mandays worked + 1	2.178	4.698	4.877	6.753	31391

Notes: The table presents plant-level markdowns estimated under the assumption of a translog specification for gross output with regular and contract workers. Panel A and B show markdowns for regular and contract workers, respectively, estimated using different labor input measures. Regular workers are employed directly, while contract workers are hired through contractors. The ASI data on headcount and total mandays worked are available from 2000-2008, while information on manufacturing mandays worked is only available from 2001-2008. Thus, the number of observations is slightly smaller when manufacturing mandays are used than when the total mandays are used. The distributional statistics are calculated using sampling weights provided in the data.

[23] Appendix A.3 presents the distribution and trend of markdowns for heterogeneous workers.

6 Empirical Strategy

This section describes the empirical strategy we employed to estimate the effects of the National Rural Employment Guarantee Act (NREGA) on labor markets in the manufacturing industry, including monopsony power, relying on a difference-in-differences (DID) design. We also discuss the identification assumptions.

6.1 Empirical Specification

To examine the implication of NREGA on firms in the manufacturing industry, we estimate the following difference-in-differences (DID) specification:

$$Y_{it} = \alpha + \beta \times \text{Post NREGA}_{dt} + \mathbf{X}'_{it}\gamma + \phi_i + \delta_{jst} + \varepsilon_{it}, \tag{12}$$

where Y_{it} is the outcome variable and Post NREGA$_{dt}$ is our treatment indicator for the post-NREGA period in NREGA phase-1 and phase-2 districts with phase-3 districts included in the control group. Given that the NREGA treatment is rolled out in multiple phases that started at different periods, the treatment variable is not defined by interacting treatment units with a post-treatment dummy. Put differently, the post-treatment periods are different for districts in different phases. Specifically, the treatment variable takes a value of 1 for (i) phase-1 districts after the 2005-2006 financial year and (ii) phase-2 districts after the 2006-2007 financial year, and 0 for (i) phase-3 districts in all periods and (ii) phase-1 and phase-2 districts before 2005-2006 financial year. We thus omit phase-2 districts from the control group during the period between the first two phases. Figure E.2 illustrates the treatment and control districts we use in our main estimation.

In our regressions, we control for the main characteristics of the firm, included in a vector $\mathbf{X'}_{it}$, i.e., the firm's age and age squared. We considered including firm size, but we decided not to control for size, which is based on employment count, considering that it is a "bad" control as one of our primary outcomes is employment. We also control for a rich set of fixed effects at the granular level to account for other push and pull factors of labor market dynamics in the manufacturing industry. Leveraging the longitudinal structure of our firm-level data, we include firm fixed effects, ϕ_i, which captures all time-invariant factors, such as location. To further control for changes over time happening at the state and industry level, such as labor market policies, weather, and other time-varying aggregate shocks, we include industry-state-year fixed effects, δ_{jst}. An example of labor market policies that need to be controlled to isolate the treatment effect of the NREGA program is the minimum wage independently set by state authorities specific across industries. Although the state-year fixed effects included in the full interaction of the three terms capture changing weather conditions at the state level, we also control for district-level rainfall shocks included in $\mathbf{X'}_{it}$ vector to further account for weather changes within the state across districts.

The standard errors are clustered at the district level, given that the NREGA treatment varies across districts (Bertrand et al., 2004). We examine the heterogeneous treatment effects by several firm characteristics, such as the firm's organization type (private or public), broad location (urban or rural), and labor productivity.

6.2 Identification and Assumptions

Several assumptions are needed to identify the causal effects of the first two phases of NREGA on labor market conditions and employer power in the manufacturing industry using a DID model in our settings. First, the key identification assumption is the parallel trends in the average outcome among the treated and control groups in the absence of the NREGA program, conditional on covariates. Second, treatment must have no causal effect before its implementation, i.e., no anticipation effect. Third, treatment should not have any spillover effect on the comparison population. We discuss each assumption below and argue that the assumptions are plausible in our context.

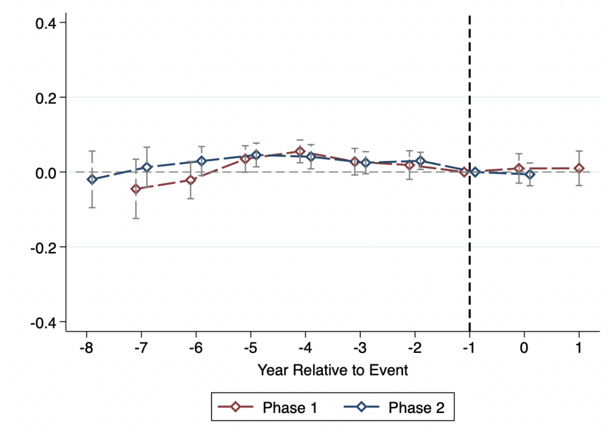
Parallel trend assumption. The availability of firm-level data for multiple years before the treatment enables us to credibly test the parallel pre-trends assumption for our main outcomes, including employment, wage, and markdown. We perform a formal test of the parallel trend assumption using event-study analysis. Specifically, we estimate the following regression, which is generally similar to Cook and Shah's (2022) specification:

$$Y_{it} = \alpha + \sum_{\tau \neq -1; \tau=-7}^{\tau=1} \gamma_{1\tau} \times I_\tau \times \text{Phase1}_d + \sum_{\tau \neq -1; \tau=-8}^{\tau=0} \gamma_{2\tau} \times I_\tau \times \text{Phase2}_d + \mathbf{X}'_{it}\gamma + \phi_i + \delta_{jst} + \varepsilon_{it}, \quad (13)$$

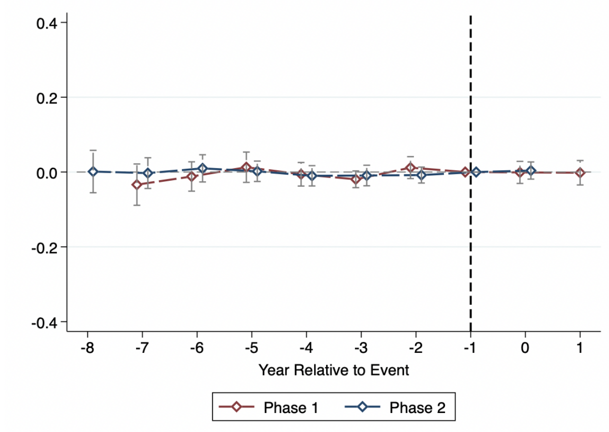
where Y_{it} is either log employment, log wage, or wage markdowns, and I_τ are lags and leads in event time, with $\tau = -1$ as the reference category. The policy event date (I_0) is the 2006-2007 financial year for Phase 1 and the 2007-2008 financial year for Phase 2. The remaining variables are similar to those in equation (12). Phase1_d and Phase2_d are the district d's treatment status for NREGA's first and second phases, respectively. The control group includes only districts never treated in the first two phases of the program, i.e., those in phase 3, which is outside of our study period. We separately plot estimates on $\gamma_{1\tau}$ and $\gamma_{2\tau}$. First, Figure 4a shows that parallel pre-trends assumption is reasonable for employment. Although some coefficients in the pre-treatment periods are significant at the 5% level, those effects are weakly significant, and most of the coefficients are essentially zero. Second, Figure 4b reports the results from event study regressions for wages and suggests that parallel pre-trends assumption holds for average wage. Third, Figure 4c shows that a parallel pre-trend is plausible for markdowns among the treated and control groups. The treated and comparison populations generally differ in our context because the NREGA program was first implemented in poorer districts. So, it is worth noting that our argument about the plausibility of parallel pre-trends assumption is conditional on attributes.

Figure 4: Test of Parallel Pre-Trends

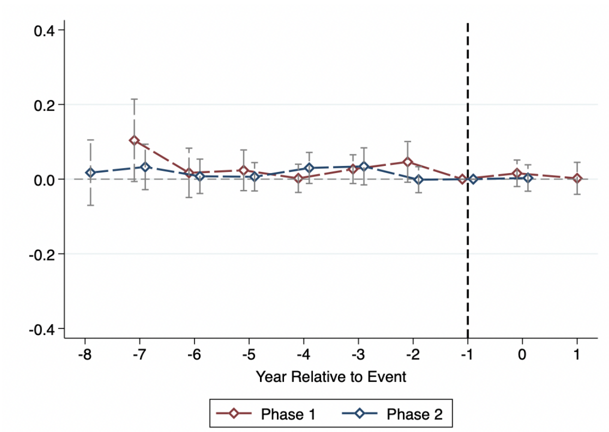
(a) $\ln L_{it}$



(b) $\ln W_{it}$



(c) $\ln \nu_{it}$

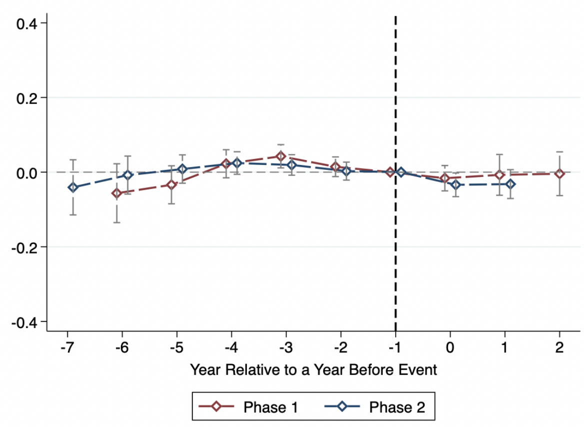


Notes: The figure reports the event study estimates from TWFE regressions testing the parallel pre-trends assumption in log employment (headcount plus one, panel (a)), log wage (panel (b)), and log markdowns (panel (c)). The horizontal axis shows the year relative to treatment, with 0 indicating the year of treatment. The vertical axis displays the estimated treatment effects by event time. All regressions include an unreported constant term and baseline controls and fixed effects. The baseline controls include the firm's age and age-squared and district-level rainfall shock. The baseline fixed effects include firm fixed effects and industry-by-state-year fixed effects. The industry fixed effects include dummies for two-digit NIC industry classification. Standard errors are clustered by districts, and 95% confidence intervals are shown.

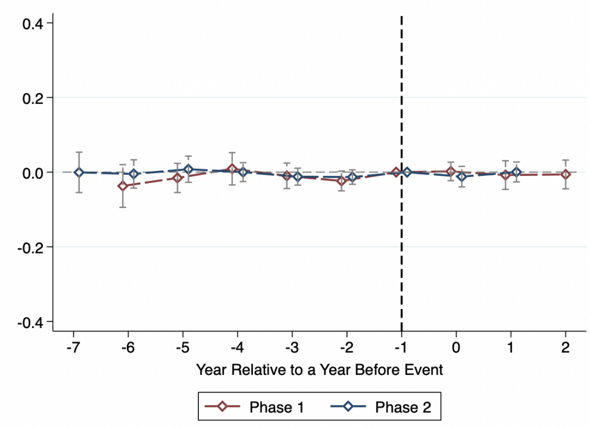
No anticipation assumption. We need firms not to anticipate the NREGA program, i.e., there should not be an effect of treatment in the future on current outcomes (e.g., Abbring and Van den Berg, 2003). To check the "no-anticipation" assumption, we conduct placebo tests by shifting the treatment date by one year before the event year. Figure 5a shows that the no-anticipation effect assumption is plausible for employment as the treatment effects in the post-treatment periods are not strongly significant. The impact of phase 1 is statistically and economically insignificant (panel (a)). For phase 2, the treatment effect is negative; however, these employment effects are weakly significant at the 5-10% level. Figures 5b and 5c suggest that the no-anticipation assumption holds for wage and markdown, respectively.[24]

Figure 5: Test of No Anticipation Effect Assumption

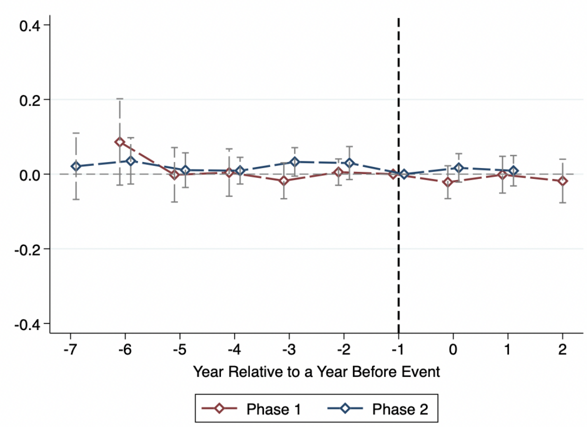
(a) $\ln L_{it}$



(b) $\ln W_{it}$



(c) $\ln \nu_{it}$



Notes: The figure reports the event study estimates from TWFE regressions testing the no anticipation effect assumption in log employment (labor headcount plus on, panel (a)), log wage (panel (b)), and log markdowns (panel (c)). The horizontal axis shows the year relative to a year before treatment, with 0 indicating a year before the treatment. The vertical axis displays the estimated treatment effects by event time. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Stable unit treatment value assumption (SUTVA). Another assumption that needs to be satisfied is the SUTVA—sometimes called a stable assignment assumption—or no spillover effects from the treatment to the control group. A treatment of NREGA through multiple phases could contaminate the comparison group. For example, treatment in early districts, i.e., phase-1 districts, could affect the labor markets in later treated districts through migration. As suggested by Imbert

[24]In Figure E.3, we also find that the treatment effects on employment, wage, and markdown in the post-treatment periods are not statistically significant when we shift back the event dates by two years. These alternative placebo checks provide credence to our test of no-anticipation assumption.

and Papp (2020b), the rural public works program reduced agricultural off-season migration from rural districts that implemented the program to districts selected to be treated but not treated yet and increased urban wages. Imbert and Papp (2020a) argue that the primary reason for this reduction in out-migration is that the cost of migration, including travel costs and income risk, explains rural-urban migration decisions.[25] It is also worth discussing the possibility of migration from urban to rural districts, i.e., potential spillover from untreated to treated districts, given the job creation in rural districts. Anyone above the age of 18 years residing in a rural area and willing to do unskilled work is eligible for obtaining a job card, a key document that records workers' entitlements under NREGA. Those who satisfy these eligibility requirements can orally request or submit a written application to the local Gram Panchayat Office to get a job card. Adult members of registered households whose names appear on the job card are then entitled to apply for NREGA work. Migrants from urban districts who are members of unregistered households are thus ineligible to work on NREGA projects. Therefore, spillover from urban to rural districts through migration is unlikely and should be negligible if it exists.

A potential decline in seasonal out-migration from rural to urban during agriculture off-season might serve as a mechanism for the spillover effect from the treated rural to untreated urban districts, especially in the agricultural industry. However, it might also have an indirect implication on the manufacturing industry. According to the ASI data, our treatment and control groups include urban and rural manufacturing firms. So, we consider that our setting is not like Imbert and Papp's (2020b). However, to avoid the potential bias of contamination from early-treated districts to later-treated districts (or untreated districts by the time of early treatment), we first exclude any later-treated districts during our study period from the control group. Hence, the control group consists of never-treated districts only. Second, we also omit never-treated districts surrounded by treated districts from the control group and report the results from this analysis as a robustness check in Section 8. Figure E.4 depicts the two alternative settings where never-treated or phase-3 districts surrounded by the treatment districts have been removed.[26]

7 Results

In this section, we first present the results from estimating the average impacts of India's public works program on manufacturing labor markets. Second, we examine the heterogeneous effects. Third, we extend our analysis with heterogeneous workers and estimate the heterogeneous impacts, focusing on firms' labor productivity and wage distribution.

[25] Other studies also provide different results regarding the impact of NREGA on rural out-migration, such as Das (2015), that suggests that the program implementation in West Bengal, India, had no significant effect on migration decision.

[26] We considered dropping never-treated districts that neighbor the treated districts; however, we decided to omit only those surrounded districts since dropping neighboring districts would have left very few never-treated districts in our control group.

7.1 Average Effects

The average effects of the NREGA program on labor market outcomes are reported in Table 6. Before discussing the effect of the NREGA program on monopsony power measured by wage markdowns, we consider the "first-stage" effects of the labor supply shock on employment. Panel A presents the results from estimating equation (12) for log employment using five separate specifications wherein more controls are added successively. The basic model in Column (1) includes year and firm fixed effects and is estimated using the ASI sample on which the markdown was estimated. It shows that the program that guaranteed employment, mostly in agriculture and construction industries, undermined employment in manufacturing. However, the estimated average effect is not statistically significant. In Column (2), we add selected firm characteristics and rainfall shocks that likely affect labor market conditions in manufacturing. The point estimate is still negative and statistically insignificant.[27] In Columns (3) and (4), we add industry-by-year and state-by-year fixed effects to further control for other factors affecting the labor market conditions. There are no significant changes in the estimates of employment effect when we include these additional fixed effects; however, the signs of the described coefficients remain the same. Finally, in our main specification, shown in Column (5), we include state-industry-year fixed effects that capture the state- and industry-specific factors such as minimum wage changes. The coefficient estimate is still insignificant when we control for these detailed fixed effects. Despite this, the negative impact of the NREGA on manufacturing employment almost doubled in magnitude.

Since NREGA provides an alternative source of employment and expands workers' outside options, the program might reduce employer power. However, we find the opposite impact, i.e., manufacturing firms' labor market power increased in NREGA districts despite the statistically insignificant estimate in our baseline analysis. Similarly, Brooks et al. (2021a) show that NREGA is associated with higher markdowns in manufacturing for their extensive margin measure of NREGA based on the number of job cards. Although they found a negative relationship between markdown and their intensive margin measure of NREGA based on the total per capita labor expenditure, the positive and statistically significant relationship between markdown and NREGA job cards was more persistent across alternative markdown measures estimated under different functional forms. Their focus was the link between India's Golden Quadrilateral (GQ) expressway expansion initiative and markdown in manufacturing, and their estimate on the NREGA-markdown link is non-causal. However, our estimated markdown effect is generally consistent. The positive association between NREGA job cards and markdown that Brooks et al. (2021a) estimated was weakly significant at the 10% level when they used markdown estimated under the De Loecker and Warzynski (2012) method. However, the statistical significance of the relationship slightly increased to a 5% level for

[27] Table E.1 presents the detailed results, reporting the effects of controls. The firm's age is associated with higher employment, suggesting an intuitive positive relationship between the firm's age and size. The wage-squared is negatively correlated with employment, but the magnitude of the coefficient is negligible compared to the coefficient on the firm's age. The rainfall shock does not affect hiring in the manufacturing industry, which is strongly consistent with Kaur (2019), who suggests a null effect of rainfall on employment in the non-agricultural sector in rural India.

Table 6: Average Effects of NREGA

	(1)	(2)	(3)	(4)	(5)
			Panel A. $\ln L_{it}$		
Post-NREGA	-0.018	-0.018	-0.006	-0.012	-0.022
	(0.019)	(0.019)	(0.016)	(0.020)	(0.020)
N	73997	72924	72924	72923	72394
R^2	0.96	0.96	0.96	0.96	0.97
			Panel B. $\ln \nu_{it}$		
Post-NREGA	0.036*	0.035*	0.015	0.005	0.000
	(0.021)	(0.021)	(0.020)	(0.016)	(0.018)
N	73997	72924	72924	72923	72394
R^2	0.87	0.87	0.87	0.87	0.89
			Panel C. $\ln W_{it}$		
Post-NREGA	-0.007	-0.007	-0.001	-0.004	0.000
	(0.015)	(0.015)	(0.014)	(0.015)	(0.014)
N	70094	69125	69125	69124	68584
R^2	0.90	0.90	0.91	0.91	0.91
			Panel D. $\ln MRPL_{it}$		
Post-NREGA	0.028*	0.028	0.014	0.001	-0.001
	(0.017)	(0.017)	(0.018)	(0.021)	(0.020)
N	70094	69125	69125	69124	68584
R^2	0.87	0.87	0.88	0.88	0.89
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
Controls		✓	✓	✓	✓
Industry-Year FE			✓	✓	
State-Year FE				✓	
State-Industry-Year FE					✓

Notes: The table presents the OLS results from estimating the effect of NREGA on log employment (labor headcount plus one, Panel A), log markdowns (Panel B), log wage (Panel C), and log MRPL (Panel D) in manufacturing between 2000 and 2008. The plant-level markdowns are estimated under the assumption of a translog production function. The marginal revenue product of labor (MRPL) was computed by multiplying the wage by the markdown. All regressions include an unreported constant term and baseline controls, including firm age, age-squared, and district-level rainfall. The industry fixed effects include dummies for the two-digit NIC industry classification. Standard errors clustered at the district level are in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

alternative markdown measures. This could be consistent with our statistically insignificant impact on the markdown measure estimated in the spirit of De Loecker and Warzynski (2012).

We now discuss the indirect effect of the non-manufacturing employment guarantee program that weakly reduced employment in manufacturing, as found above on monopsony in the manufacturing industry, which is our primary labor market outcome. Panel B presents the results from estimating the similar regressions above. As shown in Column (1), NREGA increases the markdown in manufacturing, and the coefficient estimate is statistically significant at the 10% level. In Column (2), where firm-level characteristics and rainfall shock are added, the coefficient estimate on the treatment is positive and statistically significant at the 10% level.[28] Then, once we include industry-by-year and state-by-year fixed effects in Columns (3) and (4), NREGA's markdown-increasing impact disappears, although the coefficient estimate is still positive. The program's positive impact on markdown that is statistically insignificant remains the same in our preferred model, but the magnitude of the coefficient substantially drops (Column (5)).

To better understand the markdown effect of the program, we examine how the program affected other labor market outcomes in manufacturing, including wages and the marginal revenue product of labor (MRPL). Panel C shows the estimated wage effects. Our main specification in Column (5) suggests that manufacturing wages also did not respond to the policy change. Since markdown is essentially the ratio of the MRPL and wage, we compute the MRPL using our baseline markdown estimate and wage information from the data. Panel D presents the results on NREGA's impact on MRPL, and the effect is not significant either, which is consistent with the null effects on markdown and wage. The detailed results are shown in Table E.3.

The baseline estimates of employment and wage effects are generally consistent with Agarwal et al. (2021), who estimated a weakly significant crowding-out impact on manufacturing employment, concentrated among regular workers, and null effects on wages at private manufacturers in India.[29] Although the NREGA treatment, time and industry coverage, and firm-level data are different, we also compare our baseline results with Muralidharan et al. (2023), who suggest that randomized experiment of NREGA in Andhra Pradesh increases employment, wage, and workers' bargaining power at private enterprises in non-agricultural industries using the sixth round of Economic Census of India in 2013. Since our baseline results based on manufacturing firms in the ASI data from 2000-2008 differ from their findings, we estimated our baseline specifications on a similar, but not the same, sample that consists of private manufacturers in Andhra Pradesh. Panel A of Table E.7 presents the results, showing that NREGA consistently increased employment and wages, although the effects are not statistically significant. These effects are robust to using the full (left panel) and the markdown (right panel) samples. The employment could have significantly increased in other non-agricultural and non-manufacturing industries, such as construction, where NREGA offers jobs. Manufacturing jobs are less likely to increase but more likely to decrease since NREGA tends to offer non-manufacturing work. For labor market power, we find that markdown at

[28] As reported in Table E.2, the firm's age is associated with lower markdowns, confirming our descriptive finding on the age-markdown relationship in Figure 2b, and the negative correlation is significant at the 1% level. The age-squared is positively associated with the markdown. However, similar to the employment regression above, the magnitude of the coefficient is negligible compared to the coefficient on the firm's age. The rainfall shock, on the other hand, has no impact on manufacturing firms' markdown, although the sign of the coefficient is negative.

[29] Our qualitative results remain the same when using the sample of private manufacturing firms.

private manufacturers in Andhra Pradesh increases, contrary to Muralidharan et al. (2023), despite the positive wage effect; however, the coefficient estimates on markdown and wages are statistically insignificant. This deviation in markdown effects could be due to differences in our settings, such as industry composition.

7.2 Heterogeneous Effects

The baseline results suggest null effects of the first two phases of NREGA on manufacturing labor markets, so we further examine its impact by estimating heterogeneous effects. We consider four types of heterogeneity given the nature of NREGA policy: (i) labor productivity, (ii) labor intensity, (iii) average wage, (iv) gap from minimum wage, (v) industry in which the firm operates, and (vi) urban/rural status. However, the focus is on heterogeneity in the firm's labor productivity before the policy change because the program generates vacancies for unskilled workers or workers with relatively low productivity.

Labor productivity. We define labor productivity as sales revenue per worker at the manufacturing firm and use the most recent level of labor productivity before phase 1 of the program, i.e., the 2005-2006 financial year level. Then, we estimate heterogeneous effects by interacting the treatment variable with a dummy variable, indicating whether the firm's labor productivity is below the median. Table 7 reports the results.[30] As shown in Panel A, employment falls for below-median productivity firms, remarkably consistent with Agarwal et al. (2021). It suggests a crowding-out effect for such firms, which we fail to identify in our baseline analysis that ignores the heterogeneity. Panel B then shows the markdown at these firms that experienced employment reduction increased due to the NREGA, which is the innovation we offer to the literature, showing what happens to workers' bargaining power. As the policy provides an alternative source of employment, it could have reduced monopsony power by improving workers' outside options and raising the labor supply elasticity. However, the result suggests that markdown increased at manufacturers that lost their workers.

We then consider the heterogeneous effects on wages and MRPL. The null effect on average wage implies a sluggish wage (Panel C). Although not directly related, this result is generally consistent with Kaur's (2019) results suggesting a sticky wage in India's agriculture industry. With no wage impact and markdown-increasing effect, the MRPL at firms with low labor productivity should have increased by construction. Consistent with the definition, Panel D shows that MRPL at such firms increased due to the program.

Next, we estimate the event study regressions in equation (13) to illustrate the heterogeneous impacts and determine which phase drives the aggregate treatment effect by disentangling the impacts of NREGA's first two phases. The event study regressions suggest the same heterogeneous effects

[30] Tables E.4-E.6 present the detailed results showing the effects of individual terms of the interaction.

Table 7: Heterogeneous Effect of NREGA by Labor Productivity

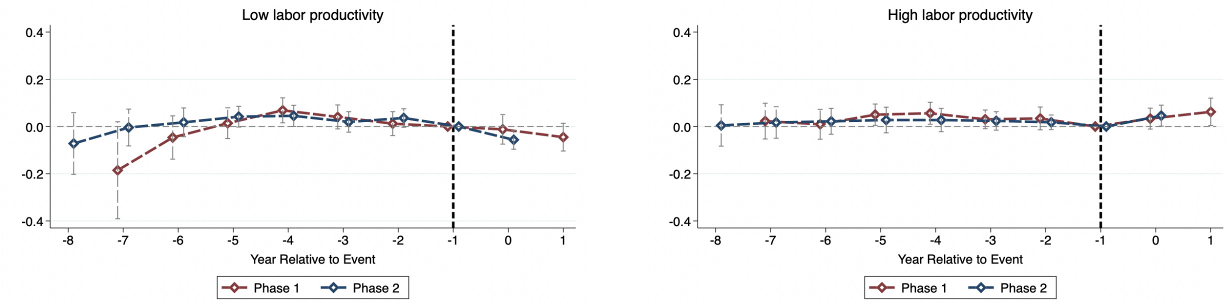
	(1)	(2)	(3)	(4)	(5)
	Panel A. $\ln L_{it}$				
Post-NREGA × Below median	-0.125***	-0.128***	-0.098***	-0.100***	-0.101***
	(0.023)	(0.022)	(0.019)	(0.019)	(0.019)
N	73511	72454	72454	72453	71921
R^2	0.96	0.96	0.96	0.96	0.97
	Panel B. $\ln \nu_{it}$				
Post-NREGA × Below median	0.106***	0.109***	0.119***	0.101***	0.086***
	(0.022)	(0.022)	(0.021)	(0.023)	(0.022)
N	73511	72454	72454	72453	71921
R^2	0.87	0.87	0.87	0.87	0.88
	Panel C. $\ln W_{it}$				
Post-NREGA × Below median	-0.032**	-0.031**	-0.021	-0.014	-0.018
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
N	69648	68695	68695	68694	68151
R^2	0.90	0.90	0.91	0.91	0.91
	Panel D. $\ln MRPL_{it}$				
Post-NREGA × Below median	0.074***	0.077***	0.104***	0.096***	0.074***
	(0.025)	(0.025)	(0.022)	(0.022)	(0.025)
N	69648	68695	68695	68694	68151
R^2	0.87	0.87	0.88	0.88	0.89
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
Controls		✓	✓	✓	✓
Industry-Year FE			✓	✓	
State-Year FE				✓	
State-Industry-Year FE					✓

Notes: The table presents the OLS estimates on the heterogeneous effect of NREGA on log employment (labor headcount plus one, Panel A), log markdowns (Panel B), log wage (Panel C), and log MRPL (Panel D) in manufacturing by labor productivity (sales revenue per labor) between 2000 and 2008. The plant-level markdowns are estimated under the assumption of a translog production function. The marginal revenue product of labor (MRPL) was computed by multiplying the wage by the markdown. All regressions include an unreported constant term, individual terms of the interaction, and baseline controls. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

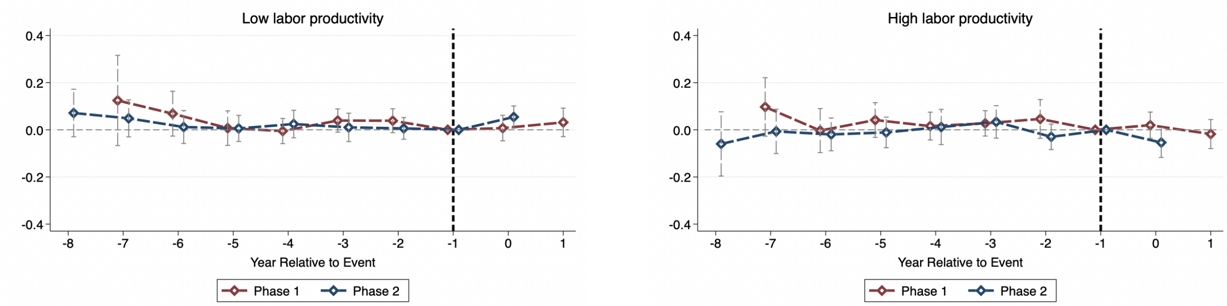
from the DID model above, and the impacts are mainly driven by the second phase of the program (Figure 6). Specifically, employment at firms with low labor productivity decreases while markdown, wage, and MRPL grow at such firms (left panels). For firms with high labor productivity, the treatment increases employment and does not strongly affect the other labor market outcomes (right panels).

Figure 6: Heterogeneous Effects by Labor Productivity

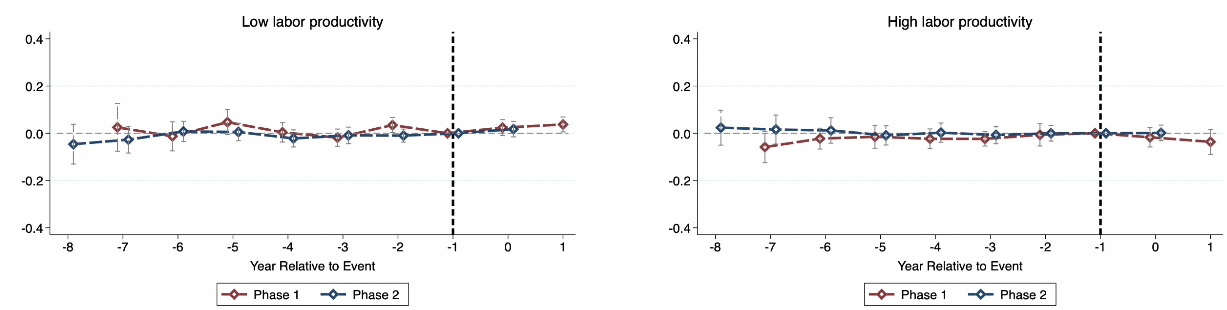
(a) $\ln L_{it}$



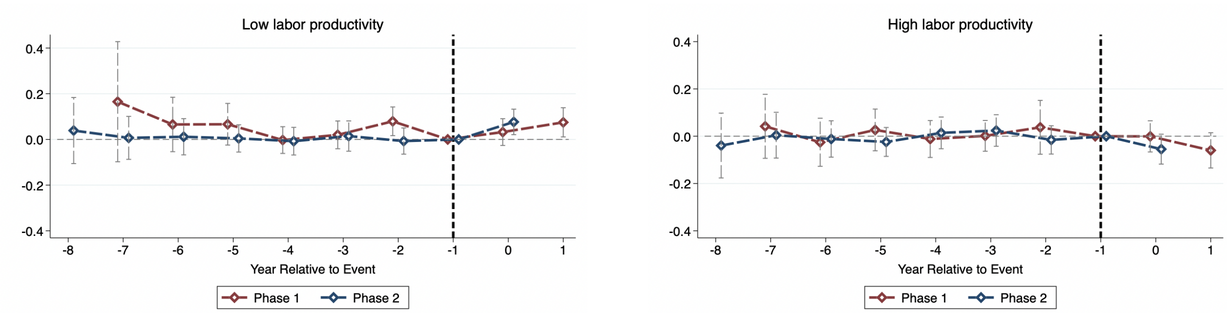
(b) $\ln \nu_{it}$



(c) $\ln W_{it}$



(d) $\ln MRPL_{it}$



Notes: The figure reports the event study estimates from TWFE regressions estimating the heterogeneous effects of NREGA on log employment (labor headcount plus one, panel (a)), log markdowns (panel (b)), log wage (panel (c)), and log MRPL (panel (d)) in manufacturing by labor productivity. The sample in the left (right) panels consists of firms whose labor productivity, measured by sales revenue per labor, is below (above) the median in the most recent period before the first phase of NREGA. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are shown.

Muralidharan et al. (2023) do not estimate the heterogeneous impacts by labor productivity. However, similar to the previous section, we also estimate the specifications heterogeneous by labor productivity based on private manufacturers in Andhra Pradesh. The heterogeneous impacts, shown in Panel B of Table E.7, are generally the same as average effects among these firms, except for employment effects in Columns (1) and (3). The crowding-out effect of NREGA on manufacturing employment is statistically insignificant in the markdown sample, potentially due to the small number of observations. The wage and MRPL effects are opposite among private firms in Andhra Pradesh compared to all manufacturers in all treatment states.

Labor intensity. The NREGA policy is a labor supply shock in manufacturing since it generates guaranteed employment opportunities primarily in the non-manufacturing industries. As the policy creates job positions outside of the manufacturing industry, it could crowd out employment in manufacturing. Thus, manufacturers who use human labor intensively in their production are likely adversely affected by this negative employment shock. Additionally, labor-intensive firms are likely to have less productive workers on average, at least by construction. So, the program that creates low-paying and less productive jobs might affect firms that use labor more intensively than capital. Studies on labor market implications of trade shocks also examine the impacts heterogeneous by labor intensity, such as Ahsan and Mitra (2014), who find positive effects of trade liberalization on labor share in small and more labor-intensive firms but negative effects in large and less labor-intensive firms in India. We thus investigate the NREGA's labor market effects heterogeneous by labor intensity, and the results shown in Table 8 suggest that the impacts are concentrated among more labor-intensive firms. This finding is consistent with our results above on heterogeneous impacts by labor productivity.

Table 8: Heterogeneous Effects of NREGA by Labor Intensity

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
Post-NREGA × Above median	-0.087***	0.083***	-0.016	0.066**
	(0.018)	(0.024)	(0.015)	(0.026)
Above median	0.023	0.010	0.004	0.011
	(0.017)	(0.023)	(0.012)	(0.022)
Post-NREGA	0.016	-0.035*	0.006	-0.027
	(0.022)	(0.021)	(0.017)	(0.024)
N	71921	71921	68151	68151
R^2	0.97	0.88	0.91	0.89

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes in manufacturing by labor intensity (labor-to-capital ratio). The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating that the firm's labor intensity measure is above the median, i.e., the firm is labor intensive. The dependent variable in Columns (1)-(4) is the log employment, log markdowns, log wage, and log MRPL, respectively. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$.

Wage distribution. To better understand which firms and workers are affected by the policy change, we conduct a heterogeneity across the distribution of the firm's average wage per worker. Table 9 reports the estimated heterogeneous effects on labor market outcomes. The NREGA jobs can be less attractive to workers at high-paying firms, so workers at low-wage firms are likely to respond to the policy shock by leaving their current employers. Consistent with this mechanism, we find that employment at low-paying firms decreases relative to high-paying firms (Column (1)), although the effect is statistically significant at the 10% level. As less productive workers with low wages leave these firms with low compensation before the shock, the average wage (Column (3)) and marginal productivity of workers (Column (4)) at such firms increase due to the changes in their employment structure. Markdowns do not change at these firms with relatively low average wages (Column (2)) since both wage and MRPL increase with close magnitudes.[31] The heterogeneous labor market effects along the average wage distribution are consistent with the heterogeneous impacts by labor productivity and labor intensity described above.

Table 9: Heterogeneous Effects of NREGA along the Wage Distribution

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
Post-NREGA × Low-wage dummy	-0.055**	0.041	0.030*	0.073***
	(0.023)	(0.025)	(0.016)	(0.028)
Low-wage dummy	0.007	0.025	-0.050***	-0.026
	(0.015)	(0.018)	(0.012)	(0.021)
Post-NREGA	-0.004	-0.015	-0.006	-0.021
	(0.021)	(0.020)	(0.015)	(0.022)
N	68202	68202	67596	67596
R^2	0.97	0.89	0.91	0.89

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes along the wage distribution. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial average wage per worker is in the bottom quartile of the wage distribution. The dependent variable in Columns (1)-(4) is the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL, respectively. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Minimum wage. We examine heterogeneous impacts around the minimum wage level because the state government sets wages of the NREGA jobs at the state's minimum wage level. Thus, NREGA jobs are likely attractive for manufacturing workers whose wages are under the minimum wage level. Such workers are incentivized to leave their current employer in response to the program, or employers would need to increase their wages to keep them. Table 10 shows the estimated

[31] Although the results are not reported, these findings on the labor market dynamics in low-paying firms relative to high-paying one are remarkably robust to other definitions of low-paying firms based on different splits of the distribution: bottom quintile, bottom tercile, and bottom three deciles.

heterogeneous labor market effects for firms with a low wage-to-minimum wage ratio relative to a high ratio. Consistent with the stated mechanism, the results suggest that the effects are concentrated among firms whose average wage is below the minimum wage. The effects are particularly strong for manufacturing plants in the bottom part of the distribution of the wage-to-minimum wage ratio. The ratio of the firm's average wage to the state's minimum wage in the bottom quartile of the distribution ranges from 0 to 1.0, i.e., the average wage of these establishments is lower than the minimum wage. This result is also consistent with the effects concentrated among low-paying firms.[32]

Table 10: Heterogeneous Effects of NREGA along the Distribution of Wage-to-Minimum Wage Ratio

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
Post-NREGA × Low W-to-MW dummy	-0.081*	0.097*	0.049	0.142**
	(0.043)	(0.055)	(0.032)	(0.058)
Low W-to-MW dummy	-0.019	0.015	0.018	0.015
	(0.047)	(0.057)	(0.020)	(0.052)
Post-NREGA	-0.019	-0.040	0.000	-0.046
	(0.036)	(0.036)	(0.022)	(0.038)
N	23075	23075	22750	22750
R^2	0.96	0.86	0.92	0.86

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes along the distribution of wage-to-minimum wage ratio. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial average wage-to-minimum wage ratio is in the bottom quartile of the distribution. The dependent variable in Columns (1)-(4) is the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL, respectively. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

The heterogeneous effects of NREGA around minimum wage can be different depending on the enforcement of minimum wage. The labor market effects concentrated at manufacturing plants whose average compensation is below the minimum wage are likely to be more significant because NREGA wages set at the state's minimum wage level will be less attractive if the minimum wages are not enforced or the NREGA jobs will be paid lower than the promised level. So, NREGA jobs will be less desirable to manufacturing workers in states with less enforced minimum wages, and the crowding-out effects on manufacturing employment and subsequent impacts on other labor market outcomes are expected to be weaker in such states.

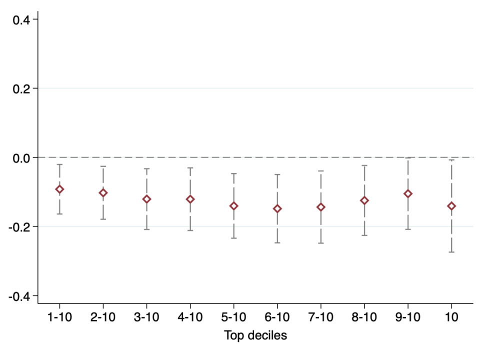
Figure 7 illustrates the estimated effects of the public works program on labor market conditions in the manufacturing industry heterogeneous by minimum wage and its enforcement. Consistent

[32]We do not report the results, but the findings are generally the same for different cuts of the distribution, including bottom three deciles and bottom tercile.

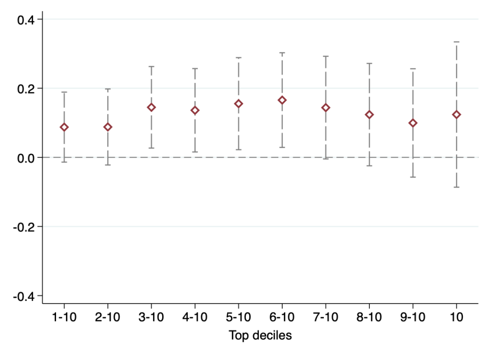
with the expectation, the employment, wage, and marginal productivity impacts at manufacturers whose average salary is below the minimum wage are generally higher with stricter enforcement, with some nonlinearity. The effects are most significant in magnitude and statistical significance among the firms in states with moderate enforcement in the sixth or seventh deciles and are weaker in states with the least and strictest enforcement. The weaker responses in states with the most stringent enforcement could be due to our measure of minimum wage enforcement because high inspection could also imply a lack of compliance with minimum wage laws. Appendix D.1 shows that these results heterogeneous by minimum wage enforcement are robust leveraging different parts of the distributions of wage-to-minimum wage ratio and minimum wage enforcement.

Figure 7: Heterogeneous Effects of NREGA by Minimum Wage and Its Enforcement

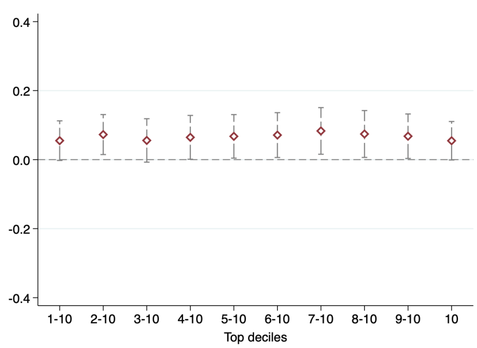
(a) $\ln L_{it}$



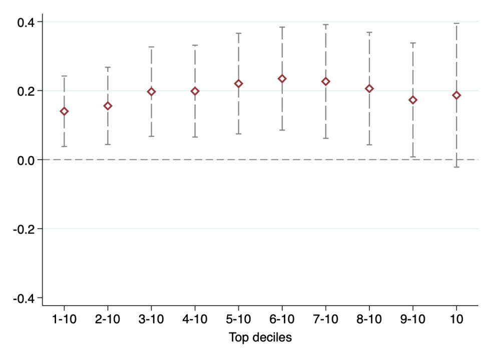
(b) $\ln \nu_{it}$



(c) $\ln W_{it}$



(d) $\ln MRPL_{it}$

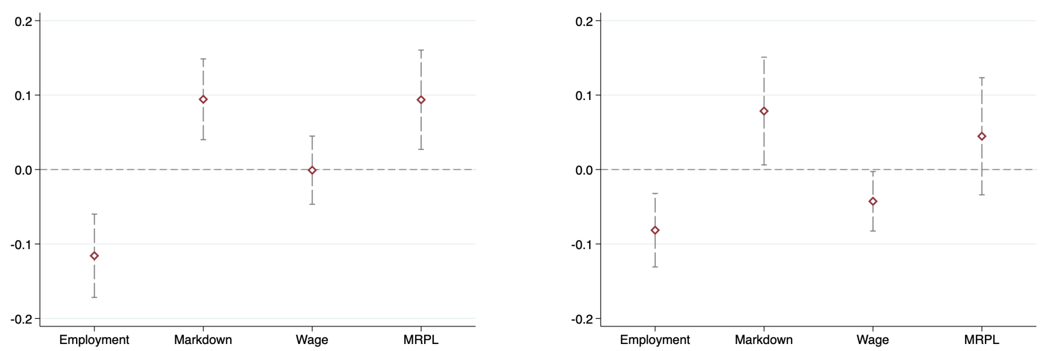


Notes: The figure presents the effects of NREGA on labor market outcomes at manufacturing firms heterogeneous by minimum wage and its enforcement. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial average wage-to-minimum wage ratio is in the bottom 3 deciles. The dependent variable in panels (a)-(d) is the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL, respectively. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. Each point estimate comes from separate regressions on samples that consist of firms in the different deciles of the minimum wage enforcement (inspections per worker) distribution. For example, the sample of firms labeled "1-10" refers to those in the 1st through the 10th deciles of the distribution, i.e., all firms, and the sample labeled "2-10" refers to a sub-sample of firms in the 2nd through the 10th deciles. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are presented.

Top five industries. Finally, we investigate the industries driving these results. We determine the top five industries with the highest sales in the pre-treatment period. The composition of dominating industries has been stable over time, with (i) chemicals and chemical products, (ii) textiles, (iii) coke, refined petroleum products, and nuclear fuel, (iv) basic metals, and (v) food products and beverages being the five industries with the highest revenue from 2000-2003. Since then, an industry of motor vehicles, trailers, and semi-trailers has taken over an industry of coke, refined petroleum products, and nuclear fuel, and the composition has been unchanged even after the NREGA program. The order of these industries has also remained relatively the same. Using sub-samples of manufacturing firms in the top five and other industries by the rank of sales revenue in 2006, we re-estimate the heterogeneous effects and find that the estimated effects are concentrated in top industries with the highest sales. Figure 8 shows that the impacts heterogeneous by labor productivity identified in Table 7 are mainly concentrated among firms in the top five industries.[33,34]

Figure 8: Heterogeneous Effects of NREGA by Labor Productivity across Industries

(a) Top five industries | (b) Other industries



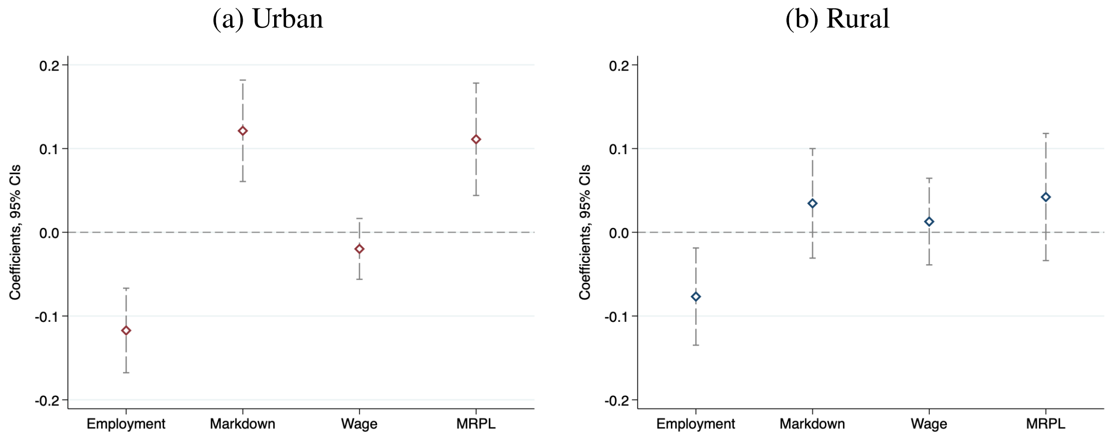
Notes: The figure depicts the effects of NREGA on labor market outcomes heterogeneous by labor productivity at manufacturing firms in the top five and other industries. The top-five industries in panel (a) are those with the highest sales revenue in the pre-NREGA period, 2006, and include (i) chemicals and chemical products, (ii) basic metals, (iii) textiles, (iv) motor vehicles, trailers, and semi-trailers, and (v) food products and beverages. Other industries in panel (b) include those remaining two-digit NIC industries. The key explanatory variable plotted is the NREGA treatment variable interacted with a dummy, indicating whether the firm's labor productivity (sales revenue per labor) is below the median. The dependent variables shown in the horizontal axis include the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are presented.

[33] Figures E.5-E.7 present the heterogeneous effects by labor intensity, wage distribution, and wage-to-minimum wage ratio, respectively, in the top five and other industries. The results persistently suggest that the heterogeneous effects are concentrated among firms in the top five sectors with the highest sales revenue.

[34] Using the number of workers instead of sales revenue to rank the industries provides qualitatively similar findings, showing that the effects are more significant in the top industries. For example, Figure E.8 presents the heterogeneous labor market effects of NREGA by labor productivity in the top five and other industries. The composition of industries with the most workers has been stable and relatively similar to the top industries by sales revenue. The top five industries by employment in the pre-NREGA period, 2006, are (i) textiles, (ii) food products and beverages, (iii) chemicals and chemical products, (iv) tobacco products, and (v) basic metals.

Urban and rural firms. The NREGA guarantees rural employment; however, the districts, the level where we defined the treatment and control groups, include both rural and urban areas.[35] However, the plant-level data from the ASI contains information on whether the manufacturing firm is urban or rural. Based on this information, we estimate heterogeneous impacts by labor productivity among urban and rural firms. The heterogeneous effects by labor productivity are more significant for urban firms (Figure 9).

Figure 9: Heterogeneous Effects of NREGA for Urban and Rural Firms by Labor Productivity



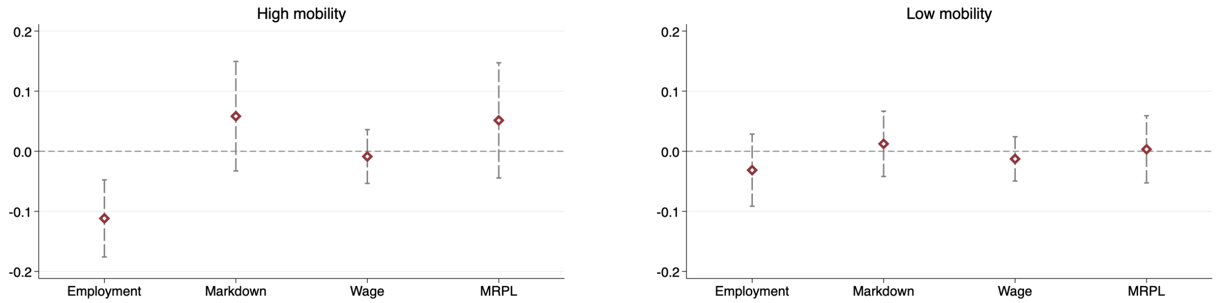
Notes: The figure presents the heterogeneous effects of NREGA on labor market outcomes at manufacturing firms with low labor productivity relative to those with high labor productivity in urban and rural areas. In the left (right) panel, the sample consists of urban (rural) firms. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial labor productivity is below the median. The dependent variables shown in the horizontal axis include the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are presented.

Labor mobility. Given the predictions from our model, we estimate the heterogeneous effects by labor or worker mobility. Internal migration across states is low in India, while internal migration across districts within the same state is relatively more prevalent (Munshi and Rosenzweig, 2016; Kone et al., 2018; Nayyar and Kim, 2018). So, we split the districts into two groups: those with high (low) worker mobility or pre-treatment total migrants-to-population ratio above (below) the national median. Figure 10 illustrates the heterogeneous effects of NREGA by firm's urban status (panel (a)) and labor productivity (panel (b)) in districts with high and low degrees of worker mobility. The labor market effects at urban-low labor productivity firms tend to be more pronounced in high mobility districts; however, it is hard to draw a clear conclusion.

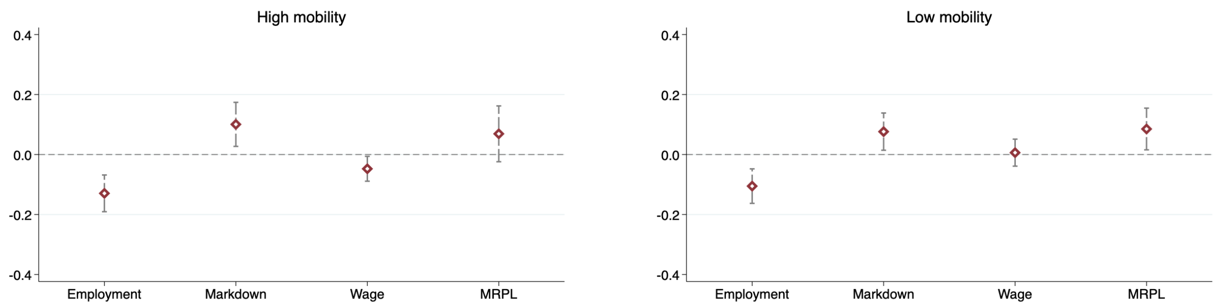
[35]The origins of administrative urban and rural settlements in India can be found in Hiranandani et al. (2024), while Tandel et al. (2019) discuss the definition of urban and rural areas according to NREGA.

Figure 10: Heterogeneous Effects of NREGA in Districts with High and Low Worker Mobility

(a) Heterogeneous by urban/rural



(b) Heterogeneous by labor productivity

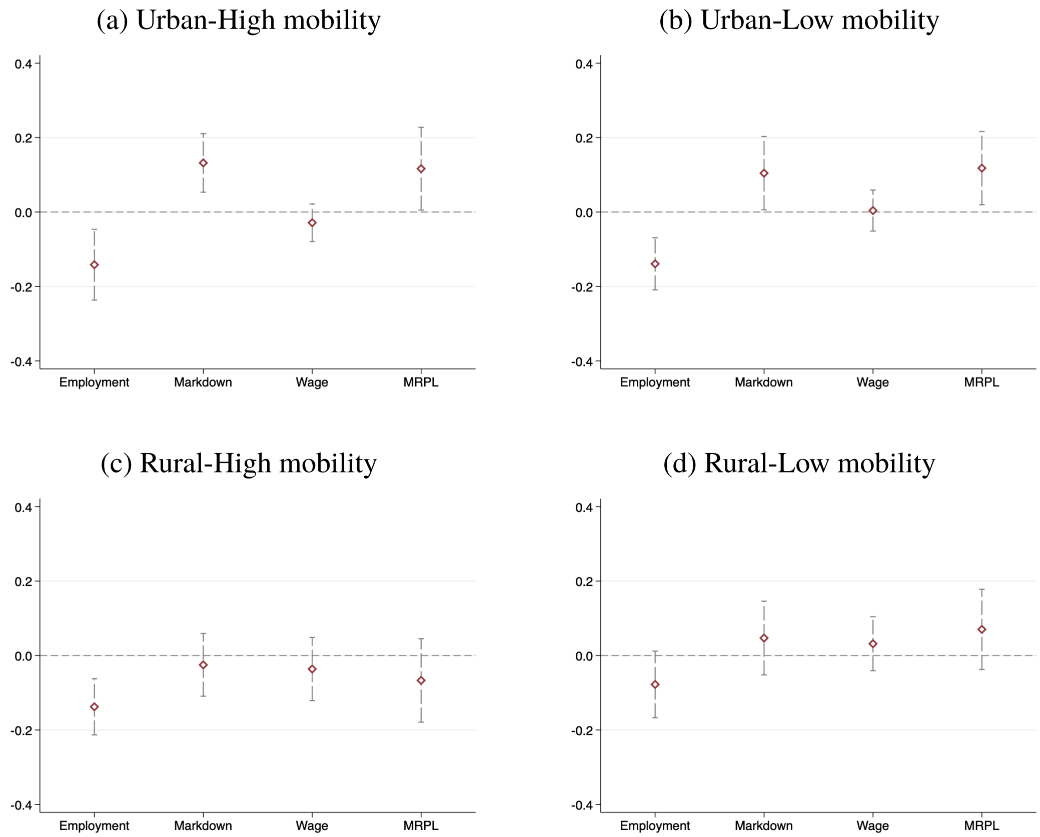


Notes: Panel (b) shows the heterogeneous effects of NREGA by firm's urban status in sub-samples of districts with high and low worker mobility. The key explanatory variable plotted is the Post-NREGA treatment variable interacted with the firm's urban dummy. Panel (c) presents the heterogeneous effects of the program by labor productivity in districts with high and low worker mobility. The key explanatory variable plotted is the Post-NREGA treatment variable interacted with a dummy, indicating whether the firm's initial labor productivity is below the median. The dependent variables shown in the horizontal axis include the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are presented.

We then consider heterogeneity in three dimensions at the same time, including (i) firms with low labor productivity, (ii) the firm's urban/rural status, and (iii) districts with high/low worker mobility. Figure 11 presents the results. The wage effects at firms with low labor productivity are null in all regressions. However, the employment-reducing, markdown-increasing, and MRPL-increasing impacts at firms with low labor productivity seem to be concentrated at urban firms in districts with high mobility. These results on heterogeneous impacts by worker mobility are robust to alternative measures of worker mobility based on migrants with different durations of residence. The results are available on request.

Appendix C provides additional results from estimating the treatment effects on worker turnover at the establishment.

Figure 11: Heterogeneous Effects of NREGA by Labor Productivity at Urban/Rural Firms in Districts with High and Low Worker Mobility



Notes: The figure shows the heterogeneous effects of NREGA on labor market outcomes by labor productivity estimated on four different sub-samples. The sample in panel (a)-(d) consists of (i) urban firms in districts with high worker mobility, (ii) urban firms in districts with low worker mobility, (iii) rural firms in districts with high worker mobility, and (iv) rural firms in districts with low worker mobility. The districts with high (low) worker mobility are the ones with migrants-to-population ratios above (below) the national median by 2001. The key explanatory variable plotted in all panels is the NREGA treatment variable interacted with a dummy, indicating whether the firm's labor productivity (sales revenue per labor) is below the median. The dependent variables shown in the horizontal axis include the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with headcount as a measure of labor input. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors are clustered by districts, and 95% confidence intervals are presented.

7.3 Heterogeneous Workers

We then estimate the impacts of NREGA for heterogeneous workers who differ by their skills and employment contracts, focusing on heterogeneity by labor productivity.

Production and non-production workers. We first consider worker heterogeneity by skills since NREGA generated unskilled jobs. Our firm-level data from ASI enables us to distinguish managers from other workers, and we consider non-managers and managers synonymous with production and non-production workers, respectively. Table 11 presents the labor market effects of the

Table 11: Heterogeneous Effects of NREGA on Production and Non-Production Workers by Labor Productivity

	Production workers				Non-production workers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln W_{it}$	$\ln MRPL_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln W_{it}$	$\ln MRPL_{it}$
	Panel A. Below median							
Post-NREGA	-0.083***	0.066**	0.011	0.077**	-0.027	0.043	0.001	0.043
	(0.026)	(0.030)	(0.017)	(0.030)	(0.026)	(0.039)	(0.032)	(0.037)
N	28244	28244	28241	28241	28244	28244	28228	28228
R^2	0.97	0.89	0.93	0.89	0.93	0.87	0.86	0.89
	Panel B. Above median							
Post-NREGA	0.014	-0.023	-0.011	-0.034	0.003	-0.035	-0.011	-0.046
	(0.027)	(0.031)	(0.020)	(0.036)	(0.027)	(0.041)	(0.028)	(0.031)
N	30086	30086	30084	30084	30086	30086	30080	30080
R^2	0.96	0.86	0.91	0.84	0.93	0.83	0.81	0.85

Notes: The table presents the labor market effects of NREGA for production (left panel) and non-production (right panel) workers at manufacturers with low (Panel A) and high (Panel B) labor productivity. The sample in Panel A (B) panel consists of firms whose labor productivity measured by sales revenue per labor is below (above) the median in the most recent period before the first phase of NREGA. The dependent variables are the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL for production and non-production workers. The plant-level markdowns for production and non-production workers are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with production (non-managers or low-skilled) and non-production (managers or high-skilled) workers. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$.

program for production and non-production workers heterogeneous by firm's labor productivity. The heterogeneous impacts around labor productivity that we identified above come from changes in production workers (left panel). This result is intuitive as the jobs generated at NREGA projects are for unskilled workers willing to complete manual tasks. The results show that production workers are separate from the manufacturing employer (Column (1) of Panel A), and markdowns and marginal productivity of remaining production workers increase (Columns (2) and (4)). The wage remains unchanged (Column (3)). In contrast, any of the labor market outcomes for production workers do not respond to NREGA shock for firms whose labor productivity is above the median (Panel B). For non-production workers or managers, we fail to find significant changes in any labor market outcomes (right panel).[36]

[36] In our baseline analysis, we use the sample splitting method because it clearly illustrates the heterogeneous treatment effects by labor productivity for different types of workers. We thus check the robustness of these results by using an interaction method. Table E.8 shows the results. We find that employment, markdown, and MRPL effects for production workers are the same as those in the baseline analysis. The interaction method also yields statistically significant impacts on non-production workers' employment, wage, and MRPL (Panel B). Despite these deviations from our baseline results, we highlight results only robust across different approaches of conducting heterogeneity analysis.

Although we focus on heterogeneity by firm's labor productivity, we investigate the heterogeneous impacts of the program for production and non-production workers along the wage distribution. As shown in Table 12, the labor market effects at low-paying firms found in Table 9 are concentrated among production workers, similar to heterogeneity by labor productivity above.

Table 12: Heterogeneous Effects of NREGA on Production and Non-Production Workers along the Wage Distribution

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
	Panel A. Production workers			
Post-NREGA × Low-wage dummy	-0.061**	0.054*	0.028*	0.082**
	(0.029)	(0.033)	(0.015)	(0.034)
Low-wage dummy	0.007	0.011	-0.053***	-0.042
	(0.023)	(0.028)	(0.012)	(0.030)
Post-NREGA	-0.014	0.003	-0.010	-0.006
	(0.022)	(0.022)	(0.015)	(0.023)
N	57855	57855	57853	57853
R^2	0.96	0.87	0.92	0.89
	Panel B. Non-production workers			
Post-NREGA × Low-wage dummy	0.037	-0.042	-0.002	-0.043
	(0.027)	(0.037)	(0.034)	(0.030)
Low-wage dummy	0.018	0.046	-0.055*	-0.009
	(0.026)	(0.032)	(0.029)	(0.025)
Post-NREGA	-0.021	0.034	-0.013	0.021
	(0.018)	(0.027)	(0.022)	(0.022)
N	57855	57855	57838	57838
R^2	0.93	0.84	0.84	0.88

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for production (top panel) and non-production (bottom panel) workers in manufacturing along the wage distribution. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's average wage per worker is in the first quintile of the wage distribution. The dependent variable in Columns (1)-(4) is the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL, respectively. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with production (non-managers or low-skilled) and non-production (managers or high-skilled) workers. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Regular and contract workers. Exploiting the information on workers with different employment contracts, we examine whether the program had a differential impact on labor market conditions of workers employed directly and through contractors. For these heterogeneous workers, as shown in Table 13, the labor market effects are concentrated among manufacturers with low labor productivity, consistent with our baseline heterogeneity analysis. Although the impacts on other labor market indicators are not statistically significant for regular workers, NREGA increases markdowns over regular workers at the 1% statistical significance level (left panel). The program, however, presents more pronounced labor market effects for contract workers as their markdown (Column (6)) and MRPL (Column (8)) respond to the treatment and the markdown effect is more significant in magnitude than for regular workers (right panel).[37] Given that the markdown for regular and contract workers is estimated for even fever firms mainly due to the combination of data limitation and estimation procedure, the sample used for this analysis is limited and should be cautiously interpreted.

Table 13: Heterogeneous Effects of NREGA on Regular and Contract Workers by Labor Productivity

	Regular workers				Contract workers			
	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$	(5) $\ln L_{it}$	(6) $\ln \nu_{it}$	(7) $\ln W_{it}$	(8) $\ln MRPL_{it}$
	Panel A. Below median							
Post-NREGA	-0.069	0.088	-0.013	0.107	-0.058	0.147*	0.003	0.150*
	(0.056)	(0.064)	(0.040)	(0.078)	(0.082)	(0.083)	(0.031)	(0.087)
N	7710	7710	5645	5645	7710	7710	7709	7709
R^2	0.98	0.88	0.91	0.89	0.92	0.90	0.87	0.92
	Panel B. Above median							
Post-NREGA	-0.010	0.029	0.032	0.071	0.063	-0.054	-0.001	-0.053
	(0.026)	(0.040)	(0.028)	(0.048)	(0.066)	(0.059)	(0.030)	(0.049)
N	8931	8931	8624	8624	8931	8931	8911	8911
R^2	0.97	0.85	0.90	0.85	0.86	0.84	0.73	0.85

Notes: The table presents the labor market effects of NREGA for regular (left panel) and contract (right panel) workers at manufacturers with low (Panel A) and high (Panel B) labor productivity. The sample in Panel A (B) consists of firms whose labor productivity measured by sales revenue per labor is below (above) the median in the most recent period before the first phase of NREGA. The dependent variables are the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL for regular and contract workers. The plant-level markdowns for regular and contract workers are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with regular and contract workers. Regular workers are employed directly, while contract workers are hired through contractors. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

8 Robustness

This section performs several robustness checks of our main results by (i) using a sample splitting method as an alternative approach to conduct heterogeneity analysis, (ii) leveraging the entire ASI sample that is available before estimating the markdown, (iii) employing worker-days as an alternative measure of labor inputs, and (iv) using alternative control groups.

[37] In Table E.9, we present the heterogeneous effects based on an interaction method, which suggests that the labor market impacts for contract workers in the manufacturing industry are essentially zero.

8.1 Heterogeneity by Sample Splitting

Heterogeneous effects can be estimated using interaction and sample-splitting methods. Sample splitting allows all the coefficients to differ across sub-samples, whereas an interaction method allows only the interacted variables to differ. Despite the differences and although the interaction method is generally preferred, we test the robustness of our results on heterogeneous effects by using the sample splitting approach as our baseline analysis with homogeneous workers leverages the interaction approach. Table 14 reports the results.

Table 14: Heterogeneous Effect of NREGA by Labor Productivity (Sub-Sampling Method)

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
	Panel A. Below median			
Post-NREGA	-0.061**	0.026	0.021	0.047*
	(0.028)	(0.022)	(0.014)	(0.025)
N	30992	30992	30992	30992
R^2	0.97	0.90	0.92	0.89
	Panel B. Above median			
Post-NREGA	0.022	-0.024	-0.010	-0.034
	(0.024)	(0.027)	(0.021)	(0.032)
N	35210	35210	35210	35210
R^2	0.96	0.89	0.90	0.86

Notes: The table presents the OLS estimates on the effect of NREGA on log employment (labor headcount plus one, Column (1)), log markdowns (Column (2)), log wage (Column (3)), and log MRPL (Column (4)) in manufacturing for firms with low (Panel A) and high (Panel B) labor productivity. The sample in Panel A (B) consists of firms whose labor productivity measured by sales revenue per labor is below (above) the median. The plant-level markdown is estimated under the assumption of a translog production function between 2000 and 2008. The marginal revenue product of labor (MRPL) is calculated by multiplying the wage by the markdown. All regressions include an unreported constant term and baseline controls. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

First, the program leads to a labor shortage for firms with low labor productivity. However, the policy did not affect employment for firms with highly productive workers (Column (1)). Second, despite being noisily estimated, the coefficient estimate on wage markdowns for firms with low labor productivity is positive, while it is negative for firms with high labor productivity (Column (2)). Similar to our baseline results, Column (3) shows that the policy change has no impact on wages at firms with low and high productivity. Finally, the MRPL impact for firms with labor productivity below the median is positive and statistically significant at the 10% level (Column (4)). Overall, results from regressions estimated on sub-samples of firms suggest that our baseline heterogeneous effects are generally robust to an alternative method.

8.2 Employment and Wage Effects on the Full Sample

In our baseline analysis, we use the ASI sample on which the markdown was estimated since the markdown is the outcome of our interest. The markdown was estimated on about one-third of the full ASI sample. We thus check whether our key findings are due to a specific sample or stay the same on the full ASI sample. We test this robustness for the employment and wage effects because markdowns and MRPL are not estimated on the full sample. The focus is on our main results on heterogeneity by labor productivity, and Table 15 presents the results.

Table 15: Heterogeneous Effect of NREGA on Employment and Wage by Labor Productivity (Full Sample)

	(1)	(2)	(3)	(4)	(5)
			Panel A. $\ln L_{it}$		
Post-NREGA × Below median	-0.146***	-0.146***	-0.135***	-0.130***	-0.134***
	(0.018)	(0.018)	(0.017)	(0.016)	(0.016)
N	225808	221566	221566	221566	221215
R^2	0.95	0.95	0.95	0.95	0.95
			Panel B. $\ln W_{it}$		
Post-NREGA × Below median	-0.011	-0.011	-0.010	-0.007	-0.003
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)
N	196160	192520	192520	192520	192203
R^2	0.87	0.87	0.87	0.87	0.87
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
Controls		✓	✓	✓	✓
Industry-Year FE			✓	✓	
State-Year FE				✓	
State-Industry-Year FE					✓

Notes: Panel A presents the OLS estimates on the heterogeneous effect of NREGA on log employment (labor headcount plus one) in manufacturing by labor productivity (sales revenue per labor) between 2000 and 2008 using the full ASI sample. Panel B shows the OLS estimates on the heterogeneous effect of NREGA on log wage by labor productivity using the full ASI sample. All regressions include an unreported constant term, baseline controls, and individual terms of the interaction. Standard errors clustered at the district level are in parentheses. Significance: $^*p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

First, Panel A shows the employment effects estimated on the full sample. The estimation result suggests that employment decreased at manufacturing plants whose labor productivity was below the median before the treatment. Second, as shown in Panel B, the heterogeneous wage effect is negative but statistically insignificant. Thus, the employment and wage effects estimated on the markdown sample in our baseline analysis are remarkably robust to using the full sample.

8.3 Using Mandays as a Labor Input

Our baseline empirical analysis uses labor headcount as a measure of labor input. The firm-level panel data from the ASI also reports total mandays worked and paid, and the labor input for production can be more precisely measured by mandays. The data on mandays paid is severely limited compared to the mandays worked, so we focus on total mandays worked as an alternative measure of labor inputs to check the robustness of our main findings. This change affects the labor market outcomes, except for wage, and thus, we check the robustness of results from the employment, markdown, and MRPL regressions. Table 16 presents the results on the heterogeneous effects by firm's labor productivity and shows that the impacts of the NREGA on employment, markdown, and MRPL are remarkably robust to an alternative measure of labor.

Table 16: Heterogeneous Effects of NREGA by Labor Productivity using Total Mandays

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln MRPL_{it}$
		Panel A. Below median	
Post-NREGA	-0.063**	0.060***	0.070***
	(0.026)	(0.020)	(0.023)
N	32632	32632	32632
R^2	0.97	0.91	0.90
		Panel B. Above median	
Post-NREGA	0.023	-0.008	-0.012
	(0.025)	(0.017)	(0.024)
N	36496	36496	36496
R^2	0.96	0.89	0.85

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes at manufacturers with low (top panel) and high (bottom panel) labor productivity. The sample in the top (bottom) panel consists of firms whose labor productivity measured by sales revenue per labor is below (above) the median in the most recent period before the first phase of NREGA. The dependent variable in Columns (1)-(3) is the log employment (labor headcount plus one), log markdowns, and log MRPL, respectively. The employment and labor input in production function estimation and the calculation of markdown and MRPL is measured by total mandays worked. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

As shown in Table E.10, the heterogeneous impacts for production and non-production workers by the firm's labor productivity are remarkably robust to this alternative employment measure. The effects of NREGA mainly come from production workers at firms with low labor productivity (see left part of the top panel). Table E.11 presents the results for workers with different employment contracts when total mandays worked are used as an employment measure and consistently suggests that effects mainly emerge from regular workers directly hired by employers at firms with labor productivity below the median.[38]

8.4 Dropping Control Districts Surrounded by Treated Districts

As discussed in Section 6.2, we drop never-treated districts that are surrounded by districts treated in the first two phases of NREGA since such control districts might have been affected by the treatment via potential migration from control to treatment group during the off-season as suggested by Imbert and Papp (2020b). Using this alternative control group, shown in panel (a) of Figure E.4, consisting of never-treated districts in our time frame and distant from phase-1 and phase-2 treatment districts, we re-estimate the effects of NREGA heterogeneous by labor productivity. Table 17 shows the estimation results that are the same as our baseline results.

Table 17: Heterogeneous Effects of NREGA by Labor Productivity using Alternative Control Group

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
Post-NREGA × Below median	-0.114***	0.100***	-0.019	0.082***
	(0.021)	(0.022)	(0.016)	(0.026)
Below median	0.032**	0.005	-0.014	-0.010
	(0.016)	(0.018)	(0.010)	(0.017)
Post-NREGA	0.014	-0.041*	0.005	-0.036
	(0.023)	(0.023)	(0.018)	(0.025)
N	59763	59763	59763	59763
R^2	0.97	0.89	0.92	0.89

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes at manufacturing firms by labor productivity (sales revenue per labor). The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's labor productivity is below the median. The dependent variable in Columns (1)-(4) is the log employment (labor headcount plus one), log markdowns, log wage, and log MRPL, respectively. The control group in these DID regressions consists of the baseline control group districts distant from or not surrounded by treated districts in the first two phases of NREGA. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output. All regressions include an unreported constant term and baseline controls and fixed effects. Standard errors clustered at the district level are in parentheses. Significance: $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$.

Then, we examine the heterogeneous workers, and the estimates are qualitatively the same as the corresponding baseline estimates. Specifically, results for production and non-production workers, shown in Table E.12, are similar to the baseline findings. Finally, Table E.13 suggests that the

[38]The ASI data further disaggregates total mandays worked into manufacturing and non-manufacturing mandays. Appendix D.2 checks the robustness of our results using manufacturing mandays as an employment measure.

program is more effective for regular than contract workers in manufacturing, consistent with the baseline findings. These results also provide credence to the plausibility of a stable assignment assumption in our setting.[39]

9 Conclusion

We provide the first evidence on the spillover impact of workfare programs on monopsony power. The world's largest public workfare and antipoverty policy that guarantees short-term work for unskilled workers at projects concentrated in agricultural infrastructures (India's NREGA program) introduces labor supply shock to labor markets in the manufacturing industry. Leveraging this policy change as a natural experiment and nationally representative data on manufacturing plants, this paper examines the indirect impact of the program on the manufacturing labor market, focusing on manufacturers' monopsony power in India. The empirical results that we have found are well in line with the theoretical predictions from the model with heterogeneous workers and NREGA that we developed based on the classic differentiation model (Boal and Ransom, 1997) or a monopsony framework in which workers have heterogeneous preferences over different employers (Card et al., 2018; Manning, 2021).

We show that the agricultural industry's hiring of unskilled workers crowded out manufacturing industry work at specific types of firms. The manufacturing plants affected by the treatment are those with low-productivity workers, firms that use labor more intensively than capital, and those that pay lower wages, particularly below the minimum wage at which wages of NREGA jobs are set. The employment responses are intuitive as the NREGA jobs are less attractive to highly productive workers with high compensation. These effects are mainly concentrated in the leading industries, in districts with high worker mobility, and among rural firms. The marginal revenue product of remaining workers at such manufacturers increases as more productive workers tend to stay while their wages are stagnant. The employment guarantee program in agriculture thus leads to the exploitation of manufacturing workers who did not separate from their employers. We argue that the firm's employment composition and wage stickiness mainly explain the expanding wedge between MRPL or workers' contribution to the firm and wages received in response to the policy change. Our findings offer some policy implications regarding workfare programs in developing countries. The main policy lesson is that spillover effects of such policies that generate employment in a particular industry might adversely affect nonparticipants' labor market conditions in other sectors.

We conclude with some caveats and directions for future research. First, this paper measures labor market power by markdowns—a direct measure of monopsony power. To further investigate the indirect effects of the NREGA on nonparticipants' wage-setting power, one can construct a more

[39] Further dropping control districts not surrounded by treated districts but neighbors with multiple treated districts as shown in panel (b) of Figure E.4 provide qualitatively the same results (available on request).

fundamental measure of labor market power by measuring workers' outside options. For example, Jäger et al. (2024) directly measure workers' fallback options by asking about workers' expected wage change if forced to leave their current employer in a survey in a developed country context of Germany. It would also be the first attempt to directly measure the workers' outside options in the developing world. Second, we focus on the manufacturing industry in this paper. So, if the data allows, future research can examine the indirect impact on other non-agricultural sectors, such as construction, services, and retail. Third, this paper focuses on the partial equilibrium effects of the program on the manufacturing industry. Future research can thus study the general equilibrium effects of the NREGA on manufacturing labor markets like Muralidharan et al. (2023), who examine the general equilibrium effects of the program on private firms.

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