

vom Berge, Philipp; Frings, Hanna; Paloyo, Alfredo R.

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High-Impact Minimum Wages and Heterogeneous Regions

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Philipp vom Berge
Hanna Frings
Alfredo R. Paloyo

High-Impact Minimum Wages and Heterogeneous Regions

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Technische Universität Dortmund, Department of Economic and Social Sciences
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Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI)
Hohenzollernstr. 1-3, 45128 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer
RUB, Department of Economics, Empirical Economics
Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de

Prof. Dr. Wolfgang Leininger
Technische Universität Dortmund, Department of Economic and Social Sciences
Economics – Microeconomics
Phone: +49 (0) 231/7 55-3297, email: W.Leininger@wiso.uni-dortmund.de

Prof. Dr. Volker Clausen
University of Duisburg-Essen, Department of Economics
International Economics
Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de

Prof. Dr. Christoph M. Schmidt
RWI, Phone: +49 (0) 201/81 49-227, e-mail: christoph.schmidt@rwi-essen.de

Editorial Office

Joachim Schmidt
RWI, Phone: +49 (0) 201/81 49-292, e-mail: joachim.schmidt@rwi-essen.de

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Philipp vom Berge, Hanna Frings, and Alfredo R. Paloyo¹

High-Impact Minimum Wages and Heterogeneous Regions

Abstract

We estimate the effects on wage and employment growth rates of the introduction and subsequent increases of a substantial minimum wage in the main construction industry of Germany. 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 2.6 to 3.1 percentage points for a one-standard-deviation increase in the minimum-wage bite, amounting to roughly half of the overall decline in the growth rate, but no significant change is observed for the West.

JEL Classification: J31, J38

Keywords: Construction sector; Germany; minimum wage; spatial heterogeneity; spatial panel data

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¹ Philipp vom Berge, Institut für Arbeitsmarkt- und Berufsforschung (IAB); Hanna Frings, RWI and Ruhr-Universität Bochum; Alfredo R. Paloyo, RWI. – We are grateful to Ronald Bachmann, Thomas K. Bauer, Daniel Baumgarten, Marion König, Joachim Möller, Achim Schmillen, Matthias Umkehrer, and participants of the 52nd Annual Congress of the European Regional Science Association, the 6th Doctoral Conference of the Ruhr Graduate School in Economics, and seminars at the RWI for helpful comments. All remaining errors are our own. – All correspondence to Hanna Frings, RWI, Hohenzollernstr. 1–3, 45128 Essen, Germany, E-Mail: hanna.frings@rwi-essen.de.

1 Introduction

Over the last two decades, there has been a considerable amount of research on the economic impacts of minimum wages. While earlier studies established a firm professional consensus that minimum wages unambiguously increase unemployment [Minimum Wage Study Commission 1981; Alston, Kearn and Vaughan 1992], renewed interest in the topic was triggered by a series of papers in *Industrial and Labor Relations Review* [Card 1992; Neumark and Wascher 1992; Katz and Krueger 1992] and especially by the influential book by Card and Krueger [1995]. Since then, the US debate over whether minimum wages are necessarily detrimental to overall employment has not subsided [Deere, Murphy and Welch 1995; Card and Krueger 2000; Neumark and Wascher 2000, 2008; Dube, Lester and Reich 2010; Neumark, Salas and Wascher 2013]. Perhaps unsurprisingly, the latest studies do not appear to be leading toward a renewed consensus as of yet.¹

Until recently, much of the state of knowledge about minimum wages stemmed from studies conducted in the US. However, regulating the price of factors of production takes on varied forms, and one can expect the institutional framework to play a decisive role. For instance, in the UK, “wages councils” initially set minimum wages for specific industries for much of the 20th century until they were replaced by the Low Pay Commission in 1997, which was tasked to establish a national minimum wage [Dickens et al. 1993; Brown 2009]. Other studies outside the US have been conducted for France [Abowd et al. 2000], Spain [Dolado, Felgueroso and Jimeno 1997], and the Netherlands [Machin and Manning 1997, including France, Spain, and the UK]. In the US itself, the federal minimum wage, currently set at USD 7.25, is a mere 44 percent of the median hourly wage of all occupations in 2011 [Bureau of Labor Statistics 2011]. This figure is much lower than the percentages seen in other OECD countries. The literature on minimum wages in Germany, which has unique but nevertheless enlightening characteristics, is relatively recent, and a full exposition is offered in the subsequent section.

Methodologically, estimating the effect of minimum wages on employment has also substantially evolved, closely tracking the developments in econometric techniques. Much earlier research on the topic used traditional panel-data estimation methods which were common

¹In 2006, PhD-holders of the AEA were about evenly split between (1) raising the minimum wage or keeping it at its current level and (2) decreasing it or completely eliminating it [Whaples 2006], although, to be sure, supporting the former does not mean one has to abandon the belief that minimum wages increase unemployment.

at that time (e.g., [Neumark and Wascher \[1992\]](#)). In this line of research, national estimates are obtained by exploiting the cross-state variation in minimum wages over time. The “new” minimum-wage research, typified by the [Card and Krueger \[1994\]](#) paper, is based on treatment group–control group comparisons: case studies comparing neighboring geographic areas where one part is affected by a change in the minimum wage (in a sense, a geographic difference-in-differences approach). The preferred method of the researcher seems to influence the outcome of the estimation exercise: panel-data methods often arrived at the result that minimum wages have an overall negative effect on employment while the two-group, two-mean comparisons tended to show either positive or neutral effects.

As noted by [Dube, Lester and Reich \[2010\]](#), both approaches have advantages and disadvantages over the other (the details of which are available in Sec. 4), particularly in terms of controlling for spatial heterogeneity. Cognizant of this, [Dube, Lester and Reich \[2010\]](#) offered a synthesis of the two approaches by essentially generalizing the [Card and Krueger \[1994\]](#) method of comparing bordering regions with different levels of the minimum wage. Using all counties in the US along a state border, where the two states have different levels of the minimum wage, allowed them to make [Card and Krueger](#)-type comparisons for many counties over time. This addresses the problems caused by spatial heterogeneity arising from the exclusive use of either the panel-data method or the single border-pair approach.

Using both the panel-data approach and the recently-generalized border-pair approach, we contribute to the emerging international literature that examines minimum-wage effects outside the well-studied US context, which is characterized by a low treatment intensity as opposed to the German case analyzed here. Our study examines the wage and employment effects of the introduction and subsequent increases of a minimum wage in the German main construction sector (*Bauhauptgewerbe*) between 1997 and 2002. This is the first sector where negotiations over a generally-applicable minimum wage were concluded.² At the time the wage floor was set, the sector was employing about 1.3 million workers, making it the largest German industry where minimum wages apply today [[Bachmann, König and Schaffner 2012](#)]. As of 2011, its share of Germany’s GDP is substantial at 4.4 percent. Furthermore, the minimum wages for East and West Germany have been adjusted almost annually since their introduc-

²The other sectors that have subsequently introduced minimum wages are waste removal, coal mining, roofing, electrical installation, commercial cleaning, painters and varnishers, nursing care, security services, industrial laundries, temporary work, and education and training services.

tion. This political propensity to regulate wages is unlikely to wane,³ necessitating a careful evaluation of their effects to inform policy.

What makes the German construction case particularly interesting is the fact that the industry was subjected to a substantial minimum wage. In contrast to the minimum-wage changes contemplated in the previous US studies, the *Bauhauptgewerbe*, particularly in former East Germany, experienced very high treatment intensities (e.g., the maximum share of main construction workers earning below the minimum wage is almost 41 percent). The behavior of the labor 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 of the wage floor. Therefore, the results here can be applied to situations where the minimum wage to be introduced and maintained is of a significant magnitude.

Since the share of workers earning below the minimum wage in this sector varies from one region to the next, one can exploit the spatial variation to estimate the impact of the wage floor on various socioeconomic outcomes [Card 1992; Stewart 2002]. Our interest lies in the effect of minimum wages on wage and employment growth rates in East and West Germany. These two variables are expected to reflect the first-order effects of minimum wages, and their response behavior at the regional level has thus far not been adequately described and quantified. The use of regional data in this case has the added benefit of overcoming some of the problems associated with individual-level data in Germany, such as the difficulty in identifying who the actual recipients of the minimum wage are. Hence, the identification of the appropriate control group for comparison (either in the form of individuals above the minimum wage or industries unaffected by the minimum wage) is not a relevant complication that can arise in our case.

Moreover, our contribution advances the literature further by, first, taking into account potential spillover effects between regional labor markets and, second, controlling for heterogeneity at a local level. The importance of those factors has been discussed in the US minimum-wage literature only recently [Dube, Lester and Reich 2010; Allegretto, Dube and Reich 2011; Kalenkoski and Lacombe Forthcoming; Neumark, Salas and Wascher 2013], and the appropriate techniques have thus far never been applied to a situation similar to the Ger-

³Indeed, US President Barack Obama called for about a 25-percent increase in federal minimum wages in his 2013 State of the Union address. In Germany, the Greens, the Social Democrats, and now the Christian Democrats (the party of the chancellor, Angela Merkel) are backing an economy-wide minimum wage.

man institutional setting.

Taking developments in spatial econometrics into account, we recognize that the presence of both spatial heterogeneity and spatial autocorrelation may bias traditional estimates of the effect of the minimum wage. Even in the more developed US literature, spatial spillovers have not been thoroughly addressed as much as the issue of spatial heterogeneity. Inadequately addressing spatial issues may explain the discrepancies in outcomes of recent German studies (e.g., König and Möller [2009]; Apel et al. [2012]). By subjecting the data to a rigorous analysis that allows for various forms of spatial effects, we attempt to rule out the possibility for those effects to have a decisive impact on our results.

Our findings are the following. 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 percentage of affected workers is associated with an increase of the growth rate of average wages by approximately 1.2 percentage points. Second, while we do not find any effects on employment in West Germany, the negative effect on East German employment was quite large. Our estimates show that an increase by one standard deviation of the percentage of affected workers is associated with a reduction in the employment growth rate by 2.6 to 3.1 percentage points. Third, we provide evidence that spatial spillover effects and regional heterogeneity do not alter our main results.

2 Minimum wages in Germany and previous evaluations

In this section, we provide the historical and institutional background for minimum wages in Germany, particularly for the main construction industry. We also review the existing evaluation literature on its effects, focusing on those studies that address the issue of wage and employment responses of the labor market. We note that the lack of concern of previous studies over the spatial dimension of the data-generating process invites further analysis.

2.1 Institutional details on Germany's minimum wages

Minimum wages in Germany are special because they do not derive from a federal or state law that mandates a specific wage floor.⁴ Unlike the US (where the majority of studies on minimum-wage effects originates), the wage floors are set via collective bargaining between employees' unions and employers' associations at the industry level. These collective-bargaining agreements (CBAs) can then be declared to be universally binding by the Federal Ministry of Labor and Social Affairs (BMAS). Once that occurs, the wage floor will apply to all workers in that particular industry, irrespective of whether they belong to the bargaining workers' union or not.

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 other European Union member states and so-called "third countries". 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 wage floor in Germany—at least for the construction sector—is anchored squarely on protectionist and anti-competitive reasons, with poverty alleviation taking a backseat.

The negotiations that ultimately led to the introduction of the minimum wage in January 1997 were rather difficult. Consequently, it is unlikely that employers anticipated the exact date of the minimum-wage introduction, thereby possibly distorting the timing of the treatment.

The evolution over time of the minimum wages established in the main construction sector since its introduction is presented in Figure 1 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. However, in real terms, the minimum wage has remained rather stable and close to the level at which it was first introduced, exhibiting an increase of roughly 5 and 10 percent for East and West Germany, respectively, for the period between 1997 and 1999. 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.

Prior to the introduction of the minimum wage in 1997, the coverage of sectoral (but not

⁴For more details on the German institutional setting, see IAB, RWI and ISG [2011] and the sources cited therein.

universally binding) CBAs in German construction was already generally high. Based on 1995 firm-level data, sectoral CBAs in West Germany covered 81 percent of establishments. [Kohaut and Bellmann 1997] In the East, the coverage rate was around 40 percent [IAB, RWI and ISG 2011]. 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 US, such as fast-food workers or teenage employees.

Another peculiarity in the introduction of the minimum wage in Germany's construction sector is that it came at a time of much economic contraction. Specifically, nominal gross value added (in billion EUR) dropped from 112.97 in 1995 to 99.21 in 1998 [IAB, RWI and ISG 2011]. The present study should therefore be taken to shed light on the question of the effects of a minimum-wage introduction of a substantial magnitude in times of an economic contraction in a specific industry.

2.2 Previous studies on wage and employment effects

While the effects of the minimum wage in Germany's construction industry have already been studied for a variety of outcomes, we review only those that specifically deal with the effects on wages, employment, or both. In ascending chronological order, the following studies are relevant: Bauer et al. [2009],⁵ Büttner and Ebertz [2009], König and Möller [2009], Rattenhuber [2011], Apel et al. [2012],⁶ Müller [2012], and Frings [Forthcoming]. The detailed results of these studies are more competently addressed therein, but we present the general conclusions derived from this collective body of research and note relevant exceptions.

First, in terms of establishing whether the minimum wage in fact increased average wages or wage growth, the consensus view is that this is indeed the case for East Germany. The results for the West German wage distribution are less consistent. Büttner and Ebertz [2009] did not explicitly examine the case of East vs. West but rather rural vs. urban areas ("countryside" vs. "city"), and their simulation showed that wages in the countryside will increase as a result of the introduction of minimum wages. Larger effects are generally found in the East relative to the West, implying that the so-called "bite" (i.e., a measure of treatment intensity explained in

⁵Bauer et al. [2009] did not explicitly analyze the case for the construction industry. Instead, they estimated the fiscal and employment effects by skill level in response to hypothetical values of the minimum wage.

⁶Apel et al. [2012] is the scientific publication that arose out of the project report that is the IAB, RWI and ISG [2011] study commissioned by the BMAS.

detail below) of the minimum wage was more intensive in the former compared to the latter.

There is more contention about the estimates on employment, where König and Möller [2009], among the first few studies, stand out in claiming that, despite the minimum wage somewhat accelerating wage growth, the employment growth rate also actually increased in West Germany. Notably, the rest of the other studies do not support this conclusion. To be fair, however, the estimated effect in König and Möller [2009] is not robust to all their specifications, and they are careful not to over-interpret their result.⁷ The results for East Germany are also not in accordance with each other: while Apel et al. [2012] and Frings [Forthcoming] found neutral employment effects despite the positive effects on wages, König and Möller [2009] and Müller [2012] conclude that the minimum wage had a negative impact on employment.

With the sole exception of Büttner and Ebertz [2009], which is a simulation and does not specifically address the minimum wage currently under investigation, none of the aforementioned studies focuses on spatial effects of the minimum-wage introduction. However, ignoring potential spillovers and unobserved spatial heterogeneity can lead both to incorrect coefficient estimates and to incorrectly estimated standard errors. We attempt to fill this research gap to find out whether the previous literature is impaired by this omission. We use spatial econometric techniques and a recently-developed approach that generalizes regional natural experiments to control for spatial effects in our subsequent analysis.

3 Data construction and description

This empirical study is based on administrative data that cover the entire population of construction workers in Germany who are subject to social security contributions. 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 all workers that were employed in the main construction sector at any point in time during the period 1993–2002.

The data contain sociodemographic as well as employment characteristics, including the

⁷Regrettably, the positive conclusion for employment in West Germany may have been oversold to the public by others, engendering a lively debate in the pages of the leading German-language business-oriented newspaper, the *Handelsblatt*. See, for instance, <http://goo.gl/VL8A9> (accessed 13 February 2013).

average daily wage.⁸ 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 and (2) the share of women among blue-collar workers in this sector is extremely low.

Unfortunately, no information on hours worked, which is necessary to calculate hourly wage rates, is available. This information is crucial because the minimum wage itself is expressed in an hourly basis. 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*). We adopt their results for our calculation of hourly wages.⁹

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.

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 cutoff date each year. That is, each male blue-collar worker employed in the main construction industry on that day remains in our operational dataset.¹⁰ One advantage of the annual data is that seasonal effects (e.g., the decline in employment in winter) become tangential for the analysis of the employment effect of the minimum wage.

The data are regionally disaggregated down to the level of districts (*Kreise und kreisfreie Städte*, NUTS 3), and we use detailed industry classifications to define the construction industry.¹¹ Most subsectors of the main construction industry belong unambiguously to the treat-

⁸Average 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 very small.

⁹Basically, full-time employed workers appear, on average, to work 40 hours per week irrespective of their individual or job characteristics.

¹⁰For completeness, two more annual datasets were constructed by considering all spells for three- and six-month intervals. The results based on these datasets do not change our conclusions and can be made available upon request. Note, however, that additional difficulties are introduced concerning the calculation of wage and employment levels using these alternative datasets since an individual may change employers or may leave the main construction industry within the extended intervals. Using one specific date circumvents these issues.

¹¹We follow IAB, RWI and ISG [2011] in the choice of the relevant subsectors. 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

ment group. In the rare cases in which a subsector does not clearly belong to the treatment group (i.e., the subsector “scaffolding” (with a numeric code of [616]) covers the production as well as the installation of scaffolds, while only the latter is covered by the CBA), it is excluded from the analysis in order to ensure that the treatment effect will not be underestimated simply because some individuals in the treatment group are not really affected by the minimum wage.

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, this study therefore concentrates on the time period from the introduction of the minimum wage in 1997 up to 2002.

The dataset is transformed from the individual to the district level. The two dependent variables are average wage and employment growth rates in each district. 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. Annual growth rates are then computed. The minimum-wage treatment is measured by the bite, which is defined as the share of workers earning below the minimum wage in the period prior to its introduction or increase. Thus, the identification of the minimum-wage effect is based on the differential treatment intensities across districts.

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 labor-market regions.

Figures 2 and 3 show the spatial distribution of the bite in 1996 prior to the minimum-wage introduction for West and East Germany, respectively. The majority of neighboring districts is clearly characterized by different treatment intensities. In West Germany, the bite varies between 0.45 percent and 27.02 percent, while at least 6.14 percent and at most 40.58 percent of all workers in each district are affected in East Germany. However, the distribution is heavily

and timber construction.

skewed to the right, which implies that 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 neighboring districts (cf. Sec. 4).

Table 1 shows the development of the average bite as well as the mean wage and employment growth rates over time. In East Germany, the average bite in 1996 prior to the minimum-wage introduction amounts to almost 22 percent. In 1997, it decreases to less than 12 percent, only to reach almost 23 percent again 1999. This pattern mirrors the development of the nominal minimum wage (Figure 1) with the only sizable increase in 1999. The general pattern is present in West Germany, although the average treatment intensity is much lower in each year compared to the East. Wage and employment growth rates appear to decrease from the mid-90s onwards. It is impossible to decide based on these descriptive statistics whether these developments are caused by the beginning recession or by the minimum-wage introduction.

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 percent with a standard deviation of 1.8; the average employment growth rate is -5.98 percent 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.

The use of districts as observational unit also creates potential problems. It is necessary to control for structural differences in terms of wage and employment growth rates that are not caused by the minimum wage. To this end, two strategies are employed. First, the average wage and employment growth rates of all other industries except construction in each specific district are added as control variables. These indicators are based on a 2-percent random sample drawn from the IEB. Second, as we explain in Sec. 4, the classification of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) is used for

both the classification of districts into nine different types (*siedlungsstrukturelle Kreistypen*¹²) as well as the definition of broader labor-market regions (*Raumordnungsregionen*). The latter is important because districts are administrative regions that are interconnected in terms of the product as well as the labor market. Consequently, they can be used to explicitly allow for spatial spillover effects within these labor-market regions in the econometric specifications.

4 Estimation strategy

In the following, we describe a statistical framework to examine the effects of the minimum-wage bite on regional wage and employment patterns. We begin with a relatively basic model that mimics the standard approach to analyze minimum-wage effects in a panel of regional data. We then extend the model in various ways to more comprehensively capture spatial dependencies or heterogeneities, noting potential strengths and weaknesses of those approaches along the way.

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 \[1992\]](#). Following [Dolton, Bondibene and Wadsworth \[2010\]](#), we separate the post-treatment effect from the more general correlation between the dependent variables and the bite by introducing an artificial (or hypothetical) bite before the minimum-wage introduction. It is calculated assuming that the 1997 minimum wage (adjusted for previous wage trends) already applied.

Our initial specification is

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

¹²The types are formed based on population density and the degree of interconnectedness with neighboring districts [[BBSR 2012](#)]. These characteristics are used to proxy unobserved determinants of structural differences in wage and employment growth rates.

where $\Delta \ln y_{it}$ 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 an indicator for the post-treatment period. Thus, β captures the treatment effect of the minimum wage. The vector $\Delta \ln \mathbf{x}_{it}$ represents mean wage and employment growth in all local industries except construction as additional controls to proxy for differences in local demand shocks. The terms μ_i and τ_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 α , β and τ_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. We do not run separate regressions to ensure comparability with the later neighborhood-effects model where splitting the sample would mean a loss of neighborhood information at the inner German border.¹³

We use growth rates as dependent variables for two reasons. First, using levels might lead to counterintuitive 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 (see Table 1). 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. Second, this strategy helps us to avoid problems arising from serial correlation in levels [Wooldridge 2010].

In Equation (1), α captures the correlation between the artificial or 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 bite. The identifying assumption for β to properly measure the treatment effect is that the correlation between the bite and the dependent variables would have stayed constant in the absence of the minimum-wage introduction.

Note that there is one potential caveat when estimating Equation (1), especially with wage growth as the dependent variable. Regional wages play a role in determining both the size of the bite and the subsequent growth rate of wages, thus violating the assumption of strict ex-

¹³In any case, estimating Equation (1) separately for East and West does not change the results qualitatively.

ogeneity of the bite.¹⁴ Additionally, measurement error or reversion to the mean will bias the estimate of α upwards in a mechanical sense.¹⁵ However, making the identifying assumption that this phenomenon does not change over time, one can still interpret β as the unbiased treatment effect of the minimum wage on regional wage growth. We will make that assumption in what follows.

As an alternative to Equation (1), we also estimate a model that allows for region-type-specific time trends:

$$\Delta \ln y_{it} = \mathbf{b}_{it}\alpha + (d \times \mathbf{b}_{it})\beta + \Delta \ln \mathbf{x}_{it}\gamma + \mu_i + \tau_t + \lambda_r I_r t + \epsilon_{it}, \quad (2)$$

where I_r is an indicator for region type r , which consists of nine categories between low-density rural areas and high-density core cities, so that $I_r t$ represents differential time trends.¹⁶ 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.

4.2 Neighbors

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 neighboring ones. Ignoring those effects can lead to 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 neighboring regions might be positive if labor 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 neighboring districts.

In contrast, if the minimum wage narrows the wage differential between districts (espe-

¹⁴We use a weaker assumption than strict exogeneity in Sec. 4.3.

¹⁵Dolton, Bondibene and Wadsworth [2012] discuss this problem as well.

¹⁶We use a classification provided by the BBSR. See BBSR [2012] for more information on how the region types are categorized.

cially for low-skilled workers), this decreases the incentive to commute long distances to more attractive jobs. Thus, there might be a negative effect on labor supply in the neighborhood 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 neighborhood effects—both in terms of general and minimum-wage-induced spillovers—to affect regional wage and employment growth rates, we augment the basic model as follows:

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

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 neighboring districts. The variables relevant for the indirect effects are marked with a bar on top and are calculated as the average over all neighbors. We specify “neighborhood” in two distinct ways. In the first variant, neighbors are other districts within a larger functional unit (*Raumordnungsregion*) that has been defined according to commuting flows and other characteristics (cf. Sec. 3). Second, we use a contiguity matrix to indicate districts with common borders.

Note that the model in Equation (3) implies that spatial spillover effects are local in nature. Thus, while it allows one district to affect its direct neighbor, we rule out that this has higher-order effects on the neighbors’ neighbors, the neighbors of those neighbors, 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 locally or 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.¹⁷

To test whether our assumption holds, we can make use of the fact that an OLS and a

¹⁷See [LeSage and Pace \[2009\]](#) for a discussion of local vs. global spillovers.

spatial error model (SEM) yield consistent estimates for Equation (3) if the model is correctly specified. While OLS assumes ϵ_{it} to be i.i.d., the SEM allows for spatial autocorrelation of the errors according to $\epsilon_{it} = \lambda \mathbf{W}\epsilon_{it} + \tilde{\epsilon}_{it}$, with $\tilde{\epsilon}_{it}$ being i.i.d. and assuming the spatial weighting matrix \mathbf{W} to be known. The parameter λ and the other coefficients are estimated using maximum-likelihood techniques.¹⁸ [Pace and LeSage \[2008\]](#) propose a spatial Hausman test for model comparison. Any significant discrepancies between OLS and SEM coefficients can be interpreted as evidence of misspecification. In that case, both models return inconsistent results, and a more general model (probably including a spatial lag of the dependent variable) might be more appropriate.

4.3 Border pairs

An approach that concentrates on problems stemming from spatial heterogeneity instead of spatial spillovers in this context was recently proposed by [Dube, Lester and Reich \[2010\]](#). 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 neighbors and assume that those form a better comparison group. We do think that the specifics of the construction sector make it vulnerable to the critique by [Dube, Lester and Reich \[2010\]](#). We thus redo our analysis applying their “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 consists 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 neighbors it has. In our case, this increases the number of observations more than five-fold (from 3,708 to 19,089).

¹⁸See [LeSage and Pace \[2009\]](#) for an overview of spatial regression models, including the SEM.

Minimum-wage effects are then estimated using the model

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

where the subscript p identifies a single pair of neighboring districts. The term τ_{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 later wage or employment growth rates.

One additional strength of the model outlined in Equation (4) is that it depends on an orthogonality assumption that is considerably weaker than the strict-exogeneity assumption used for fixed-effects panel estimation [Dube, Lester and Reich 2010]. 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 Equation (3), we again do not allow for effects from neighboring districts to affect the results. If there are strong external effects that run from one district to another, then the coefficients in Equation (4) are probably biased. However, in combining the strengths and weaknesses of the different approaches, we hope to be able to draw a consistent picture of the effect of introducing a minimum wage in the German construction sector.

5 Results

We discuss our estimation results by presenting the estimates for the wage regressions first, followed by those from the employment regressions.

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 two columns of Table 2. Using

the notation in Equation (1), the first two rows represent the coefficient vector α and the following two rows, the coefficient vector β . The estimated pre-treatment correlation between the bite and wage growth is positive for both East and West Germany.¹⁹

We estimate a significantly positive treatment effect in East Germany, which translates to an increase of the regional growth rate of wages of around 1.2 percentage points if the bite is increased from 0.22 to 0.30.²⁰ The coefficient for West Germany shows a negative treatment effect in Model (1). The effect becomes insignificantly different from zero, however, as soon as additional control variables for region-type-specific time trends are included (Model (2)).²¹

To investigate the effect of spillovers from neighboring districts, the last two columns in Table 2 report estimates for the models outlined in Equation (3). Model (3) defines close neighbors as those districts that lie within one labor-market region while Model (4) computes neighborhood averages over all districts that have a contiguous border with the observational unit. The results 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 neighboring districts does not alter our previous conclusion. Indirect treatment effects are both small and statistically insignificant.

To check whether a specification including global spillover effects might be more appropriate, we perform a spatial Hausman test for differences between OLS and SEM coefficients (cf. Sec. 4.2). To illustrate the procedure, Models (1) and (2) of Table 3 show the OLS and SEM estimates of a clearly misspecified model for wage growth, where we assume a row-standardized contiguity matrix for \mathbf{W} in the SEM. The p -value of the spatial Hausman test clearly rejects the null hypothesis that the coefficients of the OLS and the SEM model are equal. Thus, both models are potentially misspecified and bias drives the coefficient estimates apart. We perform the same test for the much richer Models (3) and (4), which derive from Equation (3) with neighbors again defined as sharing a contiguous border (see also Model (4) from Table 2). This time, we cannot reject the null hypothesis, which constitutes a necessary condition for the model to be consistent. We conclude that our models in Table 2 describe the data reasonably well,

¹⁹However, we note the potential simultaneity bias discussed in Sec. 4.

²⁰Using the distribution of regional bites in East Germany in 1996, this represents an increase of the bite by approximately one standard deviation.

²¹Note that a negative treatment effect—while counterintuitive—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 super-minimum wages are too much compensation for their workers. In this case, employers may either downgrade these wages or offer exactly the minimum wage to new employees.

making a spatial model including global spillovers unnecessary.²²

While the spatial Hausman test does not rule out the possibility of omitted-variable bias, the results are reassuring. The discrepancy between OLS and SEM estimates occurs in the presence of spatial dependence, and the bite variable shows a considerable amount of spatial correlation. One might expect that omitted variables confounding the treatment effect would also be spatially correlated, but this does not seem to be the case. The amount of residual spatial correlation, represented by the λ parameter in Table 3, is rather modest.

Finally, Table 4 depicts the main coefficients using the border-pair approach and the transformed data set. As an intermediate step, Model (1) only absorbs pair–period fixed effects from the data. In line with the previous results, we do not find any significant effect for West Germany but a significantly positive effect in East Germany, albeit weaker than before. Model (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 increases it considerably in East Germany. While still somewhat lower than in Table 2, it now lies in close proximity to the earlier results.

Overall, the results for the minimum-wage effect on wage growth at the regional level are very robust.²³ The effect of the introduction and subsequent increases of the minimum wage in West Germany is statistically indistinguishable from zero in most cases while there is a pronounced positive effect in East Germany. This is congruent to the descriptive statistics for East and West German districts before the minimum-wage introduction (cf. Figures 2 and 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

²²Observe that Model (3) in Table 2 is identical to a spatial Durbin model (SDM) including a spatial lag in both dependent and independent variables already, which is due to the special structure of the spatial weighting matrix. [Gibbons and Overman 2012] The direct and indirect effects from estimating Model (4) using an SDM are very close to the ones depicted in Table 2. Results will be provided upon request.

²³Indeed, the results are robust to the exclusion of regions that belong to the top and bottom 5 percent of the minimum-wage bite as well as to estimating the model using labor-market regions (*Raumordnungsregionen*) as the observational unit. We also repeated the regressions using a wage-gap measure (the average wage increase necessary within each district to attain full compliance) instead of the bite or using a shorter time interval (1994–1998). Those results can be provided upon request.

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.

5.2 Employment effects

Table 5 mirrors the analyses displayed in Table 2 but now uses district-wise employment growth rate 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 artificial bite. This substantiates our hypothesis that the strongly positive coefficients for α in Table 2 are not driven by structural differences but rather by a simultaneity bias.

Again, we find no significant treatment effect for West German districts. While the coefficient in Model (1) is still rather large (but statistically insignificant), it becomes negligible as soon as we add additional controls. In contrast, the estimated employment effect in East Germany is negative, large, and highly statistically significant. It starts off with a coefficient of -0.39 in the basic specification and falls slightly when adding controls for region-type time trends. The effect is even smaller when we look at our two specifications that add local spillover effects (-0.32 and -0.34).

Nevertheless, this still implies a large disemployment effect of the minimum wage in East German districts. Increasing the bite from 0.22 to 0.30 is associated with a decline of the regional growth rate of employment in construction by 2.6 to 3.1 percentage points, depending on the particular specification.²⁴ While this effect seems rather large at first glance, note that the construction industry experienced a deep recession during the observation period, starting in the mid-90s. As a back-of-the-envelope calculation, consider that the average growth rate of employment in East Germany was approximately -12 percent between 1996 and 1997. Setting the coefficient of the treatment effect to -0.35 —which is in the middle of the range of our estimates—and observing that the average bite was around 20 percent in 1996 yields a treatment effect of 7 percentage points. Thus, while employment contracted in all East German

²⁴Again, this shift in the bite represents an increase by roughly one standard deviation.

districts between 1996 and 1997, our estimates suggest that the minimum-wage introduction caused more than half of the overall decline. In this light, minimum wages may be especially dangerous in times of an economic downturn if they are set too high.²⁵

While allowing for spillover effects leads to slightly lower estimates of the direct effect as measured by β^D , we again have imprecise estimates of the indirect effects β^I . Both Models (3) and (4) show no statistically significant indications of negative spillover effects of a larger minimum-wage bite on neighboring counties.

The results for the spatial Hausman specification test are depicted in Table 6. Again, testing an obviously underspecified model leads to significant differences between OLS and SEM estimates. However, the coefficients of the richer specification are very close to each other. We therefore omit more general spatial formulations from our discussion.

To analyze whether the above results are still contaminated by spatial heterogeneity at the local level, Models (1) and (2) of Table 7 present estimates using the border-pair sample and using employment growth rate as the dependent variable. The estimates point toward minimum-wage effects of the same magnitude as in the basic model.

Analogous to the previous section, the coefficient of the treatment effect of the minimum-wage bite proves to be very robust with respect to different modeling approaches. We find no effect on employment growth at the district level in West Germany. This matches our findings for wage effects, since we do not expect to find employment effects without any measurable wage changes. However, we do find pronounced negative employment effects in East Germany, where a higher proportion of workers are directly affected by the minimum wage.

6 Discussion and conclusion

Any economic discussion of minimum wages these days is likely to elicit strong responses from economists, since the implications of recent evidence on its effects seem to turn the application of neoclassical price theory on labor markets on its head. However, this need not be the case. There are, naturally, competing theories that purport to describe the labor market. At the

²⁵Neumark, Salas and Wascher [2013] note that potential differential effects of a recession across regions may introduce a bias to the estimated minimum-wage effect in models with linear state-specific trends. Our results are robust with respect to the inclusion of region-type-specific trends (see Table 5) and also state-specific trends (not shown but available upon request).

end of the day, however, determining which theories more accurately reflect equilibrium and disequilibrium dynamics in the labor market, and are therefore more likely to survive the test of time, is a question of evidence.

To this end, we have undertaken the analysis of the effects of minimum wages in the German main construction sector on wage and employment growth rates. This is enriching to the literature because of the unique characteristics of the minimum wage in this sector. The most important elements of these are the fact that the minimum wage introduced was of a substantial magnitude, and that it was introduced during a period of economic contraction. 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. Finally, we contribute to the growing international literature on this issue.

Our results indicate that wage growth in East Germany was positively affected by the minimum wage while the West German wage growth rate was not affected at all. In terms of employment growth rates, we do not find any adverse effect in West Germany. The contraction in the employment growth rate, however, is rather stark in the East, where the bite of the minimum wage was relatively high.

We focus our attention on the minimum-wage effects in regional labor markets. We control for spatial spillovers and regional heterogeneity, two aspects which previous research has largely ignored. While some of our results are consistent with previous work, there are also important differences. Contrary to [Apel et al. \[2012\]](#), we do find negative employment effects in East Germany. We also note that the previous result which generated the most controversy—that of a positive employment effect in West Germany found by [König and Möller \[2009\]](#)—cannot be supported by the present study. Although some evidence exists elsewhere suggesting that minimum wages can induce marginal employment growth, we do not find this to be the case for the construction sector in Germany.

The evidence presented here is consistent with the view that a moderate minimum wage might have negligible effects, but that this can easily 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.

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 as well as the self-employed. Self-employment in construction rose substantially during the observation period in East Germany despite the strong decline in overall employment.²⁶ One might speculate that part of this increase was driven by former employees who registered themselves as self-employed to avoid compliance with the CBA. Unfortunately, we are unable to take this into account because of data restrictions.

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 labor market itself [Gittings and Schmutte 2012]. Bachmann, König and Schaffner [2012] study the issue of turnover in the German construction sector. They find that both accessions and separations increased in East Germany due to the minimum wage, but that job-to-job transitions declined, which they attribute to the resulting wage compression. Our results indicate that the effect on separations must have been most important, since we find net disemployment effects.

Two other caveats are in order. First, we have not considered the demand elasticity for the products and services of the construction sector in this study. Depending on this elasticity, a change in the minimum wage may manifest itself in higher output prices or lower profit margins.²⁷ Second, 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.

Although the original motivation for a minimum wage in Germany’s construction sector was anti-competitive instead of anti-poverty, more recent political discussions about this issue in general have made references to subsistence wages and social safety nets. While we advise

²⁶Between 1995 and 2000, the number of proprietors increased by nearly 70 percent [ELVIRA 2013].

²⁷The effect of minimum wages on competition, prices, and profit margins is a less-studied area within the minimum-wage literature. Bachmann, Bauer and Kröger [2012] deal with these issues for the German case, although not specifically for the main construction industry.

against directly carrying over our results to the discussion about adopting a national minimum wage spanning all sectors—construction is just one sector, a rather special one, and the circumstances during the study period were unusual—we still see our findings as a cautionary tale, reminding us that minimum-wage legislation has the inherent potential to backfire. Indeed, in the present case, we find that a strongly binding minimum wage in East Germany led to rather large negative effects on employment growth in that region. While we did not observe a similar effect in the West, this is probably because the minimum wage there was mostly not or only slightly binding.

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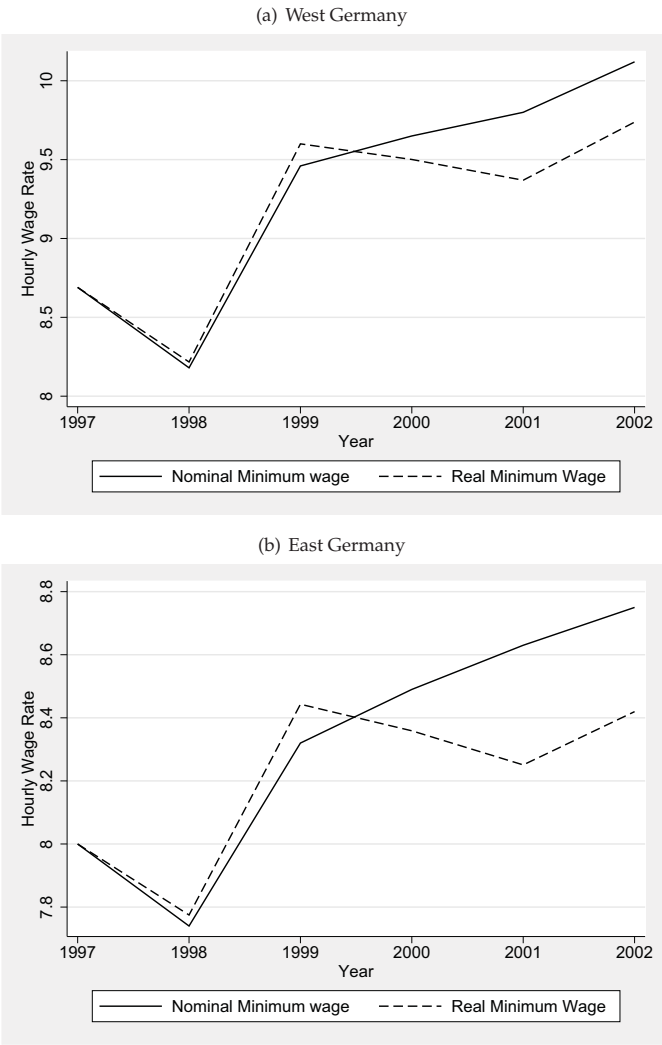
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Figures

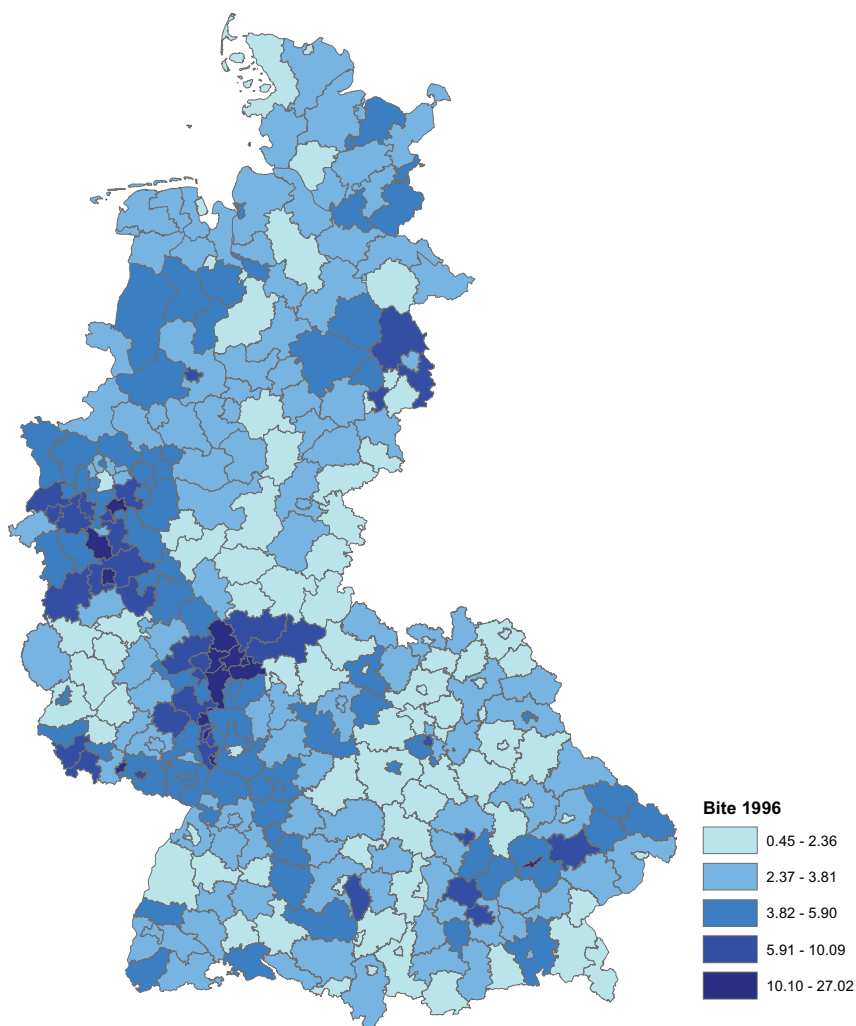
FIGURE 1
REAL AND NOMINAL MINIMUM WAGES, 1997–2002



Note: The nominal minimum wage has been deflated with the producer price index obtained from the Federal Statistical Office.

Source: Authors' calculations based on the IEB.

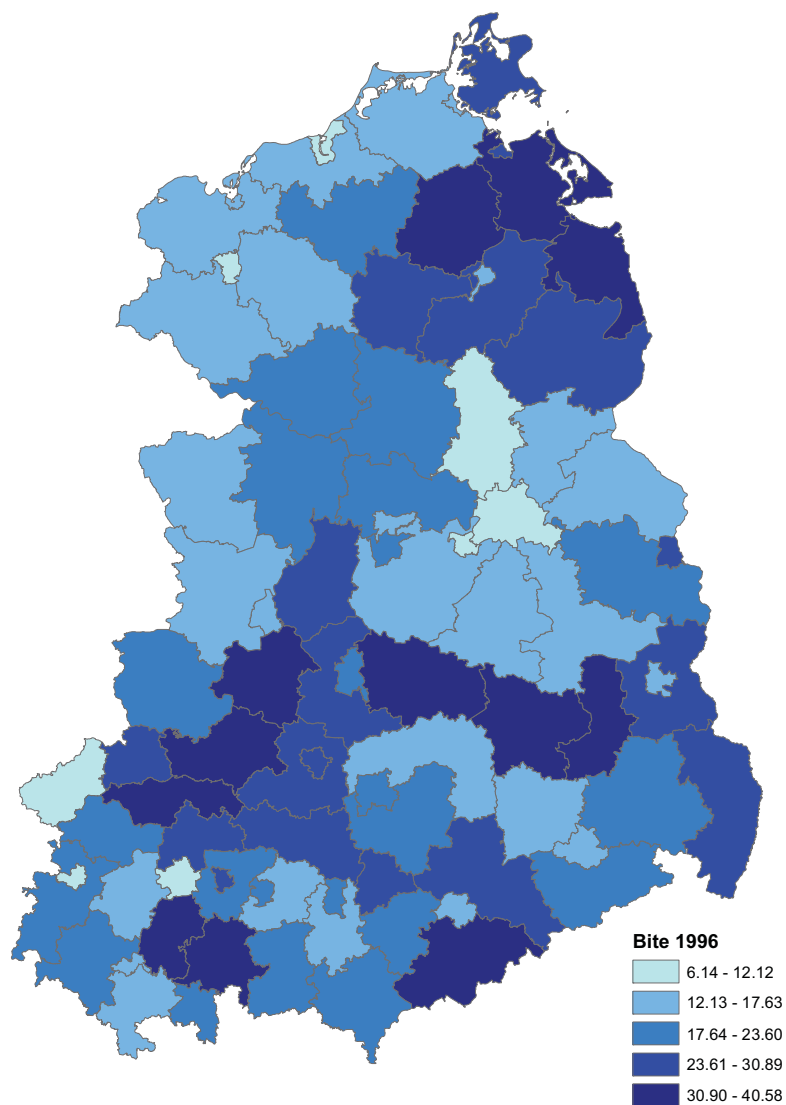
FIGURE 2
SPATIAL DISTRIBUTION OF THE MINIMUM-WAGE BITE IN 1996 — WEST GERMANY



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.

FIGURE 3
SPATIAL DISTRIBUTION OF THE MINIMUM-WAGE BITE IN 1996 — EAST GERMANY



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.

Tables

TABLE 1
MINIMUM-WAGE BITE, WAGE GROWTH, AND EMPLOYMENT GROWTH

| Year | East Germany | | | West Germany | | |
|------|--------------|-------------|-------------------|--------------|-------------|-------------------|
| | Bite | Wage growth | Employment growth | Bite | Wage growth | Employment growth |
| 1993 | 19.74 | — | — | 3.69 | — | — |
| 1994 | 21.97 | 3.49 | 9.68 | 3.86 | 2.44 | −0.85 |
| 1995 | 22.37 | 2.18 | 1.56 | 3.87 | 1.69 | −3.92 |
| 1996 | 21.60 | 1.79 | −7.64 | 3.95 | 0.08 | −8.78 |
| 1997 | 11.69 | 1.61 | −11.91 | 3.18 | −0.68 | −8.16 |
| 1998 | 11.06 | −0.67 | −15.21 | 3.18 | −0.68 | −8.16 |
| 1999 | 22.79 | 0.38 | −2.83 | 6.24 | 1.46 | 0.37 |
| 2000 | 20.42 | 1.21 | −12.91 | 6.66 | 0.59 | −2.91 |
| 2001 | 17.79 | 2.06 | −17.42 | 6.68 | 1.50 | −8.65 |
| 2002 | 15.83 | 2.08 | −16.21 | 7.22 | 1.34 | −8.87 |

Note: All numbers are in percent. Growth rates are calculated annually. The bites for the years 1993–1995 are artificial because the minimum wage was only introduced in January 1997. The artificial bite is calculated by deflating the minimum wage of 1997 with the average wage growth, separately for East and West Germany.

Source: Authors' calculations based on the IEB.

TABLE 2
EFFECT ON MEAN WAGE GROWTH

| | (1) | (2) | (3) | (4) |
|--|---------------------|---------------------|---------------------|---------------------|
| Artificial bite (West) | 0.333*** (0.043) | 0.331*** (0.044) | 0.386*** (0.053) | 0.328*** (0.044) |
| Artificial bite (East) | 0.125*** (0.024) | 0.131*** (0.024) | 0.149*** (0.030) | 0.128*** (0.024) |
| Treatment effect (West) | -0.054** (0.024) | -0.041 (0.027) | -0.072* (0.037) | -0.021 (0.029) |
| Treatment effect (East) | 0.148*** (0.022) | 0.147*** (0.021) | 0.144*** (0.025) | 0.156*** (0.022) |
| Wage growth (other industries) | 0.005 (0.016) | 0.004 (0.016) | -0.001 (0.018) | 0.006 (0.017) |
| Employment growth (other industries) | -0.009** (0.004) | -0.009** (0.004) | -0.009** (0.004) | -0.009** (0.004) |
| Artificial bite (West), neighbors | | | -0.173** (0.078) | -0.051 (0.049) |
| Artificial bite (East), neighbors | | | -0.053 (0.040) | -0.006 (0.033) |
| Treatment effect (West), neighbors | | | 0.080 (0.064) | -0.042 (0.037) |
| Treatment effect (East), neighbors | | | 0.011 (0.036) | -0.032 (0.029) |
| Wage growth (other industries), neighbors | | | 0.034 (0.033) | -0.014 (0.032) |
| Employment growth (other industries), neighbors | | | 0.001 (0.011) | -0.002 (0.010) |
| District fixed effects | Yes | Yes | Yes | Yes |
| Year indicators | Yes | Yes | Yes | Yes |
| District-type-specific trends | No | Yes | Yes | Yes |
| Wooldridge test for serial correlation (p -value) | 0.434 | 0.409 | 0.412 | 0.475 |
| Within R^2 | 0.395 | 0.399 | 0.401 | 0.400 |
| Observations | 3708 | 3708 | 3708 | 3708 |

Notes: Model (3) defines neighbors as being in the same labor-market region and Model (4) defines neighbors as sharing a common border (cf. Sec. 4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses and clustered at the district level.

Source: Authors' calculations based on the IEB.

TABLE 3
SPATIAL ERROR MODELS FOR WAGES

| | (1) OLS | (2) SEM | (3) OLS | (4) SEM |
|---|----------------------|----------------------|---------------------|---------------------|
| Artificial bite (West) | 0.221*** (0.033) | 0.279*** (0.032) | 0.328*** (0.032) | 0.330*** (0.031) |
| Artificial bite (East) | 0.140*** (0.011) | 0.144*** (0.012) | 0.128*** (0.015) | 0.129*** (0.014) |
| Treatment effect (West) | 0.026 (0.028) | 0.014 (0.028) | -0.021 (0.029) | -0.022 (0.027) |
| Treatment effect (East) | 0.032*** (0.008) | 0.062*** (0.010) | 0.156*** (0.018) | 0.155*** (0.017) |
| Wage growth (other industries) | | | 0.006 (0.013) | 0.006 (0.012) |
| Employment growth (other industries) | | | -0.009** (0.004) | -0.009** (0.004) |
| Artificial bite (West), neighbors | | | -0.051 (0.050) | -0.046 (0.048) |
| Artificial bite (East), neighbors | | | -0.006 (0.024) | -0.007 (0.023) |
| Treatment effect (West), neighbors | | | -0.042 (0.041) | -0.046 (0.039) |
| Treatment effect (East), neighbors | | | -0.032 (0.027) | -0.030 (0.026) |
| Wage growth (other industries), neighbors | | | -0.014 (0.027) | -0.017 (0.026) |
| Employment growth (other industries), neighbors | | | -0.002 (0.008) | -0.002 (0.008) |
| Post-treatment dummy | -0.011*** (0.001) | -0.013*** (0.001) | | |
| Spatial autocorrelation (λ) | | 0.474*** (0.01) | | 0.094*** (0.025) |
| District fixed effects | Yes | Yes | Yes | Yes |
| Year indicators | No | No | Yes | Yes |
| District-type-specific trends | No | No | Yes | Yes |
| Spatial Hausman test (p -value) | 0.000 | | 1.000 | |
| Within R^2 | 0.167 | 0.163 | 0.400 | 0.400 |
| Observations | 3708 | 3708 | 3708 | 3708 |

Notes: Neighbors are defined as sharing a common border. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses. The SEM models are estimated employing the user-written routine `xsmle` for Stata.

Source: Authors' calculations based on the IEB.

TABLE 4
BORDER-APPROACH MODELS FOR WAGES

| | (1) | (2) |
|--------------------------------------|---------------------|---------------------|
| Artificial bite (West) | 0.065*** (0.023) | 0.323*** (0.034) |
| Artificial bite (East) | 0.054*** (0.010) | 0.149*** (0.022) |
| Treatment effect (West) | −0.004 (0.022) | −0.019 (0.032) |
| Treatment effect (East) | 0.031** (0.013) | 0.108*** (0.013) |
| Wage growth (other industries) | 0.031* (0.017) | 0.025 (0.016) |
| Employment growth (other industries) | −0.006 (0.004) | −0.010** (0.004) |
| Pair-period fixed effects | Yes | Yes |
| District fixed effects | No | Yes |
| Adjusted R^2 | 0.373 | 0.500 |
| Observations | 19089 | 19089 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses and clustered at the district level. Column (2) employs the user-written routine `reg2hdfe` for Stata by [Guimaraes and Portugal \[2010\]](#).
Source: Authors' calculations based on the IEB.

TABLE 5
EFFECT ON EMPLOYMENT GROWTH

| | (1) | (2) | (3) | (4) |
|--|----------------------|----------------------|----------------------|----------------------|
| Artificial bite (West) | -0.152 (0.150) | -0.105 (0.151) | -0.261 (0.187) | -0.177 (0.154) |
| Artificial bite (East) | 0.012 (0.111) | -0.026 (0.110) | -0.048 (0.138) | -0.041 (0.111) |
| Treatment effect (West) | 0.130 (0.131) | 0.005 (0.130) | 0.044 (0.192) | 0.078 (0.150) |
| Treatment effect (East) | -0.385*** (0.093) | -0.368*** (0.093) | -0.319*** (0.112) | -0.340*** (0.102) |
| Wage growth (other industries) | -0.023 (0.066) | -0.017 (0.065) | 0.044 (0.080) | -0.025 (0.065) |
| Employment growth (other industries) | 0.030 (0.020) | 0.028 (0.020) | 0.009 (0.022) | 0.028 (0.020) |
| Artificial bite (West), neighbors | | | 0.595* (0.361) | 0.560* (0.300) |
| Artificial bite (East), neighbors | | | 0.074 (0.198) | 0.143 (0.179) |
| Treatment effect (West), neighbors | | | -0.156 (0.294) | -0.369 (0.251) |
| Treatment effect (East), neighbors | | | -0.129 (0.188) | -0.085 (0.160) |
| Wage growth (other industries), neighbors | | | -0.318* (0.167) | 0.035 (0.185) |
| Employment growth (other industries), neighbors | | | 0.089* (0.049) | 0.027 (0.047) |
| District fixed effects | Yes | Yes | Yes | Yes |
| Year indicators | Yes | Yes | Yes | Yes |
| District-type-specific trends | No | Yes | Yes | Yes |
| Wooldridge test for serial correlation (p -value) | 0.463 | 0.452 | 0.435 | 0.409 |
| Within R^2 | 0.379 | 0.382 | 0.385 | 0.384 |
| Observations | 3708 | 3708 | 3708 | 3708 |

Notes: Model (3) defines neighbors as being in the same labor-market region and Model (4) defines neighbors as sharing a common border (cf. Sec. 4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses and clustered at the district level.

Source: Authors' calculations based on the IEB.

TABLE 6
SPATIAL ERROR MODELS FOR EMPLOYMENT

| | (1) OLS | (2) SEM | (3) OLS | (4) SEM |
|---|----------------------|----------------------|----------------------|----------------------|
| Artificial bite (West) | -0.767*** (0.156) | -0.504*** (0.150) | -0.177 (0.156) | -0.182 (0.147) |
| Artificial bite (East) | 0.027 (0.053) | -0.006 (0.057) | -0.041 (0.070) | -0.038 (0.066) |
| Treatment effect (West) | 0.397*** (0.134) | 0.285** (0.131) | 0.078 (0.138) | 0.076 (0.130) |
| Treatment effect (East) | -0.586*** (0.039) | -0.525*** (0.048) | -0.340*** (0.086) | -0.346*** (0.080) |
| Wage growth (other industries) | | | -0.025 (0.064) | -0.017 (0.060) |
| Employment growth (other industries) | | | 0.028 (0.019) | 0.027 (0.017) |
| Artificial bite (West), neighbors | | | 0.560** (0.240) | 0.535** (0.229) |
| Artificial bite (East), neighbors | | | 0.143 (0.118) | 0.161 (0.112) |
| Treatment effect (West), neighbors | | | -0.369* (0.195) | -0.353* (0.186) |
| Treatment effect (East), neighbors | | | -0.085 (0.129) | -0.085 (0.123) |
| Wage growth (other industries), neighbors | | | 0.035 (0.130) | 0.048 (0.124) |
| Employment growth (other industries), neighbors | | | 0.027 (0.039) | 0.021 (0.037) |
| Post-treatment dummy | -0.027*** (0.006) | -0.026*** (0.007) | | |
| Spatial autocorrelation (λ) | | 0.460*** (0.018) | | 0.090*** (0.025) |
| District fixed effects | Yes | Yes | Yes | Yes |
| Year indicators | No | No | Yes | Yes |
| District-type-specific trends | No | No | Yes | Yes |
| Spatial Hausman test (p -value) | | 0.036 | | 1.000 |
| Within R^2 | 0.159 | 0.158 | 0.384 | 0.384 |
| Observations | 3708 | 3708 | 3708 | 3708 |

Notes: Neighbors are defined as sharing a common border. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses. The SEM models are estimated employing the user-written routine `xsmle` for Stata.
Source: Authors' calculations based on the IEB.

TABLE 7
BORDER-APPROACH MODELS FOR EMPLOYMENT

| | (1) | (2) |
|--------------------------------------|----------------------|----------------------|
| Artificial bite (West) | -0.089 (0.115) | -0.178 (0.152) |
| Artificial bite (East) | 0.155*** (0.053) | -0.017 (0.091) |
| Treatment effect (West) | -0.000 (0.117) | 0.088 (0.124) |
| Treatment effect (East) | -0.378*** (0.061) | -0.388*** (0.059) |
| Wage growth (other industries) | 0.003 (0.060) | -0.027 (0.063) |
| Employment growth (other industries) | 0.043** (0.020) | 0.026 (0.020) |
| Pair-period fixed effects | Yes | Yes |
| District fixed effects | No | Yes |
| Adjusted R^2 | 0.439 | 0.530 |
| Observations | 19089 | 19089 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are enclosed in parentheses and clustered at the district level. Column (2) employs the user-written routine `reg2hdfe` for Stata by [Guimaraes and Portugal \[2010\]](#).
Source: Authors' calculations based on the IEB.