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IZA DP No. 16160

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ABSTRACT

Minimum Wages, Productivity, and Reallocation*

We study the productivity effect of the German national minimum wage by applying administrative firm data. At the firm level, we confirm positive effects on wages and negative employment effects and document higher productivity even net of output price increases. We find higher wages but no employment effects at the level of aggregate industry×region cells. The minimum wage increased aggregate productivity in manufacturing. We do not find that employment reallocation across firms contributed to these aggregate productivity gains, nor do we find improvements in allocative efficiency. Instead, the productivity gains from the minimum wage result from within-firm productivity improvements only.

JEL Classification: L11, L25, J31, D24

Keywords: minimum wage, firm productivity, output prices, factor reallocation

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1 Introduction

Accompanied by intense public and academic debates, a national minimum wage was introduced in Germany on January 1st, 2015, for the first time in the country's history. The minimum wage was set to €8.50 per hour, and half a year prior to the policy's introduction, hourly wages of around 15 percent of German workers were below that threshold (Dustmann et al. 2022). Being introduced during the long-run boom that the German economy had witnessed since the end of the great financial crisis, the minimum wage did not cause any sizeable reductions in employment (Bossler and Gerner 2020; Caliendo et al. 2018) but reallocated employment toward higher-paying firms (Dustmann et al. 2022).

Absent sizeable employment reductions, a key question is whether employers or consumers shoulder the burden of the minimum wage or, instead, productivity gains compensate for its costs. For given productivity and employment levels, whether employers lose economic rents and see their profits decline depends on whether they can pass on the costs of the minimum wage to their customers. Without explicitly analyzing productivity effects, Harasztosi and Lindner (2019) show for Hungary that consumers paid 75% of a minimum wage increase whereas firm owners paid 25%. As soon as the minimum wage triggers productivity improvements in the affected firms, however, any adverse effects on workers, employers, or consumers can be reduced or even reversed into positive effects. Firm-level estimates for China (Hau et al. 2020) and Vietnam (Nguyen 2019) indeed imply positive productivity effects, whereas Bossler et al. (2020a) do not find effects on sales per worker in German firms.[1]

Beyond such firm-level results, little is known about the market-level productivity effect of the minimum wage. Such aggregate productivity changes might be generated by employment (market share) reallocation between producers of different productivity levels (Olley and Pakes 1996), potentially coupled with asymmetric productivity changes by initial size or productivity level. Without directly observing productivity, Dustmann et al. (2022) report employment reallocation toward firms with higher *predicted* initial productivity and conclude that allocative efficiency improved. Despite the extensive research into the effects of a minimum wage, there are no studies analyzing its aggregate productivity effects using micro data on firm productivity. Our study fills this gap.

[1] Clemens (2021) additionally discusses adjustment in noncash benefits, job attributes such as training offerings, and increased work effort. We focus on productivity and price adjustments. For the German case, Bossler and Broszeit (2017) show that effort levels have not been affected by the minimum wage, and Bossler et al. (2020a) report that training intensity decreased only slightly.

The firm-level productivity, price, and reallocation effects of the minimum wage have mostly been studied in isolation. This makes it impossible to, for instance, understand whether revenue productivity effects result from changes in output prices and whether firm-level productivity gains translate into aggregate productivity gains from factor reallocation. Our study is the first to provide a holistic view of all these effects and does so in the context of the implementation of a nationwide minimum wage in the largest European economy. We also present the first study on a major Western economy that utilizes high-quality production and wage data covering output, investments, intermediate inputs, wages per full time equivalent (FTE) workers and, for a subsample, even hourly wages at the worker level. Moreover, we provide the first productivity study utilizing large-scale data on prices and quantities at the granular product level, allowing us to study whether any revenue productivity effect is driven by changes in prices or quantities produced. Whereas many previous studies rely on survey data and thus are plagued by issues such as unit nonresponse and small sample sizes, we leverage the high-quality administrative data gathered through compulsory firm reporting and provided by the German statistical offices.

At the micro level, we employ a difference-in-differences framework covering three years before and one year after the introduction of the minimum wage to capture preintroduction trends and short-run effects. We find strong positive effects on wages per FTE worker in manufacturing (+6.5%) and services (+14%). These effects go hand in hand with mild negative effects on employment in manufacturing (-3.7%) and service sector firms (-3.5%). Combined, these wage and employment effects yield an increase in the total wage bill (+2.9% in manufacturing and +10.7% in services). These results are in line with findings on the effects of the German minimum wage based on social security data (Dustmann et al. 2022) and employer surveys (Bossler et al. 2018).

A first striking new result is that affected firms *increased* their revenues relative to those of the control group by 2.5% and 4% in the manufacturing and service sectors, respectively, despite reducing employment. Likewise, we find an increase in affected firms' value added relative to the control group by 1.9% (manufacturing) and 7.1% (services). The effect on value added per FTE worker is even more substantial: in the manufacturing and service sectors, the labor productivity effects amount to 5.6% and 10.6%, respectively. These strong productivity improvements in affected firms likely mitigated any employment and output price adjustments. They are therefore key to understanding why the introduction of the

German minimum wage has been found to have very limited employment effects at the macro level. Potential productivity responses have also been mostly absent in the argumentation of economists who feared strong negative effects of the German minimum wage. Importantly, one-half of the strong difference-in-difference effect for the service sector is explained by a sudden labor productivity reduction in the control group, which suggests that the minimum wage-induced labor inflow in those firms resulted in a decline in their average productivity.

Labor productivity can be boosted by total factor productivity (TFP) gains or by increasing reliance on nonlabor inputs. We do not find any effects of the minimum wage on investments per FTE. However, we find that relative to firms in the control group, affected firms are more intermediate-input intensive, which may partly explain the rise in output. Our detailed firm-product-level data for the manufacturing sector reveal that the direct effect of the minimum wage on firms' revenue TFP (TFPR) equals 3.1% whereas quantity TFP (TFPQ) increased by 2.2% more in the treated than in the control group. Output prices rose by approximately 1% more in the treatment group. We conclude that the firm-level labor productivity gains are driven by true efficiency (TFPQ) improvements.

Having established these firm-level results, we study how reallocation affects aggregate productivity within industry-region cells. The idea that factor reallocation is a key engine of growth dates at least back to Schumpeter (1942) and features prominently in a variety of growth and trade models (Grossman and Helpman 1991; Melitz 2003). In these models, reallocation is beneficial because production factors move from less productive to more productive firms thereby raising aggregate productivity. Confirming Dustmann et al. (2022), we do not find any employment effects at the level of aggregate industry×region cells. Whereas the aggregate effects on wages and productivity are close to zero in the service sector, they are positive in German manufacturing: for each percentage point that a region needed to raise wages to meet the minimum wage requirement, there is a 1.4% increase in manufacturing labor productivity. These aggregate productivity gains are completely driven by improvements in within-firm productivity. Applying established methods to measure the contribution of reallocation to aggregate productivity (Olley and Pakes 1996; Kehrig and Vincent 2021) and to allocative efficiency (Hsieh and Klenow 2009; Petrin and Sivadasan 2013), we do not find any evidence that the minimum wage induced productivity-enhancing reallocation or that it increased allocative efficiency.

Our paper relates to several strands of the literature. First, it speaks to the firm-level literature on the productivity effects of the minimum wage. Recent analyses of the effects of a minimum wage on capital-labor substitution and productivity include Bossler et al. (2020a), Hau et al. (2020), Mayneris et al. (2018), Nguyen (2019), and Riley and Bondibene (2017).[2] Analyzing the impact of the Chinese minimum wage on manufacturing firms, Hau et al. (2020) and Mayneris et al. (2018) find increases in labor productivity and TFPR. Hau et al. (2020) find evidence for capital-labor substitution due to fewer workers operating with constant capital, whereas Mayneris et al. (2018) report increased investments in physical capital and reductions in employment. Supporting these findings, Nguyen (2019) documents rising labor productivity and TFPR in Vietnamese manufacturing firms more affected by the minimum wage and finds capital-labor substitution. Not restricting the sample to manufacturing firms, Riley and Bondibene (2017) report positive effects on labor productivity and TFPR in the UK but find no evidence for capital-labor substitution. However, a major shortcoming of their data is that they rely on proxies for value added instead of a proper direct measure. Using an employer survey covering 1% of German establishments, Bossler et al. (2020a) find no impact of the German minimum wage on sales per worker and no evidence for capital-labor substitution. Bossler et al. (2020a) do not consider value added labor productivity and do not report TFP estimates. Our study therefore presents the first large scale evidence for a major Western economy that utilizes high-quality administrative data on productivity. While confirming the positive labor productivity effects found for manufacturing firms in China and Vietnam (Hau et al. 2020; Mayneris et al. 2018; Nguyen 2019), we show that the source of productivity growth in Germany is not capital deepening but direct efficiency (TFPQ) improvements. We are among the first to analyze labor productivity in the service sector and demonstrate that productivity effects are substantial in this sector as well.

Second, our work relates to the literature analyzing the price effects (Lemos 2008; Harasztosi and Lindner 2019) of the minimum wage. Earlier studies typically find rather weak pass-through of minimum wage costs to prices (see the survey of Lemos 2008). More recent studies challenge this consensus. Renkin et al. (2022) report almost complete pass-through based on US data, and Harasztosi and Lindner (2019) find that 75% of the minimum wage-induced increase in labor costs is passed on to customers via higher prices in Hungary. Importantly, most of this literature does not simultaneously analyze productivity and prices. This has two important implications: first, productivity studies are silent on whether rising

[2] Harasztosi and Lindner (2019) do not analyze productivity but document capital-labor substitution in Hungary.

revenue productivity simply reflects rising prices; and second, studies on the price-cost pass-through cannot discuss to what extent productivity improvements offset cost hikes. A few exceptions include Machin et al. (2003) and Ashenfelter and Jurajda (2022), who provide case-study evidence for specific industries and assume that either prices or productivity are sticky.[3] Hence, we present the first study on the productivity effects of the minimum wage analyzing whether the estimated productivity effects reflect output price changes or changes in quantity productivity. Our finding of only very modest price effects in manufacturing is in line with the argument of Harasztosi and Lindner (2019) that international competition limits manufacturing firms' scope for price adjustments. We extend these findings by showing that the productivity effects substantially exceed the price effects of the minimum wage in German manufacturing.

Third, our paper contributes to the macroeconomic literature on reallocation and misallocation of resources across firms. Reallocation plays a key role in the understanding of productivity growth in Schumpeterian growth models (e.g., Aghion et al. 2014) highlighting the growth of more productive firms at the expense of low-productivity firms. Another literature highlights how factor reallocation may reduce the misallocation of input factors stemming from market frictions (Hsieh and Klenow 2009; Petrin and Sivadasan 2013). Our study is similar in spirit to the study by Dustmann et al. (2022), who investigate empirically whether employment reallocation across firms in response to the introduction of the German minimum wage is directed toward firms predicted to be ex ante more productive. One major improvement of our approach over their reallocation analysis is that we actually observe firm productivity. This not only improves productivity measurement, which is key given the enormous productivity dispersion across firms even within narrowly defined industries (e.g., Syverson 2011), but also allows us to analyze *changes* in firm productivity over the course of the introduction of the minimum wage. In this sense, we provide the first paper formally studying the effect of the minimum wage on allocative efficiency and productivity gains from reallocation.

The remainder of our article proceeds as follows: Section 2 provides information on the institutional background of the minimum wage introduction. Section 3 describes the various data sets we combine and use. The empirical analysis is divided into two parts: Section

[3]Studying the UK residential care industry, Machin et al. (2003) argue that prices are sticky due to regulation. In a robustness section, they find zero effects on prices and on a crude measure of labor productivity (residents per worker). Recently, Ashenfelter and Jurajda (2022) analyze price effects in the fast food industry, explicitly arguing that this setting is characterized by fixed productivity.

4 describes the empirical framework and shows the results of our firm-level difference-in-differences analysis. In section 5, we aggregate our data to the region-industry level to study the reallocation of workers between firms.

2 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) 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. The coalition agreement including the €8.50 minimum wage was signed in November 2013, and the law 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, trainees, 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 noncompliance and suspicion-based controls. Thus, compliance with the minimum wage requires a sufficient level of information among both workers and employers. In 2018, approximately 1.3% to 2.1% of all employment relations were found to exhibit contractually agreed hourly wages below the minimum wage (Destatis 2020; Fedorets et al. 2020). 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). 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. (2020b) do not find more pronounced employment effects in industries subject to stricter enforcement in Germany.

3 Data

We combine various representative firm- and worker-level statistics supplied by the German statistical offices. Firms are required by law to provide the respective information. Other establishment-level evidence on the German minimum wage resorts to social security data (Dustmann et al. 2022) or the IAB establishment panel survey (e.g., Bossler et al. 2018). Unlike our data, the German social security data do not contain any information on establishment output or productivity. The IAB establishment panel data include this information, but being survey data, they have severe sample size limitations and are plagued by panel attrition.[4] For these reasons and because of their unique features detailed below, the official German firm data *(Amtliche Firmendaten in Deutschland)* used in our study are best suited for analyzing the productivity effects of the German minimum wage.

Manufacturing. We use yearly panel data on German manufacturing firms with at least 20 employees from 2012 to 2015 (KSE). Except for employment figures, which are declared as end of September, the data pertain to the respective calendar year.[5] The data include information

[4]Moreover, Bossler and Schank (forthcoming) report that the IAB establishment panel greatly underestimates minimum wage incidence relative to the German Structure of Earnings Survey provided by the statistical offices.

[5]Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/42221.2018.00.01.1.1.0, 10.21242/42111.2018.00.01.1.1.0, and 10.21242/42131.2017.00.03.1.1.0.

on, among others, firms' costs, total sales, investment and, most notably, quantities and sales by detailed 10-digit product codes defining approximately 6,000 different products.[6] While employment, total sales, investment, and product-level information is collected for the population of firms with at least 20 employees, detailed cost information is available only for a representative and stratified 40% sample, covering approximately 15,000 firms per year. The latter also includes information on intermediate inputs, which is key to measuring productivity. The sample rotates every 4 years, most recently in 2012 and 2016, which determines our empirical design and limits our ability to study long-term effects.

Service sector. In addition to the manufacturing sector data, we use data on service sector firms for the years 2012 to 2015 from the *AFiD-Panel Strukturerhebung im Dienstleistungsbereich (SiD)*.[7] The SiD is a yearly collected representative and stratified 15% sample of all firms with an annual revenue of at least 17,500 EUR in sectors H, J, L, M, N and S/95 of the NACE Rev.2 classification, covering about 150,000 firms per year.[8] The SiD includes, among others, information on firms' sales, investment, employment and costs, but not on output prices and quantities. Except for the employment figures, which are declared as of the end of September, the data pertain to the respective calendar year. Firms with annual revenues below 250,000 EUR are exempt from completing the full questionnaire. We therefore restrict the sample to firms with revenues above 250,000 EUR and with at least one employee. The sample is maintained over several years and redrawn at irregular intervals. Complete redraws of the sample occurred in 2011, 2014, and 2016, limiting the survey's usability for long-term analyses.

Sample definition. To study the development of firms over multiple years we restrict the sample to firms reporting from 2012 to 2015. Hence, we include only service sector firms in the sample before and after the 2014 redraw.[9] We exclude industries exempt from the

[6] 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".

[7] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/47415.2019.00.01.1.1.0.

[8] We thus omit a few highly affected industries, such as agriculture, wholesale/retail trade, accommodation and food services, and construction as these are not captured by the data.

[9] We show in Appendix C that unconditional exit probabilities are not higher after the introduction of the minimum wage. Linear probability models for sample attrition in Appendix Table C1 show no increased attrition propensity for manufacturing firms and only a slight increase of 1 percentage point in the attrition propensity for service firms. Dustmann et al. (2022) find a higher exit propensity for very small firms but no effects on firm exit for establishments having at least five employees. Furthermore, they do not find an increased likelihood of minimum wage workers moving to newly founded firms.

minimum wage.[10] Our final sample consists of 30,000 and 9,500 firms per year for the service and manufacturing sectors, respectively.

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 worker-level information on hourly wages.[11] 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.[12] 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.[13] 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. We use the VSE to cross validate our firm-level treatment indicator and to compute minimum wage exposure in industry×region cells in our reallocation analysis.

4 Firm-Level Results

4.1 Empirical Approach

We compare the evolution of outcomes at firms highly affected by the minimum wage with those of supposedly unaffected firms three years before and one year after the introduction of the minimum wage. Similar to Draca et al. (2011), we use information on the firms' average wage to define treated firms. More specifically, we use the annual wage bill per FTE averaged

[10] This applies to the following NACE Rev.2 industries: Agriculture (01) and Forestry (02); Processing and preserving of meat and production of meat products (101); Manufacture of textiles (13); Manufacture of wearing apparel (14); Temporary employment agency activities (782) and Other human resources provision (783); Landscape service activities (813); Washing and (dry-)cleaning of textile and fur products (9601); Hairdressing and other beauty treatment (9602).

[11] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/62111.2014.00.03.1.1.0.

[12] The VSE data pertain to April 2014.

[13] 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 10).

over the pretreatment years 2012-2014 from the KSE and SiD, respectively.[14] 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 bill per FTE 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 bill per FTE between €25,000 and €40,000 as being moderately exposed (*med*). Firms with an average annual wage bill per FTE of €40,000 and more constitute the control group of *high*-wage firms. We allocate approximately 12% of firms in the manufacturing sector and 25% of firms in the service sector to the exposed group of low-wage firms.

[14] Our main results hold when we use the annual wage bill per FTE averaged over the years 2012-2013 to assign the treatment status. We define the treatment status using 2012-2014 to minimize the impact of temporary wage fluctuations on treatment assignment.

Table 1: Firm Characteristics by Treatment Category, 2013

	All sectors			Manufacturing			Service		
	high	med	low	high	med	low	high	med	low
Employment (FTE)	181.76	102.84	65.52	427.25	134.28	118.53	95.00	90.21	57.78
	(796.02)	(1325.32)	(200.07)	(1229.65)	(206.25)	(181.41)	(542.67)	(1563.48)	(201.50)
Wage Bill per FTE (€1000)	57.80	32.17	19.79	49.62	32.95	21.01	60.69	31.85	19.61
	(25.69)	(5.71)	(4.61)	(8.26)	(4.75)	(3.32)	(28.93)	(6.03)	(4.74)
Labor Costs per FTE (€1000)	68.15	39.11	24.59	59.67	39.83	25.55	71.15	38.82	24.45
	(28.53)	(6.94)	(5.72)	(10.00)	(5.72)	(4.03)	(32.12)	(7.36)	(5.91)
Value Added per FTE (€1000)	118.20	71.56	54.16	83.29	53.58	34.66	130.53	78.78	57.01
	(121.50)	(66.62)	(56.77)	(37.30)	(18.54)	(11.49)	(137.49)	(76.82)	(60.08)
Intermediate Input per FTE (€1000)	145.37	84.91	50.97	165.33	101.06	59.30	138.31	78.42	49.75
	(193.23)	(112.50)	(80.12)	(123.42)	(77.05)	(49.12)	(212.04)	(123.32)	(83.62)
Investments per FTE (€1000)	9.42	6.84	5.47	8.77	6.18	3.74	9.66	7.10	5.73
	(46.10)	(25.22)	(22.69)	(11.36)	(9.79)	(7.02)	(53.20)	(29.20)	(24.14)
Value Added Labor Share	75.20	70.67	63.19	81.02	80.90	79.11	73.15	66.56	60.87
	(39.94)	(35.47)	(38.11)	(33.34)	(26.77)	(23.19)	(41.83)	(37.64)	(39.29)
Share of Part-Time Employees	18.74	23.21	37.16	8.26	11.98	21.45	22.44	27.73	39.45
	(18.78)	(22.12)	(30.72)	(7.75)	(12.98)	(22.04)	(20.09)	(23.39)	(31.14)
Share of Female Employees	37.06	39.46	46.34	21.70	27.47	46.98	42.49	44.28	46.24
	(24.12)	(28.75)	(32.65)	(13.78)	(18.92)	(25.75)	(24.65)	(30.56)	(33.54)
East Germany (0/1)	0.11	0.23	0.43	0.05	0.19	0.45	0.14	0.24	0.43
High-Productivity (0/1)	0.67	0.44	0.31	0.74	0.37	0.16	0.65	0.46	0.33
Observations	16031	14589	8661	4186	4181	1104	11845	10408	7557

Note: This table shows selected firm characteristics (means) by treatment category and sector for 2013. Standard errors are reported in parentheses. *low* denotes low-wage and thus highly exposed firms. Moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the control group of *high*-wage firms. High-productivity firms are defined as firms with ex-ante labor productivity above the region×industry-specific median.

Table 1 shows firm characteristics by treatment category for the year 2013. The wage bill per FTE in the highly exposed group of low-wage firms is €20,000 which is close to the annual salary of a full-time *minimum wage* worker ($\sim$ €18,000). Furthermore, low-wage firms tend to be smaller, exhibit lower labor productivity, export less frequently, are more often located in East-Germany (where wages are generally lower), have a higher share of female and part-time employees, and are more labor intensive.

We estimate regression models of the following form:

$$\Delta y_{it} = \alpha + \boldsymbol{T_i \beta} + \phi_r + \psi_j + \epsilon_{it}, \tag{1}$$

where the left-hand side (Δy_{it}) is the change in the outcome of interest (wages, employment, productivity), from the pre- to the postpolicy period, $T_i \in \{low, med, high\}$ is the treatment indicator, and ϕ_r and ψ_j are region and industry fixed effects. We center ϕ_r and ψ_j on their respective sample means to interpret the regression intercept as the mean change for control group firms in an average region and industry. Due to possible anticipation effects, we choose 2013 as the base year. The coefficient of interest, β, provides the differential development in y_{it} between the treated and control groups relative to the base year.

Some difference-in-differences applications and event studies on wage changes control for (or match on) pretrends in the dependent variable (e.g., Fackler et al. 2021) or propose nuanced versions of equation 1 to detect mean reversion or unstable macroeconomic conditions (Dustmann et al. 2022). We opt for the parsimonious specification in equation 1 and, for instance, do not control for pretrends for several reasons. First, controlling for pretrends can create additional bias if firms anticipate the treatment and react to it beforehand. In those cases, the treatment influences the pre-*trend*. Anticipation effects are a particular threat in our study, as described in Section 2. To avoid anticipation effects affecting our results, we build the time difference for the treatment effect between 2013 and 2015 instead of 2014 and 2015. Second, treatment effects estimated after trend filtering do not correspond to the usual "descriptive" difference-in-difference effect as soon as the trend filtering is relevant. Whereas trend filtering might be warranted in pure causal micro-level studies, it is not useful in our approach because we relate micro-level effects to aggregate market-level effects. The latter cannot be trend-filtered in a strictly comparable manner. Third, Dustmann et al. (2022) report that their efforts to control for mean reversion and macroeconomic effects do not have a sizeable impact on their results. Fourth, we have only one pretreatment change (2012 to 2013) available in our data, which is arguably too short to allows us to reliably control for trends. Instead, we always report the effect on "placebo" changes, which equal the year 2012 outcome minus the year 2013 outcome, alongside our main effects and direct the reader's attention to situations in which these changes are very different between the treatment and control groups.

Validation of Treatment Definition. The accuracy of our treatment definition crucially depends 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.[15] First, we follow Draca et al. (2011) and Hirsch et al. (2015) and construct a precise measure of firm exposure to the minimum wage. We derive the gap measure using data from individual worker z at firm i:

$$\text{GAP}_i = \frac{\sum_j h_{iz} \max\{w_{min} - w_{iz}, 0\}}{\sum_j w_{iz} * h_{iz}} \times 100, \tag{2}$$

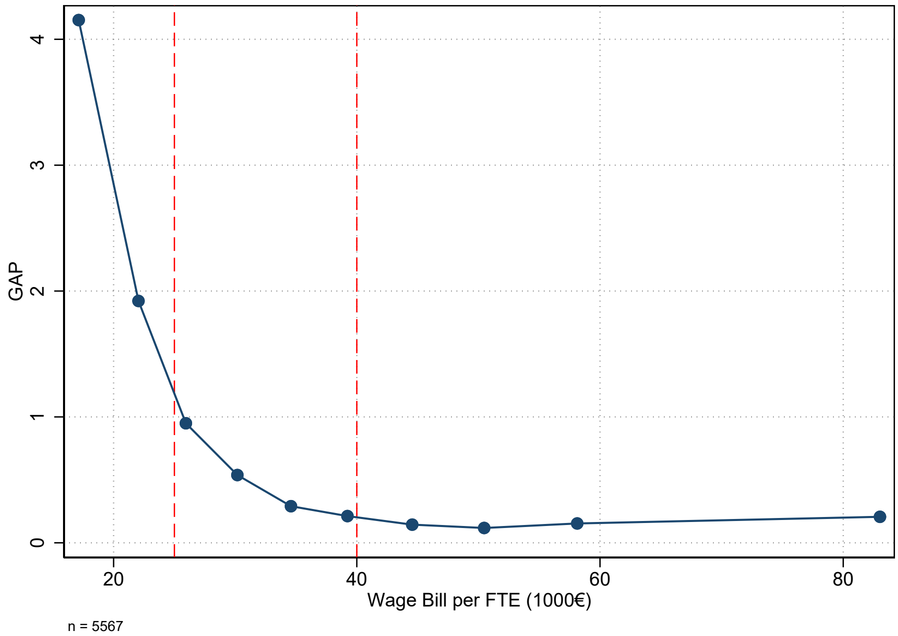
where w_{min} depicts the minimum wage (€8.5), w_{iz} is the gross hourly wage for worker z in firm i, and h_{iz} is the hours worked. The lower bound of the gap measure is zero for nonaffected firms. The measure depicts the percentage increase in a firms wage bill needed to comply with the minimum wage requirement, assuming that hours worked stay constant. Figure 1 depicts the average gap measure against the average wage divided into 10 equal-sized bins for 5,524 firms merged to the VSE data.[16] The vertical dotted lines separate our three groups of differently affected firms. The gap measure increases rapidly for firms with an average prepolicy annual wage bill per FTE below €25,000 and approaches zero for firms with an average wage of €40,000. Moreover, we find that 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

[15]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, the firm-level data 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.

[16]An alternative measure of minimum wage exposure often used in the literature is the fraction of affected workers (Harasztosi and Lindner 2019). Figure D1 in the appendix shows the relationship between this alternative measure and the average wage.

group definitions reliably capture the extent to which firms are affected by the introduction of the minimum wage. This also supports several existing studies in using average wages to measure minimum wage exposure at the firm level (e.g., Draca et al. 2011; Hau et al. 2020).

Figure 1: Gap (VSE) against Average Wage

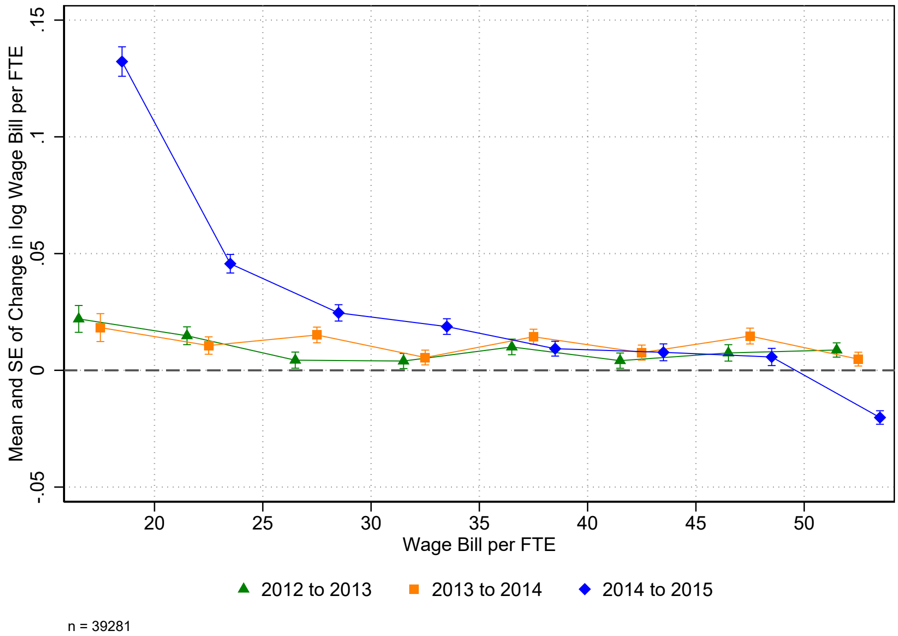


Note: The y-axis shows the percentage increase in a firm's wage bill required to comply with the minimum wage requirement. The x-axis shows the pre-treatment average of annual wage costs per FTE divided into 10 equally-sized bins. The red lines depict the thresholds for the three treatment groups: $[min; €25{,}000)$; $[€25{,}000; €40{,}000)$; $[€40{,}000; max]$.

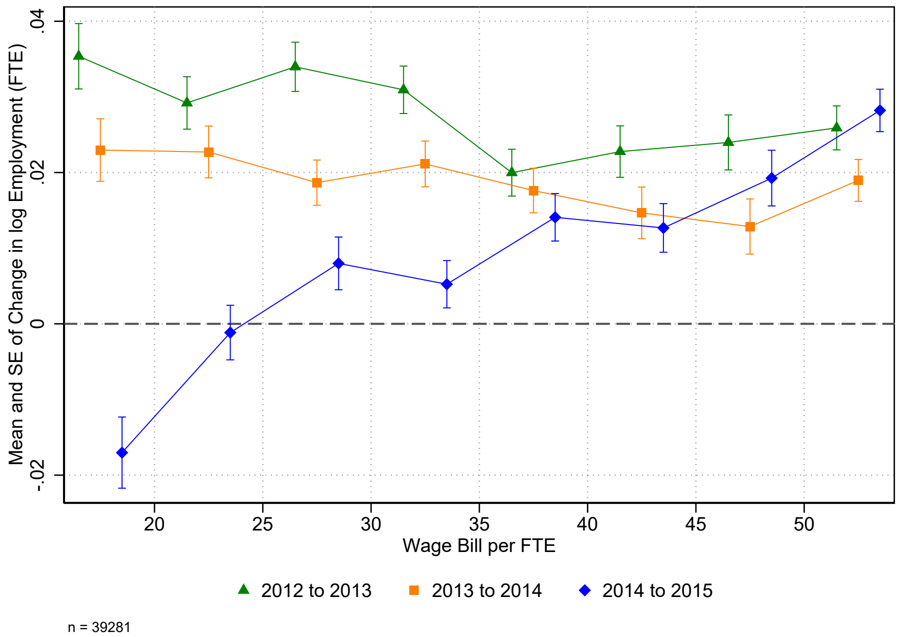
4.2 Baseline Results

Main Results. We start by presenting descriptive evidence on changes in firm wages and employment around the time of the introduction of the minimum wage in Figure 2. Panel (a) illustrates that the most pronounced increase in average wages after the minimum wage law became effective occurred in firms with a prepolicy average wage below €25,000, confirming that the minimum wage was binding for these firms. The figure plots the average yearly growth in the annual wage bill per FTE against the initial wage bin separately for 2012 to 2013, 2013 to 2014, and 2014 to 2015. For example, firms with initial wages below €20,000 experienced 11% higher growth in average wages from 2014 to 2015 (13.5%) than over the 2012 to 2013 prepolicy period (2.5%). The average wage growth of firms with initial wages above €25,000 and below €30,000 is 2% higher for the postpolicy period (2.5%) than for the prepolicy period (0.5%). In contrast, the average wage growth of firms with initial wages

Figure 2: Wages and Employment after the Introduction of the Minimum Wage



(a) Yearly Growth in Wage Bill per FTE by Ex-Ante Wage Bill per FTE (€1000)



(b) Yearly Growth in Employment (FTE) by Ex-Ante Wage Bill per FTE (€1000)

Note: In panels (a) and (b) we plot the average yearly growth in firm average wage (wage bill per FTE) and employment (FTE), respectively, against the initial level in the wage bin (i.e., the annual wage bill per FTE averaged over the pretreatment years 2012-2014) separately for the periods 2012 to 2013 (green line), 2013 to 2014 (orange line) and 2014 to 2015 (blue line). Manufacturing and service sector firms are pooled. We also report the respective standard errors.

above €35,000 is similar for the post- and prepolicy years. Firms in the highest wage bin (above €50,000) experienced a slight decline in wage growth which is driven by a few small, high-paying service sector firms.

We repeat the same exercise for yearly employment growth in panel (b). Firms with initial average wages below €20,000 shrank after the introduction of the minimum wage. Firms with initial wages below €35,000 experienced lower employment growth in the postpolicy than in the prepolicy years. Employment growth rates for firms with initial wages above €35,000 do not differ across the post- and prepolicy years.

Table 2: Employment and Wage Effects

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2013 to 2012	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2013 to 2012
Δ Log Wage Bill per FTE						
med	0.013***	0.003	0.001	0.055***	0.006	-0.003
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
low	0.065***	0.011**	0.001	0.142***	0.009	-0.021***
	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)
Constant	0.021***	0.012***	-0.008***	-0.023***	0.005	-0.004
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Δ Log Employment (FTE)						
med	-0.002	0.003	0.001	-0.017***	0.007*	-0.012***
	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)
low	-0.037***	-0.005	0.008	-0.035***	0.013**	-0.010*
	(0.007)	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)
Constant	0.022***	0.011***	-0.011***	0.048***	0.016***	-0.027***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Δ Log Total Wage Bill						
med	0.011***	0.005**	0.003	0.038***	0.013***	-0.014***
	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)
low	0.029***	0.006	0.009**	0.107***	0.022***	-0.030***
	(0.007)	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)
Constant	0.044***	0.023***	-0.019***	0.025***	0.020***	-0.031***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
N	9471	9471	9471	29810	29810	29810
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes

Note: Results from regressing the change in log wage bill per FTE, employment (FTE) and total wage bill from 2013 to 2015 (cols. 1 and 4), to 2014 (cols. 2 and 5), and to 2012 (cols. 3 and 6) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms, while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the results on the employment and wage effects of the minimum wage from our regression analysis separately for manufacturing and service sector firms. *low* denotes low-wage (highly exposed) firms, whereas *med* reflects moderately exposed firms. The constant captures the effects for the control group, i.e., high-wage firms in the average industry and region. Columns 1 and 4 show our key results on the changes from 2013 to 2015. We find strong positive effects of the minimum wage on wages per FTE in manufacturing (+6.5%) and services (+14.2%) for highly exposed relative to nonexposed firms.[17]

These effects are associated with mild negative effects on employment in manufacturing (-3.7%) and service sector (-3.5%) firms and are driven by a reduction in employment for full-time workers (see Table A1 in the appendix). Affected firms' total wage bill increased by 2.9% in manufacturing and 10.7% in services. Hence, firms began to operate with fewer but more expensive workers, and the wage increase dominated the employment reduction, leading to rising total labor costs. These results are in line with results for the German minimum wage effects based on alternative worker-level data (Dustmann et al. 2022) and employer surveys (Bossler et al. 2018). For instance, Dustmann et al. (2022) also report declining employment and rising wages in their establishment-level analysis.

Note that employment in the high-wage control group, as reflected by the regression constant grew continuously over time in both sectors. Low-wage firms followed this trend until 2014 and then suddenly lost employment as soon as the minimum wage came into effect. There is an important difference, however, in the wage growth patterns in service sector firms. High-wage control group firms had a zero pretrend until 2014 and then suddenly reduced their average wages in 2015. This hints at possible reallocation of low-wage workers from low- to high-wage firms in our data, which is also a core finding in Dustmann et al. (2022). We study the implications of worker reallocation for aggregate productivity more carefully in section 5. For now, note that these possible reallocation effects could imply spillovers of the introduction of the minimum wage to our control group. We test the impact of these possible spillover effects on our firm-level regression analysis following Berg et al. (2021) in Appendix J. We find that the impact of such spillover (i.e., reallocation) effects on our firm-level analysis is negligible.

Having established that our data yield results on employment and wages similar to those derived from other German data, we now turn to our first major contribution. Table 3 displays the results for sales, value added, and labor productivity. Columns 1 and 4 show an *increase* in total revenues in affected manufacturing (+2.5%) and service firms (+4%) relative to control group firms despite reduced employment. This increase is also associated

[17] When interpreting the results, we focus on the highly affected firms compared to the control group. Note that the findings for moderately affected firms are qualitatively similar but, as expected, quantitatively less pronounced.

Table 3: Sales, Value Added, and Labor Productivity

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2013 to 2012	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2013 to 2012
Δ Log Revenue						
med	0.014***	0.010***	-0.001	0.015***	0.007**	-0.006*
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
low	0.025***	0.011**	0.003	0.040***	0.013***	-0.010**
	(0.008)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Constant	0.028***	0.019***	-0.002	0.012***	0.006**	-0.010***
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Δ Log Value Added						
med	0.009	0.004	-0.002	0.026***	0.014**	-0.014**
	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
low	0.019*	-0.000	-0.018*	0.071***	0.028***	-0.024***
	(0.011)	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)
Constant	0.020***	0.029***	0.005	-0.023***	0.004	-0.020***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Δ Log Value Added per FTE						
med	0.011	0.002	-0.004	0.043***	0.007	-0.002
	(0.007)	(0.006)	(0.006)	(0.008)	(0.007)	(0.007)
low	0.056***	0.005	-0.027**	0.106***	0.015*	-0.014
	(0.011)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
Constant	-0.002	0.018***	0.016***	-0.071***	-0.011**	0.007
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
N	9471	9471	9471	29810	29810	29810
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes

Note: Results from regressing the change in log revenue, value added, and value added per FTE from 2013 to 2015 (cols. 1 and 4), to 2014 (cols. 2 and 5), and to 2012 (cols. 3 and 6) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms, while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with a rise in value added. Correspondingly, we document a substantial increase in labor productivity, amounting to 5.6% (10.6%) higher growth in manufacturing (services). In the service sector, however, this positive effect for low-wage firms is partly driven by a sharp decline in labor productivity in high-wage firms. As in case of the wage trends, a possible explanation for this sharp decline in productivity is the inflow of low-wage workers.

The labor productivity gains might originate in either an increase in firms' TFP or an increase in firms' capital or intermediate input intensity. In Appendix Table A2, we show that labor productivity is not driven by capital-labor substitution. In particular, investment per FTE is not differentially affected in the treatment group, which confirms the results in Bossler et al. (2020a). However, affected firms operate with an increased ratio of intermediates per FTE hinting at potential labor productivity gains from outsourcing. Appendix Table A3 shows that relative to the control group, affected firms significantly increased their total intermediate inputs but decreased total investments after the minimum wage became effective. The increase in intermediate input intensity explains the rise in sales reported above and highlights that a productivity analysis of the minimum wage crucially hinges on good intermediate input data.

Table 4: Total Factor Productivity and Prices

	Manufacturing		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2013 to 2012
Δ Log TFPR			
med	0.013***	0.005	-0.002
	(0.004)	(0.003)	(0.003)
low	0.031***	0.006	-0.005
	(0.006)	(0.005)	(0.005)
Constant	0.017***	0.012***	-0.009***
	(0.003)	(0.002)	(0.002)
Δ Log TFPQ			
med	0.004*	0.001	-0.000
	(0.002)	(0.002)	(0.002)
low	0.022***	0.004	-0.008**
	(0.004)	(0.004)	(0.004)
Constant	0.007***	0.009***	0.002
	(0.002)	(0.002)	(0.002)
Δ Log Price Index			
med	0.009***	0.004*	-0.001
	(0.003)	(0.002)	(0.002)
low	0.010**	0.002	0.003
	(0.005)	(0.003)	(0.004)
Constant	0.010***	0.003**	-0.011***
	(0.002)	(0.002)	(0.002)
N	9471	9471	9471
Region FE	yes	yes	yes
Industry FE	yes	yes	yes

Note: Results from regressing the change in log TFPR, TFPQ and the firm price index from 2013 to 2015 (cols. 1 and 4), to 2014 (cols. 2 and 5), and to 2012 (cols. 3 and 6) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms, while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Any TFP effects could reflect either real improvements in the efficiency of the production process or a price-cost pass-through of the minimum wage leading to increased output prices. In the first case, quantity TFP (TFPQ) rises, whereas in the second case, revenue TFP (TFPR) increases. Our data permit us to disentangle both channels as they contain information on sales and quantities of firms at the detailed 10-digit product level. Using these data, we calculate firm-level output price indices and estimate quantity-based production functions to calculate TFPR and TFPQ. However, we can do so only for manufacturing firms, as output quantities are not collected for service sector firms.

When constructing output price indices, we must consider that the various products of multiproduct firms are measured in different units (e.g., liters, numbers, kilograms). We therefore follow Eslava et al. (2004) and compute firm-specific Tornqvist price indices based on product price changes for all manufacturing firms in our sample.[18] To estimate TFPR and TFPQ, we follow the production function estimation routine in Mertens (2022) and apply a control function approach (e.g., Olley and Pakes 1996; Levinsohn and Petrin 2003) that controls for unobserved productivity shocks and firm-specific price variation similarly to the approach in De Loecker et al. (2016).[19]

Table 4 shows that affected firms raise their TFPR and prices by 3.1% and 1%, respectively, relative to control group firms. The difference-in-differences effect on price-adjusted TFPQ amounts to 2.2%. This suggests that one-third of the observed productivity effect can be explained by an increase in output prices whereas two-thirds of the increase in TFPR reflects gains in true technical efficiency (TFPQ).

In sum, we find improvements in labor productivity that are not driven by capital deepening, but might partly result from outsourcing of production tasks that became more costly. We report TFPR gains for the manufacturing sector of 3.1%. One-third of these gains are driven by higher output prices, whereas two-thirds of this effect are actual efficiency gains. Hence, the minimum wage led to improvements in the physical productivity of affected manufacturing firms. We note that our results on prices and TFPQ are available only for manufacturing firms and that one may argue that international competition limits the scope for price adjustments in these firms. Therefore, price effects might be larger in service sector firms, as shown for the Hungarian economy by Harasztosi and Lindner (2019).

5 Aggregate Industry-Region Results

5.1 Empirical Approach

Our previous firm-level analysis showed that the minimum wage reduced employment in low-wage firms while high-wage firms continuously grew during the minimum wage introduction.

[18]See Appendix E for further details on how to compute this index.

[19]We use a flexible translog production function that allows for firm- and time-specific output elasticities. This also accounts for changes in firms' output elasticities due to changes in relative factor prices induced by the minimum wage. See Appendix G for details on the production function estimation and the calculation of TFPR and TFPQ.

Similarly, Dustmann et al. (2022) report evidence for reallocation of low-wage workers to firms that pay higher wages, are larger, and are estimated (not actually observed) to be more productive. Because of the enormous productivity dispersion even within narrowly defined industries (Syverson 2011), a key advantage of our analysis over that of Dustmann et al. (2022) is that we directly observe productivity in the data. Moreover, the productivity approximation in Dustmann et al. (2022) prevents them from analyzing *changes* in firm productivity around the minimum wage introduction. The latter turns out to be crucial to interpreting the aggregate productivity effect of the minimum wage.

In this section, we study aggregate productivity effects and their drivers in detail. To pin down aggregate market-level results, we aggregate our firm-level data to 491 industry-region cells (2-digit NACE Rev.2 × NUTS1) and study aggregate employment, wages, and productivity in more relative to less exposed labor markets. [20]

Decomposition methods. We decompose the minimum wage effect on aggregate productivity into a within-firm and a reallocation effect using the technique introduced by Olley and Pakes (1996) and its recent extension proposed by Kehrig and Vincent (2021). We then relate our results to the Foster et al. (2001) decomposition to motivate why we deviate from the common but simplistic interpretation of the Olley and Pakes (1996) decomposition in our assessment of the minimum wage effects on aggregate productivity. For simplicity, we illustrate the decomposition methods for labor productivity, yet we analogously apply them to firm-level average wages. Following Olley and Pakes (1996), aggregate labor productivity can be expressed as the weighted sum of individual labor productivity levels:

$$\Omega_t = \sum_i s_{it}\omega_{it}, \tag{3}$$

where Ω_t is aggregate log labor productivity, ω_{it} denotes firm-level log labor productivity, and $s_{it} = \frac{L_{it}}{\sum_i L_{it}}$ is the employment weight, i.e., a measure of economic activity. Aggregate productivity can then be decomposed into unweighted average firm productivity ($\overline{\omega}_t$) and the covariance of firms' size and productivity ($Cov(s_{it}, \omega_{it})$):

[20] We explore alternative labor market definitions in Appendix B. Across all specifications, we exclude cells with fewer than 10 firms in the 2013 base year.

$$\Omega_t = \overline{\omega}_t + Cov(s_{it}, \omega_{it}). \tag{4}$$

Equation 4 shows that changes in the aggregate level can occur (i) as a result of a shift in the unweighted average, that is, a common shock that affects all firms symmetrically, and (ii) as a result of changes in the joint distribution of productivity and firm size. The latter can result either from a reallocation of labor between firms of different productivity levels or from changes in firm productivity at different positions in the firm size distribution. For example, the minimum wage could predominantly affect the least productive firms, inducing them to shed labor and thus decrease their labor market shares. As a result, the covariance of labor market share and productivity would increase. At the same time, however, the minimum wage might affect predominantly small firms with low labor market shares and induce them to become more productive, decreasing the covariance. As we detail below, the covariance term does not provide clear-cut evidence on productivity-enhancing *reallocation* processes of labor market shares if "within-firm" productivity changes are systematically related to (initial) firm size.[21]

To disentangle the multiple channels possibly influencing the covariance, we follow Kehrig and Vincent (2021) and further decompose the covariance as follows:

$$\Delta Cov(\omega_{it}, s_{it}) = Cov(s_{it_0}, \Delta\omega_{it}) + Cov(\Delta s_{it}, \omega_{it_0}) + Cov(\Delta s_{it}, \Delta\omega_{it}). \tag{5}$$

Thus, changes in the covariance can result from the covariance between initial firm size and productivity changes ($Cov(s_{it_0}, \Delta\omega_{it})$), the covariance between changes in firm size and initial firm productivity ($Cov(\Delta s_{it}, \omega_{it_0})$), and the covariance between simultaneous changes in firms' productivity and size ($Cov(\Delta s_{it}, \Delta\omega_{it})$).

Intuitively, within-firm productivity changes should include both the aggregate productivity contributions of uniform firm productivity changes across the entire firm distribution (i.e., the $\overline{\omega}_t$ in equation 4) and productivity changes within firms of different sizes (i.e., $Cov(s_{it_0}, \Delta\omega_{it})$ in equation 5). The decomposition extension provided by Kehrig and Vincent (2021) thus reveals that the reallocation term (i.e., the covariance) in the Olley and Pakes

[21] Nevertheless, the covariance provides an overall statistic on whether more productive firms are larger. This is often viewed as an indicator of allocative efficiency (e.g., Bartelsman et al. 2013; Bighelli et al. 2022).

(1996) decomposition contains elements that should be interpreted as within-firm components.[22] We demonstrate in Appendix H that summarizing the two aforementioned components into a comprehensive "within-firm" component yields the decomposition proposed by Foster et al. (2001). By removing $Cov(s_{it_0}, \Delta\omega_{it})$ from the covariance term of the Olley and Pakes (1996) decomposition, we are left with a composite covariance that consists of a term capturing the productivity growth contribution of market share reallocation conditional on firms' initial productivity and a cross-term that reflects simultaneous changes in firms' productivity and relative size. The advantage of using the Kehrig and Vincent (2021) decomposition instead of starting out directly with Foster et al. (2001) is that the former allows us to understand whether the "within-firm" component in Foster et al. (2001) is predominantly driven by productivity changes along the entire firm-size distribution or by productivity changes that are correlated with initial firm size, the latter being important in our application.

Regression Framework. In the following, we calculate the components of the decomposition in equations 4 and 5 for labor productivity and total wages per FTE in each of our 491 labor markets and regress them on the labor markets' minimum wage exposure. To identify exposed labor markets, we follow Dustmann et al. (2022) and calculate the gap measure at the labor market level, analogously to how we calculate the firm-level gap measure in equation 6, using data from individual worker z in region r industry j in 2014 from the *VSE* dataset:

$$\text{GAP}_{jr} = \frac{\sum_{z \in jr} h_{zjr} \max\{w_{min} - w_{zjr}, 0\}}{\sum_{z \in jr} w_{zjr} * h_{zjr}} \times 100, \tag{6}$$

where w_{min} depicts the minimum wage (€8.5), w_{zjr} the gross hourly wage for worker z in industry j and region r, and h_{zjr} the respective hours worked. The gap measure reflects the percentage change in the wage bill required to pay all workers within the respective labor market at least the minimum wage. The gap measure, averaged across the 490 labor markets, is 0.972. This implies that if all workers previously paid below the minimum wage received the minimum wage, ceteris paribus, the market-level hourly wage would increase by 1% on average.[23]

We then regress aggregate employment and each component of the decomposition in

[22] A similar argument has been made in Decker et al. (2017).

[23] Dustmann et al. (2022) compute this measure at the granular district level and report an unweighted average of 1.7%.

equations 4 and 5 on the industry×region-level minimum wage exposure using the following regression framework:

$$\Delta y_{jrt} = \alpha + \beta \times \text{GAP}_{jr} + \epsilon_{jrt}, \tag{7}$$

where Δy_{jrt} is the change in outcome y_{jrt} (aggregate and average labor productivity, aggregate and average wage bill per FTE, the decomposition components for both indicators, and aggregate employment) in industry j and region r from a base year t_0 to t. To account for differences in industry×region cell size, we weight all regressions by the cell-level employment in 2013. As in Section 4, we present pretreatment changes in Δy_{jrt} to spot diverging trends by exposure level.

Worker mobility across industry×region defined above is a potential threat to the validity of our empirical design. However, the worker-level results in Dustmann et al. (2022) show that this is not a major concern. In particular, the minimum wage-induced reallocation of workers to higher-paying establishments occurs entirely within regions and mostly within industries. As Dustmann et al. (2022) utilize more granular region and industry cells than we do, reallocation across cells is even less relevant in our study. Nevertheless, in Appendix B, we replicate our results allowing for i) full cross-regional worker mobility, and ii) full cross-industry worker mobility.

5.2 Decomposition Results

To put the productivity results into perspective, we start by briefly discussing aggregate employment and wage effects for our balanced panel of firms. Column 1 of Appendix Table A5 shows that from 2013 to 2015, aggregate employment growth was not statistically significantly different in more and less exposed labor markets.[24] The results for aggregate wages are shown in Appendix Table A6. We confirm that more exposed manufacturing labor markets experienced a larger increase in the aggregate wage bill per FTE (column 1), driven by an increase in the unweighted average wage bill per FTE (column 2). We do not find aggregate wage effects for the service sector.

The first major new result in Table 5 is that market-level labor productivity in the manufacturing sector increased more in labor markets more exposed to the minimum wage. Aggregate labor productivity increased by 1.4% per percentage point increase in the gap measure

[24] Again, this is in line with the results of the reallocation analysis in Dustmann et al. (2022).

Table 5: Olley and Pakes (1996) Decomposition: Log Labor Productivity

	2013 to 2015			2012 to 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$
Manufacturing						
GAP	0.014*	0.031***	-0.016*	-0.003	-0.008*	0.005
	(0.007)	(0.008)	(0.008)	(0.006)	(0.004)	(0.006)
Constant	0.026**	-0.002	0.027**	-0.001	-0.008	0.006
	(0.012)	(0.006)	(0.013)	(0.009)	(0.006)	(0.007)
N	167	167	167	167	167	167
Mean Y	0.020	0.006	0.014	-0.003	-0.007	0.004
Mean GAP	0.427	0.427	0.427	0.427	0.427	0.427
R-sq	0.008	0.125	0.011	0.001	0.011	0.004
Service Sector						
GAP	0.000	0.007	-0.006	-0.004	-0.006	0.003
	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.006)
Constant	-0.019	-0.043***	0.024	-0.005	0.016**	-0.020**
	(0.014)	(0.011)	(0.016)	(0.010)	(0.007)	(0.010)
N	324	324	324	324	324	324
Mean Y	0.010	-0.022	0.032	-0.000	-0.002	0.002
Mean GAP	1.252	1.252	1.252	1.252	1.252	1.252
R-sq	0.000	0.007	0.003	0.002	0.010	0.001

Note: Results from regressing the change in aggregate labor productivity (cols. 1 and 4), the average labor productivity (cols. 2 and 5), and the covariance of the firm labor market share and labor productivity (cols. 3 and 6) from 2013 to 2015 and 2012 to 2013, respectively, on the treatment indicator. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(column 1). Importantly, we do not find any differential 2012-2013 pretreatment change by minimum wage exposure (column 4).[25] Aggregate productivity is boosted by strong improvements in unweighted average firm productivity (column 2), which rises by 3.1% per percentage point increase in the gap measure. A striking result is that stronger minimum wage exposure corresponds with a *decline* in the covariance term (column 3). Whereas the regression constant shows a strong increase in the covariance term (being well in line with recent results in Bighelli et al. 2022) for nonaffected markets contributing to an overall improvement in aggregate productivity, the minimum wage reduces the covariance contribution. The effects of the minimum wage on aggregate productivity in the service sector are overall zero, but its components show the same patterns as in manufacturing, i.e., a reduced covariance and improved unweighted average term.

The negative covariance term seems to stand in sharp contrast to the conclusions in

[25] In unreported results, we aggregated our firm-specific price index to the industry×region-level and do not find any evidence for aggregate price changes in response to the minimum wage introduction. Hence, the reported effects on aggregate productivity are not driven by price changes.

Dustmann et al. (2022) and deserves further analysis. Table 6 presents the associated results from the covariance decomposition. A clear picture emerges: the decline in the covariance is almost exclusively driven by reductions in the covariance between initial firm size and productivity changes ($Cov(s_{it_0}, \Delta\omega_{it})$). This is true for manufacturing and the service sector alike, whereas in the service sector, this covariance component even overcompensates for the decline in the overall covariance. Columns 5 to 8 confirm that there are no differential 2012-2013 changes by minimum wage exposure for any of the covariance components.

A decline in $Cov(s_{it_0}, \Delta\omega_{it})$ may be the result of small firms increasing their productivity or large firms losing productivity over the course of reallocation. To better understand this aspect, we return to the firm-level analysis and split our sample into firms having initially more versus fewer than 100 employees.[26] Appendix Table A4 shows the corresponding triple difference-in-differences estimates. We find that small manufacturing firms affected by the minimum wage sharply increased their labor productivity but find no effects for larger firms. Hence, for the manufacturing sector, the increase in unweighted average firm productivity (Column 2, Table 5) and the decline in the covariance (Column 3, Table 5) are explained by extraordinary productivity improvements in small firms affected by the minimum wage. Essentially the same forces are at play in the service sector: initially, small low-wage firms outpace larger low-wage firms in productivity growth. However, a notable difference explaining the weaker positive effects on aggregate and average firm-level productivity in the service sector is strong productivity losses in initially larger service sector firms paying low or medium wages.

We highlighted before (and discuss in detail in Appendix H) that the term $Cov(s_{it_0}, \Delta\omega_{it})$ should be viewed as part of the "within-firm" contribution. Doing so within the framework of Foster et al. (2001) reduces the within component from 3.1% to 1.6% in manufacturing (it remains statistically significant) and to an insignificant -0.005 in services (see Appendix Table A7). The remainder of the covariance term captures the effect of the minimum wage on productivity-enhancing reallocation. For services, both the reallocation term and the cross-term are positive but statistically insignificant; for manufacturing, the former term is zero and the latter is negative (see columns 3 and 4 in Table 6 or, equivalently, columns 3 and 4 in Appendix Table A7).

We conclude that our firm-level results on reallocation confirm (and extend) those in

[26]The results are very similar when we use 50 employees instead of 100 employees as the firm-size threshold.

Dustmann et al. (2022) but lead to quite different conclusions on the aggregate productivity-enhancing effect of reallocation. Even in the most conservative scenario, where we subtract $Cov(s_{it_0}, \Delta\omega_{it})$ from the covariance term and add it to the "within-term", the minimum wage did not induce productivity-enhancing reallocation processes.[27]

[27] In Appendix B, we replicate our results allowing for i) full cross-regional worker mobility, and ii) full cross-industry worker mobility. Using these alternative aggregation levels, we still do not find any evidence for productivity-enhancing reallocation processes induced by the introduction of the minimum wage.

Table 6: Covariance Decomposition: Log Labor Productivity

	2013 to 2015				2012 to 2013			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$
Manufacturing								
GAP	-0.016*	-0.015*	0.000	-0.002***	0.005	0.004	0.002	-0.001
	(0.008)	(0.008)	(0.001)	(0.001)	(0.006)	(0.005)	(0.002)	(0.001)
Constant	0.027**	0.024*	0.006***	-0.003***	0.006	0.005	0.004***	-0.003***
	(0.013)	(0.012)	(0.001)	(0.001)	(0.007)	(0.007)	(0.001)	(0.001)
N	167	167	167	167	167	167	167	167
Mean Y	0.014	0.012	0.007	-0.005	0.004	0.004	0.005	-0.005
Mean GAP	0.427	0.427	0.427	0.427	0.427	0.427	0.427	0.427
R-sq	0.011	0.009	0.000	0.016	0.004	0.002	0.034	0.010
Service Sector								
GAP	-0.006	-0.012*	0.002	0.003	0.003	0.003	0.001	-0.002
	(0.008)	(0.006)	(0.002)	(0.004)	(0.006)	(0.007)	(0.001)	(0.002)
Constant	0.024	0.057***	0.006	-0.039***	-0.020**	-0.011	0.009***	-0.018***
	(0.016)	(0.013)	(0.005)	(0.009)	(0.010)	(0.010)	(0.002)	(0.003)
N	324	324	324	324	324	324	324	324
Mean Y	0.032	0.052	0.019	-0.039	0.002	0.015	0.015	-0.028
Mean GAP	1.252	1.252	1.252	1.252	1.252	1.252	1.252	1.252
R-sq	0.003	0.013	0.005	0.002	0.001	0.001	0.002	0.004

Note: Results from regressing the components of the covariance decomposition following equation 5 on the treatment indicator. In columns 1-4, the base year (t_0) is 2013, and we calculate changes from 2013-2015. In columns 5-8, the base year (t_0) is 2012, and we calculate changes from 2012-2013. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Allocative Efficiency

The absence of employee reallocation toward higher-productivity firms does not necessarily imply that there is also no increase in allocative efficiency. Gains in allocative efficiency emerge from a reallocation of workers from inefficiently large firms (where marginal revenue products are below wages) to inefficiently small firms (where marginal revenue products exceed wages) (Hsieh and Klenow 2009; Petrin and Sivadasan 2013). This includes scenarios in which high-productivity firms are too large due to distortions. In this case, the reallocation measures discussed in the previous section do not reliably measure allocative efficiency (Petrin and Levinsohn 2012).

To measure allocative efficiency, we follow Hsieh and Klenow (2009) and Petrin and Sivadasan (2013) and compute the dispersion of marginal revenue products of labor (MRPL) and the average absolute gap between firm-level wages and MRPL at the industry×region level.[28] Both measures capture the idea that, in a frictionless market, reallocation equalizes gaps between MRPL and wages.[29] Hence, a *decrease* in these allocative efficiency measures reflects an *increase* in allocative efficiency. We derive firms' MRPL from our production function estimates following recent studies that estimate MRPL-wage gaps (e.g., Mertens 2022; Yeh et al. 2022) and explain the approaches of Hsieh and Klenow (2009) and Petrin and Sivadasan (2013) more carefully in Appendix I.[30] Because we require estimates of firms' production function for this analysis and because we lack data on capital stocks for service sector firms, we conduct this analysis only for the manufacturing sector.

Table 7 presents the results from regressing standard measures of allocative efficiency on the industry×region-level gap measure. We do not find any evidence for an increase in allocative efficiency in response to the minimum wage. The measure based on Petrin and Sivadasan (2013) in column 2 even implies a decrease in allocative efficiency, reflected in an increase in average absolute MRPL-wage gaps.[31] Together with the previous section's results,

[28] We rely on the standard deviation as dispersion measure. The results for 90th-10th percentile differences are similar.

[29] As described in Appendix I, the framework of Hsieh and Klenow (2009) models identical wages across firms, whereas Petrin and Sivadasan (2013) allow for heterogeneous (exogenous) firm wages.

[30] Assuming that intermediate inputs are flexible and that intermediate inputs prices are exogenous to firms, which are common assumptions in the literature (e.g., Hall 2004; Yeh et al. 2022), one can estimate a firm's MRPL as $MRPL_{it} = (\theta^L_{it}/\theta^M_{it})(P^M_{it}M_{it}/L_{it})$, where θ^M_{it} and θ^L_{it} denote the output elasticity of labor and intermediate inputs, respectively, $P^M_{it}M_{it}$ is intermediate input expenditures, and L_{it} is labor inputs (see Appendix G for more details). This measure of the MRPL is based on the idea of Petrin and Sivadasan (2013) that a benchmark flexible input allows identification of market distortions in other inputs.

[31] One potential explanation for this finding is that affected (low-wage) firms pay wages equal to their marginal revenue product of labor prior to the minimum wage introduction, while unaffected cells experience a general increase in MRPL-wage gaps, e.g., due to changes in firms'/workers' labor market power.

we thus conclude that the minimum wage neither contributed to an increase in allocative efficiency nor induced any productivity-enhancing reallocation processes. All productivity gains from the minimum wage result from within-firm productivity improvements.

Table 7: Allocative Efficiency

	2013 to 2015		2012 to 2013	
	(1)	(2)	(3)	(4)
	Δ Std(Log MRPL)	$\Delta\ \overline{\lvert MRPL - w\rvert}$	Δ Std(Log MRPL)	$\Delta\ \overline{\lvert MRPL - w\rvert}$
GAP	0.001	0.321***	0.004	0.054
	(0.017)	(0.071)	(0.010)	(0.057)
Constant	0.005	0.649***	-0.009**	-0.038
	(0.004)	(0.105)	(0.005)	(0.099)
N	167	167	167	167
Mean Y	0.002	0.804	0.003	-0.016
Mean GAP	0.427	0.427	0.427	0.427
R-sq	0.000	0.038	0.001	0.001

Note: Results from regressing various indicators of allocative efficiency on the treatment indicator. Columns 1 and 3 show the change in the standard deviation of Log MRPL, and columns 2 and 4 show the change in the mean absolute difference between MRPL and average wage (in thousand €). In columns 1 and 2, the base year (t_0) is 2013, and we calculate changes from 2013-2015. In columns 3 and 4 the base year (t_0) is 2012, and we calculate changes from 2012-2013. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

In 2015, Germany introduced a national minimum wage for the first time in its history. Despite cutting deep into the wage distribution, its aggregate employment effects appear to be very modest. Against this background and recent findings of substantial worker reallocation from low-wage to high-wage firms, the contribution of this study is to analyze the productivity effects of the minimum wage at the level of firms and markets. We further explore to what extent revenue productivity effects of the minimum wage simply capture output price effects. To this end, we utilize administrative data on manufacturing and service sector firms containing detailed information on productivity, wages, employment, and prices. Our firm-level analysis documents that wages increased, employment declined, and output prices increased only modestly in response to the minimum wage. We further find substantial gains in labor productivity and total factor productivity that likely mitigated any adverse effects that the minimum wage otherwise would have had on employment and output prices. In manufacturing and services alike, we find particularly strong productivity gains for small firms affected by the minimum wage.

At the market level, we find aggregate productivity gains in the manufacturing sector for labor markets more affected by the minimum wage. Applying a recently introduced decomposition method, we argue that asymmetric firm size-dependent productivity changes are counterintuitively attributed to the covariance term of the standard Olley and Pakes (1996) decomposition, which is often interpreted as a measure of allocative efficiency. In our case, the productivity gains of *ex ante* small firms render the standard covariance term negative. Assigning firm size-dependent productivity changes to the "within-component" rather than to the covariance component of the Olley and Pakes (1996) decomposition yields a composite "within term" that accounts for firm size heterogeneity and corresponds to a decomposition similar to the one proposed by Foster et al. (2001). Using this modification, we conclude that aggregate productivity gains are exclusively driven by the "within-component". This means that firm-level productivity improvements rather than reallocation of labor toward more efficient producers caused aggregate manufacturing productivity to increase in response to the introduction of the minimum wage. We do not find any aggregate productivity gains in the service sector, nor do we see changes in the within and between components of aggregate service sector productivity.

Using German social security data, the seminal study of Dustmann et al. (2022) shows that workers left affected small firms and moved to firms that have been *classified* by the authors as more productive based on size, sector, state, and wage. Dustmann et al. (2022:323) conclude that *" minimum wages increased allocational efficiency of workers".* We confirm multiple empirical findings of Dustmann et al. (2022) using different data. However, our use of direct productivity data and a formal productivity decomposition lead us to the conclusion that the minimum wage did not induce aggregate productivity-enhancing reallocation processes. Similarly, we do not find any improvements in allocative efficiency when applying formal measures of allocative efficiency (Hsieh and Klenow 2009 and Petrin and Sivadasan 2013). Important avenues for future research into the effects of the minimum wage are analyzing worker mobility and firm productivity jointly and studying longer-run productivity effects at the firm and market levels.

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A Additional Regression Results

Table A1: Employment in Headcounts and the Share of Full-Time Employment

	Manufacturing		Service Sector	
	(1)	(2)	(3)	(4)
	2013 to 2015	2013 to 2012	2013 to 2015	2013 to 2012
Δ Log Employment (Headcount)				
med	-0.002	0.002	-0.009*	-0.009**
	(0.003)	(0.002)	(0.005)	(0.004)
low	-0.027***	0.011**	-0.022***	-0.004
	(0.006)	(0.005)	(0.007)	(0.005)
Constant	0.022***	-0.011***	0.039***	-0.025***
	(0.002)	(0.002)	(0.004)	(0.003)
Δ Share Full-Time Employment				
med	-0.011	0.056	-0.721***	-0.308
	(0.140)	(0.135)	(0.238)	(0.212)
low	-0.931***	-0.175	-1.207***	-0.598**
	(0.349)	(0.338)	(0.333)	(0.300)
Constant	-0.251***	0.014	0.110	0.619***
	(0.080)	(0.070)	(0.167)	(0.150)
N	9471	9471	29810	29810
Region FE	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes

Note: Results from regressing the change in log employment (headcount) and share of full-time employees (in percent) from 2013 to 2015 (cols. 1 and 3) and 2013 to 2012 (cols. 2 and 4) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Input Intensities

	Manufacturing			Service Sector		
	(1)	(2)	(3)	(4)	(5)	(6)
	2013 to 2015	2013 to 2014	2013 to 2012	2013 to 2015	2013 to 2014	2013 to 2012
Δ Log Intermediate Inputs per FTE						
med	0.019***	0.008*	-0.003	0.032***	-0.005	0.012
	(0.005)	(0.004)	(0.004)	(0.010)	(0.009)	(0.009)
low	0.057***	0.017**	0.005	0.058***	-0.015	0.031**
	(0.009)	(0.008)	(0.008)	(0.013)	(0.012)	(0.012)
Constant	0.009***	0.003	0.007**	0.011	-0.011*	0.046***
	(0.003)	(0.003)	(0.003)	(0.007)	(0.006)	(0.007)
Δ Investments per FTE (€1000)						
med	-0.463	-0.531*	-0.439	-0.820	-0.322	-0.518
	(0.294)	(0.281)	(0.294)	(0.651)	(0.568)	(0.638)
low	-0.287	-0.422	-0.650	-0.705	-0.244	-0.383
	(0.443)	(0.385)	(0.455)	(0.739)	(0.640)	(0.699)
Constant	0.431*	0.457**	1.005***	0.931*	0.295	0.953*
	(0.235)	(0.221)	(0.220)	(0.563)	(0.459)	(0.496)
N	9471	9471	9471	29810	29810	29810
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes

Note: Results from regressing the change in log intermediate inputs per FTE and investments per worker from the 2013 to 2015 (cols. 1 and 4), 2013 to 2014 (cols. 2 and 5), and 2013 to 2012 (cols. 3 and 6) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Total Intermediate Inputs and Investment

	Manufacturing		Service Sector	
	(1)	(2)	(3)	(4)
	2013 to 2015	2013 to 2012	2013 to 2015	2013 to 2012
Δ Log Intermediate Inputs				
med	0.017***	-0.002	0.015	0.001
	(0.005)	(0.004)	(0.009)	(0.009)
low	0.020**	0.013*	0.024**	0.021*
	(0.010)	(0.008)	(0.012)	(0.011)
Constant	0.032***	-0.004	0.059***	0.019***
	(0.004)	(0.003)	(0.007)	(0.006)
Δ Investments (€1000)				
med	-337.260**	-157.156	-348.760*	-198.986
	(169.574)	(140.545)	(208.492)	(165.971)
low	-418.683**	-257.452	-434.897**	-170.825
	(195.081)	(176.508)	(171.203)	(163.678)
Constant	438.140***	209.832	486.109**	158.423
	(163.508)	(132.190)	(211.799)	(155.376)
N	9471	9471	29810	29810
Region FE	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes

Note: Results from regressing the change in log total intermediate inputs and total investments from 2013 to 2015 (cols. 1 and 3) and 2013 to 2012 (cols. 2 and 4) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effects by Initial Firm Size (>100 employees)

	Manufacturing			Service Sector		
	(1)	(2)	(3)	(4)	(5)	(6)
	2013 to 2015	2013 to 2014	2013 to 2012	2013 to 2015	2013 to 2014	2013 to 2012
Δ Log Value-Added per FTE						
med	0.018*	0.016	-0.007	0.048***	0.008	-0.003
	(0.011)	(0.010)	(0.010)	(0.008)	(0.007)	(0.007)
low	0.060***	0.019	-0.032**	0.110***	0.015	-0.013
	(0.014)	(0.014)	(0.014)	(0.010)	(0.009)	(0.010)
large > 100	0.012	0.022**	-0.010	0.063***	0.013	0.002
	(0.011)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)
med × large > 100	-0.011	-0.023*	0.003	-0.032**	-0.009	0.013
	(0.014)	(0.013)	(0.013)	(0.016)	(0.014)	(0.014)
low × large > 100	-0.001	-0.022	0.005	-0.021	0.005	-0.005
	(0.020)	(0.019)	(0.019)	(0.018)	(0.016)	(0.016)
Constant	-0.010	0.005	0.023***	-0.081***	-0.013**	0.006
	(0.009)	(0.009)	(0.009)	(0.006)	(0.005)	(0.006)
N	9471	9471	9471	29810	29810	29810
Share Large 'high'	0.63	0.63	0.63	0.16	0.16	0.16
Share Large 'med'	0.38	0.38	0.38	0.13	0.13	0.13
Share Large 'low'	0.32	0.32	0.32	0.12	0.12	0.12
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes

Note: Results from regressing the change in value-added per FTE from 2013 to 2015 (cols. 1 and 4), to 2014 (cols. 2 and 5), and to 2012 (cols. 3 and 6) on the treatment indicator interacted with an indicator for initial firm size. large indicates firms with more than 100 employees. *low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Aggregate Employment Effects

	2013 to 2015	2012 to 2013
	(1)	(2)
	Δ Aggregate Employment (FTE)	Δ Aggregate Employment (FTE)
Manufacturing		
GAP	0.004	0.005***
	(0.003)	(0.002)
Constant	0.021***	0.005**
	(0.004)	(0.002)
N	167	167
Mean Y	0.021	0.010
Mean GAP	0.427	0.427
R-sq	0.003	0.025
Service Sector		
GAP	-0.008	-0.001
	(0.008)	(0.002)
Constant	0.045***	0.029***
	(0.016)	(0.003)
N	324	324
Mean Y	0.042	0.029
Mean GAP	1.252	1.252
R-sq	0.005	0.001

Note: Results from regressing the change in the aggregate employment from 2013 to 2015 (cols. 1) and 2012 to 2013 (cols. 2) on the treatment indicator. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Olley and Pakes (1996) Decomposition: Log Average Wage

	2013 to 2015			2012 to 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$
Manufacturing						
GAP	0.008***	0.013***	-0.005**	0.002	0.000	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Constant	0.044***	0.032***	0.012***	0.016***	0.009***	0.007***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
N	167	167	167	167	167	167
Mean Y	0.043	0.036	0.007	0.011	0.007	0.005
Mean GAP	0.427	0.427	0.427	0.427	0.427	0.427
R-sq	0.049	0.150	0.020	0.004	0.000	0.004
Service Sector						
GAP	0.006	0.008**	-0.001	0.000	0.003	-0.003
	(0.006)	(0.004)	(0.007)	(0.002)	(0.003)	(0.004)
Constant	0.023*	0.019***	0.004	0.002	0.007*	-0.004
	(0.013)	(0.004)	(0.013)	(0.004)	(0.004)	(0.005)
N	324	324	324	324	324	324
Mean Y	0.035	0.036	-0.000	0.010	0.011	-0.002
Mean GAP	1.252	1.252	1.252	1.252	1.252	1.252
R-sq	0.005	0.037	0.000	0.000	0.006	0.003

Note: Results from regressing the change in the aggregate wage per worker (cols. 1 and 4), the firm average wage per worker (cols. 2 and 5), and the covariance of firm labor market share and wage per worker (cols. 3 and 6) from 2013 to 2015 and 2012 to 2013 on the treatment indicator. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Foster et al. (2001) Decomposition: Log Labor Productivity

	2013 to 2015				2012 to 2013			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta\ \Omega_t$	$\sum s_{it_0}\Delta\omega_{it}$	$\sum \Delta s_{it}(\omega_{it_0} - \Omega_{t_0})$	$\sum \Delta s_{it}\Delta\omega_{it}$	$\Delta\ \Omega_t$	$\sum s_{it_0}\Delta\omega_{it}$	$\sum \Delta s_{it}(\omega_{it_0} - \Omega_{t_0})$	$\sum \Delta s_{it}\Delta\omega_{it}$
Manufacturing								
GAP	0.014*	0.016**	0.000	-0.002***	-0.003	-0.004	0.002	-0.001
	(0.007)	(0.007)	(0.001)	(0.001)	(0.006)	(0.005)	(0.002)	(0.001)
Constant	0.026**	0.023*	0.006***	-0.003***	-0.001	-0.002	0.004***	-0.003***
	(0.012)	(0.013)	(0.001)	(0.001)	(0.009)	(0.009)	(0.001)	(0.001)
N	167	167	167	167	167	167	167	167
Mean Y	0.020	0.018	0.007	-0.005	-0.003	-0.003	0.005	-0.005
Mean GAP	0.427	0.427	0.427	0.427	0.427	0.427	0.427	0.427
R-sq	0.008	0.010	0.000	0.016	0.001	0.001	0.034	0.010
Service Sector								
GAP	0.000	-0.005	0.002	0.003	-0.004	-0.003	0.001	-0.002
	(0.007)	(0.006)	(0.002)	(0.004)	(0.006)	(0.006)	(0.001)	(0.002)
Constant	-0.019	0.015	0.006	-0.039***	-0.005	0.004	0.009***	-0.018***
	(0.014)	(0.011)	(0.005)	(0.009)	(0.010)	(0.010)	(0.002)	(0.003)
N	324	324	324	324	324	324	324	324
Mean Y	0.010	0.030	0.029	-0.039	-0.000	0.012	0.021	-0.028
Mean GAP	1.252	1.252	1.252	1.252	1.252	1.252	1.252	1.252
R-sq	0.000	0.003	0.005	0.002	0.002	0.001	0.002	0.004

Note: Results from regressing the components of the Foster decomposition following equation H4 on the treatment indicator. In columns 1-4, the base year (t_0) is 2013 and we calculate changes from 2013-2015. In columns 5-8, the base year (t_0) is 2012 and we calculate changes from 2012-2013. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Alternative Labor Market Definitions

In our baseline specification of the main text, we use the two-digit industry $\times$ NUTS1 level to study how the introduction of the minimum wage affected aggregate productivity and productivity-enhancing reallocation processes. This aggregation features 16 regions and 46 industries (25 service sector industries and 21 manufacturing industries). This jointly allows for broad regional and industrial components in our reallocation analysis, which considers that workers can move across narrow districts and finer industries within these cells. One potential concern is that this aggregation level nevertheless ignores productivity-enhancing reallocation processes between cells, which might explain the absence of productivity effects from reallocation in our main results. In the following, we show that using alternative aggregation levels that allow for different reallocation patterns also do not yield any productivity-enhancing reallocation effects.

Below, Tables and B1 and B2 first replicate our analysis using 144 three-digit industries as aggregation level, which limits worker mobility across industries but allows for full cross-regional worker mobility.[32] Our results are comparable to our main specification. Particularly, we do not find any evidence for productivity-enhancing reallocation processes. All productivity gains instead result from within-firm productivity improvements. Despite the coefficient on aggregate productivity in manufacturing is not statistically significant, it is almost identical to the baseline specification.

Tables B3 and B4 replicate our analysis aggregating our data to 223 official labor market regions.[33] Compared to our baseline specification, this restricts worker mobility across local labor market regions but allows for full worker mobility across industries. As in the baseline specification, we weight results by initial local labor market region size. Again, we do not find evidence for productivity-enhancing reallocation processes induced by the introduction of the minimum wage. Instead, we again document only within-firm productivity improvements.

[32] We again exclude cells with less than 10 observations in the 2013 base year.

[33] Official labor market regions according to the definition of the Federal Institute for Research on Building, Urban Affairs and Spatial Development. We again exclude cells with less than 10 observations in the 2013 base year.

Table B1: Olley and Pakes (1996) Decomposition: Log Labor Productivity (three-digit industry)

	2013 to 2015			2012 to 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$
Manufacturing						
GAP	0.013	0.044***	-0.032***	-0.001	0.001	-0.002
	(0.012)	(0.004)	(0.012)	(0.009)	(0.006)	(0.006)
Constant	0.025	-0.005	0.030**	-0.003	-0.007	0.004
	(0.016)	(0.005)	(0.015)	(0.011)	(0.009)	(0.006)
N	77	77	77	77	77	77
Mean Y	0.020	0.007	0.013	-0.003	-0.003	-0.000
Mean GAP	0.263	0.263	0.263	0.263	0.263	0.263
R-sq	0.005	0.266	0.033	0.000	0.000	0.001
Service Sector						
GAP	0.003	0.001	0.003	0.020	-0.007	0.026***
	(0.013)	(0.010)	(0.014)	(0.013)	(0.006)	(0.010)
Constant	-0.021	-0.021	-0.000	-0.022	0.002	-0.024
	(0.020)	(0.015)	(0.022)	(0.019)	(0.009)	(0.015)
N	58	58	58	58	58	58
Mean Y	-0.012	-0.020	0.008	-0.018	-0.016	-0.002
Mean GAP	0.641	0.641	0.641	0.641	0.641	0.641
R-sq	0.001	0.000	0.001	0.048	0.035	0.099

Note: Results from regressing the change in aggregate labor productivity (cols. 1 and 4), average labor productivity (cols. 2 and 5), and the covariance of the firm labor market share and labor productivity (cols. 3 and 6) from 2013 to 2015 and 2012 to 2013, respectively, on the treatment indicator. Regressions are weighted by industry-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Covariance Decomposition: Log Labor Productivity (three-digit industries)

	2013 to 2015				2012 to 2013			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$
Manufacturing								
GAP	-0.032***	-0.022*	-0.004***	-0.005***	-0.002	-0.001	0.003***	-0.005***
	(0.012)	(0.012)	(0.001)	(0.001)	(0.006)	(0.006)	(0.001)	(0.001)
Constant	0.030**	0.027*	0.007***	-0.003***	0.004	0.003	0.003***	-0.002***
	(0.015)	(0.015)	(0.001)	(0.001)	(0.006)	(0.006)	(0.001)	(0.001)
N	77	77	77	77	77	77	77	77
Mean Y	0.013	0.011	0.006	-0.004	-0.000	-0.001	0.006	-0.005
Mean GAP	0.263	0.263	0.263	0.263	0.263	0.263	0.263	0.263
R-sq	0.033	0.017	0.073	0.132	0.001	0.000	0.051	0.246
Service Sector								
GAP	0.003	-0.007	0.001	0.008	0.026***	0.027***	0.001	-0.001
	(0.014)	(0.012)	(0.004)	(0.008)	(0.010)	(0.010)	(0.002)	(0.005)
Constant	-0.000	0.036**	0.009**	-0.046***	-0.024	-0.014	0.010***	-0.020***
	(0.022)	(0.017)	(0.004)	(0.013)	(0.015)	(0.015)	(0.002)	(0.004)
N	58	58	58	58	58	58	58	58
Mean Y	0.008	0.034	0.015	-0.041	-0.002	0.011	0.008	-0.022
Mean GAP	0.641	0.641	0.641	0.641	0.641	0.641	0.641	0.641
R-sq	0.001	0.005	0.002	0.021	0.099	0.099	0.002	0.001

Note: Results from regressing the components of the covariance decomposition following equation 5 on the treatment indicator. In columns 1-4, the base year (t_0) is 2013, and we calculate changes from 2013-2015. In columns 5-8, the base year (t_0) is 2012, and we calculate changes from 2012-2013. Regressions are weighted by industry-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Olley and Pakes (1996) Decomposition: Log Labor Productivity (Labor Market Regions)

	2013 to 2015			2012 to 2013		
	(1) $\Delta\ \Omega_t$	(2) $\Delta\ \overline{\omega}_{it}$	(3) $\Delta Cov(s_{it}, \omega_{it})$	(4) $\Delta\ \Omega_t$	(5) $\Delta\ \overline{\omega}_{it}$	(6) $\Delta Cov(s_{it}, \omega_{it})$
All Sectors						
GAP	0.007	0.018***	-0.011	0.004	-0.006	0.010
	(0.010)	(0.006)	(0.009)	(0.007)	(0.004)	(0.008)
Constant	0.006	-0.030***	0.036***	-0.008	0.001	-0.009
	(0.009)	(0.006)	(0.009)	(0.008)	(0.004)	(0.008)
N	221	221	221	221	221	221
Mean Y	0.012	-0.011	0.023	-0.007	-0.003	-0.004
Mean GAP	0.693	0.693	0.693	0.693	0.693	0.693
R-sq	0.002	0.050	0.007	0.001	0.008	0.008

Note: Results from regressing the change in aggregate labor productivity (cols. 1 and 4), the average labor productivity (cols. 2 and 5), and the covariance of the firm labor market share and labor productivity (cols. 3 and 6) from 2013 to 2015 and 2012 to 2013, respectively, on the treatment indicator. Regressions are weighted by labor market region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Covariance Decomposition: Log Labor Productivity (Labor Market Regions)

	2013 to 2015				2012 to 2013			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$	$\Delta\ Cov(s_{it}, \omega_{it})$	$Cov(s_{it_0}, \Delta\omega_{it})$	$Cov(\Delta s_{it}, \omega_{it_0})$	$Cov(\Delta s_{it}, \Delta\omega_{it})$
All Sectors								
GAP	-0.011	-0.014*	0.001	0.002	0.010	0.010	0.004*	-0.004
	(0.009)	(0.008)	(0.003)	(0.004)	(0.008)	(0.008)	(0.002)	(0.002)
Constant	0.036***	0.048***	0.010***	-0.022***	-0.009	-0.004	0.005***	-0.010***
	(0.009)	(0.009)	(0.002)	(0.005)	(0.008)	(0.008)	(0.001)	(0.002)
N	221	221	221	221	221	221	221	221
Mean Y	0.023	0.033	0.010	-0.020	-0.004	-0.000	0.009	-0.012
Mean GAP	0.693	0.693	0.693	0.693	0.693	0.693	0.693	0.693
R-sq	0.007	0.013	0.000	0.001	0.008	0.008	0.024	0.022

Note: Results from regressing the components of the covariance decomposition following equation 5 on the treatment indicator. In columns 1-4, the base year (t_0) is 2013, and we calculate changes from 2013-2015. In columns 5-8, the base year (t_0) is 2012, and we calculate changes from 2012-2013. Regressions are weighted by labor market region-level employment in 2013. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Survivor Bias

One major concern with restricting the sample to firms that survive the introduction of the minimum wage is that these firms might be precisely firms that can raise productivity, leading to a selection bias in our estimates.

We address this concern by analyzing how the firm exit probability is related to firms' treatment status. To test whether dropping out of the sample is related to minimum wage exposure, we estimate the following linear probability model:

$$exit_i = \alpha + \boldsymbol{T_i}\boldsymbol{\beta} + \phi_r + \psi_j + \rho_s + \epsilon_i \tag{C1}$$

where $exit_i$ is a binary indicator that is 1 if the firm drops out of the sample in 2015, $\boldsymbol{T_i}$ is the vector of treatment indicators, ϕ_r and ψ_j are (centered) region and industry fixed effects. Moreover, we control for detailed ex-ante size classes ρ_s (centered) to account for differences in the exit probability by firm size and sample composition between manufacturing and service sector firms (cf. section 3). We condition on firms that are included throughout the sample from 2012 to 2014.

Table C1 shows the results from estimating equation C1 for the manufacturing and service sector, respectively. After controlling for firm size, more affected firms do not have a higher exit probability in 2015 in the manufacturing sector. In the service sector, low-wage firms have a 1 percentage point higher exit probability, which is only a small effect.

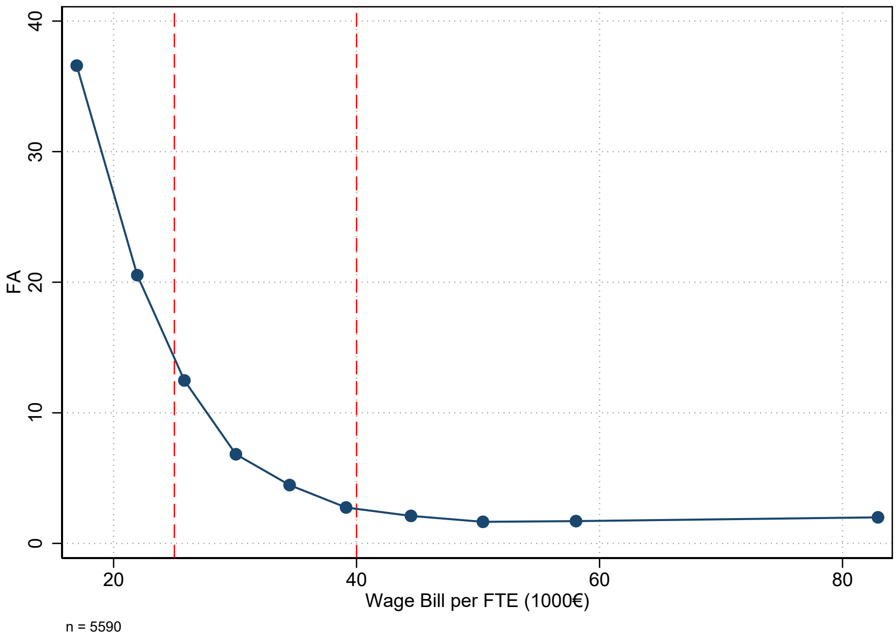
Table C1: Exit Probability and Treatment Status

	Manufacturing	Service
	(1)	(2)
med	-0.007	-0.001
	(0.005)	(0.003)
low	0.001	0.010**
	(0.010)	(0.004)
Constant	0.052***	0.055***
	(0.004)	(0.002)
N	10027	32258
Region FE	yes	yes
Industry FE	yes	yes
Size FE	yes	yes

Note: Results from regressing a dummy for exit (0/1) in 2015 on the treatment indicator. All columns control for region and industry and detailed ex-ante firm size classes. *low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Additional Descriptive Statistics

Figure D1: Fraction Affected against Average Wages



Note: The y-axis shows the fraction of affected workers at the firm level. The x-axis shows the pretreatment average of annual wage costs per FTE divided into 10 equal-sized bins. The red lines depict the thresholds for the three treatment groups: $[min;$ €25,000); [€25,000; €40,000); [€40,000; $max]$.

E Price Index

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

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

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., 2009, 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.[34]

[34] 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.

F Calculation of capital stocks

We calculate a time series of capital stocks for every manufacturing sector firm using the perpetual inventory method following Bräuer et al. (2019):

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

where K_{it}, α_{jt} and I_{it-1} denote firm i's capital stock, 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:

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

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.

The initial capital stock for the perpetual inventory method is derived from reported tax depreciation. We do not observe similar information for the service sector and therefore only derive capital stocks for manufacturing. Also, 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.[35] Given that we estimate very reasonable output elasticities from our production function estimation (see Appendix G), we are confident that our capital variables reliably reflect firms' true capital stocks.[36]

[35] We therefore calculate capital stocks, whenever available, from 2009 onwards.

[36] As firms likely tend to overstate their capital depreciation, our capital stocks are likely a closer approximation of the true capital stock used in firms' production processes than capital measures based on book values.

G Estimating total factor productivity

We estimate quantity- and revenue-based TFP for our manufacturing data sample. For service firms, we lack information on firm-specific prices. Moreover, calculating capital stocks is more challenging in the service sector data than for manufacturing. Therefore, we stick to labor productivity measures when studying service sector firms.

Starting point. To recover quantity- and revenue-based TFP measures, we must estimate a production function. 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):

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + \omega_{it} + \epsilon_{it}. \tag{G1}$$

The vector $\boldsymbol{\phi}'_{it}$ captures a second polynomial in production inputs (l_{it}, m_{it}, and k_{it}) with an additional full interaction between all three production factors.[37] We define labor as full-time equivalents, capital as deflated capital stocks (see Appendix F 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 (G1) 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 (G1). 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 and recover a

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

quantity- and revenue-based productivity measure.

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. 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 (G1):

$$B(.)_{it} = B((p_{it}, ms_{it}, A_{it}, I_i) \times \boldsymbol{\phi}^c_{it}). \tag{G2}$$

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.

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:

$$\omega_{it} = g(.)_{it} = g(e_{it}, k_{it}, l_{it}, \mathbf{\Gamma}_{it})). \tag{G3}$$

$\mathbf{\Gamma}_{it}$ 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 ($NumP_{it}$), 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.[38]

Inserting equation G2, G3, and the law of motion for productivity into the production function finally yields:

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + B(.)_{it} + h(.)_{it-1} + \zeta_{it} + \epsilon_{it}, \tag{G4}$$

which forms the basis for our estimation.[39]

Identification. We estimate equation G4 separately by two-digit NACE rev. 2 industries for the years 2009 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}.[40] 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}:

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

[39] 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 $\mathbf{\Gamma}_{it}$. Those we add linearly.

[40] 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).

$$E_{it} = ((\zeta_{it} + \epsilon_{it})\boldsymbol{\Upsilon}_{it}), \tag{G5}$$

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:

$$\boldsymbol{\Upsilon}_{it} = (\boldsymbol{J(.)}_{it}, \boldsymbol{V(.)}_{it-1}, \boldsymbol{\Xi(.)}_{it-1}, \boldsymbol{\Psi(.)}_{it-1}, \boldsymbol{\Lambda(.)}_{it-1}), \tag{G6}$$

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 (2009-2017) 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. We do not use data before 2009 as industry classifications and investment information are differently defined before 2009. 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.[41]

Results and calculating TFP. Table G1 shows averages and standard deviations of the estimated output elasticities by two-digit industries. Recap that output elasticities are firm-

[41] 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.

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.

Having estimated the output elasticities, we compute quantity- (TFPQ) and revenue-based (TFPR) total factor productivity in the following way:

$$TFPQ_{it} = q_{it} - \boldsymbol{\phi}'_{it}\boldsymbol{\beta} - B(.)_{it}, \tag{G7}$$

$$TFPR_{it} = TFPQ_{it} + p_{it}. \tag{G8}$$

Hence, $TFPR_{it}$ captures changes in productivity that are purged from price variation, whereas $TFPR_{it}$ combines price and quantity-productivity changes.

Calculating marginal revenue products of labor. In section 5.3, we also use estimates of the marginal revenue product of labor (MRPL). We derive the MRPL following recent studies estimating MRPL-wage gaps (e.g., Mertens 2022, Yeh et al. 2022). Specifically, we assume that firms' maximize profits and that intermediate input prices are exogenous to firms. We further allow that firms have wage-setting power in labor markets.[42] In such a setting, the first order conditions for intermediates and labor are given by:

$$MRPL_{it} = w_{it}(1 + \frac{1}{\epsilon^L_{it}}), \tag{G9}$$

$$MRPM_{it} = P^M_{it}, \tag{G10}$$

where $MRPM_{it}$ denotes the marginal revenue product of intermediates. P^M_{it} is the unit cost for intermediates. ϵ^L_{it} is the labor supply elasticity. Using $MRPL_{it} = MC_{it}\frac{\partial Q_{it}}{\partial L_{it}}$ and $MRPM_{it} = MC_{it}\frac{\partial Q_{it}}{\partial M_{it}}$, where MC_{it} denotes marginal costs, and combining the first order conditions with each other yields:

$$MRPL_{it} = (\theta^L_{it}/\theta^M_{it}) * (P^M_{it} M_{it}/L_{it}), \tag{G11}$$

[42] One can also additionally allow for rent-sharing in such a setting (see the discussion in Mertens 2022).

where θ_{it}^{M} and θ_{it}^{L} are the output elasticities of intermediate and labor inputs. We use equation (G11) to calculate marginal revenue products of labor from estimated output elasticites and observed input expenditures.

Table G1: Production Function Estimation: Average Output Elasticities, by Sector

Sector	Number of Observations	Capital		Labor		Intermediate Inputs		Returns to Scale	
		mean	std.dev.	mean	std.dev.	mean	std.dev.	mean	std.dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Across all industries	104,081	0.08	0.04	0.23	0.10	0.65	0.09	0.96	0.10
10 - Manufacture of food products	11,470	0.09	0.02	0.16	0.10	0.63	0.11	0.89	0.03
11 - Manufacture of beverages	946	0.20	0.02	0.08	0.05	0.60	0.06	0.88	0.05
13 - Manufacture of textiles	2,399	0.07	0.03	0.23	0.12	0.69	0.11	0.99	0.06
14 - Manufacture of wearing apparel	870	0.11	0.06	0.19	0.11	0.69	0.08	0.99	0.11
15 - Manufacture of leather and related products	325	0.09	0.06	0.22	0.09	0.66	0.04	0.97	0.13
16 - Manufacture of wood	2,821	0.06	0.03	0.16	0.06	0.70	0.07	0.92	0.06
17 - Manufacture of paper and paper products	3,074	0.04	0.02	0.18	0.07	0.72	0.05	0.95	0.06
18 - Printing and reproduction of recorded media	2,141	0.08	0.03	0.20	0.11	0.61	0.10	0.89	0.06
19 - Manufacture of coke and refined petroleum products	75	0.17	0.12	0.30	0.20	0.86	0.10	1.33	0.30
20 - Manufacture of chemicals products	5,802	0.09	0.05	0.21	0.08	0.73	0.10	1.03	0.07
21 - Manufacture of basic pharmaceutical products and pharmaceutical preparations	1,264	0.07	0.03	0.23	0.06	0.68	0.08	0.99	0.07
22 - Manufacture of rubber and plastic products	6,728	0.09	0.04	0.21	0.07	0.66	0.07	0.97	0.09
23 - Manufacture of other non-metallic mineral products	5,192	0.11	0.05	0.23	0.08	0.66	0.09	1.01	0.07
24 - Manufacture of basic metals	4,201	0.04	0.02	0.23	0.08	0.70	0.08	0.98	0.05
25 - Manufacture of fabricated metal products, except machinery and equipment	15,348	0.05	0.03	0.25	0.08	0.61	0.09	0.92	0.05
26 - Manufacture of computer, electronic and optical products	5,267	0.08	0.05	0.34	0.10	0.61	0.06	1.04	0.14
27 - Manufacture of electrical equipment	7,122	0.09	0.05	0.29	0.08	0.64	0.06	1.02	0.08
28 - Manufacture of machinery and equipment n.e.c.	16,376	0.07	0.04	0.26	0.04	0.64	0.06	0.97	0.10
29 - Manufacture of motor vehicles, trailers and semi-trailers	4,120	0.06	0.02	0.25	0.12	0.69	0.11	1.00	0.04
30 - Manufacture of other transport equipment	294	0.05	0.05	0.20	0.14	0.73	0.16	0.97	0.12
31 - Manufacture of furniture	2,112	0.08	0.04	0.26	0.14	0.73	0.05	1.07	0.15
32 - Other manufacturing	3,985	0.11	0.05	0.24	0.10	0.58	0.12	0.93	0.16
33 - Repair and installation of machinery and equipment	2,149	0.04	0.03	0.26	0.09	0.59	0.09	0.89	0.04

Note: Average output elasticities calculated after estimating the production function G1 for every NACE rev. 2 two-digit industry with sufficient observations for the years 2009 to 2017. Column 1 reports the number of observations used to calculate output elasticities for each industry. Columns 2,4, and 6 respectively report average output elasticities for intermediate, labor, and capital inputs. Column 8 reports average returns to scale. Associated standard deviations are reported in columns 3, 5, 7, and 9. The production function estimation routine controls for time dummies.

H Connection between productivity decompositions

This section shows how the productivity decomposition by Olley and Pakes (1996) and its extension by Kehrig and Vincent (2021) relate to the productivity decomposition by Foster et al. (2001).[43] Consistent with the main text, we will focus our discussion on a balanced panel of firms.

Olley and Pakes (1996) show that aggregate productivity, defined as a size-weighted average of firm-level productivity, can be decomposed in the following way:

$$\begin{aligned}\Omega_t &= \sum_i s_{it}\omega_{it} \\ &= \sum_i (s_{it} + \overline{s}_{it} - \overline{s}_{it})(\omega_{it} + \overline{\omega}_{it} - \overline{\omega}_{it}) \\ &= N_{it}\overline{s}_{it}\overline{\omega}_{it} + \sum_i (s_{it} - \overline{s}_{it})(\omega_{it} - \overline{\omega}_{it}) \\ &= \overline{\omega}_{it} + Cov(s_{it}, \omega_{it}). \end{aligned} \tag{H1}$$

Ω_t, ω_{it}, and s_{it} denote aggregate productivity, firm-level productivity, and firms share of economic activity (for our labor productivity decomposition, we use employment weights), respectively. Bars indicate unweighted averages and $Cov(.)$ denotes the covariance. Note that the covariance is not divided by the sample size. Hence, the covariance measure in the above decomposition is invariant to the sample size.

Equation H1 shows that changes in aggregate productivity are fully described by changes in the unweighted average of firm productivity and the covariance between productivity and firm size. The latter measures the extent to which larger firms are more productive, which, in some basic models, can be interpreted as a measure for allocative efficiency (e.g., Bartelsman et al. 2013).

Kehrig and Vincent (2021) show that the Olley-Pakes-decomposition can be further decomposed in the following way:

$$\Delta\Omega_t = \underbrace{\Delta\overline{\omega}_{it} + Cov(\Delta\omega_{it}, s_{it_0})}_{\text{within-firm changes}} + \underbrace{Cov(\omega_{it_0}, \Delta s_{it})}_{\text{market share changes}} + \underbrace{Cov(\Delta\omega_{it}, \Delta s_{it})}_{\text{cross-term}}. \tag{H2}$$

[43] Foster et al. (2001) refer to their decomposition as a modified version of a decomposition used by Baily et al. (1992).

In the main text, we interpret the first two terms as productivity growth resulting from within-firm changes. Intuitively, within-firm productivity changes consist of a part describing firm productivity changes across the entire firm distribution ($\Delta\overline{\omega}_{it}$) and a part accounting for size heterogeneity, i.e., the extent to which productivity changes within firms of different size contribute to the aggregate productivity index ($Cov(\Delta\omega_{it}, s_{it_0})$). The third term of this decomposition captures productivity changes due to market share changes. The last term is a cross-term. It accounts for changes in productivity due to changes in firms' size (e.g., decreasing marginal products). In our application we interpret the third and fourth terms as jointly capturing the contribution of reallocation processes to productivity growth (as in De Loecker et al. 2020).

As noted by Kehrig and Vincent (2021), we can translate the above decomposition into the productivity decomposition by Foster et al. (2001). This will show that our within-firm term and the within-firm term in the decomposition by Foster et al. (2001) are identical. To see this, rewrite the covariance terms of equation H2 as sums:

$$\begin{aligned}\Delta\Omega_t = \Delta\overline{\omega}_{it} &+ \sum_i s_{it_0}\Delta\omega_{it} - \underbrace{\sum_i s_{it_0}\overline{\Delta\omega}_{it}}_{\overline{\Delta\omega}_{it}} - \underbrace{\sum_i \overline{s}_{it_0}\Delta\omega_{it}}_{\Delta\overline{\omega}_{it}} + \underbrace{\sum_i \overline{s}_{it_0}\overline{\Delta\omega}_{it}}_{\overline{\Delta\omega}_{it}} + \\ &\sum_i \Delta s_{it}\omega_{it_0} - \underbrace{\sum_i \overline{\Delta s}_{it}\omega_{it_0}}_{0} - \underbrace{\sum_i \Delta s_{it}\overline{\omega}_{it_0}}_{0} + \underbrace{\sum_i \overline{\Delta s}_{it}\overline{\omega}_{it_0}}_{0} + \\ &\sum_i \Delta s_{it}\Delta\omega_{it} - \underbrace{\sum_i \overline{\Delta s}_{it}\Delta\omega_{it}}_{0} - \underbrace{\sum_i \Delta s_{it}\overline{\Delta\omega}_{it}}_{0} + \underbrace{\sum_i \overline{\Delta s}_{it}\overline{\Delta\omega}_{it}}_{0}\,. \end{aligned} \tag{H3}$$

Simplifying yields:

$$\Delta\Omega_t = \underbrace{\sum_i s_{it_0}\Delta\omega_{it}}_{\text{within-firm changes}} + \underbrace{\sum_i \Delta s_{it}\omega_{it_0}}_{\text{market share changes}} + \underbrace{\sum_i \Delta s_{it}\Delta\omega_{it}}_{\text{cross-term}}\,. \tag{H4}$$

which is identical to the decomposition by Foster et al. (2001).[44]

[44]Note that in the original decomposition by Foster et al. (2001), the term capturing market share changes is expressed in relation to the aggregate productivity index. In the above derivation, this aggregate productivity index cancels out and can thus be neglected. I.e., it holds that $\sum_i \Delta s_{it}(\omega_{it_0} - \Omega_{t_0}) = \sum_i \Delta s_{it}\omega_{it_0}$.

I Measures of allocative efficiency

In the following, we explain the two approaches to measure allocative efficiency of the main text (Table 7) in more detail. We first explain the approach by Hsieh and Klenow (2009) and subsequently discuss the approach by Petrin and Sivadasan (2013).[45] Both approaches rely on measuring the marginal revenue product of labor at the firm level. We describe how we estimate labor's marginal revenue product from firms' production functions in section G. Importantly, deriving the marginal revenue product from the estimated production function addresses several concerns that the literature expressed toward the approach of Hsieh and Klenow (2009). Most notably, our estimation of marginal revenue products allows for decreasing and increasing returns to scale, varying output elasticities, and imperfect competition.

Hsieh and Klenow (2009). The approach by Hsieh and Klenow (2009) is based on a standard static heterogeneous firm framework in which firms produces with a Cobb-Douglas production function and firm output is aggregated through a CES-aggregator. For identifying distortions, the choice of the Cobb-Douglas specification is not key, but we still apply it for tractability in this section. In fact, in our application, we derive marginal revenue products from a translog production function (see Appendix G). Furthermore, in this section, we consider a one-input (labor) version of this model and focus on the main insights for measuring allocative efficiency while our production function estimation also considers capital and intermediate inputs. For the complete model, we refer to Hsieh and Klenow (2009).

Aggregate output, Q_t is a CES-aggregate of N_i differentiated products:

$$Q_t = \left(\sum_{i=1}^{N_i} Q_{it}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}. \tag{I1}$$

Firms produce output using the production function $Q_{it} = L_{it}^{\alpha} A_{it}$, where we follow the notation in Hsieh and Klenow (2009) and denote firms' total factor productivity (TFPQ) by A_{it}. Suppose that firms' face distortions, τ_{it}, in the labor market that cause a wedge between the marginal revenue product of labor and the wage. Profits are given by:

$$\pi_{it} = P_{it}Q_{it} - (1+\tau_{it})wL_{it}. \tag{I2}$$

[45] Note that also the covariance in the decomposition by Olley and Pakes (1996), that we also analyze in the main text, is sometimes viewed as another simple statistic for allocative efficiency.

P_{it} denote output prices and wL_{it} are labor costs. Note that in the model of Hsieh and Klenow (2009), wages are identical across firms. The first order condition implies:

$$MRPL_{it} = (1 + \tau_{it})w. \tag{I3}$$

Hence, without distortions, marginal revenue products of labor ($MRPL_{it}$) equalize across firms. Define revenue-productivity as $TFPR_{it} = A_{it}P_{it}$. Using the CES-structure to solve for firms' prices, we utilize the first order condition to express TFPR as a function of the MRPL:

$$TFPR_{it} = \frac{\sigma}{\sigma - 1}\left(\frac{MRPL_{it}}{\alpha}\right)^{\alpha}. \tag{I4}$$

Equation (I4) implies that TFPR is equalized in the absence of firm-specific distortions. Following Hsieh and Klenow (2009), aggregate TFPQ can be expressed as:

$$TFPQ_t = \left[\sum_{i=1}^{N_i}\left(A_{it}\frac{\overline{TFPR_t}}{TFPR_{it}}\right)^{\sigma-1}\right]^{\frac{1}{\sigma-1}}, \tag{I5}$$

where $\overline{TFPR_t}$ is a geometric average of firm-level TFPR. In the absence of distortions, TFPR is equalized across firms and aggregate TFPQ is given by:

$$TFPQ_t = \left[\sum_{i=1}^{N_i} A_{it}^{\sigma-1}\right]^{\frac{1}{\sigma-1}}. \tag{I6}$$

As the impact of distortions on aggregate TFPQ is completely captured by dispersion in the marginal revenue product of labor (equation (I4)), we directly analyse the dispersion in the MRPL in the main text.[46] Notably, we are interested only in whether the minimum wage reduces or increases allocative efficiency through its impact on MRPL dispersion. Using the Hsieh and Klenow (2009) framework to precisely quantify the productivity effects of allocative inefficiences would require us to invoke much more assumptions and to apply the structural framework of Hsieh and Klenow (2009) to the data. This goes beyond the scope of this study and is unlikely to provide interesting insights as we find no statistically significant effect of the minimum wage on MRPL dispersion.

[46] As argued in Hsieh and Klenow (2009), marginal revenue product dispersion reduces aggregate TFPQ because firm-level TFPR and TFPQ are positively correlated in the data of Hsieh and Klenow (2009). This has also been shown in various other studies (e.g., Foster et al. 2008).

Petrin and Sivadasan (2013). The approach by Petrin and Sivadasan (2013) starts from the definition of aggregate productivity growth (APG) as the difference between the change in aggregate final demand and the change in aggregate costs:

$$APG_t \equiv \sum_{i=1} P_{it} dQ_{it} - \sum_{i=1} w_{it} dL_{it}, \tag{I7}$$

where dQ_{it} denotes the change in output and dL_{it} is the change in labor inputs. Note that we take period t as the reference period for output prices, P_{it}, and wages, w_{it}. Above, we only include labor as primary input. Each firms' production technology is given by $Q(L_{it}, \omega_{it})$. Firms pay a sunk fixed costs, F_{it}, that is normalized to the equivalent of forgone output, such that we can write $Q_{it} = Q(L_{it}, \omega_{it}) - F_{it}$. When Q_{it} is differentiable, we can decompose equation (I7) in the following way:

$$APG_t = \sum_{i=1} \left(P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it} \right) dL_{it} - \sum_{i=1} P_{it} dF_{it} + \sum_{i=1} P_{it} \frac{\partial Q_{it}}{\partial \omega_{it}} d\omega_{it}. \tag{I8}$$

The first term of equation (I8) denotes the productivity growth gains from reallocation, which is also the part of aggregate productivity growth on which we focus in this section. The second term denotes the value of lost output resulting from fixed or sunk costs, whereas the last term captures the gains from changes in technical efficiency. Following Petrin and Sivadasan (2013), equation (I8) assumes perfect competition. As we discuss below, in our empirical approach we also allow that firms' have market power in product markets.

Note that the reallocation term compares the value of the marginal product of labor (VMP), $P_{it} \frac{\partial Q_{it}}{\partial L_{it}}$, with its input costs. Petrin and Sivadasan (2013) show that the average absolute gap across firms between labor's VMP and wage equals the average productivity gain from adjusting labor by one unit in the optimal direction at every firm (ceteris paribus). To see this, denote IND_{it} as an indicator variable that captures the unit adjustment of labor in the optimal direction for firm i, i.e., $IND_{it} = 1$ if $P_{it} \frac{\partial Q_{it}}{\partial L_{it}} > w_{it}$ and $IND_{it} = -1$ if $P_{it} \frac{\partial Q_{it}}{\partial L_{it}} < w_{it}$. Petrin and Sivadasan (2013) then write the average productivity gain from adjusting labor by one unit in the optimal direction as:

$$\frac{1}{N} \sum_{i=1}^{N} \left(P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it} \right) IND_{it} = \frac{1}{N} \sum_{i=1}^{N} |P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it}|. \tag{I9}$$

Petrin and Sivadasan (2013) use equation (I9) to measure the extent of allocative inefficiencies based on the potential gains in aggregate productivity growth when adjusting one unit of labor into the right direction at every firm. As further discussed in Petrin and Sivadasan (2013), the value of the marginal product in equation (I9) will be replaced by the marginal revenue product if firms have output market power. We apply this adjusted version of equation (I9) when assessing the extent of allocative inefficiency in the main text.

The approaches of Petrin and Sivadasan (2013) and Hsieh and Klenow (2009) are similar in spirit. Both infer misallocation from variation in marginal revenue products across firms. The key difference between both approaches is that Petrin and Sivadasan (2013) base their approach on the definition of aggregate productivity growth and consider that (exogenous) wages may vary between firms.

J Spillover Effects

We closely follow Berg et al. (2021) to test for the presence of spillovers to our high-wage control group by exploiting variation in labor market treatment intensity. We calculate the gap measure as in equation 6 for the industry-state level (which is also the aggregation level of our reallocation analysis).

We include this aggregate gap measure into our firm-level difference-in-differences regressions in the following way:

$$\Delta y_{it} = \alpha + (GAP_{jr} \times \boldsymbol{T_i})\boldsymbol{\beta} + \phi_r + \psi_j + \epsilon_{it}. \tag{J1}$$

Δy_{it} denotes the employment growth of firm i relative to 2013, $\boldsymbol{T_i}$ is the vector of treatment indicators, and ϕ_r and ψ_j are (centered) region and industry fixed effects.[47] GAP_{jr} depicts the gap measure derived from the worker-level data (cf. equation 6) in industry j and region r. Intuitively, including the aggregate gap measure into the firm-level regression accounts for aggregate treatment levels that might create spillovers between our treatment and control group (e.g, from reallocation). Table J1 reports the regression results. We do not find higher employment growth in high-wage firms (control group) in more affected region-industry cells. The coefficient of GAP is statistically insignificant and slightly negative. Moreover, there is also no differential effect on affected firms in more versus less exposed labor markets. The interaction terms are positive, but statistically insignificant. Additionally, all other regression coefficients are closely in line with our baseline specification of the main text. We thus conclude that there are no spillovers that affect our control group within our firm-level analysis.

[47] The results also hold without controlling for industry- and region-specific developments and with clustered standard errors at the industry×region level.

Table J1: Spillover Effects for Employment

	2013 to 2015		2013 to 2012	
	(1)	(2)	(3)	(4)
	Baseline	Industry-Region Spillover	Baseline	Industry-Region Spillover
Manufacturing				
med	-0.002	-0.005	0.001	0.002
	(0.003)	(0.004)	(0.003)	(0.003)
low	-0.037***	-0.040***	0.008	0.011*
	(0.007)	(0.008)	(0.006)	(0.007)
GAP		-0.027		0.009
		(0.017)		(0.017)
med × GAP		0.024		-0.008
		(0.016)		(0.017)
low × GAP		0.023		-0.011
		(0.017)		(0.017)
Constant	0.022***	0.026***	-0.011***	-0.012***
	(0.002)	(0.003)	(0.002)	(0.003)
N	9471	9471	9471	9471
R-sq	0.012	0.013	0.008	0.008
Mean Exposure		0.273		0.273
Service Sector				
med	-0.017***	-0.020***	-0.012***	-0.011**
	(0.005)	(0.007)	(0.004)	(0.005)
low	-0.035***	-0.040***	-0.010*	-0.004
	(0.007)	(0.009)	(0.006)	(0.007)
GAP		-0.003		0.003
		(0.007)		(0.005)
med × GAP		0.007		-0.002
		(0.008)		(0.005)
low × GAP		0.007		-0.005
		(0.008)		(0.005)
Constant	0.048***	0.049***	-0.027***	-0.029***
	(0.004)	(0.006)	(0.003)	(0.004)
N	29810	29798	29810	29798
R-sq	0.006	0.007	0.005	0.005
Mean Exposure		0.924		0.924
Region FE	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes

Note: Results from regressing the change in log employment from 2013 to 2015 and from 2013 to 2012 on the treatment indicator interacted with the GAP measure computed at the two-digit industry × state level (cols. 2 and 4). Cols. 1 and 3 report our baseline specification from the main text (Table 2) to ease comparison.*low* denotes low-wage and thus highly exposed firms while moderately exposed firms are denoted by *med*. Firms with an average annual wage above €40,000 form the reference group. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.