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**WHEN THE MINIMUM WAGE CREATES JOBS: MONOPSONY, MARKDOWNS, AND
TURKEY'S WAGE HIKES**

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When the Minimum Wage Creates Jobs: Monopsony, Markdowns, and Turkey's Wage Hikes

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Abstract. The employment effect of a minimum-wage increase depends on the wage-setting power of employers. The empirical literature, however, has measured this power only indirectly, through labor-market concentration. In this paper, we estimate firm-level product markups and wage markdowns for Turkish manufacturing firms using the Entrepreneur Information System (EIS) administrative micro-data. We show that a firm's *initial markdown* predicts the sign and magnitude of its direct employment response to minimum-wage increases, including the exceptionally large January 2016 increase (33% nominal, $\approx 25\%$ real). At minimum-wage firms with no markdown, employment falls year after year. At high-markdown firms that face the same wage floor, employment does not fall. The monopsony model can explain this difference, whereas the competitive model predicts employment losses for both groups of firms. Among firms paying the minimum wage, employment growth increases with the initial markdown in every year over 2013–2019: conditional on sector-year and province-year effects, high-markdown minimum-wage firms grow 6 to 9 log points faster annually than zero-markdown firms paying the same wage.

JEL codes: D24, J23, J31, J38, J42, L11

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1. Introduction

The employment effect of minimum-wage increases remains one of the most debated questions in labor economics. The disagreement is not only methodological, because the competitive and the monopsony models make opposite predictions for the same wage-floor increase. If firms take wages as given, a binding minimum wage raises labor costs above the marginal product of labor, and employment falls. If firms set wages along an upward-sloping labor supply curve, as in the monopsony model, a minimum-wage increase can raise employment because the firm no longer has to raise wages for all existing workers to hire additional ones. The sign of the employment response therefore depends on the degree of employer wage-setting power. However, the empirical literature has not been able to measure this power directly at the firm level.

Recent studies connecting the minimum wage to monopsony have used *labor-market concentration* as a proxy for employer power. [Azar et al. \(2024\)](#) show that, in the United States, the minimum wage reduces employment less in more concentrated local labor markets, and that in the most concentrated markets it may even increase employment. [Popp \(2024\)](#) and [Faia et al. \(2026\)](#) report similar findings. However, concentration is an indirect, market-level proxy for the variable that monopsony theory actually points to, namely the firm-level *markdown*, defined as the wedge between the marginal revenue product of labor and the wage. Two firms in equally concentrated markets can have very different degrees of wage-setting power, and it is the firm's own markdown that determines how it responds to a wage floor. A direct test of the monopsony model requires three ingredients: i) firm-level markdown estimates, ii) a sequence of sizeable minimum-wage increases, and iii) a theoretical prediction on how the sign of the employment response varies with the markdown. This paper brings these three ingredients together.

Our central result is that a firm's initial markdown predicts the sign and the magnitude of its direct employment response to a minimum-wage increase. The prediction follows from a standard wage-setting model with heterogeneous productivity. A firm with little or no markdown already pays a wage close to the marginal revenue product of labor and is constrained by labor demand. For this firm, a higher wage floor lowers employment, and the size of the decline is governed by the output elasticity of labor. A firm with a positive markdown pays less than the efficient wage and is constrained by labor supply. For this firm, a moderate increase in the floor *raises* employment by moving the firm along its labor supply curve and/or its labor demand curve, and the size of the increase depends on the labor-supply elasticity, the labor-demand elasticity, and the initial markdown. Finally, a firm already paying well above the new floor is unconstrained and does not respond. Therefore, the same policy reduces employment at some firms, raises it at others, and has no effect on a third group. It is not possible to generate this pattern with the competitive model under any parameterization, whereas the monopsony model

generates it through the markdown. The markdown, which we recover from production data, tells us which firms fall into which regime.

We bring this prediction to the universe of Turkish firms using the Entrepreneur Information System (EIS, *Girişimci Bilgi Sistemi*), the population of registered firms linked to social-security employment records (SSK/SGK) and observed from 2006 to 2021, and we exploit the sequence of minimum-wage increases over 2013–2019, with the January 2016 increase as the sharpest experiment. On January 1, 2016, the statutory minimum wage was raised by 33 percent in nominal terms, or roughly 25 percent in real terms once the year’s inflation is netted out. This was an unusually large and unanticipated increase, and, unlike the other increases of the period, it was implemented on a single date rather than in two semiannual steps. In the other years, the adjustments were largely eroded by inflation, but they still affected firms differently, because firm-level prices, demand, and productivity move differently from the aggregate. Therefore, firm-level variation identifies markdown effects in every year. The data combine firm balance sheets with social-security employment records, which allows us to estimate a gross-output production function with materials, the specification required to separate markups from markdowns, and to construct the firm-level labor shares the markdown calculation needs.

Our contribution is threefold. First, we provide joint firm-level estimates of product markups and wage markdowns for the Turkish manufacturing industry, using a non-neutral translog gross-output production function estimated by system GMM. This is, to our knowledge, the first set of markdown estimates for the near-universe of Turkish manufacturing firms, and it documents substantial monopsony power with pronounced gradients across firm size, sector, and time. Second, we show that the initial markdown predicts the direction and size of employment responses to minimum-wage increases, as the three-regime theory implies. These results show that the markdown is not merely descriptive. It is a direct measure of employer power that concentration indices approximate, and it predicts which firms gain and which firms lose from a wage floor. Third, we address the main identification concern about the production approach, raised by [Rubens et al. \(2025\)](#). They show that markdowns estimated from production functions may reflect unobserved differences in the output elasticity of labor across firms rather than monopsony power. We deal with this problem in two ways. First, we show that our employment results do not change when we control for the estimated output elasticity of labor. Second, we show that our markdown estimates move together over time with measures of labor-market concentration that are calculated independently of the production function.

Our paper is most closely related to recent studies on the role of employer wage-setting power in the employment effects of the minimum wage. [Azar et al. \(2024\)](#) find that the negative employment effect of the minimum wage in the United States is smaller in more

concentrated local labor markets, and that it becomes positive in the most concentrated ones. [Popp \(2024\)](#) and [Faia et al. \(2026\)](#) obtain similar results. In all of these studies, employer power is measured by labor-market concentration, and the monopsony channel is identified from differences across local labor markets. Our approach differs from theirs in two respects. First, we replace the concentration proxy with a direct, firm-level markdown estimated from production data. Second, we identify the employment response from *cross-firm* variation in that markdown over the years. The cost of our approach is that the markdown is estimated with error, a concern we address in detail below, whereas the concentration approach is exposed to the concern that concentrated markets differ in unobserved ways. The benefit is a sharper prediction. The studies based on concentration predict that the negative employment effect of the minimum wage will be weaker in some markets. Our framework makes a stronger prediction: under the same policy and in the same economy, employment falls at low-markdown firms and increases at high-markdown firms. In other words, the sign of the employment effect changes across the markdown distribution.

Our prediction rests on the markdown, and there is now ample evidence that markdowns are large. Using U.S. data, [Berger et al. \(2022\)](#), [Yeh et al. \(2022\)](#), and [Lamadon et al. \(2022\)](#) document widespread wage markdowns. The surveys by [Manning \(2021\)](#) and [Sokolova and Sorensen \(2021\)](#) reach the same conclusion for a broader set of studies. [Amodio and De Roux \(2024\)](#), [Brooks et al. \(2021\)](#), and [Soundararajan \(2019\)](#) concentrate on developing economies and find monopsony power to be, if anything, greater where labor-market institutions are weaker. We build on this body of work by taking its central object, the markdown, and showing that its firm-level variation is important for the employment effects of minimum-wage policy. In recovering markups and markdowns jointly we follow the production approach of [De Loecker et al. \(2020\)](#) and the markdown extensions surveyed in [Syverson \(2025\)](#); the most direct prior attempt to estimate both objects together for a developing economy is [Casacuberta and Gandelman \(2023\)](#) for Uruguay, who relate them to a wage-setting institution and find that mandated wages compress markdowns on average while firms pass part of the cost into prices. We adopt their methodological template but shift the question from the *average level* of markdowns to whether their *pre-existing distribution* predicts *heterogeneous employment responses* to minimum-wage increases, a prediction their levels-based design cannot test, and one that requires direct firm-level markdown estimates.¹

These results have direct policy implications. The textbook treats the minimum wage as a uniform tax on employment. Our results indicate that its employment effects depend on the monopsony structure of the economy in which it is applied. This point is especially

¹[Gürer and Derin-Güre \(2025\)](#) use the same EIS data and estimate product markups, but not markdowns, for Turkish manufacturing. They find that markups increased after 2016, especially among high-markup firms, during the high-inflation period. We estimate markdowns in addition to markups, and we relate the markdown to the employment effects of minimum-wage policy.

important for developing economies, where employer power is often substantial and the minimum wage is a primary instrument of labor-market policy.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 develops the theoretical framework, combining the production approach to markup and markdown estimation with a wage-setting monopsony model, and derives the three employment-response regimes and the sufficient-statistic property of the markdown. Section 4 describes the EIS data and the production function estimation method, and discusses the identification problem raised by Rubens et al. (2025). The markup and markdown estimates are presented in Section 5. In Section 6, we present our main empirical results on the effects of minimum-wage increases on employment in the three groups of firms defined by their markdowns, together with a number of robustness checks, including controls for the output elasticity of labor and alternative definitions of the groups. Section 7 concludes.

2. Literature Review

Our paper is related to four strands of the literature: i) the employment effects of the minimum wage, ii) the measurement of labor market power, iii) the production approach to the estimation of markups and markdowns and the criticisms of this approach, and iv) the small number of studies that estimate markups and markdowns jointly. In this section, we briefly review these four strands and discuss how our paper is related to each of them. We do not attempt to provide a comprehensive survey of these literatures.

2.1 The minimum wage, employment, and monopsony

The employment effect of the minimum wage has been debated for three decades without any resolution. The influential quasi-experimental studies that began with Card and Krueger (1994) found small or negligible disemployment effects. A large subsequent literature, using border discontinuities, bunching estimators, and event-study designs (e.g., Cengiz et al., 2019), has continued to find small or negligible effects, while other work and successive meta-analyses report a wider range of estimates. We do not try to settle this debate. We take one observation from it. If the employment effect of the minimum wage is heterogeneous across firms in a structured way, then the search for a single representative elasticity would be misleading. Furthermore, the dispersion of estimates may reflect genuine heterogeneity in the labor-market structure that firms face rather than methodological disagreement alone.

The monopsony model provides such a structure. The basic result goes back to Robinson (1933) and has been taken up again by the recent monopsony literature (Manning, 2003, 2021). A monopsonist that wants to hire one more worker has to raise the wage of

all its existing workers as well. A minimum wage level between the monopsony wage and the competitive wage removes this cost, because the firm can now hire additional workers at the minimum wage without raising anyone else’s wage. As a result, employment *increases*. A binding floor above the competitive wage, by contrast, lowers employment as in the competitive model. The sign of the employment effect therefore depends on where the firm sits relative to its competitive benchmark, that is, on its degree of monopsony power.

These predictions have been tested in several recent empirical studies. Using U.S. data, [Azar et al. \(2024\)](#) show that the negative employment effect of the minimum wage is weaker in local labor markets where a few firms account for a large share of employment, and that the effect can even become positive in the most concentrated markets. [Popp \(2024\)](#) obtains similar results. [Faia et al. \(2026\)](#) also find that the employment effect of the minimum wage depends on monopsony power and markdowns. In these studies, employer power is measured by the concentration of employment in the local labor market, either with the Herfindahl index or with the number of competing employers, and the effect of the minimum wage is estimated from differences in concentration across local labor markets. Concentration is an attractive proxy because it can be observed without estimating a production function. Two firms in equally concentrated markets may have quite different wage-setting power. Hence, concentration need not map monotonically into the firm-level markdown that the theory points to. Our paper departs from this literature: we replace the concentration proxy with a direct, firm-level markdown and identify the employment response from cross-firm variation under the same national wage floors rather than from cross-market variation in concentration.

2.2 Measuring labor market power: markdowns

The wage markdown, the gap between the marginal revenue product of labor and the wage, is the object that the minimum-wage literature proxies with concentration. It has become measurable at the firm level, and recent work shows that it is both pervasive and large. [Yeh et al. \(2022\)](#) use U.S. manufacturing data and find that the average markdown is well above one and that it varies considerably across plants. [Berger et al. \(2022\)](#) build an oligopsony model and use it to calculate the effects of labor market power on aggregate output and on the distribution of income. [Lamadon et al. \(2022\)](#) use matched employer-employee data for the United States and find evidence of rent sharing and imperfect competition in the labor market. [Manning \(2021\)](#), [Sokolova and Sorensen \(2021\)](#), and [Syverson \(2025\)](#) review the empirical evidence and discuss the problems of identification. The last is particularly relevant here: it emphasizes that the ratio of a factor’s output elasticity to its payment share identifies only the *product* of the markup and the markdown. Isolating the markdown requires either assuming the product markup

away or estimating it independently, the strategy we adopt in Section 3.

There is also evidence that monopsony power is larger in developing countries. In these countries, labor-market institutions are weaker and workers have fewer alternative job opportunities. For example, [Amodio and De Roux \(2024\)](#) find high markdowns for Colombian manufacturing plants. Using data from China and India, [Brooks et al. \(2021\)](#) emphasize the relationship between classical monopsony power and the labor share in these countries. [Soundararajan \(2019\)](#) shows heterogeneous effects of imperfectly enforced minimum wages in low-wage labor markets. These studies are also relevant for our choice of country. Turkey is a large middle-income economy. Monopsony power is likely to be important in its labor markets, and the minimum wage is one of the main labor-market policy instruments in the country. However, there have been no direct markdown estimates for Turkey so far.

Our estimates for Turkish manufacturing, an average log markdown of roughly 0.15 in the pre-2016 period, so that wages fall short of marginal revenue product by about 15 percent at the mean, are markedly smaller than the averages these studies report: [Yeh et al. \(2022\)](#) estimate an average markdown of 1.53 for U.S. manufacturing plants, so that a worker earns only 65 cents on the marginal dollar generated, and [Amodio and De Roux \(2024\)](#) estimate a firm-level labor supply elasticity of about 2.5 for Colombia, implying that workers produce roughly 40 percent more than their wage. Part of the gap reflects the lower-bound property of our materials-margin identification (Section 3.3).

2.3 The production approach and its critics

We estimate markdowns with the production function approach. This approach is based on a simple result of cost minimization: for any input that the firm can adjust freely, the ratio of the output elasticity of that input to its share in revenue is equal to the markup ([Hall, 1988](#); [De Loecker and Warzynski, 2012](#)). [De Loecker et al. \(2020\)](#) use this method to document a pronounced rise in aggregate U.S. markups, and [Yeh et al. \(2022\)](#) extend it to recover markdowns by normalizing the labor first-order condition against a competitively priced input. The approach is attractive because it requires only production data and revenue shares, with no need to specify demand or labor-supply systems.

The approach has, however, attracted criticisms that directly challenge the credibility of any markdown estimates. Our empirical strategy addresses these criticisms. [Raval \(2023\)](#) shows that the production function approach implies the testable restriction that every flexible input should recover the same markup. Using manufacturing data from six countries, he finds that this restriction is rejected in the data, and he argues that the rejection is due to non-neutral (factor-augmenting) productivity differences across firms. [Hashemi et al. \(2022\)](#) argue that the production function approach often captures

distortions in input markets rather than market power in the output market. [De Ridder et al. \(2026\)](#) show that estimating production functions on revenue rather than quantity data biases the markup estimates while preserving their trends and dispersion. The study that is most closely related to our work is [Rubens et al. \(2025\)](#). They show that markdowns obtained from production function estimates are consistent only under Hicks-neutral technology. If output elasticities differ across firms and the estimator does not capture these differences, part of the variation in the output elasticity of labor will be attributed to the markdown. As a result, the estimated markdown and the output elasticity of labor will be negatively correlated even when the true values are unrelated. Our preferred specification allows for non-neutral technology, the case in which this bias arises. For this reason, we consider the [Rubens et al. \(2025\)](#) critique to be the main threat to identification, and we address it in Sections 4.5 and 6.4.

2.4 Markups and markdowns together, and the Turkish context

A small number of papers estimate product markups and wage markdowns jointly, which is necessary because the two are confounded in the labor-based markup ratio. The closest predecessor to our measurement exercise is [Casacuberta and Gandelman \(2023\)](#), who estimate both objects for Uruguay, a middle-income economy with a centralized wage-setting institution, and find that mandated wages compress markdowns on average while firms pass a sizeable share of the cost into product markups. Their setting and method are close to ours, but their question concerns the *average level* of markdowns and its institutional determinants; ours concerns whether the *pre-existing distribution* of markdowns predicts the heterogeneous employment response to minimum-wage increases. Related joint-estimation work includes [Mertens \(2022\)](#) for German manufacturing, among others.

For Turkey, the markup literature is thin and the markdown literature essentially absent. [Gürer and Derin-Güre \(2025\)](#) use the same EIS data we employ to document a post-2016 rise in manufacturing markups concentrated among high-markup firms during the high-inflation era, and [Colak et al. \(2025\)](#) relate market concentration to firm markups in Turkey under inflationary pressures; neither estimates markdowns, and neither connects market-power estimates to the employment effects of minimum-wage policy. We fill this gap and, to our knowledge, provide the first joint estimates of markups and markdowns for the near-universe of Turkish manufacturing firms.

3. Theoretical Framework

This section builds the theory in two steps. We first show how product and labor market power are identified from a general production function, keeping the production technology unrestricted so that the framework is consistent with the flexible translog specification

we take to the data in Section 4 (Subsections 3.1–3.3). We then specialize to a tractable parametric case to characterize how a firm’s employment responds to a minimum-wage increase, and we show that the response is governed entirely by the firm’s initial markdown (Subsections 3.4–3.6). The closed-form expressions in the second step are the tractable special case of the general framework in the first. We use them because they give simple comparative statics with unambiguous signs. The empirical work, however, does not depend on these closed-form expressions. The identification strategy in the first step follows De Loecker and Warzynski (2012), Yeh et al. (2022), and the survey by Syverson (2025). The minimum-wage analysis in the second step is closest in spirit to Bassier and Budlender (2025).

3.1 Setup

A firm i in sector s at time t produces a gross output Q_{it} with capital K_{it} , labor L_{it} , and materials M_{it} through a production function $Q_{it} = F_s(K_{it}, L_{it}, M_{it}) \Omega_{it}$, where Ω_{it} is Hicks-neutral productivity. The firm may have market power in both product and labor markets. In the product market, it faces a downward-sloping demand curve and sets a price P_{it} above marginal cost c_{it} ; we define the *markup* as $\mu_{it} \equiv P_{it}/c_{it} \geq 1$. In the labor market, it may face an upward-sloping firm-level labor supply curve $L_{it} = L^s(W_{it}; \mathbf{Z}_{it}^L)$, which implies that the firm must raise the wage to hire an additional worker; we define the *markdown* as the ratio of the marginal revenue product of labor to the wage,

$$\eta_{it} \equiv \frac{MRPL_{it}}{W_{it}} \geq 1, \quad (1)$$

If the firm takes the wage as given, $\eta_{it} = 1$. If the firm pays its workers less than their marginal revenue product, $\eta_{it} > 1$.²

3.2 Markup and markdown identification

Following De Loecker and Warzynski (2012) and Yeh et al. (2022), we obtain the markup and the markdown from the first-order conditions of the firm’s cost-minimization problem. For any input chosen flexibly and purchased in a competitive factor market, the markup equals the ratio of that input’s output elasticity to its revenue share. We use materials as the reference input, and assume throughout this subsection that materials are purchased in a competitive spot market with no adjustment costs and no buyer power, so that the

²This is the markdown definition of the underlying model, $\eta = MRPL/W \geq 1$, which runs parallel to the markup $\mu = P/c \geq 1$. Some researchers define the markdown as $(MRPL - W)/W$ instead of $MRPL/W$; the former is close to $\log(MRPL/W)$, the object we use in our empirical analysis. The labor-based ratio $\mu_L \equiv \theta_L/s_L$ that appears in the production approach identifies the *product* $\mu\eta$, not the markdown alone; isolating η requires an independent estimate of μ , which is the role of the materials margin in (2) below.

materials first-order condition holds without a markdown wedge. Under this assumption the markup is identified from the materials margin alone,

$$\mu_{it} = \frac{\theta_{M,it}}{s_{M,it}}, \quad (2)$$

where $\theta_{M,it} = \partial \ln Q_{it} / \partial \ln M_{it}$ is the output elasticity of materials and $s_{M,it} = P_{it}^M M_{it} / (P_{it} Q_{it})$ its revenue share. Equivalently, the markup can be written in terms of the firm's demand elasticity: with profit maximization implying $MR_{it} = P_{it}(1 - 1/\epsilon_Q) = c_{it}$, the markup is $\mu_{it} = (1 - 1/\epsilon_Q)^{-1}$, where throughout we take $\epsilon_Q > 1$ to denote the absolute value of the (downward-sloping) demand elasticity, so that $\mu_{it} \geq 1$.³ Labor, by contrast, may be marked down. Its first-order condition is

$$\frac{\theta_{L,it}}{s_{L,it}} = \mu_{it} \eta_{it}, \quad (3)$$

so that the labor-based “markup” $\theta_{L,it}/s_{L,it}$ identifies the *product* of the markup and the markdown. Dividing (3) by (2) isolates the markdown:

$$\eta_{it} = \frac{\theta_{L,it}/s_{L,it}}{\theta_{M,it}/s_{M,it}} = \frac{\theta_{L,it} s_{M,it}}{\theta_{M,it} s_{L,it}}. \quad (4)$$

Equation (4) is the empirical content of the framework: the markdown is the gap between the labor-based and materials-based markups. Both objects require only the output elasticities and the observed revenue shares.

3.3 The markdown is a lower bound on labor market power

We identify the markup μ_{it} in (2) from the material inputs margin under the assumption that material inputs themselves are not marked down. If in fact firms also enjoy buyer power in material input markets, the materials-based markup overstates the true product-market markup. Consequently, the markdown will be underestimated. Formally, if the first-order condition for material inputs carries a markdown wedge $\eta_{it}^M > 1$, the object recovered from the materials margin is the product $\mu_{it}\eta_{it}^M$ rather than μ_{it} , and substituting into (4) yields an estimated markdown $\hat{\eta}_{it} = \eta_{it}/\eta_{it}^M < \eta_{it}$ (Appendix A). The estimated η_{it} is therefore a lower bound on the firm's true labor market power, absent other wedges such as labor adjustment costs. The practical implication is that wherever we report a markdown, the true degree of monopsony is at least that large; our central employment result, which holds for firms with positive estimated markdowns, is if anything strengthened by the bound.

³We adopt the convention $\epsilon_Q > 1$ (the absolute value of the demand elasticity) uniformly, so that $MR = P(1 - 1/\epsilon_Q)$ and $\mu = (1 - 1/\epsilon_Q)^{-1} \geq 1$; the same sign convention is used in the minimum-wage block of Section 3.4.

3.4 Monopsony equilibrium and the minimum wage

In this subsection, we use a simple parametric version of the model in order to derive the employment response to a wage floor. We assume that the production function is Cobb-Douglas in labor, $Q = AL^{\theta_L}$ with $0 < \theta_L < 1$. The firm faces an isoelastic labor supply curve, $L = Z(W/P^c)^{\epsilon_L}$, where P^c is the consumer price index and $\epsilon_L \geq 0$ is the labor-supply elasticity faced by the firm. Product demand is also isoelastic, $Q = DP^{-\epsilon_Q}$ with $\epsilon_Q > 1$. Lower-case letters denote logarithms. The marginal revenue product of labor and the marginal cost of labor are, in logs,

$$mrpl = p + \log(1 - 1/\epsilon_Q) + \log \theta_L + a - (1 - \theta_L)l, \quad (5)$$

$$mcl = p^c + \log(1 + 1/\epsilon_L) - z/\epsilon_L + l/\epsilon_L. \quad (6)$$

The vertical distance between the mcl and labor-supply (ls) schedules is $mcl - ls = \log(1 + 1/\epsilon_L)$.⁴ The monopsonist hires where $mrpl = mcl$, yielding employment l^m and wage w^m ; the efficient (wage-taking) allocation (l^e, w^e) solves $mrpl = ls$. Comparing the two,

$$l^e - l^m = \frac{\log(1 + 1/\epsilon_L)}{1 - \theta_L + 1/\epsilon_L} \geq 0, \quad w^e - w^m = \frac{l^e - l^m}{\epsilon_L} \geq 0, \quad (7)$$

so the monopsonist restricts employment and depresses wages relative to the competitive benchmark, by an amount increasing in the markdown. Full derivations are in Appendix B.

3.5 The employment response to a minimum-wage increase

A minimum wage w^* replaces the lower envelope of the marginal-cost-of-labor schedule with a horizontal segment at w^* up to the labor-supply curve, after which the original (upward-sloping) mcl resumes. The firm's response depends on where the floor sits relative to its monopsony wage w^m and its efficient wage w^e , equivalently, on its initial markdown. Let Δw^* denote the log increase in the floor. Three cases arise (Figure 1; full derivations in Appendix B).

Case 1 (demand-constrained), $w^* > w^e > w^m$. When the floor exceeds the efficient wage, the firm has no markdown left to absorb the increase ($\log \eta = 0$ at the floor) and employment is set by labor demand. A further increase lowers employment by

$$\Delta l = -\frac{\Delta w^*}{1 - \theta_L} < 0, \quad (8)$$

⁴This distance equals the log markdown, $\log \eta$, at the unconstrained monopsony equilibrium (Case 3 in Figure 1); when a wage floor binds, the realized markdown is smaller than $\log(1 + 1/\epsilon_L)$.

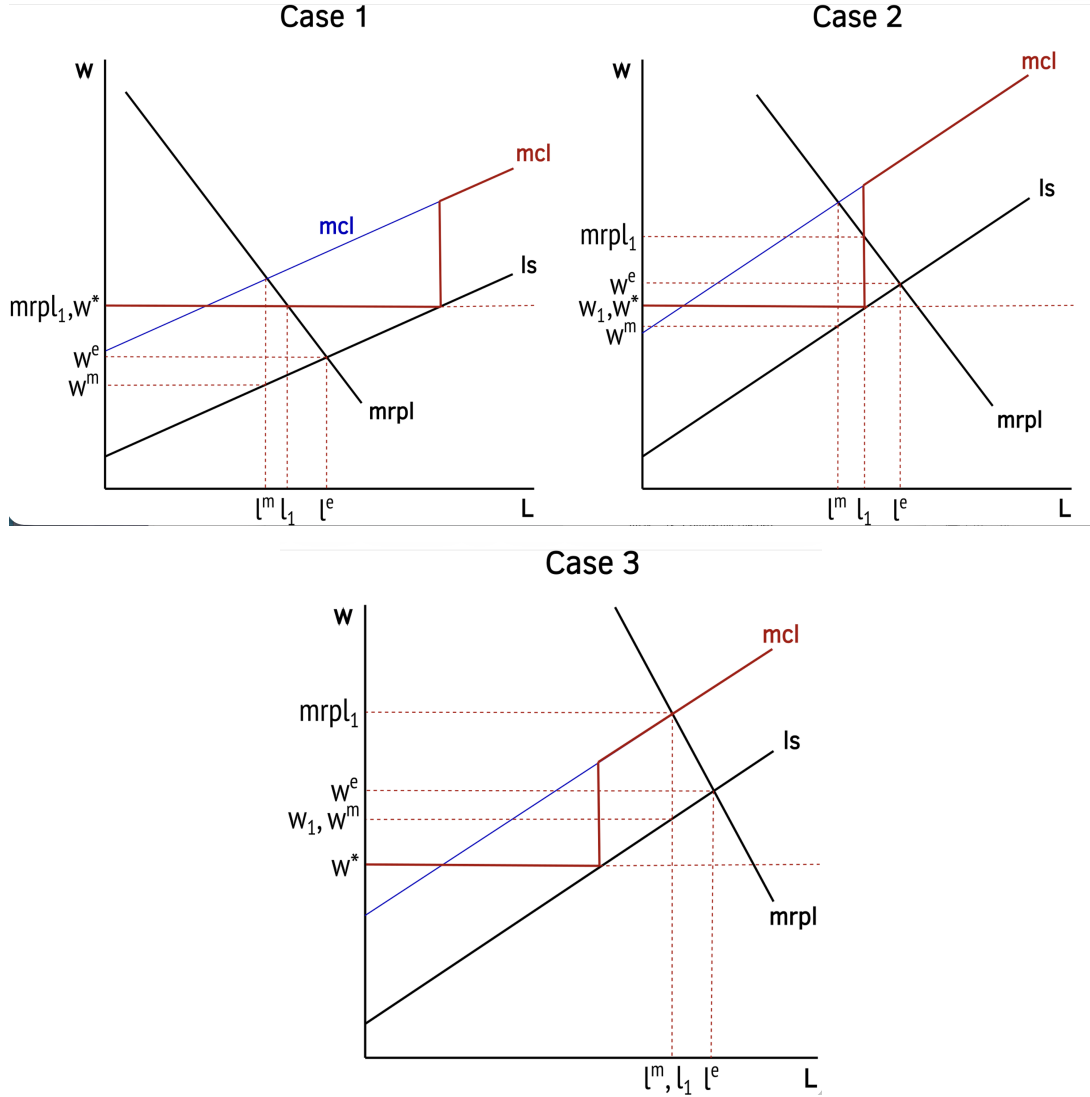


Figure 1: The employment response to a binding minimum wage under monopsony. Each panel plots the marginal revenue product of labor ($mrpl$), the labor-supply schedule (ls), and the marginal cost of labor (mcl) against (log) employment L . The minimum wage w^* replaces the lower part of the mcl schedule with a horizontal segment up to ls , after which the original upward-sloping mcl (thin blue) resumes; the post-floor schedule is drawn in red. Initial monopsony employment is l^m , the efficient (wage-taking) level is l^e , and l_1 is employment after the new floor is imposed. *Case 1* ($w^* > w^e > w^m$): the floor exceeds the efficient wage, the markdown is exhausted, and employment falls along $mrpl$ ($l_1 < l^e$). *Case 2* ($w^e > w^* > w^m$): the floor lies between the monopsony and efficient wages, the firm moves up its supply curve, and employment rises toward the efficient level ($l^m < l_1 \leq l^e$), and starts to decrease along the $mrpl$ curve when w^* exceeds w^e ; as long as $w^* < \eta w^m$, an increase in w^* increases employment relative to l^m . *Case 3* ($w^m > w^*$): the floor lies below the monopsony wage, does not bind, and employment is unchanged ($l^m = l_1$).

with the loss governed by the output elasticity of labor θ_L . The markdown remains zero.⁵

Case 2 (supply-constrained), $w^e > w^* > w^m$. When the floor lies between the monopsony and efficient wages, the firm pays the floor and employment is set by labor supply, so a moderate increase *raises* employment by

$$\Delta l = \epsilon_L \Delta w^* > 0, \quad (9)$$

moving the firm up its supply curve toward the efficient allocation, with the gain first governed by the labor-supply elasticity ϵ_L up to the efficient wage w^e . If the minimum wage w^* exceeds the efficient wage, the level of employment changes along the labor demand curve, and the change in employment is governed by the elasticity of $mrpl$, that is, the output elasticity of labor θ_L . The markdown falls. Employment increases as long as the (log) change in the minimum wage does not exceed the firm's initial (log) markdown,

$$\boxed{\Delta w^* < \log \eta_{(\text{initial})}}. \quad (10)$$

Condition (10) is the key inequality for the empirical design. A firm whose markdown exceeds the size of the hike can still set a wage below its workers' marginal product after the increase, and gains employment; the gain in employment depends on the initial (log) markdown and on the slopes of the labor demand (θ_L) and labor supply (ϵ_L) schedules. We cannot identify the value of ϵ_L for supply-constrained firms. However, the observed markdown provides an upper bound for it. The structural wedge satisfies $\log(1 + 1/\epsilon_L) \geq \log \eta$, which implies $\epsilon_L \leq 1/(\eta - 1)$ (see Case 2 in Figure 1). The largest increase in the real minimum wage during our sample period was approximately 25 percent (21 log points), in 2016. Therefore, firms with an initial log markdown above 0.25 are predicted to gain employment in all years.

Case 3 (unconstrained), $w^m > w^*$. A wage floor that lies below the monopsony wage is not binding. The firm already pays more than the minimum. Neither employment nor wages respond, $\Delta l = \Delta w = 0$, as long as the minimum wage remains below the monopsony wage w^m .

The markdown is a sufficient statistic for the sign. Taken together, the three cases yield the paper's central theoretical result. Consider firms paying the *same* wage to all employees near the minimum, so that they face the same mechanical cost shock and differ only in their initial markdown. Among such firms, the sign of the direct employment response is determined entirely by the markdown: employment falls for firms with no

⁵If the product price (p) or productivity (a) also change, then $\Delta l = (\Delta p + \Delta a - \Delta w^*)/(1 - \theta_L)$. Thus, if $\Delta w^* = \Delta p + \Delta a$, employment does not change.

markdown (Case 1) and rises for firms whose markdown exceeds the increase (Case 2 under (10)). In this sense the initial markdown is a *sufficient statistic for the sign of the direct employment response, among firms paying the same wage*. No prediction of this form exists under perfect competition, where every firm bound by the floor reduces employment. The competitive model predicts that the employment effect has the same sign for all firms, whereas the monopsony model predicts that the sign changes across the markdown distribution. We test this prediction in Section 6. These predictions concern the direct effects of the minimum wage. In the next subsection, we discuss the indirect channels that may weaken them.

3.6 Indirect effects and adjustment costs

The cases above describe *direct* effects, while holding prices, productivity, and labor demand fixed. In a high-inflation economy with strict employment protection, three channels may weaken them.

Price pass-through. If firms pass the wage increase fully into prices, so that $\Delta p = \Delta p^c = \Delta w^*$, the *mrpl*, *ls*, and *mcl* schedules all shift upward by the same amount, real wages are unchanged, and the direct employment and markdown effects vanish. With partial pass-through the direct effects are attenuated in proportion to the share of the increase absorbed by prices. This is why the high-inflation setting is both the source of the sharp real shock and a threat to identification: if Turkish firms neutralized the minimum-wage increase through prices, the case predictions would not hold. However, the 2016 increase was too large and too sudden to be neutralized through prices within the year.

Demand shifts and spillovers. Consider an exogenous increase in product demand, which also increases the demand for labor. In unconstrained firms (Case 3), both wages and employment increase. In constrained firms (Cases 1–2), only employment increases as long as the demand shift is moderate. A second channel works through on-the-job search. When workers can search for better jobs while employed, firms that are not directly bound by the floor (Case 3) may have to raise their wage offers in order to keep their workers, so that the minimum wage has a spillover effect on these firms as well.

Adjustment costs. The direct effects are also slowed down by the cost of adjusting employment, which in Turkey falls mainly on dismissals. A worker with at least one year of tenure is entitled to severance pay of one month’s wage per year of service, computed on the last wage, in addition to a notice period. For a demand-constrained firm (Case 1), the reduction in (8) is therefore not realized at once. Dismissing a worker costs the firm the

severance liability, and the liability itself rises with the minimum wage, because severance is calculated on the new, higher wage. The firm therefore prefers to reduce employment through attrition, by not replacing workers who leave, and the employment loss is spread over several years. For a supply-constrained firm (Case 2), firing costs do not bind on impact, since the firm is hiring, but they raise the expected cost of every hire and so reduce the gain along the supply curve. Adjustment costs thus attenuate the response in both regimes and spread it over time. They do not change its sign. The three channels in this subsection therefore leave the sign predictions of the same-wage comparison intact but may reduce the magnitudes and delay them; the year-by-year design of Section 6 allows the response to unfold over more than one window, and we take the channels into account in the robustness checks.

4. Data and Estimation

This section sets out the macroeconomic background against which the minimum-wage increases took place, describes the data and the construction of the markdown-based firm groups that operationalize the three regimes of Section 3, and presents the production-function estimation and our treatment of the identification concern raised by [Rubens et al. \(2025\)](#).

4.1 Macroeconomic background: wages and prices, 2009–2021

Figure 2 plots the annual log changes of the statutory minimum wage, the average wage in manufacturing, and the consumer price index over 2009–2021. Three features of this period are important for our analysis.

First, in the years before 2016 the minimum wage was adjusted routinely and predictably, in two semiannual steps, at rates of 9 to 12 log points per year against consumer inflation of 6 to 9 log points. The real wage floor therefore drifted up only slowly, and much of even that modest real gain was matched by real wage growth elsewhere in the distribution. The January 2016 adjustment was different: the minimum wage rose by 29 log points against inflation of 7.5, an unanticipated real increase of roughly 25 percent (about 21 log points), concentrated on a single date. The adjustment for 2019 was the second largest (23 log points), but it arrived after inflation had accelerated to 14 log points, so its real content was less than half that of 2016. From 2017 onward, accelerating inflation turned the annual adjustments into largely inflation-matching updates.

Second, the average manufacturing wage tracks the minimum wage very closely throughout the period. The co-movement is strongest in 2016, when average wages increased by 25 log points as the floor rose by 29 log points. The reason is that a large share of manufacturing workers is paid at or near the minimum wage, so statutory adjustments move

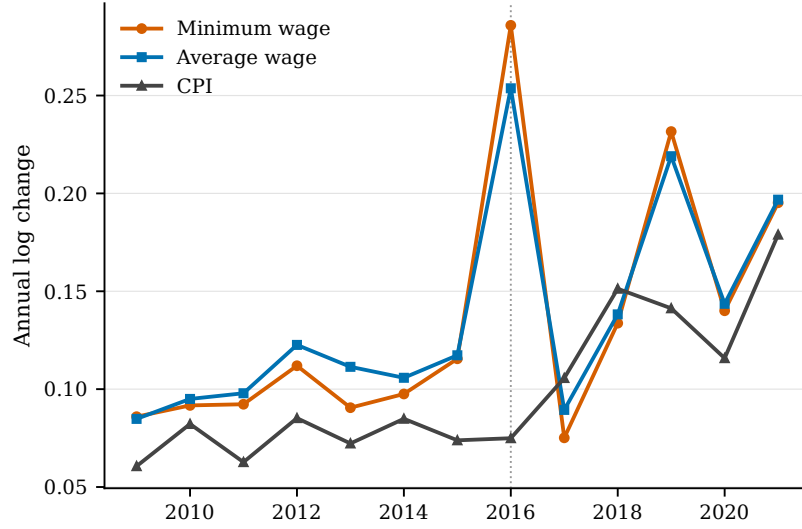


Figure 2: Minimum wage, average wages, and consumer prices: annual log changes, 2009–2021. Log changes of the statutory minimum wage, the average wage in manufacturing (EIS), and the CPI. The dotted line marks 2016: the minimum wage rose by 29 log points against CPI inflation of 7.5 log points.

a large part of the wage distribution. This close co-movement also explains why the firm groups defined in Section 4.3 are well populated, and it is consistent with the first-stage result of Section 6: for firms paying the minimum wage, statutory increases translate into wage growth almost one for one.

Third, inflation plays an important role throughout the period. It eroded each nominal adjustment within the year, it restored the pre-2016 markdown levels within two years of the 2016 collapse (Section 5.1), and it is the reason the identifying variation of Section 6 is firm-level rather than aggregate: when the floor and the price level rise together, the real bite of the floor at any particular firm depends on that firm’s own prices, demand, and productivity.

4.2 The Entrepreneur Information System

We use the Entrepreneur Information System (EIS, *Girişimci Bilgi Sistemi*), an administrative database maintained by the Ministry of Industry and Technology of Turkey. The EIS brings together firm-level data from several administrative sources. These are the enterprise registers, annual balance sheets and income statements, employee registers that are linked to social-security records (SSK/SGK), records of transactions between firms, and foreign-trade declarations. The EIS covers all registered firms in Turkey for the period 2006–2021. Two features of the data are important for our study. First, because the EIS covers the population of firms rather than a sample of publicly traded firms (e.g. Compustat) or a survey-based manufacturing panel, there is no selection on size or listing status of the kind that complicates much of the markup and markdown

literature. Second, the employer-employee linkage through social-security records lets us observe each firm’s wage bill, headcount, and the wage distribution of its workers. This information makes the markdown-based grouping below feasible.

From the balance sheets and income statements we construct firm-level gross output, intermediate materials, capital, and the revenue shares of labor and materials. The employee registers provide days worked, the wage bill, and, critically for the grouping, the position of each firm’s wages relative to the statutory minimum in a given quarter. The inter-firm transaction records provide the input-price and output-price information used to deflate nominal values at the firm level. The production functions are estimated at the NACE 2-digit level. All nominal variables are deflated with firm-level price indices, which we construct from the inter-firm transaction records and from firm-level import and export data. We drop firm-years with missing or non-positive values of output, inputs, or input shares. After these screens, the markup and markdown estimation sample includes roughly 66,000 to 110,000 manufacturing firms per year for the 2008–2021 period. The employment analysis in Section 6 uses the 2012–2019 panel described in that section.

4.3 From regimes to groups: the markdown-based classification

The theory that we presented in Section 3 predicts that a firm’s employment response to an increase in the minimum wage depends on the position of its markdown relative to the wage floor. Because markdowns are estimated at the firm level but employment heterogeneity within a firm (across worker types) is not, we isolate firms whose workforces are homogeneous with respect to the minimum wage, firms that pay all of their workers at or near a common point of the wage distribution, so that the firm-level markdown is a meaningful summary of the relevant labor. To be more specific, we classify firms by (i) the position of their wages relative to the statutory minimum in the fourth quarter and (ii) their estimated log markdown $\log \eta_i$. This yields the following groups, which map directly onto Cases 1–3 of Section 3.5.

- **Group 1 (Case 1, demand-constrained).** Firms that pay all workers within $\pm 1\%$ of the minimum wage and whose log markdown is non-positive, $\log \eta_i \leq 0$. With no markdown to absorb the increase, these firms are predicted to *lose* employment (Case 1 of Section 3.5).
- **Group 2 (Case 2, supply-constrained).** Firms that pay all workers within $\pm 1\%$ of the minimum but have a log markdown above 0.25, $\log \eta_i \geq 0.25$. The threshold is 0.25 because the real increase in the minimum wage was approximately 25% in 2016, the largest increase we observe in the analysis period. The theory predicts that Group 2 firms (whose markdown exceeds the hike) *gain* employment, with the level of the gain depending on the initial level of the markdown and the output elasticity

of labor.

- **Group 3 (Case 3, unconstrained).** Firms that pay all workers at least 25% above the minimum in the fourth quarter and have a positive log markdown. The floor lies below the prevailing wage, so the theory predicts *no* direct response (Case 3 of Section 3.5), keeping everything else constant. However, prices, productivity, and labor demand may change for a number of reasons. Furthermore, we estimate one markdown value per firm, but various categories of employees with different supply elasticities may be employed by the firm. Finally, markdowns are estimated with error, so that a firm classified into Group 3 may in fact have a markdown smaller than 0.25. Thus, the level of the markdown and the labor elasticity may still play a role for Group 3 firms, albeit to a lesser extent than for Group 2 firms.

The empirical content of the same-wage prediction of Section 3.5, the sign reversal, is thus a prediction about the *ordering* of employment responses across these groups: negative for Group 1, positive for Group 2, and null for Group 3. Section 6 tests exactly this ordering.

4.4 Production function and estimation

In order to calculate the markup in (2) and the markdown in (4), we need the output elasticities $\theta_{L,it}$ and $\theta_{M,it}$, and these are obtained from the estimation of a production function. We estimate a gross-output translog production function. The translog form has two advantages. First, the Cobb-Douglas form is a special case of the translog form, so that it can be tested against the translog. Second, the interaction terms in the translog form allow for factor-augmenting (non-Hicks-neutral) technical change, which the critics of the production approach regard as a major source of bias in markdown estimates. Let y_{it} , k_{it} , l_{it} , and m_{it} denote the logarithms of gross output, capital, labor, and intermediate inputs. The production function is

$$\begin{aligned} y_{it} = & \alpha_K k_{it} + \alpha_L l_{it} + \alpha_M m_{it} + \frac{1}{2} \alpha_{KK} k_{it}^2 + \frac{1}{2} \alpha_{LL} l_{it}^2 + \frac{1}{2} \alpha_{MM} m_{it}^2 \\ & + \alpha_{KL} k_{it} l_{it} + \alpha_{KM} k_{it} m_{it} + \alpha_{LM} l_{it} m_{it} + d_i + d_t + \omega_{it} + \epsilon_{it}, \end{aligned} \quad (11)$$

where d_i and d_t are firm and time effects, ω_{it} is Hicks-neutral productivity following an AR(1) process $\omega_{it} = \rho \omega_{i,t-1} + e_{it}$, where e_{it} are random productivity shocks assumed to be uncorrelated with ϵ_{it} , and ϵ_{it} is i.i.d. measurement error. The output elasticities are firm- and time-specific functions of the coefficients and input levels,

$$\theta_{L,it} = \alpha_L + \alpha_{LL} l_{it} + \alpha_{KL} k_{it} + \alpha_{LM} m_{it}, \quad (12)$$

$$\theta_{M,it} = \alpha_M + \alpha_{MM} m_{it} + \alpha_{KM} k_{it} + \alpha_{LM} l_{it}, \quad (13)$$

and feed directly into (2)–(4).

Biased technical change. To allow technology to be non-neutral along all three input dimensions, we let inputs enter in efficiency units, $\tilde{x}_{it} = \psi_{X,st} + x_{it}$ for $X \in \{K, L, M\}$, with sector–time augmenting parameters $\psi_{K,st}$, $\psi_{L,st}$, $\psi_{M,st}$ that are common across firms within a sector but vary at a constant rate over time. As shown below, this shifts each first-order coefficient by a term $\beta_{X,st}$ that depends on *all three* augmenting parameters through the translog interactions, so that correcting for labor-augmenting change alone, as in much of the literature, leaves the elasticities biased by the neglected capital- and materials-augmenting components. We estimate (11) by system GMM (Blundell and Bond, 2000).

To see why factor-augmenting technical change contaminates every elasticity in a translog, consider the case of constant factor-augmenting technological change, $k^e = k + \lambda_K t$, $l^e = l + \lambda_L t$, $m^e = m + \lambda_M t$, where the superscript e denotes efficiency units. The translog form

$$y = a_K k^e + a_L l^e + a_M m^e + a_{KK} k^{e2} + a_{LL} l^{e2} + a_{MM} m^{e2} \\ + a_{KL} k^e l^e + a_{KM} k^e m^e + a_{LM} l^e m^e + \omega,$$

where ω is a firm-specific neutral productivity term. When the augmented inputs are substituted into the production function, the specification becomes

$$y = a_K k + a_L l + a_M m + a_{KK} k^2 + a_{LL} l^2 + a_{MM} m^2 + a_{KL} kl + a_{KM} km + a_{LM} lm \\ + (a_K \lambda_K + a_L \lambda_L + a_M \lambda_M) t \\ + (a_{KK} \lambda_K^2 + a_{LL} \lambda_L^2 + a_{MM} \lambda_M^2 + a_{KL} \lambda_K \lambda_L + a_{KM} \lambda_K \lambda_M + a_{LM} \lambda_L \lambda_M) t^2 \\ + (2a_{KK} \lambda_K + a_{KL} \lambda_L + a_{KM} \lambda_M) kt + (2a_{LL} \lambda_L + a_{KL} \lambda_K + a_{LM} \lambda_M) lt \\ + (2a_{MM} \lambda_M + a_{KM} \lambda_K + a_{LM} \lambda_L) mt + \omega.$$

In other words, even if there is only labor-augmenting technological change ($\lambda_L > 0$, $\lambda_K = \lambda_M = 0$), all output elasticities will be time-variant; that is, all inputs should be interacted with the time variable. If instead we introduce exogenous change in the elasticities directly into the production function, as

$$y = a_K k^e + a_L l^e + a_M m^e + a_{KK} k^{e2} + a_{LL} l^{e2} + a_{MM} m^{e2} + a_{KL} k^e l^e + a_{KM} k^e m^e \\ + a_{LM} l^e m^e + a_{KT} k^e t + a_{LT} l^e t + a_{MT} m^e t + \omega,$$

the model is under-identified, and the coefficients of factor-augmenting change (λ_K , λ_L , λ_M) cannot be recovered from the estimated parameters.

The estimated time-interaction coefficients confirm that non-neutrality is the rule

rather than the exception in Turkish manufacturing. The mean output elasticity of labor trends upward in 19 of 23 NACE 2-digit sectors (a median change of +0.0024 per year), and the capital elasticity trends upward in 17, while the materials elasticity trends downward in 15 sectors (median -0.0025 per year). In other words, the output elasticity of intermediate inputs has been falling over time, while the output elasticities of labor and capital have been rising, and the speed of these changes differs across sectors. If the elasticities are assumed to be constant over time, or to change at the same rate for all inputs, these changes in technology will show up in the estimated markdowns.

The functional-form ladder. We estimate three nested specifications and report all three: (i) Cobb-Douglas, (ii) translog with neutral technical change, and (iii) translog with non-neutral (triply-augmenting) technical change, our preferred specification. Reporting the full ladder is deliberate. [Raval \(2023\)](#), [Rubens et al. \(2025\)](#) and [De Ridder et al. \(2026\)](#) criticize the production approach on the grounds that markup and markdown levels are sensitive to functional form and to the treatment of non-neutral technology. We do not claim that our preferred specification is the correct one. Instead, we report the estimates from all three specifications and check whether the same-wage prediction of Section 3.5 holds under each of them. If the employment result does not depend on the choice of functional form, then it does not depend on the modeling choice that these critics question either.

4.5 The Rubens–Wu–Xu confound

The most pointed threat to a production-approach markdown is the one identified by [Rubens et al. \(2025\)](#). They show that when output elasticities are heterogeneous in a way the estimator does not capture (a departure from Hicks neutrality), the standard production-approach markdown mis-attributes variation in the true output elasticity of labor, $\theta_{L,it}$, to variation in the markdown. The bias is not random: because a firm with a high true θ_L mechanically has a high labor cost share, the estimator assigns it a *low* markdown, producing a spurious negative correlation between θ_L and the estimated markdown. In their Monte Carlo, a true markdown of 38% is estimated at 71% once technological heterogeneity is present. This is a direct threat to us: if our markdown ranking is partly a relabeling of θ_L heterogeneity, then the employment ordering across groups could reflect differences in labor intensity rather than monopsony power.

We address the concern in two ways.

Controlling for the estimated output elasticity of labor. The RWX bias arises because the estimated markdown is correlated with $\theta_{L,it}$. Therefore, our main test in Section 6 checks whether the employment response to minimum-wage increases still differs

across markdown groups when we control for the estimated output elasticity of labor. We do this in two ways: we include $\theta_{L,it}$ directly as a control variable, and we divide firms into θ_L bins and repeat the test within each bin. Suppose that the sign reversal predicted by the same-wage comparison of Section 3.5 is observed within θ_L bins. Then the employment response is predicted by the part of markdown variation that is not related to labor intensity, and this is the part that is not affected by the RWX critique. This is our main response to the critique, and it does not require any information from outside our own estimation.

External validation against labor-market concentration. The criticisms of the production approach are directly related to the objects estimated *inside* the production function. An independent corroboration that does not rely on those internals is therefore valuable. We show that, as monopsony theory predicts, our estimated markdowns are positively associated with labor-market concentration, measured by the Herfindahl–Hirschman index of employment in the local labor market (the 2-digit sector–province cell). Because concentration is measured entirely outside the production function, agreement between the two is evidence that the markdown ordering captures genuine labor market power rather than an estimation artifact.

5. Markups and Markdowns in Turkish Manufacturing

This section presents the estimated markups and markdowns and establishes that they are well-behaved before we use them, in Section 6, to sort firms by their employment responses to minimum-wage increases. We document their levels and trends, their heterogeneity across firms, their sensitivity to functional form, and their agreement with external measures of labor market power. We close with the distribution of firms across the three markdown-based groups.

5.1 Levels and trends

Figure 3 plots the mean and median log markup and markdown for the manufacturing industry over 2008–2021. Markups rose gradually over 2008–2015, from about 3.0 to 4.9 log points at the median, before jumping in 2016. The markdown pattern is, if anything, more striking: markdowns *declined* over 2008–2015, from about 0.28 to 0.15 at the median. Two implications follow. First, product market power and labor market power moved in *opposite* directions in Turkey over the pre-2016 period. [Casacuberta and Gandelman \(2023\)](#) find a similar divergence for Uruguay. This result also shows why the two margins should be estimated jointly: a method that recovers only one of them would miss the fact that they can move in opposite directions. Second, the decline in

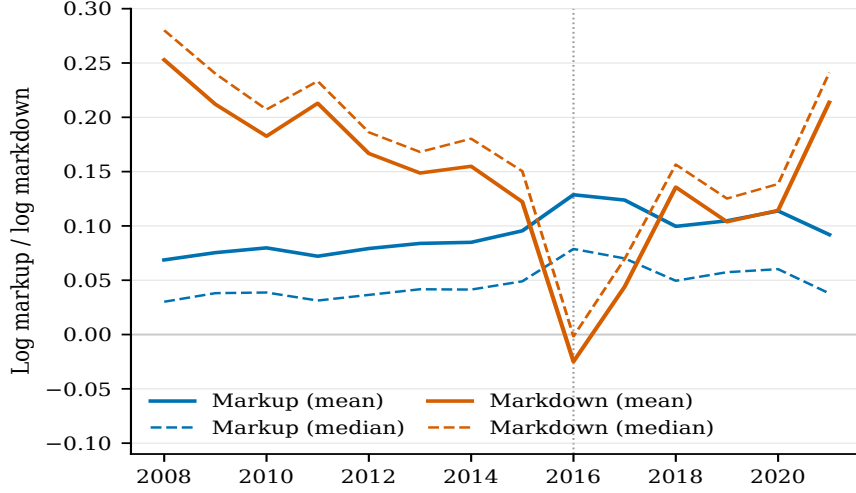


Figure 3: Markups and markdowns in Turkish manufacturing, 2008–2021. Mean and median of the firm-level log markup and log markdown. The dotted line marks 2016.

markdowns means that, on the eve of the 2016 increase, Turkish labor market power was lower than it had been, so the experiment we study is not one of rising monopsony power. It is one in which a large wage floor meets a distribution of pre-existing markdowns that is modest on average but widely dispersed.

The trend figures also record the 2016 event itself, and they do so in the direction the model requires. The median log markdown, having drifted down from roughly 0.28 in 2008 to about 0.15 on the eve of the increase, collapses to essentially zero in 2016 (-0.002), by far the largest one-year compression in the series, and then recovers toward its pre-shock level as subsequent inflation erodes the real floor. These movements are the aggregate counterpart of Cases 1 and 2. A binding floor raises the share of labor in revenue at constrained firms, and it reduces the wedge between the marginal product of labor and the wage.

The markup series moves in the opposite direction: the median log markup increases from 4.9 to 7.9 log points in 2016. This increase is consistent with firms passing part of the wage shock into output prices.⁶ Our markup series also lines up with the only independent estimates available for Turkey: it tracks the markups estimated by [Gürer and Derin-Güre \(2025\)](#) on the same EIS data, with both exhibiting the post-2016 rise concentrated among high-markup firms. Since their analysis uses a value-added specification with labor and capital while ours uses a gross-output specification with materials (required for markdown identification), the agreement on the markup margin is a useful cross-method check. The results in this section describe aggregate movements. In Section 6, we examine whether

⁶The product-market side moves differently from the labor-market side over the longer run as well: product-market concentration fell over the period while markups rose. Markups evidently respond to forces beyond concentration, such as product differentiation, quality upgrading, and the inflationary environment, which is a further reason to identify the two margins separately rather than infer either from concentration alone.

the effects of the shock across firms depend on their initial markdowns. The aggregate compression and recovery described here form the background of that firm-level analysis.

5.2 Heterogeneity across firms

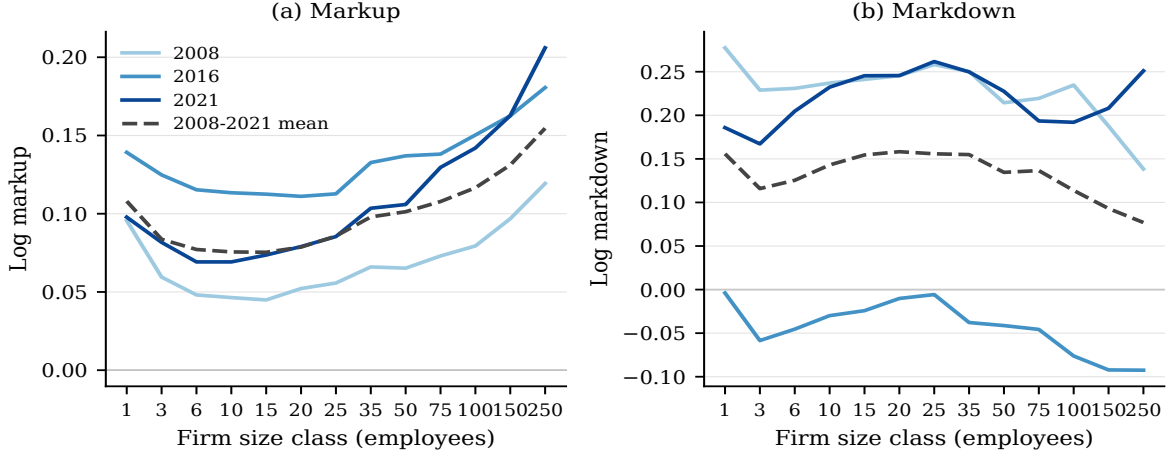


Figure 4: Markups and markdowns by firm-size class in Turkish manufacturing.

Behind these aggregate trends, there are large differences across firms. Our minimum-wage analysis is based on these differences. Figure 4 plots markups and markdowns by firm-size class in 2008, 2016, and 2021, together with the means for the 2008–2021 period. The 2008 and 2021 profiles are similar to each other, while the 2016 markdown profile in panel (b) lies far below both: the aggregate markdown collapse of Section 5.1 is visible across every size class, and it explains why the period mean sits well below the end years. The markup starts at lower levels and increases with firm size. The markdown-size relationship, on the other hand, is hump-shaped: it increases with size among small firms, peaks among mid-sized firms, and declines among the largest size classes. Figure 5 shows the sectoral distribution of markups and markdowns, confirming that both objects vary widely across two-digit sectors rather than being driven by a handful of industries. This variation matters for what follows: a sizeable share of firms sits in each of the regimes of Section 3, so the sign-reversal prediction of Section 3.5 can be tested on populated groups rather than on thin tails.

5.3 Sensitivity to functional form

In Section 4.4, we reported production function estimates obtained from Cobb-Douglas, neutral translog, and non-neutral translog functional forms. As discussed in Section 4.4, Raval (2023), Rubens et al. (2025) and De Ridder et al. (2026) criticize the production approach on the grounds that markup and markdown levels are sensitive to functional

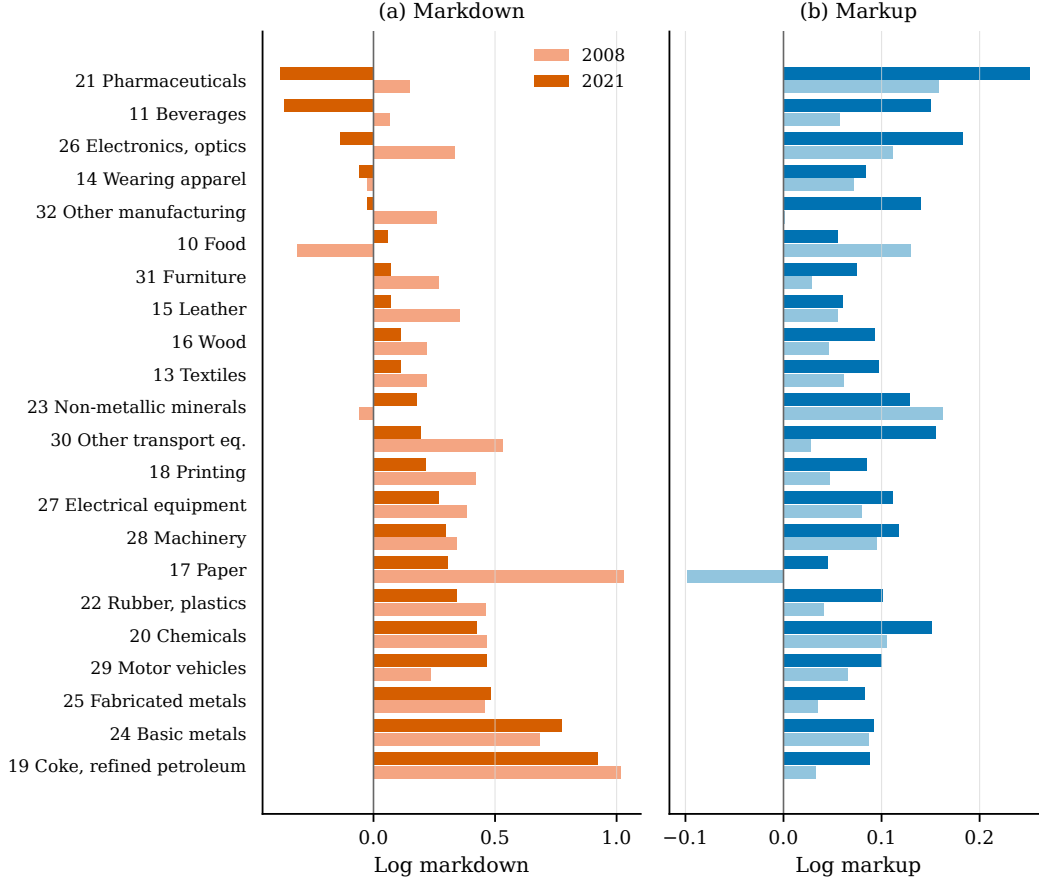


Figure 5: Markups and markdowns by manufacturing sector (NACE 2-digit), 2008 and 2021, sorted by the 2021 markdown.

form and to the treatment of non-neutral technology. Our estimates confirm this. Figures 6 and 7 plot the mean log markup and log markdown from the three specifications for 2008–2021. The three series move together over time, and all show a sharp fall in markdown in 2016. However, the Cobb-Douglas markdown is substantially higher than the other two in all years. This is because the Cobb-Douglas form does not allow output elasticities to differ across firms, so differences in technology end up in the estimated markdown.

Figure 8 compares the estimated output elasticity of labor across specifications at the sector-year level. The Cobb-Douglas estimates are, by construction, time-invariant within a sector (each sector traces a horizontal streak in panel (a)), and their correlation with the non-neutral estimates is only 0.49. The neutral translog estimates in panel (b) track the non-neutral ones far more closely (correlation 0.85), but the remaining divergence is the factor-augmenting drift documented in Section 4.4.

The functional form also matters for the employment analysis. Table C3 in the appendix re-estimates the baseline employment regressions of Section 6 using the markdown and labor elasticity implied by each specification. Under Cobb-Douglas the labor-

elasticity interactions are entirely insignificant, because the Cobb-Douglas elasticity does not vary across firms within a sector, and the Group 1 markdown interactions turn positive and significant, a pattern the theory rules out for firms with no markdown; the neutral translog shares the latter defect. Only the non-neutral translog, which allows for factor-augmenting technical change, produces results that are consistent with the theory: the markdown gradient is zero for Group 1, as the theory requires, and it remains positive for Group 2.

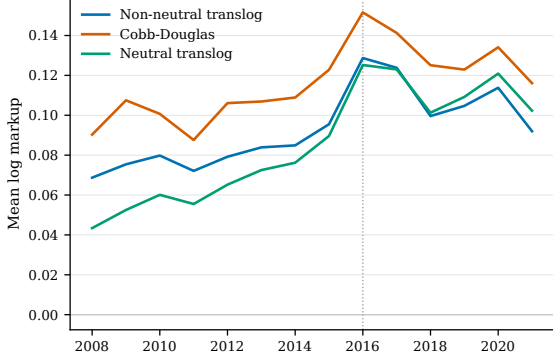


Figure 6: Mean log markup under alternative production functions, 2008–2021.

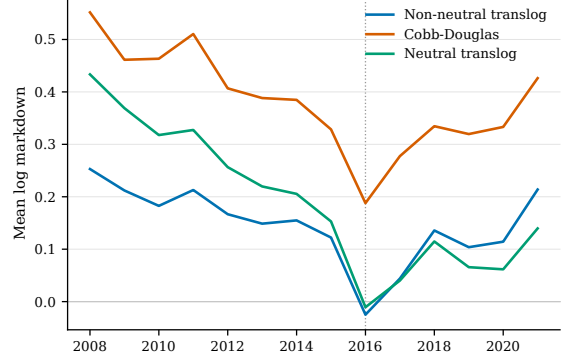


Figure 7: Mean log markdown under alternative production functions, 2008–2021.

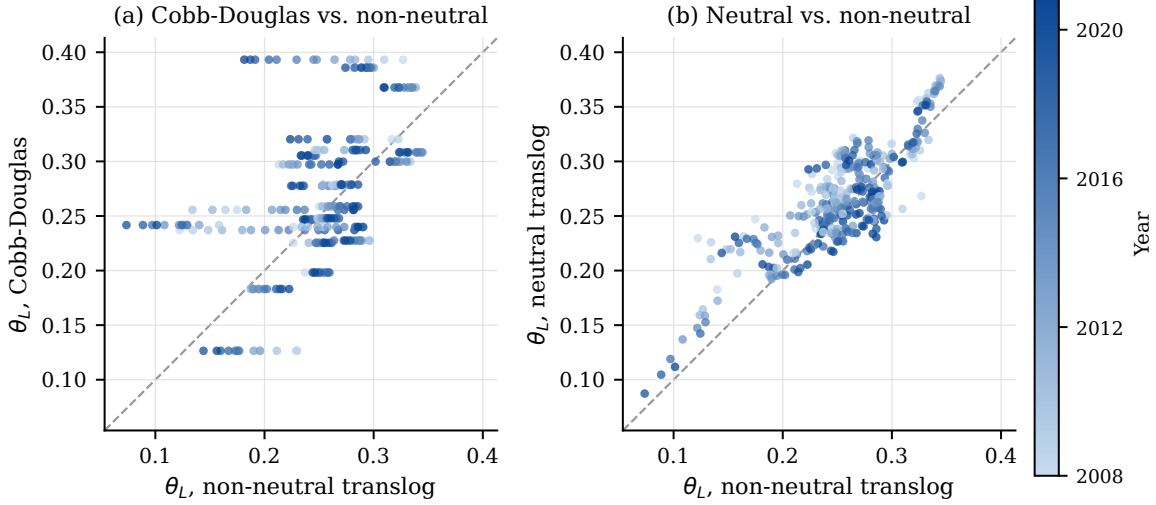


Figure 8: Output elasticity of labor under different specifications: Cobb-Douglas vs. non-neutral translog (left) and neutral vs. non-neutral translog (right). Each point is a sector–year mean, shaded by year (darker points are later years); the dashed line is the 45-degree line.

5.4 External validation

We also check whether the estimated markdowns reflect labor market power or are simply artifacts of the estimation method. Figure 9 plots the estimated markdown together

with labor-market concentration, measured by the Herfindahl–Hirschman index of employment. Concentration declined until the mid-2010s, as the entry of new firms increased the number of employers, and the markdown declined at the same time. This is what the monopsony model predicts if employer power decreases when labor markets become less concentrated. The concentration index is calculated from employment data alone and does not depend on the production function in any way. Therefore, the co-movement of the two series indicates that the markdown series measures labor market power rather than some feature of the production function estimates. This is the simplest check against the concern raised by [Rubens et al. \(2025\)](#); we return to this concern in Section 6.

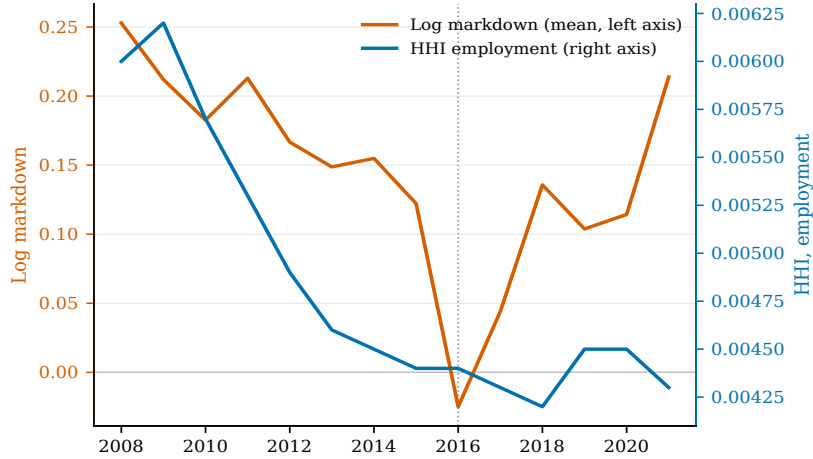


Figure 9: Markdown and labor-market concentration in Turkish manufacturing, 2008–2021. Herfindahl–Hirschman index based on employment, averages of 2-digit sector–province cells.

5.5 The three groups in the data

Table 1 reports annual descriptive statistics for the full estimation sample and for the three markdown-based groups defined in Section 4.3, over 2012–2018.

Three features of the table matter for the analysis. First, the minimum-wage groups (1 and 2) together account for a substantial share of firms and workers, so the regime predictions concern a policy-relevant subset rather than a marginal one. Second, Groups 1 and 2, the predicted losers and gainers, respectively, pay identical wages by construction (both within $\pm 1\%$ of the minimum; their mean fourth-quarter log wages coincide to three decimals in every year), so a competitive model that conditions on the wage would predict the *same* employment response for both; any divergence between them is therefore informative about monopsony rather than about wage levels. The two groups are also similar in average size (log firm size around 1.2–1.7 in both), whereas Group 3 firms are markedly larger (above 2 throughout). Third, the groups differ on observables such as size and labor intensity: Group 1 firms have markedly higher output elasticities of labor ($\theta_L \approx 0.36$ – 0.40) than Group 2 firms ($\theta_L \approx 0.23$), which is why Section 6 conditions on

Table 1: Descriptive statistics by firm group, manufacturing, 2012–2018.

Year	Firms	Empl. (000)	$\Delta \ln w$	$\Delta \ln l$	$\ln w$	$\ln l$	$\log \eta$	$\log \mu$	θ_L	Firm size	Rel. TFP
<i>Panel A: All firms</i>											
2012	70,432	2,303.5	0.108	-0.019	3.579	5.719	0.164	0.080	0.254	2.323	0.009
2013	74,327	2,389.4	0.117	-0.026	3.680	5.698	0.143	0.085	0.255	2.299	0.010
2014	77,605	2,517.1	0.122	-0.026	3.793	5.676	0.150	0.086	0.257	2.288	0.009
2015	80,668	2,687.1	0.234	-0.089	3.911	5.659	0.115	0.097	0.262	2.278	0.009
2016	83,782	2,698.3	0.095	-0.011	4.140	5.570	-0.034	0.131	0.264	2.196	0.008
2017	86,527	2,792.0	0.145	-0.091	4.232	5.570	0.038	0.124	0.261	2.168	0.012
2018	86,867	2,817.5	0.222	-0.106	4.374	5.505	0.128	0.102	0.267	2.140	0.012
<i>Panel B: Group 1 (all workers at the minimum wage, $\log \eta \leq 0$)</i>											
2012	9,869	77.9	0.099	-0.047	3.445	5.041	-0.385	0.199	0.363	1.654	-0.019
2013	8,988	65.7	0.116	-0.043	3.528	4.976	-0.389	0.205	0.374	1.589	-0.018
2014	9,241	64.7	0.125	-0.059	3.633	4.928	-0.392	0.205	0.383	1.548	-0.024
2015	10,050	68.9	0.266	-0.111	3.748	4.900	-0.393	0.216	0.389	1.528	-0.013
2016	17,770	116.9	0.088	-0.021	4.006	4.846	-0.412	0.215	0.357	1.488	0.009
2017	14,536	94.1	0.144	-0.105	4.082	4.836	-0.413	0.226	0.374	1.443	0.000
2018	12,062	74.0	0.239	-0.099	4.215	4.758	-0.408	0.209	0.402	1.397	-0.023
<i>Panel C: Group 2 (all workers at the minimum wage, $\log \eta \geq 0.25$)</i>											
2012	12,476	98.2	0.108	0.027	3.445	4.997	0.610	-0.018	0.243	1.611	0.017
2013	11,550	82.0	0.124	0.020	3.528	4.916	0.608	-0.014	0.242	1.530	0.019
2014	12,186	81.5	0.132	0.008	3.633	4.855	0.607	-0.018	0.242	1.478	0.014
2015	11,979	75.5	0.271	-0.049	3.748	4.803	0.594	-0.009	0.239	1.436	0.019
2016	9,488	53.1	0.094	0.047	4.006	4.665	0.567	0.008	0.231	1.300	0.022
2017	11,021	59.5	0.151	-0.052	4.082	4.670	0.585	0.007	0.230	1.276	0.012
2018	13,130	66.9	0.245	-0.054	4.215	4.591	0.606	-0.005	0.234	1.242	0.008
<i>Panel D: Group 3 (all workers paid $\geq 25\%$ above the minimum wage, $\log \eta > 0$)</i>											
2012	577	32.7	0.070	0.053	4.249	5.911	0.471	0.103	0.226	2.628	0.038
2013	676	37.5	0.052	0.110	4.323	5.599	0.454	0.115	0.232	2.353	0.076
2014	807	41.5	0.059	0.189	4.420	5.659	0.453	0.119	0.228	2.379	0.085
2015	713	45.1	0.101	0.089	4.510	5.588	0.395	0.128	0.243	2.343	0.069
2016	409	28.6	0.066	0.100	4.726	5.362	0.411	0.122	0.245	2.150	0.050
2017	652	39.0	0.078	0.089	4.808	5.361	0.419	0.150	0.233	2.088	0.072
2018	867	56.9	0.112	-0.017	4.962	5.589	0.425	0.164	0.237	2.342	0.096

Note: Annual means for the estimation sample of manufacturing firms. Employment is total insured employment in thousands of workers. $\Delta \ln w$ and $\Delta \ln l$ are forward log changes from the fourth quarter of year t to the fourth quarter of year $t+1$ in the average daily wage and in employment; the 2015 row therefore spans the January 2016 minimum-wage increase. Note that the year convention of this table differs from that of the regression tables: here the year is the calendar year in which the levels are observed, so the growth columns in row t correspond to row $t+1$ of Tables 2–3. $\ln w$ and $\ln l$ are the fourth-quarter log average daily wage and log employment. $\log \eta$ is the log markdown, $\log \mu$ the log markup, θ_L the output elasticity of labor from the non-neutral translog production function, firm size the log of average firm employment, and relative TFP the firm’s log TFP deviation from its sector median. Groups are defined annually from the December wage distribution: Groups 1 and 2 pay all their workers within $\pm 1\%$ of the statutory minimum wage and are split by the beginning-of-window log markdown ($\log \eta \leq 0$ vs. $\log \eta \geq 0.25$); Group 3 firms pay all workers at least 25% above the minimum and have positive markdowns.

the estimated output elasticity of labor, addressing the [Rubens et al. \(2025\)](#) concern, when estimating the employment response.

6. Minimum-Wage Increases and Employment

This section tests the central prediction of the paper. The same-wage prediction of Section 3.5 holds that a firm’s initial markdown is a sufficient statistic for the sign of its employment response to a minimum-wage increase: employment falls for firms with no markdown (Group 1), rises for firms whose markdown exceeds the increase (Group 2), and is unchanged for firms paying above the floor (Group 3). We bring this prediction to the minimum-wage increases of 2013–2019, with the exceptionally large January 2016 increase as the sharpest experiment.

6.1 The minimum-wage increases and the identifying variation

In January 2016 the Turkish government raised the statutory minimum wage by 33% in nominal terms (29 log points relative to 2015Q4). With consumer prices rising 7.5 log points over the year, the real increase was approximately 21 log points, or 25%. The shock was large, unanticipated, and, unlike the increases of 2013–2015, which were implemented in two steps each January and July, concentrated on a single date (Section 4.1 and Figure 2 place it in its macroeconomic context). It is the sharpest experiment in our window, but it is not the only source of identification.

Suppose the minimum wage rises by 10% in a year in which prices also rise by 10%. On average, there is no real shock and no predicted response. At the level of the individual firm, however, prices, demand, and productivity move very differently from the aggregate. As a result, the real effect of the same statutory increase varies across firms, and the markdown determines how this effect translates into employment. Therefore, firm-level variation allows us to identify the effect of the markdown in every year. The 2016 increase is still the cleanest single event, because its real size was too large to be offset by firm-level changes in prices, demand, or productivity. For this reason, we estimate the effects separately for each year over the 2013–2019 period instead of comparing a single pre-shock period with a single post-shock period. The last change we use is the one from December 2018 to December 2019. We do not use later years, because the employment regulations introduced during the Covid period in 2020 make it impossible to isolate the effect of the minimum wage in those years.

6.2 Empirical strategy

As we use firm-level data over time, the unit of observation is a firm-year. The outcome variable is the change in the firm’s log December employment, $\Delta\ell_{it} \equiv \ell_{it} - \ell_{i,t-1}$, measured

from the social-security records as the log of total insured worker-days in December. The change runs from December of year $t-1$ to December of year t , so the observation dated 2016 records growth across the January 2016 increase, the window of the largest shock. The estimation sample is the panel of firms observed over 2012–2019 with an output elasticity of labor inside the unit interval, 169,047 firm–years.

We estimate, by group $g \in \{1, 2, 3\}$,

$$\Delta \ell_{it} = \sum_g \mathbf{1}\{G_{it} = g\} \left[\varphi_{gt} (T_t \times \log \eta_{it}) + \psi_{gt} (T_t \times \theta_{L,it}) \right] + \alpha_{st} + \delta_{pt} + \varepsilon_{it}, \quad (14)$$

where $\Delta \ell_{it}$ is the change in log employment from the fourth quarter of $t-1$ to the fourth quarter of t , T_t are time dummies for $t = 2013, \dots, 2019$, $\log \eta_{it}$ is the firm’s log markdown (winsorized within sector), $\theta_{L,it}$ is the output elasticity of labor from the non-neutral translog estimates, and α_{st} and δ_{pt} are sector-year and province-year fixed effects that account for sectoral and regional shocks. Standard errors are clustered two-way by sector and province. Group membership is assigned annually from the wage distribution and the markdown in December of year $t-1$, the start of the growth window. An identical design with the log fourth-quarter daily wage in place of employment provides the first stage. The same models estimated on the balanced panel (65,337 firm–years) appear in the robustness tables and the online appendix.

Equation (14) is richer than a group-dummy difference-in-differences specification, for two reasons that follow from the theory. First, the model’s sufficient statistic is continuous, so that, within the set of firms paying the minimum wage, the sign and size of the direct employment response are governed by the log markdown, not by group membership per se. The groups implement the same-wage conditioning, and the markdown provides the identifying variation. The year-specific within-group gradients φ_{gt} are therefore the objects of interest. Second, interacting $\theta_{L,it}$ with each group serves two purposes. In the demand-constrained regime the response is $\Delta l = -\Delta w / (1 - \theta_L)$, so θ_L is itself a predicted determinant of the response for Group 1; and conditioning on θ_L within group ensures that the markdown gradients are identified from markdown variation *orthogonal* to labor intensity, which is the variation that survives the [Rubens et al. \(2025\)](#) critique, to which we return in Section 6.4.

The theory’s predictions map onto (14) as follows. Within Group 1, whose markdown is exhausted, the markdown should have no protective role: $\varphi_{1t} \approx 0$ in every year. Within Group 2, whose markdown exceeds the size of even the largest hike, a larger markdown means more room to absorb the floor along the labor-supply curve: $\varphi_{2t} > 0$, and the gradient should be strongest in the window of the January 2016 increase. For Group 3 firms that pay wages well above the floor, there is no direct effect. Yet, the indirect channels that we discussed in Section 3.6 allow an attenuated positive gradient. The premise of the same-wage comparison, that Groups 1 and 2 receive the same mechanical

cost shock, is testable in the wage first stage: statutory wage growth should be identical across the two groups and much smaller for Group 3.

The sector-year and province-year fixed effects absorb aggregate, sectoral, and regional shocks, including the macroeconomic environment of each minimum-wage adjustment. The markdown gradients φ_{gt} are therefore identified from cross-firm variation in the markdown *within* sector-province-year cells, so they cannot be driven by any common shock.

6.3 Main results

First stage: an identical bite in every year. The wage regressions (Table 2, columns (1)–(2)) verify the premise of the design. Fourth-quarter to fourth-quarter wage growth at firms paying the minimum is essentially identical for the two minimum-wage groups in every year of the sample: in the window spanning the January 2016 increase (the 2016 row), 26.6 log points in Group 1 and 27.1 in Group 2, close to the statutory December-to-December log increase of 25.7 log points, and within a log point of each other in every other year.⁷ Conditional on sector-year and province-year effects (column (2)), the Group 2 premium over Group 1 never exceeds 0.9 log points. Firms paying the minimum in December of year $t-1$ were paying the new minimum in December of t . By contrast, Group 3 wages grew far more slowly in the hike windows (10.1 log points in the 2016 window, 17.4 below Group 1 conditionally): the floor barely reached them. Therefore, Groups 1 and 2 receive the same statutory cost shock year after year, and the difference in their employment responses cannot be explained by the size of the wage shock.

Employment growth. Columns (3)–(4) of Table 2 report the corresponding employment growth. Group 1 firms, at the minimum with no markdown, shrink in every year, by 2 to 11 log points, with the largest contraction (-11.1 log points) exactly in the window of the 2016 hike. Group 2 firms, which pay the same wage, grow faster than Group 1 in every year: conditional on sector-year and province-year effects, the Group 2–Group 1 difference is 6 to 9 log points annually, including 6.4 log points in the 2016-hike window and 8.8 in the following year. Group 3 firms grow fastest of all, as their higher wages and positive markdowns would suggest. The competitive model, in which the wage summarizes the cost shock, predicts identical responses for Groups 1 and 2. The data instead show a stable and sizeable gap in the direction predicted by the monopsony model.

The markdown gradient. Table 3 contains the paper’s central estimates: the year-by-year, within-group association between the markdown and subsequent employment

⁷Throughout, growth rates are log changes multiplied by 100 (log points). The statutory increase of 33 percent corresponds to $100 \times \ln(1.33) = 28.5$ log points on an annual-average basis and 25.7 log points December-to-December.

Table 2: Wage and employment growth by firm group and year, manufacturing, 2013–2019.

	(1) wm1 $\Delta \ln w$	(2) wm2 $\Delta \ln w$	(3) lm1 $\Delta \ln l$	(4) lm2 $\Delta \ln l$
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>				
2013	0.0989** (0.0007)		-0.0466** (0.0055)	
2014	0.1162** (0.0008)		-0.0426** (0.0062)	
2015	0.1253** (0.0006)		-0.0585** (0.0057)	
2016	0.2656** (0.0006)		-0.1105** (0.0080)	
2017	0.0878** (0.0005)		-0.0212** (0.0053)	
2018	0.1442** (0.0006)		-0.1047** (0.0048)	
2019	0.2388** (0.0005)		-0.0991** (0.0075)	
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>				
2013	0.1084** (0.0010)	0.0085** (0.0012)	0.0271** (0.0060)	0.0682** (0.0093)
2014	0.1242** (0.0009)	0.0066** (0.0010)	0.0198** (0.0054)	0.0691** (0.0088)
2015	0.1324** (0.0008)	0.0064** (0.0009)	0.0078 (0.0060)	0.0722** (0.0091)
2016	0.2710** (0.0007)	0.0042** (0.0008)	-0.0488** (0.0057)	0.0640** (0.0084)
2017	0.0936** (0.0008)	0.0039** (0.0010)	0.0474** (0.0075)	0.0880** (0.0090)
2018	0.1510** (0.0008)	0.0050** (0.0009)	-0.0521** (0.0061)	0.0617** (0.0076)
2019	0.2446** (0.0007)	0.0045** (0.0007)	-0.0536** (0.0066)	0.0624** (0.0090)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>				
2013	0.0704** (0.0090)	-0.0364** (0.0090)	0.0530* (0.0208)	0.0939** (0.0233)
2014	0.0518** (0.0086)	-0.0744** (0.0085)	0.1104** (0.0230)	0.1565** (0.0260)
2015	0.0591** (0.0079)	-0.0733** (0.0083)	0.1894** (0.0209)	0.2426** (0.0233)
2016	0.1005** (0.0075)	-0.1735** (0.0079)	0.0886** (0.0250)	0.2021** (0.0273)
2017	0.0661** (0.0119)	-0.0292* (0.0120)	0.0998** (0.0231)	0.1492** (0.0259)
2018	0.0777** (0.0090)	-0.0765** (0.0091)	0.0888** (0.0201)	0.1970** (0.0234)
2019	0.1123** (0.0084)	-0.1351** (0.0088)	-0.0174 (0.0199)	0.1053** (0.0232)
Sector $\times$ year FE	No	Yes	No	Yes
Province $\times$ year FE	No	Yes	No	Yes
Observations	169,047	169,028	169,047	169,028
R ²	0.43895	0.44754	0.00813	0.01902
Within R ²	—	0.05128	—	0.00426

Note: Dependent variable: log wage growth (columns (1)–(2)) and log employment growth (columns (3)–(4)), fourth quarter of $t-1$ to fourth quarter of t . Columns (1) and (3) have no fixed effects and report year-by-group means; columns (2) and (4) add sector $\times$ year and province $\times$ year fixed effects, so Group 1 interactions are the omitted reference and coefficients measure differences from Group 1. Groups (defined from the wage distribution and markdown in December of year $t-1$, the start of the growth window): Group 1 pays all workers within $\pm 1\%$ of the statutory minimum wage and has $\log \eta \leq 0$; Group 2 pays all workers within $\pm 1\%$ of the minimum and has $\log \eta \geq 0.25$; Group 3 pays all workers at least 25% above the minimum and has $\log \eta > 0$. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 3: Employment growth: markdown and labor-elasticity gradients by year, manufacturing, 2013–2019.

	(1) 1em1	(2) 1em2	(3) 1em3	(4) 1em4	(5) 1em5
<i>Panel A: year $\times$ group $\times$ log markdown ($\log \eta$)</i>					
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>					
2013	-0.0257 ⁺ (0.0136)	-0.0118 (0.0142)	-0.0153 (0.0144)	-0.0034 (0.0163)	0.0218 (0.0139)
2014	-0.0147 (0.0138)	-0.0179 (0.0156)	-0.0166 (0.0151)	-0.0283 (0.0178)	0.0125 (0.0151)
2015	-0.0133 (0.0139)	-0.0148 (0.0154)	-0.0161 (0.0149)	-0.0377* (0.0172)	0.0155 (0.0141)
2016	0.0197 (0.0132)	-0.0090 (0.0143)	-0.0090 (0.0143)	-0.0111 (0.0178)	0.0209 (0.0134)
2017	-0.0360** (0.0118)	-0.0168 (0.0115)	-0.0152 (0.0114)	-0.0169 (0.0129)	0.0068 (0.0115)
2018	-0.0123 (0.0118)	-0.0509** (0.0123)	-0.0526** (0.0119)	-0.0599** (0.0146)	-0.0231* (0.0117)
2019	-0.0148 (0.0163)	-0.0499** (0.0168)	-0.0489** (0.0167)	-0.0396* (0.0161)	-0.0239 (0.0163)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>					
2013	0.1189** (0.0147)	0.1032** (0.0185)	0.1024** (0.0161)	0.0954** (0.0223)	0.0802** (0.0172)
2014	0.1242** (0.0135)	0.1340** (0.0165)	0.1196** (0.0141)	0.1624** (0.0222)	0.0995** (0.0158)
2015	0.1128** (0.0140)	0.1239** (0.0171)	0.1178** (0.0146)	0.1532** (0.0215)	0.0843** (0.0154)
2016	0.1156** (0.0176)	0.1536** (0.0193)	0.1536** (0.0193)	0.1597** (0.0268)	0.1125** (0.0187)
2017	0.1696** (0.0166)	0.1728** (0.0173)	0.1495** (0.0142)	0.1779** (0.0232)	0.1290** (0.0162)
2018	0.0762** (0.0164)	0.1301** (0.0180)	0.1314** (0.0150)	0.1407** (0.0233)	0.0927** (0.0176)
2019	0.0528** (0.0184)	0.1067** (0.0185)	0.0995** (0.0156)	0.1027** (0.0228)	0.0737** (0.0175)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>					
2013	0.0437 (0.0476)	0.0483 (0.0480)	0.0467 (0.0342)	0.1137 (0.0796)	0.1083* (0.0517)
2014	0.1407 ⁺ (0.0764)	0.1601* (0.0813)	0.1440** (0.0488)	0.2773 (0.2188)	0.1745* (0.0739)
2015	0.1893** (0.0562)	0.2099** (0.0578)	0.1478** (0.0353)	0.3582** (0.1015)	0.2225** (0.0498)
2016	0.1386 ⁺ (0.0713)	0.1745* (0.0706)	0.1745* (0.0706)	0.1628 (0.1966)	0.1538* (0.0695)
2017	0.0723 (0.0643)	0.0899 (0.0669)	0.0799 ⁺ (0.0470)	0.2518 (0.1696)	0.0005 (0.0831)
2018	0.1840** (0.0518)	0.2370** (0.0552)	0.2295** (0.0395)	0.2698* (0.1339)	0.1572* (0.0735)
2019	0.2296** (0.0611)	0.2878** (0.0595)	0.1968** (0.0342)	0.4499* (0.1759)	0.2936** (0.0559)
<i>Panel B: year $\times$ group $\times$ output elasticity of labor (θ_L)</i>					
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>					
2013	-0.1825** (0.0168)	-0.2719** (0.0271)	-0.2634** (0.0263)	-0.2957** (0.0344)	-0.2917** (0.0286)
2014	-0.1361** (0.0171)	-0.1634** (0.0271)	-0.1660** (0.0261)	-0.1537** (0.0305)	-0.1864** (0.0263)
2015	-0.1749** (0.0172)	-0.1924** (0.0244)	-0.1835** (0.0232)	-0.1868** (0.0291)	-0.2034** (0.0257)
2016	-0.2491** (0.0219)	-0.1959** (0.0283)	-0.1959** (0.0283)	-0.1961** (0.0290)	-0.1907** (0.0257)
2017	-0.1254** (0.0149)	-0.2506** (0.0210)	-0.2505** (0.0205)	-0.2382** (0.0239)	-0.2357** (0.0211)
2018	-0.2703** (0.0143)	-0.2117** (0.0228)	-0.2007** (0.0227)	-0.2148** (0.0259)	-0.2034** (0.0210)
2019	-0.2603** (0.0190)	-0.2714** (0.0262)	-0.2677** (0.0253)	-0.3076** (0.0301)	-0.2575** (0.0283)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>					
2013	-0.2048** (0.0327)	-0.3498** (0.0387)	-0.3236** (0.0340)	-0.3426** (0.0667)	-0.3814** (0.0369)
2014	-0.2293** (0.0321)	-0.2477** (0.0426)	-0.2202** (0.0346)	-0.3249** (0.0647)	-0.2649** (0.0412)
2015	-0.2403** (0.0343)	-0.2489** (0.0418)	-0.2337** (0.0353)	-0.2357** (0.0590)	-0.2543** (0.0411)
2016	-0.4634** (0.0367)	-0.3734** (0.0434)	-0.3734** (0.0434)	-0.3945** (0.0787)	-0.3578** (0.0410)
2017	-0.2453** (0.0488)	-0.3988** (0.0540)	-0.3264** (0.0374)	-0.4288** (0.0827)	-0.3786** (0.0527)
2018	-0.3885** (0.0456)	-0.2640** (0.0513)	-0.2624** (0.0367)	-0.2385** (0.0784)	-0.2633** (0.0486)
2019	-0.3499** (0.0418)	-0.3390** (0.0529)	-0.3147** (0.0422)	-0.3756** (0.0707)	-0.3435** (0.0521)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>					
2013	0.1565 (0.1303)	0.0012 (0.1360)	-0.0208 (0.0829)	0.0343 (0.4448)	0.2974* (0.1371)
2014	0.1994 (0.1288)	0.1946 (0.1344)	-0.0444 (0.0888)	-0.0146 (0.7000)	0.4479** (0.1251)
2015	0.4281** (0.1025)	0.4001** (0.1085)	0.1658* (0.0660)	0.5210 (0.3913)	0.7172** (0.1165)
2016	0.0561 (0.1220)	0.2152 (0.1320)	0.2152 (0.1319)	0.2195 (0.5694)	0.5903** (0.1314)
2017	0.2538* (0.1237)	0.1467 (0.1298)	0.0557 (0.0856)	-0.5930 (0.5397)	0.5485** (0.1554)
2018	0.0098 (0.1107)	0.1728 (0.1182)	-0.0428 (0.0770)	0.1715 (0.4747)	0.6178** (0.1409)
2019	-0.4835** (0.1007)	-0.3924** (0.1100)	-0.3790** (0.0686)	-0.6725 (0.6417)	0.0430 (0.1098)
<i>Panel C: lagged employment</i>					
$\ln l_{t-1}$					-0.1574** (0.0046)
Sector $\times$ year FE	No	Yes	Yes	Yes	Yes
Province $\times$ year FE	No	Yes	Yes	Yes	Yes
Observations	169,047	169,028	200,632	108,282	169,028
(R ² ; Within R ²)	(0.0138; -)	(0.0261; 0.0114)	(0.0240; 0.0100)	(0.0322; 0.0142)	(0.0883; 0.0746)

Note: Dependent variable: log employment growth. All columns except (1) include sector $\times$ year and province $\times$ year fixed effects. Columns (1), (2), and (5) use the baseline classification `fcattc` (thresholds fixed over time); column (3) uses the time-varying classification `fcatt` (markdown threshold 0.10 and wage threshold 10%, except 0.25 and 25% in the 2016 row); column (4) uses the stricter classification `fcate` ($\log \eta < -0.1$, $\log \eta > 0.5$, wages $\geq 50\%$). Column (5) adds lagged log employment. Empty cells: interaction not included in that specification. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

growth from equation (14). In the baseline specification (column (2)), the Group 2 gradient φ_{2t} is positive and significant in *every* year, ranging from 0.103 (s.e. 0.019) in 2013 to 0.173 (s.e. 0.017) in 2017, and it is largest in 2016 and 2017, the two windows spanning and immediately following the January 2016 increase (0.154 and 0.173). Within Group 1, firms at the same wage but with no markdown, the gradient is a precisely estimated zero through 2017 (between -0.009 and -0.018) and turns mildly negative only in 2018–2019 (-0.051 and -0.050). Firms in these two groups pay the same wage; they differ in the markdown, and the markdown predicts which firms subsequently grow. The magnitudes are economically large: evaluated at Group 2’s mean markdown of about 0.6, the 2016-window gradient implies roughly 9 log points of extra employment growth relative to a same-wage firm at the $\log \eta = 0$ boundary, and across the observed within-group markdown range (from the 0.25 threshold to values near one) the implied growth differences exceed 10 log points.

The timing of the gradient. If the Group 2 employment gains are a response to the minimum wage, the gradient should be larger in the years in which the real increase in the minimum wage is larger. Figure 10 plots the Group 2 gradient estimates from column (2) of Table 3 together with the real minimum-wage increase over the same window. The two series move together. The gradient is largest in 2016 and 2017, the window of the January 2016 increase and the window that follows it, and it is smaller in the years in which inflation eroded most of the nominal adjustment. The fact that the response extends into 2017 indicates that employment adjusts gradually in the two years after a large shock, as the adjustment costs of Section 3.6 imply. This timing supports our interpretation: the faster employment growth of high-markdown firms is a response to the minimum-wage increases, and it is strongest when the real increase is largest.

Group 3 and the markdown. Among firms paying well above the floor the gradient is positive but smaller relative to its precision, significant in most but not all years (0.048 to 0.288). This is the pattern anticipated in Section 4.3: the floor does not bind for these firms, but firm-level movements in prices, productivity, and demand, together with worker categories whose supply elasticities differ from the firm-level average, allow the markdown to retain some predictive power, attenuated relative to Group 2.

The output-elasticity channel. The θ_L interactions play their intended conditioning role. Conditional on group and markdown, higher- θ_L firms have systematically lower employment growth in all years and in both minimum-wage groups (Panel B of Table 3), the pattern expected if labor-intensive firms bear more of any labor-cost shock. The group composition already reveals why conditioning matters: mean θ_L is 0.36 to 0.40 in Group 1 against 0.23 in Group 2 (Table 1), the negative markdown–labor-intensity

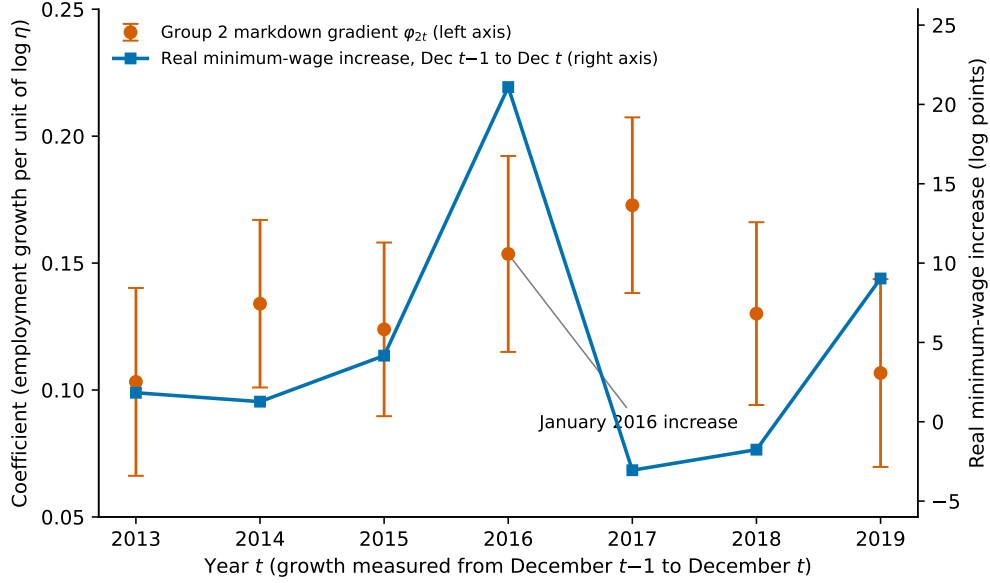


Figure 10: The Group 2 markdown gradient and the real minimum-wage increase, 2013–2019. The circles are the Group 2 markdown gradient estimates ϕ_{2t} from column (2) of Table 3, with ± 2 standard-error bands (left axis). The solid line is the real increase in the minimum wage from December $t-1$ to December t , in log points, calculated as the log change in the statutory minimum wage minus CPI inflation (right axis). Year t growth windows run from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 window spans the January 2016 increase.

correlation that [Rubens et al. \(2025\)](#) warn about. The markdown gradients above are therefore estimated holding θ_L fixed within group and year; we do not interpret the θ_L coefficients structurally.

Mean reversion does not explain the gradient. Column (5) of Table 3 adds lagged log employment, which enters at -0.157 (s.e. 0.005): employment growth at these small firms is strongly mean-reverting. The Group 2 gradients survive essentially intact, 0.074 to 0.129 across years, with the maximum still in 2017, so the markdown is not merely proxying for firms below their steady-state size. The alternative group definitions behave the same way: with time-varying thresholds (column (3)) and with the stricter thresholds (column (4)), the Group 2 gradients remain positive and significant in every year while the Group 1 gradients remain null or negative.

Robustness. The pattern is robust to a number of alternative specifications. Entering the markdown and θ_L interactions separately leaves the Group 2 gradients essentially unchanged (Table 4). Adding year-group interactions with relative TFP, capital intensity, the number of plants, firm size, or the markup one at a time attenuates but never overturns them (Tables 5–7), and the balanced-panel estimates (Table 5, column (2)) reproduce the baseline. The gradient is concentrated in low-capital-intensity sectors and among firms with fewer than ten workers, where most minimum-wage firms are located

(Tables 8–9). The intensive-margin results also have an extensive-margin counterpart: among Group 1 firms a larger markdown significantly *reduces* the probability of exit in every year, while among Group 2 firms it slightly raises it (Tables C1 and C2 in the appendix), so survivorship works against rather than for the employment gradient. Finally, Table C3 in the appendix re-estimates the baseline under the alternative production functions: as discussed in Section 5.3, the Group 2 gradient is robust across functional forms, while only the non-neutral translog delivers the theoretically required null gradient for Group 1.

6.4 Robustness: the Rubens–Wu–Xu confound

The chief threat to the interpretation of the markdown gradients is the one set out in Section 4.5: Rubens et al. (2025) show that a production-approach markdown can absorb unobserved heterogeneity in the output elasticity of labor, θ_L , producing a spurious negative relationship between θ_L and the estimated markdown. The concern is not hypothetical in our data: mean θ_L is markedly higher in Group 1 (roughly 0.36–0.40) than in Group 2 (about 0.23; Table 1), so an unconditional Group 1–Group 2 contrast could in part relabel labor intensity as labor market power.

The specification in (14) addresses this by construction: every column conditions on $\theta_{L,it}$ interacted with group and year, so the markdown gradients φ_{gt} are identified from markdown variation orthogonal to measured labor intensity. The Group 2 gradients φ_{2t} , the paper’s central result, are therefore already net of the RWX channel as far as observed θ_L carries it. We report two further checks.

The first is a stratified version of the test: Figure 11 re-estimates the Group 1–Group 2 contrast within quintiles of θ_L . The contrast is stable across quintiles, between 5.9 and 8.0 log points, with overlapping confidence intervals throughout. This result shows that the employment response is predicted by markdown variation *orthogonal* to labor intensity, which is the variation that survives the RWX critique. The second check is external: the markdown ordering is corroborated by its positive association with labor-market concentration (Section 5.4), which is constructed entirely outside the production function.

6.5 Robustness: functional form and adjustment frictions

Functional form. Section 5.3 showed that markdown *levels* move across the Cobb–Douglas, neutral-translog, and non-neutral-translog specifications. Table C3 in the appendix re-estimates (14) using the markdown, the labor elasticity, and the group assignments implied by each specification. The Group 2 gradient is positive and significant in every year under all three, so the paper’s central result does not hinge on the modeling choice that the production-approach critiques target. The functional form does

Table 4: Employment growth: separate markdown & labor-elasticity interactions.

	(1) lem2	(2) lem2m	(3) lem2l
<i>Panel A: year $\times$ group $\times$ log markdown / Group 1 (minimum wage, $\log \eta \leq 0$)</i>			
2013	-0.0118 (0.0142)	-0.0115 (0.0145)	
2014	-0.0179 (0.0156)	-0.0181 (0.0145)	
2015	-0.0148 (0.0154)	-0.0046 (0.0160)	
2016	-0.0090 (0.0143)	-0.0122 (0.0142)	
2017	-0.0168 (0.0115)	0.0005 (0.0114)	
2018	-0.0509** (0.0123)	-0.0348** (0.0122)	
2019	-0.0499** (0.0168)	-0.0394** (0.0142)	
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>			
2013	0.1032** (0.0185)	0.1164** (0.0150)	
2014	0.1340** (0.0165)	0.1320** (0.0134)	
2015	0.1239** (0.0171)	0.1252** (0.0136)	
2016	0.1536** (0.0193)	0.1358** (0.0149)	
2017	0.1728** (0.0173)	0.1537** (0.0126)	
2018	0.1301** (0.0180)	0.1351** (0.0147)	
2019	0.1067** (0.0185)	0.1304** (0.0144)	
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>			
2013	0.0483 (0.0480)	0.1522** (0.0391)	
2014	0.1601* (0.0813)	0.2791** (0.0668)	
2015	0.2099** (0.0578)	0.3891** (0.0494)	
2016	0.1745* (0.0706)	0.3379** (0.0566)	
2017	0.0899 (0.0669)	0.2246** (0.0573)	
2018	0.2370** (0.0552)	0.3635** (0.0461)	
2019	0.2878** (0.0595)	0.2930** (0.0478)	
<i>Panel B: year $\times$ group $\times$ θ_L / Group 1 (minimum wage, $\log \eta \leq 0$)</i>			
2013	-0.2719** (0.0271)		-0.3243** (0.0248)
2014	-0.1634** (0.0271)		-0.2348** (0.0246)
2015	-0.1924** (0.0244)		-0.2587** (0.0231)
2016	-0.1959** (0.0283)		-0.2750** (0.0244)
2017	-0.2506** (0.0210)		-0.2899** (0.0213)
2018	-0.2117** (0.0228)		-0.2407** (0.0217)
2019	-0.2714** (0.0262)		-0.3095** (0.0245)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>			
2013	-0.3498** (0.0387)		-0.2580** (0.0396)
2014	-0.2477** (0.0426)		-0.1417** (0.0402)
2015	-0.2489** (0.0418)		-0.1488** (0.0380)
2016	-0.3734** (0.0434)		-0.2339** (0.0413)
2017	-0.3988** (0.0540)		-0.1633** (0.0433)
2018	-0.2640** (0.0513)		-0.1505** (0.0431)
2019	-0.3390** (0.0529)		-0.2810** (0.0464)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>			
2013	0.0012 (0.1360)		-0.0712 (0.1024)
2014	0.1946 (0.1344)		0.2495* (0.1046)
2015	0.4001** (0.1085)		0.5535** (0.1017)
2016	0.2152 (0.1320)		0.2487* (0.1074)
2017	0.1467 (0.1298)		0.1361 (0.1006)
2018	0.1728 (0.1182)		0.3545** (0.0923)
2019	-0.3924** (0.1100)		-0.1615+ (0.0924)
(R ² ; Within R ²)	(0.0261; 0.0114)	(0.0213; 0.0066)	(0.0227; 0.0080)

Note: Dependent variable: log employment growth. All columns have 169,028 observations, all include sector $\times$ year and province $\times$ year fixed effects. Column (1) includes both interaction sets (baseline, column (2) of Table 3); column (2) only the markdown interactions; column (3) only the θ_L interactions. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 5: Employment growth, robustness to additional covariates. Part A: markdown gradients.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1em2	1em2 balanced	1em21 rel. TFP	1em22 k/l	1em23 plants	1em24 size	1em25 markup
<i>Year × group × log markdown</i>							
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>							
2013	-0.0118 (0.0142)	-0.0234 (0.0182)	-0.0087 (0.0186)	-0.0179 (0.0144)	-0.0093 (0.0145)	0.0075 (0.0140)	0.0137 (0.0154)
2014	-0.0179 (0.0156)	-0.0117 (0.0186)	-0.0166 (0.0196)	-0.0302+ (0.0156)	-0.0133 (0.0157)	0.0021 (0.0157)	0.0099 (0.0163)
2015	-0.0148 (0.0154)	-0.0347+ (0.0200)	-0.0070 (0.0160)	-0.0238 (0.0150)	-0.0076 (0.0152)	0.0072 (0.0148)	0.0050 (0.0174)
2016	-0.0090 (0.0143)	-0.0283 (0.0210)	-0.0078 (0.0169)	-0.0298* (0.0145)	-0.0061 (0.0145)	0.0114 (0.0135)	0.0280+ (0.0159)
2017	-0.0168 (0.0115)	0.0006 (0.0174)	-0.0179 (0.0121)	-0.0387* (0.0114)	-0.0185 (0.0118)	0.0002 (0.0114)	0.0147 (0.0139)
2018	-0.0509** (0.0123)	-0.0164 (0.0179)	-0.0384** (0.0132)	-0.0630** (0.0122)	-0.0471** (0.0127)	-0.0288* (0.0120)	-0.0234+ (0.0140)
2019	-0.0499** (0.0168)	-0.0049 (0.0314)	-0.0375* (0.0169)	-0.0692* (0.0174)	-0.0397* (0.0169)	-0.0248 (0.0177)	-0.0252 (0.0180)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>							
2013	0.1032** (0.0185)	0.1201** (0.0234)	0.0890** (0.0202)	0.0517** (0.0192)	0.1070** (0.0201)	0.0877** (0.0198)	0.1058** (0.0184)
2014	0.1340** (0.0165)	0.1284** (0.0196)	0.1092** (0.0174)	0.0914** (0.0181)	0.1401** (0.0177)	0.1143** (0.0164)	0.1352** (0.0164)
2015	0.1239** (0.0171)	0.1333** (0.0191)	0.0872** (0.0165)	0.0803** (0.0193)	0.1373** (0.0182)	0.1100** (0.0166)	0.1237** (0.0169)
2016	0.1536** (0.0193)	0.1043** (0.0236)	0.1087** (0.0180)	0.1085** (0.0203)	0.1593** (0.0213)	0.1276** (0.0203)	0.1500** (0.0195)
2017	0.1728** (0.0173)	0.1747** (0.0260)	0.1380** (0.0191)	0.1250** (0.0201)	0.1565** (0.0183)	0.1412** (0.0168)	0.1675** (0.0171)
2018	0.1301** (0.0180)	0.0624** (0.0208)	0.0870** (0.0192)	0.0710** (0.0196)	0.1420** (0.0192)	0.1076** (0.0185)	0.1252** (0.0179)
2019	0.1067** (0.0185)	0.0774** (0.0240)	0.0840** (0.0191)	0.0623** (0.0217)	0.1271** (0.0195)	0.0855** (0.0187)	0.1036** (0.0188)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>							
2013	0.0483 (0.0480)	0.0560 (0.0545)	0.0504 (0.0541)	0.0359 (0.0525)	0.0449 (0.0480)	0.0464 (0.0481)	0.0474 (0.0475)
2014	0.1601* (0.0813)	0.1419+ (0.0823)	0.1854* (0.0864)	0.1889* (0.0878)	0.1640+ (0.0838)	0.1618+ (0.0830)	0.1616* (0.0812)
2015	0.2099** (0.0578)	0.2608** (0.0620)	0.2080** (0.0623)	0.2243** (0.0612)	0.2051** (0.0601)	0.2002** (0.0560)	0.2111** (0.0572)
2016	0.1745* (0.0706)	0.2542** (0.0746)	0.1276+ (0.0732)	0.1726* (0.0710)	0.1442* (0.0703)	0.1428* (0.0694)	0.1746* (0.0706)
2017	0.0899 (0.0669)	0.1624 (0.1368)	0.0823 (0.0786)	0.1162 (0.0768)	0.0943 (0.0683)	0.0409 (0.0667)	0.0897 (0.0681)
2018	0.2370** (0.0552)	0.2948** (0.0958)	0.2156** (0.0616)	0.2263** (0.0600)	0.2193** (0.0564)	0.1916** (0.0574)	0.2427** (0.0563)
2019	0.2878** (0.0595)	0.1356* (0.0658)	0.2857** (0.0593)	0.3349** (0.0694)	0.2979** (0.0629)	0.2684** (0.0596)	0.2900** (0.0589)
Sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,028	65,337	137,029	169,028	169,028	169,028	169,028
R ²	0.02610	0.05391	0.03116	0.02976	0.02808	0.05340	0.03031
Within R ²	0.01144	0.01764	0.01492	0.01516	0.01346	0.03916	0.01571

Note: Dependent variable: log employment growth. All columns include sector×year and province×year fixed effects. Column (1) is the baseline; column (2) re-estimates it on the balanced panel; columns (3)–(7) add year×group interactions with, respectively, relative TFP, capital intensity, the number of plants, firm size, and the log markup. This part reports the markdown interactions; Parts B and C (Tables 6 and 7) report the θ_L and added-covariate interactions from the same regressions. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 6: Employment growth, robustness to additional covariates. Part B: labor-elasticity interactions.

	(1) 1em2	(2) 1im2 balanced	(3) 1em21 rel. TFP	(4) 1em22 k/l	(5) 1em23 plants	(6) 1em24 size	(7) 1em25 markup
<i>Year × group × θ_L</i>							
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>							
2013	-0.2719** (0.0271)	-0.2697** (0.0319)	-0.2496** (0.0284)	-0.2922** (0.0272)	-0.2802** (0.0276)	-0.2706** (0.0271)	-0.3297** (0.0307)
2014	-0.1634** (0.0271)	-0.2342** (0.0352)	-0.1956** (0.0286)	-0.1837** (0.0263)	-0.1788** (0.0274)	-0.1737** (0.0256)	-0.2251** (0.0299)
2015	-0.1924** (0.0244)	-0.2411** (0.0345)	-0.2008** (0.0282)	-0.2154** (0.0249)	-0.2165** (0.0276)	-0.2161** (0.0266)	-0.2354** (0.0269)
2016	-0.1959** (0.0283)	-0.2157** (0.0371)	-0.2028** (0.0293)	-0.2231** (0.0261)	-0.1985** (0.0292)	-0.1874** (0.0262)	-0.2797** (0.0292)
2017	-0.2506** (0.0210)	-0.2457** (0.0287)	-0.2594** (0.0250)	-0.2750** (0.0215)	-0.2374** (0.0218)	-0.2418** (0.0210)	-0.3201** (0.0265)
2018	-0.2117** (0.0228)	-0.2217** (0.0330)	-0.2075** (0.0236)	-0.2336** (0.0228)	-0.2236** (0.0240)	-0.2088** (0.0229)	-0.2762** (0.0242)
2019	-0.2714** (0.0262)	-0.2821** (0.0522)	-0.2573** (0.0305)	-0.2995** (0.0280)	-0.3076** (0.0281)	-0.2763** (0.0272)	-0.3205** (0.0299)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>							
2013	-0.3498** (0.0387)	-0.3454** (0.0511)	-0.3541** (0.0464)	-0.3931** (0.0391)	-0.3320** (0.0438)	-0.3625** (0.0411)	-0.3552** (0.0411)
2014	-0.2477** (0.0426)	-0.2955** (0.0570)	-0.2502** (0.0459)	-0.3039** (0.0426)	-0.2227** (0.0495)	-0.2371** (0.0462)	-0.2526** (0.0421)
2015	-0.2489** (0.0418)	-0.3080** (0.0574)	-0.2382** (0.0443)	-0.2930** (0.0427)	-0.1974** (0.0460)	-0.1915** (0.0433)	-0.2349** (0.0433)
2016	-0.3734** (0.0434)	-0.3076** (0.0591)	-0.3372** (0.0477)	-0.4384** (0.0440)	-0.3563** (0.0469)	-0.3334** (0.0442)	-0.3912** (0.0471)
2017	-0.3988** (0.0540)	-0.3696** (0.0688)	-0.3450** (0.0559)	-0.4775** (0.0546)	-0.4681** (0.0647)	-0.3431** (0.0601)	-0.4237** (0.0548)
2018	-0.2640** (0.0513)	-0.3012** (0.0614)	-0.2275** (0.0543)	-0.3293** (0.0539)	-0.2107** (0.0567)	-0.1898** (0.0557)	-0.2755** (0.0537)
2019	-0.3390** (0.0529)	-0.4178** (0.0667)	-0.2957** (0.0538)	-0.4097** (0.0543)	-0.2425** (0.0586)	-0.2610** (0.0526)	-0.3435** (0.0521)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>							
2013	0.0012 (0.1360)	0.0722 (0.1606)	-0.0334 (0.1543)	-0.0708 (0.1594)	-0.0416 (0.1699)	-0.1209 (0.1752)	-0.0846 (0.1515)
2014	0.1946 (0.1344)	0.1457 (0.1453)	0.0232 (0.1390)	0.2729* (0.1347)	0.2214 (0.1443)	0.1725 (0.1508)	0.1458 (0.1421)
2015	0.4001** (0.1085)	0.3419** (0.1109)	0.2782* (0.1287)	0.4234** (0.1349)	0.3454* (0.1401)	0.3639** (0.1188)	0.3569** (0.1321)
2016	0.2152 (0.1320)	0.2081 (0.1590)	0.1534 (0.1315)	0.1789 (0.1564)	-0.0244 (0.1672)	0.0418 (0.1603)	0.0819 (0.1330)
2017	0.1467 (0.1298)	0.0616 (0.1661)	0.1590 (0.1383)	0.2844 (0.1883)	0.2075 (0.1776)	0.0277 (0.1524)	0.0956 (0.1452)
2018	0.1728 (0.1182)	0.0271 (0.1658)	0.0649 (0.1379)	0.1658 (0.1539)	-0.0331 (0.1477)	-0.0347 (0.1421)	0.0555 (0.1371)
2019	-0.3924** (0.1100)	-0.4190** (0.1282)	-0.4761** (0.1115)	-0.2544+ (0.1494)	-0.3997** (0.1245)	-0.4493** (0.1203)	-0.4525** (0.1287)
Sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,028	65,337	137,029	169,028	169,028	169,028	169,028
R ²	0.02610	0.05391	0.03116	0.02976	0.02808	0.05340	0.03031
Within R ²	0.01144	0.01764	0.01492	0.01516	0.01346	0.03916	0.01571

Note: Continuation of Table 5; coefficients on the $\text{year} \times \text{group} \times \theta_L$ interactions from the same regressions. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 7: Employment growth, robustness to additional covariates. Part C: added-covariate interactions.

	(1) 1em2	(2) lim2 balanced	(3) 1em21 rel. TFP	(4) 1em22 k/l	(5) 1em23 plants	(6) 1em24 size	(7) 1em25 markup
<i>Year × group × additional covariate (covariate differs by column, see header)</i>							
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>							
2013			0.0987** (0.0259)	0.0204** (0.0043)	-0.0292** (0.0111)	-0.1044** (0.0072)	0.1097** (0.0204)
2014			0.1006** (0.0257)	0.0294** (0.0044)	-0.0343** (0.0103)	-0.0980** (0.0073)	0.1207** (0.0216)
2015			0.0598* (0.0249)	0.0228** (0.0042)	-0.0180 (0.0131)	-0.0832** (0.0087)	0.0893** (0.0219)
2016			0.1464** (0.0260)	0.0361** (0.0036)	-0.0652** (0.0132)	-0.1187** (0.0074)	0.1519** (0.0207)
2017			0.0957** (0.0238)	0.0280** (0.0032)	-0.0537** (0.0109)	-0.1055** (0.0058)	0.1163** (0.0211)
2018			0.1276** (0.0232)	0.0217** (0.0035)	-0.0720** (0.0109)	-0.1135** (0.0085)	0.1126** (0.0180)
2019			0.0956** (0.0219)	0.0337** (0.0038)	-0.0542** (0.0131)	-0.1222** (0.0112)	0.1010** (0.0197)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>							
2013			0.1505** (0.0339)	0.0332** (0.0047)	-0.0385** (0.0114)	-0.1005** (0.0061)	0.2344** (0.0345)
2014			0.1748** (0.0294)	0.0230** (0.0055)	-0.0503** (0.0110)	-0.1018** (0.0061)	0.2386** (0.0332)
2015			0.2554** (0.0387)	0.0249** (0.0052)	-0.0492** (0.0120)	-0.1048** (0.0061)	0.2419** (0.0366)
2016			0.1454** (0.0385)	0.0238** (0.0055)	-0.0725** (0.0132)	-0.1231** (0.0064)	0.1997** (0.0370)
2017			0.1985** (0.0336)	0.0301** (0.0063)	-0.0218 (0.0136)	-0.1143** (0.0066)	0.2449** (0.0381)
2018			0.2649** (0.0319)	0.0357** (0.0053)	-0.0954** (0.0121)	-0.1330** (0.0069)	0.3140** (0.0355)
2019			0.2341** (0.0350)	0.0245** (0.0056)	-0.1059** (0.0164)	-0.1486** (0.0076)	0.2116** (0.0304)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>							
2013			0.2833* (0.1191)	0.0008 (0.0182)	-0.0176 (0.0180)	-0.0538** (0.0110)	0.2015 (0.1442)
2014			0.3122** (0.1001)	-0.0297+ (0.0154)	-0.0410* (0.0165)	-0.0673** (0.0123)	0.0871 (0.0789)
2015			0.1754* (0.0805)	-0.0174 (0.0129)	-0.0115 (0.0179)	-0.0545** (0.0097)	0.0865 (0.0988)
2016			0.1900+ (0.1113)	-0.0075 (0.0173)	0.0060 (0.0216)	-0.0493** (0.0111)	0.2254* (0.1029)
2017			-0.0083 (0.0945)	-0.0275 (0.0203)	-0.0575 (0.0286)	-0.0441** (0.0111)	0.0562 (0.0767)
2018			0.1006 (0.0933)	-0.0030 (0.0157)	-0.0155 (0.0248)	-0.0372** (0.0087)	0.1704+ (0.0888)
2019			0.0974 (0.1034)	-0.0399+ (0.0206)	-0.0637** (0.0184)	-0.0642** (0.0087)	0.0744 (0.0837)
Sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,028	65,337	137,029	169,028	169,028	169,028	169,028
R ²	0.02610	0.05391	0.03116	0.02976	0.02808	0.05340	0.03031
Within R ²	0.01144	0.01764	0.01492	0.01516	0.01346	0.03916	0.01571

Note: Continuation of Table 5: coefficients on the added year×group×covariate interactions from the same regressions (relative TFP in column (3), capital intensity in (4), number of plants in (5), firm size in (6), log markup in (7)). Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 8: Employment growth by capital intensity and firm size. Part A: markdown gradients.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1em2	1em201	1em202	1em203	1ems1	1ems2	1ems3
	All	k/l low	k/l mid	k/l high	1–10 emp.	10–50	>50
<i>Year × group × log markdown</i>							
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>							
2013	-0.0118 (0.0142)	-0.0278 ⁺ (0.0152)	0.0141 (0.0321)	0.0216 (0.0582)	-0.0158 (0.0157)	0.0706 ⁺ (0.0381)	-0.0880 (0.2253)
2014	-0.0179 (0.0156)	-0.0208 (0.0207)	0.0063 (0.0298)	-0.0357 (0.0334)	-0.0096 (0.0161)	0.0050 (0.0487)	-0.6636 ⁺ (0.3630)
2015	-0.0148 (0.0154)	-0.0151 (0.0183)	-0.0358 (0.0312)	0.0125 (0.0517)	-0.0103 (0.0177)	0.0128 (0.0342)	0.0607 (0.2983)
2016	-0.0090 (0.0143)	-0.0185 (0.0152)	-0.0013 (0.0351)	0.0174 (0.0580)	-0.0041 (0.0147)	0.0303 (0.0436)	0.2994 ^{**} (0.1148)
2017	-0.0168 (0.0115)	-0.0163 (0.0132)	-0.0027 (0.0322)	-0.0433 (0.0364)	-0.0071 (0.0124)	-0.0099 (0.0275)	-0.8282 ^{**} (0.2022)
2018	-0.0509 ^{**} (0.0123)	-0.0684 ^{**} (0.0135)	-0.0022 (0.0286)	0.0024 (0.0420)	-0.0429 ^{**} (0.0128)	0.0222 (0.0413)	-0.4953 (0.5491)
2019	-0.0499 ^{**} (0.0168)	-0.0498 [*] (0.0202)	-0.0490 (0.0396)	-0.0581 (0.0501)	-0.0397 [*] (0.0181)	-0.0168 (0.0370)	-0.4131 (0.4826)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>							
2013	0.1032 ^{**} (0.0185)	0.1522 ^{**} (0.0230)	0.0620 [*] (0.0309)	-0.0549 (0.0413)	0.0925 ^{**} (0.0212)	0.0890 [*] (0.0408)	-0.1624 (0.2525)
2014	0.1340 ^{**} (0.0165)	0.1560 ^{**} (0.0205)	0.1050 ^{**} (0.0327)	0.0712 ⁺ (0.0379)	0.1258 ^{**} (0.0171)	0.0164 (0.0394)	-0.0648 (0.1933)
2015	0.1239 ^{**} (0.0171)	0.1422 ^{**} (0.0214)	0.1131 ^{**} (0.0384)	0.0532 (0.0360)	0.1108 ^{**} (0.0178)	-0.0015 (0.0486)	-1.016 ⁺ (0.5285)
2016	0.1536 ^{**} (0.0193)	0.1959 ^{**} (0.0236)	0.1152 ^{**} (0.0433)	-0.0069 (0.0476)	0.1312 ^{**} (0.0190)	0.0767 (0.0488)	0.0088 (0.1879)
2017	0.1728 ^{**} (0.0173)	0.1890 ^{**} (0.0209)	0.1250 ^{**} (0.0364)	0.1736 ^{**} (0.0541)	0.1530 ^{**} (0.0194)	0.1034 ⁺ (0.0578)	0.0537 (0.3178)
2018	0.1301 ^{**} (0.0180)	0.1473 ^{**} (0.0217)	0.1586 ^{**} (0.0411)	-0.0387 (0.0582)	0.1030 ^{**} (0.0185)	0.0675 (0.0533)	0.8030 (1.189)
2019	0.1067 ^{**} (0.0185)	0.1304 ^{**} (0.0230)	0.1173 ^{**} (0.0349)	-0.0254 (0.0597)	0.0920 ^{**} (0.0194)	0.0218 (0.0572)	-0.6658 (0.8682)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>							
2013	0.0483 (0.0480)	0.0426 (0.0822)	0.0006 (0.0578)	0.1759 (0.1520)	0.0839 (0.0748)	0.0862 (0.0636)	-0.1239 (0.1293)
2014	0.1601 [*] (0.0813)	0.3692 ^{**} (0.1427)	-0.0104 (0.0498)	0.1164 (0.2784)	0.2490 [*] (0.1205)	0.0007 (0.0897)	-0.1439 (0.1008)
2015	0.2099 ^{**} (0.0578)	0.3759 ^{**} (0.0907)	0.1320 (0.0807)	-0.0027 (0.1422)	0.2524 ^{**} (0.0639)	0.1426 (0.1426)	-0.1004 (0.1422)
2016	0.1745 [*] (0.0706)	0.3236 ^{**} (0.1058)	0.1322 (0.1087)	-0.2344 (0.1462)	0.2355 ^{**} (0.0899)	0.0651 (0.1450)	0.1092 (0.1307)
2017	0.0899 (0.0669)	0.1478 (0.1113)	-0.0844 (0.0941)	0.2098 (0.1500)	0.1309 ⁺ (0.0773)	-0.0734 (0.2014)	-0.1778 (0.1806)
2018	0.2370 ^{**} (0.0552)	0.2388 ^{**} (0.0813)	0.3264 ^{**} (0.0856)	-0.0363 (0.1544)	0.2300 ^{**} (0.0669)	0.2986 (0.1912)	0.0052 (0.2104)
2019	0.2878 ^{**} (0.0595)	0.3840 ^{**} (0.1076)	0.3130 ^{**} (0.0943)	0.1257 (0.1134)	0.2830 ^{**} (0.0853)	0.2092 [*] (0.0821)	-0.0187 (0.0877)
Sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,028	125,936	21,376	21,661	137,052	26,824	1,418
R ²	0.02610	0.02678	0.05278	0.05883	0.02791	0.06754	0.28777
Within R ²	0.01144	0.01350	0.01298	0.00918	0.01184	0.00656	0.06013

Note: Dependent variable: log employment growth. All columns include sector × year and province × year fixed effects. Column (1) is the baseline; columns (2)–(4) split sectors by terciles of median capital intensity; columns (5)–(7) split firms by employment. Part B (Table 9) reports the θ_L interactions from the same regressions. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ^{**} $p < 0.01$, ^{*} $p < 0.05$, ⁺ $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table 9: Employment growth by capital intensity and firm size. Part B: labor-elasticity interactions.

	(1) 1em2 All	(2) 1em201 k/l low	(3) 1em202 k/l mid	(4) 1em203 k/l high	(5) 1ems1 1–10 emp.	(6) 1ems2 10–50	(7) 1ems3 >50
<i>Year $\times$ group $\times \theta_L$</i>							
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>							
2013	-0.2719** (0.0271)	-0.2850** (0.0330)	-0.1157+ (0.0660)	-0.3627** (0.0802)	-0.2626** (0.0298)	-0.2536** (0.0627)	-1.117 (0.8470)
2014	-0.1634** (0.0271)	-0.1577** (0.0321)	-0.1510* (0.0646)	-0.1780* (0.0703)	-0.1779** (0.0297)	-0.1355+ (0.0794)	0.0129 (0.2402)
2015	-0.1924** (0.0244)	-0.1878** (0.0291)	-0.1300* (0.0567)	-0.2816** (0.0789)	-0.2074** (0.0283)	-0.2092** (0.0570)	-0.2154 (0.5028)
2016	-0.1959** (0.0283)	-0.1921** (0.0335)	-0.1260* (0.0641)	-0.2652** (0.0747)	-0.2030** (0.0311)	-0.0951 (0.0751)	-0.3513+ (0.1889)
2017	-0.2506** (0.0210)	-0.2424** (0.0234)	-0.1916** (0.0681)	-0.3516** (0.0694)	-0.2520** (0.0235)	-0.1352* (0.0530)	-2.233** (0.2233)
2018	-0.2117** (0.0228)	-0.2178** (0.0256)	-0.1140+ (0.0593)	-0.2830** (0.0697)	-0.2079** (0.0253)	-0.0860 (0.0726)	-1.660** (0.5318)
2019	-0.2714** (0.0262)	-0.2840** (0.0300)	-0.2144** (0.0813)	-0.2091** (0.0747)	-0.2592** (0.0277)	-0.1947** (0.0651)	0.2709 (0.9117)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>							
2013	-0.3498** (0.0387)	-0.4263** (0.0494)	-0.0917 (0.1052)	-0.2395* (0.0969)	-0.3085** (0.0422)	-0.4979** (0.1003)	-0.0809 (1.012)
2014	-0.2477** (0.0426)	-0.2389** (0.0536)	-0.2108* (0.0895)	-0.3416** (0.1069)	-0.2957** (0.0467)	0.0550 (0.0920)	0.8601* (0.3910)
2015	-0.2489** (0.0418)	-0.2575** (0.0497)	-0.0881 (0.1184)	-0.3924** (0.1038)	-0.2549** (0.0460)	-0.1809+ (0.1092)	0.5750 (1.076)
2016	-0.3734** (0.0434)	-0.3782** (0.0484)	-0.2843* (0.1367)	-0.4578** (0.1267)	-0.3809** (0.0445)	-0.0484 (0.1494)	-0.5078+ (0.2655)
2017	-0.3988** (0.0540)	-0.4168** (0.0660)	-0.2476+ (0.1274)	-0.4932** (0.1428)	-0.3583** (0.0587)	-0.3152* (0.1439)	-1.764** (0.4680)
2018	-0.2640** (0.0513)	-0.2507** (0.0610)	-0.2320+ (0.1286)	-0.3458** (0.1323)	-0.2016** (0.0532)	-0.2711+ (0.1560)	-2.017 (1.445)
2019	-0.3390** (0.0529)	-0.3593** (0.0656)	-0.3623** (0.1198)	-0.2270+ (0.1329)	-0.3045** (0.0551)	-0.2167+ (0.1307)	0.4150 (0.9225)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>							
2013	0.0012 (0.1360)	0.1830 (0.1960)	0.0177 (0.1751)	-0.5034 (0.4090)	0.2719 (0.2049)	-0.3904* (0.1943)	-0.7203 (0.8615)
2014	0.1946 (0.1344)	0.3274 (0.2303)	0.2388+ (0.1447)	-0.2806 (0.3304)	0.1857 (0.2134)	0.3998* (0.1995)	0.1472 (0.4765)
2015	0.4001** (0.1085)	0.3818* (0.1498)	0.6525** (0.2012)	0.0394 (0.2554)	0.5707** (0.1297)	0.4451* (0.2051)	-1.389* (0.6998)
2016	0.2152 (0.1320)	0.4202* (0.2082)	0.0828 (0.2209)	-0.0655 (0.2178)	0.2465 (0.1706)	0.2559 (0.2338)	-0.7211 (0.6009)
2017	0.1467 (0.1298)	0.2319 (0.1950)	0.4660 (0.3023)	-0.3096 (0.2107)	0.1727 (0.1617)	0.6004 (0.3909)	-0.4591 (0.8777)
2018	0.1728 (0.1182)	0.2838 (0.1831)	-0.0884 (0.2016)	0.2055 (0.2541)	0.3300* (0.1582)	0.2525 (0.3231)	-0.6320 (0.7049)
2019	-0.3924** (0.1100)	-0.1838 (0.1786)	-0.8010** (0.1966)	-0.3163 (0.2000)	-0.1348 (0.1544)	-0.2804 (0.2292)	-0.7163 (0.4994)
Sector $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,028	125,936	21,376	21,661	137,052	26,824	1,418
R ²	0.02610	0.02678	0.05278	0.05883	0.02791	0.06754	0.28777
Within R ²	0.01144	0.01350	0.01298	0.00918	0.01184	0.00656	0.06013

Note: Continuation of Table 8; coefficients on the year $\times$ group $\times \theta_L$ interactions from the same regressions. Groups are defined as in Table 2. Year t interactions refer to growth from the fourth quarter of $t-1$ to the fourth quarter of t ; the 2016 row spans the January 2016 minimum-wage increase. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

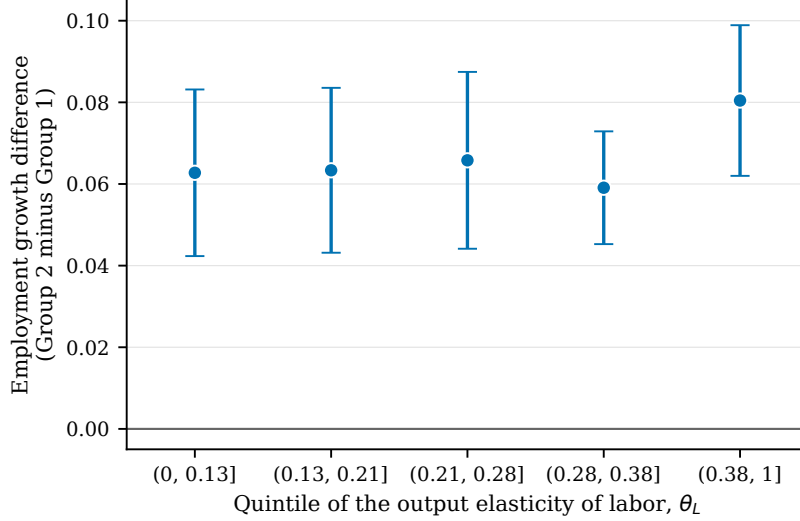


Figure 11: Employment growth difference between high-markdown and negative-markdown minimum-wage firms, by labor-elasticity quintile. Bars show the difference in mean log employment growth between Group 2 (all workers at the minimum wage, $\log \eta \geq 0.25$) and Group 1 (all workers at the minimum wage, $\log \eta \leq 0$) within each quintile of the output elasticity of labor θ_L , from a regression on quintile $\times$ group interactions with sector $\times$ year and province $\times$ year fixed effects. Whiskers show approximate 95% confidence intervals (± 2 standard errors). The difference is stable across quintiles: the markdown gradient is not an artifact of labor-intensity composition.

matter elsewhere: under Cobb-Douglas the θ_L interactions lose all significance, since the Cobb-Douglas elasticity does not vary across firms within a sector, and both the Cobb-Douglas and the neutral translog produce spuriously significant markdown gradients for Group 1, firms for which the theory requires a null. Only the non-neutral translog, which accommodates factor-augmenting technical change, delivers the full predicted pattern.

Adjustment frictions and labor hoarding. Three caveats deserve emphasis, and they do not all work in the same direction. The first is measurement error. Markdown estimates are noisy, and noise attenuates the gradients toward zero, so the estimated φ_{2t} is a conservative reading of the true effect. The second is firing costs. As discussed in Section 3.6, severance pay slows the Group 1 contraction and raises the expected cost of hiring for Group 2 firms, so firing costs too reduce the contrast between the two groups and work against the pattern we find. The third is labor hoarding. In a high-inflation economy, firms may retain workers they do not currently need in anticipation of price increases, and higher-markdown firms have more room to do so, because they can carry the higher wage bill without reaching the point where the wage exceeds the marginal revenue product of labor. Hoarding would therefore show up as a positive Group 2 gradient. This is not an alternative to the monopsony explanation, however. The room to hoard is created by the markdown itself: a firm with no markdown cannot retain workers at a wage above their marginal product, whatever it expects prices to do. Hoarding is one of the ways in which a firm with a markdown uses that markdown when

the floor rises, and it cannot explain why Group 1 firms, which pay the same wage, lose employment. We therefore read the positive Group 2 gradients as evidence of monopsony power, whether or not part of the retained employment is held in anticipation of price increases.

7. Conclusion

The employment effect of a minimum-wage increase does not have a single answer. In this paper, we have argued that the answer depends on a measurable firm-level variable, the wage markdown. A firm with no markdown behaves as the competitive model predicts: a higher floor raises its costs above the marginal product of labor, and the firm sheds labor. A firm that pays its workers below their marginal product behaves as the monopsony model predicts: a moderate floor moves the firm up its labor-supply curve, and the firm hires. Therefore, the same national policy pushes employment in opposite directions at different firms, and the direction depends on the firm’s position in the markdown distribution before the shock.

We established this in two steps. We first estimated product markups and wage markdowns jointly for the near-universe of Turkish manufacturing firms, using a gross-output translog production function that allows for non-neutral technical change, and we documented substantial and structured variation in markdowns across firms, sectors, and time. We then showed, year by year over 2013–2019, that a firm’s markdown predicts the sign and magnitude of its employment response to minimum-wage increases, with the strongest response around the January 2016 increase, which was large (33% nominal, $\approx 25\%$ real), unanticipated, and concentrated on a single date. The clearest result is the contrast between firms that paid essentially the same near-minimum wage but differed in their markdowns. Firms with no markdown lost employment in every year, whereas firms with markdowns larger than even the largest increase grew faster, and the growth difference increases with the markdown. A model that conditions on the wage alone cannot generate this difference.

Implications for measurement. Our results are also relevant for the ongoing debate on the reliability of production-approach estimates of market power. The critiques by [Raval \(2023\)](#), [De Ridder et al. \(2026\)](#), and [Rubens et al. \(2025\)](#) have rightly cautioned that markup and markdown *levels* are sensitive to functional form and to the treatment of non-neutral technology. Our findings are consistent with that caution (our own estimates move across specifications), but they also show that the markdown carries economically meaningful information that survives these concerns: the *ordering* of firms by markdown predicts their behavior under a policy shock, even after conditioning on the output elasticity of labor that is the source of the [Rubens et al. \(2025\)](#) bias, and even as the levels

themselves shift with functional form. In other words, a markdown estimate can be too noisy to be used as a level and still be informative as a ranking. This is an encouraging result for the empirical study of monopsony.

Implications for policy. The textbook treats the minimum wage as a uniform tax on employment. Our results suggest it is better understood as a redistributive instrument whose employment consequences depend on the monopsony structure of the economy it acts upon. Where employer power is substantial, a moderate floor can raise employment and compress markdowns simultaneously, moving wages toward marginal product without the job losses the competitive model predicts; where it is absent, the same floor destroys jobs. One practical implication follows: the employment effect of a national minimum wage cannot be summarized by a single elasticity, and its distributional consequences, across firms, and hence across the workers attached to them, are in principle predictable from the pre-existing distribution of markdowns. In a developing economy with substantial monopsony and a minimum wage as a primary labor-market instrument, this is a first-order consideration for policy design.

Limitations. Several caveats should be kept in mind when interpreting our results. Markdown estimates are noisy; because measurement error attenuates the cross-group contrast toward zero, our estimate of the markdown-employment relationship is a lower bound on the true relationship. The identification of the markup from the materials margin presumes competitive input markets; if firms also exercise buyer power over materials, our markdowns understate true labor market power (the lower-bound argument of Section 3.3), again biasing against the result. Firing costs slow the adjustment of Group 1 firms and spread the employment loss over several years, which is why we estimate the response year by year rather than in a single window. Labor hoarding in anticipation of inflation may contribute to the employment retained at high-markdown firms, but it presupposes a markdown: a firm with no markdown cannot hold workers at a wage above their marginal product, so hoarding cannot produce the losses we observe at Group 1 firms paying the same wage. Finally, our markdowns are measured at the firm level while the relevant labor supply operates at the worker level; we address this by restricting the sharp tests to firms with wage-homogeneous workforces, but the firm-level approximation remains a limitation. None of these concerns can generate the sign reversal we find: measurement error, materials-market buyer power, and firing costs work against it, and hoarding presupposes the markdown room that the monopsony model describes.

Future directions. Three extensions seem natural. First, the joint labor-supply and production-function estimator of [Rubens et al. \(2025\)](#) could be brought to the EIS panel, using the employer-employee linkage to construct the required labor-market shares, to

sharpen the markdown estimates and to relax the Hicks-neutrality assumption more completely. Second, the matched employer-employee structure of the EIS data invites a richer treatment of the labor market than the static monopsony model used here, in particular, an explicit job-ladder model in which markdowns arise endogenously from search frictions, allowing the minimum-wage experiment to speak to the dynamics of reallocation as well as the level of employment. Third, the same data permit a direct study of materials-market buyer power through the inter-firm transaction network, which would convert the lower-bound caveat of Section 3.3 into a measured object. We leave these to future work.

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Appendices

A. The Markdown Lower Bound

The markup is identified from the materials margin under the competitive-materials assumption of Section 3.2, $\mu_{it} = \theta_{M,it}/s_{M,it}$, and the markdown from (4), $\eta_{it} = (\theta_{L,it}/s_{L,it})/\mu_{it}$. Suppose that assumption fails and the firm exercises buyer power in the materials market, so that the true materials first-order condition carries a markdown wedge $\eta_{it}^M > 1$:

$$\frac{\theta_{M,it}}{s_{M,it}} = \mu_{it} \eta_{it}^M. \quad (\text{A.1})$$

The object recovered from the materials margin is then not the markup but the product $\mu_{it}\eta_{it}^M$. Denote the estimated (apparent) markup by $\hat{\mu}_{it} \equiv \theta_{M,it}/s_{M,it} = \mu_{it}\eta_{it}^M$. Since $\eta_{it}^M > 1$, the markup is overstated,

$$\hat{\mu}_{it} = \mu_{it} \eta_{it}^M > \mu_{it}. \quad (\text{A.2})$$

The estimated markdown divides the labor margin by this overstated markup:

$$\hat{\eta}_{it} = \frac{\theta_{L,it}/s_{L,it}}{\hat{\mu}_{it}} = \frac{\theta_{L,it}/s_{L,it}}{\mu_{it} \eta_{it}^M} = \frac{\eta_{it}}{\eta_{it}^M}, \quad (\text{A.3})$$

where the last equality uses the true labor condition $\theta_{L,it}/s_{L,it} = \mu_{it}\eta_{it}$. Because $\eta_{it}^M > 1$,

$$\hat{\eta}_{it} = \frac{\eta_{it}}{\eta_{it}^M} < \eta_{it}. \quad (\text{A.4})$$

The estimated labor markdown is therefore strictly below the true labor markdown, and the estimated markup strictly above the true markup. The estimated $\hat{\eta}_{it}$ is a lower bound on the firm's true labor market power, with the bound tightening as materials buyer power $\eta_{it}^M \rightarrow 1$. ■

B. Derivations of the Three Regimes

This appendix derives the monopsony equilibrium, the efficient benchmark, and the employment responses of Cases 1–3 of Section 3.5 from the parametric model of Section 3.4. Lower-case letters denote natural logs throughout.

B.1 Building blocks

Production is $Q = AL^{\theta_L}$, so in logs $q = a + \theta_L l$. Product demand is $Q = DP^{-\epsilon_Q}$, i.e. $q = d - \epsilon_Q p$, which inverts to $p = d/\epsilon_Q - q/\epsilon_Q$. Firm labor supply is $L = Z(W/P^c)^{\epsilon_L}$, i.e. in logs

$$l = z - \epsilon_L p^c + \epsilon_L w \iff w = p^c - z/\epsilon_L + l/\epsilon_L, \quad (\text{B5})$$

the right-hand expression being the inverse labor-supply (wage-offer) schedule, which we denote ls .

Marginal revenue product of labor. With market power in the product market, $MRPL = d(PQ)/dL = P[1 - 1/\epsilon_Q] f_L = P[1 - 1/\epsilon_Q] \theta_L AL^{\theta_L - 1}$. Taking logs,

$$mrpl = p + \log(1 - 1/\epsilon_Q) + \log \theta_L + a - (1 - \theta_L) l, \quad (\text{B6})$$

which reproduces (5). (This requires $\epsilon_Q > 1$ so that $1 - 1/\epsilon_Q > 0$; the firm operates on the elastic portion of demand.)

Marginal cost of labor. With monopsony, total labor cost is WL and, substituting the inverse supply schedule $W = P^c Z^{-1/\epsilon_L} L^{1/\epsilon_L}$,

$$MCL = \frac{d(WL)}{dL} = \frac{d}{dL} \left(P^c Z^{-1/\epsilon_L} L^{1+1/\epsilon_L} \right) = (1 + 1/\epsilon_L) P^c Z^{-1/\epsilon_L} L^{1/\epsilon_L}.$$

Taking logs,

$$mcl = p^c + \log(1 + 1/\epsilon_L) - z/\epsilon_L + l/\epsilon_L. \quad (\text{B7})$$

Comparing (B7) with the inverse supply schedule $ls : w = p^c - z/\epsilon_L + l/\epsilon_L$ shows that the mcl and ls curves are parallel in (l, w) space and separated vertically by exactly the log markdown,

$$mcl - ls = \log(1 + 1/\epsilon_L). \quad (\text{B8})$$

At the unconstrained monopsony equilibrium this distance equals the log markdown, $\log \eta$; when a wage floor binds, the realized markdown $mrpl - w$ is smaller.

B.2 Monopsony equilibrium

The monopsonist hires where $mrpl = mcl$. Equating (B6) and (B7),

$$p + \log(1 - 1/\epsilon_Q) + \log \theta_L + a - (1 - \theta_L) l^m = p^c + \log(1 + 1/\epsilon_L) - z/\epsilon_L + l^m/\epsilon_L.$$

Collecting the terms in l^m on one side gives $(1 - \theta_L + 1/\epsilon_L) l^m$ on the right, so

$$l^m = \frac{p - p^c + \log(1 - 1/\epsilon_Q) - \log(1 + 1/\epsilon_L) + \log \theta_L + a + z/\epsilon_L}{1 - \theta_L + 1/\epsilon_L}, \quad (\text{B9})$$

and the monopsony wage follows from the supply schedule (B5),

$$w^m = p^c - z/\epsilon_L + l^m/\epsilon_L. \quad (\text{B10})$$

At this point $mrpl - w^m = mcl - w^m = \log(1 + 1/\epsilon_L) = \log \eta$, so the markdown is positive whenever ϵ_L is finite and vanishes as $\epsilon_L \rightarrow \infty$.

B.3 Efficient benchmark

The efficient (wage-taking) allocation sets $mrpl = ls$, i.e. the firm hires until the marginal revenue product equals the wage on the supply curve. Equating (B6) with ls ,

$$l^e = \frac{p - p^c + \log(1 - 1/\epsilon_Q) + \log \theta_L + a + z/\epsilon_L}{1 - \theta_L + 1/\epsilon_L}, \quad w^e = p^c - z/\epsilon_L + l^e/\epsilon_L. \quad (\text{B11})$$

Subtracting (B9) from (B11), the only difference is the $-\log(1 + 1/\epsilon_L)$ term in the numerator of l^m , so

$$l^e - l^m = \frac{\log(1 + 1/\epsilon_L)}{1 - \theta_L + 1/\epsilon_L} \geq 0, \quad w^e - w^m = \frac{l^e - l^m}{\epsilon_L} \geq 0, \quad (\text{B12})$$

reproducing (7). The monopsonist restricts employment and depresses wages relative to the efficient allocation, and both gaps are increasing in the markdown $\log(1 + 1/\epsilon_L)$ and vanish as $\epsilon_L \rightarrow \infty$.

B.4 Minimum wage: the three regimes

A floor w^* replaces the marginal-cost-of-labor schedule with a horizontal segment at w^* up to the labor-supply curve, beyond which the original mcl resumes. The firm's response depends on where w^* falls relative to w^m and w^e .

Case 1 (demand-constrained), $w^* > w^e > w^m$. When the floor exceeds the efficient wage, the horizontal segment at w^* lies above the $mrpl = ls$ intersection, so employment is pinned down by labor demand: the firm hires where $mrpl = w^*$. Setting (B6) equal to w^* and solving for employment l_1 ,

$$l_1 = \frac{p + \log(1 - 1/\epsilon_Q) + \log \theta_L + a - w^*}{1 - \theta_L}. \quad (\text{B13})$$

Because the firm is on its labor-demand curve, $mrpl = w_1 = w^*$ implies the markdown is exactly one, $\eta = 1$ (log markdown zero). Differentiating (B13) with respect to the floor,

$$\Delta l = \frac{\partial l_1}{\partial w^*} \Delta w^* = -\frac{\Delta w^*}{1 - \theta_L} < 0, \quad (\text{B14})$$

which is (8): employment falls, with the loss governed by the output elasticity of labor θ_L (steeper for θ_L closer to one).

Case 2 (supply-constrained), $w^e > w^* > w^m$. When the floor lies between the monopsony and efficient wages, the firm pays the floor, $w_2 = w^*$, and employment is set by labor supply (B5) evaluated at w^* :

$$l_2 = z - \epsilon_L p^c + \epsilon_L w^*. \quad (\text{B15})$$

Differentiating,

$$\Delta l = \frac{\partial l_2}{\partial w^*} \Delta w^* = \epsilon_L \Delta w^* > 0, \quad (\text{B16})$$

which is (9): employment *rises*, with the gain governed by the labor-supply elasticity ϵ_L . Because the firm moves up its supply curve toward l^e while $mrpl$ now exceeds the wage by less than before, the markdown falls. This holds only while the floor remains below the efficient wage; the boundary of the regime is $w^* = w^e$. Since at the initial monopsony point $mrpl - w^m = \log \eta$, an increase that keeps $w^* < w^e$ is equivalent to

$$\Delta w^* < \log \eta_{(\text{initial})}, \quad (\text{B17})$$

the moderate-increase condition (10). If instead $\Delta w^* > \log \eta_{(\text{initial})}$, the floor crosses w^e , labor demand becomes binding, and employment switches to declining along the $mrpl$ schedule as in Case 1 (equation B14). This is the sense in which firms with markdowns smaller than the hike switch from gaining to losing.

Case 3 (unconstrained), $w^m > w^*$. When the floor lies below the monopsony wage, it does not bind: the firm continues to pay w^m and employ l^m , so $\Delta l = \Delta w = 0$. If the floor is raised enough to enter the interval (w^m, w^e) , the firm transitions into Case 2 and begins to gain employment along the supply curve, $\Delta l = \epsilon_L(w^{**} - w^m)$; if raised beyond w^e , it transitions into Case 1 and loses employment along $mrpl$. This establishes Case 3 of Section 3.5 and its boundary behavior.

B.5 Summary

Across the three regimes the sign of the employment response is $-$, $+$, and 0 respectively, and the governing parameter is θ_L in the demand-constrained case and ϵ_L (together with

the size of the initial markdown) in the supply-constrained case. Holding Δw^* fixed, the regime a firm occupies is determined by its initial markdown position relative to the floor, which is the content of the same-wage prediction of Section 3.5.

C. Additional Tables

Table C1: Firm exit by group and markdown, manufacturing, 2012–2017.

	(1) xm1	(2) xm2	(3) xm3	(4) xm4
<i>Panel A: year $\times$ group (Group 1 omitted)</i>				
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>				
2012	-0.0349** (0.0067)			
2013	-0.0376** (0.0059)			
2014	-0.0299** (0.0066)			
2015	-0.0413** (0.0060)			
2016	-0.0334** (0.0068)			
2017	-0.0376** (0.0061)			
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>				
2012	-0.0122 (0.0165)			
2013	-0.0281* (0.0121)			
2014	-0.0258* (0.0112)			
2015	-0.0128 (0.0149)			
2016	-0.0080 (0.0199)			
2017	-0.0153 (0.0155)			
<i>Panel B: year $\times$ group $\times$ log markdown</i>				
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>				
2012		-0.0481** (0.0081)	-0.0502** (0.0093)	
2013		-0.0304** (0.0075)	-0.0377** (0.0084)	
2014		-0.0481** (0.0096)	-0.0562** (0.0115)	
2015		-0.0584** (0.0085)	-0.0710** (0.0093)	
2016		-0.0493** (0.0105)	-0.0694** (0.0120)	
2017		-0.0576** (0.0099)	-0.0718** (0.0111)	
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>				
2012		0.0153 ⁺ (0.0084)	-0.0074 (0.0074)	
2013		0.0150* (0.0075)	-0.0161* (0.0072)	
2014		0.0373** (0.0093)	0.0122 (0.0079)	
2015		0.0402** (0.0096)	0.0051 (0.0071)	
2016		0.0267* (0.0134)	0.0103 (0.0110)	
2017		0.0322** (0.0112)	0.0164 (0.0100)	
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>				
2012		0.0206 (0.0337)	0.0266 (0.0254)	
2013		0.0166 (0.0299)	0.0053 (0.0218)	
2014		0.0539* (0.0270)	0.0341 (0.0209)	
2015		0.0794 ⁺ (0.0453)	0.0749* (0.0318)	
2016		0.0640 ⁺ (0.0364)	0.0700* (0.0295)	
2017		0.1150** (0.0387)	0.1044** (0.0283)	
Observations	142,971	142,971	142,971	142,971
R ²	0.02178	0.03150	0.02305	0.02963
Within R ²	0.00221	0.01212	0.00350	0.01021

Note: Dependent variable: exit indicator. All columns include firm age, sector $\times$ year, and province $\times$ year fixed effects. In Panel A, Group 1 interactions are the omitted reference. The θ_L interactions from the same regressions are reported in Table C2. Groups are defined as in Table 2. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table C2: Firm exit: labor-elasticity interactions.

	(1)	(2)	(3)	(4)
	xm1	xm2	xm3	xm4
<i>Year × group × θ_L</i>				
<i>Group 1 (minimum wage, $\log \eta \leq 0$)</i>				
2012		0.1816** (0.0177)		0.1866** (0.0177)
2013		0.1990** (0.0159)		0.1993** (0.0153)
2014		0.1886** (0.0181)		0.1826** (0.0177)
2015		0.1961** (0.0190)		0.1933** (0.0184)
2016		0.2178** (0.0159)		0.2330** (0.0161)
2017		0.2196** (0.0155)		0.2297** (0.0168)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>				
2012		0.1838** (0.0259)		0.1594** (0.0224)
2013		0.1690** (0.0227)		0.1580** (0.0204)
2014		0.1784** (0.0239)		0.1745** (0.0225)
2015		0.1530** (0.0294)		0.1429** (0.0244)
2016		0.2229** (0.0317)		0.2133** (0.0259)
2017		0.2505** (0.0298)		0.2337** (0.0261)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>				
2012		0.2916** (0.0820)		0.2599** (0.0641)
2013		0.2344** (0.0690)		0.2122** (0.0500)
2014		0.1934** (0.0638)		0.1851** (0.0473)
2015		0.2460** (0.0863)		0.2501** (0.0589)
2016		0.2729** (0.0951)		0.2957** (0.0807)
2017		0.2496** (0.0901)		0.3351** (0.0655)
Sector × year FE	Yes	Yes	Yes	Yes
Province × year FE	Yes	Yes	Yes	Yes
Firm age FE	Yes	Yes	Yes	Yes
Observations	142,971	142,971	142,971	142,971
R ²	0.02178	0.03150	0.02305	0.02963
Within R ²	0.00221	0.01212	0.00350	0.01021

Note: Continuation of Table C1: coefficients on the year×group× θ_L interactions from the same regressions. Groups are defined as in Table 2. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.

Table C3: Employment growth regressions under alternative production functions.

	(1) Non-neutral translog	(2) Cobb-Douglas	(3) Neutral translog
<i>Panel A: year $\times$ group $\times$ log markdown / Group 1 (minimum wage, $\log \eta \leq 0$)</i>			
2013	-0.0118 (0.0142)	0.0461** (0.0043)	0.0526** (0.0132)
2014	-0.0179 (0.0156)	0.0304** (0.0045)	0.0505** (0.0140)
2015	-0.0148 (0.0154)	0.0394** (0.0044)	0.0407* (0.0159)
2016	-0.0090 (0.0143)	0.0362** (0.0048)	0.0408** (0.0149)
2017	-0.0168 (0.0115)	0.0401** (0.0035)	0.0605** (0.0151)
2018	-0.0509** (0.0123)	0.0341** (0.0038)	0.0159 (0.0115)
2019	-0.0499** (0.0168)	0.0471** (0.0043)	0.0384* (0.0150)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>			
2013	0.1032** (0.0185)	0.0714** (0.0075)	0.0724** (0.0167)
2014	0.1340** (0.0165)	0.0638** (0.0076)	0.1196** (0.0165)
2015	0.1239** (0.0171)	0.0556** (0.0080)	0.0970** (0.0146)
2016	0.1536** (0.0193)	0.0770** (0.0084)	0.1235** (0.0181)
2017	0.1728** (0.0173)	0.0962** (0.0106)	0.1494** (0.0178)
2018	0.1301** (0.0180)	0.0654** (0.0098)	0.0952** (0.0159)
2019	0.1067** (0.0185)	0.0638** (0.0094)	0.0778** (0.0156)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>			
2013	0.0483 (0.0480)	0.0119 (0.0315)	0.0400 (0.0450)
2014	0.1601* (0.0813)	0.0480 (0.0344)	0.1658* (0.0808)
2015	0.2099** (0.0578)	0.0543* (0.0267)	0.1862** (0.0544)
2016	0.1745* (0.0706)	0.0821* (0.0340)	0.1904** (0.0654)
2017	0.0899 (0.0669)	0.0355 (0.0389)	0.1049+ (0.0629)
2018	0.2370** (0.0552)	0.0770** (0.0271)	0.2101** (0.0512)
2019	0.2878** (0.0595)	0.1499** (0.0265)	0.2959** (0.0589)
<i>Panel B: year $\times$ group $\times$ θ_L / Group 1 (minimum wage, $\log \eta \leq 0$)</i>			
2013	-0.2719** (0.0271)	-0.1259 (0.1542)	-0.2441** (0.0273)
2014	-0.1634** (0.0271)	0.0270 (0.2064)	-0.1015** (0.0308)
2015	-0.1924** (0.0244)	-0.1599 (0.1639)	-0.1679** (0.0251)
2016	-0.1959** (0.0283)	0.0764 (0.1777)	-0.1762** (0.0276)
2017	-0.2506** (0.0210)	-0.1490 (0.1504)	-0.1776** (0.0259)
2018	-0.2117** (0.0228)	0.0736 (0.1419)	-0.1610** (0.0230)
2019	-0.2714** (0.0262)	-0.3037+ (0.1645)	-0.2142** (0.0260)
<i>Group 2 (minimum wage, $\log \eta \geq 0.25$)</i>			
2013	-0.3498** (0.0387)	-0.2000 (0.1538)	-0.3314** (0.0378)
2014	-0.2477** (0.0426)	0.0170 (0.2092)	-0.2073** (0.0451)
2015	-0.2489** (0.0418)	-0.1691 (0.1642)	-0.2283** (0.0415)
2016	-0.3734** (0.0434)	-0.0130 (0.1745)	-0.3595** (0.0433)
2017	-0.3988** (0.0540)	-0.2340 (0.1617)	-0.3734** (0.0540)
2018	-0.2640** (0.0513)	0.0069 (0.1533)	-0.2305** (0.0520)
2019	-0.3390** (0.0529)	-0.4105* (0.1639)	-0.3205** (0.0535)
<i>Group 3 (wages $\geq 25\%$ above minimum, $\log \eta > 0$)</i>			
2013	0.0012 (0.1360)	0.1002 (0.1965)	-0.0466 (0.1357)
2014	0.1946 (0.1344)	0.4464+ (0.2303)	0.2398+ (0.1338)
2015	0.4001** (0.1085)	0.5203** (0.1913)	0.4312** (0.1053)
2016	0.2152 (0.1320)	0.5921** (0.2082)	0.1866 (0.1295)
2017	0.1467 (0.1298)	0.2570 (0.2003)	0.1444 (0.1287)
2018	0.1728 (0.1182)	0.5331** (0.1782)	0.2152+ (0.1166)
2019	-0.3924** (0.1100)	-0.3829* (0.1925)	-0.3941** (0.1082)
Observations	169,028	169,028	167,395
(R ² ; Within R ²)	(0.0261; 0.0114)	(0.0266; 0.0120)	(0.0255; 0.0105)

Note: Dependent variable: log employment growth from the fourth quarter of $t-1$ to the fourth quarter of t . Each column re-estimates the baseline specification using the log markdown, the output elasticity of labor, and the group classification implied by the production function named in the header: column (1) is the non-neutral (factor-augmenting) translog baseline, column (2) Cobb-Douglas, column (3) the translog with neutral technical change. All columns include sector $\times$ year and province $\times$ year fixed effects. Groups are defined as in Table 2. Significance: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. Standard errors, two-way clustered by NACE 3-digit sector and province, in parentheses.