Minimum wages and employment revisited: the role of imperfect labour markets*

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– PRELIMINARY DRAFT. PLEASE DO NOT QUOTE OR CIRCULATE –

Abstract

We study how imperfect competition in labor and product markets shapes firms' responses to minimum wage policies. Using administrative firm-level panel data and a triple-difference design around Germany's 2015 minimum wage introduction, we show that firms with monopsony power experience smaller - and for moderately affected firms even positive - employment effects. Firms in more competitive labor markets raise productivity and partially pass increased labor costs on to consumers. In contrast, we find no consistent evidence that product market power moderates price pass-through or employment effects. Our results highlight the importance of firm-level market power for understanding minimum wage impacts.

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1 Introduction

The effects of the minimum wage remain a hotly debated issue among scientists and the public alike. The highly controversial introduction of the national minimum wage in Germany in 2015 - affecting around 11% of workers earning lower wages prior to the policy in 2014 (Mindestlohnkommission 2016) - raised concerns that the minimum wage will induce severe job losses (see e.g. SVR 2013).

Traditional neoclassical theory predicts job losses following wage floors. However, empirical evidence has challenged this view, suggesting limited or no adverse employment consequences (see e.g. Belman and Wolfson 2014; Doucouliagos and Stanley 2009; Caliendo et al. 2019). A compelling explanation comes from monopsony theory, which posits that minimum wages may raise employment in the presence of employer market power (Manning 2011; Robinson 1969; Bhaskar et al. 2002). Despite abundant literature documenting the prevalence and magnitude of monopsony power among German firms (see e.g. B. Hirsch et al. 2010; B. Hirsch et al. 2018; Hirsch and Jahn 2015; Dobbelaere et al. 2024; Bachmann et al. 2022), we lack empirical evidence on how imperfect competition in firms labor market shapes the employment effect. Most studies rely on proxies such as labor market concentration (see e.g. Popp 2024; Azar et al. 2024), which have been shown to be poor measures of monopsony power (see e.g. Yeh et al. 2022). In addition, the literature so far neglects that firms' responses to the minimum wage are likely to depend on firms product market power, too: Firms ability to pass on higher labor costs to consumers may buffer the employment decline (Link 2024). Price-cost markups are widespread in many countries and sectors (e.g. De Loecker et al. 2020). This paper addresses this gap by directly measuring the extent to which firms exercise monopsony and product market power at the firm level, and examines how firm-level responses to the minimum wage vary with labor and product market imperfections.

We utilize the *Amtliche Firmendaten für Deutschland* (AFiD), a high-quality panel database covering German manufacturing firms (with at least 20 employees from 1995 - 2017) from the German Statistical Office. These data include detailed information on production inputs, output, expenditures, as well as firm-product level prices and produced quantities. This allows us to derive measures of firm-level monopsony and product market power using production function estimations.

We apply the methodology of Dobbelaere and Mairesse (2013), which extends De Loecker

and Warzynski (2012) approach to account for labor market imperfections, to derive our key metric of monopsony power from the wedge between the output elasticities of labor and intermediate inputs and their expenditures. This approach allows us to remain agnostic about the sources of monopsony power and measures the extent to which firms exercise such power by paying wages below the marginal revenue product of labor. Another attractive feature of the production function approach is that it also provides us with an estimate of the firms' product market power, that is their pricecost mark-up, which allows us to investigate how firm-level responses to the minimum wage vary with a firm's price-setting power.

We use information on firms' annual wage bill per full-time equivalent (averaged over 2012-2014) to classify firms according to their exposure to the minimum wage in 2015 (see Haelbig et al. 2023). Specifically, we distinguish between low-wage, medium-wage, and high-wage firms. We argue that low-wage firms are highly exposed to the statutory minimum wage, whereas medium-wage firms face moderate exposure. This classification enables us to test for potential nonlinear effects of the minimum wage predicted by monopsony theory. Using worker-level data on hourly wages[1], we show that minimum wage earners are concentrated in low-wage firms, but also constitute a non-negligible, albeit smaller, share of workers in medium-wage firms. This exercise validates our firm-level exposure measures. Our empirical design employs a triple-difference framework at the firm level to estimate firm responses as a function of the treatment indicator and a firm's pre-reform monopsony and product market power, respectively.

Our results are consistent with predictions from both neoclassical and monopsony theory. Firms highly affected by the minimum wage that pay their workers at least their marginal revenue product exhibit a statistically significant decline in employment of 5 percent relative to the control group following the introduction of the minimum wage. In contrast, highly affected firms that exercised labor market power by paying workers below their marginal revenue product prior to the reform experience a statistically insignificant negative employment effect half that magnitude. In line with monopsony theory (Manning 2003), we find that the relationship between minimum wage exposure and employment effects is nonlinear. While highly affected firms with monopsony power reduce employment, moderately affected firms with ex-ante monopsony power increase employment relative to the control group. These firms scale up, expanding both output and input use, while prices and productivity evolve

1. We rely on administrative linked employer - employee data from the Structure of Earnings Survey (VSE, Verdienststrukturerhebung), which can be merged for a subsample of firms in the AFiD data.

similarly to those of the control group. Interestingly, only affected firms operating in more competitive labor markets become more productive in response to the minimum wage, and partially pass on the increased labor costs to consumers by raising output prices. An analysis over a longer period, however, uncovers pre-trends in employment dynamics. Repeating the same analysis using product market power, we find no statistically significant effects on employment and price pass-through for firms affected by the minimum wage with respect to firms' pre-reform product market power.

We contribute to the literature in several ways. First, we empirically evaluate theoretical predictions on the differential impacts of the minimum wage across competitive environments. Although monopsonistic wage setting has frequently been proposed as a potential explanation for the zero or positive employment effects of minimum wages (e.g. Card and Krueger 1994; Neumark and Wascher 2002), we provide an empirical test of this hypothesis using a large-scale minimum wage introduction in a major economy. Secondly, whereas the employment effect of minimum wages has been the main focus of the literature, minimum wages may affect several other outcomes as well (see e.g. Metcalf 2008; B. T. Hirsch et al. 2015). Clearly, one reason behind the modest employment losses from minimum wages other than monopsony is that firms may be able to pass through the increased labor costs by raising product prices for consumers who then pay for the minimum wage. A growing number of studies have investigated the price effect of the minimum wage (for older surveys, see Lemos (2008) and Belman and Wolfson (2014)), and several recent studies point at a marked price pass-through (e.g. Harasztosi and Lindner 2019; Ashenfelter and Jurajda 2022; Renkin et al. 2022), with German evidence being no exception (Link 2024; Haelbig et al. 2023). That said, we lack direct evidence on how the pass-through varies with firms' market power. Several empirical studies highlight the role of product market power in shaping the transmission of cost shocks into consumer prices. For example, De Loecker et al. (2016) find an incomplete pass-through of less costly intermediates because pricecost mark-ups vary between firms. Similarly, Duso and Szücs (2017) and Ganapati et al. (2020) find incomplete pass-throughs of energy cost changes in Germany and the US, respectively, because of variation in firms product market power. Whereas many pass-through studies explicitly investigate the role of firms product market power for the pass-through of cost shocks, the minimum wage literature has not yet studied how their market power shapes the pass-through of the minimum wage. We fill this gap. What is more, the literature has almost exclusively focussed on the pass-through of cost

shocks to product prices. We thus lack evidence on how firms' product market power affects their adjustments to cost shocks in other dimensions than product prices, such as output, productivity, employment, and labor substitution. Thirdly, numerous studies demonstrate that firms possess market power in both labor and product markets. Recent work indicates that market power is a structural feature across many industries. International studies using the production function approach document that product market power is widespread in many countries and sectors (e.g. Loecker and Eeckhout 2018; De Loecker et al. 2020; Díez et al. 2021; Joachim et al. 2021). What is more, the literature finds marked monopsony power for a large fraction of firms (e.g. Dobbelaere and Mairesse 2013; Dobbelaere and Kiyota 2018). The same holds for recent studies for Germany (e.g. Dobbelaere et al. 2024; Mertens 2022, 2020) that find not only that monopsony is widespread in Germany and that firms monopsony power is sizeable, but also that there is a nonnegligible segment of the labor market involving worker monopoly. Fourth, previous studies typically use labor market concentration as a proxy for firms labor market power (Azar et al. 2024; Popp 2024). Although labor market concentration is strongly correlated with the elasticity of labor supply across markets (Azar et al. 2019), other work argues that concentration does not necessarily capture firms ability to exercise monopsony power (Yeh et al. 2022; Syverson 2019). Labor and product market concentration may arise from competitive forces, whereby economic activity becomes concentrated in the most productive firms even if these firms do not exert wage and price setting market power. Moreover, monopsony power can originate from factors unrelated to concentration, such as workers preferences over job characteristics or search frictions that generate inelastic labor supply due to imperfect information about outside options. Our approach remains agnostic regarding the sources of monopsony or product market power. In addition, it does not require defining the relevant market.

The remainder is structured as follows. Section 2 describes the firm-level data. Section 3 outlines the empirical strategy. Section 4 presents the results on how minimum wage effects vary with firms' competitive environment in labor and product markets. Section 5 concludes.

2 Data

We combine various representative statistics supplied by the German statistical offices. Firms are required by law to provide the respective information. The datasets are presented below.

Firm level dataset. We use yearly panel data on German manufacturing firms with at least 20 employees. Except for employment figures, which are declared as end of September, the data pertain to the respective calendar year.[2] The data include information on, among others, firms' costs, total sales, and investment. While employment (in headcount), wage costs, sales, and investment information is collected for the population of firms with at least 20 employees in the *Annual Report of Firms*, detailed cost and labor input information (e.g. employment in full-time equivalents (FTE)) is available only for a representative and stratified 40% sample (*Cost Structure Survey*), covering approximately 15,000 firms per year. This sample rotates every 4 years, most recently in 2012 and 2016. We use this detailed data for our production function estimations and main analysis, and therefore restrict the analysis sample to firms reporting from 2012 to 2015. For a long run analysis, we combine our baseline sample with information on employment (in headcounts) and wages spanning the years 2011-2017. We further exclude industries exempt from the minimum wage.[3] Our final sample consists of 9,565 firms per year.

Firm-product level dataset. We combine our firm-level data with the German PRODCOM data[4] that includes information on produced quantities and prices at establishment $\times$ ten-digit product level for all establishments of firms with a minimum of 20 employees in the manufacturing sector. The German product classification is based on the PRODCOM list which classifies industry production using an eight-digit numerical code where the first two digits refer to the statistical classification of economic activities (NACE). Examples of product categories are "Workwear – Long trousers for men, cotton", "Tin sheets and tapes, thicker than 0.2 mm", "Passenger cars, petrol engine $\leq$ 1,000 cubic centimeter". We utilize the price information in the product level dataset to construct a firm-specific price index following Eslava et al. (2004).[5]

Linked employer-employee dataset. In addition to our firm data, we use the German Structure of Earnings Survey (*AFiD-Modul Verdienste (VSE)*), a cross-sectional linked employer-employee dataset that covers the years 2001, 2006, 2010, and 2014 and comprises

2. Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, AFiD-Panel Industrieunternehmen (DOI: 110.21242/42221.2021.00.01.1.1.0

3. This applies 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).

4. 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

5. See Appendix E for a detailed description.

worker-level information on hourly wages.[6] We use only the 2014 wave of the VSE (corresponding to one year prior to the minimum wage becoming effective), which contains information for approximately 70,000 plants and 1 million employees from all economic sectors.[7] The sample is drawn in two steps. First, a sample of plants (not firms as in the case of our other datasets) is drawn from the population of plants with at least one employee. The second step includes a worker-level survey, either for the full workforce (plants with fewer than 10 employees) or for a randomly drawn sample of employees (all other plants). As far as the data permit, we exclude employees and industries exempt from the minimum wage.[8] Importantly, the direct link between the VSE and our firm-level datasets is limited, as all these datasets are independently drawn surveys, resulting in only small overlap between the statistics.

3 Empirical Framework

3.1 Measuring Market Power

We measure product market power using markups. In perfectly competitive markets, prices equal marginal costs, so any systematic deviation of price above marginal cost signals the presence of market power. De Loecker and Warzynski (2012) show that under the assumption that firms are price-taker on intermediate input markets and no adjustment costs for intermediate inputs, the markup μ can be inferred from the difference between a variable input's output elasticity and its revenue share:

$$\mu_{it} = \frac{P_{it}}{MC_{it}} = \frac{\epsilon_{it}^{M}}{\alpha_{it}^{M}} \tag{1}$$

where ϵ_{it}^{X} denotes the output elasticity of input factor X, α_{it}^{X} denotes the revenue share of input factor X. $\mu_{it} > 1$ indicates that firms charge prices above marginal costs. Intuitively, any gap μ between the output elasticity and the expenditure share of intermediate inputs indicates that the firm is making economic profits from its use of intermediate inputs which, on account of price-taking behaviour in the market for intermediates, must stem from above-competitive prices in the product market.

6. 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.)

7. The VSE data pertain to April 2014.

8. The VSE data allow us to identify apprentices and minors who are not eligible for the minimum wage. We further exclude industries with temporary exemptions from the minimum wage (cf. footnote 3).

Dobbelaere and Mairesse (2013) extend this methodology to also account for factor market imperfections. The approach relies on a variable input, which - unlike labor - is assumed unaffected by factor market frictions. The wage markdown is then defined as the ratio of labor's output elasticity to its revenue share, adjusted for the product market markup:

$$\gamma_{it} = \frac{MRPL_{it}}{w_{it}} = \frac{\epsilon_{it}^{L}/\alpha_{it}^{L}}{\epsilon_{it}^{M}/\alpha_{it}^{M}} \tag{2}$$

where ϵ_N^Q and ϵ_M^Q denote output elasticities of labor and intermediates, and w^N and z^M represent corresponding expenditures. Intuitively, under monopsony the economic profits stemming from the firms labor input dominate those from its intermediate inputs and thus result in a larger gap between the output elasticity of labor and labor expenditures than for intermediates. A value of γ above 1 indicates that firms pay workers below their marginal revenue product, reflecting monopsony power.

While the revenue shares α_{it}^X can be read directly from standard production data, the output elasticities of the inputs must be estimated using production functions.

Recovering Output Elasticities To recover output elasticities, we estimate firm-level production functions separately for each two-digit-sector. We assume a translog production function with second-degree interactions and estimate the output elasticities using the control function approach (Olley and Pakes 1996; Wooldridge 2009) based on the firm's input demand function for raw materials (a component of intermediate inputs). We further control for input and output price variation in the spirit of De Loecker et al. (2016). See appendix D for a detailed description of the production function estimation.

We average our derived market power measures over the pre-treatment years 2012 - 2014. Table 1 reports descriptive statistics for the estimated monopsony power and markup parameters in our sample. On average, firms pay wages equal to the marginal revenue product of labor, and they charge markups of roughly 10%.

Table 1: Market Power Parameters, 2013

Note: This table depicts descriptive statistics of the estimated firm monopsony power $\gamma_{i,pre}$ and markup parameter $\mu_{i,pre}$, averaged over the pre-treatment years 2012-2014, for our sample.

3.2 Treatment Definition

Similar to Draca et al. (2011), we use information on firms' average wages to define treated firms. Specifically, we use the annual wage bill per FTE averaged over the pre-treatment years 2012-2014 from the *Cost Structure Survey*. From this, we construct three wage categories:

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

We define firms with an average annual wage below €25,000 as highly exposed *low*-wage firms. Due to possible spillovers of the minimum wage to higher wage bins (Cengiz et al. 2019; Dustmann et al. 2022), we classify firms with an average annual wage between €25,000 and €40,000 as being moderately exposed (*med*). Firms with an average annual wage exceeding €40,000 constitute the control group of *high*-wage firms. We allocate approximately 12% of firms to the highly exposed group of low-wage firms, and 44% of firms to the moderately exposed group of medium-wage firms.

The accuracy of our treatment definition hinges on the wage spread within the firm and the distribution of low wage workers across the treatment categories. To validate our treatment indicators, we use information on workers' hourly wages from the 2014 wave of our linked employer-employee data (*VSE*) that can be matched to a subset of firms from our firm-level data.[9] First, we follow Draca et al. (2011) and B. T. Hirsch et al. (2015) and construct a precise measure of firm exposure to the minimum wage. We derive the firm-level gap measure using data from individual workers:

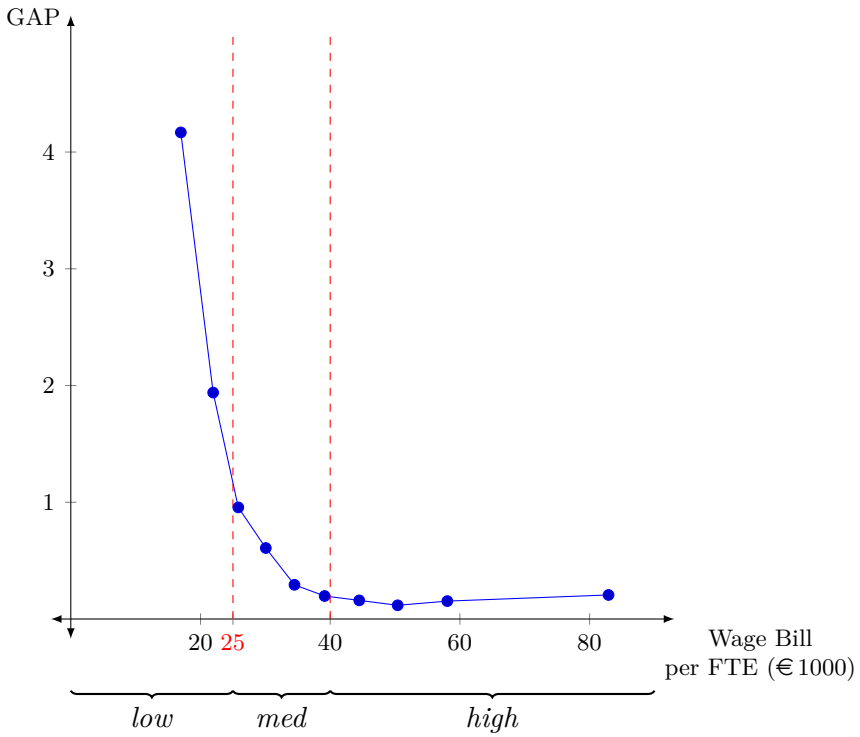
$$\text{GAP}_i = \frac{\sum_{j \in i} h_j \max\{w_{min} - w_j, 0\}}{\sum_{j \in i} w_j * h_j} \times 100 \tag{3}$$

where w_{min} depicts the minimum wage (€8.5), w_j the gross hourly wage for worker j in firm i and h_j the respective hours worked. The lower bound of the gap measure is zero for

9. The VSE contains wage information as of April 2014, which is three months before the minimum wage legislation passed parliament and eight months before the minimum wage was introduced. Potential anticipation effects are rather unlikely at this early point in time. Whereas the VSE is a plant-level survey, our other datasets are sampled at the firm level. To aggregate the plant-level exposure to firm-level exposure, we assume a uniform exposure across plants within a multiplant firm for which we observe only one plant and calculate the employment weighted average when we observe more than one plant of a firm.

unaffected firms. The measure depicts the percentage increase in a firms wage bill needed to comply with the minimum wage requirement, assuming that hours worked stay constant.

Figure 1: Gap (VSE) against Wage Bill per FTE.



Note: The y-axis shows the percentage increase in a firm's wage bill required to comply with the minimum wage requirement. The x-axis 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 treatment groups: $[min; €25{,}000)$; $[€25{,}000; €40{,}000)$; $[€40{,}000; max]$.

Figure **??** plots the average gap measure against the average wage, divided into ten equal-sized bins for the firms merged to the VSE data. The vertical dotted lines delineate the three groups of firms with differing levels of exposure. The gap measure increases sharply for firms with an average pre-policy annual wage below €25,000 and converges toward zero for firms with an average wage of €40,000. Moreover, minimum wage workers are concentrated in low-wage firms: 70% of all workers affected by the minimum wage are employed in low-wage firms, whereas only 9% are employed in high-wage firms. We therefore conclude that our firm group definitions reliably capture the extent to which firms are affected by the introduction of the minimum wage. This is consistent with existing studies that use average wages to proxy for firms minimum wage exposure (e.g. Draca et al. 2011; Hau et al. 2020).

Table **??** presents descriptive statistics for the three wage groups for 2013. Low-wage firms are smaller in terms of employment and output, less productive, and less intensive in their

use of intermediate inputs and capital. They employ a higher share of female and part-time workers, export less, and are more often located in East Germany compared with high-wage firms. In addition, low-wage firms are less likely to exhibit monopsony power or product market power. Approximately 25% of low-wage firms pay wages below the marginal revenue product of labor, whereas 45% of high-wage firms exercise labor market power. Furthermore, the average price - cost markup is 1.09 for low-wage firms and 1.15 for high-wage firms.

3.3 Difference-in-Differences

Our empirical design implements a triple difference framework at the firm level to estimate firm responses as a function of our treatment indicator and firms' pre-reform monopsony power. The main specification is:

$$\Delta y_{it} = \lambda_t + \delta \cdot \mathrm{T}_i + \delta_\theta \cdot \mathrm{T}_i \cdot \theta_{i,pre} + \mathbf{X}'_{it}\beta + \varepsilon_{it} \tag{4}$$

where Δy_{it} is the change in the variable of interest relative to 2013, $T_i \in \{low, med, high\}$ is the treatment indicator. $\theta i, pre \in \{\gamma_{i,pre}, \mu_{i,pre}\}$ denotes firm's pre-policy monopsony and product market power, respectively. We estimate specifications using both a binary measure of firm monopsony power, $\gamma_{i,pre} > 1$, which indicates that firms pay wages below their marginal revenue product of labor, and a continuous measure, $\log \gamma_{i,pre}$, where the intercept corresponds to $MRPL = w$. For product market power, we rely solely on a continuous log interaction, as pricing below marginal cost is rare and economically implausible.

X_{it} includes state and two-digit industry controls. Because of potential anticipation effects, we use 2013 as the base year. The coefficient of interest, δ, captures the differential change in y for treated firms relative to non-treated firms without market power. The coefficient δ_θ then measures the differential change in y between firms with the same treatment status but differing levels of ex-ante market power.

4 Results

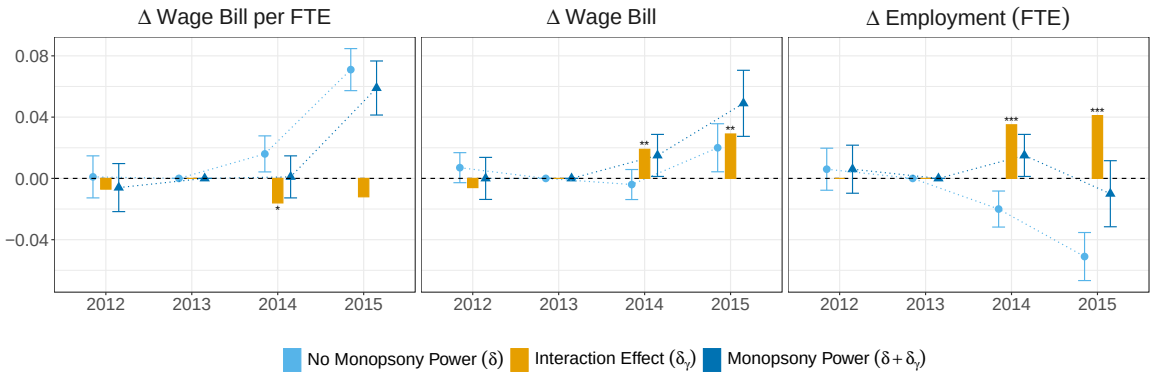
4.1 Low vs. High-Wage Firms

Wages and Employment Figure 6 reports the marginal effects of minimum wage exposure on wage bill per FTE, employment in FTE, and total wage bill for firms with and without pre-policy monopsony power ($\gamma_{i,pre} \lessgtr 1$), derived from the regression specification in Equation (4).

We document strong positive effects of the minimum wage on wage costs in manufacturing among low- and medium-wage firms after the introduction of the minimum wage in 2015. Low-wage firms with ex-ante monopsony power ($\gamma > 1$) raise wages per FTE by 7.6%, whereas low-wage firms without ex-ante monopsony power ($\gamma \leq 1$) increase wages per FTE by 5.7%, both relative to 2013 and the control group. The difference between low-wage firms with and without ex-ante monopsony power is not statistically significant. Low-wage firms operating in competitive labor markets exhibit employment reductions relative to the control group. For these firms, employment declines by 4.8% in low-wage firms. In contrast, the employment effects for firms with pre-policy monopsony power are either statistically insignificant and smaller in magnitude (-2%). The observation that firms without monopsony power reduce employment, while firms with monopsony power exhibit smaller or even positive employment responses, is consistent with a moderating effect of monopsony power on the employment consequences of the minimum wage. The total wage bill of low-wage firms increased by 3.7% and 2.7% for firms with and without ex-ante monopsony power, respectively. In all cases, the wage increase outweighs the employment reduction, resulting in higher overall wage costs.

It is important to note that we do not observe pre-trends in wages per FTE or employment for any of the firm groups.

Figure 2: Effect on Wages and Employment by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Low vs. High-Wage Firms.



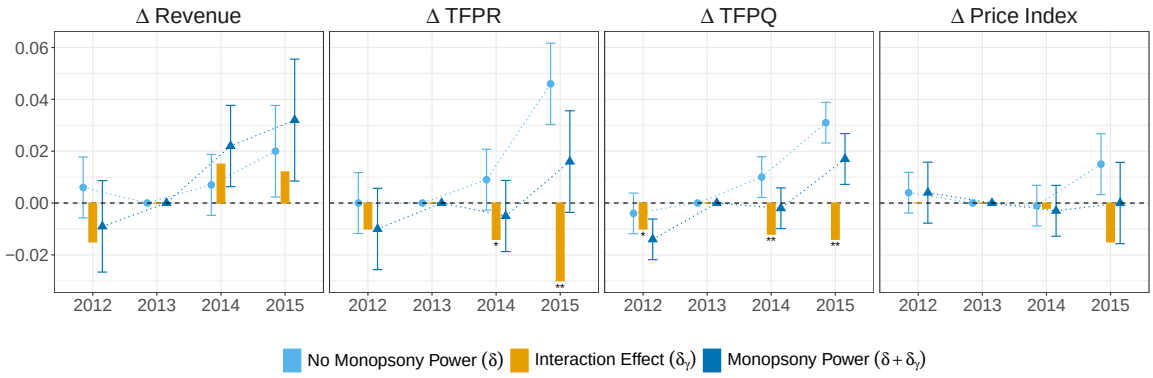
Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in wage bill per FTE (col.), employment in FTE (col. 2), and total wage bill (col. 3) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Output, Productivity, and Prices Under perfectly competitive labor markets, firms reduce both employment and output in response to the minimum wage. In contrast, models of monopsony power predict that firms expand production, as the minimum wage induces

them to hire more workers (see e.g. Bhaskar et al. 2002; Manning 2003).

Consistent with monopsony theory, monopsonistic medium-wage firms increase labor in response to the minimum wage, expand production (see Figure 3) and raise their use of intermediate and capital inputs (see Figure A1) - they scale up. The effect of the minimum wage on production output and total inputs for low-wage firms with monopsony power is positive, albeit statistically insignificant and imprecisely estimated. Both medium- and low-wage firms with monopsony power do not raise prices significantly after the introduction of the minimum wage. Their competitive counterparts, however, partly pass on the increase in labor costs to the consumer and increase prices by 1.2%. At the same time, firms without ex-ante monopsony power are able to produce the same output with lower labor input.[10] Together with unchanged intermediate and lower capital inputs (see Figure A1), this results in an increase in TFPR and TFPQ. This pattern is found to be less pronounced for medium-wage compared to low-wage firms without monopsony power. One potential explanation for this finding is that firms without monopsony power lack the employment rents of their monopsonistic competitors and thus face stronger competitive pressure to improve productivity and pass on higher labor costs to consumers when confronted with minimum wages.

Figure 3: Effect on Output, Productivity, and Prices by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Low vs. High-Wage Firms.



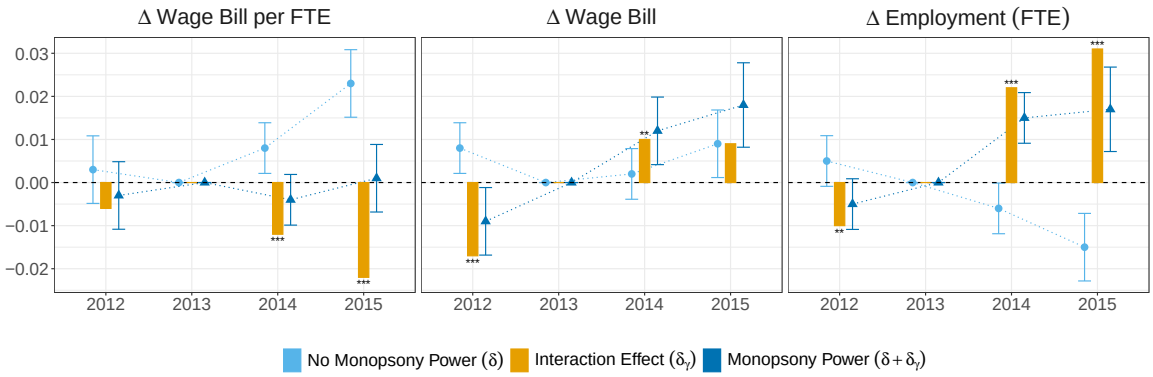
Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in revenues (col. 1), TFPR (col. 2), TFPQ (col. 3), and the firm price index (col. 4) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

10. Low-wage firms without ex-ante monopsony power experiences a relative *increase* in revenues, that however is largely driven by higher prices.

4.2 Medium vs. High-Wage firms

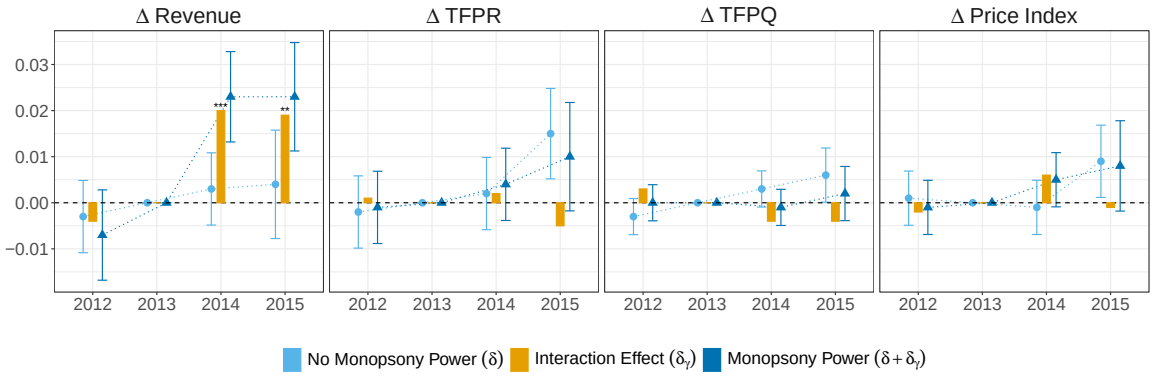
- medium-wage monopsonistic firms increase employment in response to the minimum wage → confirms nonlinearities in the response to minimum wage exposure predicted by monopsony models

Figure 4: Effect on Wages and Employment by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Medium vs. High-Wage Firms.



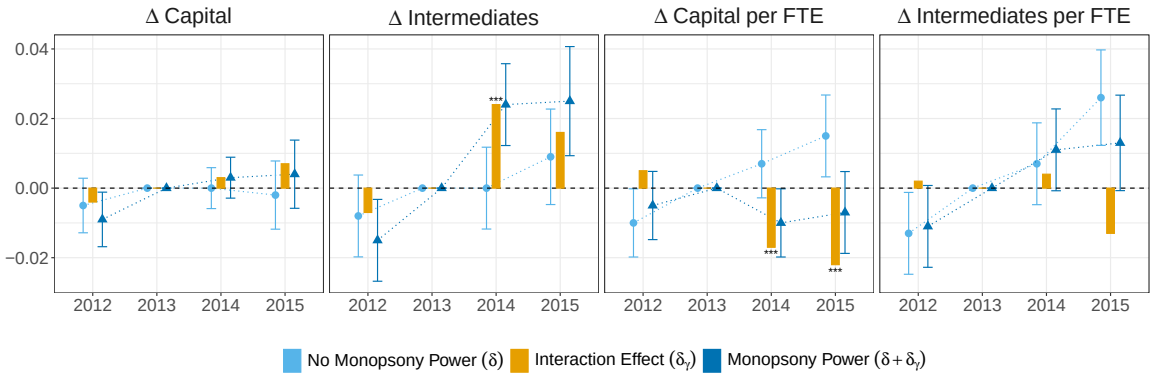
Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in wage bill per FTE (col.), employment in FTE (col. 2), and total wage bill (col. 3) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 5: Effect on Wages and Employment by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Medium vs. High-Wage Firms.



Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in wage bill per FTE (col.), employment in FTE (col. 2), and total wage bill (col. 3) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Effect on Wages and Employment by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Medium vs. High-Wage Firms.



Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in wage bill per FTE (col.), employment in FTE (col. 2), and total wage bill (col. 3) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Robustness

Continuous measure In addition to using a binary indicator of firm monopsony power, we also estimate regression equation 4 using a continuous measure of pre-policy monopsony power. More specifically, we use the log value of $\gamma_{i,pre}$.[11] In Figures **??** to **??** in the Appendix, we plot the marginal effects of minimum wage exposure for values of $\gamma = 1$, which is equal to firms paying wages according to the marginal revenue product of labor, and values of $\gamma = 1.6$, which roughly corresponds to the value of the 90th percentile of our sample. Our findings are robust to this alternative specification: Low-wage firms without monopsony power - those with $\gamma_{i,pre} = 1$ - experience a significant increase in wage costs and a significant decrease in employment after the introduction of the minimum wage. In contrast, low-wage firms with high values of monopsony power exhibit smaller employment declines, and this effect becomes less statistically significant. The effect on productivity - both TFPR and TFPQ - is positive for both types of firms, but it is more pronounced for low-wage firms operating in competitive markets. Medium-wage firms with monopsony power increase employment significantly after the reform and scale-up in terms of production output and both capital and intermediate inputs, whereas medium-wage firms without monopsony power show no employment effect, and the increase in revenues seems to be largely driven by an increase in output prices.

11. Using the log value of $\gamma_{i,pre}$ has the appealing property that it equals zero when $MRPL = w$. Thus, the coefficients on the treatment variable measure the effect of treatment for firms that pay workers their marginal revenue product.

Firm Size

Long-Run Analysis A limitation of our previous analysis is the short observation window for which detailed information on firms production inputs and outputs is available. This restricts our ability to examine pre-trends comprehensively and to assess whether the employment and output effects persist in the long run. We therefore augment our baseline sample with information on employment and wages for the universe of manufacturing firms with 20 or more employees from the *Annual Report of Firms*. It is important to note that in this dataset, employment is measured in headcounts rather than FTEs, as in our baseline specification. Figure 7 presents the marginal effects of treatment status by ex-ante monopsony power on wages and employment. Consistent with our main analysis, low-wage firms experience a pronounced increase in wages per worker following the introduction of the minimum wage in 2015. This effect persists, with wages per worker continuing to rise in the subsequent years. Importantly, this increase was not preceded by differential wage growth prior to the reform. Medium-wage firms, by contrast, exhibit only a modest increase in wages per worker, which becomes statistically significant only in 2017 for firms without monopsony power.

Turning to employment effects, we again find that low-wage firms with monopsony power do not experience a significant relative decline after the minimum wage introduction, whereas low-wage firms without monopsony power contract. However, for the latter group, the contraction begins before the minimum wage was implemented. We therefore cannot rule out that this decline is unrelated to the minimum wage. Similarly, medium-wage firms with monopsony power appear to increase employment throughout the entire period under consideration. One factor that may account for the discrepancy in the findings is the difference in how employment is measured in the two datasets.

Figure 7: Long Run: Effects on Wages and Employment by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$).

Note: Marginal effects (with 95% CI) of minimum wage exposure on change in wage bill per FTE (row 1), employment in FTE (row 2), and total wage bill (row 3) for firms with ($\gamma_{i,pre} > 1$) and and without ($\gamma_i, pre \leq 1$) ex-ante monopsony power following regression equation 4. We combine our baseline sample with information on wages and employment from the *Annual Report of Firms*. Column (1) depicts the results for low-wage firms, and column 2 depicts the results for medium-wage compared to high-wage firms. We control for industry and region specific developments.

5 Conclusion

We study firms' adjustment strategies to the minimum wage in environments with varying degrees of competition. Using state-of-the-art production function estimations and high-quality administrative data from the German Statistical Office, we estimate firm-level monopsony and product market power. We document substantial increases in wage costs among affected firms following the introduction of the minimum wage. Consistent with the monopsony hypothesis, firms with ex-ante monopsony power exhibit attenuated employment responses to the minimum wage, which even turn positive among only moderately affected firms. An analysis over a longer period, however, uncovers pre-trends in employment dynamics. Moreover, firms facing stronger competitive pressure increase productivity and pass on part of the rise in labor costs to consumers. Contrary to our expectations, however, we do not find conclusive evidence of differential price pass-through or employment effects across firms with varying degrees of product market power.

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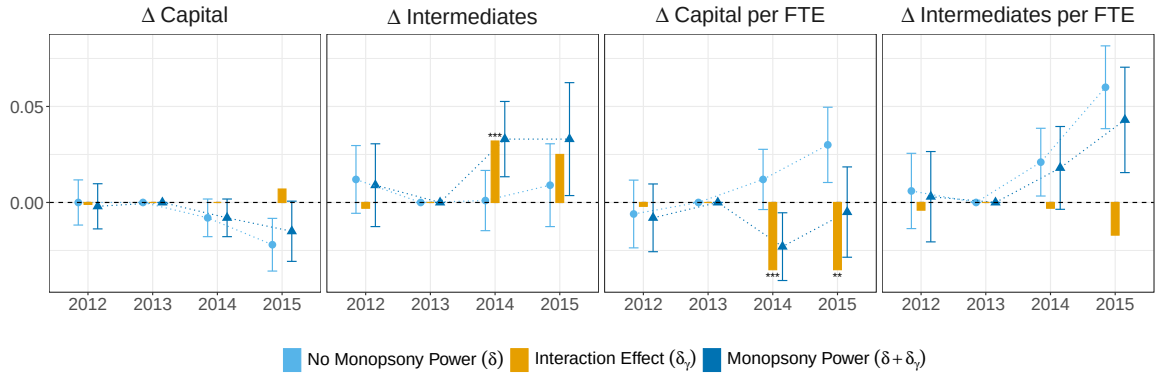
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A Tables and Figures

Figure A1: Effect on Intermediate Inputs and Capital Stock by Ex-Ante Monopsony Power ($\gamma_{pre} \lessgtr 1$), Low vs. High-Wage Firms.



Note: Marginal effects with 95% CI (in blue) and interaction term with significance levels (orange bar) of minimum wage exposure on change in revenues (col. 1), TFPR (col. 2), TFPQ (col. 3), and the firm price index (col. 4) for firms with ($\gamma_{i,pre} > 1$) and without ($\gamma_{i,pre} \leq 1$) ex-ante monopsony power following regression equation 4. Low-wage firms compared to high-wage firms. We controls for industry and region specific developments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Institutional Background

The introduction of a statutory minimum wage was a central topic in the federal election campaign in 2013. The demand for a minimum wage of €8.50 was most prominently brought into the election campaign by the social democratic party (SPD) in opposition to the conservative (CDU/CSU) and liberal (FDP) parties. However, after the federal election in September 2013, the minimum wage was decided upon in the coalition agreement between the SPD and CDU/CSU. The coalition agreement including the €8.50 minimum wage was signed in November 2013, and the law was passed in parliament in July 2014.

The general statutory minimum wage became effective in Germany on January 1st, 2015, and was introduced at a level of €8.50 gross per hour. The minimum wage is continuously adjusted by a minimum wage commission, which consists of representatives from employer and employee associations. The minimum wage was raised to €8.84 effective January 1st, 2017, and increased to €10.45 on July 1st, 2022. With the change in government in 2021, it was decided to increase the minimum wage to €12 on October 1st, 2022. Prior to 2015, several sector-specific minimum wages were in place. Sectors with existing minimum wages below €8.50 were granted a transition period through January 2017. Transitional arrangements apply to the following employees: meat industry workers, hairdressers, contract workers, laundry service providers for large customers, agriculture and forestry workers, textile industry workers and horticulture workers. 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 persons 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 non-compliance 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; Caliendo et al. 2023; Dustmann et al. 2022), there is mixed evidence on compliance with the minimum wage law in Germany. On the one hand, using 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 became 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 in the order of magnitude that is to be expected under full compliance with the minimum wage law, and Biewen et al. (2022) present evidence that compliance with the minimum wage level was achieved to a large extent by 2018.[12]

C 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):

$$K_{it} = K_{it-1}(1 - \alpha_{jt-1}) + I_{it-1}, \tag{C1}$$

where K_{it}, α_{jt} and I_{it-1} denote firm i's capital stock, the depreciation rate of capital, and investment. Investment captures firms total investment in buildings, equipment, machines, and other investment goods. Nominal values are deflated by a 2-digit industry-level deflator supplied by the German statistical office.

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:

$$LT = \alpha \int_0^\infty (1 - \alpha)^t t dt. \tag{C2}$$

12. 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, this suggests that our measure of labor input is unlikely to be downward biased for affected firms.

As the lifetime of capital goods is separately given for years and capital good types (buildings and equipment), we solve this equation for each year and capital good type separately. 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 the practical implementation, we assume that the depreciation rate of a firms whole capital stock equals the depreciation rate of newly purchased capital.

For the manufactrung sector, 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. Given that firms likely tend to report too high depreciations due to such tax incentives, 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, mitigating the impact of the first capital stock.[13] We do not observe reported tax depreciation for the service sector and therefore merge information on the book value of firms' total fixed assets from Amadeus.

Given that we estimate very reasonable output elasticities from our production function estimation (see Table D1), we are confident that our capital variables reliably reflect firms' true capital stocks.

D Estimating firm-level production functions

Starting point. For estimating firm-level production functions, we rely on an 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 consider that firms manufacture quantities (Q_{it}) by combining intermediate (M_{it}), labor (L_{it}), and capital (K_{it}) inputs. Quantity-based productivity ($e^{\omega_{it}}$) is Hicks-neutral and we assume the following flexible translog production function in logs (smaller letters indicate logs):

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + \omega_{it} + \epsilon_{it}. \tag{D1}$$

13. We therefore calculate capital stocks, whenever available, from 2009 onwards.

The vector ϕ'_{it} captures a second polynomial in production inputs (l_{it}, m_{it}, and k_{it}) with an additional full interaction between all three production factors.[14] We define labor as full-time equivalents, capital as deflated capital stocks (see Appendix C for our derivation of capital stocks), and intermediate inputs as deflated intermediate input expenditures. The two-digit industry deflators are supplied by the statistical offices.

There are three identification issues that prevent a direct estimation of equation (D1) by OLS. First, productivity is unobserved to the econometrician but known to the firm. This causes a simultaneity biases if firms' flexible production factors adjust to productivity shocks. Second, to recover a quantity-based productivity measure, we must estimate the quantity-based production function specified in (D1). Yet, although we observe product quantities for the individual products of firms, we cannot aggregate various quantities of products within firms. Third, we do not observe input prices for all production inputs. If unobserved input prices are correlated with input decisions and physical output, we face another identification issue. In the following, we describe how we solve all these identification issues for the manufacturing and service sector, respectively.

Defining an output quantity measure. As we cannot directly aggregate output quantities across multiple products, we follow Eslava et al. (2004) in calculating a firm-specific output price index from observed product price changes within firms for the manufacturing sector. We describe the methodology of constructing this Törnqvist price index in Appendix E. Having calculated firm-specific price indices, we purge price variation from observed revenue information for all firms by deflating observed revenue with this price index. This yields quasi-quantity measures of output and with a slight abuse of notation, we keep using Q_{it} for these quasi-quantities. This approach of using quasi-quantities has been recently adopted in a series of studies (e.g., Smeets and Warzynski 2013, Eslava et al. 2013, Carlsson et al. 2021).

Using a control function for unobserved input prices. To account for unobserved input price variation, we apply a firm-level-analogue of De Loecker et al. (2016) and formulate an input price control function from observed information on output prices, product market shares, firm location, and firm industry affiliation. Specifically, we add the following control function to the production function equation (D1):

14. Hence, $q_{it} = \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \beta_{ll} l_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{lk} l_{it} k_{it} + \beta_{mk} m_{it} k_{it} + \beta_{lm} l_{it} m_{it} + \beta_{lmk} l_{it} m_{it} k_{it} + \omega_{it} + \epsilon_{it}$.

$$B(.)_{it} = B((p_{it}, ms_{it}, A_{it}, I_i) \times \boldsymbol{\phi}^c_{it}). \tag{D2}$$

Comments on the notation are in order. $B(.)_{it}$ denotes a price control function consisting of the logged firm-specific output price index (p_{it}), a logged weighted average of firms' product market shares in terms of sales (ms_{it}), a headquarter location dummy (A_{it}), and a four-digit industry dummy (I_{it}). $\boldsymbol{\phi}^c_{it} = \{1, \boldsymbol{\phi}_{it}\}$, where $\boldsymbol{\phi}_{it}$ includes the same input terms as ϕ_{it}. The constant entering $\boldsymbol{\phi}^c_{it}$ highlights that elements of $B(.)_{it}$ enter the price control function linearly and interacted with $\boldsymbol{\phi}_{it}$, which is a consequence of the translog production model.

In our practical implementation, we cannot allow for all possible interactions within the price control function. To preserve a meaningful parameter space, we approximate $B(.)_{it}$ by interacting the output price index with the production inputs in $\boldsymbol{\phi}_{it}$ and add the output price index, market shares, and location and headquarter dummies linearly.

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

A notable difference between the original approach of De Loecker et al. (2016) and our approach is that De Loecker et al. (2016) estimate product-level production functions. We transfer their framework to the firm-level. To do so, we use firm-product-specific sales shares in firms total product market sales to aggregate firm-product-level information to the firm-level. Therefore, we assume that i) sales-weighted firm aggregates of product quality increase in firm 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 equal across the various products of a firm. These assumptions, or

even stricter versions of them, are always implicitly invoked when estimating firm- instead of product-level production functions.

Finally, note that even if some of the above assumptions do not hold, including the price control function is nevertheless preferable to omitting it. This is because the price control function can still absorb some of the unobserved price variation and does not require that input prices vary between firms with respect to all elements of $B(.)_{it}$. The estimation can regularly result in coefficients implying that there is no price variation at all. The attractiveness of a price control function lies in its agnostic view about existence and degree of input price variation.

As we are missing information on product prices for the service sector firms, we control for firms' regional and industry-level market shares in terms of employment and sales when estimating production functions for the service sector.

Controlling for unobserved productivity. We address the dependence of firms' flexible input decisions on unobserved productivity using a control function approach in the spirit of Olley and Pakes (1996) and Levinsohn and Petrin (2003). We base our control function on firms' demand function for raw materials (denoted by e_{it} in logs) which we separately observe as parts of total intermediate inputs. Inverting the demand function for e_{it} yields an expression for productivity:

$$\omega_{it} = g(.)_{it} = g(e_{it}, k_{it}, l_{it}, \mathbf{\Gamma}_{it})). \tag{D3}$$

$\mathbf{\Gamma}_{it}$ captures additional state variables that affect firms' demand for e_{it}. $\mathbf{\Gamma}_{it}$ should include a broad set of variables that affect demand for e_{it}. We include a dummy variable for export activity (EX_{it}), the log of the number of products a firm produces ($NumP_{it}$) (for manufacturing), a dummy for R&D activity (RD_{it}), and the average wage the firm pays into $\mathbf{\Gamma}_{it}$. The latter controls for input prices (we assume that input prices are correlated across inputs) and helps to absorb unobserved quality and price differences that shift demand for e_{it}.

We assume that productivity follows a Markov process and allow firms to shift this process. This motivates 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 $\boldsymbol{Z}_{it} = (EX_{it}, NumP_{it}, RD_{it})$ reflects that we allow for productivity being affected by export market participation, R&D

activity, and (dis)economies of scope resulting from adding or dropping products.[15]

Inserting equation D2, D3, and the law of motion for productivity into the production function finally yields:

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + B(.)_{it} + h(.)_{it-1} + \zeta_{it} + \epsilon_{it}, \tag{D4}$$

which forms the basis for our estimation.[16]

Identification. We estimate equation D4 separately by two-digit NACE rev. 2 industries for the years 1999 to 2017 using a one-step estimator as in Wooldridge (2009). This estimator uses lagged values of flexible inputs (in our case intermediates) as instruments for their contemporary values to address the endogeneity resulting from firms' flexible input decisions on realizations of ζ_{it}.[17] Similarly, we rely on lagged values for market shares and output price indices as instruments for present values because we consider these to be flexible variables as well. We define identifying moments jointly for ζ_{it} and ϵ_{it}:

$$E_{it} = ((\zeta_{it} + \epsilon_{it})\boldsymbol{\Upsilon}_{it}), \tag{D5}$$

where $\boldsymbol{\Upsilon}_{it}$ contains lagged interactions of intermediate inputs with labor and capital, contemporary interactions of capital and labor, contemporary location and industry dummies, lagged market shares, the lagged output price index, elements of $h(.)_{it-1}$ (which are lagged), and lagged interactions of the output price index with production inputs. Formally:

$$\boldsymbol{\Upsilon}_{it} = (\boldsymbol{J(.)}_{it}, \boldsymbol{V(.)}_{it-1}, \boldsymbol{\Xi(.)}_{it-1}, \boldsymbol{\Psi(.)}_{it-1}, \boldsymbol{\Lambda(.)}_{it-1}), \tag{D6}$$

15. Note that $\boldsymbol{Z}_{it}$ and $\boldsymbol{\Gamma}_{it}$ contain partly the same variables. This is not a problem as we are not interested in identifying the coefficients from the control function.

16. We approximate $h(.)_{it-1}$ with a full third order polynomial in all of its elements, except for the variables in $\boldsymbol{Z}_{it}$ and $\boldsymbol{\Gamma}_{it}$. Those we add linearly.

17. We model capital and labor as quasi-fixed factors. Input timing assumptions always depend on the specific setting. We treat labor as quasi-fixed as Germany is characterized by relatively rigid labor markets and because the labor variable is defined by the September value, whereas all other variables pertain to the full calendar year. Also other studies rely on quasi-fixed labor (e.g., De Loecker et al. 2016).

where we defined:

$$\boldsymbol{J(.)}_{it} = (l_{it}, k_{it}, l_{it}^2, k_{it}^2, lk_{it}, A_{it}, I_i),$$

$$\boldsymbol{V(.)}_{it} = (m_{it}, m_{it}^2, l_{it}m_{it}, m_{it}k_{it}, l_{it}m_{it}k_{it}, ms_{it}, p_{it}),$$

$$\boldsymbol{\Xi(.)}_{it} = (m_{it}, l_{it}, k_{it}, m_{it}^2, l_{it}^2, k_{it}^2, l_{it}m_{it}, m_{it}k_{it}, l_{it}k_{it}, l_{it}m_{it}k_{it}) \times p_{it}),$$

$$\boldsymbol{\Psi(.)}_{it} = (e_{it}, l_{it}, k_{it}, e_{it}^2, l_{it}^2, k_{it}^2, e_{it}^3, l_{it}^3, k_{it}^3, l_{it}e_{it}, e_{it}k_{it}, l_{it}k_{it}, l_{it}e_{it}k_{it}, k_{it}^2e_{it}, k_{it}^2l_{it}, l_{it}^2e_{it}, l_{it}^2k_{it}, e_{it}^2k_{it}, e_{it}^2l_{it}),$$

$$\boldsymbol{\Lambda(.)}_{it} = (EX_{it}, NumP_{it}, w_{it}),$$

where w_{it} denotes the average wage per FTE a firm pays. Note that the time frame which we use for the production function estimation is much longer (1999-2017) than the period we use to study the minimum wage effects (2012-2015). We utilize the longer time span as the production estimation requires a sufficiently large amount of observations to produce stable results. Due to having a sufficiently large sample of firms for the estimation, our production function routine does not allow for changing production function parameters before and after the minimum wage introduction. Yet, the flexible translog specification we employ still accounts for changes in firms' output elasticities due to changes in relative factor prices.[18]

Results. Table D1 shows median of the estimated output elasticities by two-digit industries. Recap that output elasticities are firm- and time-specific due to the translog production function. Overall, the results look meaningful and are comparable to other studies (e.g., De Loecker et al. 2016). 10% of our firm-year observations display a negative output elasticity with respect to at least one production factor. We drop these firms from our analysis, as these are not consistent with the underlying production model.

18. Ideally, one would like to estimate firm-specific and year-specific production functions, but this is not feasible due to data limitations. Particularly, the production function routine requires a sufficiently large amount of observations in each industry. We thus face a trade-off between flexibility and consistency of results.

Table D1: Production Function Estimation: Median Output Elasticities, by Sector

Sector	Capital	Intermediate Inputs	Labor	Returns to Scale	γ	μ	Number of Observations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)

Note: Median output elasticities calculated after estimating the production function (D1) for every NACE rev. 2 two-digit industry with sufficient observations for the years 1999 to 2017. Column 7 reports the number of observations used to calculate output elasticities for each industry. Columns 1,2, and 3 respectively report median output elasticities for capital, intermediate, and labor inputs. Column 4 reports median returns to scale. Column 5 and 6 report the median monopsony power γ and markup parameter μ. The production function estimation routine controls for time dummies.

E Price Index

We calculate a firm-level Tornqvist price index from product-level price changes weighted by the products' revenue shares in the firms' total product output, following Eslava et al. (2004):

$$P_{it} = \prod_{g=1}^{n} \left(\frac{price_{igt}}{price_{igt-1}}\right)^{\frac{1}{2}(s_{igt}+s_{igt-1})} P_{it-1}, \tag{E1}$$

where $price_{igt}$ is the price of good g and s_{igt} is the share of this good in the total sales of firm i in period 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 an industry-average as starting value for the price index series. Similarly, we follow Eslava et al. (2004) and impute missing price index values with an industry average.[19]

19. For 30% of firms, the statistical offices do not collect quantity and thus output price information. This is because the statistical offices do not view the respective information as a meaningful quantity measure for the associated products.