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Abstract

This paper studies the price and employment response of firms to the introduction of a nation-wide minimum wage in Germany. Widely throughout the economy, affected firms responded by rapidly and frequently increasing prices without cutting employment. These decisions are strongly interrelated: Firms that increased prices relatively more often also showed a less negative employment response. The relative importance of both adjustment margins is associated with product market competition and the specific economic situation firms face during the treatment period. Hence, understanding the role of price pass-through appears to be key for explaining employment effects of minimum wages.

JEL-Codes: E310, E240, J380, J310.

Keywords: minimum wage, price pass-through, employment, interrelation of firms' choices.

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1. Introduction

Firms can respond to minimum wage increases along different margins of adjustment that include changes in the size and composition of their workforce and other dimensions such as output prices, product quality, or non-wage aspects of jobs. Clemens (2021) highlights that these choices are not independent and argues that their interplay is central to understanding the weak employment effects documented in large parts of the minimum wage literature:[1] If, for instance, a firm was able to pass through the additional wage costs at least partially to its customers, the resulting shift in labor demand should counteract the disemployment effect. While there is increasing evidence on the importance of pass-through of minimum wages, little is known about whether firms' price and employment responses are indeed interrelated. Moreover, the evidence on pass-through usually stems from typical low-wage industries such as restaurants (e.g., Card and Krueger, 1994; Aaronson, 2001; Aaronson et al., 2008; Fougère et al., 2010; Allegretto and Reich, 2018; Ashenfelter and Jurajda, 2022) and retailers (Leung, 2021; Renkin et al., 2022). In contrast, there is only scant evidence that the pricing margin is of comparable importance in other industries that, e.g., differ in the competitive environment (Harasztosi and Lindner, 2019).

This paper is the first to provide a joint investigation of firms' price and employment responses to minimum wages and the degree to which these decisions are related. To this end, the paper exploits the unique setting of the introduction of a nation-wide minimum wage in Germany in 2015 (henceforth, NMW) that directly affected more than 10% of all jobs (Destatis, 2016) in many different industries. The analysis provides several novel insights. First, firms, if anything, only weakly responded to the NMW introduction along the employment margin, while price pass-through constituted an important margin of adjustment for firms throughout large parts of the economy. Second, the speed of price adjustment was relatively fast. Third, firms' price and em-

[1] For example, Neumark and Wascher (2008), Card and Krueger (2015), and Wolfson and Belman (2019) provide reviews and meta-analyses of the vast literature on the employment effects of minimum wages.

ployment responses to the NMW are strongly interrelated, as firms that, ceteris paribus, increased prices more often also showed a less negative employment response. Lastly, the relative sizes of the price and employment responses are heterogeneous with respect to various proxies of product market competition, local labor market conditions, as well as industry- or firm-specific economic conditions at the time of the NMW introduction—again suggesting that stronger price responses are, if anything, associated with weaker disemployment effects.

The analysis uses the ifo Business Survey (IBS). The IBS is a monthly survey among a representative panel of approximately 5,000 German manufacturing and services firms that collects firm-specific information on, inter alia, planned and realized changes in prices and the number of employees along the extensive margin. Despite its qualitative nature, the IBS data is unique along at least two dimensions: First, it offers the best data available to assess the minimum wage-induced price response of firms across many industries.[2] Second, it allows to study firms' responses along both the pricing and the employment margin as well as the interrelation of these choices.

The reaction of firms to the NMW is estimated in a difference-in-differences design with continuous treatment. Using wage data from the administrative Remuneration Statistic of the Federal Employment Agency, the treatment intensity is proxied by the fraction of full-time employees in each firm's industry and location that was affected by the NMW. Importantly, this bite measure is both, strongly correlated with alternative measures based on the Structure of Earnings Survey and captures whether firms perceive themselves to be affected by the NMW.

The estimation reveals that the NMW had a strongly positive effect on the frequency that affected firms increased prices, while the NMW effect on changes in employment was only moderately negative and insignificant throughout all empirical specifications. While planned price changes were not correlated with the bite of the NMW prior to the reform, this relationship was strong and significantly positive during the period around its introduction in January 2015. Using additional information on realized price changes provided by the subset of manufacturing firms confirms that firms not only planned to increase their prices more frequently in response to the NMW, but also

[2]The micro data of the German producer price index or comparable firm-level price data are not available. As shown below, the qualitative IBS data closely track quantitative changes in aggregate producer prices and employment.

implemented these changes accordingly. Given that treated firms were operating in many different industries, this suggests that price pass-through of minimum wages was a widespread phenomenon that was not limited to the restaurant and retail sectors typically analyzed in the literature.

The degree and speed of price pass-through was remarkably high. A back-of-the-envelope approximation, which exploits the close relationship between aggregated IBS data and industry-level producer price indices, yields that a 1%-increase in overall costs due to the NMW came along with a price increase of 0.85%. Moreover, the monthly frequency of the IBS allows for a detailed inspection of the adjustment dynamics. The results suggest that approximately 70% of the NMW-induced price adjustment took place during the twelve months around its introduction.

Firms' price and employment responses to the NMW are strongly related to each other. Containing firm-level information on both margins of adjustment, the IBS data allow for the construction of indicator variables capturing the joint price and employment responses. Firms that increased prices more often in response to a given NMW bite also showed a more muted, i.e., less frequently negative employment response relative to firms that—given the same NMW bite—reacted less frequently along the pricing margin. Hence, the size of the employment response appears to heavily depend on the question whether firms are able (or willing) to increase prices in response to minimum wages or not. This evidence in line with the predictions of Clemens (2021), who demonstrates that the positive relationship between price and employment responses is consistent with a standard textbook model of perfectly competitive labor markets that is augmented by the possibility that firms increase prices in response to the minimum wage.

The paper presents evidence on cross-sectional heterogeneity in firms' price and employment responses to the NMW, which also sheds light on potential channels behind the interrelation in the choice of the adjustment margin. On the one hand, the relative importance of these responses appears to strongly depend on the degree of product market competition. Specifically, the price effect is stronger—and the employment response slightly muted—if firms' export share is smaller, their goods and services are traded more locally, and competition through imports is lower. Arguably, these firms face a smaller elasticity of demand and are hence able to absorb a larger fraction of the

NMW-induced cost shock by raising prices (Weyl and Fabinger, 2013).

On the other hand, the paper provides novel evidence that the specific economic situation firms face at the time of the NMW introduction appears to be important for their choice of the adjustment margin. For instance, the price response was substantially weaker and the employment response slightly more negative if firms operated in industries with more negative output gaps. Comparably, firms' reaction to the NMW is strongly associated with their business outlook: Treated firms with more favorable expectations regarding the future development of their businesses reacted much more frequently along the pricing margin, while those with grim expectations reduced the number of employees significantly more often for a given NMW bite. Further, the disemployment effect was—given the same degree of affectedness—strongest for firms located in counties with the highest slack in the labor market. Relatedly, firms, who did not report a shortage of labor, were significantly more likely to react along the employment margin compared to those facing difficulties in satisfying their labor demand. Instead, the latter firms appear to have increased prices more frequently in response to the NMW. In contrast, credit constraints seem to play a minor role.

The paper contributes to several strands of the literature. First, it adds to increasing evidence on the importance of price effects of minimum wages. Although the view that minimum wage increases come along with higher prices seems widely accepted, existing evidence usually draws on restaurants (e.g., Card and Krueger, 1994; Aaronson, 2001; Aaronson et al., 2008; Fougère et al., 2010; Allegretto and Reich, 2018; Ashenfelter and Jurajda, 2022) and retailers (Leung, 2021; Renkin et al., 2022). While most of these papers document high degrees of pass-through, evidence is sparser and more ambiguous for other sectors: On the one hand, Machin et al. (2003) and Draca et al. (2011) show that price affects were absent in the heavily affected but price-regulated British residential care industry and Wadsworth (2010) does not find significant price effects in several low-wage industries to increases in the British national minimum wage.[3] On the other hand, Harasztosi and Lindner (2019) find evidence in favor of broad-based pass-through resulting in 75% of the costs of a large minimum wage increase in Hungary in 2001 being paid by consumers via

[3]Further, Katz and Krueger (1992) do not find evidence for price effects in the fast-food industry in Texas to changes in the federal minimum wage. Lemos (2008) surveys the early literature of the price effects of the minimum wage.

higher revenues—largely comparable with my overall finding of widespread and substantial degree of pass-through among firms operating in the more developed German economy.[4] However, while Harasztosi and Lindner (2019) find that prices were only gradually adjusted and the accumulated price effect thus took more than two years to turn significantly positive, my evidence points at a faster adjustment of prices that is more in line with the literature on restaurants or retailers.[5]

Second, the paper is the first to provide firm-level evidence on the interrelation of firms' price and employment response to minimum wages—a mechanism that is argued to explain the relatively weak employment effects documented in most of the minimum wage literature (Clemens, 2021). Existing studies usually assess firms' responses along either dimension in isolation in lack of firm-level information on both variables. Although not examining the firm-level relationship itself, Harasztosi and Lindner (2019) is a notable exemption in finding a smaller effect on revenues (interpreted as smaller price effect) and a stronger disemployment effect among groups of Hungarian firms in tradable, exporting, and manufacturing sectors.[6] My paper finds comparable cross-sectional patterns among German firms and shows that—consistent with imperfect product market competition—firm-level responses are strongly related to each other.[7] In addition, my heterogeneity results point towards the importance of the specific economic situation firms face at the time of treatment in explaining their choice of the adjustment margin. Thus, the paper provides a new, coherent perspective on the state-dependence of minimum wage effects, which could previously only be inferred from comparing estimates across different settings studied separately.[8]

Third, highlighting the importance of pass-through, the paper adds to an expanding literature

[4]Harasztosi and Lindner (2019) restrict their main analysis to effects on revenues due to a lack of price data for firms in services and only document a direct effect on prices for the subset of manufacturing firms.

[5]Specifically, my results range between those of Aaronson (2001), Aaronson et al. (2008), and Renkin et al. (2022), who find immediate pass-through among restaurants and retailers in the U.S., and those of Fougère et al. (2010) documenting that minimum wages take more than twelve months to fully pass through to French restaurant prices.

[6]Katz and Krueger (1992), Card and Krueger (1994), and Hirsch et al. (2015) are further exemptions that investigate both price and employment effects in the same study. However, they neither examine the interplay between these effects nor can look at cross-sectional heterogeneity given their narrower focus on fast-food restaurants.

[7]In line with these findings but not examining the price margin, Cengiz et al. (2019) find that U.S. state-level minimum wage changes had a negative, albeit imprecisely estimated, effect on employment in the tradable sector and an effect that is close to zero in the non-tradable sectors such as restaurants or retail.

[8]For example, Clemens and Wither (2019) document large negative employment effects to minimum wage increases during the Great Recession, while studies covering expansionary periods usually find that firms, if anything, adjust employment by reducing hiring rather than outright firing (e.g., Gopalan et al., 2021; Jardim et al., 2022).

studying the effect of minimum wages on margins other than employment. For instance, there is evidence that affected firms reacted to minimum wages via non-compliance (Weil, 2005; Garnero and Lucifora, 2022), tax evasion (Tonin, 2011), cutting fringe benefits (Clemens et al., 2018), raising hiring standards (Clemens et al., 2021; Butschek, 2022), increasing productivity (Riley and Rosazza Bondibene, 2017; Coviello et al., 2022; Ku, 2022), or capital-labor substitution (Aaronson and Phelan, 2019). Moreover, some studies have documented minimum wage effects on firms' profitability and stock market value (Draca et al., 2011; Bell and Machin, 2018), firm exit (Luca and Luca, 2019), and worker reallocation between firms (Dustmann et al., 2022). In sum, this calls for a joint assessment of different adjustment channels, which is not limited to the employment margin, to gain a comprehensive understanding of firms' responses to minimum wages.

Lastly, my study complements recent work on the NMW introduction in Germany (see Caliendo et al., 2019, for a survey). Despite the large size of the minimum wage shock and consistent with my results, the effect of the NMW on overall employment is either found to be zero (e.g., Ahlfeldt et al., 2018; Dustmann et al., 2022) or negative, but small (e.g., Caliendo et al., 2018; Bossler and Gerner, 2020). Thus, the evidence from Germany is comparable to Bailey et al. (2021) and Harasztosi and Lindner (2019) who also find small disemployment effects following very large changes in national-level minimum wages in the U.S. in the 1960s and Hungary in 2002, respectively.[9] In contrast, other margins of adjustment also appear to have been important in the context of the German NMW, including the reduction of working hours (e.g., Bossler and Gerner, 2020 and Caliendo et al., 2022), reallocation of workers to higher paying firms (Dustmann et al., 2022), and—as documented in this paper—prices.

The remainder of the paper is structured as follows. Section 2 provides information about the institutional background of the German NMW reform of 2015 and describes the data. Section 3 specifies the empirical strategy. Section 4 presents the empirical results. Section 5 concludes.

[9] In contrast, Jardim et al. (2022) find that the strong increases in Seattle's city-wide minimum wage to $11 per hour in 2015 and $13 per hour in 2016 had a strongly negative employment effect, which, however, manifested in a strong reduction in working hours rather than headcount. Relatedly, Clemens and Strain (2021) compare the employment effects of U.S. state-level minimum wage changes between 2011 and 2019 and find that employment responds more elastically to larger minimum wage increases.

2. Background and Data

2.1. The German Reform as a Unique Framework to Study Minimum Wage Effects

Institutional Background The introduction of a national minimum wage of €8.50 per hour on January 1, 2015, constituted a paradigm shift in the history of German labor market policy. Previously, wages had been determined almost exclusively through collective bargaining agreements between unions and employer associations. In a small number of industries such as construction, roofing, commercial cleaning, security, laundry service, and some handicraft sectors, the collective bargaining agreements included minimum wages binding for all firms in the industry. While the NMW in general applies to all industries, firms in sectors with industry-specific minimum wages below €8.50 were allowed to delay their compliance to the NMW until the end of 2016.[10] To prevent malpractice, the NMW was accompanied by obligatory requirements for firms to document daily working hours of each employee with a gross monthly wage below €2,958.

The NMW was implemented as follows: after the federal election of September 22, 2013, the chairpersons of the conservative parties (*CDU* and *CSU*) and the social democrats (*SPD*) signed a coalition agreement on November 27, 2013, that contained the intention to introduce a statutory minimum wage of €8.50 on January 1, 2015. The Federal Cabinet proposed the respective law on April 2, 2014, including all relevant regulations regarding its introduction and details on the exemptions. Both chambers of parliament, *Bundestag* and *Bundesrat,* approved the law on July 3 and July 11, 2014, without major changes. After its introduction, the NMW remained constant until January 2017 when it was increased to an hourly rate of €8.84.

Unique Framework for Evaluation of Minimum Wage Effects The introduction of the German NMW offers a unique setting to study the response of firms in many different sectors of the economy as it was directly set to a relatively high level of 48% of the median wage—a level lower than in France (62%), close to that of the U.K. and the average across OECD countries (49%), and

[10] These sectors include agriculture, forestry, gardening, the meat industry, manufacturing of textiles and clothing, temporary work agencies, hairdressers, and laundries. There are further exemptions for teens, apprentices, compulsory internships, voluntary workers, and long-term unemployed.

much higher than in the U.S. (36%) (OECD, 2015). Prior to the introduction, 4.0 million employees (10.7% of all jobs) that were eligible for the NMW earned less than €8.50 per hour (Destatis, 2016). *Ceteris paribus*, this corresponds to a wage increase among affected employees of on average 18% resulting in an increase in the economy-wide wage bill by 0.43% (Destatis, 2016). Thus, the cost increase associated with the German NMW introduction is much larger than the variation usually exploited in the literature that—in consequence of the smaller bite—mostly restricts the analysis to highly affected industries (e.g., restaurants) or low-wage groups (e.g., teens). There are only a few examples of minimum wage increases of comparable size such as those at the national level in the U.S. in the 1960s and in Hungary in 2002 studied by Bailey et al. (2021) and Harasztosi and Lindner (2019), respectively, as well as at the local level such as the city-wide minimum wage in Seattle studied by Jardim et al. (2022). In contrast to those, the German NMW offers a recent example of a strong national-level increase in a highly industrialized country.

Effects of NMW on Wages and Compliance Besides the evidence on the effects of the NMW described in the introduction and reviewed in Caliendo et al. (2019), there is a series of papers on the effect of the NMW on the wage distribution and compliance that are relevant for the interpretation of my results. First, the literature unanimously documents that the NMW had positive effects on gross hourly wages of low-wage workers without finding evidence for very strong spillover effects on higher wage segments (e.g., Ahlfeldt et al., 2018; Bossler and Gerner, 2020; Caliendo et al., 2022; and Dustmann et al., 2022). Second, while the various exemptions and errors in measuring hourly wages complicate the identification of compliance, there is evidence that a substantial share of eligible employees earned less than the NMW shortly after its introduction (e.g., Caliendo et al., 2022). However, Dustmann et al. (2022) show that the excess hourly wage growth of minimum-wage workers in the post-policy period is roughly in line with what would be expected under full compliance to the NMW. This suggests that non-compliance hampered the increase in labor costs only to a limited degree.

Macroeconomic Conditions The empirical findings should be interpreted in light of the fact that the German NMW was introduced in a period of relatively favorable macroeconomic conditions and low inflation: Between 2013 and 2017, real GDP grew by 9% and the unemployment rate decreased from 6.9% to 5.7%—the lowest level since the German reunification in 1990—, while consumer prices increased by on average 1% and producer prices decreased on average by 0.4%.

2.2. Data: ifo Business Survey

The evaluation of firms' price response to the NMW and its relation to adjustments along the employment margin requires firm-level micro data along both dimensions. This is particularly restrictive for prices as the micro data of the German producer price index or comparable firm-level information on prices are not available. The ifo Business Survey (IBS) circumvents this constraint by repeatedly asking a representative panel of firms*, inter alia*, about their planned and realized changes in prices and employment at monthly frequency.[11] The IBS is usually answered by individuals responsible for high-stakes decisions such as firm owners, members of the executive board, or department heads (Sauer and Wohlrabe, 2020). Given its uniqueness, the IBS does not only provide the best data source to study the price effects of the NMW at the firm level, but is also extensively used in other studies investigating firms' price setting behavior in different contexts (e.g., Bachmann et al., 2019; Balleer and Zorn, 2019; Balleer et al., 2022; Enders et al., 2019).[12]

I limit the sample to manufacturing firms (IBS-IND, 2020) and service companies (IBS-SERV, 2020) in those industries that were not allowed to pay wages below the NMW during the transition phase. Restricting the sample to the period between April 2011 and December 2017 and to firms that were surveyed for at least twelve times, the sample comprises of on average 4367 firms per survey wave (2290 manufacturers and 2077 service companies).[13] The average firm is in the

[11] Hiersemenzel et al. (2022) show that the sample of the IBS is representative of the German economy according to various criteria. The IBS is the basis for the ifo Business Climate Index, the most recognized leading indicator that closely tracks and forecasts the German economy. See Sauer and Wohlrabe (2020) for a detailed documentation.

[12] The IBS employment data is inferior to quantitative information contained in administrative employment records used by Dustmann et al. (2022) or the "IAB Establishment Panel" used by Bossler and Gerner (2020). However, these sources cannot be linked to firm-level information on price setting. Moreover, the IBS does not contain wage information but can be merged to administrative wage data at the level of industry-county cells (c.f., Section 3.1).

[13] The sample starts in April 2011 after a break in the sector classification. The services and manufacturing IBS are

sample for 5.4 years within these 6.75 years and answers the questionnaire in 84.3% of months.

The analysis of NMW effects mainly focuses on the following questions regarding planned changes in prices and employment:[14]

Q1: *"Expectations for the next 3 months: The prices of our goods/services will [1] increase, [0] stay the same, or [-1] decrease."*

Q2: *"Expectations for the next 3 months: The number of employees will [1] increase, [0] stay the same, or [-1] decrease."*

In addition, the subset of manufacturing firms reports *realized* price changes and service companies provide information on *realized* employment changes. At least in these subsamples, firms in general stick to their pricing and employment plans[15] and the results regarding the effects of the NMW are comparable as shown in Section 4.2.

The information on the extensive margin of price and employment changes contained in the IBS is highly informative for actual realizations of these variables both at the micro and the aggregate level. First, Balleer and Zorn (2019) document that the frequency of price adjustments reported to the IBS is largely the same as in administrative micro data underlying the German producer price index. Second, Bachmann et al. (2019) show that aggregated IBS data closely track quantitative changes in industry-specific producer price indices. Online Appendix C.2 demonstrates this very close co-movement over time: the correlation between average planned and realized price changes reported to the manufacturing IBS and the PPI is 0.86 and 0.85, respectively, while the correlation between mean employment plans and quantitative changes in administrative employment data is 0.77. This close relationship is exploited in a back-of-the-envelope approximation of the quantita-

conducted at the establishment- and product-level, respectively. In the latter, 0.3% of all observations refer to multiple products of the same plant per date. Following Link (2020), these observations are aggregated to the plant level by taking means and rounding. Further, the micro data do not allow to distinguish between subsidiaries of multi-plant firms and independent companies. Henceforth, both types of entities are denoted as "firms." Table B.2 in the Online Appendix provides more detailed information on the industry composition.

[14] The wording of the questions slightly differs between the manufacturing IBS and services IBS but is comparable. See Online Appendix E for the translated wording of all survey questions used in the paper.

[15] Pesaran and Timmermann (2009) show that the price plans are highly predictive for the subsequently reported realized price changes. Accordingly, Online Appendix C.1 documents that approximately 80% of manufacturing firms report realized price changes between months $t+1$ and $t+3$ that are in line with their plans of month t.

tive size of the price effect described below and presented in detail in Online Appendix D.1.

3. Empirical Strategy

3.1. Identification of the Minimum Wage Bite

To evaluate the reaction of firms to the NMW, I apply the popular minimum wage "bite" approach that exploits variation in the share of affected workers across groups, regions, or industries. What follows describes the construction of the bite measure and addresses potential concerns with it.

Construction of the Bite Measure To proxy the firm-specific bite as closely as possible, the bite measure is constructed at the level sector s-region r cells (henceforth, TI_{sr}) of firm $i \in (s,r)$ in the IBS. This requires detailed information on the wage distribution for each cell. The administrative remuneration statistic ("*Entgeltstatistik*," henceforth "RS") collected by the Federal Employment Agency (FEA) provides the most extensive source for wages in Germany as it covers all employees prone to social security in Germany. The RS is hence well suited to construct an industry-specific bite measure at disaggregated regional levels. Specifically, the RS data used to construct TI_{sr} include the deciles of the distribution of gross monthly wages of full-time employees for each two-digit industry either at the level of counties (NUTS-3 regions) or at the level of labor market regions (LMRs) that, for data protection reasons, contained at least 1,000 full-time employees (Federal Employment Agency, 2016).[16] Despite this restriction, the wage data at the level of industry-county cells cover 68.8% of all full-time employees, while covering 92.3% at the level of industry-LMR cells. The baseline specification therefore uses the industry-specific wage distribution at the county level and replaces missing values by wage data at the level of LMRs.[17]

TI_{sr} is defined as the fraction of full-time employees who earned a gross hourly wage below €8.50 in each firm's industry and location in the year prior to the NMW introduction.[18] Unfortu-

[16] There are 96 labor market regions ("*Raumordnungsregionen*") in Germany that typically describe commuting zones consisting of, on average, four out of a total number of 402 counties.

[17] Robustness checks presented in Section 4.5, which use wage data either at the level of counties only or at the level of LMRs only, show that the results do not hinge on this choice.

[18] I use 2014 wages following Caliendo et al. (2018) who show that wages followed a common trend before the NMW

nately, the RS data only contains gross monthly wages of full-time employees without providing information on hours worked. I convert the RS wage data hourly wages by means of monthly working hours collected in the Quarterly Earnings Survey ("*Vierteljährliche Verdiensterhebung*").

Online Appendix B.1 describes the construction of TI_{sr} in detail and discusses the assumptions on the wage distribution below the first decile, which are shown to not drive the results in Appendix Section B.3. Following this procedure, TI_{sr} can be assigned to 84.2% of the firms in the sample.

Variation in NMW Bite TI_{sr} captures a substantial degree of variation across firms. As shown in Table B.1 in the Online Appendix, 27% of firms were affected to at least some degree. Among firms with $TI_{sr} > 0$, roughly one in two (one in five) of the affected firms had to increase wages of more than 10% (20%) of their full-time employees due to the NMW, *ceteris paribus*. There is sufficient variation in TI_{sr} to analyze the response of firms in different sectors or regions. While services firms—of which 37% are assigned to positive values of TI_{sr}—were affected more often by the NMW than manufacturing firms (17%), there is also substantial variation in TI_{sr} within two-digit industries; see Table B.2 in the Online Appendix. In addition, one out of five firms in Western Germany and two out of three firms in Eastern Germany were affected according to TI_{sr}. Conditional on being affected, the variation in TI_{sr} is comparable between firms in the different subsets: the mean NMW bite is 0.10 and 0.14 among treated manufacturing firms and service companies, as well as 0.12 and 0.14 in Western and Eastern Germany, respectively.

Evaluation of Bite Measure To be a good proxy for the NMW bite, TI_{sr} should meet the following three requirements: First, TI_{sr} should capture the degree to which full-time employees are affected by the NMW sufficiently well despite the assumptions underlying its construction. Second, TI_{sr} should capture the variation in increased wage costs despite of the fact that it is calculated from the wage distribution of full-time employees, only. Third, the degree to which firms were affected by the NMW should be reflected by the bite in its industry and location.

To tackle the first concern, the overall NMW bite calculated from RS data is contrasted to the

reform and anticipation effects were absent. The results are similar if TI_{sr} is based on 2013 wages, see Section 4.5.

Table 1: Bites Calculated from Remuneration Statistic vs. Structure of Earnings Survey

Wage Data	Remuneration Statistic	Structure of Earnings Survey	
Source	Federal Empl. Agency	Federal Statistical Office	
Employees Covered	Full-Time only	All Types	
Representative at Region*Sector Level	yes	no	
Panel A: Fraction of Workers Affected By NMW in Manufacturing & Services Sectors Prone to NMW			
Fraction of Eligible Workers w/ $w < €8.50$			
All Employees		0.107	
Full-Time Employees	0.039	0.039	
*Panel B: Correlation b/w Bite Measures at Region*Sector Level*			
Correlation	TI	$TI^{SES,FT}$	$TI^{SES,all}$
TI	1	.	.
$TI^{SES,FT}$	0.871	1	.
$TI^{SES,all}$	0.886	0.894	1

Notes: This table compares the baseline bite measure TI based on wage data for full-time employees from the Remuneration Statistic (RS) to alternative measures using wage data from the Structure of Earnings Survey (SES) for full-time employees ($TI^{SES,FT}$) and all employees ($TI^{SES,all}$). Panel A summarizes the nationwide fraction of workers affected by the NMW in manufacturing and service sectors prone to the NMW among all workers as well as restricted to full-time employees. Panel B provides the correlations between these bite measures calculated for each two-digit industry*LMR cell for which RS data are available and the SES contains at l100 employees.

same metric based on the 2014 wave of the Structure of Earnings Survey (SES). The SES micro data contain individual-level information on wages and working hours of one million workers employed at 60,000 firms that is representative at the level of federal states and two-digit industries (SES, 2014). The SES thus allows to calculate the overall bite of the NMW by directly examining hourly wages of individual workers without imposing assumptions on the shape of the wage distribution. Strikingly, the SES and RS data deliver the same overall bite of 3.9% affected full-time employees in the manufacturing and service sectors prone to the NMW (Panel A of Table 1).

In addition, the SES allows to construct an alternative bite measure capturing the fraction of affected full-time employees at the level of industry-region cells ($TI_{sr}^{SES,FT}$).[19] In this respect, however, the SES data have at least two disadvantages relative to the RS: first, the SES data are

[19]The German Socio-Economic Panel (SOEP) is another source of wage data frequently used to calculate bite measures (e.g., Caliendo et al., 2018). However, the SOEP contains very little information on the industries in which surveyed individuals work which prevents the construction of industry-specific bites.

not representative at the level of LMR-industry cells, while the RS covers the universe of full-time employees. Second, $TI_{sr}^{SES,FT}$ can only be constructed for less than half of the firms in the IBS sample even if the SES data are required to cover a minimum of 100 employees in each LMR-industry cell, only. Despite these disadvantages, the correlation of TI_{sr} and $TI_{sr}^{SES,FT}$ is remarkably high ($\rho = 0.87$) (Panel B of Table 1). Taken together, TI_{sr} appears to closely capture the NMW bite for full-time employees despite the underlying assumptions.

The second concern is alleviated by showing that TI_{sr} is highly correlated with the degree to which firms in the respective industry-region cells were affected by increased wage costs for *all* types of employment relationships. This is crucial as part-time employees and marginally employed workers were on average more strongly affected by the NMW than full-time employees.[20] For this purpose, the SES data is used to construct $TI_{sr}^{SES,all}$ defined as the fraction of all employees who earned less than €8.50 in 2014 for all LMR-industry cells with at l100 employees in the SES data. The correlation between TI_{sr} and $TI_{sr}^{SES,all}$ is strikingly high ($\rho = 0.89$). Further, Section 4.5 shows that the main results are robust to using $TI_{sr}^{SES,all}$ instead of TI_{sr}.

Lastly, TI_{sr} is strongly correlated with firms' self-assessment of being affected by the NMW. Online Appendix B.4 documents this close relationship based on supplementary questions in the November 2014 IBS wave and demonstrates that using firms' self-assessed affectedness by the NMW as binary treatment indicator delivers qualitatively the same results as the main findings based on the continuous bite measure TI_{sr}.

Taken together, the bite measure TI_{sr} should capture the degree to which firms are affected by the NMW through its effect on labor costs well.

3.2. Empirical Model

To evaluate the effect of the NMW on firms' adjustment along the pricing and employment margin, I estimate a generalized difference-in-differences (DiD) model with continuous treatment exploit-

[20] According to the SES, the 10.7% of all eligible workers in manufacturing and services earned below €8.50 in 2014 (3.9% among full-time employees). In these sectors, 61% of jobs were full-time, 25% part-time, and 14% marginally employed. Under full compliance and a constant employment structure, the NMW would have induced an overall wage bill increase of 0.44% of which 40% would be due to full-time employees.

ing the variation in the bite of the NMW. As a first step, the NMW effect on firms' decisions, denoted β_t, is estimated separately for each month t based on the following empirical model

$$Y_{i,t}^{+3m} = \sum_{t:t\neq 2013m1} \beta_t \times TI_{sr} \times \mathbb{1}(\text{Date}_t) + \alpha_i + \delta_t \times \mathbb{1}(\text{Sector}_i) + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}^{+3m}$ corresponds to firm i's planned price or employment change in the following three months as reported in month t. The bite TI_{sr} is interacted with date dummies for each $t \in [2011m4, 2017m12]$ except for the baseline month of January 2013 when the NMW introduction could not have been anticipated. This standard method identifies the dynamic treatment effect of TI_{sr} on $Y_{i,t}^{+3m}$ in each t relative to the effect in the baseline period and after controlling for the covariates. Hence, the estimates around January 2015 should capture the NMW effect on firms' plans, while the β_t's for the dates prior to the treatment period should be zero as the NMW reform should not have influenced firms then. This parallel trend assumption is verified in Section 4.1.[21]

The baseline specification of Model (1) controls for firm fixed effects α_i and date fixed effects δ_t at the level of two-digit industries and clusters standard errors at the levels of counties, two-digit industries, and dates. Firm fixed effects capture time-invariant firm-specificities including persistent optimism or pessimism as documented in IBS data by Bachmann and Elstner (2015). Industry-specific date fixed effects flexibly control for fluctuations that similarly affect the pricing and employment adjustments of all firms in an industry. By construction, they also control for seasonality and any other source of aggregate variation, e.g., due to other policies at the national level. Given that TI_{sr} is measured at the level of industry-region cells, the sequence of treatment effects is identified from variation in TI_{sr} between firms in different regions of the same two-digit industry. Section 4.5 provides evidence that the main results are unaffected by the dimension of fixed effects or the inclusion of time-varying controls at the firm level.

To provide a more condensed examination of average minimum wage effects during the entire

[21] Another concern of the DiD approach is that the Stable Unit Treatment Value Assumption (SUTVA) is violated. If firms that were not directly subject to the NMW ($TI_{sr} = 0$) were affected via spillover effects, the empirical results would be biased. Section 4.5 provides evidence that this is unlikely to be the case.

treatment period, Equation (1) is adjusted as follows

$$Y_{i,t} = \beta \times TI_{sr} \times \mathbb{1}(t \in (\underline{t}, \bar{t})) + \alpha_i + \delta_t \times \mathbb{1}(\text{Sector}_i) + \varepsilon_{i,t}. \tag{2}$$

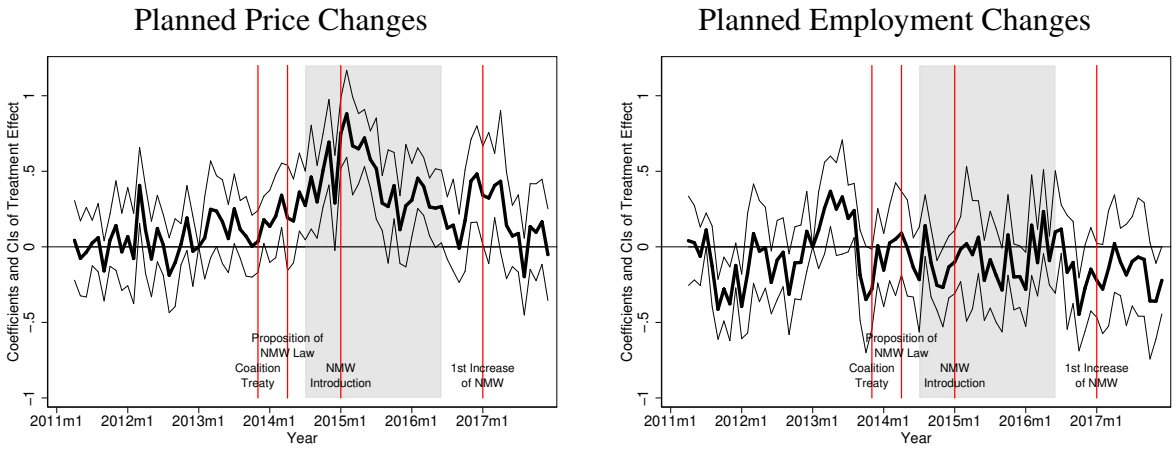
Based on the estimation results from Model (1) presented in Section 4.1, the treatment period is defined to firms' plans reported between July 2014 and June 2016 and the months thereafter are omitted from the sample to prevent the control period from being corrupted by the 2017 NMW increase. When estimating the NMW effect on realized changes in prices and employment for the subsets of firms that report these variables, the treatment period is shifted by three months.

$\hat{\beta}$ can be interpreted as follows: Relative to the control period, a firm with TI_{sr} reported planned price or employment changes—scaled as "[1] increase," "[0] stay the same," "[-1] decrease"—that were *ceteris paribus* increased by $\hat{\beta} \times TI_{sr}$ on average. Within the two-year treatment window, this firm thus reported planned price or employment changes of a one-step higher category in $\hat{\beta} \times TI_{sr} \times 24$ additional months compared to the counterfactual scenario in absence of the NMW.

When examining how firms' adjustment along the margins of prices and employment are interrelated in Section 4.3, I construct indicator variables that group observations according to the joint price and employment responses: (1) firms that plan to adjust along both margins at a given point in time, i.e., increase prices and decrease number of employees ($\mathbb{1}(\Delta\text{Price}_{i,t}^{+3m} = 1 \wedge \Delta\text{Empl.}_{i,t}^{+3m} = -1)$), (2) firms that plan to increase prices but do not plan to cut employment, (3) vice versa, and (4) firms that neither plan to increase prices nor to cut employment. Then, Model (2) is estimated separately for each of these indicators as dependent variable.

An obvious concern with this approach is that firms' actions in response to the NMW do not necessarily take place simultaneously. For instance, a firm's decision to increase prices due to the NMW at the beginning of the treatment period might affect its employment and pricing decisions in the subsequent months. To address this, I pool firms' planned responses during the control period (2011m4–2014m6) and treatment period (2014m7–2016m6) by taking means. Then, indicators of firms' joint adjustment are defined along the same lines as above to capture, e.g., whether

Figure 1: Timing of Price and Employment Response of Firms to the NMW Introduction



Notes: The figure plots the coefficients of the dynamic treatment effect of the NMW introduction in January 2015 estimated in Model (1). The dependent variables are firms' planned price or employment changes during the following three months. The effects are estimated relative to January 2013. The vertical lines in November 2013, April 2014, January 2015, and January 2017 indicate the dates of the coalition treaty mentioning the NMW introduction, the decision of the federal cabinet containing the relevant details of the NMW law, its introduction, and its first increase, respectively. The shaded area indicates the treatment period used in Model (2). The thin lines display the 90%-confidence intervals based on standard errors three-way clustered at the levels of industries, counties, and dates.

a firm planned more price increases than decreases and more employment cuts than increases ($\mathbb{1}\left(\sum_{t\in p}\Delta\text{Price}_{i,t}^{+3m}>0\wedge\sum_{t\in p}\Delta\text{Empl.}_{i,t}^{+3m}<0\right.$, where p denotes either the control or treatment period). Then, the NMW effect on each of these indicators is estimated along the lines of Model (2), which controls for firm fixed effects and period fixed effects at the level of two-digit industries.

Models (1) and (2) are estimated with OLS because standard methods for the estimation of generalized DiD models with fixed effects and non-binary ordinal data are not established in the literature, yet. However, Riedl and Geishecker (2014) show that linear panel data models generally perform quite well in comparable settings with large cross-sections and long time series.

4. Results

4.1. Timing of Price and Employment Response of Firms to the NMW Introduction

The results of estimating Model (1) show that firms reacted to the NMW introduction more strongly along the pricing margin than adjusting employment plans. The left-hand panel of Figure 1 plots

the estimated sequence of treatment effects β_t on planned price changes. Importantly, planned price changes of firms are not correlated with TI_{sr} prior to 2014. Hence, affected firms did not follow a different pre-trend in their pricing plans relative to their unaffected counterparts.

The NMW induced an appreciation of planned price changes that is clearly concentrated in the time period around its introduction. The coalition agreement on the NMW of November 2013 did not have an immediate effect. Instead, the treatment effect on firms' planned price changes appreciated over the course of 2014 as more details about the NMW became available and the introduction date approached. The treatment effect is strongest between the last quarter of 2014 and the second quarter of 2015 indicating that the bulk of the price adjustment took place immediately in the period around the NMW introduction. While the pass-through on prices slowed down during the second half of 2015, the NMW continued to induce additional planned price increases until mid-2016.[22] Overall, 70% of the NMW-induced price increases took place during the 12 months around its introduction as summarized in Panel A of Table A.1 in the Online Appendix.

This evidence in favor of relatively fast pass-through, which is based on firms in many different industries, is in line with findings of most papers studying the speed of price adjustment to minimum wages among restaurants and retailers. Specifically, my results range between those of Aaronson (2001), Aaronson et al. (2008), and Renkin et al. (2022), who find evidence in favor of immediate pass-through among for restaurants and supermarkets in the U.S., and those of Fougère et al. (2010), who document that minimum wages take more than twelve months to fully pass through to French restaurant prices. Moreover, my results stand in contrast to Harasztosi and Lindner (2019) who find that the price adjustment to the Hungarian 2001 minimum wage increase only occurred gradually and took more than two years to turn significantly positive.

In contrast, the dynamic response of firms' planned employment changes to the NMW depicted in the right-hand graph of Figure 1 delivers an ambiguous pattern. This is because the coefficients depend on the choice of the baseline period and the association between TI_{sr} and employment plans is not constant throughout the pre-reform period. Model (2) used below alleviates this issue

[22] The association between TI_{sr} and firms' pricing plans appreciated again around the first increase of the NMW in January 2017. However, TI_{sr} only imperfectly captures the degree of firms' affectedness by this increase to €8.84.

Table 2: Minimum Wage Effects at the Firm-Level: Baseline Results

	Panel A: Planned Adjustments and Expectations			
	$\Delta\text{Price}_t^{+3m}$	$\Delta\text{Empl.}_t^{+3m}$	$\Delta\text{Cond.}_t^{+6m}$	$\Delta\text{Prod./Demand}_t^{+3m}$
	(1)	(2)	(3)	(4)
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	-0.05	-0.03	0.07
	(0.06)	(0.06)	(0.07)	(0.08)
Firm FE	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes
Sample	Total	Total	Total	Total
R^2	0.324	0.321	0.363	0.381
Observations	240211	240331	240305	241045
	Panel B: Realized Adjustments and Conditions			
	$\Delta\text{Price}_t^{-1m}$	$\Delta\text{Empl.}_t^{-3m}$	Cond._t	Orders_t
	(1)	(2)	(3)	(4)
$\text{TI} \times \mathbb{1}(t \in \{2014m10, 2016m9\})$	0.29***	-0.05	0.07	0.09
	(0.06)	(0.09)	(0.09)	(0.07)
Firm FE	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes
Sample	Manuf.	Services	Total	Total
R^2	0.271	0.282	0.511	0.455
Observations	136019	115042	252059	246526

Notes: In Panel A, the dependent variables are planned price or employment changes during the following three months ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$), expected business conditions for the next six months ($\Delta\text{Cond.}_t^{+6m}$), and expected production (manufacturing firms) or demand (service firms) for the following three months ($\Delta\text{Prod./Dem.}_t^{+3m}$) of firms in the IBS. In Panel B, these are realized changes in prices during the last month ($\Delta\text{Price}_t^{-1m}$, manufacturing only), realized change in employment during the last three months ($\Delta\text{Empl.}_t^{-3m}$, services only), current business conditions (Cond._t) and current backlog of orders (Orders_t). "*TI*" is the bite of the NMW and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" and "$\mathbb{1}(t \in \{2014m10, 2016m9\})$" indicate the respective treatment period. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

by estimating the average NMW effect relative to a control period of 39 months.

4.2. Firms' Adjustment along Different Margins: Average Effects

Next, average NMW effects are estimated by means of Model (2). Table 2 summarizes the results. Section 4.5 shows that these results are robust along various dimensions.

NMW Effect on Price Changes The NMW had a strongly positive effect on the frequency that affected firms planned to increase their prices. The average treatment effect on planned price

changes reported between July 2014 and June 2016 is estimated to $\hat{\beta} = 0.37$ ($p < 0.01$), see Column (1) of Panel A. Hence, a firm that is affected to degree TI_{sr} reported additional planned price changes of a one-step higher category—i.e., increased instead of constant or constant instead of decreased prices—in $9 \times TI_{sr}$ months relative to the counterfactual scenario. Hence, a firm with $TI_{sr} = 0.25$ reported on average more than two additional price increases due to the NMW.

Firms have rolled over a substantial share of the costs generated by the NMW to their customers. To approximate the size of the price effect, Online Appendix D.1 conducts a back-of-the-envelope calculation that exploits the very close mapping between aggregated survey responses and quantitative changes in the producer price index described in Section 2.2. According to this, aggregate producer prices in the manufacturing and service sectors prone to the NMW increased by approximately 0.26% in response to the NMW. Given that the NMW increased overall costs in the sectors under consideration by approximately 0.31%, the degree of price pass-through of the NMW is substantial, i.e., a 1%-increase in overall costs due to the NMW came along with a price increase by 0.85%. Of course, the results of the back-of-the-envelope calculation should be taken with a grain of salt given its assumptions and simplifications. Nevertheless, the approximation strongly suggests that the size of the price effect is non-negligible and quantitatively important.

The NMW effect on pricing plans mostly stems from additional price increases. Table A.2 in the Online Appendix summarizes the results of estimating Model (2) separately for binarized dependent variables that either capture planned increases in price or refer to planned changes that are non-negative. As displayed in Columns (2) and (3), more than 86% of the price effect can be attributed to additionally reported planned price increases. In turn, only a small part of the effect is due to firms refraining from price cuts that otherwise would have taken place. For the sake of readability, I hence refer to "price increases" in response to the NMW when discussing results based on the trichotomous dependent variable in the remainder of the paper.

Notably, the NMW effect on *realized* price changes is comparably strong among the subset of manufacturing firms for whom this information is available, see Column (1) of Panel B in Table 2. The estimated treatment effect of $\hat{\beta} = 0.29$ ($p < 0.01$) means that a firm affected by

degree TI_{sr} reported additional price increases in $7 \times TI_{sr}$ months between October 2014 and September 2016 due to the NMW. The estimated effect on realized prices is thus remarkably close to the NMW effect on planned price changes. In light of less variation in price changes realized in the previous month compared to three-month ahead price plans, it is not surprising that the point estimate is slightly smaller in the former case. As shown in the back-of-the-envelope calculation in Online Appendix D.1, the quantitative size of the NMW effect on the overall level of producer prices in manufacturing is in the same order of magnitude irrespective of the price data used in the estimation. Hence, firms appear to have reacted to the NMW in accordance with their previous pricing plans.

NMW Effect on Employment Changes In contrast to the price effect, the relationship between planned employment changes and the NMW bite is much weaker. The average treatment effect on planned employment changes reported between July 2014 and June 2016 is only slightly negative ($\hat{\beta} = -0.05$; see Column (2) of Panel A in Table 2). Again, the coefficient of the NMW effect is comparable if it is estimated based on *realized* employment changes among services firms for which this information is available (Column (2) of Panel B). Moreover, the slightly negative effect is exclusively stemming from planned reductions in employment instead of fewer increases (Appendix Table A.2). Taking coefficients at face value, affected firms were thus more than seven times more likely to increase prices instead of reducing employment in response to the NMW.

The estimated employment effect is statistically indistinguishable from zero ($p = 0.38$). Accordingly, different specifications of Model (2) usually deliver slightly negative, but insignificant employment effects as shown in Table A.3 in the Online Appendix. Estimating the effect separately for each quarter of the treatment period in Appendix Table A.1 provides a similar picture: the association between TI_i and planned employment changes is negative in face value in seven out of eight quarters, but it is significantly different from zero only in the fourth quarter of 2014 and the third quarter of 2015. Considering potential measurement error in the bite measure and resulting attenuation bias, a negative employment reaction thus cannot be ruled out with certainty.

As the same argument also applies to the estimated price effect, however, the relative magnitude of price and employment effects should not be strongly affected by measurement error in TI_{sr}.

NMW Effect on Other Firm-Specific Variables Further, the NMW does not appear to have had a strong effect on firms' revenues and demand.[23] First, neither firms' expected business conditions for the next six months ($\Delta\text{Cond.}_{i,t}^{+6m}$) nor their current conditions ($\text{Cond.}_{i,t}$) have significantly deteriorated due to the NMW, see Column (3) of Panels A and B in Table 2. Second, neither firms' three-month ahead expected changes in production or demand ($\Delta\text{Prod./Dem.}_{i,t}^{+3m}$), nor their current backlog of orders ($\text{Orders}_{i,t}$) were negatively affected by the NMW, see Column (4). Importantly, these results suggest that potential general equilibrium effects through an NMW effect on firms' demand are unlikely to be a main driver of firms' price and employment adjustments.

4.3. Firm-Level Relationship between Price and Employment Response

Next, I examine whether and how firms' adjustments along the margins of prices and employment are interrelated: Do firms that increase prices more often in response to a given NMW bite also cut employment more often (negative relationship)? Or is the employment response of these firms muted, i.e., less negative, relative to firms that face the same NMW bite but increase prices more frequently (positive relationship)? The IBS data allow to provide direct firm-level evidence on this joint relationship as it contains information about planned adjustments in prices and employment at the same point in time.

The results point at a positive firm-level relationship between both margins of adjustment. Panel A of Table 3 summarizes the separately estimated effects of the NMW bite on each combined indicator, which groups the observations according to the joint price and employment responses as described in Section 3.2, net of controls. The relationship between the NMW bite and the frequency that firms adjusted along both margins, i.e., planned to increase prices and to cut employment, is significantly positive ($\hat{\beta} = 0.09$, $p < 0.05$). Notably, the effect of NMW bite is more than twice

[23] $\Delta\text{Cond.}_{i,t}^{+6m}$ and $\text{Cond.}_{i,t}$ are very closely related to revenues (Link, 2020). Further, the expected change in production is only asked in the manufacturing IBS, while service firms are asked about expected changes in demand.

Table 3: Firm-Level Relationship between Price and Employment Response to NMW Introduction

Panel A: Simultaneous Effects

Price Response	Empl. Response [+;0]	Empl. Response [−]	
[+]	0.22***	0.09**	0.32***
	(0.06)	(0.04)	(0.05)
[0;−]	-0.28***	-0.04	-0.31***
	(0.04)	(0.04)	(0.05)
	-0.05	0.05	
	(0.05)	(0.04)	

Panel B: Pooled Treatment & Control Periods

Price Response	Empl. Response ≥ 0	Empl. Response < 0	
> 0	0.44***	0.06	0.50***
	(0.11)	(0.11)	(0.15)
≤ 0	-0.52***	0.02	-0.50***
	(0.13)	(0.11)	(0.15)
	-0.08	0.08	
	(0.05)	(0.05)	

Notes: This table documents the joint effect of the NMW on firms' price and employment plans. Each coefficient originates from a *separate* regression where the dependent variable—i.e., an indicator that is one for the respective combination of price and employment plans ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$) as indicated for each cell—is regressed on $TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$, i.e., the bite of the NMW during the treatment period. Panel A estimates the simultaneous effect on the full sample ($N = 239,019$) controlling for firm fixed effects and time fixed effects at the level of two-digit industries. Panel B pools the data of the control period (2011m4-2014m6) and the treatment period (2014m7-2016m6) at the firm level (resulting in 8,274 firm*period observations) and controls for firm fixed effects and period fixed effects at the level of two-digit industries. Standard errors are clustered at the levels of sectors, counties, and dates (Panel A)/periods (Panel B). Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

as large with respect to the frequency that firms planned to increase prices *without* concurrently planning to decrease the number of employees ($\hat{\beta} = 0.22$, $p < 0.01$). In turn, a higher NMW bite is associated with a lower frequency that firms planned to neither increase prices nor cut employment ($\hat{\beta} = -0.28$, $p < 0.01$), while the relationship between the NMW bite and the frequency to plan to decrease employment without raising prices is insignificant. As shown in Table A.4 in the Online Appendix, the results are virtually unchanged if the regressions further control for firms' current backlog of orders and expected three-month ahead change in demand or production, which themselves are potential determinants of pricing and employment decisions.

An obvious concern with this approach is that firms' actions in response to the NMW do not necessarily take place simultaneously. For instance, a firm's decision to increase prices due to the NMW at the beginning of the treatment period might affect its employment and pricing decisions in the subsequent months. As described in Section 3.2, this is addressed by pooling firms' responses during the control and treatment period before defining indicators on their joint response, which then capture, e.g., whether a firm planned more price increases than decreases and more

employment cuts than increases, etc.

The results based on the pooled sample again reveal a positive relationship between both margins of adjustment, see Panel B of Table 3. If firms respond to the NMW by increasing their prices more often than decreasing them, this usually goes in hand with a non-negative employment response. Moreover, if firms do not cut at the employment margin, firms facing a higher NMW bite increase prices more often and while the frequency of constant and decreasing prices declines. Again, the results are comparable if the regressions additionally control for firms' current backlog of orders and expected future change in demand or production, see Panel B of Appendix Table A.4.

The positive firm-level relationship between the price and employment responses also becomes apparent using an alternative approach presented in Online Appendix D.2. Here, firms that, for a given NMW bite, reacted with fewer cuts in employment compared to other firms (identified via a positive firm-specific mean of residuals from the estimation Model (2) with $\Delta\text{Empl.}_{t}^{+3m}$ as dependent variable), are shown to have also reacted more strongly along the pricing margin and vice versa. In turn, the relationship between the NMW bite and firms' employment plans is strongly significantly negative for firms that reacted relatively less strongly along the pricing margin, while it is slightly positive among those firms that planned to increase prices more often than predicted by Model (2).

Taken together, the results show that firms' price and employment responses to the NMW are strongly related to each other. Firms that increase prices more often in response to a given NMW bite also show a more muted, i.e., less negative employment response relative to firms that face the same NMW bite but react less strongly along the pricing margin. Hence, the decision to adjust the number of employees appears to heavily depend on the question whether a firm is able or willing to increase prices in response to minimum wages or not.

As demonstrated by Clemens (2021), this positive relationship is consistent with a standard textbook model of perfectly competitive labor markets that is augmented by the possibility that firms increase prices in response to the minimum wage: the smaller the elasticity of demand, the larger the scope for firms to pass through the additional costs associated with the minimum wage

to their customers and the stronger is the resulting rightward shift of the labor demand curve that counteracts the disemployment effect.

4.4. Heterogeneity

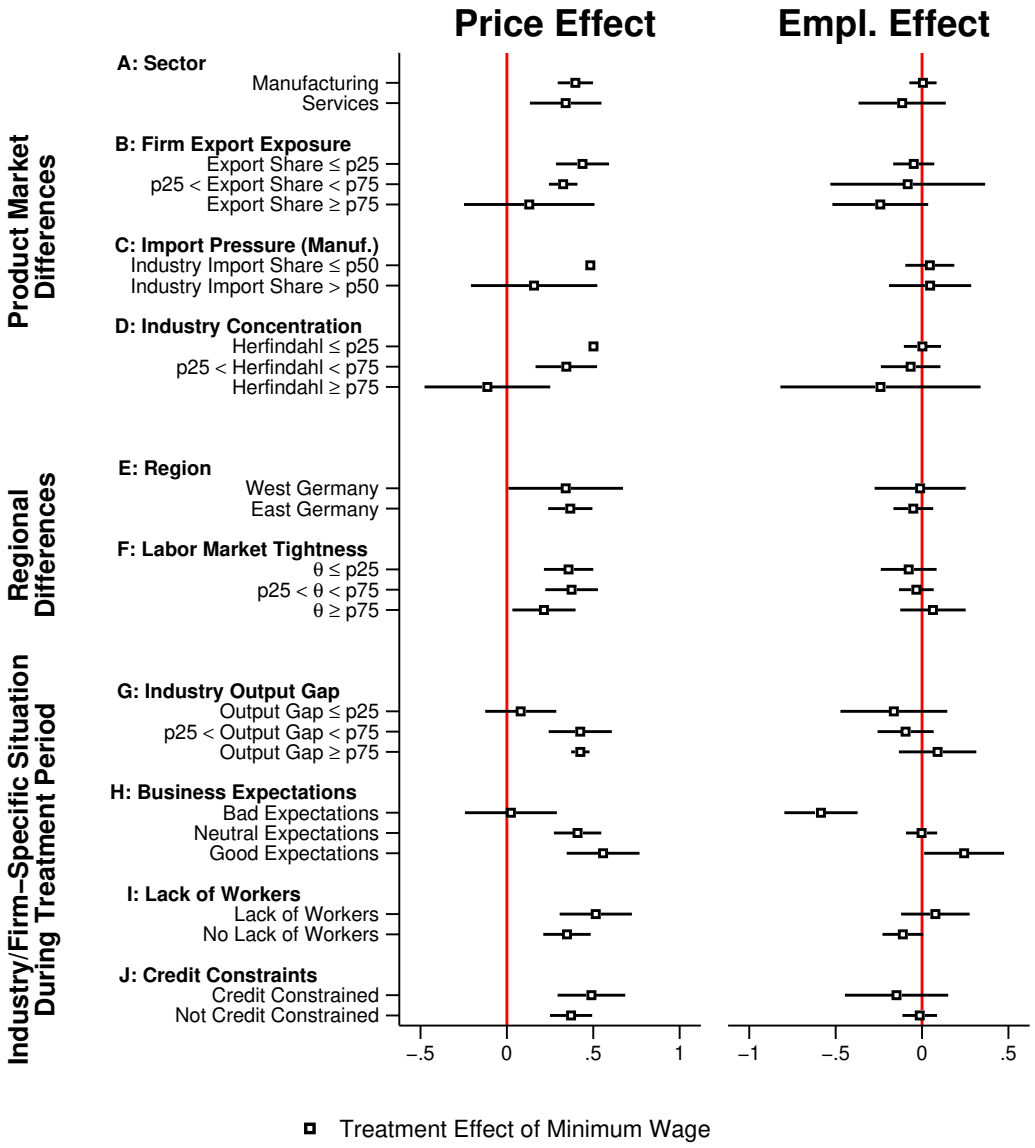
Next, I investigate the role different potential sources of heterogeneity play for the relative strength of firms' price and employment responses. In line with the evidence presented in Section 4.3, the results of the heterogeneity analysis indicate that those groups of firms that, for a given NMW bite, react more frequently along the pricing margin also tend to report employment cuts less often. To this end, the bite TI_{sr} is interacted with indicators that groups firms according to characteristics of the product markets and regions they operate in as well as according to the firm-specific or industry-specific business situation they face during the treatment period.[24] Figure 2 summarizes the results, while Tables A.5 and A.6 in the Online Appendix provide the corresponding regression output including statistical tests on the difference of treatment effects across groups.

Product Market Differences First, Panel A of Figure 2 shows that the price and employment responses of manufacturing firms were comparable to those of services companies. In both sectors, affected firms planned to increase prices significantly more often than they would have done in absence of the NMW. For a given bite, manufacturing firms appear to have planned price increases, if anything, slightly more often due to the NMW compared to services companies. In contrast, services firms appear to have reacted slightly more strongly—albeit still insignificantly—along the employment margin. Yet, the differences between the coefficients estimated in both samples are small and insignificant in either case ($p = 0.63$ and $p = 0.40$, respectively).

Second, the more firms export, the less frequently they increased prices in response to the NMW for a given bite. Panel B groups treated firms into quartiles with respect to the export share elicited

[24] The fact that some splits use firm-level variables while TI_{sr} varies at the level of sector-region cells might raise the concern of biased results. If this selection issue played a relevant role, however, this should bias the estimated price and employment effects in different directions. As the relationship between these responses across different groups is usually found to be positive, this issue hence does not seem to be too problematic. Table B.2 in the Online Appendix lists the industry-level import shares, Herfindahl indices, and output gap used.

Figure 2: Heterogeneity in Price and Employment Response of Firms to NMW



Notes: The figure plots the coefficients of the treatment effect of the NMW on planned price or employment changes in different groups of treated firms as indicated in each row. Each panel reports the results of a separate regression of $\Delta\text{Price}_t^{+3m}$ or $\Delta\text{Empl.}_t^{+3m}$ on an interaction of "$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$" with dummies that group treated firms into bins according to their sector (manufacturing vs. services), firms' export share, industry-specific import share, industry-specific Herfindahl index, region, county-level labor market tightness (i.e., vacancy-unemployment ratio), the output gap in 2015 at the level of two-digit industries (i.e., the deviation of sectoral gross value added from its HP-filtered trend), firms' average business expectations for the next six months during the treatment period ("Good Expectations": average expectation between $1/3$ and 1; "Bad Expectations": between $-1/3$ and -1), firms' self-reported lack of workers in January 2015, and firms reporting to be credit constrained in a special IBS question in June 2016. Panels B, I, and J omit the coefficients for firms that did not report to the respective survey question. Each estimation controls for time fixed effects at the level of two-digit industries and firm fixed effects. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Confidence intervals are displayed at the 95% level. The complete regression output is displayed in Tables A.5 and A.6 in the Online Appendix.

in a supplementary survey question in the IBS wave of September 2018.[25] The price response is strongest among the firms with the lowest export share, while the treatment effect is only insignificantly positive in the group that exported most. The difference between the coefficients of these groups is significant at the 10% level. On the other side, the disemployment effect of the NMW bite is strongest for treated firms in the group with the highest export share ($p < 0.1$).

Third, industry-specific import pressure is associated with a dampened price response in the manufacturing subsample. Panel C reveals that the price effect is only significantly positive for treated manufacturers that face below-median import pressure defined as the ratio of imports over revenues in each two-digit industry in 2014.[26] In contrast, firms in industries with high import pressure did not show significantly appreciated pricing plans in response to the NMW. While import pressure appears to be important for the size of the price effect, a systematic relationship between firms' employment response and import pressure cannot be detected.

Lastly, firms reacted more strongly along the pricing margin if they operated in less concentrated industries. Panel D groups treated firms into quartiles with respect to industry-specific Herfindahl indices.[27] The Herfindahl index proxies the degree of tradability of goods and services as industries that meet mostly local demand are more uniformly distributed, while those relying on national or international customers tend to be geographically concentrated (e.g., Mian and Sufi, 2014). The results show that the price effect is strongest for firms in the lconcentrated industries and insignificant for firms in the most concentrated industries. The differences between the treatment effect of firms in the most and lconcentrated markets is significant at the 1% level. Moreover, the relationship between firms' employment response and market concentration is slightly negative, but insignificant. Thus, my results are less ambiguous compared to Harasztosi and Lindner (2019) who only find a small negative, but insignificant effect of geographical concentration on the revenue elasticity of a large NMW increase in Hungary, which is their proxy for price changes.

[25] 60.4% of the firms in the treatment period can be assigned to their export share as of September 2018; the remaining firms are captured by a dummy variable $\mathbb{1}(\text{Firm Export Share} = NA)$.

[26] Import statistics are obtained from the Federal Statistical Office and are only available for manufacturing industries. The analysis is restricted to median splits as there are less than 350 manufacturing firms in the IBS with $TI_{sr} > 0$.

[27] I calculate the Herfindahl index based on county-level employment data in 2014 contained in the "Business Register Data" provided at the Research Data Center of the Statistical Offices of the Federal States (FDZ, 2014).

Taken together, the results show that price effects are stronger (and the employment response muted to some degree) when firms' export share is smaller, competition through imports is lower, and their goods and service are traded more locally. This is consistent with the theoretical result of Weyl and Fabinger (2013) who show that competition is a key determinant of pass-through as these firms are less likely to face foreign or domestic competitors that are either unaffected or hit less strongly by the German NMW. Consequently, these firms arguably face a smaller elasticity of demand for their goods or services and thus have a larger scope to increase prices in response to the NMW without experiencing a strong decline in demand (c.f., Clemens, 2021).

Regional Differences Next, I investigate the heterogeneity in responses to the NMW depending on regional differences. First, the sample is split between firms located in Western and Eastern Germany. Even though the NMW bite is on average higher in Eastern Germany, the results presented in Panel E suggest that the relationship between the NMW bite and firms' pricing and employment plans are comparable and statistically indistinguishable across both regions.

Instead, the degree of firms' employment response is correlated with local labor market conditions at the time of the NMW introduction; see Panel F. Given the same degree of affectedness, the disemployment effect is strongest for firms in counties with the most slack in the labor market according to the ratio of total vacancies over the number of unemployed. While the respective coefficients themselves are not statistically different from zero, the employment responses of firms in slack labor markets are significantly more negative compared to the group of treated firms located in the tightest local labor markets ($p < 0.05$). In turn, the price responses of firms in slack local labor markets are statistically indistinguishable from those located in tighter labor markets.

Heterogeneity in Industry- and Firm-Specific Situation During Treatment Period Moreover, there is substantial heterogeneity in firms' responses to the NMW depending on the specific economic situation they faced at the time of its introduction. To start with, firms are sorted with

respect to the output gap in their two-digit industry in 2015.[28] Results in Panel G show that firms in industries with the lowest output gap did not increase prices more often in response to the NMW, while the strong positive relationship between the bite and the probability to increase prices is roughly the same in both other groups. The difference in estimated coefficients between the lowest and highest quartile is statistically highly significant ($p < 0.01$). In turn, firms operating in industries that were more underutilized appear to have reacted more negatively along the employment margin, albeit both the face values of the estimated employment effects and the difference in coefficients between the lowest and highest category is statistically insignificant ($p = 0.21$).

Relatedly, Panel H provides evidence that the reaction of firms to the NMW strongly depended on their expectations regarding the general development of their businesses during the treatment period.[29] While the NMW bite was unrelated to firms' general business expectations (see Table 2), treated firms with on average good expectations regarding the future development of their businesses reacted much more strongly along the pricing margin. In contrast, their counterparts with grim expectations reduced the number of employees significantly more often in response to the NMW bite, while the price effect among these firms is indistinguishable from zero.

Lastly, Panels I and J of Figure 2 depict the heterogeneity in treatment effects depending on whether firms reported to face a lack of workers or to be credit constrained.[30] The employment response is heterogeneous with respect to firms' self-reported lack of workers during the period of the NMW introduction: the relationship between the NMW bite and firms' employment plans is significantly negative among the group of firms that was not constrained by labor shortages ($p < 0.1$). Given the same bite, these firms were significantly more likely to report depreciated

[28] The industry-specific output gap is calculated by applying an HP-filter to annual time series data of sector gross value added obtained from the Federal Statistical Office (Destatis code 81000-0103).

[29] The IBS asks firms whether their expected business conditions during the next six months are [1] "more favorable," [0] "about the same," or [-1] "more unfavorable". Firms are grouped as "optimistic" or "pessimistic" if their average reports during the treatment period were above 1/3 and below -1/3, respectively. Roughly 20% of treated firms are labeled "optimistic", while less than 10% of firms are "pessimistic".

[30] In January 2015, the IBS elicited whether firms' business activity was constrained by a lack of skilled workers. This is clearly only an imperfect proxy for the unsatisfied demand for low-wage workers prone to the NMW as those are usually less skilled. However, shortages with respect to both types of workers are likely to be correlated. In June 2016, firms were asked whether they had conducted loan negotiations in the past 12 months and whether the approved loan volume was as desired. See Online Appendix E for the wording of the questions.

employment plans compared to their counterparts that faced difficulties in satisfying their demand for workers. Instead, the later firms appear to have increased prices more frequently in response to the NMW. However, the difference in estimated price effects is only close to approaching statistical significance ($p = 0.13$). In contrast, heterogeneity in credit constraints only appears to play a minor role: Credit-constrained firms appear to have reacted slightly more strongly along both margins of adjustment, but the differences are statistically insignificant.

Taken together, the relative importance of the different margins of adjustment appears to not only depend on the degree of product market competition, but also on the specific economic situation firms face at the time of the NMW introduction. Hence, my results suggest that the overall disemployment effect might have been more negative and the price effect weaker if the NMW had been introduced during a period of less favorable macroeconomic conditions. This provides a new and coherent perspective on state-dependence of minimum wage effects that, to my knowledge, could so far only be inferred indirectly and was limited to the comparison of estimates across different settings studied in separate papers.

4.5. Additional Insights and Robustness

Along with providing additional insights, this section conducts several robustness checks that confirm the findings regarding firms' adjustment of pricing and employment plans to the NMW. Again, all results are comparable if the analyses were conducted with respect to realized price changes in the subsample of manufacturing firms, see Table A.7 in the Online Appendix.

First, the results do not change once controlling for sample attrition. If dropout of firms from the sample was correlated with the NMW bite, the baseline results could be biased. To accommodate this concern, the sample is restricted to firms that responded to the IBS until the end of the treatment period. As shown in Column (2) of Table 4, the estimated NMW effects on firms' planned price and employment changes are virtually the same as in the baseline regression depicted in Column (1).

Second, the results are robust to the choice of the aggregation level in the RS wage data used for the construction of the bite measure. As described in Section 3.1, 52% (84%) of firms in the

Table 4: Price and Employment Response to the Minimum Wage Introduction: Robustness

Specification	Baseline	No Attrition	RS Wage Data			SES Wage Data	
			Region Only	County Only	2013 Wages	Fulltime Workers	All Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Planned Price Change in Next 3 Months							
$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	0.38***	0.35***	0.51***	0.31***	0.35***	0.22***
	(0.059)	(0.053)	(0.061)	(0.082)	(0.053)	(0.057)	(0.070)
R^2	0.324	0.319	0.324	0.329	0.325	0.322	0.322
Observations	240211	210711	240211	147915	240543	139641	139641
Panel B: Planned Change in Number of Employees in Next 3 Months							
$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	-0.054	-0.049	-0.047	-0.086	-0.048	-0.068	-0.075
	(0.061)	(0.056)	(0.068)	(0.11)	(0.057)	(0.093)	(0.073)
R^2	0.321	0.313	0.321	0.337	0.322	0.331	0.331
Observations	240331	210811	240331	148039	240669	139773	139773
Firm FE	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes

Notes: The dependent variables are the planned price changes (Panel A) and planned employment changes (Panel B) during the following three months reported to the IBS. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "Time*Sector FE" are time fixed effects specific to two-digit industries. Column (2) is restricted to firms that remain in the IBS until the end of the treatment period. Columns (3) and (4) calculate the NMW bite based on 2014 wage data from the Remuneration Statistic only at the level of labor market regions and counties, respectively. Column (5) uses the wages as of 2013. Columns (6) and (7) calculate the bite based on wage data from the Structure of Earnings Survey for fulltime jobs only or all employees, respectively. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

sample can be matched to administrative, industry-specific wage data at the level of the county (labor market region) they are located in. Trading off the higher coverage of wage data at the level of labor market regions and the fact that firm-level wages are better reflected by county level data, the baseline specification uses county-level wage data once available and replaces missing values by data at the more aggregated level of labor market regions. If wage data at the level of labor market regions are used to determine TI_{sr} for all firms instead, the results do not change substantially; see Column (3).

Unsurprisingly, the estimated NMW effects are stronger when the construction of TI_{sr} is restricted to industry-specific wage data at the county level. Capturing the actual bite with less measurement error reduces the attenuation bias in the estimated coefficient. Regarding the price reaction of firms, the treatment effect is estimated to $\hat{\beta} = 0.51$; see Column (4). For the case

of planned employment adjustments, the estimated treatment effect increases in absolute value to $\hat{\beta} = -0.09$. Although the estimate is still insignificant, I cannot rule out that firms' employment reaction would be estimated to be significantly negative if the firm-level NMW bite was observed without measurement error. Hence, the results of the baseline specification are likely to reflect a lower bound of firms' price and employment reaction to the NMW.

Third, using the share of full-time employees who earned less than €8.50 per hour in 2013 instead of in 2014 as bite measure does not alter the results. As shown in Column (5), the coefficients of the treatment effects are slightly smaller in absolute value but are in the same order of magnitude compared to the baseline specification. This can be attributed to the fact that wages have increased irrespective of the NMW in 2014. Hence, there is larger measurement error and more variation in TI_{sr} when using 2013 wages for the identification of the bite both resulting in coefficients that are slightly smaller in absolute values.

Fourth, the documented responses are robust to the use of alternative bite measures based on the 2014 wave of the Structure of Earnings Survey. On the one hand, the SES allows to construct a bite measure along the same lines as TI_{sr} capturing the fraction of affected full-time employees ($TI_{sr}^{SES,FT}$). On the other hand, the bite can be calculated based on the fraction of all affected employees, including part-time employees and marginally employed workers ($TI_{sr}^{SES,all}$). However, the SES data have at least two disadvantages: first, they are not representative at the level of LMR-industry cells (or below), while the RS covers the universe of full-time employees. Second, $TI_{sr}^{SES,FT}$ and $TI_{sr}^{SES,all}$ can only be constructed for less than half of the manufacturing and service firms in the IBS sample even when the SES data are required to cover only a minimum of 100 employees per LMR-industry cell. Still, the coefficients of the NMW effect on price and employment expectations are virtually unchanged when using $TI_{sr}^{SES,FT}$ as bite measure; see Column (6).

Moreover, abstracting from firms that were affected by the NMW only through higher wage costs for part-time employees or marginally employed workers does not appear to sweep relevant variation under the rug. Using the fraction of all workers—including part-time and marginally employed workers—who earned below the NMW in 2014 as captured by $TI_{sr}^{SES,all}$, again uncovers

a strongly positive price effect; see Column (7). By construction, the coefficient of the price effect is smaller compared to the baseline specification given the larger variation in the bite measure. The treatment effect on planned employment changes is again slightly negative but insignificant. This is consistent with the high correlation between the bite measures presented in Table 1 and additional evidence presented in Appendix D.3 that assigns a positive NMW bite to firms that operate in industry-region cells where all full-time employees earned wages slightly above the NMW prior to its introduction, but in which some part-time employees or marginally employed might have been treated. The results indicate that the price effect is driven by firms that were already captured by the baseline bite measure.

Fifth, Table A.3 in the Online Appendix documents that the estimated NMW effect on firms' pricing plans is constant for different specifications of the control vector and does not hinge on the functional form of Model (2). Including time fixed effects specific for each county type or time fixed effects at the level of federal states to the regression does not affect the coefficients; see Columns (2) and (3). These specifications flexibly control for potential heterogeneous trends in more rural or more urban counties and shocks that heterogeneously affect firms in different federal states, respectively. Moreover, neither controlling for the firm-specific backlog of orders as a proxy for current demand in Column (4), nor controlling for expected changes in demand (for service companies) or production (for manufacturing firms) in Column (5) affects the results. Finally, the results are robust to the inclusion of a squared term of TI_{sr}, which enters insignificantly and does not affect the coefficient of the linear term of TI_{sr}; see Column (6).

Lastly, the empirical results would be biased if firms were not directly subject to the NMW ($TI_{i \in sr} = 0$) but were affected via spillover effects. The SUTVA is highly likely to hold in the case of the German NMW due to several reasons. First, existing studies do not speak in favor of strong wage spillovers of the German NMW introduction on higher wage segments (e.g., Ahlfeldt et al., 2018; Bossler and Gerner, 2020; Caliendo et al., 2022; and Dustmann et al., 2022). Second, general equilibrium effects on unaffected firms are arguably limited as the NMW introduction corresponded to an increase in the economy-wide wage bill of only 0.43%, *ceteris paribus* (Destatis,

2016). Third, even if there was sizable reallocation of workers from small, low paying firms to large, high-paying firms as documented by Dustmann et al. (2022), the fact that treatment effects are identified from industry-region level variation reduces the possibility that worker reallocation violates the SUTVA. In addition, omitting those firms from the control group that paid wages slightly above the NMW prior to the reform does not alter the results. Table A.8 in the Online Appendix shows that the estimated price and employment responses of affected firms are not different when omitting firms with $TI_{sr} = 0$ that would have been affected by a hypothetical wage floor at levels as high as $\bar{w}^{alt} \in \{€10,€12,€15\}$. Taken together, this suggests that spillovers or worker reallocation should not bias the results.

5. Conclusion

Exploiting the unique setting of the introduction of a nation-wide minimum wage floor in Germany in 2015, this paper provides several novel insights regarding the effect of minimum wages on the price setting behavior of firms and its relationship to adjustments along the employment margin. First, affected firms, if anything, only weakly responded to the NMW introduction by decreasing the number of employees. Instead, they increased prices more frequently and at high speed to absorb the increase in the wage bill. This price pass-through is a widespread phenomenon across large parts of the economy. Hence, the paper generalizes the findings of previous studies on price effects of minimum wages that usually limit the analysis to highly affected industries such as restaurants and retailers.

Second, firms' price and employment responses to the NMW are strongly related to each other: Firms that increased prices more often in response to a given NMW bite also showed a more muted, i.e., less negative employment response than firms that—all else equal—reacted less strongly along the pricing margin. In line with this, the estimated price effect is stronger—and the employment response slightly muted—among firms that arguably face weaker product market competition.

This evidence on the interplay between firms' adjustment along the price and employment margins has important implications for the general understanding of the employment response to min-

imum wages. The small effect of minimum wages has often been associated with the importance of monopsony behavior (see, e.g., Bhaskar et al., 2002 for a survey) or the role of informational frictions or search frictions in the labor market (e.g, van den Berg, 2003; Dube et al., 2016). In light of the relatively restrictive labor market institutions and employment protection in Germany, my results cannot rule out that labor market frictions partly explain the weak effect of the German minimum wage on employment as proposed by Blömer et al. (2020). However, Aaronson and French (2007) and Aaronson et al. (2008) show that the monopsony model cannot explain strong price rises after minimum wage increases. Instead, the documented positive relationship in the choice of the adjustment margin is in line with the predictions of Clemens (2021)'s standard textbook model of perfectly competitive labor markets that is augmented by imperfect product competition allowing firms to increase prices in response to the minimum wage.

Moreover, the results of the paper suggest that the specific economic situation firms face at the time of the NMW introduction is important for their choice of the adjustment margin. Firms that, at the time of the NMW introduction, were optimistic regarding the future development of their businesses or were operating in industries with more positive output gaps reacted more frequently along the pricing margin. In turn, those firms that did not report a shortage of labor or were located in regions with more slack in the labor market were more likely to react along the employment margin. This suggests that the disemployment effect might have been more negative if the NMW had not been introduced during a period of economic boom. Given that evidence on state dependence in minimum wage effects so far has been limited to comparing estimates across different settings studied in separate papers, a deeper investigation of how economic conditions matter for firms' reaction to minimum wages might constitute a fruitful avenue for future research. Taken together, the findings of this paper suggest that a joint assessment of different potential adjustment channels, which is not limited to the employment margin, is important to gain a comprehensive understanding of firms' responses to minimum wages.

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The Price and Employment Response of Firms to the Introduction of Minimum Wages

Sebastian Link

ONLINE APPENDIX

A. Supplementary Tables

Table A.1: Timing of Price and Employment Response of Firms to the NMW Introduction: Yearly and Quarterly Treatment Coefficients

	$\Delta\text{Price}_t^{+3m}$		$\Delta\text{Empl.}_t^{+3m}$	
	(1)	(2)	(3)	(4)
Panel A: Yearly Treatment Effects				
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2015m6\})$	0.49***		-0.06	
	(0.08)		(0.07)	
$\text{TI} \times \mathbb{1}(t \in \{2015m7, 2016m6\})$	0.24***		-0.05	
	(0.05)		(0.06)	
Panel B: Quarterly Treatment Effects				
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2014m9\})$		0.27**		-0.01
		(0.13)		(0.10)
$\text{TI} \times \mathbb{1}(t \in \{2014m10, 2014m12\})$		0.42***		-0.18*
		(0.14)		(0.09)
$\text{TI} \times \mathbb{1}(t \in \{2015m1, 2015m3\})$		0.70***		0.00
		(0.11)		(0.11)
$\text{TI} \times \mathbb{1}(t \in \{2015m4, 2015m6\})$		0.57***		-0.05
		(0.08)		(0.11)
$\text{TI} \times \mathbb{1}(t \in \{2015m7, 2015m9\})$		0.27**		-0.15*
		(0.12)		(0.08)
$\text{TI} \times \mathbb{1}(t \in \{2015m10, 2015m12\})$		0.19		-0.07
		(0.14)		(0.10)
$\text{TI} \times \mathbb{1}(t \in \{2016m1, 2016m3\})$		0.31***		-0.05
		(0.07)		(0.13)
$\text{TI} \times \mathbb{1}(t \in \{2016m4, 2016m6\})$		0.18**		0.08
		(0.08)		(0.12)
Firm FE	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes
R^2	0.325	0.325	0.321	0.321
Observations	240211	240211	240331	240331

Notes: The dependent variables are planned price or employment changes during the following three months ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$). "*TI*" is the NMW bite that is interacted with indicators for the respective time periods. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Minimum Wage Effects at the Firm-Level: Binarized Outcome Variables

	$\Delta\text{Price}_t^{+3m}$			$\Delta\text{Empl.}_t^{+3m}$		
	Baseline	$=1$	$\neq -1$	Baseline	$=1$	$\neq -1$
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	0.32***	0.05**	-0.05	-0.00	-0.05
	(0.06)	(0.05)	(0.02)	(0.06)	(0.03)	(0.05)
Firm FE	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes
R^2	0.324	0.297	0.338	0.321	0.324	0.270
Observations	240211	240211	240211	240331	240331	240331

Notes: The dependent variables are planned price or employment changes during the following three months as reported to the IBS. In Columns (2) and (5), the dependent variables are binarized to capture planned increases only ($\mathbb{1}(Y_t^{+3m} = 1)$). In Columns (3) and (6), the dependent variables are restricted to planned changes that are non-negative ($\mathbb{1}(Y_t^{+3m} \neq -1)$). "*TI*" is the bite of the NMW and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the level of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Minimum Wage Effects at the Firm-Level: Different Control Vector & Functional Form

	$\Delta\text{Price}_t^{+3m}$						$\Delta\text{Empl.}_t^{+3m}$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	0.40***	0.35***	0.37***	0.37***	0.35***	-0.05	-0.04	0.02	-0.05	-0.05	0.07
	(0.06)	(0.06)	(0.09)	(0.06)	(0.06)	(0.10)	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.08)
$\text{TI}^2 \times \mathbb{1}(t \in \{2014m7, 2016m6\})$						0.05						-0.28
						(0.20)						(0.19)
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time*County Type FE		yes						yes				
Time*State FE			yes						yes			
Control for Orders_t				yes						yes		
Control for $\Delta\text{Dem./Prod.}_t^{+3m}$					yes						yes	
R^2	0.324	0.325	0.327	0.324	0.324	0.324	0.321	0.322	0.324	0.321	0.321	0.321
Observations	240211	240211	240211	240211	240211	240211	240331	240331	240331	240331	240331	240331

Notes: The dependent variables are planned price or employment changes during the following three months reported to the IBS. "*TI*" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "$\text{Demand}_{i,t}$" and "$\Delta\text{Dem./Prod.}_{i,t}^{+3m}$" are firms' current backlog of orders and expected demand/production during the following three months, respectively. "Time*Sector FE," "Time*State FE," and "Time*County Type FE" are time fixed effects at the levels of two-digit industries, federal states, and county types, respectively. The Federal Office for Building and Regional Planning classifies counties into four categories: major cities, urban counties, rural counties, and sparsely populated rural counties. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Relationship b/w Price and Employment Response to NMW Introduction: Robustness

Panel A: Simultaneous Effects

Price Response	Empl. Response [+;0]	Empl. Response [-]	
[+]	0.21***	0.09**	0.31***
	(0.06)	(0.04)	(0.05)
[0;-]	-0.28***	-0.02	-0.30***
	(0.04)	(0.04)	(0.05)
	-0.07	0.07	
	(0.04)	(0.04)	

Panel B: Pooled Treatment & Control Periods

Price Response	Empl. Response ≥ 0	Empl. Response < 0	
> 0	0.41***	0.08	0.49***
	(0.11)	(0.11)	(0.15)
≤ 0	-0.54***	0.05	-0.49***
	(0.14)	(0.11)	(0.15)
	-0.13**	0.13**	
	(0.05)	(0.05)	

Notes: This table documents the joint effect of the NMW on firms' price and employment plans. Each coefficient originates from a *separate* regression where the dependent variable—that is an indicator that is one for the respective combination of price and employment plans ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$) as indicated for each cell—is regressed on $TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$, i.e., the bite of the NMW during the treatment period. Panel A estimates the simulatneous effect on the full sample ($N = 239,019$) controlling for firm fixed effects and time fixed effects at the level of two-digit industries. Panel B pools the data of the control period (2011m4-2014m6) and the treatment period (2014m7-2016m6) at the firm level (sample collapses to 8,274 firm*period observations) and controls for firm fixed effects and period fixed effects at the level of two-digit industries, and, in contrast to Table 3, for firms' current backlog of orders and expected change in demand (services) or production (manufacturing) in the next three months. Standard errors are three-way clustered at the levels of sectors, counties, and dates (Panel A)/periods (Panel B). Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Minimum Wage Effects: Heterogeneity between Different Markets and Regions

	$\Delta\text{Price}_t^{+3m}$						$\Delta\text{Empl.}_t^{+3m}$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TI $\times \mathbb{1}(t \in \{2014m7, 2016m6\})$												
$\times \mathbb{1}$(Manufacturing)	0.40***						0.01					
	(0.05)						(0.04)					
$\times \mathbb{1}$(Services)	0.34***						-0.11					
	(0.10)						(0.13)					
$\times \mathbb{1}$(Firm Export Share $\leq p25$)		0.44***						-0.05				
		(0.08)						(0.06)				
$\times \mathbb{1}(p25 <$ Firm Export Share $< p75)$		0.33***						-0.08				
		(0.04)						(0.22)				
$\times \mathbb{1}(p75 \leq$ Firm Export Share)		0.13						-0.24*				
		(0.19)						(0.14)				
$\times \mathbb{1}$(Firm Export Share $= NA$)		0.37***						-0.01				
		(0.07)						(0.05)				
$\times \mathbb{1}$(Industry Import Share $\leq p50$)			0.48***						0.05			
			(0.01)						(0.07)			
$\times \mathbb{1}$(Industry Import Share $> p50$)			0.16						0.05			
			(0.17)						(0.11)			
$\times \mathbb{1}$(Herfindahl $\leq p25$)				0.50***						0.00		
				(0.01)						(0.05)		
$\times \mathbb{1}(p25 <$ Herfindahl $< p75)$				0.34***						-0.07		
				(0.09)						(0.09)		
$\times \mathbb{1}(p75 \leq$ Herfindahl)				-0.11						-0.24		
				(0.18)						(0.29)		
$\times \mathbb{1}$(West Germany)					0.34**						-0.01	
					(0.17)						(0.13)	
$\times \mathbb{1}$(East Germany)					0.37***						-0.05	
					(0.06)						(0.06)	
$\times \mathbb{1}$(Labor Market Tightness $\leq p25$)						0.36***						-0.08
						(0.07)						(0.08)
$\times \mathbb{1}(p25 <$ Labor Market Tightness $< p75)$						0.37***						-0.03
						(0.08)						(0.05)
$\times \mathbb{1}(p75 \leq$ Labor Market Tightness)						0.21**						0.06
						(0.09)						(0.09)
H0: Coefficients Equal: p-value	0.633	0.068	0.094	0.001	0.827	0.239	0.397	0.101	0.99	0.418	0.761	0.046
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.324	0.325	0.280	0.325	0.324	0.324	0.321	0.321	0.297	0.321	0.321	0.321
Observations	240211	240211	120870	240211	240211	240211	240331	240331	120546	240331	240331	240331

Notes: This table presents the regression output underlying Panels A through F of Figure 2. The dependent variables are planned price or employment changes during the following three months. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. The product "$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$" is interacted with dummies that group treated firms into bins according to their sector (manufacturing vs. services), firms' export share, industry-specific import share, industry-specific Herfindahl index, region, and the labor market tightness (i.e., vacancy-unemployment ratio) in the county they are located in. "Time*Sector FE" are time fixed effects at the level of two-digit industries. The p-values at the bottom indicate whether the treatment effects in the highest group are statistically different from the lowest one. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Minimum Wage Effects: Heterogeneity in Industry/Firm-Specific Situation During Treatment Period

	$\Delta\text{Price}_t^{+3m}$				$\Delta\text{Empl.}_t^{+3m}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TI $\times \mathbb{1}(t \in \{2014m7, 2016m6\})$								
$\times \mathbb{1}(\text{Industry Output Gap} \leq p25)$	0.08				-0.16			
	(0.10)				(0.15)			
$\times \mathbb{1}(p25 < \text{Industry Output Gap} < p75)$	0.42***				-0.10			
	(0.09)				(0.08)			
$\times \mathbb{1}(p75 \leq \text{Industry Output Gap})$	0.43***				0.09			
	(0.03)				(0.11)			
$\times \mathbb{1}(\overline{\text{Business Exp.}} \in [-1, -1/3))$		0.02				-0.58***		
		(0.13)				(0.11)		
$\times \mathbb{1}(\overline{\text{Business Exp.}} \in [-1/3, 1/3])$		0.41***				-0.00		
		(0.07)				(0.05)		
$\times \mathbb{1}(\overline{\text{Business Exp.}} \in (1/3, 1]$		0.56***				0.24**		
		(0.11)				(0.12)		
$\times \mathbb{1}(\text{Lack of Workers} = 1)$			0.51***				0.08	
			(0.10)				(0.10)	
$\times \mathbb{1}(\text{Lack of Workers} = 0)$			0.35***				-0.11*	
			(0.07)				(0.06)	
$\times \mathbb{1}(\text{Lack of Workers} = NA)$			0.24**				0.10*	
			(0.11)				(0.05)	
$\times \mathbb{1}(\text{Credit Constrained} = 1)$				0.49***				-0.15
				(0.10)				(0.15)
$\times \mathbb{1}(\text{Credit Constrained} = 0)$				0.37***				-0.01
				(0.06)				(0.05)
$\times \mathbb{1}(\text{Credit Constrained} = NA)$				0.34***				-0.12*
				(0.07)				(0.07)
N	240211	238829	240211	240211	240331	238921	240331	240331
R^2	0.324	0.325	0.325	0.324	0.321	0.322	0.321	0.321
H0: Coefficients Equal: p-value	0.009	0.000	0.130	0.220	0.207	0.000	0.014	0.271
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: This table presents the regression output underlying Panels G through J of Figure 2. The dependent variables are planned price or employment changes during the following three months. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. The product "$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$" interacted with dummies that group treated firms into bins accoding to the output gap in 2015 at the level of two-digit industries (i.e., the deviation of sectoral gross value added from its HP-filtered trend), firms' average business expectations for the next six months during the treatment period ("Good Expectations": average expectation between $1/3$ and 1; "Bad Expectations": between $-1/3$ and -1), firms' self-reported lack of workers in January 2015, and firms reporting to be credit constrained in a special IBS question in June 2016. "Time*Sector FE" are time fixed effects at the level of two-digit industries. The p-values at the bottom indicate whether or not the treatment effects in the highest group are statistically different from the lowest one. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Minimum Wage Effect on Realized Price Changes: Robustness

	Realized Price Change During Previous Month (Manufacturing Only)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification	Baseline	No Attrition	RS Wage Data			SES Wage Data	
			Region Only	County Only	2013 Wages	Fulltime Workers	All Workers
$TI \times \mathbb{1}(t \in \{2014m10, 2016m9\})$	0.29***	0.29***	0.23**	0.46***	0.23***	0.33**	0.29*
	(0.058)	(0.057)	(0.091)	(0.12)	(0.064)	(0.15)	(0.15)
Firm FE	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes
R^2	0.271	0.260	0.271	0.261	0.272	0.256	0.256
Observations	136019	120056	136019	81407	136398	72546	72546

Notes: The dependent variable is the realized change in prices in the previous month as reported by manufacturing firms to the IBS. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Minimum Wage Effects at the Firm-Level: Test for SUTVA

	$\Delta\text{Price}_t^{+3m}$				$\Delta\text{Empl.}_t^{+3m}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		$\bar{w}^{alt}$				$\bar{w}^{alt}$		
Omit if $TI = 0$ & $TI(\bar{w}^{alt} > 0)$	Baseline	10€	12€	15€	Baseline	10€	12€	15€
TI $\times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	0.37***	0.38***	0.36***	-0.05	-0.02	-0.02	-0.05
	(0.06)	(0.07)	(0.08)	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.324	0.318	0.312	0.324	0.321	0.327	0.329	0.308
Observations	240211	194943	137135	86224	240331	195024	137156	86263

Notes: The dependent variables are planned price or employment changes during the following three months reported to the IBS. "TI" is the NMW bite, and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. In Columns (2) through (4) and (6) through (8) firms are dropped from the sample if they are unaffected according to the baseline bite measure, but would be affected by a hypothetical minimum wage of $\bar{w}^{alt}$, i.e., if $TI^{Baseline} = 0$ and $TI(\bar{w}^{alt}) > 0$. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B. Supplementary Material on Minimum Wage Bite Measure

B.1. Details on Construction of the Bite Measure

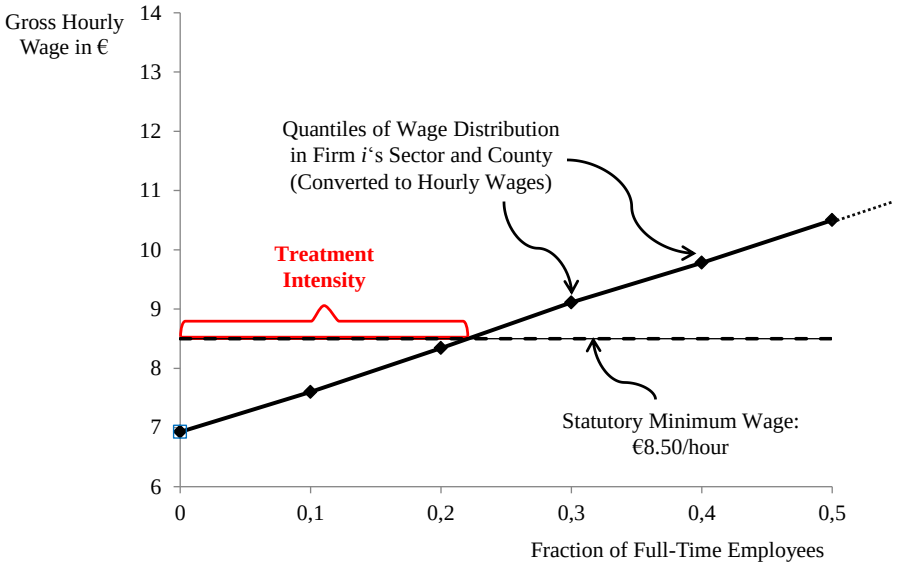
This appendix provides detailed information on the construction of the bite measure TI_{sr} summarized in Section 3.1. I obtained data on the distribution of gross monthly wages paid to full-time employees in each two-digit industry at the level of counties (NUTS-3-regions) as well as at the level of labor market regions (LMRs) in 2013 and 2014 from the Federal Employment Agency (2016). The data contains information on the following percentiles of the wage distribution in each sector-region cell: $p \in \{10, 20, 30, 40, 50, 60, 80\}$. As described in the main text, the baseline specification of TI_{sr} is based on the industry-specific wage distribution at the county-level of 2014 and missing values are replaced by wage data at the LMR-level. Robustness checks in Section 4.5 use wage data either at the level of counties or at the level of LMRs as well as TI_{sr} based on the 2013 wage distribution to show that the results do not hinge on these choices.

Monthly wages are converted to hourly wages by means of the number of paid working hours per month collected by the Quarterly Earnings Survey ("*Vierteljährliche Verdiensterhebung*"). This survey, conducted by the Statistical Offices of the Federal States, covers 40,500 German firms (7.4% of all firms) and is representative at the level of two-digit industries in both Eastern and Western Germany.[1] After calculating the average amount of monthly working hours in 2014 for each industry in Western and Eastern Germany, the monthly wages at each percentile are transformed to an hourly basis for each sector-region cell, i.e., to $w_{sr}(p)$ which denotes the p^{th} percentile of hourly wages in sector s and region r (counties or LMRs).

Then, the fraction of full-time employees who earned a gross hourly wage of less than €8.50 is calculated for each sector-region combination. Figure B.1 provides an illustration of the procedure that is based on two assumptions about the shape of the wage distribution: first, the wage level of employees between any two percentiles for which wage data are available is approximated by

[1] The quality of the data on working hours is perceived to be high as response to the survey is compulsory. The data is publicly available from the Federal Statistical Office and described in more detail here: https://www.destatis.de/EN/FactsFigures/NationalEconomyEnvironment/EarningsLabourCosts/Methods/QuarterlyEarningsSurvey.html.

Figure B.1: Illustration of the Identification of Firms' Affectedness by the Minimum Wage



Notes: This diagram illustrates the identification of firms' affectedness by the NMW indicated by the red line ("Treatment Intensity") for the example of firms in industry "55 Accommodation/Lodging" in county "09180 Garmisch-Partenkirchen." The black rhombi refer to the deciles of the wage distribution of full-time employees in 2014 after conversion to hourly wages. The wage levels between the deciles given in the data are linearly interpolated, while the values for the minimum and maximum of the wage distribution are calculated as described in the main text.

linear interpolation. Second, the wage level at the minimum of the wage distribution $w_{sr}(0)$ is assumed to be related to the wage at the 10^{th} percentile similarly as $w_{sr}(10)$ is related to $w_{sr}(20)$, i.e., $w_{sr}(0)/w_{sr}(10) = w_{sr}(10)/w_{sr}(20)$. Accordingly, the wage level at the maximum of the wage distribution is assumed to be $w_{sr}(100)/w_{sr}(80) = w_{sr}(80)/w_{sr}(60)$. Given these assumptions, the fraction of full-time employees who earned less than €8.50 per hour in 2014 in each sector-region cell—henceforth denoted as TI_{sr}—can be derived from the intercept theorem:

$$TI_{sr} = \begin{cases} 0 & \text{if } \bar{w}_{min} \leq w_{sr}(0) \\ p + 0.1 * \frac{\bar{w}_{min} - w_{sr}(p)}{w_{sr}(p+10) - w_{sr}(p)} & \text{if } w_{sr}(p) < \bar{w}_{min} \leq w_{sr}(p+10) \wedge p \in \{10,20,30,40,50\} \\ p + 0.2 * \frac{\bar{w}_{min} - w_{sr}(p)}{w_{sr}(p+20) - w_{sr}(p)} & \text{if } w_{sr}(p) < \bar{w}_{min} \leq w_{sr}(p+20) \wedge p \in \{60,80\} \\ 1 & \text{if } w_{sr}(100) \leq \bar{w}_{min} \end{cases} \tag{B.1}$$

where $\bar{w}_{min}$ denotes the level of the new statutory minimum wage of €8.50 per hour.

It is important to note that the relative ordering of sector-region combinations with respect to

Table B.1: Variation in NMW Bite Across Firms

	Total		Manufacturing		Services		West Germany		East Germany	
	# Firms	%	# Firms	%	# Firms	%	# Firms	%	# Firms	%
Firms	3741		2041		1700		3216		525	
$TI = 0$	2767	74	1698	83.2	1069	62.9	2592	80.6	175	33.3
$TI \in (0, 0.1)$	503	13.4	228	11.2	275	16.2	322	10	181	34.5
$TI \in (0.1, 0.2)$	286	7.6	82	4	204	12	202	6.3	84	16
$TI \in (0.2, 0.3)$	91	2.4	15	0.7	76	4.5	55	1.7	36	6.9
$TI \in (0.3, 0.5)$	77	2.1	13	0.6	64	3.8	44	1.4	33	6.3
$TI \in (0.5, 1)$	17	0.5	5	0.2	12	0.7	1	0	16	3
Mean TI	0.033		0.016		0.053		0.023		0.096	
Mean TI if $TI > 0$	0.129		0.097		0.145		0.120		0.144	

Notes: Distribution of firms (in January 2015) across different groups of treatment intensity as captured by the fraction of full-time employees in their two-digit industry and region who earned less than €8.50 per hour in 2014.

their fraction of full-time employees who earned less than €8.50 does not hinge on the choice of $w_{sr}(0)$ and $w_{sr}(100)$. I also computed TI_{sr} assuming that $w_{sr}(0) = 0.9 * w_{sr}(10)$ or $w_{sr}(0) = 0.7 * w_{sr}(10)$. The relative ordering of sector-region cells did not change substantially. Moreover, robustness checks presented below in Section B.3 show that the documented NMW effects do not hinge on the assumptions about $w_{sr}(0)$.

B.2. Variation in Bite Measure Across Firms

This appendix documents the variation in the bite measure across firms. As shown in Table B.1, 27% of firms in the sample were affected to at least some degree. Among firms with $TI_{sr} > 0$, roughly one in two (one in five) of the affected firms had to increase wages of more than 10% (20%) of their full-time employees due to the NMW, *ceteris paribus*. Moreover, there is sufficient variation in TI_{sr} to analyze the response of firms in different sectors or regions. While services firms—of which approximately 37% are assigned to positive values of TI_{sr}—were affected more often by the NMW than manufacturing firms (17%), there is also substantial variation in TI_{sr} within two-digit industries; see Table B.2. In addition, one out of five firms in Western Germany was affected according to TI_{sr}, while more than two out of three Eastern German firms were affected to at least some degree. Conditional on being affected, the variation in TI_{sr} is comparable between firms in the different subsets: the mean NMW bite is 0.10 and 0.14 among manufacturing firms and service companies, as well as 0.12 and 0.14 for firms in West and East Germany, respectively.

Table B.2: Variation of Minimum Wage Bite in Different Industries

Two-Digit Industry (WZ 2008)	Geogr. Herfindahl	Import Share	Output Gap	# Firms	Mean *TI*	% Firms with Fraction of Affected Full-Time Employees				
						=0%	>0%	>10%	>20%	>30%
Panel A: Firms in Manufacturing Survey of IBS										
10 Food products	0.0045	0.309	0.022	66	0.182	6.1	93.9	63.6	33.3	19.7
11 Beverages	0.0079	0.297	0.022	12	0.0046	91.7	8.3	0	0	0
15 Leather products (& related)	0.0717	3.521	-0.046	NA	NA	NA	NA	NA	NA	NA
16 Wood & products of wood (excl. furniture)	0.007	0.372	0.066	61	0.0135	86.9	13.1	3.3	1.6	1.6
17 Paper & paper products	0.008	0.414	0.023	75	0.0131	86.7	13.3	8	0	0
18 Printing and reproduction of recorded media	0.0084	NA	-0.032	96	0.0153	79.2	20.8	2.1	0	0
19 Coke and refined petroleum products	0.0827	0.330	0.008	NA	NA	NA	NA	NA	NA	NA
20 Chemicals and chemical products	0.0214	0.544	0.005	115	0.0068	88.7	11.3	4.3	0	0
21 Basic pharmaceutical products & preparations	0.0289	0.906	-0.056	14	0.0056	78.6	21.4	0	0	0
22 Rubber and plastic products	0.0058	0.389	-0.014	182	0.0189	78	22	9.3	0.5	0
23 Other non-metallic mineral products	0.0053	0.337	-0.029	116	0.0046	86.2	13.8	0	0	0
24 Basic metals	0.0163	0.544	0.020	88	0.0008	90.9	9.1	0	0	0
25 Fabricated metal products, except machinery & equipment	0.0064	0.280	-0.001	304	0.0192	73.7	26.3	5.6	0	0
26 Computer, electronic and optical products	0.01	1.384	0.006	95	0.0088	84.2	15.8	4.2	0	0
27 Electrical equipment	0.0134	0.564	-0.038	181	0.0078	92.8	7.2	4.4	1.1	0
28 Machinery and equipment n.e.c.	0.0061	0.323	-0.018	450	0.0007	98.2	1.8	0	0	0
29 Motor vehicles, trailers and semi-trailers	0.0242	0.237	-0.001	80	0.003	90	10	1.3	0	0
30 Other transport equipment	0.0377	0.895	-0.024	9	0	100	0	0	0	0
31 Furniture	0.0123	0.670	-0.006	44	0.027	75	25	6.8	6.8	2.3
32 Other Manufacturing	0.0098	NA	-0.006	44	0.0652	45.5	54.5	18.2	9.1	6.8
33 Repair and installation of machinery and equipment	0.0128	NA	0.043	7	0.0154	71.4	28.6	0	0	0
Panel B: Firms in Services Survey of IBS										
35 Electricity, gas, steam, and air conditioning supply	0.0087	NA	-0.068	7	0	100	0	0	0	0
38 Waste collection, treatment and disposal activities; materials recovery	0.0096	NA	0.055	64	0.0073	78.1	21.9	1.6	0	0
49 Land transport and transport via pipelines	0.009	NA	-0.007	130	0.1423	0.8	99.2	73.1	22.3	2.3
50 Water transport	0.1714	NA	-0.073	NA	NA	NA	NA	NA	NA	NA
51 Air transport	0.293	NA	-0.062	NA	NA	NA	NA	NA	NA	NA
52 Warehousing and support activities for transportation	0.0121	NA	-0.037	119	0.0157	76.5	23.5	8.4	0	0
53 Postal and courier activities	0.0082	NA	0.013	8	0.0837	0	100	37.5	0	0
55 Accommodation	0.009	NA	0.016	89	0.1852	1.1	98.9	70.8	37.1	13.5
56 Food and beverage service activities	0.0128	NA	0.016	58	0.381	0	100	100	98.3	81
58 Publishing activities	0.025	NA	0.004	14	0.0073	92.9	7.1	7.1	0	0
59 Motion picture, video & TV programme production, sound recording & music publishing	0.0705	NA	-0.004	10	0.0305	0	100	0	0	0
60 Radio and Television	0.0753	NA	-0.004	5	0	100	0	0	0	0
61 Telecommunications	0.0308	NA	0.013	6	0	100	0	0	0	0
62 Computer programming, consultancy and related activities	0.0202	NA	-0.012	242	0.002	95	5	0.8	0	0
63 Information service activities	0.0312	NA	-0.012	13	0.0027	84.6	15.4	0	0	0
64 Financial service activities, except insurance and pension funding	0.0156	NA	0.022	49	0	100	0	0	0	0
65 Insurance, reinsurance and pension funding, except compulsory social security	0.0471	NA	-0.046	NA	NA	NA	NA	NA	NA	NA
66 Activities auxiliary to financial services and insurance activities	0.0233	NA	0.051	11	0.0021	90.9	9.1	0	0	0
68 Real estate activities	0.0222	NA	0.005	63	0.0493	23.8	76.2	20.6	0	0
70 Activities of head offices; management consultancy activities	0.0199	NA	-0.050	90	0.0062	88.9	11.1	2.2	0	0
71 Architectural and engineering activities; technical testing and analysis	0.0121	NA	0.034	371	0.0033	91.4	8.6	0	0	0
72 Scientific research and development	0.0262	NA	0.023	39	0	100	0	0	0	0
73 Advertising and market research	0.0365	NA	0.026	63	0.0191	74.6	25.4	0	0	0
74 Other professional, scientific and technical activities	0.0207	NA	-0.045	22	0.0299	13.6	86.4	0	0	0
77 Rental and leasing activities	0.0137	NA	-0.013	23	0.0292	69.6	30.4	17.4	0	0
79 Travel agency, tour operator and other reservation service and related activities	0.0218	NA	0.056	37	0.0217	51.4	48.6	16.2	0	0
80 Security and investigation activities	0.0238	NA	-0.018	12	0.2192	0	100	83.3	41.7	33.3
81 Services to buildings and landscape activities	0.0119	NA	-0.018	58	0.1948	0	100	96.6	32.8	13.8
82 Office administrative, office support and other business support activities	0.0161	NA	-0.018	46	0.1206	6.5	93.5	54.3	15.2	2.2
84 Public administration and defence; compulsory social security	0.0074	NA	-0.014	NA	NA	NA	NA	NA	NA	NA
85 Education	0.0102	NA	-0.006	14	0	100	0	0	0	0
86 Human health activities	0.007	NA	-0.014	6	0.1021	16.7	83.3	33.3	16.7	0
87 Residential care activities	0.0048	NA	-0.004	NA	NA	NA	NA	NA	NA	NA
88 Social work activities without accommodation	0.0116	NA	-0.004	NA	NA	NA	NA	NA	NA	NA
90 Creative, arts and entertainment activities	0.0282	NA	-0.005	9	0.026	55.6	44.4	0	0	0
92 Gambling and betting activities	0.0091	NA	-0.005	NA	NA	NA	NA	NA	NA	NA
93 Sports activities and amusement and recreation activities	0.0107	NA	0.010	NA	NA	NA	NA	NA	NA	NA
94 Activities of membership organisations	0.0181	NA	-0.017	NA	NA	NA	NA	NA	NA	NA

Notes. Distribution of firms in the IBS within different two-digit industries with respect to TI_{sr}. As the sample composition slightly varies over time, the distribution refers to IBS wave of January 2015. The industry-specific Herfindahl index is calculated based on data of county-level employment in 2014 from the "Business Register Data" (URS) provided by the Research Data Center of the Statistical Offices of the Federal States (FDZ, 2014). The industry-level data on import share is obtained from the Federal Statistical Office and only available for manufacturing industries. The industry-specific output gap is calculated as the deviation of sectoral gross value added in 2015 from its HP-filtered trend based on data obtained from the Federal Statistical Office. Firm-specific values are set to "NA" in industries with fewer than four observations in the sample due to data protection reasons.

B.3. Robustness of Main Results with Respect to Assumption about the Minimum of the Wage Distribution

Table B.3: Robustness with Respect to Assumption about the Minimum of the Wage Distribution

Specification	Baseline	$w(p0)$	
		$0.7w(p10)$	$0.9w(p10)$
	(1)	(2)	(3)
Panel A: Planned Price Change in Next 3 Months			
$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37^{***}	0.36^{***}	0.36^{***}
	(0.059)	(0.069)	(0.059)
R^2	0.324	0.324	0.324
Observations	240211	240211	240211
Panel B: Planned Change in Number of Employees in Next 3 Months			
$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	-0.054	-0.070	-0.062
	(0.061)	(0.074)	(0.060)
R^2	0.321	0.321	0.321
Observations	240331	240331	240331
Firm FE	yes	yes	yes
Time*Sector FE	yes	yes	yes

Notes: The dependent variables are the planned price changes (Panel A) and planned employment changes (Panel B) during the following three months reported to the IBS. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "Time*Sector FE" are time fixed effects specific to two-digit industries. Column (1) depicts the results of the baseline specification. Columns (2) and (3) set the minimum of the wage distribution to 70% and 90% of the first wage decile when calculating the bite. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This appendix demonstrates that the results are robust to different assumptions about the minimum of the wage distribution $w(0)$, which is not given in the RS wage data. The baseline specification replicated in Column (1) is based on the assumption that $w(0)$ is related to the wage at the 10^{th} percentile ($w(10)$) proportionally to how $w(10)$ is related to wages at the 20^{th} percentile. In this specification, the minima of the wage distribution on average correspond to approximately 85% of the wage rates at the 10^{th} percentile. As documented in Columns (2) and (3) of Table B.3, the estimated treatment effects are unchanged once the wage curve below the 10^{th} percentile is assumed to be either steeper (with $w(0) = 0.7 \times w(10)$) or flatter (with $w(0) = 0.9 \times w(10)$).

B.4. Plausibility Test Exploiting Minimum Wage-Related Questions in IBS

This appendix complements the evidence presented in Section 3.1 regarding the plausibility of the NMW bite measure TI_{sr} by making use of firms' responses to a series of supplementary questions in the IBS. Specifically, the IBS wave of November 2014 has been complemented by the following set of questions referring to firms' assessments about the upcoming NMW introduction in January 2015 (English translation of German original):

SQ1: *"The statutory minimum wage will be introduced on January 1st, 2015. Is your company affected by this regulation? [1] yes, [0] no*."

"If yes, which actions are you going to undertake in reaction to the introduction of the minimum wage (multiple answers possible)?

SQ2: *No action planned: [1] yes.*

SQ3: *Reduction in staff: [1] yes.*

SQ4: *Reduction in working hours: [1] yes.*

SQ5: *Price increases: [1] yes.*

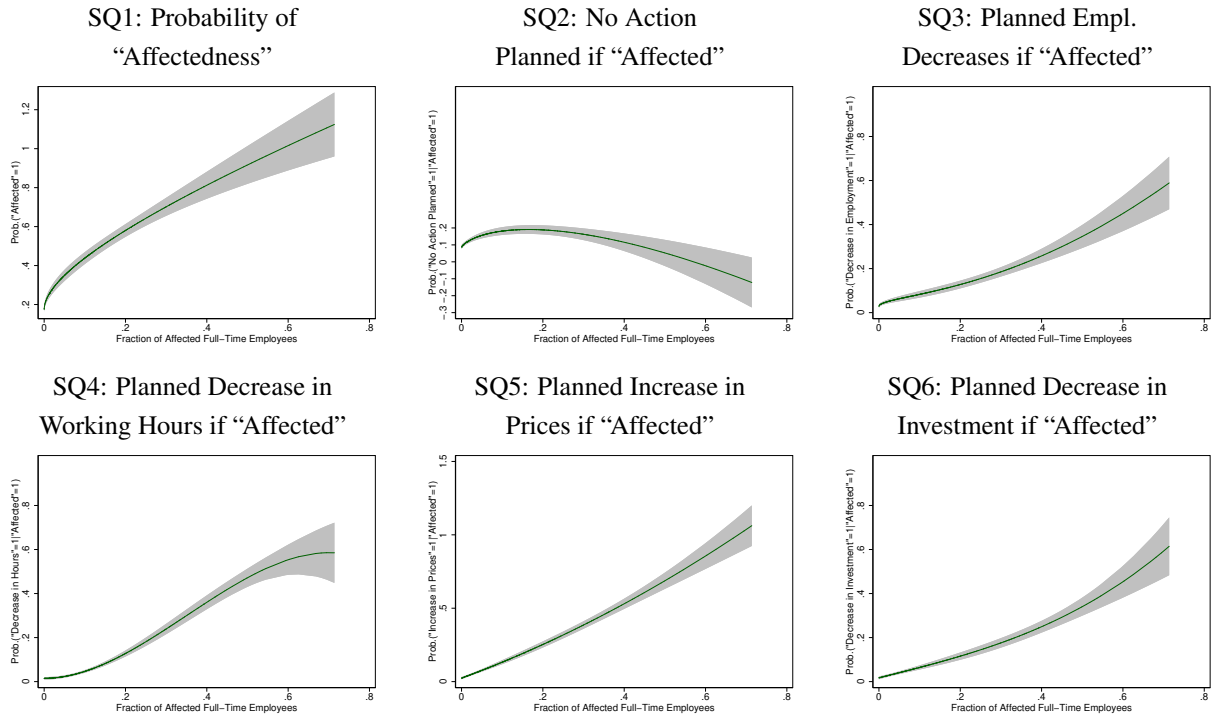
SQ6: *Decreased investment volume: [1] yes.*

SQ7: *Cuts in bonus payments: [1] yes.*

SQ8: *Other action: [1] yes."*

As the functional form of the relationship between TI_{sr} and the frequency to which firms answered the supplementary questions in the affirmative is not clear *a priori*, I estimate a fractional polynomial of degree two of TI_{sr} without adding any further covariates. Figure B.2 plots the resulting curves of the mean probability to affirm the respective question at different levels of TI_{sr} along

Figure B.2: Relationship Between TI_i and NMW-related Supplementary Questions in the IBS



Notes: Each figure plots the predicted probability (green line) to affirm to the respective supplementary question by estimating a fractional polynomial of degree two of TI_{sr} without adding any further covariates. The shaded area covers the 95%-confidence interval of the predicted probabilities.

with the 95%-confidence intervals. In addition, Table B.4 summarizes the average frequencies of responses at different levels of TI_{sr}. The question about firms' affectedness (SQ1) neither provides any information about the intensity to which firms are affected nor contains any information about the channels through which firms are affected.

The frequency that firms stated to be affected by the NMW increases substantially in TI_{sr}, see Figure B.2 and Table B.4. Only 17% of firms perceived themselves as "affected" when operating in sector-region cells with zero full-time employee earning less than €8.50 in 2014. This fraction increases to 75% for firms in sector-region cells with more than 20% of full-time employees being treated. While more than 80% of firms with $TI_{sr} > 0.2$ planned to somehow react to the NMW, the majority of firms that reported to be affected by the NMW despite $TI_{sr} = 0$ did not plan to react. Arguably, the latter firms were only affected indirectly by the NMW or perceived themselves as

Table B.4: Plausibility Check of the Treatment Intensity Measure

	Treatment Intensity $TI \in$		
	[0%]	(0%,20%]	(20%,100%)
prob("Affected" = 1)	0.173	0.389	0.753
prob("Do Not Plan to React" = 1|"Affected" = 1)	0.528	0.444	0.183
prob("Plan to Adjust Business" = 1|"Affected" = 1)	0.472	0.556	0.817
prob("Staff Reduction" = 1|"Affected" = 1)	0.177	0.187	0.310
prob("Hours Reduction" = 1|"Affected" = 1)	0.074	0.134	0.373
prob("Price Increase" = 1|"Affected" = 1)	0.147	0.285	0.611
prob("Reduction in Investment" = 1|"Affected" = 1)	0.100	0.141	0.310
prob("Reduction in Special Payments" = 1|"Affected" = 1)	0.147	0.246	0.381

Notes. "TI" refers to the fraction of full-time employees who earned an hourly gross wage of less than €8.50 in 2014 in each firm's two-digit industry and county. *prob*("Affected" = 1) displays the frequency that firms responded to be "affected" by the NMW in the supplementary questions of the IBS in November 2014 depending on TI_{sr} as indicated at the top of each column. *prob*("Plan to Adjust Business" = 1|"Affected" = 1) captures the frequency that "affected" firms stated to plan to react in at least one of the following ways: reduction in staff, reduction in working hours, price increases, decreased investment volume, cuts in bonus payments, or other action. *prob*("Do Not Plan to React" = 1|"Affected" = 1), etc. are defined accordingly.

being affected because of the obligatory and time-consuming documentation requirements.[2]

Moreover, the probability that "affected" firms according to SQ1 stated to react to the NMW increases along all margins covered by the supplementary questions SQ3 through SQ7. Interestingly, the probability of stating to increase prices (SQ5) increases most strongly in TI_{sr}. Albeit reacting less strongly compared to SQ5, the probabilities of affected firms to confirm to plan reductions in employment (SQ3), cuts in working hours (SQ4), decreases in investment (SQ6), or reductions in special payments (SQ7) also increases in TI_{sr}.

However, interpreting the correlations in a causal way is potentially misleading because the questions regarding firms' planned reactions to the NMW (SQ3-SQ7) are restricted to affected firms and one direction. For example, affected firms could only state whether they planned to reduce the number of employees or not. If firms were operating in monopsonistic labor markets, for example, they should be expected to *increase* their labor demand in response to a minimum wage that is binding at sufficiently low levels (Manning, 2003). If a non-negligible fraction of

[2] According to the National Regulatory Control Council ("*Nationaler Normenkontrollrat*"), the NMW introduction and its first adjustment in 2017 imposed annual compliance costs of €6.3 billion on firms (National Regulatory Control Council, 2017, p.19).

Table B.5: Response to Minimum Wage: Main Results Based on Self-Reported "Affectedness"

	$\Delta\text{Price}_t^{+3m}$	$\Delta\text{Empl.}_t^{+3m}$	$\Delta\text{Cond.}_t^{+6m}$	$\Delta\text{Prod./Dem.}_t^{+3m}$	Cond._t	Orders_t
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Treated}_i)$						
$\times\mathbb{1}(t \in \{2014m7, 2016m6\})$	0.020**	-0.006	0.003	0.009		
	(0.009)	(0.009)	(0.011)	(0.009)		
$\times\mathbb{1}(t \in \{2014m10, 2016m9\})$					-0.016	-0.005
					(0.012)	(0.010)
Firm FE	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes
R^2	0.315	0.313	0.368	0.384	0.509	0.456
Observations	221085	221189	221218	221824	232201	226724

Notes: The dependent variables are planned price or employment changes during the following three months ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$), expected business conditions for the next six months ($\Delta\text{Cond.}_t^{+6m}$), expected production (manufacturing firms) or demand (service firms) for the following three months ($\Delta\text{Prod./Dem.}_t^{+3m}$), as well as current business conditions and current backlog of orders of firms in the IBS (Cond._t and Orders_t). "$\mathbb{1}(\text{Treated}_i)$" is a dummy that is one if firms stated to be "affected" by the NMW in the November 2014 wave of the IBS (SQ1) and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" and "$\mathbb{1}(t \in \{2014m10, 2016m9\})$" indicate the respective treatment period. "Time*Sector FE" are time fixed effects at the level of two-digit industries. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

affected firms did so, the fraction of firms that planned to decrease their labor demand could hence be accompanied by a fraction of firms that planned to increase labor demand resulting in a total employment effect that potentially cancels out. Hence, the supplementary questions SQ3 through SQ7 themselves do not allow for causal inference on the firm-level response of the NMW due to missing counterfactuals as well as one-sided questions.

Lastly, firms' self-reported "affectedness" by the NMW is used to replicate the main findings of the paper. As the treatment-interaction term does not include a continuous bite measure, but only a dummy ("$\mathbb{1}(\text{Treated}_i)$") according to firms' answers to SQ1, the coefficients displayed in Table B.5 cannot be directly compared to the main results of Table 2. Again, the effect of the NMW on firms' pricing plans is significantly positive, and the absolute size of the coefficient of the employment response is only one quarter of the size of the price effect and is statistically insignificant. Moreover, the effects with respect to firms' expectations regarding their general business conditions, production, and demand, as well as realizations of their business conditions and their current range of orders are statistically indistinguishable from zero.

C. Descriptive Statistics of IBS Data

C.1. Relationship between Planned and Ex-post Realized Price Changes

Table C.1: Planned vs. Ex-post Realized Price Changes

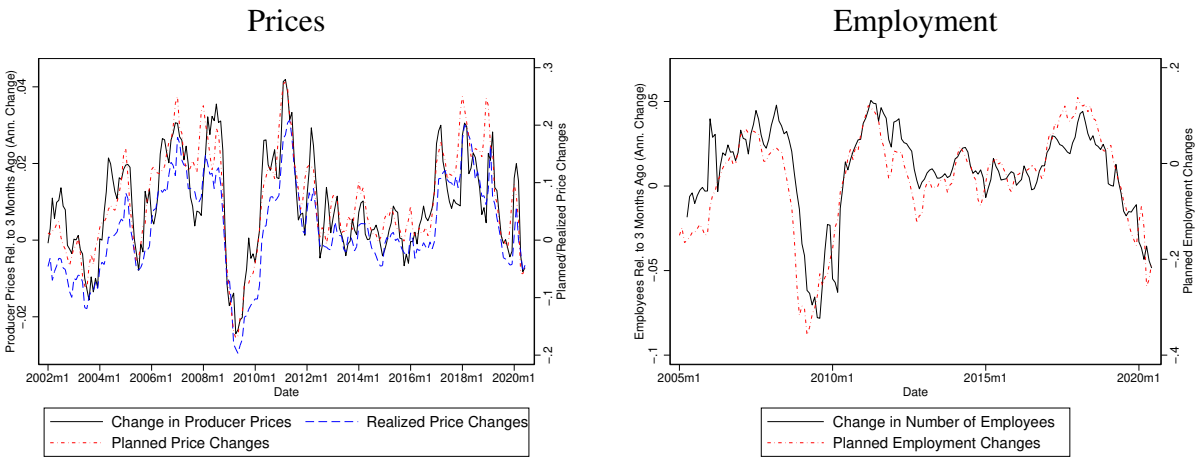
	Obs.	Fractions of Mean Realized Price Changes b/w $t+1$ & $t+3$			Sum
		>0	$=0$	<0	
$\Delta\text{Price}_t^{+3m}=1$	15732	0.58	0.39	0.03	1
$\Delta\text{Price}_t^{+3m}=0$	106892	0.08	0.84	0.08	1
$\Delta\text{Price}_t^{+3m}=-1$	8844	0.03	0.30	0.67	1

Notes: This table contrasts the micro data of planned price changes during the following three months stated in t with the mean reported (monthly) price changes during the following three months, i.e., between $t+1$ and $t+3$. The sample is restricted to the manufacturing firms in the regression sample.

This appendix demonstrates that firms in general stick to their pricing plans. For this purpose, Table C.1 contrasts the micro data of planned price changes during the following three months stated in t with the mean reported (monthly) price changes during the following three months, i.e., between $t+1$ and $t+3$. Approximately 80% of manufacturing firms report realized price changes between months $t+1$ and $t+3$ that are in line with their plans of month t. In consequence, it is not surprising that the main results are comparable when estimating the NMW effect based on either planned and realized changes in prices for the subsample of manufacturing firms as shown in Section 4.2. As the analysis of the firm-level relationship between the price and employment responses to the NMW requires information on both variables and to cover firms in all industries, the baseline estimations presented in the paper use planned changes in prices and employment.

C.2. Relationship between Aggregated IBS Data and Administrative Data

Figure C.1: Aggregated IBS Data and Quantitative Changes in Prices and Employment



Notes: The left (right) figure plots time series of the realized three-month change in producer prices $\Delta\overline{PPI}_t$ (employment $\Delta\overline{Empl}_t$) in the manufacturing sector (solid line; left axis) against mean reported price plans/realizations (employment plans) of firms in the manufacturing IBS (dashed lines; right axis). The German Federal Statistical Office provides PPI and employment data for two-digit industries. For aggregation, the time series are weighted by the average share of firms per sector in the IBS. $\Delta\overline{Empl}_t$ is seasonally adjusted using month fixed effects. See Table C.2 in the Appendix for a cross-correlogram.

This appendix documents that the qualitative price and employment changes reported to the IBS on average closely track *quantitative* price and employment changes observed in administrative data. For the subset of manufacturing firms, Figure C.1 plots the average answers to Q1 ($\overline{Price}_t^{+3m}$), Q1a ($\overline{Price}_t^{-1m}$), and Q2 ($\overline{Empl}_t^{+3m}$) against the three-month change in producer prices ($\Delta\overline{PPI}_t$) and employment ($\Delta\overline{Empl}_t$), respectively.[3] As documented in Table C.2, the time series correlation between $\Delta\overline{PPI}_t$ and average price plans is highest when price plans are lagged by one month ($\rho = 0.86$), while the correlation with average price realizations is highest at contemporaneity ($\rho = 0.85$).[4] The close relationship between aggregated survey responses and PPI data is exploited in the back-of-the-envelope approximation of the quantitative size of the NMW effect on prices presented in Online Appendix D.1. Comparably, the correlation between $\Delta\overline{Empl}_t$ and average

[3]For manufacturing, the German Federal Statistical Office provides PPI and employment data at two-digit WZ08 industry level. For aggregation, the indices are weighted by the share of firms per sector in the IBS. For services, PPIs are only available at quarterly frequency for a limited number of services sectors. Moreover, CPI data for services are not limited to domestic service providers and cannot be linked to the WZ08 classification system.

[4]The aggregate price change relative to three months in the past ($\Delta\overline{PPI}_t$) fits the average survey data on price realizations better than monthly changes in $\overline{PPI}_t$; see Table C.2.

Table C.2: Cross-Correlation b/w Average Reports to IBS and Changes in Quantitative Price Data

Panel A: Changes in PPI Relative to 3 Months Before							
Lag i	-3	-2	-1	0	1	2	3
$\rho(\overline{\Delta\text{Price}}_{t-i}^{+3m}, \Delta\overline{PPI}_t)$	0.561	0.665	0.764	0.844	**0.861**	0.809	0.689
$\rho(\overline{\Delta\text{Price}}_{t-i}^{-1m}, \Delta\overline{PPI}_t)$	0.697	0.769	0.833	**0.853**	0.804	0.691	0.568

Panel B: Changes in PPI Relative to Previous Month							
Lag i	-3	-2	-1	0	1	2	3
$\rho(\overline{\Delta\text{Price}}_{t-i}^{-1m}, \Delta\overline{PPI}_t)$	0.605	0.622	0.703	0.65	0.517	0.445	0.366

Notes: Cross-correlogram of time series of changes in weighted producer prices ($\Delta\overline{PPI}_t$) relative to three months ago (Panel A) or one month ago (Panel B) and average planned price changes for the following three months ($\overline{\Delta\text{Price}}_t^{+3m}$) or average realized price changes during the previous month ($\overline{\Delta\text{Price}}_t^{-1m}$) as reported to the manufacturing IBS. The German Federal Statistical Office provides PPI for two-digit manufacturing industries. For aggregation, the time series are weighted by the average share of firms per sector in the IBS.

employment expectations is highest when employment plans are lagged by 2 months ($\rho = 0.77$). Hence, the survey questions are useful indicators of firms' pricing and employment policies.

D. Additional Analyses

D.1. Back-of-the-Envelope Approximation of Quantitative Size of the Minimum Wage Effect on Producer Prices

To get an idea about the economic dimension of the price effect, this appendix provides a back-of-the-envelope calculation of its quantitative size. As documented in Appendix C.2, aggregated survey responses closely track *quantitative* changes in price indices from administrative sources. Restricting the time series underlying Figure C.1 to the time frame covered by the empirical analysis, I estimate semi-elasticities that map qualitative survey responses to quantitative changes in producer price indices. They are defined as follows[5]

$$\hat{\psi}^{Planned} := \frac{\mathrm{d}\Delta\overline{PPI}_t}{\mathrm{d}\overline{Price}_{t-1}^{+3m}} = 0.126 \text{ and } \quad \hat{\psi}^{Realiz} := \frac{\mathrm{d}\Delta\overline{PPI}_t}{\mathrm{d}\overline{Price}_{t}^{-1m}} = 0.134. \tag{D.1}$$

Hence, an appreciation of average price plans (realizations) in the IBS by 0.1 that lasts twelve months corresponds to an increase in producer prices by 1.26 (1.34) percentage points.

These semi-elasticities can be used to approximate the quantitative size of firms' price increases in response to the NMW. Assuming (a) that the aggregate relationship translates with comparable magnitudes into variation at the industry-region-level, (b) that the average size of NMW-induced price changes did not differ from the size of "normal" price changes, and (c) that $\hat{\psi}^{Planned}$ and $\hat{\psi}^{Realiz}$ are homogeneous across different subsets of firms, the average NMW-induced price reaction of each firm (ΔP_i) can be approximated as

$$\Delta P_i = \hat{\beta} \times TI_{i \in sr} \times \hat{\psi} \times \frac{\bar{t} - \underline{t}}{12 \text{ months}}, \tag{D.2}$$

where $\hat{\psi}$ refers to either $\hat{\psi}^{Planned}$ or $\hat{\psi}^{Realiz}$, and $\hat{\beta}$ is the treatment effect on firms' planned or

[5] As documented in Table C.2 in Appendix C.2, the time series correlation between $\Delta\overline{PPI}_t$ and average price plans is highest when price plans are lagged by one month ($\rho = 0.86$). Therefore, $\mathrm{d}\overline{Price}_t^{+3m}$ is lagged by one month when calculating the semi-elasticity. In turn, the correlation with average price realizations is highest at contemporaneity ($\rho = 0.85$).

realized price changes estimated in Section 4 based on Model (2). Moreover, the length of the treatment period in years ($\frac{\bar{t}-\underline{t}}{12 \text{ months}}$) controls for the fact that $\hat{\psi}^{Planned}$ and $\hat{\psi}^{Realiz}$ map the survey responses to *annualized* changes in producer prices.

The price effect is considerable, and firms increased their prices in response to the NMW by $\Delta P_i = 0.09 \times TI_{i \in sr}$ according to this approximation. Given the average bite of $\overline{TI}_{TI_{sr}>0} = 0.125$, affected firms hence increased their prices on average by 1.2 percentage points due to the NMW. Among manufacturing firms, the price effect does not differ substantially once it is approximated using survey data on planned price changes ($\Delta P_i^{Planned} = 0.396 \times 0.126 \times 2 \times TI_{i \in sr} = 0.10 \times TI_{i \in sr}$) or realized price changes ($\Delta P_i^{Realiz} = 0.29 \times 0.134 \times 2 \times TI_{i \in sr} = 0.08 \times TI_{i \in sr}$).

NMW Effect on Aggregate Level of Producer Prices To examine the effect of the NMW on the overall level of producer prices in Germany, I insert the average bite across all industry-region combinations ($\widetilde{TI}$) in Equation (D.2). To capture the level of overall producer prices as closely as possible, each industry-region cell is weighted by revenues.[6] Given that revenues are higher in industry-region combinations that were less strongly affected by the NMW, the revenue-weighted $\widetilde{TI} = 0.028$ is slightly smaller than the average bite of all firms in the sample ($\overline{TI} = 0.034$).

According to this back-of-the-envelope calculation, aggregate producer prices in the manufacturing and service sectors prone to the NMW increased by approximately 0.26 percent in response to the NMW; see Table D.1. As the average NMW bite largely differed between manufacturing firms and service providers, the aggregate price effect is heterogeneous among these groups. Producer prices were more strongly increased in the service sector (+0.41%) in relation to the manufacturing sector (+0.13%). Again, estimating the overall price effect for manufacturing firms based on realized price changes (+0.10%) rather than pricing plans delivers comparable results.

The estimated size of the NMW-induced increase in producer prices is remarkably close to the prediction of the "German Council of Economic Experts." In their annual report to the federal

[6]Revenue data are obtained from the Federal Statistical Office at the level of two-digit industries and federal states. From this, revenue weights are calculated for each county-sector combination using the county's employment share in the respective industry of the federal state. Moreover, I adjust the revenue weights for the fact that wage data are missing more often in Eastern Germany compared to Western Germany. See the annex below for details.

Table D.1: Quantitative Effect of NMW on the Overall Level of Producer Prices

	Quantification of Price Effect Based on			
	Planned Price Changes			Realized Price Changes
	Total	Manuf.	Services	Manuf.
PPI-Semi-Elasticity ($\hat{\psi}$)	0.126	0.126	0.126	0.134
Treatment Effect ($\hat{\beta}$)	0.369	0.396	0.340	0.290
Revenue-Weighted Treatment Intensity ($\widetilde{TI}$)	0.028	0.013	0.048	0.013
Overall Price Effect ($\Delta\widetilde{P}$ in %)	0.260	0.125	0.410	0.098
Wage Bill Increase c.p. ($\Delta\widetilde{W}$ in %)	0.457	0.262	0.727	
Wage Bill Increase $\times$ Labor Share ($\Delta\widetilde{C}$ in %)	0.305	0.192	0.463	
Pass-Through Elasticity ($\frac{\Delta\widetilde{P} \text{ in } \%}{\Delta\widetilde{C} \text{ in } \%}$)	0.85	0.65	0.89	

Notes: This table presents a back-of-the-envelope calculation of the NMW effect on the overall level of producer prices "$\Delta\widetilde{P}$" and the degree of price pass-through "$\frac{\Delta\widetilde{P}}{\Delta\widetilde{C}}$." The "PPI-Semi-Elasticity ($\hat{\psi}$)" refers to the degree to which changes in average planned/realized price changes in the IBS translate to changes in producer price indices. $\hat{\psi}$ can only be estimated for manufacturing firms and is assumed to be constant across all sectors and regions. "Treatment effect ($\hat{\beta}$)" corresponds to the estimated coefficients of Tables 2. "$\widetilde{TI}$" is the revenue-weighted treatment intensity of all industry-region combinations as calculated in Appendix D.1. "$\Delta\widetilde{W}$" and "$\Delta\widetilde{C}$" indicate the average implicit increase in the wage bill and overall costs induced by the NMW calculated from micro data of the Structure of Earnings Survey 2014 and industry-specific labor shares calculated based on data from the Federal Statistical Office.

government published two months prior to January 2015, they predicted an additional increase in CPI inflation by 0.2 percentage points due to the NMW (Sachverständigenrat, 2014). Hence, the back-of-the-envelope calculation does not seem to deliver unreasonable results despite the strong assumptions needed to interpret the qualitative effects in a quantitative way.

Price Pass-Through Elasticity Lastly, the aggregate price effect, $\Delta\widetilde{P}$, is related to the average of NMW-induced implicit cost increases in the manufacturing and service sectors under consideration. This aggregate cost increase, $\Delta\widetilde{C}$, is calculated in three steps: first, I approximate the implicit wage bill increase in each industry s induced by the NMW, ΔW_s, based on the micro data of the Structure of Earnings Survey, i.e., the increase in wage costs firms would have had to bear if they fully complied to the NMW and held their employment structure constant. Then, the industry-specific wage bill increase is multiplied with each sector's labor share, LS_s, to obtain a measure

for the cost increase in each industry, ΔC_s.[7] Finally, the aggregate cost increase is calculated as $\Delta\widetilde{C} = \sum_s(\omega_s \times \Delta C_s)$, where ω_s is the industry revenue weight as of 2014.

The resulting elasticity indicates a substantial price pass-through of the NMW. According to the approximation, the NMW increased overall costs in the sectors under consideration by 0.31%. Hence, a 1%-increase in overall costs came along with a price increase by 0.85%. The pass-through elasticity is slightly larger in the service sector (0.89) compared to the manufacturing sector (0.65). Considering the assumptions and simplifications, the results of the back-of-the-envelope calculation should be taken with a grain of salt. Nevertheless, the approximation clearly emphasizes that the size of the price effect is non-negligible and suggests that firms have rolled over a substantial share of the costs generated by the NMW to their customers.

ANNEX: Revenue-Weighted Average Treatment Intensity of the German Economy

This annex describes the calculation of the revenue-weighted average treatment intensity across all industry-region cells ($\widetilde{TI}$) that is used in the back-of-the-envelope approximation of overall price effect of the NMW introduction above. To capture overall producer prices as closely as possible, the treatment intensity of each industry-region cell, TI_{sr}, is weighted by the revenues generated in each cell, $\widetilde{revenues}_{sr}$.[8] As data on industry-specific revenues are not available at the level of counties and as the bite measure cannot be constructed for all cells due to data protection issues, the revenue weights are approximated as described in the following.

Revenue data are available at the Federal Statistical Office for two-digit industries s and federal states f ($revenues_{sf}$) in 2014. To put an appropriate weight on each TI_{sr}, the state-level revenue weights $revenues_{sr}$ are assigned to each county in proportion to its relative size in the respective federal state. This relative size is approximated by the county-specific number of full-time em-

[7] The industry-specific labor share, LS_s, is calculated based on national accounting data provided by the Federal Statistical Office at the two-digit industry level and defined as the ratio between the total compensation, TC_s, and gross value added, GVA_s. As the data only include the total compensation of employees, $TC_{empl.,s}$, rather than the wage bill of all workers in the industry (incl. self-employed), I approximate the total compensation as $TC_s \approx TC_{empl,s} \times \frac{\text{All Workers}_s}{\text{Employees}_s}$.

[8] As in the baseline specification, I use county-level information where available and fill missings by wage data at the labor market region level.

ployees that work in industry s, denoted $employees_{sr}$. The employment data are included in the RS wage data received from the Federal Employment Agency (2016). From this, the total number of full-time employees represented by industry-specific wage data can be calculated for each federal state, i.e., $employees_{sf} = \sum_{r \in f}(employees_{sr} | w_{sr} \notin \{\emptyset\})$.

The revenue weight for treatment intensities in counties for which wage data are available ($w_{sr} \notin \{\emptyset\}$) is given by

$$revenues_{sr} = revenues_{sf} \times \frac{employees_{sr}}{employees_{sf}}. \tag{D.3}$$

Consequently, industry-county cells not covered by the RS wage data receive zero weight. This implicitly assumes that the industry-specific bite in these counties is similar to the average treatment intensity in all other counties of the same federal state.

However, state-level revenues cannot be matched to RS wage data in at least one of the respective federal state's counties in 10.9% (6.0%) of all East (West) German industry-federal state cells. Given that there are higher average bites in Eastern Germany, $\widetilde{TI}$ based on the weights of Equation (D.3) would be downward biased if the asymmetry in the availability of wage data was not controlled for. For this purpose, the revenue weights ($revenues_{sr}$) are inflated by the inverse fraction of industry-specific revenues in Eastern Germany that can be assigned to wage data in any East German federal state, i.e.,

$$\xi_{s,East} = \frac{\sum_{f \in \{East\}} revenues_{sf}}{\sum_{f \in \{East\}}(revenues_{sf} | w_{sf} \notin \{\emptyset\})}, \tag{D.4}$$

where $w_{sf} \notin \{\emptyset\}$ denotes that industry-specific wage data are available in at least one county of the state f. ξ_{sWest} is defined accordingly for Western Germany.

The resulting revenue weight for TI_{sr} is hence given by

$$\widetilde{revenues}_{sr} = revenues_{sr} \times \frac{employees_{sr}}{employees_{sf}} \times \xi_{s,EW \in \{East,West\}}. \tag{D.5}$$

D.2. Firm-Level Relationship between Price and Employment Response: Alternative Approach

This appendix presents an alternative approach to investigate the firm-level relationship between the price and employment responses to the NMW. While the approach applied in Section 4.3 is based on separate regressions using indicators of the joint price and employment response as dependent variable, the positive firm-level relationship between both types of adjustment also becomes apparent when alternatively asking whether firms reacted more frequently along the pricing margin if they, for a given NMW bite, reacted with relatively fewer cuts in employment compared to other firms and vice versa. For this purpose, I proceed in three steps: First, for each firm, I calculate the mean of residuals during the treatment period from the estimation Model (2) with $\Delta\text{Empl.}_t^{+3m}$ as dependent variable. Then, firms are sorted into two groups given the sign of the average employment residuals. For a given TI_{sr}, firms with negative mean residuals reacted more frequently by reducing the number of employees, while those with positive average residuals reported relatively more appreciated employment plans. Finally, these indicators are used as interaction terms when estimating the relationship between the NMW bite and firms' pricing plans.

The results depicted in Column (1) of Table D.2 show that the association between the NMW bite and firms' pricing plans is significantly stronger among firms that reacted relatively with relatively fewer employment cuts. In turn, Column (3) shows that the relationship between TI_{sr} and firms' employment plans is strongly significantly negative for firms that reacted relatively less frequently along the pricing margin, while it is slightly positive among those firms that planned to increase prices more often than predicted by Model (2). Columns (2) and (4) show that these patterns persist once additionally controlling for firms' current backlog of orders and expected change in demand (services) or production (manufacturing) in the next three months in both steps of the regressions, which are both positively correlated with pricing and employment decisions themselves.[9]

[9] Recall that neither firms' expected production or demand changes for the following three months, nor their backlog of orders were negatively affected by the NMW *on average* (Table 2). Still, these variables are omitted from the baseline specification as it cannot be ruled out that these controls are contaminated by the treatment in a manner that is systematically related to the dummies indicating positive or negative residuals of the first step estimation.

Table D.2: Price and Employment Response for Given Adjustment Along the Other Dimension

	$\Delta\text{Price}_t^{+3m}$		$\Delta\text{Empl.}_t^{+3m}$	
	(1)	(2)	(3)	(4)
$\text{TI} \times \mathbb{1}(t \in \{2014m7, 2016m6\})$				
$\times \mathbb{1}(\overline{\text{Empl.}}^{resid,TP} \geq 0)$	0.49***	0.41***		
	(0.07)	(0.06)		
$\times \mathbb{1}(\overline{\text{Empl.}}^{resid,TP} < 0)$	0.22***	0.28***		
	(0.07)	(0.07)		
$\times \mathbb{1}(\overline{\text{Price}}^{resid,TP} \geq 0)$			0.14*	-0.01
			(0.07)	(0.07)
$\times \mathbb{1}(\overline{\text{Price}}^{resid,TP} < 0)$			-0.22***	-0.16***
			(0.05)	(0.05)
Orders_t		0.07***		0.17***
		(0.01)		(0.01)
$\Delta\text{Prod./Demand}_t^{+3m}$		0.11***		0.23***
		(0.01)		(0.01)
H0: Coefficients Equal: p-value	.003	.065	0	0
Firm FE	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes
R^2	0.325	0.346	0.321	0.411
Observations	240211	234091	240331	234156

Notes: The dependent variables are planned price or employment changes during the following three months ($\Delta\text{Price}_t^{+3m}$ and $\Delta\text{Empl.}_t^{+3m}$) of firms in the IBS. "*TI*" is the bite of the NMW and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. In Columns (1) and (2), the bite*treatment period interaction is multiplied with dummies that are one if the firm-specific mean of residuals in $\Delta\text{Empl.}_t^{+3m}$ from Model (2) was positive (incl. zero) or negative, respectively. In Columns (3) and (4), this is interacted with indicators based on the average residual in $\Delta\text{Price}_t^{+3m}$. Columns (2) and (4) additionally control for firms' current backlog of orders and expected change in demand (services) or production (manufacturing) in the next three months in both estimation steps. "Time*Sector FE" are time fixed effects at the level of two-digit industries. The p-values at the bottom indicate whether the treatment effects in both groups are statistically different from each other. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3. Different Hypothetical Levels of NMW

Table D.3: Firms' Responses to the NMW Introduction: Different Hypothetical NMW Levels

	$\Delta\text{Price}_t^{+3m}$					$\Delta\text{Empl.}_t^{+3m}$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Specification	Baseline	Threshold $\bar{w}$				Baseline	Threshold $\bar{w}$			
		6.50€	7.50€	9.50€	10.50€		6.50€	7.50€	9.50€	10.50€
$TI \times \mathbb{1}(t \in \{2014m7, 2016m6\})$	0.37***	0.75***	0.55***	0.22***	0.14***	-0.054	-0.17	-0.11	-0.051	-0.041
	(0.059)	(0.25)	(0.12)	(0.044)	(0.036)	(0.061)	(0.24)	(0.11)	(0.042)	(0.032)
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time*Sector FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.324	0.324	0.324	0.324	0.324	0.321	0.321	0.321	0.321	0.321
Observations	240211	240211	240211	240211	240211	240331	240331	240331	240331	240331
PPI-Semi-Elasticity ($\hat{\psi}$)	0.126	0.126	0.126	0.126	0.126					
Revenue-Weighted TI ($\widetilde{TI}$)	0.028	0.005	0.012	0.047	0.073					
Overall Price Effect ($\Delta\widetilde{P}$ in %)	0.260	0.095	0.174	0.265	0.260					

Notes: The dependent variable are planned price changes or planned employment changes during the following three months reported to the IBS, respectively. "TI" is the NMW bite and "$\mathbb{1}(t \in \{2014m7, 2016m6\})$" indicates the treatment period. "Time*Sector FE" are time fixed effects specific to two-digit industries. The bottom panel conducts a back-of-the-envelope calculation of the aggregate price effect as presented in Appendix D.1. Here, "$\hat{\psi}$" denotes the semi-elasticity mapping changes in price plans to quantitative producer prices. "$\widetilde{TI}$" and "$\Delta\widetilde{P}$" reflect the overall treatment intensity in the economy and the overall effect of producer prices based on revenue weights for each county-industry cell. Standard errors are three-way clustered at the levels of sectors, counties, and dates. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This appendix provides further evidence that the NMW-induced price effect is driven by firms that were already captured by the baseline bite measure even though TI_{sr} only covers the fraction of affected full-time employees. For this purpose, TI_{sr} is alternatively constructed for different hypothetical minimum wage levels $\bar{w}_{min}$. If the NMW bite measure is calculated based on thresholds above €8.50, TI_{sr} assigns a positive treatment intensity to firms that operate in industries and regions where all full-time employees earned wages slightly above the NMW prior to its introduction. However, it could be argued that these firms employed at least some part-time employees or marginally employed workers that previously earned less than €8.50. The results in Columns (4) and (5) of Table D.3 indicate that planned price changes in the period around the NMW introduction reacted less sensitive to a given variation in TI_{sr} based on the thresholds of $\bar{w}_{min} =$ €9.50 and €10.50 compared to the baseline specification. Naturally, the revenue-weighted mean of overall treatment intensity $\widetilde{TI}(\bar{w}_{min})$ is larger in these specifications, i.e., $\widetilde{TI}(9.50) = 0.047$

and $\widetilde{TI}(10.50) = 0.073$. Strikingly, when conducting the same back-of-the-envelope calculation as presented in Appendix D.1, the resulting overall effect on producer prices (+0.27% and +0.26%) is almost identical to the baseline scenario (+0.26%). This indicates that the price effect is generated by firms that were already captured by the baseline specification of the treatment intensity measure. Abstracting from firms that were affected by the NMW only through higher wage costs for part-time employees or marginally employed workers does hence not appear to be worrisome.

In turn, if TI_{sr} measures the fraction of all full-time employees who earned less than €6.50 or €7.50 in 2014, the bite measure only captures firms that are affected very strongly by the introduction of a NMW of €8.50 per hour. Unsurprisingly, the estimated effect on the overall price level is smaller when conducting the back-of-the-envelope calculation (+0.10% for $\bar{w}_{min} =$ €6.50 and +0.17% for $\bar{w}_{min} =$ €7.50) because fewer firms are considered to be affected; see Columns (2) and (3). Further, the estimated employment reaction among these highly treated firms is more negative than in the baseline specification ($\hat{\beta}^{\text{Empl.}} = -0.17$ and $\hat{\beta}^{\text{Empl.}} = -0.11$) but remains insignificant; see Columns (7) and (8).

E. Wording of Survey Questions

The following set of questions, which are asked regularly on a monthly basis in the IBS, are used in this paper (English translation of German original):

Regularly Asked Questions in the IBS Covering Firms in Services (S):

S:Q1 Planned Price Change:

"During the next 3 months, the prices of our service will [1] increase, [0] stay the same, [-1] decrease."

S:Q2 Planned Change in Employment:

"During the next (2-3) months, the number of employees will [1] increase, [0] stay the same, [-1] decrease."

S:Q2a Realized Employment Changes:

"During the past (2-3) months, the number of employees [1] increased, [0] stayed the same, [-1] decreased."

S:Q3 Current Backlog of Orders:

"We evaluate our backlog of orders as [1] comparatively large, [0] sufficient (typical for season), [-1] too small."

S:Q4 Current Business Situation:

"We evaluate our current business situation as [1] good, [0] satisfactory (typical for season), [-1] bad."

S:Q5 Expected Business Situation:

"During the next six months, our business situation will be [1] more favorable, [0] stay the same, [-1] more unfavorable."

S:Q6 Expected Change in Demand:

"During the next (2-3) months, the demand for our service and/or our revenues will [1] increase, [0] stay approximately the same, [-1] decrease."

Regularly Asked Questions in the IBS Covering Firms in Manufacturing (M):

In the manufacturing survey, firms are asked for assessments regarding specific products. However, only 0.43% of all observations between 2011 and 2017 refer to multiple products for the same firm at a given point in time. Following the procedure described in Link (2020), these observations are aggregated to the firm level by taking means across products and rounding to the next integer.

M:Q1 Planned Price Change:

"During the next 3 months, the domestic (net) sales prices for product X will—in consideration of changes in conditions—probably [1] increase, [0] roughly stay the same, [-1] decrease."

M:Q1a Realized Price Change:

"During the past month, the domestic (net) sales price for product X—in consideration of changes in conditions— [1] increased, [0] stayed the same, [-1] decreased."

M:Q2 Planned Change in Employment:

"During the next 3 months, the number of employees for the production of product X will [1] increase, [0] roughly stay the same, [-1] decrease."

M:Q3 Current Backlog of Orders:

"We evaluate our backlog of orders for product X as [1] comparatively large, [0] sufficient (typical for season), [-1] too small."

M:Q4 Current Business Situation:

"We evaluate the current business situation for product X as [1] good, [0] satisfactory, [-1] bad."

M:Q5 Expected Business Situation:

"Expectations for the next six months: the business situation for product X will be [1] more favorable, [0] stay approximately the same, [-1] more unfavorable."

M:Q6 Expected Change in Production:

"Expectations for the following three months: the domestic production of product X will [1] increase, [0] stay approximately the same, [-1] decrease."

Supplementary Questions in IBS Used in the Paper:

In addition to the regularly survey questions, I also use the following supplementary questions from both industry-specific surveys of the IBS that have been elicited in the survey waves indicated in brackets:

S1 Export Share [September 2018]:

"What percentage of your sales does your company/firm generate abroad? __%."

S2 Lack of Workers [January 2015]:

S2.1:"Our domestic production is currently constrained: [1] yes, [0] no."

S2.2:"If yes: the constraint is due to a lack of workers: [1] yes"

S3 Credit Constraints [June 2016; defined as credit constrained if one out of options [2] through [4] of S3.2 is reported]:

S3.1: "Have you signed one or more credit agreements with banks in the past twelve months? (e.g., new loans, debt rescheduling, or extension of a loan/line of credit)? [1] yes, [0] no."

S3.2: "If not: Reason: [1] not needed, [2] conditions unacceptable, [3] rejection by bank(s), [4] no realistic chance to reach credit agreement"

References Online Appendix

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