Halle Institute for Economic Research
Member of the Leibniz Association

Discussion Papers

No. 8

April 2023

Minimum Wages, Productivity, and Reallocation

Mirja Hälbig , Matthias Mertens, Steffen Müller

Authors

Mirja Hälbig
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Structural Change and Productivity
E-mail: mirja.haelbig@iwh-halle.de
Tel +49 345 7753 704

Matthias Mertens
Massachusetts Institute of Technology and Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Structural Change and Productivity, and The Competitiveness Research Network (CompNet)
E-mail: mmertens@mit.edu
Tel +49 345 7753 707

Steffen Müller
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Structural Change and Productivity, Otto von Guericke University Magdeburg, CESifo, and IZA
E-mail: steffen.mueller@iwh-halle.de
Tel +49 345 7753 708

The responsibility for discussion papers lies solely with the individual authors. The views expressed herein do not necessarily represent those of IWH. The papers represent preliminary work and are circulated to encourage discussion with the authors. Citation of the discussion papers should account for their provisional character; a revised version may be available directly from the authors.

Comments and suggestions on the methods and results presented are welcome.

IWH Discussion Papers are indexed in RePEc-EconPapers and in ECONIS.

Editor

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Address: Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany
Postal Address: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

Minimum Wages, Productivity, and Reallocation*

First version: 18.04.2023
This version: 12.04.2024

Abstract

We study the productivity effect of the German national minimum wage combining administrative firm datasets. We analyze firm- and market-level effects, considering output price changes, factor substitution, firm entry and exit, labor reallocation, and short- versus long-run effects. We document higher firm productivity even net of output price increases. Productivity gains are persistent in manufacturing and service sectors. The minimum wage also increased manufacturing productivity at the aggregate level. Neither firm entry and exit nor other forms of employment reallocation between firms contributed to these gains. Instead, aggregate productivity gains from the minimum wage solely stem from within-firm productivity improvements.

Keywords: minimum wage, pass-through, productivity, reallocation

JEL classification: D24 , J31, L11, L25

* We received valuable comments from Philipp vom Berge, Michael Boehm, Mario Bossler, Sebastian Findeisen, Bernd Fitzenberger, Thilo Kroeger, Christian Merkl, Duncan Roth, Claus Schnabel, Uta Schoenberg, Matthias Umkehrer, and participants of the EALE, EARIE, VfS conferences, the German National Productivity Dialogue, and IAB and IWH seminars.

1 Introduction

Accompanied by intense public and academic debates, Germany introduced a national minimum wage 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 affected firms, however, any adverse effects on workers, employers, or consumers can be reduced or even reversed into positive effects. Limited evidence for China (Hau et al. 2020) and Vietnam (Nguyen 2019) indeed suggests positive firm-level productivity effects, whereas Bossler et al. (2020a) do not find effects on sales per worker in German firms.[1]

While evidence on firm-level productivity effects of minimum wages is limited, even less is known about the impact of minimum wages on market-level/aggregate productivity. Aggregate productivity changes may arise from within-firm productivity improvements and/or employment (market share) reallocation between producers of different productivity levels (Ol-

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. Coviello et al. (2022) document that salespeople working at a large US retail company became more productive after minimum wage increases. 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. However, Butschek (2022) finds that German firms became more selective in their hiring decisions after the introduction of the minimum wage.

ley and Pakes 1996). The distinction between these two channels is relevant for understanding the welfare implications of productivity gains. Most notably, there are costs of reallocation that do not exist for within-firm productivity improvements, such as moving or commuting costs (Dustmann et al. 2022) and hiring costs. Without directly observing productivity, Dustmann et al. (2022) document that the German minimum wage induced employment reallocation toward firms with higher *predicted* initial productivity and conclude that allocative efficiency improved. However, whether such reallocation processes are large enough to increase formal measures of aggregate productivity or whether they are reinforced or muted by productivity changes within affected firms has not been studied yet. Against this backdrop, our article provides a thorough assessment of firm- and market-level productivity effects of the minimum wage using detailed productivity data for Germany, that covers output price changes, allows for longer-run analyses, and enables us to contrast the role of within-firm productivity improvements vs. reallocation processes in driving market-level productivity effects.

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 or true technical efficiency gains. Similarly, without studying the market-level, it is impossible to understand the quantitative relevance of within-firm productivity effects, as firms' relative sizes matter for aggregate productivity changes. Our contribution is to provide a holistic view of all these effects in the context of the implementation of a nationwide minimum wage in the largest European economy. Whereas many previous studies rely on survey data and are thus plagued by issues such as unit non-response and small sample sizes, we leverage high-quality administrative data gathered through compulsory firm reporting by the German statistical offices. We combine several administrative data sets including linked employer-employee data and the newly available business registry. This allows us to provide a first comprehensive assessment of the firm-level and aggregate productivity effects of minimum wages. 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 revenue productivity effects are driven by changes in prices or quantities produced.

At the micro level, we employ a difference-in-differences framework. 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). The effect on firm exit is negligible.

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 effects on labor productivity, measured as value added per FTE worker, amount to 5.6% (manufacturing) and 10.6% (services). Leveraging additional data from the German business registry from 2010 to 2017 containing sales and headcount information for the population of firms, we show that long-term pre-trends in productivity do not differ between the treatment and control group and that the significant productivity gains persist over time. These strong productivity improvements in affected firms likely mitigated employment and output price adjustments and are therefore key to understanding why the introduction of the German minimum wage has been found to have very limited employment effects at the aggregate level.

The documented labor productivity gains can result from gains in total factor revenue-productivity (TFPR) or from an increasing reliance on non-labor inputs. We do not find any effects of the minimum wage on investments per FTE, which implies a temporary increase in capital intensity in shrinking firms. Further, relative to firms in the control group, affected firms became 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' TFPR equals 3.1%, whereas quantity productivity (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 true efficiency (TFPQ) improvements explain a substantial part of the firm-level labor productivity gains.

Having established firm-level results, we examine how these within-firm productivity changes and reallocation processes affect 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.

We confirm the results of Dustmann et al. (2022) and document that more affected low-productivity firms lose employment relative to unaffected high-productivity firms, which continue to grow during the minimum wage introduction. This suggests that labor has been allocated away from low-productivity firms due to the minimum wage, although employment effects are generally moderate. To quantify the relevance of such reallocation effects in contrast to the documented within-firm productivity gains, we focus on aggregate industry×region cells. We define aggregate (cell-level) productivity as the employment-weighted average of firms' labor productivity. We then compare cells more affected by the minimum wage with less affected cells utilizing linked employer-employee data containing hourly wage information. Whereas the effect on aggregate productivity is close to zero in the service sector, it is positive in manufacturing: for each percentage point by which a cell had to raise wages to comply with the minimum wage requirement, there is a corresponding 1.4% increase in labor productivity. Using an established productivity decomposition method by Olley and Pakes (1996) and various extensions of it, we show that these aggregate productivity gains are completely driven by improvements in within-firm productivity rather than from firm entry and exit or other forms of labor reallocation. Therefore, we conclude that although the minimum wage did induce reallocation processes, these processes have not notably contributed to the overall increase in aggregate productivity in the German manufacturing sector.

The remainder of our article proceeds as follows: Section 2 relates our study to the existing literature. Section 3 provides information on the institutional background of the minimum wage introduction. Section 4 describes the various data sets we combine and use. The empirical analysis is divided into two parts: Section 5 describes the empirical framework and shows the results of our firm-level difference-in-differences analysis. In Section 6, we aggregate our data to the region-industry level to study the reallocation of workers between firms. Section 7 concludes.

2 Relation to literature

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 only provide 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 a main

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

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 found 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 price effects. This has two important implications: first, productivity studies are silent on whether rising 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 large scale 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.

Finally, our paper contributes to the macroeconomic literature on the reallocation of production factors across firms. The reallocation of production factors from less to more productive firms plays a key role in understanding productivity growth in Schumpeterian growth models (e.g., Aghion et al. 2014). Empirically, the contribution of reallocation processes to produc-

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.

tivity growth has been studied in various contexts.[4] Importantly, welfare implications differ depending on whether productivity gains result from within-firm improvements or reallocation. Although a welfare analysis is beyond the scope of this study, it is clear that reallocation causes costs that do not exist for within-firm productivity improvements. Private costs on the worker side include moving or commuting costs and potential utility losses from changes in non-pecuniary job characteristics (Sorkin 2018; Yi et al. 2024). Furthermore, depreciation of firm-specific human capital may result in welfare losses that extend beyond wage losses of individual workers (Lachowska et al. 2020; Fackler et al. 2021). On the firm side, productivity gains from reallocation come at the expense of additional costs for hiring and training new staff. Whether workers enjoy better or worse amenities when moving to higher-paying firms is an open question for the German labor market. Sorkin (2018) reports that compensating wage differentials matter in the US, and Dustmann et al. (2022) conclude that commuting costs increased after the introduction of the minimum wage in Germany.[5]

Our reallocation analysis is similar in spirit to Dustmann et al. (2022), who empirically investigate whether employment reallocation across firms in response to the introduction of the German minimum wage is directed toward firms *predicted* to be more productive before the minimum wage introduction. However, our data differ substantially from Dustmann et al. (2022). Although we cannot follow individual workers over time, the major advantage of our data is that we actually observe firm productivity. This not only improves productivity measurement, which is crucial given the substantial 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. Leveraging the strengths of our data, we provide the first formal analysis on whether reallocation processes between firms induced by the minimum wage exert a significant impact on aggregate productivity.

4. To name a few examples: Olley and Pakes (1996) study deregulation in the US telecommunications industry, Hsieh and Klenow (2009) analyse the role of frictions in China and India relative to the US, Backus (2020) investigates the role of competition in the US ready-mixed concrete industry, Bartelsman et al. (2013) study the transition of Eastern European countries toward market economies during the 90s, and Bighelli et al. (2023) analyse the connection between firm concentration and productivity growth.

5. In contrast to reallocation costs, within-firm productivity gains that result from increased worker effort could also be associated with a reduction in worker utility (e.g., Ippolito 2003; Hirsch et al. 2015; Ku 2022; Coviello et al. 2022) . However, according to Bossler and Broszeit (2017), job satisfaction improved and there is no evidence for higher worker engagement in Germany.

3 Institutional Background

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

The general statutory minimum wage became effective in Germany on January 1st, 2015, and was introduced at a level of €8.50 gross per hour. The minimum wage is continuously adjusted by a minimum wage commission, which consists of representatives from employer and employee associations. The minimum wage was raised to €8.84 effective January 1st, 2017, and increased to €10.45 on July 1st, 2022. With the change in government in 2021, it was decided to increase the minimum wage to €12 on October 1st, 2022. Prior to 2015, several sector-specific minimum wages were in place. Sectors with existing minimum wages below €8.50 were granted a transition period through January 2017. Transitional arrangements apply to the following employees: meat industry workers, hairdressers, contract workers, laundry service providers for large customers, agriculture and forestry workers, textile industry workers and horticulture workers. We exclude firms belonging to the respective industries from our analysis.

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

Compliance control resides with the customs authorities. Enforcement mechanisms include an obligation to record working hours for specific industries deemed at high risk of noncompliance and suspicion-based controls. Thus, compliance with the minimum wage requires a sufficient level of information among both workers and employers. While the literature unanimously documents positive effects on hourly wages for low-wage workers in response to the minimum wage (Bossler and Gerner 2020; Caliendo et al. 2023; Dustmann et al. 2022), there is mixed evidence on compliance with the minimum wage law in Germany. On the one hand, using survey data from the German Socio Economic Panel, Burauel et al. (2017) and Caliendo et al. (2023) show evidence that a substantial fraction of eligible workers earned wages below the minimum wage level shortly after the minimum wage became effective. On the other hand, using administrative data from the Federal Employment Agency and German Statistical Office, respectively, Dustmann et al. (2022) find average wage growth in the order of magnitude that is to be expected under full compliance with the minimum wage law, and Biewen et al. (2022) present evidence that compliance with the minimum wage level was achieved to a large extent by 2018.[6]

4 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 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.[7] 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.

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

7. Moreover, Bossler and Schank (2023) report that the IAB establishment panel greatly underestimates minimum wage incidence relative to the German Structure of Earnings Survey provided by the statistical offices.

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.[8] Among others, the data include information on firms' costs, total sales, investment and, most notably, quantities and sales by detailed 10-digit product codes defining approximately 6,000 different products.[9] 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 this data's usability for long-term analyses.

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)*.[10] The SiD is a yearly collected representative and stratified 15% sample of all firms with an annual revenue of at least €17,500 in sectors H, J, L, M, N, and S/95 of the NACE Rev.2 classification, covering about 150,000 firms per year.[11] 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 are exempt from completing the full questionnaire and lack information on intermediate input expenditures. We therefore restrict the sample to firms with revenues above €250,000 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

8. 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.

9. 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".

10. 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.

11. 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.

survey's usability for long-term analyses.

Main sample definition. To study the development of firms over multiple years we restrict the main sample to firms reporting from 2012 to 2015. Hence, we include service sector firms before and after the 2014 redraw.[12] We exclude industries exempt from the minimum wage.[13] 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.[14] 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.[15] 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.[16] Importantly, the direct link between the VSE and our firm-level datasets is limited, as all these datasets are in-

12. We show in Appendix Figure B1 that unconditional sample exit probabilities are unaffected by the minimum wage. Linear probability models for sample attrition controlling for sector, region, and size show no increased attrition propensity for manufacturing firms but a slight increase of 1 percentage point in the attrition propensity for service firms (Appendix Table B1). Consistent with our results, 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.

13. 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).

14. 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.

15. The VSE data pertain to April 2014.

16. 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 13).

dependently 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.

Business Registry. We complement our main firm level data with business registry data from the German Statistical Offices from 2010 to 2017.[17] The dataset includes information on firms' sales, employees, location, and industry. It is based on annual snapshots of the German business registry and includes all firms from sectors B-N and P-S (NACE Rev.2) that are located in Germany and that have at least one employee[18] or taxable annual sales of at least €17,500 in the reporting year. We restrict the sample to firms that report both sales and employees for the given year. The information in the business registry does not permit the construction of a total factor productivity or value added labor productivity measure as in our main dataset. Moreover, it lacks information on FTE and only contains headcounts as labor inputs. However, it has one key advantage: it covers the universe of active firms in each year. This feature enables us to examine (i) the effects on sales per worker over an extended period and (ii) firm entry and exit. Moreover, the data also include small firms. We use this data in two ways: Firstly, we merge the data to our main sample of firms and analyze pre-trends and long-run effects. Secondly, we use the business registry as an independent dataset to study whether firm entry and exit in response to the minimum wage introduction contributed to aggregate productivity gains.

5 Firm-Level Analysis

5.1 Empirical Approach

We compare the evolution of outcomes at firms highly affected by the minimum wage with those of supposedly unaffected firms. Our main analysis focuses on the three years before and the one year after the introduction

17. We combine the AFiD-Panel URS-Neu (data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/52121.2019.00.01.1.1.0) and the AFiD-Panel URS95 (data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/52111.2012.00.01.1.1.0).

18. Employment information is collected by the Federal Employment Agency and transmitted to the statistics. Employment numbers only include employees subject to social insurance.

of the minimum wage (2012-2015), because we can compute more sophisticated productivity measures for this period. In Section 5.6, we extent the time frame to additional years using sales per employees as productivity measure and provide evidence on long-run effects and the absence of pre-trends.

Similar to Draca et al. (2011), we use information on firms' average wages to define treated firms. Specifically, we use the annual wage bill per FTE averaged over the pre-treatment years 2012-2014 from the KSE and SiD.[19] From this, we construct three wage categories:

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

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

19. 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 the 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 (means) by treatment category for the year 2013. The average 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 are smaller, exhibit lower labor productivity, export less frequently, are more often located in East-Germany (where wages are generally lower), have higher shares 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 post-policy 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 sample means to interpret the regression intercept as the mean change for control group (high-wage) 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) pre-trends 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 pre-trends for several reasons. First, controlling for pre-trends 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 3. 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, there is only one pre-treatment change (2012 to 2013) available in our baseline analysis, which is arguably too short to reliably control for trends. Instead, we always report the effect on "placebo" changes, which equal the year 2013 outcome minus the year 2012 outcome, alongside our main effects and direct the reader's attention to cases in which these changes significantly differ between the treatment and control groups. Finally, in a robustness analysis in Section 5.6, we use additional data on sales per worker dating back to 2010 and find no evidence for pre-trends affecting our results.

5.2 Validation of Treatment Definition

The accuracy of our treatment definition hinges on the wage spread within the firm and the distribution of low wage workers across the treatment categories. To validate our treatment indicators, we use information on workers' hourly wages from the 2014 wave of our linked employer-employee data (*VSE*) that can be matched to a subset of firms from our firm-level data.[20] 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 firm-level gap measure using data from individual workers:

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

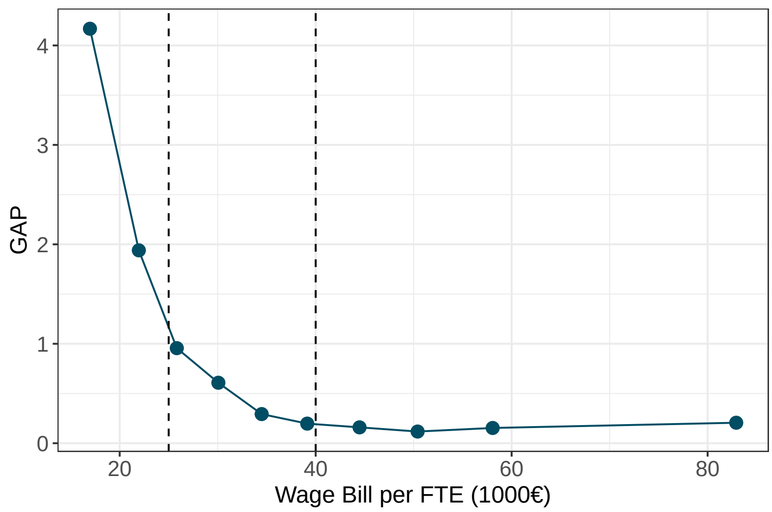
where w_{min} depicts the minimum wage (€8.5), w_{iz} is the gross hourly wage for worker z in firm i, and h_{iz} denotes hours worked. The lower bound of the gap measure is zero for unaffected firms. The measure depicts the percentage increase in a firm's wage bill needed to comply with the minimum 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 5,567 firms merged to the VSE data.[21] The vertical

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

21. An alternative measure of minimum wage exposure often used in the literature is the fraction of affected workers (Harasztosi and Lindner 2019). Figure C2 in the appendix shows the relationship between this alternative measure and the average wage.

dotted lines separate our three groups of differently affected firms. The gap measure increases rapidly for firms with an average pre-policy annual wage 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 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 firms' minimum wage exposure (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 pretreatment average of annual wage costs per FTE divided into 10 equally-sized bins. The red lines depict the thresholds for the three treatment groups: $[min; €25{,}000)$; $[€25{,}000; €40{,}000)$; $[€40{,}000; max]$. N = 5567.

5.3 Wages and Employment

We start by presenting descriptive evidence on changes in firm wages and employment around the introduction of the minimum wage in appendix Figure C1. The figure plots average yearly firm wage (Panel (a)) and employment (Panel (b)) growth against the initial wage bin, separately for 2012 to 2013, 2013 to 2014, and 2014 to 2015. Panel (a) illustrates that firms with a pre-policy average wage below €25,000 experienced the most pronounced increase in average wages after the minimum wage law became effective, confirming that the minimum wage was binding for these firms. Firms with initial wages below €20,000 experienced 11 percentage points higher growth in average wages from 2014 to 2015 (13.5%) than over the 2012-2013 pre-policy period (2.5%). Panel (b) repeats the same exercise for annual employment growth, showing a consistent pattern. Firms with initial average wages below €20,000 shrank after the introduction of the minimum wage. Employment growth rates for firms with initial wages above €35,000 do not differ between post- and pre-policy years.

Appendix Table A1 presents the associated regression results based on the specification in Equation (1) separately for manufacturing and service sector firms. We find strong positive effects of the minimum wage on wages per FTE in manufacturing (+6.5%) and services (+14.2%). These effects are associated with mild negative employment effects in manufacturing (-3.7%) and service sector (-3.5%) firms, which are driven by reductions in employment for full-time workers (appendix Table A2). 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 based on alternative German worker-level data (Dustmann et al. 2022) and employer surveys (Bossler et al. 2018).[22] In Appendix B, we show that the probability of firm exit in 2015 is not affected by the minimum wage introduction in the manufacturing sector. In the service sector, affected firms consistently show a higher exit probability of approximately 1 percentage point both before and after the minimum wage introduction.

5.4 Labor Productivity

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 2 reports the results for sales, value added, and labor productivity. Columns 1 and 4 of Panel (a) show an *increase* in total revenues in affected manufacturing (+2.5%) and service firms (+4%) relative to high-wage control group firms despite reduced employment. This increase is also associated with a rise in value added (Panel (b)). Corre-

22. Reallocation effects could imply spillovers from 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 E. We find that the impact of such spillover (i.e., reallocation) effects on our firm-level analysis is negligible.

Table 2: Sales, Value Added, and Labor Productivity

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2012 to 2013
Panel (a): 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)
Panel (b): 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)
Panel (c): 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 from 2012 to 2013 (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.

spondingly, we document a substantial increase in labor productivity (Panel (c)), amounting to 5.6% (10.6%) higher growth in manufacturing (services). Interestingly, the positive productivity effect for low-wage firms in the service sector is partly driven by a sharp decline in labor productivity in high-wage firms (constant). As in case of the wage patterns (Table A1), a possible explanation for this sharp decline in productivity is the inflow of low-wage workers. Finally, note that the increase in labor productivity between 2012 and 2013 (Column 3, Table 2) might raise concerns that our results are driven by pre-trends. In Section 5.6, we discuss this further and show that there is no legitimate concern regarding pre-trends affecting our results.

The labor productivity gains originate from either an increase in firms' revenue-TFP or a change in firms' production factor mix. Appendix Table A3 shows that labor productivity changes are not driven by an increase in investment per FTE, which confirms 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 A4 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 contributes to the rise in sales reported above and highlights that a productivity analysis of the minimum wage hinges on the availability of intermediate input data.

5.5 TFP and Prices

Table 3: Total Factor Productivity and Prices

	Manufacturing		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013
Panel (a): 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)
Panel (b): 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)
Panel (c): 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 from 2012 to 2013 (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.

Any revenue-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) and revenue-TFP (TFPR) rise, whereas in the second case, only 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 conduct this analysis 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.[23] 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 De Loecker et al. (2016).[24]

Table 3 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 increase in TFPR can be explained by an increase in output prices. The remaining two-thirds reflect gains in true technical efficiency (TFPQ).

5.6 Pre-Trends and Long-Run Effects

A limitation of our previous analysis is the short period of time for which we have information on value added labor productivity and total factor productivity. This prevented us from carefully studying the presence of pre-trends and the extent to which productivity effects prevail in the long-run. To address these issues, we combine our baseline sample with business registry data that contains information on sales and employment (headcounts) for the population of firms.[25] Using these data, we can track our sample firms from 2010 to 2017. As the business registry does not report wage information, we take the treatment definition from our main sample and

23. See Appendix G for further details on how to compute this index.

24. 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 I for details on the production function estimation and the calculation of TFPR and TFPQ.

25. Until 2012, the German business registry only includes information on employees subject to social security contributions. To ensure comparability over time, we apply this definition consistently for all years.

merge the respective firms to the business registry.[26] Moreover, we cannot compute value added labor productivity and have to instead define productivity in terms of sales per employees.

Figure 2 reports results from difference-in-differences regressions following the regression specification in Equation (1) using the merged data.[27] The first key insight is that there is no long-run pre-trend in labor productivity. In particular, the estimates for the years 2010 and 2011 indicate that the increase in labor productivity between 2012 and 2013 (also shown in Table 2) is not indicative of a long-run positive productivity pre-trend for low and medium-wage firms compared to high-wage firms. The second important result is that the positive labor productivity effects, measured in terms of sales per worker, persist and even strengthen after 2015. This points to a lasting positive impact of the minimum wage on the productivity of affected firms. What is notable is the difference in estimated effect size in our main sample and the business registry, with the business registry showing much lower, but still strong and positive effects. There are three explanations for this difference. First, the business registry data includes revenues from tax records, whereas our main sample data are based on a survey. Secondly, in the business registry, we only observe employees subject to social security contributions. Thirdly, the business registry reports employment in headcounts. In our main sample, we instead observe employment in FTE. Tables A1 and A2 show that the effect on employment in headcounts is less pronounced than the effect on FTE. This result is in line with previous studies documenting negative effects of the minimum wage on working hours (Burauel et al. 2020; Biewen et al. 2022; Bossler and Schank 2023) but only moderate effects on regular employment (Caliendo et al. 2018).

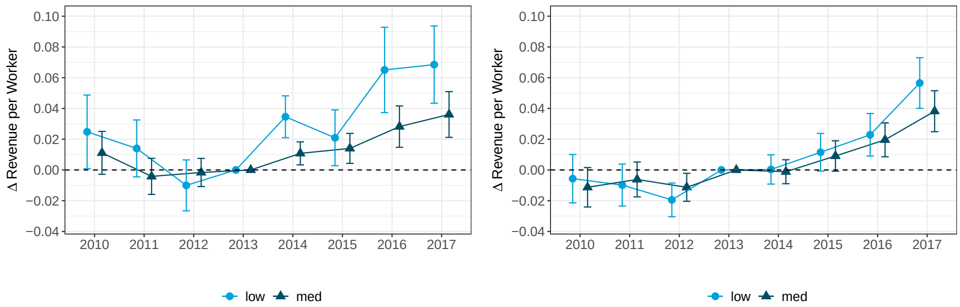
5.7 Discussion of Within-Firm Productivity Gains

We find substantial, lasting gains in labor productivity and uncover several mechanisms that contributed to them. First, we document an increase in intermediate input intensity that can be rationalized by labor becoming relatively more expensive after the introduction of the minimum wage. Such a

26. For the years 2012 to 2015, we observe the same sample of firms as in our main analysis, while we allow for entry and exit at the margins. Nonetheless, in 2010 and 2017, we still observe $\sim$ 96% of the firms from our main sample. Appendix B also shows that there is no evidence of higher exit probability for treated firms compared to previous years.

27. The regressions were estimated for each year separately. In contrast to the regression results for our main sample, we also use the difference relative to 2013 as outcome variable for the pre-period to enhance the graphical representation.

Figure 2: Pre-Trend and Long-Run Analysis for Productivity Effects



(a) Manufacturing

(b) Service Sector

Note: The graphs report results (coefficients and 90% CI) from difference-in-differences estimations that extend our baseline analysis on labor productivity in Table 2 to earlier and later years using a merge between our baseline sample and the business registry. We estimate the change in labor productivity relative to the year 2013 for each year separately. The labor productivity measure is the log of sales per employees (heads). The left figure shows results for manufacturing (N=75,094 firm × year observations), the right figure shows results for services (N=231,884 firm × year observations). All coefficients report changes relative to 2013 and are relative to changes of high-wage firms (black dashed line). Treatment groups: $[min; €25{,}000)$ for low-wage firms; $[€25{,}000; €40{,}000)$ for medium-wage firms; $[€40{,}000; max]$ for high-wage firms.

substitution of labor for intermediate inputs should increase sales per FTE and can increase even value added per FTE if the associated increase in sales per FTE is stronger than the increase in intermediates per FTE.

We also find unchanged investment per FTE. For shrinking firms, this implies a temporary increase in capital intensity, as the capital stock will only be reduced by the difference between slowly declining depreciation and instantaneously reduced investment. Holding technology fixed, capital intensity will slowly converge to its initial level, and higher labor productivity can be explained by higher capital intensity during this adjustment period. Because any productivity effect of temporarily increased capital deepening should fade away as the capital stock is adjusted downwards, our finding of longer-run productivity gains that tend to get larger over time is indicative of a rather small role of capital deepening.

For the manufacturing sector, we can additionally show that increases in output prices and in TFPQ contributed to rising labor productivity. Improved management practices have recently been discussed as a potential reason for TFP improvements in response to the minimum wage in China (Hau et al. 2020, Mayneris et al. 2018). In more general contexts, Bender et al. (2018) report that differences in management practices contribute to productivity dispersion in Germany, and Mueller (2015) concludes that wage-increasing worker co-determination in Germany increases productivity most strongly in the most poorly managed firms. Hirsch et al. (2015) show that US managers think that raising performance standards for their employees is an important tool to offset the costs of the minimum wage. Recent empirical research for the US indicates that minimum wages can indeed enhance worker effort (Coviello et al. 2022, Ku 2022). Interestingly, Bossler and Broszeit (2017) report that the minimum wage did not increases worker engagement in Germany. Another potential explanation for labor productivity gains is discussed in Butschek (2022), who demonstrated improved applicant screening that suggests improvements in worker quality of affected firms after the minimum wage introduction. We know from Bossler et al. (2020a) that training intensity did not rise in Germany.

We summarize that factor substitution, output price adjustments, improvements in worker quality, and higher TFPQ contributed to the increase in labor productivity. Based on previous studies, we further conclude that improved management practices play a potentially important role in explaining the positive effects of the minimum wage on TFPQ in Germany.

6 Aggregate Productivity Effects

To fully understand the productivity effects of the minimum wage, it is essential to examine not only the within-firm changes but also the aggregate productivity effects. The relevance of the documented within-firm changes for aggregate productivity growth hinges on the relative sizes of affected firms in their respective markets. This raises the question of whether affected firms' productivity gains are sufficiently strong to quantitatively matter at the market level. Furthermore, apart from within-firm productivity changes, reallocation processes between firms also play a role in determining the overall aggregate productivity effects of the minimum wage.

In this section, we carefully study aggregate productivity effects. Section 6.1 introduces a formal productivity decomposition that allows us to quantify the aggregate productivity growth contribution of firm-level productivity improvements, such as estimated in Section 5, and reallocation processes as documented in previous work (Dustmann et al. 2022). Section 6.2 describes our regression framework. Section 6.3 present and discusses our results, and Section 6.4 provides additional results on long-run effects and entry and exit dynamics.

6.1 Productivity Decomposition

We decompose the minimum wage effect on aggregate productivity into a within-firm and a reallocation effect following Olley and Pakes (1996). Aggregate labor productivity can be expressed as the weighted sum of firms' labor productivity levels:

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

where Ω_t is aggregate 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. Changes in aggregate productivity can be decomposed into the unweighted average of firm productivity ($\overline{\omega}_t$) and the covariance of firms' size and productivity ($Cov(s_{it}, \omega_{it})$):

$$\Delta\Omega_t = \underbrace{\Delta\overline{\omega}_t}_{\text{within-firm}} + \underbrace{\Delta Cov(s_{it}, \omega_{it})}_{\text{reallocation}}. \tag{4}$$

Equation (4) shows that changes in the aggregate level can result from (i) a shift in the unweighted average, that is, a common shock that affects all firms symmetrically, or (ii) a change in the joint distribution of productivity and firm size. Following Olley and Pakes (1996), we interpret the former as the "within-firm-component" and the latter as the "reallocation-component".

6.2 Regression Framework

We calculate the components of the decomposition in Equation (4) for 491 labor markets, defined as industry-region cells (2-digit NACE Rev.2 $\times$ NUTS1).[28] We then regress the decomposition components on the labor markets' minimum wage exposure, i.e., we compare more relative to less exposed labor markets. To identify exposed labor markets, we follow Dustmann et al. (2022) and calculate the gap measure at the labor market level. This is done analogously to our firm-level gap measure in Equation (5) utilizing the *VSE* dataset:

$$\text{GAP}_{jr} = \frac{\sum_{z \in jr} h_{zjr} \max\{w_{min} - w_{zjr}, 0\}}{\sum_{z \in jr} w_{zjr} * h_{zjr}} \times 100, \tag{5}$$

28. We exclude cells with fewer than 10 firms in the 2013 base year.

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 491 labor markets, is 0.972. This implies that if all workers who were previously paid below the minimum wage were to receive the minimum wage, ceteris paribus, the market-level hourly wage would increase by 1% on average.[29]

We regress each component of the decomposition in Equation (4) on the industry×region-level minimum wage exposure using the following regression framework:

$$\Delta y_{jrt} = \alpha + \beta \times \text{GAP}_{jr} + \epsilon_{jrt}, \tag{6}$$

where Δy_{jrt} is the change in outcome y_{jrt} (aggregate labor productivity and its decomposition components) 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 cell-level employment in 2013. As in Section 5, we present pre-treatment changes in y_{jrt}. We first present results using our balanced sample of firms for 2012-2015 (i.e., without firm entry and exit) and extent the decomposition analysis to earlier and later years in Section 6.4. There, we further extent our static decomposition to a dynamic version that accounts for entry and exit dynamics.

Worker mobility across our industry×region cells 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 F, we find similar results when we replicate our analysis allowing for (i) full cross-regional worker mobility, and (ii) full cross-industry worker mobility.

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

6.3 Main Results

6.3.1 Results

To put the productivity results into perspective, we first discuss aggregate employment and wage effects by estimating the regression according to Equation (6) with aggregate employment and wages as dependent variables. Column 1 of appendix Table A5 shows that from 2013 to 2015, aggregate employment growth was not statistically significantly different in more compared to less exposed labor markets.[30] Column 4 of appendix Table A5 shows that more exposed manufacturing labor markets experienced a larger increase in the aggregate wage bill per FTE. For the service sector, we do not find aggregate wage effects.

Table 4: Olley and Pakes (1996) Decomposition: Log Labor Productivity

	2013 to 2015			2012 to 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$	$\Delta\ \Omega_t$	$\Delta\ \overline{\omega}_{it}$	$\Delta Cov(s_{it}, \omega_{it})$
Panel (a): 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
Panel (b): 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.

Table 4 reports results from our productivity decomposition. The first major new result is that market-level labor productivity increased more strongly in more exposed manufacturing sector labor markets. Aggregate labor productivity increased by 1.4% per percentage point increase in the gap measure (Column 1). Importantly, we do not find any differential 2012-2013 pre-

30. This is in line with the results in Dustmann et al. (2022).

treatment change by minimum wage exposure (Column 4).[31] The increase in aggregate productivity results from a strong improvement in unweighted average firm productivity (Column 2), which rises by 3.1% per percentage point increase in the gap measure. This aligns well with the significant firm-level productivity gains that we estimated in Section 5. 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 for less affected markets contributing to an overall improvement in aggregate productivity, more exposed markets experience a much smaller covariance growth. The effects of the minimum wage on aggregate productivity in the service sector are overall zero, but the productivity decomposition components show the same pattern as in manufacturing, i.e., a reduced covariance and an improved unweighted average term (although statistically insignificant). We thus conclude that the minimum wage increased aggregate productivity in manufacturing by inducing within-firm productivity growth. Reallocation processes induced by the minimum wage did not contribute to aggregate productivity gains.[32]

6.3.2 Discussion

The negative covariance term appears to contradict findings in Dustmann et al. (2022) who have shown that the minimum wage caused moving workers to reallocate to more productive firms.[33] Similar to Dustmann et al. (2022), using our firm-level regression setting, Appendix D shows that the negative employment effect of the minimum wage is driven by low-wage, low-productivity firms, while high-wage, high-productivity firms continued to grow during the minimum wage introduction. Qualitatively, this result may suggest a positive productivity contribution of reallocation.

However, there are several reasons that can account for this apparent in-

31. In unreported results, we aggregated our firm-specific price index to the industry$\times$region-level and do not find 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.

32. In Appendix K, we test whether the minimum wage affected the efficiency of the worker allocation across firms using the approaches by Hsieh and Klenow (2009) and Petrin and Sivadasan (2013). We do not find evidence that the minimum wage increased allocative efficiency.

33. Crucially, Dustmann et al. (2022) demonstrate that the number of workers in affected firms that transition between firms has almost not changed in response to the introduction of the minimum wage. Rather, the minimum wage has prompted workers to shift toward firms with (ex-ante) higher *predicted* productivity, i.e., the predicted productivity gap between a moving worker's initial and new firm increased due to the minimum wage.

consistency. Firstly, the productivity effect of reallocation not only hinges on whether initially more productive firms gain market share but also on how productivity changes as firms' market shares shift. For example, if affected firms became more selective in their hiring processes (as discussed in Butschek 2022), their productivity may increase due to reallocation. Conversely, if unaffected firms absorb workers from less productive firms, their productivity may decline if these workers have a relatively low productivity.

Secondly, as demonstrated by Kehrig and Vincent (2021), changes in the covariance term can be attributed to heterogeneous shifts in productivity across the firm size distribution. For instance, if productivity gains are concentrated in small firms, the covariance may decrease, and one may argue against considering such within-firm productivity changes as part of a reallocation effect.[34] In Appendix J, we apply the decomposition method proposed by Kehrig and Vincent (2021) to decompose the covariance term and adjust for heterogeneous changes in productivity across the size distribution. Notably, the minimum wage led to a decrease in the covariance between initial firm size and *changes* in productivity, which is consistent with small (presumably affected) firms experiencing the strongest gains in productivity. However, even after subtracting this effect from the reallocation component and incorporating it into the within-firm component, we still find that aggregate productivity only increased due to within-firm changes. This extended decomposition also reveals that changes in market shares among manufacturing firms are negatively associated with changes in productivity, implying that firms that grew (shrank) experienced a decrease (increase) in productivity, which relates to our first discussion point above.

What is more, it is crucial to view the decomposition as a quantification of different drivers. The absence of a contribution from reallocation to aggregate productivity growth may simply suggest that observed reallocation processes among firms have only a quantitatively negligible impact on

34. Decker et al. (2017) raise a similar point. However, a large literature that we follow interprets the covariance term as a measure of reallocation (e.g., Olley and Pakes 1996, Melitz and Polanec 2015, Autor et al. 2020). Alternative decompositions face other issues in clearly isolating the reallocation component, which is notoriously difficult. For instance, the reallocation component in the decompositions by Foster et al. (2001) or Kehrig and Vincent (2021) abstracts from changes in firm productivity that occur due to the reallocation process. Instead, these decompositions include an additional cross-term that combines changes in firm productivity with changes in markets shares, capturing aspects of both within-firm changes and reallocation.

aggregate productivity growth, even if our firm-level analysis (Appendix D) suggests that employment shares reallocated toward *ex-ante* more productive firms. Therefore, our results indicate that despite possible labor reallocation to *ex-ante* more productive firms, reallocation did not have a quantitatively relevant impact on aggregate productivity growth.

6.4 Additional Results

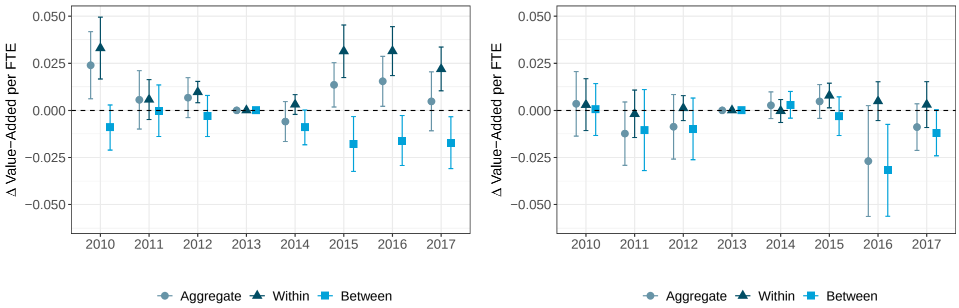
6.4.1 Long-Run Effects and Pre-Trends

Similar to our previous firm-level analysis, we now study a longer timespan (2010-2017) in our productivity decomposition analysis. Recap that for the firm-level and short-run aggregate analysis, we condition on a balanced panel of firms from 2012 to 2015 due to sample redraws. We now combine multiple representative waves of the firm level data over a longer timespan (i.e., KSE and SiD) and thus allow for firm sample entry and exit.[35] As before, we aggregate our data to the industry-region level. In Section 6.4.2, we will use the business registry data to analyse the contribution of entry and exit dynamics to productivity growth. The focus of this section is instead to study long-run effects and pre-trends using our main data over a longer time span. This allows us to also continue to use value added per FTE as productivity measure.

Figure 3 presents the minimum wage effect on our productivity decomposition terms for the extended time period. The figure reports coefficients and 90% CI from running the regression according to Equation (6) for industry-region cells over 2010-2017. The coefficients from the early years show no indication for long-run pre-trends. For manufacturing, we again find a positive effect of the minimum wage on aggregate productivity that is driven by a strong increase in the unweighted mean of firm-level productivity (the "within-firm" component). Again, worker reallocation in more exposed markets has not contributed to productivity growth. All effects prevail over the entire time period. For services, we again find no clear evidence for positive productivity effects. Overall, we conclude that our previous findings are not driven by pre-trends and that the productivity effects in manufacturing persist over several years.

35. Recap that our data features cut-off rules for sample entry: The manufacturing data contains only firms with at least 20 employees, the service sector data contains only firms with at least 250,000 EUR annual sales.

Figure 3: Static Olley-Pakes



(a) Manufacturing (b) Service Sector

Note: The graphs report regression results (coefficients and 90% CI) from using the components of the static Olley-Pakes decomposition (Equation (4)) in a difference-in-differences regression setting at the industry-region level. We regress the change in the respective component relative to the year 2013 on the GAP measure for each year separately. The labor productivity measure is the log of value added per FTE. The left figure shows results for manufacturing (N = 183), the right figure shows results for services (N = 366). The coefficients report changes relative to 2013.

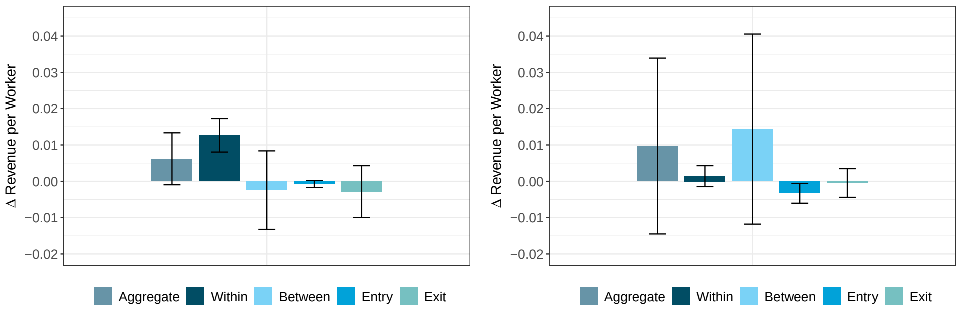
6.4.2 Entry and Exit

So far, we focused on a static productivity decomposition because our main sample does not feature entry and exit. Now, we extent this analysis using our business registry data from 2010 to 2017. This data now cover the population of firms and thus includes also small firms. Recap that we do not observe intermediate inputs in this data. Therefore, we use sales per headcounts as productivity measure. Following Melitz and Polanec (2015), we extent our previous decomposition in the following way:

$$\Delta\Omega_t = \Delta\overline{\omega}_t^C + \Delta Cov_t^C + s_t^N(\Omega_t^N - \Omega_t^C) + s_{t-1}^X(\Omega_{t-1}^C - \Omega_{t-1}^X), \tag{7}$$

where the superscripts C, N, and X indicate continuing, entering, and exiting firms, respectively. s_t^N and s_{t-1}^X are the employment shares of entering (in t) and exiting (in $t-1$) firms. Ω_t^C, Ω_t^N, and Ω_{t-1}^X denote aggregate productivity among the subset of continuing (in t), entering (in t), and exiting (in $t-1$) firms. The first term in Equation (7) is the unweighted average of firm productivity among continuing firms, the second term is the covariance between firms' market share (employment) and productivity in the subset of continuing firms, the third term is the productivity growth contribution of entering firms, and the last term captures the productivity growth contribution of exiting firms.

Figure 4: Dynamic Olley-Pakes, 2013-2015



(a) Manufacturing (b) Service Sector

Note: The graphs report regression results (coefficients and 90% CI) from using the components of the dynamic Olley-Pakes decomposition (Equation (7)) in an industry-region-level difference-in-differences regression setting based on the business registry data. The labor productivity measure is the log of sales per employees (heads). The left figure shows results for manufacturing (N = 313), the right figure shows results for services (N = 382). The coefficients report changes from 2013 to 2015.

Figure 4 shows results from using the components of Equation (7) as dependent variables in our industry-region-level regression analysis for the changes from 2013 to 2015. The key takeaways are: (i) more affected manufacturing sector labor markets experience an increase in the unweighted average firm productivity of continuing firms; (ii) reallocation between continuing firms did not contribute to productivity growth; and (iii) there is no notable productivity contribution of entry and exit dynamics.[36] We thus conclude that only within-firm productivity changes among continuing firms contributed to increasing aggregated productivity in manufacturing labor markets that were more affected by the minimum wage.

7 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 worker reallocation from low-wage to high-wage firms, the contribution of our study is to analyze the short- and longer-run productivity effects of the minimum wage for the German economy both at the firm level and the aggregate market level. We take into account entry and exit of firms and explore to what extent revenue-productivity effects of

36. Appendix Figure A1 shows that studying longer-run changes yields similar results.

the minimum wage capture factor substitution, output price changes, and changes in firms' quantity-productivity. To this end, we combine several high-quality administrative datasets on manufacturing and service sector firms, containing detailed information on productivity, wages, employment, investment, intermediate input expenditures, and prices.

Our firm-level analysis documents substantial gains in labor 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 show that these significant productivity effects are not merely short-term but persist over time. We also find increases in revenue-TFP for manufacturing firms. Two thirds of these revenue-TFP gains result from increasing efficiency, while one third is caused by increasing output prices.

At the market level, we find aggregate labor productivity gains in the manufacturing sector for labor markets that were more affected by the minimum wage. We decompose changes in aggregate productivity into within-firm productivity improvements and a component measuring the contribution of worker reallocation to aggregate productivity growth. While a welfare analysis exceeds the scope of our study, it is important to emphasize that differentiating between these two channels is crucial for understanding the welfare implications of aggregate productivity gains, particularly as reallocation processes are associated with significant costs. Although our firm-level regressions and previous studies point to worker reallocation to *ex-ante* more productive firms, we find that aggregate productivity growth in more affected manufacturing labor markets is exclusively driven by the strong within-firm productivity growth that we document in the first part of our study. Aggregate productivity growth did neither result from firm entry and exit nor from reallocation of labor toward more efficient producers. Importantly, also these aggregate productivity gains from the minimum wage persist over time. We do not document any aggregate productivity gains in the service sector, nor do we find significant changes in the within- and between-firm components of aggregate service sector productivity.

Our study provides novel insights into the productivity effects of minimum wages and raises important questions on the source of the significant within-firm productivity gains. We document that outsourcing plays a potentially important role in the German context. Rising hiring standards of affected firms as described in Butschek (2022) may offer a further mechanism contributing to the positive productivity effects that we document and may also rationalize the persistence of the effect. However, the size of the effects on hiring standards reported by Butschek (2022) is arguably too small to explain a significant portion of our results. As we discussed, improved management practices (Hau et al. 2020, Mayneris et al. 2018) may contribute to the within-firm TFP improvements whereas increased worker effort found in the US (Coviello et al. 2022, Ku 2022) seems to play only a limited role in Germany (Bossler and Broszeit 2017). Another important open question is whether the minimum wage introduction induced in particular low-productivity workers to separate from affected firms. This could not only explain the rise in productivity among affected firms, but if those workers were absorbed by high-productivity firms, this could also rationalize the relatively small productivity effects at the market level and the absence of any market-level productivity contribution from worker reallocation.

References

Aghion, Philippe, Ufuk Akcigit, and Peter Howitt. 2014. "What Do We Learn From Schumpeterian Growth Theory?" In *Handbook of Economic Growth,* edited by Philippe Aghion and Steven N. Durlauf, 2:515–563. Elsevier.

Ashenfelter, Orley, and Štěpán Jurajda. 2022. "Minimum Wages, Wages, and Price Pass-Through: The Case of McDonald's Restaurants." *Journal of Labor Economics* 40 (S1): 179–201.

Autor, David, David Dorn, Lawrence F Katz, Christina Patterson, and John Van Reenen. 2020. "The fall of the labor share and the rise of superstar firms." *The Quarterly Journal of Economics* 135 (2): 645–709.

Backus, Matthew. 2020. "Why is productivity correlated with competition?" *Econometrica* 88 (6): 2415–2444.

Bartelsman, Eric, John Haltiwanger, and Stefano Scarpetta. 2013. "Cross-Country Differences in Productivity: The Role of Allocation and Selection." *American Economic Review* 103 (1): 305–34.

Bender, Stefan, Nicholas Bloom, David Card, John Van Reenen, and Stefanie Wolter. 2018. "Management practices, workforce selection, and productivity." *Journal of Labor Economics* 36 (S1): 371–409.

Berg, Tobias, Markus Reisinger, and Daniel Streitz. 2021. "Spillover Effects in Empirical Corporate Finance." *Journal of Financial Economics* 142 (3): 1109–1127.

Biewen, Martin, Bernd Fitzenberger, and Marian Rümmele. 2022. *Using Distribution Regression Difference-in-Differences to Evaluate the Effects of a Minimum Wage Introduction on the Distribution of Hourly Wages and Hours Worked.* IZA Discussion Paper, Discussion Paper Series 15534.

Bighelli, Tommaso, Filippo Di Mauro, Marc J Melitz, and Matthias Mertens. 2023. "European firm concentration and aggregate productivity." *Journal of the European Economic Association* 21 (2): 455–483.

Bossler, Mario, and Sandra Broszeit. 2017. "Do Minimum Wages Increase Job Satisfaction? Micro-data Evidence from the new German Minimum Wage." *Labour* 31 (4): 480–493.

Bossler, Mario, and Hans-Dieter Gerner. 2020. "Employment Effects of the New German Minimum Wage: Evidence from Establishment-Level Microdata." *ILR Review* 73 (5): 1070–1094.

Bossler, Mario, Nicole Gürtzgen, Benjamin Lochner, Ute Betzl, and Lisa Feist. 2020a. "The German Minimum Wage: Effects on Productivity, Profitability, and Investments." *Jahrbücher für Nationalökonomie und Statistik* 240 (2-3): 321–350.

Bossler, Mario, Nicole Gürtzgen, Benjamin Lochner, Ute Betzl, Lisa Feist, and Jakob Wegmann. 2018. *Auswirkungen des gesetzlichen Mindestlohns auf Betriebe und Unternehmen.* IAB-Forschungsbericht 4/2018. Nürnberg.

Bossler, Mario, Ursula Jaenichen, and Simeon Schächtele. 2020b. "How Effective are Enforcement Measures for Compliance with the Minimum Wage? Evidence from Germany." *Economic and Industrial Democracy* 43 (2): 943–971.

Bossler, Mario, and Thorsten Schank. 2023. "Wage Inequality in Germany after the Minimum Wage Introduction." *Journal of Labor Economics* 41 (3): 813–857.

Burauel, Patrick, Marco Caliendo, Alexandra Fedorets, Markus Grabka, Carsten Schröder, Jürgen Schupp, and Linda Wittbrodt. 2017. "Mindestlohn noch längst nicht für alle – Zur Entlohnung anspruchsberechtigter Erwerbstätiger vor und nach der Mindestlohnreform aus der Perspektive Beschäftigter." *DIW Wochenbericht* 84 (49): 1109–1123.

Burauel, Patrick, Marco Caliendo, Markus M. Grabka, Cosima Obst, Malte Preuss, and Carsten Schröder. 2020. *Jahrbücher für Nationalökonomie und Statistik* 240 (2-3): 233–267.

Butschek, Sebastian. 2022. "Raising the bar: minimum wages and employers' hiring standards." *American Economic Journal: Economic Policy* 14 (2): 91–124.

Caliendo, Marco, Alexandra Fedorets, Malte Preuss, Carsten Schröder, and Linda Wittbrodt. 2018. "The Short-Run Employment Effects of the German Minimum Wage Reform." *Labour Economics* 53:46–62.

———. 2023. "The short-and medium-term distributional effects of the German minimum wage reform." *Empirical Economics* 64 (3): 1149–1175.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *The Quarterly Journal of Economics* 134 (3): 1405–1454.

Clemens, Jeffrey. 2021. "How Do Firms Respond to Minimum Wage Increases? Understanding the Relevance of Non-Employment Margins." *The Journal of Economic Perspectives* 35 (1): 51–72.

Coviello, Decio, Erika Deserranno, and Nicola Persico. 2022. “Minimum wage and individual worker productivity: Evidence from a large US retailer.” *Journal of Political Economy* 130 (9): 2315–2360.

De Loecker, Jan, Pinelopi K. Goldberg, Amit K. Khandelwal, and Nina Pavcnik. 2016. “Prices, Markups, and Trade Reform.” *Econometrica* 84 (2): 445–510.

Decker, Ryan A, John Haltiwanger, Ron S Jarmin, and Javier Miranda. 2017. “Declining Dynamism, Allocative Efficiency, and the Productivity Slowdown.” *American Economic Review Papers & Proceedings* 107 (5): 322–26.

Draca, Mirko, Stephen Machin, and John Van Reenen. 2011. “Minimum Wages and Firm Profitability.” *American Economic Journal: Applied Economics* 3 (1): 129–151.

Dustmann, Christian, Attila Lindner, Uta Schönberg, Matthias Umkehrer, and Philipp vom Berge. 2022. “Reallocation Effects of the Minimum Wage.” *The Quarterly Journal of Economics* 137 (1): 267–328.

Eslava, Marcela, John Haltiwanger, Adriana Kugler, and Maurice Kugler. 2004. “The Effects of Structural Reforms on Productivity and Profitability Enhancing Reallocation: Evidence from Colombia.” *Journal of Development Economics* 75 (2): 333–371.

Fackler, Daniel, Steffen Mueller, and Jens Stegmaier. 2021. “Explaining Wage Losses After Job Displacement: Employer Size and Lost Firm Wage Premiums.” *Journal of the European Economic Association* 19 (5): 2695–2736.

Foster, Lucia, John C Haltiwanger, and Cornell John Krizan. 2001. “Aggregate Productivity Growth: Lessons from Microeconomic Evidence.” In *New developments in productivity analysis,* 303–372. University of Chicago Press.

Grossman, Gene M., and Elhanan Helpman. 1991. “Trade, Knowledge Spillovers, and Growth.” *European Economic Review* 35 (2-3): 517–526.

Hafner, Lucas, and Benjamin Lochner. 2021. “Do Minimum Wages Improve Self-Rated Health? Evidence from a Natural Experiment.” *Empirical Economics* 62 (6): 2989–3014.

Harasztosi, Peter, and Attila Lindner. 2019. “Who Pays for the Minimum Wage?” *American Economic Review* 109 (8): 2693–2727.

Hau, Harald, Yi Huang, and Gewei Wang. 2020. “Firm Response to Competitive Shocks: Evidence from China’s Minimum Wage Policy.” *The Review of Economic Studies* 87 (6): 2639–2671.

Hirsch, Barry T., Bruce E. Kaufman, and Tetyana Zelenska. 2015. "Minimum Wage Channels of Adjustment." *Industrial Relations: A Journal of Economy and Society* 54 (2): 199–239.

Hsieh, Chang-Tai, and Peter J. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics* 124 (4): 1403–1448.

Ippolito, Richard A. 2003. "The impact of the minimum wage if workers can adjust effort." *The Journal of Law and Economics* 46 (1): 207–227.

Kehrig, Matthias, and Nicolas Vincent. 2021. "The Micro-Level Anatomy of the Labor Share Decline." *The Quarterly Journal of Economics* 136 (2): 1031–1087.

Ku, Hyejin. 2022. "Does minimum wage increase labor productivity? Evidence from piece rate workers." *Journal of Labor Economics* 40 (2): 325–359.

Lachowska, Marta, Alexandre Mas, and Stephen A Woodbury. 2020. "Sources of displaced workers' long-term earnings losses." *American Economic Review* 110 (10): 3231–3266.

Lemos, Sara. 2008. "A Survey of the Effects of the Minimum Wage on Prices." *Journal of Economic Surveys* 22 (1): 187–212.

Levinsohn, James, and Amil Petrin. 2003. "Estimating Production Functions Using Inputs to Control for Unobservables." *The Review of Economic Studies* 70 (2): 317–341.

Machin, Stephen, Alan Manning, and Lupin Rahman. 2003. "Where the Minimum Wage Bites Hard: Introduction of Minimum Wages to a Low Wage Sector." *Journal of the European Economic Association* 1 (1): 154–180.

Mayneris, Florian, Sandra Poncet, and Tao Zhang. 2018. "Improving or Disappearing: Firm-level Adjustments to Minimum Wages in China." *Journal of Development Economics* 135:20–42.

Melitz, Marc J. 2003. "The Impact of Trade on Intra-Industry Reallocations and Aggregate Industry Productivity." *Econometrica* 71 (6): 1695–1725.

Melitz, Marc J., and Sašo Polanec. 2015. "Dynamic Olley-Pakes productivity decomposition with entry and exit." *The RAND Journal of Economics* 46 (2): 362–375.

Mertens, Matthias. 2022. "Micro-mechanisms behind Declining Labor Shares: Rising Market Power and Changing Modes of Production." *International Journal of Industrial Organization* 81:102808.

Mindestlohnkommission. 2020. "Dritter Bericht zu den Auswirkungendes gesetzlichen Mindestlohns." https://www.mindestlohn-kommission.de/DE/Bericht/pdf/Bericht2020.pdf?__blob=publicationFile&v=2.

Mueller, Steffen. 2015. "Works councils and labour productivity: Looking beyond the mean." *British Journal of Industrial Relations* 53 (2): 308–325.

Nguyen, Dong Xuan. 2019. "Minimum Wages and Firm Productivity: Evidence from Vietnamese Manufacturing Firms." *International Economic Journal* 33 (3): 560–572.

Olley, G. Steven, and Ariel Pakes. 1996. "The Dynamics of Productivity in the Telecommunications Equipment Industry." *Econometrica* 64 (6): 1263–1297.

Petrin, Amil, and Jagadeesh Sivadasan. 2013. "Estimating Lost Output from Allocative Inefficiency, with an Application to Chile and Firing Costs." *Review of Economics and Statistics* 95 (1): 286–301.

Renkin, Tobias, Claire Montialoux, and Michael Siegenthaler. 2022. "The Pass-through of Minimum Wages into US Retail Prices: Evidence from Supermarket Scanner Data." *Review of Economics and Statistics* 104 (5): 890–908.

Riley, Rebecca, and Chiara Rosazza Bondibene. 2017. "Raising the Standard: Minimum Wages and Firm Productivity." *Labour Economics* 44:27–50.

Schumpeter, Joseph A. 1942. *Capitalism, Socialism, and Democracy.* New York: Harper & Brothers.

Sorkin, Isaac. 2018. "Ranking firms using revealed preference." *The quarterly journal of economics* 133 (3): 1331–1393.

Syverson, Chad. 2011. "What Determines Productivity?" *Journal of Economic literature* 49 (2): 326–65.

Yi, Moises, Steffen Müller, and Jens Stegmaier. 2024. "Industry mix, local labor markets, and the incidence of trade shocks." *Journal of Labor Economics* forthcoming.

Appendix - for Online Publication

A Additional Results

Table A1: Employment and Wage Effects

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2012 to 2013
Panel (a): 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)
Panel (b): 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)
Panel (c): 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 from 2012 to 2013 (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.

Table A2: Employment in Headcounts and the Share of Full-Time Employment

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2012 to 2013
Panel (a): Δ Log Employment (Headcount)						
med	-0.002	0.003	-0.002	-0.009	0.008	0.009
	(0.003)	(0.002)	(0.002)	(0.005)	(0.004)	(0.004)
low	-0.027	-0.002	-0.011	-0.022	0.010	0.004
	(0.006)	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)
Constant	0.022	0.011	0.011	0.039	0.012	0.025
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Panel (b): Δ Share Full-Time Employment (in %)						
med	-0.011	0.100	-0.056	-0.721	-0.241	0.308
	(0.140)	(0.121)	(0.135)	(0.238)	(0.208)	(0.212)
low	-0.931	-0.435	0.175	-1.207	-0.136	0.598
	(0.349)	(0.298)	(0.338)	(0.333)	(0.287)	(0.300)
Constant	-0.251	-0.121	-0.014	0.110	-0.170	-0.619
	(0.080)	(0.071)	(0.070)	(0.167)	(0.146)	(0.150)
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 employment (headcount) and share of full-time employees (in percent) from 2013 to 2015 (Cols. 1 and 4), 2013 to 2014 (Cols. 2 and 5) and 2012 to 2013 (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.

Table A3: Input Intensities

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2012 to 2013
Panel (a): Δ Log Intermediate Inputs per FTE						
med	0.019	0.008	0.003	0.032	0.008	-0.012
	(0.005)	(0.004)	(0.004)	(0.010)	(0.004)	(0.009)
low	0.057	0.017	-0.005	0.058	0.017	-0.031
	(0.009)	(0.008)	(0.008)	(0.013)	(0.008)	(0.012)
Constant	0.009	0.003	-0.007	0.011	0.003	-0.046
	(0.003)	(0.003)	(0.003)	(0.007)	(0.003)	(0.007)
Panel (b): 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 2012 to 2013 (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.

Table A4: Total Intermediate Inputs and Investment

	Manufacturing			Service Sector		
	(1) 2013 to 2015	(2) 2013 to 2014	(3) 2012 to 2013	(4) 2013 to 2015	(5) 2013 to 2014	(6) 2012 to 2013
Panel (a): Δ Log Intermediate Inputs						
med	0.017	0.011	0.002	0.015	0.002	-0.001
	(0.005)	(0.004)	(0.004)	(0.009)	(0.008)	(0.009)
low	0.020	0.012	-0.013	0.024	-0.002	-0.021
	(0.010)	(0.007)	(0.008)	(0.012)	(0.011)	(0.011)
Constant	0.032	0.015	0.004	0.059	0.005	-0.019
	(0.004)	(0.003)	(0.003)	(0.007)	(0.006)	(0.006)
Panel (b): Δ Investments (1000)						
med	-337.260	-162.233	157.156	-348.760	-292.996	198.986
	(169.574)	(152.174)	(140.545)	(208.492)	(184.377)	(165.971)
low	-418.683	-279.604	257.452	-434.897	-327.158	170.825
	(195.081)	(180.839)	(176.508)	(171.203)	(226.305)	(163.678)
Constant	438.140	224.858	-209.832	486.109	277.776	-158.423
	(163.508)	(145.252)	(132.190)	(211.799)	(165.007)	(155.376)
N	9471	9471	9471	29810	29810	29810
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes

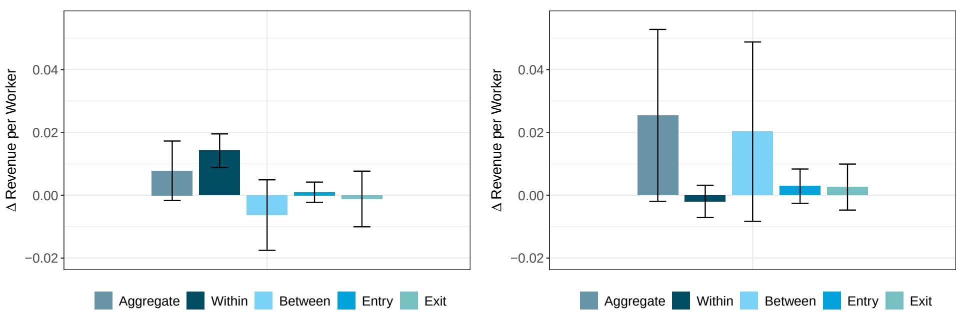
Note: Results from regressing the change in log total intermediate inputs and total investments from 2013 to 2015 (Cols. 1 and 4) and 2013 to 2014 (Cols. 2 and 5), and 2012 to 2013 (Cols. 3 and 6) on the treatment indicator. *low* denotes low-wage and thus highly exposed firms while moderately exposed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

Table A5: Aggregate Employment and Wage Effects

	2013 to 2015	2012 to 2013	2013 to 2015	2012 to 2013
	(1)	(2)	(3)	(4)
	Δ Aggregate employment (FTE)	Δ Aggregate employment (FTE)	Δ Aggregate wage	Δ Aggregate wage
Panel (a): Manufacturing				
GAP	0.004	0.005	0.008	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Constant	0.021	0.005	0.044	0.016
	(0.004)	(0.002)	(0.002)	(0.003)
N	167	167	167	167
Mean Y	0.021	0.010	0.043	0.011
Mean GAP	0.427	0.427	0.427	0.427
R-sq	0.003	0.025	0.049	0.004
Panel (b): Service Sector				
GAP	-0.008	-0.001	0.006	-0.000
	(0.008)	(0.002)	(0.006)	(0.002)
Constant	0.045	0.029	0.023	0.002
	(0.016)	(0.003)	(0.013)	(0.004)
N	324	324	324	324
Mean Y	0.042	0.029	0.035	0.010
Mean GAP	1.252	1.252	1.252	1.252
R-sq	0.005	0.001	0.005	0.000

Note: Results from regressing (i) the change in the aggregate employment from 2013 to 2015 (Col. 1) and 2012 to 2013 (Col. 2) on minimum wage exposure, and (ii) the change in the aggregate wage (i.e., employment-weighted average firm wage) from 2013 to 2015 (Col. 3) and 2012 to 2013 (Col. 4) on minimum wage exposure. Regressions are weighted by industry×region-level employment in 2013. Robust standard errors in parentheses.

Figure A1: Dynamic Olley-Pakes, 2013-2017



(a) Manufacturing (b) Service Sector

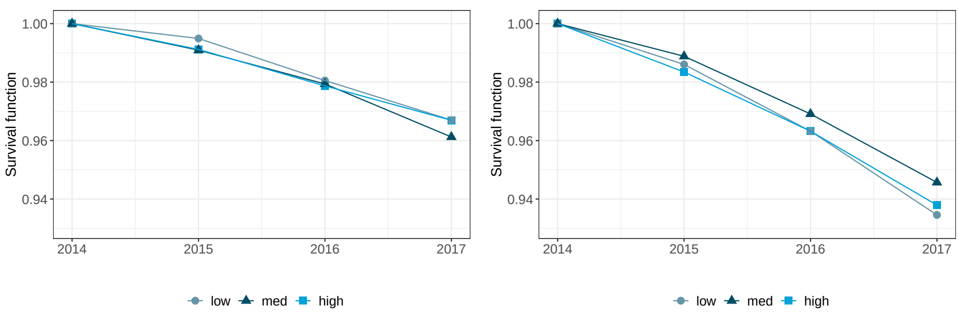
Note: The graphs report regression results (coefficients and 90% CI) from using the components of the dynamic Olley-Pakes decomposition (Equation (7)) in an industry-region-level difference-in-differences regression setting based on the business registry data. The labor productivity measure is the log of sales per employees (heads). The left figure shows results for manufacturing (N = 313), the right figure shows results for services (N = 382). The coefficients report changes from 2013 to 2017.

B Survivor Bias

In our firm-level analysis, as well as in the first part of the aggregate analysis, we restrict the sample to firms that survive the introduction of the minimum wage. One major concern with restricting the sample is that these firms might be precisely those firms that can raise productivity, leading to a selection bias in our estimates.

We address this concern by analyzing how firms' exit probability is related to firms' treatment status. We combine the treatment information from our main firm-level data[37] with the business registry, which allows us to track firms until 2017. Figure B1 shows the estimated unconditional Kaplan-Meier survival functions. Importantly, survival rates of low and high-wage firms do not differ in the manufacturing sector and only marginally differ in the service sector after the minimum wage became effective.

Figure B1: Kaplan-Meier Survival Functions, 2014-2017



(a) Manufacturing

(b) Service Sector

Note: The graphs show the estimated unconditional Kaplan-Meier survival functions, using 2014 as base year. We combine the treatment indicator from the firm-level dataset with the business registry. The left figure shows results for manufacturing (N = 10006), the right figure shows results for services (N = 32030).

Next, we estimate the following linear probability model:

$$exit_{it} = \alpha + \boldsymbol{T_i}\boldsymbol{\beta} + \phi_r + \psi_j + \rho_s + \epsilon_i, \tag{B1}$$

where $exit_i$ is a binary indicator that is 1 if the firm drops out of the sample in year t, $\boldsymbol{T_i}$ 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

37. As we construct our treatment indicator based on the firms' wage bill per FTE averaged over the years 2012 to 2014, we condition on firms that we observe throughout from 2012 to 2014 in the main data.

size classes, ρ_s, (centered) to account for differences in the exit probability by firm size. In the first part of the analyis, we condition on firms that are included throughout in the sample from 2012 to 2014 and study market exit using business registry information. In the second part of the analysis, we study whether low-wage firms already had a higher exit probability in the years preceding the minimum wage. Therefore, we use information on wage bill per FTE in year $t_0 = \{2010, 2011, 2012, 2013, 2014\}$ from the KSE and SiD data and track each cohort using the business registry until $t_0 + 3$.

Table B1 shows the results from estimating Equation (B1) for the manufacturing and service sector, respectively. In Column 1, we use the same sample as for the Kaplan-Meier survival curves in Figure B1. Column 1 shows that low-wage manufacturing firms do not have a higher probability to exit until 2017, while low-wage service sector firms have a 1 percentage point higher probability to exit until 2017. In Columns 2 to 6, we analyze whether low-wage firms had a higher probability to exit even before the introduction of the minimum wage. Low-wage service sector firms already had a higher exit probability of 0.9 to 1.1 percentage points in the years preceding the minimum wage introduction (Columns 2 and 3), and the exit probability did not increase in the years after minimum wage came into effect (Columns 4-6). We therefore conclude that our estimates do not suffer from sample selection biases.

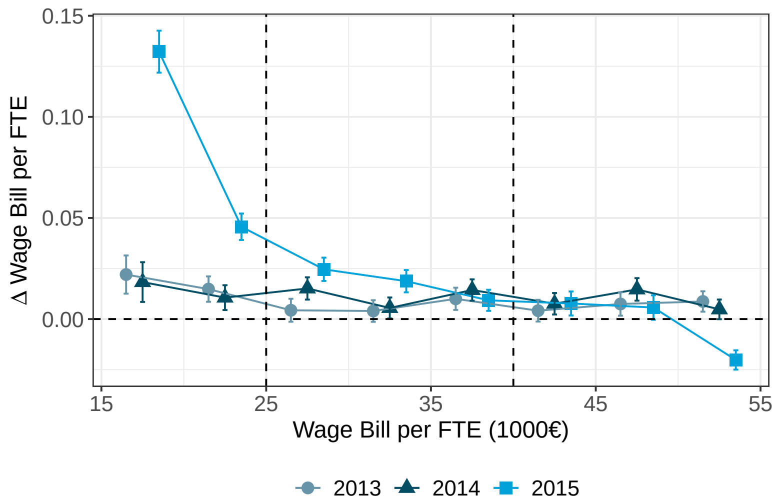
Table B1: Exit Probability and Treatment Status, Exit until $t_0 + 3$

	Main Sample	Long-Run Sample				
	(1)	(2)	(3)	(4)	(5)	(6)
Manufacturing						
med	0.000	-0.003	0.002	0.001	-0.005	-0.002
	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
low	-0.006	0.001	0.014	-0.009	-0.006	-0.004
	(0.008)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)
Constant	0.036	0.016	0.025	0.034	0.040	0.039
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
N	10006	10756	10982	11108	11178	10857
Service						
med	-0.002	-0.005	0.001	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
low	0.010	0.009	0.011	0.007	0.002	0.009
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.058	0.072	0.065	0.067	0.073	0.075
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	32030	57209	63160	63997	65032	77303
t_0	2014	2010	2011	2012	2013	2014
Region FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Size FE	yes	yes	yes	yes	yes	yes

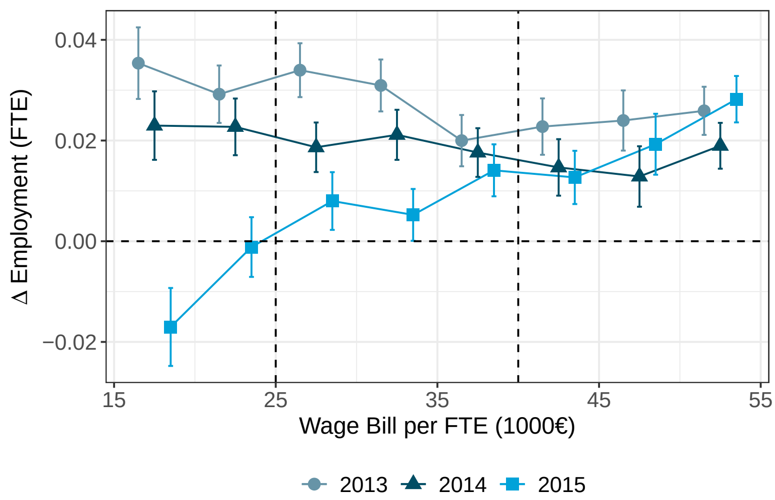
Note: Results from regressing a dummy for exit (0/1) until t + 3 on the treatment indicator. We combine the firm-level dataset with the business registry. In Column 1, exit is defined as market exit from $t_0 = 2014$ to $t_0 + 3 = 2017$. In Columns 2-6, we vary t_0 between 2010 and 2014, estimate treatment status in year t_0 from the main data, and track firms using the business registry until $t_0 + 3$. All Columns control for region and industry fixed effects and for 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.

C Additional Descriptive Statistics

Figure C1: Wages and Employment after the Introduction of the Minimum Wage



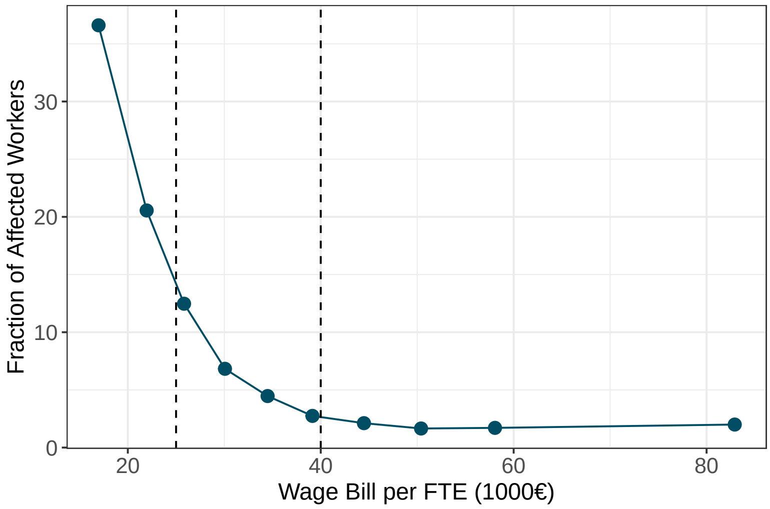
(a) Yearly Growth in Wage Bill per FTE by Ex-Ante Wage Bill per FTE (€1000)



(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 pre-treatment years 2012-2014) separately for the periods 2012 to 2013, 2013 to 2014, and 2014 to 2015. Manufacturing and service sector firms are pooled. We also report the respective 90% CI. N = 39281.

Figure C2: Fraction Affected against Average Wages

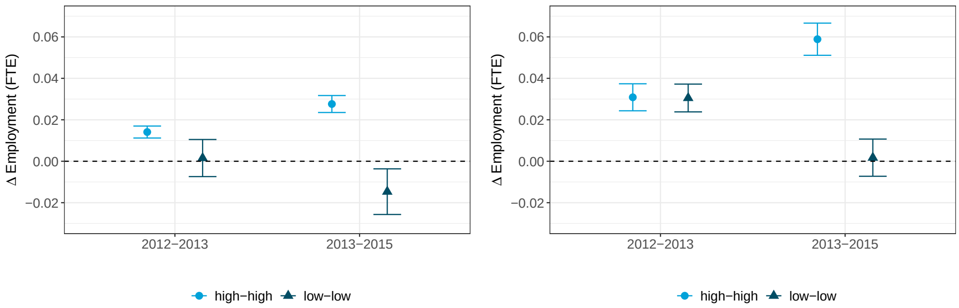


Note: The y-axis shows the fraction of affected workers at the firm level. The x-axis shows the pre-treatment average of annual wage costs per FTE divided into 10 equal-sized bins. The dashed lines depict the thresholds for the three treatment groups: $[min;$ €25,000$)$; $[$€25,000; €40,000$)$; $[$€40,000; $max]$. N = 5590.

D Employment Effects by Initial Firm Productivity

In the following, we assess if the firm-level employment effects of the minimum wage differ by firms' productivity. To do so, we expand our main regression Equation (1) with an interaction term of treatment status and *initial* firm labor productivity. High-productivity (low-productivity) firms have an ex-ante labor productivity level above (below) the industry $\times$ region specific median. Figure D1 displays the average predicted employment growth by treatment status and initial firm productivity for the years 2012 to 2013 and 2013 to 2015 for the manufacturing and service sector, respectively. We find a decline in employment growth for low-wage, low-productivity firms after the introduction of the minimum wage (2013-2015) relative to the pre-period (2012-2013), while high-wage, high-productivity firms continue to grow at the same pace. In manufacturing, we even find a decline in employment among low-productivity-low-wage firms from 2013 to 2015. In addition, we find no statistically significant difference in employment growth between low-wage, low-productivity firms and high-wage, high-productivity firms in the service sector and only small differences in employment growth of low-wage, low-productivity firms and high-wage, high-productivity firms in the manufacturing sector prior to the minimum wage introduction. Overall, our findings provide suggestive evidence for a reallocation of employment shares from low-wage, low-productivity firms to high-wage, high-productivity firms in response to the minimum wage introduction.

Figure D1: Employment Growth by Initial Firm Productivity



(a) Manufacturing (b) Service Sector

Note: The graphs report the predicted change in employment (FTE) from 2012 to 2013 and 2013 to 2015 by treatment and initial firm productivity (coefficients and 90% CI) for the manufacturing (N=9471) and service sector (N=29810), respectively. *high-high* indicates high-wage firms (average annual wage above €40,000) with an initial productivity level above the industry $\times$ region specific median. *low-low* denotes low-wage firms (average annual wage below €25,000) with an initial productivity level below the industry $\times$ region specific median.

E 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 (5) 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:

$$\Delta y_{it} = \alpha + (GAP_{jr} \times \boldsymbol{T_i})\boldsymbol{\beta} + \phi_r + \psi_j + \epsilon_{it}. \tag{E1}$$

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.[38] GAP_{jr} depicts the gap measure derived from the worker-level data (see Equation (5) of the main text) 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 E1 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.

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

Table E1: Spillover Effects for Employment

	2013 to 2015		2013 to 2012	
	(1) Baseline	(2) Industry-Region Spillover	(3) Baseline	(4) 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 $\times$ GAP		0.024		-0.008
		(0.016)		(0.017)
Low $\times$ 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 $\times$ GAP		0.007		-0.002
		(0.008)		(0.005)
Low $\times$ 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 $\times$ state level (Cols. 2 and 4). Cols. 1 and 3 report our baseline specification from Table A1 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.

F 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 does not yield any productivity-enhancing reallocation effects.

Table F1 replicates our analysis using 144 three-digit industries as aggregation levels, which limits worker mobility across industries but allows for full cross-regional worker mobility.[39] 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.

Table F2 replicates our analysis aggregating the data to 223 official labor market regions.[40] Compared to our baseline specification, this restricts worker mobility across local labor market regions but allows for full worker mobility across industries. Again, we do not find evidence of productivity-enhancing reallocation processes induced by the introduction of the minimum wage. Instead, we again document only within-firm productivity improvements.

39. We again exclude cells with less than 10 observations in the 2013 base year.

40. 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 F1: Olley and Pakes (1996) Decomposition: Log Labor Productivity (three-digit Industry)

	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) $\Delta Cov(s_{it}, \omega_{it})$
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 on the treatment indicator. Regressions are weighted by industry-level employment in 2013. Robust standard errors in parentheses.

Table F2: 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	0.004	0.015	-0.011	0.004	-0.006	0.011
	(0.010)	(0.006)	(0.008)	(0.007)	(0.004)	(0.008)
Constant	0.006	-0.029	0.035	-0.007	0.000	-0.007
	(0.009)	(0.005)	(0.009)	(0.007)	(0.003)	(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	0.569	0.569	0.569	0.569	0.569	0.569
R-sq	0.001	0.031	0.006	0.002	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 on the treatment indicator. Regressions are weighted by labor market region-level employment in 2013. Robust standard errors in parentheses.

G Price Index

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

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

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.[41]

41. 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.

H 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. (2023):

$$K_{it} = K_{it-1}(1 - \alpha_{jt-1}) + I_{it-1}, \tag{H1}$$

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:

$$LT = \alpha \int_0^\infty (1 - \alpha)^t t dt. \tag{H2}$$

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 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 firm's 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.[42] Given that we estimate very reasonable output elasticities (see Appendix I), we are confident that our capital variables reliably reflect firms' true capital stocks.[43]

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

43. 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.

I 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):

$$q_{it} = \boldsymbol{\phi}'_{it}\boldsymbol{\beta} + \omega_{it} + \epsilon_{it}. \tag{I1}$$

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.[44] We define labor as full-time equivalents, capital as deflated capital stocks (see Appendix H 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 (I1) 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 Equation (I1). 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

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

the following, we describe how we solve all these identification issues and recover a 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 G. 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 (I1):

$$B(.)_{it} = B((p_{it}, ms_{it}, A_{it}, I_i) \times \boldsymbol{\phi}^c_{it}). \tag{I2}$$

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:

$$\omega_{it} = g(.)_{it} = g(e_{it}, k_{it}, l_{it}, \boldsymbol{\Gamma}_{it}). \tag{I3}$$

$\boldsymbol{\Gamma}_{it}$ captures additional state variables that affect firms' demand for e_{it}. $\boldsymbol{\Gamma}_{it}$ 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 $\boldsymbol{\Gamma}_{it}$. 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: $\omega_{it} = h(\omega_{it-1}, \mathbf{Z}_{it-1}) + \zeta_{it} = h_{it-1}(.) + \zeta_{it}$, where ζ_{it} denotes the innovation in productivity and $\mathbf{Z}_{it} = (EX_{it}, NumP_{it}, RD_{it})$ 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.[45]

Inserting Equations (I2), (I3), and the law of motion for productivity into the production function finally yields:

$$q_{it} = \boldsymbol{\phi}_{it}'\boldsymbol{\beta} + B(.)_{it} + h(.)_{it-1} + \zeta_{it} + \epsilon_{it}, \tag{I4}$$

which forms the basis for our estimation.[46]

Identification. We estimate Equation (I4) 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

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

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

realizations of ζ_{it}.[47] Similarly, we rely on lagged values for market shares and output price indices as instruments for their present values because we consider these to be flexible variables as well. We define identifying moments jointly for ζ_{it} and ϵ_{it}:

$$E_{it} = ((\zeta_{it} + \epsilon_{it})\mathbf{Y}_{it}), \tag{I5}$$

where $\mathbf{Y}_{it}$ 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:

$$\mathbf{Y}_{it} = (\boldsymbol{J}(.)_{it}, \boldsymbol{V}(.)_{it-1}, \boldsymbol{\Xi}(.)_{it-1}, \boldsymbol{\Psi}(.)_{it-1}, \boldsymbol{\Lambda}(.)_{it-1}), \tag{I6}$$

where we defined:

$$\begin{aligned}
&\boldsymbol{J}(.)_{it} = (l_{it}, k_{it}, l_{it}^2, k_{it}^2, lk_{it}, A_{it}, I_i),\\
&\boldsymbol{V}(.)_{it} = (m_{it}, m_{it}^2, l_{it}m_{it}, m_{it}k_{it}, l_{it}m_{it}k_{it}, ms_{it}, p_{it}),\\
&\boldsymbol{\Xi}(.)_{it} = (m_{it}, l_{it}, k_{it}, m_{it}^2, l_{it}^2, k_{it}^2, l_{it}m_{it}, m_{it}k_{it}, l_{it}k_{it}, l_{it}m_{it}k_{it}) \times p_{it}),\\
&\boldsymbol{\Psi}(.)_{it} = (e_{it}, l_{it}, k_{it}, e_{it}^2, l_{it}^2, k_{it}^2, e_{it}^3, l_{it}^3, k_{it}^3, l_{it}e_{it}, e_{it}k_{it}, l_{it}k_{it}, l_{it}e_{it}k_{it}, k_{it}^2e_{it}, k_{it}^2l_{it}, l_{it}^2e_{it}, l_{it}^2k_{it}, e_{it}^2k_{it}, e_{it}^2l_{it}),\\
&\boldsymbol{\Lambda}(.)_{it} = (EX_{it}, NumP_{it}, w_{it}),
\end{aligned}$$

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

47. 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).

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.[48]

Results and calculating TFP. Table I1 shows averages and standard deviations of the estimated output elasticities by two-digit industries. Recap that output elasticities are firm- and time-specific due to the translog production function. Overall, the results look meaningful and are comparable to other studies (e.g., De Loecker et al. 2016). 10% of our firm-year observations display a negative output elasticity with respect to at least one production factor. We drop these firms from our analysis, as these are not consistent with the underlying production model.

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

$$TFPQ_{it} = q_{it} - \boldsymbol{\phi}'_{it}\boldsymbol{\beta} - B(.)_{it}, \tag{I7}$$

$$TFPR_{it} = TFPQ_{it} + p_{it}. \tag{I8}$$

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 Appendix K, 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.[49] In such a setting, the first order conditions for intermediates and labor are given by:

$$MRPL_{it} = w_{it}(1 + \frac{1}{\epsilon^L_{it}}), \tag{I9}$$

$$MRPM_{it} = P^M_{it}, \tag{I10}$$

48. 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.

49. One can also additionally allow for rent-sharing in such a setting (Mertens 2022).

where $MRPM_{it}$ denotes the marginal revenue product of intermediates. P_{it}^{M} is the unit cost for intermediates. ϵ_{it}^{L} 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:

$$MRPL_{it} = (\theta_{it}^{L}/\theta_{it}^{M}) * (P_{it}^{M}M_{it}/L_{it}), \tag{I11}$$

where θ_{it}^{M} and θ_{it}^{L} are the output elasticities of intermediate and labor inputs. We use Equation (I11) to calculate marginal revenue products of labor from estimated output elasticites and observed input expenditures.

Table I1: 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 (I1) 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.

J Alternative productivity decomposition

The decomposition by Olley and Pakes (1996) that we use in the main text decomposes aggregate productivity into the unweighted mean of productivity and the covariance between firms' size and productivity:

$$\Omega_t = \overline{\omega}_{it} + Cov(s_{it}, \omega_{it}). \tag{J1}$$

Following the literature, we interpret changes in the unweighted mean as the "within-firm contribution" and changes in the covariance as the "reallocation contribution" to aggregate productivity changes. One potential concern regarding this interpretation is that the covariance is also affected by changes in firm productivity that differ across the firm distribution. For instance, if small firms increase their productivity particularly strongly, the covariance will decline even if market shares will remain constant.

We can study this and other aspects determining changes in the covariance by following Kehrig and Vincent (2021) in decomposing the covariance term in the following way:

$$\Delta Cov(s_{it}, \omega_{it}) = Cov(\Delta\omega_{it}, s_{it_0}) + Cov(\omega_{it_0}, \Delta s_{it}) + Cov(\Delta\omega_{it}, \Delta s_{it}). \tag{J2}$$

The first term on the right-hand side captures the covariance between changes in firm productivity and initial size. The second term captures the covariance between changes in size and initial productivity. The last term is the covariance of joint changes in productivity and size, which also captures changes in productivity that result from changes in size (e.g., due to decreasing marginal products).

In Table J1, we regress all these covariance terms on the industry-region-level minimum wage exposure following the methodology of the main text. We find that the majority of the negative effect of minimum wage exposure on the covariance between productivity and size ($Cov(s_{it}, \omega_{it})$) results from a decline in the covariance between initial firm size and changes in productivity ($Cov(\Delta\omega_{it}, s_{it_0})$). Additionally, there is a small negative effect of minimum wage exposure on the covariance of changes in firm size and changes in productivity ($Cov(\Delta\omega_{it}, \Delta s_{it})$).

As discussed in the main text, one may argue to exclude changes in $Cov(s_{it}, \omega_{it})$ from the overall covariance when studying the contribution of reallocation to productivity growth. As evidenced by Table J1, this adjustment would lead us to conclude that there is only a small negative effect of minimum-wage-induced reallocation on productivity growth, resulting from changes in productivity that jointly occur with changes in firms size (e.g., because growing/shrinking firms reduce/increase their productivity). Nonetheless, even with this adjustment, we still conclude that all aggregate productivity gains in manufacturing result from productivity growth within firms.[50]

50. This is even true if we attribute the negative changes in $Cov(\Delta\omega_{it}, s_{it_0})$ to the within-firm contribution. Notably, the sum of $Cov(\Delta\omega_{it}, s_{it_0})$ and the unweighted mean yields a within-firm contribution term equivalent to the within-firm term in the decomposition of Foster et al. (2001).

Table J1: Covariance Decomposition: Log Labor Productivity

	2013 to 2015				2012 to 2013			
	(1) $\Delta\ Cov(s_{it}, \omega_{it})$	(2) $Cov(s_{it_0}, \Delta\omega_{it})$	(3) $Cov(\Delta s_{it}, \omega_{it_0})$	(4) $Cov(\Delta s_{it}, \Delta\omega_{it})$	(5) $\Delta\ Cov(s_{it}, \omega_{it})$	(6) $Cov(s_{it_0}, \Delta\omega_{it})$	(7) $Cov(\Delta s_{it}, \omega_{it_0})$	(8) $Cov(\Delta s_{it}, \Delta\omega_{it})$
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 (J2) 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.

K The Effect of Minimum Wages on Allocative efficiency

An alternative way to measure gains from reallocation is to directly study measures of factor misallocation. 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).

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. Both measures capture the idea that, in a frictionless market, reallocation equalizes gaps between MRPL and wages. In the following, we explain these two approaches to measure allocative efficiency. We first explain the approach by Hsieh and Klenow (2009) and subsequently discuss the approach by Petrin and Sivadasan (2013). A key ingredient in these approaches is the estimation of marginal revenue products that we derive from firms' production function (see Appendix I). Subsequently, we present our results from regressing these measures of allocative efficiency on minimum wage exposure at the industry-regional level following the regression approach of the main text.

K.1 Methods of Masuring Allocative Efficiency

In the following, we explain the two approaches to measure allocative efficiency that we use (Table K1) in more detail. We first explain the approach by Hsieh and Klenow (2009) and subsequently discuss the approach by Petrin and Sivadasan (2013).[51] 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 Appendix I. Importantly, by deriving the marginal revenue product from the estimated production function we can allow for decreasing and increasing returns to scale, varying output elasticities, and imperfect competition.

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

K.1.1 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 I). 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:

$$Q_t = \left(\sum_{i=1}^{N_i} Q_{it}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}. \tag{K1}$$

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 create a wedge between the marginal revenue product of labor and the wage. Profits are given by:

$$\pi_{it} = P_{it} Q_{it} - (1 + \tau_{it}) w L_{it}. \tag{K2}$$

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:

$$MRPL_{it} = (1 + \tau_{it}) w. \tag{K3}$$

Hence, without distortions, marginal revenue products of labor ($MRPL_{it}$) equalize across firms. We 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:

$$TFPR_{it} = \frac{\sigma}{\sigma - 1} \left(\frac{MRPL_{it}}{\alpha} \right)^{\alpha}. \tag{K4}$$

Equation (K4) implies that TFPR is equalized across firms in the absence of firm-specific distortions. Following Hsieh and Klenow (2009), aggregate TFPQ can be expressed as:

$$TFPQ_t = \left[\sum_{i=1}^{N_i} \left(A_{it} \frac{\overline{TFPR_t}}{TFPR_{it}} \right)^{\sigma-1} \right]^{\frac{1}{\sigma-1}}, \tag{K5}$$

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:

$$TFPQ_t = \left[\sum_{i=1}^{N_i} A_{it}^{\sigma-1} \right]^{\frac{1}{\sigma-1}}. \tag{K6}$$

As the impact of distortions on aggregate TFPQ is completely captured by dispersion in the marginal revenue product of labor (Equation (K4)), we directly analyse the dispersion in the MRPL.[52] Notably, we are only interested 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.

K.1.2 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:

$$APG_t \equiv \sum_{i=1} P_{it} dQ_{it} - \sum_{i=1} w_{it} dL_{it}, \tag{K7}$$

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

52. 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).

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 (K7) in the following way:

$$APG_t = \sum_{i=1} \left(P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it} \right) dL_{it} - \sum_{i=1} P_{it} dF_{it} + \sum_{i=1} P_{it} \frac{\partial Q_{it}}{\partial \omega_{it}} d\omega_{it}. \tag{K8}$$

The first term of Equation (K8) 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 (K8) assumes perfect competition. As we discuss below, in our empirical approach we also allow for firms having 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). Formally, 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:

$$\frac{1}{N} \sum_{i=1}^{N} \left(P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it} \right) IND_{it} = \frac{1}{N} \sum_{i=1}^{N} |P_{it} \frac{\partial Q_{it}}{\partial L_{it}} - w_{it}|. \tag{K9}$$

Petrin and Sivadasan (2013) use Equation (K9) 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 (K9) will be replaced by the marginal revenue product if firms have output market power. We apply this adjusted version of Equation (K9) when assessing the extent of allocative inefficiency.

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.

K.1.3 Results

Table K1 presents the results from regressing our standard measures of allocative efficiency on the industry×region-level gap measure. Columns 1 and 3 rely on the measure based on Hsieh and Klenow (2009). Columns 2 and 4 use the measure based on Petrin and Sivadasan (2013). 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 (i.e., the potential gains from labor reallocation between firms increase). Together with the results from the main text, we thus conclude that the minimum wage neither contributed to an increase in allocative efficiency nor led to any notable productivity-enhancing reallocation processes.

Table K1: Allocative Efficiency

	2013 to 2015		2012 to 2013	
	(1)	(2)	(3)	(4)
	Δ Std(log MRPL)	Δ $\overline{\lvert MRPL - w \rvert}$	Δ Std(log MRPL)	Δ $\overline{\lvert MRPL - w \rvert}$
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.

References

Berg, Tobias, Markus Reisinger, and Daniel Streitz. 2021. "Spillover Effects in Empirical Corporate Finance." *Journal of Financial Economics* 142 (3): 1109–1127.

Bräuer, Richard, Matthias Mertens, and Viktor Slavtchev. 2023. "Import competition and firm productivity: Evidence from German manufacturing." *The World Economy* 46 (8): 2285–2305.

Carlsson, Mikael, Julián Messina, and Oskar Nordström Skans. 2021. "Firm-Level Shocks and Labour Flows." *The Economic Journal* 131 (634): 598–623.

De Loecker, Jan, Pinelopi K. Goldberg, Amit K. Khandelwal, and Nina Pavcnik. 2016. "Prices, Markups, and Trade Reform." *Econometrica* 84 (2): 445–510.

Eslava, Marcela, John Haltiwanger, Adriana Kugler, and Maurice Kugler. 2004. "The Effects of Structural Reforms on Productivity and Profitability Enhancing Reallocation: Evidence from Colombia." *Journal of Development Economics* 75 (2): 333–371.

———. 2013. "Trade and Market Selection: Evidence from Manufacturing Plants in Colombia." Special issue: Misallocation and Productivity, *Review of Economic Dynamics* 16 (1): 135–158.

Foster, Lucia, John C Haltiwanger, and Cornell John Krizan. 2001. "Aggregate Productivity Growth: Lessons from Microeconomic Evidence." In *New developments in productivity analysis*, 303–372. University of Chicago Press.

Foster, Lucia, John Haltiwanger, and Chad Syverson. 2008. "Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability?" *American Economic Review* 98 (1): 394–425.

Hsieh, Chang-Tai, and Peter J. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics* 124 (4): 1403–1448.

Kehrig, Matthias, and Nicolas Vincent. 2021. "The Micro-Level Anatomy of the Labor Share Decline." *The Quarterly Journal of Economics* 136 (2): 1031–1087.

Levinsohn, James, and Amil Petrin. 2003. "Estimating Production Functions Using Inputs to Control for Unobservables." *The Review of Economic Studies* 70 (2): 317–341.

Mertens, Matthias. 2022. "Micro-mechanisms behind Declining Labor Shares: Rising Market Power and Changing Modes of Production." *International Journal of Industrial Organization* 81:102808.

Olley, G. Steven, and Ariel Pakes. 1996. "The Dynamics of Productivity in the Telecommunications Equipment Industry." *Econometrica* 64 (6): 1263–1297.

Petrin, Amil, and Jagadeesh Sivadasan. 2013. "Estimating Lost Output from Allocative Inefficiency, with an Application to Chile and Firing Costs." *Review of Economics and Statistics* 95 (1): 286–301.

Smeets, Valerie, and Frederic Warzynski. 2013. "Estimating Productivity with Multi-Product Firms, Pricing Heterogeneity and the Role of International Trade." *Journal of International Economics* 90 (2): 237–244.

Wooldridge, Jeffrey M. 2009. "On Estimating Firm-level Production Functions using Proxy Variables to Control for Unobservables." *Economics Letters* 104 (3): 112–114.

Yeh, Chen, Claudia Macaluso, and Brad Hershbein. 2022. "Monopsony in the US Labor Market." *American Economic Review* 112 (7): 2099–2138.

Halle Institute for Economic Research –
Member of the Leibniz Association

Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany

Postal Adress: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

Leibniz
Association

The IWH is funded by the federal government and the German federal states.