Journal of Public Economics 206 (2022) 104582

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

When the minimum wage really bites hard: The negative spillover effect on high-skilled workers ☆

Terry Gregory [a,b], Ulrich Zierahn [b,c,d]

[a] Institute of Labor Economics (IZA), Bonn, Germany
[b] ZEW – Leibniz Centre for European Economic Research, Mannheim, Germany
[c] Utrecht University, the Netherlands
[d] CESifo Research Network Affiliate, Germany

ARTICLE INFO

Article history:
Received 24 May 2021
Revised 15 November 2021
Accepted 15 December 2021
Available online 18 January 2022

JEL:
J31
J38
J24
C21
J23

Keywords:
Minimum wages
Wage effects
Spillover effects
Returns to skills
Unconditional quantile regression
Scale effect
Substitution effect
Skill supply
Labor-labor substitution

ABSTRACT

Minimum wage levels are rising around the globe. To shed more light on the possible unintended side effects of higher wage floors, we study the impact of a minimum wage introduction on wages and employment in a quasi-experimental setting where the minimum wage is set extraordinarily high during an economic downturn. We identify treatment effects along different wage and skill groups and find positive wage and employment spillover effects for medium-skilled workers with salaries just above the minimum wage. More striking, we find negative wage and employment effects for high-skilled workers who are further up the wage distribution, followed by reduced returns to skills and industry skill supply. We explain these adjustments with a substitution-scale model that predicts negative spillover effects whenever labor-labor substitutions toward high-skilled workers are overcompensated by an overall decline in industry employment. Even though we focus on a specialized industry, we lay out the general conditions under which such unfavorable adjustments may occur in other contexts.

1. Introduction

The existing literature documents positive wage responses to minimum wage increases that may spill over to workers with higher wages. However, there is less agreement on the potential negative side effects in the form of employment reductions (Neumark and Shirley, 2021). As the bulk of the literature focuses on small incremental increases in minimum wages, it remains unclear how labor markets would adjust to more substantial increases. This knowledge gap is particularly relevant in light of rising minimum wage levels in many Western economies. For example, the U.S. administration is preparing to more than double the federal minimum wage from 7.25 to 15 USD by 2025 with its

☆ We thank Jeffrey Clemens, three anonymous referees, Melanie Arntz, Stephan Dlugosz, Bernd Fitzenberger, Heinrich Kögel, Michael Maier, Joachim Möller, David Neumark, and Anna Salomons as well as participants of the seminars at Utrecht University, ZEW Mannheim, University of Regensburg, DIW-Conference on the "Evaluation of Minimum Wages," and the annual congresses of the Society of Labor Economists (SOLE), European Economic Association (EEA), and German Economic Association (VfS) for helpful comments and suggestions. We further thank the Central Pay Office (LAK) of the roofing industry for providing us with hourly wage data. Our paper was partly financed by the ZEW Sponsors' Association for Science and Practice, and by the state of Baden-Württemberg within the SEEK program, and it profited from a preceding evaluation of minimum wage effects in the German roofing industry that was financed by the German Federal Ministry of Labor and Social Affairs (BMAS). The results and conclusions derived in this study do not necessarily reflect the views of the BMAS. This paper builds on an earlier version, "When the Minimum Wage Bites Back: Quantile Treatment Effects of a Sectoral Minimum Wage in Germany," ZEW Discussion Paper No. 14-133.

E-mail addresses: gregory@iza.org (T. Gregory), u.t.zierahn@uu.nl (U. Zierahn)

https://doi.org/10.1016/j.jpubeco.2021.104582
0047-2727/

"Raise the Wage Act of 2021". Similar plans have been laid out in Germany.[1] The adoption of such policies raises the question of whether they may cause unintended side effects, particularly when introduced in the context of an economic downturn. A rising minimum wage bite not only implies that more workers are directly affected by the policy but may further result in unintended side effects for those who are indirectly affected, i.e., whose wages are higher than the minimum wage.

In this paper, we study the long-run effects of a minimum wage introduction on wages and employment for different wage and skill groups in a setting with an extraordinary large bite. We exploit a quasi-experiment provided by a particularly interesting feature of the German construction sector. Specifically, several industry-level minimum wages were introduced in the late 1990s, some of which led to a Kaitz index of 100% over the post-reform years. Such a high Kaitz index can be considered large and is likely to be associated with more negative consequences for workers.[2] The policy was enacted during a long-lasting economic downturn with falling revenues, where the negative consequences of minimum wages are more likely (see Clemens and Wither, 2019). For institutional reasons, the regulations were introduced only in selective industries within the construction sector, one of which was the roofing industry. The outcomes of uncovered yet comparable sub-construction industries thus serve as a counterfactual for the outcomes of the roofing industry in the absence of policy reform.[3]

Based on this unique setting, we establish three key results: First, we use rich administrative data that allow us to follow the approach by Firpo et al. (2009) and compare the unconditional wage distributions of treated and untreated workers before and after treatment, holding constant compositional changes that could result, for instance, from low productive workers leaving the workforce. In addition to a large set of individual and firm-level covariates, we also control for individual fixed effects in our quantile regression estimates, which only recently has become feasible to implement. Overall, we find significant real wage increases of up to 5–6% for lower-quantile workers between 1997 and 2008 as a result of the reform, rippling out to the 60th quantile. More strikingly, the minimum wage also caused a reduction in real wages by up to 5% (stagnation of nominal wages) among the highest-paid employees, suggesting that negative wage spillovers affect high-skilled workers who, hence, receive lower returns to their skills (where skills can be acquired by occupational qualification and experience). To test the identifying assumptions underlying our quantile treatment effects (foremost the common trends assumption), we conduct several checks supporting our findings: we demonstrate parallel trends in pre-treatment years in several specifications, including a dynamic specification and show that our results are robust to alternative control industries as well as to a changes-in-changes estimator proposed by Athey and Imbens (2006) that relies on the assumption of common changes as opposed to common trends.

[1] The German government released its plans on March 05, 2021 for an increase of the national minimum wage from 9.50 to 12 EUR in 2022, see "Eckpunkte zur Weiterentwicklung des Mindestlohns und Stärkung der Tarifbindung."

[2] For example, recent evidence for Seattle, where a relatively large increase in the minimum wage took place, shows significant negative effects on hours worked, effectively reducing the earnings of low-wage workers despite higher wages (Jardim et al., 2018b). Kreiner et al. (2020) further study a significant increase in the Danish youth minimum wage of 40% and find employment reductions of 33% and reduced hours worked by 45% at the discontinuity.

[3] This quasi-experiment has been exploited previously by Aretz et al. (2013) for the empirical assessment of the minimum wage on the probability to remain employed, but the authors do not assess the further implications for wages and industry employment (by different wage and skill groups) or the returns to skills and skill supplies. Moreover, in contrast to our paper, they do not provide any theory and corresponding testing.

Second, we develop a labor market model with labor-labor substitution and a scale effect that explains how a minimum wage can induce both positive and negative spillovers for workers with different skills. In our model, spillover effects are moderated by two adjustments to the minimum wage: (1) Firms substitute low- for medium-skilled workers, but not high-skilled ones, as only medium-skilled workers' tasks provide close substitutes for those with low skills. This feature is in line with distance dependent elasticity of substitution (DIDES) models, where minimum wage-induced substitution effects fade out at the top of the wage distribution (see, e.g., Teulings, 2000). (2) The minimum wage-induced cost shock to the industry leads to a decline of employment within the industry, as higher costs and prices reduce demand and production. The net effect on high-skilled workers is thus negative, whereas the effect on medium-skilled employees depends on the relative size of the scale and substitution effect. Both adjustments are moderated by the bite of the minimum wage. Altogether, our theoretical model allows us to lay out the general conditions under which negative spillovers can occur.

Third, we empirically test our model's predictions for minimum wage spillovers. In particular, we estimate the minimum wage effects on total industry employment and skill-specific employment shares in order to empirically quantify the scale and substitution effects for each skill group together with their net effects. We do this within a regional labor market approach, which is suited to capture such aggregate effects. In line with our theoretical framework, we find that positive wage spillovers to medium-skilled workers are driven by a positive substitution effect that overcompensates a negative scale effect, resulting in a net positive effect on employment and wages among medium-skilled workers. In contrast, the negative wage spillovers that impact high-skilled workers are the result of a negative scale effect without any substitution toward these workers, accompanied by negative net employment effects for them.

As a result of declining returns to skills in the industry, we further find a negative effect of the minimum wage policy on the share of high-skilled entrants. The adjustments we find only occur when the minimum wage bite is high, namely in East Germany, where the bite (as measured by the Kaitz index) reached values of 100% during our observation period. In contrast, we do not find evidence for larger adjustments in West Germany, where the bite is much smaller. While our findings can be best explained by our proposed substitution and scale model, we further test and discuss alternative mechanisms that may rationalize our findings, including unionization or changes in capital-labor substitution. Finally, we show under which circumstances one can expect such unfavorable adjustments to occur in other contexts.

Our paper contributes to several strands of the literature. First, we contribute to the empirical literature on minimum wage spillovers and labor-labor substitution. Existing studies stress that minimum wage effects can spill over and affect workers with earnings above the minimum wage (see for instance Gramlich et al., 1976; Grossman, 1983; Lee, 1999; Manning, 2003; Teulings, 2003; Neumark et al., 2004; Dickens and Manning, 2004; Autor et al., 2016; Cengiz et al., 2019; Gopalan et al., 2020). According to this research, wage floors create a spike in the wage distribution at the minimum wage and boost the wages of workers who earn somewhat more than the threshold. Depending on the bite, the effects then ripple out to wages at about 20% above the minimum wage level (Neumark and Wascher, 2008). In line with this evidence on wage spillovers, other studies demonstrate (albeit partly for specific groups and industries) that minimum wages induce labor-labor substitutions toward workers with higher productivity, experience, education, skills, or other favorable characteristics (Giuliano, 2013; Horton, 2018; Jardim et al., 2018a; Clemens and Wither, 2019; Clemens et al., 2021). In contrast, few empirical studies have discussed the existence of *negative* spillovers for workers higher up in the skill distribution. Among the few exceptions, Neumark et al. (2004) find negative wage adjustments among better-paid workers in response to the minimum wage in the United States and briefly argue that such negative spillovers may arise through scale effects, but do not further test this hypothesis. Apel et al. (2012) and Aretz et al. (2013) reveal indications of upper-tail wage compression in the aftermath of the minimum wage introduction in the German main construction and roofing industry but also do not causally test and explain these findings. We add to this literature by showing how a relatively high minimum wage can induce *negative* spillover effects for workers situated higher up in the wage distribution.

Second, we contribute to the theoretical literature on minimum wage spillovers. Conventional explanations for minimum wage spillovers put forward by researchers in prior studies state that (1) firms substitute unskilled with skilled labor as a reaction to the change in relative input prices (Hamermesh and Grant, 1979; Pettengill, 1981; Hamermesh, 1996), (2) firms adjust their wage structure to maintain an internal wage hierarchy and hence fairness perceptions, motivation, and effort among highly-paid employees (Grossman, 1983; Falk et al., 2006; Dube et al., 2018), and (3) firms that previously paid relatively high wages to attract workers must increase wages too, in order to recruit enough new employees (Manning, 2003). (4) More recently, Phelan (2019) argues that minimum wages raise the wages of low-wage jobs relative to undesirable albeit higher paid jobs, reducing labor supply for the latter. This decline, in turn, leads to higher wages for these workers, reflecting positive wage spillovers among higher paid jobs. All these mechanisms lead to increasing demand and higher wages for workers with earnings above the minimum wage. While Teulings (2000) has shown that such positive spillover effects can fade out at the top of the earnings distribution, none of these papers can explain negative spillovers for top earners. Our model extends the empirical and theoretical literature by allowing for negative minimum wage spillovers and by showing under which conditions spillovers may be either positive or negative.

Third, we also complement research on the effects of minimum wages on human capital formation. Among others, this research suggests that minimum wages reduce enrollment in high school (Neumark and Wascher, 1995; Neumark and Wascher, 2003; Chaplin et al., 2003; Lee, 2020) and in post-secondary education (Pacheco and Cruickshank, 2007; Alessandrini and Milla, 2021). One reason put forward by this literature is that higher entry wages discourage investments in skills. Moreover, some studies find that minimum wages lower participation in training (Hashimoto, 1982; Fairris and Pedace, 2004; Schumann, 2017), while others find no effects on training (Grossberg and Sicilian, 1999; Acemoglu and Pischke, 2003; Arulampalam et al., 2004). A potential explanation for these mixed findings is that the effects depend on the type of worker or training (Neumark and Wascher, 2001; Lechthaler and Snower, 2008). We add to this literature by providing an alternative mechanism: a substantial rise in the minimum wage can depress the returns to skills through declining wage and employment prospects and, in turn, discourage highly skilled individuals from entering the industry.

The structure of the paper is as follows. In Section 2, we present a substitution-scale model that can explain negative spillovers. In Section 3, we describe the institutional setting used for our quasi-experiment. In Section 4, we introduce the data and describe the minimum wage bite in our high-bite industry. In Section 5, we estimate the minimum wage effects on the distribution of earnings and returns to skills together with several robustness checks. In Sections 6 and 7, we empirically test the mechanisms proposed by our theory with respect to the substitution and scale effect and skill supply within a regional approach and test alternative explanations. Finally, Section 8 concludes.

2. Theoretical Framework

In this section, we develop a simple, stylized labor market framework with labor-labor substitution and a scale effect to explain how a minimum wage can lead to adjustments not only among low-skilled workers but also among medium and high-skilled workers located higher up in the earnings distribution (minimum wage spillovers). The model also has implications for adjustments in the returns to skills and skill supply. In the following, we briefly summarize the main assumptions of the model and derive predictions regarding the adjustments to the minimum wage (for details, see Appendix A.1).

2.1. Model and Main Assumptions

There are I firms in a given industry, producing varieties q_i of the industries' final output Q under monopolistic competition. Firms require a fixed high-skilled labor input $h_i = f$ as well as a variable labor input $n_i = \varphi q_i$, where $1/\varphi$ is labor productivity. The variable labor input n_i is composed of low and medium-skilled workers, l_i and m_i. Note that skills are defined as fixed individual attributes that are rewarded on the labor market. We focus on three skill groups for simplicity. Low, medium, and high-skilled workers earn wages w_L, w_M, and w_H, respectively. We use $\bar{w}$ as the wage cost index for the variable labor input (which is composed of medium and low-skilled workers). Firms can replace low for medium-skilled workers with constant elasticity of substitution η, whereas high-skilled workers provide no close substitute for low- and medium-skilled labor. This assumption is comparable to DIDES models (see, e.g., Teulings, 2000), where low-skilled tasks are more easily substituted with medium-skilled tasks than high-skilled tasks, so that the substitution effects fade out at the top of the skill and wage distribution. Our assumption aligns with the empirical literature on spillover effects, which finds substitution only toward workers who earn slightly above the minimum wage but not toward high-wage workers. In our case, this is particularly true due to industry regulations that require firms to be run by high-skilled workers (e.g., master craftsmen or vocationally-trained workers with sufficient work experience), thus widening the gap between low- and high-skilled workers' tasks. Finally, this assumption is justified by the fact that we do not find substitution effects toward high-skilled workers in our empirical analysis.[4]

Consumers have constant elasticity of substitution (CES) preferences for the varieties i produced by the firms with an elasticity of substitution $\sigma > 1$ between the varieties. We further assume that the industry sells its output to the rest of the economy and is too small to affect the size of the economy. Demand for the overall output of the industry is price sensitive with the constant price elasticity of demand $\epsilon < 1$. The market is governed by monopolistic competition among homogeneous firms and free entry.[5]

We solve the model as a flow equilibrium, where at each time instant t, a share δ of each type of worker exogenously retires. The supply of low-skilled workers greatly exceeds demand.[6] This

[4] Relaxing the assumption of no substitution effects toward skilled workers would allow for potential positive spillover effects of minimum wages toward skilled workers at the cost of raising model complexity. We abstract from this complexity as our empirical analysis reveals that there are no such substitution effects toward high-skilled workers.

[5] We assume free entry to avoid making the model unnecessarily complex. Introducing Melitz (2003)-type entrance costs and firm heterogeneity does not change the main results.

[6] This assumption is motivated by the high unemployment rate among low-skilled workers.

implies that they earn their reservation wage $\underline{w}$, unless there is a minimum wage w_{MW} that exceeds their reservation wage, $w_L = \max(\underline{w}, w_{MW})$. For simplicity, we assume that the minimum wage is binding only for low-skilled workers ($\underline{w} < w_{MW} < w_M, w_H$). At each time instant t, a mass of medium- E_M and high-skilled entrants E_H supply labor with wage elasticity of labor supply θ. In the steady state, the inflow (entrants) of workers equals the outflow (retirement), and expected wages correspond to actual wages. We abstract from any wage-setting frictions; wages adjust until labor supply equals labor demand, which implies no unemployment among medium- and high-skilled workers.

Using these assumptions, we show in Appendix A.1 that we can solve for medium- and high-skilled workers' equilibrium wages:

$$w_H = \left(\frac{\delta}{\sigma E_H}\right)^{1/\theta} Q_0^{\frac{\tilde{\epsilon}/\epsilon}{\theta}} \bar{w}^{-\tilde{\epsilon}/\theta} \quad (1)$$

$$w_M = \left(\frac{\delta(\sigma-1)}{\sigma E_M}\right)^{1/(\theta+\eta)} Q_0^{\frac{\tilde{\epsilon}/\epsilon}{\theta+\eta}} \bar{w}^{\frac{\eta-\tilde{\epsilon}}{\theta+\eta}}, \quad (2)$$

where Q_0, E_M, and E_H are constants and $\tilde{\epsilon} = \epsilon\frac{\sigma-1}{\sigma-\epsilon}$ is the overall price elasticity of industry-level output. Jointly with low-skilled wages $w_L = w_{MW}$ and the CES wage cost index $\bar{w}$, these equations establish the equilibrium in our industry.

2.2. Adjustments to the Minimum Wage

Based on the above model assumptions, we can derive the following propositions concerning the minimum wage adjustments:

Proposition 1 (*Scale Effect*). *The introduction or increase of a minimum wage*

a) raises average wage costs $\frac{\partial \ln \bar{w}}{\partial \ln w_{MW}} > 0$ and
b) reduces industry-level employment $\frac{\partial \ln N}{\partial \ln w_{MW}} < 0$.

Proof. Using $\frac{\partial \ln \bar{w}}{\partial \ln w_{MW}} = (1-\alpha)\frac{\partial \ln w_M}{\partial \ln w_{MW}} + \alpha$ (where α is the steady-state cost share of low-skilled workers) and the equilibrium medium-skilled wage (2), we derive $\frac{\partial \ln \bar{w}}{\partial \ln w_{MW}} = \frac{(1-\alpha)(\theta+\eta)}{\theta+(1-\alpha)\eta+\alpha\tilde{\epsilon}}$. This is strictly positive for $0 < \alpha < 1, \theta > 0, \eta > 0, \tilde{\epsilon} > 0$, showing that the introduction or increase of a minimum wage raises average wage costs.

However, rising wage costs imply a decline in demand for the variable labor input: $\frac{\partial \ln N}{\partial \ln w_{MW}} = -\tilde{\epsilon}\frac{\partial \ln \bar{w}}{\partial \ln w_{MW}}$. □

The intuition of the scale effect is as follows: The minimum wage for low-skilled workers is a cost shock that increases average wages (Proposition 1.a). This change implies that the prices charged by the industry increase, such that it can sell less to the rest of the economy due to the negative slope of industry product demand, i.e., output and net employment decline (Proposition 1.b). The size of the negative scale effect depends on the price elasticity of industry level output $\tilde{\epsilon}$ and the size of the minimum wage. The more elastic consumers respond to price changes, the larger the negative effect of a minimum wage-induced cost shock on the industry. For instance, if consumers are more price-sensitive, such as during recessions, the scale effect is larger. Moreover, the stronger the minimum wage bite, the larger the cost shock to the industry and the subsequent disemployment effects.

Proposition 2 (*Substitution Effect*). *The introduction or rise of a minimum wage*

a) reduces the wage of medium- relative to low-skilled workers $\frac{\partial \ln w_M/w_L}{\partial \ln w_{MW}} < 0$ and
b) raises the share of medium-skilled workers $\frac{\partial \ln M/N}{\partial \ln w_{MW}} > 0$.

Proof. The implications for medium-skilled workers' relative wages are (using the result for average wages as before) $\frac{\partial \ln w_M/w_L}{\partial \ln w_{MW}} = -\frac{\tilde{\epsilon}+\theta}{\theta+(1-\alpha)\eta+\alpha\tilde{\epsilon}}$. This impact is strictly negative for $0 < \alpha < 1, \theta > 0, \eta > 0, \tilde{\epsilon} > 0$, showing that the minimum wage reduces the wages of medium- relative to the low-skilled workers. The decline of medium-skilled workers' relative wages implies an increase in their employment share: $\frac{\partial \ln M/N}{\partial \ln w_{MW}} = -\eta\frac{\partial \ln w_M/w_L}{\partial \ln w_{MW}}$.

Intuitively, the minimum wage implies a rise in the relative costs for low- relative to medium-skilled workers (Proposition 2.a), leading to an increase in the share of relatively cheaper medium-skilled workers (Proposition 2.b). The effect grows stronger the larger the elasticity of substitution between worker types η and the more substantial the minimum wage bite. The reason is that a higher elasticity of substitution between worker types implies that it is easier for firms to replace the relatively more expensive low- with medium-skilled workers. In contrast, the ratio of high-skilled labor input to the variable labor input (of medium- and low-skilled workers) remains constant—there is no substitution favoring high-skilled workers in our model, as their tasks are less comparable to those of low-skilled workers.[7] This is in line with the empirical literature, which typically finds positive wage spillovers only for workers who earn slightly above the minimum wage but not for top earners (see Introduction).

Proposition 3 (*Net Effect*). *The introduction or increase of a minimum wage*

(a) raises (reduces) medium-skilled workers' wages and
(b) raises (reduces) medium-skilled employment

if the elasticity of the substitution of workers η exceeds (is lower than) the industry product demand elasticity $\tilde{\epsilon}$.

Irrespective of the relative size of these two elasticities, the introduction or increase of a minimum wage

(c) reduces the wages of high-skilled workers $\frac{\partial \ln w_H}{\partial \ln w_{MW}} < 0$ and
(d) reduces the employment of high-skilled workers $\frac{\partial \ln H}{\partial \ln w_{MW}} < 0$.

Proof. We derive $\frac{\partial \ln w_M}{\partial \ln w_{MW}} = \frac{\eta-\tilde{\epsilon}}{\theta+\eta}\frac{(1-\alpha)(\theta+\eta)}{\theta+\eta-\alpha(\eta-\tilde{\epsilon})}$. For $0 < \alpha < 1, \theta > 0, \eta > 0, \tilde{\epsilon} > 0$, this is positive (negative) if $\eta > \tilde{\epsilon}$ ($\eta < \tilde{\epsilon}$). The employment effect is analogous due to $\frac{\partial \ln M}{\partial \ln w_{MW}} = \theta\frac{\partial \ln w_M}{\partial \ln w_{MW}}$.

We take the first derivative of high-skilled equilibrium wages (1) w.r.t. $\ln w_{MW}$, which yields $\frac{\partial \ln w_H}{\partial \ln w_{MW}} = -\frac{\tilde{\epsilon}}{\theta}\frac{(1-\alpha)(\theta+\eta)}{\theta+(1-\alpha)\eta+\alpha\tilde{\epsilon}}$. This is strictly negative for $0 < \alpha < 1, \tilde{\epsilon} > 0$ and $\eta > 0$. Wages among high-skilled workers decrease, which implies that high-skilled employment declines due to the positive slope of the labor supply curve of high-skilled workers.

The introduction or increase of a binding minimum wage for low-skilled workers thus might raise or reduce the wages (Proposition 3.a) and employment (Proposition 3.b) of medium-skilled workers, depending on whether the substitution or scale effect dominates. The net effect is governed by the relative size of the

[7] This result is due to the fixed high-skilled labor input assumption, which operates jointly with homogeneous firms and free entry.

elasticity of substitution between worker types η and the price elasticity of industry product demand $\tilde{\epsilon}$. The net effect on high-skilled workers' wages (Proposition 3.c) and employment (Proposition 3.d) is always negative—they suffer from the negative scale effect but do not gain from the compensating substitution effects.[8]

Corollary 1 (*Returns to Skills and Skill Supply*). *The introduction or increase of a minimum wage*

(a) raises the entrance of medium- relative to low-skilled workers,
(b) reduces high-skilled workers' wages relative to medium-skilled workers' wages $\frac{\partial \ln w_H/w_M}{\partial \ln w_{MW}} < 0$, and
(c) reduces the employment and entrance of high-skilled workers relative to medium-skilled workers.

Proof. In the long run, the effect on the ratio of the entrance of medium- to low-skilled workers is analogous to the effect on the ratio of medium- to low-skilled employment and thus directly follows from Proposition 2.

We use the results from Proposition 3 jointly with the results from Proposition 1 to derive $\frac{\partial \ln w_H/w_M}{\partial \ln w_{MW}} = -\frac{\eta}{\theta}\frac{(1-\alpha)(\tilde{\epsilon}+\theta)}{\theta+(1-\alpha)\eta+\alpha\tilde{\epsilon}} < 0$.

The results for the effects on the high-to-medium-skilled employment ratio are proportional to the relative wage effects, $\frac{\partial \ln H/M}{\partial \ln w_{MW}} = \theta \frac{\partial \ln w_H/w_M}{\partial \ln w_{MW}} < 0$. The effects on the corresponding entrant ratio are analogous. □

The effects on skill supply are analogous to the long-term employment effects. As firms employ a larger share of medium-skilled workers in the long-run flow equilibrium, the entrance of medium- relative to low-skilled workers grows. Moreover, the wages of high-skilled workers decline relative to those of medium-skilled workers, as the former do not profit from substitution effects—the returns to skills decline. This shift is associated with a decrease in the supply of high-skilled relative to medium-skilled workers, which indicates the growing difficulty of attracting high-skilled workers in a given industry.

Overall, our model provides two main contributions. First, we extend the labor-labor substitution model to include a scale effect. By doing so, our model not only explains why medium-skilled workers alone profit from positive substitutions effects, whereas high-skilled do not (similar to the continuous version of Teulings (2000), where spillovers fade out at the top of the earnings distribution). It also explains how net spillovers become negative due to a minimum wage-induced decline in product demand and a net overall employment decline in the industry. The latter scale effect is a missing link that may be of particular importance when studying minimum wages with a large bite in the context of an economic downturn with falling revenues.[9] Second, our model is also able to explain how a minimum wage can reduce returns to skills and ultimately hamper skill supply. We are unaware of any framework to date that studies such adjustments.[10]

3. Institutional Setting

3.1. Minimum Wage Regulations

Until the introduction of a national minimum wage in 2015, German minimum wage regulations were organized at an industry level. The first industry-specific minimum wages were introduced in 1997 by three industries within the construction sector, including the main construction industry, the roofing industry and the electric trade industry. After 1997, further industries within and beyond the construction sector decided to implement a minimum wage. Industry-specific minimum wages typically differ between East and West Germany, reflecting the large wage difference between the two parts of the country. According to the association of employers (National Association of Roofers, ZVDH), there are two main reasons for the introduction of a minimum wage: first, to protect traditional craft industries in Germany against increasing cost pressure from cheap East European labor[11]; and second, to reduce significant wage differences, especially between East and West Germany.

Not all industries in the construction sector agreed to implement a minimum wage in the mid-1990s because tariff agreements are negotiated at an industry level in Germany. More specifically, the introduction of industry-specific minimum wages depends on industry-specific negotiations between the respective trade unions and employer associations. In addition to differences in the negotiation processes, industries also vary in terms of spatial organization. For example, regulations are adopted at a national level in some industries, including the roofing industry, the main construction industry, and the electric trade industry. In other industries, including the plumbing industry, collective-bargaining competence must be delegated from the regional to the national level. This feature makes policy implementation more difficult, explaining why minimum wages were introduced in some industries but not others. Note that differences in the spatial organization of negotiations did not change over time.[12] The evaluation of the minimum wage at an industry level thus provides the opportunity to compare similar industries within a quasi-experiment. Among these, the roofing industry comprises a particularly interesting case for two reasons: (1) the minimum wage was among the first to be introduced in Germany, allowing us to study its effects over a long period; (2) the minimum wage level was exceptionally high, as discussed in Section 4.2. For all these reasons, we focus on roofing for our quasi-experiment.

The minimum wage in the roofing industry was introduced in October 1997. As part of a general collective-bargaining agreement, the responsible trade union (Trade Union for Building-Agriculture-Environment, IG BAU) and the ZVDH agreed on a minimum wage of €8.2 in West and €7.7 in East Germany. All blue-collar

8 The lack of substitution favoring high-skilled workers is an assumption of our model that is guided by our empirical results—where we do not find such substitution—and is broadly in line with DIDES-type models, where the spillover effects of minimum wages fade out at the top of the wage distribution, see, e.g., Teulings (2000).

9 The existing minimum wage literature highlights that labor markets might respond via margins other than wages and employment. For instance, in his survey, Clemens (2021) points to job attributes and non-wage benefits such as health insurance. Workers in our industry have statutory health insurance, so that no adjustment along that variable is possible. More generally, the industry in our analysis is a low-wage industry where non-wage compensation is uncommon. Clemens (2021) highlights that a decline of firm profits is likely when no other margins of adjustment are available, resulting in intensified firm exits. This finding can potentially explain negative scale effects when the firms, which face tight competition in the roofing industry, reduce profits and ultimately exit the market.

10 Several authors study the role of minimum wages in relation to training, reporting mixed evidence (see above) and two opposing mechanisms (Acemoglu and Pischke, 1999). On the one hand, minimum wages hinder workers from temporarily compensating firms for training costs via lower wages, reducing incentives to gain skills. On the other hand, minimum wages lead to wage compression, leading to the substitution of trained workers whose wages decline relative to untrained workers. We complement this literature by showing that there is a scale and substitution effect that differs between skill groups, explaining why the minimum wage has heterogeneous effects for differently skilled workers.

11 In Section 5.3, we find that our results are not driven by posted workers in light of the free movement of workers in the EU.

12 If there were deviations, they would show up as pre-trends, which we extensively examine in our empirical analysis. We also demonstrate that union membership did not change between treated and control industries (see Section 6.3).

workers, including those who are marginally employed[13] in the roofing industry, are covered by minimum wage regulations. The application of the regulations thereby depends on the tasks of workers and not the scope of employment. The location of the firm (in Germany or abroad) does not play a role. Apprentices, cleaning staff, and white-collar workers are exempted from the regulations. Since 1997, the minimum wage has been raised repeatedly (see Table 2 in Section 4.2).[14]

The greatest increase occurred in March 2003 for East Germany, when trade unions and employers agreed on a national minimum wage of €9. Periods with no minimum wage regulations are the result of tariff agreements that expired before the new regulations came into force. The interruptions were short, and the continuation of the minimum wage was expected—consequently, firms did not decrease wages during these periods.[15]

3.2. Selection of Control Industries

Roofing is an industry (or sub-sector) within the construction sector. It is considered a traditional craft that provides services that include the installation of roofs on new buildings for public and private clients, repairing roofs and energy-efficient upgrading, and the installation of solar collectors. To identify potential control industries, we build on a previous evaluation study by Aretz et al. (2011) that provides an extensive and systematic analysis in search of similar industries. The main criteria guiding the selection of potential control industries were that they should (1) not be subject to any minimum wage regulation, (2) conduct similar tasks, (3) depend similarly on the business cycle of the wider construction sector, (4) have a similar market structure, and (5) not be vulnerable to spillovers from the roofing industry.

The main construction industry and the electric trade industry can be ruled out as potential control industries, as they also introduced a minimum wage in 1997. Remaining industries with similar tasks include plumbing, glazing, and painting (for details, see Aretz et al., 2011). Since the painting industry introduced a minimum wage in 2003, they cannot provide a counterfactual for the evaluation of long-run effects. Overall, we choose plumbing and glazing as preferable control industries for our difference-in-differences analyses for a number of reasons—note that although we generally identify employees by the industry coding of the firm, we use the terms such as roofing and roofers interchangeably throughout the paper.[16]

First, similar to the roofing industry, both are part of the construction sector and therefore share many basic characteristics—the most important features are discussed below. In contrast to the roofing industry, however, the control industries did not introduce a minimum wage in the early 1990s due to mainly political reasons. Thus, these industries generally reflect a counterfactual situation for the roofing industry without the introduction of a minimum wage.

Second, with few exceptions, both control industries experienced very similar business cycle trends (see Fig. 1), namely a severe and long-lasting economic downturn in the aftermath of the boom period in the 1990s. Decreasing investments in housing and industrial buildings resulted in decreasing sales and revenues that led firms to increasingly lay off workers, especially in East Germany.

Third, the plumbing and glazing industries are very similar to the treated roofing industry in terms of their market structure. Building on Aretz et al. (2011), Table 1 contrasts some important indicators for the selected industries during the year before the policy reform. Overall, the comparison shows a very similar market size for roofers and plumbers in terms of the number of firms and their revenues. Most firms operating in these industries are relatively small. Compared to other industries, our treated and control industries are highly regulated, as reflected by the fact that a master craftsman diploma is required to offer one's services on the market.[17] Moreover, with a share of craftsmen and trained workers of around 70%, roofing firms operate with relatively well-trained staff.[18] This share is also very high in our control industries. The number of companies per €1 million in industry revenues, as a measure of competition, is the same in roofing and plumbing. Also, value added, investments per employee, and labor cost shares among roofing firms are very similar to those in plumbing.

Fourth, the potential for spillovers between these industries is low. Among others, these industries are highly regulated and have very specific skill requirements. Consequently, workers cannot simply switch jobs among them, which is particularly true for trained workers. However, even for untrained workers, opportunities for roofers to find better local employment in one of the control industries were low during the investigated time period due to the severe economic downturn in the entire construction sector. In fact, we find few worker transitions among these industries (see Section 4, where we discuss the identification assumptions of our empirical approach). Moreover, it is very unlikely that East German roofers took advantage of the more stable West German economy due to the generally low residential mobility of roofers.

Overall, the evidence suggests that our selected control industries are generally very similar to our treated industry in terms of market conditions. In fact, we find that our main outcome variables follow common trends in the treatment and control industries before the introduction of the minimum wage, as we show in Section 5.3. Note that we use both plumbing and glazing as control industries in our baseline specification, but we test the robustness of our results using plumbing separately as the control industry.

4. Data and Descriptive Statistics

4.1. Administrative Linked Employer-Employee Data

We use linked employer-employee data from the Institute for Employment Research (IAB) as our main data source. This source

[13] Marginal employment or minijobs refers to special employment relationships (geringfügige Beschäftigung) in Germany that are exempted from income tax. They are characterized by low earnings (the threshold is around €450 per month) and are mostly part-time jobs.

[14] For a more detailed visualization of the minimum wage levels, including interruption periods, see Appendix Fig. A.2.

[15] Inspections of hourly wages on a monthly basis (LAK data, see Section 4.1) for these periods by quantile of the hourly wage distributions show no downward adjustments of hourly wages for quantiles 0.1, 0.25, 0.5, 0.75, or 0.9, see Appendix A.6 for details.

[16] The following industry coding identifies the industries: roofing (WZ93/WZ03: 45.22.1), plumbing (WZ93: 45.33.1 and 45.33.2./WZ03: 45.33.0), glazing (WZ93/WZ03:45.44.2). Note that building construction, including building bridges and tunnels, was initially among the potential control industries, but it turned out to show a very different economic trend.

[17] The master craftsman ("Meister") diploma is the highest professional qualification in crafts industries. The requirements to become a master craftsman are usually an education in the crafts, the passage of an examination (following a successfully completed apprenticeship), and at least three to five years of experience as a journeyman ("Geselle"). Only then can training courses for the Master's examination ("Meisterprüfung") be taken. The duration of the courses depends on the craft and can take four to six years.

[18] These numbers refer to occupational qualifications that were aggregated into three categories (for the full spectrum of categories, see footNote 29): untrained workers, trained workers who successfully completed an apprenticeship training program, and workers who also obtained a master craftsman's certificate. We document numbers on occupational qualification for illustrative purposes, although we focus on the role of skills in our paper. By "skills," we mean workers' industry-specific productivity—this not only depends on their occupational qualification, but their abilities, experience, and motivation.

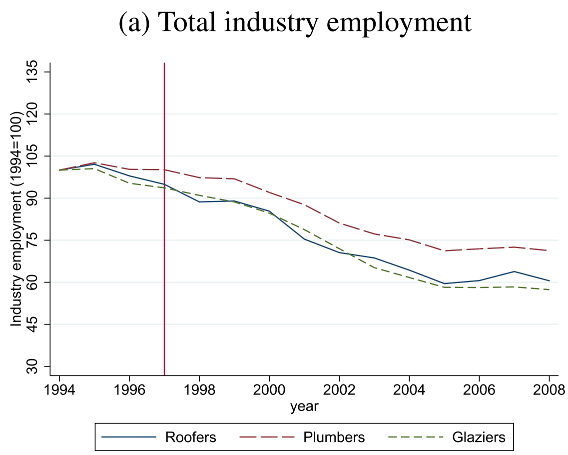

Fig. 1. Business Cycle Trends for Treated and Control Industries. Notes: Revenues are taken from the German sales tax statistics provided by the Federal Statistical Office. Employment figures are based on the BA data (see Section 4.1). The vertical lines represent the introduction of the minimum wage in October 1997.

Table 1
Various Economic Indicators for Roofers and Selected Control Industries.

	Treated	Untreated		
	Roofers	Plumbers	Glaziers	Source, year
	(1)	(2)	(3)	(4)
Number of firms	10,958	34,650	3,305	A, 1996
Number of employees	87,170	235,070	16,065	A, 1996
Avg. number of employees per establishment	7.9	6.7	4.8	A, 1996
Avg. gross daily wage/full-time employee (in €)	66.2	68.6	66.3	A, 1996
Avg. establishment employee structure: (in %)				A, 1996
untrained	29.3	11.1	19.3	
trained	66.7	83.4	73.9	
master craftsmen	3.7	5	6.1	
part-time workers	0.2	0.2	0.3	
Share of companies by revenues (in 1,000):				B, 1996
< 100 DM	6.8	8.8	13.6	
100–500 DM	24.6	33.7	42.8	
500–1,000 DM	26.1	23.5	21.5	
1,000–2,000 DM	25.1	19.3	13.5	
$> 2,000$ DM	17.4	14.6	8.5	
Number of companies/1 Mio. industry revenue	1.4	1.5	2.2	B, 1996
Value added in € per employee	37,195	35,949	32,931	C, 2001
Share of labor costs (in %)	36	32.5	49	C, 2001
Investments/employee (in €)	1,472	1,229	2,482	C, 2001

Notes: A - BA data, sub-samples projected to 100%, statistics based on a selected sample of covered workers, for details see Section 4.1; B - German sales tax statistics of the German Federal Statistical Office (Umsatzsteuerstatistik); C - Cost Structure Survey of the German Federal Statistical Office (Kostenstrukturerhebung).

matches firm data from the IAB Establishment History Panel (BHP) with individual-level data drawn from Integrated Employment Biographies (IEB). Both are generated via labor administration and social security data processing. The data contain all workers subject to social security contributions by their employers. We have access to IEB sub-samples for roofers (75%), plumbers (30%), and glaziers (75%).[19] The data include individual employment histories for these workers on a daily basis, including several worker characteristics such as age, sex, occupational status, gross daily wages, occupational qualification, and a firm identifier. The firm-level BHP data consist of information on the firm's workforce structure, including the number of workers in certain occupational qualification groups. For the analysis, we use annual cross-sections at the cut-off date of June 30th. We focus on male workers aged 19–65 and drop marginal employment (recorded in the data only from 1999 onward), apprentices, and white-collar workers, who are not covered by the minimum wage regulations.[20] In total, we are left with 788,611 yearly observations for 171,190 roofers and 1,059,475 observations for 233,024 workers from uncovered control industries (plumbing, glazing) for the time period 1994–2008.

The advantage of the BA data is that they allow us to conduct comparisons of daily wage developments between treated and untreated industries. Since the BA data do not include information on hours worked, we further exploit a full sample of all roofers provided by the Central Pay Office (Lohnausgleichskasse, LAK) of the roofing industry. The LAK data collect, among others, monthly information from firms on the number of actual working hours for each worker as well as their gross wages from the year 1995 onward. Since the reporting is mandatory for firms, which may receive a penalty for non-compliance, the information is highly likely to comprise all blue-collar roofers. In total, we are able to exploit 1,055,137 June observations for 206,753 roofers across the period 1995–2010. However, we do not choose LAK as the main

[19] The data also include painters, which we do not further exploit here as they introduced a minimum wage in 2003, precluding any long-term analyses.

[20] Although we drop white-collar workers from our primary analysis, we conduct separate regressions for this group that confirm the presence of a negative scale effect for uncovered workers. See Section 6.2.

Table 2
Indicators of the Minimum Wage Bite.

Date of next minimum wage regulation	West Germany		East Germany	
	Hourly minimum wage level in € (1)	Kaitz index in % (2)	Hourly minimum wage level in € (3)	Kaitz index in % (4)
01.10.97	8.2	65	7.7	82
01.09.01	8.9	67	8.4	89
01.03.03	9.0	67	9.0	95
01.04.04	9.3	68	9.3	98
01.05.05	9.6	70	9.6	99
01.01.06	10.0	73	10.0	100
01.01.07	10.0	73	10.0	100
01.01.08	10.2	73	10.2	101
01.01.09	10.4	73	10.4	100

Notes: The Kaitz index is defined as the minimum wage level divided by the median hourly wage. Figures are calculated in June prior to the next minimum wage regulation and are based on LAK data.

data source because it is only available for roofers, which precludes comparisons between treated and untreated industries. Nevertheless, we use the LAK data to calculate the minimum wage bite, hourly wages, and hours worked in the roofing industry. In particular, we use the East–West specific minimum wage bite (see next section) in our regional regressions in Section 6. Details of the LAK data set are reported in Appendix A.4.

4.2. Minimum Wage Bite and the Distribution of Earnings

In the following section, we provide descriptive facts on the bite and trends in earnings. The bite of the minimum wage was relatively high in the roofing industry (see Table 2, more details in Appendix A.3): In East Germany, the Kaitz index was 82% when it was introduced in 1997, rising to 100% in 2006. That is, the minimum wage equals the median wage in 2006. These numbers are large, as demonstrated by a comparison to the average bite among OECD countries of around 50%, ranging between 30% and 70% in most countries in 2018 (see Fig. 7 in Appendix A.3). Note that zooming in on low-wage industries generally leads to larger Kaitz indexes for those cases, which makes them particularly interesting when studying the consequences of a high minimum wage, despite challenges related to generalizability. The figures also exceed those of other high-bite cases, such as Machin et al. (2003), who focus on the strongly affected low-wage care industry in the UK.[21] Another example is provided by Freeman and Freeman (1991), who study the case of Puerto Rico, where the ratio of the minimum wage to average manufacturing wages peaked at 75%. Other examples include sector-specific minimum wages in Germany, where the Kaitz indexes exceeded 80% in the main construction industry and the electric trade industry, as measured six years after the reform (Ganserer et al., 2021). However, all these values lie below the bite of the East German roofing industry. With a Kaitz index of 65% in the year of its introduction (rising to 73% in 2006), the bite was much lower in West Germany.

We expect the effect of the minimum wage to explain the significant changes in the earnings distribution that we see in Fig. 2. Accordingly, the minimum wage coincides with a strong compression of hourly wages both at the bottom and the top of the distribution. The wage compression at the top thereby reflects nominal wage stagnation. In contrast, we do not observe larger changes in West Germany, where the bite was low. These descriptive findings are striking because wage compression at the top can have negative long-term consequences such as declining returns to skills and reduced incentives to invest in skills. The strong compression at the bottom and top of the East German earnings distribution is thereby not driven by adjustments in hours worked. To demonstrate this finding, we inspect trends in monthly wages and hours worked for selected quantiles of the hourly wage distribution (see Fig. 8 in Appendix A.6). Whereas wages (both hourly and monthly) show a compression comparable to Fig. 2, monthly hours worked follow comparable patterns across wage quantiles. The only exception is the lowest quantile in West Germany, which we discuss in Appendix Section A.6. Regarding compliance, we find that actual wage growth lags somewhat behind the level necessary to fully comply with the minimum wage regulations, although this deviation declines toward the end of the observation period due to stricter controls in the industry (see Appendix A.3 for a more detailed discussion).[22]

The wage compression at the top thus reflects wage restraints among skilled workers, where skills are defined as all fixed individual attributes such as occupational qualification and experience, which are rewarded by the labor market in terms of higher wages. To demonstrate this effect, we highlight average yearly wage changes and other worker characteristics for selected wage quantiles (see Table 8 in Appendix A.5). In line with our expectations, top earners have higher occupational qualifications, more tenure, are older, and work in larger firms in both parts of Germany.

5. Minimum Wage Effects on Wages and Returns to Skills

5.1. Estimation Approach

In this section, we estimate the causal impact of the minimum wage at each quantile of the earnings distribution. We use a worker's position in the earnings distribution as a measure of their skills.[23] By studying the effects along the earnings distribution, we are not only able to analyze the minimum wage effect on the earnings of low-skilled workers but also spillovers to medium and high-skilled workers. For identification, we exploit a quasi-experiment: For institutional reasons, the minimum wage was introduced only

[21] Machin et al. (2003) find that 32% of workers were paid less than the (age-specific) minimum wage before it was introduced. Our corresponding indicator shows that up 55% of workers in East Germany earned an hourly wage below the minimum wage in 2006, when the indicator reached its peak (see Table 7 in Appendix A.3).

[22] Note that the kernel density estimates in Fig. 2 suggest greater non-compliance than is actually the case, which can be partly explained by the bandwidths (0.2) we choose for better visualization.

[23] The advantage of this approach over, e.g., occupational qualification is that we can take into account multiple dimensions (such as occupational qualification, experience, or ability) and obtain a continuous measure of skills.

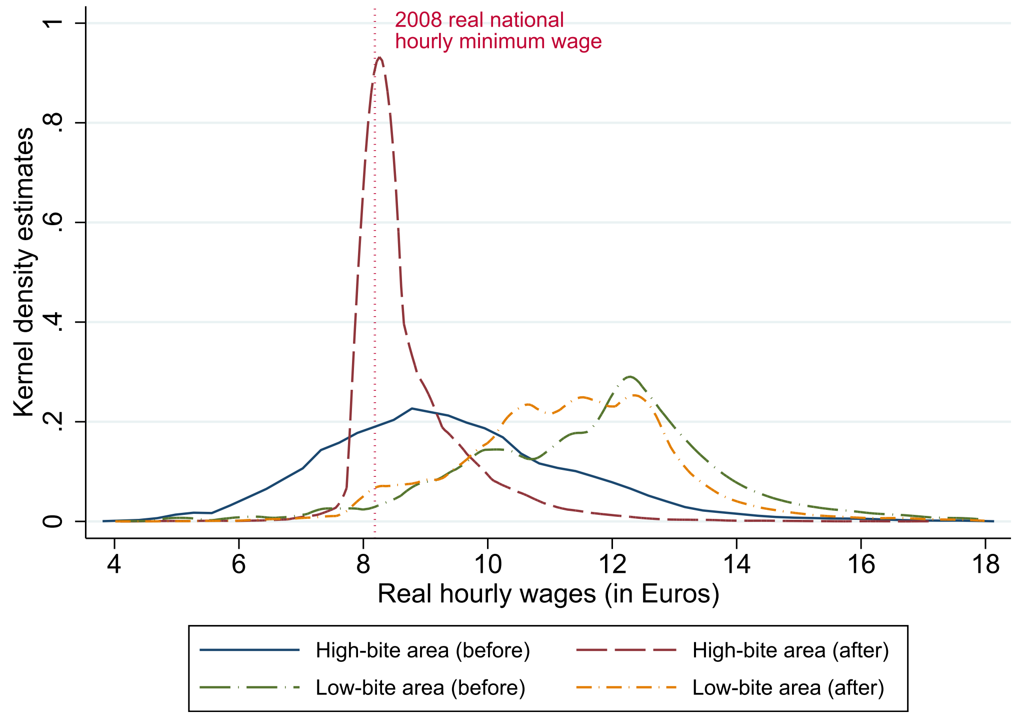

Fig. 2. Hourly Wage Distributions Before and After the Minimum Wage Introduction. *Notes:* The figure shows kernel density estimates (with a bandwidth of 0.2) of the real hourly wages for East Germany (high-bite area) and West Germany (low-bite area) before (1995) and after (2008) the reform, based on a full sample of all roofers using the LAK data. For better readability, values below €5 and above €17 are not displayed. The vertical line shows the national minimum wage level in 2008 (compare Table 2), adjusted to prices. Hourly wages and the 2008 minimum wage level are adjusted to prices in 1994.

in parts of the construction sector, including the roofing industry. We compare the wage distributions of treated workers (roofers) with the counterfactual distributions of control workers (plumbers and glaziers) before and after the minimum wage introduction.[24] For a detailed discussion on the selection process and comparability of the control industries, see Section 3.2.

For the estimation, we apply an unconditional quantile regression technique developed by Firpo et al. (2009).[25] The advantage of an unconditional quantile regression compared to more traditional conditional quantile regression procedures is that it allows for the estimation of quantile treatment effects on the overall earnings distribution. In traditional approaches, conditional quantiles do not average up to their population counterpart as in mean regression approaches, since the Law of Iterated Expectations cannot be applied to quantiles. Therefore, we implement Firpo et al. (2009)'s method to estimate the effect of the minimum wage on the unconditional (marginal) distribution of wages, holding constant time-varying observable and time-constant unobservable factors. Note that an aggregated regional approach yields similar results compared to the quantile regression approach (see wage effects by skill group in Section 6.4 and Appendix Fig. 16).[26]

The unconditional quantile regression approach consists of two steps. In a first step, we define the influence function $IF(y;q^\tau)$ of our wage outcome Y at sample quantile q^τ, which is then transformed (recentered) such that it aggregates back to the overall wage distribution Y. The so-called recentered influence function (RIF) can be expressed as the weighted probability that a worker's wage Y lies above a certain quantile:

$$\begin{aligned} RIF(Y;q^\tau) &= q^\tau + IF(Y;q^\tau) \\ &= q^\tau + \frac{\tau - \mathbb{1}\{Y \leqslant q^\tau\}}{f_Y(q^\tau)}, \end{aligned} \tag{3}$$

where $f_Y(q^\tau)$ is the density at that point. Eq. 3 essentially transforms conditional into unconditional quantiles before running the regressions. In a second step, we regress the RIF on the explanatory variables, using a fixed-effects panel model.[27] Our panel model of daily wages[28] Y of worker i at time t is thereby defined as follows:

[24] For a graphical illustration of the difference-in-differences (DiD) approach applied to quantiles, see Havnes and Mogstad (2015).

[25] Bunching estimation approaches as used by others (see, e.g., Cengiz et al., 2019; Derenoncourt and Montialoux, 2021) are not suitable in our case where we expect the minimum wage to generate negative spillover effects at the top. The reason is that these approaches focus on local effects close to the minimum wage and rely on the assumption that the effects of the minimum wage fade out and become negligible beyond a certain point. Other papers, such as Autor et al. (2016) (building on Lee (1999)), rely on an instrumental variable approach to estimate the effects of U.S. state minimum wages on wage inequality. A key drawback of this approach is that it relies on the assumption that U.S. state minimum wages are exogenous to other factors affecting state-level wage distributions after controlling for state fixed effects and trends. In contrast, the availability of an untreated control industry enables us to implement an identification strategy that does not require these assumptions.

[26] Due to the limited numbers of observations in industry-region cells, the regional approach in Section 6 only allows us to estimate region-industry wage effects for broad groups (we choose our three skill groups: low-, medium-, and high-skilled workers). We thus stick to quantile regression as our main approach to estimate the effects at each quantile of the wage distribution.

[27] Note that neglecting the individual fixed effects would lead to an upward bias in these estimates due to a selection of workers with negative unobserved characteristics who leave the labor market.

[28] Since we use daily wages from our cross-industry BA data as a dependent variable (see Section 4.1), we cannot rule out that our estimates are driven by adjustments in hours worked rather than hourly wages. Previous research has reached mixed conclusions about the minimum wage effect on hours worked (Zavodny, 2000; Couch and Wittenburg, 2001; Neumark et al., 2004). Nevertheless, descriptive inspections of roofers' hourly wages, monthly hours worked, and monthly wages based on the roofing-specific LAK data suggest that adjustments in wages relate mainly to hourly wages. The only exception is the lowest quantile in the West-German wage distribution, where we cannot rule out that adjustments in hours worked lead to a downward bias in our estimated wage effects. Other quantiles in the West and the East are not affected (see Section A.6).

$$Y_{it} = \alpha + \gamma Post_t + \delta(Post_t \times D_i) + \eta \mathbf{X}_{it} + \upsilon_i + \epsilon_{it},$$

where D_i refers to the treatment variable that takes the value one for treated roofers and zero for untreated plumbers and glaziers. $Post_t$ takes the value one for the post-reform period (t_1: years 1998–2008) and zero for the pre-reform period (t_0: years 1994–1997). The term $\mathbf{X}_{it}$ includes a large set of individual and firm-level covariates, including age, industry tenure, occupational qualification (six categories), occupational status (three categories), a part-time dummy, the firm qualification structure (three categories), and firm size (four categories).[29] In addition, υ_i controls for time-constant unobservable effects. The coefficient δ^{τ} gives us the unconditional quantile treatment effect (UQTE) of the minimum wage introduction (and subsequent increases) at the τth quantile of the earnings distribution. Following Bertrand et al. (2004), we cluster our standard errors at the individual level to account for serial correlation.

5.1.1. Identifying Assumptions

The following main assumptions are needed for identification: First, we assume that differences in outcomes between treated and control workers would have stayed the same in the absence of the policy reform (common trends assumption). As outlined in Section 3, the fact that the roofing industry could introduce a minimum wage whereas plumbing and glazing could not lies in differences in the spatial organization of negotiations, which did not change over time. If these differences directly impacted trends in our outcome variables, then they would affect the outcome variables before the introduction of the minimum wage. However, our evidence in Section 3.2 shows that the industries of both treated roofers and untreated plumbers and glaziers have very comparable market structures, experience similar trends in several important economic indicators, and are highly comparable regarding other regulations. We further provide several placebo tests and robustness checks with a dynamic specification in Section 5.3 to demonstrate common trends in the pre-treatment years, despite smaller exceptions. Our results are also robust to the changes-in-changes estimator proposed by Athey and Imbens (2006), which relies only on the assumption of common changes rather than common trends (although at the expense of an additional identifying assumption that the distribution of unobservables within each group is time-invariant, see Section 5.3). Overall, our evidence supports the assumption that our control industries closely mimic the counterfactual situation in the treated industry, conditional on covariates.

Second, there should not be any indirect effects from treated to untreated workers (stable unit treatment value assumption, SUTVA), such as worker transitions between both industries. Mobility among our industries is highly unlikely because industry regulations imply significant differences in the skill requirements of those industries, creating hurdles that prevent workers from switching among industries (see Section 3).[30] Indeed, we find that only 0.35% of the observations in our entire data set show a change in the industry coding between 1994 and 2008; there is hardly any labor mobility among roofers and our control industries (plumbers and glaziers). Moreover, as the entire construction sector experienced an economic downturn during the observation period, it is unlikely that demand effects in the plumbing and glazing industry indirectly affected the roofing industry.

Another potential source for the indirect effects of the minimum wage on the untreated industries could result from a complementary in the production function: A minimum wage-induced increase in the cost of roofing services could increase the overall costs of new buildings, which would, in turn, reduce the demand for untreated plumbers and glaziers. Put differently, the negative shock on our treated industry could impact the untreated industries. Although we cannot test this effect, this would bias our results toward zero, implying that our estimated effects are a lower bound estimate of the true effect.

Third, we assume no anticipatory behavior. To test this assumption, we use our dynamic specification to identify potential anticipatory effects. We then conduct robustness checks where we reassign critical years to show that our main results still hold (see Section 5.3).

Finally, note that it might generally be the case that observed wage changes over time between treatment and control industry reflect compositional changes rather than changes due to the minimum wage. For instance, if low-productivity workers drop out of the workforce or if smaller firms stop operating, this might bias the estimates. However, our rich set of time-varying individual- and firm-level covariates, as well as time-invariant individual unobservables, largely control for such selection effects.

5.2. Results

Fig. 3 displays the UQTEs for 19 different quantiles (from 5th to 95th). Table 3 in the next section shows detailed figures together with several robustness checks for selected quantiles τ=0.1, 0.25, 0.5, 0.75, 0.9 (Panels C and E, baseline model). The results for the UQTEs reveal a large degree of heterogeneity in the minimum wage effects across quantiles as indicated by the deviations of the RIF-FE coefficients from the coefficients of a fixed effects (FE) estimation of Eq. 4. In particular, real wages at the lowest quantiles increased by up to 5–6% in East Germany as a result of the minimum wage. Fig. 3 shows that the positive wage effects extend beyond the median worker. These effects coincide with a location shift of the entire distribution (positive effect on the median). However, these favorable effects were accompanied by real wage losses at a similar magnitude among high-wage workers in East Germany (up to more than 5%). All effects thereby dynamically evolve over time, as demonstrated in Appendix A.8. Further note that the results for East Germany are not affected by pre-trend differences (see Section 5.3).

In contrast, we do not find any larger wage effects for West Germany. Generally, this is not surprising, given that the minimum wage bite was relatively low there. However, note that we find small pre-existing differences in trends between industries in West Germany (Section 5.3), which may bias our results for this part of the country. Indeed, when focusing on a changes-in-changes model, we detect lower tail wage increases in the West, too, albeit small. Since the identification assumptions of the changes-in-changes model seem to be violated (see the discussion in Section 5.3), the results for the lowest quantiles in West Germany remain unclear (between zero and slightly positive).

Whereas the wage compression at the bottom and middle of the distribution in East Germany is in line with existing theories on minimum wage spillovers, the compression at the top remains unexplained. In Section 6, we show that these results can be

[29] Educational attainment: 1 - low-level qualification without vocational training (reference group), 2 - low-level qualification with vocational training, 3 - high school degree (Abitur) without vocational training, 4 - high school and vocational training degree, 5 - university of applied sciences degree, 6 - university degree; occupational status: 1 - unskilled worker (reference group), 2 - skilled worker, 3 - master craftsman or foremen (manual or clerical); firm qualification structure: 1 - share of workers without vocational training (reference group), 2 - with vocational training, 3 - with university or college degree; firm size: less than 6 workers (reference group), between 6 and 10, between 11 and 20, more than 20.

[30] A further possible indirect effect could emerge through spillovers from one of the two other minimum wage industries (such as main construction) if the treated and control industry are differently exposed to such spillovers. We expect such effects to be of minor importance because we selected the control industry based on several criteria, among others, that it is part of the same construction sector and that it shares many basic characteristics (see Section 3.2).

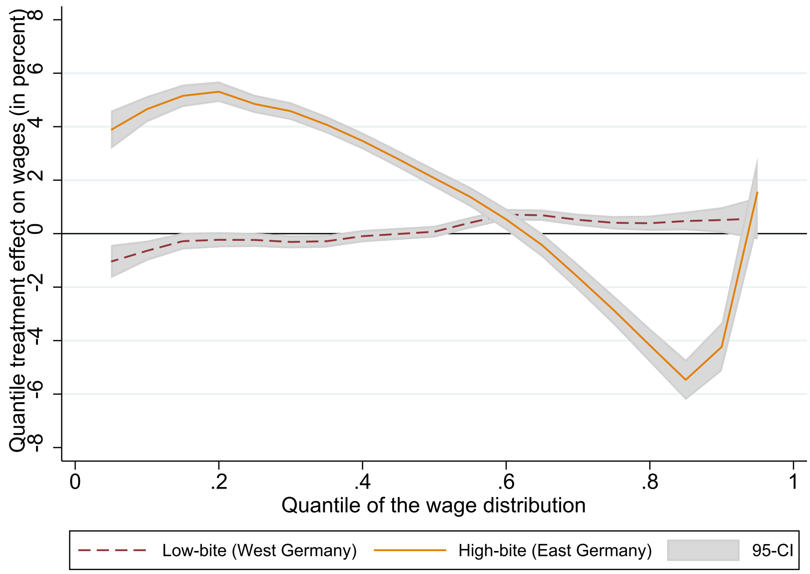

Fig. 3. Minimum Wage Effects on the Unconditional Real Daily Wage Distribution. *Notes:* The figure shows estimates for East and West Germany of the unconditional quantile regression estimator (RIF-FE) outlined in Eq. 4. All models include individual fixed effects, post-year dummies, and several individual and firm-specific covariates. The 95% confidence intervals are based on robust standard errors clustered at the individual level. Detailed regression estimates for selected quantiles are shown in Table 3. Quantile treatment effects reflect the impact of the minimum wage introduction on wages (in %). Only estimates for quantiles between $\tau \geq 0.05$ and $\tau \leq 0.95$ are shown.

explained by the substitution-scale model we laid out in Section 2. According to this model, a high-bite minimum wage can induce a negative net labor demand effect on high-skilled workers whenever the negative scale effect dominates the positive substitution effect, such as during an economic downturn.

5.2.1. Within or Between-Group Compression Effect?

Unconditional quantile regressions look at the overall distribution of earnings. In contrast, traditional conditional quantile regression estimates are restricted to the impact on the wage distribution within worker groups with similar characteristics (within-group inequality). Comparing results from both methods thus allows us to shed light on the role of within vs. between-group inequality in driving the observed shifts in the overall wage distribution. For this, we re-estimate Eq. 4 using the conditional quantile regression approach first proposed by Koenker and Bassett (1978). We refer to these as conditional quantile treatment effects (CQTEs). The results are shown in Fig. 11 in Appendix A.7. The CQTEs show almost equal effects across quantiles; that is, within groups of workers with different characteristics, wages have actually emerged quite similarly. Thus, the bulk of the compression effects at the lower and upper tail can be explained by a between-group compression effect. This result means that workers with lower and higher skills have squeezed together in terms of wages, indicating that returns to skills decreased in reaction to the policy reform. The result is in line with our theoretical predictions according to which a strong minimum wage bite lowers the returns to skills and, hence, depresses skill supply in a context where the negative scale effect dominates the positive substitution effect for skilled workers (see Sections 6 and 7).

5.2.2. Results Based on a Balanced Sample

Our findings might be driven by workers leaving or entering the industry, or by job-to-job transitions within the industry. To rule out such explanations, we re-estimate our model in two versions. First, we restrict the sample to workers who remain in the same industry throughout the entire observation period. Second, we restrict the sample to workers who remain at the same firm throughout. The results are shown in Table 11, Panels C.II-III and F.II-III (Section 5.3). The almost unchanged results (only slightly less significant for the highest quantile) suggest that our observed wage changes are not or are only partly driven by workers entering from other industries. When restricting the sample to those who remain within firms, the effects at the top decile grow more insignificant, indicating that the overall negative effects on top earners are partly driven by workers switching firms within the industry.

5.3. Robustness

5.3.1. Placebo Tests

To test whether our UQTEs in Fig. 3 are contaminated by heterogeneous trends in treatment and control industries before the policy change, we conduct a placebo test. We restrict the data to the pre-reform years 1994–1995 and replace the $Post_t$ dummy in Eq. 4 with a linear time trend. The $Post_t \times D_i$ interaction effect informs us of potential differences in pre-treatment trends between treated and control industries (Panels A.I and D.I in Table 3). We do not find any differences in East Germany, where the interactions are very close to zero and insignificant. In West Germany, however, we find significant but small differences in pre-trends. They point toward a worse trend in roofers' wages, indicating a downward bias in our estimated effects for West Germany.[31] When extending the time period to 1994–1996 and re-estimating the same model with a linear time trend, some of the effects in the East turn significant, although they remain very small (Panel A.II). Even if extrapolated to a time period of ten years, these differences are still smaller than the estimated treatment effects by at least one order of magnitude in the East, whereas these differences are about the same order of magnitude as the estimated treatment effects in the West (Panel D.II).

As an alternative test for pre-trends, we estimate dynamic UQTEs by replacing our $Post_t$ variable and treatment interaction in Eq. 4 with a full set of year dummies and treatment-by-year interactions (for raw differences between treatment and control

[31] Descriptive statistics in Appendix A.6 suggests that this may be driven by pre-trends in hours worked at lower quantiles in West Germany.

Table 3
Robustness Checks for Unconditional Quantile Regressions in Fig. 3.

Dependent variable: log real daily wage	N	FE	Quantile Regression Estimates (RIF-FE)				
			$\tau = 0.1$	$\tau = 0.25$	$\tau = 0.5$	$\tau = 0.75$	$\tau = 0.9$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High-bite (East Germany)							
A. Placebo tests on pre-reform years:							
I. Sample 1994–1995	91096	−0.00	−0.01	−0.00	0.00	−0.01	−0.01
		(−2.46)	(−1.30)	(−0.61)	(1.58)	(−1.20)	(−0.90)
II. Sample 1994–1996	137635	0.00***	0.00	0.01***	0.01***	0.00	−0.00
		(5.14)	(1.06)	(3.83)	(8.93)	(1.72)	(−0.45)
B. Dynamic specification (pre-reform year interactions):							
Interaction with 1995 dummy	471122	−0.00	−0.01	−0.00	0.00	0.01	−0.01**
		(−2.03)	(−2.20)	(−0.72)	(1.50)	(2.54)	(−3.02)
Interaction with 1996 dummy		0.01***	0.00	0.01***	0.02***	0.02***	−0.01
		(5.59)	(0.51)	(4.62)	(8.97)	(6.02)	(−1.54)
C. Robustness of baseline model:							
Baseline model	471122	1.49***	4.66***	4.85***	2.07***	−2.87***	−4.23***
		(10.28)	(18.80)	(28.06)	(12.00)	(−10.28)	(−9.16)
I. Assign 1996 to post-period	471122	2.16***	5.49***	6.32***	2.65***	−3.41***	−4.31***
		(11.64)	(17.72)	(29.20)	(12.09)	(−9.54)	(−7.33)
II. Only those remaining in an industry	44096	2.04***	9.61***	6.75***	−0.13	−6.39***	−3.47*
		(4.75)	(15.08)	(13.43)	(−0.23)	(−7.97)	(−2.75)
III. Only those remaining at a firm	33703	2.43***	8.90***	7.63***	0.85	−7.88***	−1.80
		(4.69)	(12.85)	(13.25)	(1.20)	(−7.77)	(−1.06)
IV. Only plumbers in the control group	439460	1.98***	5.36***	5.42***	2.54***	−2.36***	−4.05***
		(13.28)	(20.56)	(30.50)	(14.15)	(−8.28)	(−8.53)
Low-bite (West Germany)							
D. Placebo tests on pre-reform years:							
I. Sample 1994–1995	181405	−0.01***	−0.01**	−0.01***	−0.01***	−0.01***	−0.01***
		(−13.19)	(−2.84)	(−6.27)	(−5.74)	(−6.29)	(−3.77)
II. Sample 1994–1996	267343	−0.00***	−0.00	−0.01***	−0.00	−0.00***	−0.01***
		(−8.05)	(−2.03)	(−4.91)	(−0.71)	(−4.40)	(−3.53)
E. Dynamic specification (pre-reform year interactions):							
Interaction with 1995 dummy	1141076	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***
		(−8.80)	(−3.45)	(−5.78)	(−6.86)	(−7.79)	(−4.36)
Interaction with 1996 dummy		−0.01***	−0.00	−0.00	−0.00	−0.01***	−0.01*
		(−4.65)	(−1.64)	(−1.71)	(−1.93)	(−3.47)	(−2.66)
F. Robustness of baseline model:							
Baseline model	1141076	−0.11	−0.64***	−0.24	0.07	0.41**	0.51
		(−1.19)	(−3.34)	(−1.78)	(0.66)	(3.22)	(2.11)
I. Assign 1996 to post-period	1141076	−0.17	−0.63	−0.23	0.15	0.36	0.02
		(−1.11)	(−1.96)	(−1.05)	(0.85)	(1.80)	(0.05)
II. Only those remaining in an industry	216750	0.52***	1.05***	0.66**	1.00***	0.26	−0.43
		(3.51)	(3.31)	(3.11)	(6.35)	(1.25)	(−1.06)
III. Only those remaining at a firm	176896	0.36	0.71	0.52	0.90***	0.18	−0.40
		(2.20)	(2.00)	(2.18)	(4.99)	(0.76)	(−0.86)
IV. Only plumbers in the control group	1042950	−0.38***	−1.02***	−0.76***	−0.18	0.31	0.67*
		(−3.89)	(−5.07)	(−5.56)	(−1.60)	(2.35)	(2.61)

Notes: t-statistics in parentheses. Significance levels: * 5%, ** 1%, *** 0.1%. All models include individual fixed effects and several individual and firm-specific covariates (see Eq. 4) and are estimated with robust standard errors clustered at the individual level. Placebo tests in Panels A and D show regressions of the outcome variable on treatment status, time trend, and their interaction based on pre-treatment years 1994–1995 (Placebo I) and 1994–1996 (Placebo II). Panels B and E present a dynamic specification of Eq. 4 with treatment, year dummies, and their interactions, and where only the coefficients of the pre-treatment years are reported (relative to 1994). Coefficients reflect the impact of the minimum wage introduction on wages (in %).

industries across quantiles and years, see Appendix Fig. 9). The results are depicted until 1996 in Table 3 (Panels B and E). For the full set of interactions see Fig. 10 in the Appendix. All effects are estimated relative to our initial year 1994. Overall, these results largely confirm our former placebo tests.

This firstly indicates that the results for West Germany might be biased by pre-trends such that we are likely to underestimate the treatment effect, which might explain the lack of wage increases at the bottom of the West German wage distribution in our estimations. Secondly, there are no differences in pre-trends in East Germany, despite anticipatory behavior—firms slightly adjusted wages the year before the introduction of the minimum wage in 1997. Therefore, we test the robustness of our results with 1996 as a treatment year—all results remain robust in the East, and effect sizes remain very similar (Panel C.I). This is not surprising, given that the adjustments in 1996 were tiny. Accordingly, the results for East Germany are not contaminated by these anticipatory effects.

5.3.2. Competition from Eastern Europe

The dynamic treatment effects in Fig. 10 in the Appendix show that the majority of our treatment effects arose before the year 2004, the year when the free movement of workers in the European Union went into effect in Germany. This highlights that our effects are not driven by Eastern European competition. This is not surprising because our control industries are subject to the same policy—factors that influence control and treatment industries in the same way are generally controlled for in our approach, as long as they are constant over time.

5.3.3. Alternative Control Industry

Generally, the selection of our control industries was based on a detailed preliminary analysis, where initially no industry was excluded. Both plumbing and glazing showed significant commonalities with the roofing industry (compared to any other untreated industry, see Section 3.2). In our preferred baseline specification, we thus pool plumbing and glazing to create one control industry.

Nevertheless, the main findings are also robust when comparing roofers to plumbers (see Robustness IV in Table 3), with few exceptions.[32] For East Germany, our results are robust using plumbers alone. The effect sizes change slightly, but the results remain robust. For West Germany, the results are more sensitive to the choice of the control industry, reflecting the potential problems discussed before (placebo tests, dynamic treatment effects). In line with our previous findings, there is no clear evidence related to the effects of the minimum wage on the West German wage structure, whereas results for East Germany are clear and robust.

5.3.4. Changes-in-Changes

As a further check, we estimate the QTE based on the changes-in-changes model proposed by Athey and Imbens (2006). The main advantage over our unconditional quantile regression estimator is that it allows for the identification of unconditional quantile treatment effects under the less strict assumption that treated and control industries experience common changes (in contrast to common trends), based on the assumption of a time-invariant distribution of unobservables. The results in Fig. 4 report, in contrast to our UQTE estimations, positive wage effects at lower quantiles in West Germany (4 percentage points at $\tau = 0.1$), reinforcing the indications from our placebo tests toward a downward bias in the baseline estimations in the West. For East Germany, however, the results confirm our baseline findings for UQTE, despite some differences in magnitudes: the negative wage effect at the top of the wage distribution in East Germany is somewhat smaller (1.8 percentage points at $\tau = 0.75$ and 1 percentage point at $\tau = 0.9$), whereas the positive effect is larger (9.6 percentage points at $\tau = 0.1$).

We generally prefer the difference-in-differences estimator over the changes-in-changes estimator for the following reason: Although the changes-in-changes estimator makes less strict assumptions related to pre-trends (which seem violated for lower quantiles in West Germany), it imposes the assumption that the distribution of unobservables is time-invariant. However, a comparison of the difference-in-differences model with and without individual fixed effects suggests that workers with negative unobserved characteristics leave the market, i.e., a violation of the latter assumption. Since our focus lies in explaining the wage compression in East Germany (which does not suffer from pre-trends), we prefer the difference-in-differences estimator as our baseline. For West Germany, the results for lower quantiles remain inconclusive (although somewhere between zero and slightly positive).

6. Minimum Wage Effects on Total and Skill-Specific Employment

6.1. Empirical Approach

The positive wage effects at the bottom of the wage distribution and the positive wage spillovers for workers with earnings just above the minimum wage in East Germany can be explained by the existing minimum wage theories outlined in the introduction. However, such theories leave the negative minimum wage effects at the top of the East German wage distribution unexplained. Section 2 introduces a labor market model with labor-labor substitution and a scale effect to explain how spillover effects can turn negative at the top of the distribution. In particular, our theory predicts that a minimum wage driven increase in labor costs for low-skilled workers induces a cost shock that causes the industry to shrink—with negative effects for all workers (Proposition 1). The increase of low-skilled workers' wages leads firms to substitute low- for medium-skilled workers due to the change in relative input prices (Proposition 2). High-skilled workers do not benefit from substitution effects, as their tasks are not suitable substitutes for low-skilled workers' tasks, in line with the existing literature that finds that spillover effects fade out quickly as earnings rise. On net, employment of high-skilled workers decreases, whereas the employment effect on medium-skilled workers is ambiguous, depending on the relative size of the scale and substitution effect (Proposition 3).

To test these predictions, we decompose the minimum wage effect on high-skilled employment (H) (net effect) into the effect on the high-skilled employment share (substitution effect) and the effect on total employment (scale effect) as follows:

$$\underbrace{\frac{\partial \ln H}{\partial \ln w_{MW}}}_{\text{net employment effect}} = \underbrace{\frac{\partial \ln\left(\frac{H}{L+M+H}\right)}{\partial \ln w_{MW}}}_{\text{substitution effect}} + \underbrace{\frac{\partial \ln(L+M+H)}{\partial \ln w_{MW}}}_{\text{scale effect}}. \quad (5)$$

We proceed analogously for low- (L) and medium- (M) skilled workers. To estimate the respective effects, we follow a regional DiD approach, since we are interested in aggregate rather than individual-level outcomes. To prepare the data, we proceed in two steps.

First, we create a continuous skill measure to classify workers. Similar to other studies (e.g., Combes et al., 2008), we define skills as all fixed individual attributes that are rewarded on the labor market, including ability, experience, and (vocational) education. To estimate these, we use the individual-level BA data and conduct an individual-level regression of the log daily wages on a set of dummies for year, industry, East, industry-by-year and East-by-year interactions as well as individual-level fixed effects. From this regression, we extract the time-constant individual fixed effect for each individual, which we define as an individual's skill level. We then rank individuals based on this skill distribution by East/West and industry and define three equal-sized terciles, representing low-, medium-, and high-skilled workers.[33] The key advantage of this approach is that we can take into account multiple dimensions of skills (such as ability, experience, and vocational education) in a continuous measure, similarly to our quantile regressions approach. Nevertheless, note that our core findings are robust using different skill measures, including occupational qualification groups (see Section 6.4).

In a second step, we compute figures on employment and average wages for the total workforce and each skill group.[34] We compute these figures at the level of local labor markets (henceforth referred to as regions) for each industry and year between 1994–2008. We use the definition of 150 local labor markets proposed by Eckey and Türck (2006), which defines regions as places where people live and work, based on commuting patterns (see Appendix A.10 for a corresponding spatial map).

Based on this region-industry panel, we then estimate the minimum wage effect on our outcome variable (Y_{rt}) in region r, industry j, and year t as follows:

$$\ln Y_{jrt} = \alpha + \beta Bite_{ejt} + \eta_t^{post} + \theta_{e\times t}^{post} + \nu_{rj} + \epsilon_{jrt}, \quad (6)$$

[32] We do not use glaziers separately as a control group because they are too few of them.

[33] Regressions within the East/West and industry cells yield almost exactly the same ranking of workers by skills.

[34] We also conducted regressions based on a sample of only full-time workers, which yield comparable results.

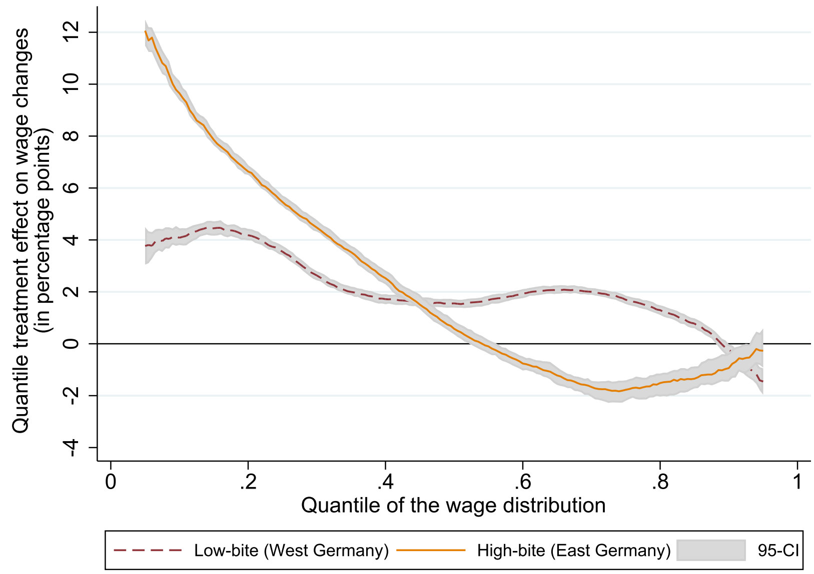

Fig. 4. Minimum Wage Effects on the Unconditional Real Daily Wage Distribution Based on the Changes-in-Changes (CiC) Model. *Notes:* The figure shows estimates from Athey and Imbens (2006)'s changes-in-changes estimator for East and West Germany. All models include several individual and firm-specific covariates. The 95% confidence intervals are based on bootstrapped standard errors using 100 replications. Quantile treatment effects reflect the impact of the minimum wage introduction on wage changes (in percentage points). Only estimates for quantiles between $\tau \geqslant 0.05$ and $\tau \leq 0.95$ are shown.

where $Bite_{ejt}$ is our treatment measure. As a treatment, we use the Kaitz index that equals the minimum wage level w_{ejt}^{MW} divided by the median wage w_{ejt}^{p50} for individuals from the treated industry during the post-reform period (1997–2008) and zero otherwise, see Table 2. Note that our results are robust using a dummy approach instead of a continuous treatment and assuming non-zero values for the Kaitz index in pre-treatment years (see Section 6.4 and Appendix A.15.2). We further control for post-reform year dummies η_t^{post} and interactions with East and West Germany $\theta_{e\times t}^{post}$ to control for any other trends related to both parts of the country. The term ν_{rj} represents industry-region fixed effects. The parameter β then captures the effect of an increase in the minimum wage bite (including its largest increase from zero after the minimum wage introduction). We use skill-specific employment shares, overall employment, and skill-specific net employment as outcome variables to estimate the substitution, scale, and net effects from Eq. 6. To account for size differences between regions, regressions are weighted by (skill-specific) regional employment in pre-treatment years.[35] Following Bertrand et al. (2004), we report robust standard errors clustered by region in our results tables to account for serial correlation.

Note that for the treated industry, the bite varies only between East and West Germany (index e) and by post-reform years (we essentially take the values from Table 2).[36] This way we exploit the institutional differences in the minimum wage reform while precluding any endogenous regional variation in the bite. Using the bite instead of a post-reform dummy further allows us to pool East and West Germany into a single regression and exploit the differences in the size of the bite to estimate the effects of the reform. Pooling the data also helps prevent small sample sizes, especially in East Germany.[37] However, we test the robustness of our approach when splitting the sample into East and West Germany and using a dummy treatment instead of a continuous treatment, as before, see Section 6.4.

Identification comes from comparing local outcomes between treated and control industries before and after the introduction of the minimum wage. The main difference to the approach in Section 5 is that we compare industries across regions and time instead of individuals (by quantile) since we are interested in aggregate industry effects. Otherwise, the identification assumptions are similar (see Section 5.1.1). In particular, we assume that treated roofers and untreated plumbers and glaziers would have experienced the same changes in outcomes on a local labor market level had the minimum wage not been introduced. In Section 6.4, we show that treatment and control industries follow largely the same trends before the reform.

6.2. Results

The results for the substitution and scale effect are shown in Fig. 5 and can be interpreted as percentage changes in employment (shares) as a result of a ten percentage point increase in the minimum wage bite (descriptive statistics on employment trends by skill group for treated and control workers are shown in Appendix Fig. 13). Our regional regressions suggest a substantial substitution of low- (-2.9%) with medium-skilled workers (+2.5%), whereas the share of high-skilled workers is unaffected by the minimum wage. The finding is in line with our theoretical predictions (and other

[35] Our dependent variable is aggregated from the random sample of workers to the region-sector level, where region-sector cells substantially differ in size. The variance of our outcome variables is thus substantially larger in small cells compared to large cells. We weight by regions' initial sizes to control for this heteroskedasticity. We show in Appendix A.16 that the results are robust when abolishing weights.

[36] Note that by dividing the subsequent minimum wage by the contemporaneous median wage, we also receive values for periods with a gap (where the minimum wage was shortly suspended, see Fig. 6 in Appendix A.2).

[37] We do not pool East and West Germany in our quantile regressions for two main reasons. First, the wage structures significantly differ between East and West Germany, which creates large obstacles for quantile regressions. Differences in wage structures do not matter at the regional level, as we focus on the overall level of employment (and wages) and skill-specific regional outcomes, where the definition of skills is adjusted, among others, for differences in levels between East and West. Second, we do not face any challenges with sample size in the quantile regression approach, which relies on the individual-level version of our data.

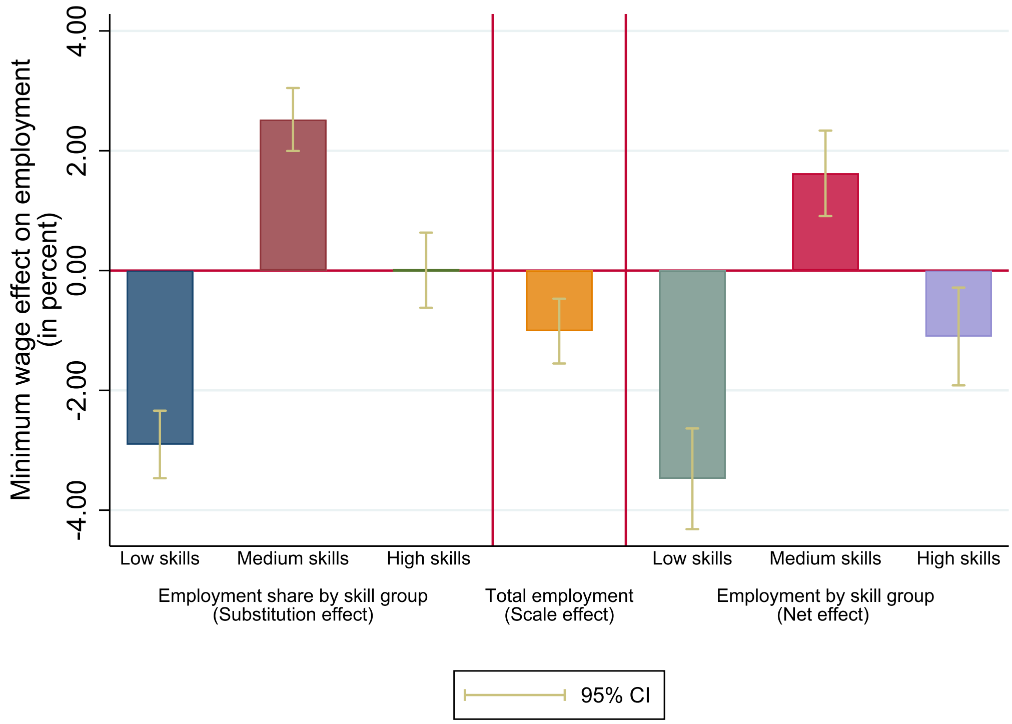

Fig. 5. Minimum Wage Effects on Total Employment (Scale Effect) and Skill-Specific Employment Share (Substitution Effect). *Notes:* The figure shows the regional estimates of skill-specific employment shares, total employment, and skill-specific absolute employment based on region-industry-year cells between 1994–2008 according to Eq. 6. All models include industry-region fixed effects, post-reform year dummies, and interactions with East/West Germany. Regressions are weighted by regional (skill-specific) employment in pre-treatment years. Whiskers mark the 95% significance interval. Robust standard errors are clustered by region. The figure reflects the impact of a 10 percentage point increase in the minimum wage bite (in %). Detailed regression tables are shown in Table 5.

minimum wage studies, see Introduction), according to which medium-skilled workers provide close substitutes for low-skilled ones, whereas high-skilled workers do not. The relatively large substitution effect among medium-skilled workers potentially reflects the strong minimum wage bite in the industry, which required substantial adjustments within firms in order to cope with minimum wage-induced cost increases. However, these positive substitution effects are counteracted by the negative scale effect, which has so far been largely neglected in other studies. In particular, we find a substantial decline in overall employment (-1%)[38] resulting from increased labor costs (and falling firm revenues, see Appendix A.13) such that the net impact on high-skilled workers is negative (-1.1%). For medium-skilled workers, the positive substitution effect suffices to overcompensate for the negative scale effect, resulting in a net positive employment effect for these workers (+1.6%). For low-skilled workers, the scale effect enforced the negative substitution effect (on net, −3.5%).

The above findings are in line with our labor-labor substitution-scale model and are able to explain the compression of wages at the highest quantiles in East Germany that we demonstrated in the previous section. In particular, it is the substitution of workers away from low- to higher-skilled workers that fades as skills rise. Since the scale effect is negative and significant, the resulting net labor demand effect on high-skilled workers is negative, ultimately causing the downward pressure on the wages of top earners. These adjustments are only effective when the bite of the minimum wage is sufficiently large, as in the case of East Germany. While results for the West German wage distribution in Section 5 are inconclusive due to heterogeneous pre-trends, we show in Section 6.4 that they do not affect our results for employment.[39]

The fact that we find employment responses to the minimum wage implies that the wage effects are partly driven by selection. Indeed, we show in Section 5.3 that wage effects are less negative for high-skilled workers (and more positive for low-skilled workers) when focusing on those who remain in their initial jobs. However, controlling for selection does not qualitatively affect our conclusions for wage responses—wage effects remain negative at the top and positive at the bottom of the East German wage distribution.

Finally, note that the dynamic treatment effects indicate that the employment effects of minimum wages take time to unfold, as already demonstrated for wages. In particular, employment changes are not concentrated around the year the minimum wage was introduced, where the change in the bite was largest, but instead unfold slowly over time. This finding is in line with recent studies that highlight the role of dynamics for the employment effects of minimum wages (see, e.g., Clemens and Strain, 2017; Meer and West, 2016).

6.2.1. Firm Revenues and White-Collar Workers

To provide more evidence on the negative scale effect, which is crucial to explain our findings, we estimate the minimum wage effect on regional revenues (firm averages, industry totals, and firm averages per worker) and uncovered white-collar workers based on Eq. 6. The data on firm revenues are calculated from separate

[38] This number implies that the elasticity of overall employment with respect to the minimum wage is 0.1, which is remarkably close to the median of estimates for the United States (-0.112) as reported by Neumark and Shirley (2021).

[39] Note that a pre-trend in West German employment would actually lead to more conservative estimates for the role of the bite: A pre-trend in West German employment would overstate the employment effect in the West, where the bite is lower, and understate the true effect of the bite on employment because we exploit differences in the bite between East and West to estimate the effect of the minimum wage on employment.

micro data for all active firms in Germany. The details of the data set together with the results are presented in Appendix A.13. Accordingly (compared to Table 9), a ten percentage point increase in the minimum wage bite causes lower total industry revenues (-2.8%), average firm revenues (-3.1%), and revenues per worker (-3.6%). Overall, the results confirm a negative aggregate demand shock.[40]

Our baseline estimations exclude white-collar workers. Any scale effect on the industry should, however, also affect these workers. As an additional check for the presence of a scale effect, we therefore estimate the effects of the minimum wage on uncovered white-collar workers in Appendix A.17. These are mostly female office clerks with mid-level skills. The results in Appendix Table 13 show that the net employment effect for this group is comparable in size to the negative scale effect from our main specification. The results for East Germany are insignificant due to the small sample, as this group makes up only a small share of the workforce (8.6% of all roofers, excluding marginal employment).

6.3. Alternative Mechanisms

6.3.1. Capital Adoption

Our effects could also be driven by capital-labor substitution. In particular, a minimum wage driven increase in labor costs could induce firms to invest in automation technologies in order to substitute for low-skilled labor (Lordan and Neumark, 2018; Aaronson and Phelan, 2019), although this would only affect higher-skilled workers through complementarities (i.e., if low- and high-skilled workers are complements). As an alternative to the latter more standard approach to capital-labor substitution, a minimum wage could slow down the automation of medium-skilled work, consequently lowering the relative demand for high-skilled workers (Downey, 2021).

To test these alternative capital-labor hypotheses, we follow the literature and investigate the impact of the minimum wage policy reform on task-biased technological change (TBTC). We assume that routine jobs are more likely to be substituted relative to non-routine jobs. We then exploit data on occupation task requirements from the U.S. Department of Labor's Dictionary of Occupational Titles (DOT 1977) following Autor et al. (2003) and David and Dorn (2013). The task data is merged at a 3-digit occupational level (distinguishing among 120 occupations within our data set) and measures manual, routine, and abstract task content by occupation. We classify workers according to their main task as judged by the maximum value of the standardized occupational measures and calculate the regional share of manual, routine, and abstract workers. We then estimate our regional model as before according to Eq. 6, using the share of workers by task group as a dependent variable.

The results depicted in Table 4 do not indicate a preference for substituting non-routine for routine workers in response to the minimum wage. In particular, we do not find any sizable or significant shifts in the structure of employment along the dimension of tasks. If anything, we find evidence for an increase in the share of routine workers, although the coefficient is weakly significant (at the 10% significance level) and small in magnitude (10% increase in the bite causes a 0.17% increase in the routine share). This provides weak evidence in favor of a slow down in automation as discussed by Downey (2021), suggesting that our findings can generally be rationalized by changes in labor-capital substitution. However, for automation to explain all of our effects, we should expect a large decline in abstract tasks because fewer investments in automation imply a declining demand for abstract task workers who are complementary to technology. The absence of a sizable and significant decline in abstract workers suggests that capital-labor substitution is not the main explanation for our findings.

Table 4
Minimum Wage Effects on the Task-Structure of Employment.

	Log share of employment by tasks		
	Manual (1)	Routine (2)	Abstract (3)
Minimum wage bite	0.37	0.17*	−0.12
	(0.49)	(1.88)	(-0.08)
N	6683	6683	6683

Notes: The data include regional estimates of task-specific employment shares based on region-industry-year cells between 1994–2008 according to Eq. 6. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects and are weighted by pre-treatment total regional employment. Robust standard errors are clustered by region. The data reflect percentage changes in the outcome variable in reaction to a ten percentage point increase in the minimum wage bite (in %).

6.3.2. Trade Unions

A further mechanism that could potentially explain our findings is a change in unionization. A long-standing stream of literature argues that unionization reduces wage inequality for men, while having little impact on women (for reviews of the literature, see Card et al., 2004; Card et al., 2020). Part of the effect can be explained by the positive selection of low-wage workers and the negative selection of high-wage workers into union jobs, although there remains a sizable direct effect (Card, 1996). Declining unionization, therefore, contributes to rising wage inequality (Card et al., 2020). Accordingly, rising unionization in the roofing industry relative to our control industries could be an alternative explanation for declining wage compression at both tails of the wage distribution.

To explore this hypothesis empirically, we explore the German Socioeconomic Panel, an interdisciplinary longitudinal study of private households for Germany. The data allow us to calculate the share of workers who identify themselves as members of a trade union (for details, see Appendix A.14). According to the data, the German roofing industry is indeed strongly unionized. The share of union members was about 18% in the 1990s in roofing, although it was about 24% among plumbers (see Appendix Fig. 17). Generally, time-constant differences in unionization between the roofing industry and our control industries are controlled for in our empirical approach. However, unionization could have increased more in the roofing industry relative to our control industry, potentially explaining the wage compression we document.

Appendix Fig. 17 suggests that trends in union membership were parallel between roofers and plumbers. In particular, union membership declined for both from the 1990s to the 2000s and then slightly increased until the 2010s. These trends in roofing and plumbing are generally in line with the overall decline of union membership in Germany. The finding that unionization trends were equal in both industries suggests that changes in unionization do not play a major role in explaining our findings. Nevertheless, there could be other related mechanisms stemming from union power or objectives (rather than union participation) that we cannot test here (for instance, the minimum wage introduction could have signaled an increase in the roofing union's power). However, for unionization to explain all the changes in the wage structure, we should expect changes in relative union membership rates, as union members should respond either to a significant change in their union's power or its objectives. The absence of responses related to union membership suggests that changes in the union's power or objectives only play a minor role.

[40] These results are generally in line with the literature review by Clemens (2021), which shows that minimum wages can lead to declining firm profits and rising firm closures when no other margins of adjustment are available.

Table 5
Substitution and Scale Effect by Skill Group as Shown in Fig. 5.

Skills:	N	Sub effect			Scale effect	Net effect		
		Low	Medium	High	All	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Placebo tests on pre-reform years								
I. Sample 1994–1995 (plumbers/glaziers)	900	0.01	0.01**	-0.01**	-0.00	0.00	0.01	-0.02*
		(0.69)	(2.44)	(-2.09)	(-0.18)	(0.10)	(1.39)	(-1.92)
II. Sample 1994–1996 (plumbers/glaziers)	1350	-0.02***	0.02***	-0.00	-0.01	-0.02	0.01	-0.01
		(-2.94)	(4.99)	(-0.22)	(-1.20)	(-1.60)	(0.79)	(-1.48)
B. Dynamic specification (pre-reform year interactions)								
Interaction with 1995 dummy	6750	0.01	0.01**	-0.01**	-0.00	0.00	0.01	-0.02*
		(0.69)	(2.43)	(-2.09)	(-0.18)	(0.10)	(1.39)	(-1.92)
Interaction with 1996 dummy		-0.04***	0.04***	-0.00	-0.02	-0.04	0.01	-0.02
		(-2.94)	(4.98)	(-0.22)	(-1.19)	(-1.60)	(0.79)	(-1.48)
C. Robustness of baseline model								
Baseline model	6750	-2.90***	2.52***	0.01	-1.01***	-3.48***	1.62***	-1.10***
		(-10.18)	(9.48)	(0.02)	(-3.69)	(-8.17)	(4.49)	(-2.66)
Assign 1996 to post-reform period	6750	-2.85***	2.54***	-0.06	-1.19***	-3.58***	1.43***	-1.37***
		(-9.64)	(9.91)	(-0.21)	(-4.51)	(-8.80)	(3.73)	(-3.86)
Only plumbers in control group	4500	-3.13***	2.59***	0.07	-1.36***	-4.15***	1.31***	-1.39***
		(-11.04)	(9.20)	(0.21)	(-4.92)	(-9.44)	(3.45)	(-3.21)
D. Skill groups								
Alternative skill measure	6648	-3.44***	0.96***	-0.02	-1.01***	-2.37***	-0.14	-1.07**
		(-4.50)	(8.33)	(-0.04)	(-3.69)	(-3.56)	(-0.48)	(-2.02)

Notes: t-statistics in parentheses. Robust standard errors clustered by region. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects, post-reform year dummies, and their interactions with East/West. Substitution and scale effects are weighted with pre-treatment regional employment. Net effects are weighted with pre-treatment region-skill-specific employment. Placebo tests in Panels A show regressions of the outcome variable on treatment status, time trend, and their interaction based on the pre-treatment years 1994–1995 (Placebo I) and 1994–1996 (Placebo II). Panels B present a dynamic specification of Eq. 4 with treatment, year dummies, and their interactions and where only the coefficients of the pre-treatment years are reported here (all relative to 1994). Panel D shows a specification where workers are defined as follows: without apprenticeship training (low-skills); with apprenticeship training (medium skills); with apprenticeship training as well as at least six years of work experience or a master craftsman's degree (high skills). Results in Panel C and D reflect the impact of a ten percentage point increase in the minimum wage bite (in %).

Altogether, this suggests that while our central findings of negative wage effects on high-skilled workers in Fig. 3 can best be explained by the substitution-scale model outlined in Section 2, changes in capital-labor substitutions as well as unionization only play a minor role.

6.4. Robustness

6.4.1. Placebo Tests

To test for differences in trends between treatment and control industries before the introduction of the minimum wage, we follow an approach similar to that described in Section 5.3. That is, we restrict the sample to the pre-reform years and regress our regional outcomes on a time trend, its interaction with treatment D_i—which takes the value one for treated roofers and zero for untreated plumbers and glaziers, and region-industry fixed effects. Panel A.I in Table 5 shows the results when focusing on the pre-treatment years 1994–1995, Panel A.II for the years 1994–1996. For some of the outcome variables, particularly the substitution effects, we find some evidence of differences in pre-trends, although these are small compared to the treatment effects. Even when extrapolating these trends to a time period of ten years, these differences are still smaller by one order of magnitude than the treatment effects. In addition, the effects—similar to those along the wage distribution—point toward anticipatory effects in 1996. The effects mostly disappear when treating 1996 as a post-reform year, and our estimated treatment effects are robust when assigning 1996 as a post-reform year (Panel C). Firms apparently adjusted before the official introduction of the minimum wage, although these adjustments were small.

As an alternative test for differences in trends before the introduction of the minimum wage in the regional approach, we adopt a similar approach to our placebo tests but replace the time trend (baseline and interactions with treatment) with a full set of year dummies and extend the regression to the entire observation period. The results are shown in Panel B of Table 5 for our three skill groups (for a visualization of the full set of interactions, see Fig. 14 in the Appendix). The results confirm our placebo tests: There is evidence for anticipatory behavior in 1996, whereas in 1995 most interactions equal zero (all relative to 1994).

6.4.2. Estimates by East and West Germany

Although we pool East and West Germany in our regional-level estimations for reasons of sample size and to stress the role of the bite, we test the robustness of our results by splitting the data into East and West Germany (see Fig. 15 in Appendix A.11). Standard errors increase due to the smaller samples for East and West, as expected, but the overall picture persists, particularly for East Germany, where the bite is more significant.

6.4.3. Alternative Control Industries

Similar to Section 5.3, we also test our results against using only plumbers as a control industry. The results in Table 5 confirm our baseline findings when using only plumbers (Panel C). However, we do not use glazing alone because the aggregate regional employment figures would be based on few observations due to the small sample size, inducing potentially large biases.

6.4.4. Wage Effects by Skill Group

To test whether the regional level approach resembles the quantile regression estimates for wages in Section 5, we estimate Eq. 6 using skill-specific average regional wages as the dependent variable. The results in Fig. 16 in the Appendix resemble the quantile regression results in Section 5. In particular, they demonstrate that an increase of the minimum wage bite (Kaitz index) by ten percentage points led to a wage increase among low-skilled workers by 0.48%, whereas high-skilled workers experienced wage reductions of 0.28%. Note that the minimum wage bite in the East increased up to 100 percentage points, which corresponds to wage increases for low-skilled workers of 4.8% and real wage reductions of 2.8% for high-skilled workers. These numbers are remarkably close to the corresponding findings from our quantile regressions for East Germany, which again strengthens the robustness of the quantile regressions in Section 5.

6.4.5. Alternative Skill Groups

We check the robustness of our results to the choice of our skill measure. As an alternative skill measure, we use *occupational qualification*, defined as follows[41]: without apprenticeship training (low skills); with apprenticeship training (medium skills); with apprenticeship training and at least six years of work experience or a master craftsman degree (high skills). The results of the baseline model with this alternative skill measure are shown in Panel D of Table 5. Overall, they confirm our results, except that the substitution of medium- for low-skilled workers is now smaller so that the net effect of medium-skilled workers is near zero and insignificant compared to the positive baseline effect for this group at baseline. We prefer our main specification over this approach because it allows us to take into account multiple dimensions of skills in a continuous way (see discussion in Section 6.1).

6.4.6. Alternative Treatment

As alternative treatments, we conduct two robustness checks. First, we exchange the minimum wage bite measure with a dummy variable ($treat \times post$) similar to the quantile regression approach described in Eq. 4. As expected, the alternative approach largely confirms our result, although the interpretation is different: we find that the minimum wage policy reform reduced the demand for high-skilled workers by 7% on average. Given that the minimum bite increased by 73 percentage points in the West and 100 percentage points in the East, both approaches provide comparable results (see Appendix A.15.1 for a more detailed discussion).[42]

Second, we show that the results using the bite measure are robust to assuming either (a) the lowest wage received in the industry, (b) the first percentile, or (c) the fifth percentile instead of a zero minimum wage for the calculation of the Kaitz index prior to the introduction of the minimum wage. The effects all have the same sign and significance compared to the baseline model in the paper. The only difference is that the effects are larger (see Appendix A.15.2).

7. Minimum Wage Effects on Skill Supply

In Section 5, we have shown that the minimum wage introduction led to wage compression at both ends of the wage distribution in East Germany. We have also demonstrated that the bulk of this compression is explained by a between- rather than within-group compression effect, meaning that workers with lower and higher skills have been squeezed together in terms of wages. This finding suggests reduced returns to skills in high-bite East Germany and no large changes in low-bite West Germany. In the present section, we now test the wider implications for skill supplies in the industry.

According to our theoretical model, we expect (A) an increase in the entrance of medium- relative to low-skilled workers (Corollary 1) as firms restructure toward medium-skilled workers due to the minimum wage-induced change in relative wages. Moreover, we expect (B) a decline in the supply of high- relative to medium-skilled workers, as high-skilled workers do not profit from substitution effects, while at the same time suffering from a scale effect and, hence, declining returns to their skills (Corollary 1). Moreover, as before, we expect the effects to rise following the minimum wage bite.

To test these predictions, we follow a similar regional DiD approach as that described in Section 6. In particular, we estimate the effect of the minimum wage on: (A) share of medium-skilled entrants and (B) the share of high-skilled entrants. Entrants are defined in our data as workers who have no prior working experience in the industry. Regarding the definition of skills, we would ideally use the same skill measure as for the analyses in Section 6. However, this requires workers to have already entered the labor market and gained some work experience. Therefore, we make use of an alternative concept of skills: we assign entrants who have not participated in a training program to the group of low-skilled entrants; we then assign those in apprenticeship training to the group of medium-skilled entrants. Trainees can be identified separately in the data via an indicator on workers' occupational status. To identify high-skilled entrants, we assume that those apprentices with the highest level of school education are those with the highest skills and, hence, the most likely to enter a master craftsman program.[43]

Table 6 shows the results. Our estimates are weighted to account for heteroskedasticity due to differences in the size of regions (see Appendix A.16). All effects are percentage changes in reaction to a ten percentage point increase in the minimum wage bite. Starting with our measure for the entrance of medium relative to low skills, we find an increase in medium-skilled entrants in the industry (Column 1), reflecting a rise in the relative demand for medium-skilled workers. However, the quantitative size of the effect (+0.47%) is smaller compared to the increase in relative demand (+2.52%) found in Table 3. One reason for this gap could be that our dependent variable (share of new apprentices among all entrants) only provides a proxy for medium skills, leading to an underestimation of the increase in medium-skilled entrants. Another reason could be that firms restructure toward medium-skilled workers not only via hiring relatively more medium-skilled workers but also via a higher separation rate for low-skilled workers.[44]

Turning to our measure for the entrance of high- relative to medium-skilled workers: In line with our predictions, we find a decline in high-skilled workers entering the industry (-9.09%) in response to the minimum wage increase (Column 2). This effect is large in relative terms, but not in absolute terms. The baseline share of high-school degrees among new apprentices is 2%. An increase of the bite by ten percentage points thus reduces this share by roughly 0.18 percentage points. This change implies that the share of high-school degrees among new apprentices declined by about 1.8 percentage points in the East, where the bite rose to 100 percentage points, and less in the West, where the bite was much smaller. This is not associated with an actual decline in the

[41] This definition follows industry-specific regulations: only high-skilled workers are allowed to open a handicraft business, where *high-skilled* is defined as workers with either a vocational training degree and six years of work experience or a master craftsman degree.

[42] The dummy-variable approach solely relies on the introduction of the minimum wage and is thus robust to potential endogenous increases of the minimum wage after its introduction. The similarity of the results from the dummy-variable approach and our baseline approach indicates that our baseline estimates are not driven by endogenous increases in the bite.

[43] We would ideally use those who registered for a master craftsman certificate. Since information is only available for enrollment in an apprenticeship training program but not a master craftsman program, we make use of the fact that new workers entering the industry have to complete the apprenticeship training program before being able to start a master craftsman's program. This is typically a favorable selection of workers (in terms of skills) who completed an apprenticeship training degree. We proxy these higher skills by the highest school degree (i.e. Abitur). While this admittedly limits comparability with our other analyses, it is the best proxy for high-skilled entrants, given our data.

[44] In fact, complementary work confirms this explanation by showing that the minimum wage significantly increased the separation rate of low-skilled workers in East Germany (Aretz et al., 2013).

Table 6
Minimum Wage Effects on Industry Skill Supply.

	Share of all entrants to the treated industry: Medium-skilled (1)	High-skilled (2)
Minimum wage bite	0.47***	−9.09**
	(4.46)	(-2.39)
N	6741	6672

Notes: t-statistics in parentheses. Significance levels: * 10%, ** 5%, *** 1%. This table shows estimates of Eq. 6 using the share of apprentices among all entrants (Column 1) and the share of apprentices with a high-school degree among all entrants (Column 2) as outcome variables. All estimates include industry-region fixed effects. Column 1 is weighted with the pre-treatment number of entrants, whereas Column 2 is weighted by the pre-treatment number of apprentices. Robust standard errors are clustered by region. The results reflect the impact of a ten percentage point increase in the minimum wage bite (in %).

share of high-skilled entrants, but rather with a slower increase in this share relative to the control group in a context where the share of high-skilled workers is generally rising.

This finding reflects the reduced real wages of high-skilled workers and returns to skills, which reduced the incentives for high-skilled workers to enter the industry. This might also explain why the industry faced increasing problems attracting high-skilled workers in the aftermath of the policy reform, as reported by Aretz et al. (2011), or the observed increase in solo self-employment (in search of alternative income) found by Ganserer et al. (2021). The effect is sizable and suggests that the decline in high-skilled employment in the industry can be mostly explained by a sharp decline in high-skilled entrants and less so by the exit of high-skilled workers. This is in line with the fact that skills are highly industry-specific, preventing the mobility of workers between industries.

8. Conclusion

We investigated the impact of introducing or increasing a minimum wage on workers' wages, returns to skills, and skill supply in light of a particularly interesting case: For institutional reasons, Germany introduced its first minimum wages in 1997 only in parts of the German construction sector, including the German roofing industry. There, the minimum wage was subsequently raised several times. According to the Kaitz index—reaching levels of 100% in East Germany, the bite of the minimum wage has to be considered exceptionally high. In addition, the minimum wage was introduced during a long-lasting economic downturn with falling revenues, which tends to require further adjustments among firms. Overall, this setting is of particular interest against the background of internationally rising minimum wages and allows us to draw the following conclusions to understand (high-bite) minimum wages:

First, a minimum wage may induce positive wage spillovers for workers with wages slightly above the minimum wage. For the investigated industry, we find significant real daily wage increases of up to about 6% for lower-quantile workers that ripple out to the 60th quantile in high-bite East Germany between 1997 and 2008. We explain these findings within a substitution-scale model that predicts a shift in labor demand toward medium-skilled workers in response to the minimum wage-induced change in relative input prices. Thus, the policy seems to have met its goal of improving the earnings of low-wage workers (at least for those who did not lose their job due to the minimum wage) and reducing overall wage inequality.

Second, a minimum wage can reduce the earnings of high-skilled workers. According to our estimates, the minimum wage caused a reduction in the real daily wages of up to about 5% in East Germany for the highest quantiles (stagnating nominal wages) that mostly comprise skilled and experienced workers. We show that this striking finding is in line with our substitution-scale model that predicts negative spillovers whenever the negative scale effect dominates the positive substitution effect, such as in an economic downturn with falling demand (and revenues) in combination with an increasing minimum wage bite. For medium-skilled workers in our investigated industry, these negative scale effects were overcompensated by positive substitution effects, resulting in a net positive impact on wages and employment. High-skilled workers, however, did not profit from positive substitution effects, as their tasks are not suitable substitutes for low-skilled minimum wage workers, resulting in a net negative impact on employment and wages. The proposed mechanism may be a missing link in explaining negative spillovers from minimum wage policies, especially during an economic downturn.

Third, a minimum wage may hamper skill supply. Our results suggest that the minimum wage has worsened the selection of workers entering the industry. In particular, we find a positive effect on medium-skilled entrants, whereas our estimates suggest a negative effect on high-skilled entrants. Our substitution-scale model explains this effect with deteriorating wage and employment perspectives of high- relative to medium-skilled workers, leading to lowered returns to skills in the industry. Deteriorating returns to observable skills are also reflected by the fact that our observed upper-tail wage-compression effect is solely driven by a reduction in between-group inequality. The results may explain reports by industry insiders who state that the industry is facing increasing problems in attracting high-skilled workers. We provide additional evidence suggesting that changes in capital-labor substitution and unionization do not play a major role for our findings.

Although we focus on a specific industry (with a relatively narrow skill distribution), such adjustments may also occur in contexts where the following conditions are met: (a) the minimum wage bite is set relatively high in an economic environment where firms have little room for alternative margins of adjustment so that profits and revenues decline (such as during an economic downturn), thus triggering a negative scale effect on all workers; (b) high-skilled workers' tasks do not provide close substitutes for low-skilled workers' tasks such that positive spillover effects on the former via labor-labor substitution are small, which is more likely whenever the skill distribution is wider (such as at the national level); and (c) the mobility of workers toward other industries is low due to skill specialization (such as through industry-specific apprenticeship programs in Germany) so that declining demand for high-skilled workers leads to real wage declines.

Finally, note that our work may have further implications beyond wages, employment, and returns to skills. In particular, our complementary work (see Ganserer et al., 2021) suggests that solo self-employment (as one type of alternative work arrangement that is on the rise worldwide, see Boeri et al., 2020) significantly increased in German industries that introduced a minimum wage in the 1990s (we also show this for the roofing industry). As suggested by our companion work, the increase in solo self-employment, which for institutional reasons in German craft industries is driven by high-skilled workers, could reflect a further effect of the deteriorating wage and employment perspectives of high-skilled workers in reaction to the minimum wage.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A

A.1. Theory

In this appendix, we show the details of our theoretical framework.

A.1.1. Production

There are I firms in the industry, producing varieties q_i of the industries' final output Q under monopolistic competition. Firms require a fixed high-skilled labor input $h_i = f$ and a variable labor input $n_i = \varphi q_i$. Modeling high-skilled workers as a fix input implies no substitution with lower skilled workers. This is similar to DIDES models, where low-skilled tasks are more easily substituted by medium than by high-skilled tasks. In our case, this is regulated by the master craftsman's requirement, according to which only master craftsmen or vocationally trained workers with sufficient work experience are allowed to lead a roofing firm. Nevertheless, it occurs more broadly in industries where high and low-skilled workers perform substantially different tasks, precluding substitution between them.

The variable labor input is composed of low and medium-skilled workers. Firms can replace low by medium-skilled workers with constant elasticity of substitution $\eta, n_i = \left(l_i^{\frac{\eta-1}{\eta}} + m_i^{\frac{\eta-1}{\eta}}\right)^{\frac{\eta}{\eta-1}}$. Firms' wage costs for low and medium-skilled workers are $\bar{w}n_i = w_L l_i + w_M m_i$, where $\bar{w}$ is the wage resp. factor cost index. Firms optimally choose the composition of low and medium-skilled workers, which implies

$$m_i = n_i\left(\frac{w_M}{\bar{w}}\right)^{-\eta} \tag{7}$$

$$\bar{w} = \left(w_M^{1-\eta} + w_L^{1-\eta}\right)^{\frac{1}{1-\eta}} \tag{8}$$

A.1.2. Consumption

Consumers have constant elasticity of substitution (CES) preferences for the varieties i produced by the firms with elasticity of substitution $\sigma > 1$ between the varieties, $U = \left[\int_0^I q_i^{\frac{\sigma-1}{\sigma}} di\right]^{\frac{\sigma}{\sigma-1}}$.[45] Total roofing sales are $R = \int_0^I p_i q_i di$. Utility maximization implies a downward sloping demand curve for each variety,

$$q_i = \left(\frac{p_i}{P}\right)^{-\sigma}\frac{R}{P} \tag{9}$$

with the CES price index $P = \left[\int_0^I p_i^{1-\sigma}\right]^{1/(1-\sigma)}$. The industry sells its output to the rest of the economy and is too small to affect the size of the economy. We assume that demand for the overall output of the industry is price sensitive with the constant price elasticity of demand $\epsilon < 1, Q = Q_0 P^{-\epsilon}$, where $Q = R/P$ are real sales.

A.1.3. Labor Demand

Due to monopolistic competition, prices are a markup on marginal costs, where $\bar{w}$ are wages:

$$p_i = \frac{\bar{w}}{\varphi}\frac{\sigma}{\sigma-1} \tag{10}$$

Free entry implies that new firms enter the market until profits decline to zero, from which we derive firm size:[46]

$$q_i = (\sigma-1)\varphi f \tag{11}$$

Firm size is exogenous. Without loss of generalizability, we normalize $\varphi \equiv \sigma/(\sigma-1)$ and $f \equiv 1/\sigma$.[47] Using equilibrium firm size (11) and the price markup (10) in the product demand Eq. (9) then provides

$$\bar{w}^{\sigma} = RP^{\sigma-1} \tag{12}$$

This equation tells us that the wage, at which firms break even, increases in the size of the market R—the larger the market, the higher the wage that firms can afford. Moreover, firms charge the same prices, so that the price index simplifies to $P = \bar{w}Q^{1/(1-\sigma)}$, where Q is total roofing output. Lower-case letters refer to firm-level variables, upper-case letters refer to industry-level variables. We rearrange Eq. (12) to receive the industry-level product demand equation:

$$Q = \bar{w}^{-\tilde{\epsilon}}Q_0^{\tilde{\epsilon}/\epsilon} \tag{13}$$

where $\tilde{\epsilon} \equiv \epsilon\frac{1-\sigma}{\epsilon-\sigma}$ is the wage elasticity of total roofing product demand, which depends on the price elasticity of roofing demand ϵ and the elasticity of substitution between firms' varieties σ. Demand for high-skilled workers H is proportional, as there is no firm heterogeneity:

$$H = \frac{1}{\sigma}\bar{w}^{-\tilde{\epsilon}}Q_0^{\tilde{\epsilon}/\epsilon} \tag{14}$$

We derive demand for medium-skilled workers by combining (7) with (13) and the production function.

$$M = \frac{\sigma-1}{\sigma}Q_0^{\tilde{\epsilon}/\epsilon}w_M^{-\eta}\bar{w}^{\eta-\tilde{\epsilon}} \tag{15}$$

The demand for medium-skilled workers is thus a decreasing function of wages for medium-skilled workers w_M and a de- or increasing function of average wages $\bar{w}$ depending on the relative sizes of the elasticity of demand $\tilde{\epsilon}$ and the substitution elasticity between low and medium-skilled workers η.

A.1.4. Labor Supply

At each time instant t, there is a huge mass of low-skilled workers L_t^S who are searching for work. Their mass exceeds aggregate demand for low-skilled workers $L_t^S \geq L_t$,[48] so that they only earn their reservation wage, unless there is a minimum wage that exceeds their reservation wage $w_L = \max(\underline{w}, w_{MW})$. At each time instant, δL_t low-skilled workers retire and are replaced by other low-skilled entrants, $E_t^L = \delta L_t$.

Assume that at each time instant t there is a mass of medium E_M and high-skilled entrants E_H, who supply one unit of labor with an extensive labor supply wage elasticity of θ. At each time instant, all workers face the exogenous retirement risk δ. Labor supply for

[45] This implies that the varieties produced by the roofing firms are no perfect substitutes. Note, however, that the results of the paper also hold if varieties are perfect substitutes (i.e. $\sigma \to \infty$).

[46] We assume free entry to keep the analysis as simple as possible. Introducing Melitz (2003)-type entrance costs and firm heterogeneity doesn't change the main results.

[47] See Baldwin et al. (2003, p. 23) for the innocuousness of these normalizations. These normalizations do not affect our key results.

[48] This assumption is motivated by the high unemployment rate among low-skilled workers.

medium- and high-skilled workers thus is $M_t = (1-\delta)M_{t-1} + E_M w^{\theta}_{M,e}$ and $H_t = (1-\delta)H_{t-1} + E_H w^{\theta}_{H,e}$. $w_{M,e}$ and $w_{H,e}$ denote expected medium- and high-skilled workers' wages.

In the steady state, the inflow (entrants) of workers equals the outflow (retirement), and expected wages correspond to actual wages. Steady-state labor supply of medium- and high-skilled workers thus is $M = \frac{E_M}{\delta} w^{\theta}_M$ and $H = \frac{E_H}{\delta} w^{\theta}_H$. We abstract from any wage setting frictions, which implies no unemployment among medium- and high-skilled workers. We solve the steady state labor market equilibrium by plugging labor demand (15) into steady state labor supply and drop all time indices. In the steady state, medium-skilled workers' wages are

$$w_M = \left(\frac{\delta(\sigma-1)}{\sigma E_M}\right)^{1/(\theta+\eta)} Q_0^{\frac{\tilde{\epsilon}/\epsilon}{\theta+\eta}} \bar{w}^{\frac{\eta-\tilde{\epsilon}}{\theta+\eta}} \tag{16}$$

We proceed analogously for high-skilled workers to get

$$w_H = \left(\frac{\delta}{\sigma E_H}\right)^{1/\theta} Q_0^{\frac{\tilde{\epsilon}/\epsilon}{\theta}} \bar{w}^{-\tilde{\epsilon}/\theta} \tag{17}$$

A.2. Minimum Wage Regulations

Fig. 6

A.3. Minimum Wage Bite

This appendix builds on Aretz et al. (2011) to analyze the bite of the minimum wage in more detail. Fig. 2 indicates a strong bite of the minimum wage, as reflected by the significant compression at the lower tail of the East German wage distribution. Table 2 provides direct evidence on the size of the minimum wage bite using several indicators for East and West Germany, separately. The data refer to the June 30th prior of each new minimum wage regulation. The starting date of each new minimum wage regulation is depicted jointly with the subsequent new minimum wage level in Columns (1) and (2).[49] Columns (3)-(5) show statistics for workers with an hourly wage below the next minimum wage including its share among the workforce (Column 3), the average individual wage gap, defined as the difference between individuals hourly wage and the expected hourly wage if firms fully comply with the new regulations (Column 4), as well as the average annual hourly wage growth (Column 5). If actual wage growth of workers below the next minimum wage is smaller than the wage gap, firms do not fully comply with the minimum wage regulations. Column (6) shows the annual wage growth for workers with a hourly wage at or above the next minimum wage. Column (7) shows the Kaitz index, defined as the ratio between the minimum wage level and the median wage in the industry. (See Table 7).

The indicators show large differences between East and West Germany. For West Germany, the share of workers with a binding minimum wage (Column 3) increased moderately from 3.8% to 5.2% between 1997 and 2007, before dropping again slightly in the year thereafter. According to the Kaitz index, the bite in West Germany is high and lies in the upper range of what has been found for other countries. The figures for the wage gap and actual wage growth among West German workers (Columns 4 and 5) reveal that actual wage growth lags behind what is necessary to fully comply with the minimum wage regulations. However, this deviation declines toward the end of the observation period. The latter might be explained by stronger controls after 2006, as reported by industry insiders (Aretz et al., 2011). Despite the lack of compliance, the figures for wage growth range between 3.6% and 8.1% for affected workers. The salaries of non-affected workers increased only moderately by 0.7–3.3%, which suggests a decline in wage inequality. (See Fig. 7).

For East Germany, we observe a much stronger bite of the minimum wage. According to Column (3), 13.4% of all East German roofers earned a wage below the 1997 wage floor in June 1997. The share increased rapidly after 2002, when the minimum wage was raised to the same level in East and West, which implied a rapid rise in the East. In June 2005, more than half of the workers (55.3%) had a wage below the 2006 minimum wage level. In fact, the Kaitz index approached the value of 100 in 2005, that is, the median wage equals the minimum wage. Compared to the findings for a strongly affected low-wage industry in the UK (Machin et al., 2003), the bite in the German roofing industry seems extraordinarily large. Machin et al. (2003) find that 32% of the workers were paid below the (age-specific) minimum wage before it was introduced. The mere size of affected workers in East Germany might also explain the higher compliance (i.e. lower deviation of wage gap and actual wage growth, Columns (4) and (5)) compared to West Germany. The more workers earn a minimum wage in a firm, the harder it is to circumvent the regulations. More strikingly, Column (4) shows that East German workers with salaries above the wage floor experienced almost no nominal wage growth or even suffered from wage losses. In the recovery period toward the end of our time series, wages increased only moderately in nominal terms.

A.4. LAK data

The LAK is a public service institution of the employer association ZVDH and the trade union IG Bau in Germany. The main objective is to help insure employees against several structural disadvantages of the industry. For instance, the agency compensates roofers for earnings losses caused by bad weather, ensures a thirteenth monthly income, administrates working-time accounts and old age benefits and promotes vocational education in the industry. For these purposes, the office collects monthly information from firms on the number of actual working hours for each worker as well as their gross wages and the length of their current employment from the year 1995 onward. Since the reporting is mandatory for firms, and may impose a penalty for non-compliance, the information is highly likely to comprise all blue-collar roofers. The information is complemented with further worker characteristics including the date of birth and sex of workers as well as an establishment identifier to calculate further firm-level characteristics. Since the data do not comprise information on (vocational) education and training, we drop workers below 19 years of age that should eliminate most apprentices that are not covered by the minimum wage regulations. We further drop workers above 65 to avoid issues related to early retirement. Furthermore, we focus on men only, since female workers account for only a small fraction in this industry (less than 2%). Moreover, we drop observations where workers are reported to be sick, on vacation, serving in the military, and those with missing and unrealistically high (or low) wages and drop marginal employment.[50]

[49] Note that the indicators may slightly underestimate the bite due to the fact that hourly wages may contain overtime compensation that is not subject to the minimum wage. Overtime hours account for 6% of the working hours in June. This may lead to an estimated hourly wage that is up to 1.6% too high depending on the applied overtime compensation scheme ranging from no additional compensation to a markup of 25%. Since we do not know which scheme is applied and since the resulting imprecision appears to be rather marginal, we left the data uncorrected.

[50] In particular, we drop observations where the hourly wages falls below (above) 50% (150%) of the median hourly wage.

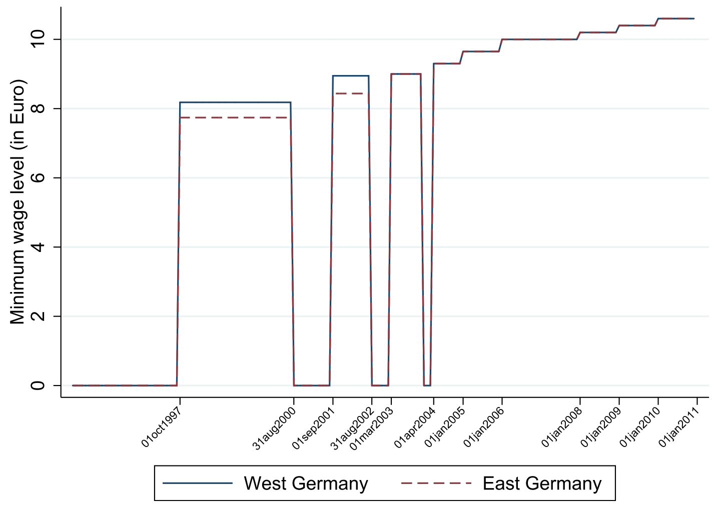

Fig. 6. Minimum wage level in the German Roofing Industry.

Table 7
Indicators of the Minimum Wage Bite Measured in June Prior to the Next Minimum Wage Regulations.

		Workers with an hourly wage:				
			below next minimum wage		at/above next minimum wage	
Date of next minimum wage regulation (1)	minimum wage (in €) (2)	Share of all workers (in %) (3)	Wage gap[a] (in %) (4)	Annual Wage growth[b] (in %) (5)	Annual Wage growth[b] (in %) (6)	Kaitz Index[c] (7)
West Germany						
01.10.97	8.2	3.8	16.9	3.6	2.3	65
01.09.01	8.9	1.5	9.6	7.0	1.5	67
01.03.03	9.0	1.5	10.0	5.6	2.5	67
01.04.04	9.3	2.1	9.3	5.9	1.5	68
01.05.05	9.6	2.7	8.6	4.6	0.7	70
01.01.06	10.0	4.1	7.8	5.0	1.2	73
01.01.07	10.0	4.4	8.2	7.0	3.3	73
01.01.08	10.2	5.2	6.9	5.6	2.3	73
01.01.09	10.4	4.6	6.5	8.1	3.1	73
East Germany						
01.10.97	7.7	13.4	12.2	6.7	-0.0	82
01.09.01	8.4	14.0	4.1	4.7	0.7	89
01.03.03	9.0	33.9	4.3	4.2	0.2	95
01.04.04	9.3	43.8	3.9	4.2	0.4	98
01.05.05	9.6	46.7	4.3	4.0	0.2	99
01.01.06	10.0	55.3	4.1	4.1	0.2	100
01.01.07	10.0	45.0	1.6	1.9	1.0	100
01.01.08	10.2	53.2	2.7	3.3	1.4	101
01.01.09	10.4	49.8	2.4	3.3	0.7	100

Own cacluations based on the LAK data.
[a] The individual wage gap is calculated as follows $wgap_{it} = \left(w_{MW_{i,t+1}} - w_{it}\right)/w_{it}$.
[b] Wage growth corresponds to the actual observed percentage nominal wage change $(w_{it+1} - w_{it})/w_{it}$ between the June preceding and the June following the new minimum wage regulation.
[c] The Kaitz index is defined as the minimum wage divided by the median wage.

Finally, we focus on monthly observations in June to make the data comparable to the BA data and to avoid distortions due to seasonal fluctuations during the months October to April where compensation payments by the LAK are more relevant. In total, we are able to exploit 1,055,137 June observations for 206,753 roofers across the period 1995–2010.

A.5. Changes in the Earnings Distribution

Table 8

A.6. Wages and Hours Worked

To explore in more detail the potential role of hours worked in driving daily wages, Fig. 8 shows monthly hours worked, real hourly wages and real monthly wages based on the LAK data. Panels to the left represent West Germany and panels to the right represent East Germany. Note that the LAK data is only available for a full sample of roofers, but not for our control industries (for a comparison of relative trends between treated and control workers based on daily wages, see Fig. 9).

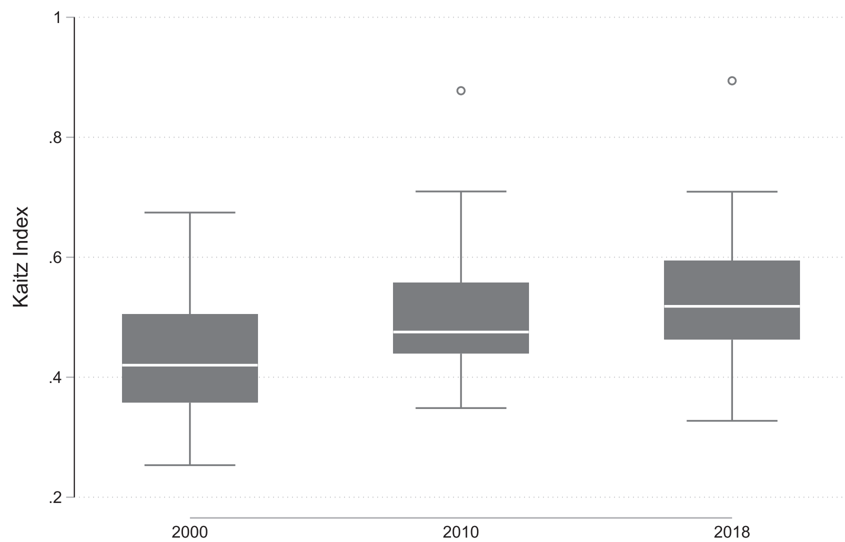

Fig. 7. Kaitz Index in OECD Countries. *Notes:* The figure shows the Kaitz index (= minimum wage/ median wage) for 31 OECD countries for the years 2000, 2010 and 2018. The mean of the Kaitz index is 0.44 (2000), 0.51 (2010) and 0.54 (2018). Own illustration using data from OECD.stat.

Table 8
Average Worker Characteristics by Quantiles of the Real Daily Wage Distribution.

	Quantile of the daily wage distribution				
	$\tau = 0.1$	$\tau = 0.25$	$\tau = 0.5$	$\tau = 0.75$	$\tau = 0.9$
real daily wage (in €)	49.4	56.2	62.9	69	75.8
yearly growth of real daily wages (in %)	3.8	1.2	-.1	-1.2	-2
nominal daily wage (in €)	54.8	62.4	69.7	76.5	84
yearly growth of nominal daily wages (in %)	5.4	2.8	1.5	.4	-.5
share of unskilled workers (non-technicians)	48.9	39.3	25.6	17	16.8
share of skilled workers (technicians)	49.5	59.8	73.4	81.3	77.7
share of master craftmen	.9	.7	.8	1.6	5.4
without vocational traning degree	25	21.8	14.3	9.9	9.9
with vocational traning degree	53.3	62.3	73.6	79.8	78.6
with university degree	.1	.1	.1	.1	.3
tenure in industry (in days)	1289.8	1666.9	2005.2	2291.6	2384.8
average age	32.6	33.6	35.9	38.4	40

Notes: Based on BA data. All figures shown in the table reflect average yearly values of roofers. Real wages are inflation-adjusted to prices in 1994.

Regarding trends before the introduction of the minimum wage, Panel (b) in Fig. 8 suggests that hourly wages developed relatively similar in West Germany across wage quantiles. In contrast, monthly hours worked show an upward trend for the lowest quantile, relative to the other quantiles, although only before and not after the minimum wage introduction (compare Panel a).

This affects the estimated wage effects in Section 5 as follows: The increase in hours worked for the lowest quantile workers in pre-treatment years in West Germany only matters to the extend that it also occurred relative to the control industries. Unfortunately, we can not test this directly, because the detailed LAK data used for Fig. 8 is available only for the treated roofing industry, i.e. not for the control industries.

Instead of testing this directly, let us assume that monthly hours worked also increased relative to the control industries in the years before the introduction (which, as noted, we cannot test). This would imply that part of the daily wage increase is driven by hours worked. In that case, we are underestimating the wage increase in our quantile estimates for the lowest wage groups in West Germany.

This proposed downward bias for the lowest quantile in the West can actually explain our robustness checks in Section 5.3. Using a changes-in-changes approach that corrects for differential trends, we find more positive wage effects. Accordingly, this additional analysis suggests that the downward bias for the lowest quantile in the West that we document in our paper is driven by an increase in hours worked for the lowest quantile before, but not after the minimum wage introduction.

Besides the lowest quantiles in West Germany discussed above, we do not find any other diverging trends in hours worked between quantiles, neither over time nor between East and West. In contrast, hourly wages did develop differently across quantiles in the aftermath of the policy reform (see Panel (b) in Fig. 8). This is particularly visible in the East, and less so in the West. In the East, trends are more positive for lower quantiles and more negative for upper quantiles, reflecting wage compression at both ends of the distribution.

These developments are mirrored in monthly wages: Monthly wages by quantile fluctuate over the years around different trends. These trends thereby point toward a similar direction as discovered for hourly wages, i.e. wage compression in the East and hardly any diverging trends between quantiles in West Germany. Taken together, this evidence suggests that while there are fluctuations over the years in monthly wages that are driven by hours worked, they point to a similar pattern across quantiles as observed for hours worked.

A.7. Daily Wage Gap Between Treated and Control Workers

Fig. 9

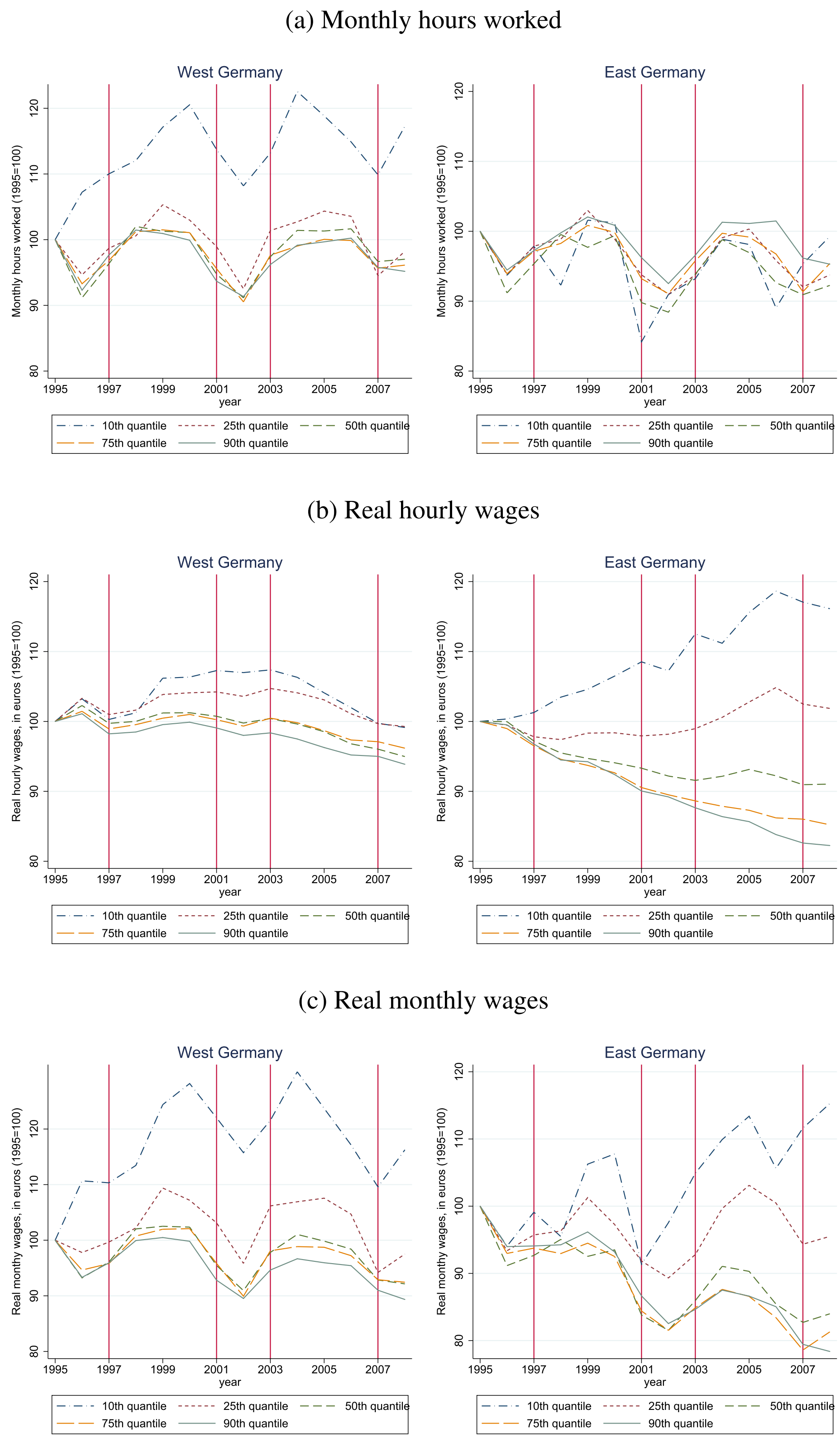

Fig. 8. Development of Hourly Wages, Monthly Wages and Hours Worked. *Notes:* Based on LAK data (roofers only). The vertical lines represent the introduction of the minimum wage in October 1997, the first increase in September 2001, the adjustment of the East German minimum wage level to higher West levels in March 2003 as well as the year 2007 where a regular increase was suspended.

A.8. Dynamic Treatment Effects

As an alternative test for pre-trends, we estimate dynamic UQTEs by replacing our $Post_t$ variable and treatment interaction in Eq. 4 with a full set of year dummies and year-treatment-interactions (for raw differences between treatment and control industries across quantiles and years, see Appendix Fig. 9). The results are depicted until 1996 in Table 3, for the full set of inter-

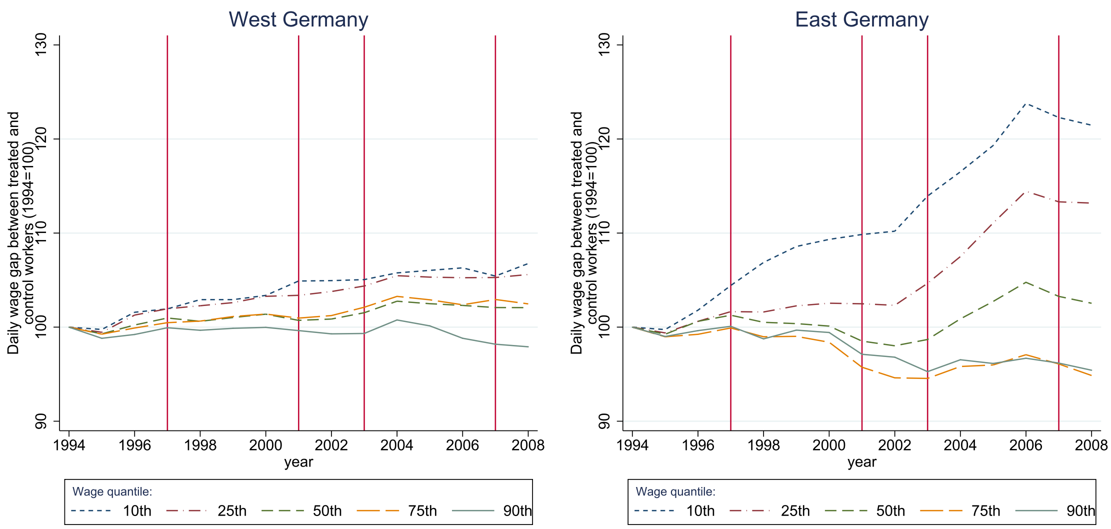

Fig. 9. Daily Wage Gap Between Treated and Control Workers. *Notes:* The figure shows the wage gap between treated and untreated workers by quantile of the wage distribution based on the raw data. A positive (negative) value reflects a higher (lower) wage of treated compared to the untreated workers. The vertical lines represent the introduction of the minimum wage in October 1997, the first increase in September 2001, the adjustment of the East German minimum wage level to higher West levels in March 2003 as well as the year 2007 where a regular increase was suspended (compare Fig. 6).

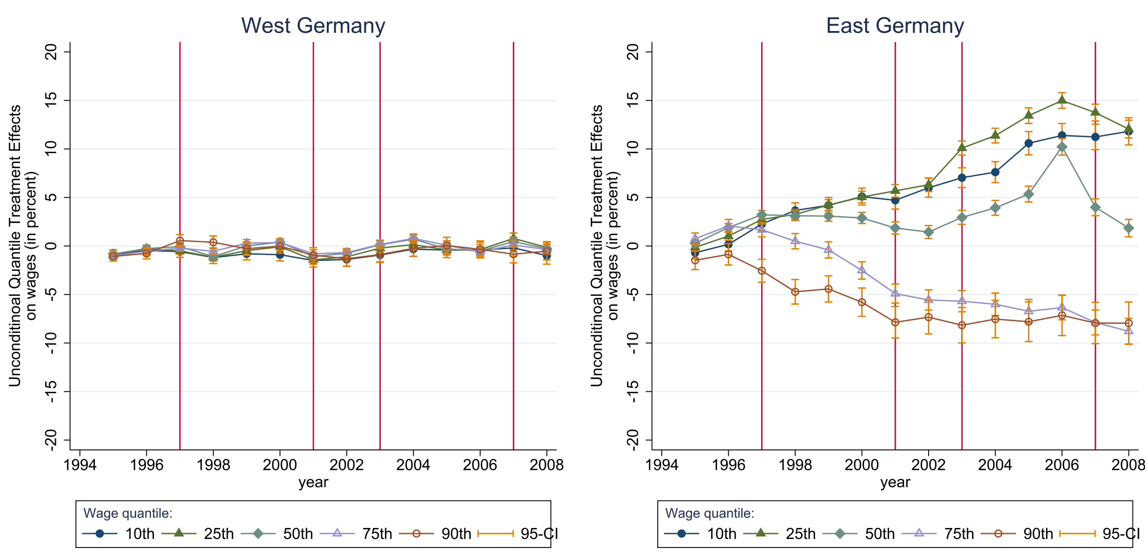

Fig. 10. Minimum Wage Effects on the Unconditional Real Daily Wage Distribution Over Time (Relative to 1994). *Notes:* The figures show unconditional quantile regression estimates of Eq. 4 for selected quantiles by East and West Germany, where the term $Post_t$ is replacing by year dummies (baseline dummies and interactions). All effects are relative to starting year 1994. All models include individual fixed effects as well as several individual and firm-specific covariates. Whiskers mark the 95% significance interval. Robust standard errors clustered by individuals. The vertical lines represent the introduction of the minimum wage in October 1997, the first increase in September 2001, the adjustment of the East German minimum wage level to higher West levels in March 2003 as well as the year 2007 where a regular increase was suspended (compare Fig. 6). Quantile treatment effects reflect the impact of the minimum wage introduction on wages (in %).

actions see Fig. 10. All effects are estimated relative to our starting year 1994.

In East Germany, there are no differences in 1995 (except for a small difference in the top quantile), but we again find indications for anticipation behavior in 1996, the year before the introduction of the minimum wage (Panel B). Firms apparently adjusted wages upward already before the minimum wage was formally in place, but these differences are small: extrapolated over a time period of 10 years, they are smaller by at least one order of magnitude compared to the estimated treatment effects. This is reflected in the fact that our results are remarkably robust to defining 1996 as a treatment year (Panel C.I).

In West Germany, we find significant differences in pre-treatment year 1995 (Panel E), although these are small compared to the estimated treatment effects. A closer inspection of the year-treatment-interactions for West Germany (see Fig. 10) reveals that there is no clear break in these interactions after the introduction of the minimum wage—the interactions fluctuate around zero across the whole time period for all quantiles. Accordingly, we do not find a clear effect of the minimum wage on the West German wage distribution, in line with the relatively small bite of the West German minimum wage (see Table 2). For East Germany, the interaction effects in Fig. 10 instead reflect the results from Table 3. This assures us that the compression effect at upper wage quantiles in

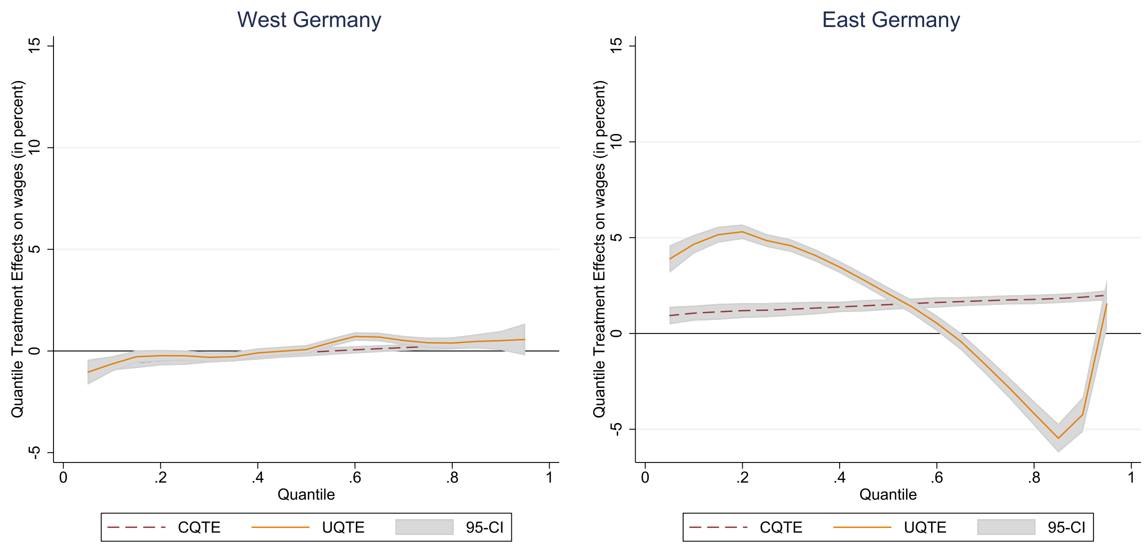

Fig. 11. Minimum Wage Effects on the Unconditional and Conditional Daily Wage Distribution. *Notes:* This figure shows CQTE and UQTE estimates based on Eq. 4 by East and West Germany. All models include individual fixed effects, post-year dummies as well as several individual and firm-specific covariates. 95% confidence intervals are based on robust standard errors clustered by individuals. Standard errors for CQTE are bootstrapped with 100 replications. Quantile treatment effects reflect the impact of the minimum wage introduction on wages (in %).

East Germany reflects the causal impact of the minimum wage reform, whereas the reform had no clear effect on the West German wage distribution.

A.9. Conditional vs. Unconditional Quantile Regression

Fig. 11

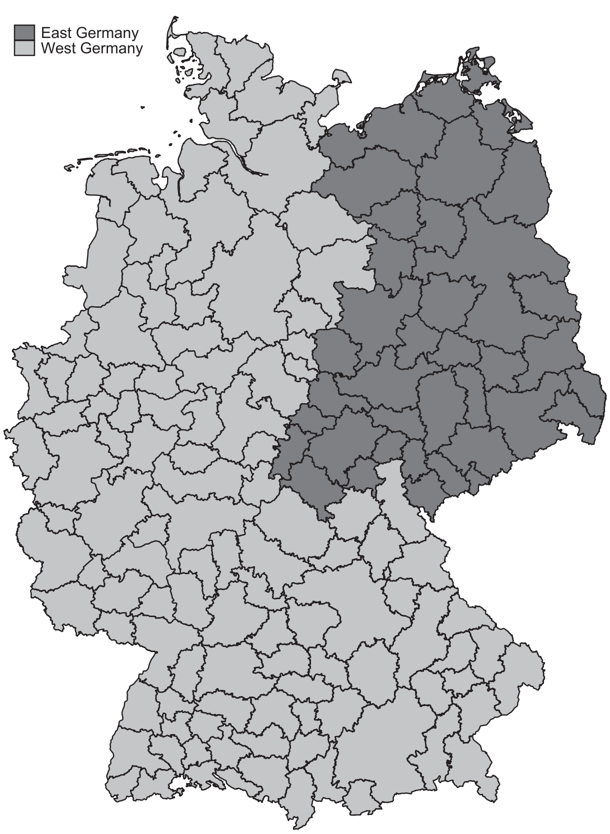

Fig. 12. 150 German Local labor Market. *Notes:* Following the definition of local labor markets by Eckey and Türck (2006).

A.10. Local labor Markets

Fig. 12

A.11. Substitution- and Scale Effects

Fig. 13

As an alternative test for differences in trends before the introduction of the minimum wage in the regional approach, we proceed analogous to our placebo tests, but replace the time trend (baseline and interactions with treatment) with a full set of year dummies and extend the regression to the entire observation period again. If there are no pre-treatment trends, the interaction effects for pre-treatment years should be zero. Results are shown in Panel B of Table 5 for our three skill groups (for a visualization of the full set of interactions, see Fig. 14). The results confirm our placebo tests: There is evidence for anticipation behavior in 1996, whereas in 1995 most interactions are zero (all relative to 1994). Any deviations are small, especially when compared to the estimated treatment effects—if differences in pre-trends bias our results, these biases are small compared to the size of the estimated effect. Fig. 14 further highlights that effects are close to zero before the reform, but grow thereafter, and in particular the effects jump whenever there was a significant increase in the minimum wage. To test the robustness of our results to the indication of anticipation behavior, we re-assign 1996 as a treatment year, similar to the quantile regressions in Section 5.3. Our results are robust to re-assigning those years to the post-reform period (see Panel C in Table 5).

A.12. Skill-Specific wage effects

Fig. 16.

A.13. Firm Revenues

To provide further support on negative aggregate demand shocks in response to the minimum wage (negative scale effects in Section 6), Table 9 shows the results using firm revenues on

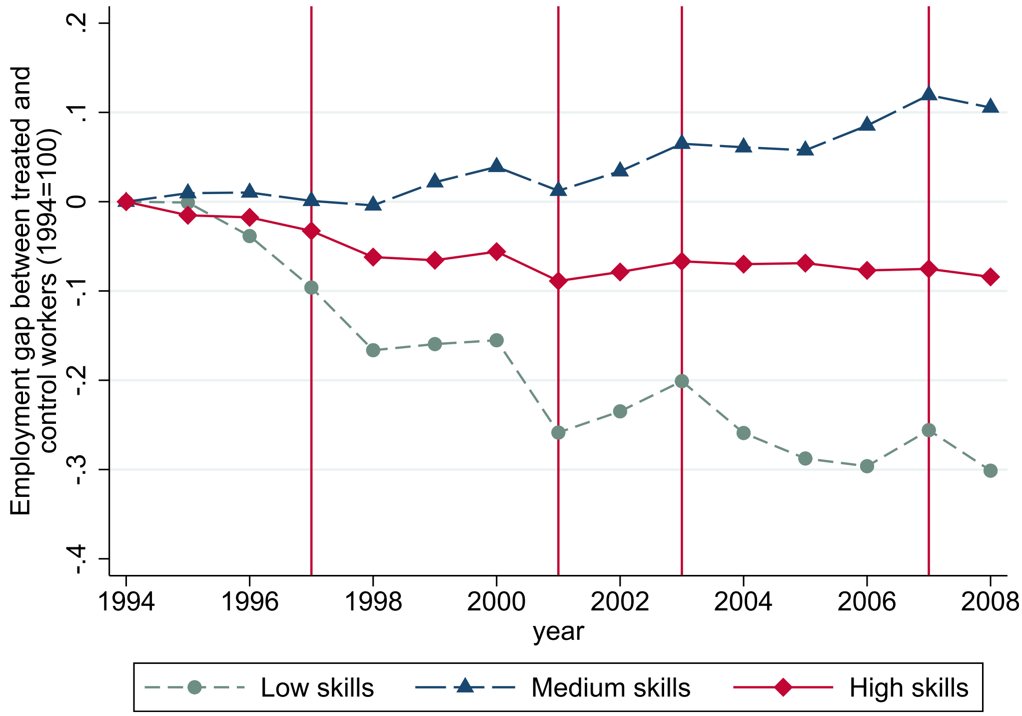

Fig. 13. Employment Gap Between Treated and Control Workers. *Notes:* The figure shows the employment gap between treated and untreated workers by quantile of the wage distribution based on the raw data. A positive (negative) value reflects a higher (lower) number of treated compared to the untreated workers. The vertical lines represent the introduction of the minimum wage in October 1997, the first increase in September 2001, the adjustment of the East German minimum wage level to higher West levels in March 2003 as well as the year 2007 where a regular increase was suspended (compare Fig. 6 in the paper).

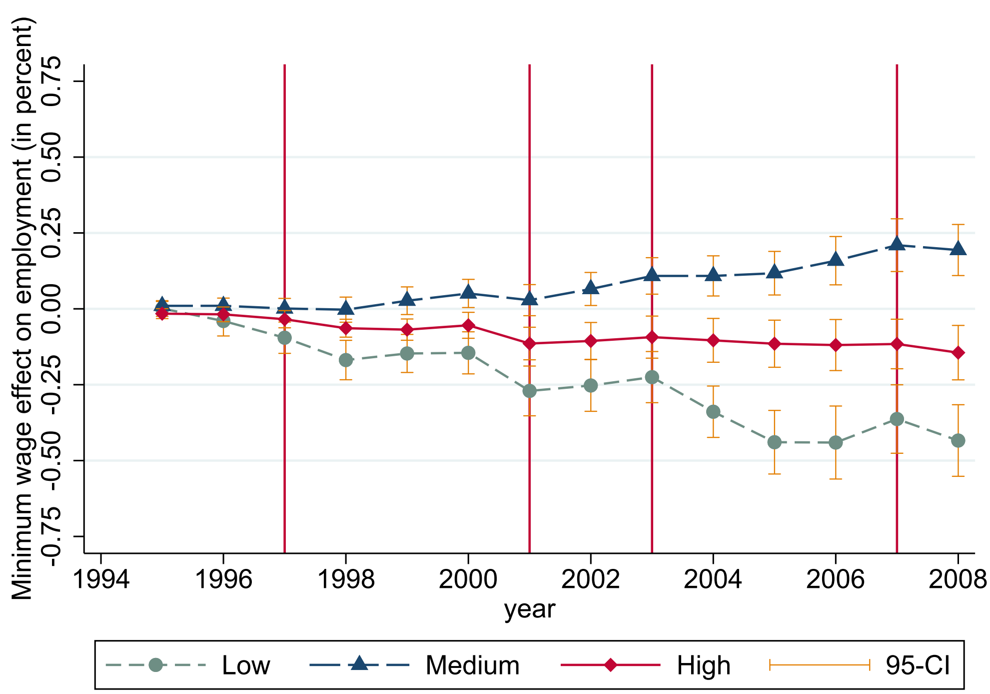

Fig. 14. Minimum Wage Effects on Net Employment by Skill Group and Year (Relative to 1994). *Notes:* The figures show regional estimates of skill-specific net employment by East and West Germany according to Eq. 6, where our $Post_t$ variable is replaced by year dummies (baseline dummies and interactions). All effects are relative to starting year 1994. All models include industry-region fixed effects. All regressions are weighted by regional (skill-specific) employment in pre-treatment years. Whiskers mark the 95% significance interval. Robust standard errors clustered by region. The vertical lines represent the introduction of the minimum wage in October 1997, the first increase in September 2001, the adjustment of the East German minimum wage level to higher West levels in March 2003 as well as the year 2007 where a regular increase was suspended (compare Fig. 6).

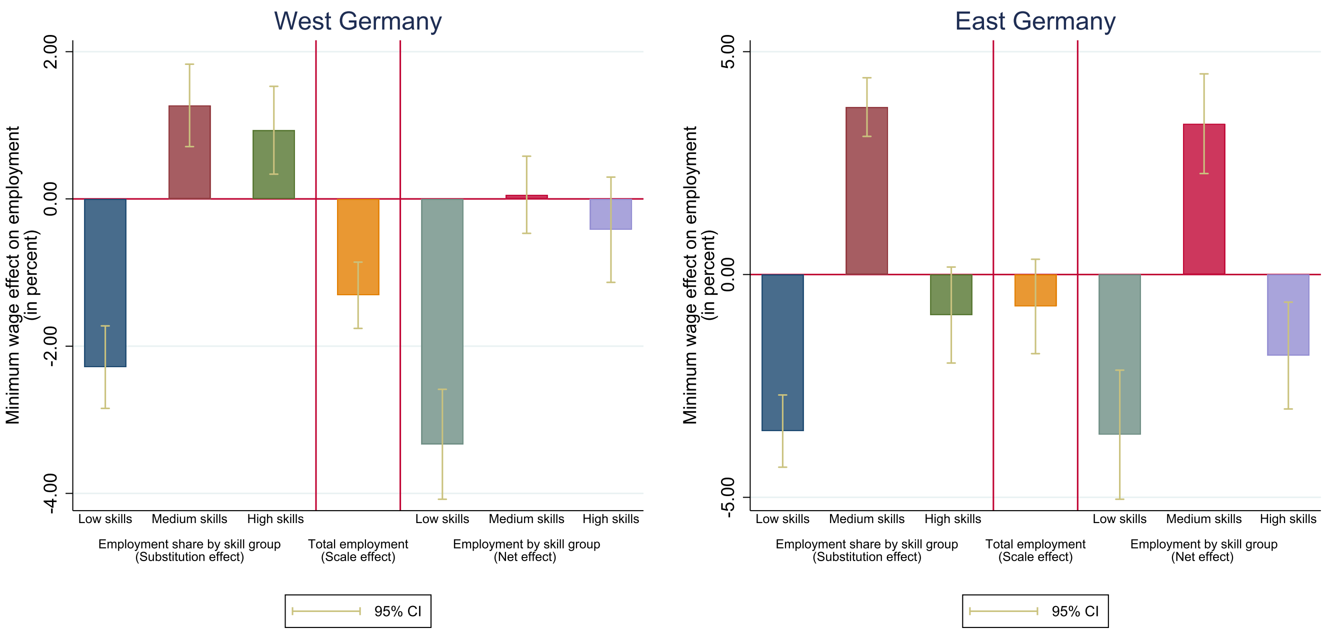

Fig. 15. Minimum Wage Effects on Total Employment (Scale Effect) and Skill-Specific Employment Share (Substitution Effect) by East and West Germany. *Notes:* The figures show regional estimates of skill-specific employment shares, total employment and skill-specific absolute employment based on region-industry year cells between 1994–2008 according to Eq. 6. All models include industry-region fixed effects, post-reform year dummies. Regressions are weighted by regional (skill-specific) employment in pre-treatment years. Whiskers mark the 95% significance interval. Robust standard errors clustered by region. Figures reflect the impact of a 10 percentage points increase in the minimum wage bite (in %). Detailed regression tables are shown in Table 5.

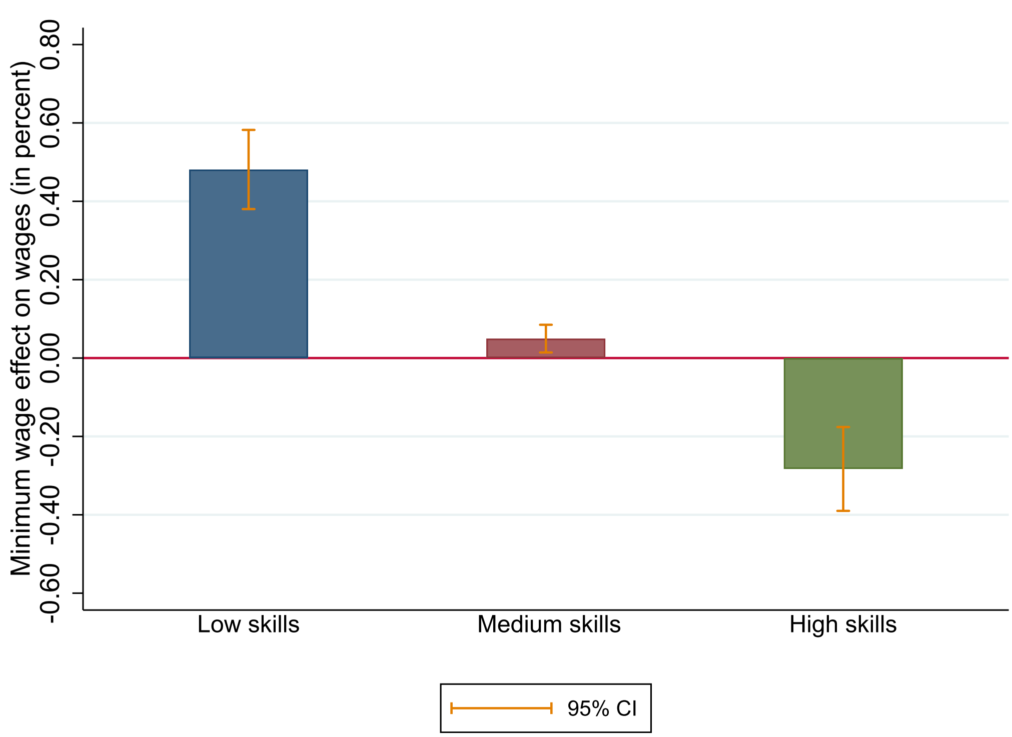

Fig. 16. Minimum Wage Effects on Wages by Skill Group. *Notes:* The figures show regional estimates of skill-specific wage effects by East and West Germany according to Eq. 6. All models include industry-region fixed effects. Regressions are weighted by regional (skill-specific) employment in pre-treatment years. Whiskers mark the 95% significance interval. Robust standard errors clustered by region. Figures reflect the impact of a 10 percentage points increase in the minimum wage bite (in %).

the left hand side of Eq. 6. Revenues are taken from the Mannheimer Unternehmenspanel (MUP), a database that collects information on all active firms in Germany. Data is only available for roofers and plumbers, i.e. we do not have data for glaziers. We look at log total industry revenues, average firm revenues as well as average firm revenues per worker, all defined at the regional level.

The regressions are weighted with pre-treatment regional revenues to control for size differences between regions.

A.14. Unionization

Fig. 17.

A.15. Alternative Treatments for Regional-Level Analysis

A.15.1. Dummy Approach

In our main specification for the substitution and scale effects we rely on the bite as a continuous measure for the treatment. The main reason for using the bite for the regional level analyses

Table 9
Minimum Wage Effects on Industry- and Firm Revenues.

	log total industry revenues (1)	log average firm revenues (2)	log firm revenues per worker (3)
Minimum wage bite	−2.81***	−3.13***	−3.64***
	(-4.00)	(-4.85)	(-4.24)
N	4496	4496	4496

Notes: Revenues based on MUP data. t-statistics in parentheses. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects and are weighted with pre-treatment total regional revenues. Robust standard errors clustered by region. Figures reflect percentage changes in the outcome variable in reaction to a 10 percentage points increase in the minimum wage bite (in %).

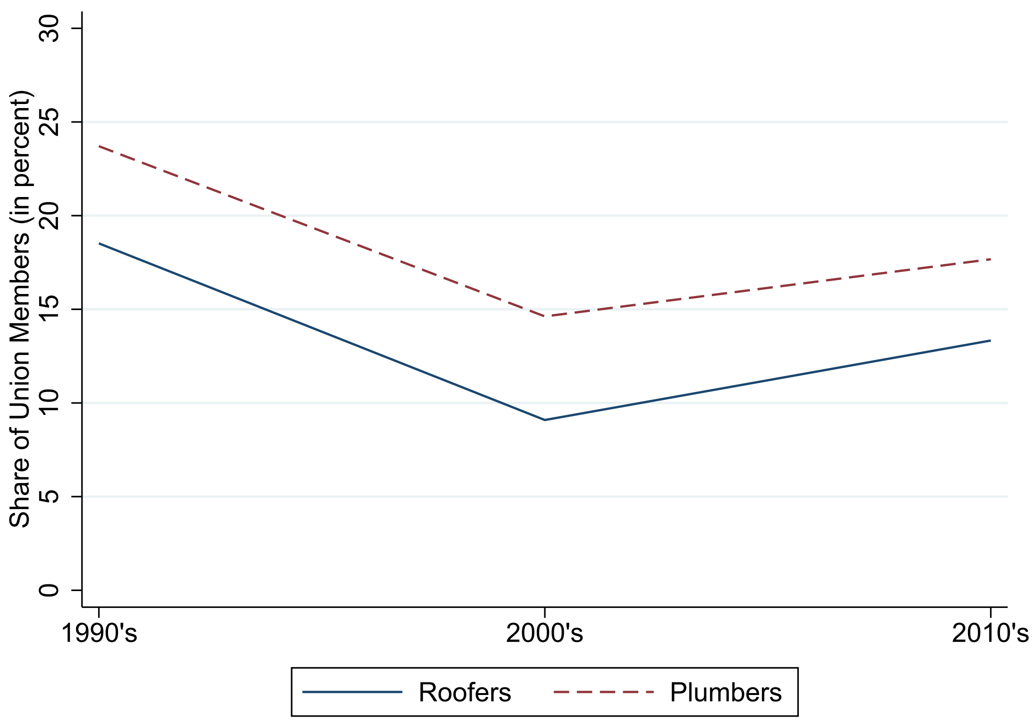

Fig. 17. Unionization among Roofers and Plumbers. *Notes:* The figure shows the share of union members among roofers and plumbers. Figures are based on the German Socioeconomic Panel (SOEP v36 EU-edition). We pool observations because of small sample sizes. Roofers and plumbers are classified using ISCO-88 codes due to the lack of detailed industry codes.

Table 10
Substitution and Scale Effect by Skill Group – Alternative Treatment (Dummy).

Skills:	N (1)	Sub effect Low (2)	Sub effect Medium (3)	Sub effect High (4)	Scale effect All (5)	Net effect Low (6)	Net effect Medium (7)	Net effect High (8)
Baseline model (dummy)	6750	-0.19***	0.16***	0.01	-0.08***	-0.24***	0.08***	-0.07**
		(-9.49)	(7.74)	(0.59)	(-4.51)	(-8.47)	(3.45)	(-2.60)
Assign 1996 to post-reform period	6750	-0.16***	0.14***	0.01	-0.09***	-0.23***	0.05**	-0.09***
		(-9.51)	(8.76)	(0.37)	(-6.04)	(-9.48)	(2.40)	(-4.58)
Only plumbers in control group	4500	-0.20***	0.16***	0.01	-0.10***	-0.29***	0.06**	-0.09***
		(-9.97)	(7.49)	(0.64)	(-5.85)	(-9.74)	(2.44)	(-3.26)

Notes: t-statistics in parentheses. Robust standard errors clustered by region. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects, post-reform year dummies as well as their interactions with East/West. Substitution and scale effects are weighted with pre-treatment regional employment. Net effects are weighted with pre-treatment region-skill-specific employment. The results reflect a $100\times\beta\%$ change in (skill-specific) regional employment as a result of the policy reform.

Table 11
Substitution and Scale Effect by Skill Group – Alternative Bite.

	N		Sub effect		Scale effect		Net effect	
Skills:		Low	Medium	High	All	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline model	6750	-2.90***	2.52***	0.01	-1.01***	-3.48***	1.62***	-1.10***
		(-10.18)	(9.48)	(0.02)	(-3.69)	(-8.17)	(4.49)	(-2.66)
A. Alternative bite (min)	6750	-11.53***	11.68***	-1.98	-3.14**	-12.69***	9.35***	-5.63***
		(-8.97)	(12.02)	(-1.24)	(-2.05)	(-5.88)	(5.60)	(-2.90)
B. Alternative bite (p1)	6750	-13.40***	12.04***	-0.84	-3.67***	-14.82***	9.21***	-5.19***
		(-11.02)	(12.04)	(-0.59)	(-2.74)	(-7.95)	(6.15)	(-2.68)
C. Alternative bite (p5)	6750	-21.06***	19.40***	-2.59	-4.67**	-22.04***	16.41***	-8.46***
		(-12.25)	(12.39)	(-1.17)	(-2.07)	(-7.28)	(6.79)	(-3.03)

Notes: t-statistics in parentheses. Robust standard errors clustered by region. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects, post-reform year dummies as well as their interactions with East/West. Substitution and scale effects are weighted with pre-treatment regional employment. Net effects are weighted with pre-treatment region-skill-specific employment. Numbers reflect the impact of a 10 percentage points increase in the minimum wage bite (in %).

Table 12
Weighted vs. Unweighted Results.

	N		Sub effect		Scale effect		Net effect	
Skills:		Low	Medium	High	All	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Weighted Results								
Baseline model	6750	-2.90***	2.52***	0.01	-1.01***	-3.48***	1.62***	-1.10***
		(-10.18)	(9.48)	(0.02)	(-3.69)	(-8.17)	(4.49)	(-2.66)
Only plumbers in control group	4500	-3.13***	2.59***	0.07	-1.36***	-4.15***	1.31***	-1.39***
		(-11.04)	(9.20)	(0.21)	(-4.92)	(-9.44)	(3.45)	(-3.21)
B. Unweighted Results								
Only plumbers in control	4500	-2.19***	-2.19***	-0.36	-1.27***	-3.46***	1.26***	-1.65***
		(-8.15)	(10.93)	(-0.96)	(-4.50)	(-8.72)	(3.73)	(-3.32)

Notes: t-statistics in parentheses. Robust standard errors clustered by region. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects, post-reform year dummies as well as their interactions with East/West. Substitution and scale effects are weighted with pre-treatment regional employment. Net effects are weighted with pre-treatment region-skill-specific employment in Panel A but not in Panel B. Results reflect the impact of a 10 percentage points increase in the minimum wage bite.

is as follows. For the regional level analyses fewer observations are available due to aggregation. To avoid noisy estimates, we pool across East and West. We rely on the bite to take account of the large differences in the bite between East and West. This has the additional benefit of exploiting the level of the bite to identify its effect.

To test the sensitivity of our results, Table 10 provides estimates of the same regression models but relying on a pre-post-treatment dummy, instead. The alternative approach confirms our results but the interpretation is now different. For instance, while we find in our main specification that a 10% larger bite reduces net employment of high-skilled workers by 1.1%, Table 10 shows that the minimum wage policy reform reduced the demand for high-skilled workers by 7% on average. Given that the minimum bite increased by 73 percentage points in the West and 100 percentage points in the East, both approaches provide comparable results although the pooled dummy-variable approach is closer to the West German than to the East German results. The latter is not surprising since the West German labor market dominates the pooled results because of its size. Considering the different interpretation, all other coefficients are also remarkably similar in size and significance.

A.15.2. Alternative Bite

As a robustness check, we re-estimate our baseline model using a Kaitz index in pre-treatment years where we assume the minimum wage to be (A) the lowest hourly wage received. To take account of outliers, we additionally conduct the same analysis assuming the minimum wage to be (B) the average wage at the 1st and (C) 5th percentile of the hourly wage distribution. The results are depicted in Table 11 below. The size and significance of all effects are the same as in the baseline model (shown in the first row). The only differences is that the effects are all larger.

The intuition behind the larger marginal effect sizes is that the increase in the minimum wage appears smaller when we assume that the pre-minimum wage was the preceding lowest wage instead of zero. Comparing the same change in employment to smaller increases in the minimum wage implies that the marginal effect must be larger. This is also reflected in the rising coefficients when moving from results (A) to (C) in Table 11.

Since the hourly wage data is only available for the roofing, not for the control industries, the approach introduces a new assumption—the minimum wage received of plumbers was equal to that of roofers in pre-treatment years. We therefore stick to our more conservative estimates at baseline.

Table 13
Minimum Wage Effects on Employment of Uncovered White-Collar Workers.

Dependent variable: log number of white-collar workers	West Germany	East Germany	Germany
	(1)	(2)	(3)
Minimum wage bite	−1.28***	−1.05	−1.16**
	(-2.98)	(-1.20)	(-2.43)
N	5024	1705	6729

Notes: The figures show regional estimates of white-collar employment based on region-industry-year cells between 1994–2008 according to Eq. 6 in the paper. t-statistics in parentheses. Robust standard errors clustered by region. Significance levels: * 10%, ** 5%, *** 1%. All estimates include industry-region fixed effects, post-reform year dummies as well as their interactions with East/West. Effects are weighted with pre-treatment regional employment. Numbers reflect the impact of a 10 percentage points increase in the minimum wage bite (in %).

A.16. Weights

We weight all analyses in the regional-level estimations in Sections 6 and 7 to take account of the large size differences between region-sector cells. The outcome variables are aggregated from the random sample of workers to the level of region-sector cells, which implies large variances in those cells that are based on few observations, leading to heteroskedasticity. The weights serve to give lower weight to observations with high variance. When weighting to correct for heteroskedasticity, Solon et al. (2015) recommend to compare weighted results to unweighted results to check for model misspecification or endogenous sampling.

Note that glaziers are a particularly small sample and that in many regions, there are only few glazier observations: The average number of observations in a region is 405 for plumbers but only 65 for glaziers, the minimum value for plumbers is 16 but 0 for glaziers, and the 5th percentiles are 67 for plumbers but only 3 for glaziers. These small cell sizes are likely to lead to large errors in unweighted regressions.

We drop glaziers and re-run our estimations without weights. Table 12 reports results for our baseline model from Table 5, for the weighted model using plumbers as a control group, as well as the latter model without weights. Comparing the weighted and unweighted results highlights that there are only little differences between the two models, indicating that model misspecification or endogenous sampling do not play a major role.

A.17. Scale Effect for Uncovered Workers

White-collar workers, which are exempted from the minimum wage regulations, are a relatively small fraction of the workforce in the investigated industry. They make up 8.6% of all roofers in our data (excluding marginal employment), mostly female office clerks with medium skills.

To test whether the scale effect also affected these uncovered white-collar workers, we estimated the net employment effect on this group. The results are shown in Table 13 below. Generally, due to the relatively low share of these worker, we face relatively small sample sizes (at least when distinguishing between East and West Germany). Despite this, we find that the negative scale effect is present for this group as well: Overall employment in the roofing industry for this group declined in response to the minimum wage, even though the minimum wage is not binding for them. Although the coefficients all have a negative sign, the coefficient for East-Germany is not significant. This is could be driven by the drop in sample size in East Germany.

References

Aaronson, D., Phelan, B.J., 2019. Wage shocks and the technological substitution of low-wage jobs. Econ. J. 129 (617), 1–34.

Acemoglu, D., Pischke, J.-S., 1999. The structure of wages and investment in general training. J. Polit. Econ. 107 (3), 539–572.

Acemoglu, D., Pischke, J.-S., 2003. Minimum wages and on-the-job training. Res. Labor Econ. 22, 159–202.

Alessandrini, D., Milla, J., 2021. Minimum wage effects on human capital accumulation: Evidence from canadian data. IZA DP No. 14178.

Apel, H., Bachmann, R., Bender, S., vom Berge, P., Fertig, M., Frings, H., König, M., Paloyo, A., Schaffner, S., Tamm, M., et al., 2012. Arbeitsmarktwirkungen der Mindestlohneinführung im Bauhauptgewerbe. J. Labour Market Res. 45 (3–4), 257–277.

Aretz, B., Arntz, M., Gottschalk, S., Gregory, T., Niefert, M., Rammer, C., Schröder, H., Schütz, H., 2011. ZEW-Studie zu den Wirkungen eines Mindestlohns in der Dachdeckerwirtschaft. Research report for the Federal Ministry of Labour and Social Affairs, ZEW - Zentrum für Europäische Wirtschaftsforschung/ Center for European Economic Research.

Aretz, B., Arntz, M., Gregory, T., 2013. The minimum wage affects them all: Evidence on employment spillovers in the roofing sector. German Econ. Rev. 14 (3), 282–315.

Arulampalam, W., Booth, A.L., Bryan, M.L., 2004. Training and the new minimum wage. Econ. J. 114, C87–C94.

Athey, S., Imbens, G., 2006. Identification and inference in nonlinear difference-in-differences models. Econometrica 74 (2), 431–497.

Autor, D.H., Levy, F., Murnane, R.J., 2003. The skill content of recent technological change: An empirical exploration. Quart. J. Econ. 118 (4), 1279–1333.

Autor, D.H., Manning, A., Smith, C.L., 2016. The contribution of the minimum wage to u.s. wage inequality over three decades: A reassessment. Am. Econ. J.: Appl. Econ. 8, 58–99.

Baldwin, R., Forslid, R., MArtin, P., Ottaviano, G., Robert-Nicoud, F., 2003. Economic Geography and Public Policy. Princeton University Press.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Quart. J. Econ. 119 (1), 249–275.

Boeri, T., Giupponi, G., Krueger, A.B., Machin, S., 2020. Solo self-employment and alternative work arrangements: A cross-country perspective on the changing composition of jobs. J. Econ. Perspect. 34 (1), 170–195.

Card, D., 1996. The effect of unions on the structure of wages: A longitudinal analysis. Econometrica 64 (4), 957–979.

Card, D., Lemieux, T., Riddel, W., 2004. Unions and wage inequality. J. Labor Res. 25, 519–562.

Card, D., Lemieux, T., Riddel, W., 2020. Unions andwage inequality: The roles of gender, skill and public sector employment. Canad. J. Econ. 53 (1), 140–173.

Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. Quart. J. Econ. 134 (3), 1405–1454.

Chaplin, D., Turner, M., Pape, A., 2003. Minimum wages and school enrollment of teenagers: a look at the 1990s. Econ. Educat. Rev. 22 (1), 11–21.

Clemens, J., 2021. How do firms respond to minimum wage increases? understanding the relevance of non-employment margins. J. Econ. Perspect. 35 (1), 51–72.

Clemens, J., Kahn, L.B., Meer, J., 2021. Dropouts need not apply? the minimum wage and skill upgrading. J. Labor Econ. 39 (51), S107–S149.

Clemens, J., Strain, M.R., 2017. Estimating the employment effects of recent minimum wage changes: Early evidence, an interpretative framework, and a pre-commitment to future analysis. NBER Working Papers 23084, National Bureau of Economic Research.

Clemens, J., Wither, M., 2019. The minimum wage and the great recession: Evidence of effects on the employment and income trajectories of low-skilled workers. J. Public Econ. 170, 53–67.

Combes, P.-P., Duranton, G., Gobillon, L., 2008. Spatial wage disparities: Sorting matters! J. Urban Econ. 63 (2), 723–742.

Couch, K.A., Wittenburg, D.C., 2001. The response of hours of work to increases in the minimum wage. South. Econ. J., 171–177

David, H., Dorn, D., 2013. The growth of low-skill service jobs and the polarization of the us labor market. Am. Econ. Rev. 103 (5), 1553–1597.

Derenoncourt, E., Montialoux, C., 2021. Minimum wages and racial inequality. Quart. J. Econ. 136 (1), 169–228.

Dickens, R., Manning, A., 2004. Has the national minimum wage reduced uk wage inequality? J. Roy. Stat. Soc. Ser. A 167 (4), 613–626.

Downey, M., 2021. Partial automation and the technology-enabled deskilling of routine jobs. Labour Econ. 69, 101973.

Dube, A., Giuliano, L., Leonard, J., 2018. Fairness and frictions: the impact of unequal raises on quit behavior. Am. Econ. Rev. 109, 620–663.

Eckey, Kosfeld, Türck, 2006. Abgrenzung deutscher arbeitsmarktregionen. Raumforschung und Raumordnung 64, 299–309.

Fairris, D., Pedace, R., 2004. The impact of minimum wages on job training: An empirical exploration with establishment data. South. Econ. J. 70 (3), 566–583.

Falk, A., Fehr, E., Zehnder, C., 2006. Fairness perceptions and reservation wage - The behavioral effects of minimum wage laws. Quart. J. Econ. 121 (4), 1347–1381.

Firpo, S., Fortin, N.M., Lemieux, T., 2009. Unconditional quantile regressions. Econometrica 77 (3), 953–973.

Freeman, A.C., Freeman, R.B., 1991. Minimum wages in puerto rico: Textbook case of a wage floor? NBER Working Paper 3759, NBER.

Ganserer, A., Gregory, T., Murmann, S., Zierahn, U., 2021. Minimum wages and solo self-employment. Unpublished.

Giuliano, L., 2013. Minimum wage effects on employment, substitution, and the teenage labor supply: Evidence from personnel data. J. Labor Econ. 31 (1), 155–194.

Gopalan, R., Hamilton, B.H., Kalda, A., Sovich, D., 2020. State minimum wages, employment, and wage spillovers: Evidence from administrative payroll data. State Minimum Wages, Employment, and Wage Spillovers: Evidence From Administrative Payroll Data (Mar 1, 2020). Journal of Labor Economics, Forthcoming.

Gramlich, E.M., Flanagan, R.J., Wachter, M.L., 1976. Impact of minimum wages on other wages, employment, and family incomes. Brookings Papers on Econ. Activity 1976 (2), 409–461.

Grossberg, A.J., Sicilian, P., 1999. Minimum wages, on-the-job training, and wage growth. South. Econ. J. 65 (3), 539–556.

Grossman, J., 1983. The impact of the minimum wage on other wages. J. Human Resour. 18 (3), 359–378.

Hamermesh, D.S., 1996. Labor Demand. Princeton University Press.

Hamermesh, D.S., Grant, J., 1979. Econometric studies of labor-labor substitution and their implications for policy. J. Human Resour. 14, 518–542.

Hashimoto, M., 1982. Minimum wage effects on training on the job. Am. Econ. Rev. 72 (5), 1070–1087.

Havnes, T., Mogstad, M., 2015. Is universal child care leveling the playing field? J. Public Econ. 127, 100–114.

Horton, J.J. (2018). Price floors and employer preferences: Evidence from a minimum wage experiment. unpublished.
Jardim, E., Long, M.C., Plotnick, R., Inwegen, E. v., Vigdor, J., Wething, H., 2018a. Minimum wage increases and individual employment trajectories. Technical report, NBER Working Paper 25182.
Jardim, E., Long, M.C., Plotnick, R., Inwegen, E. v., Vigdor, J., Wething, H., 2018b. Minimum wage increases, wages, and low-wage employment: Evidence from seattle. Technical report, NBER Working Paper 23532.
Koenker, R.W., Bassett, G.W., 1978. Regression quantiles. Econometrica 46 (1), 33–50.
Kreiner, C.T., Daniel, R., Skov, P.E., 2020. Do lower minimum wages for young workers raise their employment? evidence from a danish discontinuity. Rev. Econ. Stat. 102 (2), 339–354.
Lechthaler, W., Snower, D.J., 2008. Minimum wages and training. Labour Econ. 15, 1223–1237.
Lee, C., 2020. Minimum wage policy and community college enrollment patterns. ILR Rev. 73 (1), 178–210.
Lee, D.S., 1999. Wage inequality in the United States during the 1980s: Rising dispersion or falling minimum wage? Quart. J. Econ. 114 (3), 977–1023.
Lordan, G., Neumark, D., 2018. People versus machines: The impact of minimum wages on automatable jobs. Labour Econ. 52, 40–53.
Machin, S., Manning, A., Rahman, L., 2003. Where the minimum wage bites hard: Introduction of minimum wages to a low wage sector. J. Eur. Econ. Assoc. 1 (1), 154–180.
Manning, A., 2003. Monopsony in motion: Imperfect competition in labor markets. Princeton University Press.
Meer, J., West, J., 2016. Effects of the minimum wage on employment dynamics. J. Human Resour. 51 (2), 500–522.
Melitz, M.J., 2003. The impact of trade on intra-industry reallocation and aggregate industry productivity. Econometrica 71 (6), 1696–1725.
Neumark, D., Schweitzer, M., Wascher, W., 2004. Minimum wage effects throughout the wage distribution. J. Human Resour. 39 (2), 425–450.
Neumark, D., Shirley, P., 2021. Myth or measurement: What does the new minimum wage research say about minimum wages and job loss in the united states? NBER Working Papers 28388.
Neumark, D., Wascher, W., 1995. Minimum-wage effects on school and work transitions of teenagers. Am. Econ. Rev. 85 (2), 244–249.
Neumark, D., Wascher, W., 2001. Minimum wages and training: Revisited. J. Labor Econ. 19 (3), 563–595.
Neumark, D., Wascher, W., 2003. Minimum wages and skill acquisition: another look at schooling effects. Econ. Education Rev. 22 (1), 1–10.
Neumark, D., Wascher, W., 2008. Minimum wages. MIT Press.
Pacheco, G.A., Cruickshank, A.A., 2007. Minimum wages and school enrollment of teenagers: a look at the 1990's. Econ. Education Rev. 26 (5), 574–587.
Pettengill, J., 1981. The long run impact of a minimum wage on employment and the wage structure. Report Minimum Wage Study Commiss. 6, 63–104.
Phelan, B.J., 2019. Hedonic-based labor supply substitution and the ripple effect of minimum wages. J. Labor Econ. 37 (3), 905–947.
Schumann, M., 2017. The effects of minimum wages on firm-financed apprenticeship training. Labour Econ. 47, 163–181.
Solon, G., Haider, S.J., Wooldridge, J.M., 2015. What are we weighting for? J. Human Resources 50 (2), 301–316.
Teulings, C.N., 2000. Aggregation bias in elasticities of substitution and the minimum wage paradox. Int. Econ. Rev. 41 (2), 359–398.
Teulings, C.N., 2003. The contribution of minimum wages to increasing wage inequality. Econ. J. 113 (490), 801–833.
Zavodny, M., 2000. The effect of the minimum wage on employment and hours. Labour Econ. 7 (6), 729–750.