Medicine or Poison? Minimum Wages for Chinese Firms and Workers

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Abstract: This paper investigates firms' responses to minimum wage increases along two key margins: markups, defined as the ratio of price to marginal cost, and markdowns, defined as the gap between the marginal product of labour and the wage. Using the Chinese Annual Survey of Industrial Firms (CASIF) from 2002 to 2007 and exploiting city-time variation and provincial border discontinuities, we document sharp contrasts before and after China's 2004 minimum wage reform. Prior to the reform, when enforcement was weak and part-time workers were largely uncovered, more exposed firms reduced wages and widened markdowns, while markups showed little systematic response. After the reform, when enforcement strengthened and coverage expanded, more exposed firms raised wages and reduced markdowns, yet markups remained broadly stable. Our mechanism analysis shows that this stability resulted from a transition from evasive tactics, such as reliance on uncovered workers, to structural adjustments involving employment reductions, capital deepening, and productivity improvements. These adjustments varied by ownership. Foreign firms recorded the largest wage gains, the steepest markdown reductions, and the strongest efficiency improvements, enabling them to sustain markups. Private firms adjusted in the same direction but less strongly, while state-owned enterprises exhibited the weakest responses and the highest exit rates.

Key Words: Minimum Wage, labour Market, Market Concentration, Monopsony
JEL Classification: J3; J42; J2

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[2]We are grateful for the valuable comments and support from Christopher Woodruff and Roberto Samaniego. We extend special thanks to Lixin Tang for providing data on cross-provincial city pairs. We also appreciate the insightful feedback from seminar participants at Cardiff Business School, the 2023 ISI Delhi Conference, and the 2024 Asian and Australasian Society of Labour Economics Conference. The authors used AI-assisted language tools solely for readability improvements. All conceptual development, analysis, and interpretation remain the sole responsibility of the authors.

1. Introduction

Governments often regard minimum wage policies as a straightforward tool for improving worker welfare and reducing income inequality. In practice, however, their effects depend on how firms strategically adjust to higher labour costs, with adjustments across wages, prices, employment, input choices, exporting, and investment, among other margins. A substantial body of research has documented such responses, and in China recent studies have examined comparable effects (Mayneris et al., 2018; Fan et al., 2018; Gan et al., 2016; Li et al., 2016; Hau et al., 2020). Yet how firms adjust markups (the margin between prices and costs) and markdowns (the gap between workers' productivity and their pay) remains underexplored, even though China's complex economy and regional disparities provide a rich setting for such analysis.

Addressing this gap is important because markups and markdowns are not only firm-level pricing outcomes but also direct indicators of market power. Evidence from the minimum wage literature suggests that firms' responses are often conditioned by the degree of market power they exercise in product and labour markets. For instance, in product markets, firms with greater pricing power can pass higher labour costs on to consumers, protecting profit margins but reducing consumer welfare, whereas in more competitive settings, limited pass-through compresses profits and compels adjustment through alternative channels (Draca et al., 2011; Harasztosi & Lindner, 2019). In labour markets, the degree of monopsony power shapes whether higher minimum wages translate into increased pay or lead to employment losses (Card & Krueger, 1994; Aaronson & French, 2007; Neumark et al., 2014; Dube et al., 2010; Azar et al., 2025). Similar dynamics also arise in other contexts, including trade liberalisation and technological change, where shifts in firm-level market power mediate the distributional consequences of policy reforms (De Loecker et al., 2020; Berger et al., 2025).

Given the central role of market power, typically measured through firm-level markups and markdowns, a natural question is whether policy shocks like minimum wage hikes can themselves shift these margins. Evidence from other policy domains suggests they can. For instance, Brandt et al. (2017) show that China's WTO accession reduced tariffs, with output tariff cuts lowering markups but raising productivity, while input tariff cuts increased both. Building on the same event, Kondo et al. (2024) find that input tariff reductions reduced labour markdowns. Within the industrial organisation literature, Brooks et al. (2021a) find that large-scale infrastructure investments in India reduced labour monopsony power. However, the direct impact of minimum wage policy on these firm-level margins remains underexplored until now.

We advance this literature by providing the first unified analysis of how minimum wages in China influence both product markups and labour markdowns. This contribution is meaningful because China's 2004 reform created a clear divide between weak and strong enforcement regimes, while strong ownership heterogeneity (Hau et al., 2020) allows us to capture varied firm responses. Using panel data from the Chinese Annual Survey of Industrial Firms (CASIF) for 2002–2007, we examine how firms adjusted strategically to minimum wage changes under these differing enforcement conditions. China's rapid structural transformation and the widespread adoption of minimum wages make it a particularly valuable setting to study how stronger enforcement shapes firms' ability to adjust along both the product-market (markups) and labour-market (markdowns) margins.

Credibly estimating the effects of minimum wage policies presents two main challenges: (i) firm-level datasets rarely report the distribution of wages within firms, making it impossible to determine how many workers are directly affected by policy changes and thus to measure the policy's bindingness; and (ii) minimum wage setting may be endogenous, with statutory rates often responding to local economic conditions. To address the first challenge, we use the Impact Function (IF) approach of Hau et al. (2020), which infers firm-level exposure from the gap between the average wage and the local statutory minimum wage. This continuous, nonlinear measure captures temporal variation in firm-level exposure without requiring detailed wage distribution data or imposing arbitrary thresholds. To address the second challenge, we employ two complementary specifications. Our baseline regression uses the full firm-level panel with extensive fixed-effect controls to absorb unobserved sectoral and local shocks. As an alternative, we use a border discontinuity design that exploits China's provincial-level minimum wage system, where neighbouring cities on different sides of a provincial border often have different minimum wage rates despite similar economic conditions (Fan et al., 2018, 2021). Together, these approaches allow us to examine how minimum wage policies, under varying levels of enforcement credibility, affect firms' price-setting behaviour and the distribution of economic rents between firms and workers.

Our results reveal a marked shift in firms' pricing behaviour following the 2004 reform that strengthened minimum wage enforcement. Before the reform, when enforcement was weak, firms more exposed to minimum wage increases paradoxically reduced their average wages relative to less-exposed firms, reflecting partial compliance or the use of regulatory loopholes. After the reform, when the minimum wage became a binding constraint, exposed firms significantly raised wages, indicating that such strategies were no longer viable. These wage dynamics are mirrored in markdowns: before the reform, more exposed firms increased their markdowns, thereby expanding labour-market rents, whereas after the reform markdowns declined substantially. Markup responses, by contrast, were minimal and not robustly significant, suggesting that firms did not systematically raise price–cost margins in response to higher wages.

This finding motivates a closer examination of adjustment mechanisms, since in the absence of alternative strategies, markups would normally be expected to move in the same direction as markdowns. Before the reform, more exposed firms relied on more labour-intensive, reducing capital intensity and labour productivity. After the reform, the evidence indicates that firms absorbed higher labour costs by combining cost-reducing adjustments—reducing employment, increasing capital intensity, and experiencing higher exit rates— with productivity enhancements that raised both total factor productivity and value added per worker. These adaptations allowed firms to sustain markups even as labour rents declined. Overall, stronger enforcement compressed rents in the labour market while leaving product-market rents largely intact.

The magnitude and nature of these adjustments differed sharply across ownership types. Before the reform, when enforcement was weak, private firms showed the clearest evidence of cost evasion: minimum wage hikes in these firms were associated with lower average wages, higher markdowns, reduced capital intensity, and expanded employment, reflecting reliance on cheaper uncovered labour and other cost-saving strategies. Foreign firms also expanded employment, though less strongly than private firms, and their most exposed establishments experienced significant productivity declines, consistent with dependence on low-wage labour. SOEs, by contrast, exhibited no significant changes in wages, markdowns, or productivity. After the reform, when enforcement tightened, the pattern reversed. Foreign-owned firms showed the largest response across multiple dimensions: wage gains, markdown reductions, employment contractions, capital deepening, and productivity improvements. These adjustments enabled them to slightly raise markups despite higher labour costs. Private firms responded in similar ways, but more moderately, and their markups remained stable. SOEs showed only modest markdown declines, no meaningful productivity growth, and falling markups, alongside the highest exit rate. Overall, stronger enforcement reshaped firms' markups and markdowns unevenly: foreign firms reduced markdowns and modestly increased markups, private firms saw markdown declines but stable markups, while SOEs experienced only slight markdown reductions and were the only group with falling markups.

These findings make two contributions to the literature. First, our paper builds on methodological advances that allow researchers to estimate both markups and markdowns at the firm level (De Loecker & Warzynski, 2012; Yeh et al., 2022; Brooks et al., 2021b). These indices have been applied to major shocks, such as WTO accession and infrastructure investment (Brandt et al., 2017; Brooks et al., 2021a; Kondo et al., 2024), but their application to minimum wage policy remains limited. Du and Wang (2020) provide the first evidence that minimum wage changes affect product markups in China, while Casacuberta and Gandelman (2023) show that wage councils in Uruguay influenced both markups and markdowns. We extend this literature by jointly analysing markups and markdowns in the context of China's 2004 minimum wage reform, contrasting firm behaviour when the policy was weakly versus strongly binding, and highlighting the role of enforcement credibility in shaping adjustment margins.

Second, we contribute to the literature on minimum wage enforcement and compliance. When coverage is narrow and sanctions are weak, the legal minimum often fails to bind because firms can shift to uncovered contracts or simply fail to pay it. Recent work documents these institutional channels: Kim and Samaniego (2024) show how limited monitoring and penalties enable increasing non-compliance in response to minimum wage hikes; Mansoor and O'Neill (2021) show in the Indian context that the wage and consumption gains from minimum wages are sharply reduced in low-compliance regimes; and Soundararajan (2019) shows that weak compliance diluted the impact of minimum wages in India's textile sector. The study most directly related to ours is Mayneris et al. (2018), who highlight how China's 2004 reform, which strengthened enforcement, shaped heterogeneous firm-level responses to minimum wage increases. We extend this literature by comparing firm behaviour before and after the 2004 reform, showing how changes in enforcement credibility reshaped adjustment margins, including markups and markdowns.

Third, our study contributes to the literature by showing how ownership structure and managerial capacity shape firms' responses to policy shocks. Hau et al. (2020), building on the management practices literature in economics (Bloom and Van Reenen, 2007, 2010; Bloom et al., 2010), document ownership-specific productivity responses to minimum wage policies in China. We extend this line of research to the distribution of rents in product and labour markets, showing that the patterns they identify, including not only productivity improvements but also cost-saving strategies such as reducing employment and capital deepening, translate into changes in both markups and markdowns.

The remainder of the paper is organized as follows. Section 2 provides an overview of China's minimum wage policy and describes the firm-level data, including the construction of key variables and summary statistics before and after the 2004 reform. Section 3 outlines the empirical strategy, including the baseline specification and the border discontinuity design. Section 4 presents the main results and discusses the underlying adjustment mechanisms. Section 5 reports robustness checks, including pre-trend analysis, sub-sample tests, and alternative measures. Section 6 examines heterogeneous effects by ownership type. Section 7 concludes.

2. Data

This section begins by outlining the institutional features of China's minimum wage policy, with particular attention to the 2004 reform. It then presents descriptive statistics on minimum wages and firm-level outcomes before and after the reform.

2.1 Minimum wage policy in China

China's minimum wage system is decentralized in practice but hierarchical in design, and it has undergone major institutional reform. Since the early 1990s, provinces have held authority to set wage floors, tailoring them to local economic conditions and labour market dynamics. Within provinces, counties are grouped by development level and usually adopt uniform standards within each group. Counties can propose adjustments or request lower-tier rates, but final decisions rest with provincial governments. This structure produces substantial geographic variation in minimum wages, even across economically similar counties. The variation reflects both responsiveness to regional disparities and limits to local autonomy. Peripheral counties, for example, often lack the influence to shape group-level standards and instead receive wage levels driven by provincial priorities.

While this structure generates meaningful spatial variation that we later exploit in our empirical design, the overall effectiveness of China's minimum wage policy was historically limited by both gaps in legal coverage and weak enforcement. Prior to 2004, the policy applied only to full-time employees with formal labour contracts, leaving part-time, temporary, and informal workers outside its scope. Firms often adjusted employment structures to move workers into these uncovered categories, thereby avoiding minimum wage obligations without technically breaking the law. At the same time, even when violations occurred, enforcement mechanisms lacked credibility. Penalties were modest, typically set between 20% and 100% of unpaid wages, and implementation was uneven across regions.

The 2004 reform addressed several critical weaknesses of the previous regime. Most notably, it extended legal coverage to part-time and casual workers by introducing a parallel hourly minimum wage alongside the monthly wage for full-time employees. The reform also increased penalties for violations, setting them between 100% and 500% of unpaid wages, and required local governments to review and adjust minimum wages at least every two years, with changes made public within seven days. These measures transformed the minimum wage from a weakly enforced guideline into a more transparent and enforceable policy instrument.

Figure 1 illustrates how the 2004 institutional shift affected both minimum wage trends and firm-level compliance behavior. Panel A plots the trajectory of real monthly minimum wages from 2001 to 2007 (CPI-deflated to 1998 levels), with the vertical line at 2003.5 marking the onset of the reform period. While statutory wages rose steadily prior to 2004, the slope increases noticeably after the reform, indicating an intensification of provincial wage-setting. Panel B, which aligns with our empirical specification, displays the annual log growth rate of real minimum wages. After a modest deceleration in 2003, growth accelerates sharply in 2004 and remains elevated through 2006, consistent with a structural break in policy implementation. A marked decline in 2007 suggests a tapering of post-reform momentum.

Panels C and D document the corresponding changes in firm-level compliance patterns. Panel C presents kernel density distributions of normalized wages, defined as the log difference between a firm's average wage and the applicable statutory minimum, for selected years. In 2001 and 2003, the distribution is wide and left-skewed, with a significant share of firms paying below the legal minimum. By 2005 and 2007, the distribution compresses and shifts rightward, reflecting increased alignment with statutory wage floors. Panel D plots two key metrics from 2001 to 2007: non-compliance, defined as the share of firms paying average wages below the minimum wage in year t, and exposure, defined as the share of firms whose average wage in year t–1 fell below the minimum set for year t. Before the reform, non-compliance rates consistently exceeded exposure, reflecting firms' reliance on contract types not formally covered by the regulation, as well as outright violations. After the reform, this relationship reverses, with non-compliance falling below exposure, indicating a fundamental shift in enforcement credibility and regulatory reach. [3]

Taken together, the four panels in Figure 1 indicate that the 2004 reform significantly raised wage floors, reduced non-compliance, and transformed the minimum wage into a credible labour cost shock. We leverage this institutional change to examine how firms adjust their pricing and wage-setting behavior, captured by markups and markdowns, focusing on mechanisms of adjustment such as cost-saving strategies and productivity-enhancing investments. The next section presents descriptive statistics for these outcomes across the pre- and post-reform periods, laying the groundwork for the empirical analysis that follows.

2.2. Data and Market Concentration

The firm-level data used in this paper comes from the Chinese Annual Survey of Industrial Firms (CASIF), conducted by the National Bureau of Statistics (NBS). This widely utilized dataset includes all state-owned enterprises (SOEs) and medium to large non-SOEs with annual sales exceeding 5 million RMB. CASIF provides comprehensive information on firm characteristics, including location, industry, ownership type, employment, and detailed financial statements, making it a valuable resource for analyzing firm behavior and market dynamics. Crucially, the panel includes detailed data on revenue, capital, labour, and intermediate inputs, allowing us to estimate markups and markdowns using production function–based methods, following De Loecker and Warzynski (2012) and Brooks et al. (2021).

[3] While we use the term "non-compliance" for consistency, we acknowledge that, prior to 2004, many of these instances may not have constituted legal violations because of limited policy coverage.

To ensure data quality, we follow standard practices in the literature to clean and prepare the dataset. We exclude firms with implausible financials, such as cases where liquid assets exceed total assets, total fixed assets surpass total assets, or net fixed assets (i.e., original value minus accumulated depreciation) exceed total assets. Observations are also dropped if the firm employs fewer than eight workers or reports missing, zero, or negative wage data. To maintain consistency in regional analysis, we exclude firms that changed their registered location during the study period. We retain only those firm-year observations for which a valid one-year lag is available, as our analysis is based on first-differenced outcome variables. Since our primary outcomes are firm-level markups and markdowns, we further restrict the sample to observations where these measures can be computed. After applying all restrictions, the final cleaned dataset consists of 1,077,737 firm-year observations, with our core analysis focusing on the years 2002 to 2007.

We compute firm-level markups and markdowns following the conceptual framework of De Loecker and Warzynski (2012), hereafter the DLW approach. In this framework, markups are defined as the ratio of output price to marginal cost, capturing rents in product markets, while markdowns are defined as the ratio of the marginal product of labour to the wage, reflecting rents in labour markets. Although our representative indices are based on the DLW approach, we also follow Brooks et al. (2021b) in estimating markups and markdowns using three alternative methods: the baseline DLW approach, a constant returns to scale (CRS) variant, and a Cobb–Douglas (CD) specification. These approaches differ in their production function assumptions and estimation strategies but yield conceptually comparable measures of market power. A detailed description of each index is provided in the Appendix. Table 1 presents descriptive statistics for markups and markdowns, using the DLW estimates as our main indices.

To measure firm-level exposure to minimum wage shocks, we adopt the continuous exposure approach of Hau et al. (2020). We construct a firm-specific impact function, $IF_{nt}(= \omega_{t-1}/MW_{t-1})^{-k}$, which reflects the proximity of a firm's average wage to the statutory minimum in a given year and location. Higher values of IF indicate greater exposure, as they correspond to firms whose average wages are close to or below the minimum wage. In contrast, firms with higher average wages face little exposure to minimum wage hikes. This continuous measure improves upon the binary treatment indicators used in earlier work (e.g., Draca et al., 2011; Mayneris et al., 2018) by capturing heterogeneous treatment intensity. The interaction term $IF \times \triangle ln(MinimumWage)$ captures the firm-specific treatment effect of minimum wage changes, scaled by exposure.

Table 1 reports descriptive statistics for the full sample, as well as separately for the pre-reform (2002–2003) and post-reform (2004–2007) periods. Panel A summarizes policy-related variables, including the log change in the minimum wage, the exposure function (IF), and their interaction, while Panel B presents log-differenced firm-level outcomes. All figures report average annual log changes in output.

The average statutory minimum wage growth rose from 5.6% in the pre-reform period to 9.3% post-reform, reflecting the intensification of policy implementation after 2004. In contrast, the mean value of the exposure function IF declined moderately from 0.630 to 0.519, indicating that firms raised wages relative to the minimum, thereby reducing their exposure despite stronger policy pressure. Accordingly, the average value of the interaction term $IF \times \triangle ln(MinimumWage)$ increased from

3.2% to 4.5%, driven primarily by local minimum wage growth rather than systematic increases in firm-level exposure.

Turning to firm outcomes in Panel B, average real wages grew much faster after the reform, rising from 6.8% per year pre-reform to 12.7% post-reform. Consistent with this, markdown patterns also reversed: they increased by 3.0% before the reform but declined by 1.5% afterward, reflecting that wage gains surpassed labour productivity increases, since markdowns capture the wedge between the two. By contrast, Markups changed more modestly, at 1.3% pre-reform and 0.9% post-reform. Employment growth accelerated from 1.2% to 2.9%, while capital–labour ratios shifted from a decline of 1.6% to essentially no change. Firm exit rates dropped markedly, from 14.2% before the reform to 6.5% afterward. Productivity measures also improved: TFP growth remained high, though it eased slightly from 16.0% to 13.4%, while value-added per worker rose more strongly, from 16.5% to 22.4%. Although these before–after comparisons suggest monopsonistic labour markets, where binding minimum wages can raise both wages and employment, they cannot be taken at face value. The observed patterns may also reflect broader structural forces such as China's WTO accession and rapid export expansion that coincided with the reform period. To disentangle the role of minimum wages from these concurrent shifts, we now turn to our empirical specification, which exploits variation across firms and regions to identify the causal impact of the policy.

3. Empirical Specification

A central challenge in studying the effects of minimum wage policy using firm-level data is the absence of information on individual workers. In our setting, only average wage payments per firm per year are observed, leaving the researcher unable to directly identify the share of workers affected by the minimum wage. To overcome this limitation, we adopt and extend the empirical strategy developed by Hau et al. (2020), who propose a method for estimating the incidence of minimum wage shocks when only firm-level average wages are available. Their framework models firm heterogeneity in wage responses through a nonlinear impact function of the form, $IF_{nt}(=\omega_{t-1}/MW_{t-1})^{-k}$, where ω_{t-1} denotes the firm's average wage in the previous period and MW_{t-1} is the corresponding minimum wage. Interacting this term with the log change in the minimum wage between periods $t-1$ and t, the model captures how the impact of minimum wage changes varies depending on a firm's proximity to the statutory minimum wage in the previous year. Firms closer to the minimum wage are assumed to experience larger marginal effects, and the curvature of this relationship is governed by a single parameter, k: a larger value of k places greater weight on firms whose average wage in the previous year was further below the statutory minimum wage, thereby amplifying the estimated effect for those more exposed to the policy. To operationalize this framework, we estimate the curvature parameter k that empirically capture the relationship between firms' initial exposure and their subsequent wage adjustments to policy shocks, using a wage regression as formally specified below.

$$\triangle ln\omega_{nict} = \beta_0 + \beta_1 IF_{nt} + \beta_3 \left[IF_{nt} \times \triangle lnMW_{ct}\right] + \mu_{i,t} + \delta_{c,t} + \gamma_o + \epsilon_{nt} \tag{1}$$

where $\triangle ln\omega_{nict}$ denotes the log change in the average wage paid by firm n, operating in industry i and located in city c, between periods $t-1$ and t. $\triangle lnMW_{ct}$ is the log change in the statutory minimum wage at the city-year level between periods $t-1$ and t. The regression includes industry-year fixed effects $\mu_{i,t}$, city-year fixed effects $\delta_{c,t}$, and ownership fixed effects γ_o to account for common shocks across sectors, regions, and ownership types, respectively.

Importantly, the inclusion of city-year fixed effects absorbs all variation in the level of minimum wage changes across space and time. Consequently, identification of the interaction term $IF_{nt} \times \triangle lnMW_{ct}$ relies on within-city-year comparisons across firms with differing exposure to the policy. That is, while the policy shock is held constant within each city-year, firms differ in how binding the minimum wage is for them, proxied by IF_{nt}. Controlling separately for IF_{nt}, the interaction term captures how the same minimum wage shock has differential effects depending on firms' initial exposure, thereby isolating the amplification mechanism at the heart of our framework.

To estimate the curvature parameter more accurately, we follow Hau et al. (2020) in dividing firms into three size-based groups: small firms with fewer than 200 employees, medium firms with 200 to 999 employees, and large firms with 1,000 or more employees, in order to account for heterogeneity in wage adjustments by firm size. For each group, we conduct a grid search over 100 values of $k \in (0.01, 1.00)$ in 0.01 increments. For each candidate value of k, we construct the nonlinear exposure term, $IF_{nt} = (\omega_{t-1}/MW_{t-1})^{-k}$, and estimate a log-difference regression as specified Equation (1). We record the coefficient estimates, standard errors, and residual sum of squares (RSS) across iterations, and select the value of k that minimizes the RSS within each firm size group.[4]

Once the optimal curvature parameter k is estimated for each firm size group, we follow Hau et al.'s (2020) theoretical derivation to reinterpret the exposure term in elasticity form. Specifically, we translate $IF(k)$ into an adjusted exposure term $IF(k+1)$, based on the identity:

$$\frac{dln\omega_n}{dlnMW} = \frac{MW}{\omega_n} \cdot \frac{d\omega_n}{dMW} = \frac{MW}{\omega_n} \cdot IF_n(k) = IF_n(k+1) \tag{2}$$

This transformation allows us to interpret the interaction term $IF_{nt} \times \triangle lnMW_{ct}$ as capturing the elasticity of firm wages with respect to minimum wage changes—that is, the percentage change in firm-level wages associated with a 1% change in the minimum wage, scaled by firm exposure. Accordingly, all subsequent regressions examining the effects of minimum wage policy on firm outcomes are estimated using $IF(k+1)$, where the estimated elasticity of firm wages is treated as fixed. This

[4] While our estimation procedure differs from Hau et al. (2020)'s nonlinear least squares (NLLS) method, it remains closely aligned in purpose and structure. Hau et al. estimate the curvature parameter k using a level-difference specification with NLLS to capture the nonlinear relationship between wage changes and firm-level exposure to the minimum wage. Their approach includes industry-year fixed effects but omits more granular controls such as city-year or ownership fixed effects in order to maintain tractability within a nonlinear framework. By contrast, our setting requires richer controls to account for variation in local policy implementation and institutional structure. Incorporating high-dimensional fixed effects renders the NLLS estimator computationally unstable and sensitive to starting values. To address this, we adopt a linearized grid-search procedure within a log-difference specification. This approach stabilizes the scale of both dependent and explanatory variables and enables robust estimation using standard fixed-effects regressions.

reflects the idea that firms' wage responsiveness to minimum wage shocks is the core adjustment margin, and serves as the primary mechanism through which minimum wage policy propagates to other firm outcomes such as employment, productivity, or pricing behavior.

This approach is particularly practical in settings like ours, where detailed wage distributions are unobserved and only average firm-level wages are available. Interpreting $IF(k+1)$ as a firm-specific wage elasticity provides a structurally grounded and tractable proxy for exposure to minimum wage shocks. The credibility of this approach, however, relies on two key assumptions: (1) that average wages meaningfully reflect a firm's proximity to the minimum wage, and (2) that wage costs are the primary channel through which minimum wage policy affects firm behavior. We examine each of these assumptions in turn below.

First, our estimation strategy relies on the standard assumption that average wage levels provide a meaningful proxy for a firm's exposure to the minimum wage. The CASIF dataset is compiled by the National Bureau of Statistics for official statistical purposes, and firms report data under strict confidentiality, reducing incentives to misreport. Moreover, the survey targets registered industrial firms, which tend to operate in formal labour markets, where average wages are reliably measured. The potential concern arises not from misreporting but from enforcement credibility: before 2004, when coverage was incomplete, our impact function may deviate from true exposure if some workers were outside the scope of the policy. In our setting, this mismeasurement reflects how much the IF proxy misses true exposure under weak enforcement.Conditional on rich industry–year, city–year, and firm fixed effects that absorb systematic differences in wage structures and enforcement intensity, it is reasonable to assume that the remaining firm-level noise is not systematically correlated with true exposure. Under this assumption, any measurement error attenuates coefficients toward zero, so our pre-reform estimates should be interpreted as conservative. After 2004, stricter enforcement and broader coverage reduced this concern, making our exposure measure more reliable and strengthening the credibility of our identification.

Second, our framework assumes that the primary channel through which minimum wage policy affects firm outcomes such as markups, markdowns, or exit is through wage costs. This is a standard assumption in the literature and aligns with economic models where labour cost shocks directly affect firm behaviour. Other channels may exist, including compliance-related fixed costs, regulatory uncertainty, or shifts in consumer demand, but these are typically second-order and less readily observed in firm-level data. By focusing on wage costs, our approach captures the most direct and empirically tractable pathway through which the policy operates. Nonetheless, we interpret our estimates as reflecting wage-driven effects while recognising that ancillary mechanisms may also contribute.

The estimated curvature parameters are 0.33 for small firms, 0.36 for medium firms, and 0.49 for large firms, broadly in line with Hau et al. (2022). These estimates imply that the impact function places more weight on large firms with wages close to or below the minimum, suggesting stronger adjustment pressures when the policy binds. For example, with $k = 0.33$ (small firms), a low-wage firm where the minimum wage is 80% of the average wage is 58% more exposed than a high-wage firm where it is only 20% $\left[0.8^{0.33}/0.2^{0.33} \approx 1.58\right]$. For large firms with $k = 0.49$, this gap increases to 97% $\left[0.8^{0.49}/0.2^{0.49} \approx 1.97\right]$. This pattern aligns with the intuition that, conditional on similar wage levels, larger firms bear greater compliance costs due to the larger number of affected workers.[5]

We then apply the elasticity-adjusted transformation, using $k + 1 = 1.33, 1.36, 1.49$ for small, medium, and large firms, respectively. Using the same example as before—comparing a firm where the minimum wage is 80% of the average wage to one where it is 20%—relative exposure is given by $[(0.8)^{k+1}/(0.2)^{k+1}]$. This implies that the minimum wage impact is 6.32 times larger for small firms ($4^{1.33} \approx 6.32$), increasing to 7.89 times larger for large firms ($4^{1.49} \approx 7.89$), reflecting stronger adjustment pressures as firm size rises. These values define the firm-specific wage elasticity term $IF(k + 1)$, which we interact with the log change in the minimum wage to estimate how exposure to wage cost shocks translates into changes in firm-level outcomes. Our baseline regression specification is as follows:

$$\begin{aligned} \Delta \ln y_{nict} = \beta_0 + \beta_1 IF_{nt} + \beta_2 [IF_{nt} \times \Delta \ln MW_{ct}] \\ + \beta_3 [IF_{nt} \times \Delta \ln MW_{ct} \times Reform_t] + \nu_n + \mu_{it} + \delta_{ct} + \gamma_o + \epsilon_{nt} \end{aligned} \tag{3}$$

where y_{nict} denotes outcomes such as markup, markdown, average wage, employment, productivity, or exit. All other variables and fixed effects are as defined in Equation (1), with the addition of firm fixed effect, v_n, and the reform indicator $Reform_t$, which equals 1 from 2004 onward to capture institutional changes in policy enforcement.

This empirical formulation centers on capturing how the effects of minimum wage shocks vary across firms with differing exposure levels, and how this relationship changes after institutional reform. The two key interaction terms, $IF_{nt} \times \triangle lnMW_{ct}$, and the triple interaction, $IF_n \times \triangle lnMW_{ct} \times Reform_t$, form the core of our identification strategy. The first captures heterogeneous treatment effects driven by firm exposure prior to the reform, while the second identifies how this relationship evolved in the post-reform period. As in Equation (1), the inclusion of city-year fixed effects ensures that identification comes from within-city-year comparisons: although the level of the minimum wage shock is absorbed by these fixed effects, the interaction terms exploit variation in how that common shock is differentially amplified depending on each firm's position in the wage distribution. This structure allows us to isolate firm-specific effects of minimum wage changes while controlling for broader spatial and temporal confounders, thereby mitigating bias from concurrent policy changes and other confounding influences.

While our empirical strategy follows the core logic of Hau et al. (2020), we extend their approach by allowing treatment effects to vary across policy regimes. By including the post-reform indicator and its interaction with wage exposure, we test whether the 2004 reform, which broadened coverage and strengthened enforcement, altered the responsiveness of firms to minimum wage shocks. Our identification relies on the assumption that, absent the reform, the relationship between firm exposure

[5]The curvature parameter k is re-estimated whenever the main outcome regression specification changes, to ensure consistency in the identification strategy. Specifically, the wage regression (Equation 1) mirrors the fixed effects used in each corresponding outcome regression. For instance, the full-sample baseline includes city-by-year fixed effects; the border-sample analysis uses city-pair-by-year fixed effects and restricts the wage regression to the border sample (rather than the full sample); and the specification following Hau et al. (2020) omits city-year dummies entirely. For the robustness check using the no-attrition sample (i.e., firms that survive through 2007), the curvature is re-estimated using only those restricted sample. Across all versions, the estimates preserve the same ranking: small firms consistently exhibit the lowest curvature, medium firms higher, and large firms (1,000+ workers) the highest.

and outcome dynamics would have remained stable over time. Under this assumption, any systematic post-reform divergence in this relationship can be interpreted as evidence consistent with an institutional change affecting how firms respond to minimum wage policy.

To strengthen the credibility of our identification strategy, we implement an alternative empirical design using a Border District Database (BDD) comprising city pairs in China that straddle provincial borders.[6] In China, minimum wage levels are set by provincial governments based on broad economic indicators, with limited input from peripheral cities. As such, border cities are more likely to adopt provincial policies passively, generating plausibly exogenous variation in minimum wage shocks across neighboring cities. By focusing on firms located in these border cities, we move from within-city-year comparisons to cross-city comparisons between adjacent locations that likely share similar unobserved economic characteristics such as labour market structure or industrial composition, but experience different minimum wage changes. Identification, therefore, comes from within-pair differences in minimum wage shocks and firm exposure. This quasi-experimental setup helps reduce concerns that our results are driven by local policy discretion or endogenous wage-setting behavior. Following is the formal regression specification:

$$\begin{aligned}\Delta \ln y_{nict} = {} & \beta_0 + \beta_1 IF_{nt} + \beta_2 [IF_{nt} \times \Delta \ln MW_{ct}] \\ & + \beta_3 [IF_{nt} \times \Delta \ln MW_{ct} \times Reform_t] + \nu_n + \mu_{it} + \sigma_{pt} + \gamma_o + \epsilon_{nt} \end{aligned} \tag{4}$$

This specification mirrors our main regression in Equation (3), with all variables and fixed effects defined identically, except that we replace city-year fixed effects δ_{ct} with city-pair-year fixed effects σ_{pt}, reflecting the cross-city identification structure in the BDD design. These fixed effects absorb unobserved local characteristics shared within each pair, such as labour market integration or geographic proximity. We assign a unique identifier to each city pair and include firms located in multiple pairs more than once. Finally, to maintain comparability across jurisdictions, we exclude pairs involving municipalities with independent wage-setting authority (e.g., Beijing, Shanghai), where local discretion could confound the cross-border variation.

4. Results

We structure the results in three steps. First, we examine wages, which provide the most direct test of enforcement credibility and establish whether the minimum wage became binding after the 2004 reform. We then turn to markdowns and markups, our main outcomes of interest, to assess how the minimum wage affected the division of rents across labour and product markets under varying enforcement. Finally, we investigate the mechanisms that explain the stability of markups in the face of higher wage costs, distinguishing between cost-saving strategies (employment reductions, capital deepening, and firm exit) and productivity-enhancing adjustments (TFP and value-added per worker improvements).

[6] We thank Professor Lixin Tang for generously sharing the cross-provincial city-pair information used in constructing the Border District Database (BDD).

Throughout the analysis, we report estimates from both the full sample and the border sample. The full sample exploits firm-level variation within city–year and industry–year groups, providing estimates representative of the national policy environment, while the border sample leverages quasi-experimental variation across adjacent provinces with differing minimum wage levels. To prevent double-counting of firms that appear in multiple city–pair comparisons, regressions in the border sample are weighted by the inverse number of appearances.[7]

4.1 Wages (Compliance test)

Table 2 presents results from the wage regressions, which reveal a clear reversal in firm behavior around the 2004 reform. Prior to the reform, the interaction term $IF_{nt} \times \Delta lnMW_{ct}$ is negative and significant, indicating that more exposed firms, those with lower initial wages, reduced average wages in response to minimum-wage increases. This counter-intuitive result, at odds with both competitive and monopsony models that predict wage gains, reflects evasive practices under weak enforcement, such as reliance on uncovered workers or payments made outside formal wage contracts. Following the reform, however, the triple interaction term $IF_{nt} \times \Delta lnMW_{ct} \times Reform_t$ turns positive and significant. This result confirms that the reform effectively limited firms' ability to circumvent compliance, likely by closing loopholes and strengthening enforcement credibility. With fewer opportunities to evade coverage or underpay legally covered workers, firms more exposed to minimum wage hikes could no longer hold wages below statutory floors to the same extent, implying that the policy became more binding in practice.

To illustrate the economic significance of these patterns, consider a representative 11% increase in the minimum wage ($\triangle lnMW = 0.1$). Before the reform, a low-wage small firm (defined as employing fewer than 200 workers) at the 10th percentile of the wage distribution ($\omega_n/MW = 0.896$) reduced average wages by 5.8% ($\approx -0.502 \times (0.896)^{-1.33} \times 0.1$), whereas a high-wage small firm at the 90th percentile ($\omega_n/MW = 3.947$) reduced wages by only 0.8%($\approx -0.502 \times (3.947)^{-1.33} \times 0.1$). After the reform, the pattern reversed: the 10th-percentile small firm ($\omega_n/MW = 1.067$) increased wages by 11.1%($\approx 1.217 \times (1.067)^{-1.33} \times 0.1$), compared to the 90th-percentile firm ($\omega_n/MW = 3.814$) that increased wages by 2.1%($\approx 1.217 \times (3.814)^{-1.33} \times 0.1$).[8]

4.2 Main Results

Table 3 reports our core estimates of how firms' markups and markdowns responded to minimum wage increases. Markdown responses closely mirror the wage patterns in Section 4.1. Before

[7] For the border sample, the raw number of observations appears larger because firms enter multiple times across city–pair comparisons. The effective sample size is smaller once repeated observations and weighting are taken into account.

[8] All percentage effects reported throughout the paper are computed analogously by substituting the relevant coefficients, estimated curvature parameters from wage regression, and wage quantiles into the expression $\hat{\beta} \times (\omega_n/MW)^{-(k+1)} \times \Delta lnMW$. For brevity, we illustrate the calculation only once. In subsequent examples, we report effects for small firms (defined as those employing fewer than 200 workers), to maintain clarity and consistency.

2004, the interaction term $IF_{nt} \times \Delta lnMW_{ct}$ is positive and significant, indicating that more exposed firms increased markdowns when minimum wages rose. After the reform, the triple interaction $IF_{nt} \times \Delta lnMW_{ct} \times Reform_t$ turns negative and highly significant, showing that markdowns fell once compliance became binding. Because markdowns measure the wedge between wages and the marginal product of labour, this decline indicates that wages rose faster than labour productivity at the time of the reform.

The magnitudes are sizable. For an 11% minimum wage increase, a 10th-percentile firm raised markdowns by 2.5% before the reform, compared to just 0.3% for a 90th-percentile firm. After the reform, the 10th-percentile firm reduced markdowns by 5.7%, while the 90th-percentile firm reduced them by 1.1%. This reversal in both sign and magnitude underscores how enforcement tightened the bindingness of the policy, particularly at the lower end of the wage distribution.

By contrast, markup responses are muted. In the full sample, the interaction term is insignificant, while in the border sample it is positive and significant pre-reform, but the magnitude is small, less than one-sixth of the corresponding markdown effect. After the reform, coefficients are insignificant in both samples. This suggests that firms absorbed higher wage costs without systematic changes in markups.

Since our estimated markups reflect the ratio of price to marginal cost, they would ordinarily move in tandem with markdowns absent other adjustments. Their stability despite significant changes in wages and markdowns suggests two offsetting channels: (i) output prices rose in line with higher wages, or (ii) marginal costs were contained through cost-cutting or productivity improvements. Because CASIF does not report prices, we cannot directly test pricing. Instead, we examine firm adjustments affecting marginal costs through two channels: cost-saving and productivity-enhancing strategies. These adjustments are analyzed in Sections 4.3 and 4.4.

4.3 Cost-Saving Adjustment

Table 4 presents results for employment (Columns 1–2), capital–labour ratios (Columns 3–4), and firm exit (Columns 5–6). Employment adjustments illustrate one important cost-saving margin. Before the reform, more exposed firms expanded their workforce, consistent with substitution toward uncovered or informally paid workers as a way to contain labour costs. After the reform, this relationship reverses: highly exposed firms significantly reduced employment, indicating that stronger enforcement and broader coverage curtailed their ability to rely on these low-cost labour segments.

Capital intensity also shifted sharply around the reform. Before 2004, more exposed firms reduced their capital–labour ratios when minimum wages increased, favouring cheap labour over investment in machinery or technology. This pattern is consistent with weak enforcement: when labour costs could still be contained through evasion, there was little incentive to substitute toward capital. After the reform, however, the relationship turns positive and significant. Once low-wage strategies were closed off, firms increased capital intensity, reflecting a move toward more capital-deepening methods of economizing on labour costs.

Firm exit patterns presents another cost-saving margin. Before the reform, more exposed firms were significantly less likely to exit than less exposed firms. In the full-sample setting, where firms face the same statutory minimum within a city, this suggests that highly exposed firms relied on business models built around uncovered or informal labour, while less exposed firms depended more on formally covered workers. In the border sample, where statutory minima vary across adjacent provinces, the same pre-reform pattern may additionally suggest that firms located in provinces with higher legal minima could benefit from stronger local demand while still evading compliance with the regulation. After the reform, both samples show a clear reversal: exit rates rose more for highly exposed firms once enforcement credibility improved, indicating that labour-intensive or evasive models became unsustainable.

The effects are also economically meaningful. For a representative 11% minimum wage increase, firms at the 10th percentile of the wage distribution expanded employment by 2.4% before the reform, compared with 0.3% for those at the 90th percentile. After the reform, 10th-percentile firms reduced employment by 3.0%, while 90th-percentile firms reduced it by 0.6%. Capital–labour ratios for 10th-percentile firms declined by 2.4% before the reform but increased by 3.7% afterward, with corresponding changes of 0.3% lower and 0.7% higher, respectively, for 90th-percentile firms. Exit probabilities for 10th-percentile firms fell by 1.4 percentage points before the reform but rose by 1.3 points afterward, compared with changes of 0.2 points lower and 0.2 points higher for 90th-percentile firms. Taken together, these magnitudes show how enforcement closed off evasion strategies and forced labour-intensive firms to adjust through job cuts, capital substitution, or exit.

4.4 Productivity-Enhancing Adjustment

Table 5 reports our findings on how firms adjusted along the productivity enhancement margin. Columns (1)–(2) report results for log changes in total factor productivity (TFP), and Columns (3)–(4) for value-added per worker (YML).[9] In the full sample, the TFP regressions show that the interaction term $IF_{nt} \times \Delta lnMW_{ct}$ is negative and statistically significant prior to the reform, while the triple interaction term $IF_{nt} \times \Delta lnMW_{ct} \times Reform_t$ turns positive and significant. This shift indicates that, under weak enforcement, exposed firms relied on cheap labour rather than investing in productivity. Once the minimum wage became binding, they undertook efficiency-enhancing reforms that improved TFP.

In contrast, the border-sample estimate for the post-reform interaction is small and imprecisely estimated. Because the standard error exceeds the coefficient and the magnitude is less than one-tenth of the full-sample estimate, we treat this result as uninformative rather than contradictory. Accordingly, we place greater weight on the full-sample specification in Column (1), which is preferred for two reasons. First, our main specification with full sample includes city–year fixed effects, which absorb local policy endogeneity, such as wage setting in response to labour market conditions. This ensures identification comes exclusively from within-city-year differences across firms. While some

[9]The TFP sample size is smaller because the construction of $lnTFP_{nt}$ relies on lagged values of wages, following Hau et al.(2020)'s revenue-share method: $\ln TFP_{nt} = \ln Y_{nt} - \alpha_L \ln(\omega_{n,t-1} L_{n,t}) - \alpha_K \ln K_{n,t}$, where $\alpha_L = \frac{\omega_{n,t-1} L_{n,t}}{\omega_{n,t-1} L_{n,t} + (r_s+\delta_s) K_{n,t}}$ and $\alpha_K = \frac{(r_s+\delta_s) K_{n,t}}{\omega_{n,t-1} L_{n,t} + (r_s+\delta_s) K_{n,t}}$. Specifically, the estimation requires data from both year t and $t-1$, and the calculation of $\triangle lnTFP_{nt}$ further requires consecutive $lnTFP_{nt}$ observations. For example, if a firm enters the sample in 2004, $lnTFP_{n,2004}$ is missing due to lack of 2003 data, and consequently $\triangle lnTFP_{n,2005}$ is also missing. In contrast, variables like markups can be computed from first-year data, allowing more complete panels.

residual confounding factors may remain, they would affect all firms similarly and are unlikely to bias estimates of firm-level exposure. Second, as a robustness check, we implement a SYSTEM GMM specification following Hau et al. (2020), which addresses potential bias coming from autocorrelation of the dependent variable. The coefficient on the post-reform interaction remains positive and statistically significant in these regressions, reinforcing the robustness of the full-sample results. [10]

Results for value-added per worker (Columns 3–4) further reinforce the shift in firm behavior. Prior to the reform, the exposure interaction is negative and statistically significant, indicating decreased value-added per worker among more exposed firms. After the reform, the coefficient turns positive and significant, suggesting that firms facing higher minimum wage pressures subsequently achieved higher value-added per worker.

The productivity responses are equally notable. For an 11% minimum wage increase ($\triangle lnMW_{ct} = 0.1$), TFP at low-wage firms fell by 1.7% before the reform but rose by 3.2% afterward. Value-added per worker growth declined by 2.1% before the reform and increased by 4.6% afterward for low-wage firms.

Throughout Tables 3-5, estimates from the border sample are qualitatively consistent with the full-sample results, though generally smaller in magnitude and less precise. This attenuation is expected given the smaller effective sample size in the border design and the greater estimation noise it entails. Importantly, the main patterns persist across outcomes, including wages, employment, exit, capital intensity, and value added. The one exception is TFP, where border-sample estimates are imprecise and statistically insignificant. Appendix Tables A.1–A.4 further confirm the robustness of our results: relaxing city–year fixed effects to permit cross-city comparisons, as in Hau et al. (2020), and dropping the border-sample weights leave the key coefficients unchanged in sign and broadly similar in magnitude across markups, markdowns, cost-side margins, and productivity outcomes. These checks underscore that our findings are not artifacts of specification choices.

Taken together, the results point to a fundamental shift in firm behaviour once the minimum wage became binding. The reform first shows up directly in wages, where evasion gave way to compliance. This shift carried through to markdowns, which fell as labour gained at the expense of firms, while markups remained broadly stable. Firms absorbed rising labour costs by cutting jobs, substituting toward capital, and in some cases exiting, and over time they also improved efficiency through gains in TFP and labour productivity. Overall, stronger enforcement redirected rents toward workers and forced firms to adjust on the cost and productivity margins, while leaving markups largely unchanged.

[10] We implement the system GMM estimator following Hau et al. (2020), instrumenting endogenous variables with lagged differences. In the TFP regressions, the lagged dependent variable is instrumented with its own lagged value (at lag 2), which satisfies the standard diagnostic tests, confirming the robustness of the our main regression in Equation (4). By contrast, in the markup and markdown regressions, only longer lags (e.g. the fourth lag) satisfy these diagnostics, but using these instruments produces coefficient estimates consistent with our main results. Given that the dependent variables display only modest autocorrelation, and to preserve clarity of exposition, we focus our reporting on the fixed-effects specification in Equation (4). Full system GMM results are available upon request.

5. Robustness

To assess the credibility of the empirical strategy and the reliability of the main findings, we conduct a series of robustness checks. First, we test whether firm-specific exposure to minimum wage changes is correlated with outcome variation at a lagged period (t–2), in order to rule out the possibility that the results are driven by pre-existing trends rather than changes in institutional context. Second, we re-estimate our main regressions on a restricted "no-attrition" sample that includes only firms observed through the final year of the panel, to assess whether selective exit affects our conclusions. Third, we replicate the analysis using alternative indices of markups and markdowns to ensure that the main outcome results are not sensitive to measurement choice. In each case, we retain the same baseline regression framework in equation (4), altering only the outcome or sample definition relevant to the specific exercise. [11] We now discuss each check in turn.

5.1. Pre-existing trend

Table 6 reports a pre-trend analysis using outcome variables measured two years earlier ($t-2$). The regressions test whether firm-specific exposure or its interaction with the reform structure is systematically associated with earlier outcome dynamics unrelated to the reform. Significant correlations at this lead time would raise concerns that the main results reflect underlying trends rather than changes in firm behavior tied to institutional change.

The results show no statistically significant relationship between the exposure variables ($IF_{nt} \times \Delta lnMW_{ct}$ and $IF_{nt} \times \Delta lnMW_{ct} \times Reform_t$) for most key outcomes, including markup, labour productivity (APL and YML), and employment. Two exceptions emerge. First, for markdowns, the coefficient on $IF_{nt} \times \Delta lnMW_{ct}$ is significantly negative at $t-2$. Second, for capital intensity (K/L), the triple interaction term is significantly negative. In both cases, the coefficients have the opposite sign of the main results in Section 4, so they do not constitute evidence of pre-trends.

5.2. Regression with no-attrition sample

Next, to address concerns about potential sample selection bias due to firm exits, we re-estimate our main regressions using a restricted sample that includes only firms observed through the final year of the panel (2007). This approach excludes firms that exited the market during the study period, thereby isolating a more stable group of survivors and allowing us to assess whether attrition influences our findings. Table 7 shows that, relative to the full-sample results, the estimates with the no-attrition sample are more pronounced across most outcomes in both the pre- and post-reform periods, likely reflecting that surviving firms tend to have greater capacity to adjust to cost shocks. Markdown effects are only slightly smaller, but the consistency of their sign and significance reinforces the overall interpretation of our results.

[11] Border-sample estimates for all robustness checks yield qualitatively similar results. For brevity, these are not reported here but are available from the authors upon request.

5.3. Regression with different measures of markup and markdown

Table 8 presents the estimated effects of minimum wage shocks on firm-level markups and markdowns using three alternative indices of markup and markdown: DLW, CRS, and CD. The results are broadly consistent across methods, particularly for markdowns, where we find robust and statistically significant reductions in firms' wage-setting power following the 2004 reform. This consistency across different indices reinforces the credibility of our main finding that minimum wage increases compressed markdowns.

Markup estimates are less consistent across indices. Results based on the DLW and CD measures are largely insignificant, suggesting limited systematic changes in firms' price–cost margins. In contrast, the CRS-based estimates display a more distinctive pattern: firms more exposed to minimum wage hikes appear to raise markups before the reform, but this effect weakens or reverses in the post-reform period. Although the CRS-based estimates differ somewhat from the DLW baseline, they still suggest that any pre-reform markup gains were modest and did not persist once enforcement improved. This reinforces our broader interpretation that markups remained largely stable, with firms adjusting instead through cost-saving and productivity-enhancing channels.[12]

6. Heterogeneity analysis

Finally, we examine heterogeneity in treatment effects by ownership type. The CASIF dataset distinguishes state-owned enterprises (SOEs), private domestic firms, and foreign-invested firms, groups that differ in management quality, institutional constraints, and access to finance. Hau et al. (2020) shows that these differences shape firms' responses to wage policy through productivity and capital intensity. We build on this evidence by examining how ownership structures condition firms' ability to adjustment.

In the pre-reform period, when enforcement was weak, firms relied mainly on evasive strategies. Wage and markdown responses are statistically significant only for private firms: an 11 percent minimum wage increase reduced wages by about 7.1 percent and raised markdowns by roughly 2.7 percent for a representative low-wage private firm, consistent with practices that lowered effective pay. Foreign-owned firms and SOEs also showed higher markdowns, although these estimates were not statistically significant. Markup responses diverged: SOEs recorded a small but positive and statistically significant increase in markups, while foreign firms experienced declines. The latter pattern mirrors their reliance on more labour-intensive strategies, with higher employment, lower TFP, and reduced value added per worker. On the cost side, exit risks were lower for exposed firms, with SOEs 2.3 percentage points less likely to exit, private firms 0.8 points less likely, and foreign firms 1.1 points less likely.

[12] Brooks et al. (2021) define the constant-returns scale (CRS) markup as the ratio of revenue to total cost, which directly embeds labor costs in the denominator. A minimum-wage hike that raises wage bills therefore mechanically reduces CRS Markup, producing movements that closely parallel markdown changes. Elasticity-based measures, such as the DLW method, instead recover markups from estimated output elasticities and cost shares, making them less mechanically sensitive to wage shocks and more dependent on production-function estimation. The Cobb–Douglas (CD) markup is essentially a re-scaled CRS measure, constructed by multiplying the CRS markup by the ratio of the mean inverse materials share to each firm's inverse materials share. The reweighting can dampen or amplify the CRS markup depending on the firm's materials intensity, making CD markups align more closely with CRS in labor-intensive firms but diverge in material-intensive ones. These construction differences explain why CRS markup tends to track markdown movements more closely than DLW or CD markups.

Productivity results further underscore the heterogeneity: foreign firms saw declines of about 6 percent in TFP and 4.5 percent in value added per worker, while private firms recorded smaller declines of roughly 3 percent in capital–labour ratios and 2.7 percent in value added per worker; SOEs again showed no significant productivity changes. Overall, the pre-reform evidence suggests that firms coped primarily by drawing on uncovered or less-productive labour rather than by raising efficiency, with private firms most visibly pursuing labour-intensive strategies.

The post-reform period reveals a very different picture. In response to an 11 percent increase in the minimum wage, wages rose across all ownership types, increasing by 19.7 percent for foreign firms, 11.6 percent for private firms, and 4.8 percent for SOEs. Markdown effects turned strongly negative, with exposed foreign firms reducing markdowns by 10.6 percent, private firms by 5.7 percent, and SOEs by 2.9 percent. Markup responses also diverged: foreign firms, despite large wage increases and steep markdown reductions, managed to raise markups by about 0.9 percent, while private firms broadly maintained their markups and SOEs experienced a decline of about 0.4 percent. On the cost-reduction margin, employment contracted sharply among exposed firms: foreign firms reduced their workforces by 5.2 percent and private firms by 3.1 percent, while SOEs showed no statistically significant change. Foreign-owned firms increased their capital–labour ratios by 6.2 percent and private firms by 3.7 percent. Exit risks also rose, with SOEs becoming 2.6 percentage points more likely to exit, compared with 0.7 points for private firms and 1.2 points for foreign firms. On the productivity-enhancement margin, foreign firms improved TFP by 13.6 percent, and raised value added per worker by 10.1 percent. Private firms achieved improvements as well, though of smaller magnitudes, while SOEs showed no statistically significant gains. Overall, the post-reform evidence suggests that foreign and private firms combined cost-cutting with productivity enhancements to offset higher labour costs, while SOEs, facing modest wage increases but no efficiency gains and higher exit risks, saw their markups fall. [13]

Taken together, these results show that, in the post-reform period, in response to minimum wage increases, foreign firms combined the sharpest wage gains and markdown reductions with the largest productivity improvements, enabling them to sustain or even expand markups. They drew simultaneously on cost-side adjustments, such as employment cuts, and efficiency-enhancing strategies, such as capital deepening and productivity growth. Private firms followed similar channels more moderately, leaving markups broadly stable. SOEs proved least adaptable, with only modest wage responses, no productivity gains, declining markups, and the highest exit risk. Overall, the evidence highlights that the effects of minimum wage depend not only on the credibility of enforcement but also on ownership structure and managerial capacity.

7. Conclusion

This paper investigates how Chinese firms adjusted their price–cost margins in product and labour markets, along with other operational margins, to minimum wage shocks under different enforcement regimes, with particular attention to the institutional shift brought by the 2004 reform. We exploit

[13] Results are reported using DLW markups as the baseline. Estimates with CRS and CD markups yield qualitatively similar ownership patterns, though magnitudes vary because CRS markups more directly track markdown changes. Full tables are available on request.

firm-level variation in exposure to minimum wage changes and the institutional shift in enforcement associated with the 2004 reform to trace how firm adjustment evolved across periods.

Our results show that enforcement capacity shaped the direction of adjustment. Before 2004, when coverage was narrow and penalties for non-compliance were weak, more exposed firms responded to minimum wage hikes by reducing wages and widening markdowns, consistent with evasive practices such as greater reliance on part-time or otherwise uncovered workers to contain labour costs. After 2004, stronger enforcement and broader coverage reversed this pattern: in response to minimum wage hikes, wages rose and markdowns declined significantly, indicating that firms could no longer rely on such strategies to suppress effective pay. While higher labour costs should, in principle, reduce product-market rents, we find that markups did not generally decline. The stability of markups despite rising labour costs raises a question regarding the mechanisms through which firms preserved price–cost margins. The evidence indicates that more exposed firms shifted their adjustment margins: on the cost side, they reduced employment, increased capital intensity, and in some cases exited; on the efficiency side, they achieved gains in total factor productivity and value added per worker. In this way, stronger enforcement compressed rents in the labour market but left product-market rents largely intact.

Responses also varied sharply by ownership. Before the reform, private firms relied most heavily on evasive cost-saving strategies, lowering wages, widening markdowns, and expanding employment at the expense of efficiency. Foreign firms likewise pursued labour-intensive adjustments, increasing employment but with productivity losses and lower markups. SOEs, by contrast, showed little responsiveness on any margin, and their modest markup increases largely mirrored markdown shifts, reflecting an absence of substantive adjustments elsewhere. After the reform, patterns reversed. Foreign firms exhibited the sharpest wage increases and markdown reductions, combined with strong productivity gains, reflecting the most pronounced adjustments on both the cost side (employment reductions and higher capital intensity) and the efficiency side (productivity improvements). Private firms followed similar channels more moderately, leaving markups broadly stable. SOEs proved least adaptable, with limited wage responses, no productivity gains, falling markups, and elevated exit risk. Overall, the evidence indicates that foreign firms were best able to offset rising labour costs, while SOEs remained least able to adapt to binding wage floors.

These findings suggest that policymakers considering minimum wage implementation should account for firms' behavioral responses. The bindingness of the minimum wage and firms' capacity to adjust determine whether, and to what extent, the law reshapes rents in both product and labour markets. Ultimately, firm behavior is central to how rents are divided between firms and workers and between firms and consumers. A limitation of our study is that CASIF does not distinguish between full-time and part-time workers, preventing us from directly testing whether changes in workforce composition drove pre-reform outcomes.In addition, the absence of firm-level price data leaves the role of pricing in shaping markups unresolved. Future research using richer data on employment structure and prices could more directly assess how these mechanisms shaped the evolution of both markups and markdowns.

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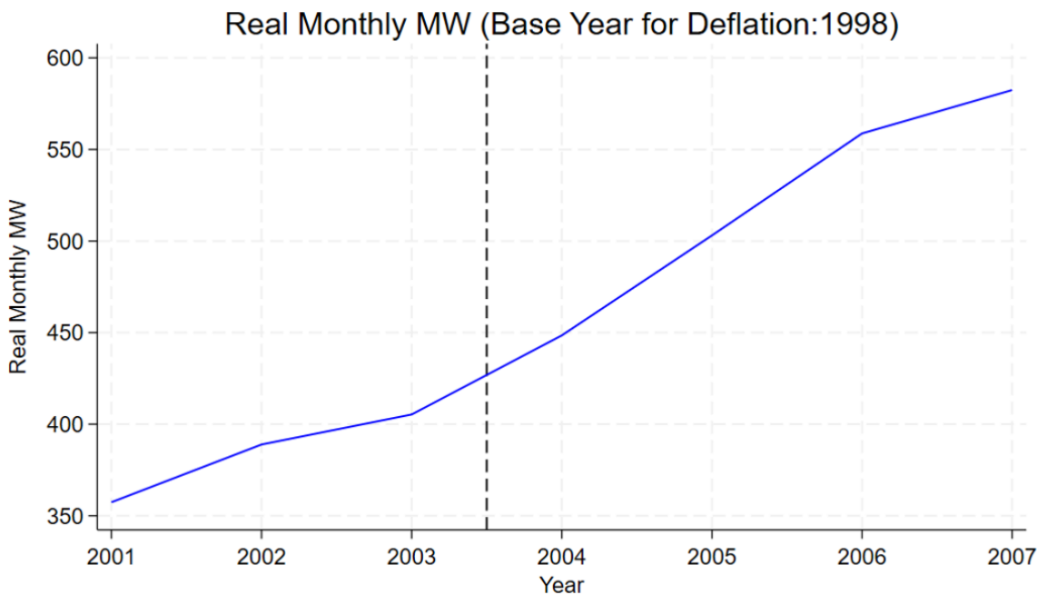
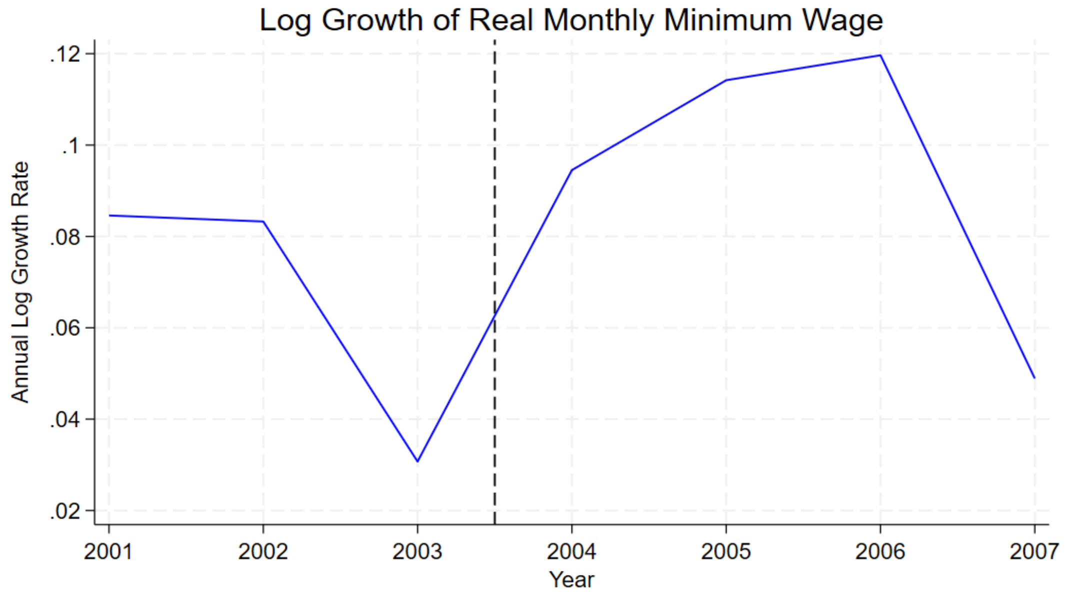
Figures and Tables

Figure 1. Minimum Wage and Compliance

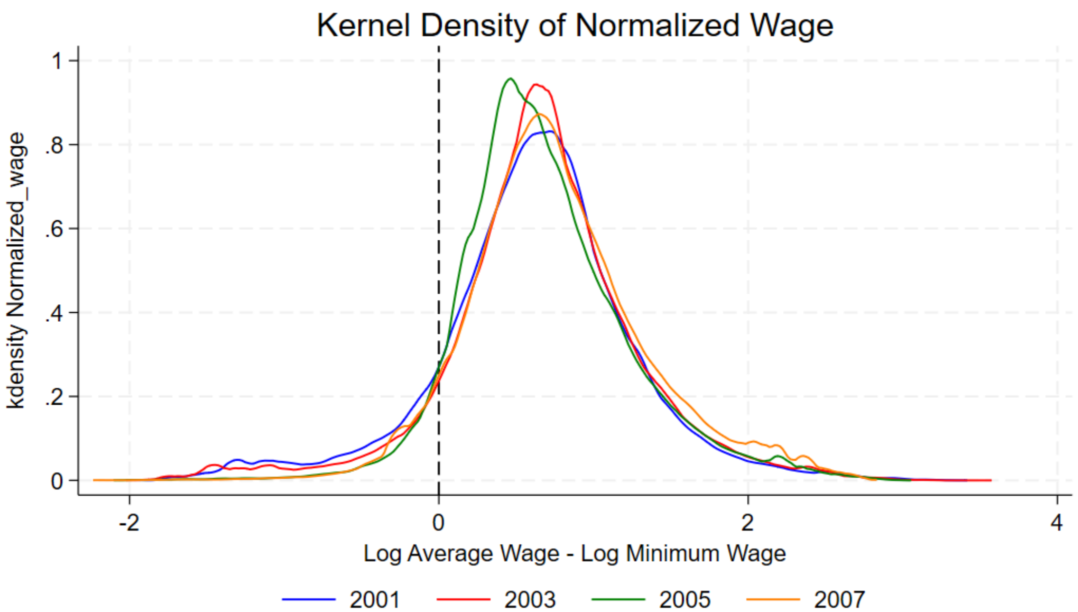
Panel A.



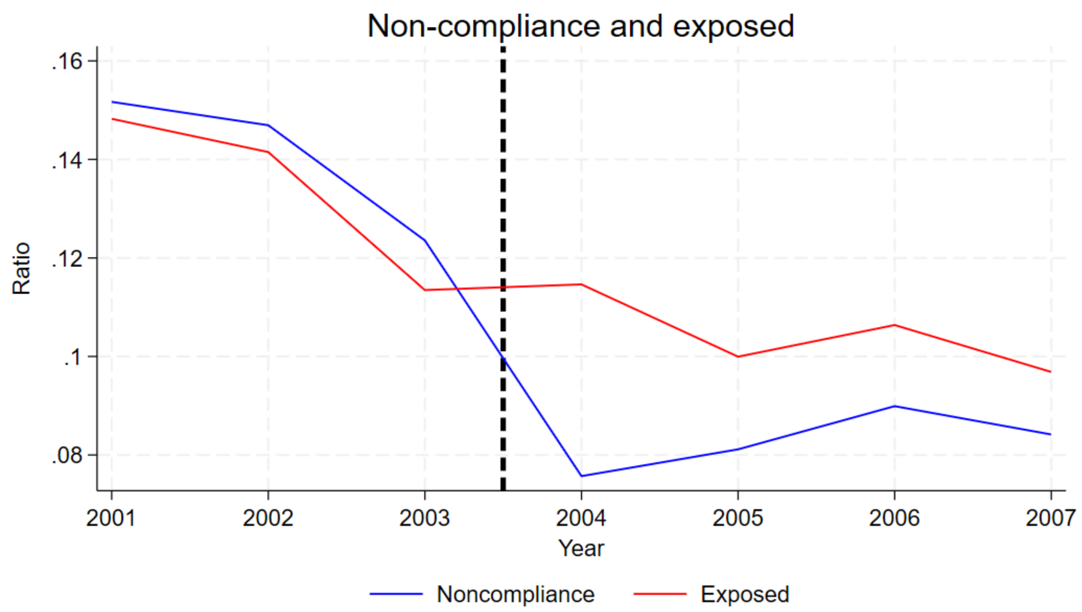
Panel B.



Panel C.



Panel D.



Notes: Panels A and B plot the level and log growth of real monthly minimum wages (deflated to 1998 CPI), with the vertical line indicating the onset of the 2004 reform. Panels C and D report firm-level compliance measures. Panel C shows kernel density distributions of normalized wages (log average wage minus log minimum wage) for selected years. Panel D plots non-compliance (share of firms with average wages below the statutory minimum) and exposure (share of firms whose lagged average wage fell below the new minimum).

Table 1. *Summary Statistics*

	Observation (1)	Mean (2)	STD (3)	P25 (4)	P50 (5)	P75 (6)
Panel A: Policy Variables						
IF(k+1) × Δln(Minimum Wage)						
All Years	1,077,737	0.042	0.094	-0.001	0.027	0.058
Pre-Reform	261,812	0.032	0.087	-0.001	0.012	0.039
Post-Reform	815,925	0.045	0.096	0.001	0.032	0.062
Δlog (Minimum Wage)						
All Years	1,077,737	0.084	0.103	-0.006	0.078	0.120
Pre-Reform	261,812	0.056	0.073	-0.006	0.055	0.094
Post-Reform	815,925	0.093	0.110	0.009	0.083	0.132
IF(k+1)						
All Years	1,077,737	0.546	0.632	0.270	0.424	0.627
Pre-Reform	261,812	0.630	0.884	0.272	0.415	0.645
Post-Reform	815,925	0.519	0.524	0.270	0.427	0.622
Panel B: Firm-Level Outcome Variables						
Δlog (Real Average Wage)						
All Year	1,077,737	0.113	0.543	-0.106	0.077	0.317
Pre-Reform	261,812	0.068	0.562	-0.113	0.048	0.249
Post-Reform	815,925	0.127	0.536	-0.104	0.088	0.340
Δlog (Markdown)						
All Years	1,077,737	-0.004	0.644	-0.295	0.001	0.284
Pre-Reform	261,812	0.030	0.635	-0.236	0.035	0.297
Post-Reform	815,925	-0.015	0.646	-0.313	-0.011	0.279
Δlog (Markup)						
All Years	1,077,737	0.010	0.203	-0.078	0.007	0.096
Pre-Reform	261,812	0.013	0.210	-0.076	0.008	0.098
Post-Reform	815,925	0.009	0.201	-0.078	0.007	0.095
Δlog (Employment)						
All Year	1,077,737	0.025	0.406	-0.054	0	0.105
Pre-Reform	261,812	0.012	0.409	-0.080	0.000	0.112
Post-Reform	815,925	0.029	0.405	-0.043	0.000	0.105
Δlog (K/L)						
All Year	1,077,737	-0.005	0.761	-0.258	-0.054	0.210
Pre-Reform	261,812	-0.016	0.709	-0.238	-0.046	0.172
Post-Reform	815,925	-0.001	0.777	-0.265	-0.056	0.222
Firm Exit						
All Year	1,077,737	0.084	0.277	0	0	0
Pre-Reform	261,812	0.142	0.349	0	0	0
Post-Reform	815,925	0.065	0.247	0	0	0
Δlog (TFP)						
All Year	781,352	0.140	0.884	-0.276	0.148	0.566
Pre-Reform	182,202	0.160	0.973	-0.282	0.161	0.612
Post-Reform	599,150	0.134	0.854	-0.274	0.145	0.554
Δlog (Value-Added per Worker)						
All Year	1,025,458	0.211	0.836	-0.179	0.185	0.578
Pre-Reform	237,501	0.165	0.914	-0.238	0.155	0.561
Post-Reform	787,957	0.224	0.810	-0.162	0.194	0.582

Notes: Firm-level data are from the Chinese Annual Survey of Industrial Firms (CASIF), covering 2001–2007. The analysis uses first-differenced variables starting in 2002. All outcome variables are expressed in **log-differences**, except for **firm exit**, which is a binary indicator equal to 1 if a firm is observed in year *t* but not in *t+1*. **Log Real Average Wage** is defined as the log of total labor compensation divided by employment (in thousands of RMB), deflated using the 1998 Consumer Price Index (CPI = 100), and winsorized at the 1st percentile. **Markdowns and Markups** are estimated following Brooks et al. (2021b) and winsorized at the 3rd percentile by industry-year. **Employment** is measured as the number of workers reported in the survey. **Log Capital Intensity** is defined as ln(*K/L*), where *K* is real capital stock and *L* is employment. **Firm Exit** is a binary outcome equal to 1 if a firm is present in year *t* but absent in year *t+1*. **Log TFP** is constructed following Hau et al. (2020) using a revenue-share approach: $\ln TFP_{nt} = \ln Y_{nt} - \alpha_L \ln(\omega_{n,t-1} L_{nt}) - \alpha_K \ln(K_{nt})$ where labor and capital elasticities are computed from observed cost shares: $\alpha_L = \frac{\omega_{n,t-1} L_{nt}}{\omega_{n,t-1} L_{nt} + (r_t + \delta_t) K_{nt}}$, $\alpha_K = \frac{(r_t + \delta_t) K_{nt}}{\omega_{n,t-1} L_{nt} + (r_t + \delta_t) K_{nt}}$. Here, $\omega_{n,t-1}$ denotes lagged average wage, L_{nt} employment, K_{nt} capital stock, r_t the rental rate of capital, and δ_t the depreciation rate. **Log Value-Added per Worker** is defined as ln(*(Y – M)/L*), where *Y* is total sales, *M* is intermediate input expenditure, and *L* is employment; observations with *M* > *Y* are excluded to avoid undefined values.

Table 2. *Wage Adjustment to Minimum Wage Changes (Dependent Variable: Δln W)*

	(1)	(2)
	Δlog (W)	
	Full Sample	**Border Sample**
IF × ΔlogMW × Reform	1.217***	0.913***
	(0.182)	(0.138)
IF × ΔlogMW	-0.502***	-0.464***
	(0.105)	(0.100)
IF	0.548***	0.632***
	(0.026)	(0.028)
ΔlogMW		0.092
		(0.061)
Observations	994,731	1,069,908
Firm FE	Yes	Yes
Ownership FE	Yes	Yes
Industry-Year FE	Yes	Yes
City-Year FE	Yes	No
Citypair-Year FE	No	Yes

Notes: This table reports the estimated effects of minimum wage shocks on log changes in firm-level average wages. Column (1) reports estimates from the full sample; column (2) reports estimates from the border sample of firms located in counties adjacent to provincial minimum wage borders. In the border sample regressions, we apply weights equal to the inverse of the number of times a given firm appears due to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 (full sample) and 1.25, 1.28, 1.41 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 3. *Market Concentration Adjustment to Minimum Wage Changes (Dependent Variable: Δln Markup/Markdown)*

	(1)	(2)	(3)	(4)
	Δlog (Markdown)		**Δlog (Markup)**	
	Full Sample	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	-0.625***	-0.465***	0.006	-0.017
	(0.104)	(0.082)	(0.011)	(0.013)
IF × ΔlogMW	0.216***	0.198***	0.011	0.031**
	(0.074)	(0.071)	(0.010)	(0.013)
IF	-0.346***	-0.398***	0.011***	0.012***
	(0.015)	(0.016)	(0.001)	(0.001)
ΔlogMW		0.010		-0.001
		(0.053)		(0.013)
Observations	994,731	1,069,908	994,731	1,069,908
Firm FE	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	No	Yes	No
Citypair-Year FE	No	Yes	No	Yes

Notes: This table reports the estimated effects of minimum wage shocks on log changes in firm-level markups and markdowns. Columns (1) and (3) report estimates from the full sample; columns (2) and (4) report estimates from the border sample of firms located in counties adjacent to provincial minimum wage borders. In the border sample regressions, we apply weights equal to the inverse of the number of times a given firm appears due to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 (full sample) and 1.25, 1.28, 1.41 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 4. *Cost-Side Mechanisms: Employment, Capital Intensity, and Firm Exit Responses to Minimum Wage Changes*

	(1)	(2)	(3)	(4)	(5)	(6)
	Δlog (L)		**Δlog (K/L)**		**Firm Exit**	
	Full Sample	**Full Sample**	**Full Sample**	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	-0.331***	-0.331***	0.399***	0.239***	0.147***	0.122***
	(0.065)	(0.065)	(0.090)	(0.092)	(0.019)	(0.022)
IF × ΔlogMW	0.205***	0.205***	-0.213***	-0.174**	-0.119***	-0.095***
	(0.042)	(0.042)	(0.059)	(0.076)	(0.018)	(0.021)
IF	-0.117***	-0.117***	0.139***	0.164***	0.001*	0.001
	(0.008)	(0.008)	(0.009)	(0.012)	(0.001)	(0.001)
ΔlogMW				0.024		0.004
				(0.040)		(0.009)
Observations	994,731	994,731	994,731	1,069,908	994,731	1,069,908
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	No	Yes	No
Citypair-Year FE	No	No	No	Yes	No	Yes

Notes: This table reports the estimated effects of minimum wage shocks on log changes in firm-level employment, capital intensity, and firm exit. Columns (1), (3) and (5) report estimates from the full sample; columns (2), (4) and (6) report estimates from the border sample of firms located in counties adjacent to provincial minimum wage borders. In the border sample regressions, we apply weights equal to the inverse of the number of times a given firm appears due to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 (full sample) and 1.25, 1.28, 1.41 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 5. *Productivity-Side Mechanisms: TFP, and Value-Added per Worker Responses to Minimum Wage Changes*

	(1)	(2)	(3)	(4)
	Δlog (TFP)		**Δlog (YML)**	
	Full Sample	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	0.349***	-0.023	0.500***	0.283**
	(0.130)	(0.085)	(0.112)	(0.118)
IF × ΔlogMW	-0.148*	0.017	-0.185***	-0.106
	(0.086)	(0.080)	(0.073)	(0.097)
IF	0.190***	0.049***	0.191***	0.227***
	(0.016)	(0.012)	(0.014)	(0.016)
ΔlogMW		0.046		0.047
		(0 .052)		(0.056)
Observations	721,847	765,129	941,366	1,017,686
Firm FE	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	No	Yes	No
Citypair-Year FE	No	Yes	No	Yes

Notes: This table reports the estimated effects of minimum wage shocks on log changes in firm-level total factor productivity(TFP) and value-added per worker(YML). Columns (1) and (3) report estimates from the full sample; columns (2) and (4) report estimates from the border sample of firms located in counties adjacent to provincial minimum wage borders. In the border sample regressions, we apply weights equal to the inverse of the number of times a given firm appears due to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 (full sample) and 1.25, 1.28, 1.41 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 6. *Robustness Check: Pre-trend test (at t-2)*

	(1) Δlog (Wage)	(2) Δlog (Markdown)	(3) Δlog (Markup)	(4) Δlog (L)	(5) Δlog (K/L)	(6) Δlog (TFP)	(7) Δlog (YML)
IF × ΔlogMW × Reform	-0.041	0.101	0.023	-0.002	-0.086*	0.065	0.037
	(0.078)	(0.071)	(0.019)	(0.041)	(0.046)	(0.095)	(0.083)
IF × ΔlogMW	0.112	-0.193***	-0.016	0.001	0.076*	-0.117	-0.062
	(0.083)	(0.073)	(0.018)	(0.041)	(0.045)	(0.090)	(0.080)
IF	0.030***	0.019***	-0.001	0.003	-0.009**	-0.009*	-0.017***
	(0.005)	(0.004)	(0.001)	(0.003)	(0.004)	(0.005)	(0.005)
Observations	530,605	526,750	526,750	530,605	530,605	383,515	485,553
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from a pre-trend test using lagged outcome variables measured at $t-2$. All regressions use the full sample and follow the main specification, including firm, ownership, industry-year, and city-year fixed effects. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. Curvature parameters $k+1$ are set to 1.33, 1.36, 1.49 by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 7. *Rosbustness Check: Sample without attrition*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δlog (Wage)	**Δlog (Markdown)**	**Δlog (Markup)**	**Δlog (L)**	**Δlog (K/L)**	**Δlog (TFP)**	**Δlog (YML)**
IF × ΔlogMW × Reform	1.423***	-0.620***	0.002	-0.425***	0.526***	0.637***	0.649***
	(0.211)	(0.127)	(0.015)	(0.076)	(0.107)	(0.171)	(0.135)
IF × ΔlogMW	-0.686***	0.209**	0.013	0.280***	-0.340***	-0.359**	-0.311***
	(0.126)	(0.095)	(0.014)	(0.048)	(0.068)	(0.124)	(0.069)
IF	0.540***	-0.331***	0.011***	-0.113***	0.139***	0.204***	0.194***
	(0.028)	(0.016)	(0.001)	(0.009)	(0.010)	(0.018)	(0.015)
Observations	821,190	821,190	821,190	821,190	821,190	608,177	785,956
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents results from a balanced sample. All regressions use the full sample and follow the main specification, including firm, ownership, industry-year, and city-year fixed effects. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. Curvature parameters *k+1* are set to 1.35, 1.39, 1.5 by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 8. *Firm Market Concentration impact of minimum wage changes across different indices (full sample)*

	(1) DLW	(2) CRS	(3) CD
Panel A. Δlog (Markdown)			
IF × ΔlogMW × Reform	-0.625***	-0.582***	-0.629***
	(0.104)	(0.095)	(0.103)
IF × ΔlogMW	0.216***	0.192***	0.232***
	(0.074)	(0.064)	(0.072)
IF	-0.346***	-0.320***	-0.336***
	(0.015)	(0.014)	(0.015)
Panel B. Δlog (Markup)			
IF × ΔlogMW × Reform	0.006	-0.044***	0.002
	(0.011)	(0.011)	(0.010)
IF × ΔlogMW	0.011	0.040***	0.005
	(0.010)	(0.011)	(0.009)
IF	0.011***	-0.013***	0.002**
	(0.001)	(0.001)	(0.001)
Observation	994,731	994,731	994,731
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes
City-Year FE	Yes	Yes	Yes

Notes: This table reports the estimated effects of minimum wage shocks on firm-level markups and markdowns using alternative indices of market power, based on our main specification and the full sample. Columns (1)– (3) correspond to DLW, CRS, and CD indices, respectively. All regressions use the full sample and include firm, ownership, industry-year, and city-year fixed effects. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure to minimum wage changes prior to the 2004 reform, while the triple interaction with $Reform_t$ captures post-reform differences. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table 9. *Regression results by ownership*

VARIABLES	(1) Δlog (Wage)	(2) Δlog (Markdown)	(3) Δlog (Markup)	(4) Δlog (L)	(5) Δlog (K/L)	(6) Firm Exit	(7) Δlog (TFP)	(8) Δlog (YML)
Panel A: Private Firms								
IF × ΔlogMW × Reform	1.272***	-0.626***	0.002	-0.337***	0.406***	0.074***	0.347**	0.520***
	(0.177)	(0.109)	(0.012)	(0.063)	(0.099)	(0.015)	(0.140)	(0.105)
IF × ΔlogMW	-0.619***	0.237***	0.016	0.259***	-0.263***	-0.069***	-0.154	-0.233***
	(0.109)	(0.080)	(0.011)	(0.047)	(0.069)	(0.015)	(0.114)	(0.076)
IF	0.569***	-0.347***	0.011***	-0.129***	0.155***	0.002**	0.217***	0.211***
	(0.024)	(0.015)	(0.001)	(0.008)	(0.009)	(0.001)	(0.019)	(0.014)
Observations	687,284	687,284	687,284	687,284	687,284	687,284	484,902	655,898
Panel B: SOEs								
IF × ΔlogMW × Reform	0.527***	-0.320***	-0.047*	-0.102	0.103	0.286***	-0.029	-0.056
	(0.124)	(0.108)	(0.028)	(0.070)	(0.086)	(0.042)	(0.133)	(0.170)
IF × ΔlogMW	-0.132	0.068	0.047*	0.013	-0.039	-0.195***	-0.024	0.111
	(0.082)	(0.092)	(0.026)	(0.051)	(0.075)	(0.038)	(0.122)	(0.152)
IF	0.458***	-0.321***	0.012***	-0.116***	0.127***	0.002***	0.036***	0.139
	(0.016)	(0.011)	(0.003)	(0.007)	(0.009)	(0.003)	(0.013)	(0.012)
Observations	77,865	77,865	77,865	77,865	77,865	77,865	62,179	69,048
Panel C: Foreign-Owned Firms								
IF × ΔlogMW × Reform	2.143***	-1.151***	0.103***	-0.573***	0.681***	0.131***	1.485***	1.106***
	(0.435)	(0.282)	(0.033)	(0.148)	(0.180)	(0.035)	(0.408)	(0.269)
IF × ΔlogMW	-0.499	0.301	-0.094***	0.060***	-0.169	-0.095***	-0.519**	-0.389**
	(0.307)	(0.238)	(0.034)	(0.015)	(0.132)	(0.030)	(0.252)	(0.171)
IF	0.544***	-0.363***	-0.012***	-0.065***	0.104***	0.001	0.219***	0.156***
	(0.063)	(0.035)	(0.004)	(0.014)	(0.020)	(0.001)	(0.033)	(0.030)
Observations	220,706	220,706	220,706	220,706	220,706	220,706	167,542	207,961
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City -Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of minimum wage shocks on firm outcomes by ownership type, using the full sample and the main specification. Panels A, B, and C present results for private, state-owned (SOEs), and foreign-owned firms, respectively. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions use the full sample and include firm, ownership, industry-year, and city-year fixed effects. Curvature parameters *k+1* are set to 1.33, 1.36, 1.49 by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.1 Robustness: Wage Adjustment to Minimum Wage Changes (Dependent Variable: Δln W)

	(1)	(2)
	Δlog (W)	
	Full Sample	**Border Sample**
IF × ΔlogMW × Reform	0.869***	0.885***
	(0.136)	(0.120)
IF × ΔlogMW	-0.242***	-0.501***
	(0.084)	(0.099)
IF	0.519***	0.634***
	(0.025)	(0.028)
ΔlogMW	0.052	0.123**
	(0.057)	(0.050)
Observations	994,731	1,069,908
Firm FE	Yes	Yes
Ownership FE	Yes	Yes
Industry-Year FE	Yes	Yes
City-Year FE	Yes	No
Citypair-Year FE	No	Yes

Notes: This table presents robustness checks on the estimated effects of minimum wage shocks on log changes in firm-level average wages. Column (1) reports estimates from the full sample of firms, following Hau et al. (2020), with firm, ownership, and industry–year fixed effects. Columns (2) reports estimates from the border sample, restricted to firms located in counties adjacent to provincial minimum wage borders; these regressions include city–pair–year fixed effects and do not apply weights based on firms' exposure to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.35, 1.38, 1.53 (full sample) and 1.23, 1.28, 1.43 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.2 *Robustness: Firm Market Concentration and Exposure to Minimum Wage Changes*

	(1)	(2)	(3)	(4)
	Δlog (Markdown)		Δlog (Markup)	
	Full Sample	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	-0.411***	-0.493***	0.010	-0.020
	(0.091)	(0.086)	(0.012)	(0.014)
IF × ΔlogMW	0.083	0.254***	0.001	0.032**
	(0.067)	(0.074)	(0.011)	(0.014)
IF	-0.330***	-0.405***	0.011***	0.012***
	(0.015)	(0.018)	(0.001)	(0.001)
ΔlogMW	-0.024	-0.029	0.014	0.011
	(0.053)	(0.049)	(0.010)	(0.012)
Observations	994,731	1,069,908	994,731	1,069,908
Firm FE	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	No	Yes	No
Citypair-Year FE	No	Yes	No	Yes

Notes: This table presents robustness checks on the estimated effects of minimum wage shocks on log changes in firm-level markups and markdowns. Columns (1) and (3) report estimates from the full sample of firms, following Hau et al. (2020), with firm, ownership, and industry–year fixed effects. Columns (2) and (4) report estimates from the border sample, restricted to firms located in counties adjacent to provincial minimum wage borders; these regressions include city–pair–year fixed effects and do not apply weights based on firms' exposure to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters $k+1$ are set to 1.35, 1.38, 1.53 (full sample) and 1.23, 1.28, 1.43 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.3 *Robustness: Cost-Side Mechanisms—Employment, Capital Intensity, and Firm Exit Responses to Minimum Wage Exposure*

	(1)	(2)	(3)	(4)	(5)	(6)
	Δlog (L)		**Δlog (K/L)**		**Firm Exit**	
	Full Sample	**Border Sample**	**Full Sample**	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	-0.221***	-0.157***	0.324***	0.191**	0.145***	0.118***
	(0.048)	(0.052)	(0.072)	(0.079)	(0.031)	(0.022)
IF × ΔlogMW	0.111***	0.144***	-0.155***	-0.158**	-0.116***	-0.088***
	(0.035)	(0.054)	(0.051)	(0.072)	(0.028)	(0.020)
IF	-0.108***	-0.133***	0.131***	0.160***	0.000	0.000
	(0.008)	(0.009)	(0.009)	(0.012)	(0.001)	(0.001)
ΔlogMW	0.036	-0.031	-0.002	0.022	0.003	-0.000
	(0.029)	(0.027)	(0.031)	(0.031)	(0.008)	(0.010)
Observations	994,731	1,069,908	994,731	1,069,908	994,731	1,069,908
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Year FE	Yes	No	Yes	No	Yes	No
Citypair-Year FE	No	Yes	No	Yes	No	Yes

Notes: This table presents robustness checks on the estimated effects of minimum wage shocks on log changes in employment, capital intensity, and firm-exit. Columns (1), (3), and (5) report estimates from the full sample of firms, following Hau et al. (2020), with firm, ownership, and industry–year fixed effects. Columns (2), (4), and (6) report estimates from the border sample, restricted to firms located in counties adjacent to provincial minimum wage borders; these regressions include city–pair–year fixed effects and do not apply weights based on firms' exposure to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.35, 1.38, 1.53 (full sample) and 1.23, 1.28, 1.43 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.4 *Robustness: Productivity-Side Mechanisms—TFP and Value-Added per Worker*

	(1)	(2)	(3)	(4)
	Δlog (TFP)		**Δlog (YML)**	
	Full Sample	**Border Sample**	**Full Sample**	**Border Sample**
IF × ΔlogMW × Reform	0.198**	-0.063	0.342***	0.234**
	(0.099)	(0.097)	(0.086)	(0.104)
IF × ΔlogMW	-0.042	0.046	-0.058	-0.085
	(0.085)	(0.093)	(0.061)	(0.091)
IF	0.179***	0.048***	0.179***	0.226***
	(0.015)	(0.011)	(0.013)	(0.014)
ΔlogMW	0.061	0.081	0.058	0.071
	(0.050)	(0.050)	(0.044)	(0.050)
Observations	721,849	765,129	941,366	1,017,686
Firm FE	Yes	Yes	Yes	Yes
Ownership FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
City-Year FE	Yes	No	Yes	No
Citypair-Year FE	No	Yes	No	Yes

Notes: This table presents robustness checks on the estimated effects of minimum wage shocks on log changes in total factor productivity (TFP) and value-added per worker (YML). Columns (1) and (3) report estimates from the full sample of firms, following Hau et al. (2020), with firm, ownership, and industry–year fixed effects. Columns (2) and (4) report estimates from the border sample, restricted to firms located in counties adjacent to provincial minimum wage borders; these regressions include city–pair–year fixed effects and do not apply weights based on firms' exposure to multiple matches. The interaction term $IF_i \times \Delta lnMW$ captures firm-specific exposure before the 2004 reform; the triple interaction with $Reform_t$ captures post-reform differences. All regressions include firm, ownership, and industry-year fixed effects. City-year fixed effects are included in the full sample; city-pair-year fixed effects in the border sample. Curvature parameters *k+1* are set to 1.35, 1.38, 1.53 (full sample) and 1.23, 1.28, 1.43 (border sample) by firm size. Standard errors are clustered at the city level (full sample) and multiway clustered by city and city-pair-year (border sample). ***p < 0.01, **p < 0.05, *p < 0.1.

Appendix A. Construction of Markup and Markdown Indices

A markup is defined as the ratio of a firm's price to its marginal cost, representing the extent to which a firm charges above the cost of producing an additional unit of output. As a key indicator of market power, higher markups suggest that firms can set prices above competitive levels, often due to factors such as product differentiation, market concentration, or other barriers to competition. However, observing marginal costs directly is challenging, as they are rarely recorded in firm-level datasets. Further complicating this process is that many datasets only provide revenue rather than prices, requiring careful separation of price effects from quantity effects.

In this context, the De Loecker and Warzynski (2012, henceforth DLW) methodology offers a practical and widely-used approach to estimating markups. This method leverages firms' cost minimization behavior, which links the marginal revenue product of an input to its marginal cost. By observing a firm's expenditure on a price-taking input and estimating the input's output elasticity from a production function, the DLW method allows markups to be inferred even when marginal cost and price are unobserved. Crucially, the DLW framework is applicable to datasets where only revenue is reported, as it relies on input expenditure shares and estimated production elasticities, rather than requiring direct price data. Using the DLW approach, firm-specific markups, μ_{nt}^M, is expressed as:

$$\mu_{nt}^M \equiv \frac{\theta_{nt}^M}{\alpha_{nt}^M} \tag{A.1}$$

where θ_{nt}^M is the output elasticity with respect to a price-taking input M, and α_{nt}^M is that specific input's cost share in total revenue. This method assumes that input M is purchased in a perfectly competitive market. Under this condition, firms will choose input levels such that the marginal revenue product equals the input's marginal cost. The validity of interpreting the markup as a measure of output market power therefore hinges on this assumption. If the input market is imperfectly competitive, the wedge between θ_{nt}^M and α_{nt}^M reflects distortions in both input and output markets, making it difficult to isolate product market power.

Following standard practice in the industrial organization literature, we treat materials as the flexible, price-taking input, allowing the estimated markup μ_{nt}^M to reflect output market power alone. In contrast, markups computed using labour input often conflate pricing power in the product market with monopsony power in the labour market—arising from search frictions, labour market concentration, or heterogeneous worker preferences. To disentangle these effects, the markdown index, which proxies for monopsony power, is constructed as the ratio of labour-based markups to material-based markups:

$$\mu_{nt} \equiv \frac{\mu_{nt}^L}{\mu_{nt}^M} \tag{A.2}$$

Here, μ_{nt}^L denotes the markup estimated using labour input, and μ_{nt}^M corresponds to the markup based on materials input.x' By dividing the two indices, the markdown index isolates input market imperfections from output market distortion, providing a clearer measure of monopsony power in the input market. We follow the DLW framework to estimate the production function and obtain the input elasticities (θ_{nt}^M, θ_{nt}^L) using the Ackerberg et al. (2015) approach, applying a translog gross output production function with three inputs: labour, materials, and capital. The input cost share, $(\alpha^M_{nt}, \alpha^L_{nt})$,are directly calculated from the CASIF data.

While this approach is a standard way of estimating markup and markdown indices, it has two important limitations, as pointed out by Brooks et al. (2021b). First, the approach assumes a constant production function across firms within an industry, differing only by a factor-neutral productivity parameter. Second, identifying the production function typically requires assumptions that prevent the separate estimation of the output elasticity of materials—a key parameter needed to apply the De Loecker and Warzynski (2012) markup formula.

Brooks et al. (2021b) propose two alternative markup indices that are different assumptions than DLW method, namely the CRS (Constant Returns to Scale) method, and the CD (Cobb-Douglas) method. The CRS method calculates markups as the ratio of a firm's total sales to its total costs, including labour, materials, and capital. This is essentially capturing gross profit margin under the assumption that the production function is CRS and firm is price-taking in the input market. While it avoids imposing specific functional forms and allows for firm heterogeneity, the assumption of perfect input markets contradicts the goal of estimating markdowns, which rely on the presence of monopsony power. The CD method uses markup formula, $\mu^M_{nt} \equiv \frac{\theta^M_{nt}}{\alpha^M_{nt}}$, but assumes a Cobb-Douglas production function, where output elasticities are fixed (θ^M) and calibrated at the industry level instead of being directly estimated. This assumption simplifies the estimation process and addresses the identification issues associated with the DLW approach. While this method remains robust under non-CRS production functions and accommodates monopsony power in factor markets beyond materials, it does so at the expense of reduced firm-level flexibility.

These three markup estimation methods—DLW, CRS, and Cobb-Douglas—naturally yield three corresponding versions of the markdown index, since markdowns are constructed as the ratio of labour- to materials-based markups (Equation A.2) To ensure the robustness and interpretability of the markdown measure, we follow Brooks et al. (2021b) and estimate the comovement between markdowns and a firm's labour market share using the following regression:

$$\frac{\mu^L_{nt}}{\mu^M_{nt}} = \Gamma_t + \delta_n + \beta s^L_{nt} + \epsilon_{nt} \tag{A.3}$$

where $s^L_{nt} = \frac{\omega_{nt} l_{nt}}{\sum_{n \in l} \omega_{nt} l_{nt}}$ denotes firm n's share in the labour market at time t. We normalize markdowns to 1 for firms with negligible market share, assuming no monopsony power. This normalization ensures that markdowns are interpretable as relative measures of labour market power. Additionally, all markdown estimates are winsorized at the 3% tails by year and industry to limit the influence of outliers. This normalization and winsorization procedure is applied separately to each markdown index, corresponding to the three underlying markup estimation methods. Following Kondo et al. (2024), we define labour markets as segmented both geographically and by occupation. We use provinces to define geographic boundaries and 4-digit industries to capture occupational segmentation, assuming that workers are not perfectly mobile across these dimensions.