Who Gains from Minimum Wages? Firm Incidence and Contract Labour in India's Manufacturing

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Abstract

This paper examines how minimum-wage increases transmit within firms that employ both directly hired permanent workers and contractor-hired contract workers in India's registered manufacturing sector. Using a plant-level panel from the Annual Survey of Industries linked to a newly compiled state–industry–year minimum-wage series, we identify the incidence and adjustment margins of minimum-wage changes. The results reveal strongly asymmetric effects: minimum-wage increases raise permanent workers' average wages and total payroll while leaving contract wages largely unchanged; adjustment occurs mainly through reductions in contract employment, and correspondingly markdowns compress for permanent workers but not for contract workers. Overall, the evidence suggests that in contractor-intensive workplaces minimum-wage policy primarily benefits core permanent employees while shifting adjustment onto contract workers, implying that broader earnings gains require compliance mechanisms that effectively reach contractor-mediated labour.

Keywords: minimum wages; contract labour; firms; India; markdowns; labour market regulation

JEL Codes: J31; J38; J42; L25; O17

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1 Introduction

Minimum wages are among the most widely used labour-market policies in the world, and recent research has reshaped how economists think about their effects. A large body of evidence shows that minimum-wage increases substantially raise earnings at the bottom of the wage distribution, often with limited net employment effects, pushing the literature beyond the textbook competitive benchmark toward models with frictions, imperfect competition, and firm adjustment on multiple margins (Card and Krueger, 1994; Cengiz et al., 2019; Dube and Lindner, 2024; Manning, 2003). At the same time, a parallel literature documents that many labour markets exhibit meaningful employer wage-setting power, so wages can lie below marginal revenue product and minimum wage policy can reallocate rents within firms rather than simply moving along a competitive labour-demand curve (Burdett and Mortensen, 1998; Card et al., 2018; Lamadon et al., 2022). These insights motivate a natural question for minimum-wage policy in developing countries: when statutory wage floors rise in settings with weak enforcement and fragmented employment relationships, who actually benefits, and through which firm-level margins does adjustment occur?

This question is particularly salient when production is organised through triangular employment, where workers perform tasks within the plant but their employment relationship is mediated by contractors. In such "fissured" settings, minimum-wage law applies on paper, yet enforcement and bargaining can differ sharply between directly hired workers and workers hired through intermediaries (Ashenfelter and Smith, 1979; Autor, 2003). The institutional wedge matters because the same statutory change can translate into different effective wage floors within the same firm: permanent workers may face tighter monitoring and clearer liability, while contract workers' pay is mediated by contractors and constrained by weaker attachment, limited grievance mechanisms, and enforcement frictions. In this environment, the key incidence question is not only whether minimum wages raise average wages, but whether they do so uniformly across worker types, or whether firms respond by reallocating tasks and headcount across employment relationships.

We study this question in registered manufacturing in India, where the institutional setting makes within-firm incidence a central margin of adjustment. India's minimum-wage regime is highly decentralised: states notify and revise wages by scheduled employment categories and skill tiers rather than through a single national adjustment, generating fragmented and staggered statutory variation across place and industry. At the same time, organised manufacturing has become increasingly reliant on contract labour: a large share of production workers are hired through contractors, earn substantially less than permanent workers, and face weaker job protection and bargaining leverage. Recent evidence also highlights that contract labour plays a central role in how formal firms adjust to shocks. For example, Chakraborty and Others (2024) show that rising import competition increases formal-sector employment share partly because high-productivity formal firms expand while relying on contract workers to avoid stringent firing costs, underscoring that contract work is not a marginal phenomenon but a core adjustment margin in India's formal manufacturing. In such a setting, minimum-wage increases could plausibly raise wages for both groups, or instead amplify dualism by protecting incumbent permanent workers while shifting adjustment onto contract workers.

To answer these questions and study how minimum-wage changes transmit within firms across employment types, we construct a plant-level panel from the Annual Survey of Industries (ASI) and link it to a newly compiled series of statutory minimum wages at the state $\times$ NIC-5 $\times$ year level. India's minimum wages are defined over "scheduled employments"–legal occupation-based categories that do not correspond directly to standard industrial classifications such as NIC codes. We therefore map these categories into NIC-5 industries and merge them to plants based on state and detailed industry codes, yielding variation across both geography and industry that aligns with the structure of production in the data. We align policy changes with plant outcomes using a one-year lag, consistent with adjustment dynamics documented in prior work (Mansoor and O'Neill, 2021; Soundararajan, 2019). This construction provides rich variation in statutory minimum wages across state–industry cells over time, while preserving the level of disaggregation needed to study heterogeneity within manufacturing.

A second empirical challenge is that ASI does not observe the wage distribution within plants; it reports average wages by worker category. We therefore adapt the nonlinear exposure approach developed by Hau et al. (2020), which is designed for exactly this data environment. The key idea is that the effect of a statutory minimum-wage change should be stronger in plants whose pre-period wages are closer to the statutory floor, and this relationship is convex. We estimate the curvature that maps proximity-to-minimum-wage into exposure using plant-level wage responses, and then use the resulting exposure index to trace how minimum-wage shocks transmit through wages, employment composition, labour costs, and labour-market power measures at the plant level. This framework allows us to study minimum-wage incidence inside firms even when the share of affected workers is unobserved.

The results deliver a sharp answer: minimum-wage increases in India's organised manufacturing primarily benefit permanent workers, while contract workers see little wage gain. Across specifications, permanent wages rise strongly in more exposed plants, with exposure-elasticities of permanent real wages in the range of 0.16–0.22 across baseline fixed-effects and dynamic models. In contrast, contract wages display weak and non-robust pass-through; once we absorb high-dimensional fixed effects and dynamic structure, the estimated contract-wage response is close to zero.

Employment responses reinforce the same asymmetry. Permanent headcount is essentially unchanged, while contract employment declines in more exposed plants, indicating that firms adjust along the contract margin rather than through across-the-board employment cuts. This adjustment is reflected in labour expenditure: increases in total labour costs are concentrated in payments to permanent workers, while total payments to contract workers do not rise and often decline as contract headcount falls.

Measures of labour-market power point in the same direction. Wage markdowns, the gap between workers' wages and their marginal revenue product, decline for permanent workers following minimum-wage increases, consistent with a compression of firm rents. In contrast, markdowns for contract workers show little systematic change. Taken together, the evidence points to a within-firm incidence pattern in which statutory wage floors raise earnings and reduce rent extraction for directly employed workers, while contractor-mediated workers experience employment contractions without commensurate wage gains.

The broader picture that emerges is that minimum-wage regulation interacts strongly with organisational form. In workplaces where employment is fissured between direct and contractor-mediated labour, the statutory minimum wage does not operate as a uniform floor that lifts all covered workers. Instead, firms adjust through internal reallocation across worker types: wages rise where employment relationships make enforcement and bargaining more effective, while employment contracts shift or shrink where mediation weakens pass-through. This pattern is consistent with the view that firms respond to labour-cost shocks through within-firm input reallocation rather than solely through scale contraction, and that contractual segmentation serves as a central margin of adjustment.

Our contribution is threefold. First, we show that minimum-wage effects within firms differ sharply by worker type. Existing firm-level studies of minimum-wage incidence typically estimate average effects because the underlying data do not distinguish between permanent and contract workers. As a result, they cannot examine how firms adjust across employment arrangements. This matters especially in developing economies, where production increasingly relies on contractor-mediated work. Using data from India's organised manufacturing sector, where contract labour is pervasive, we are able to observe this distinction directly. We show that minimum-wage increases raise wages for permanent workers but have limited effects on contract wages, while employment adjustment occurs through contract labour. This uncovers an important within-firm margin of minimum-wage incidence that existing firm-level studies cannot detect.

Second, we contribute to the empirical study of minimum wages in India by constructing a comprehensive statutory minimum-wage series and matching it to plant-level data at a highly disaggregated industry level. Existing studies have linked minimum wages to microdata using either narrower sectoral coverage or more aggregated industry classifications. For example, Soundararajan (2019) focuses on a limited set of industries, while the analysis in Mansoor and O'Neill (2021) is conducted at the NIC-3 digit industry level. In contrast, we compile statutory minimum wages and conduct the analysis at the state $\times$ NIC-5 digit $\times$ year level, which allows us to exploit much finer variation across industries within states over time. This linkage enables us to study minimum-wage incidence within a consistent plant-level panel and, importantly, to examine how firms adjust wage bills, employment composition, and labour costs separately for permanent and contract workers within the same establishment.

Third, we contribute to the literature on labour-market power and rent allocation by showing that minimum-wage incidence differs sharply across worker types within the same firm. A growing body of work uses firm-level data to study markups, markdowns, and the distribution of rents between firms and workers (Card et al., 2018; De Loecker and Warzynski, 2012). We bring that perspective to minimum-wage policy in a dual labour market. Rather than treating the minimum wage only as a wage-floor intervention, we ask whether it changes firms' wage-setting power differently for permanent and contract workers. Our results show that markdowns compress for permanent workers when minimum wages rise, consistent with reduced wage-setting power over directly employed labour, but do not compress for contract workers and in some specifications even increase. This adds a rent-allocation perspective to the minimum-wage literature and highlights that organisational form is central to understanding who gains from statutory wage regulation inside the firm. In particular, differences in wage-setting across permanent and contract employment mediate how the gains from minimum-wage increases are distributed.

These findings speak to current policy debates on wage regulation and labour reform. Many developing countries are expanding the scope and ambition of minimum-wage policy, often alongside reforms aimed at simplifying wage schedules and improving compliance. Our results suggest that in economies where formal production relies heavily on contract labour, the distributional consequences of minimum wage increases depend critically on whether enforcement and liability effectively reach contractor-mediated workers. Without credible enforcement in fissured segments, minimum wage increases can raise earnings for core permanent workers while shifting adjustment onto contract workers through reduced employment and weaker bargaining positions, potentially deepening within-firm inequality.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and the prevalence of contract labour in organised manufacturing. Section 3 develops a conceptual framework and testable predictions on wages, employment composition, and markdowns by worker type. Section 4 describes the data, the construction of the minimum-wage linkage, and key descriptive patterns. Section 5 outlines the empirical strategy based on nonlinear exposure and fixed-effects specifications. Section 6 presents the results on wages, employment, and labour-market power, and Section 7 concludes.

2 Institutional and policy setting

This paper is situated at the intersection of two defining institutional features of Indian labour markets. The first is a highly differentiated minimum-wage regime in which statutory wage floors vary across states, sectors, and skill categories and are revised through state-level notifications rather than a single national adjustment. The second is the prevalence of triangular employment in registered manufacturing, where a substantial share of production labour is hired through contractors rather than directly by the plant. The interaction of these institutions matters because the same statutory change can translate into different effective wage floors within the same firm, depending on monitoring, bargaining power, and the employment relationship. These features imply that firms' choices between permanent employment and third-party contracting are central to minimum-wage incidence when enforcement and bargaining conditions differ across worker types.

2.1 Minimum-wage institutions

In India, minimum wages are set and revised by state governments, with statutory wage floors varying across industry, skill category, and often by geographic zone within states. In the empirical analysis, we use the unskilled minimum wage as a benchmark statutory floor, following the literature; this benchmark closely tracks movements in the broader wage schedule across skill tiers (see Appendix B for details).

Figure 1 summarises the cross-sectional distribution of real minimum-wage changes across state $\times$ industry cells and highlights a central feature of the system: adjustments are fragmented, discrete, and staggered rather than nationally synchronised. In some years, many cells exhibit no real change, while a subset experiences sizeable increases, often exceeding 20 percent; in other years, adjustments are more widespread but remain heterogeneous in magnitude. This pattern reflects the decentralised process through which states revise industry-specific wage floors at different times and by different amounts. As a result, both the timing and magnitude of statutory changes vary substantially across cells.

This variation is central to the empirical strategy. Plants operating in different state–industry environments face different statutory wage shocks within the same year, and these shocks are neither uniform nor purely driven by aggregate trends. Mapping this structure to plant-level data provides the cross-sectional and intertemporal variation needed to construct continuous measures of exposure to minimum-wage changes. The analysis exploits this staggered and heterogeneous variation, while absorbing common macro shocks and time effects, to identify how firms adjust wages, employment composition, and labour-market power when the statutory floor changes.

2.2 Contract labour and triangular employment

Contract labour has become a central feature of organised manufacturing in India over the past two decades (Basu and Others, 2021; Goldar, 2023; Kannan and Raveendran, 2009; Ramaswamy, 2013). Its expansion has contributed to a segmented workforce in which contract workers typically earn lower wages, have weaker bargaining power, and face more precarious employment conditions. This form of employment introduces elements of informality within formal production and provides firms with a flexible margin of adjustment (Kapoor and Krishnapriya, 2023; Sapkal, 2016; Shyam Sundar, 2007).

Under the Contract Labour (Regulation and Abolition) Act, 1970, contract workers are hired by or through a contractor but work within the establishment, creating a triangular employment relationship between the worker, the contractor, and the principal employer. While wage payment is formally the responsibility of the contractor, the principal employer retains oversight and contingent liability in cases of non-payment. This arrangement allows firms to organise production within the establishment while mediating employment relationships through contractors, creating scope for both cost flexibility and uneven enforcement of labour standards.

Evidence from firm-level and field studies shows that contract workers are often integrated into production, including in semi-skilled and skilled tasks, but remain sharply differentiated from permanent workers in wages, job security, and access to benefits (Das and Pandey, 2004; Singh and Others, 2016). Contract labour may therefore act as a partial substitute for regular labour, while preserving differences in employment conditions and bargaining power.

Figure 2 illustrates these features in our data. Panel A shows that contract labour accounts for a substantial share of production workers throughout the period, ranging from roughly one-third to two-fifths of employment. Panel B shows a persistent wage gap: permanent workers earn around 1.7 to 1.9 times the wages of contract workers. Together, these patterns point to a pronounced within-firm dualism in both employment composition and pay.

These features make contractor-mediated employment a central margin of adjustment in organised manufacturing. In a setting combining statutory wage regulation with triangular employment and uneven enforcement, the effects of minimum-wage changes depend not only on the level of the wage floor but also on how it is mediated across employment relationships. This setting therefore provides a natural context to examine whether minimum-wage changes translate into wage gains, employment reallocation, and shifts in bargaining power differently for permanent and contract workers within the same firm.

3 Conceptual framework and hypotheses

This paper examines how statutory minimum-wage changes are transmitted inside firms that employ both directly hired permanent workers and contract workers supplied through third-party firms. The central question is whether minimum-wage regulation affects labour uniformly within the establishment, or whether its incidence differs across employment relationships.

The empirical analysis proceeds in two steps. We first examine aggregate firm-level adjustment: total employment, total production cost, capital intensity, markups, and markdowns. This establishes whether minimum-wage exposure leads firms to adjust overall scale, input mix, or rent allocation. We then turn to the core question of the paper: whether wage, employment, expenditure, and markdown responses differ between permanent and contract workers within the same firm.

3.1 Aggregate firm-level adjustment

A minimum-wage increase raises the cost of employing workers whose pay is close to the statutory floor. At the firm level, this shock may be absorbed in several ways. Firms may reduce total employment, substitute toward capital or other inputs, pass costs through into output prices, or absorb part of the shock through lower markups or reduced labour rents. These responses are not mutually exclusive, and their empirical importance depends on the degree of exposure to the wage floor, product-market conditions, and the availability of adjustment margins.

In our setting, the aggregate firm-level outcomes serve two purposes. First, they show whether minimum-wage shocks generate broad changes in firm behaviour beyond wages. Second, they establish whether any observed worker-level effects are accompanied by changes in overall scale or production technology. If exposed firms do not systematically reduce total employment, raise capital intensity, or alter markups, then the main adjustment is unlikely to operate through broad firm contraction or technological substitution. Instead, the relevant margin may be the allocation of labour costs and rents across worker types within the firm.

This motivates our first set of empirical tests, which estimate the effects of minimum-wage exposure on total employment, total cost of production, capital–labour ratios, firm-level markups, and firm-level markdowns. These outcomes provide a benchmark for interpreting the later worker-type results.

3.2 Within-firm incidence: permanent versus contract labour

The key mechanism in the paper concerns differences between permanent and contract labour. Permanent workers are directly employed and paid by the firm. Contract workers, by contrast, are supplied through third-party firms. This distinction matters because the two groups differ in wage-setting, compliance, monitoring, and adjustment costs.

For permanent workers, the link between the statutory wage floor and observed remuneration is likely to be more direct. If minimum-wage compliance is more visible or enforceable for directly employed labour, exposed firms should display stronger wage pass-through for permanent workers. Since permanent employment relationships are less flexible, firms may also be less willing or able to adjust permanent headcount in response to minimum-wage increases.

For contract workers, the transmission of minimum-wage regulation is less direct. Their wages and payments are mediated through third-party firms, and the establishment observes contract labour partly as a labour-service cost rather than as an internal wage bill. This may weaken pass-through to contract-worker wages. At the same time, contract labour may provide a flexible margin of adjustment. Firms facing higher labour costs may reduce contract employment or contract-labour expenditure rather than adjust permanent staff.

These considerations generate the central empirical predictions. Minimum-wage exposure should raise average wages more strongly for permanent workers than for contract workers. Employment responses should be concentrated, if anywhere, on the contract margin rather than the permanent margin. Total payments to permanent workers should rise when wage gains are not offset by employment reductions, while payments to contract labour may remain flat or fall if firms reduce contract use. Finally, markdowns should compress more clearly for permanent workers if minimum wages raise pay relative to productivity. For contract workers, markdown responses may be weaker or unstable if adjustment occurs through third-party mediation or through employment rather than wages.

As an additional decomposition, we examine whether the permanent-worker response is concentrated among direct production workers or extends to supervisors, managers, and other permanent staff. This exercise is not central to the identification strategy, but helps interpret whether minimum-wage pass-through operates narrowly through workers closest to the statutory floor or more broadly through the firm's internal wage structure.

4 Data and descriptive evidence

This section describes the data, the construction of key outcomes by worker type, and the statutory minimum-wage database that we compile and merge to plants. The goal here is descriptive and credibility-building: we document what is observed in the plant panel, how we translate India's multi-layered minimum-wage schedules into a usable state–industry series at NIC-5, and why there is substantial cross-sectional and intertemporal variation in statutory shocks. We then introduce two additional descriptive exhibits, the distribution of minimum-wage exposure across plants and a raw binned association between wage growth and exposure, without making causal claims.

4.1 Data sources and sample construction

Our primary dataset is a plant-level panel constructed from India's Annual Survey of Industries (ASI), which covers the registered (organised) manufacturing sector. We append annual ASI rounds over the study period and track establishments over time using the ASI panel identifier, resulting in an unbalanced panel where plants enter and exit the sample across years. The analysis is restricted to manufacturing industries (NIC-2008 10–33), and we implement standard sample-cleaning steps to address missing or implausible values in employment and wage-bill components. Where descriptive statistics aim to represent the population of registered manufacturing, we use ASI sampling multipliers to weight plant observations; where the focus is within-plant dynamics, we report unweighted patterns as a transparency check.

ASI is particularly well-suited for our question because it distinguishes worker categories and wage-bill components in a way that allows us to separately observe outcomes for directly employed permanent workers and contract workers. This distinction underpins the paper's focus on within-plant dualism and the possibility that the same statutory minimum-wage change transmits differently through direct versus contractor-mediated employment relationships.

4.2 Key variables

We organise variables into three blocks that map directly to the paper's conceptual predictions: wages and labour costs by worker type, employment and employment composition by worker type, and labour-market power objects (markups/markdowns), with a final set of secondary plant outcomes (capital intensity and performance proxies). For wages, we construct real average remuneration separately for permanent and contract workers using wage bills divided by headcounts. A key measurement choice is the treatment of non-wage compensation. ASI reports non-wage components such as bonuses and welfare-related expenditures, which in practice are more plausibly tied to directly employed workers. In the baseline, we therefore treat permanent-worker remuneration as wages plus non-wage components, and contract-worker remuneration as wages only, and we later show that the main patterns are robust to alternative allocations.

For quantities, we use plant-level counts of permanent and contract workers and summarise composition using contract intensity measures, such as the contract share among production workers. For labour-market power, we later estimate markups and markdowns from plant data using production-function and cost-share objects. At this stage, we keep the intuition non-technical: markups capture the wedge between output prices and marginal costs, while markdowns capture the wedge between workers' marginal products and pay. The analysis considers these objects overall and, where feasible, separately by worker type to assess whether minimum-wage changes compress wage–productivity wedges differently for permanent and contract labour.

4.3 Minimum-wage linkage: compilation and mapping to plants

A central contribution of the empirical design is the construction of a statutory minimum-wage series that aligns India's minimum-wage schedules to a state × industry × year panel at NIC-5, enabling a clean merge to ASI plants using plant state and NIC-5 classification. The core source is the Labour Bureau's annual *Report on the Working of the Minimum Wages Act, 1948*, which compiles statutory minimum wages fixed by states for detailed scheduled employments. A key feature of this administrative source emphasized in prior empirical work is that it reports minimum wage data as effective on December 31st of each year (Soundararajan, 2019). In our setting, the report provides a single benchmark rate for unskilled workers for each relevant state–schedule cell, which makes it well-suited for building a consistent panel series.

Because the Labour Bureau tables are organised by scheduled employments rather than industrial classification codes, we first convert schedule descriptions into NIC-based industry categories to construct a state–industry–year panel. Following Mansoor and O'Neill (2021), we combine the Labour Bureau minimum-wage reports with survey microdata by first constructing a state–industry–year minimum-wage database. A key step in this process is reconciling two different classification systems: the Labour Bureau reports minimum wages for "scheduled employments" described in text, whereas the survey data record workers and firms using NIC industry codes. We therefore build a concordance that maps schedule descriptions to NIC categories so that each observation in the microdata can be assigned the relevant statutory wage.

In our context, we implement this idea for registered manufacturing by compiling the unskilled minimum wage for each state–schedule–year cell and mapping each schedule into a NIC-5 manufacturing industry, using a transparent concordance as documented in an appendix. Aggregating across mapped schedules yields an annual statutory wage floor at the state × NIC-5 × year level. This NIC-5 resolution is substantively valuable for ASI-based work because it preserves within-state heterogeneity across narrowly defined manufacturing activities, exactly the granularity at which minimum-wage updates are often staggered and uneven.

A second design choice is the timing convention used to merge the minimum wage series to ASI outcomes. We match worker outcomes to the minimum wage effective as of December 31st in the previous year, and merge the minimum-wage value for calendar year t to the subsequent ASI year $t+1$, which allows for an adjustment window before outcomes are observed (Soundararajan, 2019). This is particularly important in our plant-level setting, where wage-setting and employment composition may respond with delay to statutory updates, and where we want exposure to be defined in a way that is credibly predetermined relative to the main ASI outcomes.

Finally, we extend the series beyond the years covered by the Labour Bureau report volumes available to us through 2020 by compiling 2021 unskilled minimum wages directly from state notifications, applying the same unit conventions and the same schedule-to-NIC-5 mapping logic to maintain comparability over time. With the resulting panel in hand, we construct plant-level exposure by assigning each ASI plant the statutory unskilled minimum wage corresponding to its state and NIC-5 industry in the relevant lagged year, and then using year-to-year changes in this statutory floor as the source of identifying variation.

5 Methodology

A central challenge in studying minimum wage effects with firm-level data is the absence of information on the distribution of worker wages within firms. Because the ASI reports only the average wage w_{it} for each firm-year, we cannot directly identify the share of workers actually affected by minimum wage changes. To address this limitation, we adopt the nonlinear exposure framework of Hau et al. (2020), which is designed precisely for settings where only firm-average wages are observed.

Hau et al. (2020) model a firm's exposure to minimum wage policy using a convex impact function that depends on its proximity to the statutory minimum wage. It defines the exposure term as

$$IF_{it} = \left(\frac{w_{i,t-1}}{mw_{s,t-1}} \right)^{-(k+1)}, \tag{1}$$

where $w_{i,t-1}$ is the firm's average wage in period $t-1$, $mw_{s,t-1}$ is the corresponding statutory minimum wage in year $t-1$, and $k > 0$ is the curvature parameter determining how sharply exposure increases as firms approach the binding constraint. Interacting this term with the minimum wage shock, $\Delta \ln MW_{s,t}$, allows the effect of a given minimum wage change to vary systematically with initial exposure. Firms closer to the minimum wage are assumed to experience larger marginal wage adjustments, with the degree of curvature determined by k.

Following Hau et al. (2020), we estimate the curvature in a level-difference nonlinear least squares (NLLS) regression, as level changes allow more precise estimation of convexity. The key identity underlying this approach is:

$$\frac{d \ln w_i}{d \ln mw} = \frac{MW}{w_i} \frac{dw_i}{dMW} = \frac{MW}{w_i} \times IF_i(k) = IF_i(k+1). \tag{2}$$

This identity implies two things: first, when the wage response is estimated in levels, the relevant impact function has curvature k; second, when the specification is written in log changes, the corresponding impact function should instead use curvature $k+1$. We therefore first estimate k using level-difference NLLS wage regression, and then mechanically transform it to $k+1$ when constructing the exposure term used in our log-difference outcome regressions. Hau et al. (2020) estimate k separately by firm size. In our case, the ASI panel has substantially fewer observations, and splitting by size generates unstable NLLS estimates. We therefore follow their procedure but estimate a single pooled curvature parameter k.

After estimating k, we construct the log-elasticity exposure term as

$$IF_{it}(k+1) = \left(\frac{w_{i,t-1}}{mw_{s,t-1}} \right)^{-(k+1)}, \tag{3}$$

and interact it with the minimum wage shock:

$$IF_{it}(k+1) \times \Delta \ln MW_{s,t}. \tag{4}$$

This interaction term represents the nonlinear exposure of firm i to the contemporaneous minimum wage increase and serves as the main treatment variable in all outcome regressions. It captures heterogeneity across firms in how wage shocks translate into broader adjustments in employment, productivity, costs, market concentration, and other performance margins.

This approach is especially attractive in settings, such as ours, where the wage distribution within firms is unobserved and only average wages are available. Interpreting $IF(k+1)$ as a proxy for firm-specific minimum wage exposure provides a tractable and theoretically grounded measure. Two caveats, however, underlie the validity of this strategy.

The first concerns mechanisms: the framework assumes that the primary channel through which minimum-wage policy affects firms is wage-cost pressure. This assumption is standard in models of binding wage floors, where mandated minimum wages operate principally through labour-cost adjustments. Other channels, such as compliance costs, uncertainty about regulatory changes, or local demand spillovers, may also operate but tend to be more diffuse and difficult to isolate in firm-level data. Our estimates should therefore be interpreted as capturing the component of firms' responses driven by wage-cost pressures, while acknowledging that secondary mechanisms may coexist.

The second consideration concerns measurement. The validity of the exposure term requires that average wages accurately reflect a firm's proximity to the minimum wage.

Although ASI wage reporting is generally reliable, enforcement intensity varies across states, industries, and years. If enforcement is uneven, average wages may diverge from the true bindingness of the minimum wage, introducing measurement error in the exposure variable. Our empirical strategy addresses this concern by structuring identification around high-dimensional fixed effects that absorb systematic heterogeneity in enforcement regimes. In the first specification, we include state × industry, industry × year, and state × year interactions, which remove broad differences in institutional environments and compliance patterns. In the second, more restrictive specification, we include state × industry × year interactions, assigning a separate intercept to each state–industry–year cell and thereby ensuring that identification arises solely from variation in exposure among firms facing the same minimum-wage schedule and enforcement conditions. Under either structure, compliance heterogeneity is absorbed by the fixed effects, leaving only within-cell variation in exposure to identify minimum-wage effects.

To make this connection explicit, and to formalize how the fixed-effects structure addresses the measurement concern, we specify the empirical model as follows:

$$\begin{aligned} \Delta \ln y_{it} = \beta_0 + \beta_1 \left(IF_{it} \times \Delta \ln MW_{s,t} \right) + \beta_2 IF_{it} + \beta_3 \Delta \ln MW_{s,t} \\ + \phi_i + \mu_{j\times t} + \gamma_{j\times s} + \delta_{s\times t} + \varepsilon_{it}. \end{aligned} \tag{5}$$

where ϕ_i denotes firm fixed effects; $\mu_{j\times t}$ and $\gamma_{j\times s}$ absorb industry–year and industry–state shocks; and $\delta_{s\times t}$ absorbs all time-varying state-level factors, including variation in enforcement intensity. The separate inclusion of $\Delta \ln MW_{s,t}$ allows for level shifts common to all firms within each minimum-wage schedule. The coefficient of interest, β_1, measures the elastic response of the outcome to a minimum-wage shock for a firm with exposure IF_{it}.

As a robustness exercise, we estimate an alternative fixed-effects structure that replaces the three two-way interactions with a single, more demanding 5-digit industry–state–year triple interaction, $\eta_{j\times s\times t}$, while retaining firm fixed effects, rural/urban dummies, and organisation type dummies:

$$\Delta \ln y_{it} = \alpha_0 + \alpha_1 \left[IF_{it} \times \Delta \ln MW_{s,t} \right] + \alpha_2 IF_{it} + \phi_i + \eta_{j\times s\times t} + \varepsilon_{it}. \tag{6}$$

This specification absorbs nearly all variation at the minimum-wage-schedule level and identifies effects from differential exposure among firms operating within the same industry–state–year regime, and therefore facing the same statutory wage rules. In both specifications, the high-dimensional fixed effects also absorb unobserved heterogeneity in enforcement intensity, an important feature of Indian labour markets documented by Soundararajan (2019) and Mansoor and O'Neill (2021). These models constitute our baseline LSDV specifications. They are estimated by partialling out the fixed effects directly, with standard errors clustered at the industry–state–year level.

To complement the static specification and allow for dynamic adjustment, we also estimate a one-step Arellano–Bond dynamic panel GMM model. The dynamic specification includes the first and second lags of the dependent variable. The first lag is treated as endogenous and instrumented using deeper lags in differences, while the second lag is treated as predetermined. Internal GMM instruments are constructed from lags $t - 2$ and $t - 3$ of the dependent variable, and the instrument matrix is collapsed to limit instrument proliferation. Across most outcomes, the Arellano–Bond tests reject first-order serial correlation in differences, as expected, but do not reject second-order serial correlation, supporting the validity of the moment conditions. Estimation is implemented using `xtabond2`, with exogenous controls entered through standard instruments and standard errors clustered at the industry–state–year level.

Because the triple-interaction specification absorbs an extremely high-dimensional set of fixed effects, we estimate this specification using the static LSDV framework only. Implementing a dynamic GMM estimator in this setting would be computationally infeasible and would generate an excessively large and weak instrument set, undermining reliable inference. Consistent with standard practice in the GMM literature, we therefore restrict the dynamic specification to the baseline fixed-effects structure, where the identifying variation is sufficient to support valid internal instrumentation.

6 Results

This section presents the empirical results in four stages, aligned with the identification strategy. We begin by documenting the relationship between minimum wage changes and firm-level wage adjustment (Section 6.1). This step validates the impact-function framework: the curvature parameter used to construct exposure is inferred from wage responses, and the interpretation of subsequent firm outcomes rests on this wage mechanism. We then examine whether firms adjust along five additional margins in response to minimum wage shocks: employment, total cost of production, capital–labour intensity, product markups, and labour markdowns (Section 6.2). The third stage introduces worker heterogeneity by comparing permanent and contract employees in terms of wage pass-through and employment effects (Section 6.3). Finally, we disaggregate permanent workers by occupational role, focusing on direct production staff, supervisors and managers, and other permanent employees, to assess variation in wage and employment responses within the core workforce (Section 6.4). These stages move from establishing wage pass-through at the aggregate level to a detailed picture of how firms and different categories of workers respond to minimum wage shocks.

6.1 Wage responses

We begin by estimating the nonlinear mapping between minimum wage changes and firm-level wage adjustments. Following Hau et al. (2020), we first infer the convexity parameter k from level changes in average wages and local minimum wages. This step exploits the fact that estimating in levels reduces the curvature of the impact function by one unit, yielding sharper identification of k and, in turn, a more accurate impact factor for subsequent log-based regressions.

Across the full ASI sample (2014–2022), the NLLS estimates indicate statistically significant convexity. Table 2 reports $k \approx 1.13$ in the specification including industry–year, industry–state, and state–year interactions, and $k \approx 1.11$ when absorbing industry–state–year fixed effects. These values imply pronounced heterogeneity in exposure: firms closer to the minimum wage threshold face substantially larger wage pass-through from a given statutory change. For example, a low-wage firm for which the minimum wage equals 75 percent of its average wage is roughly 3.5 times more exposed to a minimum wage hike than a high-wage firm for which the minimum wage equals only 25 percent of its average wage, underscoring the steep non-linearity in treatment intensity.

Guided by Hau et al. (2020), we translate these level-based curvature estimates into the log specification by adding one to the convexity parameter, yielding $k + 1 \approx 2.13$ and 2.11, respectively. We then estimate the wage pass-through in log terms using the interaction $IF \times \Delta \ln MW$, where IF denotes the firm-specific impact factor $IF(k + 1)$. All wage regressions control for firm fixed effects and a rich set of interaction fixed effects to capture local demand and cost conditions.

Table 2 shows that firm-level real wages rise significantly in response to minimum wage hikes. Across both fixed-effect structures, we find statistically and economically meaningful effects of minimum wage changes on wage growth. We additionally implement a two-lag Arellano–Bond GMM estimator to address persistence in wage outcomes; the resulting estimates are very similar and available on request. In the preferred LSDV specification, the coefficient on $IF \times \Delta \ln MW$ is 0.166 (SE 0.027, $p < 0.01$).

To illustrate the magnitude, consider an 11 percent minimum wage increase, $\Delta \ln MW = 0.1$. Firms at the 25th percentile of the w_s/MW distribution are far more exposed to the wage floor than those at the 75th percentile, with impact factor values of 0.345 and 0.070, respectively. This differential implies roughly a 0.5 percentage-point larger annual wage gain for the more exposed firms, $0.166 \times (0.345 - 0.070) \times 0.10$. Relative to an average annual real wage growth rate of 2.4 percent, this effect is equivalent to about two to three months of additional trend wage growth. This highlights that statutory wage changes generate economically meaningful pass-through for firms most exposed to the wage floor.

The results are highly robust across specifications and sample choices. Columns (2)–(4) yield tightly clustered coefficients between 0.14 and 0.16, with statistical significance maintained even under the saturated fixed-effects design. Table A1 confirms that these findings extend to the reduced sample of low-skill, unskilled-intensive industries, defined using independent KLEMS labour quality and PLFS unskilled share benchmarks. In this restricted sample, the level-based curvature estimate remains sizeable at $k = 1.229$ across fixed-effect structures, and the corresponding log-based pass-through coefficients on $IF \times \Delta \ln MW$ range from 0.126 to 0.102 under increasingly stringent controls, each significant at the 1 percent level. This consistency across both the full and reduced samples underscores that the observed wage pass-through is not driven by sample composition or specification choice, but reflects a stable empirical relationship. Taken together, these patterns provide clear evidence of strong and heterogeneous wage pass-through in response to minimum wage shocks.

6.2 Other firm behavioural changes

We next examine whether minimum wage shocks generate adjustments beyond wages, focusing on total employment, total cost of production, capital–labour intensity, and measures of product and labour market power. Columns (1)–(2) report LSDV estimates: column (1) includes separate industry–year, industry–state, and state–year fixed effects; column (2) replaces these with a saturated industry–state–year fixed effect. Column (3) extends to a dynamic panel specification, implementing a two-lag Arellano–Bond GMM estimator to address persistence in the outcome variable. We include two lags of the dependent variable: the first lag is treated as endogenous and instrumented with its own first and second lags in levels, while the second lag enters directly and is treated as exogenous. All other regressors are entered directly and treated as exogenous. Standard errors are clustered at the industry–state–year level. We report Arellano–Bond AR(1) and AR(2) tests for serial correlation.

This structure mirrors Hau et al. (2020), with the interaction $IF \times \Delta \ln MW$ identifying heterogeneous firm responses to local labour cost shocks. Standard errors are clustered at the industry–state–year level throughout.

Table 3 summarises the results for the non-wage outcomes. Using the full ASI sample and the baseline fixed-effects specification, several coefficients are statistically significant. Employment declines modestly, capital–labour intensity increases, and labour markdowns compress. These patterns are consistent with limited input-side adjustment in response to higher labour costs, involving mild substitution away from labour and small changes in factor allocation.

However, these effects are not robust across specifications. When moving to the saturated industry–state–year fixed effects, statistical significance disappears for all non-wage outcomes, with the exception of markdowns. Likewise, the dynamic GMM estimates are generally small and imprecise, and the markdown effects also lose significance. Taken together, the loss of statistical precision under tighter controls indicates that the baseline estimates do not reflect stable responses to minimum wage shocks. In contrast to the strong and persistent wage effects documented in Table 2, we find no consistent or economically meaningful adjustment along non-wage margins once more demanding controls for local conditions and persistence are introduced. Results for product markups and labour markdowns reinforce this conclusion. Tables A5 and A6 report estimates for multiple standard measures of markups and markdowns, respectively, including the DLW, CRS, and CD indices. Across these measures, minimum wage shocks do not generate consistent or robust responses.

The reduced-sample analysis leads to the same conclusion. As shown in Table A2, restricting the sample to low-skill, unskilled-intensive industries yields uniformly small and statistically insignificant effects on non-wage outcomes across most specifications. With the exception of a marginally significant effect on total cost of production in the baseline specification, point estimates remain close to zero in magnitude. Although CRS markups are negative and statistically significant across specifications in the reduced sample, this pattern does not extend to other markup measures, limiting its interpretation as a systematic response.

Overall, while baseline specifications suggest modest responses along some non-wage margins, these effects are fragile and disappear once tighter fixed effects or dynamic controls are imposed. In contrast to the strong and stable wage pass-through documented in Table 2, we find little evidence of systematic adjustment in aggregate employment, input use, or product-market power following minimum wage shocks. Taken together, these results indicate that firms do not primarily respond by scaling overall activity or altering production technologies. Instead, any meaningful adjustment is likely to occur within the labour input itself, a possibility we examine directly in the next subsection.

6.3 Heterogeneous wage responses by worker type: permanent versus contract employees

Tables 4 and A3 present the central evidence on heterogeneous wage incidence across worker types. The key finding is that wage pass-through operates through permanent workers, with contract workers showing virtually no corresponding gains. Although contract workers are formally covered by the statutory wage floor, we do not observe meaningful wage pass-through for this group in either the full ASI sample or the reduced low-skill sample. This asymmetry is striking: it implies that the welfare benefits associated with minimum wage regulation accrue primarily to permanent workers, while contract workers experience little improvement in earnings despite exposure to the same statutory shock.

Beginning with the full sample, the pass-through estimates for permanent employees are large and robust across specifications. Real wages for permanent workers rise sharply, with elasticities around 0.22 in the baseline LSDV model, 0.21 under saturated fixed effects, and 0.16 in the dynamic GMM estimator; all are significant at the 1 percent level. These magnitudes mirror the wage regressions in Table 2 and confirm strong wage growth for permanent workers among firms closest to the statutory minimum.

By contrast, contract workers display much weaker wage adjustment. The LSDV estimates suggest modest pass-through, but these effects are not robust: precision falls sharply in the saturated fixed-effects and GMM specifications, and the dynamic coefficient is essentially zero. In other words, while the permanent-worker wage effects remain large and significant across specifications, the contract-worker effects dissipate once we apply more demanding controls. This divergence is not driven by specification choice: the permanent–contract gap persists under identical controls, fixed effects, and dynamic structure.

The same contrast appears in the employment dimension. Employment losses are concentrated among contract workers. Permanent headcount remains unchanged across specifications, with small and insignificant coefficients. In contrast, contract employment falls. Although the magnitudes differ across estimators, the pattern is clear: firms adjust employment on the contract margin, not through reductions in permanent staff.

Turning to labour costs, increases in total payroll expenditure are concentrated among permanent workers. These increases mirror the permanent-wage pass-through and reflect higher average pay rather than changes in headcount, since permanent employment effects are near zero. By contrast, the total payment bill for contract workers does not rise; coefficients are small, statistically insignificant, and generally negative. Combined with the contract-employment reductions, this pattern suggests that firms accommodate higher statutory pay for permanent staff by scaling back payment on contract labour.

Evidence from labour markdowns further reinforces the asymmetric incidence of minimum wage shocks across worker types. Across specifications, markdowns compress for permanent workers following minimum wage increases, with negative and statistically significant estimates under the DLW measure in both LSDV and dynamic GMM specifications. These patterns are consistent with rising compensation per unit of output for permanent employees and mirror the strong wage and payroll responses documented above.

By contrast, contract workers exhibit no comparable markdown compression. Estimates for contract-worker markdowns are generally small and imprecise, and in several cases turn positive under dynamic GMM. While the strength of the results varies across markdown definitions, the qualitative conclusion is stable: minimum wage shocks reduce labour market power for permanent workers but do not generate corresponding gains for contract workers. If anything, the markdown evidence points in the opposite direction for contract workers, with estimates suggesting a relative increase in markdowns rather than compression.

The permanent–contract asymmetry is preserved in the reduced sample of low-skill, unskilled-intensive industries, ruling out explanations based on industry composition or workforce skill mix. Wage, payroll, and markdown responses for permanent workers remain strong, while contract-worker outcomes remain weak or insignificant across specifications. Together with the markdown evidence, these results confirm that minimum wage adjustments operate primarily through the permanent workforce, rather than through uniform improvements across all worker categories.

We illustrate magnitudes using permanent-worker outcomes that are statistically strong and stable across specifications and samples. Under the preferred specification, an 11 percent increase in the statutory minimum wage generates a sizeable predicted rise in permanent-worker wages. Using the 25th and 75th percentiles of the $IF(k+1)$ distribution, approximately 0.07 and 0.345, as reference points, the implied difference in wage gains is roughly 0.6 percentage points. Relative to average annual real wage growth of 2.8 percent for permanent workers, this corresponds to approximately two to three months of additional wage growth.

A similar calculation applies to labour cost. The elasticity of permanent-worker payroll implies around a 0.5 percentage-point larger annual increase in payroll for firms at the 75th versus the 25th percentile of $IF(k+1)$, equivalent to about two months of additional trend growth given an average annual increase of 2.9 percent. Finally, the markdown elasticity for permanent workers implies a 0.36 percentage-point larger markdown reduction for more exposed firms, which accelerates the typical annual markdown decline of 1.8 percent by about two to three months. Taken together, these comparisons show that the pass-through for permanent workers is not only statistically robust but also economically substantial, shifting wage growth, payroll costs, and markdown compression by meaningful fractions of their underlying trends.

The adjustment to minimum wage shocks shows up primarily within the labour input. The robust result is on the permanent-worker margin: across specifications and samples, permanent workers exhibit clear wage pass-through and higher payroll growth, accompanied by markdown compression in the stronger specifications. Contract workers do not show comparable improvements in wages or payroll. Evidence of contract-side adjustment is more tentative: contract employment declines in the full sample but not in the reduced sample, and contract markdown responses vary by measure and estimator—often failing to compress, and sometimes moving in the opposite direction under dynamic GMM. Overall, the evidence points to asymmetric incidence within firms: minimum wage increases improve outcomes for permanent workers, while parallel gains for contract workers are not observed.

6.4 Wage heterogeneity among permanent workers: direct production, supervisory, and other roles

Tables 5 and A4 disaggregate the wage and employment responses for permanent employees into three mutually exclusive groups: direct production workers, supervisors and managers, and all other permanent workers. The wage pass-through estimates are strong, positive, and highly robust for the first two groups, whereas results for other workers are more mixed. Estimated employment effects, by contrast, are uniformly small and statistically insignificant across specifications and samples, indicating that permanent-staff headcounts remain stable regardless of role. The ASI does not allow a reliable allocation of non-wage labour costs across permanent-worker subgroups, nor does it permit separate measurement of markdowns by worker type. As a result, total payments and markdown outcomes cannot be estimated consistently at this level of disaggregation.

Real wages for direct production workers respond strongly to minimum wage shocks, with elasticities of 0.164 in the baseline LSDV model, 0.188 under saturated fixed effects, and 0.117 in the dynamic GMM estimator. Supervisors and managers exhibit similarly sized responses, with elasticities ranging from 0.219 to 0.244 in the LSDV specifications and a positive, statistically significant effect in the GMM specification. These estimates mirror the aggregate permanent-worker effects and indicate that wage gains among permanent staff are not confined to a single occupational group.

Although supervisors and managers are not directly bound by the unskilled minimum wage schedule, India maintains separate statutory minimum wages for skilled labour that move closely with unskilled schedules over time. In this setting, the unskilled minimum wage serves as a noisy but informative proxy for broader movements in statutory wage floors faced by higher-skill workers. From this perspective, the positive elasticities for supervisors and managers likely reflect exposure to correlated statutory wage adjustments for skilled labour, rather than purely indirect spillovers from unskilled wage increases. At the same time, within-firm wage-structure considerations may reinforce these effects, as increases in production-worker pay compress wage differentials and induce firms to adjust supervisory compensation to preserve hierarchy and retention incentives. Together, these mechanisms imply that statutory wage shocks transmit through firms' broader wage structures, extending beyond the lowest-paid occupations.

The estimates for other permanent workers remain positive and statistically significant in the full sample, but the corresponding coefficients in the reduced-sample setting lose precision and are no longer significant across specifications. This difference suggests that pass-through among other permanent roles is less stable once the analysis is restricted to sectors in which unskilled minimum wages are most relevant.

Employment responses for all three permanent-worker categories are uniformly small and imprecise. Across the full sample and reduced sample, coefficients are close to zero and never attain conventional significance levels. These patterns reinforce the findings above: permanent-staff employment does not adjust materially to statutory wage shocks, even when disaggregated by occupational role.

We illustrate magnitudes using the preferred specification and an 11 percent minimum wage increase, $\Delta \ln MW = 0.1$. Comparing firms with high exposure to the wage floor to firms with much lower exposure, the wage pass-through for direct production workers implies roughly a 0.45 percentage-point additional wage gain. Given an average annual growth rate of 2.3 percent for this group, this translates into two to three months of extra trend wage growth. A similar calculation for supervisors and managers points to an additional wage gain of about 0.6 percentage points. Against an average annual wage growth rate of 3.4 percent for supervisory staff, this also corresponds to roughly two to three months of extra wage growth.

Overall, minimum wage shocks generate clear and heterogeneous wage gains within the permanent workforce. Direct production workers and supervisory staff benefit consistently and substantially, while other permanent roles show less stable effects across samples. Across all categories, permanent-staff employment remains essentially unchanged. These patterns, combined with the preceding results, underscore that the minimum wage primarily operates through wage adjustment rather than employment change, and that the gains are concentrated among workers most directly linked to production and organisational oversight.

7 Conclusion

This paper examines who gains from minimum-wage increases inside firms when production relies on both directly hired permanent workers and contract workers supplied through third-party firms. Using plant-level data from India's ASI linked to a newly compiled state–industry–year minimum-wage series, we show that minimum-wage shocks do not transmit uniformly across worker types within the same establishment.

The central finding is that minimum-wage increases primarily benefit permanent workers. Exposed plants experience robust increases in permanent-worker wages and payroll, while permanent employment remains largely unchanged. Contract workers, by contrast, do not experience comparable wage gains. Instead, adjustment appears partly on the contract margin: contract employment and payments do not rise, and in several specifications contract employment declines. The markdown evidence points in the same direction. Markdowns compress for permanent workers, suggesting that minimum-wage increases reduce rents extracted from directly employed labour, but similar compression is not observed for contract workers.

These results imply that the incidence of minimum-wage regulation depends critically on employment structure. In contractor-intensive workplaces, statutory wage floors may raise earnings for core permanent workers without delivering equivalent gains to contractor-mediated labour. Minimum wages therefore affect not only the level of labour costs, but also their allocation across employment relationships inside the firm.

The findings have two broader implications. First, firm-level averages can mask substantial redistribution within establishments. A minimum-wage increase may appear to generate wage pass-through with limited aggregate employment effects, while the gains and adjustment costs are unevenly distributed across worker types. Second, the effectiveness of minimum-wage policy in dual labour markets depends on whether compliance and liability extend meaningfully to third-party employment arrangements. Where enforcement is weaker for contract labour, minimum-wage increases may reinforce rather than reduce within-firm segmentation.

Overall, the paper highlights organisational form as a key determinant of minimum-wage incidence. In settings where formal firms rely heavily on contract labour, understanding who gains from wage regulation requires looking inside the firm, not only at average wages or total employment.

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Figures

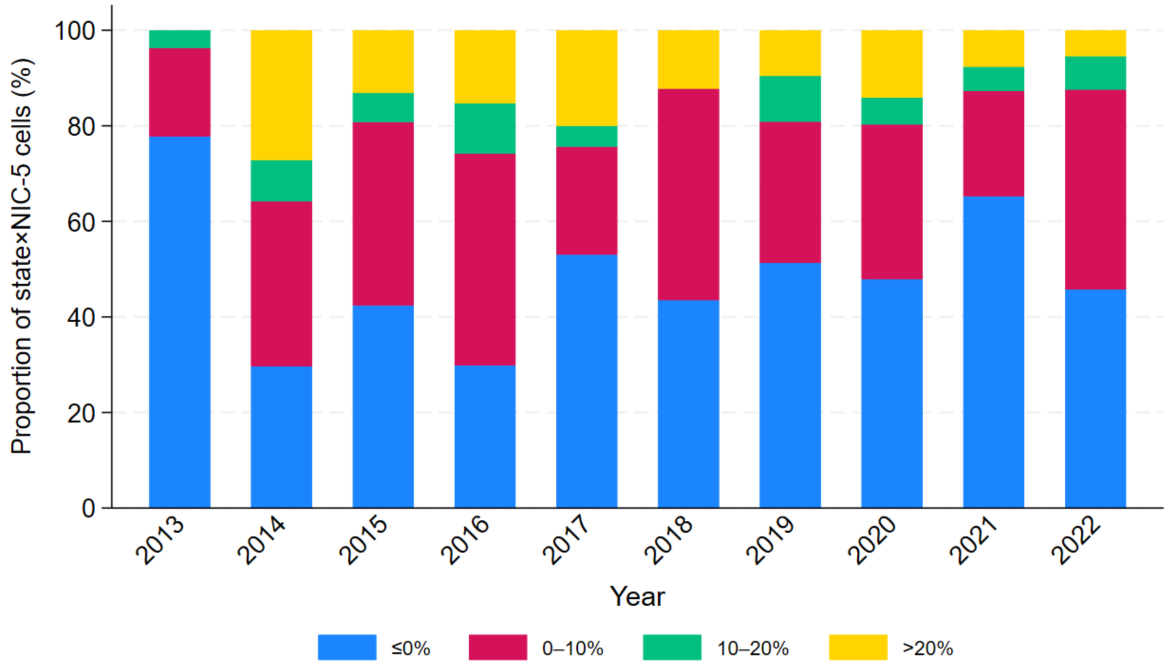

Figure 1: Distribution of real minimum-wage changes across state × NIC-5 cells, 2013–2022 (stacked percent).

(a) Contract labour share in registered manufacturing

(b) Real wage gap between permanent and contract workers

Figure 2: Dual labour market in registered manufacturing, 2012–2022, showing contract intensity and the permanent–contract wage gap.

Tables

Table 1: Descriptive Statistics

Variable	Mean	SD	N
Treatment Variable			
IF$(k+1)$	0.379	0.791	94,922
IF $\times \Delta$ ln Minimum Wage	0.007	0.101	94,922
Δ ln Minimum Wage	0.026	0.133	94,922
Wage Outcomes			
Δ ln Average Wage (All Workers)	0.022	0.230	94,922
Δ ln Average Wage (Permanent Workers)	0.026	0.398	94,922
Direct Workers	0.021	0.293	88,174
Supervisor & Manager Workers	0.031	0.602	92,272
Other Workers	0.021	0.546	84,922
Δ ln Average Wage (Contract Workers)	0.020	0.413	60,328
Employment Outcomes			
Δ ln Employment (All Workers)	0.008	0.273	94,922
Δ ln Employment (Permanent Workers)	0.003	0.380	94,922
Direct Workers	0.000	0.692	94,922
Supervisor & Manager Workers	0.014	0.523	94,922
Other Workers	0.005	0.792	94,922
Δ ln Employment (Contract Workers)	0.037	1.312	94,922
Labour Expenditure Outcomes			
Δ ln Labour Cost (All Workers)	0.028	0.293	94,922
Δ ln Labour Cost (Permanent Workers)	0.027	0.365	94,922
Δ ln Labour Cost (Contract Workers)	0.119	5.025	94,922
Markdown Outcomes			
Δ ln Markdown (All Workers)	-0.018	0.388	94,992
Δ ln Markdown (Permanent Workers)	-0.016	0.441	94,992
Δ ln Markdown (Contract Workers)	-0.022	0.604	60,328
Other Firm Outcomes			
Δ ln Capital-Labour Ratio (K/L)	0.007	0.630	94,922
Δ ln Markup	0.002	0.229	94,922
Δ ln Total Cost of Production	0.058	0.372	94,922

Notes: This table reports descriptive statistics for the main firm-level variables used in the analysis. Wage and employment outcomes are further decomposed across permanent worker categories (direct workers, supervisors/managers, and other workers). All variables are measured at the firm-year level.

Table 2: Estimated Effects of Minimum Wage Changes on Firm-Level Wages

	(1) NLLS	(2) LSDV	(3) NLLS	(4) LSDV
k	1.130***	$k+1$ fixed	1.112***	$k+1$ fixed
	(0.021)		(0.023)	
$IF(k) \times \Delta MW$	0.230***		0.029	
	(0.046)		(0.018)	
$IF(k)$	135.561***		121.442***	
	(1.313)		(1.345)	
ΔMW	-0.001***			
	(0.000)			
$IF(k+1) \times \Delta \log MW$		0.166***		0.142***
		(0.026)		(0.035)
$IF(k+1)$		0.203***		0.219***
		(0.013)		(0.016)
$\Delta \log MW$		-0.004		
		(0.013)		
Observations	100,451	94,922	89,403	82,996
Firm FE	No	Yes	No	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	Yes	No	Yes
Industry-Year-Province FE	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes	Yes

Notes: Reported are estimated effects of minimum-wage changes on yearly firm-level wages using ASI data, 2014–2022. The key regressor is the interaction of the minimum-wage impact factor, $IF(k+1)$, with the local minimum-wage change, $\Delta \log MW$. The interaction $IF \times \Delta \log MW$ captures heterogeneous exposure to minimum-wage shocks following Hau et al. (2020). We estimate the curvature parameter k from wage responses. With industry-year, industry-state, and state-year fixed effects, $k = 1.130$ and $k+1 = 2.130$. With industry-state-year fixed effects, $k = 1.112$ and $k+1 = 2.112$. Standard errors are reported in parentheses and clustered at the industry-state-year level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3: Regression Results for Firm-Level Outcomes

	(1) LSDV	(2) LSDV	(3) DGMM
Panel A. Log Change in firm-level employment			
$IF \times \Delta \log MW$	-0.050***	-0.038	-0.076***
	(0.018)	(0.025)	(0.023)
AR(1)			-39.27
AR(2)			-0.39
Panel B. Log Change in firm-level total cost of production			
$IF \times \Delta \log MW$	0.049*	0.045	0.036
	(0.026)	(0.028)	(0.032)
AR(1)			-31.17
AR(2)			-1.10
Panel C. Log Change in firm-level K/L			
$IF \times \Delta \log MW$	0.115***	0.069	0.093**
	(0.040)	(0.055)	(0.041)
AR(1)			-7.76
AR(2)			-1.78
Panel D. Log Change in firm-level markup			
$IF \times \Delta \log MW$	0.031	0.048	0.039***
	(0.021)	(0.035)	(0.015)
AR(1)			-25.48
AR(2)			0.37
Panel E. Log Change in firm-level markdown			
$IF \times \Delta \log MW$	-0.079**	-0.087**	-0.051
	(0.034)	(0.038)	(0.040)
AR(1)			-30.18
AR(2)			0.39
Observations	94,922	82,996	35,928
Firm FE	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	No	Yes
Industry-Year-Province FE	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes

Notes: This table reports estimated effects of minimum-wage shocks on year-over-year log changes in firm-level outcomes. Columns (1)–(2) report least-squares dummy variable (LSDV) estimates, while column (3) reports dynamic panel estimates using difference GMM. The key regressor is $IF_i \times \Delta \log MW$, where IF_i is the firm-specific impact factor based on the firm's lagged average wage relative to the applicable minimum wage. All specifications include firm fixed effects, rural/urban controls, and organisation-type controls. Column (1) includes industry-year, year-province, and industry-province fixed effects; column (2) includes industry-year-province fixed effects; and column (3) uses the same fixed-effect structure as column (1). Standard errors are reported in parentheses and are clustered at the state-industry level for LSDV models and at the industry-year-province level for GMM. The sample period is 2014–2022. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Permanent and Contract Worker Outcomes

	Permanent Workers			Contract Workers		
	(1) LSDV	(2) LSDV	(3) DGMM	(4) LSDV	(5) LSDV	(6) DGMM
Panel A. Log change in firm-level average wage						
$IF \times \Delta \ln MW$	0.223***	0.214***	0.158***	0.146***	0.117	0.004
	(0.040)	(0.054)	(0.046)	(0.050)	(0.082)	(0.040)
AR(1)			-5.50			-3.30
AR(2)			1.02			-0.73
Observations	94,922	82,996	35,928	57,718	48,445	19,895
Panel B. Log change in firm-level employment						
$IF \times \Delta \ln MW$	0.007	-0.034	0.016	-0.136**	-0.052	-0.193*
	(0.030)	(0.035)	(0.065)	(0.061)	(0.079)	(0.102)
AR(1)			-21.20			-28.43
AR(2)			0.85			-1.54
Observations	94,922	82,996	35,928	94,922	82,996	35,928
Panel C. Log change in firm-level labour cost						
$IF \times \Delta \ln MW$	0.180***	0.158***	0.162**	-0.430	0.001	-0.595*
	(0.033)	(0.040)	(0.068)	(0.276)	(0.330)	(0.324)
AR(1)			-22.31			-31.32
AR(2)			-1.14			-1.90
Observations	94,922	82,996	35,928	94,922	82,996	35,928
Panel D. Log change in firm-level markdown						
$IF \times \Delta \ln MW$	-0.130***	-0.122**	-0.135*	0.051	0.064	0.236**
	(0.049)	(0.059)	(0.073)	(0.071)	(0.128)	(0.103)
AR(1)			-31.67			-25.63
AR(2)			-0.04			-0.33
Observations	94,922	82,996	35,928	57,718	48,445	19,894
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	No	Yes	Yes	No	Yes
Industry-Year-Province FE	No	Yes	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimated effects of minimum-wage shocks on year-over-year log changes in worker-type-level firm outcomes. Columns (1)–(3) report results for permanent workers, and columns (4)–(6) report results for contract workers. Within each worker block, the first two columns report least-squares dummy variable (LSDV) estimates, while the third column reports dynamic panel estimates using difference GMM. The key regressor is $IF_i \times \Delta \ln MW$, where IF_i is the firm-specific impact factor based on the firm's lagged average wage relative to the applicable minimum wage. Standard errors are reported in parentheses and are clustered at the state-industry level for LSDV models and at the industry-year-province level for GMM. The sample period is 2014–2022. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Wages and Employment among Permanent Workers by Worker Type

	Direct Workers			Supervisor & Manager			Other Workers		
	(1) LSDV	(2) LSDV	(3) DGMM	(4) LSDV	(5) LSDV	(6) DGMM	(7) LSDV	(8) LSDV	(9) DGMM
Panel A. Log change in firm-level average wage									
$IF \times \Delta \log MW$	0.164***	0.188***	0.117***	0.219***	0.244***	0.192**	0.209***	0.259***	0.128**
	(0.032)	(0.047)	(0.023)	(0.066)	(0.083)	(0.082)	(0.057)	(0.073)	(0.062)
AR(1)			-22.85			-23.26			-18.22
AR(2)			0.065			0.26			-1.12
Observations	87,572	75,853	32,660	91,955	80,324	34,716	83,977	72,824	30,794
Panel B. Log change in firm-level employment									
$IF \times \Delta \log MW$	-0.016	-0.044	0.045	0.026	0.003	0.017	-0.013	-0.020	-0.035
	(0.052)	(0.053)	(0.103)	(0.020)	(0.028)	(0.029)	(0.033)	(0.047)	(0.037)
AR(1)			-17.28			-31.10			-27.85
AR(2)			1.32			-1.09			-0.74
Observations	94,922	82,996	35,928	94,922	82,996	35,928	94,922	82,996	35,928
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year, Year-Prov., Ind.-Prov. FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Ind.-Year-Prov. FE	No	Yes	No	No	Yes	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Organisation Type Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Permanent workers are divided into direct workers, supervisors and managers, and other permanent workers. The dependent variables are year-over-year log changes in average wages and employment. Columns (1), (2), (4), (5), (7), and (8) report least-squares dummy variable estimates. Columns (3), (6), and (9) report dynamic panel GMM estimates. Standard errors are reported in parentheses. Ind.-Year, Year-Prov., and Ind.-Prov. refer to industry-year, year-province, and industry-province fixed effects, respectively. Ind.-Year-Prov. refers to industry-year-province fixed effects. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Appendix Tables

Table A1: Robustness of Wage Growth Estimates in the Reduced Sample

	(1) NLLS	(2) LSDV	(3) NLLS	(4) LSDV
k	1.229***	$k+1$ fixed	1.229***	$k+1$ fixed
	(0.034)		(0.036)	
$IF(k) \times \Delta \log MW$	0.154***		0.020	
	(0.048)		(0.020)	
$IF(k)$	94.698***		84.420***	
	(1.983)		(1.996)	
ΔMW	-0.001***			
	(0.000)			
$IF(k+1) \times \Delta \log MW$		0.126***		0.102***
		(0.027)		(0.034)
$IF(k+1)$		0.126***		0.127***
		(0.014)		(0.016)
$\Delta \log MW$		-0.036*		
		(0.020)		
Observations	34,829	32,515	31,162	28,538
Firm FE	No	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	Yes	No	Yes
Industry-Year-Province FE	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes	Yes

Notes: This table reports the reduced-sample robustness version of Table 2. The reduced sample is restricted to low-skill, unskilled-intensive manufacturing industries. Reported coefficients estimate the relationship between minimum-wage changes and firm-level wage growth. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2: Robustness of Firm Outcomes in the Reduced Sample

	(1) LSDV	(2) LSDV	(3) DGMM
Panel A. Log change in firm-level employment			
$IF \times \Delta \log MW$	-0.019	0.014	-0.018
	(0.018)	(0.029)	(0.038)
AR(1)			-22.95
AR(2)			-0.72
Panel B. Log change in firm-level total cost of production			
$IF \times \Delta \log MW$	0.055*	0.030	0.098
	(0.031)	(0.029)	(0.074)
AR(1)			-16.87
AR(2)			1.74
Panel C. Log change in firm-level K/L			
$IF \times \Delta \log MW$	0.059	0.046	0.072
	(0.047)	(0.057)	(0.055)
AR(1)			-4.41
AR(2)			-1.97
Panel D. Log change in firm-level markup			
$IF \times \Delta \log MW$	-0.018	-0.005	-0.020
	(0.019)	(0.025)	(0.018)
AR(1)			-14.57
AR(2)			1.50
Panel E. Log change in firm-level markdown			
$IF \times \Delta \log MW$	-0.043	-0.058*	-0.042
	(0.026)	(0.034)	(0.058)
AR(1)			-17.38
AR(2)			0.86
Observations	32,515	28,538	11,282
Firm FE	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	No	Yes
Industry-Year-Province FE	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes

Notes: This table reports the reduced-sample robustness version of Table 3. The reduced sample is restricted to low-skill, unskilled-intensive manufacturing industries. The dependent variables are year-over-year log changes in firm-level outcomes. Columns (1)–(2) report least-squares dummy variable estimates, while column (3) reports dynamic panel GMM estimates. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3: Robustness of Worker Outcomes in the Reduced Sample: Permanent vs Contract Workers

	Permanent Workers			Contract Workers		
	(1) LSDV	(2) LSDV	(3) DGMM	(4) LSDV	(5) LSDV	(6) DGMM
Panel A. Log change in firm-level average wage						
$IF \times \Delta \log MW$	0.228***	0.216**	0.178***	0.112*	0.059	-0.043
	(0.059)	(0.087)	(0.053)	(0.064)	(0.065)	(0.089)
AR(1)			2.85			-6.14
AR(2)			1.23			-2.23
Observations	32,515	28,538	11,282	17,332	14,446	5,186
Panel B. Log change in firm-level employment						
$IF \times \Delta \log MW$	0.026	0.006	0.150	0.003	0.013	-0.104
	(0.046)	(0.051)	(0.115)	(0.080)	(0.099)	(0.211)
AR(1)			-11.58			-19.18
AR(2)			1.05			-1.37
Observations	32,515	28,538	11,282	32,515	28,538	11,282
Panel C. Log change in firm-level labour cost						
$IF \times \Delta \log MW$	0.185***	0.152***	0.273**	-0.232	-0.067	-0.224
	(0.046)	(0.047)	(0.109)	(0.395)	(0.500)	(0.689)
AR(1)			-11.43			-20.88
AR(2)			0.52			-1.59
Observations	32,515	28,538	11,282	32,515	28,538	11,282
Panel D. Log change in firm-level markdown						
$IF \times \Delta \log MW$	-0.134***	-0.111**	-0.211*	-0.012	-0.078	0.219*
	(0.052)	(0.053)	(0.124)	(0.055)	(0.083)	(0.115)
AR(1)			-17.61			-11.88
AR(2)			-0.01			-1.70
Observations	32,515	28,538	11,282	17,332	14,446	5,186
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	No	Yes	Yes	No	Yes
Industry-Year-Province FE	No	Yes	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the reduced-sample robustness version of Table 4. The reduced sample is restricted to low-skill, unskilled-intensive manufacturing industries. Columns (1)–(3) report results for permanent workers, and columns (4)–(6) report results for contract workers. Within each worker block, the first two columns report least-squares dummy variable estimates, while the third column reports dynamic panel GMM estimates. The dependent variables are year-over-year log changes in worker-type-level firm outcomes. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Regression Results among Permanent Workers by Worker Type

	Direct Workers			Supervisor & Manager			Other Workers		
	(1) LSDV	(2) LSDV	(3) DGMM	(4) LSDV	(5) LSDV	(6) DGMM	(7) LSDV	(8) LSDV	(9) DGMM
Panel A. Log change in firm-level average wage									
$IF \times \Delta \log MW$	0.177***	0.239**	0.145***	0.183***	0.261***	0.146	0.084	0.088	0.112
	(0.062)	(0.117)	(0.029)	(0.059)	(0.088)	(0.103)	(0.091)	(0.126)	(0.104)
AR(1)			-10.37			-19.51			-15.15
AR(2)			1.06			-0.96			-1.16
Observations	29,165	25,255	9,886	31,239	27,388	10,810	28,956	25,321	9,784
Panel B. Log change in firm-level employment									
$IF \times \Delta \log MW$	0.000	-0.039	0.296	0.080**	0.054	-0.001	0.064	0.094	0.072
	(0.086)	(0.097)	(0.209)	(0.038)	(0.045)	(0.074)	(0.049)	(0.064)	(0.075)
AR(1)			-10.68			-18.52			-14.80
AR(2)			1.44			-0.40			-0.08
Observations	32,515	28,538	11,282	32,515	28,538	11,282	32,515	28,538	11,282
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year, Year-Prov., Ind.-Prov. FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Ind.-Year-Prov. FE	No	Yes	No	No	Yes	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Organisation Type Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the reduced-sample robustness version of Table 5. The reduced sample is restricted to low-skill, unskilled-intensive manufacturing industries. Permanent workers are divided into direct workers, supervisors and managers, and other permanent workers. The dependent variables are year-over-year log changes in average wages and employment. Columns (1), (2), (4), (5), (7), and (8) report least-squares dummy variable estimates. Columns (3), (6), and (9) report dynamic panel GMM estimates. Standard errors are reported in parentheses. Ind.-Year, Year-Prov., and Ind.-Prov. refer to industry-year, year-province, and industry-province fixed effects, respectively. Ind.-Year-Prov. refers to industry-year-province fixed effects. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Markup

	(1) LSDV	(2) LSDV	(3) DGMM
Panel A. Full Sample			
DLW markup	0.031	0.048	0.039***
	(0.021)	(0.035)	(0.015)
CRS markup	-0.015	0.006	0.012
	(0.015)	(0.022)	(0.015)
CD markup	0.024	0.053	0.031
	(0.030)	(0.047)	(0.022)
Observations	94,922	82,996	35,928
Panel B. Reduced Sample			
DLW markup	-0.018	-0.005	-0.020
	(0.019)	(0.025)	(0.018)
CRS markup	-0.046***	-0.031**	-0.047**
	(0.016)	(0.015)	(0.020)
CD markup	-0.044**	-0.019	-0.054
	(0.021)	(0.036)	(0.033)
Observations	32,515	28,538	11,282
Firm FE	Yes	Yes	Yes
Industry-Year, Year-Province, Industry-Province FE	Yes	No	Yes
Industry-Year-Province FE	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes
Type of Organization Dummy	Yes	Yes	Yes

Notes: This table reports estimated effects of minimum-wage shocks on alternative markup measures. Panel A reports results for the full sample, while Panel B reports results for the reduced sample of low-skill, unskilled-intensive manufacturing industries. Columns (1)–(2) report least-squares dummy variable estimates, while column (3) reports dynamic panel GMM estimates. DLW, CRS, and CD refer to De Loecker–Warzynski, constant-returns-to-scale, and Cobb–Douglas markup measures, respectively. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A6: Markdown

	Overall			Permanent			Contract		
	(1) LSDV	(2) LSDV	(3) DGMM	(4) LSDV	(5) LSDV	(6) DGMM	(7) LSDV	(8) LSDV	(9) DGMM
Panel A. Full Sample									
DLW markdown	-0.079**	-0.087**	-0.051	-0.130***	-0.122**	-0.135*	0.051	0.064	0.236**
	(0.034)	(0.038)	(0.040)	(0.049)	(0.088)	(0.073)	(0.071)	(0.128)	(0.103)
CRS markdown	-0.048	-0.059*	-0.037	-0.100**	-0.088	-0.116**	0.082	0.084	0.223***
	(0.032)	(0.035)	(0.029)	(0.045)	(0.054)	(0.057)	(0.068)	(0.127)	(0.084)
CD markdown	-0.066*	-0.074*	-0.032	-0.116**	-0.100	-0.113	0.081	0.138	0.294**
	(0.040)	(0.045)	(0.046)	(0.053)	(0.062)	(0.075)	(0.087)	(0.150)	(0.143)
Observations	94,922	82,996	35,928	94,922	82,996	35,928	57,718	48,445	19,894
Panel B. Reduced Sample									
DLW markdown	-0.043	-0.058*	-0.042	-0.134***	-0.111**	-0.211*	-0.012	-0.078	0.219*
	(0.026)	(0.034)	(0.058)	(0.052)	(0.053)	(0.124)	(0.055)	(0.083)	(0.115)
CRS markdown	-0.018	-0.032	-0.013	-0.090*	-0.083*	-0.144	-0.000	-0.056	0.188*
	(0.026)	(0.026)	(0.060)	(0.049)	(0.048)	(0.110)	(0.047)	(0.076)	(0.104)
CD markdown	-0.024	-0.049	-0.021	-0.093*	-0.088	-0.164	-0.036	-0.107	0.146
	(0.034)	(0.046)	(0.075)	(0.056)	(0.057)	(0.130)	(0.066)	(0.099)	(0.124)
Observations	32,515	28,538	11,282	32,515	28,538	11,282	17,332	14,446	5,186
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year, Year-Prov., Ind.-Prov. FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Ind.-Year-Prov. FE	No	Yes	No	No	Yes	No	No	Yes	No
Rural / Urban Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Organisation Type Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimated effects of minimum-wage shocks on alternative markdown measures. Panel A reports results for the full sample, while Panel B reports results for the reduced sample of low-skill, unskilled-intensive manufacturing industries. Columns (1)–(3) report overall markdowns, columns (4)–(6) report permanent-worker markdowns, and columns (7)–(9) report contract-worker markdowns. Within each outcome block, the first two columns report least-squares dummy variable estimates, while the third column reports dynamic panel GMM estimates. DLW, CRS, and CD refer to De Loecker–Warzynski, constant-returns-to-scale, and Cobb–Douglas markdown measures, respectively. Standard errors are reported in parentheses. Ind.-Year, Year-Prov., and Ind.-Prov. refer to industry-year, year-province, and industry-province fixed effects, respectively. Ind.-Year-Prov. refers to industry-year-province fixed effects. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

A Data and Variable Construction

We work with firm-level manufacturing panel data. For each firm-year, we construct the key outcomes and inputs as follows. Unless otherwise stated, outcome variables are measured as year-over-year log differences.

Wages. We compute each firm's average wage as the real wage bill divided by total employment. The wage bill includes reported wages and salaries for each worker category. We construct real average wages for all workers combined, permanent workers, and contract workers. For permanent workers, we further distinguish between direct production workers, supervisors and managers, and other employees. All nominal values are deflated using the state-year CPI. The outcome variables are defined as log changes in these real average wage measures.

Employment. Total employment is measured as the firm's total number of persons engaged, defined as the sum of permanent workers and contract workers hired through third-party agencies. We record employment separately for permanent and contract workers and compute the log change for each category. For permanent workers, we further classify employment into three ASI-defined groups: direct production workers, supervisors and managers, and other non-production employees. We then compute the log change in employment for each subgroup.

Total cost of production. ASI defines total cost of production as the sum of materials consumed, labour emoluments, fuel and electricity, and other manufacturing and operating expenses. We use the ASI's reported total cost of production directly, with no adjustments, and take its log change as the outcome variable.

Capital–labour intensity. Capital–labour intensity is defined as real fixed capital, deflated using the capital deflator, divided by total employment. This ratio captures the firm's capital intensity. We use the log change in the capital–labour ratio as the outcome variable.

Labour costs. In the ASI, workers are classified into permanent workers and contract workers hired through third-party agencies. Firm-level labour cost is measured as total emoluments, comprising wage payments, bonuses, welfare expenditures, and employer social contributions. Consistent with institutional features of Indian manufacturing, we assign all non-wage benefits to permanent workers. Although contract workers are formally eligible for some benefits, evidence from administrative records and firm-level surveys suggests that such payments are rare in practice (Das and Pandey, 2004; Singh and Others, 2016). Using ASI panel data from 2001–02 to 2007–08, bonuses and welfare expenditures paid to contract workers account for only 0.5 percent of total non-wage benefits. We therefore define contract-worker labour cost as wage payments only, while permanent-worker labour cost includes both wages and non-wage benefits. As a robustness check, we also construct an alternative measure in which non-wage benefits are allocated proportionally to each worker type based on its share of the total wage bill. This provides an upper-bound estimate of benefit incidence among contract workers. Results using this alternative definition are very similar and are available upon request. The outcome variable is the log change in total labour cost by worker category.

Markups. Markups are constructed using the translog revenue-production framework, following De Loecker and Warzynski (2012) and related extensions. Markups measure the wedge between output prices and marginal costs. We construct DLW, Cobb–Douglas, and constant-returns-to-scale markup measures and use the log change in each markup measure as an outcome variable.

Markdowns. Markdown indices capture rents extracted from labour and are defined as the ratio of a worker's marginal revenue product to wage remuneration. Remuneration is measured as wages plus bonuses, welfare benefits, and employer contributions, following **?**. As with labour costs, we assume that non-wage components accrue only to permanent workers, while contract workers receive wages alone. We construct markdown measures for all workers and separately for permanent and contract workers using the translog system developed by **?**. We use the log change in each markdown index as the outcome variable.

B Measuring Statutory Minimum Wages

A practical challenge in constructing minimum-wage measures in India is that statutory schedules are multi-tiered, with different wage floors defined by skill category, industry, and, in some cases, geographic zone. While these schedules can be aligned with firms along the industry and geographic dimensions, firm-level data do not report the skill composition of workers. As a result, it is not possible to assign the appropriate skill-specific minimum wage to each firm.

Given this constraint, the empirical literature adopts two main approaches. One approach uses the unskilled minimum wage as a proxy for the broader statutory schedule. This strategy implicitly treats the unskilled wage as a summary measure of minimum-wage policy, assuming that wage floors across skill tiers move closely together. An alternative approach restricts attention to sectors in which the workforce is predominantly low-skilled, so that the unskilled minimum wage more closely corresponds to the relevant binding constraint.

We follow the first approach in constructing our baseline measure. Specifically, we use the unskilled minimum wage as a benchmark statutory floor, consistent with widely used administrative series such as those compiled by the Labour Bureau, which are typically reported in a headline form corresponding to the unskilled category. Mansoor and O'Neill (2021), for example, apply the unskilled minimum wage across industries, treating it as a proxy for the statutory schedule.

To assess the empirical validity of this proxy, we examine minimum-wage schedules by skill tier using data for 17 states and 81 industry groups over 2017–18 to 2019–

20. Wage levels are highly correlated across tiers: the correlation between unskilled and semi-skilled wages is 0.991, between unskilled and skilled wages is 0.945, and between unskilled and high-skilled wages is 0.857. More importantly, year-to-year percentage changes co-move closely within state–industry cells. The correlation between unskilled and semi-skilled changes is 0.981, between unskilled and skilled changes is 0.979, and between unskilled and high-skilled changes is 0.970. Regressions of tier-specific changes on unskilled changes yield R^2 values above 0.94 across all tiers.
These results indicate that the unskilled wage closely tracks both the level and evolution of the broader wage schedule. Nevertheless, because the mapping from statutory schedules to firm-level exposure cannot account for worker skill composition, this approach may introduce measurement error when firms employ predominantly higher-skilled workers. In the empirical analysis, we address this concern using an alternative sample construction that more closely aligns the statutory wage with the relevant workforce.

C Sample Selection

We implement two complementary empirical strategies that differ in how the statutory minimum wage is mapped to firms.
The first strategy follows Mansoor and O'Neill (2021) and uses the full sample of registered manufacturing firms in the Annual Survey of Industries (ASI). We restrict the sample to firms whose two-digit NIC-2008 industry codes fall between 10 and 33, consistent with the standard manufacturing classification. In this specification, the unskilled minimum wage is applied uniformly across firms within a given state–industry cell, treating it as a proxy for the broader statutory schedule. This approach relies on the assumption, supported by the evidence in Appendix B, that minimum wages across skill tiers move closely together over time.
The second strategy addresses potential measurement error arising from the inability to observe firm-level skill composition. Following Soundararajan (2019), we restrict the sample to industries in which the workforce is predominantly low-skilled, so that the unskilled minimum wage is more likely to represent the relevant binding wage floor. To identify such industries, we combine two independent measures of labour skill. The first is the India KLEMS Labour Quality Index (LQI), averaged over 2011–12 to 2022–23, which captures the skill intensity of labour inputs. The second is the share of unskilled workers by industry, constructed from the Periodic Labour Force Survey (PLFS). Industries that fall below the median in labour quality, based on KLEMS, and above the median in unskilled share, based on PLFS, are classified as unskilled-intensive.
This intersection yields ten two-digit NIC-2008 industries that both datasets consistently identify as low-skilled: 12 (tobacco products), 13 (textiles), 14 (wearing apparel), 15 (leather products), 16 (wood products), 23 (non-metallic minerals), 24 (basic metals), 25 (fabricated metal products), 31 (furniture), and 32 (other manufacturing). These sectors are well documented to rely heavily on unskilled and semi-skilled labour. In this restricted sample, the unskilled minimum wage is more likely to correspond to the

wage floor faced by the modal worker, thereby reducing concerns about mismeasurement.

For both samples, we apply consistent filtering criteria. We retain only firm-year observations with strictly positive values of output, employment, and wage payments to ensure that log-difference outcomes are well defined. We exclude firms that change state location or industry classification over the sample period in order to preserve stable exposure to statutory minimum wages. Because the analysis focuses on differential adjustment between permanent and contract labour, we further restrict the sample to firms that employ both worker types at least once between 2014 and 2022. This restriction ensures that the estimated effects capture within-firm reallocation rather than differences across structurally distinct firms.

Finally, to mitigate the influence of extreme outliers, we trim observations in which the exposure index (*relw*), the log change in real average wages, or the log change in employment falls below the 1st percentile or above the 99th percentile of their respective distributions. We also restrict the panel to consecutive firm-year observations (*yeargap* = 1), consistent with the use of year-over-year log changes.

D Markup and Markdown Estimation

This appendix describes how we construct markup and markdown measures for permanent and contract workers. The procedure combines elements from **?** and **?**, adapted to the Indian firm-level data environment.

A markup is defined as the ratio of a firm's output price to its marginal cost and measures the extent to which the firm charges above the cost of producing an additional unit of output. Higher markups indicate stronger product-market power, arising for example from product differentiation, entry barriers, or market concentration. In practice, marginal costs and prices are rarely observed separately in firm-level data; most production datasets only report revenues and cost aggregates. Estimating markups therefore requires an indirect approach that combines information on input expenditure with assumptions on technology and firm optimization.

We follow the production-function approach of De Loecker and Warzynski (2012), which exploits the first-order condition for a variable, price-taking input. Under cost minimisation, the ratio of that input's output elasticity to its cost share in revenue identifies the firm's markup. For a flexibly chosen input M, which we take to be materials, the markup for firm i in year t is

$$\mu_{it}^{M} = \frac{\theta_{it}^{M}}{\alpha_{it}^{M}}, \tag{7}$$

where θ_{it}^{M} is the output elasticity with respect to materials and α_{it}^{M} is the materials expenditure share in total revenue. This formula requires that materials be purchased in a perfectly competitive market, so that the wedge between θ_{it}^{M} and α_{it}^{M} reflects output-market distortions. If materials markets themselves exhibit monopsony power or other frictions, the resulting index conflates input- and output-market distortions. In line with the industrial-organisation literature, we treat materials as the flexible, price-taking input and interpret μ_{it}^{M} as a measure of product-market power.

Recent work by **?** and **?** clarifies that markups constructed from labour inputs generally conflate two distinct sources of distortion: product-market power and labour-market rents. A labour-based markup reflects not only firms' ability to charge prices above marginal cost in output markets, but also their ability to pay wages below the marginal revenue product of labour when labour markets are imperfectly competitive. This insight motivates a decomposition strategy that separates output- and labour-market distortions by comparing markups constructed from different variable inputs. When materials are assumed to be competitively supplied, a materials-based markup captures distortions arising from product-market power. In contrast, a labour-based markup embeds both product-market power and labour-market distortions. Taking the ratio of the labour-based markup to the materials-based markup nets out the common output-market component, leaving a residual that reflects distortions specific to the labour market. This ratio, commonly referred to as a markdown, measures the extent to which firms pay wages below the marginal revenue product of labour, abstracting from product-market power.

In the Indian context, an important feature is the coexistence of permanent and contract workers. We therefore extend the standard framework to allow for two distinct labour inputs. Let $\mu_{it}^{L,p}$ and $\mu_{it}^{L,c}$ denote the labour-based markups computed using permanent and contract workers, respectively, and let μ_{it}^{M} be the materials-based markup. We define two markdown indices:

$$md_{it}^{p} = \frac{\mu_{it}^{L,p}}{\mu_{it}^{M}}, \qquad md_{it}^{c} = \frac{\mu_{it}^{L,c}}{\mu_{it}^{M}}. \tag{8}$$

These indices capture rents extracted from permanent and contract workers separately. A value above one indicates that wages lie below the marginal revenue product of the corresponding labour type, after netting out product-market power.

To implement this framework, we estimate a second-order translog gross-output production function with four inputs: permanent labour, contract labour, capital, and materials. Estimation uses the control-function approach of **?**. The production function includes all first-order terms, second-order terms, and interactions among inputs, allowing for flexible substitution patterns both between labour types and across labour, capital, and materials. From the estimated coefficients, we compute firm-specific elasticities of output with respect to materials, θ_{it}^{M}, and combine them with observed materials cost shares to obtain the DLW markup, μ_{it}^{M}.

Following **?**, we complement this baseline measure with two alternative markup indices that rely on different identifying assumptions. First, the constant-returns-to-scale (CRS) markup is defined as the ratio of total revenue to the sum of expenditures on labour, materials, and capital services:

$$\mu_{it}^{CRS} = \frac{Y_{it}}{W_{it}L_{it} + rK_{it} + P_{it}^{M}M_{it}}, \tag{9}$$

where the user cost of capital r is set to 13 percent, reflecting an 8 percent cost of capital and 5 percent depreciation. This measure makes no parametric assumptions on the production function and allows for rich firm heterogeneity, but it attributes all profits in excess of the user cost of inputs to product-market power and assumes perfectly competitive input markets. Second, the Cobb–Douglas (CD) markup applies the DLW formula under the assumption that the production function is Cobb–Douglas, which implies that markups vary inversely with the materials share. Because this approach does not identify the overall level of the markup, **?** set the scale so that the average CD markup matches the average gross-profit, CRS markup. In practice, the CD markup is obtained by rescaling the CRS markup according to a firm's inverse materials share, raising the markup for firms that use relatively little materials and lowering it for firms that are more materials-intensive. This adjustment introduces the Cobb–Douglas structure while preserving the CRS benchmark on average.

These three methods—DLW, CRS, and Cobb–Douglas—generate three corresponding sets of product-market markups, μ_{it}^{DLW}, μ_{it}^{CRS}, and μ_{it}^{CD}. For each markup definition, we construct labour-based markups for permanent and contract workers using their wage bills and cost shares. Specifically, we compute the wage shares of permanent and contract labour in revenue, $\alpha_{it}^{L,p}$ and $\alpha_{it}^{L,c}$, and define labour-based markups as

$$\mu_{it}^{L,l} = \frac{\theta_{it}^{L,l}}{\alpha_{it}^{L,l}}, \qquad l \in \{p, c\}, \tag{10}$$

where $\theta_{it}^{L,l}$ denotes the corresponding labour elasticity. Combining these labour-based markups with each product-market markup $m \in \{DLW, CRS, CD\}$ yields six markdown indices:

$$md_{it}^{l,m} = \frac{\mu_{it}^{L,l}}{\mu_{it}^{M,m}}, \qquad l \in \{p, c\}, \quad m \in \{DLW, CRS, CD\}. \tag{11}$$

Following **?**, we interpret the comovement of markdowns with firms' labour-market shares as evidence of rents associated with labour-market concentration. We define labour markets as state–four-digit-industry cells in a given year. Within each labour market, we compute each firm's wage-bill share for permanent and contract workers, denoted s_{it}^{p} and s_{it}^{c}. For each combination of markdown method m and labour type l, we estimate the relationship between markdowns and labour-market shares using the instrumental-variables specification

$$md_{it}^{l,m} = \Gamma_t + \delta_i + \beta_p^m s_{it}^p + \beta_c^m s_{it}^c + \varepsilon_{it}. \tag{12}$$

Both labour-market shares are treated as endogenous and instrumented with the firm's output share in the same state–industry–year market, together with lagged labour-market shares. We rescale the markdowns so that the implied intercept in the above equation equals one. This ensures that the markdown of a firm with negligible labour-market share is normalised to unity. The normalisation allows values above one to be interpreted as proportional distortions associated with labour-market rents.

We also verify that the normalisation is not sensitive to how labour-market shares are specified. Omitting the permanent-worker share when normalising markdowns for contract workers, or vice versa, produces results that are essentially identical to the specification that includes both shares in the same equation. This confirms that our markdown measures are robust to alternative ways of modelling labour-market shares.