




# High-impact minimum wages and heterogeneous regions

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Received: 2 November 2017 / Accepted: 2 March 2019 / Published online: 18 March 2019  
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## Abstract

We estimate the effects of the introduction of and subsequent increases in a substantial minimum wage in Germany's main construction industry on wage and employment growth rates. Using a regional dataset constructed from individual employment histories, we exploit the spatial dimension and border discontinuities of the regional data to account for spillovers between districts and unobserved heterogeneity at the local level. The results indicate that the minimum wage increased the wage growth rate for East Germany but did not have a significant impact on the West German equivalent. The estimated effect on the employment growth rate reveals a contraction in the east of about 1.2 percentage points for a one-standard-deviation increase in the minimum-wage bite, amounting to roughly one quarter of the overall decline in the growth rate. We observe no change for the West.

**Keywords** Construction sector · Germany · Minimum wage · Spatial heterogeneity · Spatial panel data

**JEL Classification** J31 · J38

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This paper is based on an earlier version published as Ruhr Economic Paper No. 408. The earlier draft was co-authored by Alfredo R. Paloyo, and the project benefited substantially from his insights and collaboration. We are grateful to Ronald Bachmann, Thomas K. Bauer, Daniel Baumgarten, Joachim Möller, Marion Penninger, Achim Schmillen, Matthias Umkehrer, Till von Wachter, several anonymous referees, and participants of the 52nd Annual Congress of the European Regional Science Association, the 6th Doctoral Conference of the Ruhr Graduate School in Economics, the 27th Annual Conference of the European Society for Population Economics, the 25th Annual Conference of the European Association of labour Economists, the 3rd SEEK Conference, the 47th Annual Conference of the Canadian Economics Association, the 18th Spring Meeting of Young Economists, the 5th International Doctoral Meeting of Montpellier, Verein für Socialpolitik 2013, the Econometric Society Australasia Meeting 2013, and seminars at the University of Utrecht, the Berliner Netzwerk Arbeitsmarktforschung, the RWI, and the Australian National University for helpful comments. All remaining errors are our own.

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**Electronic supplementary material** The online version of this article (<https://doi.org/10.1007/s00181-019-01661-0>) contains supplementary material, which is available to authorized users.

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## 1 Introduction

The last 25 years have seen an enormous improvement in empirical minimum-wage research, with the major methodological progress driven by a literature focussing on the USA and UK (Card and Krueger 1994; Dube et al. 2010; Dolton et al. 2012; Neumark et al. 2014; Allegretto et al. 2017). While the discussion on the precise effect of the US minimum wage on teen employment is still ongoing, a better understanding of the more general effects of minimum wages requires empirical results from different countries and institutional settings. As minimum wages tend to be comparatively low in the USA and UK, case studies of labour markets affected by significantly higher minimum wages are particularly important (Manning 2016).

In this paper, we examine the wage and employment effects of the introduction of and subsequent increases in a sectoral minimum wage in German main construction (*Bauhauptgewerbe*) between 1997 and 2002, soon after the German reunification boom started to wear off. The minimum wage was introduced at 84% (64%) of the median hourly wage in East Germany (West Germany). Our study therefore clearly departs from previous Anglo-Saxon studies by focusing on high-impact minimum wages, introduced in a bread-earning industry during a time of economic contraction. While we find no wage or employment effects of the minimum wage in West Germany, our estimates point to a clear wage increase in East Germany. Results on employment effects of the minimum wage in East Germany are ambiguous.

While the introduction of the minimum wage in the German construction sector has been studied before (König and Möller 2009; Müller 2010; Frings 2013), we are the first to use regional variation in the treatment intensity of the minimum wage for identification in the German case. We measure treatment intensity by the minimum wage's bite at the district level, defined as the share of workers earning below the minimum wage in the period prior to its introduction or increase. The source of variation that we exploit therefore originates from changes in the nominal minimum wage over time and from differences in the regional wage structure. We argue that our regional bite measure is more robust to measurement error in hourly wages than the individual minimum-wage affectedness used in previous German studies. Additionally, our identification strategy allows us to take advantage of recent methodological improvements suggested in the empirical literature that help to reduce bias stemming from spatial heterogeneity and spillovers at the local level.

The importance of spatial heterogeneity has been discussed in the US minimum-wage literature only recently (Dube et al. 2010; Allegretto et al. 2011, 2017; Neumark et al. 2014; Neumark and Wascher 2017), and the appropriate techniques have thus far never been applied to a situation similar to the German institutional setting. We fill this research gap by applying different techniques to account for spatial heterogeneity, including more traditional panel approaches with region-specific trends (Neumark and Wascher 2008; Neumark et al. 2014) as well as the contiguous border approach proposed by Dube et al. (2010) that builds upon the case study approach advocated by Card and Krueger (1995).

Both approaches have advantages and disadvantages over the other in terms of controlling for spatial heterogeneity. The more traditional panel approaches are sensitive to diverging regional employment trends that are independent of the minimum wage

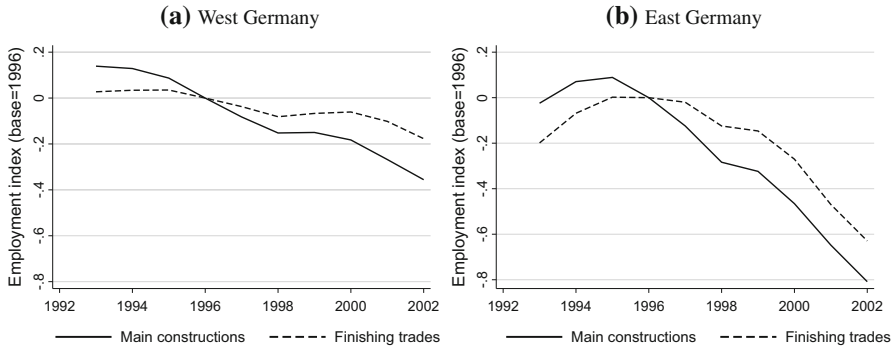
but correlated with the minimum-wage bite. For example, to the extent that regions with a lower wage level—and thus a higher bite—show structurally lower employment growth rates than regions that are characterized by higher wages, this difference in employment growth would be incorrectly attributed to the minimum wage. In contrast, the case study approach of comparing a panel of neighbouring regions naturally reduces spatial heterogeneity, but the entire identification strategy rests on the assumption that the minimum wage in the treatment region does not influence employment in the control region. Given the regional proximity, the assumption that no regional spillover effects exist is not necessarily plausible.

We recognize that the presence of spatial autocorrelation may bias estimates of the minimum-wage effects when we employ a regional identification strategy. Even in the more developed US literature, spatial spillovers have often been ignored (as exceptions, see Kalenkoski and Lacombe 2013; Dolton et al. 2015). We therefore include specifications that explicitly take spatial autocorrelation into account by estimating the effect of the bite of neighbouring districts.

Studying the minimum wage in Germany's main construction sector provides an interesting complement to the international literature on minimum wages for a number of reasons. First, it was introduced at 8.00€ in East Germany—corresponding to 84% of the median hourly wage of construction workers—and 8.70€ in West Germany—still amounting to 64% of the median hourly wage. In contrast, the US federal minimum wage, currently set at \$7.25 per hour, is a mere 42% of the median hourly wage of all occupations in 2015 (Bureau of Labor Statistics 2015). These high nominal minimum wages translated into a high bite, especially in East Germany. Averaged across all districts, almost 22% of construction workers earned less than the minimum wage prior to its introduction. Despite the higher nominal minimum wage in West Germany, its real intensity was much lower, as the average bite of roughly 4% shows. This can be explained by the higher coverage rate of collective bargaining agreements in West compared to East Germany. The behaviour of the labour market in response to a slight change in the minimum wage may not necessarily be similar to how it will react to a significant and sustained increase in the wage floor. Therefore, our results contribute to the discussion on the effects of strongly binding minimum wages.

Second, the vast majority of studies focuses on low-wage sectors or youth employment. Obviously, the minimum wage is expected to have the biggest effect in these labour-market segments, but the price of this focus is that much less is known about the consequences for prime-aged, male, full-time employment, which is often decisive for household income. We close this gap by studying minimum-wage effects for the main construction sector, which makes up a significant fraction of the German economy. At the time the wage floor was set, it was employing about 1.3 million workers, making it the largest German industry where sectoral minimum wages apply until today (Bachmann et al. 2015). As of 2016, its share of Germany's GDP of almost 5% is substantial.

Third, main construction has two further properties that make minimum-wage research on this industry especially worthwhile. Following reunification, the East German construction sector experienced a strong boom until the mid-1990s. After the first wave of public investments subsided, an economic contraction hit that coin-



**Fig. 1** Employment in main construction and finishing trades. *Note:* The employment level in both industries is indexed with 1996 as the base year. The minimum-wage introduction took place in 1997. *Source:* Authors' calculations based on the IEB

cided with the minimum-wage introduction. Specifically, nominal gross value added (in billion €) dropped from 113.0 in 1995 to 99.2 in 1998 (IAB, RWI and ISG 2011). Figure 1 shows the labour-market development for two sub-sectors of the construction industry: main construction and finishing trades. Both sub-sectors are clearly affected by the recession, while the minimum wage was only introduced in main construction. We will use this fact to separate the treatment effect we are interested in from the general economic development (Sect. 3). While posing a challenge for identification of the minimum-wage effect, the recessionary environment does have the advantage of estimating lower bounds of any disemployment effect (Dolton and Bondibene 2012; Clemens and Wither 2014).

Our findings are summarized as follows. First, we conclude that the new wage floor had a negligible impact on wage growth in West Germany since wages were relatively high to begin with and the percentage of directly affected workers was therefore very small. In East Germany, however, where wages in the construction sector were considerably lower, the new minimum wage led to a significant increase in the wage growth rate. In this case, an increase by one standard deviation of the share of affected workers is associated with an increase in the growth rate of average wages by roughly 0.8 percentage points. Second, while we do not find any employment effect in West Germany, the effect on East German employment is consistently negative, but statistically insignificant at conventional levels in our preferred specifications. However, these estimates likely represent an upper bound of the true treatment effect and we are able to show that estimates representing a lower bound are clearly negative and of high economic and statistical significance.<sup>1</sup> We conclude that an increase by one standard deviation of the share of affected workers is associated with a reduction in the employment growth rate by a little more than one percentage point in East Germany.

<sup>1</sup> We estimate an upper bound, because we net out the effect of the economic cycle by measuring employment growth in main construction relative to employment growth in finishing trades (given in Sect. 3). Insofar as finishing trades as a neighbouring sector was also affected by the minimum-wage introduction in main construction, the estimated employment effects will be biased towards zero and the true effect is likely to be more negative. For a complete discussion refer to Sect. 5.2.

Third, we provide evidence that spatial spillover effects are not driving factors in this instance.

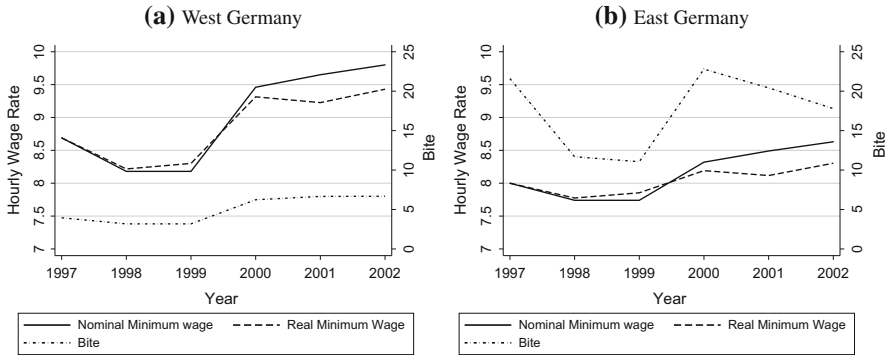
## 2 Institutional setting

Prior to the introduction of the statutory minimum wage in 2015, minimum wages in Germany were special in the sense that they were exclusively set via collective bargaining between employees' unions and employers' associations at the industry level. Today, these sectoral minimum wages still exist and complement the federal minimum wage. They arise when collective bargaining agreements (CBAs) are declared to be universally binding by the Federal Ministry of Labour and Social Affairs (BMAS). Once that occurs, the wage floor applies to all workers in that particular industry, irrespective of whether they belong to the bargaining workers' union.

One of the reasons to have a minimum wage established through a CBA is that, in combination with the Posting of Workers Law (*Arbeitnehmer-Entsendegesetz*), it also applies to workers sent by firms from abroad. Therefore, in contrast to the motivation for minimum wages in other countries, where wage regulation is typically considered an anti-poverty measure (Sabia and Burkhauser 2010), the sectoral wage floors in Germany—at least for the construction sector—are anchored squarely on protectionist and anti-competitive reasons. In a sense, therefore, the industry under investigation is not typical of other low-wage industries where minimum wages exist. It certainly is structurally different from the subjects of previous studies in the USA, such as fast-food workers or teenage employees.

Given that employers and workers have to jointly initiate a minimum wage introduction, self-selection into the minimum-wage regime might constitute a type of policy endogeneity that is problematic for any evaluation study. Specifically, we do not expect to find any effects on wages and employment if basically all firms in the affected industry already pay the collectively bargained wage rate that becomes the minimum wage. Indeed, prior to the introduction of the minimum wage in 1997, the coverage of sectoral (but not universally binding) CBAs in German construction was already generally high in West, but not in East Germany. Based on 1995 firm-level data, sectoral CBAs in West Germany covered 81% of establishments (Kohaut and Bellmann 1997). In the East, the coverage rate was around 40% (IAB, RWI and ISG 2011). Thus, policy endogeneity might constitute a problem in West Germany, but less so in the East. This is especially true since the minimum wage was only in the employer's interest as so far as the firm already paid wages in excess of the minimum-wage rate. Firms with a lower wage level, which are usually smaller and not members in the industry's employer association, had much less reason to support the minimum wage. Thus, for the affected firms in East Germany, the minimum-wage introduction was just as exogenous as a statutory minimum-wage set by the state.

Despite the involvement of both labour unions and employer associations, the introduction of the minimum wage appeared doubtful in 1996. In Germany, the social partners are predominantly organized at the industry level. In addition, the sectoral trade unions and employer associations are members of umbrella organizations that represent the workers' and employers' interest, respectively, across all sectors and



**Fig. 2** Real and nominal minimum wages, 1997–2002. *Note:* The nominal minimum wage has been deflated with the producer price index obtained from the Federal Statistical Office. The figure shows the minimum-wage rates in place in January of the year in question. Note that increases usually take effect in September, while employment and average wages are measured on June 30 each year. The figure shows the share of workers affected by the increase taking effect in September of the year in question, measured in June of the previous year. *Source:* Nominal minimum wage—Own data collection. Bite—Authors' calculations based on the IEB

regions. While the sectoral employer association of the main construction sector was highly interested in a minimum wage in order to reduce competition from abroad, the employers' umbrella organization (*Bundesvereinigung der deutschen Arbeitgeber*, BDA) naturally opposed any minimum-wage introduction in Germany. Since the Posting of Workers Law required the agreement of the umbrella organization in addition to that of the sectoral employer association (a requirement that was dropped in 1999), the BDA was able to inhibit the minimum-wage introduction in the construction sector. Only when the employer association in the main construction sector threatened to leave the umbrella organization, did the BDA finally agree. Thus, it only became certain in September 1996 that a minimum wage would be introduced as of January 1997 (Hunger 2003; Eichhorst 2005). Due to the timing of these events, anticipation effects that might bias results, such as reduced employment levels prior to the introduction, are extremely unlikely.<sup>2</sup>

The evolution over time of the minimum wages established in the main construction sector since its introduction is presented in Fig. 2 separately for East and West Germany. The differential minimum wages between East and West Germany reflect the fact that wages are, on average, lower in the East. In general, one can observe that the nominal minimum wage has been increasing over time except for a dip in 1998–1999. In real terms, the minimum wage exhibits an increase of roughly 5% and 10% for East and West Germany, respectively, for the period between 1997 and 2000. The minimum wage's bite, i.e. the share of affected workers in main construction measured 1 year prior to the introduction or increase, shows that in East Germany almost one quarter of all construction workers earned less than the minimum wage before its introduction.

<sup>2</sup> A formal test to rule out such anticipation effects would be desirable. Unfortunately, we only observe 4 years in the pre-treatment period (1993–1996) which is not sufficient to run placebo regressions. We cannot extend our observation period further back since employment data for East Germany are missing before and during the time of unification.

A second peak in the bite can be observed with the minimum wage's increase in 2000. In West Germany, the bite equally follows the development of the nominal minimum wage, but at a much lower level as average wages are considerably higher (Fig. 2). Therefore, if there is any effect on wage and employment growth rates, one can expect it to materialize in the years immediately after its introduction.

Previous work has shown that the minimum wage increased average wages and wage growth in East Germany, while hardly any effects can be established for the West German wage distribution. There is more contention about the estimates on employment in West Germany, where most studies find no effects, while König and Möller (2009) provide some evidence that the employment growth rate actually increased after the introduction of the minimum wage. The results for East Germany are also not consistent: while Apel et al. (2012) and Frings (2013) found neutral employment effects despite the positive effects on wages, König and Möller (2009) and Müller (2010) conclude that the minimum wage had a negative impact on employment.

### 3 Data construction and description

This study is based on administrative data for construction workers in Germany. The data were drawn from the Integrated Employment Biographies (IEB, *Integrierte Erwerbsbiographien*) at the Research Data Center based at the Institute for Employment Research of the Federal Employment Agency. The dataset covers workers who were employed in the main construction sector or in finishing trades at any point in time during the period 1993–2002 and who are subject to social security contributions. The analysis is limited to full-time employed men for two reasons: (1) part-time employment is rare among blue-collar workers in the main construction sector who are eligible for the minimum wage and (2) the share of women among blue-collar workers in this sector is extremely low. We are unable to consider the case of posted or foreign workers in Germany.

The data contain sociodemographic as well as employment characteristics, including the average daily wage. Average daily wages are right-censored at the social security contribution limit, i.e. the wage at which social security contributions no longer increase. Because the majority of construction workers earn wages below this limit, any possible downward bias of average wages should be negligible, and is therefore not taken into account.

Unfortunately, no detailed information on hours worked is available, which is necessary to calculate hourly wage rates. IAB, RWI and ISG (2011) impute the number of hours usually worked for full-time workers in main construction based on available information from the census (*Mikrozensus*) for the years 1993–2002. We adopt their results for our calculation of hourly wages. This involves estimating a linear model for the usual working hours as a function of various individual, firm, and job characteristics, as well as indicators for the federal state, available in the *Mikrozensus*. The estimated parameters are then used to calculate the cross-sample predicted values based on data available from the IEB. Ultimately, full-time employed workers

appear, on average, to work roughly 40h per week irrespective of their individual or job characteristics.<sup>3</sup>

One advantage of using spatial variation for the identification of the minimum-wage effect is that any error in measurement of the hourly wage rates should not bias the results as long as the error is random across individuals within regions. Stated differently, even if wage rates are incorrect at the individual level, these measurement errors should cancel out at the aggregate district level. In contrast, such an error is more critical when trying to identify individuals who are (not) affected by the minimum wage, which is the strategy employed in previous studies in this context. Moreover, if one supposes that worker substitution takes place within the construction industry, the level of aggregation at the district level implies that we are able to abstract from this issue. Even though the individual-level error may persist, the higher-level aggregation of our data allows us to circumvent potential problems that are present in earlier evaluations of the minimum wage in Germany.

The IEB are spell data with specific days for the beginning and end of each spell. We transform the data into annual observations using June 30 as the cut-off date each year, which is the administrative sampling period for this dataset. That is, each male blue-collar worker employed in the main construction industry on that day remains in our operational dataset. One advantage of the annual data is that seasonal effects (e.g. the decline in employment in winter) become immaterial for the analysis of the employment effect of the minimum wage.

We use detailed industry classifications to define the main construction<sup>4</sup> as well as finishing trades.<sup>5</sup> The observation period of our operational dataset ends in 2002, which is not due to data limitations per se, but the fact that a second, higher minimum-wage rate was introduced for skilled workers in 2003. Unfortunately, the data do not allow us to unambiguously identify which minimum-wage rate is applicable to which worker. In order to avoid measurement error, which cannot be eliminated via aggregation, this study concentrates on the time period from the introduction of the minimum wage in 1997 up to 2002. Similarly, we cannot extend our observation period prior to 1993 since employment data for East Germany are missing before and during the time of unification.

The data are regionally disaggregated down to the level of districts (*Kreise und kreisfreie Städte*, NUTS 3), which allows to transform the dataset from the individual to the district level. The mean wage of all construction workers eligible for the minimum wage in each district is calculated, while employment corresponds to a head count of full-time male workers. The minimum-wage treatment is measured by the bite, which

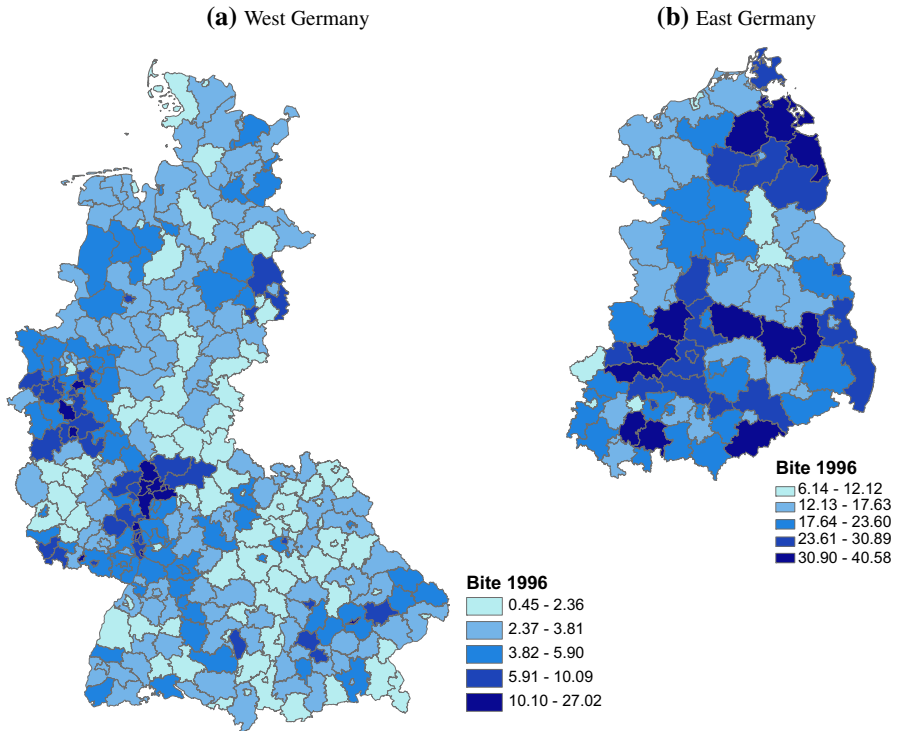
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<sup>3</sup> We provide evidence of this in the online material.

<sup>4</sup> We follow IAB, RWI and ISG (2011) in the choice of the relevant sub-sectors. These are based on the classification scheme of 1973 and include the following economic groups (prefixed by their numeric codes): [590] general civil engineering activities, [591] building construction and civil engineering, [592] civil and underground construction, [593] construction of chimneys and furnaces, [594] plasterers and foundry dressing shops, and [600] carpentry and timber construction.

<sup>5</sup> The relevant sub-sectors in the classification scheme of 1973 are: [610] plumbing and piping, [612] glazing, [613] paint shops and wall tilers, [614] floor tilers and pavements, [615] stove and furnace fitting, and [616] scaffolding, facade cleaning.





**Fig. 3** Spatial distribution of the minimum-wage bite in 1996. *Note:* The bite is defined as the share of workers earning below the minimum wage in the period prior to its introduction or increase. *Source:* Authors' calculations based on the IEB

is defined as the share of workers in main construction earning below the minimum wage within each district in the period prior to its introduction or increase.

The choice of the district level as the unit of observation is motivated by two reasons. First, Thompson (2009) points out that the minimum-wage bite may differ heavily between regions. If regions used in an analysis are too large, one will estimate the average effect of an average minimum-wage bite, which is not necessarily informative. Indeed, the minimum wage does show considerably more variation at the district level compared to, for instance, broader labour-market regions. A second advantage of using district-level data compared to more aggregated spatial units is the identification of spatial heterogeneity in terms of average wage and employment growth rates. The mean wage growth rate over all regions and time periods amounts to 1.1% with a standard deviation of 1.8; the average employment growth rate is  $-6.0\%$  with a standard deviation of 8.7. For wage and employment growth rates alike, most of the variation is found over time and not between regions. Nevertheless, possibly deviating reactions of individual districts to the minimum wage can only be measured if the analysis is carried out at this regional level.

Figure 3 shows the spatial distribution of the bite in 1996 prior to the minimum-wage introduction for West and East Germany, respectively. The majority of neighbouring districts is clearly characterized by different treatment intensities. In West Germany,

the bite varies between less than 1% and 27%, while at least 6% and at most 41% of all construction workers in each district are affected in East Germany. However, the bite of the minimum wage is very low for the majority of regions, while a few regions are affected heavily.

Even though the treatment intensity is much higher in East compared to West Germany, this variation in the bite is not exploited for the identification of the minimum-wage effect. Instead, different treatment intensities within East and West Germany are used, especially the variation between neighbouring districts (cf. Sect. 4). Two distinct reasons exist for estimating separate treatment effects for East and West Germany. First, the two labour markets still function quite differently, especially in terms of structural differences in employment growth. Since the treatment intensity is systematically higher in East Germany, the identification would be strongly driven by differences between East and West German regions. As employment growth differs for many reasons besides the minimum wage, simultaneous estimation might bias the results. Second, the relationship between the minimum wage bite and employment growth is not necessarily linear for all possible treatment intensities. Existing theory and previous empirical research suggests that moderate minimum wages are not necessarily harmful to employment. As treatment intensity is moderate in the West and very high in the East, it does not seem appropriate to impose a linear model to both parts of Germany.

Up to this point the identifying assumption is that employment in main construction would have developed similarly across all districts in the absence of the minimum wage introduction. This is a strong assumption. Specifically, three sources of spatial heterogeneity exist that we explicitly address to avoid biased estimation. First, high-bite (low-wage) and low-bite (high-wage) regions might follow structurally diverging wage and employment growth paths that are independent of the minimum wage. Second, economic shocks might either be local in nature or affect districts differently. And third, the recession in the construction industry that began in the late 1990s (Fig. 1) might have affected high-bite and low-bite regions differently.

Two strategies are applied to allow for structural differences between districts in terms of wage and employment growth. First, linear trends are added to the model—either for district types or for broader labour-market regions. Second, each district is allowed to follow a different wage and employment growth path dependent on the wage level prior to the minimum wage introduction. This is implemented by adding a hypothetical bite to the model (Sect. 4). In a nutshell, we allow for diverging linear trends for each district-type, for each labour-market region, and depending on the pre-treatment wage level.

Local shocks should affect all industries in a district in a similar way. To control for the occurrence of such shocks, the average wage and employment growth rates of all industries *except* construction in each specific district are added to the model. These indicators are based on the weakly anonymous Sample of Integrated Labour-Market Biographies (Years 1975–2008), which is based on a 2% random sample drawn from the IEB (Dorner et al. 2010).

Finally, the recession in the construction industry that coincides with the minimum wage introduction and the preceding boom could have affected low-bite and high-bite regions differently. This would imply a violation of the common trend assumption,

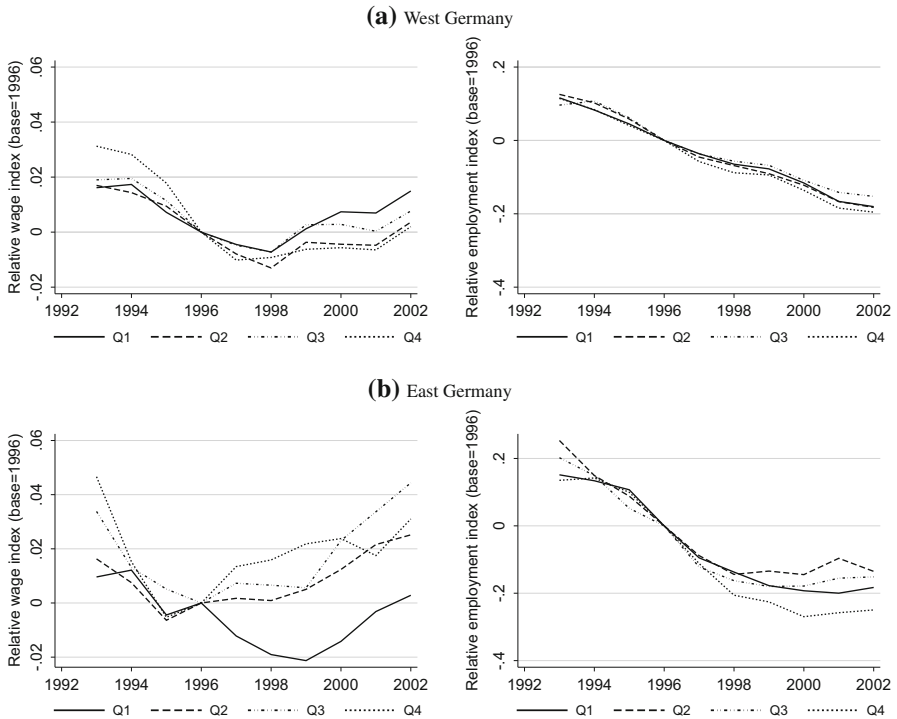
which is not solved by either adding linear trends (as the economic cycle does not follow a linear trend) or by controlling for general, local shock (as the strong boom and the deep recession was specific to the construction sector; given in Sect. 1). Fortunately, finishing trades is a sub-sector of the construction industry that experienced a comparable economic cycle, but was not subject to the minimum wage introduction. We therefore transform our dependent variables, average wages and total employment in main construction, in such a way that the economic cycle which turns with the minimum wage introduction is controlled for. Specifically, we re-define our outcome variables—average wage growth and total employment growth—as the ratio of growth rates in main construction and finishing trades. Thus, we generate the average wage and the total employment growth rate in the main construction industry *relative* to finishing trades at the district level.

Figure 4 provides first tentative evidence of our results in graphical form by showing the development of the transformed dependent variables by treatment intensity, i.e. the districts are divided into four groups according to the quartiles of the bite measured in 1996, separately for East and West Germany. The recession in the construction industry is still visible—wages and employment both decrease until the late 1990s, in East and West Germany alike. However, in comparison with Fig. 1 the magnitude is much smaller: While Fig. 1 shows that employment in main construction contracted by more than 55% in East Germany between 1995 and 2002, the employment losses were only about 20 percentage points higher in main construction relative to finishing trades (Fig. 4).

Even more importantly, districts with different treatment intensities do follow a similar trend up to 1996. Even though the pre-treatment trends are not perfectly parallel, there is no systematic correlation with the treatment itself. For example, employment in main construction relative to finishing trades decreases more quickly in districts with a medium bite in East Germany from 1993 until 1995, but the employment development is very similar across high-bite and low-bite districts up to 1997. The employment growth paths of these East German districts actually only diverge after the minimum wage introduction in 1997, with high-bite regions showing somewhat higher employment losses compared to low-bite regions. This potential treatment effect is much more visible for wages in East Germany: all district with above median minimum wage bites shows a higher wage growth from 1997 onwards compared to districts with below-median bites. In contrast, no potential treatment effects can be visually detected for West Germany.

## 4 Estimation strategy

In the following, we describe the statistical framework to examine the effects of the minimum-wage bite on regional wage and employment patterns. We begin with a benchmark model that mimics the standard approach to analyse minimum-wage effects in a panel of regional data. We then extend the model in various ways to more comprehensively capture spatial dependencies and heterogeneities.



**Fig. 4** Average wages and employment in main construction relative to finishing trades by treatment intensity. *Note:* The employment and wage level in both industries are indexed with 1996 as the base year. Based on these indices, the relative growth rate is calculated as  $[\ln(\text{main construction}) - \ln(\text{finishing trades})]_{t+1} - [\ln(\text{main construction}) - \ln(\text{finishing trades})]_t$ . The minimum wage introduction took place in 1997. The districts are divided into four groups according to the quartiles of the distribution of the bite measured in 1996, separately for East and West Germany. Q1 represents the districts with the lowest bite; Q4 represents the districts with the highest bite. *Source:* Authors' calculations based on the IEB

#### 4.1 Basic model

We are interested in estimating wage and employment effects of the minimum-wage introduction and subsequent increases in the German construction sector using regional panel data. Since there is no variation in nominal minimum wages (except for the difference between East and West German districts), we combine the panel approach in Neumark and Wascher (1992) with the idea of using the level of the minimum-wage bite as in Card (1992). Recent studies on the statutory minimum wage in Germany have also adopted a similar identification approach drawing from spatial variation in the bite (e.g. Caliendo et al. 2018). Following Dolton et al. (2010), we separate the post-treatment effect from the more general correlation between the dependent variables and the bite by introducing a hypothetical bite before the minimum-wage introduction. This captures any potentially differential trends across regions before the minimum wage applied. Recall that the bite is defined as the share of workers in main construction earning below the minimum wage within each district in the period

prior to its introduction or increase.<sup>6</sup> The hypothetical bite is calculated accordingly, assuming that the 1997 minimum wage (adjusted for previous wage trends) already applied.

Our initial specification is

$$\Delta \ln y_{it+1} = \mathbf{b}_{it}\boldsymbol{\alpha} + (d \times \mathbf{b}_{it})\boldsymbol{\beta} + \Delta \ln \mathbf{x}_{it}\boldsymbol{\gamma} + \mu_i + \boldsymbol{\tau}_t + \epsilon_{it}, \quad (1)$$

where  $\Delta \ln y_{it+1}$  constitutes wage or employment growth in district  $i$  between time  $t$  and  $t + 1$ ,  $\mathbf{b}_{it}$  is the minimum-wage bite for district  $i$  in year  $t$ , and  $d$  is an indicator for the post-treatment period. Thus,  $\boldsymbol{\beta}$  captures the treatment effect of the minimum wage. Note that the dependent variable  $\Delta \ln y_{it+1}$  is always expressed relatively to finishing trades, while the main explanatory variable  $\mathbf{b}_{it}$  refers only to the main construction industry. The vector  $\Delta \ln \mathbf{x}_{it}$  represents mean wage and employment growth in all local industries except main construction and finishing trades as additional controls to proxy for differences in local demand shocks. The terms  $\mu_i$  and  $\boldsymbol{\tau}_t$  represent district and time-period fixed effects. We do not need to include the post-treatment indicator  $d$  as a separate control as long as we include full time-period indicators.

Observe that  $\boldsymbol{\alpha}$ ,  $\boldsymbol{\beta}$ , and  $\boldsymbol{\tau}_t$  are vectors containing two elements since we estimate separate effects for East and West German districts to allow for additional flexibility regarding treatment effects.<sup>7</sup> This amounts to estimating a model that is fully interacted with a dummy for East Germany. Allowing for differential coefficient estimates for East and West Germany captures the structural differences between these two regions. Reexamining Fig. 3 shows that there is very little overlap between the treatment intensities in the two regions.<sup>8</sup>

Equation (1) incorporates three ways to deal with spatial heterogeneity. First, the dependent variables refer to wage and employment growth in main construction relative to finishing trades in order to ensure that the strong economic cycle specific to the construction industry does not affect low-bite and high-bite regions differently. Second, local demand shocks are controlled for by adding wage and employment growth in all industries, except for main construction and finishing trades. And third,  $\boldsymbol{\alpha}$  captures the correlation between the hypothetical minimum-wage bite and the wage or employment growth rate before the actual minimum-wage introduction. If it were statistically significant, it could indicate that there are some structural differences between regions in the pre-treatment period that cannot be adequately captured by the other control variables and that are correlated with the minimum-wage intensity. The identifying assumption for  $\boldsymbol{\beta}$  to properly measure the treatment effect is accordingly that the correlation between the bite and the dependent variables would have stayed constant in the absence of the minimum-wage introduction.

<sup>6</sup> The results of all models are robust to using the Kaitz Index instead of the bite. The corresponding tables can be obtained from the authors upon request.

<sup>7</sup> We do not present the results from separate regressions for East and West Germany. This is to ensure comparability with the later neighbourhood-effects model, where splitting the sample would mean a loss of neighbourhood information at the inner German border. In any case, estimating Eq. (1) separately for East and West does not change the results qualitatively.

<sup>8</sup> Note the range of values indicated in the figures' legends.

As an alternative to Eq. (1), we also estimate a model that additionally allows for region-type-specific or labour-market-region-specific time trends:

$$\Delta \ln y_{it+1} = \mathbf{b}_{it}\boldsymbol{\alpha} + (d \times \mathbf{b}_{it})\boldsymbol{\beta} + \Delta \ln \mathbf{x}_{it}\boldsymbol{\gamma} + \mu_i + \boldsymbol{\tau}_t + \lambda_r I_{rt} + \epsilon_{it}, \quad (2)$$

where  $I_r$  is an indicator for region type  $r$  or alternatively for labour-market region (LMR)  $r$ .

The district types are based on the classification of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) that assigns each district to one of nine different types (*siedlungsstrukturelle Kreistypen*), from low-density rural areas to high-density core cities (BBSR 2012). Equation (2) therefore allows for different patterns in wage and employment growth rates depending on regional characteristics that might be linked to agglomeration or urbanization processes. Furthermore, population density is a crucial factor in determining the spatial wage structure in Germany (Büttner and Ebertz 2009), which indicates that wage and employment growth rates may also be closely linked to this characteristic. The labour-market regions are also obtained from BBSR (*Raumordnungsregionen*). Including trends at this level is important because districts are administrative regions that are interconnected in terms of the product as well as the labour market, while labour-market regions are functional units.

In all specifications, we use growth rates as dependent variables for two reasons. First, using levels will lead to unintended correlations between the dependent variable and the bite after the fixed-effects transformation. For example, if employers actually commit to the new minimum wage, wages should stay up while the bite drops in the periods after the introduction. The sign of the correlation might therefore change over time and complicate the identification of a minimum-wage effect. This problem is circumvented in a specification using growth rates. In a recent paper, Meer and West (2016) argue for the use of growth rates as well because the effect of minimum wages on employment is more apparent in its dynamics rather than in the levels (for a variety of reasons, such as inflation erosion of the minimum wage and attenuation of the effect on levels because of the inclusion of time trends, among others). The use of the growth rate as an outcome variable does not imply that the minimum wage introduction changes the growth rate of employment permanently. In terms of Eq. (1) the growth rate returns to its initial magnitude as soon as the bite drops to zero, which occurs in the period after the introduction/increase in case of full compliance with the minimum wage. In terms of interpretation, changes in the growth rate simply map the path to the employment level in the minimum-wage induced equilibrium.

Note that there is one potential caveat when estimating Eqs. (1) and (2), especially with wage growth as the dependent variable. Regional wages play a role in determining both the degree of the minimum wage treatment intensity and the subsequent growth rate of wages, thus violating the assumption of strict exogeneity of the bite.<sup>9</sup> Additionally, measurement error or reversion to the mean will bias the estimate of  $\boldsymbol{\alpha}$  upwards in a mechanical sense (Dolton et al. 2012). However, making the identifying assumption that this phenomenon does not change over time, one can still interpret  $\boldsymbol{\beta}$

<sup>9</sup> We use a weaker assumption than strict exogeneity in Sect. 4.3.

as the unbiased treatment effect of the minimum wage on regional wage growth. We will make that assumption in what follows.

### 4.2 Neighbours

One might criticize the above models on the ground that they do not adequately control for spatial spillover effects. Local characteristics might not only have effects in the home district but also in neighbouring ones. Ignoring those effects can lead to an omitted-variable bias if local characteristics are spatially correlated. Similarly, the effect of a high bite in a particular region might not be confined to that region. For example, while the direct employment effect to that region might be negative, the indirect effect to neighbouring regions might be positive if labour demand rises in those regions as a result. This could happen if firms are forced out of business and construction orders are taken by firms from neighbouring districts. In contrast, if the minimum wage narrows the wage differential between districts (especially for low-skilled workers), this decreases the incentive to commute long distances to more attractive jobs. Thus, there might be a negative effect on labour supply in the neighbourhood of a high-bite district as workers decide to search for jobs closer to their homes (and possibly displacing lower-skilled workers there).

To allow for these kinds of neighbourhood effects—in terms of both general and minimum-wage-induced spillovers—to affect regional wage and employment growth rates, we augment the basic model as follows:

$$\begin{aligned} \Delta \ln y_{it+1} = & \mathbf{b}_{it} \boldsymbol{\alpha}^D + (d \times \mathbf{b}_{it}) \boldsymbol{\beta}^D + \Delta \ln \mathbf{x}_{it} \boldsymbol{\gamma}^D \\ & + \bar{\mathbf{b}}_{it}^N \boldsymbol{\alpha}^I + (d \times \bar{\mathbf{b}}_{it}^N) \boldsymbol{\beta}^I + \Delta \ln \bar{\mathbf{x}}_{it}^N \boldsymbol{\gamma}^I \\ & + \mu_i + \tau_t + \lambda_r I_{rt} + \epsilon_{it}. \end{aligned} \tag{3}$$

Here,  $\boldsymbol{\alpha}^D$ ,  $\boldsymbol{\beta}^D$  and  $\boldsymbol{\gamma}^D$  capture the direct effects, while  $\boldsymbol{\alpha}^I$ ,  $\boldsymbol{\beta}^I$  and  $\boldsymbol{\gamma}^I$  capture the indirect effects from neighbouring districts. The variables relevant for the indirect effects are marked with a bar on top and are calculated as the average over all neighbours. We specify “neighbourhood” in two distinct ways. In the first variant, neighbours are other districts within a larger functional unit (*Raumordnungsregion*) that has been defined according to commuting flows and other characteristics (cf. Sect. 3). This addresses the concern about the mobility of labour. Second, we use a contiguity matrix to indicate districts with common borders.

Note that the model in Eq. (3) implies that spatial spillover effects are local in nature. Thus, while it allows one district to affect its direct neighbour, we rule out that this has higher-order effects on the neighbours’ neighbours, the neighbours of those neighbours, and so on. While this assumption restricts the way spatial effects might take hold, we believe it is a sensible choice. Demand for construction work is relatively localized since buildings cannot be shipped like other goods. Factors of production have to be transported to the production site. While there are some big players that bid for contracts nationwide, most workers are employed in small- or medium-sized firms that operate regionally. Even large building companies often maintain local

establishments to better serve local markets. Thus, we do not expect local shocks to have ripple effects that propagate to distant districts.<sup>10</sup>

### 4.3 Border pairs

An approach that concentrates on problems stemming from spatial heterogeneity instead of spatial spillovers was proposed by Dube et al. (2010). It generalizes the method used by Card and Krueger (1994) to identify minimum-wage effects at state borders. The authors note that conventional panel models assume that each region can be readily compared to all the other regions irrespective of distance. This assumption is problematic if markets are localized and economic conditions in one part of the country are quite different from the ones in another part. For example, a local demand shock might hit adjacent regions similarly while the rest of the country remains unaffected. In this case, it may be a superior strategy to compare regions only to their direct neighbours and assume that those form a better comparison group. We thus redo our analysis, applying the “border-pair approach” to test whether our results are robust when using contiguous district pairs as units of comparison.

Implementing this estimation strategy requires us to change the structure of our dataset. Instead of the usual panel, the new data consist of the universe of all district pairs in Germany that have a common border segment. This means that each district can enter the dataset several times depending on the number of direct neighbours it has. In our case, this increases the number of observations more than fivefold (from 3708 to 19,080).

Although the structure is similar to the strategy employed by Dube et al. (2010), there is an important difference. In the German case, there is no policy discontinuity *per se* located at the border. The discontinuity arises because of the variation in treatment intensity between two regions which is not the result of any difference in statutory minimum wages (except between East and West Germany). Thus, the identification of the effect comes from the variation in the minimum-wage bite between bordering pairs over time.

Minimum-wage effects are then estimated using the model

$$\Delta \ln y_{ipt+1} = \mathbf{b}_{it}\boldsymbol{\alpha} + (d \times \mathbf{b}_{it})\boldsymbol{\beta} + \Delta \ln \mathbf{x}_{it}\boldsymbol{\gamma} + \mu_i + \tau_{pt} + \epsilon_{ipt}, \quad (4)$$

where the subscript  $p$  identifies a single pair of neighbouring districts. The term  $\tau_{pt}$  is a specific pair–period effect and treated as a nuisance parameter. Effectively, the approach treats each district pair as a natural experiment where the difference in the continuous bite variable proxies treatment intensity. It then pools all individual estimates to get an average relation between the minimum-wage bite and wage or employment growth rates.

One additional strength of the model expressed in Eq. (4) is that it depends on an orthogonality assumption that is considerably weaker than the strict-exogeneity

<sup>10</sup> See LeSage and Pace (2009) for a discussion of local vs. global spillovers. As noted by Vega and Elhorst (2013, p. 11), global spillovers generally should be theoretically motivated, but they are “difficult to justify and have been overused in applied studies”.



assumption used for fixed-effects panel estimation (Dube et al. 2010). We now only need to assume that the difference in local bites is contemporaneously uncorrelated with the difference in local residuals in wages (or employment). This is relevant since strict exogeneity is questionable, especially for the wage regressions where regional wages not only enter the dependent variable but also influence the minimum-wage bite on the right-hand side of the equation. The border-pair approach thus allows us to get an idea of whether the inherent simultaneity in our wage equations contaminates the fixed-effects results.

One drawback of this approach is that, unlike the model outlined in Eq. (3), we again do not allow for effects from neighbouring districts to affect the results. If there are strong external effects that run from one district to another, then least-squares estimation of the parameters in Eq. (4) will be biased. However, in combining the strengths and weaknesses of the different approaches, we draw a consistent picture of the effect of introducing a minimum wage in the German construction sector.

## 5 Results

### 5.1 Wage effects

The estimates of the minimum-wage effect on wage growth rates in East and West Germany resulting from the basic specification are presented in the first three columns of Table 1. Column (1) only includes district fixed effects, while Columns (2) and (3) additionally include linear trends for district types and labour-market regions, respectively. Using the notation in Eq. (1), the first two rows represent the coefficient vector  $\alpha$  and the following two rows, the coefficient vector  $\beta$ . The estimated pre-treatment correlation between the bite and wage growth is positive for both East and West Germany. Recall that the bite is defined as the share of workers in main construction earning below the minimum wage within each district in the period prior to its introduction or increase. The same is true for the hypothetical bite, assuming that the 1997 minimum wage (adjusted for previous wage trends) already applied. Therefore, low-wage regions appear generally to be characterized by structurally higher wage growth rates than high-wage regions.

We estimate a significantly positive treatment effect in East Germany, almost exactly equal to 0.1 across all specifications. The treatment effect for West Germany has a small and negative, but always statistically insignificant, point estimate in all specifications. Note that a negative treatment effect for West Germany—while counter-intuitive—is theoretically possible in districts with a high fraction of workers earning just above the minimum wage. Setting a minimum wage can then serve as an anchor for employers, who might perceive that supra-minimum wages overcompensate their workers. In this case, employers may either downgrade these wages or offer exactly the minimum wage to new employees. The other possible scenario of adjustments occurring on the intensive margin does not seem likely, as we provide evidence in the online material that the usual hours of work remained relatively stable for both East and West Germany over the sample period.

**Table 1** The minimum wage bite's effect on mean wage growth. *Source:* Authors' calculations based on the IEB

	(1)	(2)	(3)	(4)	(5)
Hypothetical bite (West)	0.312*** (0.057)	0.309*** (0.058)	0.352*** (0.060)	0.365*** (0.063)	0.351*** (0.060)
Hypothetical bite (East)	0.123*** (0.028)	0.127*** (0.029)	0.144*** (0.034)	0.137*** (0.033)	0.139*** (0.034)
Treatment effect (West)	-0.050 (0.036)	-0.036 (0.036)	-0.039 (0.037)	-0.042 (0.042)	-0.039 (0.043)
Treatment effect (East)	0.103*** (0.020)	0.103*** (0.019)	0.095*** (0.026)	0.098*** (0.025)	0.105*** (0.029)
Wage growth (other industries)	-0.003 (0.022)	-0.002 (0.022)	0.001 (0.023)	0.001 (0.023)	0.001 (0.023)
Employment growth (other industries)	-0.004 (0.008)	-0.004 (0.008)	-0.004 (0.008)	-0.004 (0.008)	-0.004 (0.008)
Hypothetical bite (West), neighbours				0.036 (0.103)	-0.040 (0.073)
Hypothetical bite (East), neighbours				-0.073 (0.045)	-0.024 (0.059)
Treatment effect (West), neighbours				0.097 (0.087)	0.016 (0.063)
Treatment effect (East), neighbours				0.078 (0.053)	-0.052 (0.061)
Wage growth (other industries), neighbours				0.045 (0.035)	0.043 (0.051)
Employment growth (other industries), neighbours				0.019* (0.011)	0.014 (0.016)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
District-type-specific trends	No	Yes	No	No	No
Labour-market-region-specific trends	No	No	Yes	Yes	Yes
$R^2$	0.138	0.193	0.209	0.211	0.211
Observations	3708	3708	3708	3681	3708

Column (4) defines neighbours as being in the same labour-market region and Column (5) defines neighbours as sharing a common border (cf. Sect. 4). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors are enclosed in parentheses and clustered at the labour-market-region level

To investigate the effect of spillovers from neighbouring districts, the last two columns in Table 1 report estimates for the models described by Eq. (3). Column (4) defines close neighbours as those districts that lie within one labour-market region, while Column (5) computes neighbourhood averages over all districts that have a contiguous border with the observational unit. The indirect effects are generally sta-

**Table 2** Border approach models for wages. *Source:* Authors' calculations based on the IEB

	(1)	(2)
Hypothetical bite (West)	0.097 (0.063)	0.332*** (0.072)
Hypothetical bite (East)	0.046** (0.023)	0.141*** (0.046)
Treatment effect (West)	−0.033 (0.041)	−0.049 (0.040)
Treatment effect (East)	0.017 (0.025)	0.086*** (0.028)
Wage growth (other industries)	−0.026 (0.036)	−0.024 (0.037)
Employment growth (other industries)	−0.006 (0.011)	−0.009 (0.011)
Pair-period fixed effects	Yes	Yes
District fixed effects	No	Yes
$R^2$	0.551	0.592
Observations	19080	19080

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are enclosed in parentheses and two-way clustered at the level of the labour-market region for both districts of a pair. We use the user-written routine `reghdfe` for Stata by Correia (2017)

tistically insignificant, although the point estimates are partly sizable in terms of magnitude. It is unclear whether the true effects are noisy or whether these effects are simply imprecisely estimated. In either case, the treatment effects themselves prove to be very robust to the inclusion of local spillover effects. This holds irrespective of what spatial structure is assumed. Allowing for indirect effects from neighbouring districts does not alter our previous conclusion.

Table 2 depicts the main coefficients using the border-pair approach and the transformed data set. That is, all estimations are based on Eq. 4. As an intermediate step, Column (1) only absorbs pair-period fixed effects from the data. In contrast with the previous results, we do not find any significant treatment effect for West and East Germany. Column (2) recognizes the fact that while adding pair-period effects controls for spatial heterogeneity at a very low level, there might still be heterogeneity that is unique to a single district. Additionally absorbing those district fixed effects does not change the treatment effect for West Germany but considerably increases its economic and statistical significance in East Germany. While still somewhat lower than in Table 1, it now lies in close proximity to the earlier results.

The consistently estimated treatment effect of 0.1 implies an increase in wage growth of 0.8 percentage points if the bite is increased from 22 to 30%.<sup>11</sup> The effect of the introduction and subsequent increases in the minimum wage in West Germany

<sup>11</sup> Using the distribution of regional bites in East Germany in 1996, this represents an increase in the bite by approximately one standard deviation.

is statistically indistinguishable from zero in most cases while there is a pronounced positive effect in East Germany.<sup>12</sup>

This is congruent with the descriptive statistics for East and West German districts before the minimum-wage introduction (cf. Fig. 3). In West Germany, the bite is quite low on average throughout the observational period. There were probably very few firms in each district that had to adjust wages for a significant fraction of their workforce. If there were only a few workers who experienced wage increases due to the new wage floor, those changes will not be visible in district-level aggregated data. Conversely, there are strong differences in East Germany, where the minimum wage does pose a significant hurdle. Here, a relatively large fraction of all construction workers received wage increases, which led to a statistically and economically significant effect on regional wage growth.

With significant wage effects being confined to East Germany, we expect employment effects—if there are any—to be found only there, too. This is consistent with the evidence uncovered by, for example, Dolton et al. (2010) for the UK. Although their outcome variable is wage inequality, they do note that where the national minimum wage in the UK has a strong bite, wage inequality declines, indicating a compression of the wage distribution. Similar results are provided in a number of other studies for the UK, such as Dickens and Manning (2004) and Dickens et al. (1999). Additionally, Machin and Manning (1997) find some evidence that the minimum wages also reduced wage inequality in their study of four countries (France, the Netherlands, Spain, and, again, the UK).

## 5.2 Employment effects

Table 3 mirrors the analyses displayed in Table 1 but now uses district-wise employment growth rate in main construction relative to finishing trades as the dependent variable. In this case, we do not find a significant correlation between the minimum-wage bite and employment growth rate in the pre-treatment period as captured by the hypothetical bite. This substantiates our hypothesis that the strongly positive coefficients for  $\alpha$  in Table 1 are not driven by structural differences but rather by a simultaneity bias.

Again, we find no economically or statistically significant treatment effect for West German districts. Given that wage growth was not affected by the minimum wage introduction, this result is to be expected. In contrast, the employment effect in East Germany is consistently estimated with a negative sign and statistically significant in Columns (1) and (2) of Table 3. However, as soon as we add linear trends at the labour-market-region level [Columns (3)–(5)], the point estimate halves in magnitude and loses statistical significance at conventional levels. The estimates using the border-pair sample (Columns (1) and (2) of Table 4) confirm this results. Controlling for spatial heterogeneity across labour-market regions, either by adding linear trends, by

<sup>12</sup> These results are robust to the exclusion of regions that belong to the top and bottom 5% of the minimum-wage bite. The same is true for the inclusion of district-type or labour-market region-specific linear trends in the models based on the border-pair approach. Those results can be provided upon request. Further, the online material provides the results of regressions using a shorter time interval (1994–1998).

**Table 3** The minimum wage bite's effect on employment growth. *Source:* Authors' calculations based on the IEB

	(1)	(2)	(3)	(4)	(5)
Hypothetical bite (West)	-0.040 (0.298)	-0.013 (0.308)	-0.100 (0.292)	-0.174 (0.303)	-0.094 (0.290)
Hypothetical bite (East)	-0.028 (0.200)	-0.028 (0.204)	-0.127 (0.271)	-0.107 (0.274)	-0.128 (0.263)
Treatment effect (West)	0.033 (0.197)	-0.019 (0.198)	-0.029 (0.232)	0.039 (0.243)	-0.038 (0.228)
Treatment effect (East)	-0.241* (0.131)	-0.249* (0.131)	-0.124 (0.190)	-0.162 (0.183)	-0.181 (0.186)
Wage growth (other industries)	0.160 (0.108)	0.162 (0.108)	0.151 (0.111)	0.141 (0.110)	0.149 (0.113)
Employment growth (other industries)	0.041 (0.030)	0.041 (0.030)	0.040 (0.030)	0.040 (0.030)	0.039 (0.030)
Hypothetical bite (West), neighbours				0.420 (0.410)	-0.239 (0.465)
Hypothetical bite (East), neighbours				0.322 (0.200)	0.183 (0.239)
Treatment effect (West), neighbours				-0.434 (0.347)	0.099 (0.391)
Treatment effect (East), neighbours				-0.241 (0.258)	0.362 (0.290)
Wage growth (other industries), neighbours				-0.183 (0.193)	0.159 (0.258)
Employment growth (other industries), neighbours				0.052 (0.048)	-0.006 (0.048)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
District-type-specific trends	No	Yes	No	No	No
Labour-market-region-specific trends	No	No	Yes	Yes	Yes
$R^2$	0.080	0.112	0.132	0.134	0.134
Observations	3708	3708	3708	3681	3708

Column (4) defines neighbours as being in the same labour-market region and Column (5) defines neighbours as sharing a common border (cf. Sect. 4). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors are enclosed in parentheses and clustered at the labour-market-region level

explicitly modelling spatial spillovers, or by using the border-pair approach, therefore appears to be of high importance.

Based on these results, it is not clear if we simply estimate a smaller but nevertheless negative treatment effect imprecisely, or if the minimum wage introduction truly had no adverse employment effect in East Germany despite a positive effect on wage growth.

**Table 4** Border approach models for employment. *Source:* Authors' calculations based on the IEB

	(1)	(2)
Hypothetical bite (West)	0.002 (0.349)	-0.116 (0.457)
Hypothetical bite (East)	0.038 (0.117)	-0.052 (0.204)
Treatment effect (West)	-0.067 (0.359)	-0.045 (0.379)
Treatment effect (East)	-0.169 (0.153)	-0.149 (0.120)
Wage growth (other industries)	0.032 (0.147)	0.076 (0.151)
Employment growth (other industries)	0.064 (0.050)	0.077 (0.049)
Pair-period fixed effects	Yes	Yes
District fixed effects	No	Yes
$R^2$	0.528	0.560
Observations	19080	19080

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are enclosed in parentheses and two-way clustered at the level of the labour-market region for both districts of a pair. We use the user-written routine `reghdfe` for Stata by Correia (2017)

If the focus were only on statistical significance, we would conclude that the minimum wage had no adverse employment effect since our preferred specifications are clearly those controlling for spatial heterogeneity across labour-market regions. We would therefore not draw too much inference from Columns (1) and (2) of Table 4. However, it is also important to note that the point estimate of the East German treatment effect is negative across all specifications, stable in magnitude, and of non-negligible economic significance.

Indeed, the transformation of our dependent variable does have pros and cons. Recall from Sect. 3 that we measure regional employment growth in main construction relative to finishing trades because of the strong recession affecting the entire construction sector. The recession poses a threat to the identification of the treatment effect to the extent that low-wage (high-bite) regions suffer more than high-wage (low-bite) regions from the economic downturn. We believe this to be likely as structurally weak regions do react more strongly to the economic cycle and descriptive evidence of the pre-treatment period, which was characterized by a boom in East Germany, points into this direction. We therefore opt for the transformation of the dependent variables to prevent a downward bias of the treatment effect. While measuring employment in main construction relative to finishing trades helps to net out the economic downturn's impact, this transformation might lead to an upward-bias of the employment effect insofar as finishing trades, as a subsequent sector to main construction in the production process, is indirectly affected by the introduction of the wage floor. If the minimum wage causes a decrease in the entire construction sector's output, employment would

fall in both sub-industries, main construction and finishing trades. Unfortunately, we have no way of investigating this issue more deeply.

It is likely, however, that the true treatment effect lies between those estimates using the employment growth rate in main construction only (upper bound) and those estimates based on the transformed variable (lower bound). Therefore, Tables 5 and 6 show all specifications contained in Tables 1, 2, 3 and 4, but with the dependent variables relating to main construction only. In terms of employment effects, the point estimates are much larger in magnitude, always negative, and of high statistical significance.

In conclusion, we prefer the models with the transformed dependent variable, since adequately controlling for the downturn is likely the bigger issue. We acknowledge that spillover effects of the minimum wage in main construction to finishing trades might have introduced some bias, thereby leading to smaller and statistically insignificant point estimates. Our interpretation is that the treatment effect is indeed negative, but that the data are too noisy to achieve statistical significance once we transform the dependent variable and control for spatial heterogeneity. We thus expect the true treatment effect for East Germany to roughly equal  $-0.15$ . An increase in one standard deviation in the regional bite therefore implies a decrease in the growth rate of employment of roughly 1.2 percentage points.

The magnitude of this disemployment effect has to be interpreted against the background of the deep recession that the construction industry experienced during the observation period, starting in the mid-1990s. As a back-of-the-envelope calculation, consider that the average growth rate of employment in East Germany was approximately  $-12\%$  between 1996 and 1997. Setting the coefficient of the treatment effect to  $-0.15$  and observing that the average bite was around  $20\%$  in 1996 yields a treatment effect of 3 percentage points. Thus, while employment contracted in all East German districts between 1996 and 1997, our estimates suggest that the minimum-wage introduction caused one quarter of the overall decline. Note that this share is lower compared to König and Möller (2009), whose results attribute between 28 and 57% of the overall decline of East German employment in main constructions to the minimum wage. At least part of this difference is due to the fact that our results control better for business cycle effects. When low-wage workers are affected stronger by a recession, using medium-wage workers as a control group will overestimate the effect of the minimum wage.

## 6 Discussion and conclusion

This paper studies the effects of a sectoral minimum wage in the German main construction sector on regional labour markets with a specific focus on spatial spillovers and regional heterogeneity. Our results indicate that wage growth in East Germany was positively affected by the minimum wage while the West German wage growth rate did not react at all. In terms of employment, we do not find any effect in West Germany. There is, however, some evidence that the minimum wage caused a contraction in employment growth in the East, where the bite of the minimum wage was relatively high. While the employment estimates lack statistical significance at conventional levels, our results consistently point towards a reduction in the regional employment

growth rate by about one percentage point for a one-standard-deviation increase in the regional share of affected workers.

We argue that our regional perspective has some advantages over the existing studies using individual data to study sectoral minimum wages in Germany. First, the higher level of aggregation circumvents much of the measurement errors that plague other studies. More explicitly, the difficulties associated with the identification of treatment and control groups elsewhere do not materialize here. Second, worker substitution taking place at the individual level is a further problem that we are able to sidestep with our approach. Third, focusing on local labour markets allows us to extend the analysis beyond job destruction and gain insights into the overall effect of the sectoral minimum wage including job creation. Our results also emphasize the importance of controlling adequately for heterogeneity in local labour markets.

The focus on the main construction sector is enriching to the literature because of the unique characteristics of the minimum wage in this sector. First, the minimum wage introduced was of a substantial magnitude, and second, it was introduced during a period of economic contraction, particularly in East Germany. Much of the previous research on the impacts of minimum wages has provided evidence of modest changes in the minimum wage during less turbulent periods of the economy. The evidence presented here is consistent with the view that a moderate minimum wage might have negligible effects, but that this can change if it is allowed to cut too deeply into the wage distribution. In this case, it will benefit some workers, but this comes at the cost of making other workers (the displaced ones and those who are unable to find employment) worse off.<sup>13</sup> Third, we do not focus on young workers or a typical low-wage sector. We therefore provide evidence of a high-impact minimum wage affecting male, prime-aged, and skilled workers who are typically the breadwinner of the household. This finding has important consequences for the minimum wage's effect on (relative) poverty and inequality. While those workers staying in employment benefit from the wage increase, those workers losing their job are severely hit by the income loss.

Our analysis is limited by the fact that we are unable to take into account the presence of posted (i.e. foreign) workers in Germany. One might also wonder about the response of self-employment, which rose substantially during the observation period in East Germany despite the strong decline in overall employment.<sup>14</sup> Part of this increase could be driven by former employees who registered themselves as self-employed to avoid compliance with the minimum wage. While this is possible, we present some rough calculations in the online material showing that this is unlikely to be the principal driver of the observed employment decline.<sup>15</sup> While posted workers do not enter the analysis, the overall employment effect is likely to be more negative if

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<sup>13</sup> The displaced workers in the construction industry might find jobs in other, uncovered sectors. This study is therefore not able to make general statements on the development of (un-)employment in the entire German economy. Still, the study is able to shed light on the employment development in the affected industry itself, which constitutes an interesting case study on the effects of minimum wages on the labour market.

<sup>14</sup> Between 1995 and 2000, the number of proprietors increased by nearly 70% (ELVIRA 2013).

<sup>15</sup> Even under generous assumptions about the amount of employment replaced by self-employment, self-employment could only account for about 14% of the total employment losses observed during this period.



they were included. Posted workers usually received lower wages than native workers before the new minimum wage eroded at least a significant part of that price advantage. Moreover, we have examined the “raw” effect of the minimum wage on employment and wage growth rates but have not taken into account other channels of adjustment, particularly employment turnover. A decrease in turnover might indicate that firms are investing more in their employees as a result of the minimum wage, and such an investment can have a profound impact on employment stability or the health of the labour market itself (Gittings and Schmutte 2015). Finally, we have not examined both the mobility of construction firms and the changes in the number of firms. If these firms are sufficiently mobile, they may adjust by moving their operations to regions which are less affected by the minimum wage. While we do not expect this mobility to be too important due to the nature of the market for products of the construction sector, this is another channel of adjustment that is left for further research.

While we advise against directly carrying over our results to the assessment of the national minimum wage spanning all sectors, our findings serve as a cautionary tale, reminding us that the effect of any minimum-wage legislation on the labour market is connected to the size of the minimum-wage bite and can be influenced by the economic cycle. On the one hand, the effect of the statutory minimum wage in Germany is likely to be less negative compared to our identified treatment effect, as the overall bite is lower<sup>16</sup> and the German economy today is in a much better state compared to main construction in the late 1990s. Further transmission channels, e.g. through the product market, are also more likely to mitigate any adverse employment effects. The first studies on the short-run effect of the statutory minimum wage confirm this expectation: The estimated employment effects are neutral or moderately negative (Bossler and Gerner 2016; Garloff 2016; Caliendo et al. 2018). On the other hand, the economic cycle will turn at some point and it is possible that adverse employment effects of the statutory minimum wage will only materialize then. Indeed, Dolton and Bondibene (2012) provide evidence that at least the negative effect on youth unemployment is aggravated by minimum wages during a downturn.

**Data disclaimer** For our analyses, we use administrative data of the Institute for Employment Research. The data are social data with administrative origin which are processed and kept by IAB according to the German Social Code III. There are certain legal restrictions due to the protection of data privacy. The data contain sensitive information and therefore are subject to the confidentiality regulations of the German Social Code (Book I, Section 35, Paragraph 1). The data are held by the Institute for Employment Research (IAB), Regensburger Str. 104, D-90478 Nürnberg, iab@iab.de, phone: +49 911 1790. Our raw data, computer programs, and results have been archived by IAB to meet the objective of good scientific practice. If you wish to access the data for replication purposes, please get in contact with the authors. Computer programs are also available from the authors upon request.

## Compliance with ethical standards

**Conflict of interest** The authors declare that they have no conflict of interest.

**Ethical approval** This article does not contain any studies with human participants or animals performed by any of the authors.

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<sup>16</sup> In 2014, 1 year before the statutory minimum wage was introduced, the bite for full-time workers was a little over 4% (Mindestlohnkommission 2016).

## Appendix

**Table 5** Panel approach models without accounting for finishing trades. *Source:* Authors' calculations based on the IEB

	Wage growth				
	(1)	(2)	(3)	(4)	(5)
Hypothetical bite (West)	0.332*** (0.043)	0.330*** (0.042)	0.366*** (0.042)	0.390*** (0.041)	0.367*** (0.039)
Hypothetical bite (East)	0.125*** (0.025)	0.130*** (0.026)	0.142*** (0.028)	0.141*** (0.028)	0.144*** (0.028)
Treatment effect (West)	-0.053** (0.027)	-0.040 (0.028)	-0.037 (0.033)	-0.045 (0.033)	-0.036 (0.036)
Treatment effect (East)	0.148*** (0.023)	0.147*** (0.019)	0.149*** (0.018)	0.149*** (0.018)	0.156*** (0.020)
Wage growth (other industries)	0.003 (0.018)	0.002 (0.018)	0.001 (0.018)	0.000 (0.018)	0.001 (0.018)
Employment growth (other industries)	-0.008** (0.004)	-0.008** (0.004)	-0.008** (0.004)	-0.008** (0.004)	-0.008** (0.004)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
District-type-specific trends	No	Yes	No	No	No
Labour-market-region-specific trends	No	No	Yes	Yes	Yes
Local spillover effects	No	No	No	Yes	Yes
R <sup>2</sup>	0.237	0.444	0.441	0.459	0.459
Observations	3708	3708	3708	3681	3708
	Employment growth				
	(1)	(2)	(3)	(4)	(5)
Hypothetical bite (West)	-0.153 (0.187)	-0.106 (0.196)	-0.044 (0.210)	-0.129 (0.210)	-0.133 (0.205)
Hypothetical bite (East)	0.013 (0.129)	-0.026 (0.127)	-0.045 (0.160)	-0.044 (0.159)	-0.047 (0.158)
Treatment effect (West)	0.130 (0.134)	0.004 (0.148)	-0.138 (0.202)	-0.062 (0.189)	-0.016 (0.185)
Treatment effect (East)	-0.386*** (0.124)	-0.368*** (0.129)	-0.311** (0.147)	-0.315** (0.149)	-0.331** (0.144)
Wage growth (other industries)	-0.007 (0.062)	-0.001 (0.061)	0.005 (0.062)	-0.001 (0.063)	0.006 (0.064)
Employment growth (other industries)	0.023 (0.019)	0.020 (0.019)	0.017 (0.019)	0.017 (0.020)	0.016 (0.020)
District fixed effects	Yes	Yes	Yes	Yes	Yes

**Table 5** continued

	Employment growth				
	(1)	(2)	(3)	(4)	(5)
Year indicators	Yes	Yes	Yes	Yes	Yes
District-type-specific trends	No	Yes	No	No	No
Labour-market-region-specific trends	No	No	Yes	Yes	Yes
Local spillover effects	No	No	No	Yes	Yes
$R^2$	0.258	0.460	0.449	0.476	0.476
Observations	3708	3708	3708	3681	3708

The dependent variable is the growth rate of mean wages/employment in main construction only; i.e. finishing trades are not used as a control group. Column (4) defines neighbours as being in the same labour-market region, and Column (5) defines neighbours as sharing a common border (cf. Sect. 4). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors are enclosed in parentheses and clustered at the labour-market-region level

**Table 6** Border approach models without accounting for finishing trades. *Source:* Authors' calculations based on the IEB

	Wage growth		Employment growth	
	(1)	(2)	(1)	(2)
Hypothetical bite (West)	0.064*	0.322***	-0.089	-0.177
	(0.036)	(0.057)	(0.253)	(0.300)
Hypothetical bite (East)	0.054***	0.149***	0.155*	-0.016
	(0.018)	(0.041)	(0.085)	(0.127)
Treatment effect (West)	-0.004	-0.018	-0.001	0.087
	(0.032)	(0.054)	(0.241)	(0.255)
Treatment effect (East)	0.031*	0.108***	-0.379***	-0.389***
	(0.018)	(0.018)	(0.099)	(0.091)
Wage growth (other industries)	0.028	0.023	0.004	-0.024
	(0.028)	(0.026)	(0.092)	(0.096)
Employment growth (other industries)	-0.004	-0.008	0.039	0.024
	(0.007)	(0.006)	(0.032)	(0.034)
Pair-period fixed effects	Yes	Yes	Yes	Yes
District fixed effects	No	Yes	No	Yes
$R^2$	0.687	0.739	0.720	0.755
Observations	19080	19080	19080	19080

The dependent variable is the growth rate of mean wages/employment in main construction only; i.e. finishing trades are not used as a control group. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors are enclosed in parentheses and two-way clustered at the level of the labour-market region for both districts of a pair. We use the user-written routine `reghdfe` for Stata by Correia (2017)


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**Publisher's Note** Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.

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