ESSAYS ON LABOR MARKET POWER

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This dissertation examines the origins and consequences of firms' labor market power—a key source of income inequality—by leveraging various economic and policy shocks, including technological change, a public employment program, and foreign capital liberalization. The findings suggest that automation threat, changes in workforce composition, and labor reallocation across firms are important drivers of employer power in the labor markets. The dissertation also provides evidence that capital market integration reduces labor misallocation through increasing labor market competition.

BIOGRAPHICAL SKETCH

Tsenguunjav Byambasuren was born in Ulaanbaatar, Mongolia. He earned a Bachelor of Science in Economics (with Honors) at the University of Finance and Economics ('13) in Mongolia and a Master of Science in Applied Economics and Management at Cornell University ('19). Before coming to Cornell, Tsenguunjav worked as an Economist at the Central Bank of Mongolia.

This dissertation is dedicated to the memory of my grandmother, Dolgorsuren Dagii, who passed away while I was pursuing my PhD.

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CHAPTER 1

INTRODUCTION

Global income inequality rose sharply during the 19th century and has remained persistently high since then. Today, both within-country and between-country disparities are as high as they were a century ago [74, 75, 221]. The pattern of increased inequality is associated with the globally declining share of earnings earned by workers in total income [201]. Put differently, the decline in labour share made total income less evenly distributed and more concentrated at the top of the distribution, favoring capital and leaving workers with a smaller portion of economic gains [142, 43].

The declining global labour share since the early 1980s [148] is indeed one of the striking stylized facts that undermined a long-standing fundamental assumption of economic models concerning long-run economic growth, a constant share of value added going to labour [146]. Therefore, many strands of economics literature focused on seeking the underlying reasons for this aggregate decline in the labour share. Most of the explanations proposed to explain this aggregate decline of labor share are rooted in firm behavior, and one of the three leading theories in the literature is firms' wage-setting power [150]. The standard models where the labour market is perfectly competitive assume that the market sets the wage. However, recent estimates of labor market power or monopsony power using different measures, including the elasticity of labor supply to an individual firm, labor market concentration, and wage markdowns—the wedge between the marginal revenue product of labor (MRPL) and the wage—suggest that firms set wages, often below MRPL, rather than receive them from the market [170, 68, 23]. Due to this growing consensus on monopsonistic wage setting, the literature has been actively exploring the sources of employer power. In "Essays on Labor Market Power", I study the origins and consequences of firms' market power in the labor markets, contributing to this fast-growing literature.

In the first chapter of this dissertation (Byambasuren 2025), I examine the displacement threat from automation or labor-saving technological changes as a potential driver of firms' wage-setting power. Using plant-level longitudinal data, I first show that firms have wage-setting power in German manufacturing, with the average worker receiving 79 cents on the marginal euro, by estimating wage markdowns based on the production function approach [224]. It suggests that the German manufacturing labor market is not perfectly competitive. Allowing for job task heterogeneity among workers, I also show that workers performing routine (nonroutine manual) tasks are subject to the lowest (highest) degree of monopsony power.

Leveraging the estimated wage markdowns and exposure of local labor market regions to industrial robots instrumented by plausibly exogenous shifts-share push factors [8, 86, 9], I find that the automation threat proxied by robot exposure is a significant source of the employers' wage-setting power over routine task-performing workers who face the highest risk of displacement by industrial robots. Robot exposure increases employer power, particularly in districts with weaker trade unions in East Germany, with spatial frictions. This paper provides the first causal estimate on the link between automation threat and employer power. I then develop a simple wage bargaining model with heterogeneous workers building on the right-to-manage model by [188] to formalize the role of automation threat in the bargaining process through its impact on the firm's outside options. The model provides theoretical results consistent with the main empirical findings and suggests that separate bargaining between the firm and unions representing different workers is crucial in mediating the heterogeneous effects of automation threat on wage markdowns and bargaining outcomes for workers with different exposure to displacement risks.

The labor market imperfection could be more severe in developing countries due to their poor institutions and weak law enforcement. In my second chapter (Byambasuren, Chau, and Soundararajan 2025), we study the labor market power in India and the role of changes in workforce composition in firms' monopsony power. We leverage the world's largest workfare and India's flagship antipoverty program, the National Rural Employment Guarantee Act (NREGA), which generates jobs in non-manufacturing industries, and provide the first evidence on its indirect impact on monopsony power in the manufacturing industry. Some existing studies investigate the spillover effects of labor market policies like NREGA on non-participants' labor market outcomes via general equilibrium effects [185] and crowding out effects [135]. These few studies focus on the spillover effects of the NREGA on private-sector labor markets. However, another crucial but largely overlooked aspect of this policy is the indirect effect on labor markets in manufacturing.

The estimated markdowns using nationally representative panel data from the Annual Survey of Industries (ASI) suggest that workers at an average manufacturing firm earn 0.72 rupees on the marginal rupee in India. We develop a monopsony model by extending the setup in [69] and [170] (classic differentiation model as referred in [49]) with NREGA features and heterogeneous workers who differ by their origin and skills. Guided by the model predictions, our empirical analysis shows that, at manufacturing firms, the policy increases the firm's average markdown by crowding out mobile workers and increasing the share of immobile workers with low labor supply elasticity in the workforce. The marginal revenue product of labor increases while wages remain stagnant in response to the policy. These labor market effects are concentrated among production and regular workers who are initially subject to lower monopsony.

Growing evidence suggests that trade liberalization is a critical factor in shaping monopsony power [105, 157, 195]. Yet, there is little evidence on the role of foreign capital liberalization, except [167], who suggest that foreign direct investment (FDI) liberalization during China's accession to the WTO increased wage markdowns via increasing search friction in the country. In my third chapter (Byambasuren, Chau, Nguyen, and Soundararajan 2025), we study the impact of India's foreign capital liberalization on firms' monopsony power, which in turn is a source of labor misallocation. Leveraging the 2006 episode of FDI liberalization in India that approves FDI up to at least 51% of domestic firms' equity [41], we find that the average effect of the policy reform is essentially zero and the impact is highly heterogeneous. In particular, for firms with ex-ante high MRPL (markdown), the liberalization increases employment and wage bills and reduces MRPL and wage markdowns, with no impact on wages, relative to low MRPL (markdown) firms.

We also document that the liberalization increases employment and wages and reduces markdowns for men at high male MRPL firms, but reduces MRPL and markdowns for women at high female MRPL firms.

CHAPTER 2

AUTOMATION THREAT AND LABOR MARKET POWER

Abstract

This paper studies the role of automation threat in firms' labor market power. Employing the production approach, I show that workers in an average German manufacturer receive 79 cents on the marginal euro. Using automation threat proxied by local labor market regions' exposure to industrial robots instrumented by plausibly exogenous shift-share factors, I find that robot exposure increases employer power over routine workers in areas with weaker unions in East Germany with spatial frictions. The empirical findings are consistent with the wage bargaining model where employers retain the "right-to-manage" their workforce composition and unions representing different workers separately bargain with the firm.

2.1 Introduction

There is a growing consensus that firms, rather than markets, set wages [45, 161, 224, 105], while the potential sources of the firm's labor market power are the current topics of active investigation.[1] Automation, on the other hand, has been found as a significant source of changes in wages [e.g., 27], employment [e.g., 3, 6, 7], and wage inequality [e.g., 7, 9]. These studies, however, often assume that labor markets are perfectly competitive despite the empiri-

[1][49], [169], and [24] provide comprehensive surveys on monopsony literature over its development stages. For recent literature review on monopsony, see [170], [68], [23], and [29, 30].

cal evidence on monopsony power. A few papers, such as [77] and [10], show that introducing labor market imperfection presents notable differences in the effects of technological progress on wages and employment. However, little is known about the impact of automation on labor market power.

This paper first estimates the wage markdowns—the ratio of the marginal revenue product of labor to the wage—as a measure of firms' labor market power in German manufacturing. I then quantify the impact of automation on wage markdowns through threats from industrial robots, emphasizing the job tasks conducted by workers, spatial frictions, and labor union coverage. Germany is an ideal environment to investigate the role of automation threat in employer power as it is one of the leading countries in industrial robots [8, 86] and it has a uniquely flexible bargaining system with regional and occupational differences [143]. I also develop a wage bargaining model with heterogeneous workers to explore the threat from labor-saving technologies as a determinant of the firm's and workers' bargaining position and employer power in the labor market via its impact on firms' outside options, which cannot be explained by the canonical models of automation like [6] that characterize the actual automation. While the classical monopsony model with an upward-sloping labor supply curve does not characterize the firm's outside options, in the proposed wage bargaining model, where the labor supply curve can be upward-sloping, the firm's outside option plays an important role.

I use an establishment panel survey data (IAB Establishment Panel) and novel matched longitudinal employer-employee data (LIAB) from Germany. The detailed firm-level longitudinal survey data of the IAB Establishment Panel with direct and comprehensive information to estimate production function under labor market imperfections such as labor headcounts. It enables me to accurately measure markdowns at the establishment[2] or firm level between 1997 and 2018 using "production approach" derived from the duality of the firm's profit maximization and cost minimization problems [180, 177, 57, 224, 92].[3] Using worker-level job tasks data for 2006, 2012, and 2018 from the Federal Institute for Vocational Education and Training (BIBB), I define routine and nonroutine workers and measure the firm's monopsony power over those workers with different exposure to displacement risks and distinct outside options. Additionally, I measure the automation threat based on the local labor market region's exposure to industrial robots using the industry-level data on the stock of robots since 1993 obtained from the International Federation of Robotics (IFR).[4]

I focus on estimating the causal impact of automation threats on labor market power using an automation measure defined at the level of local labor markets mainly because threats are more pertinent to the state of automation at some aggregate level, such as industry or local labor markets, rather than at the firm level. Although the threat mechanism could be investigated using data on the firm's actual robot adoption via comparing adopters and non-adopters,[5]

[2]The German data provides employer information at the establishment level, a single production unit, rather than the firms in the legal sense. A potential issue with using establishment as a level of observation is that multiple establishments in a firm could be subject to common shocks and influence each other. However, more than 70% of the establishments in my data are those in a single firm, reflecting the German economy in which a large portion of firms are small and medium enterprises. I, thus, interchangeably use the terms establishment, plant, firm, and employer throughout the paper.

[3]The production function is estimated using the semi-structural control function approach offered by [11]. [192] developed the control function approach, which was further refined by [163] and [219] with different functional forms and specifications.

[4]The German establishment panel also contains direct information on the firm's robot adoption from 2014 to 2018, which I used to analyze the firm's robot adoption and compare the actual robot adoption with the exposure of local labor markets to robots.

[5]There are at least two reasons why automation threats could also exist in robot-adopting firms. First, existing studies on the labor market effects of automation suggest that worker displacement does not occur instantly, and the displacement effect materializes after some periods following an automation shock. For example, [48] show that it takes five years for automation to have displacement effects at the Dutch firms. So, it is likely that workers at automating firms

the threat effect is likely to stem more from a shock that has not happened yet, which affects labor market outcomes via expectation. For example, [73] study the effects of threats from automation whose arrival date is uncertain on wages, employment, and age composition of the labor force in occupations through the lens of an overlapping generations model and in the context of teamsters at the dawn of motor trucks in the U.S. early 1900s. In this paper, the identification of causal effect relies on a shift-share instrumental variable (IV) design that instruments robot exposure of the local labor market in Germany with robot exposure of the same labor markets in other high-income European countries [8, 86, 112].

This study contributes to several strands of literature. First, this paper contributes to the literature investigating the labor market effects of automation by empirically showing that automation threat affects employer power. I contribute to this literature by providing the first reduced-form evidence on the causal impact of automation threat or potential robot adoption on labor market power. Separate from the labor market effects of the actual event of automation, [73] show that an automation shock not yet materialized makes younger workers avoid an occupation facing obsolescence, yielding changes in the age structure and employment compression in the occupation and increases the wages of workers entering the occupation via compensating differential. However, the impact of automation technologies, particularly displacement threats from automation which has not happened yet, on employer power in wage negotiation is understudied. There are several unanswered questions about the po-

can still be subject to automation threats, especially during the early stages of robot adoption, before they have been displaced from their workplace. Second, the remaining workers after displacement might be subject to a displacement risk in the future, although not replaceable by the current technologies. So, those remaining workers could still be subject to threats from further automation at the current robot-adopting firm. However, it is challenging to isolate the effect of automation threat from that of the actual adoption when exploiting variation from actual adoption.

tential role of automation threat in the wage-setting process and wage negotiation between employers and workers, such as whether there is any role of displacement threat from automation in industrial relations between employers and workers. So, I estimate the causal effect of robot exposure on labor market power at the local labor market level by employing a shift-share IV strategy to fill this gap in the literature. In the empirical literature investigating the link between automation technologies and labor market power, a few existing papers estimate the non-causal empirical relationship between the proxy of automation technologies and labor market power. For example, [153] provide one of the earliest estimates on the empirical relationship between automation technologies and monopsony power by estimating a positive relationship between ICT investment and firm-level wedge between marginal revenue product of labor (MRPL) and wage across U.S. manufacturing plants. [176], on the other hand, finds that ICT usage plays a minor role in workers' bargaining power across French manufacturing firms. However, this paper provides a causal interpretation for the link between automation threat and employer power and finds that threats from automation grant more power to employers than workers in the labor market.

Second, my work is related to a growing literature examining the prevalence, evolution, and worker heterogeneity of monopsony power. I show that a worker in a median (average) German manufacturer receives only 89 cents (79 cents) on the marginal euro. This markdown estimate is consistent with [32]'s estimates of labor supply elasticity, which suggest an upward-sloping labor supply curve to an individual firm. Using the aggregation method suggested by [224], I also show that the aggregate markdown in German manufacturing has decreased since 1997, with some plateau between 2000 and 2008. This aggregate markdown estimate and employment-based labor market concentration measured by the Herfindahl-Hirschman Index (HHI) present generally similar patterns over time, specifically until the Great Recession in 2009, after which markdown presented sharp declines.[6] Focusing on workers performing different job tasks, including routine, nonroutine manual, and nonroutine cognitive tasks, I quantify markdowns for heterogeneous workers and find that routine (nonroutine manual) task-performing workers are subject to the lowest (highest) degree of monopsony power in German manufacturing.[7] Using these measures, I estimate the heterogeneous effects of automation threat on monopsony power for these workers who vary by their degrees of exposure to displacement risks.

Third, this paper contributes to the literature on explaining the changes in workers' and firms' bargaining positions and labor market power by offering a wage bargaining model, highlighting the role of automation threat. I investigate the impact of automation threat on wage bargaining outcomes, focusing on a channel of firms' outside options. [162] show that automation threat weakens workers' bargaining power via improving firms' outside options by extending the standard Diamond-Mortensen-Pissarides model. However, in this paper, I develop an alternative framework building on the right-to-manage wage bargaining model by [188] and investigate bargaining by unions representing

[6]The sharp decline of wage markdowns in the post-Great Recession periods is strongly consistent with a rise of wages and decline in markdown after the Great Recession [99], driven by women's wages in the bottom part of the wage distribution, slowing the rise in inequality [97].

[7]Some studies show that monopsony power differs by worker characteristics such as gender [128, 65], distaste for commuting [83], and job tasks being performed by the worker [32] using administrative and experimental data. These studies mainly estimate the elasticity of labor supply for different workers as a measure of monopsony power. Although [32] document the heterogeneity in monopsony power for routine, nonroutine manual, and nonroutine cognitive task-performing workers by estimating labor supply elasticity, this study examines the same heterogeneity using a different method, i.e., quantifying markdowns. The estimated markdowns are generally consistent with their estimates of labor supply elasticity for the three types of workers.

different workers as an underlying mechanism for heterogeneous effects of automation threat.[8] This model better represents the bargaining and industrial relations in Germany, where most bargaining between employer and workers is on wages [64].[9,10] Unlike the U.S. and the U.K., collective bargaining in Germany mainly occurs at the industry-region level between the trade union and employers association, mostly concerning wages. Agreements on managerial decisions are co-determined at the firm level [143]. Although working conditions such as the number of hours are bargained at the industry-region level, employment is not a bargaining topic in the country, potentially because it is hard to set individual firms' employment levels in sectoral or regional agreements. The employment is instead left for the firm to decide unilaterally [129].[11] The model shows that the main empirical findings are generally consistent with the theoretical results.[12] It also suggests that separate bargaining (as opposed to

[8]Threats of displacement, either by labor-saving technologies or alternative labor, can also affect firms' and workers' bargaining positions via changing the unionization, which is likely to affect the bargaining outcomes by changing the bargaining strength. The empirical evidence on the impact of automation threat on unionization provides mixed results. For example, [183] show that the threat of offshoring reduces unionization rates via eroding the union's bargaining position in Denmark, leveraging globalization as an exogenous shock in firms' ability to offshore. On the other hand, [114] suggest a fear of losing jobs to automation is positively associated with workers' intentions to join a union in the U.S.

[9]I employ this static model by assuming that the employer and the workers have perfect information since there will be no actual back-and-forth negotiations at the unique equilibrium of the bargaining game under perfect information [110].

[10]The solution to the right-to-manage model is not Pareto-efficient, and thus [174] propose the static model of efficient bargaining procedure where the union and firm simultaneously determine wages and employment, ensuring Pareto efficiency. Although bargaining over wages and employment is Pareto efficient, bargaining over employment is rarely observed. Despite mixed results from various empirical studies testing the predictions of collective bargaining models, empirical evidence consistently suggests two stylized facts: (i) the marginal productivity of labor is not equal to the outside wage, and (ii) employment and bargained wages can be negatively correlated in some contexts. Since these results do not contradict the conclusions of the right-to-manage and those of the insiders-outsiders model without discrimination against entrants ([63], pp. 458-462), I build on the right-to-manage model.

[11]Many studies employ the right-to-manage model to examine various topics related to the German labor market, such as [71], [59], [58], [129], and [101].

[12]The measures of labor market power in the empirical analysis (wage markdown—monopsony or wage-setting power) and the theoretical model (bargaining power) are likely consistent with each other or positively correlated in the German context because bargaining

joint bargaining) between the firm and the union representing various workers is a potential mechanism through which automation threat presents heterogeneous effects on routine and nonroutine workers.

The rest of the paper proceeds as follows. Section 2.2 describes context and data. Section 2.3 discusses the construction of markdowns and presents the estimates for German manufacturing. Section 2.4 lays out the empirical strategy to identify the effects of automation threat on wage markdowns, presents the results from the labor market-level analysis, and checks the robustness of the main findings. Section 2.5 examines the labor market effects at the plant level and explores potential mechanisms. Section 2.6 presents a wage bargaining model that formalizes the role of automation threat in firms' bargaining process and provides new insights. Finally, Section 2.7 concludes.

2.2 Context and Data

This section first presents the wage-setting system in Germany and its evolution over the study period between the late 1990s and late 2010s. Then, I briefly describe the datasets leveraged for constructing the key variables.

2.2.1 Context

The German system is based on contracts and mutual agreements under the dual system of collective bargaining and co-determination. Wages, hours, working conditions, and other agreements, usually renegotiated between unions,

strategies of German firms generally remain the same during the tenure of the workers [64].

employer associations, and firms on an annual and biannual basis, are largely regulated by long-lasting sectoral and regional collective bargaining. Agreements on firm-level major and minor or daily managerial decisions are co-determined by employer and workers through representation on corporate boards and works councils. Table 2.1 summarizes the dual system of employee representation in Germany.

In the remainder of this section, I focus on three salient features relevant to my analysis. First, the collective bargaining coverage in Germany has been eroded since the mid-1980s and the pace of the decentralization accelerated after the mid-1990s [123]. The decline in sectoral bargaining continued between 2000 (48% of establishments and 59% of employment) and 2011 (33% of establishments and 48% of employment). The erosion of collective bargaining agreements generally continued until 2019, indicating that the bargaining system is becoming more flexible and is shifting from the industry-region level to the firm level [143]. The relatively widespread use of "hardship" and "opening" clauses that have been increasingly common, leading to wage dispersion even with relatively large-scale union agreements [204] could be one of the reasons for the flexible bargaining system in the country.[13] The works council coverage, however, has been relatively stable, potentially due to the 2001 Works Constitution Act aimed to facilitate their formation. This flexible collective bargaining sys-

[13]There is an active debate about the underlying factors driving these changes in collective bargaining and worker representation trends, and several reasons have been proposed. Studies suggest that the increased intensity of the shift in wage setting from industry or region to the firm during the 1990s is primarily rooted in German reunification. First, the unprecedented de-unionization or decentralization of the wage-setting process is intensified because unions and workers are forced to accept firms' deviations from the union agreements during economic and fiscal difficulties in Germany due to the reunification [98]. Second, opportunities for offshoring to other low-wage central and eastern European countries previously blocked behind the Iron Curtain expand employers' outside options and thus their bargaining power by providing them with cheaper production inputs [85]. Third, small and unproductive firms exited the employer associations as they could not keep up with the wage floors set by large and productive firms since the early 1980s [100].

tem allowing firms to set wages is unique compared to the more rigid bargaining system of many of its European neighbors. In 2020, the industry-region level bargaining was 43%, while the firm-level bargaining agreements that are mainly to set higher standards for typically very large and highly productive firms than industry-region-level agreements was about 8% [143].

Second, there are notable regional differences in the collective bargaining coverage. The collective bargaining coverage has been significantly higher in West Germany (68% and 48% of the labor force covered in 1998 and 2018, respectively) than in East Germany (52% and 35% in 1998 and 2018, respectively), indicating more significant worker protection in the West relative to the East. The coverage of works councils has also been higher in West Germany than in West Germany [143]. In contrast, in other European countries such as France and Italy, union wages are often bargained at the national level with no regional differences as much as in Germany.

Third, another indication of an unusually flexible collective bargaining system in Germany over my study period is the presence of unions representing different occupation, skill, and experience groups, particularly before 2015. For example, a union confederation of *Deutscher Beamtenbund* (DBB) contains several occupation-specific unions. In 2015, the "unity law" has been passed jointly lobbied by unions and employer associations. Unions argue that they support the law as it narrows the wage inequality between high-skilled and low-skilled workers, and employer associations support the law due to the high demand for wage increases and the threat of strikes from occupation-specific unions representing high-skilled or hard-to-replace workers [143]. Workers frequently bargain, driven by managers who actively bargain, and there is heterogeneity in bargaining by different workers [64]. These indicate that high-skilled workers generally have higher bargaining power and are protected by unions more than low-skilled workers, e.g., via occupation-specific unions representing high-skilled workers, especially before the introduction of the unity law. Even with this law undermining the occupation-specific representation, the worker's voice is more likely higher for high-skilled and hard-to-replace workers. The topics in bargaining between occupation-specific unions and employers associations include, for example, specifying wage and salary floors at the industry-region level.

While the country has a wide range of union presence, bargaining can also happen at the firm level and between occupation-specific unions and employer associations. This nuanced environment makes Germany an ideal environment to examine the role of automation threat in labor market power as the automation threat can have heterogeneous effects on regions with different levels of worker protection and workers in occupations subject to various degrees of displacement risk.

2.2.2 Data

I use four main datasets to construct the key variables, including the automation threat across regions and wage markdowns for heterogeneous workers, and conduct the empirical analysis. The first two datasets are the IAB Establishment Panel survey (IAB-BP) and matched employer-employee data (LIAB) from Germany provided by the Research Data Center (FDZ) of the Federal Employment Agency in the Institute for Employment Research (IAB). The third dataset is the BIBB Establishment survey that reports worker-level representative data on activities or tasks performed at the workplace along with occupation information. I obtain this data from the Federal Institute for Vocational Education and Training (BIBB). These three datasets are mainly used for measuring the wage markdowns for heterogeneous workers. The fourth primary dataset provides the global information on stock of industrial robots across industries in different countries, which I used for approximating the automation threat. The data comes from the International Federation of Robotics (IFR), a widely used data source in the automation literature. Appendix A.1 describes these four main datasets in detail. I also use other data sources, including the UN Comtrade and EU KLEMS, to construct shift-share measures on industry-level net exports and ICT investment as additional covariates, described in Section 2.4.

2.3 Markdown Estimation

2.3.1 Production Approach

I estimate wage markdown, a wedge (or misallocation in the language of [132] and [12]) between the marginal revenue product of labor (MRPL) and the wage, as a measure of labor market power. I use the production function approach by closely following [224] who have shown that the cost minimization problem implies the following expression that measures markdowns accounting for markups:

$$\nu_{jt} = \frac{\theta^L_{jt}}{\alpha^L_{jt}} \cdot \mu^{-1}_{jt}, \tag{2.1}$$

where ν_{jt} is the markdown for firm j in year t, $\theta^L_{jt} = (\partial F(l_{jt})/\partial l_{jt})(l_{jt}/F(l_{jt}))$ is the output elasticity of labor, $\alpha^L_{jt} = W_{jt}(l_{jt})/R_{jt}(l_{jt})$ denotes a firm's labor share of revenue $R_{jt}(l_{jt})$, and $\mu_{jt} = p_{jt}/\lambda_{jt}$ is the firm's price (p_{jt})-cost (λ_{jt}) markup.[14] The markdown equals unity ($\nu_{jt} = 1$) in perfectly competitive labor markets. In labor markets with imperfect competition, on the other hand, employers have market power if $\nu_{jt} > 1$. The markdown less than unity is not intuitive under the profit-maximization assumption, but in practice, it can happen, and it implies that the employer pays wages to its workers higher than their productivity, or it could be a measurement error.

The wage markdown has been quantified by estimating and calculating its components in equation (2.1). I obtain the output elasticity of labor, θ^L_{jt}, from the production function estimation. I estimate production function using "proxy variable" method [192, 163, 11]. Appendix A.3 discusses the production function estimation in detail. The firm-level markups are estimated based on the production function estimation as in [90], who show that $\mu_{jt} = \theta^M_{jt}(\alpha^M_{jt})^{-1}$ where θ^M_{jt} is the output elasticity of a variable input M_{jt} other than labor, e.g., material inputs, and α^M_{jt} is the share of expenditures on input M_{jt} in total sales revenue. An expenditure on labor as a share of revenue is calculated directly from the data, where labor cost is measured by the total annual wage bill. Table 2.2 summarizes the main variables used for markdown estimation, estimated total factor productivity (TFP), and average daily wage.

The degree of monopsony power is commonly measured by the wage elasticity of labor supply to the firm using, for example, a method pioneered by [169], who also has shown that the markdown is proportional to the elasticity

[14] The equation simplifies to the proportionate wage and MRPL gap and is analytically equivalent to the ratio of MRPL over wage.

of labor supply based on profit maximization problem. The monopsony power can also be indirectly measured by labor market concentration based on the Herfindahl index [31]. Another indirect or proxy measure of employer power, which is sufficient for measuring the direction of change in employer power, is the number of firms in the market relative to the number of workers. For example, [77] used the ratio of employers to workers as a measure of employer power. However, estimating markdowns using the production approach has two main advantages over other measures of monopsony power. First, it provides an establishment-specific measure of monopsony power that varies over time. It allows me to show how "shock" in automation threat affects the firms' wage-setting power at the level of establishments or local labor markets. Second, this empirical approach is generic and not restricted to any of the different theories of labor market power, such as oligopsony, classic differentiation, and equilibrium search models to quantify markdowns.[15] Appendix A.4 briefly lays out other measures of monopsony power and discusses their linkages with wage markdowns.

2.3.2 Estimated Markdowns in German Manufacturing Plants

There are two main reasons why I focus on the manufacturing industry. First, labor input must satisfy an assumption VI of [224], which states that the firm uses labor only for output production, not marketing, hiring, and other purposes. Second, most of the actions in automation happen among manufacturers. As illustrated in Figure 2.1, more than three-quarters of robot adopters are manufacturing plants, indicating that robot adopters are highly concentrated in the

[15]See [49] for a systematic review of these theories of monopsony power.

manufacturing industry. Table 2.3 reports the share of robot users across German plants to further analyze the prevalence of robot adopters. In 2018, only 1.48% of all surveyed plants, which are representative, used robots. Most of the plants in the survey are non-manufacturing firms, and less than 1% of the non-manufacturing firms are robot users. Although the manufacturing industry is robot-intensive, as indicated above, only 7.19% of the manufacturing plants were robot users in 2018. Thus, robot adoption is relatively rare, even in the manufacturing industry.

I present the results of markdown estimation in Table 2.4. The plant-level estimates clearly show that labor market power in German manufacturing is sizable and larger than unity. The average establishment throughout the period charges a markdown of 1.27–that is, a plant's marginal revenue product of labor is, on average, 27 percent higher than the wage it pays its workers. Alternatively, taking the reciprocal, a markdown of 1.27 implies that a worker receives around 79 cents on the marginal euro generated. Furthermore, I find that labor market power is widespread across manufacturing plants. Half charge a markdown of 1.13 (89 cents on the marginal euro), and the interquartile range is around 0.7. The presence of some firms with markdown smaller than unity is consistent with [94] who suggest that 30% of representative German plants pay wages higher than MRPL.[16]

[16]The sample size for the baseline markdown estimation for all establishments in the sample is larger than that for markdown estimates for establishments in East and West Germany shown in Table 2.7 where around a quarter of the total number of firms are excluded due to a lack of spatial information. Thus, the markdown was estimated for the remaining three-quarters of firms for which whether they are from East or West Germany is known. Due to this sampling difference, the median and mean estimate for East and West Germany are higher than the baseline estimate of markdowns on the full sample in Table 2.4. Despite this reduced sample size, the estimated markdowns are relatively stable. In Appendix A.5.1, I check the robustness of my baseline markdown estimates where a common production function was estimated on the full sample by estimating the production function for East and West Germany separately and summarizing the estimated markdowns for all firms. The result shows that overall markdown estimates for Germany are strongly robust.

My estimate on the wage markdowns is consistent with [32] and [178], suggesting that the German labor market is not perfectly competitive.[17] The market power in an average employer that I have estimated is smaller than that found in other countries, for example, 65 cents in the U.S. [224], 50 cents in Brazil [105], and 71 cents in Colombia [18] earned for each marginal dollar. Overall, I find that both average and median manufacturing plants operate in a market with monopsonistic competition.[18]

I analyze the relationship between markdowns and the firm's idiosyncratic characteristics by focusing on establishment size and productivity to characterize the estimated plant-level markdowns. I thus estimate the markdowns on selected characteristics to investigate the heterogeneity of markdowns.[19] Figure 2.2 shows that larger (panel (a)) and more productive (panel (b)) firms posit higher markdown on wages in the German manufacturing industry, and these findings are consistent with the results from U.S. manufacturing firms [224]. It also provides some credence to my baseline estimate of markdowns.

Markdowns in East and West Germany. Using the German administrative data, [125] show a large wage discount in East Germany compared to West Germany and suggest that East-born workers are less likely to move to districts in the West exhibiting a strong home bias and spatial friction. The suggested

[17]However, in contrast, [177] suggests that German manufacturers do not have labor market power on the median (implied wage markdowns $\nu_{it} = 0.88$) using the AFiD-data over the period 2000-2014.

[18]Appendix A.5.2 shows that my markdown estimates are generally robust to the Cobb-Douglas production function. I discuss my estimates of markups in Appendix A.5.3. In Appendix A.5.4, I show the robustness of the baseline markdown estimates by including the key explanatory variable of interest, robot exposure, in the production function estimation, in the spirit of studies like [54].

[19]Studies in the literature tend to control for firm's age in such regressions, but I exclude age due to limited information on firm's age in the IAB establishment panel. However, the qualitative findings on the relationship between the selected firm characteristics and markdown remain the same even if I include the firm's age, although the sample size substantially drops.

wage gap could indicate a presence of labor market power in East Germany, and the spatial friction in the form of home bias is a potential source of firms' wage-setting power as workers' outside option is limited to local districts in the East. Using the LIAB data, I first estimate similar wage regression to show wage heterogeneity between regions:

$$Y_{jt} = \beta \mathbb{I}_{j,East} + \mathbf{X}'_{jt}\delta + \gamma_k + \mu_t + \varepsilon_{jt}, \tag{2.2}$$

where Y_{jt} is (log) average real daily wage at firm j in year t, $\mathbb{I}_{j,East}$ is a dummy for whether firm j is located in the East, $\mathbf{X}'_{jt}$ is a vector firm-level covariates, including workers' average education, female share, and firm size, and γ_k and μ_t are industry and year fixed effects, respectively. Table 2.5 presents the results, and the East-West wage gap is estimated at $\beta = -0.199$ (SE: 0.003), which is remarkably similar to [125]'s estimate.[20] Figure 2.3 further shows the wage gap between East and West Germany in each twentiles of the firm size distribution. Consistent with [125], the average real wage paid by the firm increases with firm size in both regions. The real wage discount in East Germany is present for each firm size, which is consistent with imperfect mobility of workers which in turn is conducive to different levels of market power.[21]

Using the same specification as in equation (2.2), I then examine the heterogeneity in wage markdowns across East and West Germany. The results shown in Table 2.6 indicates a markdown premium in East Germany, which is relatively small but strongly significant, $\beta = 0.040$ (SE: 0.013).[22] The wage markdown used in Table 2.6 is estimated based on production function common across regions.

[20] In Table 2.5, I include Berlin as part of East Germany. I checked the robustness of these results by excluding Berlin from the sample and found that the results are substantially robust. The results suggest an East-West wage gap of $\beta = -0.226$ (SE: 0.003).

[21] East Germany in Figure 2.3 includes Berlin, and the relationships and patterns of job ladders remain unchanged when I exclude Berlin from the sample, and the results are available on request.

[22] The results are robust to excluding Berlin from the sample. The East-West markdown gap slightly drops to $\beta = 0.036$ (SE: 0.013) when I drop Berlin from the analysis.

However, manufacturing plants from East and West Germany are likely to be different, so I estimate the production function and wage markdowns for subsamples of East and West German manufacturing separately to summarize the heterogeneity of markdowns by region. Table 2.7 shows the results, and the median markdown for plants in East Germany is 6.4% higher than that for plants in West Germany. The median and mean markdowns indicate that labor markets in East Germany are less competitive or are more monopsonistic, potentially due to weaker collective bargaining and works council coverage in East Germany than in West Germany [143].

Similar to the wage-size ladder in East and West Germany, I also investigate the markdown gap between East and West Germany in each twentiles of the firm size (total number of workers) distribution. Figure 2.4 plots the average firm size against the firms' average wage markdowns for twentiles of the firm size distribution in East and West Germany. Average wage markdown increases with firm size in West Germany but decreases with firm size in East Germany. The positive relationship between firm size and markdown in West Germany, a more developed region, is consistent with markdown heterogeneity by size in the U.S. [224]. The negative relationship between markdown and firm size in East Germany, a relatively underdeveloped region, is similar to that in India [62]. Leveraging employment shares as a firm size, I also examine the heterogeneity of markdown across East and West German plants. Figure 2.5 show the results in the East and the West. The opposite relationship between size and markdown in different regions might be nullifying each other, yielding a weakly positive relationship on the full sample.

Relationship between Markdown and Union Coverage. As discussed in

Section 2.2.1, collective bargaining agreements concerning wage and salary are usually negotiated between unions and employer associations at the industry-region level. I first examine the relationship between worker protection and wage by calculating the daily average salary per worker at firms in different quartiles of the union coverage. As shown in Figure 2.6, the firm's average wage per worker grows as the union coverage increases along its distribution. Controlling for rich sets of fixed effects, Table 2.8 also shows a positive and statistically significant relationship between union coverage and wage, suggesting that workers less protected by trade unions have lower wages.

Then, to examine the heterogeneity in labor market power by trade unions, I provide descriptive evidence on wage markdowns at firms with different union coverage. We expect wage markdown to be lower at firms with high union coverage as unions protect workers from their employers and advocate their interests and demands. As expected, Table 2.9 shows that markdown is smaller for firms with higher union coverage. The median markdown is almost equal to unity at firms in the top 3 quartiles (Panel A) and top 9 deciles (Panel B) of the union coverage distribution, and markdowns at firms in the first quartile and first decile are noticeably higher than unity. These results are consistent with higher markdown in East Germany, as found above, and weaker worker protection in East Germany via low union coverage, as shown in [143].

2.3.3 Aggregated Markdowns

Thus far, this section focuses on plant-level markdown estimates. Now, I discuss how I construct aggregate markdowns at the local labor market and year level. I aggregate the establishment-level markdowns at the local labor market level using the weighted harmonic mean of micro-level markdowns following [224]. This method of defining aggregate markdown as a function of micro-level markdowns is similar to that used for aggregating firm productivities in [132] and [141]. One of the advantages of this aggregation method is that we do not need to impose any specific structures in labor and output markets. The aggregate measure is thus consistent with various monopsony models. Additionally, several studies document that the labor market is local as workers find it costly to search for jobs far from their homes [171, 172] and in different occupations and industries that require different sets of skills [147]. To account for the local nature of labor markets, I use weights based on sales [88].

In doing so, I first define the *local* labor market. Following [45], I use an industry-geographical area pair as a local labor market. I focus on three-digit industries (ISIC Rev.4, or equivalently, WZ2008 classification) and states. This results in about 80 sectors within manufacturing and 17 geographical areas. Appendix A.5.5 details the aggregation approach. Figure 2.7 illustrates the resulting time trend of aggregate markdowns, $\mathcal{V}_t$, depicting a downward trend since 1997, with some plateau between 2000-2008. The wage markdowns sharply declined since the Great Recession in 2009, consistent with a rise in wages and decline in markdowns since the recession, mainly driven by an increase in wages at the bottom part of the distribution [97, 99].

2.3.4 Comparing Aggregate Markdowns with Labor Market Concentration

To provide additional evidence on the situation of labor market power in Germany, I calculate labor market concentration using the Herfindahl-Hirschmann Index (HHI). Using the matched employer-employee data structure, I construct the HHI for labor markets at the occupation (3-digit KldB 1988), region, and year level. Using industry as part of the definition of labor markets is not ideal for calculating labor market concentrations. However, I also use sector (3-digit ISIC Rev.4) instead of occupations to be consistent with the markdown measure and compare aggregate markdowns with HHI. Additionally, I apply a range of alternative definitions for profession, industry, and geography for robustness checks. Given that my markdown measure is quantified using the IAB BP data, I also leverage the IAB BP data to calculate the labor market concentration. The HHIs are computed for the entire economy and manufacturing firms since the markdown is estimated only for manufacturing plants. Appendix A.5.6 shows the formulas for calculating the HHIs.

Table 2.10 shows summary statistics for labor market concentration in German manufacturing for alternative market definitions. In our baseline market definition as a 3-digit KldB 1988 occupation by 141 commuting zones by year, the average overall HHI is 5800. The average HHI implies that the equivalent number of firms recruiting is only 1.7 on average. Looking at percentiles of the HHI beyond the mean, the 75th percentile of HHI is 10,000. To put this number into perspective, a market with one firm having 75% of vacancies and another one with 25% yields an HHI of 10,000. 76% of the labor market is highly concentrated (above 2,500), and 13% of the market is moderately concentrated (have an HHI between 1,500 and 2,500). The remaining 11% have a low concentration (below 1,500 HHI).

Table A.15 reports the summary statistics for labor market concentration in all industries, indicating that manufacturing labor market is more concentrated than non-manufacturing labor market. The average HHI in Germany suggests that only 2.4 firms recruit in a local labor market.

Previous studies using only production data, such as [224], are constrained in comparing the markdown measure with industry-based HHIs primarily because such datasets do not have information on vacancies by occupation. Fortunately, our matched data provide a unique opportunity to compare occupation-based and industry-based measures of HHI and aggregate markdowns. HHIs calculated using 3-digit occupations and 3-digit industries are comparable.

To compare the HHIs with my measure of markdowns, I first calculate the bivariate correlation between the HHIs and wage markdowns across local labor markets (three-digit industry-state cells). I find that the cross-sectional correlation between $\mathcal{V}_{klt}$ and HHI_{klt} is weak: across years, this correlation is close to zero, negative sometimes and rarely statistically significant.[23] Despite this weak cross-section correlation, Figure 2.8 demonstrates that time trends in *aggregate* labor market concentration (HHI_t) and markdowns ($\mathcal{V}_t$) are substantially the same until the Great Recession in 2009. The correlation between aggregate HHI and aggregate markdowns between 1997-2008 is 0.82, and the co-movement of markdowns and labor market concentration over this period is consistent with previous studies [38, 45, 224]. However, the two measures have departed from each other since the 2009 Great Recession. As discussed above, aggregate mark-

[23] I provide details in Appendix A.5.7.

downs sharply declined since 2009 because of an increase in wages and the strength of collective bargaining, which cannot be captured by the HHI measure.

2.3.5 Markdowns for Heterogeneous Workers

Now I relax an assumption of homogeneous workers and consider heterogeneous workers with different exposure to displacement risk or automation threat. Workers are divided into groups based on their potential likelihood of being directly affected by labor-saving technologies or automation. Using those worker classifications, I measure markdown for such workers by estimating production functions with heterogeneous labor inputs.

Definition of Heterogeneous Workers. Using the BIBB/BAuA Employment Surveys and following an approach offered by [21] and later used by, for example, [32], I calculate task intensity measure for an individual i as

$$\mathrm{TI}_{ikt} = \frac{\text{number of activities in category } k \text{ performed by } i \text{ at time } t}{\sum_k \text{number of activities in category } k \text{ performed by } i \text{ at time } t}, \qquad (2.3)$$

where $t = \{2006, 2012, 2018\}$, and k indicates routine, nonroutine manual, and nonroutine cognitive tasks. I follow [210] to classify job activities into these three broader task categories k. Then, I aggregate the individual-level task intensity measures at the occupational groups by taking averages of individual task intensities by occupational categories. The population weights in the BIBB datasets are applied to calculate representative aggregate measures. It provides a continuous measure of task intensity for each routine, nonroutine manual, and nonroutine cognitive task category for each 3-digit occupation. For pre-2006 periods, I use the task intensity measure fixed at the 2006 level. Finally, I merge these task intensity measures to the matched employer-employee data by occupation and year combinations.[24]

The BIBB/BAuA Employment Surveys enable me to construct task intensity measures specifically for Germany, while [26]'s measure is constructed for the U.S. context. This feature is critical when determining worker heterogeneity by tasks because tasks performed at different occupations are likely to differ across countries [72]. But, as a robustness check, I use [26]'s static measure of task intensity developed for 1990 U.S. occupations using data from O*NET.

I define workers directly and indirectly affected by automation or robots in different ways based on tasks performed at the workplace and their education level.

Routine, Nonroutine Cognitive, and Nonroutine Manual Workers: The difference between workers in terms of the risk of being replaced by robots needs to be considered when examining the impacts of actual automation and automation threat on workers because these shocks might have different implications on employers' labor market power given that recent technological change is biased toward replacing routine tasks [27, 117]. Depending on the potential risk of displacement and the realized impact of robots, automation threats might have different implications on labor market power for workers who differ in their tasks performed at work. In mechanical terms, automation threat could have differential effects on such workers, given its heterogeneous impacts on their productivity and wages, leading to a differential impact on their mark-

[24]Although the task intensity measure changes over time, and thus, the same occupation can be classified in different task categories, such instances are not prevalent, given that occupations are grouped into the same category for almost 90% of the time between the study period from 1998 to 2018.

downs. Due to these nuanced mechanisms, the effects are likely to be highly heterogeneous for workers performing different tasks. Hence, I first examine the heterogeneity by job tasks concentrating on routine, nonroutine cognitive, and nonroutine manual tasks task-performing workers.

I consider that a worker is a routine, nonroutine cognitive, or nonroutine manual worker if the maximum of the three normalized task intensity indices is RTI_{ijt}, NRCTI_{ijt}, or NRMTI_{ijt}, respectively, for worker i at firm j in year t. Note that I added employer index j since I use the linked data for this analysis, and RTI_{ijt}, NRCTI_{ijt}, and NRMTI_{ijt} denote TI_{ikjt} index in equation (2.3) when task category k is routine, nonroutine cognitive, and nonroutine manual, respectively. These indices are normalized to have mean zero and unit standard deviation.

Defining three types of labor inputs performing different tasks allows more heterogeneity for estimating the markdown and the impact of automation threat on labor market power. This grouping of workers is similar to that in [32], who measure monopsony power for such workers by estimating the labor supply elasticity. So, I can also compare my estimates of markdown for these workers with their results. Relatedly, [95] calculates the concentration of labor demand for workers performing various job tasks in Norway and shows that labor demand is less concentrated (or more competitive) for routine job tasks than that for nonroutine job tasks, especially in smaller local labor markets. This finding is consistent with my result that routine task-performing workers have the lowest markdown. Table A.16 summarizes the employment, labor cost, and daily wage for routine, nonroutine cognitive, and nonroutine manual workers.

High- and Low-skilled Workers: Although some highly-educated workers perform routine tasks and face automation risks, such as bank tellers, low-education workers are generally subject to automation risks more than high-education workers [4]. Also, from the perspective of labor market power, the outside employment options for low-education and high-education workers are likely to be different, so markdowns for workers with different educational attainment are expected to be unequal [224]. Even if markdowns for such workers are equal, the implication of automation threat on their markdowns could be different. So, I distinguish workers by education categories as (i) low-education: workers without a vocational training degree, and (ii) high-education: workers with a vocational training degree or a degree from a University or a University of the Applied Sciences.

Low- and high-education workers are *not* synonymous with low- and high-skilled workers; however, some studies refer to education as skills [21, 224] potentially because education level and ability or skills tend to be positively correlated. Hence, this categorization can be considered as a split of low-skilled and high-skilled workers. The impact of automation threat might be more nuanced among workers categorized by skills or education than job tasks if automation in Germany is more consistent with skill-biased technological change. Table A.17 presents some descriptive statistics for high- and low-skilled workers.

Estimated Markdowns for Heterogeneous Workers. I estimate the production function with heterogeneous labor inputs, then quantify the markdown for those workers. Table 2.11 shows the estimated plant-level markdowns for heterogeneous workers in the German manufacturing industry who differ by job tasks performed at their workplaces (top panel) and their skills or education level (bottom panel). Heterogeneous workers are included in the production function as separate inputs.

The estimated markdowns for workers who differ by their job tasks performed at the workplace suggest that (i) these workers are also subject to monopsony power in median and average manufacturing plants, and (ii) routine workers are subject to less monopsony power than nonroutine cognitive (NRC) and nonroutine manual (NRM) workers (Panel A of Table 2.11). Specifically, I find that NRM, NRC, and routine workers receive 50 cents, 62 cents, and 77 cents on each euro generated, respectively, on average.[25]

The estimated markdowns for high-skilled and low-skilled workers show that (i) the two types of workers face monopsony power in median and average manufacturing plants, and (ii) the markdown for low-skilled or low-educated workers is larger than the markdown for high-skilled or high-educated workers (Panel B of Table 2.11).[26] Appendix A.5.9 checks the robustness of markdowns for heterogeneous workers focusing on heterogeneity by job tasks to alternative measures of task contents and shows that the markdown estimates are generally robust.[27]

[25]These results are consistent with [32] which suggests that the German labor market is imperfectly competitive using administrative data on individual labor market histories (SIAB) from 1985-2014. The markdowns for workers performing different tasks implied from their estimated labor supply elasticities: 62 cents per euro or ν_{it} = 1.602 for NRM, 49 cents per euro or ν_{it} = 2.043 for NRC, and 63 cents per euro or ν_{it} = 1.589 for routine workers.

[26]The distribution of markdowns for workers performing different tasks illustrates that markdowns are highest for manual workers, second-highest for cognitive workers, and lowest for routine workers (Figure A.10). Markdowns are always relatively higher for low-skilled workers (Figure A.11).

[27]Appendix A.5.8 presents the time evolution of aggregate markdowns for heterogeneous workers.

2.4 Labor Market-Level Analysis

In this section, I describe the empirical strategy I employ to estimate the causal impact of exposure to automation on labor market power at the local labor market level, which relies on a shift-share instrumental variable (IV) design. The section also discusses the identification assumptions.

2.4.1 Empirical Specification

To investigate the effect of automation threat, proxied by predicted exposure to robots at the local labor markets, on labor market power measured by wage markdowns, I estimate the following equation:

$$\Delta Y_{rt} = \gamma_t + \beta_1 \Delta \text{Robot}\,\widehat{\text{exposure}}_{rt} + \beta_2 \Delta \widehat{\text{Trade}}_{rt} + \beta_3 \Delta \widehat{\text{ICT}}_{rt} + \mathbf{X'}_{rt-1}\delta + \mu_{REG(r)} + \epsilon_{rt}, \tag{2.4}$$

where ΔY_{rt} is the annual change in one of the labor market outcomes, including markdown, employment, and wage aggregated at the local labor market region r (district or Kreis) in Germany and year $t \in [1998, 2018]$. I define region, not a combination of region and industry, as a local labor market because the data on the stock of industrial robots from the International Federation of Robotics (IFR) is at the industry level for a given country, so that one cannot use the combination of region and industry. Then, the reasons that I prefer to use region instead of industry are two-fold. First, existing studies from the literature examining the labor market effects of robots using the IFR data used geographical locations as the baseline local labor markets, such as districts or Kreise in Germany [86], commuting zones in the U.S. [8], and cities in China [112]. Second, spatial difference, e.g., between East and West Germany, plays a critical role in labor market dynamics in the German context, as shown in this paper and others like [125].

The annual change in automation threat or local labor market region's "predicted" exposure to robots in Germany, $\Delta\text{Robot}\widehat{\text{ exposure}}_{rt}$, is constructed as

$$\Delta\text{Robot}\widehat{\text{ exposure}}_{rt} = \sum_k \frac{L_{krt-1}}{L_{rt-1}} \frac{\Delta\text{Robot stock}_{kt}}{L_{kt-1}}, \tag{2.5}$$

where L_{kt-1} is the employment in industry k in previous year, L_{krt-1}/L_{rt-1} is the Germany's employment weight of industry k in region r in previous year, and $\Delta\text{Robot stock}_{kt}$ is the change in stock of industrial robots in industry k of Germany between the previous and the current year. The research design in this paper exploits substantial variation in industry compositions across local labor markets. This variation further creates variation in exposure to technological change, e.g., industrial robots. However, the robot data for Germany over longer periods, only available from the IFR as described in Section 2.2.2, are collected only at the industry level. Hence, I follow [8], similar to [86] and [112], and use a shift-share design to allocate each industry's robots stock across kreise or districts according to their shares of the industry's total employment. So, I call this a "predicted" local exposure and denote it with a hat. In my baseline analysis, I use industrial robots in automotive, i.e., k = automotive, because the predicted exposure to robots in all industries fails to satisfy the relevance assumption according to [191]'s weak-instrument test, which is suitable in my setting, although the assumption is satisfied according to the more traditional approach of [213] and [154].[28] The automobile is the dominant industry that

[28] Table A.18 presents the results from testing the relevance assumption for robots in all industries using the two alternative methods. The instruments are strong enough for robots in all industries under [154]'s traditional approach; however, they are weak according to [191]'s approach that is more suitable for overidentified models like in this paper.

drives the penetration of manufacturing robots in Europe, including Germany and the U.S. (Figure 2.9). Thus, the focus on the automotive industry does not sacrifice much variation in industrial robots as most of the variation in robot exposure comes from automotive robots. Despite that, I used the predicted exposure to robots in all industries as a robustness check in my heterogeneity analysis, where the relevance assumption was reasonable. I discuss this identification assumption and the relevant testing approach below.

The terms $\Delta\widehat{\text{Trade}}_{rt}$ and $\Delta\widehat{\text{ICT}}_{rt}$ are the predicted local exposures to net exports and ICT investment, respectively, which was similarly constructed as robot exposure. The annual change in trade exposure, $\Delta\widehat{\text{Trade}}_{rt}$, is measured by the yearly change in German net exports vis-à-vis China and 21 Eastern European countries for every manufacturing industry k using UN Comtrade data, normalized by the employment in the previous year to account for industry size. The annual change in exposure to ICT investment, $\Delta\widehat{\text{ICT}}_{rt}$, is defined by the annual change in real gross fixed capital formation volume per worker for computing and communication equipment using data on installed equipment at the industry level reported in the EU KLEMS database.

The vector X_{rt-1} contains demographic characteristics of the local workforce in the previous period, including the share of females, share of foreigners, share of workers over 50 years old, shares of workers with no educational training, vocational training, and university degree, and shares of workers in broad industry groups. The demographic controls are at the levels of the previous period instead of annual changes to prevent endogenous adjustments on the local labor force after the shock to contaminate the effects of changes in robot exposure or automation threat on changes in markdown. The time fixed effects γ_t

controls for time-varying factors common across regions such as nation-wide federal policies and broad region dummies $\mu_{REG(r)}$ indicating if the region r is located in the north, west, south, or east of Germany capture the time-invariant regional differences across the broad regions.

2.4.2 Identification and Assumptions

I use variation in predicted robot exposure across industries to identify the causal effect of automation threat on employer power, assuming that some sectors are more likely to adopt industrial robots than others. However, variation in exposure to robots across industries in Germany could be due to differences in industry-level demands. Hence, to address biases resulting from this endogenous distribution of robots across industries and time, I use a shift-share instrumental variable approach that introduces the plausibly exogenous and supply-driven variation in robot exposure. [8] proposed this strategy for identifying the impacts of automation, which was later used by [86], [9], and [112]. In this setting, robot adoptions in other high-income advanced countries introduce the plausibly exogenous and supply-driven variation in predicted robot exposure in Germany, which I consider creates variation in potential robot adoption and thus automation threat.[29] Specifically, I instrument a variable of predicted exposure to robots in Germany, $\Delta\text{Robot}\widehat{\text{exposure}}_{rt}$, with non-German exposure variables $\Delta\text{Robot}\widehat{\text{exposure}}_{ort}$ that are constructed using data on the contemporaneous industry-level annual change in robot exposure in other high-income

[29]The instrument is constructed for each country c = (Spain, France, Italy, Norway, Sweden, and the United Kingdom) as similar to [86], and thus I estimate the over-identified model.

European countries:

$$\Delta\text{Robot }\widehat{\text{exposure}}_{ort} = \sum_{k} \frac{L_{krt-j}}{L_{rt-j}} \frac{\Delta\text{Robot stock}_{okt}}{L_{kt-j}}, \tag{2.6}$$

where ΔRobot stock$_{okt}$ is the realized stock of robots in industry k on other high-income European countries at year t and employment counts are at the level from the j years prior to the period t. Following the literature, I set $j = 10$ or use employment levels from the prior decade. In all other respects, equations (2.5) and (2.6) are the same.

Validity of Instruments. For this instrumental variable estimation approach to work, the constructed shift-share instruments must satisfy four main assumptions: (i) relevance, (ii) independence, (iii) exclusion restriction, and (iv) monotonicity. First, there must be a strong correlation between changes in Germany's robot exposure and those in other high-income European countries. To inform the validity of the relevance assumption, Figure 2.10 depicts the first-stage relationship between the annual changes in exposure to industrial robots in the automotive industry in Germany and six other advanced countries included in the set of instruments. The scatter plots show that the endogenous regressor is strongly associated with the individual IVs, providing some credence to the inclusion restriction. The existing studies suggest that these shift-share instruments satisfy relevance assumption for the U.S. [for example, 9], Germany [86], and China [112] based on the traditional test and the popular rule-of-thumb—the F-statistic on the excluded instruments being more than 10 in the first-stage regression [211, 213, 154]. However, [191] recently proposed a more appropriate test of weak instruments for overidentified models with a single endogenous variable where standard errors are clustered, like in this paper and many other cases in the shift-share literature. Therefore, I check the strength of my instruments using the Montiel Olea-Pflueger weak IV test and show that the endogenous regressor and the instruments are strongly correlated, suggesting a validity of relevance assumption. Section 2.4.3 presents the results from this formal test.

Second, a shift-share instrumental variable framework I use in this paper yields consistent estimates if the "shifts" or shocks are orthogonal to unobserved factors that determine the outcomes [52].[30] This condition will hold if shocks to the robot adoption in other high-income European countries are exogenous to changes in local economic conditions in Germany, regardless of whether local exposures to these shocks (i.e., variation in the share component) are endogenous. Given that I estimate an overidentified model in which the number of instruments exceeds the number of endogenous regressors, I can formally test the orthogonality assumption. Employing the overidentifying restrictions test (all IVs are uncorrelated to ϵ_{rt}), I provide evidence on whether the instruments satisfy the orthogonality condition [202, 203, 122, 17]. Third, another assumption that has to be satisfied is the exclusion restriction assumption, which is not directly testable. Following the existing studies that used the same instruments to investigate the employment and wage effects of industrial robots [8, 86, 112], I assume that the changes in robot exposure in other high-income European countries considered as instruments affect the labor market outcomes in Germany only through changing the robot exposure in Germany.

Fourth, since I combine multiple instrumental variables (IVs) for a single endogenous variable or a treatment using a two-stage least squares (2SLS) approach, I am required to satisfy the well-known assumption of monotonicity,

[30] See [113] for settings where identification comes from the orthogonality of the "share" component of the shift-share instruments.

i.e., the 2SLS estimate is a positively weighted average of local average treatment effects (LATEs), to interpret my IV estimates as causal [134]. In my setting, the endogenous variable is the stock of industrial robots in Germany, which I instrument for using robots stock in six other countries. This condition is satisfied if the choice behavior or Germany's robot adoption is effectively homogeneous, while the treatment effects of each instrument are likely heterogeneous in most cases. However, [179] fortunately show that the 2SLS estimates can be a positively weighted average of LATEs under a weaker and verifiable condition of "partial" monotonicity in the case of a binary endogenous variable even if the monotonicity condition is violated. Although the endogenous variable in this paper, the annual change in the stock of robots per 1000 workers, is continuous, I carry out an analysis proposed by [179] to indirectly check the partial monotonicity assumption.

Panel A of Table 2.12 reports coefficients from regressing $\Delta \text{Robot}\,\widehat{\text{exposure}}_{rt}$ on each instrument separately along with the coefficients from regressing $\Delta \text{Robot}\,\widehat{\text{exposure}}_{o_i rt}$ on $\Delta \text{Robot}\,\widehat{\text{exposure}}_{o_j rt}$ where o_i is arbitrarily Spain and $o_j = \{\text{France, Italy, Norway, Sweden, UK}\}$. These models also control for baseline covariates. Column 1 shows that controlling for the covariates (but not the other instruments), the correlation between each instrument and the treatment is positive and statistically significant. It implies that the weights for each complier group must be positive under the partial monotonicity assumption. Similarly, column 2 demonstrates that the partial correlations between the selected pair of instruments from the six instruments are also positive. It also implies that 2SLS weights are positive even if the traditional monotonicity assumption is violated. The joint distribution of the two instruments, thus, is sufficient to yield positive weights. Panel B of Table 2.12 presents the same results when the treatment and instruments are defined as binary variables, indicating whether the value is above mean because the formal statistical tests proposed by [179] are for binary treatment and binary instruments. The results suggest that the partial monotonicity assumption is satisfied even for binary cases. As the formal tests for positive and negative 2SLS weights were proposed for the case when there are only two instruments, I consider all possible pairs of the six instruments. Consistent with the strong positive correlations in Panel B of Table 2.12, the null hypothesis of negative weights is strongly rejected ($p = 0.000$), and the null hypothesis of positive weights is not rejected ($p = 1.000$) for all cases (Table 2.13). These findings provide credence to the validity of the partial monotonicity assumption in my context and allow me to interpret my IV/2SLS estimates as causal.

Statistical Inference. I cluster the standard errors at the level of local market regions or Kreise since the variations that I exploit are at the kreis level.[31] Additionally, as pointed out by [13], conventional standard errors on shift-share explanatory variables such as $\Delta\text{Robot}\,\widehat{\text{exposure}}_{rt}$ might be underestimated because regression residuals are likely to be correlated across regions with similar industry shares. Hence, they propose to compute the standard errors by allowing the correlation amongst error terms within region-industry share groups. I apply their method of calculating cluster-robust variance. In doing so, I closely follow [86]'s procedure and similarly use employment shares across industries.

[31] I also cluster the standard errors by 50 aggregated labor market regions as a robustness check, and Section 2.4.4 discusses the results. I am grateful to Wolfgang Dauth for sharing the crosswalk from German Kreise to these aggregate regions.

2.4.3 Results

I first present the baseline results from estimating the effect of robot exposure on employment, wages, and firms' wage-setting power. I then investigate the heterogeneous impacts for workers performing different tasks across regions.

Baseline Employment and Wage Effects. Before examining the consequences of robot exposure on labor market power, I study the employment and wage effects in my setting. Since the automation literature suggests that industrial robots, as a routine-biased technological change, have highly differential impacts on labor market conditions for workers performing different tasks, I estimate the employment and wage effects for routine, nonroutine manual, and nonroutine cognitive workers. Panel A of Table 2.14 presents the employment effects for these heterogeneous workers. Although the point estimates are not statistically significant, the robot exposure reduces the employment of routine workers, increases nonroutine manual workers' employment, and has zero employment effect on nonroutine cognitive workers in manufacturing.[32]

As shown in Panel B of Table 2.14, the wage effects of robot exposure for heterogeneous workers are also not statistically significant. However, the results show that robot exposure increases the wages of routine workers and reduces the nonroutine workers' average daily compensation. The wage-reducing impact for nonroutine cognitive workers is much more significant in magnitude, which would drive the overall wage impact to be negative in manufacturing.[33] The subsequent section will examine the employment and wage effects at the

[32] These results on employment effects for workers performing different tasks in manufacturing are, in fact, consistent with the results from [86], who also found weak employment effects for the same type of workers with generally the same direction of impacts.

[33] Although [86] found a statistically significant negative effect on wages in manufacturing, the sign of the wage impact that I estimate is consistent.

plant level.

Baseline Markdown Effects. Table 2.15 presents the baseline results obtained from estimating the reduced-form model in equation (2.4) under four separate specifications wherein more controls are added successively. In panel A, I regress the annual change in aggregate markdowns on the change in robot exposure using ordinary least squares (OLS) between 1998-2018. The result shows that robot exposure is positively associated with employers' labor market power, although the relationship is not statistically significant. In panel B, I estimate the impact of robot exposure on employer power using IV (2SLS) regressions. The effective F-statistic of [191] is above the threshold of 21 for the case of 10% potential bias and a 5% significance, and it is well above the rule-of-thumb threshold of 10, indicating robot adoptions in other high-income European countries provide significant variation in German robot exposure. Hansen's J-statistic suggests that the excluded IVs are exogenous and valid instruments. The IV estimates are similar in sign and close in magnitude to the OLS counterparts. The results from my preferred specification, shown in column (4), suggest that automation threat increases wage markdowns; however, the impact is not statistically significant in the baseline.[34]

Heterogeneous Effects. Since plants in East Germany are relatively small and less productive partly due to higher monopsony power compared to West German firms [e.g., 34], as documented in Section 2.3.2, I first consider heterogeneity across East and West Germany. Before examining the markdown effect, I investigate the employment and wage effects of robot exposure heterogeneous

[34]The positive but not strongly significant association between robot exposure and firms' labor market power is consistent with [176] who shows that workers at firms with ICT have lower bargaining power than those at firms without ICT but the relationship is not strongly significant in France.

across regions by estimating the equation (2.4) on sub-samples of East and West German districts (Table 2.16).

The displacement effect for routine workers is more significant in magnitude in East Germany than in the West, although the effects are not statistically significant (Panel A). The robot exposure reduces the wages of workers performing different tasks in East and West Germany and the wage effects are larger in magnitude in the East; however, these effects are statistically insignificant as well (Panel B).

Then we investigate the markdown effects heterogeneous across East and West Germany. First, Table 2.17 presents the results for all workers. As shown in the preferred specification in column (4), the impact of robot exposure on wage markdowns is not statistically significant for establishments from both regions. However, the effect is positive for East German firms and negative for West German firms, indicating heterogeneity across space in markdown effects of automation threat.

Second, findings from existing studies in the automation literature suggest that the labor market effects of automation are highly heterogeneous across worker types, and the impact mainly concentrates among routine task-performing workers [e.g., 8, 86]. Thus, I investigate the role of automation threat in firms' wage-setting power for workers performing different tasks, and Table 2.18 presents the results from the IV (2SLS) regressions. The results indicate that an increase in the labor market's exposure to industrial robots leads to higher wage markdowns over workers performing routine and manual tasks (columns (1) and (2)). In contrast, I find that robot exposure reduces markdowns over non-routine cognitive workers, i.e., robots might provide power to cognitive workers who are likely to be complementary with robots (column (3)). However, these markdown impacts for workers performing different tasks are generally statistically insignificant.

To further examine the heterogeneous effects of exposure to automation on the labor market, I combine the two dimensions of heterogeneity analyzed above and estimate the regressions for heterogeneous workers in East and West Germany. The estimation results suggest that an increase in robot exposure in the local labor market increases firms' wage-setting power for routine task-performing workers in East Germany (panel (a) in Figure 2.11). The coefficient estimate is positive, and conventional standard errors suggest that it is statistically significant at the 1% level; however, it is essentially zero when unconventional (or corrected for shift-share design) standard errors are applied. Despite this, the result is intuitive for several reasons. First, workers' outside options in East Germany are more limited than in West Germany as the East is relatively underdeveloped and has fewer and smaller employers [34]. Second, existing studies such as [125] show that workers in East Germany have a significant home bias that further shrinks workers' outside options. Third, firms in the automotive industry in East Germany might not have industrial robots installed since most of the major car manufacturers are in West Germany, i.e., automation threat is more prevalent in East Germany. The robot exposure does not affect wage markdowns over other workers in East Germany even under conventional standard errors. In the West, as shown in panel (b) of Figure 2.11, the point estimates for routine, nonroutine manual, and nonroutine cognitive workers are relatively smaller than the counterparts in East Germany with the same signs. However, the impacts are still not persistently estimated and generally not statistically significant.

Then, I investigate an additional heterogeneity by union coverage, which plays a central role in the German labor market and presents a significant heterogeneity in wage markdowns according to Section 2.3.2. Table 2.19 presents the heterogeneous effects on employment, mainly suggesting displacement effects for routine workers, except in West German districts with high union coverage, and the employment effects are not statistically significant. Table 2.20 then reports the heterogeneous effects on wages across districts with different union coverage in East and West Germany. The results suggest that the wage effects are more significant in magnitude in the East, consistent with the results above, and the impact in the East is slightly larger for routine workers. However, the wage effects are statistically insignificant, even under the additional heterogeneity by regions.

Figure 2.12 illustrates the results on the markdown effects. Further splitting the sample of districts around the national median of union coverage reveals that the robot exposure increases markdown for routine workers in local labor markets with low union coverage in East Germany. The effective F-statistic of [191] is below the threshold of 22 for the case of 10% potential bias and a 5% significance. However, it is above the threshold of 14 for the case of 20% potential bias and a 5% significance and the rule-of-thumb threshold of 10, indicating that the IVs provide plausible variations in the German local labor market's exposure to robots in the automobile industry that can be leveraged to identify a causal effect of robot exposure on wage markdowns for heterogeneous workers in low union districts from East Germany. The effective F-statistics for other sub-samples also suggest that the instruments are sufficiently strong. The estimated effect for such workers is 0.05 and statistically significant at the 1% and 5% significance levels under conventional and unconventional approaches (top left panel). The coefficient estimate for routine workers in low union coverage districts in West Germany has the same sign and is close in magnitude; however, it is not statistically significant (bottom left panel). For routine workers in high union coverage districts in East and West Germany, the coefficient estimates are approximately ten times smaller in magnitude and statistically insignificant (top-right and bottom-right panels).

In addition to the reasonings discussed above for routine workers in East Germany, this result, suggesting that the impact is more significant for districts with low union coverage, is consistent with the descriptive finding in Section 2.3.2 and findings from existing studies in the same context. For example, [86] suggest that the displacement effect is significant for districts with low union coverage, and thus, the displacement or automation threat is expected to be more in such areas.[35] The main findings indicate that, although not strongly significant, there is a pattern of displacement effect on routine workers. The impact of automation threat proxied by the robot exposure on wage markdowns is highly heterogeneous, increasing markdowns over routine workers in districts with weak worker protection in East Germany.

2.4.4 Robustness

I perform a battery of robustness checks focusing on the heterogeneous effects of robot exposure on labor market power, which is a primary labor market outcome in this paper.

[35]I also estimate the heterogeneous effects of robot exposure on wage markdowns in districts with high and low union coverage for all and heterogeneous workers. The results, available on request, generally suggest that the impacts are not statistically significant until we combine the heterogeneity by East/West, union coverage, and worker types.

Common Production Function for East and West Germany. In the baseline heterogeneity analysis by regions, I use wage markdowns based on production function estimated for East and West Germany since the production process and utilization of production inputs can vary across East and West regions. Thus, I check the robustness of my results on the effect of robot exposure on wage markdown using markdowns based on production function commonly estimated for East and West Germany, i.e., on the full sample of German manufacturing firms. Table 2.21 suggests the findings for heterogeneous workers in East and West districts stay unchanged. The results shown in Table 2.22 show that my primary finding is highly robust to alternative estimation procedure of wage markdowns, and the markdown effect for routine workers in low union districts from East Germany is more precisely estimated. Similarly to the baseline, the markdown impact of robots is not significant in districts from East Germany with high union coverage and all districts from West Germany, even for routine workers.

Alternative Split of Union Coverage. In the baseline analysis, I split the districts around the national median of union coverage to estimate the heterogeneous effects of robot exposure on markdowns in high and low union districts. Thus, I check the robustness of the results heterogeneous by union coverage using an alternative split, which also informs about which part of the distribution drives the impacts. The effects are concentrated in the bottom part of the distribution, specifically in the bottom eight deciles (Table 2.23). The effective F-statistic of [191] slightly suffers and goes below the threshold of 12 for the case of 30% potential bias and a 5% significance. However, the effective F-statistic of 9.4 is not well below the rule-of-thumb threshold of 10 for low union districts in East Germany. So, we can interpret the estimated coefficient as causal.

The markdown-increasing impact of robot exposure among workers performing routine tasks in low union districts from East Germany is robust under this alternative split of union coverage (column (1) in Panel A). For West Germany, the effective F-statistic goes down to 6.6 for low union districts, so the estimates are not necessarily causal. However, the estimated effects on markdowns for routine, nonroutine manual, and nonroutine cognitive workers are qualitatively the same as in the baseline (Panel B). I fail to estimate the impacts for districts in the top two deciles of the union coverage distribution because of statistical power issues.

Percentage Changes. I use the absolute changes in aggregate markdowns and predicted robot exposures in my baseline analysis. So, I test the robustness of my main findings from the IV regressions in Figure 2.12 by employing percentage changes in the outcome and the key explanatory variables. Table 2.24 reports the results. The main finding that robot exposure increases markdown for routine workers in districts from East Germany with low union coverage is robust as the estimate is still statistically significant at the 5% level. The markdown effects for groups are generally not statistically significant, the same as the baseline results.

Alternative Clusters at the Aggregate Regions. In my baseline analysis, I clustered the standard errors by districts or kreise at which my treatment variable is defined. As an alternative to this choice of cluster, I use aggregate regions as clusters following [86] who clustered the standard errors at the level of 50 aggregate labor market regions. Table 2.25 presents the 2SLS results for all and heterogeneous workers on the full sample, and the qualitative results are the same as those in Tables 2.15 and 2.18. I failed to check the robustness of my results for heterogeneous workers estimated on sub-samples of East and West Germany and districts with high and low union coverage as the number of clusters became too small when I split the sample. However, the results seem to be unaffected by the choice of clusters.

Adding a Treatment of Robot Exposure in Other Industries. As mentioned in Section 2.4, I leverage exposure to robots in the automotive industry in my baseline analysis because the instruments were not strong enough according to [191]'s weak IV test when I use robots in all manufacturing industries. Here, I check the robustness of my main findings by adding the local market's exposure to robots in other industries in Germany as an additional treatment variable instrumented by non-automotive robots in other high-income European countries. As shown in Table 2.26, the impacts of automation exposure on markdowns for routine workers in districts from East and West Germany with different degrees of union coverage are generally robust. However, the statistical significance tends to suffer under the unconventional approach.[36] I find that automotive robots drive the impact since the coefficient estimate on exposure to automotive robots is stronger than the estimate on exposure to other robots in magnitude and statistical significance. This result is consistent with [86], who studied the employment and wage effects of robots in Germany. We keep the full set of baseline controls, such as trade shocks and fixed effects, which minimize the omitted variables bias.

Industrial Robots in All Industries. As discussed earlier, I use robots in the automotive industry in my baseline analysis because the first-stage F-statistic

[36] In this specification with two endogenous variables, I show [154]'s statistic to check the relevance of excluded instruments because [191]'s weak IV test is not designed for the case of multiple endogenous variables. The joint F-statistic indicates that the instruments provide plausible variations in robot exposure in Germany that can be leveraged to identify causal effects.

of [191] was not large enough on the full sample with manufacturing plants from East and West Germany. Despite the failure of this formal test statistic, the first-stage relationship between the annual changes in exposure to industrial robots in all industries for Germany and other high-income European countries is strongly positively associated when estimated on the full sample (Figure A.12). However, this test statistic changes when I split the sample into East and West Germany, and it is reasonably plausible for sub-samples. The effective F-statistic of [191] is approximately equal to the threshold of 10 for the case of 30% potential bias and a 5% significance for low union districts in East Germany (Panel A of Table 2.27) and above the threshold of 18 for the case of 10% potential bias and a 5% significance for high union districts in West Germany (Panel B of Table 2.27). These values are either close to or higher than the rule-of-thumb threshold of 10, after which the weak instrument problem does not appear to affect the validity of conventional t statistics in the case of clustered standard errors [19]. The estimation results with alternative endogenous variables and instruments are generally robust, although the statistical significance tends to suffer under the unconventional approach. The effect of robot exposure on wage markdown for routine workers in low union districts from East Germany is positive (column (1) in Panel A). The other results are generally the same as the baseline counterparts.

Alternative Group of Instruments. In my baseline analysis, I instrument Germany's robot exposure by robot exposure in six other high-income European countries. As discussed earlier, [191]'s weak IV test and a traditional rule-of-thumb test suggest that these six instruments are jointly relevant. Figure 2.10 also shows a strong correlation between Germany's robot exposure and an individual country's robot exposure for all six countries. However, controlling for other covariates and fixed effects could change the relationship between the endogenous variable and individual instruments. Table 2.28 thus presents the relationship between instruments and the endogenous variable from the first-stage regression. Although the six instruments jointly satisfy the relevance assumption, the relationships between the endogenous variable and instruments from France and Italy are essentially zero, conditional on baseline covariates and fixed effects. The first-stage relationship for Spain, Norway, Sweden, and the UK is consistently positive and statistically significant at the 1% level (Column (1)), and it also remains the same in a specification including only these four countries (Column (2)). Leveraging robot exposure in Spain, Norway, Sweden, and the UK, Table 2.29 shows that my main results are remarkably robust to an alternative set of instruments that consist of the four countries.

2.4.5 On the Mechanism

The heterogeneous effects and robustness checks around those results generally inform the mechanisms through which firms increasingly set their workers' wages below the MRPL in response to an increase in their exposure to robots, i.e., what enables the firms to have wage markdowns over their workers. In addition, information on the number of robots at the firm from the IAB Establishment Panel data enables us to check whether the effects of robot exposure that we identify are the exposure effects or if it also captures the impact of actual robot adoption. Leveraging nationally representative survey data on realized robot adoption and industry-level information on the stock of robots in other high-income European countries, I estimate the relationship between actual robot adoption in Germany and robot exposure outside of the country using the following regression:

$$\text{Actual robot adoption}_{dt} = \alpha + \beta \text{Robot exposure shock}_{dt} + \phi_d + \varphi_{st} + \varepsilon_{dt}, \quad (2.7)$$

where $\text{Actual robot adoption}_{dt}$ is the number of robots adopted by German firms aggregated at the local labor market region or district d and expressed as per 1,000 workers in year t between 2014 and 2018, $\text{Robot exposure shock}_{dt}$ is the average stock of robots per 1,000 workers in other high-income European countries[37] defined at the same local labor market region level, ϕ_d and φ_{st} are respectively the district and state-by-year fixed effects. Since state-by-year fixed effects are controlled, state and time fixed effects are not necessary. Panel A of Table 2.30 presents the results, which suggest that exogenous variation in robot exposure from external sources does not predict the actual robot adoption in the country as the correlation is not statistically significant despite the positive coefficient.

In panel B, I report the results when I use annual changes in actual robot adoption and robot exposure shock. The relationship is negative and statistically insignificant.[38]

To further investigate the robot adoption behavior, Figure 2.13 shows the distribution of the average number of robots per plant in 2018 within the manufacturing sector. The first takeaway is that many firms in the bottom deciles use only a single robot in their production. The second observation is that the average number of robots used at the firm discretely changes along the distri-

[37]The countries are Spain, France, Italy, Norway, Sweden, and UK, which are included in my set of instruments, and I compute the simple arithmetic average of robots stock per 1,000 workers in these countries.

[38]Appendix A.2 conducts the robustness checks of the relationship between actual robot adoption in Germany and average robot exposure in other high-income European countries. Notably, the qualitative findings remain the same.

bution. The third takeaway is that robots are highly concentrated among robot adopters [93]. This discrete nature of robot adoption suggests that robot adoption is lumpy, especially in the manufacturing sector, which is consistent with data on robot adoption among manufacturing firms in different country contexts [see, e.g., 133].

Therefore, I control actual robot adoption at German manufacturing firms in addition to Germany's robot exposure instrumented by robot exposure in other countries. Since the data on robots used at the firm is available for only five years between 2014 and 2018, I first estimate the baseline IV regression over the same period. Table 2.31 presents the results.

The estimated effects of robot exposure on wage markdowns are not statistically significant in all specifications, except for a negative coefficient estimate for nonroutine manual workers in East German districts with high union coverage, potentially due to a small number of observations and weak statistical power. The relevance test results are also slightly noisy. However, these estimates are helpful to compare with those obtained by including actual robot adoption as an additional control, shown in Table 2.32. The qualitative and quantitative results generally remain the same, indicating that controlling for actual robot adoption does not affect the impact of robot exposure. The primary mechanism at play is thus the robot exposure.

2.5 Firm-Level Analysis

Now I switch the focus from local labor market analysis to establishment-level analysis, which complements the previous models. This section first checks the robustness of baseline effects from the local labor market approach by estimating the effects of automation exposure on markdown, employment, and wages at the firm level. Then, I further examine the potential mechanisms through which automation exposure affects labor market power by conducting additional heterogeneity analysis at the granular level.

2.5.1 Empirical Specification

I use a design that compares the outcomes of manufacturing firms that operate in local labor market regions with varying exposures to automation. Particularly, the estimating equation is

$$\Delta Y_{jt} = \beta \Delta \text{Robot} \widehat{\text{exposure}}_{rt} + \mathbf{Z'}_{jt-1}\gamma + \mathbf{X'}_{rt-1}\delta + \phi_j + \mu_{st} + \pi_{kt} + \varepsilon_{jt}, \tag{2.8}$$

where ΔY_{jt} represents the annual change in one of the outcomes, including markdown, employment, and wage, for firm j in Germany at year $t \in [1998, 2018]$. The term $\Delta \text{Robot} \widehat{\text{exposure}}_{rt}$ is the same as in equation (2.4), the annual change in local labor market's exposure to robots in Germany's automotive industry.[39,40] The vector $\mathbf{Z'}_{jt-1}$ includes firm-level controls, i.e., dummies for six plant size groups based on the number of employees in the previous year.[41] The vector $\mathbf{X'}_{rt-1}$ contains local labor market characteristics included in equation (2.4) and annual changes in trade exposure and ICT exposure at the labor market level. Leveraging the longitudinal structure of the IAB Establish-

[39] I use robot exposure at the local labor market level, instead of industry's exposure to robots, because the baseline results found in Section 2.4.3 suggest that the markdown effects are highly heterogeneous across regions particularly, East and West Germany. Thus, I employ the local labor market's exposure to robots to conduct heterogeneity across regions in the firm-level analysis.

[40] I also add robot exposure in other industries for a robustness check of the firm-level results.

[41] Given that information on plant's opening year has many missing observations in the IAB Establishment Panel data, I do not control for plant's age in the regressions.

ment Panel data, I control for a rich set of fixed effects at the granular level, including the firm fixed effects ϕ_j, federal state-by-year fixed effects μ_{st}, and two-digit industry-by-year fixed effects π_{kt}. The error term ε_{jt} captures the remaining unobserved, time-varying, and firm-specific factors. Heteroskedasticity-robust standard errors are clustered at the district or kreis level.

I consider three main outcomes in specification (2.8), including (i) plant-level markdowns estimated in Section 2.3, (ii) log employment or number of employees at the firm obtained from the IAB Establishment Panel data, and (iii) log wage per worker calculated by dividing total wage bill by total number of workers using data from IAB Establishment Panel. The identification strategy is similar to that was used for the local labor market-level analysis, i.e., instrument Germany's robot exposure with robots in other high-income European countries.

2.5.2 Employment and Wage Effects

To understand the consequences of automation threat on labor market power, I first analyze the employment and wage effects. Table 2.33 presents the baseline employment effects.

As shown in column (1), automation exposure reduces employment; however, the impact is only significant at the 10% level. However, when I estimate the effects on the employment of heterogeneous workers performing different tasks, I find that routine workers have been displaced due to automation exposure, which does not affect the employment of nonroutine manual and nonroutine cognitive workers. These results are strongly consistent with evidence from [86], who suggest that automation displaces routine workers in manufacturing with no employment impact on other workers at the individual level in the same context of the German labor market.

Then, I estimate the heterogeneous employment effects by East and West regions, and Table A.19 reports the results. The employment effects mainly come from East Germany, where the heterogeneous impacts across workers performing different tasks are more pronounced. In particular, the labor displacement effect for routine workers is still statistically significant at the 1% level, and the displacement effect becomes slightly more significant for nonroutine manual workers. Also, the results show that automation technologies complement nonroutine cognitive workers in East Germany. Like the local labor market-level analysis, I further split the sample of East and West German firms into those residing in districts with different union coverage. The results shown in Table A.20 suggest that the displacement effect of routine workers in East Germany concentrates among plants in low-union districts (top-left panel), which is consistent [86].

I next study the wage effects of robot exposure. Table 2.34 presents the results for wage changes. For both cases of homogeneous (column (1)) and heterogeneous (columns (2)-(4)) workers, wages do not respond to changes in the automation exposure. The null wage effects for heterogeneous workers performing different tasks are generally consistent with the results from [86]. As shown in Table A.21, I estimate the heterogeneous effects on average wages by region. The results indicate that the average compensation of heterogeneous workers is not still responsive even when we split the sample into East and West German establishments. Furthermore, I explore the wage effects by conducting additional heterogeneity by union coverage. However, I fail to find a significant impact on average wages in any of the cases, except for a weakly positive impact of robot exposure on wages of nonroutine cognitive workers in low-union districts (see Table A.22).

2.5.3 Markdown Effects

I present the results from estimating the effects of labor market exposure to automation on plant-level wage markdowns to check if the firm-level analysis replicates my baseline results from the local labor market approach. As shown in Panel A of Table 2.35, the impact of robot exposure on plant-level markdown is positive but not statistically significant for the full sample of Germany (column (1)) and the establishments from East Germany (column (2)). Although the coefficient estimate is statistically insignificant, it is negative for the establishment from West Germany (column (3)). Then, in Panel B of Table 2.35, I estimate the effect for heterogeneous workers performing different tasks and find that the effects are not significant, although the estimates are positive.

I estimate the markdown effects of automation exposure heterogeneous by region and job tasks, similar to the local market-level analysis. Table 2.36 shows the results, suggesting that wage markdown over routine workers increases as robot exposure intensifies in East Germany, and the positive effect is statistically significant at the 5% level. For other workers in East Germany and all three types of workers in West Germany, the impact of robot exposure on markdown is essentially zero. All these results are strongly consistent with those from the specification at the local markets, discussed in Section 2.4.3.

Table 2.37 further investigates the markdown effects heterogeneous across regions with different degrees of union coverage. The robot exposure weakly increases plant-level markdowns over routine workers in low-union districts from East Germany (column (1) in the top-left panel). The markdown effects in all other cells are strongly insignificant.[42]

The empirical evidence from firm-level and labor market-level analyses consistently suggests that exposure to industrial robots leads to lower employment and higher wage markdowns for routine workers. The employment and markdown effects are particularly significant in districts from East Germany with low labor protection. The wages are generally stagnant.

2.5.4 Further on the Mechanisms

The local labor market- and firm-level analyses yield three main results. First, workers performing routine tasks have been displaced by robots since the employment of such workers tends to decline as the local labor market gets more exposed to automotive robots. Second, robot exposure provides labor market power to employers, particularly over routine tasks-performing workers, who are subject to the risk of displacement from industrial robots. Third, this impact is heterogeneous across regions and unions. Thus, I further explore the mechanisms underlying these results by analyzing additional firm-level heterogeneity on markdown effects and discuss other potential channels through which automation exposure might affect employer power. This section also estimates

[42]The plant-level results on employment, wage, and markdown effects of robot exposure are robust to adding a local market's exposure to non-automotive robots in Germany instrumented by non-automotive robots in other high-income European countries as an additional treatment variable. The results from this robustness check are available on request.

additional heterogeneous effects that contribute to a better understanding of the labor market and employer power effects of robot exposure using firm-level data.

Unions. Several results in Sections 2.4.3 and 2.5 on heterogeneous impacts around unions suggest a potential mechanism. First, most importantly, automation exposure increases markdown over routine workers in districts with low union coverage in East Germany. This result is intuitive as workers are less protected in areas with low union coverage or less worker representation, enabling employers to threaten and intimidate workers in wage negotiation. Put differently, when employers' exposure to alternative sources of labor increases, further improving their outside options, the employer's power or their voice in bargaining with workers. This phenomenon is likely to occur in places where an individual worker bargains their wage with their employer without the unionized force of their fellow workers in the industry, for example, through trade unions. Second, nonroutine workers' markdown decreases and average wage increases, especially for nonroutine cognitive workers, in districts with low union coverage in East Germany. Third, there are no markdown effects at the establishment from West Germany, even under the union coverage heterogeneity.

Displacement Threats from Potential Automation. The employer power-increasing impact of robot exposure instrumented by plausibly exogenous shift-share factors is not the impact of actual robot adoption, but it could be the impact of robots that have not been realized yet. To further investigate this mechanism, I estimate the relationship between robot exposure predicted from the first-stage regression and actual robot adoption (Table 2.38). As expected, robot exposure predicted from the first-stage regression is positively correlated with the robot exposure, an outcome in the first-stage regression, and the relationship is highly significant for both automobile and all industrial robots (panel A). However, the robot exposure predicted from the first-stage regression is not associated with the actual robot adoption for any of the automobile and all industrial robots (panel B), confirming that the identified impact is not the effect of actual robot adoption.[43] Therefore, the impact of automation exposure on firms' labor market power that I have identified is likely through the channel of threats from potential automation.

Changes in Outside Options. Non-robot-adopting manufacturers still hire workers performing routine tasks and negotiate wages with them at some point, e.g., at the beginning of employment during hiring or on the job. Labor displacement or intensity of local labor market region's exposure to robots improves non-robot adopting firms' outside options via additional potential workers displaced away from other firms. Put differently, it provides more alternatives for firms to hire and reduces routine workers' layoff options. These changes in employers' and workers' outside options would translate into their bargaining and wage-setting power.

Additionally, as suggested by the empirical results in Sections 2.4.3 and 2.5, robot exposure increases employers' labor market power in East Germany, and [34] show that manufacturing firms in East Germany are smaller in size and less productive than West Germany and suggest that it is due to higher monopsony power. Given smaller plants, outside options for workers in East Germany are limited due to few available positions. Workers' job flows biased towards their home region is also significantly large in East Germany [125], suggesting

[43] This section zeroes in on robots in the automotive industry, which was the focus of the baseline analysis. However, the results stay the same when I use exposure to robots in all industries as an alternative (Table A.24).

a strong home bias in workers' preference. Therefore, it is likely that routine workers' response to automation threats could be more effective in East Germany due to the pre-existing condition of the region.

Heterogeneous Effects among Firms with Different Size. The probability of future robot adoption is higher for larger firms [156, 93]; thus, the displacement thread is likely to be more prevalent among larger firms. Hence, I estimate the plant-level effects of robot exposure heterogeneous by firm size. Table A.23 presents the heterogeneous effects on markdowns for all and heterogeneous workers. Although the coefficient estimates are not statistically significant, the impacts of robot exposure on plant-level markdowns for all workers are positive for small and large firms; however, the magnitude of the estimate is much more significant for large firms than for small firms (Column (1)). The estimated effects for heterogeneous workers in Columns (2)-(4) suggest that the markdown-increasing impact is most significant for routine workers at large firms; however, the estimates are not statistically significant. Figure 2.14 further investigates the heterogeneous effects by firm size on top of the heterogeneity by East and West Germany. The results suggest that robot exposure increases wage markdown for routine workers at large firms in East Germany.[44] Consistent with the markdown effect, I find that robot exposure leads to a decrease in wages at large firms, although the coefficient estimates are not statistically significant (Figure A.7).

The labor displacement effects for routine workers are intuitively concentrated among large firms that are more likely to adopt robots (Figure A.8). However, this employment effect is statistically significant in West Germany, where

[44] Appendix A.6.1 shows that the heterogeneous effects of robot exposure on plant-level wage markdowns by firm size are strongly robust to alternative definitions of large firms.

robot adoption is potentially more likely to be realized. The displacement threat, which should not affect employment, might be driving the effects of robot exposure on routine workers' bargaining power in East Germany.

Heterogeneous Effects at Different Periods. As discussed in Section 2.3.3 and shown in Table 2.7, aggregate wage markdowns leveled off at a relatively high level until the 2009 Great Recession. After the recession, the aggregate markdown sharply decreased primarily due to wage increases as workers became more mindful of their wages. As presented in Section 2.2.1, wage inequality presents an upward trend and peaks in the late 2000s [53]. So, I consider there could be heterogeneity around this period in the effect of robot exposure on markdowns. Figure 2.15 reports the markdown effects of robot exposure in East and West Germany around the late 2000s, suggesting that the effects were concentrated before 2009.[45] Consistent with the changes in workers' awareness of their wages after the Great Recession and the decline in wage inequality after 2010, the impacts of robot exposure on wages of workers who are complementary or not substitutable are positive in East and West Germany after 2009 (Figure A.9).

Another reason that I identify significant markdown effects of robot exposure could be that most of the variations in robot exposure in other high-income Europeans that introduce variations in robot exposure in Germany occurred before 2009 (see panel (b) of Figure 2.9). The penetration of manufacturing robots in the automobile industry grew until 2008 in European countries other than Germany, while it leveled off and even presented a slight downward trend from

[45]Since the task intensity measure used for the classification of routine, nonroutine manual, and nonroutine cognitive workers did not change until 2012 (see Section 2.3.5), the results suggesting the effects are mainly concentrated before the 2009 Great Recession reassure that the baseline findings are not affected by the worker classification based on task intensity measure discretely changing over time.

then until 2019.

Heterogeneous Effects across Different Industries. To better understand which industries drive the labor market effects of exposure to automation, I estimate the heterogeneous effects by sectors with different intensities in automobile robots (Figure 2.16). Although I use robots only in the automobile industry, firms in various manufacturing industries in different districts have varying exposure or intensity to automobile robots because automobile robots are predicted to the local labor market regions based on their employment shares, and thus establishments operate in different industries in those local communities have unequal exposure to automobile robots. The average annual change in district-level automobile robots per 1,000 workers in the country is 0.248 between 1998-2018. I define industries with annual change higher than this national average as industries intensive in automobile robots, i.e., robot-intensive industries. The robot-intensive industries include those that produce (i) food, (ii) beverages, (iii) leather products, (iv) wood products, (v) paper products, (vi) pharmaceuticals, (vii) fabricated metals (excluding machinery and equipment), (viii) machinery and equipment, (ix) motor vehicles, and (x) other manufacturing. The regression results show that the markdown-increasing effects are intuitively more significant in robot-intensive industries in East Germany.

I then estimate the employment effects of robot exposure for firms operating in robot-intensive and non-robot-intensive industries. The estimation results, available on request, show that the negative impact on routine workers' employment is statistically significant for firms in the robot-intensive industries. However, the employment effect for routine workers is not statistically significant for plants in industries where automobile robots are not prevalent. These results are intuitive and consistent with the expectation. Then, I add heterogeneity by East and West regions. Figure 2.17 depicts the results from these regressions. The employment effect is negative and statistically significant for routine workers employed at plants in both robot-intensive and non-robot-intensive industries in East Germany. The displacement effect, however, is more significant in robot-intensive industries in magnitude and statistical significance.

Firm Mobility across Regions. The firm's labor market outcomes, like wage markdowns, could change due to the relocation of the firm in response to changes in the local labor market region's exposure to industrial robots. The estimated impacts of robot exposure could be thus partly due to the changes in the location. Controlling for state-by-year fixed effects accounts for the firm mobility across states or changes that potentially lead firms' move across states over time [42]. In addition to state-by-year and firm fixed effects, we can add district fixed effects, and it can capture district-level time-invariant characteristics that attract firms. Appendix A.6.2 checks the robustness of the heterogeneous effects of robot exposure on wage markdowns in East and West Germany by adding district fixed effects, showing that the results are substantially robust.

The district fixed effects are not included in specifications for East and West German districts with different union coverage since the coefficient estimate on predicted robot exposure was dropped when I added the district fixed effects on top of all the other fixed effects and controls. It could indicate that there is not much firm mobility across districts. Thus, it is implausible to consider the results are primarily driven by firm mobility across regions.

2.6 A Wage Bargaining Model of Automation Threat

The main empirical findings suggest that automation threat, proxied by exposure to industrial robots, increases wage markdown, a measure of monopsony and labor market power, for workers performing routine workers. This markdown impact is particularly significant in East German local labor market regions with low union coverage or weak worker protection.

This section presents a simple conceptual model of wage bargaining. The framework is based on the right-to-manage model of collective bargaining (as proposed by [188]), where a union and an employer (or employers association) bargain over wages only and then the firm unilaterally chooses an employment level at the bargained wage. The changes in the potential adoption of labor-saving technologies are likely to affect firms' outside options, which feed into the wage negotiation between the employer and workers. So, I offer a bargaining model to characterize the impact of automation threat on firms' labor market power. I do not seek to estimate or calibrate the model; instead, I derive qualitative predictions on hypotheses, some of which I empirically test with German data in the previous sections.

2.6.1 Setup

Consider a firm that employs two types of workers (routine and nonroutine) to produce an output via the following production function:

$$Q = F(l_L, l_H), \tag{2.9}$$

where l_L and l_H are the firm's employment of routine and nonroutine labor, respectively. This production function is assumed to be constant returns to scale and thus exhibits diminishing marginal product of labor. I assume that the output market is perfectly competitive (the firm is a price-taker in the output market).

Let $W_L(l_L)$ and $W_H(l_H)$ be the labor supply curve of routine and nonroutine workers, respectively, given the number of workers l_L and l_H. $W_L(l_L)$ and $W_H(l_H)$ are the opportunity cost of working for a firm—a firm must pay at least W_L and W_H for l_L and l_H, but the firm can choose to pay more. Let w_L and w_H be the actual wages paid by the firm to routine and nonroutine workers, respectively. The firm's objective function or the profit function is thus:

$$\pi(w_L, w_H) = Q - w_L l_L - w_H l_H, \tag{2.10}$$

where the output price is normalized to unity.

Given the heterogeneous workers in the model and the significant role of worker group-specific unions in the German labor market, particularly before the 2015 "unity law",[46] I consider two types of bargaining, including *separate* and *joint* bargaining. I consider each of these two cases below.

Separate Bargaining. The firm simultaneously bargains with a union (or unions) representing routine and nonroutine workers separately. The Nash bargaining problem between the firm and the union representing routine and nonroutine workers is:

$$\operatorname*{Max}_{w_L} \left(Q - w_L l_L - w_H l_H - \bar{\pi}_L\right)^{\alpha} \left(w_L l_L - W_L(l_L) l_L\right)^{1-\alpha},$$

$$\operatorname*{Max}_{w_H} \left(Q - w_L l_L - w_H l_H - \bar{\pi}_H\right)^{\beta} \left(w_H l_H - W_H(l_H) l_H\right)^{1-\beta},$$

[46] In 2015, the German government passed the "unity law" with support from both unions and employer associations to undermine employers' bargaining with occupation-specific unions.

where α and β are the firm's bargaining strength over routine and nonroutine workers, and $\bar{\pi}_L$ and $\bar{\pi}_H$ are the firm's threat point or the fallback profit for bargaining with routine and nonroutine workers, respectively.

The firm's fallback profit when the firm's agreement with routine workers falls apart, $\bar{\pi}_L$, depends on the number of robots and price of robots as the firm purchases robots as a production input to complete routine jobs in the complete or partial absence of routine workers. The price or rent of robots is denoted by r, and the change in r characterizes the threat of automation. For example, the automation threat increases when the robots become more affordable or in the event of a decrease in the rents of robots. The relationship between the price of robots and the threat point for routine workers is:

$$\frac{\partial \bar{\pi}_L}{\partial r} < 0,$$

since the firm's outside option expands and the threat point increases as the price of robots decreases or automation threat increases.[47] I assume that the firm's threat point for bargaining with nonroutine workers $\bar{\pi}_H$ does not directly depend on r because robots are unlikely substitutes for nonroutine workers.

Joint Bargaining. The firm jointly bargains with a union representing both types of workers or negotiates with a labor union that maximizes the aggregate utility of routine and nonroutine workers. The Nash bargaining problem is represented by the following maximization problem:

$$\underset{w_L, w_H}{\text{Max}} \left(Q - w_L l_L - w_H l_H - \bar{\pi}_{LH}\right)^{1-\gamma_L-\gamma_H} \left(w_L l_L - W_L(l_L) l_L\right)^{\gamma_L} \left(w_H l_H - W_H(l_H) l_H\right)^{\gamma_H},$$

[47]The production and the fallback profit when the firm does not reach an agreement with routine workers ($Q = F(l_H, k)$ and $\bar{\pi}_L$, respectively, where k is the number of robots) cannot be zero in either case in which routine and nonroutine workers are substitutes or complements as long as robots can complete routine tasks. Put differently, the impact of automation threat on the firm's fallback profit for bargaining with routine workers $\frac{\partial \bar{\pi}_L}{\partial r}$ is unlikely to be zero, which is supported by the empirical findings.

where γ_L and γ_H are the firm's bargaining strength over routine and nonroutine workers, respectively, and $\bar{\pi}_{LH}$ is the firm's threat point for bargaining with a union representing both types of workers. The impact of r on the threat point for the union maximizing the aggregate utility of routine and nonroutine workers theoretically can be as follows, e.g., depending on the production function (i.e., whether the two types of labor are substitutes or complementary):

$$\begin{cases} \frac{\partial \bar{\pi}_{LH}}{\partial r} > 0 \\ \frac{\partial \bar{\pi}_{LH}}{\partial r} = 0 \\ \frac{\partial \bar{\pi}_{LH}}{\partial r} < 0 \end{cases}$$

where the first case is highly unlikely, and the second case is not supported by the empirical evidence.[48] So, I consider that the firm's fallback profit under joint bargaining $\bar{\pi}_{LH}$ increases when the robot adoption becomes more affordable (or the price of robots r drops).

2.6.2 Automation Threat and Wage Bargaining

Given the impact of automation threat or the price of robots on the firm's threat point, I derive the effects of threat point on the firm's bargaining outcomes to show the effects of automation threat on bargaining position and wage markdowns. I consider these impacts under the two types of bargaining processes.

Separate Bargaining. First, consider the Nash bargaining problem between the firm and routine workers, and the first order condition with respect to w_L

[48] The firm's threat point under joint bargaining $\bar{\pi}_{LH}$ can be zero if the firm cannot produce anything with only robots and without using any human labor, and thus the $\frac{\partial \bar{\pi}_{LH}}{\partial r}$ can be zero.

yields:

$$w_L l_L = (1-\alpha)(Q - w_H l_H - \bar{\pi}_L) + \alpha W_L(l_L) l_L. \tag{2.11}$$

Second, the first order condition from the Nash bargaining problem between the firm and non-routine workers with respect to w_H similarly yields:

$$w_H l_H = (1-\beta)(Q - w_L l_L - \bar{\pi}_H) + \beta W_H(l_H) l_H. \tag{2.12}$$

Solving (2.11) and (2.12), we derive the size of the pie that goes to routine and nonroutine workers, i.e., the bargaining outcomes for routine and nonroutine workers as

$$w_L l_L = \frac{(1-\alpha)(\beta Q + (1-\beta)\bar{\pi}_H - \beta W_H(l_H) l_H - \bar{\pi}_L) + \alpha W_L(l_L) l_L}{1-(1-\alpha)(1-\beta)}, \tag{2.13}$$

and

$$w_H l_H = \frac{(1-\beta)(\alpha Q + (1-\alpha)\bar{\pi}_L - \alpha W_L(l_L) l_L - \bar{\pi}_H) + \beta W_H(l_H) l_H}{1-(1-\alpha)(1-\beta)}. \tag{2.14}$$

The comparative statics yield

$$\begin{aligned} \frac{\partial w_L}{\partial \bar{\pi}_L} &= \frac{\partial Y_L}{\partial \bar{\pi}_L} = -\frac{1-\alpha}{1-(1-\alpha)(1-\beta)} < 0, \\ \frac{\partial w_H}{\partial \bar{\pi}_L} &= \frac{\partial Y_H}{\partial \bar{\pi}_L} = \frac{(1-\alpha)(1-\beta)}{1-(1-\alpha)(1-\beta)} > 0, \\ \frac{\partial (Q - Y_L - Y_H)}{\partial \bar{\pi}_L} &= \frac{\partial Q}{\partial \bar{\pi}_L} - \frac{\partial Y_L}{\partial \bar{\pi}_L} - \frac{\partial Y_H}{\partial \bar{\pi}_L} = \frac{\beta(1-\alpha)}{1-(1-\alpha)(1-\beta)} > 0, \end{aligned} \tag{2.15}$$

where $Y_L = w_L l_L$ and $Y_H = w_H l_H$ are the wage income or size of the pie that goes to routine and nonroutine workers, respectively. It gives the following proposition:

Proposition 1 *Suppose that the firm separately bargains with the union(s) representing routine and nonroutine workers.*

1. *w_L and Y_L decrease when $\bar{\pi}_L$ or automation threat increases.*

2. *w_H and Y_H increase when $\bar{\pi}_L$ or automation threat increases.*

3. *An increase in wage and bargaining outcome for nonroutine workers equals $1-\beta$ times a decline in wage and bargaining outcomes for routine workers, where $0 \leq \beta \leq 1$. It indicates an overall decrease in workers' wages and bargaining outcomes.*

4. *The firm profit, $Q - Y_L - Y_H$, increases when $\bar{\pi}_L$ or automation threat increases.*

An increase in automation threat due to an expansion of the potential of automation raises firms' outside options and their fallback profit from negotiation with routine workers, $\bar{\pi}_L$. Given this, the firm can set wages below the marginal product of labor for routine workers and save some profits by reducing labor costs of such workers, Y_L. Nonroutine workers expect this to occur and can request the firm to raise their wages above and beyond their marginal product of labor, which would increase the labor costs of nonroutine workers, Y_H. Separate bargaining, e.g., via worker group-specific unions, can moderate heterogeneous effects of automation threat on the firm's bargaining outcomes over different workers. An increase in nonroutine workers' wage and bargaining outcome equals $1-\beta$ times a decline in routine workers' wage and bargaining outcome, where $0 \leq \beta \leq 1$, indicating an increase in the firm's profit due to automation threat at the expense of routine workers.

However, l_L and l_H are independent of $\bar{\pi}_L$, primarily following the empirical evidence on the insignificant or imprecisely estimated employment effects of robot exposure in German manufacturing according to this and other papers like [86], possibly since robot exposure mainly proxies the automation threats rather than actual robot adoption, which could have a more significant impact on employment. Since there is no employment effect of automation threat or

$\bar{\pi}_L$ in the model, $\bar{\pi}_L$ also does not affect the marginal product of labor, and it affects the wage markdowns only through its impact on wages. It provides the following proposition:

Proposition 2 *Under the separate bargaining regime, wage markdowns over routine (nonroutine) workers increase (decrease) when $\bar{\pi}_L$ or automation threat increases.*

Joint Bargaining. Now consider that the firm jointly bargains with a union (or unions) representing different types of workers. Suppose a labor union that represents the two types of workers for simplicity, and the results remain the same even if there are multiple unions. In this case of joint bargaining, the union maximizes the aggregate utility of all types of workers. The union, thus, considers both routine and nonroutine workers. Similar to the separate bargaining, the displacement threat of routine workers will also increase in response to the decline in the price of robots. However, this situation will be considered in the bargaining action of the union that also represents the nonroutine workers.

Consider the Nash bargaining problem between the firm and the union representing different types of workers, described above, and the first order conditions with respect to wages of routine and nonroutine workers yield the following, respectively:

$$w_L l_L = \left(\frac{\gamma_L}{1-\gamma_H}\right)(Q - w_H l_H - \bar{\pi}_{LH}) + \left(\frac{1-\gamma_L-\gamma_H}{1-\gamma_H}\right) W_L(l_L) l_L, \tag{2.16}$$

and

$$w_H l_H = \left(\frac{\gamma_H}{1-\gamma_L}\right)(Q - w_L l_L - \bar{\pi}_{LH}) + \left(\frac{1-\gamma_L-\gamma_H}{1-\gamma_L}\right) W_H(l_H) l_H. \tag{2.17}$$

Solving (2.16) and (2.17), we derive the size of the pie that goes to routine and nonroutine workers, i.e., the bargaining outcomes for such workers as

$$w_L l_L = \gamma_L(Q - \bar{\pi}_{LH} - W_H(l_H)l_H) + (1 - \gamma_L)W_L(l_L)l_L, \tag{2.18}$$

and

$$w_H l_H = \gamma_H(Q - \bar{\pi}_{LH} - W_L(l_L)l_L) + (1 - \gamma_H)W_H(l_H)l_H. \tag{2.19}$$

The comparative statics yield

$$\begin{aligned} \frac{\partial w_L}{\partial \bar{\pi}_{LH}} &= \frac{\partial Y_L}{\partial \bar{\pi}_{LH}} = -\gamma_L < 0, \\ \frac{\partial w_H}{\partial \bar{\pi}_{LH}} &= \frac{\partial Y_H}{\partial \bar{\pi}_{LH}} = -\gamma_H < 0, \\ \frac{\partial (Q - Y_L - Y_H)}{\partial \bar{\pi}_{LH}} &= \frac{\partial Q}{\partial \bar{\pi}_{LH}} - \frac{\partial Y_L}{\partial \bar{\pi}_{LH}} - \frac{\partial Y_H}{\partial \bar{\pi}_{LH}} = \gamma_L + \gamma_H > 0, \end{aligned} \tag{2.20}$$

and provide the following proposition:

Proposition 3 *Suppose that the firm jointly bargains with the union representing routine and nonroutine workers.*

1. *w_L and Y_L decrease when $\bar{\pi}_{LH}$ or automation threat increases.*
2. *w_H and Y_H decrease when $\bar{\pi}_{LH}$ or automation threat increases.*
3. *The firm profit, $Q - Y_L - Y_H$, increases when $\bar{\pi}_{LH}$ or automation threat increases.*

Under the joint bargaining between the firm and the union representing all workers, the effects of automation threat on wages and bargaining outcomes of heterogeneous workers are homogeneous, i.e., the impacts are in the same direction for different workers. The intuition is that nonroutine workers are worse off due to the presence of routine workers under the threat of displacement in the union under the joint bargaining regime. Routine workers, on the other hand, might be better off under joint bargaining compared to their state under separate bargaining via redistribution effect due to the presence of nonroutine workers who are not subject to the automation threat. However, it depends on the values of bargaining parameters, α, β, and γ_L.

Given the assumption of no employment effects, we have the following proposition on the markdown impact of automation threat:

Proposition 4 *Under the joint bargaining regime, wage markdowns over routine and nonroutine workers increase when $\bar{\pi}_{LH}$ or automation threat increases.*

2.6.3 Discussion

The empirical findings suggest that the causal impact of automation threat on wage markdowns is heterogeneous for workers performing different tasks in German manufacturing. In particular, automation threat increases markdowns over routine workers and reduces markdowns over non-routine workers. The main qualitative predictions from the wage bargaining model developed in this Section consistently show the heterogeneous effects of automation threat on wages, bargaining outcomes, and wage markdowns of different workers under the separate bargaining between the firm and the workers. Due to the data limitation, for example, on union coverage for heterogeneous workers, I cannot directly show whether the heterogeneous effects of automation on labor market power over different workers are mediated through separate bargaining. However, I argue that separate bargaining plays an important role in the impacts I identified in the empirical analysis for several reasons. First, as described in [143], the collective bargaining system in Germany is unusually flexible, and some unions are organized at the occupation, skill, and experience group level. A group of workers is likely to avoid jointly bargaining with the employer by teaming up with another group of vulnerable workers, and this behavior could have been one of the reasons for creating occupation-specific unions in the first place. Second, heterogeneous impacts of robot exposure on wage markdowns around the Great Recession suggest that the results are concentrated before 2009, i.e., when the bargaining between the firm and occupation-specific unions was more prevalent. So, joint bargaining is unlikely, especially in the presence of a venue to bargain separately, e.g., through occupation-specific unions, and thus, separate bargaining is plausible.

2.7 Conclusion

There is growing evidence that the labor market is not perfectly competitive and employers have substantial market power in the labor markets. In this paper, I document that workers earn 79 cents on each marginal euro generated in an average German manufacturing plant. I also find that workers performing nonroutine manual (routine) tasks are subject to the highest (lowest) degree of labor market power in the manufacturing industry. To explore a driving force that gives employers such power in the labor market, I provide empirical evidence on automation threat as a significant source of labor market power. Using administrative and survey data from Germany, I show that exposure to industrial robots equips firms with more power in the labor market over workers in occupations that mainly perform routine tasks. This impact is particularly pronounced in local labor markets with weaker labor protections in East Germany, where the labor market is less competitive and workers have limited outside options. The firm-level analysis also shows that the effect of robot exposure on wage markdowns is concentrated among large firms and those in robot-intensive industries before 2009, in which most of the automation actions took place and worker group-specific unions were more prevalent. These empirical results are generally consistent with qualitative predictions from the wage bargaining model developed in the paper.

This evidence has three critical implications for understanding the wage-setting process in the labor markets. First, workers' mobility and skill sets play a substantial role in setting the wages, given that immobile workers who perform nonroutine cognitive tasks and low-skilled, nonroutine manual task-performing workers are subject to higher markdowns than routine workers. Second, although routine workers have lower markdowns than nonroutine manual and nonroutine cognitive workers, labor-saving technologies biased towards displacing routine tasks provide wage-setting power to employers over workers in occupations performing routine tasks potentially via threats. Third, a potential threat mechanism is more significant in regions where workers have limited outside options potentially due to specific preferences (e.g., home bias) and characteristics of local labor markets (e.g., the strength of trade unions).

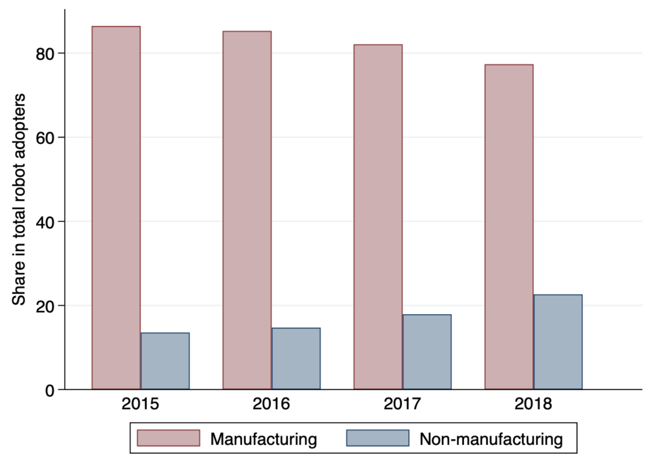
This paper made notable contributions to several strands of literature. First, this study provides the first causal interpretation of the effects of automation threat or robot exposure on labor market power. The lack of relationship between actual robot adoption in Germany and robot exposure shock from other high-income European countries suggests that the implication of automation technologies from external sources on labor market power is mainly through automation threats. Second, I investigate the relationship between the task content of jobs and labor market power and examine the effects of automation threat on labor market power for heterogeneous workers performing routine, nonroutine manual, and nonroutine cognitive tasks. Third, the paper also adds to the literature globally measuring the labor market power by providing an estimate of monopsony power in German manufacturing using a dataset that has not been used before in the context. Finally, by developing a simple wage bargaining model incorporating the threat of automation, I formalize the role of automation threat in firms' and workers' bargaining power. The proposed model with heterogeneous workers also provides new insight, suggesting that the bargaining type plays a critical role in the interaction between automation threat and firms' and workers' bargaining outcomes and wage markdowns.

I conclude with some caveats and directions for future research. First, the empirical results on the heterogeneous effects of automation threat on markdowns over different workers and the presence of unions representing specific groups of workers imply the role of the bargaining regime suggested by the theoretical model. However, due to data limitations on union coverage for heterogeneous workers, this paper could not directly test the importance of separate bargaining. So, if the data allows, future research can explicitly examine the role of bargaining regimes. Second, the wage bargaining model proposed in this paper might explain the industrial relations in East Germany better than in West Germany since the impact of automation threat on wage markdowns over routine workers in German manufacturing is more significant in the East, while the effect is essentially zero in the West. As shown in this paper and other papers, the labor market competition and other related conditions are different across East and West Germany, potentially due to the underlying differences across regions, such as differences in workers' pre-existing outside options and preferences. Future research could thus explore a model that can characterize the regional differences in explaining the heterogeneous effects in East and West Germany.

2.8 Figures and Tables

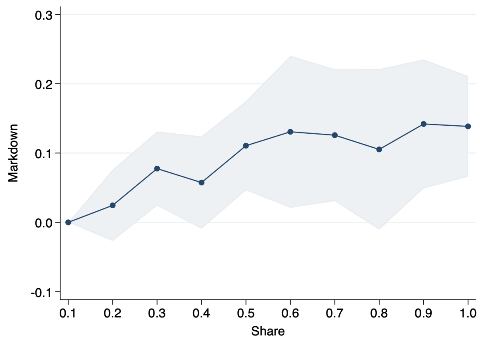
2.8.1 Figures

Figure 2.1: Robot Adopters by Industry

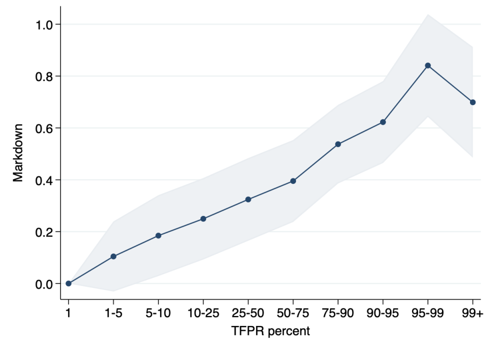


Notes: The figure plots the share of manufacturing and non-manufacturing robot adopters in the total number of robot adopters between 2015-2018 using data from the IAB Establishment Panel (IAB BP). The 2014 data was not presented for compliance with data privacy.

Figure 2.2: Relationship between Plant-Level Markdown and Firm Characteristics



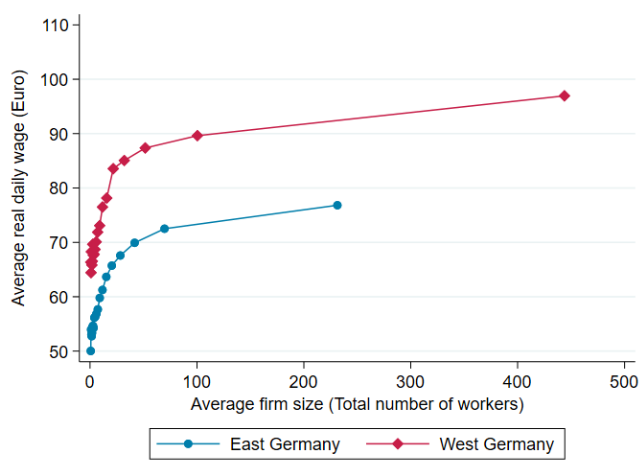
(a) Size



(b) Productivity

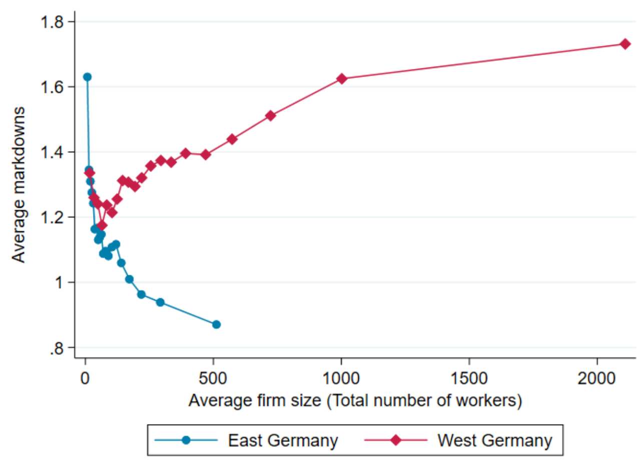
Notes: Based on the IAB Establishment Panel from 1997-2018. Panel (a) illustrates the OLS coefficients from estimating plant-level markdowns on size (measured by employment share) indicators. The smallest size indicator is omitted, and coefficients thus reflect deviations relative to this reference group. The plants included in the reference group labeled as "0.1" are those with employment shares $s \in (0, 0.1]$. Other indicators are similarly defined. Panel (b) shows the OLS coefficients from estimating plant-level markdowns on productivity. The first percentile of productivity is omitted, and coefficients thus reflect deviations relative to this reference group. All regressions include dummies for three-digit industry, district, and year fixed effects. Standard errors are clustered at the level of three-digit WZ 2008 industries.

Figure 2.3: Wage-Size Ladders



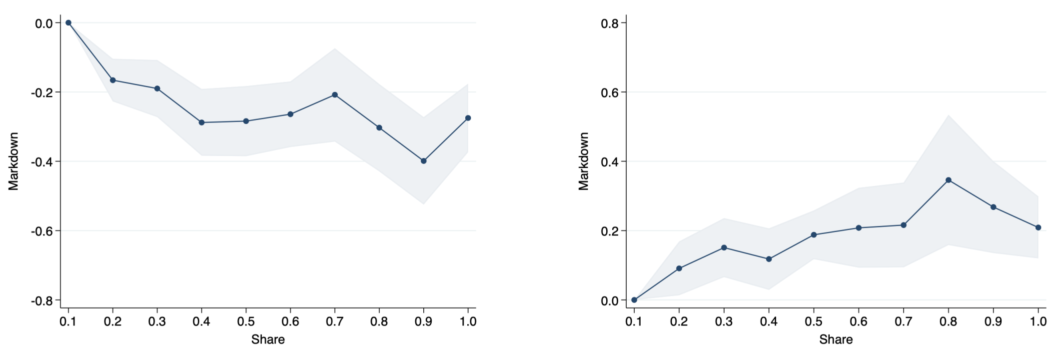
Notes: The figure plots the average number of workers for each twentile of the firm size distribution against the average real daily wage of firms in the twentile, where the wages and size are residualized by industry and year fixed effects.

Figure 2.4: Markdown-Size Ladders



Notes: The figure plots the average number of workers for each twentile of the firm size distribution against the average wage markdowns in the twentile, where the markdowns and size are residualized by industry and year fixed effects.

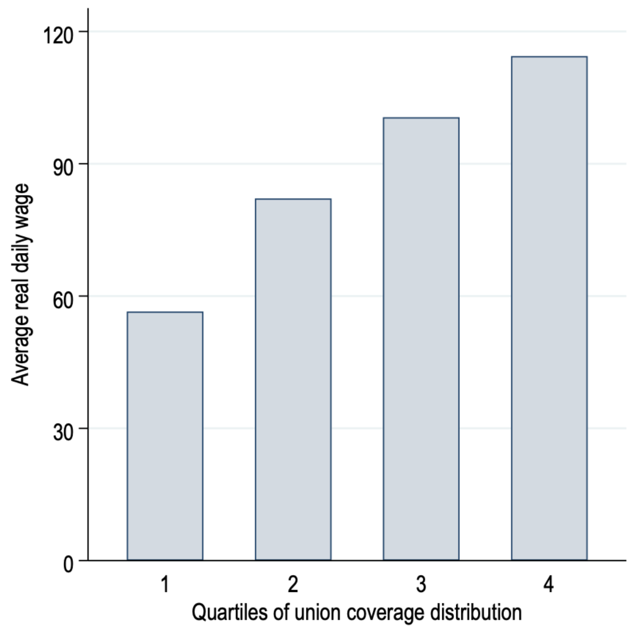
Figure 2.5: Markdown-Size Relationship in East and West Germany



(a) East Germany (b) West Germany

Notes: Based on the IAB Establishment Panel (IAB BP) from 1997-2018. The data on firms' location of operation comes from the LIAB and matched with the IAB BP. The figure plots the point estimates and 95% confidence intervals from estimating plant-level markdowns on size (measured by employment share) indicators in East (panel (a)) and West (panel (b)) Germany. The wage markdowns are estimated separately for East and West German firms, separately. The smallest size indicator is omitted, and coefficients thus reflect deviations relative to this reference group. The plants included in the reference group labeled as "0.1" are those with employment shares $s \in (0, 0.1]$. Other indicator variables are similarly defined. All regressions include dummies for three-digit industry, district, and year fixed effects. Standard errors (SEs) are clustered at the level of three-digit WZ 2008 industries.

Figure 2.6: Average Real Daily Wage along the Distribution of Union Coverage

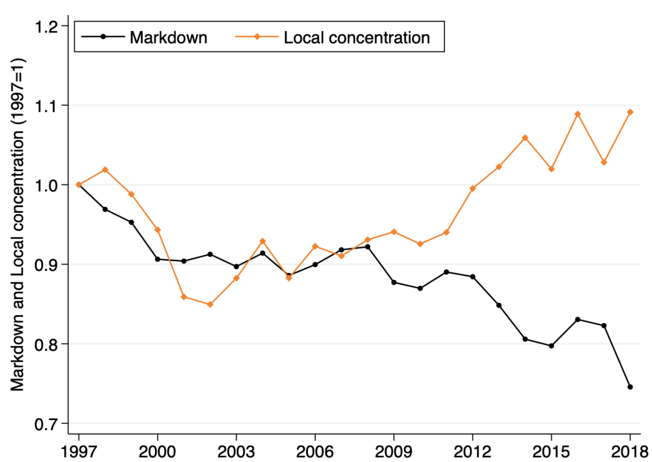


Notes: Based on the IAB Establishment Panel and the matched employer-employee (LIAB) data. The figure show the average real daily wage per worker at the firm in different quartiles of the union coverage. The firm-level union coverage is measured by the share of workers covered by trade unions in total workers using the IAB establishment panel data. The average daily wage per worker at the firm is calculated using the LIAB data.

Figure 2.7: Time Evolution of the Aggregate Markdown

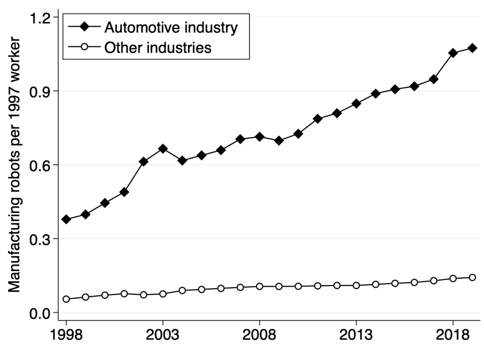
Notes: Markdowns are constructed using the IAB Establishment Panel (IAB BP) data from 1997-2018 under the assumption of translog production and aggregated according to expressions (A.23) and (A.25). The employment share of labor market ω_{klt} is based on total number of employees.

Figure 2.8: Aggregate Markdowns and Local Concentration

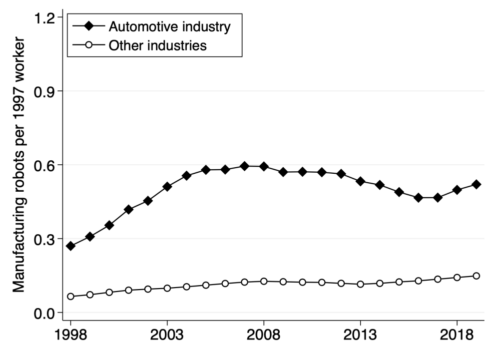


Notes: Based on the IAB Establishment Panel (IAB BP). The solid black line shows the time trend of the aggregate markdown as in equation (A.25), and the orange line shows the time trend of employment-based labor market concentration as in equation (A.29). The aggregate markdown and local concentration index are normalized relative to their initial value by 1997.

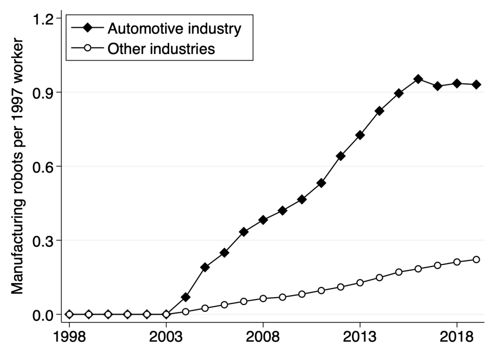
Figure 2.9: Penetration of Manufacturing Robots in Automotive and Other Industries



(a) Germany



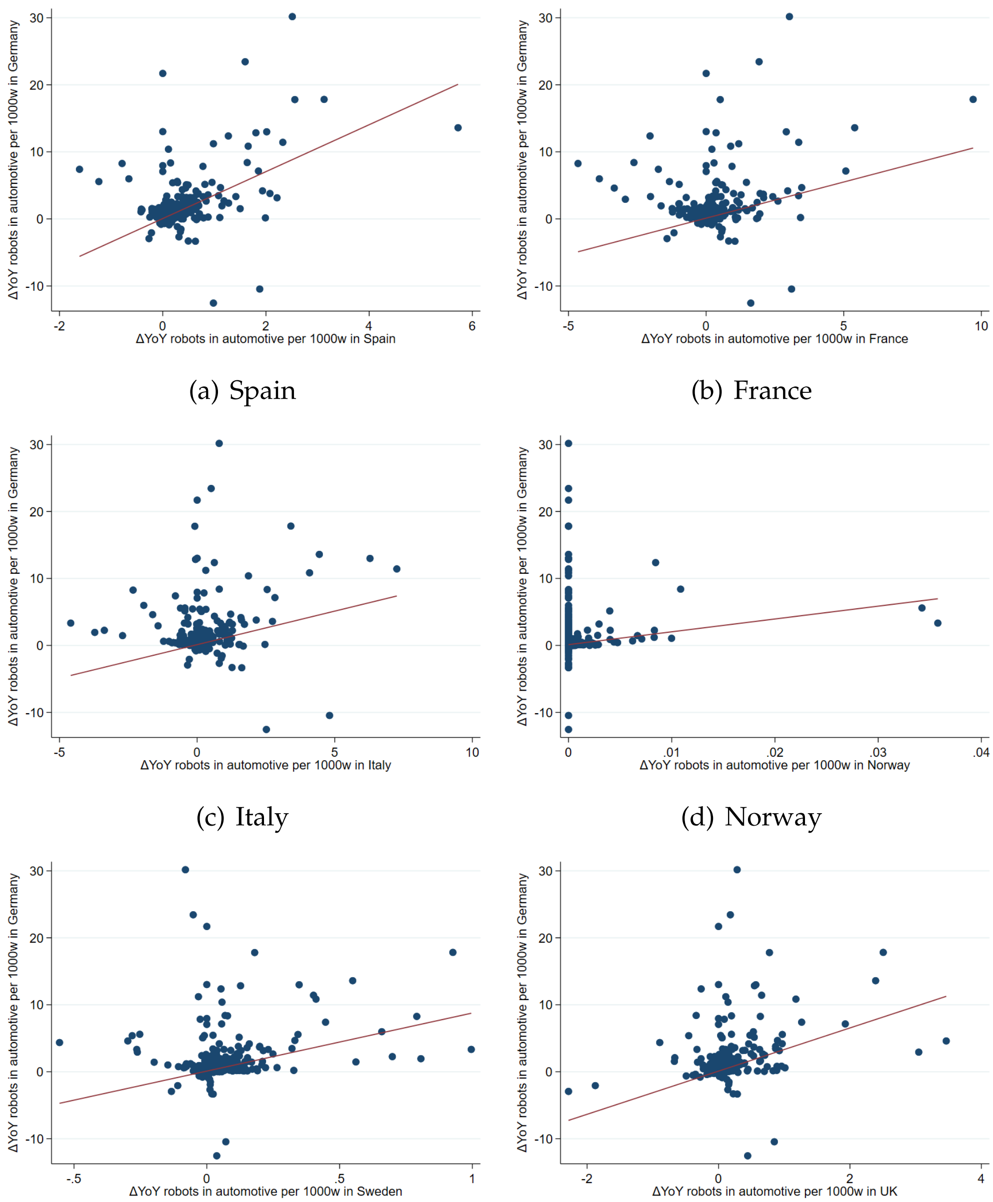
(b) Other Europe



(c) U.S.

Notes: The figure shows the penetration of manufacturing robots across industries (automotive and other) for selected countries, including Germany, between 1998 and 2019 using data on robot stock from the IFR. Other Europe include France, Italy, Norway, Spain, Sweden, and the United Kingdom. Robot penetration is defined as the robot stock normalized by the dependent employment in full-time equivalents (FTEs) in Germany obtained from the matched employer-employee (LIAB) data.

Figure 2.10: 2SLS First-Stage Relationship



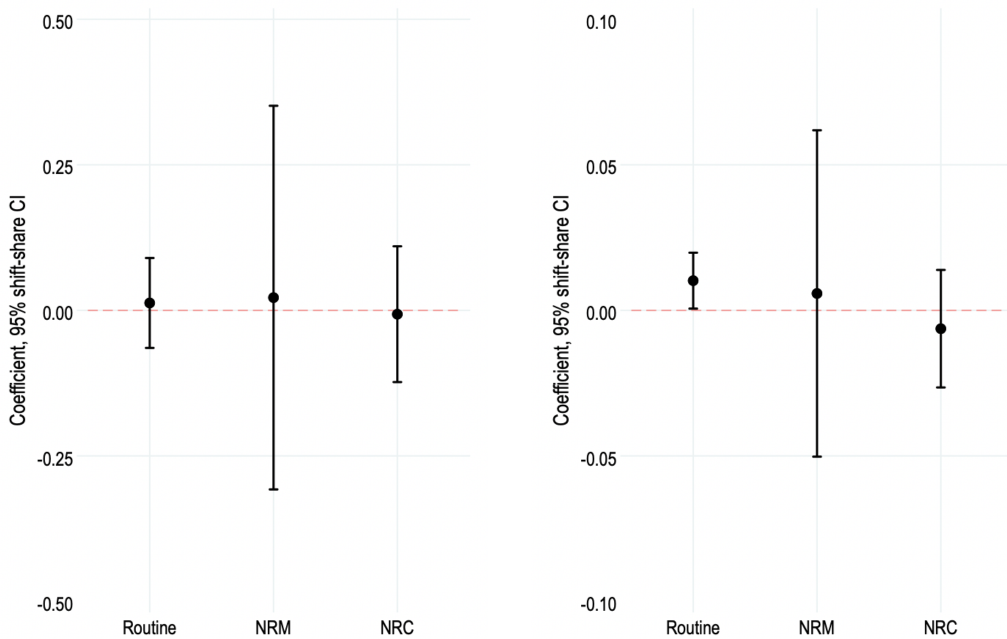
(a) Spain (b) France

(c) Italy (d) Norway

(e) Sweden (f) UK

Notes: These scatter plots show the first-stage relationship between the annual changes in exposure to industrial robots in the automotive industry for Germany and other high-income European countries between 1998 and 2018.

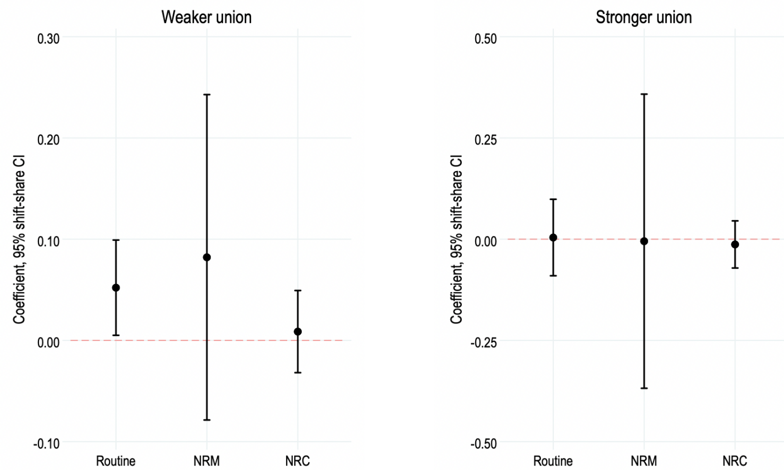
Figure 2.11: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers in East and West Germany



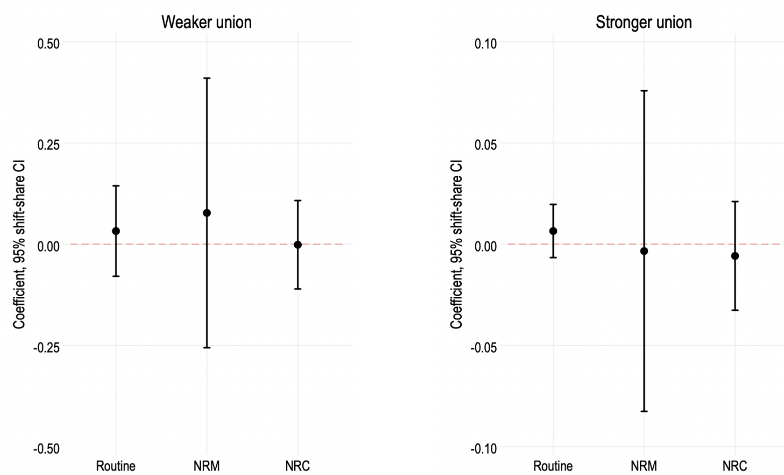
(a) East Germany (b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018 on the annual change in aggregate markdowns for heterogeneous workers in East and West Germany, respectively. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in panels (a) and (b) is the annual change in aggregate markdowns for routine, nonroutine manual (NRM), and nonroutine cognitive (NRC) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. All specifications control for constant, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors are clustered by local labor market regions, and 95% shift-share confidence intervals are presented.

Figure 2.12: Heterogeneous Effects of Robot Exposure on Wage Markdowns



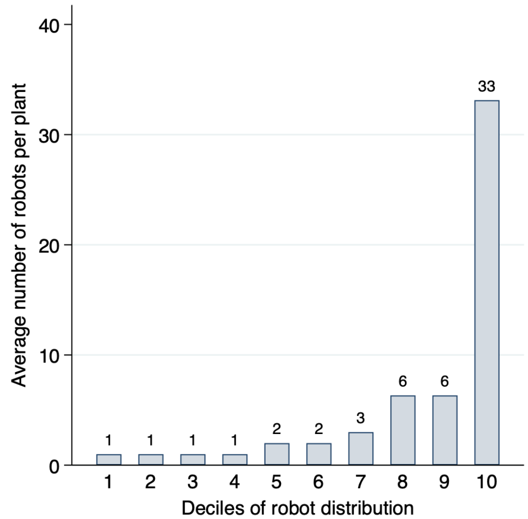
(a) East Germany



(b) West Germany

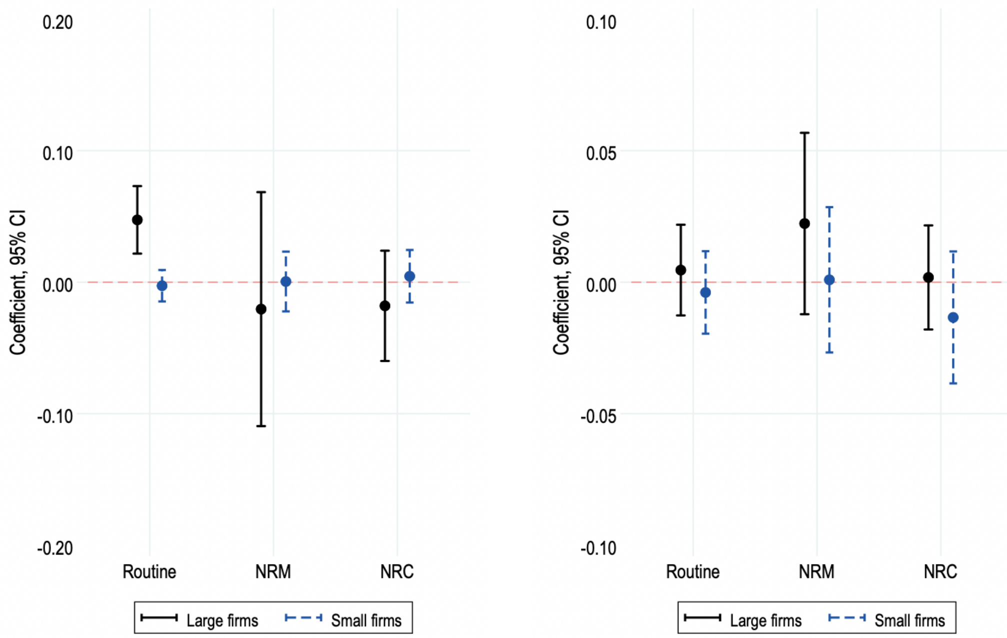
Notes: The left and right sub-figures in panel (a) present the IV (2SLS) estimates on the effects of annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018 on the annual change in aggregate markdowns for heterogeneous workers in districts from East Germany with union coverage below and above the national median, respectively. The left and right sub-figures in panel (b) depict the counterparts for districts from West Germany. The union coverage of the district is measured by the share of workers covered by unions in total workers in the district. The sample in the left and right sub-figures in panel (a) consists of districts from East Germany whose union coverage is below and above the national median, respectively. The sample in the left and right sub-figures in panel (b) consists of districts from West Germany whose union coverage is below and above the national median, respectively. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in panels (a) and (b) is the annual change in aggregate markdowns for routine nonroutine manual (NRM), and nonroutine cognitive (NRC) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. All specifications control for constant, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors are clustered by local labor market regions, and 95% shift-share confidence intervals are presented.

Figure 2.13: Distribution of Robots across Robot Adopters (2018)



Notes: Based on the IAB Establishment Panel (IAB BP) data. The figures depict the distribution of the average number of robots per manufacturing plant in 2018. Sampling weights provided in the data are applied.

Figure 2.14: Plant-Level Effects of Robot Exposure on Wage Markdowns at Large and Small Firms

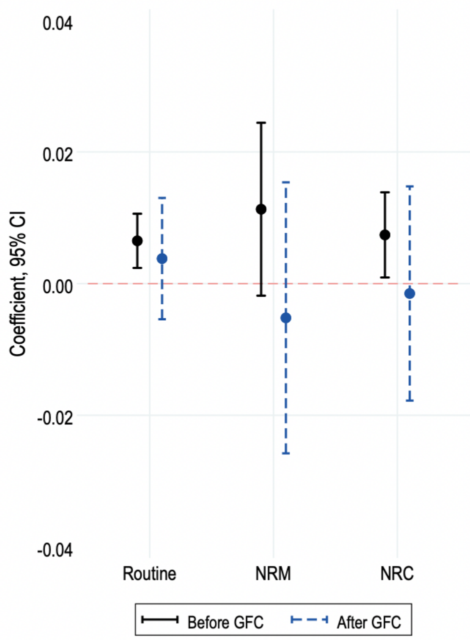


(a) East Germany

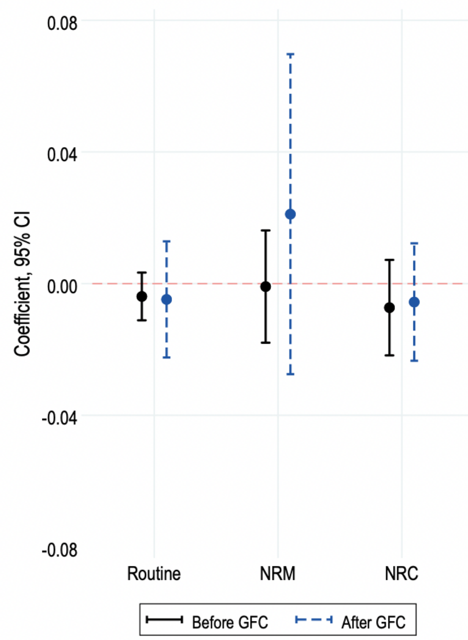
(b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual change in markdowns of firms with different size in districts from East and West Germany, respectively, between 1998 and 2018. Small firms are those in the bottom 7 deciles of the size distribution in the previous period, while large firms are plants in the top 3 deciles. In all regressions, the dependent variable is the annual change in plant-level markdowns for routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Table 2.36, except for plant size dummies. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

Figure 2.15: Plant-Level Effects of Robot Exposure on Wage Markdowns around the Great Recession



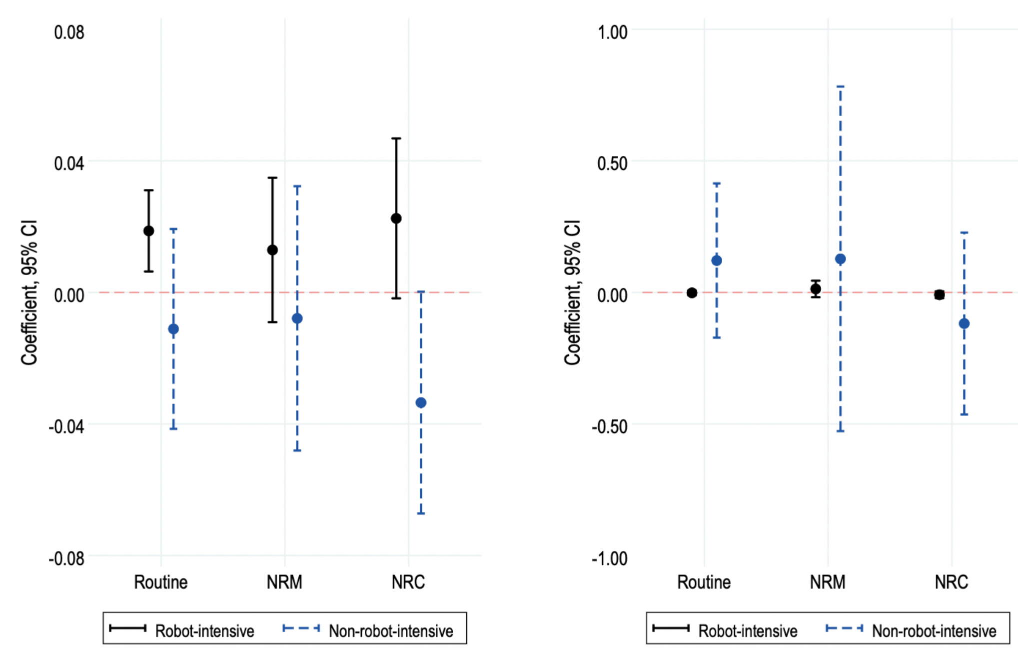
(a) East Germany



(b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual change in plant-level markdowns in districts from East and West Germany, respectively, before (1998-2008) and after (2009-2018) the Great Recession. In all regressions, the dependent variable is the annual change in plant-level markdowns for routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Table 2.36. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

Figure 2.16: Plant-Level Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers across Industries in East and West Germany

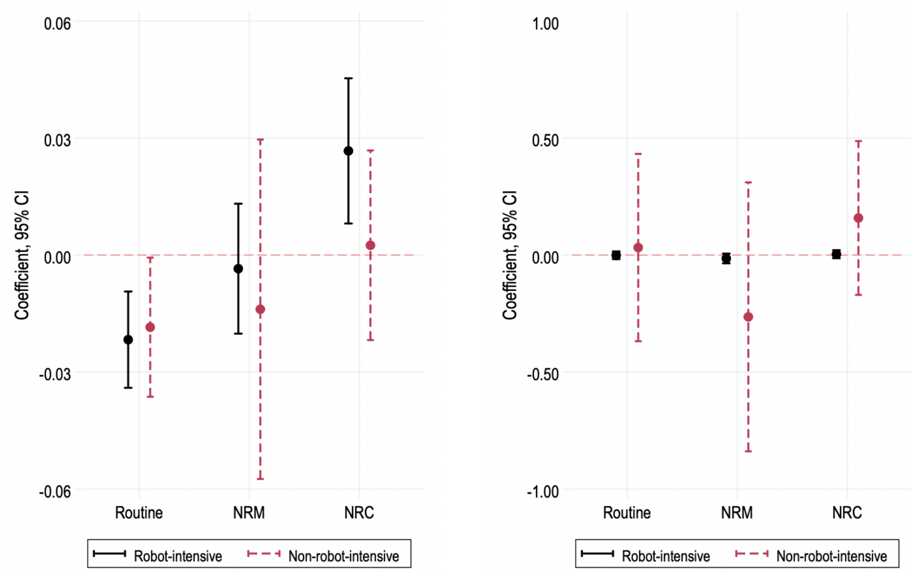


(a) East Germany

(b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual change in plant-level markdowns for plants in robot-intensive and non-robot-intensive industries in districts from East and West Germany, respectively, between 1998 and 2018. In all regressions, the dependent variable is the annual change in plant-level markdowns for routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Table 2.36. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

Figure 2.17: Plant-Level Effects of Robot Exposure on Employment of Heterogeneous Workers across Industries in East and West Germany



(a) East Germany (b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual percentage change in plant-level employment at plants in robot-intensive and non-robot-intensive industries in districts from East and West Germany, respectively, between 1998 and 2018. In all regressions, the dependent variable is the annual percentage change in plant-level employment of routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Figure 2.16. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

2.8.2 Tables

Table 2.1: Two Pillars of German Industrial Relations

	Collective bargaining	Codetermination
Level of negotiation	Industry or region level	Company, establishment, or plant level
Bargaining parties	Unions, employer associations, and firms	Employers and representatives of workers in two forms (representation on corporate boards and works councils)
Negotiation topics	Schedules of minimum requirements for wages, hours, working conditions, entitlements, and promotion criteria for workers in different industries, regions, and occupations, and with different levels of skills and experience.	*Representation on corporate boards*: Major decisions and the appointment, supervision, and dismissal of top corporate management. *Works councils*: Day-to-day managerial decision-making
Negotiation outcomes	Sector-regional level collective agreements	Firm-level agreements

Source: [123], [151], and [143]

Table 2.2: Summary Statistics

	Mean	SD	Min	Max	N
Log TFPR	0.017	0.287	-1.200	1.335	12630
Log revenue	7.548	1.663	3.578	14.243	12806
Log output	7.651	1.654	3.788	14.674	12806
Log capital	7.089	1.695	2.934	14.350	12806
Log labor	3.093	1.240	0.693	8.545	12806
Log material inputs	6.843	1.799	2.945	13.909	12806
Material cost (% revenue)	0.487	0.190	0.020	0.990	12806
Labor cost (% revenue)	0.270	0.131	0.017	1.000	12806
Daily wage (€)	72.11	41.934	1.005	722.534	9966

Notes: The table summarizes the main firm-level characteristics, including revenue productivity (TFPR), sales revenue, production output and inputs, input costs as a share of revenue, and the average daily wage paid to a worker. Variables cover the period 1997-2018 and come from the IAB Establishment Panel except for the daily wage, which comes from the matched employer-employee (LIAB) data. The unit of observation is the firm, and sampling weights are applied.

Table 2.3: Share of Robot Adopters by Manufacturing and Non-manufacturing in 2018

	Weighted (%)	Unweighted (%)	Number of Surveyed Plants
Manufacturing	7.19	12.48	1,755
Non-manufacturing	0.96	0.92	6,953
Total	1.48	3.25	8,708

Notes: Based on the IAB Establishment Panel data. The second column shows the share of robot adopters in 2018 calculated using survey weights, while the third column reports the share without survey weights. The last column reports the number of surveyed plants, including adopters and non-adopters.

Table 2.4: Estimated Plant-Level Markdowns in German Manufacturing

	Median	Mean	$IQR_{75\text{-}25}$	SD
Wearing apparel	2.064	2.067	0.871	0.665
Leather and related products	1.669	1.647	0.820	0.493
Beverages	1.616	1.562	0.813	0.651
Wood and wood products (excl. furniture)	1.324	1.555	0.828	0.670
Other transport equipment	1.310	1.326	0.969	0.550
Chemicals and chemical products	1.305	1.451	0.938	0.649
Rubber and plastics	1.291	1.429	0.640	0.549
Other non-metallic minerals	1.290	1.371	0.619	0.585
Furniture	1.279	1.506	0.696	0.616
Textiles	1.254	1.502	0.898	0.783
Paper and paper products	1.234	1.283	0.414	0.371
Basic pharmaceutical products	1.156	1.221	0.568	0.605
Food products	1.145	1.281	0.704	0.559
Repair and installation of machinery and equipment	1.122	1.286	0.708	0.564
Motor vehicles, trailers, and semi-trailers	1.118	1.205	0.568	0.480
Fabricated metals, excl. machinery and equipment	1.107	1.232	0.648	0.529
Machinery and equipment	1.061	1.185	0.482	0.489
Basic metals	1.033	1.194	0.601	0.487
Electrical equipment	1.028	1.078	0.469	0.360
Computer, electronic, and optical products	0.971	1.106	0.546	0.474
Other manufacturing	0.950	1.029	0.491	0.404
Printing and reproduction of recorded media	0.873	0.972	0.470	0.411
Whole sample	**1.129**	**1.271**	**0.670**	**0.565**
Sample size	12,794			

Notes: Markdowns are estimated using the IAB Establishment Panel from 1997-2018 under the assumption of a translog specification for gross output. Each industry group in manufacturing corresponds to the manufacturing categorization of the Federal Statistical Office. The distributional statistics are calculated using sampling weights provided in the data.

Table 2.5: Wage Gap between East and West Germany

	Dependent variable: Log average real daily wage		
	(1)	(2)	(3)
East dummy	-0.173	-0.153	-0.199
	(0.003)	(0.003)	(0.003)
N	207758	207758	207758
R^2	0.04	0.27	0.39
Year fixed effects	✓	✓	✓
Industry fixed effects		✓	✓
Firm characteristics			✓

Notes: The table presents the results from OLS regressions estimating the (log) average real daily wage on a dummy for whether the plant is located in East Germany. The dependent variable, the average real daily wage paid by the firm is constructed using the LIAB data. The industry-fixed effects include dummies for three-digit industries. The firm characteristics include workers' average education, the share of female workers in total workers, and firm size measured by the total number of workers.

Table 2.6: Markdown Gap between East and West Germany

	Dependent variable: Plant-level wage markdowns		
	(1)	(2)	(3)
East dummy	0.045	0.026	0.040
	(0.013)	(0.013)	(0.013)
N	9432	9432	9432
R^2	0.02	0.22	0.22
Year fixed effects	✓	✓	✓
Industry fixed effects		✓	✓
Firm characteristics			✓

Notes: The table presents the results from OLS regressions estimating the plant-level wage markdowns on a dummy for whether the plant is located in East Germany. The dependent variable, plant-level wage markdown is estimated using the IAB Establishment Panel data under the translog specification. The industry-fixed effects include dummies for three-digit industries. The firm characteristics include workers' average education, the share of female workers in total workers, and firm size measured by the total number of workers. Regressions are weighted by sampling weights provided in the data.

Table 2.7: Estimated Plant-Level Markdowns in East and West German Manufacturing

	Median	Mean	$IQR_{75\text{-}25}$	SD	N
East Germany	1.256	1.364	0.687	0.535	4400
West Germany	1.181	1.316	0.702	0.577	4996

Notes: Markdowns for East and West German manufacturing establishments are estimated using the IAB Establishment Panel from 1997-2018 under the assumption of a translog specification for gross output. The production function and markdowns are separately estimated for East and West German plants. The distributional statistics are calculated using sampling weights provided in the data.

Table 2.8: Relationship between Wage and Union Coverage

	Dependent variable: Log average real daily wage		
	(1)	(2)	(3)
Union coverage	0.347	0.361	0.420
	(0.137)	(0.146)	(0.148)
N	11142	8847	8319
R^2	0.86	0.91	0.93
Firm fixed effects	✓	✓	✓
Year fixed effects	✓		
District-by-Year fixed effects		✓	✓
Industry-by-Year fixed effects			✓

Notes: The table presents the results from OLS regressions estimating the relationship between the (log) average real daily wage and union coverage. The firm-level union coverage is measured by the share of workers covered by trade unions in total workers using the IAB Establishment Panel data. The dependent variable, the average real daily wage paid by the firm, is constructed using the LIAB data. The district fixed effects include dummies for kreise. The industry fixed effects include dummies for three-digit industries. The unit of observation is the plant. Standard errors clustered by firms are in parentheses.

Table 2.9: Estimated Plant-Level Markdowns for Firms with Different Union Coverage in German Manufacturing

	Median	Mean	SD	Min	Max	N
	Panel A. Union coverage quartiles					
First quartile	1.163	1.308	0.597	0.111	3.656	3229
Top 3 quartiles	1.089	1.206	0.500	0.018	3.641	9577
	Panel B. Union coverage deciles					
First decile	1.258	1.407	0.645	0.416	3.656	1321
Top 9 deciles	1.072	1.178	0.483	0.018	3.641	11485

Notes: Markdowns are estimated using the IAB Establishment Panel and the linked employer-employee (LIAB) data from 1997-2018 under the assumption of a translog specification for gross output. The sample was divided into quartiles (panel A) and deciles (panel B) of the firm's union coverage. The distributional statistics are calculated using sampling weights provided in the data.

Table 2.10: Summary Statistics for Labor Market Concentration (Manufacturing, 2018)

	Mean	Min	Max	25th Pctile	75th Pctile	fraction moderately concentrated	fraction highly concentrated
	Panel A. By Occupation × Region						
Baseline geographical definition: 141 CZs							
HHI (By 3-digit KldB 1988)	5800	204	10000	2638	10000	0.13	0.76
Alternative occupational definition:							
HHI (By 3-digit KldB 2010)	5285	145	10000	2200	10000	0.15	0.70
HHI (By 2-digit KldB 1988)	4907	183	10000	2000	8828	0.17	0.66
HHI (By 2-digit KldB 2010)	4022	177	10000	1429	5547	0.18	0.55
HHI (By 1-digit Blossfeld)	2871	150	10000	909	3863	0.18	0.38
Alternative geographical definition:							
HHI (By Kreis)	6747	313	10000	3750	10000	0.10	0.86
HHI (By 258 CZs)	6327	253	10000	3333	10000	0.12	0.82
HHI (By 42 regions)	4814	75	10000	1724	9260	0.16	0.63
HHI (By Federal state	4152	75	10000	1250	6250	0.16	0.54
	Panel B. By Industry × Region						
Baseline geographical definition: 141 CZs							
HHI (By 3-digit ISIC Rev.4)	6003	198	10000	3061	10000	0.11	0.80
Alternative industrial definition:							
HHI (By 2-digit ISIC Rev.4)	4328	162	10000	1746	6250	0.18	0.62
Alternative geographical definition:							
HHI (By Kreis)	7103	284	10000	4400	10000	0.07	0.91
HHI (By 258 CZs)	6645	310	10000	3750	10000	0.09	0.86
HHI (By 42 regions)	4721	113	10000	1911	7278	0.15	0.66
HHI (By Federal state)	4021	69	10000	1511	5702	0.18	0.57

Notes: Based on data from the Employee History (BeH). The table shows summary statistics for the labor market Herfindahl-Hirschman Index (HHI) for the manufacturing sector under various market definitions using German matched employer-employee (LIAB) data from the Federal Employment Agency. In the top panel, the baseline is calculated using 141 commuting zones (CZs) for the geographic market definition and 3-digit KldB 1988 codes for the occupational market definition. In the bottom panel, I use industry instead of occupation in the definition of labor market. The baseline is calculated using 141 CZs for the geographic market definition and 3-digit ISIC Rev.4 (WZ2008) industry codes for the industrial market definition. The calculation under alternative market definitions is done by changing the baseline along one dimension. Note that regions are a cluster of kreis (or counties in the U.S.), and there are 42 regions in Germany.

Table 2.11: Estimated Plant-Level Markdowns for Heterogeneous Workers

	Median	Mean	$IQR_{75\text{-}25}$	SD	N
	Panel A. NRC, routine, and NRM workers				
Routine workers	1.153	1.291	0.669	0.623	3178
Nonroutine cognitive (NRC) workers	1.356	1.613	0.880	0.904	3178
Nonroutine manual (NRM) workers	1.492	1.985	1.508	1.645	3178
	Panel B. High-skilled and low-skilled workers				
High-skilled workers	1.108	1.246	0.592	0.527	4223
Low-skilled workers	1.610	2.198	1.731	2.015	4223

Notes: Markdowns are estimated using the IAB Establishment Panel and the linked employer-employee (LIAB) data from 1997-2018 under the assumption of a translog specification for gross output with heterogeneous labor inputs. Labor inputs of production are heterogeneous by tasks performed at the workplace (panel A) and skill or education level (panel B). In the top panel, I group workers based on task intensity measures constructed using the BIBB/BAuA Employment Surveys. The distributional statistics are calculated using sampling weights provided in the data.

Table 2.12: Testing for Positive 2SLS Weights

	Germany's exposure to robots (1)	Spain's exposure to robots (2)
	Panel A. Continuous treatment and continuous instruments	
France's exposure to robots	1.010	0.385
	(0.281)	(0.033)
Italy's exposure to robots	0.913	0.348
	(0.226)	(0.071)
Norway's exposure to robots	149.839	14.775
	(43.861)	(4.265)
Sweden's exposure to robots	7.669	1.654
	(1.104)	(0.477)
UK's exposure to robots	2.928	0.694
	(0.323)	(0.192)
Spain's exposure to robots	3.285	1.000
	(0.232)	—
	Panel B. Binary treatment and binary instruments	
France's exposure to robots	0.546	0.763
	(0.044)	(0.029)
Italy's exposure to robots	0.547	0.712
	(0.045)	(0.029)
Norway's exposure to robots	0.412	0.411
	(0.057)	(0.060)
Sweden's exposure to robots	0.592	0.637
	(0.047)	(0.031)
UK's exposure to robots	0.618	0.699
	(0.045)	(0.023)
Spain's exposure to robots	0.650	1.000
	(0.042)	—

Notes: The table displays regressions of the variable listed in each column on the variable listed on each row. In panel A, treatment and instrumental variables are continuous, i.e., annual change in robots per 1,000 workers. In panel B, treatment and instrumental variables are defined as binary variables, specifically, taking a value of 1 if the annual change in robots per 1,000 workers is above the mean and 0 otherwise. All models control for covariates. Standard errors clustered at the district level are in parentheses.

Table 2.13: Formal Test for Partial Monotonicity

	p-value: positive weights (1)	p-value: negative weights (2)
	Panel A. Exposure to robots in Spain and another country	
France's exposure to robots	1.000	0.000
Italy's exposure to robots	1.000	0.000
Norway's exposure to robots	1.000	0.000
Sweden's exposure to robots	1.000	0.000
UK's exposure to robots	1.000	0.000
	Panel B. Exposure to robots in France and another country	
Italy's exposure to robots	1.000	0.000
Norway's exposure to robots	1.000	0.000
Sweden's exposure to robots	1.000	0.000
UK's exposure to robots	1.000	0.000
	Panel C. Exposure to robots in Italy and another country	
Norway's exposure to robots	1.000	0.000
Sweden's exposure to robots	1.000	0.000
UK's exposure to robots	1.000	0.000
	Panel D. Exposure to robots in Norway and in another country	
Sweden's exposure to robots	1.000	0.000
UK's exposure to robots	1.000	0.000
	Panel E. Exposure to robots in Sweden and in another country	
UK's exposure to robots	1.000	0.000

Notes: The table presents the results from a formal test for partial monotonicity according to [179]. The p-value in column (1) comes from a test of the null hypothesis that the 2SLS weights are all positive, and the p-value in column (2) comes from a test of the null hypothesis that at least one weight is negative.

Table 2.14: Effect of Robot Exposure on Employment and Wages of Heterogeneous Workers in Manufacturing Industry

	Dependent variable: Annual log difference in employment and wage		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. Employment		
ΔPredicted robot exposure	-0.0026	0.0014	-0.0000
	(0.0021)	(0.0022)	(0.0023)
	[0.1069]	[0.0403]	[0.0953]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	133.163	133.163	133.163
Critical value 2SLS (τ = 10%)	19.510	19.511	19.511
Hansen's J-stat p-value	0.608	0.637	0.223
	Panel B. Wages		
ΔPredicted robot exposure	0.0001	-0.0002	-0.0020
	(0.0011)	(0.0006)	(0.0018)
	[0.0261]	[0.0156]	[0.0218]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	133.163	133.163	133.163
Critical value 2SLS (τ = 10%)	19.510	19.510	19.511
Hansen's J-stat p-value	0.717	0.257	0.243

Notes: N = 4599 local labor market regions-by-year (district-by-year). The table presents the IV (2SLS) results from estimating the annual log difference in employment (number of workers, Panel A) and log difference in wages (average daily wage, Panel B) of heterogeneous workers on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the German local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in Panels A and B is the annual log difference in the number of workers and average daily wage, respectively, of routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications control for constant, broad region dummies, time fixed effects, and demographic characteristics of districts or kreise in the previous period. The broad region dummies indicate if the region is located in the north, west, south, or east of Germany. The demographic controls are constructed using the matched employer-employee data (LIAB) and include the share of females, the share of foreigners, the share of workers over 50 years old, the shares of workers with no vocational training, vocational training, and university degree, and employment shares across broad industries (agriculture, food products, consumer goods, industrial goods, capital goods, construction, consumer-related services, business-related services, and public sector). Exposure to net exports and ICT equipment is measured by the annual change in German net exports vis-à-vis China and 21 Eastern European countries (in 1,000 euros per worker) and by the annual change in German ICT equipment (in 1,000 euros per worker), respectively. Standard errors clustered at the level of local labor markets or districts are in parentheses. Shift-share standard errors are in brackets.

Table 2.15: Robot Exposure and Wage Markdowns

	Dependent variable: Annual change in aggregate markdowns			
	(1)	(2)	(3)	(4)
	Panel A. OLS			
ΔPredicted robot exposure	0.0012	0.0007	0.0007	0.0007
	(0.0029)	(0.0031)	(0.0031)	(0.0031)
	[0.0006]	[0.0010]	[0.0010]	[0.0010]
	Panel B. 2SLS			
ΔPredicted robot exposure	0.0007	-0.0000	0.0000	0.0001
	(0.0032)	(0.0035)	(0.0035)	(0.0035)
	[0.0180]	[0.0164]	[0.0242]	[0.0038]
Montiel Olea-Pflueger weak IV test				
Effective F-statistic (α = 5%)	43.973	46.212	46.225	46.251
Critical value 2SLS (τ = 10%)	21.230	21.309	21.308	21.314
Hansen's J-stat p-value	0.360	0.359	0.358	0.358
Year fixed effects	✓	✓	✓	✓
Broad region dummies	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Manufacturing share	✓			
Broad industry shares		✓	✓	✓
ΔNet exports in 1,000 euros per worker			✓	✓
ΔICT equipment in 1,000 euros per worker				✓

Notes: N = 4599 local labor market regions-by-year (district-by-year). Panel A presents the OLS results from estimating the annual change in aggregate markdowns on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. Panel B reports results from the 2SLS IV regressions where the German local labor market's exposure to robots in the automotive industry is instrumented by installations of automotive robots in other high-income European countries. All specifications control for constant, broad region dummies, year fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors clustered at the level of local labor markets or districts are in parentheses. Shift-share standard errors are in brackets.

Table 2.16: Heterogeneous Effects of Robot Exposure on Employment and Wages

	Dependent variable: Annual log difference in employment and wage					
	East Germany			West Germany		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. Employment					
ΔPredicted robot exposure	-0.008	0.021	0.023	-0.001	0.002	-0.000
	(0.007)	(0.009)	(0.010)	(0.002)	(0.002)	(0.002)
	[0.052]	[0.018]	[0.040]	[0.052]	[0.018]	[0.040]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	75.92	75.92	75.92	125.38	125.38	125.38
Critical value 2SLS (τ = 10%)	23.04	23.04	23.04	20.14	20.14	20.14
Hansen's J-stat p-value	0.93	0.87	0.77	0.48	0.70	0.27
N	1596	1596	1596	6048	6048	6048
	Panel B. Wages					
ΔPredicted robot exposure	-0.004	-0.005	-0.010	0.000	-0.000	-0.0015
	(0.003)	(0.002)	(0.004)	(0.001)	(0.001)	(0.002)
	[0.006]	[0.005]	[0.009]	[0.006]	[0.005]	[0.009]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	75.92	75.92	75.92	125.38	125.38	125.38
Critical value 2SLS (τ = 10%)	23.04	23.04	23.04	20.14	20.14	20.14
Hansen's J-stat p-value	0.73	0.78	0.67	0.70	0.05	0.26
N	1596	1596	1596	6048	6048	6048

Notes: Panels A and B present the IV (2SLS) results from estimating the annual log difference in employment (number of workers) and log difference in wages (average daily wage) of heterogeneous workers, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. In Panels A and B, the sample in the left and right sub-panels consists of districts from East and West Germany, respectively. All specifications control for constant, year fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.17: Heterogeneous Effects of Robot Exposure on Wage Markdowns

	Dependent variable: Annual change in aggregate markdowns			
	(1)	(2)	(3)	(4)
	Panel A. East Germany			
ΔPredicted robot exposure	0.004	0.004	0.004	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
	[0.227]	[0.117]	[0.048]	[0.013]
N	1449	1449	1449	1449
Montiel Olea-Pflueger weak IV test				
Effective F-statistic (α = 5%)	1211.06	73.26	70.98	71.08
Critical value 2SLS (τ = 10%)	22.27	23.04	23.04	23.04
Hansen's J-stat p-value	0.65	0.62	0.61	0.60
	Panel B. West Germany			
ΔPredicted robot exposure	-0.004	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)
	[0.008]	[0.156]	[0.020]	[0.004]
N	3150	3150	3150	3150
Montiel Olea-Pflueger weak IV test				
Effective F-statistic (α = 5%)	77.86	86.32	86.30	86.86
Critical value 2SLS (τ = 10%)	22.82	22.69	22.69	22.69
Hansen's J-stat p-value	0.33	0.32	0.32	0.32
Year fixed effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Manufacturing share	✓			
Broad industry shares		✓	✓	✓
ΔNet exports in 1,000 euros per worker			✓	✓
ΔICT equipment in 1,000 euros per worker				✓

Notes: Panels A and B present the IV (2SLS) results from estimating the annual change in aggregate markdowns in East and West Germany, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in Panels A and B is the annual change in aggregate markdowns where production function is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. All specifications control for constant, year fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.18: Effect of Robot Exposure on Wage Markdowns for Heterogeneous Workers

	Dependent variable: Annual change in aggregate markdowns		
	Routine	Nonroutine manual	Nonroutine cognitive
	(1)	(2)	(3)
ΔPredicted robot exposure	0.009	0.007	-0.004
	(0.006)	(0.007)	(0.005)
	[0.004]	[0.010]	[0.006]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	46.25	46.25	46.25
Critical value 2SLS (τ = 10%)	21.31	21.31	21.31
Hansen's J-stat p-value	0.25	0.25	0.56
R^2	0.02	0.01	0.01

Notes: N = 4599 local labor market regions-by-year (district-by-year). The table presents the IV (2SLS) results from estimating the annual change in aggregate markdowns for heterogeneous workers on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the German local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications control for constant, broad region dummies, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors clustered at the level of local labor markets or districts are in parentheses. Shift-share standard errors are in brackets.

Table 2.19: Heterogeneous Effects of Robot Exposure on Employment

	Dependent variable: Annual log difference in employment					
	Below the median			Above the median		
	Routine	NRM	NRC	Routine	NRM	NRC
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. East Germany					
ΔPredicted robot exposure	-0.015	0.030	-0.009	-0.009	0.014	0.031
	(0.020)	(0.015)	(0.032)	(0.009)	(0.011)	(0.013)
	[0.052]	[0.018]	[0.040]	[0.052]	[0.018]	[0.040]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	15.72	15.72	15.72	271.20	271.20	271.20
Critical value 2SLS (τ = 10%)	21.82	21.82	21.82	22.31	22.31	22.31
Critical value 2SLS (τ = 20%)	14.10	14.10	14.10	14.43	14.43	14.43
Critical value 2SLS (τ = 30%)	11.21	11.21	11.21	11.49	11.49	11.49
Hansen's J-stat p-value	0.51	0.96	0.39	0.81	0.84	0.78
N	589	589	589	1007	1007	1007
	Panel B. West Germany					
ΔPredicted robot exposure	-0.001	-0.003	-0.001	0.002	0.010	-0.001
	(0.003)	(0.003)	(0.003)	(0.007)	(0.004)	(0.008)
	[0.052]	[0.018]	[0.040]	[0.052]	[0.018]	[0.040]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	67.81	67.81	67.81	8.95	8.95	8.95
Critical value 2SLS (τ = 10%)	21.48	21.48	21.48	22.56	22.56	22.56
Critical value 2SLS (τ = 20%)	13.80	13.80	13.80	14.63	14.63	14.63
Critical value 2SLS (τ = 30%)	10.94	10.94	10.94	11.66	11.66	11.66
Hansen's J-stat p-value	0.66	0.43	0.79	0.41	0.41	0.30
N	3241	3241	3241	2807	2807	2807

Notes: Panels A and B present the IV (2SLS) results from estimating the annual log difference in employment (number of workers) of heterogeneous workers in East and West Germany, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the local labor market region's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. In Panels A and B, the sample in the left and right sub-panels consists of districts with union coverage below and above the national median, respectively. All specifications control for constant, year fixed effects, and demographic characteristics of districts or kreise in the previous period. Unit of observation: local labor market region (kreis or district). Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.20: Heterogeneous Effects of Robot Exposure on Wages

	Dependent variable: Annual log difference in wages					
	Below the median			Above the median		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	-0.005	-0.006	-0.006	-0.005	-0.007	-0.010
	(0.009)	(0.004)	(0.017)	(0.005)	(0.003)	(0.004)
	[0.006]	[0.005]	[0.009]	[0.006]	[0.005]	[0.009]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	15.72	15.72	15.72	271.20	271.20	271.20
Critical value 2SLS (τ = 10%)	21.82	21.83	21.82	22.31	22.31	22.31
Critical value 2SLS (τ = 20%)	14.10	14.10	14.10	14.43	14.43	14.43
Critical value 2SLS (τ = 30%)	11.21	11.21	11.21	11.49	11.49	11.49
Hansen's J-stat p-value	0.35	0.65	0.29	0.85	0.86	0.83
N	589	589	589	1007	1007	1007
	Panel B. West Germany					
ΔPredicted robot exposure	0.000	-0.000	0.000	-0.001	0.001	-0.009
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.004)
	[0.006]	[0.005]	[0.009]	[0.006]	[0.005]	[0.009]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	67.81	67.81	67.81	8.95	8.95	8.95
Critical value 2SLS (τ = 10%)	21.48	21.48	21.48	22.56	22.56	22.56
Critical value 2SLS (τ = 20%)	13.80	13.80	13.80	14.63	14.63	14.63
Critical value 2SLS (τ = 30%)	10.94	10.94	10.94	11.66	11.66	11.66
Hansen's J-stat p-value	0.42	0.18	0.55	0.27	0.41	0.47
N	3241	3241	3241	2807	2807	2807

Notes: Panels A and B present the IV (2SLS) results from estimating the annual log difference in wages (average daily wage) of heterogeneous workers in East and West Germany, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the local labor market region's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. In Panels A and B, the sample in the left and right sub-panels consists of districts with union coverage below and above the national median, respectively. All specifications control for constant, year fixed effects, and demographic characteristics of districts or kreise in the previous period. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.21: Heterogeneous Effects of Robot Exposure on Markdowns for Heterogeneous Workers (Production Function Estimated on the Full Sample)

	Dependent variable: Annual Δ in aggregate markdowns		
	Routine	Nonroutine manual	Nonroutine cognitive
	(1)	(2)	(3)
	Panel A. East Germany		
ΔPredicted robot exposure	0.024	0.035	-0.015
	(0.008)	(0.011)	(0.006)
	[0.024]	[0.053]	[0.020]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	71.08	71.08	71.08
Critical value 2SLS (τ = 10%)	23.04	23.04	23.04
Hansen's J-stat p-value	0.56	0.81	0.52
N	1449	1449	1449
R^2	0.03	0.03	0.02
	Panel B. West Germany		
ΔPredicted robot exposure	0.005	0.000	-0.003
	(0.003)	(0.008)	(0.003)
	[0.005]	[0.022]	[0.009]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	86.86	86.86	86.86
Critical value 2SLS (τ = 10%)	22.69	22.69	22.69
Hansen's J-stat p-value	0.36	0.43	0.68
N	3150	3150	3150
R^2	0.02	0.01	0.01

Notes: Panels A and B present the IV (2SLS) results from estimating the annual change in aggregate markdowns for heterogeneous workers in East and West Germany, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers at manufacturing establishments from East and West Germany, respectively, where production function with heterogeneous workers is estimated on the full sample. All specifications control for constant, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.22: Heterogeneous Effects of Robot Exposure on Markdowns for Heterogeneous Workers (Production Function Estimated on the Full Sample)

	Dependent variable: Annual Δ in aggregate markdowns					
	Below the median			Above the median		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.109	0.086	0.050	0.000	0.010	-0.036
	(0.029)	(0.037)	(0.015)	(0.005)	(0.015)	(0.007)
	[0.026]	[0.071]	[0.028]	[0.072]	[0.153]	[0.085]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	14.94	14.94	14.94	256.97	256.97	256.97
Critical value 2SLS (τ = 20%)	14.23	14.23	14.23	14.46	14.46	14.46
Hansen's J-stat p-value	0.48	0.77	0.91	0.70	0.91	0.42
N	527	527	527	922	922	922
	Panel B. West Germany					
ΔPredicted robot exposure	0.005	-0.000	0.001	0.005	0.009	-0.005
	(0.008)	(0.024)	(0.008)	(0.002)	(0.006)	(0.003)
	[0.031]	[0.078]	[0.062]	[0.011]	[0.021]	[0.012]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	13.53	13.53	13.53	33.50	33.50	33.50
Critical value 2SLS (τ = 20%)	13.51	13.51	13.51	14.48	14.48	14.48
Hansen's J-stat p-value	0.36	0.38	0.59	0.86	0.54	0.69
N	1660	1660	1660	1490	1490	1490

Notes: The left and right sub-panels of Panel A present the IV (2SLS) results from estimating the annual change in aggregate markdowns for heterogeneous workers in districts from East Germany with union coverage below and above the national median, respectively, on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. Panel B's left and right sub-panels report the counterparts for districts from West Germany. The union coverage of the district is measured by the share of workers covered by unions in total workers in the district. The sample in the left and right sub-panel of Panel A consists of districts from East Germany whose union coverage is below and above the national median, respectively. The sample in the left and right sub-panel of Panel B consists of districts from West Germany whose union coverage is below and above the national median, respectively. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers at manufacturing establishments from East and West Germany, respectively, where production function with heterogeneous workers is estimated on the full sample. All specifications control for constant, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets. NRC, nonroutine cognitive; NRM, nonroutine manual.

Table 2.23: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers (Alternative Split of Union Coverage)

	Dependent variable: Annual Δ in aggregate markdowns		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. East Germany		
ΔPredicted robot exposure	0.023	0.059	-0.026
	(0.006)	(0.030)	(0.005)
	[0.008]	[0.029]	[0.008]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	9.40	9.40	9.40
Critical value 2SLS (τ = 30%)	11.85	11.85	11.85
Hansen's J-stat p-value	0.34	0.76	0.75
N	1238	1238	1238
	Panel B. West Germany		
ΔPredicted robot exposure	0.014	0.010	-0.016
	(0.010)	(0.023)	(0.024)
	[0.133]	[0.306]	[0.150]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	6.59	6.59	6.59
Critical value 2SLS (τ = 30%)	11.86	11.86	11.86
Hansen's J-stat p-value	0.36	0.34	0.46
N	2590	2590	2590

Notes: Panels A and B present the IV (2SLS) results from estimating the annual change in aggregate markdowns for heterogeneous workers in districts from East and West Germany, respectively, with union coverage in the bottom eight deciles of the distribution on the annual change in predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018. The union coverage of the district is measured by the share of workers covered by unions in total workers in the district. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. All specifications control for constant, time fixed effects, and demographic characteristics of districts or kreise in the previous period. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.24: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers (Percent Changes)

	Dependent variable: Annual Δ in aggregate markdowns					
	Below the median			Above the median		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	14.908	27.304	2.607	0.274	5.273	-7.025
	(4.249)	(12.314)	(2.643)	(1.238)	(6.369)	(1.485)
	[7.588]	[14.185]	[7.431]	[15.674]	[26.129]	[14.982]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	5.04	5.04	5.04	243.88	243.88	243.88
Critical value 2SLS (τ = 30%)	11.49	11.49	11.49	11.05	11.05	11.05
Hansen's J-stat p-value	0.71	0.76	0.83	0.53	0.46	0.74
N	527	527	527	922	922	922
	Panel B. West Germany					
ΔPredicted robot exposure	7.224	5.281	0.656	0.964	0.020	-0.660
	(3.926)	(5.406)	(1.463)	(0.433)	(2.172)	(0.878)
	[14.955]	[22.188]	[9.452]	[2.129]	[5.986]	[2.145]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	18.10	18.10	18.10	44.67	44.67	44.67
Critical value 2SLS (τ = 30%)	9.81	9.81	9.81	11.58	11.58	11.58
Hansen's J-stat p-value	0.71	0.77	0.26	0.77	0.90	0.74
N	1660	1660	1660	1490	1490	1490

Notes: The table checks the robustness of IV (2SLS) results by using percent changes in aggregate markdowns for heterogeneous workers and predicted exposure to robots in the automotive industry per 1,000 workers. The sample in the left and right sub-panel of Panel A consists of districts from East Germany whose union coverage is below and above the national median, respectively. The sample in the left and right sub-panel of Panel B consists of districts from West Germany whose union coverage is below and above the national median, respectively. The dependent variable in Panels A and B is the annual percent change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. The key explanatory variable is the annual percent change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. All specifications include the full set of baseline control variables. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.25: Effect of Robot Exposure on Wage Markdowns
(Alternative Clusters at the Aggregate Regions)

	Dependent variable: Annual Δ in aggregate markdowns			
	All workers	Heterogeneous workers		
		Routine	NRM	NRC
	(1)	(2)	(3)	(4)
ΔPredicted robot exposure	0.000	0.009	0.007	-0.004
	(0.003)	(0.006)	(0.010)	(0.004)
	[0.004]	[0.004]	[0.010]	[0.006]
Montiel Olea-Pflueger weak IV test				
Effective F-statistic (α = 5%)	43.00	43.00	43.00	43.00
Critical value 2SLS (τ = 10%)	21.56	21.56	21.56	21.56
Hansen's J-stat p-value	0.50	0.15	0.18	0.44

Notes: N = 4599 local labor market regions-by-year (district-by-year). The table checks the robustness of the IV (2SLS) results from estimating the effect of robot exposure on aggregate wage markdowns for all workers (column (1)) and heterogeneous workers (columns (2)-(4)) by using 50 aggregate regions to cluster the standard errors. The key explanatory variable is the annual change in the German local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. The dependent variable is the annual change in aggregate markdowns for all (column (1)), routine (column (2)), nonroutine manual (column (3)), and nonroutine cognitive (column (4)) workers. All specifications control for constant, broad region dummies, time fixed effects, and the full set of baseline control variables. Standard errors clustered by the 50 aggregate regions are in parentheses. Shift-share standard errors are in brackets.

Table 2.26: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers (Robots in Automobile and Other Industries)

	Dependent variable: Annual Δ in aggregate markdowns					
	Below the median			Above the median		
	Routine	NRM	NRC	Routine	NRM	NRC
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.058	0.069	0.005	0.007	-0.011	-0.011
(*automobile industry*)	(0.009)	(0.034)	(0.008)	(0.004)	(0.024)	(0.006)
	[0.068]	[0.237]	[0.059]	[0.044]	[0.158]	[0.056]
Kleibergen-Paap weak ID test	32.35	32.35	32.35	83.73	83.73	83.73
Hansen's J-stat p-value	0.94	0.86	0.85	0.64	0.58	0.49
N	527	527	527	922	922	922
	Panel B. West Germany					
ΔPredicted robot exposure	0.033	0.077	-0.002	0.006	-0.000	-0.006
(*automobile industry*)	(0.021)	(0.077)	(0.013)	(0.003)	(0.012)	(0.005)
	[0.089]	[1.231]	[0.095]	[0.005]	[0.017]	[0.003]
Kleibergen-Paap weak ID test	14.55	14.55	14.55	20.33	20.33	20.33
Hansen's J-stat p-value	0.90	0.79	0.54	0.71	0.33	0.91
N	1660	1660	1660	1490	1490	1490

Notes: The table checks the robustness of IV (2SLS) results by adding a treatment variable of annual change in the local labor market's exposure to non-automotive robots instrumented by non-automotive robots in other high-income European countries. The sample in the left and right sub-panel of Panel A consists of districts from East Germany whose union coverage is below and above the national median, respectively. The sample in the left and right sub-panel of Panel B consists of districts from West Germany whose union coverage is below and above the national median, respectively. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. The key explanatory variable is the annual change in the local labor market's exposure to robots in the automotive industry instrumented by installations of automotive robots in other high-income European countries. All specifications include the full set of baseline control variables. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.27: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers (Robots in All Industries)

	Dependent variable: Annual Δ in aggregate markdowns		
	Routine	Nonroutine manual	Nonroutine cognitive
	(1)	(2)	(3)
	Panel A. East Germany		
ΔPredicted robot exposure	0.0065	0.0012	0.0001
	(0.0032)	(0.0090)	(0.0025)
	[0.0063]	[0.0275]	[0.0062]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	9.852	9.852	9.852
Critical value 2SLS (τ = 30%)	10.171	10.165	10.165
Hansen's J-stat p-value	0.757	0.321	0.427
N	1238	1238	1238
	Panel B. West Germany		
ΔPredicted robot exposure	-0.0014	0.0042	0.0002
	(0.0008)	(0.0026)	(0.0011)
	[0.0085]	[0.0244]	[0.0097]
Montiel Olea-Pflueger weak IV test			
Effective F-statistic (α = 5%)	35.041	35.041	35.041
Critical value 2SLS (τ = 10%)	17.560	17.561	17.562
Hansen's J-stat p-value	0.389	0.123	0.807
N	2590	2590	2590

Notes: The table checks the robustness of IV (2SLS) results on the heterogeneous effect of robot exposure on wage markdowns for heterogeneous workers in districts from East and West Germany with different union coverage by using total robots in all industries instead of automotive robots as in the baseline analysis. Panels A and B contain East and West German districts in the bottom eight deciles of union coverage distribution, respectively. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. The key explanatory variable is the annual change in the local labor market's exposure to robots in all industries instrumented by installations of robots in other high-income European countries. All specifications include the full set of baseline control variables. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.28: First-Stage Regression

	Dependent variable: ΔPredicted robot exposure in Germany	
	(1)	(2)
ΔPredicted robot exposure in Spain	3.187	2.709
	(0.250)	(0.198)
ΔPredicted robot exposure in Norway	102.651	118.352
	(27.469)	(29.974)
ΔPredicted robot exposure in Sweden	2.244	2.612
	(0.251)	(0.276)
ΔPredicted robot exposure in UK	0.827	0.778
	(0.169)	(0.240)
ΔPredicted robot exposure in France	-0.381	
	(0.357)	
ΔPredicted robot exposure in Italy	-0.007	
	(0.247)	
N	4599	4599
R^2	0.39	0.38

Notes: The table presents the OLS coefficients from first-stage regressions. The dependent variable is the annual change in predicted robot exposure in Germany, and the main explanatory variables are the annual change in predicted robot exposure in six (Column (1)) and four (Column (2)) other high-income European countries. All specifications control for constant, time fixed effects, broad region dummies, and demographic characteristics of districts or kreise in the base period.

Table 2.29: Heterogeneous Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers (Alternative Group of Instruments)

	Dependent variable: Annual change in aggregate markdowns					
	Below the median			Above the median		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.052	0.082	0.009	0.009	-0.012	-0.020
	(0.010)	(0.032)	(0.007)	(0.004)	(0.027)	(0.006)
	[0.023]	[0.078]	[0.021]	[0.046]	[0.174]	[0.028]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	16.57	16.57	16.57	203.81	203.81	203.81
Critical value 2SLS (τ = 20%)	14.53	14.52	14.52	14.92	14.92	14.92
Hansen's J-stat p-value	0.70	0.75	0.77	0.46	0.70	0.33
N	527	527	527	922	922	922
	Panel B. West Germany					
ΔPredicted robot exposure	0.034	0.077	-0.004	0.006	-0.011	-0.001
	(0.021)	(0.075)	(0.014)	(0.003)	(0.019)	(0.006)
	[0.065]	[0.188]	[0.064]	[0.008]	[0.046]	[0.016]
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	22.84	22.84	22.84	56.73	56.73	56.73
Critical value 2SLS (τ = 20%)	13.53	13.53	13.53	14.42	14.42	14.42
Hansen's J-stat p-value	0.83	0.62	0.24	0.27	0.30	0.47
N	1660	1660	1660	1490	1490	1490

Notes: The table checks the robustness of IV (2SLS) results using an alternative set of instruments that consist of the annual changes of predicted exposure to automotive robots in Spain, Norway, Sweden, and the UK. The sample in the left and right sub-panel of Panel A consists of districts from East Germany whose union coverage is below and above the national median, respectively. The sample in the left and right sub-panel of Panel B consists of districts from West Germany whose union coverage is below and above the national median, respectively. The dependent variable in Panels A and B is the annual change in aggregate markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers where production function with heterogeneous workers is estimated on the sub-sample consisting of manufacturing establishments from East and West Germany, respectively. All specifications include the full set of baseline control variables. Standard errors clustered at the local labor market level are in parentheses. Shift-share standard errors are in brackets.

Table 2.30: Relationship between Actual Robot Adoption and Robot Exposure Shock

	Dependent variable: Actual robot adoption		
	(1)	(2)	(3)
	Panel A. Robots per 1000 workers		
Robot exposure shock	0.020	0.114	0.072
	(0.020)	(0.181)	(0.173)
N	1671	1667	1657
R^2	0.05	0.54	0.56
	Panel B. ΔRobots per 1000 workers		
ΔRobot exposure shock	-0.202	-0.083	-0.206
	(0.164)	(0.288)	(0.282)
N	1330	1323	1315
R^2	0.03	0.45	0.47
Year fixed effects	✓	✓	
State fixed effects	✓		
District fixed effects		✓	✓
State-by-Year fixed effects			✓

Notes: The sample at the level in panel A covers periods between 2014 and 2018, while the sample in panel B for annual changes covers 2015-2018. The actual robot adoption is measured by aggregating the number of robots adopted by the firm at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. The robot exposure shock into the local labor market regions or districts is measured by the average robots stock in six other European countries (Spain, France, Italy, Norway, Sweden, and UK) "predicted" to districts using employment shares and expressed as per 1,000 workers. The actual robot adoption and robot exposure shock are normalized by the number of workers in the previous period. Standard errors clustered by districts are in parentheses.

Table 2.31: Heterogeneous Effects of Robot Exposure on Wage Markdowns (2014-2018)

	Low union coverage			High union coverage		
	Routine	NRM	NRC	Routine	NRM	NRC
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.001	-0.095	0.065	-0.001	-0.039	-0.001
	(0.052)	(0.222)	(0.060)	(0.004)	(0.017)	(0.004)
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	7.24	0.93	5.72	2877.77	106.25	4763.26
Critical value 2SLS (τ = 30%)	11.17	10.24	11.26	10.50	11.52	9.41
Hansen's J-stat p-value	0.58	0.65	0.47	0.67	0.51	0.74
N	77	77	77	199	199	199
	Panel B. West Germany					
ΔPredicted robot exposure	-0.005	0.015	-0.011	0.007	0.009	0.011
	(0.009)	(0.020)	(0.025)	(0.007)	(0.013)	(0.015)
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	451.84	173.79	5.52	78.35	76.05	78.09
Critical value 2SLS (τ = 30%)	9.62	10.64	11.63	11.59	11.33	11.57
Hansen's J-stat p-value	0.51	0.26	0.41	0.37	0.67	0.32
N	303	303	303	289	289	289

Notes: The table presents the results from estimating the specifications in Figure 2.12, where the dependent variable is the annual change in aggregate markdowns, on the sample between 2014 and 2018.

Table 2.32: Heterogeneous Effects of Robot Exposure on Wage Markdowns (Controlling for Actual Robot Adoption)

	Low union coverage			High union coverage		
	Routine	NRM	NRC	Routine	NRM	NRC
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.002	-0.091	0.065	-0.002	-0.040	0.001
	(0.054)	(0.224)	(0.060)	(0.004)	(0.017)	(0.004)
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	7.13	0.90	5.59	2619.67	113.18	4437.34
Critical value 2SLS (τ = 30%)	11.15	10.21	11.24	10.54	11.54	9.38
Hansen's J-stat p-value	0.60	0.64	0.50	0.67	0.60	0.67
N	77	77	77	199	199	199
	Panel B. West Germany					
ΔPredicted robot exposure	-0.005	0.015	-0.011	0.006	0.006	0.010
	(0.009)	(0.020)	(0.025)	(0.006)	(0.011)	(0.014)
Montiel Olea-Pflueger weak IV test						
Effective F-statistic (α = 5%)	447.10	172.66	5.50	78.35	75.83	78.10
Critical value 2SLS (τ = 30%)	9.61	10.63	11.63	11.60	11.37	11.58
Hansen's J-stat p-value	0.50	0.22	0.41	0.36	0.67	0.32
N	303	303	303	289	289	289

Notes: The table presents the effects of annual change in predicted robot exposure on annual change in aggregate markdowns using a regression where the district-level actual robot adoption (number of robots adopted at the firm aggregated at the district level, expressed per 1,000 workers) has been added to a specification in Table 2.31.

Table 2.33: Plant-Level Effects of Robot Exposure on Employment

	Dependent variable: Annual % change in plant-level employment			
	All workers	Heterogeneous workers		
		Routine	NRM	NRC
	(1)	(2)	(3)	(4)
ΔPredicted robot exposure	-0.008	-0.020	-0.009	0.012
	(0.005)	(0.007)	(0.013)	(0.008)
N	7623	7623	7623	7623

Notes: Column (1) presents the results from estimating the annual percentage change in employment at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018 using the IV (2SLS) regressions. Columns (2)-(4) report the effects of automation exposure on the employment of heterogeneous workers performing different tasks, and the dependent variable is the annual percentage change in the number of routine workers (column (2)), nonroutine manual–NRM workers (column (3)), and nonroutine cognitive–NRC workers (column (4)). All specifications control for constant, six plant size groups based on the number of employees at the establishment in the previous year, and demographic characteristics of districts or kreise in the previous year. The firm, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table 2.34: Plant-Level Effects of Robot Exposure on Wages

	Dependent variable: Annual % change in plant-level average wage			
	All workers	Heterogeneous workers		
		Routine	NRM	NRC
	(1)	(2)	(3)	(4)
ΔPredicted robot exposure	0.002	0.008	-0.006	0.012
	(0.007)	(0.010)	(0.006)	(0.013)
N	7623	7623	7623	7623

Notes: Column (1) presents the results from estimating the annual percentage change in average wage at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018 using the IV (2SLS) regressions. Columns (2)-(4) report the effects of automation exposure on the average wage of heterogeneous workers performing different tasks, and the dependent variable is the annual percentage change in the average wage of routine workers (column (2)), nonroutine manual–NRM workers (column (3)), and nonroutine cognitive–NRC workers (column (4)). All specifications control for constant, six plant size groups based on the number of employees at the establishment in the previous year, and demographic characteristics of districts or kreise in the previous year. The firm, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table 2.35: Plant-Level Effects of Robot Exposure on Wage Markdowns

	Panel A. All workers		
	Germany (1)	East Germany (2)	West Germany (3)
ΔPredicted robot exposure	0.009	0.009	-0.007
	(0.010)	(0.006)	(0.010)
N	7623	3649	3823
	Panel B. Heterogeneous workers		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
ΔPredicted robot exposure	0.007	0.012	0.001
	(0.007)	(0.008)	(0.009)
N	7623	7623	7623
Firm characteristics	✓	✓	✓
Regional demographics	✓	✓	✓
Firm fixed effects	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓

Notes: Panel A presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers between 1998 and 2018 using the IV (2SLS) regressions. In column (1) of Panel A, the sample consists of all firms in the IAB Establishment Panel for which wage markdowns have been estimated. The sample in columns (2) and (3) of Panel A consists of plants from East and West Germany, respectively. Panel B reports the effects of automation exposure on plant-level markdowns for heterogeneous workers performing different tasks, and the dependent variable is the annual change in plant-level markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications control for constant, six plant size groups based on the number of employees at the establishment in the previous year, and demographic characteristics of districts or kreise in the previous year. The firm, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table 2.36: Plant-Level Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers in East and West Germany

	Dependent variable: Annual change in plant-level markdowns		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. East Germany		
ΔPredicted robot exposure	0.012	-0.002	0.002
	(0.005)	(0.009)	(0.008)
N	3649	3649	3649
	Panel B. West Germany		
ΔPredicted robot exposure	-0.002	0.013	-0.005
	(0.004)	(0.014)	(0.006)
N	3823	3823	3823
Firm characteristics	✓	✓	✓
Regional demographics	✓	✓	✓
Firm fixed effects	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓

Notes: Panel A presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in East Germany between 1998 and 2018 using the 2SLS IV regressions. Panel B reports the results from the IV (2SLS) regressions for West Germany. In both panels, the dependent variable is the annual change in plant-level markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications control for constant, six plant size groups based on the number of employees at the establishment in the previous year, and demographic characteristics of districts or kreise in the previous year. The firm, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table 2.37: Plant-Level Effects of Robot Exposure on Wage Markdowns for Heterogeneous Workers in Districts from East and West Germany with Different Union Coverage

	Dependent variable: Annual change in plant-level markdowns					
	Bottom 8 deciles			Top 2 deciles		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	0.018	0.004	-0.004	-0.037	-0.004	0.004
	(0.010)	(0.008)	(0.007)	(0.044)	(0.054)	(0.049)
N	3149	3149	3149	224	224	224
	Panel B. West Germany					
ΔPredicted robot exposure	0.000	0.000	-0.004	-0.001	-0.002	-0.000
	(0.011)	(0.022)	(0.011)	(0.002)	(0.003)	(0.003)
N	3273	3273	3273	182	182	182
Firm characteristics	✓	✓	✓	✓	✓	✓
Regional demographics	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The left sub-panel of Panel A presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in districts from East Germany whose union coverage is in the bottom eight deciles of the distribution in the previous period between 1998 and 2018 using the IV (2SLS) regressions. The right sub-panel of Panel A reports the results from the IV (2SLS) regressions for plants in districts from East Germany with high union coverage (i.e., districts in the top two deciles of the distribution of district-level union coverage). Panel B's left and right sub-panels show the corresponding results for West Germany. In all panels, the dependent variable is the annual change in plant-level markdowns for routine workers (columns (1) and (4)), nonroutine manual–NRM workers (columns (2) and (5)), and nonroutine cognitive–NRC workers (columns (3) and (6)). All specifications include the same set of controls and fixed effects as in Table 2.36. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table 2.38: Relationship between Robot Exposure, Robot Exposure Predicted from the First-Stage of 2SLS, and Actual Robot Adoption

	Automobile robots			All industrial robots		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Dependent variable: ΔRobot exposure					
ΔRobot exposure predicted from the first-stage	0.589	0.288	0.278	0.630	0.350	0.362
	(0.068)	(0.080)	(0.077)	(0.054)	(0.063)	(0.063)
N	1023	1021	1011	1023	1021	1011
R^2	0.42	0.64	0.68	0.41	0.77	0.80
	Panel B. Dependent variable: ΔActual robot adoption					
ΔRobot exposure predicted from the first-stage	0.388	-0.046	-0.116	0.013	-0.035	-0.051
	(0.308)	(0.113)	(0.178)	(0.074)	(0.060)	(0.057)
N	815	811	803	815	811	803
R^2	0.04	0.36	0.42	0.04	0.49	0.52
Year fixed effects	✓	✓		✓	✓	
State fixed effects	✓			✓		
District fixed effects		✓	✓		✓	✓
State-by-Year fixed effects			✓			✓

Notes: The table presents the results from OLS regressions estimating the relationship between the annual change in the local labor market's exposure to robots in the automotive industry (left panel) and all industries (right panel) predicted from the first stage of the IV (2SLS) estimation and the annual change in robot exposure defined by the equation (2.5) (top panel) and annual change in actual robot adoption (bottom panel) in Germany between 2015 and 2018. The first-stage regression controls for instruments and covariates in equation (2.4). The actual robot adoption is measured by aggregating the number of robots adopted by the firm at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. Standard errors clustered by districts are in parentheses.

CHAPTER 3

PUBLIC WORKS PROGRAM, LABOR SUPPLY, AND MONOPSONY

Abstract

This paper studies the role of changes in workforce composition in monopsony power. We develop a monopsony model in which a firm's wage markdown–the wedge between MRPL and wage–is a weighted average of markdowns for different workers. We empirically test 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 increases average markdowns by crowding out mobile workers and effectively increasing the share of immobile workers with low labor supply elasticity.

3.1 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 [78, 80, 103] and through risk-sharing [217, 20].[1] Such non-labor market policy interventions also impact non-targeted groups' labor market outcomes by changing the labor supply and

[1] There is also evidence against the risk-sharing effect of cash transfer policies via network mechanisms, such as [124] who show that unconditional cash transfers in Kenya had a negative spillover effect on ineligible households within the community.

wages of participants who work for nonparticipants [36] and via enabling ineligible individuals related to the eligibles to search for better jobs [22].

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 [185] and crowding out effects [135, 225]. These few studies, estimating the spillover effects of the NREGA program that generates public work focus on private-sector labor markets. Additionally, [108] 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 ap-

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

proach. 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 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., 87, 116]. 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 [67, 214].[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 monopsony power over contract

[3] One can review [35] who summarize the potential issues with the staggered DID design and alternative solutions proposed in the current literature and [109] 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 infor-

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 [28, 137], agricultural wages [44], and incomes of the rural poor [185]. The second strand of the literature on the indirect effects of the NREGA studies its spillover impacts on child labor [140, 165], private works

mation as an instrument, and production parameters and markdowns are not estimated for the first period.

[135, 185, 225], and urban labor markets [137]. However, an indirect effect of the program on manufacturing labor markets is understudied, while [14] provide employment and wage impacts in manufacturing. Our findings on employment and wage effects are strongly consistent with [14], 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 [69] and [170]. 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, [56, 57] 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 [130], 50 cents in Brazil [105],

[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.

and 71 cents in Colombia [18] and even those in developed countries, for example, 65-80 cents in the U.S. [224][6] and 79 cents in Germany [61]. The median markdown estimates for India from [56, 57] are below unity and around 0.5, implying that workers have market power in most manufacturing firms.[7] However, other studies such as [185] 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 [180, 177, 57, 224, 91] by closely following [224]. 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 [177, 157, 105, 130, 160], infrastructure investments [56, 193], foreign direct investment [167], wage-setting collusion between firms [91], displacement threat from automation [61] contribute to labor market power. However, evidence on the drivers of monopsony power is still needed [68]. Among studies on NREGA, [185] find that an experiment of the public employment pro-

[6] Other studies also suggest that labor market power exists in the U.S. labor market [45, 161].

[7] Another study suggesting perfectly competitive labor markets in developing countries is [157] 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 [128, 66, 205], distaste for commuting [84], job tasks being performed by the worker [32, 61], and for production and non-production workers [224] using administrative and experimental data. Also, some studies examine the heterogeneity of monopsony power by labor market tightness [127] and industries [33]. These studies on heterogeneity in labor market power mainly estimate labor supply elasticities for different workers as a measure of monopsony power, except for [224] and [61] who estimate markdowns across heterogeneous labor.

gram 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 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 3.2 provides background on the NREGA program, focusing on its aspects related to the study. Section 3.3 presents the model of labor supply, labor hiring, and the NREGA. Section 3.4 describes the data, and Section 3.5 discusses the method used for estimating the markdowns and the estimated markdowns for India's manufacturing firms. Section 3.6 presents the empirical strategy employed to identify the causal effects. Section 3.7 discusses the empirical results, and Section 3.8 presents the robustness checks. Finally, Section 3.9 concludes.

[9] This paper deviates from [185] 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.

3.2 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 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 3.1 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 [44, 216]. The Ministry of Rural Development, Government of India, outlines the complete list of works permissible under the MGNREGA.[10] As mentioned above, the implementation 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 [196] using mid-1990s district-level data on agricultural wages,

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

agricultural productivity, and the share of scheduled caste individuals. The existing studies suggest that the NREGA increased agricultural wages [e.g., 44] and agricultural production, e.g., aggregate yields and crop production [216]. [135] 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,[11] 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

[11]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, [189] show that only about half of the individuals registered in the government administrative data as NREGA workers existed and worked in Odisha. 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 occurs even if the local government funds the program.

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, [225] finds that the second phase of the NREGA, ignoring the other phases, did not crowd out workers out of private sector jobs. [220] also fails to find an improvement in productivity in the early years. However, [135] 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.

3.3 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.

We 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?

3.3.1 Labor Supply in the Presence of NREGA

We adapt the firm-specific labor supply setup in [69] and [170] 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 of worker-type os in firm i ($\log W_i^{os}$), a_i^{os} is a non-wage employment amenities shifter adjusted for effort and commuting costs, and η_i^{os} is a type I 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 to choose employer i over others in $\mathcal{I}^{os}$ is given by the familiar multinomial form:

$$L_i^{os}(w_i^{os}) = \frac{\exp(\beta^s(w_i^{os} + \tau_i^{os} + a_i^{os}))L^{os}}{\sum_{i' \in \mathcal{I}^{os}} \exp(\beta^s(w_{i'}^{os} + \tau_{i'}^{os} + a_{i'}^{os}))} \tag{3.1}$$

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 equation (3.1) [170]. Denote ℓ_i^{os} as $\log L_i^{os}$:

$$\ell_i^{os}(w_i^{os}) \approx \beta^s \left[(w_i^{os} - \bar{w}^{os}) + (a_i^{os} - \bar{a}^{os}) + (\tau_i^{os} - \bar{\tau}^{os})\right] + \ell^{os}, \tag{3.2}$$

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 (3.2) 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:

$$\begin{aligned} \ell_i^{us}(w_i^{us}) &= \beta^s \left[(w_i^{us} - \bar{w}^{us}) + (a_i^{us} - \bar{a}^{us})\right] + \ell^{us}, \quad s = H, L, \\ \ell_i^{oH}(w_i^{oH}) &= \beta^H \left[(w_i^{oH} - \bar{w}^{oH}) + (a_i^{oH} - \bar{a}^{oH})\right] + \ell^{oH}, \quad o = u, r. \end{aligned} \tag{3.3}$$

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 (3.1), 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 captures worker type-specific (β^s) and firm-specific effects (s_i^{os}) – the larger the share, the less elastic the labor supply. This dependency on relative employment shares suggests that labor supply elasticity can change due to the NREGA program. From (3.2), 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

$$\tau_i^{rL} - \bar{\tau}^{rL} < (\geq)\ 0. \tag{3.4}$$

Henceforth, the NREGA 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 (3.4) is (not) satisfied.[12]

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

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

Summarizing,

Proposition 5 *An NREGA shifts labor supply ℓ_i^{os} backwards and raises labor supply*

[12]This is consistent with [39], 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 [185] makes a similar assessment about the pro-competitive effects of the NREGA program.

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:

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

3.3.2 Labor Hiring in the Presence of NREGA

We formulate the hiring problem in an AKM setup [2], 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 (3.1), 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:

$$\mu_i^{os} = \frac{A_i^s \rho^{os} - W_i^{os}}{W_i^{os}} = \frac{1}{\beta^s (1 - s_i^{os})} = \frac{1}{\epsilon_i^{os}}. \tag{3.5}$$

The log wage of worker os in firm i is:

$$w_i^{os} = logA_i^s + \log \rho^{os} + \log\left(1 + \frac{1}{\beta^s(1 - s_i^{os})}\right). \tag{3.6}$$

From (3.2), (3.5), and (3.6),

Proposition 6 *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 NREGA 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:

$$\bar{\mu}_i^{oL} = (1 - \theta_i^{rL})\mu_i^{uL} + \theta_i^{rL}\mu_i^{rL} \tag{3.7}$$

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 6, 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 (6), 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 (3.7) 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 NREGA 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 6.

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.[13] 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 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., 121]. It allows 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 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:

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

[13] There may be general equilibrium-type effects. We explore these in Section 3.7.3 using a production function that allows for complementarities between skilled and unskilled workers within a firm.

3.4 Data

3.4.1 NREGA Data

The data on policy change, NREGA, that we investigate in this paper is based on [135], 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.[14] 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.

3.4.2 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

[14] Several studies provide data on NREGA districts, such as [44] and [225]; however, these datasets do not cover all districts, given their specific research questions and empirical methods. For instance, [225] 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.

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×4-digit NIC Industry stratum and thus are not necessarily panel.

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[15] and sales. Although the data

[15]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

does not report the quantity of goods produced and sold at the product level, the available information is sufficient for estimating unbiased markdowns.[16] 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.

3.4.3 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 CHIRPS satellite 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

non-manufacturing mandays worked from 1999-2008.

[16] As shown in [224], 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 [155, 50].

would also affect the labor market conditions of the manufacturing industry.[17] 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.[18] 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×Two-digit NIC Industry×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

[17] Several studies study the combined impacts of NREGA and rainfall shocks on labor markets. For instance, [168] 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, [216] 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, [225] also shows that, after the NREGA rollout, private sector wages increase substantially for women but not men, and these effects are concentrated during the main agricultural season. Additionally, [145] 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.

[18] https://censusindia.gov.in/census.website/data/census-tables

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., 212] and imperfect [e.g., 40] 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 thus leverage the number of inspections per worker at the state level to measure minimum wage enforcement following [209], 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.

3.4.4 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 3.1 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.

3.5 Estimating Markdowns

3.5.1 Estimation Method

We estimate the plant-level markdowns using the "production" approach following [224]. The duality of a firm's profit maximization and cost minimization problem yields the following expression for wage markdown,

$$\nu_{it} = \frac{\theta^L_{it}}{\alpha^L_{it}} \cdot \mu^{-1}_{it}, \tag{3.8}$$

where ν_{it} is the markdown for firm i in year t, θ^L_{it} is the output elasticity of labor, α^L_{it} 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^L_{it}) and price markup (μ_{it}), we estimate the production function using a "proxy variable" method [192, 163, 11]. We measure markup in the spirit of [90], who show that

$$\mu_{it} = \frac{\theta^M_{it}}{\alpha^M_{it}}, \tag{3.9}$$

where θ^M_{it} is the output elasticity of a variable input M_{it} other than labor, e.g., material inputs, and α^M_{it} 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^L_{it}) and the share of material cost in revenue (α^M_{it}) 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:

$$\begin{aligned} \theta^L_{it} &= \hat{\beta}_l + \hat{\beta}_{kl} k_{it} + \hat{\beta}_{lm} m_{it} + 2\hat{\beta}_{ll} l_{it}, \\ \theta^M_{it} &= \hat{\beta}_m + \hat{\beta}_{km} k_{it} + \hat{\beta}_{lm} l_{it} + 2\hat{\beta}_{mm} m_{it}, \end{aligned} \tag{3.10}$$

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., $\theta^L_{it} = \hat{\beta}_l$ and $\theta^M_{it} = \hat{\beta}_m$.

To estimate the production function and identify parameters β, we follow

[224] who also closely followed [90] and perform IV-GMM[19] estimation, relying on the refined control function approach proposed by [11]. 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

$$\begin{aligned} y_{it} &= f(\mathbf{x}_{it};\beta) + \omega_{it} + \varepsilon_{it} \\ &= f(\mathbf{v}_{it}, \mathbf{k}_{it};\beta) + \omega_{it} + \varepsilon_{it}, \end{aligned} \tag{3.11}$$

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.

3.5.2 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 3.2 shows the baseline plant-level mark-

[19] The intuition of estimating production parameters using the proxy variable method can be thought through the IV logic [219, 224] 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}.

downs. 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 [56, 57], who argue that there is no labor market power in India's manufacturing, mainly due to a difference between [224]'s and their methods.[20] However, our estimates are generally consistent with [185], 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 plus one to avoid losing observations when taking natural logs. Table 3.3 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 base-

[20]Specifically, [224] estimate the total wedge between wage and MRPL, while [56, 57] 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.

line estimates by using the Cobb-Douglas production function as an alternative functional form. As shown in Appendix B.1, 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 B.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 3.2(a) shows that larger firms charge significantly lower markdowns in India's manufacturing industry. This contradicts the previous findings from the U.S., where plant-level markdowns are associated with larger firms [224].[21] Second, as shown in Figure 3.2(b), firm age is also negatively correlated to markdowns in India. However, for an advanced economy in the U.S., [224] suggest that the age-markdown relationship is positive but only weakly significant over the age distribution. Third, Figure 3.2(c) 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

[21]This descriptive finding is one of the reasons that we prefer to use [224]'s 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.

from those for U.S. manufacturing.

Aggregate markdowns. Now we discuss the aggregate markdowns—the weighted harmonic means of plant-level markdowns—constructed following [224]. Figure 3.3 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 [56] 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 our baseline measure and suggests that the markdown estimate is not strongly specific to our choice of functional form. Appendix B.2 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 B.1.2, we discuss the trend of aggregate markups. Figure B.7 also presents the trends in other components of markdowns. The labor share presents a downward trend, and the labor output elasticity has been stable.

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 3.4 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 [32] and [61], 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.

Second, we estimate the markdowns for workers with different employment contracts: regular workers who are directly employed and contract workers hired through contractors. Table 3.5 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]

[23] Appendix B.1.3 presents the distribution and trend of markdowns for heterogeneous workers.

3.6 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.

3.6.1 Empirical Specification

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

$$Y_{it} = \alpha + \beta \times \text{Post NREGA}_{dt} + \mathbf{X}'_{it}\gamma + \phi_i + \delta_{jst} + \varepsilon_{it}, \tag{3.12}$$

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 B.8 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 [46]. 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.

3.6.2 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 [79]'s specification:

$$Y_{it} = \alpha + \sum_{\tau \neq -1; \tau=-7}^{\tau=1} \gamma_{1\tau} \times I_\tau \times \text{Phase1}_d + \sum_{\tau \neq -1; \tau=-8}^{\tau=0} \gamma_{2\tau} \times I_\tau \times \text{Phase2}_d + \mathbf{X}'_{it}\gamma + \phi_i + \delta_{jst} + \varepsilon_{it}, \tag{3.13}$$

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 (3.12). 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 3.4(a) 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 3.4(b) shows that a parallel pre-trend is plausible for markdowns among the treated and control groups. Additionally, Figures 3.4(c) and 3.4(d) report the results from event study regressions for wages and MRPL and suggest that parallel pre-trends assumption holds for average wage and MRPL. 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.

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., 1]. To check the "no-anticipation" assumption, we conduct placebo tests by shifting the treatment date by one year before the event year. Figure 3.5(a) 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 3.5(c)-3.5(d) suggest that the no-anticipation assumption holds for markdown, wage, and MRPL, respectively.[24]

[24]In Figure B.9, we also find that the treatment effects on employment, markdown, wage, and MRPL 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.

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 [137], 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. [136] 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

[25]Other studies also provide different results regarding the impact of NREGA on rural out-migration, such as [81], that suggests that the program implementation in West Bengal, India, had no significant effect on migration decision.

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 [137]'s. 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 3.8. Figure B.10 depicts the two alternative settings where never-treated or phase-3 districts surrounded by the treatment districts have been removed.[26]

3.7 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.

[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.

3.7.1 Average Effects

The average effects of the NREGA program on labor market outcomes are reported in Table 3.6. 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 (3.12) 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

[27]Table B.11 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 [149], who suggests a null effect of rainfall on employment in the non-agricultural sector in rural India.

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, [56] 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 [56] estimated was weakly significant at the 10% level when they used markdown estimated under the [90] method. However, the statistical significance of the relationship slightly increased to a 5% level for alternative markdown measures. This could be consistent with our statistically insignificant impact on the markdown measure estimated in the spirit of [90].

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 B.13.

[28]As reported in Table B.12, the firm's age is associated with lower markdowns, confirming our descriptive finding on the age-markdown relationship in Figure 3.2(b), 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.

The baseline estimates of employment and wage effects are generally consistent with [14], 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 [185], 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 B.17 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 private manufacturers in Andhra Pradesh increases, contrary to [185], 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.

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

3.7.2 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 3.7 reports the results.[30] As shown in Panel A, employment falls for below-median productivity firms, remarkably consistent with [14]. 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 work-

[30] Tables B.14-B.16 present the detailed results showing the effects of individual terms of the interaction.

ers.

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 [149]'s 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 (3.13) 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 from the DID model above, and the impacts are mainly driven by the second phase of the program (Figure 3.6). 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).

[185] 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 B.17, 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 [16], 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 3.8 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.

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 3.9 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.

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 3.10 shows the estimated 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 mini-

[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.

mum wage. This result is also consistent with the effects concentrated among low-paying firms.[32]

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 are less attractive if the minimum wages are not enforced or the NREGA jobs pay lower than the promised level. So, NREGA jobs are 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 3.7 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 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 B.4.1 shows that these results heterogeneous by mini-

[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.

mum wage enforcement are robust leveraging different parts of the distributions of wage-to-minimum wage ratio and minimum wage enforcement.

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 3.8 shows that the impacts heterogeneous by labor productivity identified in Table 3.7 are mainly concentrated among firms in the top five industries.[33,34]

Urban and rural firms. The NREGA guarantees rural employment; how-

[33]Figures B.11-B.13 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 B.14 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.

ever, 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 3.9).

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 [184, 159, 187]. 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 3.10 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.

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 3.11 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

[35] The origins of administrative urban and rural settlements in India can be found in [126], while [215] discuss the definition of urban and rural areas according to NREGA.

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 B.3 provides additional results from estimating the treatment effects on worker turnover at the establishment.

3.7.3 Worker Heterogeneity

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 3.11 presents the labor market effects of the 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]

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 3.12, the labor market effects at low-paying firms found in Table 3.9 are concentrated among production workers, similar to heterogeneity by labor productivity above.

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 3.13, 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]

[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 B.18 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.

[37]In Table B.19, we present the heterogeneous effects based on an interaction method, which

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.

3.8 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.

3.8.1 Heterogeneity by Sample Splitting

Heterogeneous effects can be estimated using interaction and sample-splitting methods. Sample splitting allows all the coefficients to differ across subsamples, 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 3.14 reports the results.

First, the program leads to a labor shortage for firms with low labor pro-

suggests that the labor market impacts for contract workers in the manufacturing industry are essentially zero.

ductivity. 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.

3.8.2 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 3.15 presents the results.

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.

3.8.3 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 3.16 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.

As shown in Table B.20, 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 B.21 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]

[38]The ASI data further disaggregates total mandays worked into manufacturing and non-

3.8.4 Dropping Control Districts Surrounded by Treated Districts

As discussed in Section 3.6.2, 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 [137]. Using this alternative control group, shown in panel (a) of Figure B.10, 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 3.17 shows the estimation results that are the same as our baseline results.

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 B.22, are similar to the baseline findings. Finally, Table B.23 suggests that the 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]

manufacturing mandays. Appendix B.4.2 checks the robustness of our results using manufacturing mandays as an employment measure.

[39]Further dropping control districts not surrounded by treated districts but neighbors with multiple treated districts as shown in panel (b) of Figure B.10 provide qualitatively the same results (available on request).

3.9 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 [49] or a monopsony framework in which workers have heterogeneous preferences over different employers [69, 170].

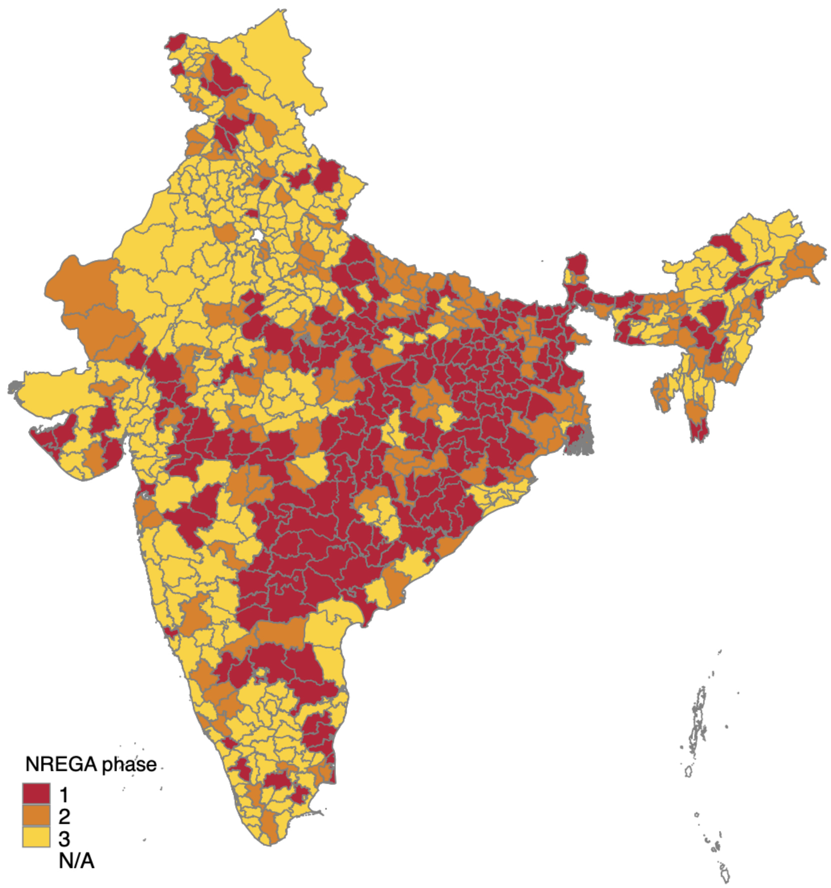
We show that the agricultural industry's hiring of unskilled workers crowded out manufacturing industry work at specific types of firms. The manufacturing plants affected by the treatment are those with low-productivity workers, firms that use labor more intensively than capital, and those that pay lower 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 fundamental measure of labor market power by measuring workers' outside options. For example, [144] 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 [185], who examine the general equilibrium effects of the program on private firms.

3.10 Figures and Tables

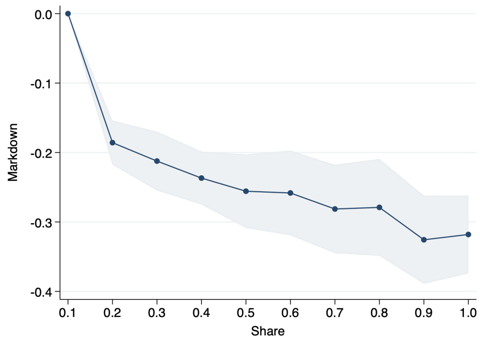
3.10.1 Figures

Figure 3.1: NREGA Phases

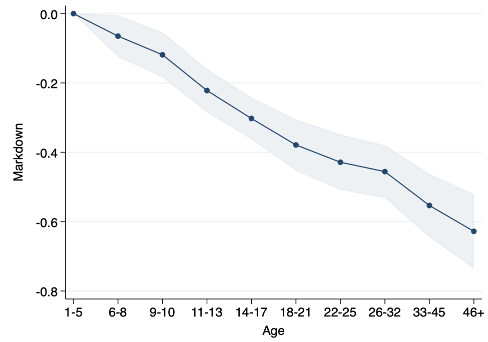


Notes: The figure presents the three phases in which the NREGA program has been implemented in India at the district level.

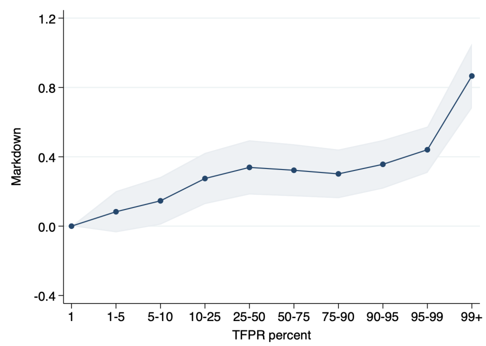
Figure 3.2: Relationship between Markdown and Firm Characteristics



(a) Markdown-Size



(b) Markdown-Age



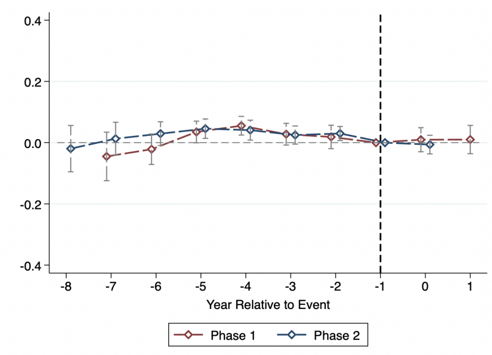
(c) Markdown-TFPR

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).

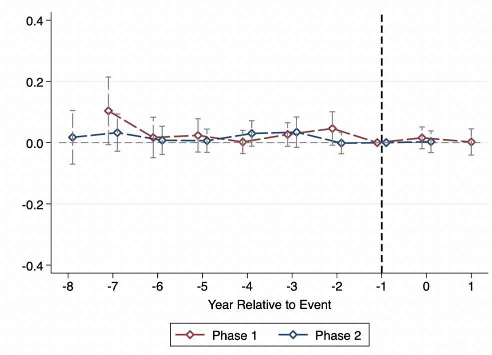
Figure 3.3: Time Evolution of the Aggregate Markdown

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).

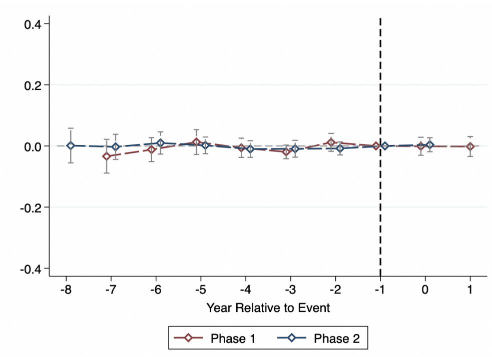
Figure 3.4: Test of Parallel Pre-Trends



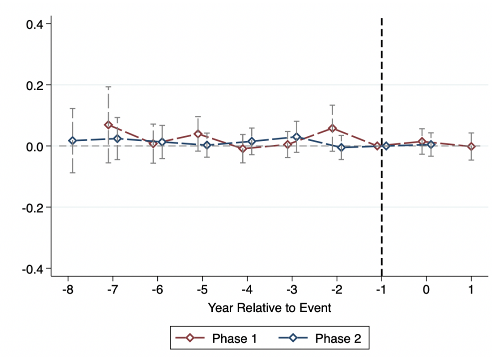
(a) $\ln L_{it}$



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



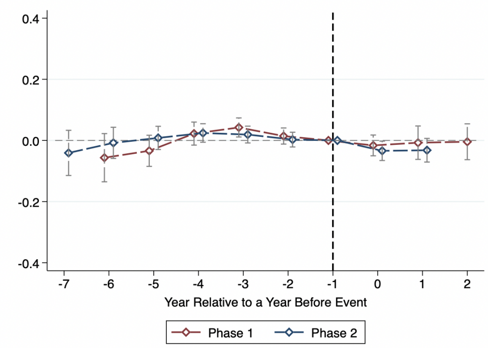
(c) $\ln W_{it}$



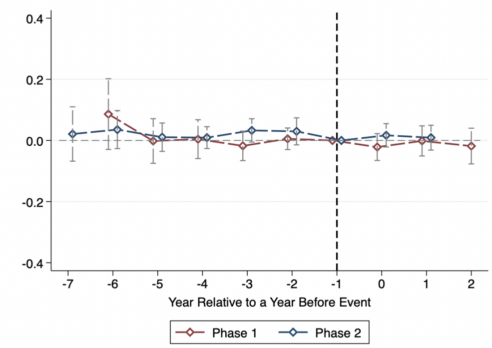
(d) $\ln MRPL_{it}$

Notes: The figure reports the event study estimates from TWFE regressions testing the parallel pre-trends assumption in log employment (headcount plus one, panel (a)), log markdowns (panel (b)), log wage (panel (c)), and log MRPL (panel (d)). 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.

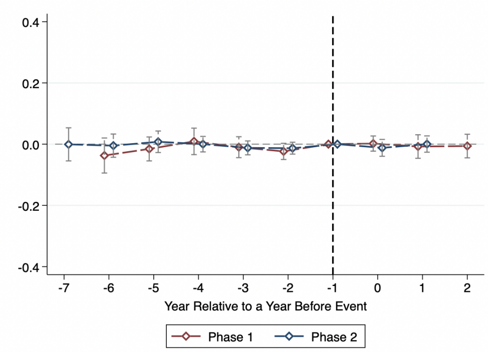
Figure 3.5: Test of No Anticipation Effect Assumption



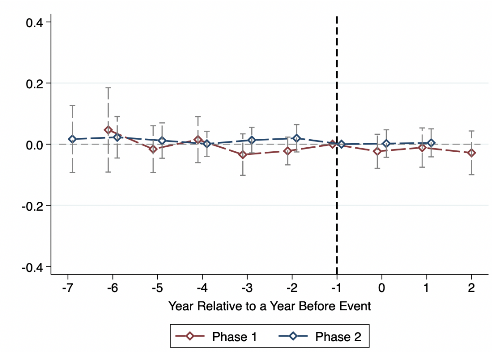
(a) $\ln L_{it}$



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



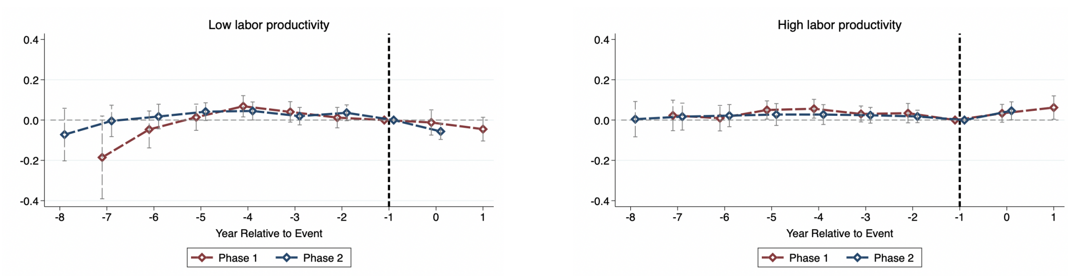
(c) $\ln W_{it}$



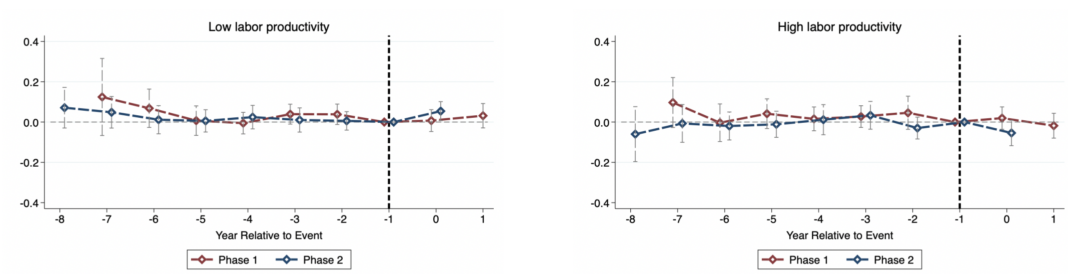
(d) $\ln MRPL_{it}$

Notes: The figure reports the event study estimates from TWFE regressions testing the no anticipation effect assumption in log employment (labor headcount plus on, panel (a)), log markdowns (panel (b)), log wage (panel (c)), and log MRPL (panel (d)). 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.

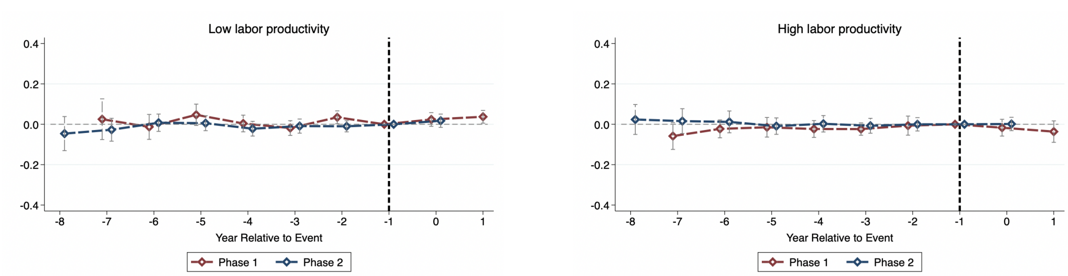
Figure 3.6: Heterogeneous Effects by Labor Productivity



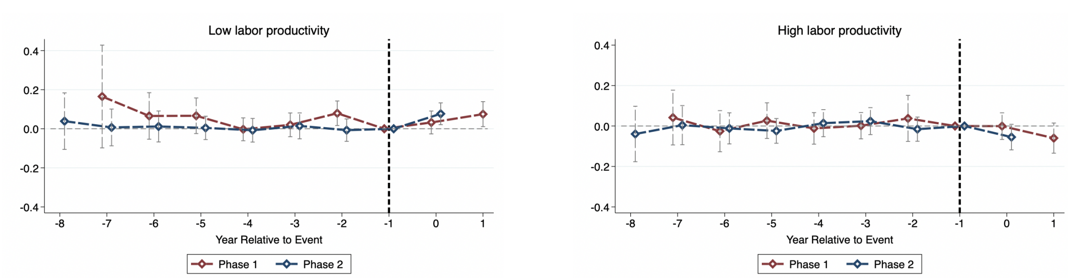
(a) $\ln L_{it}$



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



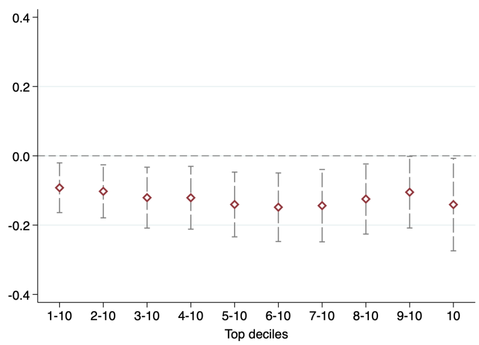
(c) $\ln W_{it}$



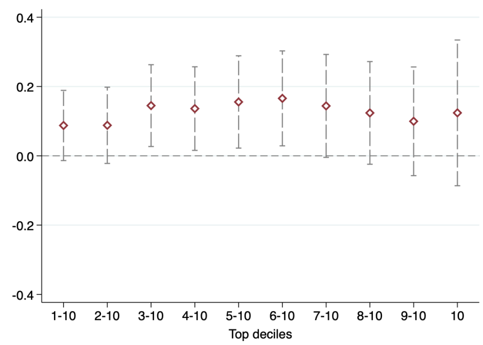
(d) $\ln MRPL_{it}$

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.

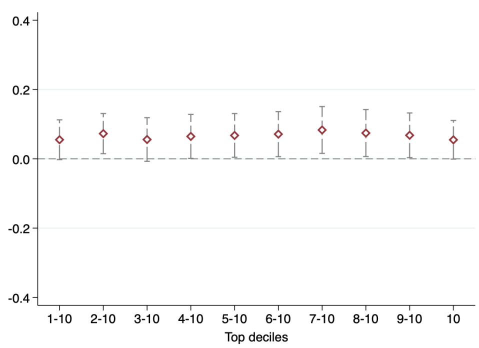
Figure 3.7: Heterogeneous Effects of NREGA by Minimum Wage and Its Enforcement



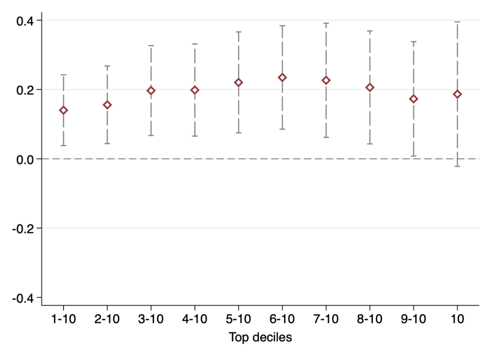
(a) $\ln L_{it}$



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



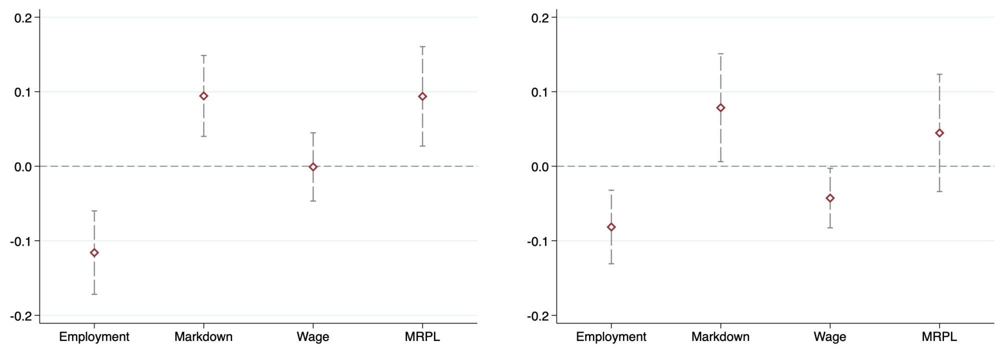
(c) $\ln W_{it}$



(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.

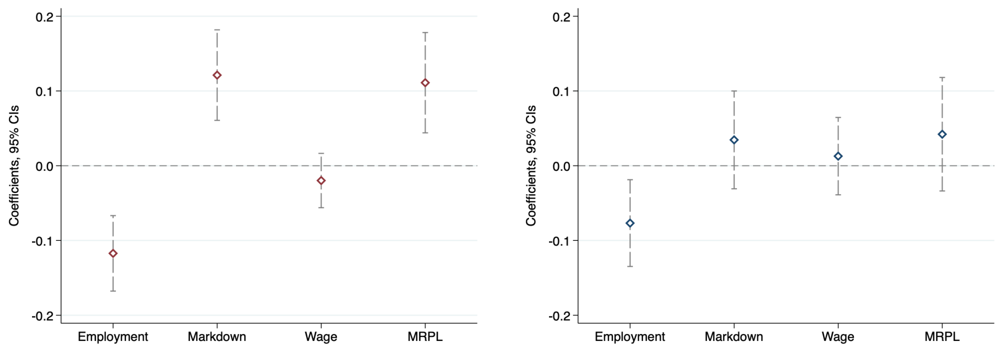
Figure 3.8: Heterogeneous Effects of NREGA by Labor Productivity across Industries



(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.

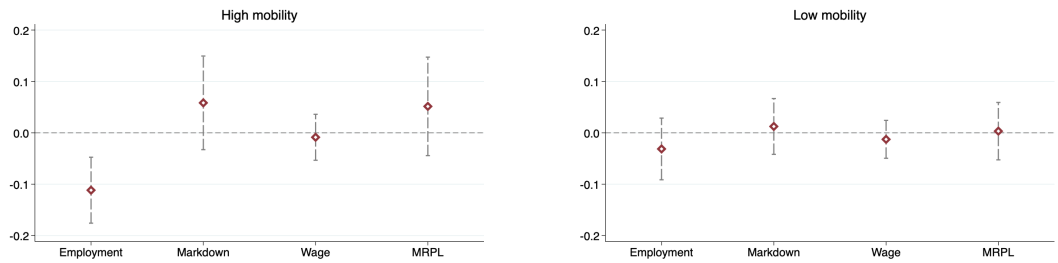
Figure 3.9: Heterogeneous Effects of NREGA for Urban and Rural Firms by Labor Productivity



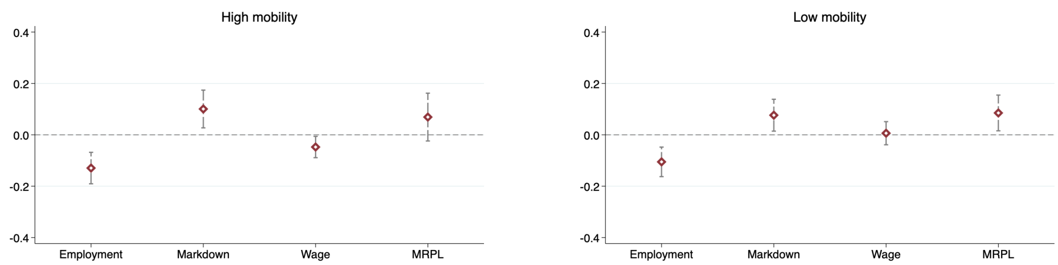
(a) Urban (b) Rural

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.

Figure 3.10: Heterogeneous Effects of NREGA in Districts with High and Low Worker Mobility



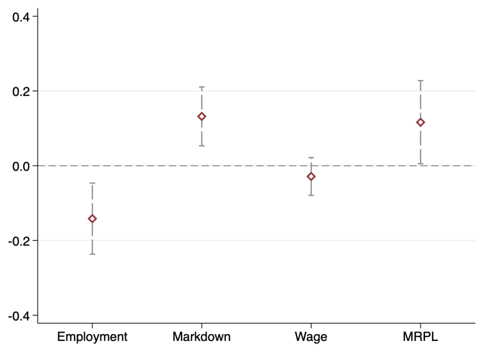
(a) Heterogeneous by urban/rural



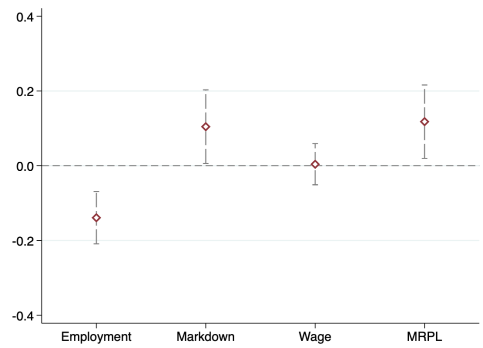
(b) Heterogeneous by labor productivity

Notes: Panel (b) shows the heterogeneous effects of NREGA by firm's urban status in subsamples 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.

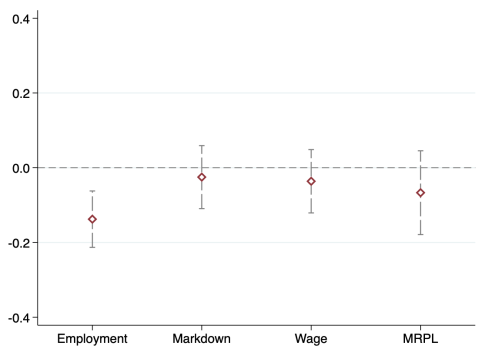
Figure 3.11: Heterogeneous Effects of NREGA by Labor Productivity at Urban/Rural Firms in Districts with High and Low Worker Mobility



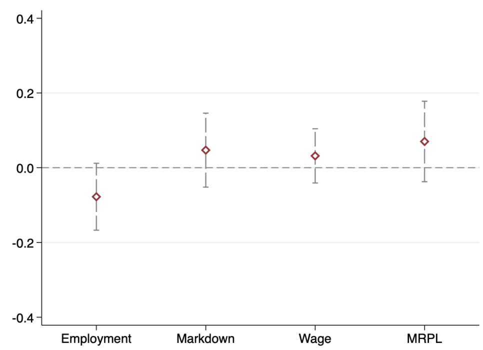
(a) Urban-High mobility



(b) Urban-Low mobility



(c) Rural-High mobility



(d) Rural-Low 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.

3.10.2 Tables

Table 3.1: 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.

Table 3.2: 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.

Table 3.3: 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.

Table 3.4: 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.

Table 3.5: 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.

Table 3.6: 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 ***$p < 0.01$.

Table 3.7: 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 ν_{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 ***$p < 0.01$.

Table 3.8: 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 ***$p < 0.01$.

Table 3.9: 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 ***$p < 0.01$.

Table 3.10: 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 ***$p < 0.01$.

Table 3.11: 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 ***$p < 0.01$.

Table 3.12: 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 $^{***}p < 0.01$.

Table 3.13: Heterogeneous Effects of NREGA on Regular and Contract Workers by Labor Productivity

	Regular workers				Contract 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	-0.069	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 ***$p < 0.01$.

Table 3.14: 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 ***$p < 0.01$.

Table 3.15: 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 ***$p < 0.01$.

Table 3.16: 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 ***$p < 0.01$.

Table 3.17: 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 ***$p < 0.01$.

CHAPTER 4

CAPITAL MARKET INTEGRATION, LABOR MARKET DISTORTION, AND LABOR MISALLOCATION

Abstract

This paper examines the labor market effects of foreign capital liberalization in India, with a focus on labor misallocation and employer market power–a key source of misallocation. We estimate the firm-specific labor markdowns–the wedge between MRPL and wage–as a proxy for monopsony power. Exploiting a foreign direct investment liberalization episode as a natural experiment, we employ difference-in-differences and event study designs to identify the causal impacts of capital market integration on the labor market. For firms with ex-ante high MRPL, liberalization increases employment by 22% and wage bills by 20% and reduces MRPL by 11% and labor markdowns by 13%, with no impact on wages relative to low MRPL firms. The liberalization increases employment and wages and reduces markdowns for men at high male MRPL firms, but reduces MRPL and markdowns for women at high female MRPL firms.

4.1 Introduction

An inefficient allocation of production inputs, or factor misallocation, is a significant source of welfare loss in a country and plays a key role in explaining differences in income and productivity across countries [197, 132]. A large and growing literature on sources of resource misallocation, typically measured by dispersion in marginal productivity of factors, has examined the role of economic factors, such as adjustment costs [25] and dispersion in markups [102, 194].[1]

Globalization via the liberalization of trade and capital changes the allocation of production factors across firms and the efficiency of allocation, which influences a country's aggregate production efficiency. Studies analyzing the role of globalization in misallocation of individual production factors focus on the effect of trade liberalization on misallocation of labor [222] and capital [152].[2] Except [41], who study the impact of foreign capital liberalization on misallocation, primarily focusing on capital misallocation, in India, the effect of foreign capital liberalization on misallocation, particularly labor misallocation, is understudied, and the associated mechanisms and heterogeneity are less clear. No study yet examines the role of foreign capital liberalization in labor misallocation through the lens of imperfect competition in the labor market. This paper thus provides micro-level evidence on the role of labor market distortions and labor market competition in labor misallocation by estimating the effects of foreign capital inflows on domestic firms' labor market outcomes across heterogeneous firms, with heterogeneous workers.

We utilize India's foreign capital liberalization as a natural experiment to study the labor market effects of capital market integration. From the early 1990s until the mid-2000s, India has been deregulating the foreign ownership of domestic firms in several episodes. The policy introduces an automatic approval of foreign equity investments up to 51% of domestic firms' equity shares

[1] The causes and effects of misallocation have been extensively reviewed in [198].

[2] Specifically, [222] suggest that China's trade liberalization via lower input tariffs after its accession into the World Trade Organization (WTO) in 2001 improves labor misallocation by reducing the labor market power (equivalently, employer power or monopsony power). In South Africa, [152] find that a one standard deviation increase in import tariffs creates 2.8-6.2 percent aggregate productivity losses via increasing the misallocation of capital among import-competing domestic firms.

for given industries in several episodes. This reform is unique as it increases the overall size of financial capital available for firms and changes the allocation of resources across firms, enabling us to investigate the effects on misallocation, particularly labor misallocation, and labor market power. The labor market power and the misallocation of labor will decrease as the within-industry dispersion of MRPL declines if the increased foreign capital allows firms with high MRPL to grow faster than those with low MRPL.

Leveraging industries to which the program was introduced in 2006 as the treatment group and never-treated industries as the control group,[3] we employ a difference-in-differences (DID) and event-study methods to estimate the causal effects of capital market integration on labor market outcomes. Our regression model also controls for rich sets of fixed effects, including firm, year, firm age, and pre-treatment firm size-by-year fixed effects, which capture various unobserved determinants and enable us to isolate the impacts of the reform. Similar to other studies that implemented a similar strategy to identify the effects of the same policy, we find that the identification assumptions, primarily no selection and the common pre-trends assumptions, are plausible in our setting.

We use two firm-level panel datasets to measure the labor market power and MRPL and quantify the labor market effects of the policy reform. The primary source of the firm panel is India's Annual Survey of Industries (ASI) over the period 2000-2018, a nationally representative survey of manufacturing establishments. The ASI data has at least three unique features enabling us to introduce innovations to the literature. First, the national representativeness of

[3]For robustness check, we also consider including industries treated in 1991 in the control group and find that the results remain the same.

the data enables us to infer welfare implications, which were restricted in some earlier works, such as [41], due to data limitations. Second, it reports the headcount of workers at the firm, enabling us to disentangle the effects on wage bills into employment and wage effects. Third, the data provide information on heterogeneous workers, for example, labor costs and headcounts of male and female workers at the firm, allowing us to analyze the impact of the policy change on different workers. The secondary firm-level data is Prowess, a panel of large and medium-sized publicly traded Indian companies over the same period. This data provides at least two advantages. First, it allows us to check the robustness of estimates of key variables, such as wage markdowns. Second, the data collects information on firms' equity composition, enabling us to calculate the fraction of foreign equity shares in total equity shares and estimate heterogeneous effects by ex-ante foreign equity exposures. Both firm-level panel datasets provide detailed information necessary to quantify the wage markdowns as a measure of labor market distortions or a proxy for labor market power using the production function approach [224].[4]

We find that the 2006 reform reduced the MRPL by 11% for high MRPL firms compared to low MRPL firms. Moreover, this paper decomposes the effect on wage bills, leveraging detailed information on employment and wages. In particular, we show that an increase in wage bills by 20% is driven by an employment increase by 22%, not through an increase in wages, as we fail to find any wage effect. The positive employment effect indicates a pattern of labor reallocation towards high MRPL firms, a reason for improving misallocation. For firms with initially high MRPL, liberalization reduces wage markdowns by 13% relative to low MRPL firms. The markdown-reducing impact of the policy

[4]The ASI and Prowess provide establishment- and firm-level data, respectively, and we interchangeably use the terms establishment, firm, and employer throughout the paper.

coincides with the labor reallocation and misallocation reduction, suggesting lower labor market distortions due to the reform as a potential mechanism. Our analysis with heterogeneous workers suggests that the liberalization increases employment by 60%, wage bills by 61%, and wages by 17% and reduces markdowns by 20% for men at high male MRPL firms. For women, we find that the liberalization has no effect on their employment and wages, but reduces their MRPL by 82% and markdowns by 79% at firms with high female MRPL compared to low female MRPL firms.

This study contributes to several strands of literature. Our first contribution is to the literature on misallocation, especially on labor misallocation in developing countries. The literature investigating how capital account liberalization affects domestic firms focuses on its impact on productivity, sectoral and capital misallocation, and welfare [118, 218, 223, 173]. We make two contributions to this literature. First, we quantify the labor wedge, a potential source of misallocation, and measure MRPL consistent with the wedge. Existing studies, such as [41], focus on measuring MRPL based on a simple Cobb-Douglas production function estimated without accounting for the endogeneity problem due to unobserved firm-specific productivity. Our paper instead estimates the production function using the "proxy variable" method [11] and measures MRPL, allowing for imperfect competition in both output and labor markets. The foreign capital liberalization may affect labor misallocation through several channels, such as (i) relaxing firms' financial constraints [107], (ii) labor reallocation and the associated changes in labor market power, and (iii) general equilibrium effects.[5]

[5]Related to the second channel, [167] examine the effect of China's foreign direct investment (FDI) reform around its WTO accession in the early 2000s on labor market power. But their focus is not on misallocation. They find that capital liberalization increases firms' market power in the labor markets, which is opposite to the effect of trade liberalization suggested by the existing studies in the monopsony literature.

We thus, second, quantify how much the change in labor wedge contributes to changes in labor misallocation due to foreign capital integration. Leveraging India's foreign capital liberalization episodes in 2001 and 2006, [41] show that the reforms reduced MRPL by 28% and increased wage bills by 24% for firms with high ex-ante MRPL relative to low MRPL firms. While our results on the MRPL and wage bill effects of the 2006 foreign capital liberalization episode are consistent with their findings, we provide new results by quantifying the employment, wage, and labor wedge effects.

Second, we contribute to the literature on the firm's monopsony power. We add to this literature in two ways. First, we contribute to the fast-growing literature exploring the determinants of labor market power, particularly the strand that examines foreign capital in terms of trade and investment. The existing studies focus on trade shock to investigate the labor reallocation as a source of the firm's wage-setting power [177, 105, 195, 160, 158, 222, 131].[6] However, there is no study yet on the link between FDI and monopsony. An exception is [167] who show that the FDI liberalization increases wage markdowns using the relaxation of FDI regulation during China's WTO accession in 2001. Our results suggest the average effect of India's FDI reform on labor market power is essentially zero, but the effects are highly heterogeneous across firms with

[6] Specifically, [195] shows that export expansion through bilateral trade agreement (BTA) between the U.S. and Vietnam reduces labor markdown, measured by directly dividing marginal revenue product of labor (MRPL) to wage, by 3.0% in Vietnam at the firm level. The reduction happens mostly for female workers, i.e., markdown on female workers decreases by 13.5% due to the BTA shock. Following [111] approach, the author estimates a mean markdown level of 2.14 and a median of 1.49. Using firm-level data in China, [158] suggest that input tariff reductions due to joining WTO are associated with lower labor markdowns and that the labor markdown decline is more for skill-intensive firms than non-skill-intensive firms. On the contrary, [105] finds that Brazil's 1990s trade liberalization increased labor market concentration and thus the firm's labor market power, using Brazil's matched employer-employee data. Using administrative data from the AFiD database on German manufacturers, [177] suggests that an increase in export demand increases a firm's labor market power. Import competition, however, reduces the labor market power.

different ex-ante MRPL and wage markdowns. Second, we relate to studies measuring monopsony power for heterogeneous workers such as male and female workers. In terms of measurement, we estimate markdowns in a developing country using two alternative datasets and introduce worker heterogeneity, focusing on the gender dimension. For example, existing studies measuring monopsony power for male and female workers estimate labor supply elasticities [128, 66, 205]. We, however, provide the first estimate on wage markdowns over male and female workers to provide a gender-specific measure of monopsony power. We find that women are subject to higher labor market power than men, consistent with other studies.

Third, we complement the literature on the social and economic impacts of foreign firms via multinational firms and foreign capital, especially on the labor market and a vulnerable group like women. Studies on the gender-specific labor market effects of multinational firms provide mixed results. For example, [206] suggest that multinationals improve profitability and productivity by intensively hiring from a marginalized social group, women, in the local managerial market in South Korea. Using the same FDI liberalization episode that we study in this paper, [164] show that economic integration reduces rape via empowering women due to an increase in women's relative income and an introduction of gender equality norms. However, [115] find that foreign firms pay women disproportionately higher wages than domestic firms at the cost of disproportionately lower amenities relative to men. Women sort away from foreign firms due to poorer amenities, leading to a smaller share of women at foreign firms than domestic firms. Our results suggest that the effects of FDI inflows on manufacturing firms' labor market outcomes are mainly driven by male workers, while the reform reduces the labor misallocation and markdowns for female workers, with no impact on their employment and wages.

The rest of the paper is structured as follows. Section 4.2 provides background on the capital market and the FDI liberalization in India. Section 4.3 describes the data and discusses the estimation of markdown and MRPL for manufacturing firms. Section 4.4 presents the empirical strategy used to estimate the causal effects of capital market integration. Section 4.5 discusses the empirical results, while Section 4.6 checks the robustness of our main findings. Finally, Section 4.7 concludes.

4.2 Background

In the early 1990s, India implemented a structural adjustment program to overcome the balance of payment crisis in 1991. This reform covers trade liberalization, the relaxation of licensing restrictions, and the opening to foreign investment. Before 1991, most of the manufacturing industries in India were heavily regulated by the Foreign Exchange Regulation Act of 1973, which restricted foreign ownership to below 40% in most sectors and placed restrictions on the use of foreign brand names, the remittances of dividends generated in India by foreign affiliates and the use local content in output [208].

The reform in 1991 relaxed such restrictions by allowing automatic approval of foreign ownership up to 51% in given industries. This new policy was controversial given that India had been accustomed to protectionist policies for decades, and many businesses were concerned about the potential domination of foreign capital [207]. Thus, only certain industries were chosen for the reform, and politics played a significant role in the industry selection. Firms in concentrated industries and state-owned firms were more successful in preventing the entry of foreign capital [76].

More industries joined the liberalization over time, increasing the cap on foreign equity and/or enabling the automatic approval route. According to the NIC industry classification, there are 400 five-digit industries in India. The policy was introduced into 124, 1, 5, 5, and 19 five-digit industries during the episodes of the reform in 1991, 1998, 2000, 2001, and 2006, respectively [41]. In contrast to the 1991 episode, the 2000s reform was motivated by a political consensus to boost foreign investments to counteract a declining trend in FDI during the 1998-1999 period [138, 139].

4.3 Data and Measurement

In this section, we first describe the data used in our empirical analysis. Second, we present the methodological approach for measuring the key variables, including markdowns and MRPL, and discuss the estimated measures. Third, we provide descriptive statistics on the main variables in our analysis.

4.3.1 FDI Liberalization Data

We extract the FDI policy change data at the industry level from [41]. Industries in India are classified based on the National Industries Classification (NIC). The data contains the list of 5-digit NIC-2008 industries and their year of reform. An industry is classified as under reform or treated if the policy allows automatic approval of foreign investment and/or increases the cap on foreign ownership to at least 51%. To match with the firm-level data, we use concordance tables to convert the industries to NIC-2004 and NIC-1998 versions.

4.3.2 Firm-Level Data

ASI Data. The primary firm-level data in our empirical analysis is the panel version of the Annual Survey of Industries (ASI), which offers comprehensive data for India's industrial sector. It is a nationally representative survey of all factories registered under The Factories Act of 1948, which are defined as factories employing at least 10 workers and do not use electricity or employing at least 20 workers without electricity. Our panel version spans from 1998-1999 to 2017-2018, where each round covers the financial year from April 1 to March 31.

We restrict the sample to manufacturing firms and extract detailed information on the firm's production inputs and output for firm-level markdown estimation. It includes labor headcount, wage bills, material expenditures, capital stock, and sales revenue. The data also contains information on employment and labor payments for various types of heterogeneous workers, which allows us to estimate markdowns and examine the heterogeneity of FDI effects on labor market outcomes for different types of workers, particularly male and female workers.

We match the ASI firms with the FDI data by industry. Since ASI data only reports 4-digit NIC industries as the most granular level, we first collapse the FDI data to the 4-digit level and define the year of reform of a 4-digit industry as the last reform year of its 5-digit sub-class. The 4-digit industry is defined as treated if any 5-digit sub-industry had reform.

Prowess Data. To complement our analysis, we also use the firm panel from Prowess database conducted by the Center for Monitoring the Indian Economy (CMIE). This dataset covers medium and large private firms and all publicly traded firms, which account for more than 70% of the organized industrial activities [41]. It also contains detailed information on the firm's income and balance sheet, allowing us to estimate the markdowns. However, Prowess only provides consistent information for labor costs, while data for labor headcount is largely missing. For example, there are 52,809 (83%) observations with missing headcount in the raw sample. Thus, we use labor costs as labor input in our markdown estimation to maintain a large sample size.

One advantage of Prowess data is that it contains detailed information on the ownership composition of firms' equity shares, which allows us to calculate the percentage of foreign equity shares in total equity shares at the industry level and estimate the heterogeneous FDI effects by an ex-ante exposure to foreign investment.[7,8] To define the fraction of foreign equity shares in total equity shares, we sum the foreign equity shares and total equity shares of all Prowess firms at the 4-digit NIC industry level. We then calculate the percentage of foreign equity shares and take the value of the 2000-2001 financial year to determine the industry's foreign equity exposure prior to the reform. We merge this measure of ex-ante exposure to foreign investment with the ASI data by 4-digit NIC industries to conduct the heterogeneity analysis.

[7]Prowess reports the number of equity shares of the company sourced from the stock exchanges. The number of foreign equity shares is the sum of shares held by (i) foreign promoters, (ii) foreign institutional investors as non-promoters, and (iii) foreign venture capital investors as non-promoters.

[8]A caveat when using Prowess to estimate industry-level foreign equity is that the data only reports equity information for publicly-listed firms, which consists of only 24% of the firms in our sample.

We match Prowess firms with the FDI data by their industry. Since Prowess already contains the firm's industry at the 5-digit level, we can directly merge the two datasets. We also restrict the sample to manufacturing firms between 1994-2019 to avoid complications from the major liberalization period in the early 1990s. In addition, the number of firms grew over time and became more stable after 1995.

4.3.3 Additional Data

We also control for other changes in trade policy at the industry level that might be correlated with the FDI reform treatment and the firm's outcomes. Specifically, we use trade data from the UN-COMTRADE database to construct the industry's exposure to Chinese import competition. The industries from UN-COMTRADE are classified based on the Standard Industrial Classification (ISIC) Revision 3.1 at 4-digit level, which can be mapped one-to-one onto the NIC-2004. We also use the tariff data from the World Integrated Trade Solution (WITS) database and the input-tariff table to construct a measurement for input and output tariffs at the industry level.

4.3.4 Measuring Markdowns and MRPL

Estimation Approach. The establishment-level wage markdowns have been estimated using production approach under different assumptions. In the misallocation literature, [41] employ marginal returns to production inputs, including capital and labor, as a measure of misallocation of the inputs or input wedge for India's manufacturing. They assume a Cobb-Douglas (revenue) production function and thus do not impose the assumptions associated with production function estimation techniques. The wage markdown–wedge between the marginal revenue product of labor (MRPL) and the wage, has been estimated by [195] for Chinese manufacturing industry based on [111]'s nonparametric approach, relaxing the functional form assumption.

However, neither of these approaches takes the price-cost markups in the output market into account. Accounting for the markups, [57] estimate markdowns in the contexts of India and China by estimating production function under various functional forms, with a third-order translog production function specific across 2-digit industries as the primary form. In their markdown estimation, they impose a condition in which small firms with negligible share in the market have no market power in the labor market. The relationship between firm size and markdown has been shown positive and statistically significant for several counties, such as the U.S. [224], Germany [61], and Mexico [104]. This assumption is intuitive; however, it might not hold in all contexts. For example, [62] find that markdown and firm size, measured by employment share similar to the aforementioned studies, is negatively correlated in India's manufacturing sector. Additionally, [61] show that markdown-firm size relationship is negative in East Germany, which is less developed than West Germany where the relationship is positive. Thus, in this paper, we use a more flexible production approach by [224] that does not assume the firm size-markdown link is positive or firms with small market share do not have market power in the labor markets.

From the firm's profit maximization and cost minimization problems, the wage markdown η_{it} is defined as follows

$$\eta_{it} = \frac{\theta_{it}^{L}}{\alpha_{it}^{L}} \mu_{it}^{-1}, \tag{4.1}$$

where θ_{it}^{L} is the output elasticity of labor for firm i in time t, α_{it}^{L} is the share of labor cost or wage bill in revenue, and μ_{it} is the price-cost markup in the output market, which is further defined as

$$\mu_{it} = \frac{\theta_{it}^{M}}{\alpha_{it}^{M}}, \tag{4.2}$$

where θ_{it}^{M} is the output elasticity of a variable input other than the labor, intermediate materials in our case, and α_{it}^{M} is the share of cost on materials in revenue.[9] The shares of labor and material costs in revenue can be calculated directly from the data. The output elasticities of labor and materials are derived from the production function estimation. We use the standard method of [11], as in [90], to estimate the production function. For our baseline analysis, we employ a second-order translog production function at the 2-digit industry level and use an industry-specific Cobb-Douglas functional form as a robustness check. Identifying the consistent estimates of the production parameters and, thus, the output elasticities requires that the firms dynamically optimize their decisions in discrete times and the intermediate materials are fully flexible. The details of the estimation procedure and underlying assumptions are provided in [224]. We also include the policy reforms we investigate, FDI liberalization defined at the industry-year level, in our markup and markdown estimation procedure, like [54].

[9]Solving for the profit maximization problem shows that the markdown, the ratio of the MRPL to the wage, is the inverse labor supply elasticity plus one. The cost minimization problem with respect to labor input yields that the inverse labor supply elasticity plus one is the ratio of output elasticity of labor to the share of labor cost in revenue, multiplied by the inverse of price markup. Equating these two gives equation (4.1). In this approach, we assume that intermediate materials are flexible input or that the market for intermediate materials is perfectly competitive. The cost minimization problem with respect to this flexible input of intermediate materials yields equation (4.2), showing that the price markup is the ratio of the output elasticity of intermediate materials to the costs of intermediate materials in revenue.

Consistent with the definition of markdown in equation (4.1), essentially the ratio of the marginal revenue product of labor (MRPL) to wage, the MRPL, our measure of labor misallocation, is defined as the wage multiplied by the markdown,

$$\mathrm{MRPL}_{it} = \eta_{it} W_{it}, \tag{4.3}$$

where W_{it} is the average wage of workers at firm i at time t.

Markdown Estimates in India's Manufacturing. We estimate the markdown in India's manufacturing using two datasets, including the ASI and Prowess. Table 4.1 presents the estimated wage markdowns using the ASI data from 2000-2018. The results suggest that the labor market in the manufacturing sector is imperfectly competitive as the median and average markdowns are more than unity. In particular, workers in an average firm receive 0.54 rupees for each rupee generated. This estimate is 25% lower than the estimate of 0.72 rupees on the marginal rupee suggested by [62] using the ASI data from 2000-2008. This could be because markdown has been high in a decade between 2008 and 2018.

To analyze this, Figure 4.1 depicts the trend of aggregate markdowns between 2000 and 2018, indicating several intriguing results. First, although the markdown trend after 2008 is generally downward, the markdown has been high for most of the post-2008 periods, potentially yielding a higher average markdown from 2000-2018. Second, the upward trend from 2000-2008 is strongly consistent with [62], who show a similar pattern over the same period. Third, the aggregate wage markdown presents an inverted U-shape pattern over the period 2000-2018 and peaks in 2009 at the dawn of the Great Recession, with a sharp and continuous decline since then. This pattern of markdown after the 2009 Global Financial Crisis (GFC) is strongly similar to the markdown pattern in Germany [61, 99]. Figure 4.2 shows that the aggregate markdown presents a similar trend under an alternative Cobb-Douglas production function.

The wage markdowns estimated based on Prowess data from 1995-2019 are presented in Table 4.2. The results suggest that the publicly-listed manufacturing firms operate in a monopsonistic environment, similar to the findings from the ASI data above. The markdown estimates, however, are persistently higher than those from the ASI data, with a median (average) markdown estimate of 2.692 (3.283). Despite the level differences, as shown in Figure 4.3, the median wage markdown estimates based on the two datasets are fairly correlated at the two-digit industry level, with a pairwise correlation of 0.45 (SE: 0.22, p-value: 0.05).

We evaluate several potential reasons for the difference between the two estimates. First, fixing the time coverage from 2000 to 2018, similar to the ASI data, we find the median (average) estimate of 2.704 (3.297), suggesting that the difference is not due to the difference in time coverage. Second, the markdown has not been estimated for two 2-digit manufacturing industries, including (i) office, accounting, and computing machinery and (ii) medical, precision, and optical instruments, watches, and clocks. So, we calculate the median and average markdowns estimated using the ASI data by excluding these two sectors, which yield 1.374 and 1.871, respectively. These estimates are not different from the baseline estimates, suggesting that the absence of the two industries also does not explain the difference between markdown estimates from the ASI and Prowess data. Third, the labor inputs in the production function are approximated by labor cost when using Prowess data that does not report the headcount of workers. Thus, we estimate the wage markdowns by using the ASI data and proxying labor inputs with real labor costs as we did in Table 4.2, and the median and average markdowns are estimated at 1.536 and 1.762, which are not too different from the estimates that leverage headcounts. The measure of labor inputs thus seems to be not the main driver of the difference. However, the distinction between the markdowns from the ASI and Prowess could be due to the underlying differences between the two data sources, i.e., the publicly-listed and non-publicly-listed firms. Although the ASI data does not report whether the establishment is publicly listed, we compute the average and median markdowns for public and private limited companies that can sell shares to the public and trade them on stock exchanges. The median and average markdowns for such firms are 1.490 and 2.031, respectively, higher than national estimates and estimates for other corporations. Hence, it is likely that the manufacturers included in Prowess data inherently have higher markdowns.

Despite the level differences, the evolution of wage markdowns using Prowess data (Figure 4.4) is highly comparable to the trend of aggregate markdown based on the ASI data. In particular, the time evolution presents an inverted U-shaped trend under both translog and Cobb-Douglas production functions, with a peak around the Great Recession. While we estimate the establishment-level markdowns using Prowess data from 1995-2019, the aggregate markdown has been plotted after 2000 because the markdown trend was noisy over the pre-2000 period, for which very few manufacturing firms covered in Prowess data.

The relationships of the firm's idiosyncratic characteristics, including size, age, and productivity with the wage markdown based on the ASI data from 2000-2018 (Figure 4.5) are strongly consistent with the results from [62] who used the ASI data from 2000-2008. In particular, markdown is negatively correlated with firm size and age and is positively correlated with productivity. These relationships are similar when using Prowess data, except for the markdown-size relationship, which is positive and statistically significant at the top part of the distribution (Figure 4.6). This difference between the markdown-size relationship using the ASI and Prowess data could be another indication of the difference between firms included in the two datasets. The relationship for publicly-listed firms in Prowess is similar to that in developed countries like the U.S. and Germany.

4.3.5 Descriptive Statistics

Table 4.3 shows the summary statistics for ASI firms' characteristics, such as markdown and MRPL, by the treatment (FDI liberalization) status. An observation is at the firm-year level. The two groups are relatively similar in capital, sales revenue, employment, and wages, except that the treated (liberalized) firms are slightly older and have higher markdowns on average. As a supplement, we also show the summary statistics for Prowess sample in Table C.1. Compared to the ASI firms, Prowess firms are much older and higher in all characteristics.[10] Compared to the control group, the treated firms in Prowess data are also relatively similar in capital and sales, while their employment is smaller on average.

[10]Since Prowess records the monetary value in US dollars, we convert it to Indian Rupees to be comparable with the ASI data. The wages in Prowess are measured by total labor cost instead of wages per worker due to limited information on employment headcount.

As the ASI contains employment information for different types of workers, we also explore how gender-specific (male and female) employment evolved for the control and treated industries. As shown in Figure 4.7, the employment share of female workers has been declining in treated industries, while that in control industries moderately grew after the treatment.

4.4 Empirical Strategy

In this section, we first describe the empirical strategy we employ to estimate the causal impact of FDI liberalization on labor misallocation, monopsony, and other labor market outcomes. We then discuss the identification assumptions.

4.4.1 Empirical Specification

We estimate the following difference-in-differences (DID) specification to investigate the labor market effects of capital liberalization:

$$Y_{ijt} = \alpha + \beta \text{Reform}_j \times \text{Post}_t + \mathbf{X}'_{it}\delta + \mu_i + \gamma_t + \varepsilon_{ijt}, \tag{4.4}$$

where Y_{ijt} is one of the outcomes for firm i in industry j at time t, including the logs of employment, wage bill, wage per worker, MRPL, and markdowns. Reform_j is a dummy variable equal to 1 if the industry is liberalized during the 2006 FDI reform and to zero if the industry is never received FDI liberalization. Post_t is an indicator variable equal to 1 if the period is 2006-2007 fiscal year or after, zero otherwise.[11] Our treatment variable is coded at the 4-digit level. The

[11]To check the robustness of our results and assess the anticipation effect, we also consider the periods 2005-2006 fiscal year and after as post-treatment period.

vector $\mathbf{X}'_{it}$ contains firm-level covariates, including firm age and pre-treatment firm size-by-year fixed effects. The firm fixed effects μ_i control all time-invariant unobserved factors, and the year fixed effects γ_t account for all time-varying factors common across firms. The standard errors are two-way clustered at the 4-digit industry and year level to account for any serial correlation.

To analyze the heterogeneous effects of liberalization on changes in input usage among firms within industries, for example, by firms' ex-ante labor misallocation, we estimate the following specification:

$$\begin{aligned} Y_{ijt} = \alpha + \beta_1 \text{Reform}_j \times \text{Post}_t \times I_i^{High\ MRPL} + \beta_2 \text{Reform}_j \times \text{Post}_t \\ + \beta_3 \text{Post}_t \times I_i^{High\ MRPL} + \mathbf{X}'_{it}\delta + \mu_i + \gamma_t + \epsilon_{ijt}, \end{aligned} \tag{4.5}$$

where $I_i^{High\ MRPL}$ is an indicator if the firm i's ex-ante marginal revenue product of labor (MRPL) is high (or above the industry median), and other variables are the same as those in equation (4.4). The average MRPL over the 1999-2000 and 2000-2001 financial years has been used as an ex-ante MRPL in the baseline analysis to avoid the potential spillover effect of the FDI reform introduced in mid-2001 across industries.[12] The time-invariant and firm-specific terms, including $\text{Reform}_j \times I_i^{High\ MRPL}$, Reform_j, and $I_i^{High\ MRPL}$, are captured by the firm fixed effects, and the terms changing over time but common across firms like Post_t are captured by the year fixed effects. The MRPL is defined as the markdowns multiplied by the wage, following our definition of wage markdowns in this paper. The markdown is defined according to our measure in Section 4.3.4.

[12]The 2000-2001 financial year starts on April 1, 2000, and ends on March 31, 2001.

4.4.2 Identification and Assumptions

In our empirical analysis, we focus on the set of industries liberalized in 2006 partly due to the limitation in the time coverage of the ASI firm panel, which starts in the 1999-2000 financial year. Restricting the industries to 2006 reform industries provides more pre-reform periods for the event-study analysis to check the pre-trends. It also allows us to avoid the confounding effect of other reforms in the early 1990s. In addition, since the FDI shock that we study in this paper occurs once and involves a binary treatment, the ordinary least squares (OLS) two-way fixed effects estimator is unbiased for the average treatment effect on the treated (ATT) [87].

Our main empirical specification is the triple difference regression in equation (4.5) that compares firms with high MRPL and high markdowns with low MRPL and low markdowns between treated and untreated industries before and after 2006. The key identification assumption is the parallel trends assumption. In the absence of 2006 reform, outcomes of firms with high and low MRPL and markdowns in treated and untreated industries evolve in parallel. Consistent with [190], we use the following event-study specification to test the parallel pre-trends for our triple difference design:

$$\begin{aligned} Y_{ijt} = \alpha &+ \sum_{\tau\neq-1;\tau=-6}^{\tau=12} \beta_{1\tau} \times I_\tau \times \text{Reform}_j \times I_i^{High\,MRPL} + \sum_{\tau\neq-1;\tau=-6}^{\tau=12} \beta_{2\tau} \times I_\tau \times \text{Reform}_j \\ &+ \sum_{\tau\neq-1;\tau=-6}^{\tau=12} \beta_{3\tau} \times I_\tau \times I_i^{High\,MRPL} + \mathbf{X}'_{it}\delta + \mu_i + \gamma_t + \epsilon_{ijt}, \end{aligned} \tag{4.6}$$

where I_τ are lags and leads in event time, with $\tau = -1$ as the base period. The remaining variables are similar to those in equation (4.5). Below, we discuss potential identification threats.

Selection of Treated Firms. The rationales for liberalized industries are not provided in official documents such as the Economic Surveys. However, using our data, we calculate industry-level characteristics before the 2006 reform and compute the relationship between the reform status and those characteristics to examine whether the initial conditions can predict the likelihood of receiving the reform. All regressions are weighted by the industry's total capital stock in 2005. We report the regression results in Table 4.4. There is little evidence that an industry's reform status is correlated with its ex-ante dispersion in labor or capital misallocation (columns1–2). Columns 3 and 4 show that industries with a higher number of firms or more capital stock are not more likely to receive a reform. Lastly, columns 5 and 6 show that political economy characteristics such as the Herfindahl Index and sales share of state-owned firms are also not predictors for the 2006 reform. Thus, the potential selection bias is implausible in our setting.

We also examine the distribution of employment before and after the 2006 reform by different initial characteristics. We classify the firms based on whether they were initially exposed to (i) high foreign investment, (ii) high MRPL, and (iii) high TFPR. Figure 4.8 shows that before 2006, on average, there was no difference in pre-reform employment across firms with high or low labor wedge and productivity. However, firms in industries with higher initial exposure to foreign investment tend to have higher employment. After 2006, the employment gap shrinks among high and low foreign equity groups, while it is now higher for firms with high initial MRPL and TFPR. It might indicate that firms with higher initial labor distortion or higher productivity could expand their employment after the FDI reform, which is in line with the trade literature on the heterogeneous effects of trade along firm productivity, which reallocate the production resources toward more productive firms [175, 199].

Endogeneity of Foreign Equity Flows. Foreign equities can be flowing into specific firms within an industry. However, our empirical strategy does not exploit an exogenous variation in the amount of foreign capital received but leverages an exogenous shifter to the potential amount of foreign capital. Thus, we do not require the foreign capital inflows to be randomly allocated across firms within treated industries. Instead, our estimates are valid as long as the parallel trends are satisfied.

Measurement Error in MRPL and Markdowns. The MRPL and markdowns are our main outcomes in equations (4.4) and (4.5), but it is also an independent variable in equation (4.5). We thus discuss potential biases from measurement error both in the outcome and explanatory variables. In general, measurement errors either firm-specific and time-invariant or time-varying but common across firms should not create bias since we control for firm and year fixed effects. However, measurement error can bias the point estimates if it is idiosyncratic or firm-specific and time-varying.

Classical measurement errors in MPRL and markdowns as the outcome variables would not bias our point estimates. However, the idiosyncratic measurement error in MRPL and markdowns as independent variables on the right-hand side of the equation may introduce bias as we might misclassify firms with high and low ex-ante MRPL and markdowns. To avoid this potential attenuation bias, we remove outliers by removing firms with extreme values of MRPL and markdowns.

4.5 Results

This section first presents the baseline results. Second, we discuss the heterogeneous effects of the policy by the ex-ante degree of labor misallocation or MRPL and labor market power. We then introduce worker heterogeneity, focusing on gender.

4.5.1 Baseline Results

First-Stage Effects. We start with the first-stage effects of foreign capital liberalization on foreign equity inflows. The inflow of foreign equities into India has been increasing over time. But, industries that have been liberalized experienced a sudden increase in foreign capital. As shown in Figure 4.9, foreign equities in industries that liberalized in 2001 and 2006 increased visibly faster than in industries that never liberalized. For both the 2001 and 2006 reform industries, the dynamic pattern shows that transmission of the policy to the observed foreign capital is not immediate and takes some time. For example, foreign equities in 2006 reform industries presented a noticeable jump in 2009. Despite the lag, we can see the first-stage effect, i.e., the foreign capital integration strongly increases foreign equities.

Average Effects. To examine the labor market consequences of foreign capital integration, we focus on effects on employment, wage bill, wage per worker, MRPL, and markdown. Table 4.5 presents the baseline results on the average effects of liberalization on these outcomes. We fail to find strong average effects, except for the weakly positive wage effects (column 3). The coefficient estimate on employment is surprisingly negative, but this average employment effect is not statistically significant. The average impact on the wage bill is positive, while the coefficients on MRPL and markdown are negative. But these estimates are essentially zero.

4.5.2 Heterogeneous Effects

Ex-ante MRPL. Table 4.6 presents the estimates of the differential effects of the policy by the pre-reform level of labor misallocation using our main specification in equation (4.5). In response to the liberalization, employment increases by 22% at high MRPL firms compared to low MRPL firms (column 1). Consistent with [41], high MRPL firms also experience a relative increase in their wage bills by 20% (column 2). Owing to our firm-level data from the ASI, which reports the number of workers, we calculate the wage per worker and estimate the wage effect, which is one of the innovations of this paper. Our finding on the wage effect reveals that the wage bill effect is explained by the employment effect, as there is no wage impact from foreign capital liberalization (column 3).

For firms with high MRPL before the reform, MRPL decreases by 11% relative to firms with low initial MRPL (column 4). This misallocation-correcting effect appears to be driven by a 13% decline in labor market power, measured by wage markdowns, for high MRPL firms compared to low MRPL ones (column 5). This is consistent with the wage effect of the policy, i.e., from equation (4.3)

$$\underbrace{\%\Delta\mathrm{MRPL}_{it}}_{\text{labor misallocation effect}} = \underbrace{\%\Delta\eta_{it}}_{\text{markdown effect}} + \underbrace{\%\Delta W_{it}}_{\text{wage effect}},$$

where $\%\Delta$ indicates proportional changes. The underlying cause of declines in markdowns and labor misallocation is labor reallocation from low MRPL firms to high MRPL firms identified by the positive employment effect. More financial resources allow high MRPL firms to expand on hiring, which yields reductions in labor market power and labor misallocation within treated industries.

We estimate the heterogeneous effects by ex-ante MRPL using the event-study specification in equation (4.6) to show the dynamic effects and formally test the parallel pre-trends assumption. Figure 4.10 illustrates the impacts for high MRPL firms relative to low MRPL ones. The reference year denoted by "-1" is the year before the 2006 reform, the 2005-2006 fiscal year in our baseline specification. We find no effect on all labor market outcomes of our interest in the treated industries except for a few years before the program. Therefore, we consider that our estimated effects are causal and valid because no pre-trends assumption is plausible for most pre-treatment periods. The positive effects on employment and wage bills at high ex-ante MRPL are different from zero and persistent over time. Although slightly less precisely estimated, the dynamic impacts on MRPL and markdowns for high MRPL firms are negative, particularly in the medium and the long term. For high MRPL firms, the liberalization has null effects in general, with some negative impacts in the medium and long term.

To further examine the identification strategy, Figure 4.11 shows the relative effects for both high and low MRPL firms. The liberalization has no or negligible, if not null, effects on the five outcomes for most of the post-treatment periods.

Ex-ante Exposure to Foreign Equities. In Table 4.7, we analyze which firms the foreign equities flow into by estimating foreign equity regressions across industries with different initial exposure to foreign equities. The foreign equities flow into industries with high ex-ante foreign equity exposure (column 1). However, among treated industries, those with low exposure received foreign equities, and industries with high exposure experienced outflow of foreign equities (column 2).

Then, we estimate the labor market effects of the reform, heterogeneous by foreign equity exposure. Table 4.8 reports the results. The number of observations was reduced by more than 50% due to a lack of information on the amount of foreign equities in Prowess data. This could lead to less precisely estimated effects, and indeed, the estimates are noisier for employment and wage bills. For firms with low initial exposure to foreign capital, employment and wage bills increase by 7% and 11%, respectively, relative to firms with high initial exposure to foreign capital (columns 1–2). However, these effects are not statistically significant. The impact on wages is also essentially zero (column 3). The FDI inflows relatively reduce MRPL and markdown by 46% and 44%, respectively, for firms with low foreign capital exposure compared to those with high exposure (columns 4–5).

4.5.3 Worker Heterogeneity

We consider a worker heterogeneity in gender–male and female workers. We estimate the specification in equation (4.4), but the regressions are estimated on a reduced sample in which gender-specific markdowns have been estimated. The sample is approximately one-fourth of our baseline sample. As shown in Table 4.9, average effects are essentially zero for male and female workers.

The differential effects by ex-ante MRPL are, however, gender-specific and are mainly concentrated among male workers (Table 4.10). The firms growing in size due to the inflows of foreign equities demand men more than women. The effects on the five outcomes for male workers (Panel A) are all in the same direction as those in the heterogeneous effects in Table 4.6. However, the wage effect of the policy is significant for men at the 1% level (Panel A, column 3), and the MRPL effects are no longer statistically different from zero (Panel A, column 4). For female workers, the employment and wage bill effects are essentially zero, while the impacts on MRPL and markdowns are negative and statistically significant at least at the 5% level.

4.6 Robustness

Our focus is to check the robustness of our main findings on the effects of the reform heterogeneous by ex-ante labor misallocation. We conduct the robustness checks using (i) firm splits based on markdowns, which is a potential source of labor misallocation, instead of misallocation or MRPL (ii) an alternative control group, (iii) an alternative reference period in the event study analysis, (iv) 2001 as an ex-ante period, and (v) residualized ex-ante MRPL.

Ex-ante Markdowns. As a potential source of labor misallocation, we conduct the heterogeneity analysis by ex-ante level of labor market power. Table 4.11 reports the results from estimating a specification similar to equation (4.5) but with $I_i^{High\ Markdown}$ instead of the $I_i^{High\ MRPL}$. The qualitative effects on the five outcomes are remarkably the same. The magnitude of the estimated coefficients is generally comparable, and the statistical significance slightly worsens compared to the heterogeneous effects by ex-ante MRPL. Among employers with high market power in the labor markets, employment and wage bills increase by 20% and 24%, respectively, and MRPL and markdowns reduce by 8% and 10%, respectively, relative to employers with low initial market power. The wage effect is positive but not statistically significant, similar to the heterogeneity by labor misallocation.

Alternative Control Group. In the baseline analysis, our treatment variable is based on a treatment group with industries liberalized during the 2006 reform and a control group with never-treated industries. Here, we check the robustness of our main results by including the 1991 reform industries in the control group like [41] and keeping the treatment group the same as the baseline. Table 4.12 reports the estimates of the heterogeneous effects of the policy from equation (4.5) using the alternative treatment variable. As we add firms operating in 1991 reform industries in the control group, the observations increased by 46% from 23,901 to 34,793. The results qualitatively remain the same. Table C.2 presents the results for male and female workers, and our baseline results for heterogeneous workers are substantially robust to the alternative control group.

Alternative Reference Period. To examine the robustness of our event study results and test an identification assumption of no anticipation effect, we estimate the specification in equation (4.6) by replacing the reference year of 2005-2006 with the 2004-2005 fiscal year. Figure 4.12 presents the results. Moving the base period to a year earlier does not change the qualitative findings. The immediate impact in the first period after the base year (or period "0") is statistically insignificant at the 5% level for the five outcomes, except for employment and wage bills. The coefficient estimates on employment and wage bills are marginally significant at period "0" in the event study graphs, indicating potential anticipation effects on these outcomes. However, overall, we consider that there is no substantial anticipation effect.

MRPL in 2001 as Ex-ante MRPL. The baseline analysis uses the average MRPL between 2000 and 2001 as the ex-ante MRPL to estimate the heterogeneous effects by MRPL. The baseline regressions estimating heterogeneous effects by industry's ex-ante foreign equity exposure are based on the share of foreign equities in 2001, which is the first period available in Prowess that reports firms' equity composition. To make these sets of regressions consistent, we use the 2001 value of MRPL to estimate the heterogeneous effects by ex-ante MPRL. As shown in Table 4.13, the results are remarkably robust.

Residualized Ex-ante MRPL. We finally check the robustness of heterogeneous effects by ex-ante MRPL using an alternative measure of ex-ante MRPL, which is the baseline measure residualized with selected controls. Since the treatment is at the 4-digit level and ex-ante MRPL is defined as the average MRPL over 2000-2001, we residualize the baseline measure using 2-digit industry fixed effects. Table 4.14 presents the results, and the baseline findings remain unchanged.

4.7 Conclusion

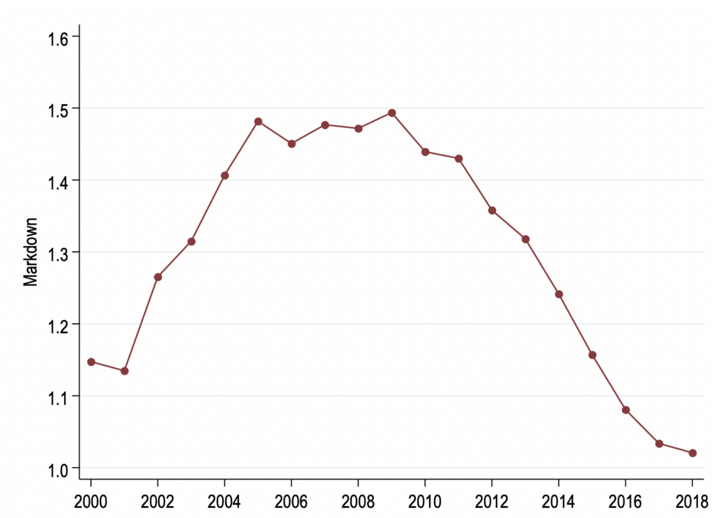
This paper examines the impacts of foreign capital integration on labor markets, focusing on labor misallocation across firms and imperfect competition in the labor markets. In doing so, we use the industry-time variation from India's 2006 foreign capital liberalization episode. The policy allowed some industries to receive automatic approval for foreign equity investments and raised caps on foreign equity. We introduce three innovations to the literature. First, we quantify wage and employment effects, which enables us to disentangle the wage bill effect. Second, we investigate worker heterogeneity and highlight the gender dimension. Third, we examine the potential mechanisms through which foreign capital liberalization affects the misallocation of labor and provide direct evidence on the impacts on the labor wedge.

Our analysis shows that the resulting labor reallocation across firms reduced labor misallocation in treated industries by weakening the firm's labor market power—measured by labor markdowns using the production function approach. For firms with high ex-ante MRPL and markdowns, the inflow of foreign equity increased employment and wage bills relative to low MRPL and low markdown firms. However, the policy has no impact on wages, indicating that the wage bill effect is completely explained by the employment effect. The labor market power and misallocation of labor relatively declined at firms with high MRPL and markdowns. The analysis with heterogeneous workers suggests that the labor market and misallocation effects are highly heterogeneous across male and female workers.

4.8 Figures and Tables

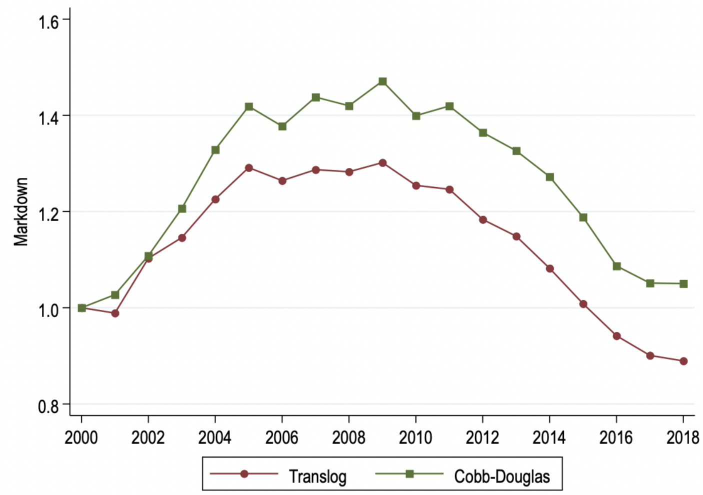
4.8.1 Figures

Figure 4.1: Trend of the Aggregate Markdown



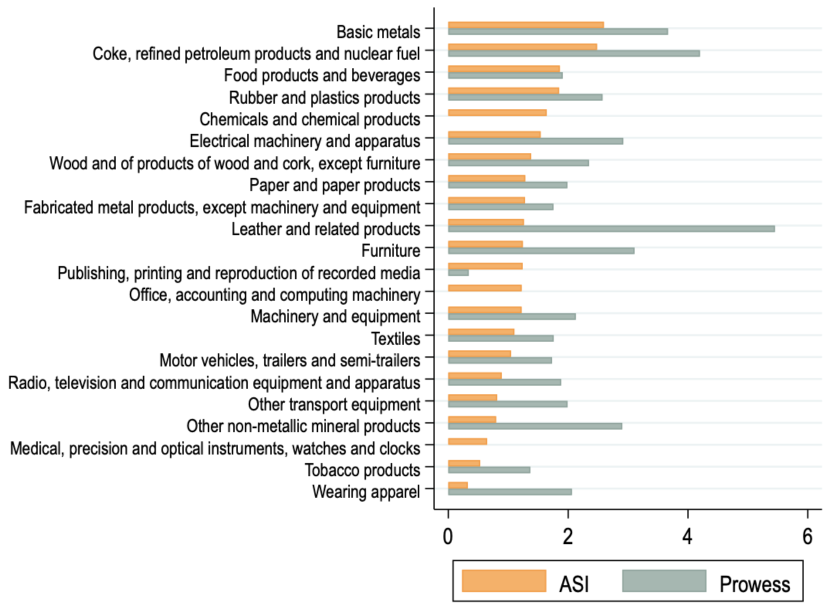
Notes: The establishment-level markdowns are constructed using the ASI data from 2000-2018 under the assumption of translog production where labor inputs are measured by headcount. The establishment-level markdowns are aggregated at the year level using employment shares of the labor market (combination of 4-digit NIC-1998 industry and states).

Figure 4.2: Trend of the Aggregate Markdown using ASI Data under Translog and Cobb-Douglas Specifications



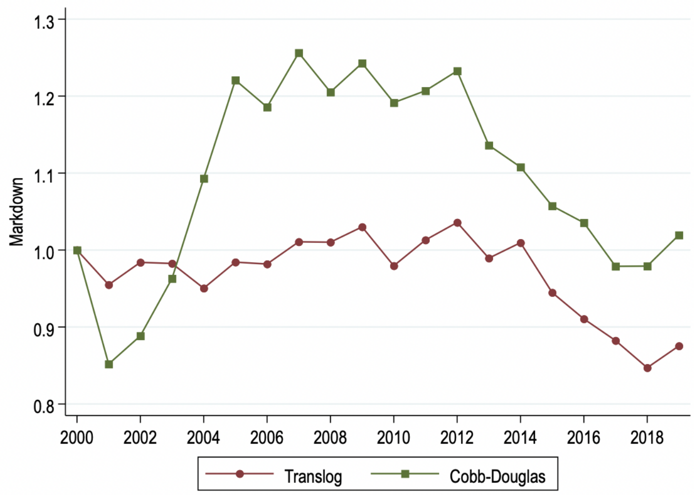
Notes: The establishment-level markdowns are constructed using the ASI data from 2000-2018 under the assumption of translog and Cobb-Douglas production where labor inputs are measured by headcount. The establishment-level markdowns are aggregated at the year level using employment shares of the labor market (combination of 4-digit NIC-1998 industry and states).

Figure 4.3: Correlation between Median Wage Markdowns Estimated using ASI and Prowess Data



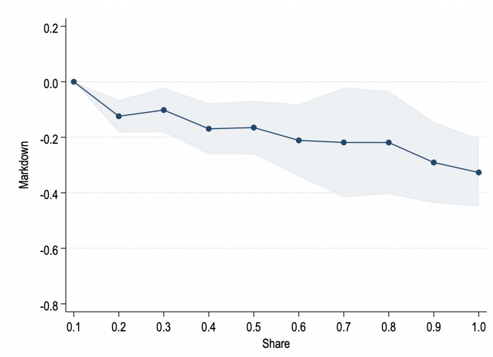
Notes: The figure plots the median wage markdowns at the two-digit NIC-1998 industry level where the establishment-level markdowns are constructed using the ASI data from 2000-2018 (orange) and Prowess data from 2000-2019 (teal) under the assumption of translog production function. The median estimate is calculated using sampling weights provided in the data for the ASI data, while no weights are provided in Prowess data.

Figure 4.4: Trend of the Aggregate Markdown using Prowess Data under Translog and Cobb-Douglas Specifications

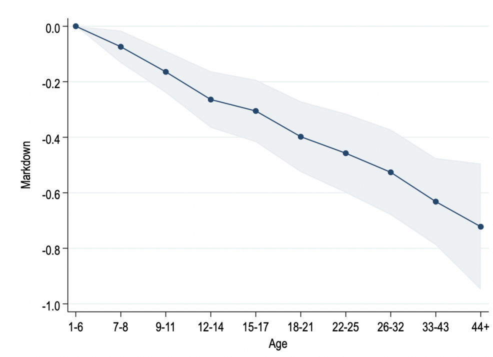


Notes: The figure depicts the aggregate markdowns (normalized to their value by 2000) from 2000-2019. The establishment-level markdowns are constructed using Prowess data from 1995-2019 under the assumption of translog and Cobb-Douglas production, where labor inputs are measured by real labor cost. The establishment-level markdowns are aggregated at the year level using employment shares of the labor market (combination of 2-digit NIC-1998 industry and districts).

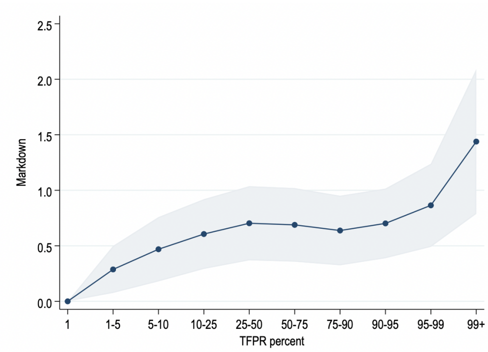
Figure 4.5: Relationship between Markdown and Firm Characteristics (ASI Data)



(a) Markdown-Size Relationship



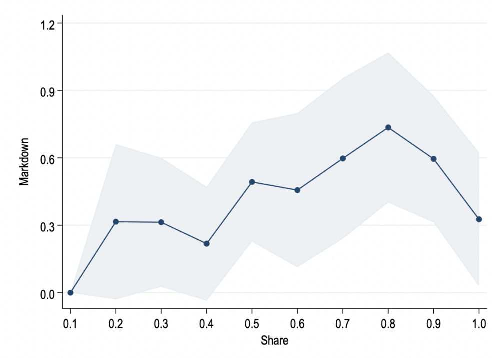
(b) Markdown-Age Relationship



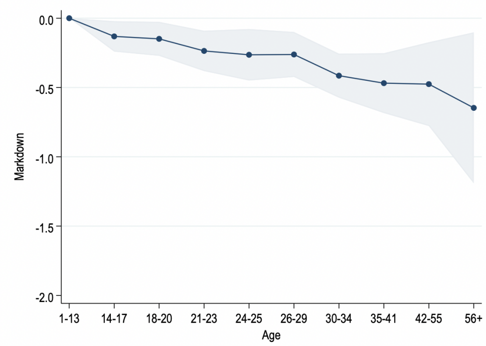
(c) Markdown-TFPR Relationship

Notes: Based on the ASI data from 2000-2018, 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 establishment-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 firm age and 4-digit industry, state, 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 establishments 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 establishment-level markdowns on indicators of age deciles. The regression controls for indicators for firm size and 4-digit industry, state, 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 establishment-level markdowns on productivity. The regression controls for 4-digit industry, state, 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 (34 clusters).

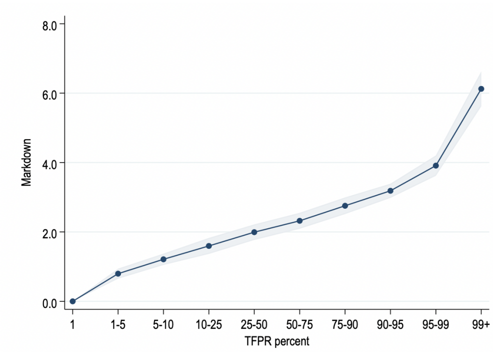
Figure 4.6: Relationship between Markdown and Firm Characteristics (Prowess Data)



(a) Markdown-Size Relationship



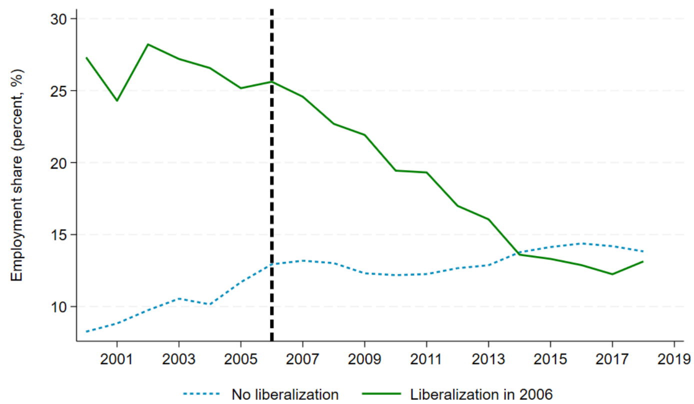
(b) Markdown-Age Relationship



(c) Markdown-TFPR Relationship

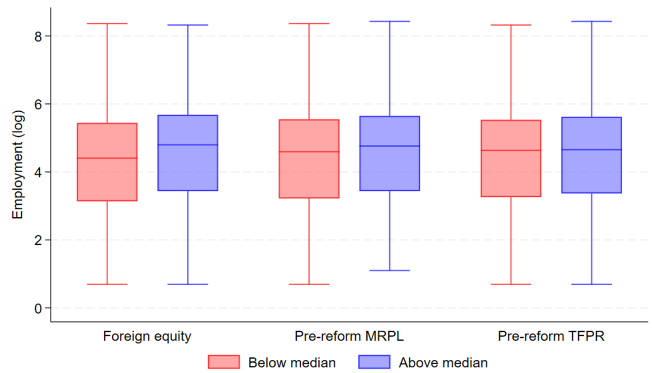
Notes: Based on Prowess data from 1995-2019. Panel (a) illustrates the point estimates and 95% confidence intervals from estimating firm-level markdowns on size (measured by employment share) indicators. In the production function estimated separately for each two-digit industry group, labor inputs are measured by real labor cost. The regression controls for indicators for firm age and 2-digit 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 firms 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 firm-level markdowns on indicators of age deciles. The regression controls for indicators for firm size and 2-digit 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 firm-level markdowns on productivity. The regression controls for 2-digit 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 districts. The qualitative results remain the same when the SEs are clustered at the 2-digit industry level (20 clusters), except for the markdown-size relationship, which becomes generally statistically insignificant, especially in the bottom part of the distribution.

Figure 4.7: Trend in Share of Female Workers by Treatment Status

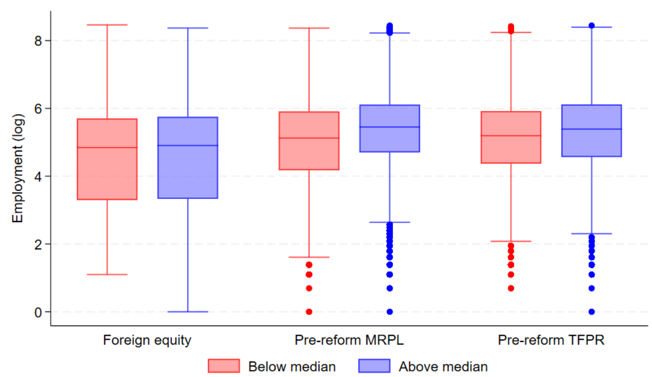


Notes: Based on ASI data. The figures plots the share of female workers at the establishment over time for industries that were liberalized in 2006 (green solid line) and for industries without liberalization (blue short dashed line).

Figure 4.8: Distribution of Firm-Level Employment by Initial Characteristic



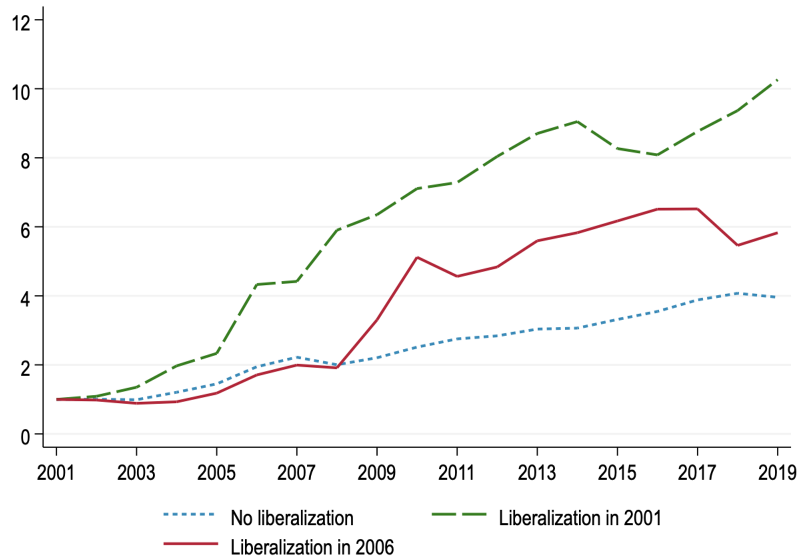
(a) Before 2006 Reform



(b) After 2006 Reform

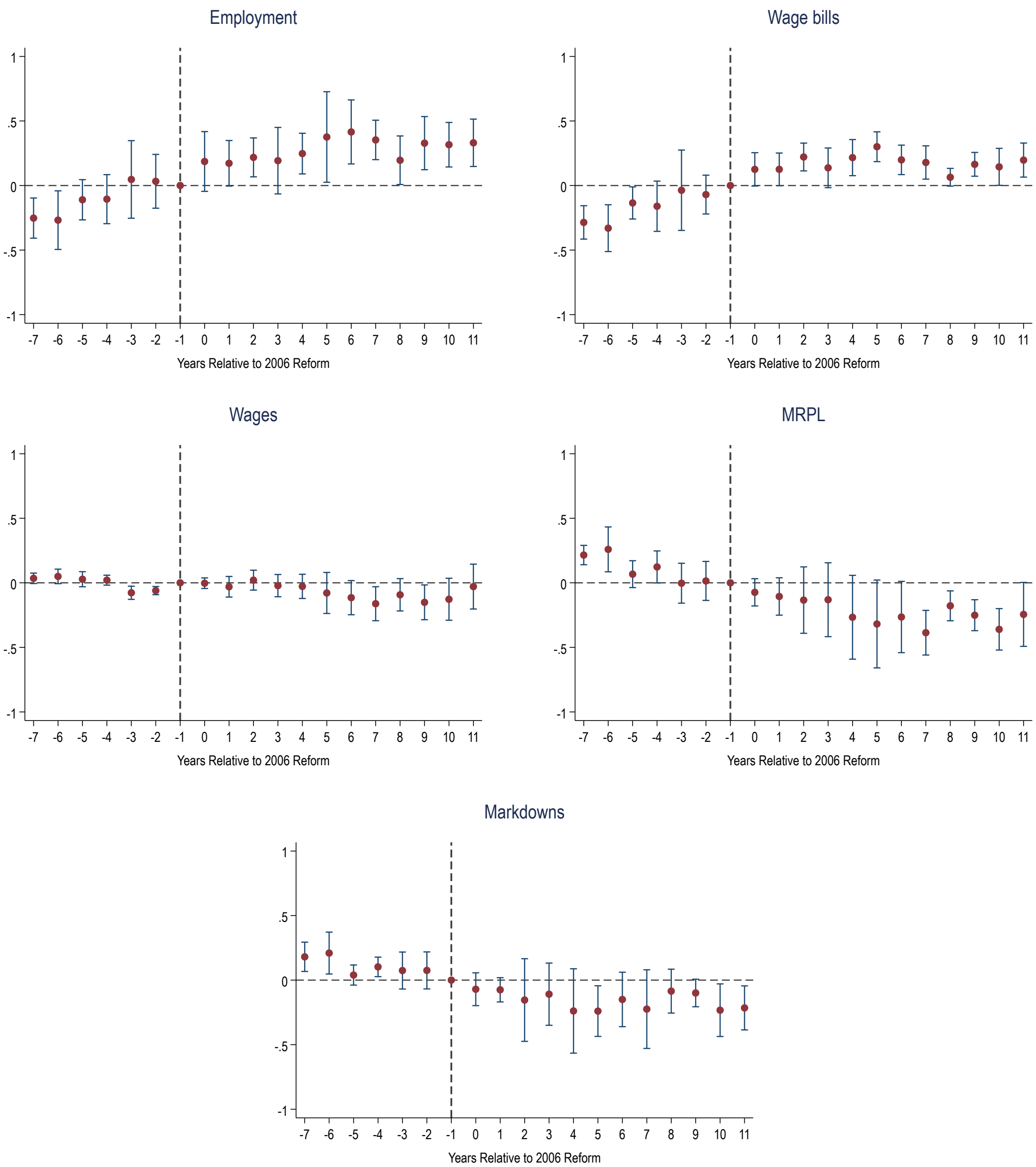
Notes: The figure plots the distribution of firm-level employment before and after the 2006 reform by initial characteristics. Based on the ASI data from 2000-2018 on which markdown has been estimated. The firms are classified based on whether they are below or above the median of initial share of foreign equity, marginal revenue product of labor (MRPL) and revenue productivity (TFPR).

Figure 4.9: Foreign Equities



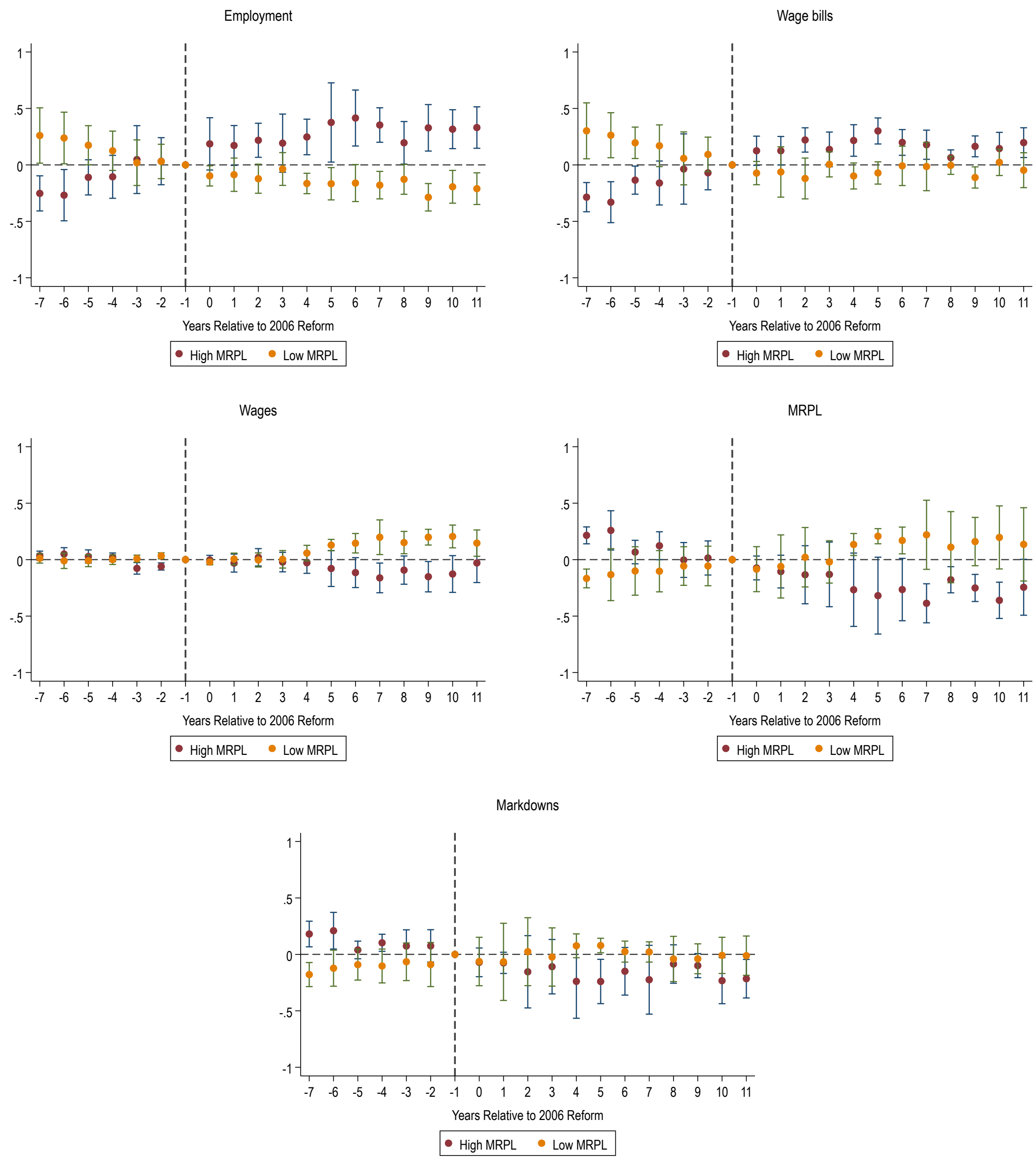
Notes: The figure plots the total amount of foreign equities in Prowess for industries that have been liberalized in 2001 (green long dashed line), in 2006 (red solid line), and for those that have not been liberalized during the period 1995-2019 (blue short dashed line). The amounts for these three groups of industries are normalized to their 2001 level.

Figure 4.10: Event Study: Relative Effects of Foreign Capital Liberalization on High MRPL Firms



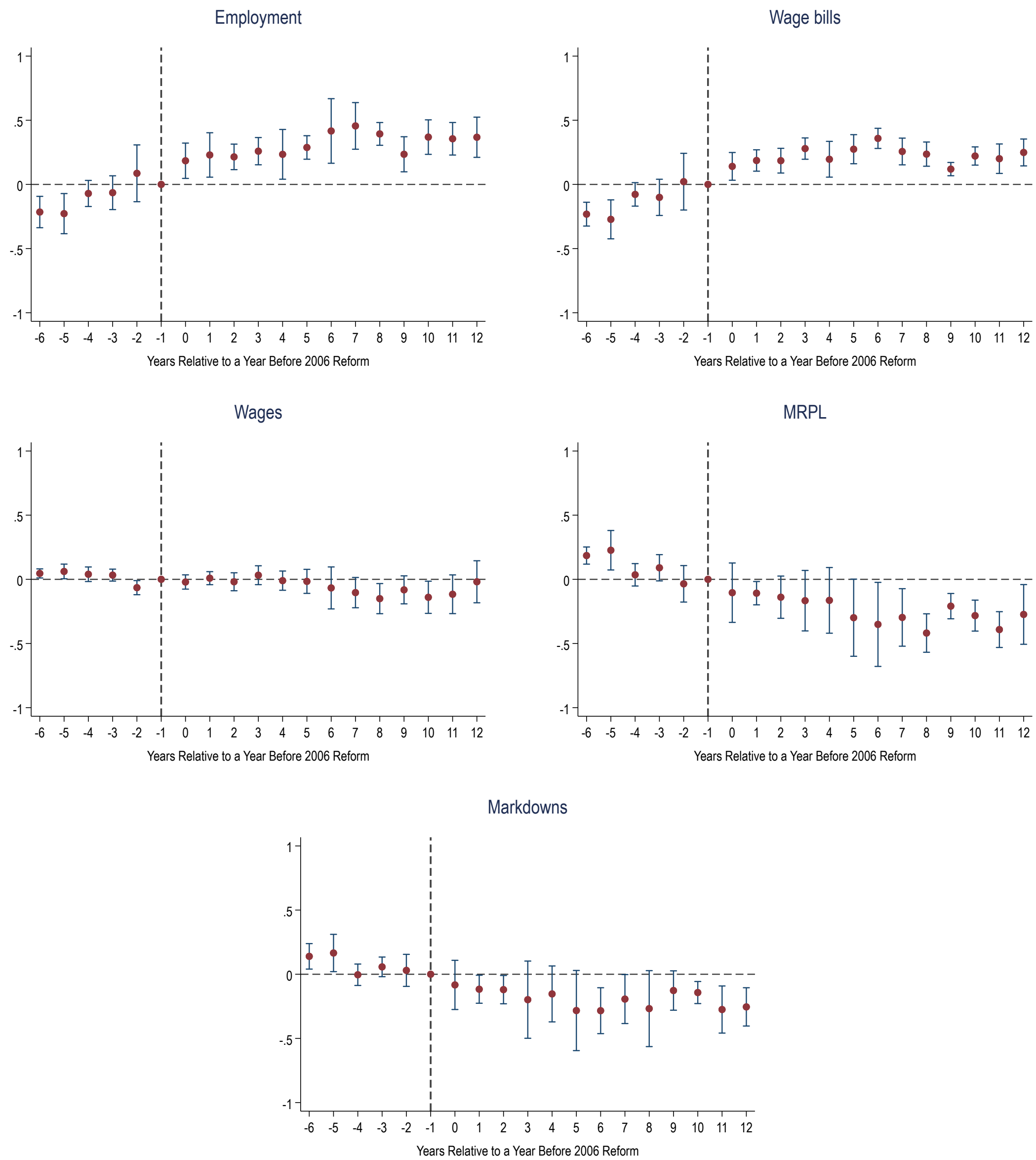
Notes: The figure plots results from event study regressions estimating the effects of India's 2006 FDI reform for high ex-ante MRPL firms relative to low MRPL firms. Each point is the estimated coefficient $\beta_{1\tau}$ in regression (4.6) of employment, wage bills, wage per worker, MRPL, and markdown regressions, where τ represents the year relative to the reform period. All outcomes are in logs, and the reform is normalized to take place in year 0, which is the 2006-2007 financial year. Standard errors are two-way clustered at the 4-digit industry and year level, and 95% confidence intervals are shown.

Figure 4.11: Event Study: Relative Effects of Foreign Capital Liberalization on High and Low MRPL Firms



Notes: The figure plots results from event study regressions estimating the effects of India's 2006 FDI reform for high and low MRPL firms. Each point is the estimated coefficient $\beta_{1\tau}$ in regression (4.6) of employment, wage bills, wage per worker, MRPL, and markdown regressions, where τ represents the year relative to the reform period. All outcomes are in logs, and the reform is normalized to take place in year 0, which is the 2006-2007 financial year. Standard errors are two-way clustered at the 4-digit industry and year level, and 95% confidence intervals are shown.

Figure 4.12: Event Study on the Relative Effects of Foreign Capital Liberalization on High MRPL Firms



Notes: The figure plots results from event study regressions estimating the effects of India's 2006 FDI reform for high ex-ante MRPL firms relative to low MRPL firms. Each point is the estimated coefficient $\beta_{1\tau}$ in regression (4.6) of employment, wage bills, wage per worker, MRPL, and markdown regressions, where τ represents the year relative to the reform period. All outcomes are in logs, and the reform is normalized to take place in year 0, which is the 2005-2006 financial year. Standard errors are two-way clustered at the 4-digit industry and year level, and 95% confidence intervals are shown.

4.8.2 Tables

Table 4.1: Estimated Establishment-Level Markdowns in India's Manufacturing using ASI Data

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Basic metals	2.598	3.124	2.800	2.021	15724
Coke, refined petroleum products and nuclear fuel	2.483	2.914	2.569	1.956	2978
Food products and beverages	1.863	2.283	1.864	1.622	44340
Rubber and plastics products	1.850	2.240	1.685	1.473	11642
Chemicals and chemical products	1.643	2.071	1.601	1.490	21023
Electrical machinery and apparatus	1.542	1.963	1.513	1.478	13021
Wood and of products of wood and cork, except furniture	1.383	1.630	1.118	1.091	5108
Paper and paper products	1.285	1.507	0.980	0.913	7833
Fabricated metal products, except machinery and equipment	1.281	1.582	1.139	1.120	13001
Leather and related products	1.262	1.701	1.414	1.419	6314
Furniture	1.245	1.634	1.232	1.340	8667
Publishing, printing and reproduction of recorded media	1.242	1.638	1.281	1.334	3491
Office, accounting and computing machinery	1.224	1.532	1.149	1.273	629
Machinery and equipment	1.223	1.580	1.269	1.233	18646
Textiles	1.103	1.450	1.150	1.224	26118
Motor vehicles, trailers and semi-trailers	1.047	1.216	0.739	0.757	11873
Radio, television and communication equipment and apparatus	0.893	1.294	1.122	1.234	3882
Other transport equipment	0.817	1.241	1.138	1.246	5894
Other non-metallic mineral products	0.798	1.105	0.897	0.973	27051
Medical, precision and optical instruments, watches and clocks	0.649	1.083	0.976	1.256	3549
Tobacco products	0.533	1.100	1.355	1.362	5071
Wearing apparel	0.327	0.582	0.475	0.757	11917
Whole sample	**1.314**	**1.750**	**1.502**	**1.474**	**267772**

Notes: Markdowns are estimated for 71,264 unique manufacturing establishments using the ASI data from 2000-2018 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 manufacturing industry group 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.

Table 4.2: Estimated Firm-Level Markdowns in India's Manufacturing using Prowess Data

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Leather and related products	5.451	5.969	5.956	3.735	328
Coke, refined petroleum products and nuclear fuel	4.200	4.417	2.372	1.926	7100
Basic metals	3.668	4.160	2.967	2.471	4248
Furniture	3.107	3.372	2.408	1.942	796
Electrical machinery and apparatus	2.920	3.235	2.117	1.718	2755
Other non-metallic mineral products	2.905	3.817	3.550	3.112	1790
Rubber and plastics products	2.574	2.575	1.100	1.000	3646
Wood and of products of wood and cork, except furniture	2.348	2.658	1.775	1.420	262
Machinery and equipment	2.128	2.188	1.042	0.840	3818
Wearing apparel	2.063	2.900	2.500	2.426	904
Other transport equipment	1.991	3.057	3.399	2.871	472
Paper and paper products	1.987	2.285	1.864	1.454	1416
Food products and beverages	1.910	2.204	1.657	1.784	4326
Radio, television and communication equipment and apparatus	1.884	2.600	1.989	2.109	1803
Textiles	1.759	2.459	2.051	2.176	4714
Fabricated metal products, except machinery and equipment	1.754	1.964	1.496	1.206	1638
Motor vehicles, trailers and semi-trailers	1.731	1.969	1.223	1.275	3087
Tobacco products	1.369	1.441	1.776	1.417	116
Publishing, printing and reproduction of recorded media	0.342	0.836	0.168	1.278	33
Whole sample	**2.522**	**3.016**	**2.312**	**2.119**	**43252**

Notes: Markdowns are estimated for 4,778 unique manufacturing establishments using Prowess data from 1995-2019 under the assumption of a translog specification for gross output. The labor inputs are measured by the real labor cost in the production function, estimated separately for each 2-digit industry group. Each manufacturing industry group corresponds to the categorization of the National Industry Classification (NIC-1998) at the 2-digit level. The markdown was not estimated for two 2-digit manufacturing industries, including (i) office, accounting, and computing machinery, and (ii) medical, precision, and optical instruments, watches, and clocks, because no firms from these industries are included in Prowess data. The markdown was not estimated for the manufacturing of pharmaceuticals, medicinal chemical and botanical products due to data limitation.

Table 4.3: Summary Statistics

	No liberalized			Liberalized		
	Mean	SD	Median	Mean	SD	Median
Firm Age	22.1	19.3	17.0	25.2	18.6	21.0
Capital stock (log)	16.2	2.3	16.4	16.0	2.3	16.0
Sales revenue (log)	18.7	2.1	18.9	18.7	2.0	18.8
Employment (log)	4.6	1.5	4.8	4.7	1.3	4.9
Wage (log)	5.4	0.7	5.4	5.3	0.9	5.4
Markdown	1.6	1.4	1.2	1.8	1.5	1.4
MRPL (log)	5.6	0.9	5.6	5.6	1.1	5.8
N	234,653			12,151		

Notes: The table presents the summary statistics for firms' characteristics by treatment (FDI liberalization) status. The sample is based on the ASI data from 2000-2018 on which wage markdown has been estimated. An observation is at the firm-year level. Capital stock is the net fixed assets. Employment is the number of workers. Wage is the compensation per worker. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdown.

Table 4.4: Correlation between Industry Pre-Reform Characteristics and Reform Status

	Liberalized = 1					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (Variance in MRPL)	0.008					
	(0.011)					
Log (Variance in MRPK)		0.008				
		(0.015)				
Log (Num. Firms)			0.001			
			(0.015)			
Log (Avg. Capital Stock)				-0.042		
				(0.031)		
Herfindahl Index					-0.099	
					(0.148)	
State-Owned Firms Share of Total Sales						-0.091
						(0.061)
N	130	130	130	130	130	129

Notes: The table presents the correlation between industry pre-reform characteristics and the liberalization status. The sample is based on the ASI data from 2000-2018 on which wage markdown has been estimated. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdown. The marginal revenue product of capital (MRPK) is the ratio between sales revenue and capital. Capital stock is the real net fixed assets.

Table 4.5: Average Effects of the Foreign Capital Liberalization

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j$	-0.036	0.009	0.050*	-0.009	-0.058
	(0.057)	(0.080)	(0.025)	(0.137)	(0.131)
N	23901	23901	23900	23900	23901
R^2	0.94	0.97	0.90	0.86	0.83

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the average effects of FDI liberalization on employment (headcount), wage bill, wage per worker, MRPL, and markdown in columns 1–5, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.6: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante MRPL

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.219***	0.195***	0.013	-0.115***	-0.128***
	(0.045)	(0.061)	(0.023)	(0.017)	(0.024)
$\text{Post}_t \times \text{Reform}_j$	-0.143**	-0.086	0.043	0.048	0.004
	(0.064)	(0.098)	(0.030)	(0.134)	(0.121)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.171***	0.147***	-0.077***	-0.215***	-0.138***
	(0.046)	(0.037)	(0.017)	(0.052)	(0.039)
N	23901	23901	23900	23900	23901
R^2	0.95	0.97	0.90	0.86	0.83

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante MRPL. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.7: Heterogeneous Effects on Foreign Equity Inflows by Firms' Ex-ante Exposure to Foreign Equity

	Dependent variable: Foreign equities	
	(1)	(2)
$\text{Post}_t \times I_k^{\text{High Exposure}}$	165.431**	88.353**
	(68.297)	(38.872)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High Exposure}}$		-185.227***
		(47.168)
$\text{Post}_t \times \text{Reform}_j$		127.993***
		(20.182)
Year FE	✓	✓
4-digit Industry FE	✓	✓
N	30828	16502
R^2	0.77	0.64

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization on foreign equities by firms' ex-ante foreign equity exposure. The ex-ante foreign equity exposure is based on the fraction of foreign equities shares in total equity shares, defined at the 4-digit industry (NIC-2004) in 2001. The foreign equity exposure is high (low) if the fraction is above (below) the within-industry median. The outcome variable is the total foreign equities at the 4-digit level in million Rupees. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.8: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante Exposure to Foreign Equity

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{Low Exposure}}$	0.069	0.114	-0.018	-0.463***	-0.444***
	(0.105)	(0.107)	(0.025)	(0.098)	(0.110)
$\text{Post}_t \times \text{Reform}_j$	-0.045	-0.034	0.018	0.085	0.066
	(0.073)	(0.086)	(0.017)	(0.068)	(0.074)
$\text{Post}_t \times I_k^{\text{Low Exposure}}$	0.069	0.086	0.011	-0.054	-0.066
	(0.066)	(0.079)	(0.033)	(0.083)	(0.083)
N	11652	11652	11652	11652	11652
R^2	0.93	0.96	0.91	0.87	0.80

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization along the distribution of industry-level ex-ante foreign equity share. The ex-ante foreign equity share is the share of foreign equities in total equities, defined at the 4-digit industry (NIC-2004) in 2001. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.9: Average Effects of the Foreign Capital Liberalization on Male and Female Workers

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
	Panel A. Male workers				
$\text{Post}_t \times \text{Reform}_j$	0.220	0.210	0.068	-0.138	-0.206
	(0.138)	(0.131)	(0.042)	(0.110)	(0.140)
N	5504	5504	5504	5504	5504
R^2	0.94	0.96	0.88	0.83	0.83
	Panel B. Female workers				
$\text{Post}_t \times \text{Reform}_j$	-0.200	-0.218	0.071	0.358	0.286
	(0.199)	(0.150)	(0.042)	(0.371)	(0.388)
N	5504	5504	5504	5504	5504
R^2	0.89	0.92	0.87	0.87	0.82

Notes: Based on ASI data from 2000-2018 on which wage markdowns over male and female workers have been estimated. The table presents the results from OLS regressions estimating the average effects of FDI liberalization on employment (headcount), wage bill, wage per worker, MRPL, and markdown for male (Panel A, columns 1–5) and female (Panel B, columns 1–5) workers. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns over male and female workers are estimated under the assumption of translog production function with heterogeneous workers in the manufacturing industry. The marginal revenue product of labor (MRPL) for male and female workers are computed by multiplying the wage per male and female worker with establishment-level markdowns over the corresponding type of workers. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.10: Heterogeneous Effects of the Foreign Capital Liberalization on Male and Female Workers by Firms' Ex-ante MRPL

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
	Panel A. Male workers				
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.600***	0.614***	0.171***	-0.029	-0.200**
	(0.140)	(0.191)	(0.041)	(0.043)	(0.077)
$\text{Post}_t \times \text{Reform}_j$	-0.140	-0.157	-0.031	-0.113	-0.082
	(0.134)	(0.157)	(0.043)	(0.078)	(0.089)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.163**	0.148**	-0.049	-0.200***	-0.152**
	(0.063)	(0.060)	(0.034)	(0.066)	(0.060)
N	5504	5504	5504	5504	5504
R^2	0.94	0.96	0.88	0.83	0.83
	Panel B. Female workers				
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.049	0.051	-0.029	-0.818***	-0.790**
	(0.167)	(0.211)	(0.144)	(0.203)	(0.324)
$\text{Post}_t \times \text{Reform}_j$	-0.212	-0.233	0.080	0.752*	0.671
	(0.167)	(0.180)	(0.101)	(0.407)	(0.496)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.325**	0.244*	-0.137***	-0.343***	-0.206**
	(0.116)	(0.119)	(0.032)	(0.074)	(0.075)
N	5504	5504	5504	5504	5504
R^2	0.90	0.92	0.87	0.87	0.82

Notes: Based on ASI data from 2000-2018 on which wage markdowns over male and female workers have been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante MRPL. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown for male (Panel A) and female (Panel B) workers, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns over male and female workers are estimated under the assumption of translog production function with heterogeneous workers in the manufacturing industry. The marginal revenue product of labor (MRPL) for male and female workers are computed by multiplying the wage per male and female worker with establishment-level markdowns over the corresponding type of workers. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.11: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante Markdown

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High Markdown}}$	0.201***	0.236***	0.012	-0.084*	-0.096**
	(0.050)	(0.059)	(0.032)	(0.044)	(0.040)
$\text{Post}_t \times \text{Reform}_j$	-0.128*	-0.098	0.045	0.029	-0.015
	(0.064)	(0.085)	(0.033)	(0.143)	(0.134)
$\text{Post}_t \times I_k^{\text{High Markdown}}$	0.116***	0.165***	0.042*	-0.124**	-0.166***
	(0.039)	(0.050)	(0.023)	(0.043)	(0.051)
N	23878	23878	23877	23877	23878
R^2	0.95	0.97	0.90	0.86	0.83

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante markdown. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.12: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante MRPL (Alternative Control Group)

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.195$***$	0.188$***$	0.031	-0.048$**$	-0.079$***$
	(0.040)	(0.054)	(0.024)	(0.021)	(0.021)
$\text{Post}_t \times \text{Reform}_j$	-0.173$**$	-0.135	0.032	0.032	0.000
	(0.063)	(0.095)	(0.025)	(0.130)	(0.119)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.180$***$	0.140$***$	-0.089$***$	-0.259$***$	-0.170$***$
	(0.042)	(0.037)	(0.018)	(0.056)	(0.043)
N	34793	34793	34790	34790	34793
R^2	0.95	0.97	0.90	0.85	0.82

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante MRPL. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is the alternative treatment variable, a dummy indicating the 2006 FDI reforms, with never treated and 1991 reform industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.13: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante MRPL (Ex-ante Period = 2001)

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.243∗∗∗	0.184∗∗∗	-0.028	-0.197∗∗∗	-0.168∗∗∗
	(0.053)	(0.060)	(0.017)	(0.027)	(0.023)
$\text{Post}_t \times \text{Reform}_j$	-0.151∗∗	-0.067	0.068∗∗	0.085	0.017
	(0.068)	(0.097)	(0.029)	(0.136)	(0.125)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.160∗∗∗	0.152∗∗∗	-0.060∗∗	-0.154∗∗∗	-0.095∗∗∗
	(0.042)	(0.037)	(0.023)	(0.045)	(0.031)
N	21402	21402	21401	21401	21402
R^2	0.94	0.97	0.90	0.87	0.83

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante MRPL. The ex-ante MRPL is defined as the 2001 value of MRPL. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table 4.14: Heterogeneous Effects of the Foreign Capital Liberalization by Firms' Ex-ante MRPL (Residualized Ex-ante MRPL)

	Employment	Wage bill	Wage	MRPL	Markdown
	(1)	(2)	(3)	(4)	(5)
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.215$***$	0.185$***$	0.016	-0.111$***$	-0.127$***$
	(0.040)	(0.049)	(0.022)	(0.021)	(0.026)
$\text{Post}_t \times \text{Reform}_j$	-0.142$**$	-0.082	0.042	0.046	0.004
	(0.058)	(0.092)	(0.031)	(0.136)	(0.122)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.154$***$	0.139$***$	-0.074$***$	-0.209$***$	-0.135$***$
	(0.046)	(0.038)	(0.017)	(0.052)	(0.039)
N	23868	23868	23867	23867	23868
R^2	0.95	0.97	0.90	0.86	0.83

Notes: Based on ASI data from 2000-2018 on which wage markdown has been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' ex-ante MRPL. The ex-ante MRPL is residualized by 2-digit industry fixed effects. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown, respectively. All dependent variables are in logs. The treatment is our baseline treatment variable, a dummy indicating the 2006 FDI reforms, with never treated industries in the control group. The establishment-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage per worker with establishment-level markdowns. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

APPENDIX A

CHAPTER 1 OF APPENDIX

A.1 Data Appendix

This appendix first describes the four main datasets used for measuring the key variables and conducting the empirical analysis. Then I discuss the construction and approximation of some variables, including capital stock, wage, and education.

A.1.1 Establishment Data

Since the key outcome in this paper is wage markdowns measured using a production function approach, the primary dataset in this paper is the production data. The firm-level production data come from the IAB Establishment Panel (IAB-BP), which covers a large representative sample of establishments in German manufacturing. The longitudinal structure of the IAB-BP data enables me to use the control function method, which uses lagged information for identification to estimate the production function and then wage markdowns. The IAB-BP data includes comprehensive information necessary for production function estimation, such as annual revenue, number of workers or headcount[1], pur-

[1]Some studies such as [89], [224], [41], and [166] use the total wage bill as a proxy measure of labor; however, compensation of employees is less representative of physical labor inputs than labor headcounts at the firm with wage-setting power where workers are underpaid and thus the labor cost underestimates the labor inputs and introduces measurement error in markdown estimates. Although the estimated effect of automation threat on markdown will be consistent even in the presence of this measurement error in the dependent variable, which will be captured in the error term, the measurement error might erase the non-zero causal effect. Hence, it is ideal to use headcounts as labor inputs in estimating production function and markdown under the relaxation of perfectly competitive labor market assumption.

chase of intermediate materials, and investments.

A unique feature of the IAB-BP data is that it is the first data with direct information on robot use. Other studies mostly use indirect or proxy measures of robot adoption such as imports of robots and automation technologies [133, 5, 37, 51, 96], ICT investment or usage [153, 176], and investment in and costs of automation technologies [15, 48]. An exception to this unique feature of my data on automation is the Spanish administrative data, used by [156], which reports direct information on robots but only on the extensive margin. But the IAB BP survey data also provide information on the firm's robot use on the intensive margin (number of robots used by the firm), providing greater flexibility to analyze the firm's robot adoption in comparison with aggregate-level information on robots and robot exposure.

For the establishment data, I also extract the district (or *kreis*) where the plant is located from the Establishment History Panel (BHP), which contains more general information on the industry, location, and total employment for each establishment. Using the unique establishment identifier, I merge this dataset with the IAB BP and the matched data to import the district information. So, regions in this paper will be at the district level unless otherwise noted. To estimate the production function and thus quantify markdown using the production approach, I approximate the firm's capital stock, and the details on the procedure are provided below.

The IAB establishment panel survey began in 1993 with only West German plants included, and plants from East Germany have been covered since 1996 (https://iab.de/en/the-iab/surveys/the-iab-establishment-panel/). Therefore, I consider the sample of firm-level data spanning the periods 1996-

2018 to construct the nationally representative estimate of markdowns and estimate the impact automation threats on labor market power in Germany.

A.1.2 Matched Employer-Employee Data

I use the longitudinal version of matched employer-employee data (LIAB) mainly to construct the control variables, normalize the shock variables, and perform analysis with heterogeneous workers. The LIAB records employment trajectories for each employee who worked at one of the plants in the establishment sample for at least one day over the period. The worker's information in the matched data contains the employment history of each employee with social security records. Specifically, I use data from the Employee History (Beschäftigtenhistorik—BeH). The information on employees includes variables such as daily wage[2] and detailed occupation classifications at the 5-digit level from 1975 to 2019.

The establishment codes in the LIAB match those in the IAB-BP. Thus, for example, I calculate shares of female and foreign workers in the establishment using the LIAB data and merge it with the IAB-BP data to construct the demographic controls included in the regressions. For my analysis allowing for heterogeneous workers, I allocate the plant's total labor cost recorded in the IAB-BP data to workers performing different tasks using the share of each worker's annual earnings in the establishment recorded in the LIAB data. A worker's annual earnings is a multiplication of imputed daily wage and the number of

[2]Following the literature, I impute workers' top-coded wage information and educational attainment recorded in the German administrative data. I provide details on these imputation procedures below.

days worked in a given year.[3]

A.1.3 Worker-Level Job Tasks

In this paper, I highlight worker heterogeneity based on the risks of displacement from labor-saving automation technologies, including differences in tasks performed by the worker and the worker's skill level measured by educational attainment. I focus on worker heterogeneity by task differences since recent technological change is more biased towards routine tasks. I use three waves of worker-level representative cross-sectional data from the Federal Institute for Vocational Training and Training (BIBB)–so-called "BIBB/BAuA Employment Surveys (2006, 2012, and 2018)"–for my analysis in which workers differ by their job tasks performed at their workplaces. This data contains information on occupational skill requirements or qualifications and working conditions in Germany for around 20,000 individuals in the active labor force. Although there are existing task intensity measures for occupations in other countries like the U.S. [26] and the U.S. and Germany are similarly advanced countries, I used this worker-level data from Germany to accurately measure task contents for occupations in the German context because occupational task contents can differ across different countries [72].

Using the BIBB/BAuA Employment Surveys, I categorize activities that employees perform at the workplace into routine, nonroutine manual, and nonroutine cognitive tasks to group workers into categories that differ by their exposure to automation technologies. The BIBB Employment Survey has been col-

[3] The LIAB records parallel episodes if an individual simultaneously does multiple jobs. I restrict the data to the highest-paying job of an employee as the main episode following the literature, e.g., [86].

lected every 6-7 years since 1979, but I use three waves that employ the German Classification of Occupations 2010 (KldB 2010). The earlier surveys–so-called "BIBB/IAB Employment Surveys (1985-1986, 1991-1992, and 1998-1999)" employ the KldB 1998. Using the KldB 2010 occupation classifications, I merge the task intensity measures aggregated at the 3-digit occupation level in the BIBB/BAuA surveys to the LIAB data by occupation. For years before 2006, I used the fixed task intensity measure for 2006.

A.1.4 Industry-Level Robots Stock

The main limitation of information on the firm's robot adoption in the IAB-BP dataset is that a retrospective question was asked only once in 2019 about the firm's use of robots over the five years preceding the survey year from 2014 to 2018. It provides relatively restrictive periods. So, I use industry-year panel data on the stock of industrial robots in 50 countries, including Germany, reported by the International Federation of Robotics (IFR) since 1993 as the primary measure of automation that spans for more periods. [119, 120] introduced the use of IFR's robots stock data, which have been later used by [8] for the U.S. and by [86] for Germany. The data come from annual surveys of robot suppliers and cover 90% of the world. The robot stock is disaggregated for 20 manufacturing industries.[4] I predict the local labor market exposure to robots based on the industry-level

[4]Following [119, 120] and [86], I drop the IFR industries: all other manufacturing, all other non-manufacturing, and unspecified. It does not significantly affect the representativeness of the data as these three groups of industries only account for 5% of the total stock of robots in Germany. I also ignore agriculture, mining, electricity/gas/water supply, construction, and education to be consistent with my markdown estimation, performed for only manufacturing plants. The establishments in non-manufacturing industrial sectors reported in the IAB-BP data are too few. Thus, the estimated markdowns are noisy. I exclude non-industrial sectors in the markdown estimation and in this paper mainly because information on production prices is not available for those industries. So, I cannot deflate sales revenue, capital, and intermediate materials for non-industrial plants.

robots stock using employment weights, and the annual change in the number of robots is normalized by workforce size. In doing these, I use employment counts from the BEH recorded in the matched employer-employee data (LIAB). Section 2.4.1 discusses the construction of our primary measure of local labor market exposure to robots in more detail, particularly in equation (2.5).

A.1.5 Construction and Approximation of Some Key Variables

I first describes how I approximate the capital stock in the IAB Establishment Panel. I then explain how I impute education records and top-coded wage information in the worker-level German administrative data.

Capital Stock Approximation. I use a *perpetual inventory* method following [181, 182] to compute the stock of capital, one of the key ingredients in the production function estimation. One of the key inputs in using the perpetual inventory approach is industry-specific average economic lives of capital goods, an inverse of depreciation rate, which is obtained from [182] at the time-consistent 2-digit industry level for the periods 1993-2014. I merge this information with EP data at the 2-digit industry level, which I generate from the 3-digit industry classification provided in the EP data.[5] Given that the economic lives information is provided up to 2014 while my analysis spans until 2018, I extrapolate economic lives for four years between 2014-2018 by (i) keeping it constant and the 2014 level and (ii) using 3-year moving average.[6] Another issue with ap-

[5] Federal employment agency reports the time-consistent classification of economic activities at different aggregation levels (https://statistik.arbeitsagentur.de/DE/Navigation/Grundlagen/Klassifikationen/Klassifikation-der-Wirtschaftszweige/Klassifikation-der-Wirtschaftszweige-2008/Klassifikation-der-Wirtschaftszweige-2008-Nav.html).

[6] Since the average economic lives have been substantially stable over the years from 1993

proximating capital stock is the starting value of the capital stock.

Also [181] proposes two approaches to compute the time series of capital stock using either the average replacement investments over the whole sample period (KT) or the first three years (K3) for each firm. I define these two types of capital stock series, following the procedure, and which version of capital stock to use depends on the analysis. The latter performs better than the former when the capital stock has a time trend, as it uses the short-term average as a starting point. However, due to noisy investment data, the capital stock generated in this way, K3, is likely to be misleading. However, the perpetual inventory routine slowly corrects the K3. So, K3 might be less appropriate when using between-firm information and OLS regression. However, it might be more suitable for estimators that use only within-firm information using the GMM method. Since the ACF method of production function estimation uses GMM to estimate production function parameters, I primarily use the capital stock K3 in my analysis despite fewer observations than KT.[7]

Imputation of Wages. I observe the nominal daily wage of each worker registered for social security purposes at the firm. Since the wage data comes from social security records, it is generally highly reliable. However, a common challenge of the wage data from the Social Security notifications is that the wage information is recorded only until the Social Security contribution assessment ceiling. If a worker's wage exceeds this upper earnings limit, this value will be entered as her wage, which differs by year and location.[8] Although only about

to 2014 with small variance, an extrapolation for four years is not expected to affect the results in any economically meaningful way. Also, there is no record of any events that might have changed the dynamic pattern of the average economic lives of capital goods. The results from production function estimation using these two different capital stocks are extremely similar.

[7] I also use KT in my production function estimation as a robustness check and find that estimates on production function parameters remain the same.

[8] The nominal wages and the assessment ceilings are deflated by the consumer price index

5% of the observations are subject to this top-coding procedure, this censorship affects some groups of workers, e.g., high-skilled male workers above certain ages in regular full-time employment. To address this censoring problem, I use a two-step imputation procedure proposed by [100], widely employed in the literature, e.g., by [70]. First, I run a series of Tobit wage regressions—fit separately by year, East and West Germany, and three educational groups—on worker characteristics, including gender, age range, and tenure.

Imputation of Educational Attainments. I use the information on workers' educational attainment to impute the right-censored wages. However, the highest level of workers' educational attainment in the German administrative data is inconsistent over time. For example, the educational attainment of an individual with a university degree is recorded as an apprenticeship even if the individual has a university degree but did an apprenticeship later on. Following [106], I correct such inconsistent developments in educational attainment.

A.2 Robustness of the Relationship between Actual Robot Adoption and Robot Exposure Shock

To check the robustness of the relationship between actual robot adoption and robot exposure documented in Section 2.4.5, this appendix first examines the relationship between actual robot adoption and robot exposure shock in the automotive industry (Table A.1).

from the Federal Statistical Office to calculate the real wages.

Table A.1: Relationship between Actual Robot Adoption and Robot Exposure Shock in Automotive Industry

	Dependent variable: Actual robot adoption		
	(1)	(2)	(3)
	Panel A. Robots in automotive per 1000 workers		
External exposure to robots in automotive	0.214	1.079	1.138
	(0.170)	(1.712)	(1.648)
N	1671	1667	1657
R^2	0.03	0.44	0.47
	Panel B. ΔRobots in automotive per 1000 workers		
ΔExternal exposure to robots in automotive	-0.030	-0.141	-0.257
	(0.075)	(0.092)	(0.205)
N	1330	1323	1315
R^2	0.03	0.39	0.43
Year fixed effects	✓	✓	
State fixed effects	✓		
District fixed effects		✓	✓
State-by-Year fixed effects			✓

Notes: The table presents the results from OLS regressions estimating the relationship between actual robot adoption in Germany and average robot exposure in the automotive industry in other high-income European countries at the local labor market region level. The sample at the level in panel A covers periods between 2014 and 2018, while the sample in panel B for annual changes covers 2015-2018. The actual robot adoption is measured by aggregating the number of robots adopted by the firm in the automotive industry at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. The robot exposure shock into the local labor market regions or districts is measured by the robots stock in the automotive industry in six other European countries (Spain, France, Italy, Norway, Sweden, and UK) "predicted" to the district using employment shares and expressed as per 1,000 workers. The actual robot adoption and robot exposure are normalized by the number of workers in the previous period. In panel A, the relationship was estimated at the level, while panel B shows the relationship between the annual changes. Standard errors clustered by districts are in parentheses.

Second, I estimate the relationship between actual robot adoption and robot exposure shock for all industries and automobile industries separately for East and West Germany (Tables A.2 and A.3).

Table A.2: Relationship between Actual Robot Adoption and Robot Exposure Shock in East Germany

	Dependent variable: Actual robot adoption					
	All industries			Automobile industry		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Robots per 1000 workers					
Robot exposure shock	0.213	0.492	0.373	2.302	4.253	4.605
	(0.087)	(0.844)	(0.867)	(3.448)	(6.332)	(6.315)
N	356	356	356	356	356	356
R^2	0.14	0.49	0.53	0.05	0.51	0.54
	Panel B. ΔRobots per 1000 workers					
ΔRobot exposure shock	0.209	1.166	1.130	6.724	2.904	7.905
	(0.858)	(1.737)	(1.720)	(7.407)	(3.614)	(5.873)
N	283	282	282	283	282	282
R^2	0.05	0.32	0.34	0.04	0.34	0.39
Year fixed effects	✓	✓		✓	✓	
State fixed effects	✓			✓		
District fixed effects		✓	✓		✓	✓
State-by-Year fixed effects			✓			✓

Notes: The table presents the results from OLS regressions estimating the relationship between actual robot adoption and average robot exposure in other high-income European countries at the local labor market region level in East Germany for industrial robots in all industries (left sub-panel) and automobile industry (right sub-panel). The sample at the level in panel A covers periods between 2014 and 2018, while the sample in panel B for annual changes covers 2015-2018. The actual robot adoption is measured by aggregating the number of robots adopted by the firm at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. The robot exposure shock into the local labor market regions or districts is measured by the robots stock at the industry level in six other European countries (Spain, France, Italy, Norway, Sweden, and UK) "predicted" to districts using employment shares and expressed as per 1,000 workers. The actual robot adoption and robot exposure shock are normalized by the number of workers in the previous period. The relationship in panel A was estimated at the level, while panel B shows the relationship between the annual changes. Standard errors clustered by districts are in parentheses.

Table A.3: Relationship between Actual Robot Adoption and Robot Exposure Shock in West Germany

	Dependent variable: Actual robot adoption					
	All industries			Automobile industry		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Robots per 1000 workers					
Robot exposure shock	-0.014	-0.001	0.007	0.136	-0.221	-0.279
	(0.015)	(0.089)	(0.091)	(0.125)	(0.357)	(0.348)
N	1315	1311	1301	1315	1311	1301
R^2	0.03	0.55	0.56	0.02	0.35	0.39
	Panel B. ΔRobots per 1000 workers					
ΔRobot exposure shock	-0.249	-0.234	-0.374	-0.055	-0.154	-0.295
	(0.152)	(0.224)	(0.235)	(0.079)	(0.107)	(0.207)
N	1047	1041	1033	1047	1041	1033
R^2	0.02	0.50	0.51	0.01	0.43	0.46
Year fixed effects	✓	✓		✓	✓	
State fixed effects	✓			✓		
District fixed effects		✓	✓		✓	✓
State-by-Year fixed effects			✓			✓

Notes: The table presents the results from OLS regressions estimating the relationship between actual robot adoption and average robot exposure in other high-income European countries at the local labor market region level in West Germany for industrial robots in all industries (left sub-panel) and automobile industry (right sub-panel). The sample at the level in panel A covers periods between 2014 and 2018, while the sample in panel B for annual changes covers 2015-2018. The actual robot adoption is measured by aggregating the number of robots adopted by the firm at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. The robot exposure shock into the local labor market regions or districts is measured by the robots stock at the industry level in six other European countries (Spain, France, Italy, Norway, Sweden, and UK) "predicted" to districts using employment shares and expressed as per 1,000 workers. The actual robot adoption and robot exposure shock are normalized by the number of workers in the previous period. The relationship in panel A was estimated at the level, while panel B shows the relationship between the annual changes. Standard errors clustered by districts are in parentheses.

Third, I conduct the robustness by estimating the relationship between firm-level actual robot adoption and district-level robot exposure shock as follows:

$$\text{Actual robot adoption}_{jdt} = \alpha + \beta \text{Robot exposure shock}_{dt} + \phi_j + \mu_{kt} + \varphi_{st} + \varepsilon_{jdt}, \quad \text{(A.1)}$$

where Actual robot adoption$_{jdt}$ is the number of robots used by the firm j in district d per 1,000 workers in year t, ϕ_j is the firm fixed effects, μ_{kt} is the industry-by-year fixed effects, and all other terms are the same as those in equation (2.7). Table A.4 presents the estimation results, showing that the relationship between firm-level actual robot adoption and district-level robot exposure shock is essentially zero. It suggests that the baseline findings in Section 2.4.5 are substantially robust.

Table A.4: Relationship between Firm-Level Actual Robot Adoption and District-Level Robot Exposure Shock

	Dependent variable: Firm-level actual robot adoption			
	(1)	(2)	(3)	(4)
	Panel A. Robots per 1000 workers			
Robot exposure shock	0.047	0.065	0.068	0.114
	(0.029)	(0.152)	(0.153)	(0.195)
N	6442	6418	6418	6215
R^2	0.02	0.17	0.18	0.75
	Panel B. ΔRobots per 1000 workers			
ΔRobot exposure shock	0.034	-0.148	-0.169	-0.160
	(0.060)	(0.104)	(0.104)	(0.128)
N	5275	5256	5256	5050
R^2	0.01	0.11	0.12	0.46
	Panel C. ΔRobots per 1000 workers			
ΔRobot exposure predicted from the first-stage	0.008	-0.029	-0.028	-0.025
	(0.017)	(0.024)	(0.024)	(0.029)
N	4616	4606	4606	4433
R^2	0.01	0.10	0.10	0.46
Year fixed effects	✓	✓		
State fixed effects	✓			
District fixed effects		✓	✓	
State-by-Year fixed effects			✓	✓
Firm fixed effects				✓

Notes: The table presents the results from OLS regressions estimating the relationship between the firm-level actual robot adoption and district-level robot exposure shock. The sample at the level in panel A covers periods between 2014 and 2018, while the sample in panels B and C for annual changes covers 2015-2018. The firm-level actual robot adoption is measured by the number of robots adopted by the firm per 1,000 workers. The robot exposure shock in panels A and B is measured by the average robot stock in all industries in other high-income European countries (Spain, France, Italy, Norway, Sweden, and UK) "predicted" to districts using employment shares and expressed as per 1,000 workers. The district's exposure to robots in panel C is predicted from the first stage of the IV (2SLS) regression. The actual robot adoption and robot exposure are normalized by the number of workers in the previous period. Standard errors clustered by districts are in parentheses.

A.3 Production Function Estimation

I bring the data to the following production function to estimate parameters $\boldsymbol{\beta}$:

$$y_{jt} = f(\mathbf{x}_{jt}; \boldsymbol{\beta}) + \omega_{jt} + \varepsilon_{jt}, \tag{A.2}$$

where y_{jt} is log output, $\mathbf{x}_{jt}$ is a vector of log inputs, both fully variable inputs (e.g., intermediate materials m_{jt}) and not fully variable inputs (e.g., labor l_{jt}[9] and capital k_{jt}). The firm-specific productivity ω_{jt} embeds the constant term. The error term ε_{jt} reflects measurement error in gross outputs y_{jt} defined as revenue deflated by the producer price index for industrial products at the 2-digit industry level.[10] I write the production function in general terms as I estimate the log transformation of the production function $f(\cdot)$ in various functional forms (e.g., Cobb-Douglas and translog) with translog[11] as the primary specification given its flexibility.

The main challenge in estimating the firm-level production function in equation (A.2) is the classical problem of endogeneity of inputs, i.e., input demand is likely to be correlated with unobservables, particularly the firm's productivity.

[9]In this paper, I use the number of workers as a labor input, while one can approximate the labor by wage bills. For example, [166] argue that wage bills better capture heterogeneous labor inputs as they account for workers' ability differences. The use of wage bills generally addresses ability differences of workers as, for example, high-skilled labor inputs cost more, and wage bills will reflect it. However, wage bills will be a biased measure of labor input for labor markets with imperfect competition because wage bills undervalue productivity when an employer has some monopsony power to pay less to its workers than wages in competitive markets. Hence, in our setting with imperfect competition in the labor market, it is better to use the headcount of employees as a labor input.

[10]I obtained the producer price index (PPI) from the Federal Statistical Office of Germany. The PPI is only available for industrial products in the mining, agriculture, and manufacturing sectors, which is another reason I focus on the manufacturing industry in this study. I calculate the annual average PPI by averaging monthly PPIs.

[11]The output elasticities of labor and intermediate materials are calculated as $\theta^L_{jt} = \hat{\beta}_l + \hat{\beta}_{kl}k_{jt} + \hat{\beta}_{lm}m_{jt} + 2\hat{\beta}_{ll}l_{jt}$ and $\theta^M_{jt} = \hat{\beta}_m + \hat{\beta}_{km}k_{jt} + \hat{\beta}_{lm}l_{jt} + 2\hat{\beta}_{mm}m_{jt}$, respectively. Here $\hat{\beta}_l$ and $\hat{\beta}_m$ are parameter estimates on labor and intermediate materials, $\hat{\beta}_{ll}$ and $\hat{\beta}_{mm}$ are parameter estimates on quadratic terms, $\hat{\beta}_{kl}$, $\hat{\beta}_{lm}$, $\hat{\beta}_{km}$, $\hat{\beta}_{lm}$ are parameter estimates on cross terms, and l and m are, respectively, log labor and log intermediate materials.

To address this challenge and provide a consistent estimate of production function parameters, I rely on the refined control function approach proposed by [11] (ACF). The ACF method is designed for value-added production functions, and [111] suggest that we cannot accurately identify gross output production function parameters using the ACF approach without further assumptions. Hence, our data that reports the firm's revenue and purchases of intermediate materials enable me to employ the ACF approach. The identification strategy behind the control function method of ACF (also [192] and [163]) relies on the assumption that firms dynamically optimize their decisions in discrete times. The intuition behind identifying consistent estimators using control function or "proxy variable" methods can be thought through the logic of IV estimators [219, 224].

Let's separate a vector of log inputs $\mathbf{x}_{jt}$ into $\mathbf{v}_{jt}$ (log of fully flexible inputs $\mathbf{V}_{jt}$) and $\mathbf{k}_{jt}$ (log of non-fully flexible or fixed inputs $\mathbf{K}_{jt}$). Thus, the production function can be denoted as $f(\mathbf{x}_{jt};\boldsymbol{\beta}) = f(\mathbf{v}_{jt}, \mathbf{k}_{jt};\boldsymbol{\beta}) = \ln(F(\mathbf{V}_{jt}, \mathbf{K}_{jt};\boldsymbol{\beta}))$.

Recall that firm-specific productivity ω_{jt} unobserved by an econometrician but observed by the firm generates a problem of endogeneity for estimating the above production function. To address this problem, [163] suggest using the demand for intermediate materials[12] m_{jt} as a proxy for productivity, which is given by

$$m_{jt} = m_t(\omega_{jt}; \mathbf{k}_{jt}, \mathbf{c}_{jt}), \tag{A.3}$$

where $\mathbf{c}_{jt}$ denotes a vector of any additional factors that affect a firm's demand for material inputs, such as input prices.

[12]The control function approach is also called as "proxy variable" method as it uses the intermediate inputs (in cases of ACF and LP) or investment (in case of OP) as a proxy variable. Investments, i_{jt}, rather than intermediate inputs, m_{jt}, can also be used as the proxy variable in the ACF procedure; however, one would lose the ability to allow serially correlated, unobserved, firm-specific input price shocks to i_{jt} and l_{jt}. Hence, the ACF method primarily uses intermediate inputs as a proxy variable.

Under the assumption of strict monotonicity that the control function $m_t(\cdot)$ is strictly increasing in ω_{jt}[13], one can invert the equation (A.3) and express the productivity as

$$\omega_{jt} = m_t^{-1}(m_{jt}; \mathbf{k}_{jt}, \mathbf{c}_{jt}) = g_t(m_{jt}; \mathbf{k}_{jt}, \mathbf{c}_{jt}). \tag{A.4}$$

Substituting equation (A.4) into the production function in (A.2), we obtain the production as a function of only observables

$$\begin{aligned} y_{jt} &= f(\mathbf{v}_{jt}, \mathbf{k}_{jt}; \boldsymbol{\beta}) + g_t(m_{jt}; \mathbf{k}_{jt}, \mathbf{c}_{jt}) + \varepsilon_{jt} \\ &= \Phi_t(\mathbf{v}_{jt}, \mathbf{k}_{jt}, \mathbf{c}_{jt}) + \varepsilon_{jt} \\ &= \phi_{jt} + \varepsilon_{jt}. \end{aligned} \tag{A.5}$$

I implement the ACF procedure to estimate the production function, which adopts a two-stage procedure where each stage uses a different moment condition. To perform the procedure, I take $\mathbf{v}_{jt} = m_{jt}$, $\mathbf{k}_{jt} = (k_{jt}, l_{jt})'$, and $\mathbf{c}_{jt}$ contains additional controls, the firm fixed effects and year fixed effects. Equation (A.5) is the first-stage estimation. The first stage is performed by OLS regression of y_{jt} on third-degree polynomial in $\tilde{\mathbf{x}}_{jt} = (k_{jt}, l_{jt}, m_{jt})'$ with interaction terms and $\mathbf{c}_{jt}$ to obtain $\hat{\phi}_{jt}$. For translog production technology, we have

$$\mathbf{x}_{jt} = (k_{jt}, l_{jt}, m_{jt}, k_{jt}l_{jt}, k_{jt}m_{jt}, l_{jt}m_{jt}, k_{jt}^2, l_{jt}^2, m_{jt}^2)'. \tag{A.6}$$

Similar to OP and LP models, the ACF model assumes that the firm's information set at t, I_{jt}, includes current and past productivity shocks $\{\omega_{j\tau}\}_{\tau=0}^{t}$ but does not include future productivity shocks $\{\omega_{j\tau}\}_{\tau=t+1}^{\infty}$. Hence, the transitory

[13]Intuitively, the strict monotonicity assumption implies that more productive firms use more intermediate materials, which is plausible. Another advantage of proxying a firm's productivity at time t with its materials purchase at period t is that intermediate inputs purchased in period t are likely to be mainly used in production at time t. Although firms can store some materials for future production, this is likely relatively small.

shocks ε_{jt} satisfy $\mathbb{E}(\varepsilon_{jt}|I_{jt}) = 0$. Under this assumption, the first-stage moment condition is

$$\mathbb{E}(\varepsilon_{jt}|I_{jt}) = \mathbb{E}[y_{jt} - \phi_{jt}|I_{jt}] = 0. \tag{A.7}$$

In the first stage of ACF, none of the parameters will be estimated, but it generates an estimate $\hat{\phi}_{jt}$ using the above moment condition. Now, we turn to the second-stage estimation. The firm productivity is assumed to evolve according to the following distribution, known to the firm,

$$p(\omega_{it+1}|I_{jt}) = p(\omega_{jt+1}|\omega_{jt}), \tag{A.8}$$

which is stochastically increasing in ω_{jt}. Using this assumption on the evolution of productivity shocks and information set above, one can decompose ω_{jt} into its conditional expectation at $t-1$ and an innovation term, i.e.,

$$\omega_{jt} = \mathbb{E}(\omega_{jt}|I_{jt-1}) + \xi_{jt} = \mathbb{E}(\omega_{jt}|\omega_{jt-1}) + \xi_{jt} = h(\omega_{jt-1}) + \xi_{jt}, \tag{A.9}$$

where $\mathbb{E}(\xi_{jt}|I_{jt-1}) = 0$. Substituting this into production function in (A.2), we get

$$\begin{aligned} y_{jt} &= f(\mathbf{x}_{jt};\boldsymbol{\beta}) + h(\omega_{jt-1}) + \xi_{jt} + \varepsilon_{jt} \\ &= f(\mathbf{x}_{jt};\boldsymbol{\beta}) + h\left[\phi_{t-1} - f(\mathbf{x}_{jt-1};\boldsymbol{\beta})\right] + \xi_{jt} + \varepsilon_{jt}, \end{aligned} \tag{A.10}$$

where the second line follows from the definition of ϕ_{t-1}.

Since $\mathbb{E}(\xi_{jt}|I_{jt-1}) = 0$ and $\mathbb{E}(\varepsilon_{jt}|I_{jt}) = 0$ (which also implies $\mathbb{E}(\varepsilon_{jt}|I_{jt-1}) = 0$), the second stage of ACF estimation procedure uses the following moment condition:

$$\begin{aligned} &\mathbb{E}(\xi_{jt} + \varepsilon_{jt}|I_{jt-1}) \\ &\quad = \mathbb{E}[y_{jt} - f(\mathbf{x}_{jt};\boldsymbol{\beta}) - h\left(\hat{\phi}_{t-1} - f(\mathbf{x}_{jt-1};\boldsymbol{\beta})\right)|I_{jt-1}] = 0, \end{aligned} \tag{A.11}$$

where ϕ_{t-1} is replaced by its estimate from the first stage. [219] pointed out that the functions ϕ_t and h can be thought of as IV estimators. Additionally, [224] discuss how the identification of the ACF estimator can be interpreted through the logic of an IV estimator. We transform conditional moments into unconditional moments for actual estimation. To illustrate the second-stage moment conditions, suppose that the productivity process is defined as

$$\omega_{jt} = s_t(\omega_{jt-1}) + \xi_{jt}. \tag{A.12}$$

Then, I approximate the productivity in the data as

$$\omega_{jt}(\boldsymbol{\beta}) = \hat{\phi}_{jt} - f(\mathbf{x}_{jt};\boldsymbol{\beta}). \tag{A.13}$$

Then, I approximate $s_t(\cdot)$ with $\mathcal{P}^{\text{th}}$-order polynomial in its arguments

$$\begin{aligned}\omega_{jt}(\boldsymbol{\beta}) &= \Omega_{jt-1}(\boldsymbol{\beta})'\rho(\boldsymbol{\beta}) + \xi_{jt} \\ &= \sum_{p=0}^{\mathcal{P}} \rho_p \omega_{jt-1}^{p}(\boldsymbol{\beta}) + \xi_{jt}.\end{aligned} \tag{A.14}$$

Thus, the innovations to productivity are constructed as a function $\boldsymbol{\beta}$ as

$$\xi_{jt} = \omega_{jt}(\boldsymbol{\beta}) - \Omega_{jt-1}(\boldsymbol{\beta})'\hat{\rho}(\boldsymbol{\beta}), \tag{A.15}$$

where $\hat{\rho}(\boldsymbol{\beta}) = (\{\hat{\rho}_p\}_{p=1}^{\mathcal{P}})'$ is obtained by regressing $\Omega_{jt-1}(\boldsymbol{\beta})$ on $\omega_{jt}(\boldsymbol{\beta})$ with OLS, and I set $\mathcal{P} = 3$ following [90] and [224].

Following [90] and [224], I define the instrument $\mathbf{z}_{jt} \in \mathbb{R}^Z$ as the vector that contains one-period lagged values of every polynomial term in $f(\mathbf{x}_{jt};\boldsymbol{\beta})$ including l_{jt} and m_{jt} but capital at the current period k_{jt}. Thus, the system of second-stage moment conditions for GMM estimation to identify $\boldsymbol{\beta} \in \mathbb{R}^Z$ is defined as

$$\mathbb{E}(\xi_{jt}(\boldsymbol{\beta})\mathbf{z}_{jt}) = \mathbf{0}_{Z\times 1}. \tag{A.16}$$

Now, I briefly discuss assumptions behind the moment conditions. First, labor input l_{jt} is assumed to be chosen at period t, $t-1$, or somewhere between the two periods at $t-b$ where $0 < b < 1$. It allows labor to have some dynamic pattern and addresses the fact that labor inputs are more flexible than capital. Given some adjustment costs and other frictions in the labor market, for example, due to labor contracts, l_{jt} is modeled to be chosen at $t-b$, not all the points between t and $t-1$. In this sense, labor is not a perfectly variable input in the ACF, which is a weaker assumption than the OP in which labor is perfectly variable. The assumption that labor is chosen after time $t-1$ implies that l_{jt} is correlated with ξ_{jt}.

Second, the capital k_{jt} is assumed to be accumulated according to the following form:

$$k_{jt} = \kappa(k_{jt-1}, i_{jt-1}), \tag{A.17}$$

where investment i_{jt-1} is chosen in period $t-1$. Thus, we assume that the firm's choice of capital at time t is predetermined in period $t-1$ with choices of k_{jt-1} and i_{jt-1}. So it is safe to assume that k_{jt} is orthogonal to $\xi_{jt} + \varepsilon_{jt}$. For other terms in the "instrument", they all take their one-period lagged values, which must be orthogonal to the current period innovations (except for capital investment) because firms cannot observe their idiosyncratic shocks in the future.

A.4 Overview of Monopsony Measures

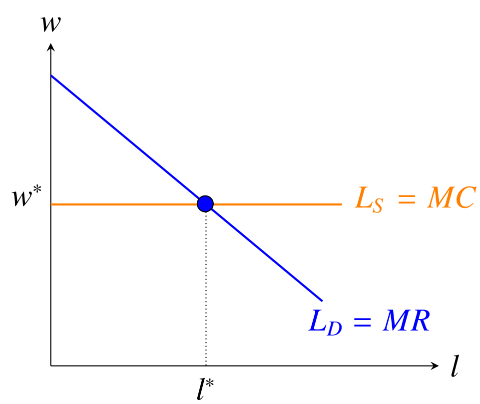
There are several different but related approaches to measuring employer power [see 170, for a recent survey on measures of monopsony]. The choice of method to use depends on the objectives of the analysis, the framework under consideration, and the data available to the researcher. In the traditional model, the labor market has a single buyer. Since there is only one buyer, that buyer faces the entire market's labor supply curve, upward sloping—in contrast to the horizontal labor supply curve for an individual firm in the perfectly competitive labor market with many employers. In the early stage of the literature, monopsony power has been measured as "potential monopsony power" in the language of [55] by estimating wage elasticity of labor supply to the firm under the assumption of an isolated labor market with a single firm. We rarely use the traditional model with this assumption because, in practice, it is unlikely that there is only one employer in the labor market.

The literature suggests several sources of upward-sloping labor supply curve to an individual firm in the presence of other firms. As reviewed by [49] and later summarized by [186], they include (i) collusion and Cournot competition among firms, (ii) workers' heterogeneous preferences for firms, (iii) the presence of workers' moving costs to change employers, (iv) search friction, and (v) efficiency wages at large firms. The labor supply elasticity still can be functional to quantify the labor market power; however, there are other measures, such as job separation rate, if models of job search [60] are used to interpret the source of monopsony power.

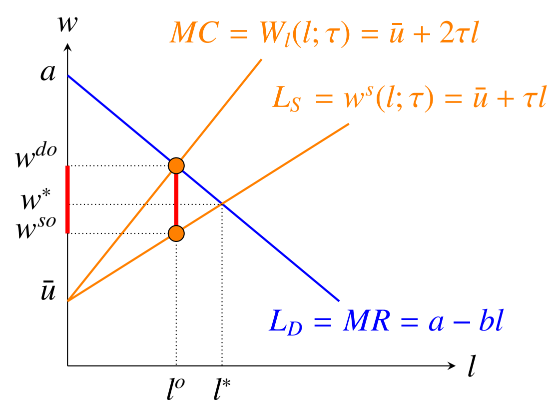
This appendix first briefly shows the relationship between markdowns and labor supply elasticity using a simple model with an arbitrary functional form assumption. Consider a revenue function $R(l) = (a - bl/2)l$ and the associated profits $R(l) - W(l)$ where $W(l) = w^s(l)l$ denotes total labor cost. An inverse labor supply function is given by $w^s(l) = \bar{u} + \tau l$ where $\bar{u}$ is the constant utility when a worker does not work, and $\tau \in [0, T]$ is the mobility cost or travel cost for the worker, which is assumed to be exogenous at this point, and $\tau \equiv T/L$ where L is a population of workers. It is worth noting that, in this model for illustration, I use mobility cost τ as the source of the upward-sloping labor supply curve, i.e., the labor supply curve to an individual firm will be a horizontal line $w^s(l) = \bar{u}$

if we shut down the mobility cost or set $\tau = 0$. Figure A.1 shows the labor market equilibrium under perfect (panel (a)) and imperfect (panel (b)) competition. The first-order condition for profit maximization problem implies that profits are maximized at an employment level where the marginal revenue product of labor (MRPL), $R_l(l) = a - bl$, generated to the firm equals the marginal cost of labor, $W_l(l) = \bar{u} + 2\tau l$. Since the marginal cost of labor exceeds the wage, l^o number of workers will be hired by the firm, which is less than the socially efficient amount l^*. The firm pays a wage of w^{so} less than the socially efficient level, w^*.

Figure A.1: Individual Firm's Labor Market Equilibrium



(a) Perfect competition



(b) Monopsony

Notes: Panel (a) depicts the labor market equilibrium for an individual firm under perfect competition, while panel (b) illustrates a basic model of monopsony.

The profit maximization problem in the basic monopsony model is

$$\max_{l \geq 0} R(l) - w^s(l)l, \tag{A.18}$$

where I ignore the index of firm i and time t for notational simplicity at the moment. The first-order condition of this maximization problem is

$$R_l(l) = \left(\frac{w_l(l)l}{w(l)} + 1 \right) w(l) = \left(\varepsilon_S^{-1} + 1 \right) w(l), \tag{A.19}$$

and, thus, the markdown ν, a wedge between the MRPL and the wage, is

$$\nu \equiv \frac{R_l(l)}{w(l)} = \varepsilon_S^{-1} + 1 \tag{A.20}$$

where $R_l(l) = \frac{\partial R(l)}{\partial l}$ is the MRPL, $w(l)$ is the wage, and $\varepsilon_S = \frac{\partial l}{\partial w(l)} \frac{w(l)}{l}$ is the elasticity of labor supply.

As shown by the optimality condition in equation (A.20) and Figure A.2(b), the wedge between the MRPL and the monopsony wage is directly linked to the wage elasticity of labor supply to an individual firm. In addition to measuring the monopsony by estimating the elasticity of labor supply on the right-hand side of (A.20) as mentioned above, we can compute the degree of monopsony power by estimating the wedge between the (nominal) wage w^{so} and MRPL w^{do} on the left-hand side of (A.20), which is expressed by the distance between w^{so} and w^{do} in Figure A.2(b).

Second, I review other methods of measuring monopsony power, starting with different variants of labor supply elasticity. In a dynamic setting, a measure of monopsony based on a model pioneered by [169] indirectly quantifies the wage elasticity to the firm by estimating its two components using the following steady-state relationship:

$$\varepsilon_{Nw} = \varepsilon_{Rw} - \varepsilon_{qw}, \tag{A.21}$$

where ε_{Nw} is the wage elasticity of labor supply to the firm, ε_{Rw} is the wage elasticity of the share of recruits hired from employment, and ε_{qw}, is the wage elasticity of workers' separation decisions to either employment or unemployment. [170] calls this a "modern" monopsony in which labor market frictions play a critical role.

The classical monopsony in static settings has also been recently revived, and [69] argue that the labor supply curve that an individual firm faces would be imperfectly elastic due to idiosyncratic non-wage amenities offered by firms even if there are a small number of firms in the labor market. The idea here is that a wage decline, for example, does not necessarily lead all existing workers to leave because some might still like their idiosyncratic non-wage aspects. In this strand, the wage elasticity of the labor supply curve to an individual firm j is derived as:

$$\frac{1}{\varepsilon_j} = \frac{1 - s_j}{\varepsilon} \tag{A.22}$$

where s_j is the market share of the firm, and ε is the inverse of the elasticity of labor supply faced by the firm as the labor supply is given by $n_j = \varepsilon^{-1}(w_j - b_j)$ where n_j is log employment, w_j is log wage, and b_j is a labor supply shifter. [170] calls this as a "new classical" monopsony in which non-wage amenities play in key role.

The measures of monopsony described above and in Section 2.3 are derived from theories. But there are also some measures borrowed from other fields of economics. For example, one can use concentration ratios for vacancies and employment using the Herfindahl index borrowed from Industrial Organization (IO) literature [31]. Relatedly, perfectly elastic labor supply (or $\varepsilon \approx 0$) implies perfect competition in the labor market, which is consistent with the monopsony model, if a firm j's market share is small (or $s_j \approx 0$) according to equation (A.22). One could also use the number of employers in the labor market relative to the number of workers as a measure of (inverse) employer power or monopsony. In particular, if the ratio of employers to workers is lower, employer power is higher. Intuitively, the wage elasticity of labor supply positively relates to the number of firms in the market since workers' quit rate and labor supply elasticity would be higher in a market with more employers or vacancies. For example, [77] used this measure to analytically examine the impact of monopsony power on wage inequality in a labor market with search frictions.

A.5 Additional Results on Markdowns

A.5.1 Robustness of Estimated Markdowns for German Manufacturing

In this paper, I show that estimated markdowns for East and West Germany are higher in the East than in the West. In this appendix, I check the robustness of my baseline markdown estimates for German manufacturing by pooling the markdowns estimated separately for East and West Germany. The country-level median and average markdowns in Table A.5 are highly similar to those in Table 2.4. The markdown distribution across industries within manufacturing is also consistent with my baseline estimates.

Table A.5: Estimated Plant-Level Markdowns in German Manufacturing

	Median	Mean	$IQR_{75\text{-}25}$	SD
Leather and related products	2.185	2.021	1.395	0.748
Wearing apparel	2.014	1.992	1.151	0.699
Furniture	1.704	1.819	1.055	0.705
Wood and wood products (excl. furniture)	1.524	1.629	0.882	0.560
Paper and paper products	1.437	1.447	0.604	0.498
Beverages	1.430	1.488	0.395	0.544
Repair and installation of machinery and equipment	1.320	1.517	0.691	0.646
Other transport equipment	1.319	1.346	0.829	0.507
Rubber and plastics	1.294	1.388	0.586	0.512
Other non-metallic minerals	1.284	1.435	0.754	0.645
Chemicals and chemical products	1.277	1.431	0.855	0.603
Motor vehicles, trailers, and semi-trailers	1.244	1.359	0.731	0.550
Basic pharmaceutical products	1.241	1.313	0.588	0.634
Fabricated metals, excl. machinery and equipment	1.193	1.322	0.666	0.535
Food products	1.179	1.306	0.682	0.563
Electrical equipment	1.154	1.225	0.562	0.481
Machinery and equipment	1.116	1.229	0.551	0.517
Basic metals	1.063	1.172	0.431	0.419
Textiles	1.046	1.238	0.562	0.460
Computer, electronic, and optical products	1.017	1.078	0.583	0.416
Other manufacturing	0.992	1.096	0.465	0.438
Printing and reproduction of recorded media	0.968	1.020	0.395	0.431
Whole sample	**1.200**	**1.331**	**0.726**	**0.569**
Sample size	9,431			

Notes: Markdowns are estimated for East and West German establishments separately using the IAB Establishment Panel from 1997-2018 under the assumption of a translog specification for gross output. The plant-level markdowns estimated separately for East and West German establishments are pooled to calculate the nationally representative estimate. Each industry group in manufacturing corresponds to the manufacturing categorization of the Federal Statistical Office. The distributional statistics are calculated using sampling weights provided in the data. Industries of wearing apparel and leather and related products are censored in this table because industry-specific markdowns were estimated for less than 20 establishments in these two industry groups, and thus the number of observations slightly declined.

To further analyze the production technologies in East and West Germany and compare them with that estimated on the full sample, Table A.6 presents production parameters and output elasticities estimated on different samples. Despite some differences in production parameters across East and West Germany, I find that output elasticities separately estimated for East/West Germany are comparable to those estimated on a nationally representative sample covering the entire country. Thus, using the same production function for East and

West German manufacturing firms as I did in my baseline markdown estimation is reasonable.

Table A.6: Components of Markdown Estimates under Translog Function

	Full sample (Germany)	East Germany	West Germany
	(1)	(2)	(3)
	Panel A. Production parameters		
β_k	0.146	0.155	0.202
β_l	0.723	0.823	0.656
β_m	0.260	0.140	0.253
β_{kl}	0.036	0.033	0.064
β_{km}	-0.039	-0.038	-0.051
β_{lm}	-0.133	-0.133	-0.143
β_{k^2}	0.006	0.005	0.001
β_{l^2}	0.052	0.037	0.043
β_{m^2}	0.075	0.083	0.083
	Panel B. Elasticities		
θ_l	0.383 (0.118)	0.369 (0.122)	0.395 (0.122)
θ_m	0.604 (0.128)	0.609 (0.136)	0.586 (0.136)

Notes: Panel A presents production function parameters estimated on the full sample (Column 1), sub-sample of East German establishments (Column 2), and sub-sample of West German firms (Column 3) using the IAB Establishment Panel data in 1997-2018 under translog specification. In Panel B, I show the mean value of output elasticities estimated on different samples, and standard errors are in parenthesis. The elasticities are calculated using sampling weights provided in the data.

The literature usually estimates industry-specific production function to account for heterogeneity in production across industries [e.g., 224, 57]. However, in my baseline analysis, I estimate a production function common across sectors, mainly because the number of manufacturing firms in the primary firm-level data is relatively small, although the survey is nationally representative. Estimating the production function for each two-digit industry provides noisier markdown estimates than the baseline markdowns, as shown in Table A.7. Therefore, I prefer to employ production functions similar across industry groups in my baseline analysis, which provides more stable results. However, the overall markdowns are generally consistent with my baseline markdowns in median and average manufacturers.

Table A.7: Estimated Plant-Level Markdowns in German Manufacturing (Industry-Specific)

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD
Furniture	5.261	6.178	3.359	3.134
Other non-metallic minerals	5.064	6.231	4.275	3.133
Repair and installation of machinery and equipment	3.024	3.948	3.233	2.406
Other manufacturing	2.604	2.986	1.737	1.713
Textiles	2.523	3.141	2.106	2.061
Paper and paper products	1.909	2.168	1.980	1.165
Wood and wood products (excl. furniture)	1.507	1.981	1.842	1.543
Fabricated metals, excl. machinery and equipment	1.481	1.677	0.797	0.902
Rubber and plastics	1.376	1.652	0.691	1.412
Motor vehicles, trailers, and semi-trailers	1.331	1.605	0.729	1.083
Beverages	1.317	1.771	1.327	1.461
Machinery and equipment	1.307	1.360	0.507	0.484
Food products	1.052	1.230	0.701	0.622
Basic metals	1.050	1.119	0.722	0.538
Chemicals and chemical products	1.047	1.142	0.750	0.562
Other transport equipment	1.027	1.190	0.544	0.638
Computer, electronic, and optical products	0.985	1.219	0.680	1.318
Electrical equipment	0.942	1.005	0.868	0.664
Printing and reproduction of recorded media	0.803	0.879	0.581	0.574
Basic pharmaceutical products	0.623	0.693	0.691	0.647
Whole sample	**1.413**	**2.111**	**1.321**	**2.125**
Sample size	12,588			

Notes: Markdowns are estimated separately for each two-digit industry group using the IAB Establishment Panel from 1997-2018 under the assumption of a translog specification for gross output. Each industry group in manufacturing corresponds to the manufacturing categorization of the Federal Statistical Office. The distributional statistics are calculated using sampling weights provided in the data.

A.5.2 Markdown Trend under Cobb-Douglas Specification

As an alternative to my baseline choice of the functional form of the production function, translog, I estimate the production function and thus markdowns using Cobb-Douglas specification. Figure A.2 illustrates the time trend of aggregate markdowns. The result suggests that my estimates are not entirely but generally robust to this different functional form.

Figure A.2: Time Evolution of Aggregate Markdowns under Cobb-Douglas Specification

Notes: Markdowns are constructed using the IAB Establishment Panel (IAB BP) data from 1997-2018 under the assumption of Cobb-Douglas production and aggregated according to expression (A.23) and (A.25). The employment share of labor market ω_{klt} is based on total number of employees.

A.5.3 Markups

Table A.8 reports the markup estimates. The summary statistics are provided for each industry group. The results indicate a presence of market power in output markets: producers have about 31 percent (26 percent) of market power at the plant-year level at the mean (median). Compared to the markdowns, variations of markups across and within industry groups are relatively smaller than variations of markdowns. The IQR and standard deviation are 19.3 and 18.7 percent, respectively.

Although these estimates of markups are informative, they are subject to bias because physical outputs are proxied by revenues deflated by 2-digit industry-level prices [155, 50]. So, one should take these markup estimates as lower bounds for market power in output markets. Fortunately, our estimates of markdown, which is my main focus in this paper, are still valid with these estimates of markups as the bias cancels out in the equation (2.1). So, the markdowns estimated using deflated revenues are not subject to [50]'s critique when the markups are used to obtain estimates for markdowns. A formal proof can be found in Appendix O.6 of [224].

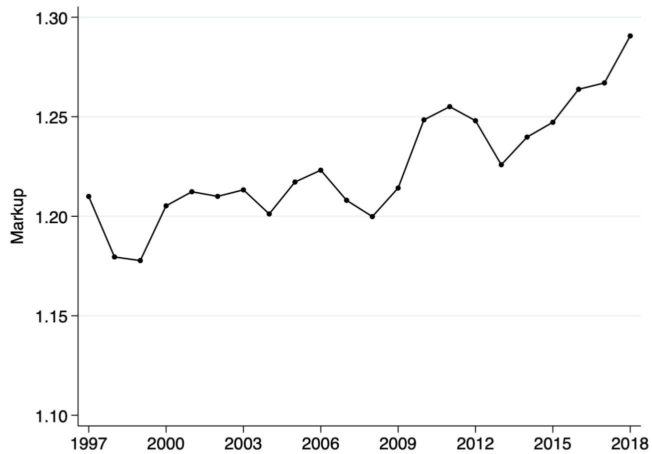
Figure A.3 presents the time series for the aggregate markup. The markup is aggregated at the market level according to equation (A.24). Then, I aggregate markups across markets through employment weights. As briefly discussed above, firm-level markups estimated using deflated revenues instead of physical outputs are biased, and thus, the aggregate markups are also biased. While we should take the markup estimates cautiously, a trend in aggregate markups could be informative. The markup in German manufacturing has been monotonically increasing since 1997 until 2018.

Table A.8: Estimated Plant-Level Markups in German Manufacturing

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD
Printing and reproduction of recorded media	1.422	1.435	0.250	0.214
Food products	1.357	1.381	0.244	0.184
Other manufacturing	1.342	1.372	0.179	0.158
Computer, electronic, and optical products	1.334	1.398	0.314	0.223
Beverages	1.333	1.417	0.324	0.322
Basic pharmaceutical products	1.284	1.312	0.241	0.147
Textiles	1.281	1.335	0.281	0.205
Fabricated metals, excl. machinery and equipment	1.276	1.317	0.167	0.190
Furniture	1.243	1.250	0.098	0.076
Wood and wood products (excl. furniture)	1.237	1.335	0.228	0.253
Paper and paper products	1.237	1.248	0.162	0.127
Other non-metallic minerals	1.235	1.274	0.173	0.143
Motor vehicles, trailers, and semi-trailers	1.226	1.270	0.116	0.172
Repair and installation of machinery and equipment	1.223	1.269	0.057	0.122
Machinery and equipment	1.218	1.256	0.123	0.151
Rubber and plastics	1.205	1.231	0.109	0.103
Basic metals	1.199	1.211	0.133	0.100
Electrical equipment	1.196	1.226	0.110	0.114
Other transport equipment	1.185	1.271	0.251	0.190
Leather and related products	1.182	1.197	0.035	0.066
Chemicals and chemical products	1.176	1.216	0.107	0.141
Wearing apparel	1.162	1.206	0.055	0.151
Whole sample	**1.258**	**1.310**	**0.193**	**0.187**
Sample size	12,794			

Notes: Markups are estimated using the IAB-BP data from 1997-2018 under the assumption of a translog specification for gross output. Each industry group in manufacturing corresponds to the manufacturing categorization of the Federal Statistical Office. The distributional statistics are calculated using sampling weights provided in the data.

Figure A.3: Time Evolution of Markups across German Manufacturing Plants



Notes: Markups are constructed using the IAB Establishment Panel data from 1997-2018 under the assumption of translog production and aggregated according to expressions (A.24) and (A.26). The employment share of labor market ω_{jlt} is based on total number of employees.

A.5.4 Markdown Estimation Controlling for Robot Exposure

The studies estimating the production function and markups using the production approach tend to include the key explanatory variable of interest in the production function. For example, [54] include their measure of trade liberalization in the estimation of production parameters to examine the effects of trade liberalization in China on markups and productivity of Chinese manufacturing firms. In this appendix, I similarly include the measure of exposure to industrial robots in the production function estimation and check the robustness of the baseline markdown estimates. Table A.9 compares the markdown estimates from this analysis with the baseline measure, showing that the estimated markdown remains the same when including Germany's exposure to industrial robots in the production function estimation.

Table A.9: Estimated Plant-Level Markdowns with and without Robot Exposure in the Production Function Estimation

	Mean	SD	Min	Max	N
Baseline measure (without robot exposure)	1.271	0.565	0.018	3.656	12,806
Alternative measure (with robot exposure)	1.279	0.532	0.002	3.390	9,564

Notes: Markdowns are estimated using the IAB Establishment Panel from 1997-2018 under the assumption of a translog specification for gross output. The distributional statistics are calculated using sampling weights provided in the data.

To further illustrate the similarity between the two markdown measures, I regress the baseline measure on the alternative measure conditional on plant and year fixed effects and find a coefficient of 0.993 (SE: 0.000, p-value: 0.00). Although the two measures are almost identical, I use the baseline markdown measure estimated without robot exposure in the production function estimation as it is estimated for 30% more observations than the alternative measure.

A.5.5 Markdown and Markup Aggregation

The aggregate markdowns and markups are defined, respectively, as

$$\mathcal{V}_{klt} = \frac{\left(\sum_{j \in F_t(k,l)} s_{jt} \cdot \frac{\theta^L_{jt}}{\theta^L_{klt}} \cdot \left(\nu_{jt}\mu_{jt}\right)^{-1}\right)^{-1}}{\left(\sum_{j \in F_t(k,l)} s_{jt} \cdot \frac{\theta^M_{jt}}{\theta^M_{klt}} \cdot \mu^{-1}_{jt}\right)^{-1}}, \tag{A.23}$$

and

$$\mathcal{M}_{klt} = \left(\sum_{j \in F_t(k,l)} s_{jt} \cdot \frac{\theta^M_{jt}}{\theta^M_{klt}} \cdot \mu^{-1}_{jt}\right)^{-1}, \tag{A.24}$$

where θ^L_{klt} and θ^M_{klt} are, respectively, the average output elasticities of labor and intermediate materials in the industry k, location l, and year t. Here $s_{jt} = \frac{p_{jt}y_{jt}}{P_{klt}Y_{klt}}$

are sales weights[14] and $F_t(k,l)$ denotes the set of firms in local labor market (k,l).

I further aggregate the markdowns and markups across labor markets using employment weights [200] to examine whether monopsony power in German manufacturing has increased over time. Specifically, I define

$$\mathcal{V}_t = \sum_{k\in K}\sum_{l\in L}\omega_{klt}\mathcal{V}_{klt}, \tag{A.25}$$

and

$$\mathcal{M}_t = \sum_{k\in K}\sum_{l\in L}\omega_{klt}\mathcal{M}_{klt}, \tag{A.26}$$

where ω_{klt} is the employment share of labor market (k,l).

A.5.6 Measuring Labor Market Concentration

Given that I have worker-level administrative data matched with their employer, I first count workers at each establishment and then construct the HHI in labor market (o,l) and time t as

$$\text{HHI}_{mt} = \sum_{j=1}^{I} s_{jmt}^2, \tag{A.27}$$

where s_{jmt}^2 is the market share of firm j in market $m=(o,l)$ as a number between 0 and 100, and o and l denotes occupation and geography index, respectively. In the alternative definition, I calculate (A.27) for market $m' = (k,l)$ where k is the industry index. A firm's market share in a given market m (or m') and time

[14] I use sales weights strictly following [224], while the plant-level measures can also be aggregated using employment weights. The pattern and interpretation of aggregate measures are the same when the employment weights are employed because the sales and the number of workers are positively correlated, i.e., firms with higher sales employ more workers. The pairwise correlation between the log employment and log sales revenue is 0.944 (SE: 0.003, p-value: 0.00). Controlling for firm, year, district-by-year, and industry-by-year, I also find that the coefficient on log revenue in the regression of log employment is 0.333 (SE: 0.023).

t is defined as the sum of workers at a given firm in a given market and time divided by the total workers in that market and time. The average HHIs are calculated by weighted average using employment as weights. Formally,

$$\mathrm{HHI}_{lt} = \sum_{o \in O} \omega_{olt} \mathrm{HHI}_{olt} \quad (\text{or } \mathrm{HHI}_{lt} = \sum_{k \in K} \omega_{klt} \mathrm{HHI}_{klt}), \tag{A.28}$$

and

$$\mathrm{HHI}_{lt} = \sum_{k \in K} \sum_{l \in L} \omega_{klt} \mathrm{HHI}_{klt}. \tag{A.29}$$

A.5.7 Cross-Sectional Correlation between Markdown and Labor Market Concentration

Table A.10 presents the cross-sectional correlation (across labor markets–a combination of 3-digit industries and federal states) between the aggregate markdown $\mathcal{V}_{klt}$ and labor market concentration HHI_{klt}. The correlation calculated using the same dataset (IAB Establishment Panel–IAB BP) is positive and statistically significant at the 1% level on average; however, the correlation coefficient is 0.02, which is close to zero (second column).

Table A.10: Correlation between Employment HHIs and Aggregate Markdowns across Local Labor Markets

Year	$\rho(\mathcal{V}_{jlt}, \mathrm{HHI}^{IAB\text{-}BP}_{jlt})$	$\rho(\mathrm{HHI}^{IAB\text{-}BP}_{jlt}, \mathrm{HHI}^{LIAB}_{jlt})$	$\rho(\mathcal{V}_{jlt}, \mathrm{HHI}^{LIAB}_{jlt})$
1998	0.156**	0.143**	0.203***
2000	0.045	0.149**	0.129**
2002	0.085*	0.213***	0.056
2004	0.055	0.203***	0.103**
2006	0.011	0.220***	0.085*
2008	-0.021	0.237***	0.074
2010	-0.042	0.330***	0.038
2012	0.026	0.266***	0.131**
2014	-0.028	0.223***	0.020
2016	-0.014	0.138**	0.045
2018	0.072	0.258***	0.122
Average	**0.024****	**0.215*****	**0.081*****

Notes: Markdowns are estimated using the IAB Establishment Panel (IAB BP) data from 1997-2018 under the assumption of a translog specification for gross output. The cross-market correlations are calculated at the 3-digit ISIC-state level for every other year. Aggregate markdowns are calculated according to equation (A.23) whereas labor market concentration HHI_{klt} is calculated according to equation (A.27) using either IAB BP and matched employer-employee (LIAB) data, which are highlighted in the superscript. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

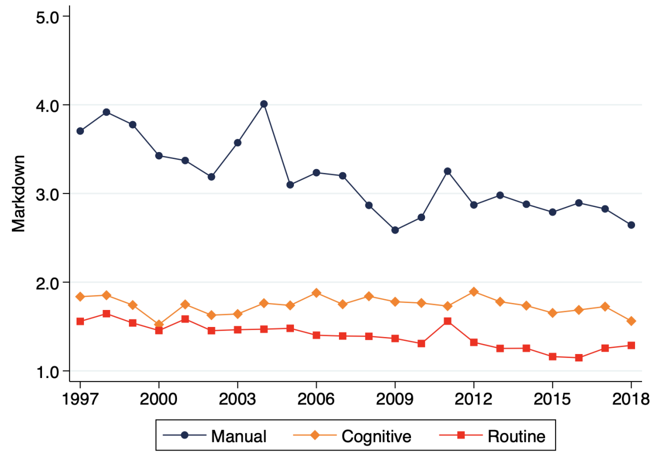
To check the robustness of my baseline employment HHI measure calculated using IAB BP data, I compute the same index according to equation (A.27) based on the matched employer-employee data (LIAB). The cross-section correlation between the two HHIs is strong, positive, and almost always statistically significant at the 1% level (third column). Across years and on average, the correlation between aggregate markdown and LIAB-based HHI is mostly positive but rarely statistically significant (fourth column), consistent with the results in the second column.

A.5.8 Trends in Aggregate Markdowns for Heterogeneous Workers

I aggregate the plant-level markdowns for heterogeneous workers using equations (A.23) and (A.25) similar to the baseline analysis where workers are homogeneous to show how employers' labor market power has changed for different workers in German manufacturing over time. Figure A.4 illustrates the trends of aggregate markdowns, $\mathcal{V}_t$, over workers performing routine, nonroutine manual, and nonroutine cognitive tasks. Markdowns for workers performing manual and routine tasks have been decreasing, and the decline is more intensive in magnitude for manual task-performing workers.[15] Labor market power for nonroutine cognitive workers has been stable between 1997-2018.

[15] A downward trend in markdown for routine workers is strongly consistent with [32] who show that labor supply elasticity, proportional to the inverse of markdown, has been increasing for routine workers.

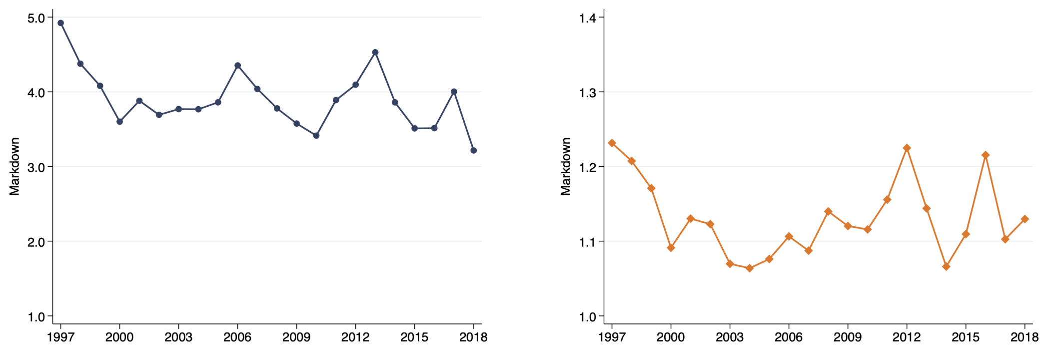
Figure A.4: Time Evolution of the Aggregate Markdowns for Workers Performing Different Tasks



Notes: The figure depicts the time evolution of aggregate markdowns for nonroutine cognitive, routine, and manual workers between 1997 and 2018. Plant-level markdowns are constructed using the IAB Establishment Panel and matched employer-employee (LIAB) data under the assumption of translog production with heterogeneous labor inputs and aggregated according to expressions (A.23) and (A.25). The employment share of labor market ω_{klt} is based on the total number of employees. The classification of nonroutine cognitive, routine, and nonroutine manual task-performing workers is based on the BIBB/BAuA Employment Surveys.

Figure A.5 illustrates the time evolution of aggregate markdowns for workers with different skills. The results for low-skilled and high-skilled workers are generally consistent with workers performing various tasks. Specifically, the pattern of employers' labor market power for low-skilled or low-educated workers is downward-sloped, potentially driven by manual workers. The markdown for high-skilled or high-educated workers has been relatively stable between 1997-2018, similar to cognitive workers.

Figure A.5: Time Evolution of the Aggregate Markdowns for Workers with Different Skills



(a) Low-skilled (b) High-skilled

Notes: The figure plots the time evolution of aggregate markdowns for low-skilled workers (no vocational training) and high-skilled workers (with at least vocational training) from 1997-2018. Plant-level markdowns are constructed under the assumption of translog production with heterogeneous labor inputs and aggregated according to expressions (A.23) and (A.25). The employment share of labor market ω_{klt} is based on the total number of employees.

A.5.9 Robustness of Markdowns for Heterogeneous Workers

In my baseline analysis, I define heterogeneous workers performing different tasks based on task intensity measures constructed using Germany's BIBB/BAuA Employment Surveys and an approach by [21]. But this appendix checks the robustness of my results on markdowns for heterogeneous workers performing different tasks to the use of alternative task intensity measures proposed by [26].[16]

Classification of Workers. Since [26] create their measures of task content or task inputs for each occupation in the U.S. using O*NET data, the values of the indices could be different from the values of indices constructed using the Ger-

[16] I obtained [26]'s occupational task measures from David Dorn's website: https://www.ddorn.net/data.htm#Occupational%20Tasks

man dataset of BIBB/BAuA Employment Surveys. However, it is reasonable to consider that these two measures are comparable. Specifically, they build three measures of abstract, routine, and manual task inputs for their constructed version of 3-digit 1990 U.S. Census occupations ("occ1990dd"). I match them with German administrative data through Germany's 5-digit KldB 2010 occupation classifications based on several crosswalks. First, I obtain [26]'s version of 3-digit 1990 U.S. Census occupations matched with 3-digit 2000 U.S. Census occupations ("occ2000") from [3]'s data appendix of task measure construction. Then, I match that with the 6-digit 2000 Standard Occupational Classification (SOC) via 3-digit 2000 U.S. Census occupations using their crosswalks.[17] After that, using crosswalks obtained from the Institute for Structural Research (IBS), I matched the "occ1990dd" to the 6-digit 2010 SOC and then to the 4-digit 2008 International Standard Classification of Occupations (ISCO-08). Finally, I match it with the 5-digit German Klassifikation der Berufe 2010 (KldB 2010) via 4-digit ISCO-08 using a crosswalk obtained from Germany's Federal Employment Agency (Bundesagentur für Arbeit).[18] After all these crosswalks, I have [26]'s three measures for abstract, routine, and manual task inputs merged to Germany's linked employer-employee data at the 5-digit occupations level.

The three indices for abstract, routine, and manual task inputs in each occupation o in 1980, which are scaled between zero and ten, are denoted as $T^A_{o,1980}$, $T^R_{o,1980}$, and $T^M_{o,1980}$, respectively, before merging with the matched data. But after matching these with the linked data (LIAB), I denote them as T^A_{ijt}, T^R_{ijt}, or T^M_{ijt}

[17]The data files of task measure construction and the crosswalks are available on David Autor's website.

[18]The crosswalk between 4-digit ISCO-08 and 5-digit KldB 2010 can be downloaded from https://statistik.arbeitsagentur.de/DE/Statischer-Content/Grundlagen/Klassifikationen/Klassifikation-der-Berufe/KldB2010-Fassung2020/Arbeitsmittel/Generische-Publikationen/Umsteigeschluessel-KLDB2020-ISCO08.xlsx.

although the values are the same across worker i, firm j, and year t within an occupation o. Since I have an individual index i, I drop the occupation index o. Then, following [4], I normalize these three measures to have mean zero and unit standard deviation. Using these indices, I determine whether a worker i at firm j in year t is an abstract, routine, or manual worker if the maximum of the three normalized tasks inputs measure is T^{A}_{ijt}, T^{R}_{ijt}, or T^{M}_{ijt}, respectively. Table A.11 summarizes the employment, wage bill, and daily wage for abstract, routine, and manual workers.

Table A.11: Summary Statistics (Abstract, Routine, and Manual Workers)

	Abstract			Routine			Manual		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Log labor	2.553	1.393	6659	2.828	1.450	8142	2.364	1.379	6607
Labor cost (% revenue)	0.066	0.100	9718	0.126	0.122	9718	0.071	0.103	9718
Daily wage (€)	115.9	71.60	6657	74.67	37.91	8132	66.85	45.74	6602

Notes: The table summarizes the employment, labor cost, and daily wages for abstract, routine, and manual workers over the period 1997-2018. The classification of workers is based on [26]'s task content/inputs measures. Employment and wage bill information comes from the IAB Establishment Panel while daily wage comes from the matched employer-employee (LIAB) data. The unit of observation is the firm, and sampling weights are applied.

Estimated Markdowns for Heterogeneous Workers. Table A.12 presents the estimated plant-level markdowns for heterogeneous workers, generally consistent with my baseline results. Specifically, routine workers are subject to the lowest degree of monopsony power, while manual workers are subject to the highest labor market power on average. Markdown for manual workers is also the highest in the median firm; however, abstract workers have slightly lower markdown than routine workers, a different result from the baseline. This difference could be due to contextual differences and resulting differences in task contents for occupations.

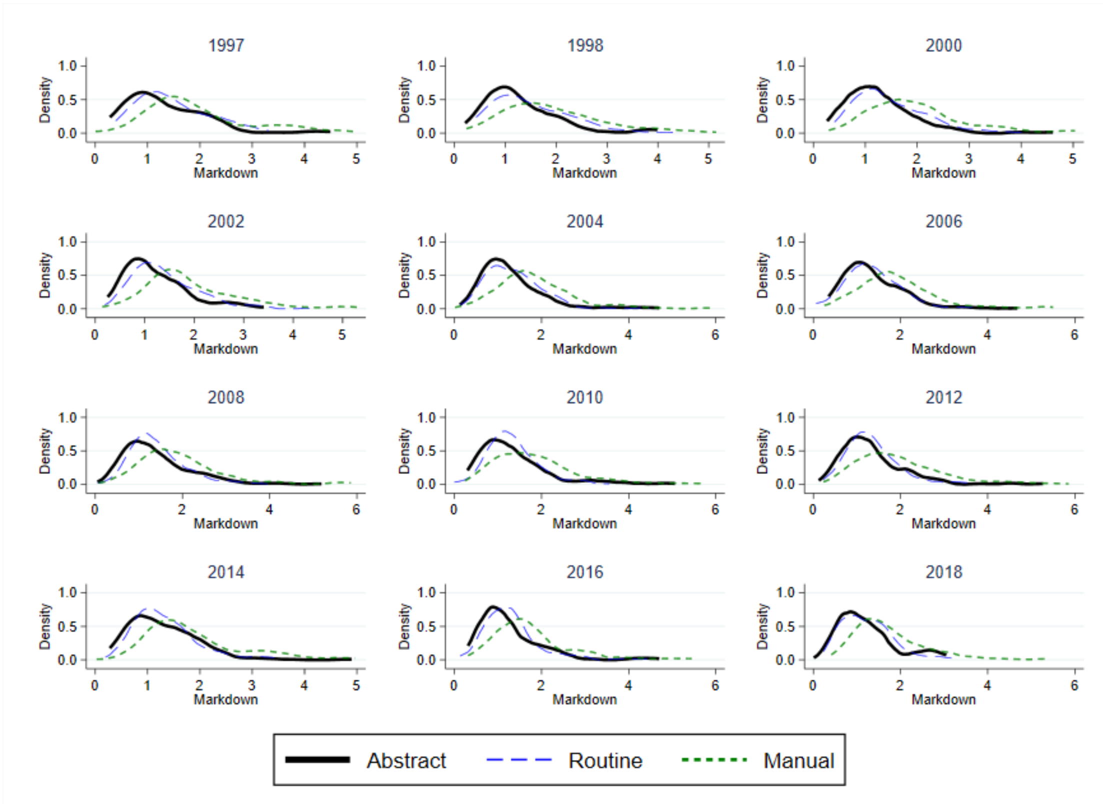
Table A.12: Estimated Plant-Level Markdowns for Workers Performing Routine, Abstract, and Manual Job Tasks in German Manufacturing

	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Routine workers	1.075	1.185	0.656	0.566	3779
Abstract workers	1.069	1.280	0.866	0.807	3779
Manual workers	1.634	2.310	1.355	2.354	3779

Notes: Markdowns are estimated using the IAB Establishment Panel and the linked employer-employee (LIAB) data in 1997-2018 under the assumption of a translog specification for gross output with heterogeneous labor inputs. Labor inputs of production are heterogeneous by tasks performed at the workplace. I classify workers based on [26]'s task contents measures. The distributional statistics are calculated using sampling weights provided in the data.

The distribution of markdowns for abstract, routine, and manual workers, plotted in Figure A.6, is generally the same for nonroutine cognitive, routine, and nonroutine manual workers in the baseline analysis.

Figure A.6: Distributions of Wage Markdowns for Abstract, Routine, and Manual Workers



Notes: Based on the IAB Establishment Panel and matched employer-employee (LIAB) data. The classification of abstract, routine, and manual task-performing workers is based on [26]'s task contents measures. The figure depicts the markdown distributions for abstract, routine, and manual workers every other year from 1997-2018.

A.6 Additional Results on Firm-Level Effects

A.6.1 Robustness of Heterogeneous Effects by Firm Size

In Section 2.5.4, I define firms in the top 3 deciles of the firm size distribution as large firms and show that markdown effects of robot exposure concentrate among such firms. This section, however, checks the robustness of that result using alternative definitions of large firms based on different parts of the firm size distribution. Table A.13 shows that the baseline effects heterogeneous by firm size are remarkably robust to various definitions of large firms where the impacts are concentrated.

Table A.13: Plant-Level Effects of Robot Exposure on Wage Markdowns at Large Firms in East Germany (Different Parts of the Firm Size Distribution)

	Dependent variable: Annual change in plant-level markdowns		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. Top 2 quintiles		
ΔPredicted robot exposure	0.044	0.024	0.006
	(0.010)	(0.023)	(0.022)
N	1428	1428	1428
	Panel B. Top tercile		
ΔPredicted robot exposure	0.135	0.043	-0.009
	(0.052)	(0.060)	(0.036)
N	652	652	652
	Panel C. Top quartile		
ΔPredicted robot exposure	0.101	-0.042	-0.021
	(0.049)	(0.085)	(0.048)
N	338	338	338
	Panel D. Above median		
ΔPredicted robot exposure	0.025	0.003	-0.007
	(0.014)	(0.023)	(0.023)
N	1413	1413	1413

Notes: The table presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers for large firms between 1998 and 2018 using the IV (2SLS) regressions under various definition of large firms. In Panels A-D, large firms are defined as those in the top 2 quintiles, top tercile, top quartile, and above the median of the firm size distribution, respectively. Columns (1)-(3) report the effects of automation exposure on the markdowns over heterogeneous workers performing different tasks, and the dependent variable is the annual change in the markdowns over routine workers (column (1)), nonroutine manual–NRM workers (column (2)), and nonroutine cognitive–NRC workers (column (3)). All specifications include the same set of controls and fixed effects as in Table A.23. Standard errors clustered at the level of local labor markets or districts are in parentheses.

A.6.2 Additional Robustness of Firm-Level Results

The firm location can vary across the regions over time potentially due to the firm mobility across districts or a firm can have multiple plants in different places with the same firm identification. So, we can control for district fixed effects in addition to the firm fixed effects. Table A.14 shows the robustness of IV (2SLS) estimates in Table 2.36 by adding district or kreis fixed effects. The results from this robustness check are qualitatively and almost quantitatively similar to the baseline results.

Table A.14: Plant-Level Effects of Robot Exposure on Wage Markdowns in East and West Germany (Controlling for District Fixed Effects)

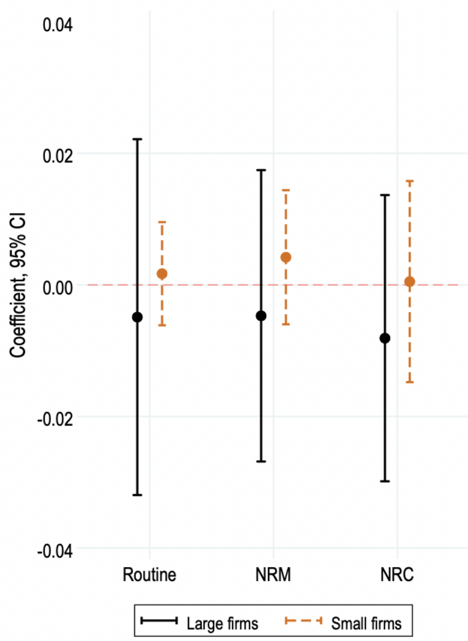
	Dependent variable: Annual change in plant-level markdowns		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. East Germany		
ΔPredicted robot exposure	0.011	-0.003	0.003
	(0.005)	(0.009)	(0.008)
N	3649	3649	3649
	Panel B. West Germany		
ΔPredicted robot exposure	-0.002	0.013	-0.005
	(0.004)	(0.014)	(0.006)
N	3823	3823	3823

Notes: Panel A presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in East Germany between 1998 and 2018 using the 2SLS IV regressions. Panel B reports the results from the IV (2SLS) regressions for West Germany. In both panels, the dependent variable is the annual change in plant-level markdowns for routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications control for constant, six plant size groups based on the number of employees at the establishment in the previous year, and demographic characteristics of districts or kreise in the previous year. The demographic controls are constructed using the matched employer-employee data (LIAB) and include the share of females, the share of foreigners, the share of workers over 50 years old, the shares of workers with no vocational training, vocational training, and university degree, and employment shares across broad industries (agriculture, food products, consumer goods, industrial goods, capital goods, construction, consumer-related services, business-related services, and public sector). The local labor market characteristics also contain the annual changes in exposure to net exports and ICT equipment. The firm, district, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

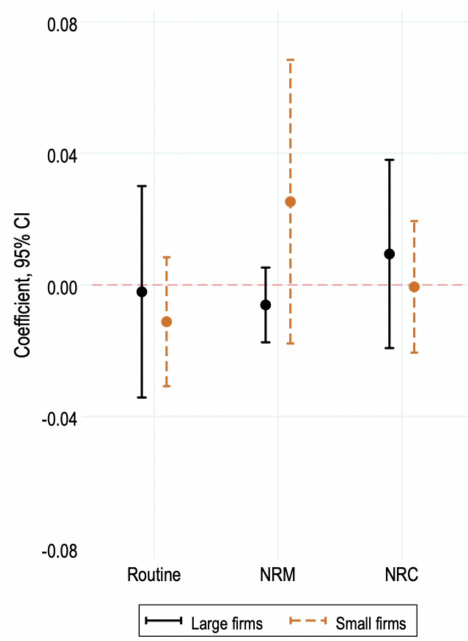
A.7 Additional Figures and Tables

A.7.1 Additional Figures

Figure A.7: Plant-Level Effects of Robot Exposure on Wages of Heterogeneous Workers at Large and Small Firms in East and West Germany



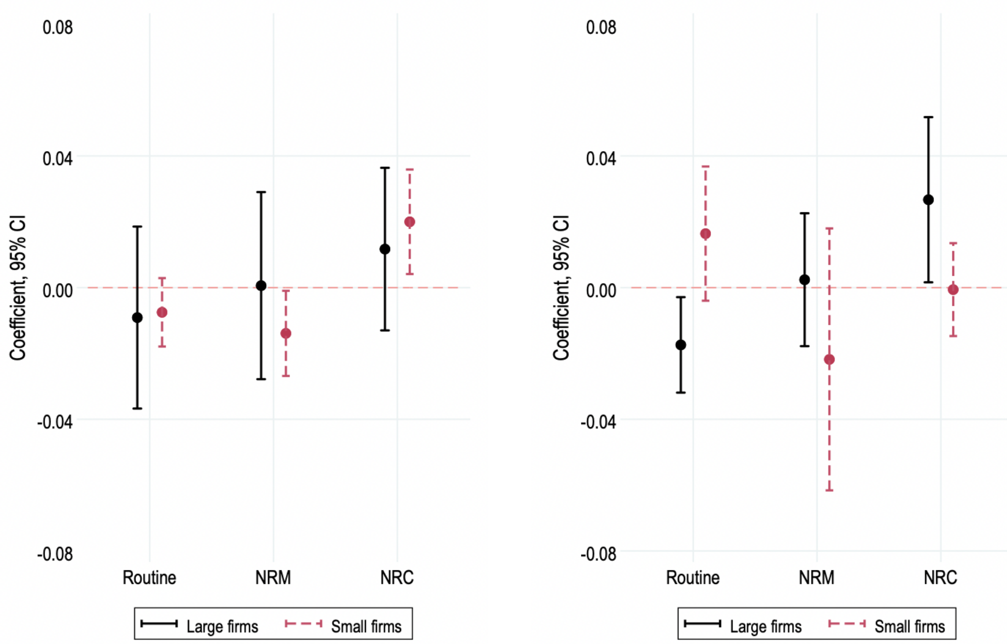
(a) East Germany



(b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual percentage change in average wage at firms with different size in districts from East and West Germany, respectively, between 1998 and 2018. Small firms are those in the bottom 7 deciles of the size distribution in the previous period, while large firms are plants in the top 3 deciles. In all regressions, the dependent variable is the annual percentage change in the average wage of routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Figure 2.14. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

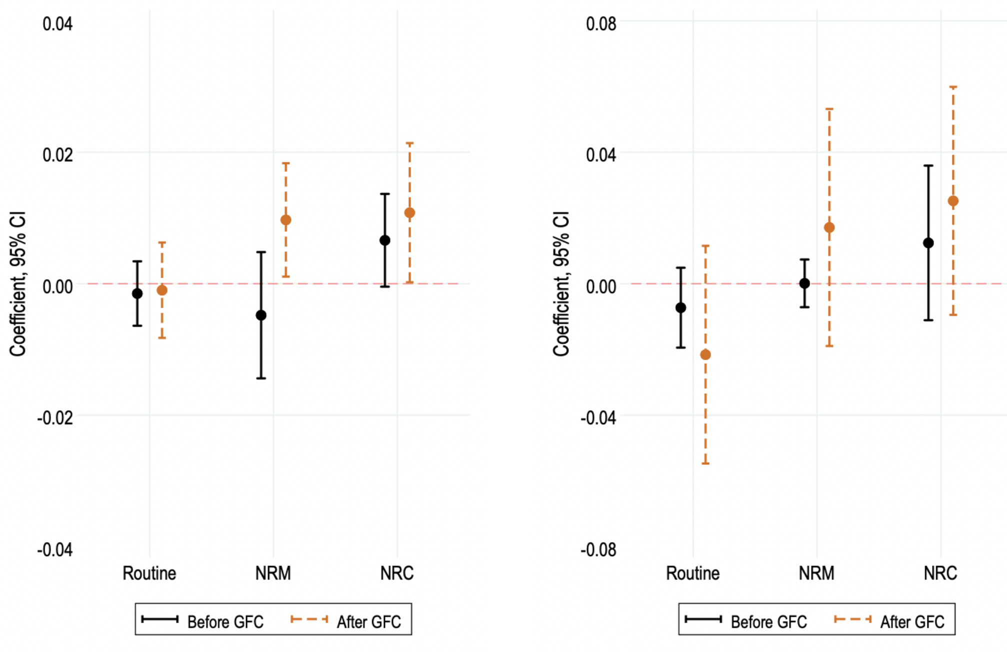
Figure A.8: Plant-Level Effects of Robot Exposure on Employment of Heterogeneous Workers at Large and Small Firms in East and West Germany



(a) East Germany (b) West Germany

Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual percentage change in employment at firms with different size in districts from East and West Germany, respectively, between 1998 and 2018. Small firms are those in the bottom 7 deciles of the size distribution in the previous period, while large firms are plants in the top 3 deciles. In all regressions, the dependent variable is the annual percentage change in plant-level employment of routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Figure 2.14. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

Figure A.9: Plant-Level Effects of Robot Exposure on Wages of Heterogeneous Workers in East and West Germany around the Great Recession

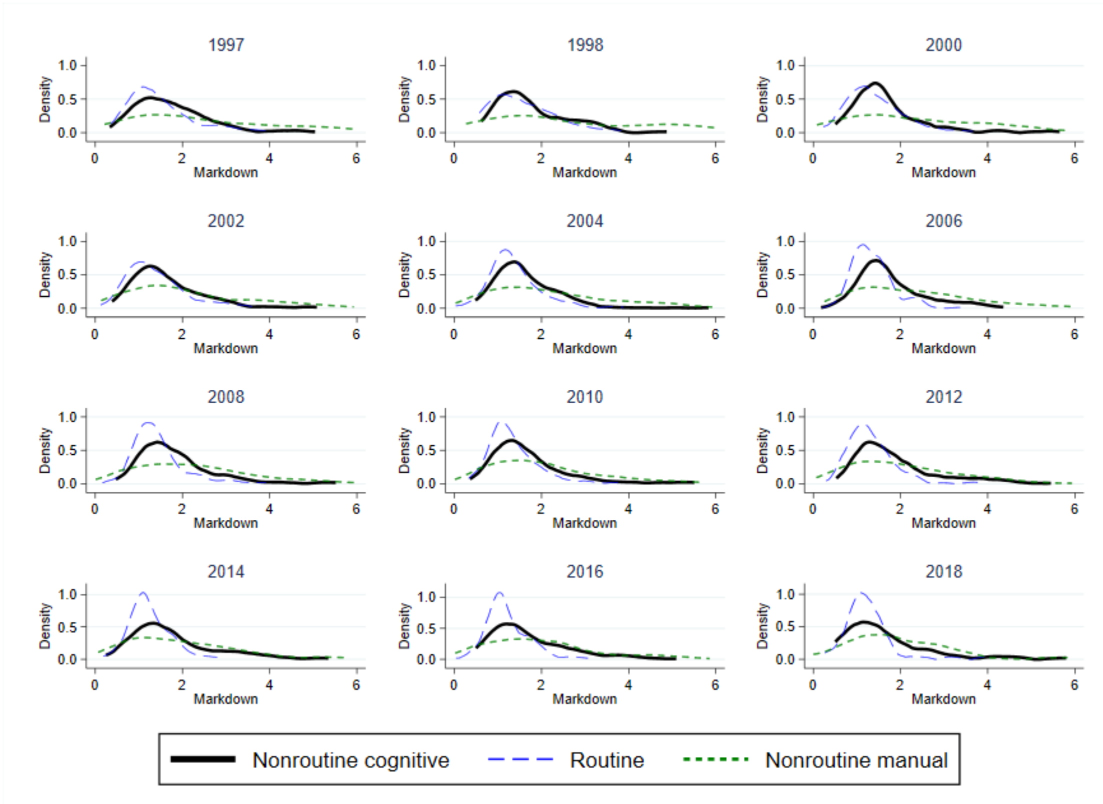


(a) East Germany

(b) West Germany

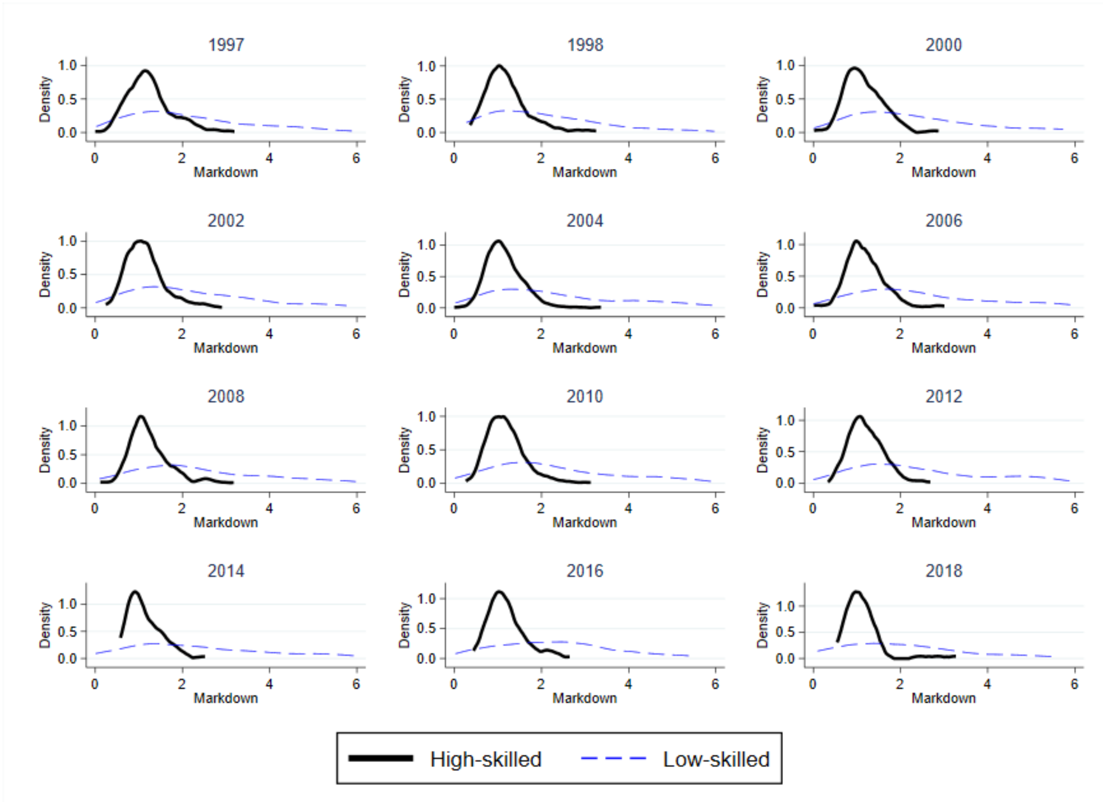
Notes: Panels (a) and (b) present the IV (2SLS) estimates on the effects of annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers on the annual percentage change in average wage at firms in districts from East and West Germany, respectively, before (1998-2008) and after (2009-2018) the Great Recession. In all regressions, the dependent variable is the annual percentage change in the average wage of routine workers, nonroutine manual (NRM) workers, and nonroutine cognitive (NRC) workers. All specifications include the same set of controls and fixed effects as in Table 2.36. Standard errors clustered by local labor market regions (kreise or districts), and 95% confidence intervals are presented.

Figure A.10: Distributions of Wage Markdowns for NRC, Routine, and NRM Workers



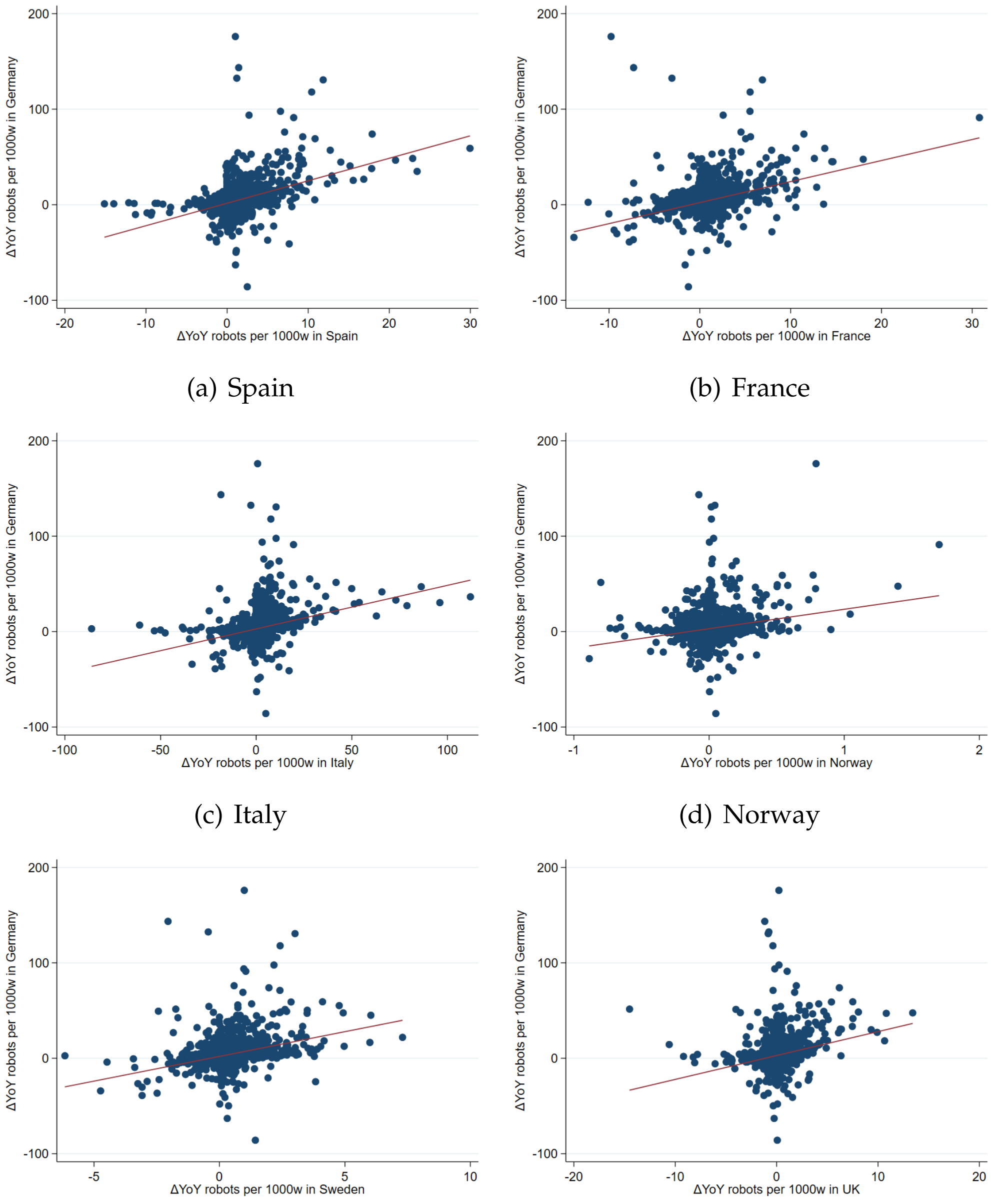
Notes: Based on the IAB Establishment Panel and matched employer-employee (LIAB) data. The classification of nonroutine cognitive, routine, and nonroutine manual task-performing workers is based on the BIBB/BAuA Employment Surveys. The figure depicts the markdown distributions for NRC, routine, and NRM in a given year over the period 1997-2018. NRC, nonroutine cognitive; NRM, nonroutine manual.

Figure A.11: Distributions of Wage Markdowns for Workers with Different Skills



Notes: Based on the IAB Establishment Panel and matched employer-employee (LIAB) data. The figure depicts the markdown distributions for high-skilled (with at least vocational training) and low-skilled (no vocational training) workers every other year from 1997-2018.

Figure A.12: 2SLS First-Stage Relationship (Robots in All Industries)



(a) Spain

(b) France

(c) Italy

(d) Norway

(e) Sweden

(f) UK

Notes: These scatter plots show the first-stage relationship between the annual changes in exposure to industrial robots in all industries for Germany and other high-income European countries between 1998 and 2018.

A.7.2 Additional Tables

Table A.15: Summary Statistics for Labor Market Concentration (All Industries, 2018)

	Mean	Min	Max	25th Pctile	75th Pctile	fraction moderately concentrated	fraction highly concentrated
	Panel A. By Occupation × Region						
Baseline geographical definition: 141 CZs							
HHI (By 3-digit KldB 1988)	4243	34	10000	1357	6250	0.16	0.56
Alternative occupational definition:							
HHI (By 3-digit KldB 2010)	3472	31	10000	950	5000	0.17	0.45
HHI (By 2-digit KldB 1988)	2980	40	10000	779	4286	0.18	0.39
HHI (By 2-digit KldB 2010)	1784	37	10000	446	2081	0.14	0.21
HHI (By 1-digit Blossfeld)	961	25	10000	277	1094	0.09	0.08
Alternative geographical definition:							
HHI (By Kreis)	5246	37	10000	2000	10000	0.15	0.68
HHI (By 258 CZs)	4869	37	10000	1765	10000	0.15	0.64
HHI (By 42 regions)	2916	27	10000	698	4075	0.17	0.37
HHI (By Federal state)	2257	10	10000	422	3001	0.13	0.29
	Panel B. By Industry × Region						
Baseline geographical definition: 141 CZs							
HHI (By 3-digit ISIC Rev.4)	4557	30	10000	1528	7812	0.15	0.61
Alternative industrial definition:							
HHI (By 2-digit ISIC Rev.4)	3365	26	10000	885	5000	0.16	0.45
Alternative geographical definition:							
HHI (By Kreis)	5552	43	10000	2356	10000	0.14	0.72
HHI (By 258 CZs)	5178	34	10000	2000	10000	0.15	0.68
HHI (By 42 regions)	3398	24	10000	797	5000	0.15	0.46
HHI (By Federal state)	2837	8	10000	562	4043	0.14	0.38

Notes: Based on data from the Employee History (BeH). The table shows summary statistics for the labor market Herfindahl-Hirschman Index (HHI) under various market definitions using German matched employer-employee (LIAB) data from the Federal Employment Agency. In the top panel, the baseline is calculated using 141 commuting zones (CZs) for the geographic market definition and 3-digit KldB 1988 codes for the occupational market definition. In the bottom panel, I use industry instead of occupation in the definition of labor market. The baseline is calculated using 141 CZs for the geographic market definition and 3-digit ISIC Rev.4 (WZ2008) industry codes for the industrial market definition. The calculation under alternative market definitions is done by changing the baseline along one dimension. Note that regions are a cluster of kreis (or counties in the U.S.), and there are 42 regions in Germany.

Table A.16: Summary Statistics (NRC, Routine, NRM Workers)

	NRC			Routine			NRM		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Log labor	2.298	1.473	7346	2.905	1.423	8686	1.941	1.436	5229
Labor cost (% revenue)	0.072	0.095	9849	0.158	0.123	9849	0.032	0.077	9849
Daily wage (€)	88.91	57.85	7336	74.25	39.76	8678	67.98	40.83	5225

Notes: The table summarizes the employment, labor cost, and daily wages for workers performing different tasks between 1997-2018. The classification of workers is based on task intensity measures constructed using the BIBB/BAuA Employment surveys. Employment and wage bill information comes from the IAB Establishment Panel, while daily wage comes from the matched employer-employee (LIAB) data. The unit of observation is the firm, and sampling weights are applied. NRC, nonroutine cognitive; NRM, nonroutine manual.

Table A.17: Summary Statistics (High-skilled and Low-skilled Workers)

	High-skilled			Low-skilled		
	Mean	SD	N	Mean	SD	N
Log labor	3.221	1.355	9563	1.979	1.483	6165
Labor cost (% revenue)	0.230	0.131	9957	0.032	0.073	9957
Daily wage (€)	78.83	43.53	9552	44.54	32.00	6157

Notes: The table summarizes the employment, labor cost, and daily wages for workers with different skills between 1997-2018. High-skilled workers have vocational training and university degrees, whereas low-skilled workers have no vocational training. Employment and wage bill information comes from the IAB Establishment Panel, while daily wage comes from the matched employer-employee (LIAB) data. The unit of observation is the firm, and sampling weights are applied.

Table A.18: Test of Relevance Assumption for Robots in All Industries

	Dependent variable: Annual change in aggregate markdowns			
	(1)	(2)	(3)	(4)
ΔPredicted robot exposure	-0.0003	-0.0004	-0.0004	-0.0004
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Montiel Olea-Pflueger weak IV test				
Effective F-statistic (α = 5%)	4.835	4.854	4.860	4.852
Critical value 2SLS (τ = 10%)	22.393	22.492	22.492	22.492
Critical value 2SLS (τ = 20%)	14.527	14.602	14.602	14.602
Critical value 2SLS (τ = 30%)	11.579	11.644	11.644	11.644
Kleibergen-Paap weak ID test	45.668	56.181	56.424	56.503
Hansen's J-stat p-value	0.832	0.779	0.776	0.778
Year fixed effects	✓	✓	✓	✓
Broad region dummies	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Manufacturing share	✓			
Broad industry shares		✓	✓	✓
ΔNet exports in 1,000 euros per worker			✓	✓
ΔICT equipment in 1,000 euros per worker				✓

Notes: N = 4599 local labor market regions-by-year (district-by-year). The table presents results from the IV (2SLS) regressions where the German local labor market's exposure robots in all industries are instrumented by installations of all robots in other high-income European countries. The table also tests the inclusion restriction or relevance assumption in this case using [191]'s and [154]'s weak IV tests. All specifications control for constant, broad region dummies, year fixed effects, and demographic characteristics of districts or kreise in the previous period. The broad region dummies indicate if the region is located in the north, west, south, or east of Germany. The demographic controls are constructed using the matched employer-employee data (LIAB) and include the share of females, the share of foreigners, the share of workers over 50 years old, the shares of workers with no vocational training, vocational training, and university degree, and employment shares across industries. The manufacturing share represents the employment share of manufacturing workers in total employment. Broad industry shares are the shares of workers in nine broad industry groups (agriculture, food products, consumer goods, industrial goods, capital goods, construction, consumer-related services, business-related services, and public sector). Exposure to net exports and ICT equipment is measured by the annual change in German net exports vis-à-vis China and 21 Eastern European countries (in 1,000 euros per worker) and by the annual change in German ICT equipment (in 1,000 euros per worker), respectively. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.19: Plant-Level Effects of Robot Exposure on Employment of Heterogeneous Workers in East and West Germany

	Dependent variable: Annual % change in plant-level employment		
	Routine	Nonroutine manual	Nonroutine cognitive
	(1)	(2)	(3)
	Panel A. East Germany		
ΔPredicted robot exposure	-0.016	-0.013	0.022
	(0.004)	(0.006)	(0.006)
N	3649	3649	3649
	Panel B. West Germany		
ΔPredicted robot exposure	-0.001	-0.017	0.006
	(0.008)	(0.014)	(0.008)
N	3823	3823	3823
Firm characteristics	✓	✓	✓
Regional demographics	✓	✓	✓
Firm fixed effects	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓

Notes: Panel A presents the results from estimating the annual percentage change in employment at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in East Germany between 1998 and 2018 using the IV (2SLS) regressions. Panel B reports the results from the 2SLS IV regressions for West Germany. In both panels, the dependent variable is the annual percentage change in plant-level employment of routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications include the same set of controls and fixed effects as in Table 2.33. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.20: Plant-Level Effects of Robot Exposure on Employment of Heterogeneous Workers in Districts from East and West Germany with Different Union Coverage

	Dependent variable: Annual % change in plant-level employment					
	Bottom 8 deciles			Top 2 deciles		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	-0.012	-0.012	0.005	-0.013	-0.023	0.076
	(0.006)	(0.007)	(0.008)	(0.015)	(0.028)	(0.024)
N	3149	3149	3149	224	224	224
	Panel B. West Germany					
ΔPredicted robot exposure	0.008	0.001	-0.015	-0.008	0.000	0.003
	(0.009)	(0.014)	(0.017)	(0.006)	(0.004)	(0.004)
N	3273	3273	3273	182	182	182
Firm characteristics	✓	✓	✓	✓	✓	✓
Regional demographics	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The left sub-panel of Panel A presents the results from estimating the annual percentage change in employment at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in districts from East Germany whose union coverage is in the bottom eight deciles of the distribution in the previous period between 1998 and 2018 using the IV (2SLS) regressions. The right sub-panel of Panel A reports the results from the IV (2SLS) regressions for plants in districts from East Germany with high union coverage (i.e., districts in the top two deciles of the distribution of district-level union coverage). Panel B's left and right sub-panels show the corresponding results for West Germany. In all panels, the dependent variable is the annual percentage change in plant-level employment of routine workers (columns (1) and (4)), nonroutine manual–NRM workers (columns (2) and (5)), and nonroutine cognitive–NRC workers (columns (3) and (6)). All specifications include the same set of controls and fixed effects as in Table 2.33. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.21: Plant-Level Effects of Robot Exposure on Wages of Heterogeneous Workers in East and West Germany

	Dependent variable: Annual % change in plant-level average wage		
	Routine (1)	Nonroutine manual (2)	Nonroutine cognitive (3)
	Panel A. East Germany		
ΔPredicted robot exposure	-0.005	-0.001	-0.006
	(0.003)	(0.004)	(0.007)
N	3649	3649	3649
	Panel B. West Germany		
ΔPredicted robot exposure	-0.005	0.013	0.016
	(0.010)	(0.015)	(0.015)
N	3823	3823	3823
Firm characteristics	✓	✓	✓
Regional demographics	✓	✓	✓
Firm fixed effects	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓

Notes: Panel A presents the results from estimating the annual percentage change in average wage at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in East Germany between 1998 and 2018 using the IV (2SLS) regressions. Panel B reports the results from the 2SLS IV regressions for West Germany. In both panels, the dependent variable is the annual percentage change in the average wage of routine (column (1)), nonroutine manual (column (2)), and nonroutine cognitive (column (3)) workers. All specifications include the same set of controls and fixed effects as in Table 2.34. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.22: Plant-Level Effects of Robot Exposure on Wages of Heterogeneous Workers in Districts from East and West Germany with Different Union Coverage

	Dependent variable: Annual % change in plant-level average wage					
	Bottom 8 deciles			Top 2 deciles		
	Routine (1)	NRM (2)	NRC (3)	Routine (4)	NRM (5)	NRC (6)
	Panel A. East Germany					
ΔPredicted robot exposure	-0.001	-0.004	0.011	-0.011	0.009	-0.025
	(0.007)	(0.005)	(0.007)	(0.018)	(0.025)	(0.018)
N	3149	3149	3149	224	224	224
	Panel B. West Germany					
ΔPredicted robot exposure	-0.002	-0.005	0.035	0.001	-0.003	0.015
	(0.020)	(0.012)	(0.016)	(0.006)	(0.003)	(0.011)
N	3273	3273	3273	182	182	182
Firm characteristics	✓	✓	✓	✓	✓	✓
Regional demographics	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
State-by-Year fixed effects	✓	✓	✓	✓	✓	✓
Industry-by-Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: The left sub-panel of Panel A presents the results from estimating the annual percentage change in average wage at the plant on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers in districts from East Germany whose union coverage is in the bottom eight deciles of the distribution in the previous period between 1998 and 2018 using the IV (2SLS) regressions. The right sub-panel of Panel A reports the results from the IV (2SLS) regressions for plants in districts from East Germany with high union coverage (i.e., districts in the top two deciles of the distribution of district-level union coverage). Panel B's left and right sub-panels show the corresponding results for West Germany. In all panels, the dependent variable is the annual percentage change in the average wage of routine workers (columns (1) and (4)), nonroutine manual–NRM workers (columns (2) and (5)), and nonroutine cognitive–NRC workers (columns (3) and (6)). All specifications include the same set of controls and fixed effects as in Table 2.34. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.23: Heterogeneous Effects of Robot Exposure on Plant-Level Markdowns by Firm Size

	Dependent variable: Annual change in plant-level markdowns			
	All workers	Heterogeneous workers		
		Routine	NRM	NRC
	(1)	(2)	(3)	(4)
	Panel A. Small firms			
ΔPredicted robot exposure	0.001	-0.000	0.020	-0.002
	(0.008)	(0.008)	(0.011)	(0.012)
N	4833	4833	4833	4833
	Panel A. Large firms			
ΔPredicted robot exposure	0.015	0.015	0.008	-0.003
	(0.020)	(0.021)	(0.015)	(0.010)
N	3714	3714	3714	3714

Notes: The table presents the results from estimating the annual change in plant-level markdowns on the annual change in the local labor market's predicted exposure to robots in the automotive industry per 1,000 workers for small (top panel) and large (bottom panel) firms between 1998 and 2018 using the IV (2SLS) regressions. Small firms are those in the bottom 7 deciles of the size distribution in the previous period, while large firms are plants in the top 3 deciles. Column (1) shows the effects for all workers. Columns (2)-(4) report the effects of automation exposure on the markdowns over heterogeneous workers performing different tasks, and the dependent variable is the annual change in the markdowns over routine workers (column (2)), nonroutine manual–NRM workers (column (3)), and nonroutine cognitive–NRC workers (column (4)). All specifications control for constant and demographic characteristics of districts or kreise in the previous year. The firm, state-by-year, and industry-by-year fixed effects are also controlled in each specification. Standard errors clustered at the level of local labor markets or districts are in parentheses.

Table A.24: Relationship between Robot Exposure, Robot Exposure Predicted from the First-Stage of 2SLS, and Actual Robot Adoption

	(1)	(2)	(3)
	Panel A. Dependent variable: ΔRobot exposure		
Robot exposure predicted from the first-stage	0.630	0.350	0.362
	(0.054)	(0.063)	(0.063)
N	1023	1021	1011
R^2	0.41	0.77	0.80
	Panel B. Dependent variable: ΔActual robot adoption		
ΔRobot exposure predicted from the first-stage	0.013	-0.035	-0.051
	(0.074)	(0.060)	(0.057)
N	815	811	803
R^2	0.04	0.49	0.52
Year fixed effects	✓	✓	
State fixed effects	✓		
District fixed effects		✓	✓
State-by-Year fixed effects			✓

Notes: The table presents the results from OLS regressions estimating the relationship between the annual change in robot exposure predicted from the first stage of the 2SLS estimation and annual change in robot exposure defined by equation (2.5) (top panel) and annual change in actual robot adoption (bottom panel) in Germany between 2015 and 2018. In this table, robots in all industries are considered. The first-stage regression controls for instruments and covariates in equation (2.4). The actual robot adoption is measured by aggregating the number of robots adopted by the firm at the district level using sampling weights provided in the IAB Establishment Panel data and expressed as per 1,000 workers. Standard errors clustered by districts are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

APPENDIX B

CHAPTER 2 OF APPENDIX

B.1 Additional Results on Markdowns

B.1.1 Markdowns under Cobb-Douglas Specification

As an alternative to our baseline functional form of the translog production function, we estimate the production function and thus markdowns using Cobb-Douglas specification. Table B.1 presents the mean and median estimates of plant-level markdowns under the assumption of a Cobb-Douglas production function. The estimated markdowns are higher than our baseline estimates, but it verifies the presence of labor market power in India's manufacturing industry.

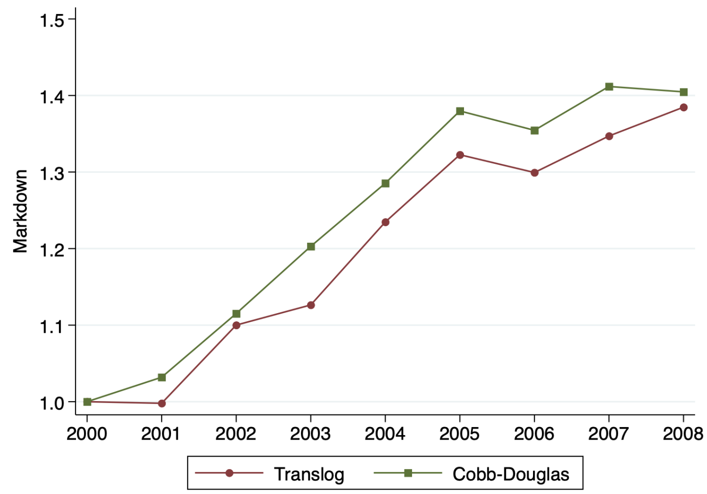
Table B.1: Estimated Plant-Level Markdowns in India's Manufacturing under Cobb-Douglas Specification

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Basic metals	2.425	4.166	4.438	5.203	5396
Food products and beverages	2.212	3.345	2.744	3.899	13731
Paper and paper products	1.887	2.365	1.776	1.774	2690
Rubber and plastics products	1.654	2.241	2.045	1.936	3278
Tobacco products	1.606	5.718	7.275	8.315	2047
Chemicals and chemical products	1.573	2.533	2.223	3.031	10759
Wood and of products of wood and cork, except furniture	1.507	2.307	1.769	2.500	1717
Electrical machinery and apparatus	1.456	2.614	2.002	3.715	4149
Machinery and equipment	1.287	1.917	1.562	2.188	7348
Fabricated metal products, except machinery and equipment	1.147	1.666	1.403	1.655	3894
Textiles	1.044	1.517	1.328	1.612	10594
Furniture	0.999	1.638	1.388	2.025	2565
Other transport equipment	0.955	1.515	1.193	1.904	2194
Leather and related products	0.947	1.432	1.169	1.964	1971
Coke, refined petroleum products, and nuclear fuel	0.923	1.236	1.195	1.247	1083
Other non-metallic mineral products	0.861	1.481	1.514	1.799	9311
Motor vehicles, trailers, and semi-trailers	0.721	1.011	0.841	0.891	3224
Publishing, printing, and reproduction of recorded media	0.702	1.183	1.117	1.455	1440
Medical, precision, and optical instruments, watches and clocks	0.649	1.014	0.841	1.151	2019
Radio, television, and communication equipment and apparatus	0.555	0.935	0.813	1.157	1512
Office, accounting, and computing machinery	0.259	0.595	0.348	1.569	212
Wearing apparel	0.095	0.131	0.109	0.122	1835
Whole sample	**1.310**	**2.240**	**1.955**	**3.144**	**92969**

Notes: Markdowns are estimated for 34,575 unique manufacturing establishments using the ASI data from 2000-2008 under the assumption of a Cobb-Douglas 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.

Figure B.1 illustrates the time evolution of (normalized) aggregate markdowns under the Cobb-Douglas and translog specifications. The markdown trends under two different functional forms are remarkably similar, suggesting that the patterns are not subject to specific function form assumptions.

Figure B.1: Time Evolution of the Aggregate Markdowns under Translog and Cobb-Douglas Specifications



Notes: The plant-level markdowns are constructed using the ASI data from 2000-2008 under the assumptions of translog and Cobb-Douglas 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).

B.1.2 Markups

Table B.2 reports the estimated markups. While our markup estimates should be interpreted cautiously [155, 50], we find that manufacturers have about 37 percent (31 percent) of market power in the product market at the mean (median). Compared to markdowns, markups have less variation, with an IQR of 24 percent and a standard deviation of 28 percent.

Table B.2: Estimated Plant-Level Markups in India's Manufacturing

Industry Group	Median	Mean	$IQR_{75\text{-}25}$	SD	N
Tobacco products	2.071	2.175	0.621	0.472	2047
Wearing apparel	1.610	1.600	0.181	0.139	1835
Medical, precision, and optical instruments, watches and clocks	1.607	1.647	0.373	0.263	2019
Office, accounting, and computing machinery	1.495	1.504	0.252	0.236	212
Chemicals and chemical products	1.463	1.490	0.219	0.257	10759
Radio, television, and communication equipment and apparatus	1.413	1.443	0.261	0.198	1512
Other non-metallic mineral products	1.408	1.485	0.258	0.266	9311
Publishing, printing, and reproduction of recorded media	1.396	1.429	0.218	0.201	1440
Motor vehicles, trailers, and semi-trailers	1.377	1.394	0.159	0.145	3224
Machinery and equipment	1.352	1.379	0.249	0.181	7348
Electrical machinery and apparatus	1.334	1.358	0.199	0.138	4149
Furniture	1.287	1.318	0.163	0.154	2565
Fabricated metal products, except machinery and equipment	1.270	1.291	0.160	0.250	3894
Paper and paper products	1.264	1.284	0.070	0.086	2690
Rubber and plastics products	1.264	1.287	0.164	0.129	3278
Textiles	1.253	1.284	0.112	0.127	10594
Leather and related products	1.234	1.246	0.120	0.096	1971
Coke, refined petroleum products and nuclear fuel	1.232	1.271	0.121	0.351	1083
Other transport equipment	1.232	1.265	0.206	0.156	2194
Food products and beverages	1.200	1.245	0.174	0.319	13731
Wood and of products of wood and cork, except furniture	1.196	1.229	0.080	0.129	1717
Basic metals	1.160	1.189	0.146	0.103	5396
Whole sample	**1.308**	**1.368**	**0.238**	**0.278**	**92969**

Notes: Markups are estimated 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.

Figure B.2 illustrates the trend of aggregate markup as it could be informative, although the markup estimate is biased. We find that aggregate markups presented about a 7% drop between 2000 and 2001 and leveled off since then.

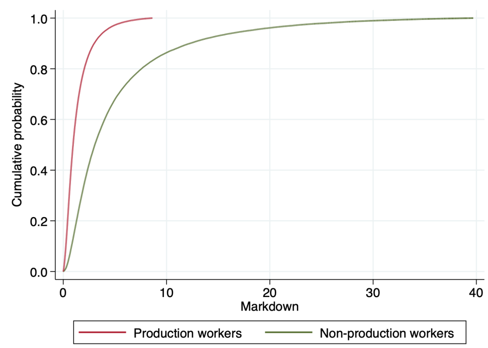
Figure B.2: Time Evolution of the Aggregate Markup

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

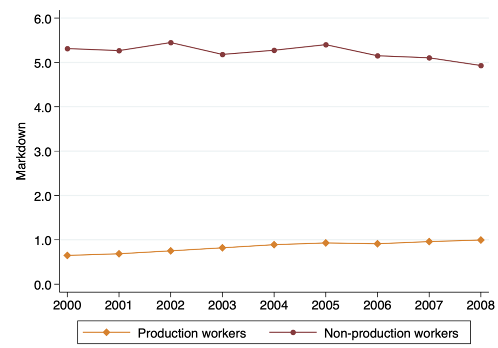
B.1.3 Additional Results on Markdowns for Heterogeneous Workers

Figure B.4(a) illustrates the distribution of markdowns for production and non-production workers measured by headcount, our baseline measure of labor input, clearly showing that non-production workers stochastically dominate production workers regarding markdowns. Then, we aggregate the plant-level markdowns at the year level and plot the trends of markdowns for these workers in Figure B.4(b). The production workers' markdowns present an upward trend, while markdowns for non-production workers were stable between 2000 and 2008.

Figure B.3: Markdowns for Production and Non-Production Workers



(a) Cumulative Probability Distribution

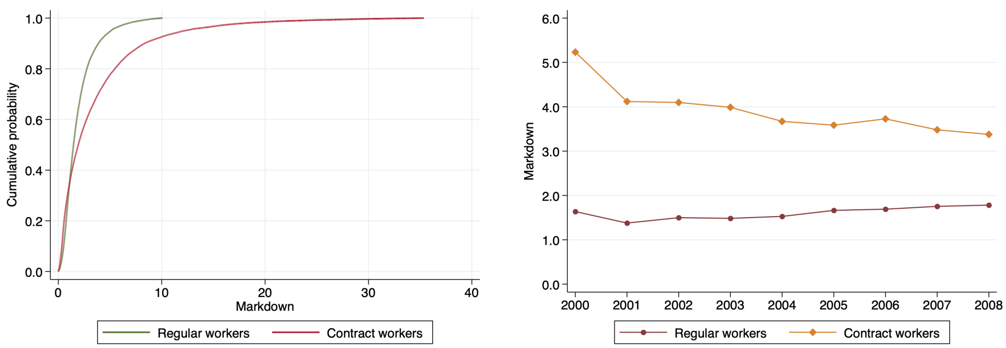


(b) Aggregate Markdown Trends

Notes: Panel (a) plots the cumulative distribution function (CDF) of markdowns for production (non-managers or low-skilled) and non-production (managers or high-skilled) workers in India's manufacturing. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of translog production where heterogeneous labor inputs. The production function is estimated separately for each two-digit industry group, and labor inputs are measured by headcount. In panel (b), 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).

On the other hand, markdowns for contract workers stochastically dominate markdowns for regular workers along the distribution (Figure B.5(a)). In terms of evolution over time, as shown in Figure B.5(b), aggregate markdowns for contract workers depict a downward trend, while markdowns for regular workers have been growing over the study period.

Figure B.4: Markdowns for Regular and Contract Workers



(a) Cumulative Probability Distribution (b) Aggregate Markdown Trends

Notes: Panel (a) plots the cumulative distribution function (CDF) of markdowns for regular and contract workers in India's manufacturing. The plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of translog production where heterogeneous labor inputs. The production function is estimated separately for each two-digit industry group, and labor inputs are measured by the headcount of workers hired directly (regular workers) and employed through contractors (contract workers). In panel (b), 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).

B.2 Labor Market Reforms and Wage Markdowns

This section evaluates the empirical relationship between reforms in labor laws and labor market power in India's manufacturing industry.

B.2.1 Empirical Specification

We estimate the following equation

$$\text{Markdown}_{st} = \alpha + \beta \text{Reform}_{st} + \pi_s + \mu_t + \varepsilon_{st}, \tag{B.1}$$

where Markdown_{st} is the aggregate markdown for state s in year $t \in [2000, 2008]$ and Reform_{st} is the reforms of labor laws in state s on the intensive margin. The plant-level markdowns are aggregated at the state-by-year level using employment weights. Since the labor reforms are defined at the state level, we include state fixed effects, π_s, and year fixed effects, μ_t. The standard errors are clustered at the state level [46].

B.2.2 Data

The data on reforms of labor laws are obtained from [82], who refined state-level measures of labor regulations proposed by [47] and extended the data between 1947 and 2017. The labor acts are categorized into three broad groups: (i) pro-worker, (ii) pro-employer, and (iii) neutral. In addition to these three individual indicators, two aggregate indicators have been proposed. First, the composite index or score of the Besley-Burgess index is defined as follows

$$\text{BB score} = 1\times\text{No. pro-worker acts} - 1\times\text{No. pro-employer acts} + 0\times\text{No. neutral acts}, \tag{B.2}$$

where the components are the three individual indicators on the intensive margin. Given the definition of the BB score, it measures how friendly the labor regulations are for workers. So, if the state has more pro-employer acts than pro-worker acts, the BB score takes a negative value. The second aggregate indicator counts the total number of acts without considering whether the change is pro-workers, pro-employers, or neutral. Therefore, five indicators determine the state and the development of labor regulations in India. There have been reforms of labor regulations in 19 states of India. However, we dropped West Bengal from this analysis as an outlier since its cumulative score of the Besley-

Burgess index is about six times larger than the score for other states.

B.2.3 Results

Table B.3 presents the results from estimating the association between aggregate markdowns and labor market reforms using equation (B.1). The cumulative score of the Besley-Burgess (BB) index measures the friendliness of the labor market to workers. Intuitively, we find that our aggregate markdown, a measure of employer power, is negatively associated with the cumulative score of the BB index. However, the relationship is statistically significant at the 5% level (Column (1)). The labor market acts in favor of workers or pro-worker acts that protect workers in the labor market are associated with lower employer power; however, the relationship is essentially zero (Column (2)). The pro-employer acts, however, are associated with higher employer power, and the positive relationship is also statistically significant at the 5% level (Column (3)). Consistent with the expectation, the neutral acts are not correlated with the markdowns (Column (4)). We also fail to find a significant relationship between the total acts and aggregate markdowns (Column (5)), potentially indicating a heterogeneity of the relationship.

Table B.3: Relationship between State-Level Markdowns and Labor Market Reforms

	Dependent variable: State-level markdowns				
	(1)	(2)	(3)	(4)	(5)
Cumulative score	-0.018**				
	(0.008)				
Cumulative pro-worker acts		-0.009			
		(0.041)			
Cumulative pro-employer acts			0.020**		
			(0.007)		
Cumulative neutral acts				-0.007	
				(0.017)	
Cumulative total acts					0.010
					(0.010)
N	153	153	153	153	153
R^2	0.87	0.87	0.87	0.87	0.87

Notes: The table presents the relationship between aggregate markdowns and labor market reforms measured by cumulative Besley-Burgess (BB) indicators at the state level. The plant-level markdowns are aggregated at the state level using employment weights. All regressions control for state and year fixed effects. The standard errors clustered by states are in parenthesis. Significance: $^*p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

In Table B.4, we further examine the relationship by splitting the labor market reforms in the current year from the cumulative changes by the previous year. The results are generally the same as those in Table B.3 in the signs, statistical significance, and the magnitude of the relationship. Overall, these findings suggest that the aggregate markdowns are generally consistent with changes in the labor market laws based on the signs of the estimated coefficients, validating our estimated measure of labor market power. The relationships tend to be weakly significant, and it could be due to the ineffectiveness of the reforms, an interesting question that is not our focus in this paper.

Table B.4: Relationship between State-Level Markdowns and Current and Previous Labor Market Reforms

	Dependent variable: State-level markdowns				
	(1)	(2)	(3)	(4)	(5)
BB Score	-0.021**				
	(0.008)				
Cumulative score (one-year lagged)	-0.016*				
	(0.009)				
Pro-worker acts		-0.023			
		(0.038)			
Cumulative pro-worker acts (one-year lagged)		-0.002			
		(0.040)			
Pro-employer acts			0.021***		
			(0.006)		
Cumulative pro-employer acts (one-year lagged)			0.019**		
			(0.009)		
Neutral acts				-0.025	
				(0.018)	
Cumulative neutral acts (one-year lagged)				-0.001	
				(0.016)	
Total acts					0.005
					(0.011)
Cumulative total acts (one-year lagged)					0.012
					(0.010)
N	153	153	153	153	153
R^2	0.87	0.87	0.87	0.87	0.87

Notes: The table presents the relationship between aggregate markdowns and labor market reforms measured by cumulative Besley-Burgess (BB) indicators at the state level. The plant-level markdowns are aggregated at the state level using employment weights. All regressions control for state and year fixed effects. The standard errors clustered by states are in parenthesis. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

B.3 Additional Results on Heterogeneous Effects and Mechanisms

Section 3.7.2 discusses our baseline results on heterogeneous effects to examine the mechanisms. This appendix provides additional results to investigate the mechanisms further by analyzing the impact on worker flows.

B.3.1 Effects on Worker Flows

Since we find that the creation of non-manufacturing jobs in rural areas under the NREGA crowds out employment at certain firms in the manufacturing industry, i.e., a reduction in the number of workers, we examine the effects on worker flows or turnover at manufacturing firms.

The annual ASI Volume I data used in the baseline analysis does not contain information on workers' flow at the firm over time. Fortunately, the ASI Volume II data reports the stock and flow of regular workers directly employed (i.e., not contract workers) in each month throughout the financial year, such as the number of workers in employment on the first and last day of the month, hiring or addition of workers during the month, and separation of workers during the month due to death or retirement and other reasons. Using this monthly data from the ASI Volume II, we compute the annual workers' flow (net change, addition, and separation) and match the data with ASI Volume I data, which is panel but different from the version used in our baseline analysis. Thus, we re-estimate the wage markdowns using this new ASI data that spans between 2001 and 2008 to determine if the data is comparable to that used in our baseline analysis. As shown in Table B.5, the median and average markdowns estimated over 2002-2008[1] are 1.387 and 1.899, respectively, generally consistent with our baseline estimates in Table 3.2. The markdown estimates are slightly higher than the baseline estimates, which is intuitive due to the growing trend between 2000 and 2008, as shown in Figure 3.3.

[1]The year 2001 has been excluded since the production function uses the lagged information as an instrument to identify the production parameters.

Table B.5: Estimated Plant-Level Markdowns in India's Manufacturing using Different ASI data

	(1) Median	(2) Mean	(3) $IQR_{75\text{-}25}$	(4) SD	(5) N
Markdowns	1.387	1.899	1.664	1.628	65310

Notes: The table presents plant-level markdowns estimated using the volume I of ASI data matched with volume II between 2002-2008. The production function was estimated under the assumption of translog specification with headcount as a measure of labor input.

We first estimate the treatment effects on worker flows during a year. The results in Table B.6 suggest that the impacts are essentially zero at the baseline. As shown in Column (5), the firm's age is intuitively associated with greater separation of workers due to death or retirement. We focus on separations due to reasons other than death and retirement, which are not directly related to the program under investigation. Although statistically insignificant, total separation and separation of workers due to non-death and non-retirement reasons increased in response to the introduction of NREGA jobs.

Table B.6: Effects of NREGA on Flow of Regular Workers

	(1) Net hiring	(2) Hiring	(3) Total separation	(4) Separation due to death or retirement	(5) Separation due to other causes
Post-NREGA	0.041	0.086	0.041	-0.016	0.076
	(0.079)	(0.078)	(0.062)	(0.030)	(0.067)
Firm age	0.004	-0.000	0.003	0.004**	-0.000
	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)
Firm age^2	-0.000	-0.000	-0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rainfall	-0.569	0.023	0.502	0.030	0.434
	(0.535)	(0.526)	(0.451)	(0.222)	(0.488)
N	33100	47749	47943	47943	47943
R^2	0.64	0.70	0.76	0.72	0.73

Notes: Based on the ASI data (volumes I and II) from 2002-2008 on which markdown has been estimated. The dependent variable in Columns (1)-(5) is the net hiring (addition minus separation), hiring or addition, total separation, separation due to death or retirement, and separation due to other causes, respectively. These outcomes are in log terms, and a constant 1 has been added before taking logs. All regressions include an unreported constant term and baseline fixed effects. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Since the average effects are not statistically significant, we estimate the heterogeneous effects by labor productivity. Panel A of Table B.7 presents the results. At firms with low labor productivity, the gross and net hiring decrease, while the separation of workers due to causes other than death or retirement and the total separation increase. However, these effects are still not statistically significant and are not precisely estimated under this heterogeneity.

In Section 3.7.2, we also estimate heterogeneous effects along the wage distribution and show that the employment, wage, and MRPL impacts are concentrated among low-paying firms (Table 3.9). So, we also estimate the heterogeneous effects along the wage distribution and present the results in Panel B of Table B.7. The program still leads workers to separate from low-paying firms due to causes unrelated to death or retirement; however, the program negatively affects the total separation. Despite these, we find that such firms reduce their hiring in total and on the net, and these effects are statistically significant at the 5% level.

Overall, these findings from heterogeneity by firm's initial labor productivity and wage distribution inform that employment reduction that we identified at low-labor productivity and low-paying firms are mainly driven by reductions in additional hiring and thus in net hiring. It indicates a compositional change in the manufacturing firms' employment, contributing to a change in wage markdown over their workers.

Table B.7: Heterogeneous Effects of NREGA on Flow of Regular Workers by Labor Productivity

	(1) Net hiring	(2) Hiring	(3) Total separation	(4) Separation due to death or retirement	(5) Separation due to other causes
	Panel A. Heterogeneity by labor productivity				
Post-NREGA × Below median	-0.110	-0.129	0.002	-0.059	0.048
	(0.075)	(0.107)	(0.070)	(0.043)	(0.078)
Below median	0.069	0.046	0.040	-0.010	0.041
	(0.063)	(0.065)	(0.057)	(0.034)	(0.060)
Post-NREGA	0.087	0.139*	0.040	0.008	0.057
	(0.084)	(0.083)	(0.071)	(0.030)	(0.078)
N	33100	47749	47943	47943	47943
R^2	0.64	0.70	0.76	0.72	0.73
	Panel B. Heterogeneity along wage distribution				
Post-NREGA × Low-wage firm	-0.185**	-0.229**	-0.013	-0.053	0.029
	(0.089)	(0.102)	(0.089)	(0.038)	(0.093)
Low-wage dummy	-0.000	0.102	0.122**	-0.004	0.113*
	(0.067)	(0.066)	(0.059)	(0.020)	(0.058)
Post-NREGA	0.099	0.139	0.024	-0.001	0.048
	(0.090)	(0.090)	(0.071)	(0.035)	(0.077)
N	31781	46302	46495	46495	46495
R^2	0.64	0.69	0.75	0.72	0.73

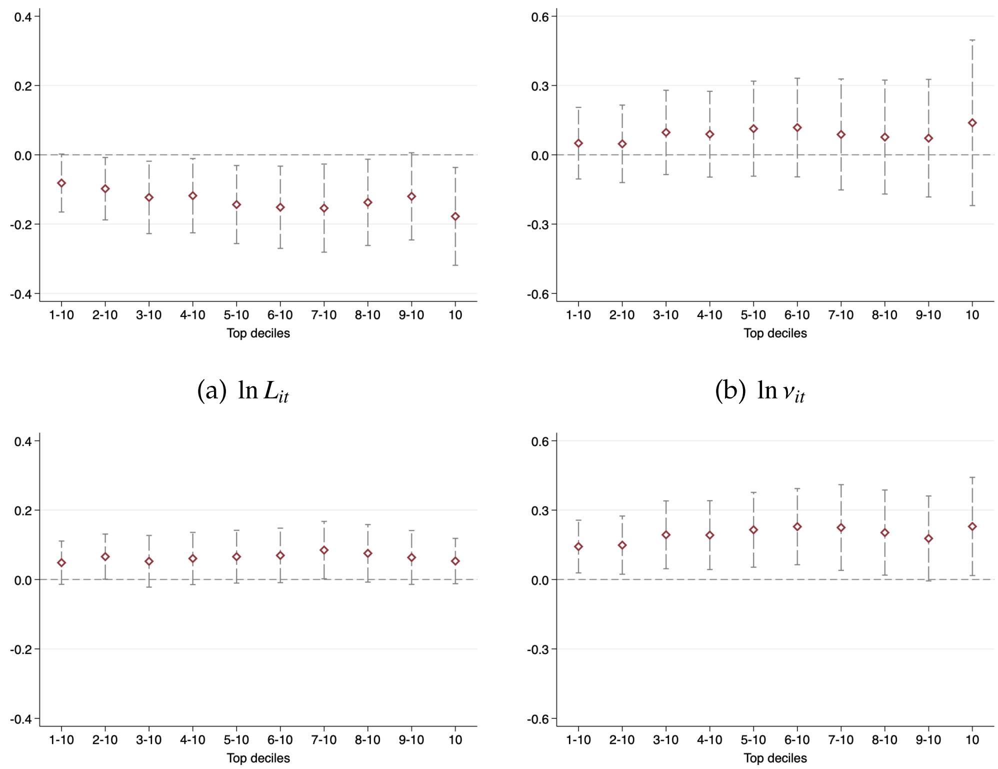
Notes: Based on the ASI data (volumes I and II) from 2002-2008 on which markdown has been estimated. In panel A, the key explanatory variable in each column is the NREGA treatment variable interacted with a dummy, indicating whether the firm's labor productivity is below the median. In panel B, the key explanatory variable in each column is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial labor average wage per worker is in the bottom 3 deciles of the wage distribution. The dependent variable in Columns (1)-(5) is the net hiring (addition minus separation), hiring or addition, total separation, separation due to death or retirement, and separation due to other causes, respectively. These outcomes are in log terms, and a constant 1 has been added before taking logs. 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 ***$p < 0.01$.

B.4 Additional Robustness Checks

B.4.1 Robustness of Heterogeneous Effects by Minimum Wage Enforcement

In Section 3.7.2, we discuss the average effects of NREGA on labor market conditions in manufacturing heterogeneous by minimum wage level and enforcement of minimum wage.

Figure B.5: Heterogeneous Effects of NREGA by Minimum Wage and Its Enforcement using Quartiles of Wage-to-Minimum Wage Ratio



(a) $\ln L_{it}$

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

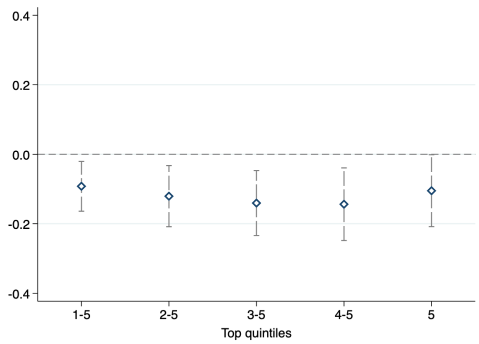
(c) $\ln W_{it}$

(d) $\ln MRPL_{it}$

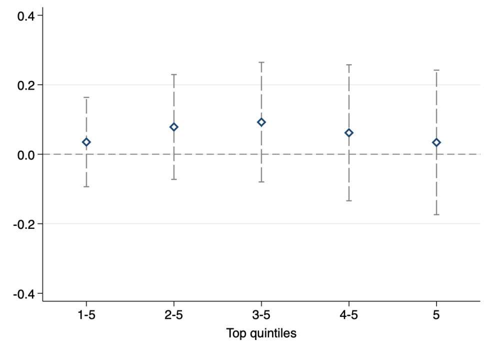
Notes: The figure presents the effects of NREGA on labor market outcomes at manufacturing firms heterogeneous by minimum wage and its enforcement. The key explanatory variable is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial average wage-to-minimum wage ratio is in the bottom quartile. 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 subsample 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.

Figure B.5 checks the robustness of our baseline results by using firms in the bottom quartile of the wage-to-minimum wage ratio distribution as firms with low ratios, while the baseline analysis uses firms in the bottom 3 deciles. The sample on which each specification is estimated is the same as the baseline analysis, i.e., firms in different deciles of enforcement distribution.

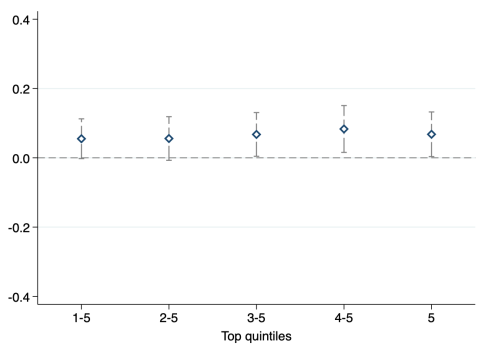
Figure B.6: Heterogeneous Effects of NREGA by Minimum Wage and Its Enforcement using Quintiles of Minimum Wage Enforcement



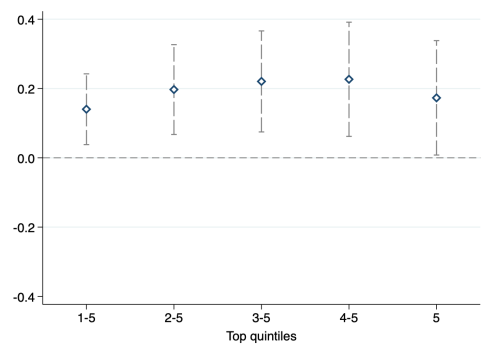
(a) $\ln L_{it}$



(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 quintiles of the minimum wage enforcement (inspections per worker) distribution. For example, the sample of firms labeled "1-5" refers to those in the 1st through the 5th quintiles of the distribution, i.e., all firms, and the sample labeled "2-5" refers to a sub-sample of firms in the 2nd through the 5th quintiles. 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.

In Figure B.6, however, we change the sample of firms in states with different minimum wage enforcement to quintiles instead of using deciles. We employ our baseline definition of low wage-to-minimum wage based on the bottom 3 deciles in these regressions. The results in this section suggest that our results on the heterogeneous effects by minimum wage and its enforcement are robust.

B.4.2 Robustness of Main Results to Using Manufacturing Mandays

In Section 3.8.3, we check the robustness of our main findings using total mandays worked at manufacturing firms. The ASI data provides information on mandays by separating manufacturing from non-manufacturing mandays. Since information on non-manufacturing mandays is severely limited and the plants covered in the ASI data are manufacturing firms, we are particularly interested in manufacturing mandays worked. This appendix thus examines the robustness of our main results using manufacturing mandays as an additional measure of employment. The labor market effects of NREGA heterogeneous by labor productivity, shown in Table B.8, indicate that the findings are substantially robust.

Table B.8: Heterogeneous Effects of NREGA by Labor Productivity using Manufacturing Mandays

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln MRPL_{it}$
		Panel A. Below median	
Post-NREGA	-0.066**	0.065***	0.084***
	(0.026)	(0.019)	(0.021)
N	31041	31041	31041
R^2	0.97	0.90	0.90
		Panel B. Above median	
Post-NREGA	0.026	-0.011	-0.024
	(0.026)	(0.021)	(0.027)
N	34654	34654	34654
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 manufacturing mandays worked. The plant-level markdowns are estimated using the ASI data from 2001-2008 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 ***$p < 0.01$.

Table B.9 presents the treatment effects on labor market conditions for production and non-production workers heterogeneous by labor productivity. The effects are concentrated among production workers at firms with low labor productivity, consistent with our results using labor headcount and total mandays worked.

Table B.9: Heterogeneous Effects of NREGA on Production and Non-Production Workers by Labor Productivity using Manufacturing Mandays

	Production workers			Non-production workers		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$
	Panel A. Below median					
Post-NREGA	-0.100∗∗∗	0.096∗∗∗	0.107∗∗∗	-0.032	0.058∗	0.067∗∗∗
	(0.026)	(0.029)	(0.026)	(0.028)	(0.033)	(0.025)
N	27827	27827	27827	27827	27827	27827
R^2	0.97	0.89	0.90	0.93	0.86	0.89
	Panel B. Above median					
Post-NREGA	0.004	-0.004	-0.024	0.015	-0.006	-0.040
	(0.031)	(0.033)	(0.039)	(0.030)	(0.035)	(0.025)
N	29242	29242	29242	29242	29242	29242
R^2	0.95	0.86	0.84	0.93	0.81	0.85

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for production and non-production workers 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 for production workers, respectively. The dependent variable in Columns (4)-(6) is the same outcomes for non-production workers. The employment and labor input in production function estimation and the calculation of markdown and MRPL is measured by manufacturing mandays worked. The plant-level markdowns are estimated using the ASI data from 2001-2008 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 ***$p < 0.01$.

Furthermore, we evaluate the labor market impacts for regular and contract workers using manufacturing mandays as an employment measure. Our baseline results show that the shock mainly affected the regular workers at firms with low labor productivity. The estimated effects, shown in Table B.10, indicate generally the same findings. The statistical significance of the estimated coefficients is more substantial, and the effects are more precisely estimated for regular workers.

Table B.10: Heterogeneous Effects of NREGA on Regular and Contract Workers by Labor Productivity using Manufacturing Mandays

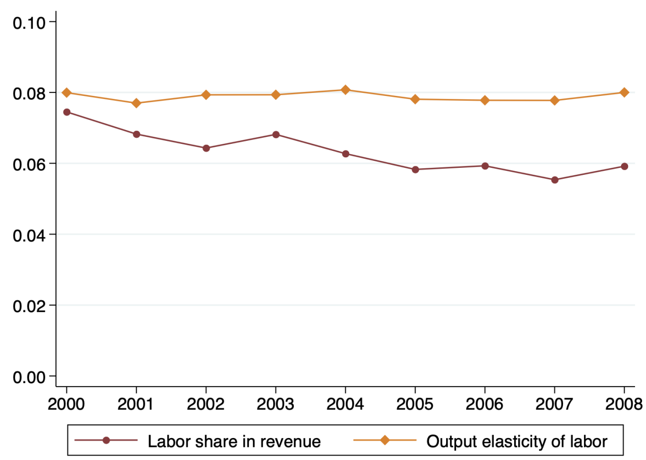
	Regular workers			Contract workers		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$
	Panel A. Below median					
Post-NREGA	-0.121**	0.106**	0.158***	-0.121*	0.106	0.125*
	(0.055)	(0.052)	(0.052)	(0.071)	(0.070)	(0.072)
N	8025	8025	5996	8025	8025	8025
R^2	0.98	0.88	0.91	0.92	0.91	0.93
	Panel B. Above median					
Post-NREGA	-0.007	0.055*	0.075**	0.105*	-0.067	-0.057
	(0.032)	(0.032)	(0.034)	(0.061)	(0.062)	(0.048)
N	9111	9111	8738	9111	9111	9111
R^2	0.96	0.87	0.86	0.87	0.84	0.86

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for regular and contract workers 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 for regular workers, respectively. The dependent variable in Columns (4)-(6) is the same outcome for contract workers. The employment and labor input in production function estimation and the calculation of markdown and MRPL is measured by manufacturing mandays worked. The plant-level markdowns are estimated using the ASI data from 2001-2008 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 ***$p < 0.01$.

B.5 Additional Figures and Tables

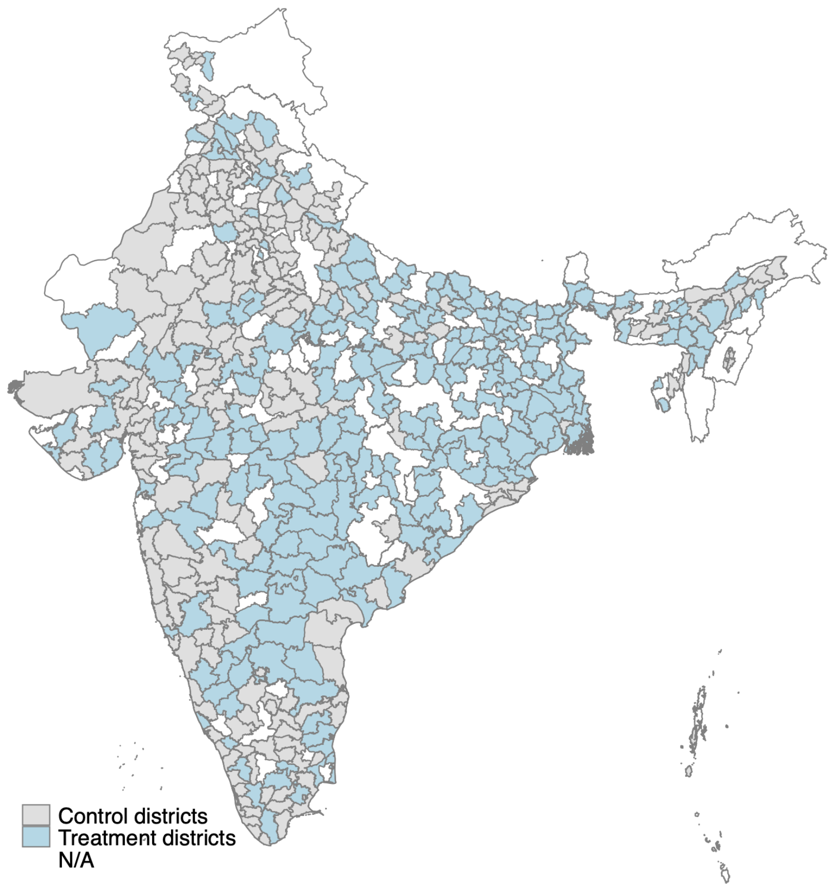
B.5.1 Additional Figures

Figure B.7: Time Evolution of Labor Share and Output Elasticity of Labor



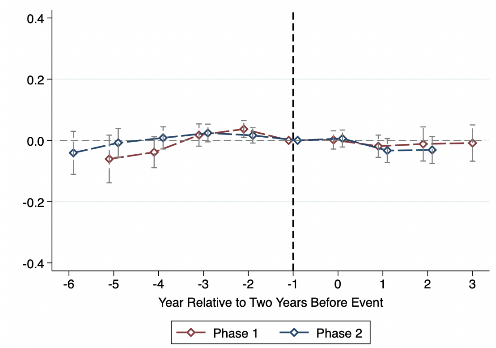
Notes: The figure shows the trends of labor share in revenue and output elasticity of labor. The plant-level measures are aggregated at the year level using employment shares of the labor market (combination of 4-digit NIC-1998 industry and states).

Figure B.8: Treatment and Control Groups

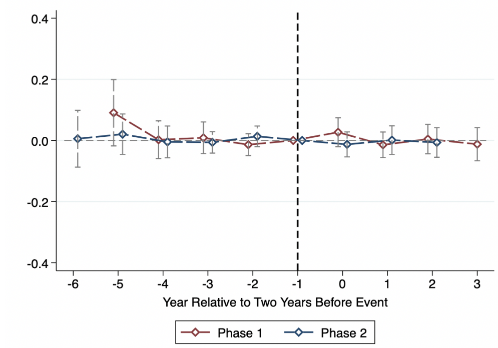


Notes: Based on the sample in which plant-level markdown has been estimated. Treatment districts are districts where the first two phases of the NREGA program have been implemented. Control districts are districts where the third phase of the NREGA program has been implemented. The districts with "N/A" are the ones where plant-level markdown was not estimated using the "production" approach.

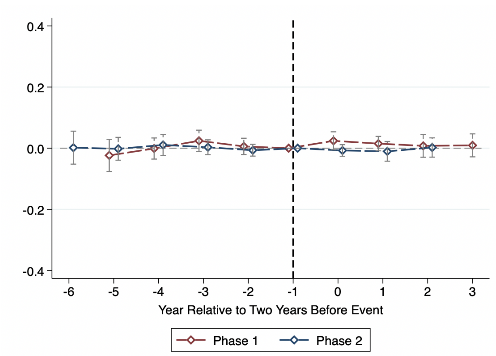
Figure B.9: Test of No Anticipation Effect Assumption (Two Years Before Treatment)



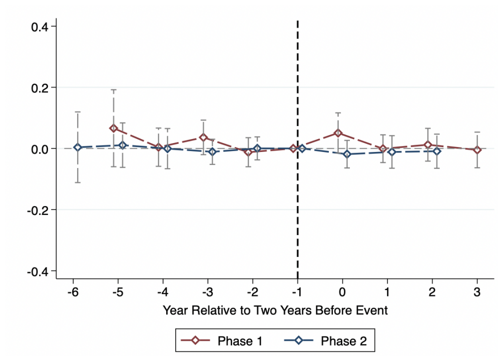
(a) $\ln L_{it}$



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



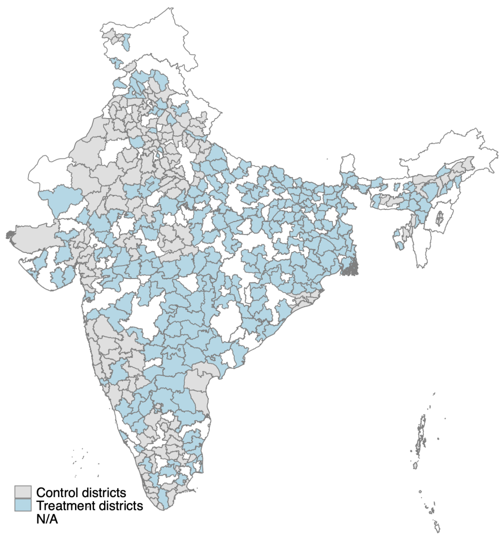
(c) $\ln W_{it}$



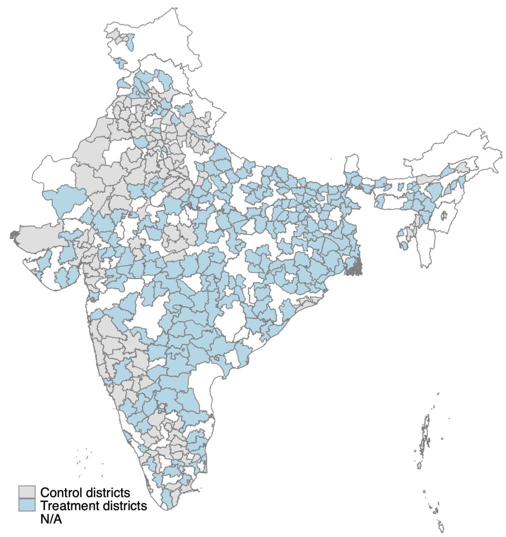
(d) $\ln MRPL_{it}$

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

Figure B.10: Alternative Treatment and Control Groups



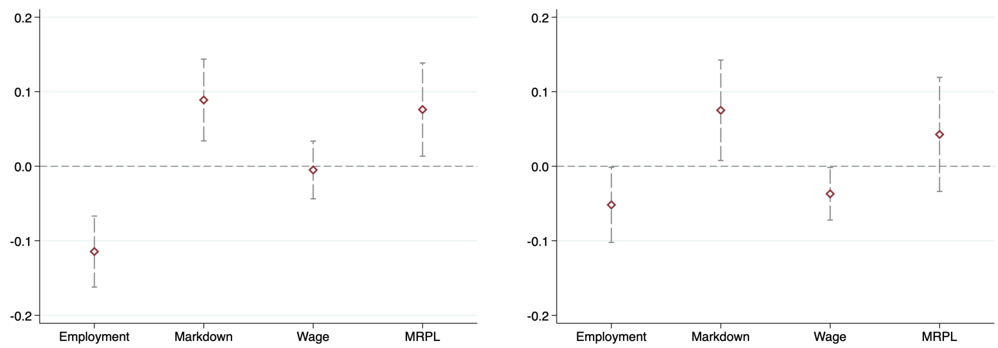
(a) First Alternative



(b) Second Alternative

Notes: Based on the sample in which plant-level markdown has been estimated. Treatment districts are districts where the first two phases of the NREGA program have been implemented. Control districts in panel (a) are districts where the third phase of the NREGA program has been implemented, except for 44 districts surrounded by treatment districts. Control districts in panel (b) further exclude another 29 phase-3 districts surrounded by treatment districts.

Figure B.11: Heterogeneous Effects of NREGA by Labor Intensity in Top-Five and Other Industries

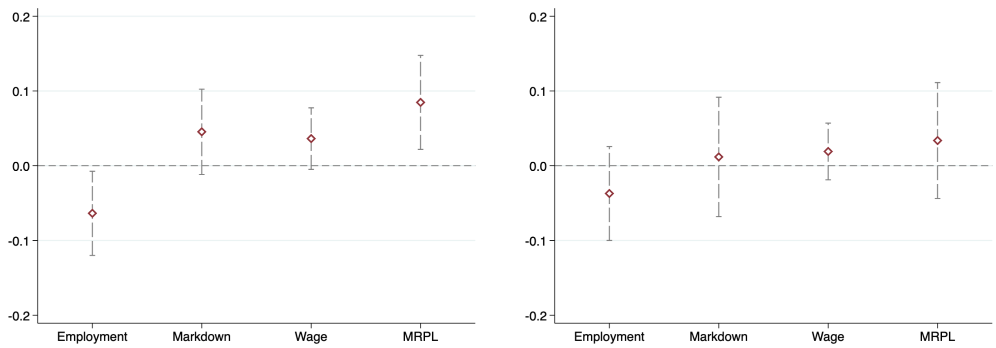


(a) Top five industries

(b) Other industries

Notes: The figure depicts the effects of NREGA on labor market outcomes heterogeneous by labor intensity (labor-to-capital ratio) 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 intensity measure is above the median, i.e., the firm is labor intensive. 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.

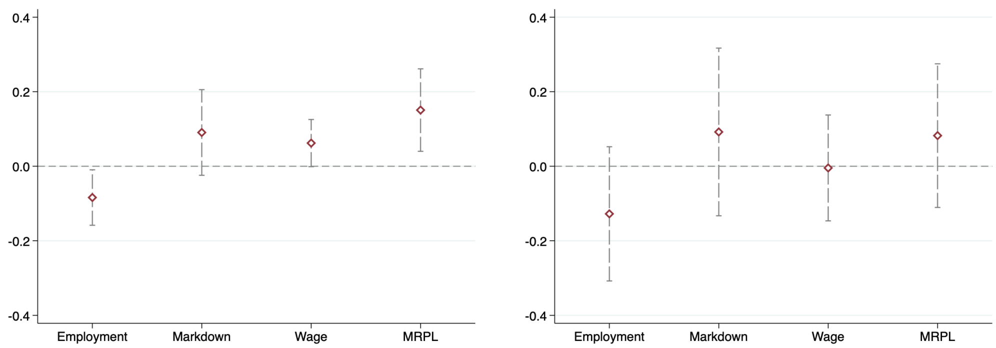
Figure B.12: Heterogeneous Effects of NREGA along the Wage Distribution in Top-Five and Other Industries



(a) Top five industries (b) Other industries

Notes: The figure depicts the heterogeneous effects of NREGA on labor market outcomes at manufacturing firms in the top five and other industries along the wage distribution. 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 initial average wage per worker is in the bottom 3 deciles. 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.

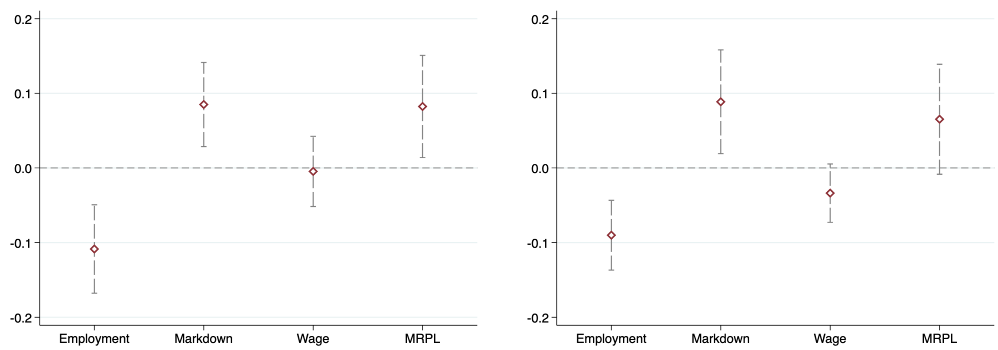
Figure B.13: Heterogeneous Effects of NREGA along the Distribution of Wage-to-Minimum Wage Ratio in Top-Five and Other Industries



(a) Top five industries (b) Other industries

Notes: The figure depicts the heterogeneous effects of NREGA on labor market outcomes at manufacturing firms in the top five and other industries along the distribution of wage-to-minimum wage ratio. 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 initial average wage-to-minimum wage ratio is in the bottom 3 deciles. 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.

Figure B.14: Heterogeneous Effects of NREGA by Labor Productivity in Top-Five and Other Industries (based on the Number of Workers)



(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 number of workers 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.

B.5.2 Additional Tables

Table B.11: Average Effect of NREGA on Employment

	Dependent variable: $\ln L_{it}$				
	(1)	(2)	(3)	(4)	(5)
Post-NREGA	-0.018	-0.018	-0.006	-0.012	-0.022
	(0.019)	(0.019)	(0.016)	(0.020)	(0.020)
Firm age		0.006***	0.005***	0.005***	0.005***
		(0.001)	(0.001)	(0.001)	(0.001)
Firm age^2		-0.000***	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Rainfall		0.002	-0.057	0.010	0.046
		(0.103)	(0.103)	(0.135)	(0.164)
N	73997	72924	72924	72923	72394
R^2	0.96	0.96	0.96	0.96	0.97
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
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) in manufacturing between 2000 and 2008. All regressions include an unreported constant term. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table B.12: Average Effect of NREGA on Markdown

	Dependent variable: $\ln \nu_{it}$				
	(1)	(2)	(3)	(4)	(5)
Post-NREGA	0.036*	0.035*	0.015	0.005	0.000
	(0.021)	(0.021)	(0.020)	(0.016)	(0.018)
Firm age		-0.003***	-0.003***	-0.003***	-0.003***
		(0.001)	(0.001)	(0.001)	(0.001)
Firm age^2		0.000**	0.000**	0.000**	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Rainfall		-0.229*	-0.157	-0.196	-0.317*
		(0.133)	(0.140)	(0.179)	(0.188)
N	73997	72924	72924	72923	72394
R^2	0.87	0.87	0.87	0.87	0.89
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
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 markdowns in manufacturing. The plant-level markdowns are estimated under the assumption of a translog production function in manufacturing between 2000 and 2008. All regressions include an unreported constant term. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table B.13: Average Effects of NREGA on Wages and MRPL

	Dependent variable: ln W_{it} or ln $MRPL_{it}$				
	(1)	(2)	(3)	(4)	(5)
	Panel A. 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)
Firm age		0.002***	0.002**	0.002**	0.002**
		(0.001)	(0.001)	(0.001)	(0.001)
Firm age^2		-0.000***	-0.000***	-0.000**	-0.000**
		(0.000)	(0.000)	(0.000)	(0.000)
Rainfall		0.133*	0.111	-0.081	-0.050
		(0.072)	(0.071)	(0.106)	(0.110)
N	70094	69125	69125	69124	68584
R^2	0.90	0.90	0.91	0.91	0.91
	Panel B. 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)
Firm age		-0.002	-0.002*	-0.002*	-0.002**
		(0.001)	(0.001)	(0.001)	(0.001)
Firm age^2		0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
Rainfall		-0.055	-0.015	-0.244	-0.344*
		(0.134)	(0.139)	(0.186)	(0.200)
N	70094	69125	69125	69124	68584
R^2	0.87	0.87	0.88	0.88	0.89
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓			
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 wage (top panel) and log MRPL (bottom panel) in manufacturing between 2000 and 2008. The MRPL was computed by multiplying wage with plant-level markdowns estimated under the assumption of a translog production function. All regressions include an unreported constant term. Standard errors clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table B.14: Heterogeneous Effect of NREGA on Employment by Labor Productivity

	Dependent variable: $\ln L_{it}$				
	(1)	(2)	(3)	(4)	(5)
Post-NREGA × Below median	-0.125***	-0.128***	-0.098***	-0.100***	-0.101***
	(0.023)	(0.022)	(0.019)	(0.019)	(0.019)
Below median	0.038**	0.037**	0.029*	0.031**	0.023
	(0.017)	(0.017)	(0.015)	(0.016)	(0.014)
Post-NREGA	0.046**	0.047**	0.044**	0.035	0.025
	(0.022)	(0.022)	(0.020)	(0.022)	(0.022)
N	73511	72454	72454	72453	71921
R^2	0.96	0.96	0.96	0.96	0.97
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) in manufacturing by labor productivity (sales revenue per labor) between 2000 and 2008. 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 ***$p < 0.01$.

Table B.15: Heterogeneous Effect of NREGA on Markdown by Labor Productivity

	Dependent variable: $\ln \nu_{it}$				
	(1)	(2)	(3)	(4)	(5)
Post-NREGA × Below median	0.106***	0.109***	0.119***	0.101***	0.086***
	(0.022)	(0.022)	(0.021)	(0.023)	(0.022)
Below median	-0.006	-0.005	0.001	0.006	0.018
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Post-NREGA	-0.018	-0.021	-0.045*	-0.042**	-0.039*
	(0.023)	(0.023)	(0.024)	(0.021)	(0.022)
N	73511	72454	72454	72453	71921
R^2	0.87	0.87	0.87	0.87	0.88
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 markdowns in manufacturing by labor productivity (sales revenue per labor). The plant-level markdowns are estimated under the assumption of a translog production function between 2000 and 2008. 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 ***$p < 0.01$.

Table B.16: Heterogeneous Effect of NREGA on Wages and MRPL by Labor Productivity

	Dependent variable: $\ln W_{it}$ or $\ln MRPL_{it}$				
	(1)	(2)	(3)	(4)	(5)
	Panel A. $\ln W_{it}$				
Post-NREGA × Below median	-0.032**	-0.031**	-0.021	-0.014	-0.018
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Below median	-0.020**	-0.019**	-0.020**	-0.019**	-0.018*
	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)
Post-NREGA	0.009	0.009	0.009	0.002	0.008
	(0.019)	(0.019)	(0.018)	(0.019)	(0.018)
N	69648	68695	68695	68694	68151
R^2	0.90	0.90	0.91	0.91	0.91
	Panel B. $\ln MRPL_{it}$				
Post-NREGA × Below median	0.074***	0.077***	0.104***	0.096***	0.074***
	(0.025)	(0.025)	(0.022)	(0.022)	(0.025)
Below median	-0.024	-0.023	-0.019	-0.013	-0.002
	(0.015)	(0.016)	(0.015)	(0.016)	(0.017)
Post-NREGA	-0.007	-0.010	-0.037*	-0.041*	-0.033
	(0.021)	(0.022)	(0.021)	(0.023)	(0.024)
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 wage (top panel) and log MRPL (bottom panel) in manufacturing by labor productivity (sales revenue per labor) between 2000 and 2008. The marginal revenue product of labor (MRPL) was computed by multiplying wage with plant-level markdowns estimated under the assumption of a translog production function. 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 ***$p < 0.01$.

Table B.17: Effects of NREGA using Establishments similar to [185]

	Full sample		Markdown sample			
	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln L_{it}$	$\ln W_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln W_{it}$	$\ln MRPL_{it}$
	Panel A. Average effects					
Post-NREGA	0.012	0.052	0.031	-0.046	0.029	-0.021
	(0.060)	(0.033)	(0.045)	(0.037)	(0.047)	(0.055)
N	9391	7601	1445	1445	1397	1397
R^2	0.95	0.83	0.96	0.90	0.91	0.90
	Panel B. Heterogeneous effects by labor productivity					
Post-NREGA × Below median	-0.158***	0.052	-0.094	0.212*	0.114	-0.081
	(0.052)	(0.049)	(0.091)	(0.100)	(0.089)	(0.048)
Below median	0.017	0.002	0.108	0.236***	0.154*	-0.082
	(0.050)	(0.051)	(0.090)	(0.051)	(0.077)	(0.057)
Post-NREGA	0.052	0.022	0.084	-0.163**	-0.085	0.073
	(0.077)	(0.030)	(0.060)	(0.054)	(0.063)	(0.057)
N	7412	6087	1423	1423	1375	1375
R^2	0.96	0.84	0.96	0.90	0.90	0.91

Notes: The table presents the effects of NREGA on labor market outcomes on full and markdown samples based on establishments similar to [185], i.e., private firms in Andhra Pradesh. These regressions, however, are based on manufacturing firms between 2000 and 2008, while [185] covered all non-agricultural sectors in 2013. The left panel employs the full ASI sample, while the right panel uses the ASI sample on which the markdown was estimated. Panel A shows the average effects, while panel B displays the impacts heterogeneous by labor productivity (sales revenue per labor). The key explanatory variable in panel B is the NREGA treatment variable interacted with a dummy, indicating whether the firm's initial labor productivity is below the national median. The dependent variables 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 clustered at the district level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

Table B.18: Heterogeneous Effect of NREGA on Production and Non-Production Workers by Labor Productivity (Interaction Method)

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
	Panel A. Production workers			
Post-NREGA × Below median	-0.104∗∗∗	0.093∗∗∗	-0.037∗∗∗	0.055∗
	(0.018)	(0.026)	(0.014)	(0.030)
Below median	0.023	0.037	-0.022∗∗	0.016
	(0.018)	(0.024)	(0.010)	(0.022)
Post-NREGA	0.014	-0.023	0.013	-0.010
	(0.023)	(0.025)	(0.017)	(0.029)
N	60176	60176	60165	60165
R^2	0.96	0.87	0.92	0.89
	Panel B. Non-production workers			
Post-NREGA × Below median	-0.070∗∗∗	0.023	-0.080∗∗∗	-0.058∗
	(0.024)	(0.035)	(0.027)	(0.030)
Below median	-0.001	0.004	0.025	0.029
	(0.021)	(0.025)	(0.021)	(0.023)
Post-NREGA	0.022	0.006	0.027	0.033
	(0.020)	(0.032)	(0.024)	(0.025)
N	60176	60176	60150	60150
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 by labor productivity (sales revenue per labor) in the most recent period before the first phase of NREGA. 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 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 ***$p < 0.01$.

Table B.19: Heterogeneous Effect of NREGA on Regular and Contract Workers by Labor Productivity (Interaction Method)

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
	Panel A. Regular workers			
Post-NREGA × Below median	-0.072*	0.001	-0.030	-0.026
	(0.037)	(0.049)	(0.036)	(0.066)
Below median	0.009	-0.003	-0.031	-0.031
	(0.026)	(0.049)	(0.032)	(0.059)
Post-NREGA	-0.012	0.054	0.035	0.101**
	(0.026)	(0.036)	(0.028)	(0.043)
N	17633	17633	15207	15207
R^2	0.98	0.86	0.91	0.89
	Panel B. Contract workers			
Post-NREGA × Below median	-0.071	0.056	-0.043	0.012
	(0.059)	(0.064)	(0.028)	(0.063)
Below median	-0.011	0.012	-0.003	0.008
	(0.055)	(0.063)	(0.027)	(0.064)
Post-NREGA	0.045	0.009	0.016	0.027
	(0.057)	(0.053)	(0.027)	(0.046)
N	17633	17633	17613	17613
R^2	0.89	0.89	0.79	0.91

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for regular (top panel) and contract (bottom panel) workers in manufacturing by labor productivity (sales revenue per labor in the most recent period before the first phase of NREGA). 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 plant-level markdowns are estimated using the ASI data from 2000-2008 under the assumption of a translog specification for gross output with regular and contract 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 $^{***}p < 0.01$.

Table B.20: Heterogeneous Effects of NREGA on Production and Non-Production Workers by Labor Productivity using Total Man-days

	Production workers			Non-production workers		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$
	Panel A. Below median					
Post-NREGA	-0.091***	0.068**	0.073***	-0.043	0.059*	0.061**
	(0.025)	(0.028)	(0.027)	(0.029)	(0.035)	(0.028)
N	28806	28806	28803	28806	28806	28792
R^2	0.97	0.90	0.89	0.93	0.88	0.91
	Panel B. Above median					
Post-NREGA	0.009	-0.028	-0.036	0.017	-0.016	-0.039
	(0.028)	(0.034)	(0.038)	(0.029)	(0.035)	(0.026)
N	30289	30289	30277	30289	30289	30272
R^2	0.96	0.85	0.84	0.93	0.84	0.86

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for production and non-production workers 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 for production workers, respectively. The dependent variable in Columns (4)-(6) is the same outcomes for non-production workers. 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 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 ***$p < 0.01$.

Table B.21: Heterogeneous Effects of NREGA on Regular and Contract Workers by Labor Productivity using Total Mandays

	Regular workers			Contract workers		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$	$\ln L_{it}$	$\ln \nu_{it}$	$\ln MRPL_{it}$
	Panel A. Below median					
Post-NREGA	-0.119**	0.097*	0.130**	-0.094	0.048	0.079
	(0.051)	(0.050)	(0.056)	(0.068)	(0.062)	(0.065)
N	8006	8006	5961	8006	8006	8006
R^2	0.98	0.86	0.90	0.91	0.91	0.93
	Panel B. Above median					
Post-NREGA	-0.016	0.023	0.039	0.078	-0.025	-0.007
	(0.029)	(0.036)	(0.041)	(0.058)	(0.062)	(0.057)
N	9144	9144	8806	9144	9144	9127
R^2	0.97	0.85	0.85	0.87	0.85	0.86

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for regular and contract workers 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 for regular workers, respectively. The dependent variable in Columns (4)-(6) is the same outcomes for contract workers. 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 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 ***$p < 0.01$.

Table B.22: Heterogeneous Effects of NREGA on Production and Non-Production Workers by Labor Productivity using Alternative Control Group

	(1)	(2)	(3)	(4)
	$\ln L_{it}$	$\ln \nu_{it}$	$\ln W_{it}$	$\ln MRPL_{it}$
	Panel A. Production workers			
Post-NREGA × Below median	-0.107***	0.097***	-0.035**	0.061**
	(0.018)	(0.027)	(0.014)	(0.030)
Below median	0.027	0.039	-0.024**	0.015
	(0.019)	(0.026)	(0.011)	(0.024)
Post-NREGA	0.006	-0.015	0.012	-0.003
	(0.024)	(0.025)	(0.018)	(0.029)
N	52523	52523	52523	52523
R^2	0.96	0.87	0.93	0.89
	Panel B. Non-production workers			
Post-NREGA × Below median	-0.071***	0.023	-0.083***	-0.059**
	(0.025)	(0.036)	(0.028)	(0.030)
Below median	-0.001	-0.002	0.024	0.022
	(0.022)	(0.024)	(0.022)	(0.024)
Post-NREGA	0.014	0.007	0.039	0.046*
	(0.021)	(0.034)	(0.026)	(0.026)
N	52523	52523	52523	52523
R^2	0.94	0.85	0.84	0.89

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 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 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 ***$p < 0.01$.

Table B.23: Heterogeneous Effects of NREGA on Regular and Contract Workers by Labor Productivity using Alternative Control Group

	(1) $\ln L_{it}$	(2) $\ln \nu_{it}$	(3) $\ln W_{it}$	(4) $\ln MRPL_{it}$
	Panel A. Regular workers			
Post-NREGA × Below median	-0.093**	0.008	-0.025	-0.017
	(0.040)	(0.053)	(0.036)	(0.067)
Below median	0.018	-0.007	-0.036	-0.042
	(0.029)	(0.054)	(0.031)	(0.065)
Post-NREGA	-0.012	0.062	0.036	0.098**
	(0.027)	(0.038)	(0.030)	(0.046)
N	13453	13453	13453	13453
R^2	0.97	0.87	0.91	0.89
	Panel B. Contract workers			
Post-NREGA × Below median	-0.082	0.058	-0.043	0.014
	(0.064)	(0.070)	(0.031)	(0.070)
Below median	-0.007	-0.013	-0.010	-0.023
	(0.061)	(0.062)	(0.031)	(0.063)
Post-NREGA	0.013	0.017	0.018	0.035
	(0.061)	(0.059)	(0.030)	(0.052)
N	13453	13453	13453	13453
R^2	0.89	0.88	0.77	0.91

Notes: The table presents the heterogeneous effects of NREGA on labor market outcomes for regular (top panel) and contract (bottom panel) workers in manufacturing 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 plant-level markdowns 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 ***$p < 0.01$.

APPENDIX C

CHAPTER 3 OF APPENDIX

C.1 Additional Tables

Table C.1: Summary Statistics for Prowess Sample

	No liberalized			Liberalized		
	Mean	SD	Median	Mean	SD	Median
Firm Age	30.6	18.8	26.0	32.4	17.0	29.0
Capital stock (log)	18.4	1.8	18.4	18.3	1.7	18.2
Sales revenue (log)	19.7	1.7	19.7	19.8	1.6	19.8
Employment (log)	6.4	1.4	6.5	5.9	1.2	5.7
Wage (log)	17.7	1.7	17.7	17.8	1.6	17.8
Markdown	3.0	2.1	2.5	3.2	1.9	2.9
MRPL (log)	18.5	1.9	18.5	18.8	1.7	18.8
N	234,653			12,151		

Notes: The table presents the summary statistics for firms' characteristics by treatment (FDI liberalization) status. The sample is based on Prowess data from 2001-2019 on which wage markdown has been estimated. An observation is at the firm-year level. Capital stock is the net fixed assets. Employment is the number of workers. Wage is total wage bill. The firm-level wage markdowns are estimated under the assumption of translog production function in the manufacturing industry. The marginal revenue product of labor (MRPL) is computed by multiplying the wage bill with firm-level markdown. All dollar values have been converted to Rupees using the exchange rates.

Table C.2: Heterogeneous Effects of the Foreign Capital Liberalization on Male and Female Workers by Firms' Ex-ante MRPL (Alternative Control Group)

	Employment (1)	Wage bill (2)	Wage (3)	MRPL (4)	Markdown (5)
	Panel A. Male workers				
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.621***	0.643***	0.173***	0.004	-0.170**
	(0.139)	(0.197)	(0.039)	(0.049)	(0.076)
$\text{Post}_t \times \text{Reform}_j$	-0.195	-0.218	-0.039	-0.111	-0.072
	(0.135)	(0.163)	(0.041)	(0.074)	(0.087)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.134**	0.120**	-0.046	-0.226***	-0.180**
	(0.056)	(0.055)	(0.029)	(0.074)	(0.066)
N	6562	6562	6562	6562	6562
R^2	0.94	0.95	0.88	0.82	0.82
	Panel B. Female workers				
$\text{Post}_t \times \text{Reform}_j \times I_k^{\text{High MRPL}}$	0.087	0.079	-0.059	-0.741***	-0.681*
	(0.176)	(0.211)	(0.144)	(0.218)	(0.338)
$\text{Post}_t \times \text{Reform}_j$	-0.182	-0.199	0.092	0.724*	0.632
	(0.174)	(0.190)	(0.102)	(0.415)	(0.508)
$\text{Post}_t \times I_k^{\text{High MRPL}}$	0.262**	0.195	-0.107***	-0.392***	-0.285***
	(0.120)	(0.118)	(0.036)	(0.078)	(0.087)
N	6562	6562	6562	6562	6562
R^2	0.89	0.92	0.87	0.87	0.81

Notes: Based on ASI data from 2000-2018 on which wage markdowns over male and female workers have been estimated. The table presents the results from OLS regressions estimating the heterogeneous effects of FDI liberalization by firms' pre-reform average MRPL between 2000 and 2005. The outcomes in columns 1–5 are employment (headcount), wage bill, wage per worker, MRPL, and markdown for male (Panel A) and female (Panel B) workers, respectively. All dependent variables are in logs. The treatment is the alternative treatment variable, a dummy indicating the 2006 FDI reform, with never treated and 1991 reform industries in the control group. The establishment-level wage markdowns over male and female workers are estimated under the assumption of translog production function with heterogeneous workers in the manufacturing industry. The marginal revenue product of labor (MRPL) for male and female workers are computed by multiplying the wage per male and female worker with establishment-level markdowns over the corresponding type of workers. All specifications control for firm, firm age, and state-by-year fixed effects. Standard errors two-way clustered at the 4-digit industry and year level are in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, and ***$p < 0.01$.

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