Wage-Setting Power and Firms' Adjustment Margins to Minimum Wages*

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Abstract

This paper studies how firms with varying wage-setting power adjust to the introduction of the German minimum wage in 2015 across a host of adjustment margins. Using administrative panel data on manufacturing firms and estimates of firm-level wage markdowns, we find that firms without wage-setting power reduce employment, increase productivity, and raise prices. By contrast, employment and price responses are muted for firms with wage-setting power which instead adjust through output expansion and increased use of intermediate inputs. Among the latter, firms with moderate exposure to the minimum wage even expand employment and output, consistent with the standard monopsony model.

Keywords: minimum wage, labor market power, firm adjustment margins

JEL classification: J38, J42, J23, J31

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1 Introduction

A growing literature argues that firms' wage-setting power shapes how they absorb and transmit labor cost shocks such as minimum wage increases (Manning 2021; Azar and Marinescu 2024; Kline 2025). In particular, firms with different wage-setting power may adjust to higher labor costs through fundamentally different margins, including employment, prices, output, productivity, input substitution, and profits. Understanding this heterogeneity in firms' adjustment behavior is crucial for evaluating the effects of labor market policies including minimum wages.

Against this background, this paper studies how firms' wage-setting power shaped their adjustment to the introduction of the German minimum wage in 2015.[1] To measure firms' wage-setting power, we estimate the wage markdown (i.e., the gap between the marginal revenue product of labor and firms' wages) at the level of the individual firm using the production-function approach pioneered by Dobbelaere and Mairesse (2013) and more recently brought to prominence by Yeh et al. (2022). We then examine how firms with different wage-setting power adjusted to the new minimum wage along several margins, including employment, prices, output, productivity, input use, operating surplus, and firm exit. Our analysis relies on high-quality administrative panel data on German manufacturing firms from the *Amtliche Firmendaten für Deutschland* (AFiD), covering the years 1999–2017.

We find substantial heterogeneity in firms' adjustment to the minimum wage depending on their wage-setting power. Firms that lack wage-setting power reduce employment and raise output prices, which is in line with partial passthrough of higher labor costs to consumers. For these firms, we further observe increases in productivity. By contrast, employment and price responses are significantly muted for firms with wage-setting power, even though they face similar wage increases. Instead, these firms adjust by expanding output and raising intermediate input use and show reduced operating surplus. Moreover, firms with wage-setting power that are moderately exposed to the minimum wage even expand employment and output, in line with the non-monotonic predictions of the textbook monopsony model. Our findings thus point at important heterogeneity in how firms absorb and transmit labor-cost shocks depending on their wage-setting power and are consistent with standard models of the labor market.

Our paper contributes to several strands of the literature. First, we contribute to the general literature on the effects of minimum wages. Monopsonistic wage setting has long been proposed as an explanation for the modest employment effects of minimum wages (e.g., Card and Krueger 1994; Neumark and Wascher 2002; Manning 2021; Dube and Lindner 2024). Recent empirical work indeed finds less

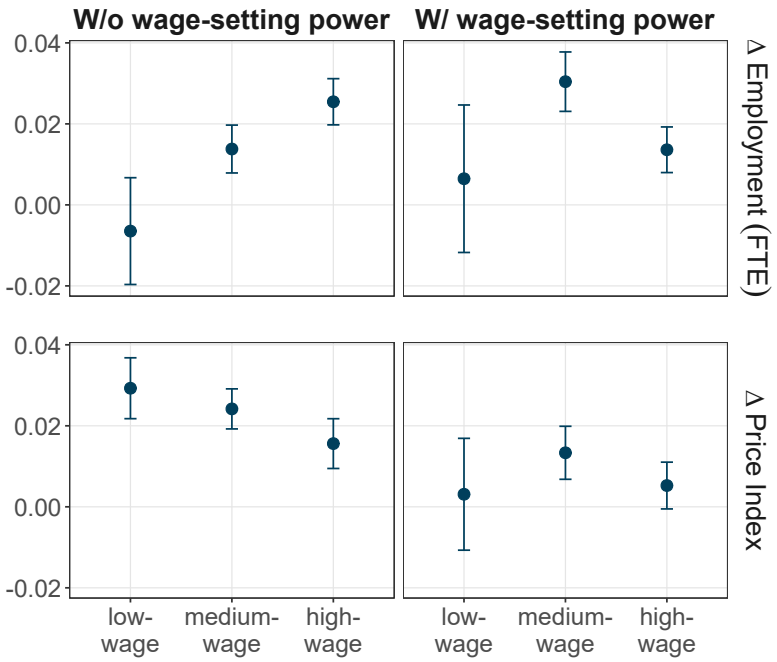
[1] For details on the introduction of German minimum wage, see Appendix B.

adverse employment effects in more concentrated or monopsonistic labor markets (Okudaira et al. 2019; Wiltshire 2023; Azar et al. 2024). That said, evidence on other outcomes than employment is lacking. Our contribution to this literature is to broaden the analysis to additional outcomes and to show that firms with different wage-setting power respond to labor-cost shocks through fundamentally different adjustment margins.

Second, we contribute to the growing literature on the passthrough of minimum wages to consumer prices. Although monopsonistic wage setting is frequently invoked as an explanation for nonnegative employment effects of minimum wages, a robust finding is substantial price passthrough of minimum wage increases (e.g., Harasztosi and Lindner 2019; Ashenfelter and Jurajda 2022; Renkin et al. 2022; Hälbig et al. 2023; Link 2024). As stressed by Kline (2025), these findings create a puzzle for standard monopsony models: If minimum wages increase employment and lead to growing output, then prices are expected to fall rather than rise. As possible explanations, Kline discusses sticky prices, changing product quality, and firm heterogeneity in exposure to the minimum wage and demand conditions. We contribute to resolving this puzzle by showing that price passthrough differs by firms' wage-setting power. Firms that lack wage-setting power not only reduce employment but also raise prices following the minimum wage introduction, whereas price passthrough is significantly muted for firms with wage-setting power, even though they face similar wage increases.

These patterns are already evident from the descriptive evidence presented in Figure 1. The figure shows the average firm-level changes in employment (in full-time equivalents, FTE) and prices between 2013 and 2015 for exposed low- and medium-wage firms and the high-wage control group, separately for firms with and without wage-setting power. For firms without wage-setting power, employment growth is lower and price growth is higher the more they are exposed to the minimum wage. By contrast, prices change little for firms with wage-setting power. Employment growth is highest for medium-wage firms and is similar among low- and high-wage firms, which is consistent with the non-monotonic employment responses predicted by the standard monopsony model. Through the lens of this model, highly exposed low-wage firms are more likely to face a minimum wage that exceeds their marginal productivity and thus to reduce employment whereas moderately exposed medium-wage firms are more prone to expand employment along the labor supply curve.

Third, we contribute to the recent, evolving literature on the specific minimum wage effects in imperfect labor markets by investigating heterogeneous firm responses to minimum wages based on firm-level estimates of wage markdowns as a direct firm-level measure of their wage-setting power. In contrast to our approach, studies in this strand of the literature rely on measures of employer concentration



Notes: The figure reports average unconditional firm-level changes in employment (in FTE) and the price index between 2013 and 2015, together with 95% confidence intervals, of low-, medium-, and high-wage firms separately for firms with (i.e., $\bar{\gamma}_i > 1$) and without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Wage categories: $[min; €25{,}000)$, $[€25{,}000; €40{,}000)$, $[€40{,}000; max]$. N = 10,247 firms.

Figure 1: Average Firm-Level Employment and Price Changes between 2013 and 2015 for Low-, Medium-, and High-Wage Firms by Firms' Wage-Setting Power.

in local labor markets as a proxy of firms' wage-setting power (e.g., Azar et al. 2024; Popp 2024).[2] These concentration measures, however, (i) only capture one source of monopsony power, i.e., granularity in local labor markets; (ii) just measure firms' potential (rather than their actual use of their) wage-setting power; (iii) face the manifold challenges of correct market delineation; and (iv) are likely endogenous because employment shares and wages are simultaneously determined (see also the recent surveys by Azar and Marinescu 2024; Kline 2025).

To avoid these issues, we rely on firm-level estimates of wage markdowns using the production-function approach that has been used to study firms' wage-setting power in many contexts (e.g., Dobbelaere and Mairesse 2013; Dobbelaere and Kiyota 2018; Mertens 2020, 2022; Yeh et al. 2022; Dobbelaere et al. 2024). This approach not only provides us with a direct measure of firms' actual use of (rather than their potential for) wage-setting power, but also allows us to remain agnostic about the relevant labor market and the source of their wage-setting power, which may arise

[2] One notable exception is the study by Okudaira et al. (2019) that also relies on estimates of wage markdowns to measure firms' wage-setting power. That said, Okudaira et al. aggregate estimated output elasticities and cost shares to the regional level (by using median input usage and cost shares at the prefecture level) and thus do not base their analysis on varying wage-setting power at the level of the individual firm.

from search frictions, worker preferences, bargaining, or several other labor market imperfections. In short, this approach enables us to directly link firms' responses to the minimum wage to differences in their realized wage-setting power. Although our high-quality data, which include firm-level product prices, allow us to address several important pitfalls in the estimation of production functions, remaining biases would admittedly affect our measure of wage-setting power.

The remainder of the paper is structured as follows. Section 2 describes the data and sample construction and outlines the empirical strategy and the estimation of firm-level wage markdowns. Section 3 presents our baseline results for highly exposed firms. Section 4 presents additional results for moderately exposed firms and discusses robustness checks, and Section 5 concludes.

2 Data and Empirical Framework

2.1 Administrative firm data

Our analysis combines several administrative datasets provided by the German Statistical Offices that we briefly describe here and in more detail in Appendix C. Firms are legally required to report the underlying information, so the data offer comprehensive and high-quality coverage of the German manufacturing sector.

Our main analysis is based on yearly panel production data on German manufacturing firms with at least 20 employees that we use in our production function estimation and construction of wage markdowns at the level of the individual firm. We combine these firm-level production data with PRODCOM data containing establishment $\times$ ten-digit product-level information on produced quantities and factory-gate sales. A key advantage of these data is that they allow us to construct firm-specific output price indices following Eslava et al. (2004) and described in detail in Appendix G. Doing so enables us to separate price variation from changes in physical output and to construct firm-level quasi-quantities (by dividing sales by the firm-specific price index). Consequently, we can avoid bias from not having separate price and quantity information in the estimation of output elasticities, which is at the heart of the Bond et al. (2021) critique, and use this information to study price and output adjustments to the minimum wage by firms' wage-setting power.

We additionally use the 2014 German Structure of Earnings Survey (*AFiD-Modul Verdienste*, VSE) to validate our treatment classification using worker-level wage data and annual Business Register data to identify firm exits and construct alternative measures of local labor market concentration used in robustness checks.

Our baseline estimation sample combines the firm-level production data with the firm-product-level data. Several key variables required for our analysis, however, are

collected for a sample of firms that rotates every four years. We therefore restrict the sample to firms that we continuously observe between 2012 and 2015 (i.e., between the redraws in 2012 and 2016). Doing so ensures consistent firm coverage over the full baseline period and allows us to follow firms continuously before and after the introduction of the minimum wage. We further exclude industries temporarily exempt from the statutory minimum wage.[3] Our final baseline sample comprises 10,247 manufacturing firms per year.

2.2 Production Function Approach to Wage-Setting Power

Measuring wage markdowns at firm level. To estimate firm-year-level wage markdowns that provide a direct firm-level measure of realized wage-setting power, we adopt the production-function approach (Dobbelaere and Mairesse 2013; Yeh et al. 2022). Assuming that firms can flexibly adjust intermediate inputs at exogenous prices, the first-order conditions for maximizing short-run profits imply the following expression for the wage markdown γ_{it} of firm i in year t:

$$\gamma_{it} = \frac{MRPL_{it}}{P^L_{it}} = \frac{\theta^L_{it}}{\theta^M_{it}} \frac{P^M_{it} M_{it}}{P^L_{it} L_{it}} \tag{1}$$

where $MRPL_{it}$ denotes the marginal revenue product of labor and P^X_{it} and θ^X_{it}, respectively, denote the input price and output elasticity of either labor ($X = L$) or intermediates ($X = M$). The wage markdown γ_{it} directly measures the ratio of the firm's marginal revenue product of labor to its wages and can therefore take values below or above unity. It may thus reflect a variety of factors that drive a wedge between the two, including monopsony power, worker bargaining power, adjustment costs, or other frictions.

Key to our analysis is whether firms' responses to the minimum wage vary with their wage markdowns that capture their wage-setting power. In this analysis, a natural distinction is between firms that pay wages below the marginal revenue product of labor (i.e., $\gamma_{it} > 1$) and firms that do not (i.e., $\gamma_{it} \leq 1$). In marking down their wages below marginal productivity, the former firms clearly exercise unilateral wage-setting power. Firms paying marginal-product or even higher wages, by contrast, do not exhibit such unilateral wage-setting power. As a shorthand, we refer to these two groups as firms with and without wage-setting power, respectively.

[3] Temporary exemptions apply to the following NACE Rev. 2 industries: processing and preserving of meat and production of meat products (101); manufacture of textiles (13); manufacture of wearing apparel (14).

Recovering output elasticities. To compute γ_{it} from Equation (1), we need information on input expenditures (directly observed in the data) and output elasticities. We recover firms' output elasticities (θ_{it}^X) by estimating industry-specific production functions (i.e., at the level of NACE Rev. 2 two-digit industries). Estimation closely follows Mertens (2022), who estimates production functions with the same data in a different context. We briefly summarize the estimation procedure here and relegate details to Appendix E.1. It assumes a translog production function that allows output elasticities to vary both across firms and over time and reads:

$$\begin{aligned} q_{it} = {} & \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \beta_{ll} l_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{mm} m_{it}^2 \\ & + \beta_{lk} l_{it} k_{it} + \beta_{lm} l_{it} m_{it} + \beta_{km} k_{it} m_{it} + \beta_{lkm} l_{it} k_{it} m_{it} + \omega_{it} + \epsilon_{it} \end{aligned} \tag{2}$$

In Equation (2), lower case letters refer to variables in logs; q_{it} denotes the log quasi-quantity, which we obtain from deflating log firm sales by a firm-specific price index that we derive from firm-product-level price information and which eliminates firm-specific price variation in firm output up to a constant (see Appendix G for details on the price index construction); k_{it} denotes the log capital stock (see Appendix F for details on the capital stock construction); ω_{it} denotes quantity-based total factor productivity; and ϵ_{it} is an idiosyncratic error term.

We estimate the parameters in Equation (2) using a control function approach similar to Wooldridge (2009). Crucially, with our data on output prices, we can account for the price bias in production function estimation that stems from (typically unobserved) firm-specific input and output price variation by implementing a firm-level adaptation of the procedure by De Loecker et al. (2016).[4] This adaptation involves two steps: First, as detailed above, we purge firm sales of output price variation using firm-specific output prices. Second, we follow De Loecker et al. (2016) and construct an input price control function from product prices, market shares, and firm location and industry dummies. The intuition behind this procedure is that high-quality products (i.e., products with high prices and market shares) require high-quality inputs. After estimation of the production function (2), we compute output elasticities as $\theta_{it}^X = \frac{\partial q_{it}}{\partial x_{it}}$.

Production function results. Appendix Table E1 provides descriptive statistics for the 243,628 estimated output elasticities and implied wage markdowns of 39,452 firms belonging to 20 two-digit industries. Average output elasticities for labor, intermediates, and capital are 0.3, 0.64, and 0.1, respectively. Firms pay on average

[4] For detailed discussion on the price bias, see De Loecker et al. (2016) and Bond et al. (2021). Note that the output price bias does not affect the ratio yielding γ_{it} in Equation (1) as it cancels out, whereas the input price bias still affects γ_{it}. That said, both biases matter for computing total factor productivity (as described further below).

wages about 5% below the marginal revenue product of labor, and the 10th and 90th percentiles of γ_{it} are 0.48 and 1.74, respectively, and thus point at substantial variation in firms' wage-setting power.[5] In what follows, we average the wage-setting power measure over the pre-treatment years 2012–2014 to reduce noise and denote this average by $\bar{\gamma}_i$.[6]

Total factor productivity. In our analysis, we study firm responses to minimum wages in quantity-based (TFPQ) and revenue-based (TFPR) total factor productivity. We can directly compute TFPQ from the production function estimates as $\hat{\omega}_{it} = q_{it} - \boldsymbol{\phi}'_{it}\hat{\boldsymbol{\beta}}$, where $\boldsymbol{\phi}_{it}$ denotes the vector of all production factors and their interactions in Equation (2). TFPR, in turn, we obtain using the log of the firm-specific price index p_{it} as $\hat{\omega}_{it} + p_{it}$.

2.3 Wage Markdowns vs. Labor Market Concentration

Appendix Figure A1 relates firms' wage markdowns to an alternative proxy for their wage-setting power: local labor market concentration, measured by the Herfindahl–Hirschman Index (HHI) based on employment shares within industry × region cells. Wage markdowns are positively associated with concentration, implying that firms operating in more concentrated labor markets tend to exhibit greater wage-setting power on average. However, and in line with Yeh et al. (2022), the relationship is weak (correlation coefficient: 0.076) and displays substantial dispersion, with wage markdowns varying widely even within narrowly defined concentration ranges.

This pattern is consistent with recent work emphasizing that, whereas concentration measures capture one source of monopsony and therefore provide an indirect proxy for firm monopsony power, wage markdowns provide a more direct and conceptually preferred measure of firm monopsony power in that they quantify the equilibrium wedge between the marginal revenue product of labor and wages (Azar and Marinescu 2024; Kline 2025). The substantial within-market heterogeneity further suggests that firms may exercise wage-setting power even in unconcentrated labor markets due to other factors than granularity, including search frictions, worker preferences, or imperfect information.

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[5] Given that collective wage bargaining and employee codetermination through works councils are common in Germany, it is not surprising to find that a meaningful share of firms pay above marginal-product wages that may reflect rent sharing. See also the detailed discussion in Dobbelaere et al. (2024).

[6] To measure firms' wage-setting power, we ignore post-reform wage markdowns because these may have been affected by the reform, a possibility that Faia et al. (2026) investigate in detail.

2.4 Treatment Definition

Treatment measure. Following Draca et al. (2011) and Hau et al. (2020), we use firms' average yearly wage bill per FTE, averaged over the pre-treatment years 2012–2014, to determine firms' exposure to the minimum wage. We distinguish between three wage groups:

$$\underbrace{[min; €25{,}000)}_{low} \qquad \underbrace{[€25{,}000; €40{,}000)}_{medium} \qquad \underbrace{[€40{,}000; max]}_{high}$$

Low-wage firms with average annual wages below €25,000 are classified as highly exposed. Since minimum wage spillovers may extend beyond directly affected workers (Dustmann et al. 2022), *medium*-wage firms with average annual wages between €25,000 and €40,000 are classified as moderately exposed. Finally, *high*-wage firms with average annual wages of at least €40,000 constitute the control group. About 11% of the firms in our data belong to the low-wage group, 44% to the medium-wage group, and the remaining 45% to the high-wage group.

Appendix Table A1 reports pre-reform firm characteristics by wage group. Low-wage firms are smaller, less productive, and less capital intensive than high-wage firms. They also employ much larger shares of part-time and female workers and are substantially less likely to export. Interestingly, low-wage firms exhibit lower wage markdowns on average: 32% of low-wage firms pay wages below workers' marginal revenue product *vis-à-vis* 53% of high-wage firms.

Treatment validation. The validity of our treatment classification hinges on whether firms that pay lower wages on average exhibit greater exposure to the minimum wage. We assess this question in Appendix D using worker-level wage information from the 2014 wave of the Structure of Earnings Survey (VSE), which can be matched to a subset of firms in our firm-level data.[7] The validation exercise confirms that minimum wage exposure is strongly concentrated among firms in the low-wage group, whereas exposure is close to zero among firms in the high-wage group. The appendix further shows that our treatment classification closely tracks firms' implied wage bill increases under full compliance with the statutory minimum wage.

[7] The VSE records wages as of April 2014 and thus about three months before the minimum wage legislation passed parliament and eight months before the policy came into effect. Anticipation effects are therefore unlikely to materialize in the wage distribution observed in these data. For further institutional background, refer to Appendix B.

2.5 Difference-in-Differences Design

To estimate how firms' adjustment to the minimum wage varies with their pre-reform wage-setting power, we rely on a triple-differences design at the firm–year level. Our main specification reads:

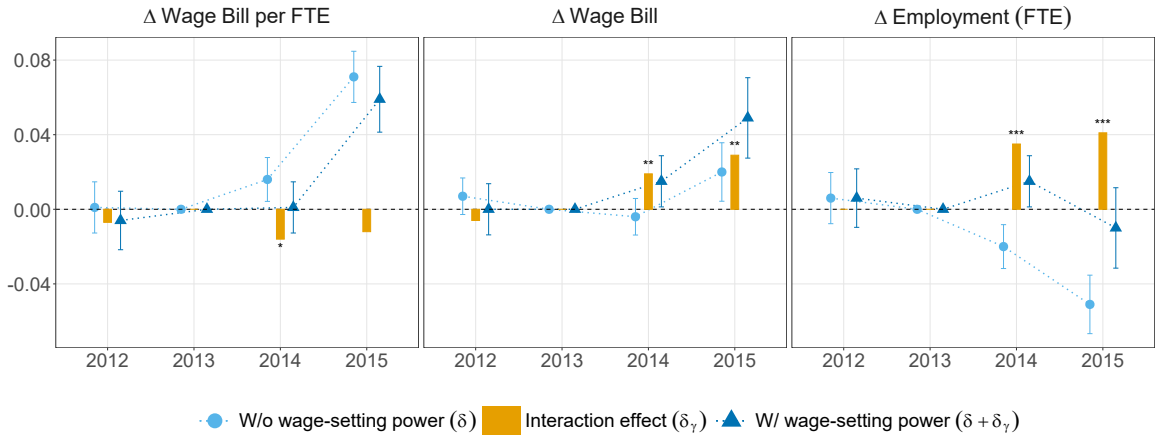
$$\Delta y_{it} = \lambda_t + \mathbf{T}_i'\boldsymbol{\delta} + M_i\mathbf{T}_i'\boldsymbol{\delta}_\gamma + \delta_M M_i + \mathbf{C}_{it}'\boldsymbol{\beta} + \varepsilon_{it} \tag{3}$$

where Δy_{it} denotes the change in the respective outcome variable relative to 2013, λ_t is a fixed year effect, $\mathbf{T}_i$ are two dummy variables indicating low- or medium-wage firms (i.e., firms' exposure to the minimum wage), M_i is a dummy variable indicating firms with wage-setting power (i.e., firms with $\gamma_i > 1$ that mark down wages below the marginal revenue product of labor), $\mathbf{C}_{it}$ denotes a vector of controls, and ε_{it} is an error term. The coefficient vector $\boldsymbol{\delta}$ captures the differential change in the outcome for treated firms without wage-setting power relative to untreated high-wage firms without wage-setting power, which form the left-out reference group. The interaction effects $\boldsymbol{\delta}_\gamma$ give the difference in firm adjustment when comparing treated firms with wage-setting power to treated firms without. As controls we include in $\mathbf{C}_{it}$ state and two-digit industry dummies. We use 2013 as the base year to rule out anticipation effects that may have led firms to adjust already in the pre-reform year 2014.

As an alternative to the baseline specification (3), we also estimate a specification that includes a continuous measure of firms' wage-setting power. In this alternative specification, we substitute $\log \gamma_i$ for the dummy for below-marginal-product wages M_i. In this specification, $\boldsymbol{\delta}$ gives the differential change in the outcome between treated firms that pay marginal-product wages (i.e., with $\gamma_i = 1$ and thus $\log \gamma_i = 0$) and untreated high-wage firms with such wages, which form the reference group. The interaction effects $\boldsymbol{\delta}_\gamma$, in turn, provide the difference in the treatment effect between firms whose wage markdown differs by 100 log points.

3 Results for Low-Wage Firms as the Treatment Group

Wages and employment. Figure 2 reports estimates of the average minimum wage effect on the wage bill per FTE, the total wage bill, and employment in FTE, separately for firms with and without (pre-reform) wage-setting power ($\bar{\gamma}_i \gtreqless 1$), comparing highly exposed low-wage firm to the control group of high-wage firms. The minimum wage significantly increases wage costs among low-wage manufacturing firms, indicating that the policy is binding. Relative to 2013 and the control group, wages per FTE rise by 7.1% on average for firms with wage-setting power and by 5.9% for firms without. This difference between groups is not statistically significant.



Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on the wage bill per FTE, the total wage bill, and employment (in FTE), and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure 2: Wage and Employment Effects for Low-Wage Firms by Wage-Setting Power.

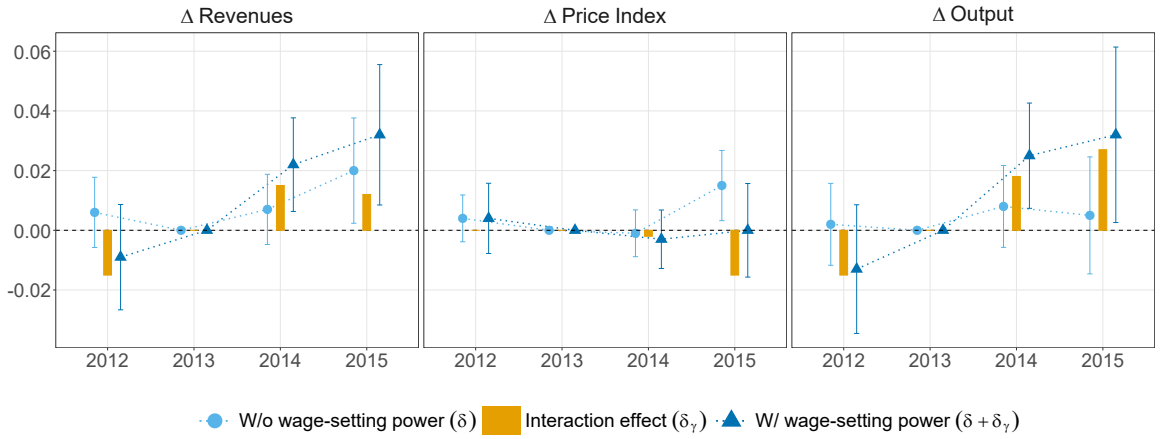
Employment declines by 5.1% on average among low-wage firms that lack wage-setting power, which corresponds to an own-wage elasticity (OWE) of labor demand of −0.72.[8] This statistically significant estimate is somewhat larger in absolute value than the OWE of about −0.5 reported by Azar et al. (2024) for unconcentrated labor markets (i.e., HHI < 0.25). It also lies near the upper end of estimates summarized by Dube and Zipperer (2024), though more negative elasticities are commonly found for manufacturing industries (e.g., Harasztosi and Lindner 2019).

By contrast, the employment effect for firms with wage-setting power is small (−1%) and statistically insignificant and corresponds to an OWE of just −0.17. This estimate is close to the near-zero elasticities often reported in the broader minimum wage literature and consistent with the view that firms' monopsony power mutes the employment response to the minimum wage. It is also statistically significantly different from the estimate for firms without wage-setting power. More generally, the substantial effect heterogeneity between these two groups of firms suggests that aggregate minimum wage effects may mask variation in firms' labor market power.

The total wage bill increases on average by 4.9% and 2.0% for firms with and without wage-setting power, respectively, implying that wage gains more than offset employment losses. Estimates for 2012 provide no evidence of differential pre-trends across groups.

Output and prices. In competitive labor markets, the introduction of a binding minimum wage increases marginal costs and thus is expected to reduce employment and output, which, in turn, leads to higher prices, thereby generating positive price

[8] Following Dube and Zipperer (2024), we compute the OWE as the employment effect divided by the wage effect.



Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on revenues, the firm-level price index, and output (quasi-quantities measured as revenue deflated by the firm-level price index), and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure 3: Output and Price Effects for Low-Wage Firms by Wage-Setting Power.

passthrough. Consistent with this prediction, one of the most robust findings in the minimum wage literature is that minimum wages increase consumer prices, with many studies documenting substantial or even full price passthrough (see, e.g., the survey by Dube and Lindner 2024). At the same time, however, estimated employment effects are often small or statistically insignificant, which sits uneasily with the competitive model. As stressed by Aaronson and French (2007), Aaronson et al. (2008), and Kline (2025), these two findings create no less tension for standard monopsony models. In monopsonistic labor markets, a minimum wage may raise employment by inducing firms to expand hiring and production (e.g., Bhaskar et al. 2002; Manning 2003), which should put downward pressure on prices and is therefore difficult to reconcile with the large positive price responses commonly observed in the data.

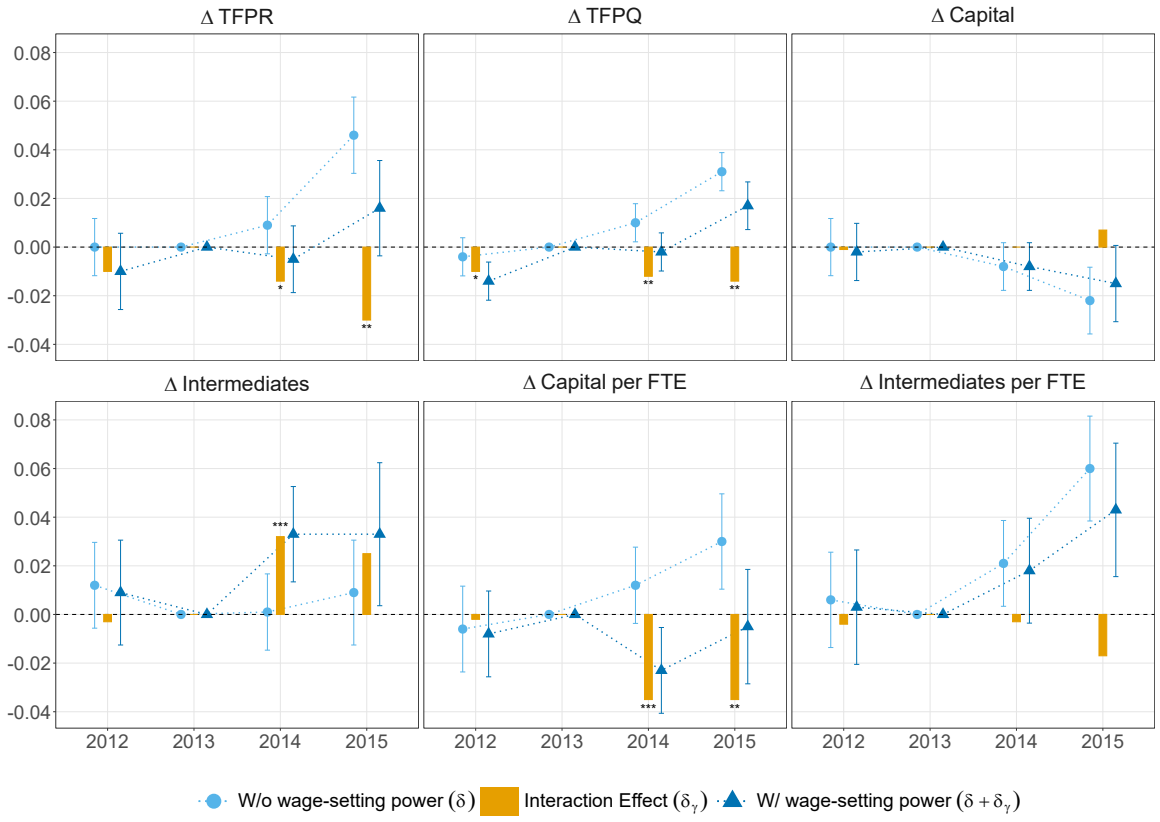
Our results suggest that at least part of this apparent puzzle reflects effect heterogeneity across firms with varying wage-setting power. Figure 3 shows our results for price and (quasi-)quantity adjustments. In line with standard monopsony models, we find that the minimum wage increases output among low-wage firms with wage-setting power. These firms' revenues increase by 3.2% on average, which is entirely driven by higher quantities rather than higher prices (price changes are small and statistically insignificant for this group). By contrast, firms without wage-setting power also experience revenue increases (+2.0%), but these are driven by higher output prices (+1.5%) instead of rising output.

Overall, our estimated employment and price responses reveal striking differences in the adjustment of firms with varying wage-setting power. After the introduction of the minimum wage, firms without wage-setting power exhibit employ-

ment declines alongside substantial price passthrough. By contrast, employment and price responses are muted for firms with wage-setting power. Together, these findings suggest that aggregate price passthrough conceals marked heterogeneity in firms' responses to minimum wage shocks depending on their wage-setting power.

Productivity and input substitution. After the minimum wage introduction, firms without wage-setting power reduce labor and capital inputs, while intermediate inputs remain largely unchanged (see Figure 4). Together with broadly constant output, these findings suggest that firms without wage-setting power maintain production with lower overall input use. In line with this interpretation, these firms' TFPR and TFPQ rise substantially, which is suggestive of efficiency improvements.

By contrast, firms with wage-setting power exhibit a completely different adjustment pattern. These firms expand output while increasing intermediate inputs, whereas employment and capital inputs change little. Productivity responses are modest, suggesting that these firms primarily adjust through scale expansion rather than efficiency improvements.



Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on TFPR, TFPQ, capital inputs, intermediate inputs, capital inputs per FTE, and intermediate inputs per FTE and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

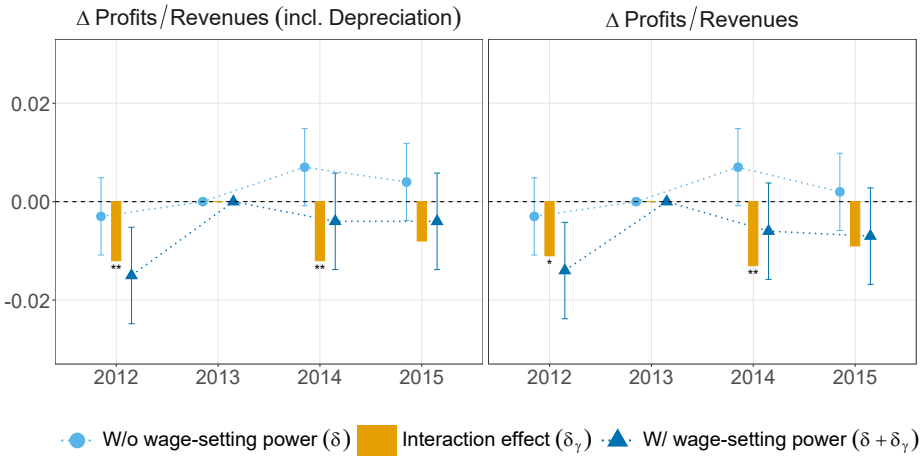
Figure 4: Productivity and Input Effects for Low-Wage Firms by Wage-Setting Power.

Taken together, our results suggest that firms without wage-setting power respond to higher labor costs primarily through productivity improvements and price passthrough, whereas firms with wage-setting power adjust more strongly through quantity expansion and input scaling.

Profitability. The effects on profitability, measured as gross operating profits (i.e., value added minus labor costs) relative to revenues, lend further support to our interpretation of these differing adjustment patterns. Among firms without wage-setting power, profits relative to revenues do not fall after the introduction of the minimum wage despite higher labor costs (see Figure 5). Together with the observed increases in productivity and output prices, this finding suggests that these firms absorb a substantial share of the cost shock through efficiency gains and price passthrough.

By contrast, firms with wage-setting power show a slight decline in profitability, consistent with partial internal absorption of the labor-cost shock. Although the effect heterogeneity between the two groups is statistically significant, the negative estimate for firms with wage-setting power is imprecise; the event plot also reveals some pre-trends in profitability for these firms. Overall, the evidence provides tentative support for the idea that firms with wage-setting power absorb a larger share of the minimum wage shock through lower surplus rather than price passthrough.

Our findings are broadly consistent with several studies documenting reductions in firm profits following minimum wage increases (e.g., Draca et al. 2011; Mayneris et al. 2018; Harasztosi and Lindner 2019), although the estimated effects are often modest and overall the evidence remains mixed (see the discussion in Dube and



Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on gross operating profits over revenues including or excluding depreciation and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure 5: Profitability Effects for Low-Wage Firms by Wage-Setting Power.

Lindner 2024). Our results suggest that this mixed picture may in part reflect heterogeneous adjustment among firms with varying wage-setting power. Firms without wage-setting power seem to be able to stabilize profitability through productivity improvements and price passthrough, whereas we find little evidence of such offsetting adjustments among firms with wage-setting power. These firms may therefore absorb a larger share of the labor-cost shock internally.

4 Additional Results and Robustness

Results for medium-wage firms as the treatment group. Compared to the group of low-wage firms, medium-wage firms are less clearly exposed to the minimum wage (see Appendix Figure D1) and likely comprise affected and unaffected firms. That said, examining this group offers the opportunity of assessing the non-monotonic predictions of the standard monopsony model, in particular that moderate minimum wages may raise employment whereas high minimum wages will eventually reverse these gains.

Appendix Figure A2 reports event-study estimates with medium-wage firms as the treatment and high-wage firms as the control group. Compared to the low-wage treatment group, wage responses are substantially attenuated, which is consistent with weaker treatment intensity. Despite these minor wage effects, employment responses again differ significantly by wage-setting power. Medium-wage firms with wage-setting power increase employment by 1.7%, whereas firms without wage-setting power reduce employment by 1.5%, albeit less strongly than low-wage firms without wage-setting power. In line with this adjustment pattern, medium-wage firms with wage-setting power expand output and increase intermediate input use, while capital inputs change little, indicating adjustment along the scale margin rather than through capital–labor substitution. At the same time, we find little evidence of price passthrough or productivity improvements among these firms. This combination of higher employment, output expansion, and stable prices closely echoes the predictions of standard monopsony models.

Continuous measure of firms' wage-setting power. As a robustness check, we re-estimate Equation (3) using a continuous measure of wage-setting power, replacing the binary indicator for firms paying below marginal-product wages with $\log \bar{\gamma}_i$. This specification allows treatment effects to vary smoothly with firms' wage-setting power rather than imposing a discrete threshold at $\bar{\gamma}_i = 1$.

Appendix Figure A3 reports effects on all firm outcomes considered so far, evaluated at $\bar{\gamma}_i = 1$ and $\bar{\gamma}_i = 1.5$. The results closely mirror the baseline findings and suggest that heterogeneity in firms' adjustments varies continuously with their wage-

setting power. After the introduction of the minimum wage, firms without wage-setting power exhibit substantial increases in wage costs, significant employment reductions, and large price passthrough. For firms with considerable wage-setting power, employment responses and price passthrough are substantially muted, even though they face similar wage increases. Productivity responses (TFPR and TFPQ) are much more pronounced among firms without wage-setting power, whereas firms with considerable wage-setting power exhibit larger increases in output and intermediate inputs. At the same time, we find little evidence of capital deepening or capital–labor substitution for these firms. Overall, the results suggest that firms' responses to the minimum wage vary smoothly with their wage-setting power so that the observed effect heterogeneity is unlikely to arise solely from differences between monopsonistic and non-monopsonistic firms.

Firm size. Low-wage firms are much smaller than high-wage firms (see Appendix Table A1), raising the concern that the observed effect heterogeneity reflects differences in firm size rather than wage-setting power. To address this concern, we re-estimate Equation (3) restricting the sample to small firms with fewer than 50 and fewer than 100 employees in the base year, respectively (see Appendix Figures A4 and A5).

Doing so leaves the qualitative heterogeneity patterns in employment, productivity, quantity, and input adjustments across firms with varying wage-setting power intact. By contrast, price effects become substantially weaker and statistically insignificant once we restrict the sample to small firms. This finding suggests that price passthrough is concentrated among larger firms that may be in a better position to increase prices in response to higher labor costs.

Obtaining firms' wage markdowns from revenue elasticities. To measure firms' wage-setting power, we have so far relied on the wage markdown from Equation (1), which assumes exogenous intermediate input prices. This assumption is problematic if firms possess market power in intermediate input markets. As a robustness check, we therefore follow Hashemi et al. (2022) and Treuren (2025), who derive wage markdowns that are robust to such market power from revenue rather than output elasticities as:

$$\gamma_{it}^{rev} = \frac{\tilde{\theta}_{it}^{L}}{\frac{P_{it}^{L} L_{it}}{P_{it}^{Q} Q_{it}}} \tag{4}$$

where $\tilde{\theta}_{it}^{L} = \left(1 + \frac{\partial P_{it}^{Q}}{\partial Q_{it}} \frac{Q_{it}}{P_{it}^{Q}}\right) \theta_{it}^{L}$ denotes the revenue elasticity of labor and $\frac{P_{it}^{L} L_{it}}{P_{it}^{Q} Q_{it}}$ is the labor share in sales. A drawback of this approach is that it does not permit computation of TFPQ, which therefore cannot be analyzed as an outcome. Appendix E.2

describes the estimation procedure in detail.

Appendix Table E2 reports the 240,496 estimated revenue elasticities and implied wage markdowns for 38,949 firms belonging to 20 two-digit industries. The wage markdowns are somewhat smaller than those obtained from output elasticities. On average, firms pay wages that are 8% above the marginal revenue product of labor, compared to the baseline specification where wages are 5% below the marginal revenue product. This pattern is consistent with firms exerting some market power in intermediate input markets, although we cannot rule out alternative explanations related to measurement and estimation of the production function. That said, Appendix Figure E1 documents that the two wage markdowns are very strongly correlated. Only in the upper tail of the markdown distribution the correlation is weaker.

Appendix Figures A6 and A7 reproduce the main event-study results using the wage markdowns computed from revenue elasticities. The results closely mirror the baseline findings, which suggests that our findings are not sensitive to the precise construction of the wage markdown measure and lends further support to the view that firms' adjustment to minimum wage shocks varies fundamentally with their wage-setting power.

Firm exit and sample selectivity. One concern is that the observed effect heterogeneity reflects selective firm exit rather than differential adjustment among firms. This concern is particularly relevant because our main estimation relies on a balanced panel of manufacturing firms. To assess whether selective attrition can drive our findings, we examine whether firm exit following the introduction of the minimum wage varies with firms' minimum wage exposure and their wage-setting power.

Appendix Table A2 reports estimates of Equation (3) using an indicator for firm exit as the dependent variable. Reassuringly, we find no evidence that minimum wage exposure significantly increases the probability of firm exit, nor do we find differential exit patterns across firms with varying wage-setting power. The estimates are small and statistically insignificant throughout. These results suggest that our baseline findings are unlikely to be driven by selective firm survival.

5 Conclusions

This paper has studied how firms' adjustment to the introduction of the German minimum wage in 2015 varied with their wage-setting power. Using administrative production and product-level data for German manufacturing firms, we estimated firm-level wage markdowns and investigated how firms' responses across multiple adjustment margins differ by their ex-ante wage-setting power.

We documented substantial heterogeneity in firms' adjustment behavior. Firms without wage-setting power respond to the minimum wage by reducing employment, increasing productivity, and raising output prices. They maintain output despite lower labor and capital inputs, consistent with productivity enhancements and partial passthrough of higher labor costs to consumers. By contrast, employment responses and price passthrough are substantially muted among firms with wage-setting power, even though they face similar wage increases. These firms primarily adjust through output expansion and increased use of intermediate inputs, whereas productivity responses are modest.

Our results also provide evidence consistent with the non-monotonic predictions of standard monopsony models. Heavily exposed low-wage firms with wage-setting power exhibit small negative employment responses. By contrast, moderately exposed medium-wage firms with wage-setting power expand employment and output while showing no price passthrough. The combination of higher employment, expanding output, and stable prices closely aligns with the predictions of standard monopsony models for a modest minimum wage.

More broadly, our results help reconcile two prominent findings in the minimum wage literature. Whereas monopsony models are frequently invoked to explain the modest employment effects of minimum wages, many empirical studies document substantial price passthrough, which sits uneasily with the predictions of these models. Our results suggest that these adjustment patterns coexist because firms with varying wage-setting power adjust along different margins. Firms without wage-setting power primarily adjust through employment reductions, productivity improvements, and price increases, whereas firms with wage-setting power respond more strongly through output expansion.

That said, several limitations remain. Our analysis focuses on larger manufacturing firms and therefore does not speak to adjustment patterns among smaller firms or in non-tradable sectors, where minimum wage exposure may differ substantially from our setting. Moreover, while our approach allows us to identify heterogeneity in firms' adjustments by wage-setting power, it does not allow us to pin down the underlying sources of that power, such as granularity, search frictions, bargaining, or worker preferences. Extending the analysis to a broader set of sectors and linking firms' wage-setting power to specific mechanisms remains a promising avenue for future research.

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Online Appendix

A Additional Tables and Figures

Table A1: Pre-Reform Firm Characteristics by Wage Group.

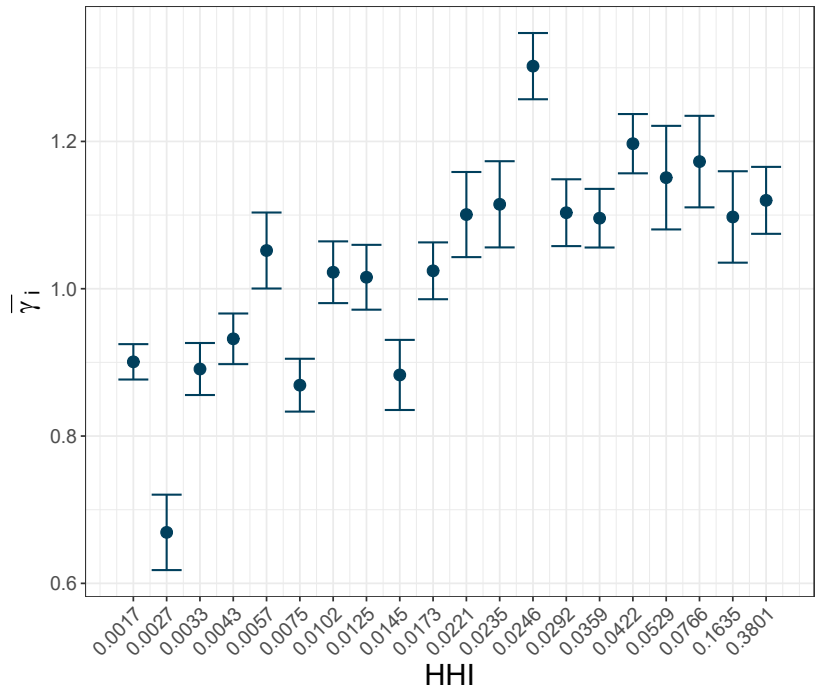
	High	Medium	Low
Employment (FTE)	407.06	135.91	118.66
	(1140.81)	(250.67)	(179.75)
Wage bill per FTE	49.50	33.01	20.92
	(8.20)	(4.76)	(3.38)
Value-added per FTE	84.03	54.49	34.57
	(44.05)	(34.20)	(12.31)
Intermediate inputs per FTE	178.96	106.16	60.44
	(160.31)	(86.86)	(56.36)
Investments per FTE	9.18	6.26	3.81
	(15.21)	(10.77)	(7.18)
Capital per FTE	135.95	95.94	69.75
	(158.15)	(93.38)	(98.65)
Share of part-time employees	8.29	11.86	21.57
	(7.86)	(12.84)	(21.93)
Share of female employees	21.44	27.08	47.56
	(13.80)	(18.72)	(25.92)
Exporter (0/1)	0.92	0.80	0.53
East Germany (0/1)	0.05	0.19	0.44
$\bar{\gamma}_i$	1.12	1.00	0.92
	(0.57)	(0.53)	(0.52)
$\bar{\gamma}_i > 1$ (0/1)	0.53	0.39	0.32
Observations	4595	4508	1144

Notes: This table shows selected firm characteristics (means and standard deviations in parentheses) by firm category for the pre-reform year 2013. The three categories are highly exposed low-wage firms, moderately exposed medium-wage firms, and the control group of high-wage firms with an average annual wage above €40,000.

Table A2: Cumulative Probability of Firm Exit.

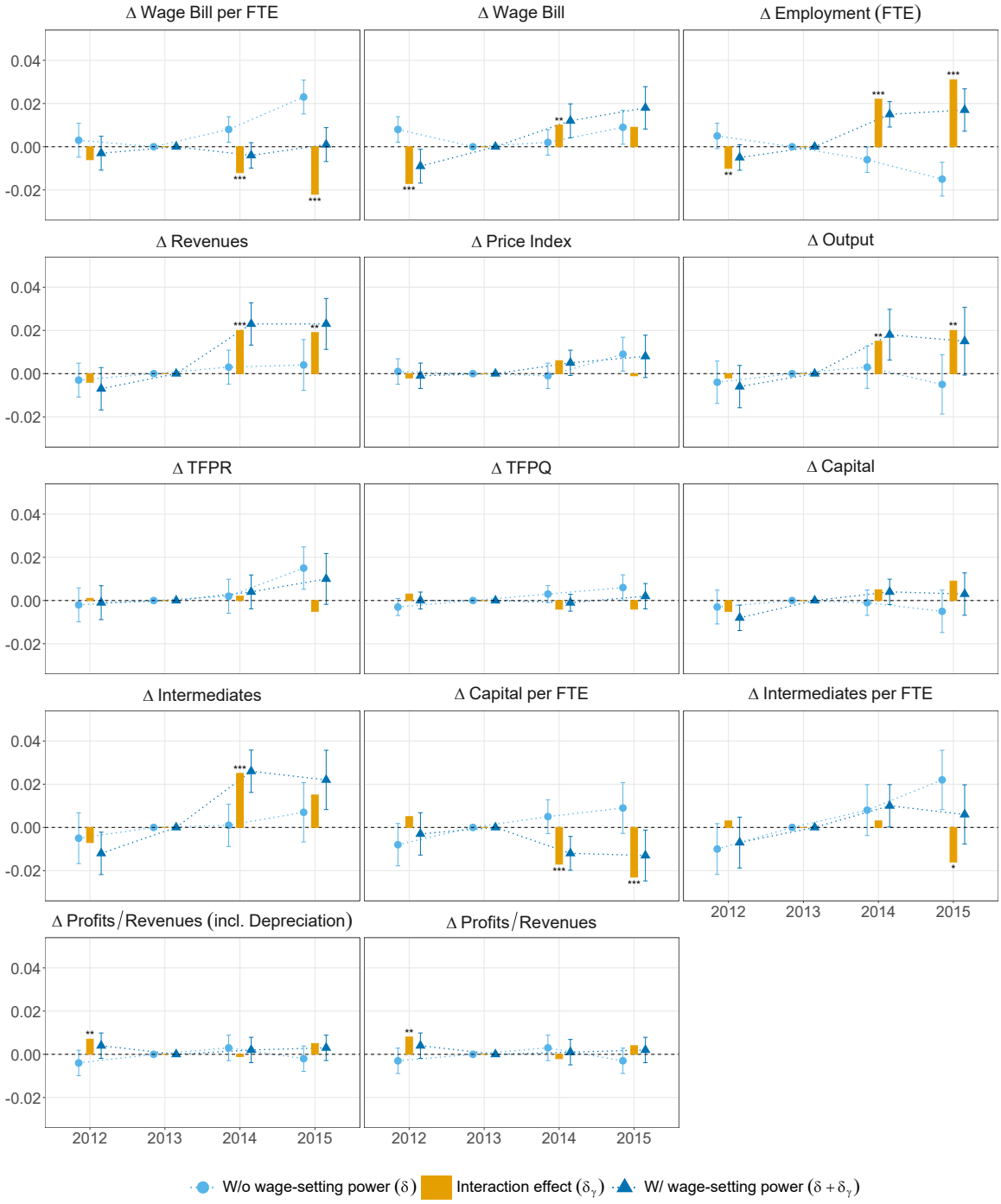
	Exited by year		
	(1) 2015	(2) 2016	(3) 2017
Low	-0.001	0.001	0.005
	(0.002)	(0.004)	(0.005)
Medium	-0.004	-0.005	0.005
	(0.004)	(0.006)	(0.008)
$\bar{\gamma}_i$	-0.001	-0.005	-0.005
	(0.003)	(0.004)	(0.005)
Low $\times$ $\bar{\gamma}_i$	-0.002	-0.002	-0.005
	(0.003)	(0.006)	(0.007)
Medium $\times$ $\bar{\gamma}_i$	0.007	0.016	0.000
	(0.006)	(0.010)	(0.012)
Constant	0.008***	0.020***	0.030***
	(0.002)	(0.003)	(0.004)
Observations	10702	10702	10702

Notes: The table reports estimates from linear probability models where the dependent variable is a dummy variable indicating whether a firm has exited the market by the end of the respective year. Hence, the reported effects are cumulative effects (e.g., a firm exiting in 2015 is an exiting firm in 2015, in 2016, and in 2017). Robust standard errors are reported in parentheses and asterisks indicate significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).



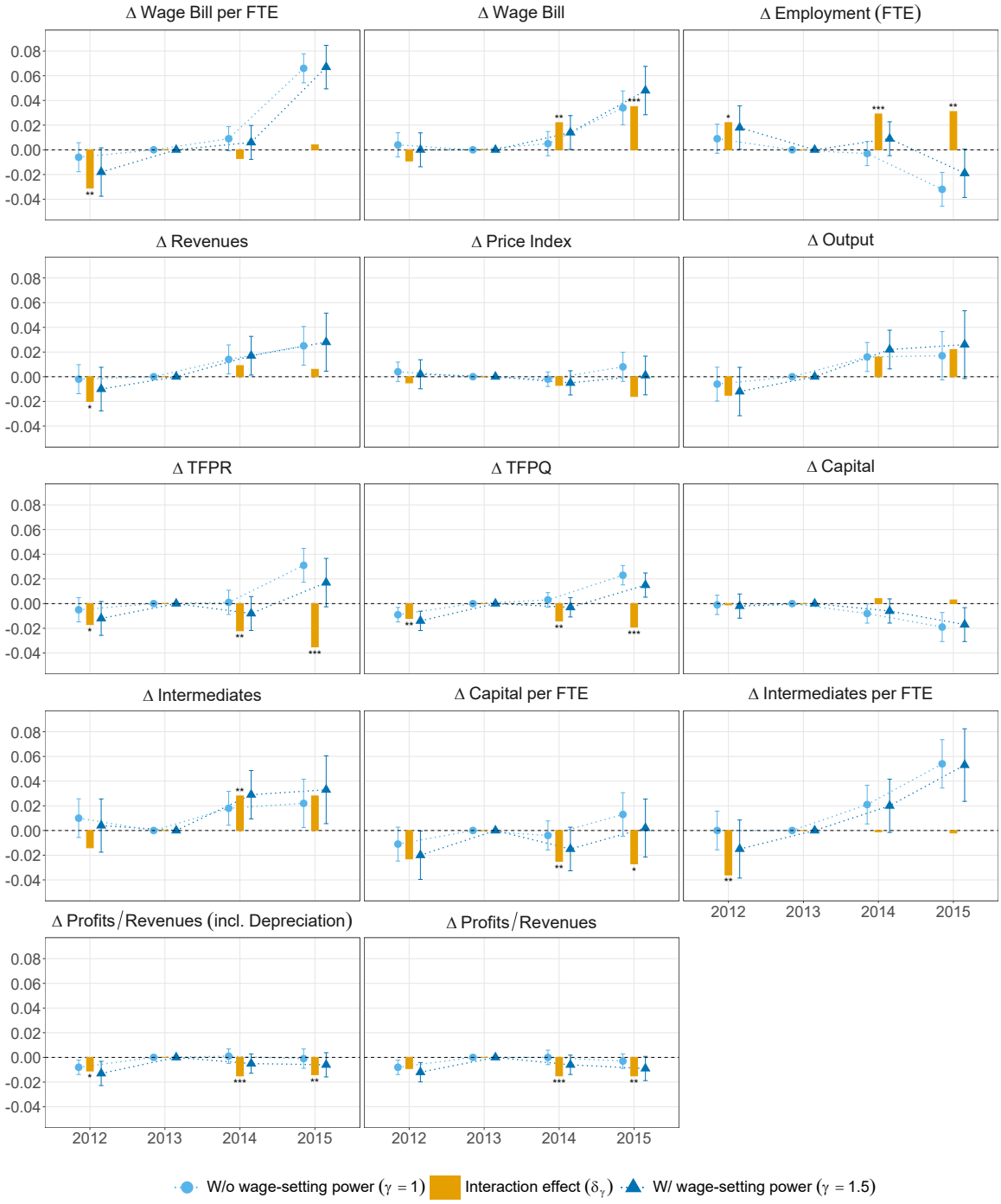
Notes: The figure relates firms' estimated wage markdowns $\bar{\gamma}_i$ to local labor market concentration measured by the Herfindahl–Hirschman Index (HHI). The HHI is computed from employment shares in the German Business Register at the two-digit NACE Rev. 2 industry × NUTS 1 region level. The figure reports mean wage markdowns within twenty equally sized HHI bins and their 95% confidence intervals (blue). N = 10,247 firms.

Figure A1: Wage Markdowns vs HHI.



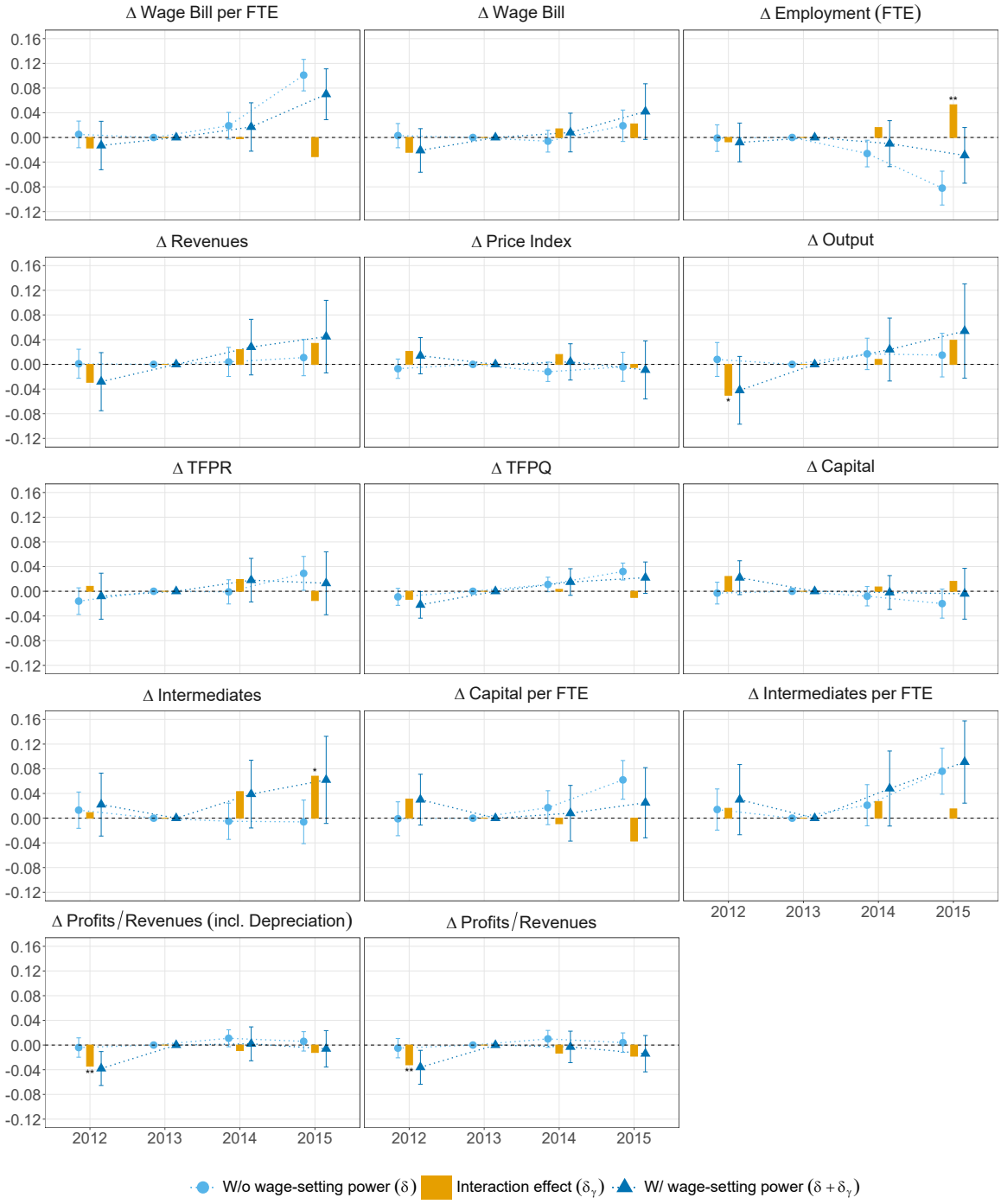
Notes: The figure reports estimated effects of minimum wage exposure for medium-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, TFPQ, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including or excluding depreciation, and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure A2: Minimum Wage Effects for Medium-Wage Firms by Wage-Setting Power.



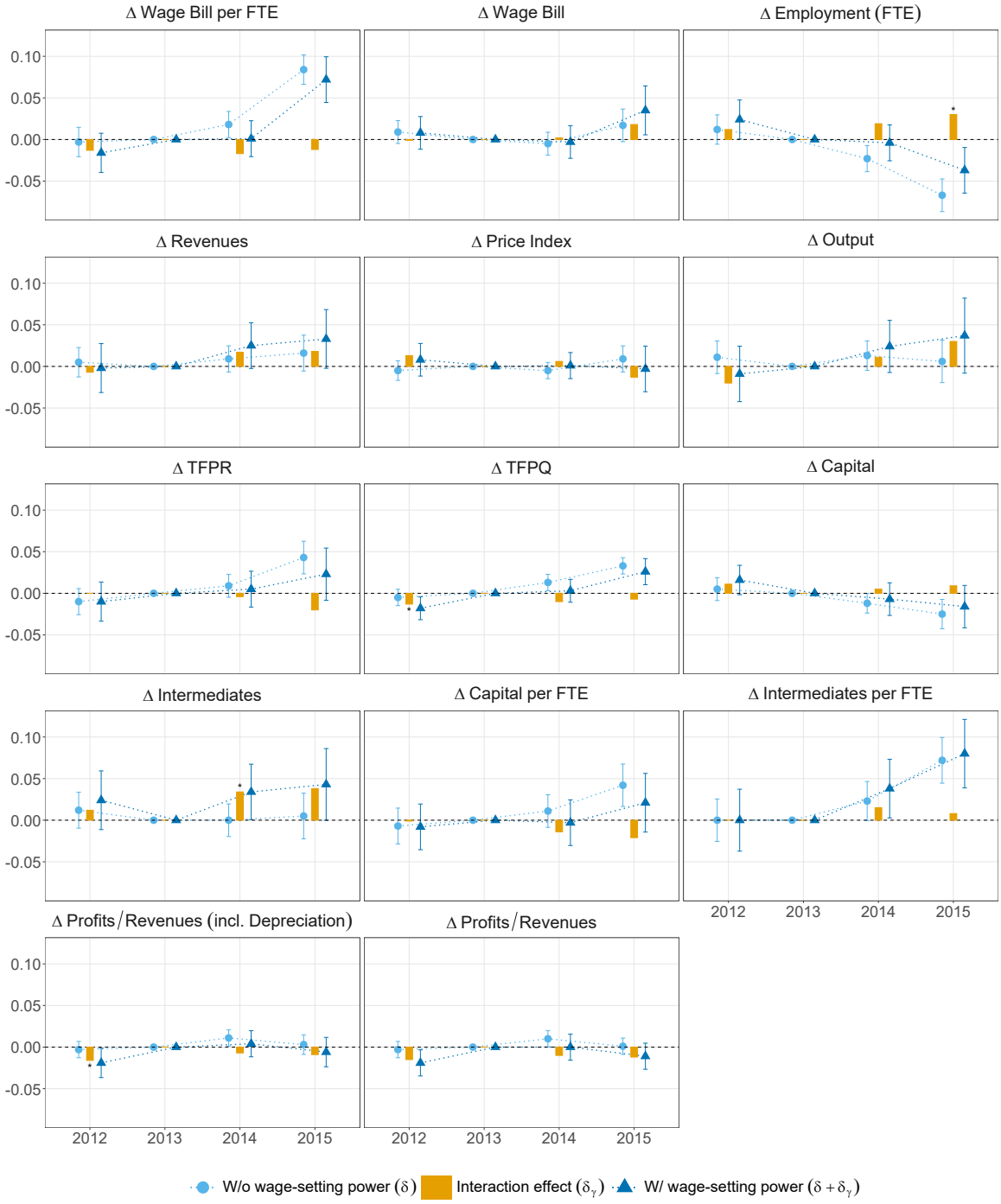
Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, TFPQ, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including or excluding depreciation, and their 95% confidence intervals (blue), evaluated for firms with strong (i.e., $\bar{\gamma}_i = 1.5$) and firms without (i.e., $\bar{\gamma}_i = 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects and include log $\bar{\gamma}_i$ instead of the dummy for $\bar{\gamma}_i > 1$. N = 10,247 firms.

Figure A3: Minimum Wage Effects for Low-Wage Firms by Log Wage-Setting Power.



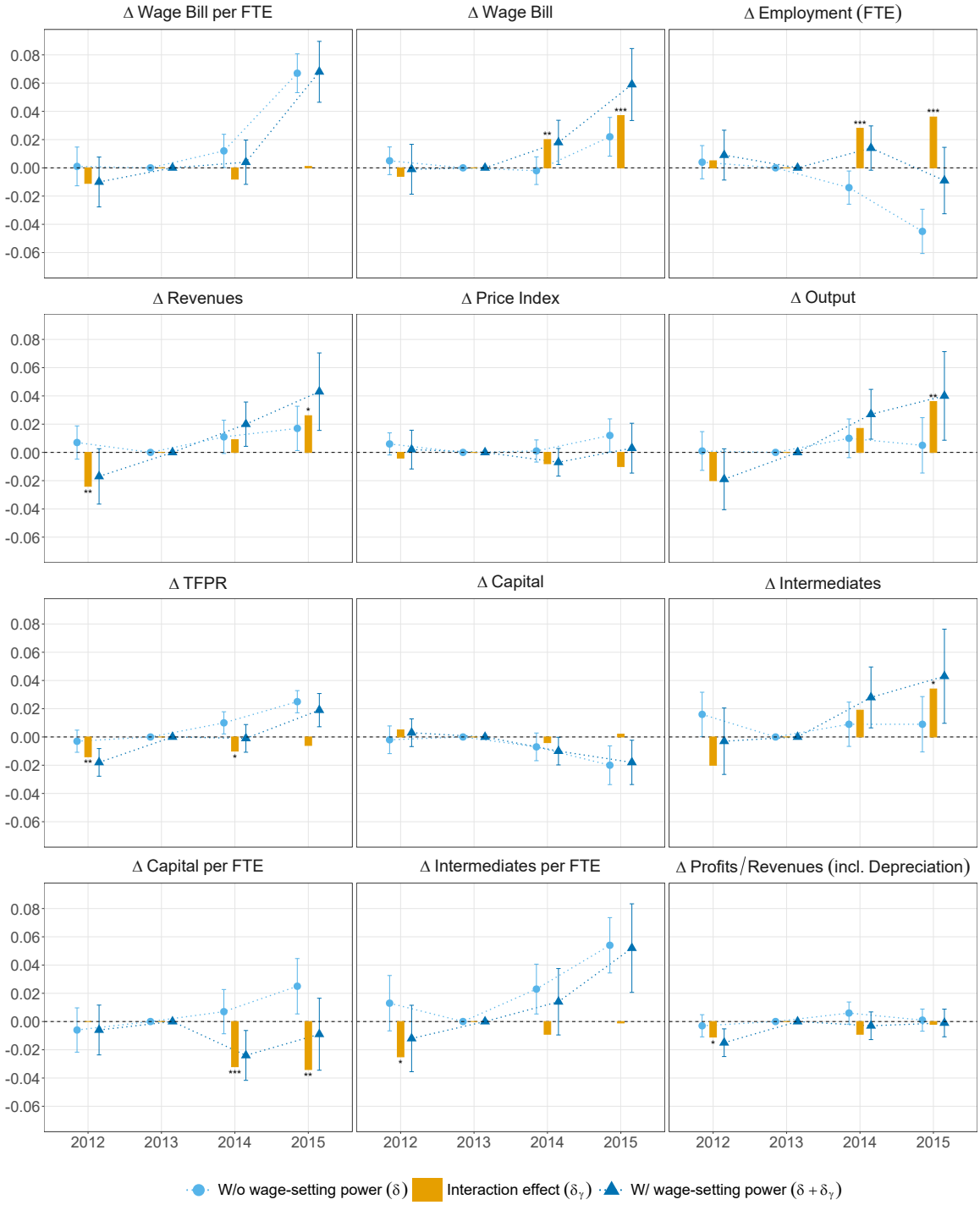
Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, TFPQ, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including or excluding depreciation, and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects and restrict the sample to firms with less than 50 employees in the base year. N = 2,788 firms.

Figure A4: Minimum Wage Effects for Low-Wage Firms by Wage-Setting Power when Restricting the Sample to Small Firms with Less than 50 Employees.



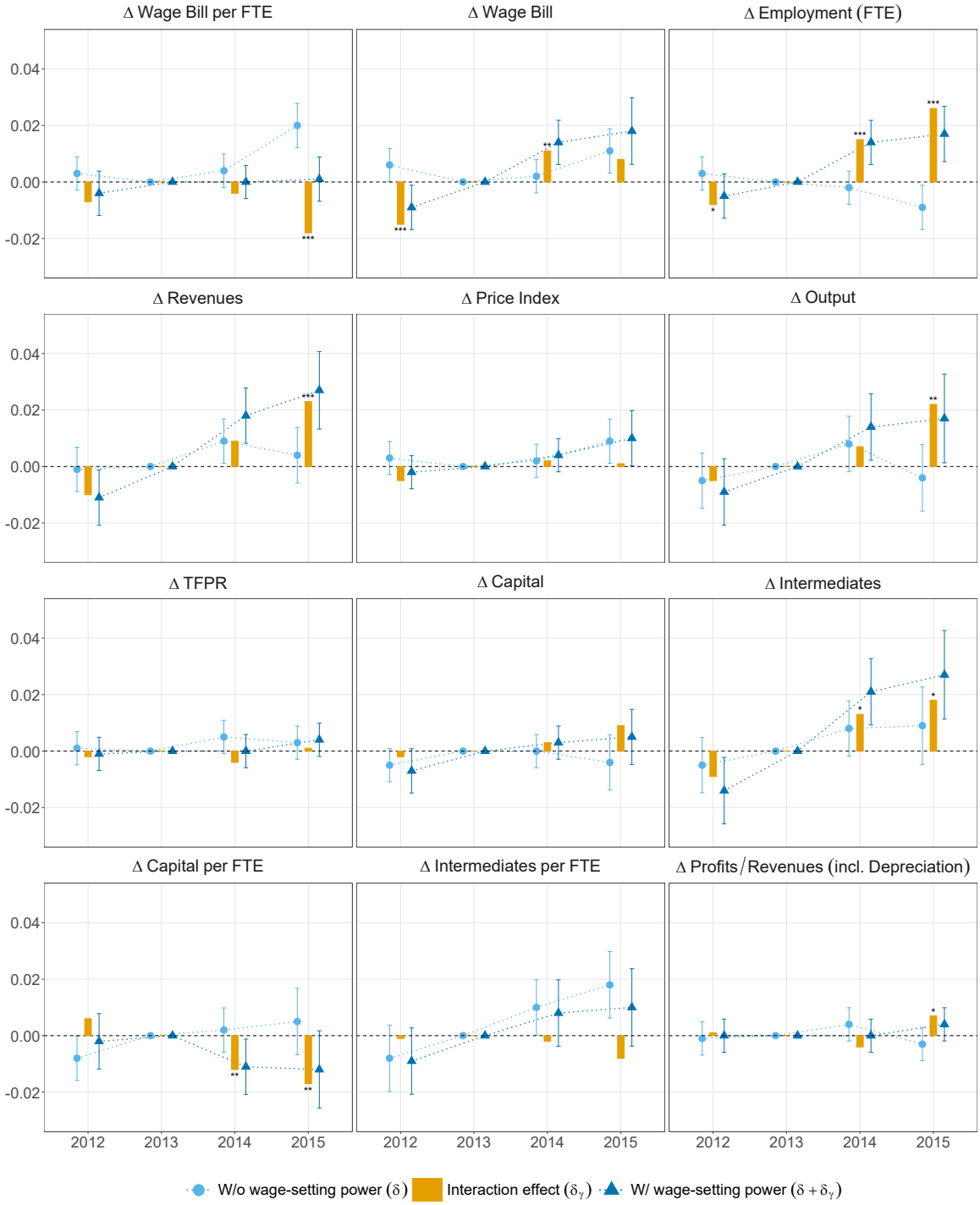
Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, TFPQ, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including or excluding depreciation, and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i > 1$) and firms without (i.e., $\bar{\gamma}_i \leq 1$) wage-setting power. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects and restrict the sample to firms with less than 100 employees in the base year. N = 5,304 firms.

Figure A5: Minimum Wage Effects for Low-Wage Firms by Wage-Setting Power when Restricting the Sample to Small Firms with Less than 100 Employees.



Notes: The figure reports estimated effects of minimum wage exposure for low-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including depreciation, and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i^{rev} > 1$) and firms without (i.e., $\bar{\gamma}_i^{rev} \leq 1$) wage-setting power computed from revenue instead of output elasticities. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure A6: Minimum Wage Effects for Low-Wage Firms by Wage-Setting Power when Relying on Revenue Elasticities instead of Output Elasticities.



Notes: The figure reports estimated effects of minimum wage exposure for medium-wage firms on the wage bill per FTE, the total wage bill, employment (in FTE), revenues, the firm-level price index, output (quasi-quantities measured as revenue deflated by the firm-level price index), TFPR, capital inputs, intermediate inputs, capital inputs per FTE, intermediate inputs per FTE, and gross operating profits over revenues including depreciation, and their 95% confidence intervals (blue), for firms with (i.e., $\bar{\gamma}_i^{rev} > 1$) and firms without (i.e., $\bar{\gamma}_i^{rev} \leq 1$) wage-setting power computed from revenue instead of output elasticities. Orange bars show the estimated interaction effects together with significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Estimates come from OLS regressions of Equation (3) with cluster-robust standard errors that control for state and two-digit industry fixed effects. N = 10,247 firms.

Figure A7: Minimum Wage Effects for Medium-Wage Firms by Wage-Setting Power when Relying on Revenue Elasticities instead of Output Elasticities.

B Institutional Background

The introduction of a statutory minimum wage was a central topic in the federal election campaign in Germany in 2013. The demand for a minimum wage of €8.50 per hour was a cornerstone of the campaign of the Social Democratic Party (SPD) in opposition to the then-ruling conservative (CDU/CSU) and liberal (FDP) parties. However, after the federal election in September 2013, the minimum wage of €8.50 was included in the coalition agreement between the SPD and CDU/CSU signed in November 2013. The law introducing the minimum wage was passed in parliament in July 2014.

The statutory minimum wage came into effect in Germany on January 1, 2015, and was introduced at a level of €8.50 gross per hour. Since then, the minimum wage has been adjusted regularly based on proposals by the independent Minimum Wage Commission, which includes representatives of trade unions and employer associations. The minimum wage was raised to €8.84 effective January 1, 2017, and increased to €10.45 on July 1, 2022. Following the change in government in 2021, it was decided to increase the minimum wage to €12 on October 1, 2022, departing from the recommendation of the Minimum Wage Commission.

Before 2015, several sector-specific minimum wages were in place. Sectors with existing minimum wages below €8.50 were granted a transition period until January 2017. Transitional arrangements applied to the following groups of employees: meat industry workers, hairdressers, contract workers, laundry service providers for large customers, agriculture and forestry workers, textile industry workers, and horticulture workers. To avoid contamination from these transitional arrangements, we exclude firms belonging to the respective industries from our analysis.

Nearly all employees in Germany are eligible for the statutory gross minimum wage. However, permanent exemptions apply to minors, apprentices, those completing obligatory internships, volunteers, long-term unemployed workers for their first six months in a new job, and participants in programs aimed at reintegrating unemployed into work. As far as the data permit, we account for individual minimum wage eligibility when using the worker-level data.

Compliance control resides with the customs authorities. Enforcement mechanisms include an obligation to record working hours for specific industries deemed at high risk of noncompliance and suspicion-based controls. Thus, compliance with the minimum wage requires a sufficient level of information among both workers and employers. While the literature unanimously documents positive effects on hourly wages for low-wage workers in response to the minimum wage (Bossler and Gerner 2019; Dustmann et al. 2022; Caliendo et al. 2023), there is mixed evidence on compliance with the minimum wage law in Germany. On the one hand, us-

ing survey data from the German Socio Economic Panel, Burauel et al. (2017) and Caliendo et al. (2023) show evidence that a substantial fraction of eligible workers earned wages below the minimum wage level shortly after the minimum wage had become effective. On the other hand, using administrative data from the Federal Employment Agency and German Statistical Office, respectively, Dustmann et al. (2022) find average wage growth of the magnitude that is to be expected under full compliance with the minimum wage law, while Biewen et al. (2022) present evidence that compliance with the minimum wage level was achieved to a large extent by 2018.[9]

C Details on the Administrative Data

Our analysis combines several administrative datasets provided by the German Statistical Offices. Firms are legally required to report the underlying information, so the data offer comprehensive and high-quality coverage of the German manufacturing sector.

Firm-level dataset. Our main analysis uses yearly panel production data on German manufacturing firms with at least 20 employees.[10] The data contain detailed information on firms' revenues, costs, employment in full-time equivalents (FTE), wages, investment, and intermediate inputs that we use in our production function estimation and construction of wage markdowns at the level of the individual firm.

While some variables are available for the full population of firms with at least 20 employees, several key variables required for our analysis – most importantly employment in FTE and intermediate inputs – are collected for a representative, stratified 40% sample of firms. This sample rotates every four years, most recently in 2012 and 2016. Since we use these variables in our analysis, we restrict the sample to firms that we continuously observe between 2012 and 2015.

Firm-product-level dataset. We combine the firm-level production data with German PRODCOM data containing establishment × product-level information on pro-

[9] According to customs estimates, noncompliance is more likely due to overtime hours not being recorded or remunerated, breaks being withheld, or inaccurate deduction of certain employer-side expenses (e.g., board and lodging) from workers' salaries (Mindestlohnkommission 2020). This particular form of noncompliance could lead to an overestimation of productivity in affected firms if labor inputs are understated. However, Hafner and Lochner (2021) find a reduction in actual working hours and perceived time pressure among the workers most likely to be affected by the reform in Germany. In addition, Bossler et al. (2020) do not find more pronounced employment effects in industries subject to stricter enforcement in Germany. Taken together, these pieces of evidence suggest that our measure of labor input is unlikely to suffer from downward bias for affected firms.

[10] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, AFiD-Panel Industrieunternehmen (DOI: 10.21242/42221.2021.00.01.1.1.0).

duced quantities and factory-gate sales for all manufacturing establishments with at least 20 employees.[11] The German product classification distinguishes nearly 6,000 ten-digit product categories, including highly detailed product definitions, such as "workwear – long trousers for men, cotton" or "passenger cars, petrol engine ≤ 1,000 cubic centimeters".

Structure of Earnings Survey. To validate our treatment classification, we additionally use the German Structure of Earnings Survey (*AFiD-Modul Verdienste*, VSE), a linked employer–employee dataset that includes worker-level information on hourly wages.[12] The VSE is available for the years 2001, 2006, 2010, and 2014. We only use the 2014 wave, which is the year before the introduction of the statutory minimum wage in 2015.[13] As far as the data permit, we exclude employees and industries exempt from the minimum wage. As a case in point, the VSE allows us to identify apprentices and minors who are not eligible for the minimum wage.

The VSE is sampled at the establishment level and covers approximately 70,000 establishments and 1,000,000 employees across all sectors of the German economy. It relies on a two-stage sampling design: First establishments are drawn from the population of plants with at least one employee and then, depending on establishment size, either all employees or a random subset of the employees of the selected establishments are surveyed. Since the VSE and the firm-level datasets described above are independently sampled, their overlap is limited. We therefore use the VSE solely for validating our treatment classification based on direct measures of firms' minimum wage exposure from worker-level wage data.

Business Register data. We additionally use annual Business Register data from the German Statistical Offices covering the years 2002–2017 to identify firm exits.[14] The Business Register covers the universe of economically active firms and establishments in Germany above minimum reporting thresholds and contains, among other things, information on industry classifications, employment, sales, and location. We use these data primarily to track firm exit following the introduction of the minimum wage and to construct alternative measures of local labor market concentration used to check robustness.

[11] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, AFiD-Panel Produkte (DOI: 10.21242/42131.2018.00.03.1.1.0).

[12] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, AFiD-Modul Verdienste (DOI: 10.21242/62111.2014.00.03.1.1.0).

[13] Note that the VSE records wages as of April 2014.

[14] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, AFiD Panel URS (DOI: 10.21242/52111.2012.00.01.1.1.1.; DOI: 10.21242/52121.2019.00.01.1.1.0.)

D Treatment Validation

To validate our treatment classification based on firms' average wage bill per FTE, we construct a direct measure of firms' exposure to the minimum wage using worker-level wage information from the Structure of Earnings Survey (VSE). Following Draca et al. (2011) and Hirsch et al. (2015), we define the firm-level gap measure as

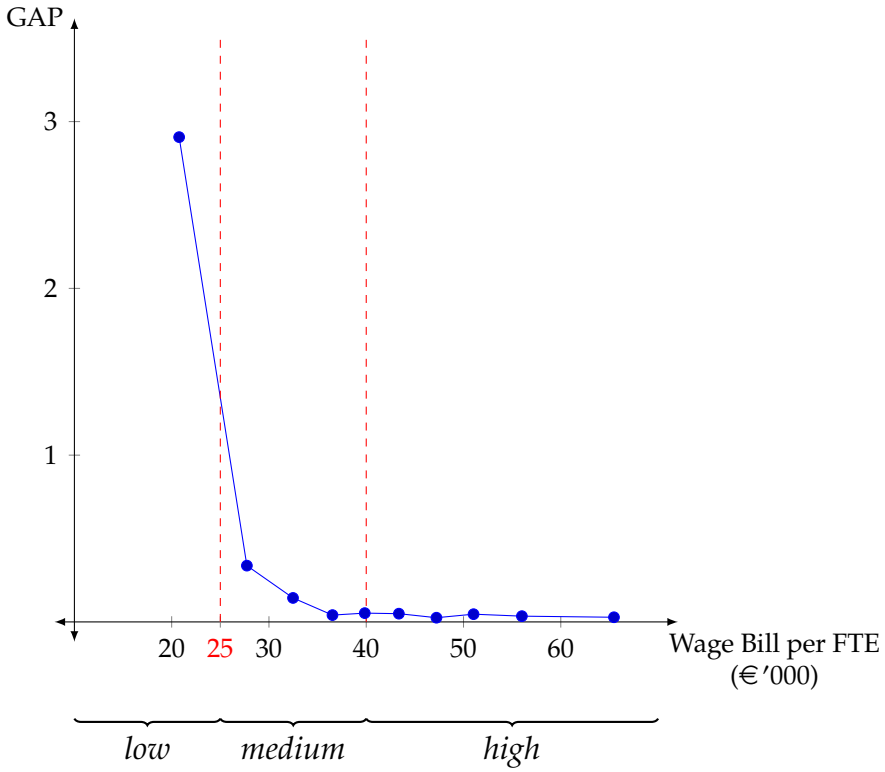
$$\text{GAP}_i = \frac{\sum_j h_j \max\{w_{min} - w_j, 0\}}{\sum_j w_j h_j} \times 100 \tag{D1}$$

where w_{min} denotes the statutory minimum wage (€8.50), w_j the gross hourly wage of worker j employed at firm i, and h_j this worker's hours worked. The measure captures the percentage increase in a firm's wage bill required to comply with the minimum wage assuming fixed employment and hours, where unaffected firms exhibit a gap of zero.[15]

Figure D1 plots the average gap measure against firms' average pre-reform wage bill per FTE, where we group firms into ten equally sized bins according to their average wage level. The vertical dashed lines indicate the thresholds used to define our low-, medium-, and high-wage groups. The figure documents a strong negative relationship between firms' average wage bill per FTE and the estimated gap measure. Exposure rises sharply for firms with average annual wages below €25,000, and the gap is close to zero for firms with average wages above €40,000. What is more, minimum wage workers are heavily concentrated among low-wage firms: 62% of affected workers are employed in low-wage firms, whereas only 14% are employed in high-wage firms.

Overall, these results indicate that our treatment-group classification based on firms' average wage bill per FTE provides a reliable proxy for firms' actual exposure to the minimum wage introduced in 2014. This finding is consistent with previous studies using average wage measures to proxy minimum wage exposure at the firm level (e.g., Draca et al. 2011; Hau et al. 2020).

[15] The VSE is collected at the establishment level, whereas our main datasets are sampled at the firm level. To aggregate establishment-level exposure to the level of the firm, we assume identical exposure across establishments belonging to the same firms whenever only a single establishment is observed. For firms with multiple observed establishments, we use employment-weighted averages across establishments.



Notes: The ordinate shows the percentage increase in a firm's wage bill required to comply with the minimum wage as computed from the VSE. The abscissa shows the pretreatment average of annual wage costs per FTE divided into 10 equally-sized bins. The red lines depict the thresholds for the three considered groups of low-, medium-, and high-wage firms: $[min; €25{,}000)$, $[€25{,}000; €40{,}000)$, $[€40{,}000; max]$. N = 1,787 firms.

Figure D1: Gap Measure of Minimum Wage Exposure against Wage Bill per FTE.

E Estimating Firm-Level Production Functions

E.1 Quantity-Based Production Functions and Output Elasticities

Basic assumptions. To estimate firm-level production functions, we rely on the established control function approach developed by Olley and Pakes (1996) and further extended by Levinsohn and Petrin (2003), Wooldridge (2009), and De Loecker et al. (2016). The precise implementation follows Mertens (2022). We assume that firms produce output (Q_{it}) using labor (L_{it}), intermediates (M_{it}), and capital (K_{it}) and that quantity-based productivity ($e^{\omega_{it}}$) is Hicks-neutral. We further assume the following flexible translog production function, where small letters denote logs:

$$\begin{aligned} q_{it} &= \boldsymbol{\phi}_{it}'\boldsymbol{\beta} + \omega_{it} + \epsilon_{it} \\ &= \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \beta_{ll} l_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kk} k_{it}^2 \\ &\quad + \beta_{lk} l_{it} k_{it} + \beta_{mk} m_{it} k_{it} + \beta_{lm} l_{it} m_{it} + \beta_{lmk} l_{it} m_{it} k_{it} + \omega_{it} + \epsilon_{it} \end{aligned} \tag{E1}$$

In our implementation, labor is measured in full-time equivalents, capital by deflated capital stocks (see Appendix F for our construction of capital stocks), and intermediate inputs by deflated intermediate-input expenditures, using two-digit industry-level deflators provided by the Statistical Offices.

There are three identification issues that prevent unbiased estimation of Equation (E1) by OLS. First, productivity is unobserved by the econometrician but known to the firm, which induces simultaneity bias if firms adjust their input choices to productivity shocks. Second, to recover a quantity-based productivity measure, we must estimate the quantity-based production function specified in (E1). Although we observe quantities for the individual products of firms, we cannot simply aggregate product-level quantities to the firm level. Third, we do not observe input prices for all production inputs. If unobserved input prices are correlated with input choices and physical output, we face an additional identification problem. In the following, we describe in detail how we address each of these issues.

Measuring firm-level output. As we cannot directly aggregate output quantities across multiple products, we follow Eslava et al. (2004) and calculate a firm-specific output price index from observed product price changes within firms in the manufacturing sector. We describe the methodology for constructing this Törnqvist price index in Appendix G. With the firm-specific price indices at hand, we purge price variation from observed revenues for all firms by deflating revenues with these price indices. Doing so yields quasi-quantity measures of output; in a slight abuse of notation, we continue to denote these quasi-quantities by Q_{it}. Using such quasi-quantities has been adopted in a series of studies (e.g., Eslava et al. 2013; Smeets and Warzynski 2013; Carlsson et al. 2021).

Controlling for unobserved input prices. To account for unobserved input price variation, we apply a firm-level adaptation of the approach of De Loecker et al. (2016). To this end, we formulate an input price control function from observed information on output prices, product market shares, firm location, and industry affiliation and add it to the right-hand side of the production function equation (E1). Following the notation in De Loecker et al. (2016), this price control function is given by:

$$B(.)_{it} = B((p_{it}, ms_{it}, \mathbf{A}_{it}, \mathbf{I}_{it}) \times \boldsymbol{\phi}^{c}_{it}) \tag{E2}$$

where p_{it} denotes the log of the firm-specific output price index, ms_{it} the log of the weighted average of firms' product market shares in terms of sales, $\mathbf{A}_{it}$ a set of dummies for firm location, and $\mathbf{I}_{it}$ a set of dummies for four-digit industry. In (E2), $\boldsymbol{\phi}^{c}_{it} = \{1, \boldsymbol{\phi}_{it}\}$ signifies that the arguments of the price control function enter linearly

and, owing to the translog specification, through interactions with all input terms included in ϕ_{it}.

In our practical implementation, we do not allow for all possible interactions among the arguments of the price control function to maintain a parsimonious parameterization. Instead, we restrict the interactions to those involving the output price index. Specifically, the average market share and the location and industry dummies enter linearly, whereas the output price index enters both linearly and through interactions with the input terms included in ϕ_{it}.

The intuition behind the price-control function $B(.)_{it}$ is that firms' output prices, product market shares, location, and industry affiliation are informative about their input prices. In particular, we expect that producing expensive high-quality products requires expensive high-quality inputs. As discussed in detail in De Loecker et al. (2016), this expectation motivates the inclusion of a control function containing output price information on the right-hand side of the production function to control for unobserved input price variation stemming from input quality differences across firms. Conditional on the arguments of $B(.)_{it}$, we assume that there are no remaining input price differences across firms. Although this assumption may sound restrictive, it is actually more general than those imposed in most other studies that estimate production functions without an input price control function. In the absence of a control function, researchers implicitly assume that firms face identical input and output prices within industries.

A notable difference between the approach of De Loecker et al. (2016) and ours is that they estimate product-level production functions, whereas we adapt their framework to the firm level. To do so, we aggregate firm-product-level information to the level of the firm using firm-product-specific sales shares in firms' total product market sales. In so doing, we assume that (i) sales-weighted firm-level aggregates of product quality are increasing in firm-level aggregates of product prices and input quality; (ii) firm-level input costs for inputs entering as deflated expenditures are increasing in firm-level input quality; and (iii) product price elasticities are identical across a firm's products. Note that these assumptions, or even stronger ones, are always implicitly invoked whenever firm-level rather than product-level production functions are estimated.

Finally, even if some of these assumptions are violated, including the price control function remains preferable to omitting it. The reason is that it will still absorb some of the unobserved price variation even if only a subset of the arguments of $B(.)_{it}$ is informative about firms' input prices. By contrast, omission risks leaving any systematic input price variation unaccounted for. In short, the appeal of the price control function lies in its agnostic treatment of the existence and magnitude of input price variation.

Controlling for unobserved productivity. To address the dependence of firms' flexible input choices on unobserved productivity shocks, we apply a control function approach in the spirit of Olley and Pakes (1996) and Levinsohn and Petrin (2003). We base our control function on the firm's demand function for energy and raw materials (e_{it}, in logs), which we observe as separate parts of total intermediate inputs. Inverting the demand function for e_{it} yields the firm's productivity as:

$$\omega_{it} = g(.)_{it} = g(e_{it}, k_{it}, l_{it}, \mathbf{\Gamma}_{it}) \tag{E3}$$

where $\mathbf{\Gamma}_{it}$ denotes a broad set of additional state variables that affect firms' demand for e_{it}. In our implementation, $\mathbf{\Gamma}_{it} = (EX_{it}, NumP_{it}, RD_{it}, w_{it})$ includes a dummy variable for export activity (EX_{it}), the log of the number of products the firm produces ($NumP_{it}$), a dummy for R&D activity (RD_{it}), and the firm's average wage per FTE (w_{it}). We include the latter to control for input prices (as input price are arguably correlated across inputs). Doing so helps to absorb unobserved quality and price differences that shift demand for e_{it}.

We assume that productivity follows a first-order Markov process and allow its evolution to depend on firm activities. Specifically, we assume the following law of motion for productivity: $\omega_{it} = h(\omega_{it-1}, \boldsymbol{Z}_{it-1}) + \zeta_{it} = h_{it-1}(.) + \zeta_{it}$, where ζ_{it} denotes the innovation in productivity and the inclusion of $\boldsymbol{Z}_{it-1} = (EX_{it-1}, NumP_{it-1}, RD_{it-1})$ allows productivity to be affected by export market participation, R&D activity, and (dis)economies of scope arising from changes in the firm's product portfolio.[16]

Identification. Inserting Equations (E2) and (E3) and the law of motion for productivity into the production function gives:

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + B(.)_{it} + h(.)_{it-1} + \zeta_{it} + \epsilon_{it} \tag{E4}$$

which forms the basis for our estimation.[17] We estimate Equation (E4) separately for each two-digit (NACE Rev. 2) industry for the years 1999 to 2017 using the one-step estimator as put forward by Wooldridge (2009). To address the endogeneity resulting from the impact of firms' flexible input choices on realizations of ζ_{it}, this estimator uses lagged values of flexible inputs (in our case intermediates) as instruments for their contemporaneous values.[18] In the same vein, we instrument current market

[16] Note that the overlap between variables included in $\boldsymbol{Z}_{it-1}$ and $\mathbf{\Gamma}_{it}$ poses no problem because we are not interested in identifying their coefficients in the control function.

[17] We approximate $h(.)_{it-1}$ with a full third-order polynomial in all of its arguments, while the variables in $\boldsymbol{Z}_{it-1}$ and $\mathbf{\Gamma}_{it}$ enter linearly.

[18] Note that our implementation treats capital and labor as quasi-fixed factors, which is also done by other studies in the literature (e.g., De Loecker et al. 2016). We decided to treat labor as quasi-

shares and output price indices by their lagged values.

Identifying moment conditions are given by:

$$E\left[(\zeta_{it} + \epsilon_{it})\boldsymbol{\Upsilon}_{it}\right] = \mathbf{0} \tag{E5}$$

where $\boldsymbol{\Upsilon}_{it}$ denotes the vector of instruments. It contains lagged intermediate inputs (linearly and squared), interactions of intermediate inputs with labor and capital, contemporaneous values for capital and labor (linearly and squared), interactions of capital and labor, contemporaneous location and industry dummies, lagged market shares, the lagged output price index, the lagged state variables that enter $h(.)_{it-1}$, and lagged interactions of the output price index with production inputs. In symbols, we thus have:

$$\boldsymbol{\Upsilon}_{it} = (\boldsymbol{J}_{it}, \boldsymbol{V}_{it-1}, \boldsymbol{\Xi}_{it-1}, \boldsymbol{\Psi}_{it-1}, \boldsymbol{\Lambda}_{it-1}) \tag{E6}$$

with the following vectors of arguments:

$$\begin{aligned}
\boldsymbol{J}_{it} &= (l_{it}, k_{it}, l_{it}^2, k_{it}^2, l_{it}k_{it}, \mathbf{A}_{it}, \mathbf{I}_{it}) \\
\boldsymbol{V}_{it-1} &= (m_{it-1}, m_{it-1}^2, l_{it-1}m_{it-1}, m_{it-1}k_{it-1}, l_{it-1}m_{it-1}k_{it-1}, ms_{it-1}, p_{it-1}) \\
\boldsymbol{\Xi}_{it-1} &= ((m_{it-1}, l_{it-1}, k_{it-1}, m_{it-1}^2, l_{it-1}^2, k_{it-1}^2, l_{it-1}m_{it-1}, m_{it-1}k_{it-1}, l_{it-1}k_{it-1}, \\
&\qquad l_{it-1}m_{it-1}k_{it-1}) \times p_{it-1}) \\
\boldsymbol{\Psi}_{it-1} &= (e_{it-1}, l_{it-1}, k_{it-1}, e_{it-1}^2, l_{it-1}^2, k_{it-1}^2, e_{it-1}^3, l_{it-1}^3, k_{it-1}^3, \\
&\qquad l_{it-1}e_{it-1}, e_{it-1}k_{it-1}, l_{it-1}k_{it-1}, l_{it-1}e_{it-1}k_{it-1}, \\
&\qquad k_{it-1}^2e_{it-1}, k_{it-1}^2l_{it-1}, l_{it-1}^2e_{it-1}, l_{it-1}^2k_{it-1}, e_{it-1}^2k_{it-1}, e_{it-1}^2l_{it-1}) \\
\boldsymbol{\Lambda}_{it-1} &= (EX_{it-1}, NumP_{it-1}, RD_{it-1}, w_{it-1})
\end{aligned} \tag{E7}$$

Note that the time period for the production function estimation (1999–2017) is much longer than the period (2012–2015) for studying the minimum wage effects. Using this longer time frame is dictated by the need for a sufficiently large number of observations per firm to produce stable estimates. For the same reason, we cannot allow for production-function parameters to vary before and after the introduction of the minimum wage. Nonetheless, the flexible translog specification still allows firms' output elasticities to vary with relative factor prices.[19]

fixed for two reasons connected to the institutional setting and our data. Specifically, Germany is characterized by a fairly rigid labor market and the labor variable is based on reported values for September (whereas all other variables pertain to the full calendar year).

[19] Ideally, one would like to estimate firm-year-specific production functions, but this is not feasible due to insufficient degrees of freedom. In particular, the estimation routine requires a sufficiently large number of observations in each industry to produce stable results. We thus face a trade-off between flexibility and estimation stability.

Table E1: Estimated Output Elasticities and Wage Markdowns (Quantity-Based Production Function).

	Mean	SD	p90	p75	p50	p25	p10	Obs
Returns to scale	1.04	0.11	1.18	1.11	1.03	0.96	0.91	243,628
Capital output elasticity	0.10	0.06	0.17	0.13	0.10	0.07	0.04	243,628
Labor output elasticity	0.30	0.11	0.44	0.37	0.30	0.22	0.15	243,628
Intermediates output elasticity	0.64	0.10	0.77	0.70	0.64	0.58	0.52	243,628
Wage markdown	1.05	0.60	1.74	1.30	0.93	0.66	0.48	243,628

Notes: Output elasticities are obtained from estimates of the production function (E1) for all NACE Rev. 2 two-digit industries with sufficient observations for the years 1999 to 2017. Wage markdowns are calculated according to Equation (1).

Estimation results. Table E1 provides descriptive statistics for the 243,628 estimated output elasticities and implied wage markdowns of 39,452 firms belonging to 20 two-digit industries. Overall, the estimates appear plausible and are comparable to those reported in other studies (e.g., De Loecker et al. 2016). About 10% of firm-year observations display a negative output elasticity with respect to at least one production factor. We drop firms with such negative elasticities from our analysis because these estimates are not consistent with the underlying production model.

E.2 Revenue-Based Production Functions and Revenue Elasticities

As pointed out in the main text, computing the wage markdown from output elasticities involves the assumption that the prices of intermediate inputs are exogenous. This assumption is not needed, though, when relying on revenue elasticities to compute wage markdowns, as stressed by Hashemi et al. (2022) and Treuren (2025). Doing so, however, comes at the cost of no longer being able to compute firms' TFPQ, which is an outcome of interest. We therefore use this approach for the robustness analysis only.

To obtain the required revenue elasticities, we estimate revenue-based production functions using an approach that closely follows the estimation of quantity-based production functions detailed in Appendix E.1. Our starting point is a translog production function with log sales as the dependent variable:

$$\begin{aligned} r_{it} &= \boldsymbol{\phi}_{it}'\tilde{\boldsymbol{\beta}} + \tilde{\omega}_{it} + \tilde{\epsilon}_{it} \\ &= \tilde{\beta}_j^l l_{it} + \tilde{\beta}_j^k k_{it} + \tilde{\beta}_j^m m_{it} + \tilde{\beta}_j^{ll} l_{it}^2 + \tilde{\beta}_j^{kk} k_{it}^2 + \tilde{\beta}_j^{mm} m_{it}^2 \\ &\quad + \tilde{\beta}_j^{lk} l_{it} k_{it} + \tilde{\beta}_j^{lm} l_{it} m_{it} + \tilde{\beta}_j^{km} k_{it} m_{it} + \tilde{\beta}_j^{lkm} l_{it} k_{it} m_{it} + \tilde{\omega}_{it} + \tilde{\epsilon}_{it} \end{aligned} \tag{E8}$$

where notation follows the same conventions introduced in Appendix E.1 and tildes denote the revenue-based counterparts of the objects from the quantity-based pro-

duction function.[20] We obtain revenue elasticities by partially differentiating Equation (E8), i.e., $\frac{\partial r_{it}}{\partial x_{it}} = \tilde{\theta}^X_{it}$.

As with the quantity-based production function in Appendix E.1, two key challenges in estimating Equation (E8) are controlling for unobserved revenue productivity and controlling for unobserved input price variation. Again, we closely follow the approach described in Appendix E.1.

Controlling for unobserved input prices. In contrast to the quantity-based production function, we do not control for output prices by including them on the right-hand side of the production function because doing so may absorb variation that identifies the revenue elasticity (as revenues equal prices times quantities). We thus drop p_{it} from the arguments of the price control function. To nonetheless control for input price variation, we include as arguments the intermediate-input market shares (ms^M_{it}) and sets of dummies for firm location and four-digit industry:[21]

$$\tilde{B}(.)_{it} = \tilde{B}((ms^M_{it}, \mathbf{A}_{it}, \mathbf{I}_{it}) \times \boldsymbol{\phi}^c_{it}) \tag{E9}$$

where $\boldsymbol{\phi}^c_{it} = \{1, \boldsymbol{\phi}_{it}\}$ so that, as before, the arguments enter the production function linearly and through interactions with $\boldsymbol{\phi}_{it}$. Similar to before, in implementation we maintain a parsimonious parametrization by restricting the interactions, here to those involving the intermediate-input market shares. As before, we include $\tilde{B}(.)_{it}$ as a control function in Equation (E8).

Controlling for unobserved revenue productivity. To control for unobserved revenue productivity, we specify a control function akin to the one for quantity productivity in Equation (E3). Inverting firms' demand for energy and raw materials (e_{it}) allows us to express revenue productivity as:

$$\tilde{\omega}_{it} = \tilde{g}(.)_{it} = \tilde{g}(e_{it}, k_{it}, l_{it}, \boldsymbol{\Gamma}_{it}) \tag{E10}$$

where the tilde indicates that the control function may differ in its functional form from the control function for quantity productivity. As in the quantity-based spec-

[20] Note that $\tilde{\omega}_{it}$ in general differs from revenue-based total factor productivity which one gets from applying the accounting definition on the residual from the quantity-based production function to obtain TFPR as $\hat{\omega}_{it} + p_{it}$. It instead depends on the underlying demand system. For details, see Treuren (2025). He also discusses in detail what assumptions are required to rationalize a revenue-based translog production function from an underlying structural quantity-based translog production function. These assumptions are not needed here because we treat the revenue-based translog production function merely as a flexible empirical reduced form in the estimation of revenue elasticities.

[21] In the baseline quantity-based approach, we assume exogenous intermediate input prices and accordingly do not control for intermediate-input market shares. That said, our results remain unchanged when including these in the main specification.

ification, $\mathbf{\Gamma}_{it}$ includes a set of state variables that affect demand for e_{it} and, in our implementation, is given by $\mathbf{\Gamma}_{it} = (EX_{it}, NumP_{it}, RD_{it}, w_{it})$.

As before, we assume that revenue productivity follows a first-order Markov process influenced by firm actions. Specifically, $\tilde{\omega}_{it} = \tilde{h}\left(\tilde{\omega}_{it-1}, \mathbf{Z}_{it-1}\right) + \tilde{\zeta}_{it}$, where $\tilde{\zeta}_{it}$ denotes the innovation in revenue productivity and $\mathbf{Z}_{it-1} = (EX_{it-1}, NumP_{it-1}, RD_{it-1})$.

Identification. Inserting Equations (E9) and (E10) and the law of motion for revenue productivity into the revenue production function yields our empirical model:[22]

$$r_{it} = \boldsymbol{\phi}'_{it}\tilde{\boldsymbol{\beta}} + \tilde{B}(.)_{it} + \tilde{h}(.)_{it-1} + \tilde{\zeta}_{it} + \tilde{\epsilon}_{it} \tag{E11}$$

We estimate Equation (E11) separately for each two-digit (NACE Rev. 2) industry for the years 1999 to 2017 using the one-step estimator put forward by Wooldridge (2009) and described in detail in Appendix E.1.

Here, the identifying moment conditions are given by:

$$E[(\tilde{\zeta}_{it} + \tilde{\epsilon}_{it})\tilde{\mathbf{\Upsilon}}_{it}] = \mathbf{0} \tag{E12}$$

The vector of instruments reads:

$$\tilde{\mathbf{\Upsilon}}_{it} = (\boldsymbol{J}_{it}, \tilde{\boldsymbol{V}}_{it-1}, \tilde{\mathbf{\Xi}}_{it-1}, \mathbf{\Psi}_{it-1}, \mathbf{\Lambda}_{it-1}) \tag{E13}$$

where:

$$\begin{aligned}
\boldsymbol{J}_{it} &= (l_{it}, k_{it}, l_{it}^2, k_{it}^2, l_{it}k_{it}, \mathbf{A}_{it}, \mathbf{I}_{it}) \\
\tilde{\boldsymbol{V}}_{it-1} &= (m_{it-1}, m_{it-1}^2, l_{it-1}m_{it-1}, m_{it-1}k_{it-1}, l_{it-1}m_{it-1}k_{it-1}, ms_{it-1}^M) \\
\tilde{\mathbf{\Xi}}_{it-1} &= ((m_{it-1}, l_{it-1}, k_{it-1}, m_{it-1}^2, l_{it-1}^2, k_{it-1}^2, l_{it-1}m_{it-1}, m_{it-1}k_{it-1}, l_{it-1}k_{it-1}, \\
&\qquad l_{it-1}m_{it-1}k_{it-1}) \times ms_{it-1}^M) \\
\mathbf{\Psi}_{it-1} &= (e_{it-1}, l_{it-1}, k_{it-1}, e_{it-1}^2, l_{it-1}^2, k_{it-1}^2, e_{it-1}^3, l_{it-1}^3, k_{it-1}^3, \\
&\qquad l_{it-1}e_{it-1}, e_{it-1}k_{it-1}, l_{it-1}k_{it-1}, l_{it-1}e_{it-1}k_{it-1}, \\
&\qquad k_{it-1}^2 e_{it-1}, k_{it-1}^2 l_{it-1}, l_{it-1}^2 e_{it-1}, l_{it-1}^2 k_{it-1}, e_{it-1}^2 k_{it-1}, e_{it-1}^2 l_{it-1}) \\
\mathbf{\Lambda}_{it-1} &= (EX_{it-1}, NumP_{it-1}, RD_{it-1}, w_{it-1})
\end{aligned} \tag{E14}$$

Estimation results and comparison to output-based specification. Table E2 provides descriptive statistics for the 240,496 estimated revenue elasticities and the implied wage markdowns of 38,949 firms belonging to 20 two-digit industries. Wage markdowns obtained from revenue elasticities are smaller than those obtained from output elasticities. On average, firms' wages are about 8% higher than the marginal

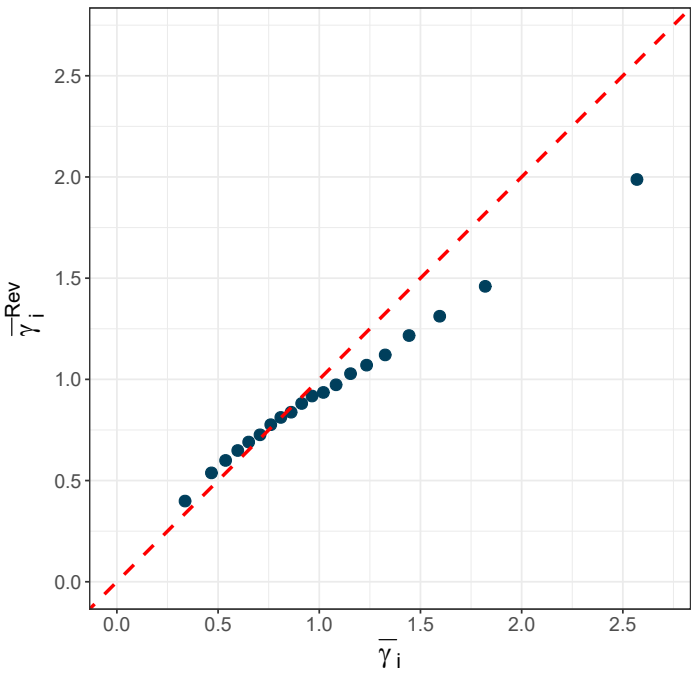
[22] As in the quantity-based specification, we approximate $\tilde{h}(.)_{it-1}$ with a full third-order polynomial in all of its arguments, while variables in $\boldsymbol{Z}_{it-1}$ and $\mathbf{\Gamma}_{it}$ enter linearly.

Table E2: Estimated Revenue Elasticities and Wage Markdowns (Revenue-Based Production Function).

	Mean	SD	p90	p75	p50	p25	p10	Obs
Returns to scale	1.00	0.10	1.12	1.05	0.99	0.93	0.90	240,496
Capital revenue elasticity	0.09	0.05	0.15	0.12	0.09	0.06	0.03	240,496
Labor revenue Elasticity	0.26	0.10	0.37	0.32	0.26	0.19	0.13	240,496
Intermediate revenue elasticity	0.65	0.10	0.77	0.71	0.65	0.59	0.53	240,496
Wage markdown	0.92	0.43	1.38	1.10	0.86	0.66	0.51	240,496

Notes: Revenue elasticities are obtained from estimates of the revenue-based production function (E11) for all NACE Rev. 2 two-digit industries with sufficient observations for the years 1999 to 2017. Wage markdowns are calculated according to Equation (4).

revenue product based on revenue elasticities, compared to 5% below the marginal revenue product in the baseline specification based on output elasticities. This pattern is consistent with firms exerting some market power in intermediate input markets, although we cannot rule out alternative explanations related to measurement and estimation of the production function. That said, Figure E1 documents that wage markdowns obtained from the two specifications are highly correlated (correlation coefficient: 0.9).



Notes: The ordinate shows the average pretreatment wage markdown obtained from revenue elasticities. The abscissa shows the average pretreatment wage markdown obtained from output elasticities. Firms are grouped into 20 equally-sized bins according to their wage markdowns based on output elasticities. N = 10,247 firms.

Figure E1: Binned Scatter Plot of the Average Pretreatment Wage Markdown Obtained from Revenue Elasticities against the One Obtained from Output Elasticities.

F Calculation of capital stocks

We calculate a time series of capital stocks for every firm using the perpetual inventory method following Bräuer et al. (2023) and assuming the following law of motion:

$$K_{it} = K_{it-1}(1 - \alpha_{jt-1}) + I_{it-1} \tag{F1}$$

where K_{it}, α_{jt} and I_{it-1} denote firm i's capital stock, depreciation rate of capital, and investment, respectively. Investment captures firms' total investment in buildings, equipment, machines, and other investment goods. Nominal values are deflated by a two-digit industry-level deflator supplied by the German Statistical Offices.

We derive the industry- and year-specific depreciation rate from official information on the expected lifetime of capital goods (supplied by the Statistical Offices). Specifically, we formulate the lifetime of a capital good LT as a function of its depreciation rate and solve for the depreciation rate α from:

$$LT = \alpha \int_0^\infty (1 - \alpha)^t t \, dt \tag{F2}$$

which assumes geometric depreciation. As the lifetime of capital goods is given separately for years and capital good types (buildings and equipment), we solve Equation (F2) for each year-capital-good combination. To derive a single industry-specific depreciation rate, we weight the depreciation rates for buildings and equipment, respectively, with the industry-level share of building capital in total capital and equipment capital in total capital (this information is supplied by the Statistical Offices). For practical implementation, we assume that the depreciation rate for a firm's entire capital stock is identical to the depreciation rate for newly purchased capital.

The initial capital stock for the perpetual inventory method is derived from reported tax depreciation. We do not use the reported tax depreciation when calculating capital stock series as tax depreciation may vary due to state-induced tax incentives and might therefore not reliably reflect the true amount of depreciated capital. Since tax incentives may induce firms to overstate depreciation, our first capital values within a capital series are likely overestimated. However, over time, observed investment decisions gradually receive a larger weight in estimated capital stocks, thereby mitigating the impact of any inaccuracy in the first observed capital stock.[23]

The estimated output elasticities appear reasonable (see Table E1), suggesting that our capital variables provide reliable proxies for firms' true capital stocks.

[23] We therefore calculate capital stocks, whenever available, from 2009 onward.

G Price Index

We calculate a firm-level Törnqvist price index from product-level price changes, weighted by products' shares in firms' revenues, following Eslava et al. (2004). The price index reads:

$$P_{it} = \prod_{g=1}^{n} \left(\frac{price_{igt}}{price_{igt-1}} \right)^{\frac{1}{2}(s_{igt}+s_{igt-1})} P_{it-1} \tag{G1}$$

where $price_{igt}$ is the price of good g produced by firm i in year t and s_{igt} is the share of this good in the total sales of firm i in year t. For the first year of data (i.e., 1999), we set the price index equal to one. For firms entering the data at a later stage, we follow Eslava et al. (2004) and use the corresponding industry average as the starting value for the price index series. Similarly, we follow Eslava et al. (2004) and impute missing values using the corresponding industry average.[24]

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[24] For 30% of firms, the Statistical Offices do not collect quantity information because reported quantities are not considered meaningful measures of physical output for the firms' products. Hence, output prices cannot be constructed for these firms.

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