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Labor Market Frictions and Spillover Effects from Publicly Announced Sectoral Minimum Wages

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Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

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Gökay Demir[1]

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Abstract

I analyze the spillover effects of publicly announced sectoral minimum wages in Germany. My identification strategy exploits exposure to sectoral minimum wages across workers and industries outside the minimum wage sector in a triple differences estimation. Sub-minimum wage workers in related industries outside of the minimum wage sector experience an increase in wages, job-to-job transitions, and reallocation from low-paying to high-paying establishments after the public announcement of Germany's first sectoral minimum wage. The reduction of information frictions, rather than the strategic interaction of employers, appears to be the main mechanism for these effects. When examining the spillover effects of other sectoral minimum wages from various contexts, I only discover positive spillover effects on sub-minimum wage workers in related industries outside the minimum wage sectors if the typical employment relationship in the minimum wage sector is comparable to that of the workers in my sample.

JEL-Code: J31, J38, J42, J62

Keywords: Spillover; labor market frictions; minimum wages; information frictions

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1 Introduction

One of the most striking findings in labor economics is the coexistence of good (high-wage) and bad (low-wage) jobs. Firms differ in the wages they pay to equally skilled workers in similar jobs (Slichter, 1950; Abowd et al., 1999; Card et al., 2013). Although the continuing existence of bad jobs can be generally explained by labor market frictions, it is an open question what kind of labor market frictions are at work here. One way to reveal the presence and types of labor market frictions that are important for the existence of bad jobs is using wage and information shocks on the potential outside options of workers. Publicly announced sectoral minimum wages are such wage and information shocks to workers with wages below the minimum wage in similar jobs outside the targeted sector and may therefore have spillover effects on these workers. If publicly announced sectoral minimum wages result in wage increases in other sectors merely because of the strategic responses of firms in these other sectors to the minimum wage, legislating additional sectoral minimum wages might be a good policy tool to raise wages of bad jobs. However, if the main mechanism for spillovers is public disclosure of the sectoral minimum wage and sharing of relevant wage information for workers outside the minimum wage sector, unsolicited and widely publicized wage information on specific sectors might be a better approach.

Although examining spillover effects from sectoral minimum wages on firms and workers outside the targeted sector would contribute significantly to our understanding of the existence, types, and consequences of labor market frictions, the empirical evidence on spillover effects and its mechanisms is scarce (Staiger et al., 2010; Derenoncourt et al., 2021; Bassier, 2021).[1] Three challenges have prevented researchers from studying spillover effects. First, there was little theoretical and empirical interest on labor market frictions until recently (Manning, 2021; Sokolova and Sorensen, 2021; Card, 2022).[2] Second, large linked-employer-employee data were not available, which would be necessary to uncover relevant mechanisms of spillover effects. Third, identification challenges have impeded researchers from examining spillover effects. It is difficult to find exogenous wage increases targeted at specific firms or sectors, identify groups of economic actors who are subject to their spillover effects, and then find a proper control group for them.

In this paper, I study the spillover effects of publicly announced sectoral minimum wages in Germany on workers outside the targeted sector in similar jobs. Because of its relatively large size and the fact that it was the first sectoral minimum wage in Germany, I focus on the spillover effects from the main construction sector minimum wage, which was publicly announced in 1996 and introduced in 1997. This minimum wage was introduced to curb wage competition from the posting practices of foreign firms within the European Union and was set below the entry-level wages of firms covered by

[1] A broader literature examines vertical spillover effects of minimum wages on the wage distribution within a targeted sector, state, or country (Gramlich et al., 1976; Grossman, 1983; Lee, 1999; Neumark et al., 2004; Autor et al., 2016; Cengiz et al., 2019; Gopalan et al., 2021; Fortin et al., 2021; Gregory and Zierahn, 2022).

[2] Robinson (1933) was the first to study monopsony power in the labor market. However, her ideas did not catch on for reasons laid out in Card (2022).

collective bargaining agreements. Moreover, it had little or no effect on employment within the main construction sector (König and Möller, 2009; Möller et al., 2011; Möller, 2012; Frings, 2013; Vom Berge and Frings, 2020). Spillovers resulting from reallocation from the main construction sector to other sectors were consequently minimal, making spillovers resulting from strategic responses or information transmissions more observable. I am able to address earlier challenges in the literature by utilizing high-quality administrative linked-employer-employee data, a triple differences design, and the most recent theoretical developments on frictional labor markets.

The triple differences design exploits three dimensions of comparison. First, I compare sub-minimum wage workers to workers with higher wages outside of the main construction sector. Second, spillover effects are particularly relevant in industries[3] with sub-minimum wage employees for whom the minimum wage sector represents an outside option. I classify these "outside option industries" as industries which had high outflows of low-wage workers to the minimum wage sector. I compare outside option industries to industries which had low outflows of low-wage workers to the minimum wage sector, referred to as "non-outside option industries". I assume that the minimum wage sector and outside option industries share one common labor market with similar tasks and transferable skills. Non-outside option industries are outside this common labor market and can therefore be used as a proxy for the counterfactual scenario, i.e., the absence of the minimum wage introduction. Third, I compare the labor market outcomes of sub-minimum wage workers to workers with higher wages in outside option versus non-outside option industries before and after the public announcement of the minimum wage.

I find that the main construction sector minimum wage led to an average increase in wage growth of 2.1% and an average increase in job-to-job transitions of 3.7 percentage points for sub-minimum wage workers in outside option industries. The wage spillover effects are about one-third of the wage effects within the main construction sector, which I estimate using the same data and identification strategy. The results are robust to controlling for region-specific and industry-specific shocks, and are not driven by an increase in establishment closure. In addition, the results are robust to different definitions of the key independent variables which indicate the exposure to the main construction sector minimum wage. For example, by using occupation flows instead of industry flows to define outside and non-outside options, I account for the possibility that occupations, not industries, form one labor market. I additionally analyze the spillover effects at the establishment-level.[4] Using a similar triple differences specification, I find that more exposed establishments on average increased mean wages and lost workers. The effects for establishments appear much later than the effects for workers, which suggests that worker behavior, not establishment behavior, is driving the spillover effects.

One prominent channel to explain these spillover effects are models of strategic spatial complemen-

[3] "Industries" refer to individual 3-digit entries in the German Classification of Economic Activities, while "sectors" refer to multiple (3-digit or 5-digit) industries that are collectively covered by a minimum wage regulation.

[4] Because I only observe establishments and not firms in the data, I refer to establishments when discussing the empirical analysis and firms when discussing theoretical and institutional considerations.

tarity (Bhaskar and To, 1999; Bhaskar et al., 2002; Staiger et al., 2010). I use a simple version of these theoretical models in which firms respond to wage changes from other firms to retain workers, with the intensity of firms' reactions depending on their geographic proximity to other firms. By definition, outside option industries are already "close" to the main construction sector in terms of task similarity and transferability of skills. Therefore, I assume that only geographic proximity is relevant for my empirical tests of this model to be conclusive on strategic complementarity. If strategic complementarity were at play, firms that are closer together would be more responsive to one another's wage changes and the wage spillovers should be driven by remaining within the same establishment or moving to the main construction sector. However, I find that the intensity of spillover effects did not increase with geographic proximity to the main construction sector and that wage spillover effects were mainly driven by switching establishments but not moving to the main construction sector. To test whether the reduction of information frictions can explain the results, I use the simple equilibrium model in Jäger et al. (2022). In this model, workers can have information costs resulting in biased beliefs about their outside options in the labor market, no incentive to search for jobs, and receiving a marked-down wage while staying in low-paying firms. Consistent with an information shock story, I find an increase in wage spillovers and job-to-job transitions right at the public announcement and before the introduction of the minimum wage in 1996, reallocation from low-paying to high-paying establishments, and a larger wage response for workers with arguably higher information costs about their outside options in the labor market.

To understand the broader economic contexts favoring positive spillover effects, I use the same triple differences strategy to examine the wage spillover effects from other sectoral minimum wages in Germany. I find that only minimum wages in sectors with a relatively high share of full-time workers had positive wage spillover effects for sub-minimum wage workers in outside option industries, while minimum wage sectors with a relatively high share of part-time female workers had negative wage spillover effects. Since I concentrate on full-time employees in my sample, this suggests that positive wage spillover effects only occur if the workers in the minimum wage sector are in a comparable employment contract to the workers in my sample.

My findings imply that information frictions play a significant role for the coexistence of good and bad jobs. The public announcement of sectoral minimum wages can result in unanticipated benefits from the dissemination of relevant pay information for workers doing similar jobs. Therefore, providing unsolicited and publicly published wage information would be the optimal course of action to break the coexistence of good and bad jobs, reallocate workers from less productive to more productive establishments, and thereby raise the welfare of the economy as a whole.

This paper contributes to an emerging literature on cross-employer spillover effects of wage-setting changes at major employers in three ways (Staiger et al., 2010; Derenoncourt et al., 2021; Bassier, 2021).[5]

[5]Other related papers include second-order wage spillover effects of decentralized wage bargaining for teachers (Willén, 2021), wage spillovers across establishments within the same firm (Hjort et al., 2020), and market-level effects of privatization

First, I am able to analyze the supply side spillover response to sectoral minimum wages using social security administrative data, which reveals reallocation effects that were previously obscured in firm-level studies. Second, this paper proposes a new research design to study individual-level spillover effects using a triple-differences strategy. Third, the paper uses different theoretical models to test for the mechanisms of the spillover effects.

My paper is related to three other strands of the literature. First, a growing literature studies the role of workers' outside options and their impact on wages (Beaudry et al., 2012; Caldwell and Danieli, 2018; Caldwell and Harmon, 2019; Schubert et al., 2021). Methodologically, I use this literature to define industries for which the minimum wage sectors are potential outside options. Empirically, I add to this literature by showing that after minimum wages were publicly announced in their potential outside options, employees move to better paying establishments and experience positive wage spillovers. Second, this paper relates to the literature on the role of labor market institutions in disrupting the coexistence of good and bad jobs (Acemoglu, 2001; Dustmann et al., 2022). I show that labor market institutions can also have a signaling effect that goes far beyond the actual target group. Third, this paper relates to the literature on pay transparency (Card et al., 2012; Mas, 2017; Baker et al., 2019; Perez-Truglia, 2020; Cullen and Perez-Truglia, 2022; Roussille, 2022; Brütt and Yuan, 2022) and fairness concerns at the workplace (Breza et al., 2018; Dube et al., 2019). I add to this literature by showing that wage transparency can be particularly effective in equalizing wages when it is unsolicited and published prominently in the media.

This paper is structured as follows. Section 2 provides an overview of the institutional setting for sectoral minimum wages in Germany. Section 3 presents the linked-employer-employee data and the sampling procedure. In Section 4, I detail the empirical strategy to estimate spillover effects. Section 5 presents the main results, robustness checks, theoretical model details and mechanisms. Section 6 discusses the findings and concludes.

2 Institutional Background

Due to European trade integration, sectors in Germany that had been largely spared from international trade up to the beginning of the 1990's were then facing fierce wage competition. European firms could send workers to another EU member state on the terms and conditions of its country of domicile, while domestic firms had to continue to comply with internal regulations (Bosch and Zühlke-Robinet, 2000; Muñoz, 2022). The main construction sector in particular was affected by foreign wage competition. Although there were of course beneficiaries from cheaper construction products in Germany, an opposition

of state-owned enterprises (Arnold, 2022). Furthermore, an older literature analyzes the spillover effects of unionization on non-union wages in the same industry due to a "threat effect" or a labor supply shock from workers of the unionized firms reallocating to the non-unionized firms (Lewis, 1963; Freeman and Medoff, 1981; Moore et al., 1985; Podgursky, 1986; Neumark and Wachter, 1995; Fortin et al., 2021; Farber et al., 2021).

to the European posting practice formed relatively quickly with the demand to limit the market opening in order to prevent low-wage competition within the main construction sector.

To curb wage competition within the main construction sector and set a minimum wage in this sector, collective bargaining agreements could be declared generally binding under Section 5 of the Collective Bargaining Agreement Act[6]. Sectoral minimum wages can be extended to foreign firms through the Posting of Workers Law which came into force in March 1996.

The main construction sector already had a relatively high collective bargaining coverage in 1995 of approximately 80% in West Germany and 40% in East Germany (Möller et al., 2011). While the unions proposed setting the minimum wage at the level of the lowest wage group of the existing collective bargaining agreement (10.35 Euro), construction employers demanded the introduction of a new wage level below the existing one (Eichhorst, 2005). The two sides (trade union and employer association) agreed on a minimum hourly wage of 8.69 Euro in West Germany and 8.00 Euro in East Germany, which came into force at the beginning of 1997. In mid-1997, the minimum wage in the main construction sector was lowered slightly to 8.18 Euro in West and 7.74 Euro in East Germany and raised again to 9.46 Euro in West and 8.32 Euro in East Germany in mid-1999.

The federal ministry of labor and social affairs is obliged to publish sectoral minimum wages in the German Federal Bulletin (*Bundesanzeiger*) and did so for the main construction sector on November 12, 1996 in the German Federal Bulletin of November 16, 1996, No. 215, p. 12102. The introduction of the main construction sector minimum wage also received large media attention. For example, Germany's most watched news program, the *Tagesschau* (Zubayr and Gerhard, 2005), reported on it on November 12, 1996.[7]

Taking stock, two features of the sectoral minimum wage in the main construction sector make it particularly valuable for this paper. First, the main construction sector minimum wage was introduced because of within-sector concerns, making it an exogenous variation in outside wages for workers and firms not in the minimum wage sector. Second, public attention to the minimum wage (e.g., through news broadcasts) is likely to represent an information shock that is different for workers who are more likely to be aware of the already relatively high wages in the main construction sector, for firms covered by collective bargaining agreements, than for workers who have fewer points of contact with the main construction sector. I am able to exploit these potential information differences in the mechanisms Section 5.4.

In the years following the introduction of the minimum wage in the main construction sector, other sec-

[6]§5 of the Collective Bargaining Agreement Act (*Tarifvertragsgesetz*) states that on request of the collective bargaining parties a collective agreement can be declared generally binding by the federal ministry of labor and social affairs (BMAS). This law requires an agreement of the majority of three representatives of the employer association and three representatives of the trade union to pass the general binding declaration. Furthermore, the general binding declaration has to be of public interest and until 2014, the employers bound by the collective agreement must at least employ 50% of the workers in the scope of the collective agreement.

[7]https://www.tagesschau.de/multimedia/video/video-229995.html around minute 4:55.

toral minimum wages were also introduced.[8] The Temporary Work Law (*Arbeitnehmerüberlassungsgesetz*) is another piece of legislation which, since changes in the law in 2011, allows enacting a minimum wage in the temporary work sector to prevent misuse of temporary work. Table 1 gives an overview of all sectoral minimum wages in Germany that were enacted using the Collective Bargaining Agreement Act, Posting of Workers Law, Temporary Work Law, or combinations of these three pieces of legislation, and whose spillover effects I study in this paper.[9]

3 Data

3.1 The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018

The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018, together with additional establishment-level information from the Establishment History Panel (BHP), provides high quality administrative variables. By using the information on establishments, detailed industry codes, wages and employment biographies of individuals, this data allows me to convincingly estimate spillover effects of sectoral minimum wages in Germany. The SIEED and BHP are provided by the Research Data Centre of the BA at the IAB. Schmidtlein et al. (2020) provide a detailed description of the SIEED.

The main data source of the SIEED is the Employee History (Beschäftigtenhistorik - BeH). The BeH in turn is based on the integrated notification procedure for health, pension and unemployment insurance. This notification procedure started on 1 January 1973 (1 January 1991 in East Germany) and made it mandatory for employers to report information on all of their employees covered by social security to the responsible social security agencies at least once a year. Misreporting is a legal offense. For further details on the notification procedure see Bender et al. (1996); Wermter and Cramer (1988). Because the BeH only covers employees subject to social security, civil servants and self-employed individuals or unemployment spells are not included in it.

The SIEED is constructed in a three-step procedure. A 1.5% random sample of the population of establishments in the BeH is taken in the first step. All individuals who worked at least one day in one of these establishments between 1975 and 2018 are drawn in the second step. The full employment biographies for these individuals are taken from the BeH in the third step. The employment biographies span the years 1975–2018 and cover employment spells in both sampled and non-sampled establishments. Due to the sampling procedure, the SIEED is representative for establishments in Germany but not for persons. The data contains information on the exact (to the day) spell time period, person and establishment identifiers, personal information such as age, gender, nationality, place of residence, education,

[8]See e.g. Popp (2021) for an overview of prerequisites for all sectoral minimum wages in Germany. For the context of this study it is only important that sectoral minimum wages were exogenous from the perspective of workers and firms outside the targeted sectors.

[9]The sectoral minimum wages in industrial laundries (introduced 2009), specialized hard coal mining (introduced 2009), public training services (introduced 2012) and money and value services (introduced 2015) cannot be studied as the 5-digit industry classification that I use in this paper is not granular enough to identify these sectors.

detailed occupation codes, the daily wage[10] and type of job (e.g., part-time vs. full-time). To this data, I merge additional establishment-level information on the place of work and detailed industry codes from the BHP.

3.2 Sample Construction

Sectoral minimum wages are hourly wages. A drawback of the SIEED is that it does not record an employees' hours worked, which in turn means that exact hourly wages are unknown. To ensure comparability between daily wage rates as an outcome variable and to calculate hourly wages for the definition of treated workers or establishments, I proceed in two steps. First, I focus on full-time workers who in general have similar working hours. Second, I set the weekly working hours to 40 hours and then use the daily wages and the imputed weekly working hours to calculate the nominal hourly wages. Using the consumer price index of the Federal Statistical Office, I convert gross daily wages into real wages when using wages as an outcome variable in the analysis.

To identify the national minimum wage sectors, I use the 1973 3-digit, 1993 5-digit, 2003 5-digit and 2008 5-digit German Classification of Economic Activities (WZ). The first four digits in the WZ are based on the Statistical Classification of Economic Activities in the European Community (NACE). Table A1 summarizes the industry codes that I use to identify and classify the minimum wage industries. If an establishment has one of the industry codes listed in Table A1 during the observation period, I classify it as belonging to the respective minimum wage sector. I use the evaluation studies on sectoral minimum wages in Germany, which were commissioned by the Federal Ministry of Labor and Social Affairs, as aids for delimiting the minimum wage sectors in Table A1 (Möller et al., 2011; Aretz et al., 2011; Kirchmann et al., 2011a,b,c; Bosch et al., 2011; Egeln et al., 2011). Table A2 presents descriptive statistics on the minimum wage sectors. The minimum wage sectors vary widely in terms of their bite (share of workers within a sector with wages below the minimum wage), share of full-time workers, and composition of workforce.

In the data preparation, I largely follow the guide in Dauth and Eppelsheimer (2020). In the empirical analysis, I focus on workers aged 18 to 65. Since I am interested in spillover effects of sectoral minimum wages and not in the effects on the minimum wage sectors themselves, I omit all observations of establishments belonging to a minimum wage sector. To include East Germany in the data, I restrict the main analysis period to start from the year 1992 onward. I create an annual panel by selecting all employment spells that include June 30 as the cutoff date, since this date coincides with the measurement of the variables in the BHP. I deal with multiple employment spells of a worker in a year by keeping her main job, defined as the employment spell with the highest wage or longest tenure in case of a tie. I trim extremely low daily wages of full-time workers by dropping observations with real daily wages below the

[10] The information on the daily wage is censored at the yearly varying social security contribution.

mean real daily wage of the first percentile of real daily wages.

For the mechanisms analyses, I calculate the share of the main construction sector in a labor market region. I proceed in four steps and use the delineation of labor market regions from Kosfeld and Werner (2012). First, I use the raw data and keep only panel establishments. Second, for each labor market region, I calculate the relative share of full-time workers in the main construction sector using only the pre-introduction years 1992–96. Third, I split the distribution of shares of main construction sector full-time workers across labor market regions into terciles, weighted by the number of full-time workers in each labor market region. Fourth, I merge this information to my sample. In a similar way, I calculate the main construction sectors' first minimum wage bite in each labor market region. Again, I only use panel establishments and calculate the share of workers earning a wage below 8.69 Euro in West Germany and 8.00 Euro in East Germany in each labor market region for the years 1992–96 within the main construction sector.

Abowd et al. (1999) (hereafter AKM) introduced an estimation strategy to isolate worker-specific and establishment-specific wage premia by using additive fixed effects for workers and establishments. Card et al. (2013) use the AKM estimation strategy to study the role of establishment-specific wage premia in generating recent increases in wage inequality in West Germany. The establishment-specific wage premia can be interpreted as a proportional pay premium or discount that is paid by an establishment to all employees, e.g., due to rent-sharing, efficiency wage premium, or strategic wage posting behavior (Card et al., 2013). The estimation strategy of AKM requires a connected set of establishments linked by worker mobility to identify the fixed effects. I use the AKM establishment fixed effects provided by Bellmann et al. (2020) and estimated for the universe of workers and establishments in the German social security data. These estimated AKM establishment fixed effects are available for the five sub-periods 1985–92 (for West Germany only), 1993–99, 1998–2004, 2003–10, and 2010–17.

My analysis estimates spillover effects on the worker as well as the establishment level. To estimate establishment-level responses to sectoral minimum wages, I keep only panel establishments that were sampled in a first step for the data (see Section 3.1) and collapse the worker level data to the establishment level. Thus, in my analyses I use a worker-year panel and an establishment-year panel. In the respective analysis samples, I only keep workers or establishments that appeared at least once before and once after the treatment (the public announcement of the sectoral minimum wages).

3.3 Exposed Groups and Descriptives

Workers

I begin by assigning workers outside the main construction sector to different groups, based on the expected intensity of their exposure to the minimum wage from the main construction sector. Formally, I assign workers to three wage groups based on their nominal hourly wage in year t. Using the nominal minimum wages in West Germany (including Berlin) and East Germany as thresholds, I define the groups in the following way:

Definition of Wage Groups

	Treated Group	**Partially Treated Group**	**Control Group**
Hourly Wage (in Euro) West	$h_{i,t} < 8.69$	$8.69 \leq h_{i,t} < 8.69 + 40\%$	$8.69 + 40\% \leq h_{i,t} < 8.69 + 80\%$
Hourly Wage (in Euro) East	$h_{i,t} < 8.00$	$8.00 \leq h_{i,t} < 8.00 + 40\%$	$8.00 + 40\% \leq h_{i,t} < 8.00 + 80\%$

The variable $h_{i,t}$ refers to the nominal hourly wage of worker i in year t. Although the main construction sector minimum wage was adjusted several times during my observation period, I use only the introductory minimum wage to define the groups because it was mainly this wage that was publicly announced and received greater media attention. I use a partially treated group in this paper mainly for three reasons. First, the adjustments to the minimum wage are covered by the partially treated group, the range of which was defined large enough. Second, because I use imputed hours to calculate hourly wages, the partially treated group could include workers in the treated group who were incorrectly assigned to the partially treated group due to measurement error. Third, the minimum wage in the main construction sector could also affect workers who are just above the minimum wage threshold, for example, because the increased wage in the main construction sector, together with already better non-pecuniary characteristics for some workers, now represents a better deal for these workers.[11] I try different bandwidths to define the partially treated and control group in Section 5.2 and find no qualitative change in the patterns of my results. Using data on the years prior to its introduction (1992–95), Table 2 illustrates descriptive statistics for worker groups affected by the minimum wage outside the main construction sector. These groups differed widely from each other. Workers in the treated groups had a higher share of women, non-German nationality, young and low-educated workers and were more likely to work in smaller establishments in rural districts, compared to the control group. In Section 4, I describe how my methodology deals with these issues.

Establishments

In the establishment-level approach, I exploit the continuous variation in the exposure to the main construction sector minimum wage across establishments. This approach is based on a large literature exploiting regional variation in the bite of federal minimum wages (e.g. Card, 1992; Bailey et al., 2021; Dustmann et al., 2022). Derenoncourt et al. (2021) and Bossler and Gerner (2020) recently employed this method to examine exposure to minimum wages across employer-by-occupation-by-commuting-zone

[11] This theoretical consideration stems from a model with strategic complementarity whose predictions I review in Section 5.4.

cells and establishments, respectively. Formally, I define the exposure $D_{j(i)}$ of an establishment j to the main construction minimum wage as

$$D_{j(i)} = \frac{\sum_{i \in j(i)} \sum_{t \in [1992,1995]} \mathbb{1}(h_{i,t} < MW + 40\%)}{N_{j(i),t \in [1992,1995]}}, \tag{1}$$

where MW refers to the minimum wage and $N_{j(i),t \in [1992,1995]}$ is the number of workers in an establishment for the time period 1992–95. Thus, I define exposure of an establishment to the main construction sector minimum wage as the fraction of workers paid a nominal hourly wage below the threshold for partially treated workers in the pre-introduction period of 1992-95.

Figure 1 shows the distribution of the exposure measure across establishments. Many establishments pay all of their workers an hourly wage below the cutoff. These establishments are characterized by a very small number of workers (1–4 workers), which naturally makes it more likely to have an exposure value of 1. Apart from this, the figure shows a continuous and relatively uniform distribution across exposure bins.

Industries

Furthermore, I also classify industries with workers for whom the main construction sector was considered an outside option (herein: outside option industries) and were therefore more likely exposed to the main construction sector minimum wage. In the empirical analysis, I compare the outcomes of workers in these industries with those of workers in other industries for whom the main construction sector was not considered as an outside option (herein: non-outside option industries). To define outside option and non-outside option industries, I use an employment flows approach as in Schubert et al. (2021). I begin with constructing the share of separations from a 3-digit industry k to the main construction sector as follows

$$\pi_{k \to \text{main construction}} = \frac{\#\text{ of separations from industry } k \text{ to the main construction sector in year } t \text{ to } t+1}{\#\text{ of separations from industry } k \text{ in year } t \text{ to } t+1}. \tag{2}$$

I define separations as any employer transition.[12] To construct $\pi_{k \to \text{main construction}}$, I only use separations of workers who are in the treated or partially treated group at year t. I also choose the longest possible time period from 1985 to 1994.[13] This means that I construct $\pi_{k \to \text{main construction}}$ for West Germany in a first step and extrapolate it to East Germany. Figure 2 illustrates the distribution of $\pi_{k \to \text{main construction}}$ for the 1992–95 period, weighted by the number of workers in each industry in that

[12]This accounts for the possibility that for some industries only employers within the same industry are considerable outside options. Defining separations as industry transitions, instead of employer transitions, would thus overstate the role of some industries for workers' job choice.

[13]For consistency, I restrict the West German sample to 1985, since information in the variables was changed from that year onward.

time period. This distribution is heavily skewed to the left, with many industries having a low or no share of outflows to the main construction sector. This is as expected, because I use employer transitions instead of industry transitions and the share of the main construction sector in the economy (see Table A2) is not too high.

I proceed by classifying industries in the top 10^{th} percentiles of the employment weighted distribution (whole sample in 1992–95) of $\pi_{k\rightarrow\text{main construction}}$ as outside option industries and industries in the lowest 10^{th} percentiles as non-outside option industries. Table A3 lists the 3-digit industries in the outside option industries classification and Table A4 lists the 3-digit industries in the non-outside option industries classification. Table A3 shows that workers from industries which rely more on manual tasks (e.g., "manufacture of wooden containers") are classified as outside option industries, whereas industries which are more service-oriented (e.g., "Telecommunications") are classified as non-outside option industries for the main construction sector.[14]

I use this binary approach of different industry groups in my analysis instead of continuous variation of $\pi_{k\rightarrow\text{main construction}}$ for two reasons. First, because non-outside option industries are an additional control group in my analyses, they should not be affected by spillover effects from the minimum wage in the main construction sector. Therefore, I use the lowest part of the distribution in $\pi_{k\rightarrow\text{main construction}}$ by still keeping a large number of observations. Second, the main construction sector has shown to be an important outside option for workers in outside option industries, as evidenced by the fact that these industries are at the top of the $\pi_{k\rightarrow\text{main construction}}$ distribution. The main construction sector and outside option industries share one common labor market with transferable skills and similar (manual) tasks. Therefore, these industries should be affected by spillover effects from the minimum wage in the main construction sector.

4 Empirical Strategy

Difference-in-differences

When comparing the evolution of outcomes (e.g. wages) for workers with lower wages versus higher wages over time, one will typically observe higher wage growth for workers with lower wages, e.g. due to mean reversion (Ashenfelter and Card, 1982). In my empirical methodology I therefore compare the *changes* in outcomes for treated and control group workers over two-year windows (between t and $t+2$), similar to e.g., Dustmann et al. (2022); Currie et al. (1996); Clemens and Wither (2019); Burauel et al. (2020). In the following, I describe the estimation approach using wages as the dependent variable, but the same arguments apply for other outcome variables as well. Formally, I estimate the following difference-in-differences (DiD) specification to compare different worker groups, outside the main construction sector,

[14] Note that Table A3 still includes some construction industries which did not have a binding minimum wage at the time, such as e.g. scaffolding which is included in the WZ 93 industry code 452.

around the time of the introduction of the main construction sector minimum wage:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{t=1992, t\neq 1993}^{1997} \beta_t Treated_{i,t} \times Year_t + \sum_{t=1992, t\neq 1993}^{1997} \gamma_t Partial_{i,t} \times Year_t + \delta X_{i,t} + \epsilon_{i,t}. \tag{3}$$

Here $w_{i,t}$ refers to the log (deflated daily) wage of worker i in year t. In Equation 3, I regress (deflated daily) log wage growth of worker i between the years t and $t+2$ on the interaction of a year indicator $Year_t$ with the indicator variable $Treated_{i,t}$, which is equal to 1 if worker i falls into the treated group and 0 if worker i falls into the control group at the baseline year t. I include a similar interaction term of the year indicator with the indicator variable $Partial_{i,t}$ which is equal to 1 if worker i falls into the partially treated group at baseline year t and 0 if the worker is in the control group. The coefficients β_t and γ_t trace out the *change* in the wage growth for the treated and partially treated group relative to the control group and relative to the baseline period of 1993 to 1995. I estimate the DiD specifications including one pre-introduction period $t = 1992$ and four post-introduction periods $t \geq 1994$. Thus, the change in wage growth for treated relative to control group workers from 1992-94 serves as a placebo test. α_i are person fixed effects, ζ_t are year fixed effects and in $X_{i,t}$ I include additional controls. Specifically, I include 1-digit industry, federal state, region type, and the treatment group dummies measured at baseline year t.[15] I cluster the standard errors at the worker level.

The inclusion of worker fixed effects α_i is very important in the context of this study for two reasons. First, the worker fixed effects purge time-invariant unobserved worker-specific effects on wage growth, such as e.g. ability or motivation to climb up the job ladder. Second, around the time of the introduction of the main construction sector minimum wage, many macroeconomic trends affected the treated and control groups differently, such as e.g. technological change (Dustmann et al., 2009; Goos et al., 2009), deepening trade relations with China and Eastern Europe (Dauth et al., 2014, 2021) and migration (D'Amuri et al., 2010; Glitz, 2012). Worker fixed effects, which, in a regression with a differenced outcome, is analogous to controlling for worker-specific linear trends in a non-differenced regression (Allegretto et al., 2017), help to account for these group-specific macroeconomic trends.[16]

Triple differences

My main empirical strategy is a triple differences estimator (DiDiD). This approach confirms that the estimated DiD effects in Equation 3 are indeed spillover effects from the main construction sector minimum wage. Using the outside option and non-outside option industry groups, I estimate the DiDiD specification as follows:

[15] I also estimate different specifications of Equation 3 without worker fixed effects. In this case, I additionally control for age, education, gender and nationality.

[16] In the robustness checks, I drop the assumption that the mentioned economic factors can be viewed as group-specific macroeconomic trends and instead treat them as region-specific and industry-specific shocks.

$$
\begin{aligned}
w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + &\sum_{t=1992, t\neq 1993}^{1997} \beta_t Treated_{i,t} \times Option_{i,t} \times Year_t \\
&+ \sum_{t=1992, t\neq 1993}^{1997} \gamma_t Partial_{i,t} \times Option_{i,t} \times Year_t + \delta X_{i,t} + \epsilon_{i,t}.
\end{aligned} \tag{4}
$$

The only change from Equation 3 to Equation 4 is that I include the triple interaction of the treated groups, the variable $Option_{i,t}$, and the year indicator. The variable $Option_{i,t}$ is equal to 1 if worker i is employed at an outside option industry (Table A3) in year t and 0 if she is employed at a non-outside option industry (Table A4). I include all respective double interactions and indicators together with the same control variables as in Equation 3 in $X_{i,t}$. The coefficients of interest, β_t and γ_t, now essentially compare the DiD for workers in outside option industries relative to the DiD for worker in non-outside option industries.

The DiDiD estimates of Equation 4 primarily have two advantages over the estimates in Equation 3. First, the DiDiD specification confirms the working hypothesis that after the minimum wage was announced in 1996, workers in industries similar to main construction (outside option industries) should also experience a larger change in their wage growth than workers in industries less similar to main construction (non-outside option industries). Second, the DiDiD estimates also remove any group-specific time shocks. Olden and Møen (2022) derive the formal identifying assumptions of the triple differences estimator and show that the estimator does not require two parallel trends assumptions, but only one parallel trends assumption, to have a causal interpretation. Intuitively, any contemporaneous shock to the outcome variable that affects all workers in the treated groups or all workers in the control group across outside option and non-outside option industries will be differenced out. In Section 5.1, statistically and/or economically insignificant effects for β_t and γ_t in the pre-announcement period indicate that the DiDiD parallel growth assumption holds. The spillover effect from the main construction sector minimum wage should have only affected workers in the treated group and to a larger extent within outside option industries and therefore does not get filtered out by the DiDiD specification.

For further intuition, Figure 3 illustrates the identification strategy. Because the main construction sector and outside option industries share one common labor market, I draw them together in one circle. However, because the minimum wage was only implemented in the main construction sector, there is a dividing line between these two sectors. The area for the main construction sector is dot-filled gray because I concentrate on the spillover effects on the outside option industries in this paper rather than the within-sector effects. I expect the spillover effects to affect only the treated group (green persons in the figure) in outside option industries and not the control group (orange persons in the figure). Non-outside option industries make up another control group that is outside the broad circle in the figure since it does not share a labor market with the main construction sector. The DiD in non-outside option industries thus represents the counterfactual scenario in the absence of the main construction sector minimum wage.

In addition to the event-study analysis in Equation 4, I also estimate the triple differences by pooling pre- and post-announcement periods:

$$w_{i,t+2}-w_{i,t} = \alpha_i + \zeta_t + \beta\, Treated_{i,t} \times Option_{i,t} \times Post + \gamma\, Partial_{i,t} \times Option_{i,t} \times Post + \delta X_{i,t} + \epsilon_{i,t}. \quad (5)$$

The dummy *Post* equals 0 for the years of 1992 and 1993, and equals 1 for the years 1994, 1995, 1996 and 1997. All other variables remain the same as in Equation 4.

Establishment-level analysis

To analyze the spillover effects from the main construction sector minimum wage on establishments, I exploit the continuous variation in the exposure $D_{j(i)}$ of an establishment j in the following event-study DiD specification:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{t=1992, t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Year_t + \epsilon_{j,t}. \quad (6)$$

$y_{j,t}$ denotes the outcome of interest, α_j are establishment fixed effects and ζ_t are year fixed effects. The coefficients γ_t trace out how establishments with higher exposure to the main construction sector minimum wage responded to it relative to establishments with lower exposure and relative to the base year 1995. For the years $t > 1995$, the coefficients estimates for γ_t yield the causal spillover effect of the main construction sector minimum wage if the parallel trends assumption holds. Specifically, the underlying assumption for the DiD specification in Equation 6 is that more exposed establishments would have evolved similarly, in terms of the potential outcomes, compared to less exposed establishments in the absence of the main construction sector minimum wage. In Section 5.3, I provide suggestive evidence of this parallel trends assumption by visualizing the coefficient estimates for γ_t for the years prior to the minimum wage announcement $t < 1995$. Coefficient estimates of $t < 1995$ which are statistically and/or economically insignificant hint towards a plausible parallel trends assumption.

To further validate the hypothesis that the spillover effects stem from the main construction sector minimum wage rather than contemporaneous shocks to low-wage jobs, I estimate a DiDiD specification. I use the same intuition as for the individual-level analysis. Formally, I estimate the following DiDiD specification:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{t=1992,t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Option_{j(i),t} \times Year_t + \delta X_{j,t} + \epsilon_{j,t}. \quad (7)$$

I estimate a triple interaction and include all respective double interactions as well as the $Option_{j(i),t}$ variable in $X_{j,t}$.[17] The DiDiD specification in Equation 7 has the additional advantage of filtering out any group-specific time shocks to establishments with different levels of exposure, while at the same time supporting the hypothesis that the main construction sector minimum wage should have a larger spillover effect to establishments in outside option industries.

Similar to Equation 4, the underlying parallel trends assumption in Equation 7 is that the gap in the potential outcome variable between outside and non-outside option industries would have evolved similarly for establishments with different levels of exposure, in the absence of the main construction sector minimum wage (Cunningham, 2021). In other words, any contemporaneous shock to the outcome variable, not induced by the minimum wage, which affects establishments with high levels of exposure but not low levels of exposure or vice versa, should be similar within outside option industries as in non-outside option industries. Again, in Section 5.3, I provide suggestive evidence for this assumption in an event-study figure, by showing that the coefficient estimates of $\gamma_t < 1995$ are statistically insignificant. If this assumption holds, $\gamma_t > 1995$ identifies the causal spillover effect of the main construction sector minimum wage on establishments in outside option industries with higher exposure.

I weight both, DiD and DiDiD, regressions by using the average number of full-time employees within each establishment in the 1992–95 pre-period. I cluster the standard errors at the establishment level.

5 Results

5.1 Wages and Reallocation

Figure 4 illustrates the individual-level results using the DiD estimator of Equation 3 with the 2-year change in wage growth as the outcome variable.[18] The y-axis shows the DiD coefficient estimates and the x-axis shows the time period. The time period 1993–95 is the reference period. If the public announcement of the main construction sector minimum wage had immediate wage growth effects, I would expect a positive coefficient for treated workers right at 1994–96. The vertical line serves as a dividing line between the pre- and post-treatment period. Because treated workers have a higher relative two-year wage growth compared to the control group already in 1992–94, the parallel growth

[17] To be more specific, $X_{j,t}$ includes: $Option_{j(i),t}$, $Year_t \times Option_{j(i),t}$, $D_{j(i)} \times Option_{j(i),t}$, and $\sum_{t=1992,t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Year_t$.

[18] In Table A5, I estimate various specifications of Equation 3 in which I subsequently add control variables and also display the results for the partially treated group. Without the usage of worker fixed effects, I find negative effects on the relative 2-year wage growth of treated and partially treated workers across all time periods. Only with the inclusion of worker fixed effects, with which biases due to worker-specific trends and unobservable person-specific heterogeneity are removed, I find a positive change in the wage growth for the treated group beginning in 1994–96 relative to 1993–95 and relative to the control group. Figure 4 illustrates the coefficients estimates of column 3 in Table A5.

assumption seems not to hold. I observe positive coefficients of the DiD for treated workers also in the post-announcement period.

In Figure 5, I estimate my baseline DiDiD specification from Equation 4 using the change in wage growth as the outcome variable. Here, the y-axis shows the DiDiD coefficients from the triple interaction in which I, intuitively, compare the DiD in outside option industries with the DiD in non-outside option industries. In contrast to the DiD estimator, the DiDiD estimator has the advantage of removing biases due to group-specific time shocks, such as shocks affecting the wage growth of all low-wage workers (in outside and non-outside option industries). I find a positive and statistically significant coefficient in the pre-period of 1992–94 for treated workers in outside option industries. The coefficient quadruples in size from 1992–94 to 1994–96, right at the public announcement of the main construction sector minimum wage. Specifically, the relative wage growth of treated workers in outside option industries increased by 2% in 1994–96 relative to 1993–95. For the time periods of 1995–97, 1996–98, and 1997–99 the size of the DiDiD coefficient increases slightly more. In column 4 of Table A6, I present the baseline specification illustrated in Figure 5, together with standard errors, the number of observations, and the partially treated group.[19] Without the inclusion of worker fixed effects in columns 1 and 2 of Table A6, I find similar patterns compared to the specification with worker fixed effects, with no statistically and economically significant coefficient in the pre-period of 1992–94. Table A6 further shows that the DiDiD estimates are similar with or without the inclusion of additional controls such as industry or federal state fixed effects. In Figure A.1, I estimate the DiDiD specification excluding (ancillary) construction industries from the outside option industry classification and find that the wage spillover effects were not driven by these construction industries.[20]

To gain intuition on the validity of the triple differences specification, I estimate the DiD specification of Equation 3 separately by non-outside option and outside option industries in Table A7. I observe a positive and statistically significant coefficient of the DiD estimate in the pre-period of 1992–94 for both, non-outside option and outside option industries. In other words, I observe a common shock to either all treated group or control group workers in 1992–94. The triple differences specification, illustrated in Figure 5, is able to partly filter this common group-specific time shock out. Assuming that the DiD in non-outside option industries represents the counterfactual wage growth change in outside option industries, I find that in the absence of the public announcement of the minimum wage (captured in 1994–96) and introduction of the minimum wage (captured in 1995–97) in the main construction sector, no change in wage growth would have been present. In 1996–98 and 1997–99, I find a negative shock to the relative wage growth of workers in the treated group in the counterfactual scenario (non-outside

[19] I observe a similar spike in wage growth for the partially treated group. However, the coefficient is much smaller in magnitude. Because I use the partially treated group mainly to catch measurement errors that may arise from imputation of hours worked and minimum wage adjustments (see Section 3.3), I focus on the treated group below.

[20] Specifically, I drop the 3-digit industries 451, 452, 454, and 455 from the list of outside option industries (see Table A3).

option industries). Reassuringly, most of the action in the triple differences estimations in Figure 5 comes from higher wage growth of treated workers relative to control group workers in outside option industries.

Based on Table A7, I assume that the positive and statistically significant coefficient of the DiDiD in 1992–94 is a one-time common shock to all treated or control group workers. Moreover, to gain more pre-periods for the placebo check, in Figure A.2, I estimate the triple differences specification with 1-year wage growth instead of 2-year wage growth as the outcome. I find that the common pre-period shock occurred mainly in 1993–94 and no significant pre-trend for 1992–93. Finally, in Figure A.3, I use different bandwidths to define the control group. "Treated - Base" refers to the bandwidths of the baseline estimation defined in Section 3.3 and illustrated in Figure 5. I additionally define a control group with broader bandwidths ("Treated - Broad") with $MW + 60\% \leq h_{i,t} < MW + 120\%$ and tighter bandwidths ("Treated - Tight") with $MW + 20\% \leq h_{i,t} < MW + 40\%$, where MW refers to the minimum wage. The tradeoff in using narrower or wider bandwidths is that narrower bandwidths allow comparisons between treated and control group workers who are more similar to each other, while wider bandwidths make potential identification threats such as spillover effects to the control group or substitution between groups less likely (Stewart, 2004). Indeed, I find that using a narrow bandwidth for the control group completely eliminates the pre-trend in the 1992–94 period. The wider the bandwidth for the control group, the larger the coefficient in the 1992–94 pre-period. In all three cases, however, I find a sharp increase in the coefficients immediately upon the public announcement of the minimum wage in the main construction sector in 1994–96. Therefore, I interpret the sharply increasing and positive coefficients in the post-announcement period for treated workers in outside option industries as spillover effects from the sectoral minimum wage in the main construction sector.

In Table 3, I estimate the pooled pre- vs. post-period triple differences specification of Equation 5. On average, wage growth of treated workers in outside option industries increased by 2.1% in the post-period relative to the pre-period. To compare the effect size, I use a similar triple differences specification to estimate the wage growth effects *within* the main construction sector in Figure A.4 and Table A8.[21] I find that the wage spillover effects are about one-third of the wage effect within the main construction sector.

In frictional labor markets, the publicly announced introduction of the main construction sector minimum wage should lead to an increase in reallocation of workers (e.g. Bhaskar et al., 2002; Jäger et al., 2022). I test this prediction by using the specification of Equation 4 with the change of jobs as the

[21] Specifically, I use a sample including all workers in establishments within the main construction sector (see Table A1) and non-outside option industries. With this sample, I estimate a triple differences specification similar to Equation 4. The only change is that instead of comparing the DiD of treated vs. control group workers in outside option industries to the DiD in non-outside option industries, I compare the DiD in the main construction sector to the DiD in non-outside option industries.

outcome variable. The outcome variable takes the value 0 if the worker did not change establishments from t to $t+2$ and 1 if the worker did change establishments from t to $t+2$. Figure 6 illustrates the results.[22] I find small statistically significant negative effects for the pre-period of 1992–94 for workers in the treated group. After the public announcement of the main construction sector minimum wage, I find a sharp increase in the probability of switching jobs for treated group workers in outside option industries. Specifically, treated workers in outside option industries had a 5.6 percentage points and 5.5 percentage points higher likelihood of switching jobs in 1994–96 and 1995–97 respectively, relative to the reference period 1993–95. For the subsequent periods, the DiDiD coefficient is insignificant in 1996–98 and 1997–99 for treated group workers in outside option industries. As I show in Figure A.1, the results on the probability to switch establishments were not driven by the (ancillary) construction industries in the outside option industry classification. Overall, Table 3 illustrates that the probability that more exposed workers decided to leave their job to find a new employer increased by 3.7 percentage points in the post-period relative to the pre-period.

5.2 Robustness Checks

The triple differences specification of Equation 4 and estimated in Figures 5 and 6 is robust to macroeconomic shocks, mean reversion, worker-specific unobserved heterogeneity and group-specific time shocks, such as shocks to the low-wage labor market. However, around the time of the introduction of the main construction sector minimum wage, other potential shocks are not captured by my identification strategy and could therefore bias the results. Specifically, migration from East Germany and Eastern Europe, the integration of East Germany to the German economy, city and state specific policy changes, structural changes in the German labor market, international trade and technological change could potentially bias the estimations. I proceed in three steps to probe the robustness of my results to these kinds of shocks. Tables 4 and 5 illustrate the results of various robustness checks for the different outcome variables.

First, I include labor market region (LMR) times year fixed effects. These fixed effects exploit variation within labor market regions across differentially exposed individuals and therefore control for region-specific shocks such as migration shocks to specific labor market regions, city and state specific policy changes, and international trade shocks with different effects across regions. I find that the inclusion of these fixed effects does not change the results qualitatively. Thus, the positive wage spillover and reallocation effects were not driven by region-specific shocks.

Second, I include 1-digit industry times year fixed effects. These fixed effects exploit variation within 1-digit industries across differentially exposed individuals and therefore control for industry-specific shocks, such as technological change or also international trade shocks and structural changes to the German economy, which affected some industries differently than others. I find that the inclusion of

[22] Table A9 illustrates the results in table form and includes number of observations, standard errors, and the partially treated group.

industry times year fixed effects does not change the results qualitatively.

Third, I include both, labor market region times year and 1-digit industry times year fixed effects. Again, the positive wage spillover effects and the increase in job-to-job changes are robust to the inclusion of these fixed effects.

In the fifth column of Tables 4 and 5, I exclude all observations in establishments during their closing year.[23] Demand shocks during the observation period could bias my results. Excluding observations that are affected by establishment closure should capture these shocks on the demand side. I find virtually no change in the coefficients for the wage spillover and reallocation estimations.

I also check the robustness of my results to different definitions of the key independent variables of interest in the last two columns of Tables 4 and 5. First, I define a time-constant version of the $Treated_{i,t}$ and $Partial_{i,t}$ variable ($Treated_i$ and $Partial_i$) so that variation in these variables, with the inclusion of worker-fixed effects, only comes from changes in the outcome variable for the same individuals over time and not from switchers from e.g., the treated group to the control group. To do so, I classify an individual as belonging to the (partially) treated group if one observation between 1992 and 1995 of the individual is classified as (partially) treated. I proceed similarly for control group workers. Intuitively, I relax the no-carryover assumption of my baseline estimation, where I implicitly assumed that potential outcomes depend only on current treatment status and not on the entire treatment history (Roth et al., 2022). I find qualitatively similar results for the wage spillover and reallocation effects with these time-constant versions of the $Treated_i$ and $Partial_i$ variables. Second, one could argue that the relevant labor market definition of workers is based on occupations instead of industries. Therefore, I define the $Option_{i,t}$ variable based on employment flows within 3-digit occupations instead of employment flows within 3-digit industries (see Section 3.3). In the last columns of Tables 4 and 5, I find that the patterns of spillover effects using occupation flows are similar to the baseline specification using industry flows.

5.3 Establishments

To shed light on demand-side responses and to compare the results with the existing empirical evidence on cross-employer spillover effects (Staiger et al., 2010; Derenoncourt et al., 2021; Bassier, 2021), I analyze the spillover effects from the main construction sector minimum wage from the perspective of establishments.

Figure 7 plots the coefficient estimates for γ_t for the DiD specification from Equation 6 as well as the coefficient estimates for γ_t for the DiDiD specification from Equation 7. The outcome variable in these figures are log (daily) average wages of an establishment. I find no statistically significant effect on average wages on more exposed establishments using the DiD specification. In line with previous research on cross-employer wage spillovers, the DiDiD estimates in Figure 7 show that more exposed

[23]To make sure that these are real establishment closures and not just an establishment takeover or ID change, I use the heuristic in Hethey and Schmieder (2010) and the variables created for it in the BHP.

establishments increased average wages following the introduction of the main construction sector minimum wage. Wage growth evolved similarly for establishments with different levels of exposure in outside option and non-outside option industries in the years prior to the minimum wage introduction. However, after the introduction, establishments in outside option industries with higher levels of exposure increased their average wages relatively more, compared to establishments in non-outside option industries and establishments with lower levels of exposure. Specifically, the coefficient estimates from 1992–97 are statistically insignificant and increase only after the introduction of the main construction sector minimum wage to 5.3% in 1998 and 6.2% in 1999.[24]

Note that while workers experienced higher wage growth already right at the public announcement of the main construction sector minimum wage (see Figure 5), establishments increased average wages only after the introduction in 1998. Thus, while employees reacted very quickly and strongly to the public announcement of the minimum wage, for example by changing jobs, establishments responded rather relatively late to the minimum wage. In addition, I show in Section 5.4 that the wage spillover effects can be explained mainly by establishment switches. This is consistent with the results here, as they show that it is primarily a change in worker behavior that drives the results in this study.

In Figure 8, I estimate the DiD and DiDiD specifications on the establishment level using the log number of full-time employed workers as the outcome variable. Again, using the DiD specification I do not find that more exposed establishments experienced a change in their number of full-time employees. However, using the DiDiD specification, I find that more exposed establishments in outside option industries experienced on average a loss of their full-time employment force. The negative employment effects for more exposed establishments in outside option industries amounted to 33.9% in 1997 and are relatively imprecise estimates. This result is in general consistent with labor market models which incorporate frictions, as these models predict a loss in employment for more exposed establishments.

5.4 Mechanisms

The spillover effects from the main construction sector minimum wage are consistent with labor market models that include frictions. However, it is unclear whether strategic interactions between firms or the removal of information frictions can explain the spillover effects. Based on theoretical considerations, I will explore the mechanisms for spillover effects in this section.

5.4.1 Strategic Complementarity

To understand whether strategic complementarity can explain the spillover effects, I use a simple version of the theoretical models in Bhaskar and To (1999); Bhaskar et al. (2002); Bhaskar and To (2003) which in turn build on the spatial model of Salop (1979). A version of this model is also applied in Staiger

[24]Tables A10 and A11 illustrate the results in table form including the number of observations and standard errors.

et al. (2010), who find evidence for strategic complementarity in their spillover effects.

In the spatial model of strategic complementarity, workers have heterogeneous preferences for employers due to transportation costs. Therefore, I ignore other non-pecuniary job characteristics by which heterogeneous preferences of workers may arise. Thus, I assume that all non-pecuniary job characteristics, except transportation costs, are similar for the main construction sector and outside option industries. Note that because I use employment flows to determine outside and non-outside option industries, outside option industries are already "close" to the main construction sector in terms of task similarity, transferability of skills and possibly other non-pecuniary characteristics by revealed preference.

Suppose that workers are uniformly distributed along a straight line. Two sectors, A and B, are located at distance d_r from each other at the straight line. The distance d_r between the two sectors can vary by local labor market region (LMR) r. I assume that each LMR is a closed labor market. Workers have to pay transportation costs τ for each distance unit traveled. An individual located at x_r^* distance units from sector A is indifferent between working for sector A or sector B if:

$$w_r^A - \tau x_r^* = w_r^B - \tau(d_r - x_r^*), \tag{8}$$

where sector A pays wage w_r^A in LMR r and sector B pays w_r^B. Solving for x_r^* gives:

$$x_r^* = \frac{w_r^A - w_r^B + d_r\tau}{2\tau}. \tag{9}$$

This point of indifference, x_r^*, is sector A's labor supply L_r^A.

Each firm in the respective sectors maximizes profits given β, the marginal benefit of employing a worker. Substituting labor supply into the profit maximization problem and then solving for the optimal wage using the first-order condition provides the wage-setting equation in this model:

$$w_r^A = \frac{\beta + w_r^B - d_r\tau}{2}. \tag{10}$$

Wages increase with β and the wage of competitor B. However, whenever the distance d_r between sectors A and B is larger, the wage response of sector A to an increase in sector B wages will not be as high. In other words, sector A can set its wages more independently from sector B's wages (and vice versa) whenever the distance between these two sectors is larger. The optimal labor demand given labor supply is:

$$L_r^A = \frac{\beta + d_r\tau - w_r^B}{4\tau}. \tag{11}$$

Labor in sector A increases with β and decreases with the wage in sector B. However, the decreasing effect of w_r^B on L_r^A is lower whenever the distance to the competitor is larger.

I can use this model to derive testable predictions on wage spillover and reallocation effects from the main construction sector minimum wage. I assume that the share of the main construction sector in a LMR is negatively correlated with the distance to its competitors in the LMR. With respect to wages, the model predicts:

1. **Outside option industries increased wages more in LMRs with a higher share of the main construction sector.**

I test this prediction in the second column of Table 6. I use the terciles of the distribution of the share of the main construction sector among LMRs described in Section 3.2. LMRs in the lowest tercile have shares of the main construction sector that range from 0% to 4%, LMRs in the middle tercile have shares of the main construction sector that range from 4.1% to 7.2%, and LMRs in the highest tercile have shares of the main construction sector that range from 7.2% to 36.9%. I interact these terciles with the baseline triple interaction. In contrast to the prediction, I find that treated workers in outside option industries in LMRs with a higher share of the main construction sector experience a lower wage growth compared to similar workers in LMRs with a lower share of the main construction sector.

2. **Outside option industries increased wages more in LMRs with a higher bite of the main construction sector.**

Intuitively, a higher bite means that more establishments in the main construction sector have to adjust their wages upward, and therefore more establishments in outside option industries will have to increase their wages. Since, by definition, hardly any establishment in the main construction sector would have to adjust its wages in labor market regions with a low bite, no establishment in outside option industries would have to adjust wages either. To test this prediction, I use the bite of the main construction sector minimum wage, calculated for each LMR using the pre-period (see Section 3.2). Because the bite measure varies strongly between West and East Germany (see Table A2), I divide the sample to West and East Germany and standardize the bite measure across LMRs within these two samples, weighted by the number of employees in each LMR, to have mean 0 and standard deviation 1. The third and fourth columns of Table 6 illustrate the results.

I find that West German treated workers in outside option industries within LMRs with a higher main construction sector minimum wage bite do not experience a different change in their wage growth compared to workers in LMRs with a lower bite. However, for East Germany, I do indeed find that treated workers in outside option experience a higher wage growth in LMRs that have a higher bite.

3. **The wage increase stemmed mostly from staying within the same establishment or switching to the main construction sector.**

Since every establishment outside the main construction sector would respond similarly to the minimum wage in the main construction sector, the net wage (wage minus transportation costs) of the current establishment would not change relative to all other establishments within the outside option industries. Therefore, in the simple strategic complementarity model presented above, it would only be rational for workers in the outside option industries to remain in the same establishment or increase reallocation to the main construction sector.

In Figure 9, I re-estimate the specification of triple differences for wage changes and job-to-job changes by excluding switchers to the main construction sector. In contrast to the model prediction, I find that the wage spillover and reallocation effects were not driven by switchers to the main construction sector.

Furthermore, in the last two columns of Table 6, I compare workers who made at least one job-to-job transition to any establishment in the post-period (switcher) to workers who stayed in the same establishment during the post-period.[25] I find that switchers had higher wage growth during the post-period than stayers. Moreover, as the last column of Table 6 shows, the increase in wage growth stems mostly from switching to any establishment, not switching to the main construction sector.

In Figure 10, I use a slightly different approach by using sub-samples for stayers vs. switchers, i.e. comparing stayers to stayers and switchers to switchers over time. This approach should alleviate concerns that switchers generally have higher wage growth than stayers. Again, I find a higher change in wage growth after the public announcement of the main construction sector minimum wage for switchers compared to stayers. This finding is again not driven by switchers to the main construction sector, as excluding these switchers in Figure A.5 shows. Thus, switchers to any establishment were driving the overall positive wage spillover effects for sub-minimum wage workers in outside option industries. Moreover, this analysis allows me to rule out the possibility that bargaining within the existing employment relationship drove the wage spillover effects.

Taking stock, I have sketched a simple spatial model of strategic complementarity in this section. I tested the predictions of the model and found no or only weak evidence for strategic complementarity. Thus, strategic complementarity does not seem to explain the spillover effects from the main construction sector minimum wage. In the next chapter, I present a model that fits the patterns of the spillover effects better.

5.4.2 Biased Beliefs about Outside Options

In this section, I apply the theoretical model of Jäger et al. (2022) to my context and derive testable predictions. I will present the main components of the model relevant to my context and refer the interested reader to Jäger et al. (2022) for details. In the theoretical model of Jäger et al. (2022), workers form beliefs about their outside options in the labor market. Biased beliefs about outside options can

[25] More specifically, I define a variable "Switch" which takes the value 1 if a worker changed establishments from t to $t+2$ in 1994–97 at least once, and 0 if a worker stayed at the same establishment in the 1994–97 period.

cause workers to stay in lower-paying firms and receive marked down wages.

In the model, first N homogenous firms enter the labor market. Then, L workers are randomly assigned to firms and supply labor inelastically. Workers learn their wages and potentially update their beliefs about the external wage distribution. Assume the existence of two types of workers who differ in their cost to gather complete information about the labor market. A share α of workers are experts who face no information costs $c_E = 0$ and are always perfectly informed about their outside options in the labor market. The remaining share $1 - \alpha$ are amateur workers who face information costs $c_A > 0$ and can therefore form biased beliefs about their outside options. Amateur's job search decision depends on their beliefs about the benefits of job search

$$\widetilde{w}^{max}(w_j, w_{j-1}) - w_j > c_A, \tag{12}$$

where w_j is the wage of a worker in her current firm j. $\widetilde{w}^{max}(w_j, w_{j-1})$ is the belief about the highest wage. Thus, workers search for new jobs if they believe that the wage they could potentially earn is higher than their current wage plus search costs. The belief about the highest potential wage is a weighted average of the actual highest wage and worker's current wage:

$$\widetilde{w}^{max} = \gamma w_j + (1 - \gamma) w^{max}. \tag{13}$$

The variable $\gamma \in [0, 1]$ captures the degree of anchoring on the current wage. If, e.g., $\gamma = 1$ then workers fully anchor their belief about potential outside options on their current wage. With $\gamma = 0$, workers have accurate beliefs. Empirically, Jäger et al. (2022) show that especially low-wage workers anchor their beliefs about outside options on their current wage and therefore underestimate wages elsewhere.

In the theoretical model, firms maximize their profits given the labor costs per worker. The competitive wage is w^* and equals the marginal product of labor. Jäger et al. (2022) also model how a segmented labor market of firms paying the competitive wage (high-wage firms) and firms paying a marked down wage (low-wage firms) can emerge. For such a segmented labor market to emerge, the only profitable departure from the competitive wage w^* is to pay a wage below w^*, but still large enough to retain a firm's stock of amateur workers. Any downward deviation from the competitive wage will result in an immediate loss of a firm's stock of expert workers.

The reservation wage of amateur workers to not become informed is given by Equation 12. The most profitable deviation is to exactly pay the reservation wage. Considering the formation of biased beliefs in Equation 13 and using it in Equation 12 gives:

$$w' = w^* - \frac{c_A}{1 - \gamma}. \tag{14}$$

w' is the most profitable deviation and represents a markdown of the competitive wage w^*. The markdown from the competitive wage is higher with higher information costs c_A and higher anchoring γ. Deviant firms only retain their amateur workforce and therefore employment in these firms is

$$l(w') = (1 - \alpha)\frac{L}{N}. \tag{15}$$

The deviant wage w' and employment $l(w')$ describe the behavior of low-wage firms in the labor market. For completeness, high-wage firms pay the competitive wage and employ all expert workers in the labor market (plus a share of amateur workers who initially sorted into those firms).

I derive testable predictions from this model for the context of this paper by modeling the public announcement and introduction of the main construction sector minimum wage as a reduction in information costs c_A and an update in $\widetilde{w}^{max}$. The public announcement and introduction of the minimum wage informs workers on what they could potentially earn in the labor market. Given the high anchoring on current wages (Jäger et al., 2022), the public announcement and introduction reduced biased beliefs about outside options in the labor market. This information shock should primarily affect workers who have similar job tasks as well as transferable skills (outside option industries) and earn a wage below the minimum wage (treated workers). The testable predictions are as follows:

A. **The reallocation of treated workers in outside option industries from low-wage to high-wage establishments increased.**

This prediction follows naturally from the Jäger et al. (2022) model. Through the publicly announced introduction of the main construction sector minimum wage, treated workers in outside option industries learn what wages they could earn in the labor market. They learn that they are working in a low-paying establishment that pays them a marked down wage, and as a result move to a better-paying establishment.

To test this prediction, I follow the approach in Dustmann et al. (2022). I define the change in the establishment j average wage or AKM establishment effect for worker i as $q^{l=t}_{j(i,t+2)} - q^{l=t}_{j(i,t)}$, where $q^{l}_{j(i,t+2)}$ denotes the time l characteristics of establishment j at which worker i is employed in year $t+2$. Thus, I measure the establishment average wage or AKM establishment effect in the baseline period t in both periods. For workers who remain employed at their baseline establishment from t to $t+2$, this measure of establishment quality is zero by construction. Using this approach, I make sure that any change in establishment average wage or AKM establishment effect reflects compositional changes only and not improvements in the quality of establishments over time.

In the first panel of Figure 11, I show the results for the change in average establishment (daily) imputed wages.[26] I find that treated workers in outside option industries had a higher likelihood of

[26] Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB

switching to establishments which pay a higher average wage to their workforce in the post-period. Specifically, treated workers in outside option industries switched to establishments that on average have a 0.8% higher mean wage than their previous establishment in 1994–96 and up to 1.3% in 1997–99.

In the second panel of Figure 11, I show the triple differences results using the change in AKM establishment fixed effects as the outcome variable. While a negative coefficient would indicate that workers moved to establishments with a lower pay premium to the same worker type, a positive coefficient indicates that workers moved to establishments with a higher pay premium to the same worker type. Because the AKM effects for West and East Germany are only available from 1993 onward, I cannot estimate a pre-period placebo test for the baseline specification (see Section 3.2). The triple differences coefficient is statistically insignificant in 1994–96 and increases in size in the following years from 0.6% in 1995–97 to up to 1.4% in 1997–99.

In Table A13, I re-estimate the specifications in Figure 11 by excluding switches to the main construction sector from t to $t+2$ and by excluding establishments during their closing year from the sample. I find that the results presented here are not driven by switches to the main construction sector or by establishment closure. Rather, the results suggest that, consistent with the prediction of Jäger et al. (2022)'s model, more exposed low-wage workers switched to better-paying establishments after their biased beliefs about wages in the labor market were updated.

B. **The increase in wage growth was mainly due to switches in establishments, although not necessarily to switches to the main construction sector.**

The intuition for this prediction is similar to the intuition of prediction A. Workers in low-paying establishments learn about their establishment quality which pays them a marked down wage and reallocate to better paying establishments which pay them a higher wage.

In Table 6, Figure 10, and Figure A.5, I showed that in contrast to the prediction of a spatial model with strategic complementarity, most of the wage growth stems from switching to any establishment and not from staying within the same establishment or switching to the main construction sector. The model in Jäger et al. (2022) can rationalize this result. Equation 12 models the job search decision of workers with biased beliefs and positive information costs. As workers update their biased beliefs about potential outside wages through the public announcement, they start searching for new jobs. Job search is not directed to the main construction sector in this case. Furthermore, the fact that the wage spillover effects and job-to-job transitions occurred precisely before the introduction of the minimum wage but after the public announcement in 1996, is also consistent with an information shock story.

C. **The spillover effects were heterogeneous by initial information cost level. Expert work-**

in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In comparison to the censored wage variable, the imputed wage variable has the benefit that it can more accurately represent job-to-job transitions to establishments with better workforce composition. For details on the imputation procedure see Ganzer et al. (2022). Table A12 illustrates the results in table form and includes the number of observations and standard errors.

ers were not affected by the publicly announced introduction of the main construction sector minimum wage.

The model in Jäger et al. (2022) distinguishes between employees with high information costs (amateurs) and employees with no information costs (experts). Only amateurs should be affected by the information shock. Experts were aware of the wages already above the minimum wage in establishments with collective bargaining agreements in the main construction sector (see Section 2). Consequently, the public announcement of the minimum wage, which is below the entry-level wage in establishments covered by collective agreements, should have had no effect on experts.

Since it is not possible to precisely identify amateurs and experts in my data set, I make two assumptions. First, non-German workers are more likely to have higher information frictions about their outside options in the labor market in general, compared to native workers. Second, I also expect workers with less labor market experience to have less information about possible outside options in the labor market than workers with more labor market experience.

In Figure 12, I estimate the triple differences specification of Equation 4 separately for sub-samples of German, non-German, workers with 0 to 5 years, 5 to 10 years or more than 10 years of labor market experience.[27] In line with the model predictions, I find on average larger effects for non-German compared to German workers and larger effects for workers with only little labor market experience compared to workers with more labor market experience. Thus, the results suggest that workers who are more likely to have higher information frictions about their outside options also experienced on average higher wage spillover effects compared to their better informed counterparts, following the publicly announced introduction of the main construction sector minimum wage.

Prediction C can also rationalize the findings in the second column of Table 6. Namely, treated workers in LMRs with a lower share of the main construction sector experienced higher wage spillover effects compared to LMRs with a higher share of the main construction sector. Workers in LMRs with a higher share of the main construction sector were more likely to be informed about the already high entry-wages in the main construction sector. Consequently, the publicly announced introduction of the main construction sector minimum wage should only be an information shock for workers in LMRs with a low share of the main construction sector.

Taking stock, I presented the theoretical model of Jäger et al. (2022) and applied its insights to my context. The results suggest that as a result of the publicly announced introduction of the minimum wage treated workers in outside option industries updated their biased beliefs about the wages they could earn in the labor market. The information shock revealed information about workers' current establishment quality. Therefore, workers moved to better-paying establishments and experienced higher wage growth.

[27]Table A14 shows the results in table form including the partially treated group, the number of observations and standard errors.

5.5 Other Sectoral Minimum Wages

Up to this point, I have analyzed in detail the spillover effects from the minimum wage in the main construction sector. In this subsection, I zoom out and analyze the spillover effects of other sectoral minimum wages. The goal is to understand which economic contexts favor positive spillover effects and which are more likely to lead to no or negative spillover effects.[28]

By using the same identification strategy on the worker-level as for the analysis of the main construction sector minimum wage, I can analyze the wage spillover effects of other sectoral minimum wages on exposed workers in outside option industries. The electrical trade and roofing sector minimum wages were introduced at the same time as the main construction sector minimum wage. Therefore, I use Equation 4 to estimate the spillover effects from these sectors. However, the 3-digit industries that fall into the outside option and non-outside option classification differ from the industries that fall into these categories in the main construction sector. To estimate the spillover effects from all other sectoral minimum wages, I use a generalized version of Equation 4 in which I use three pre-periods:

$$w_{i,t+2}-w_{i,t} = \alpha_i + \zeta_t + \sum_{\tau=-3}^{3} \beta_\tau Treated_{i,t} \times Option_{i,t} \times \mathbb{1}_{[t=\tau]} + \sum_{\tau=-3}^{3} \gamma_\tau Partial_{i,t} \times Option_{i,t} \times \mathbb{1}_{[t=\tau]} + \delta X_{i,t} + \epsilon_{i,t}, \tag{16}$$

where $\tau = -3$ are 3 periods prior to the announcement of the sectoral minimum wage and $\tau= 3$ is the period in which the sectoral minimum wage was introduced. The reference period is $\tau = -1$. I define treated (sub-minimum wage) workers as workers with an hourly wage below the respective minimum wage, and use the same thresholds as in Section 3.3 to define the partially treated and control group. I define outside option and non-outside option industries by using the procedure outlined in Section 3.3 and use the same control variables as in Equation 4.

Figure 13 illustrates the results. The y-axis displays the coefficient estimates of the triple interaction and the x-axis indicates the time period. As information treatments could be important in the context of spillover effects (Section 5.4), I expect to find spillover effects one year prior to the introduction of each minimum wage (e.g., I expect spillover effects from the painting & varnishing sector minimum wage in 2000-02). I find positive wage spillover effects on sub-minimum wage workers in outside option industries from the electrical trade minimum wage, the roofing minimum wage, the painting & varnishing minimum wage, the waste removal minimum wage, the security minimum wage, and the temporary work minimum wage. I find negative wage spillover effects on sub-minimum wage workers in outside option industries from the commercial cleaning minimum wage and the nursing care minimum wage. Positive

[28]Because I do not want to capture possible effects of the federal minimum wage, I do not analyze the spillover effects from the scaffolding sector, stonemasonry sector, hairdressing sector, textile & clothing sector, chimney sweeping sector, slaughtering & meat processing sector, and the agriculture, forestry & gardening sector. These sectoral minimum wages were either introduced shortly before or right at the federal minimum wage was introduced which makes it difficult to distinguish possible anticipation or direct effects from the federal minimum wage introduction (see Table 1).

wage spillover effects range from 1.1% from the minimum wage in the roofing sector to 4.3% from the minimum wage in the security sector. Negative wage spillover effects range from 2.3% from the minimum wage in the commercial cleaning sector to 3.8% from the minimum wage in the nursing care sector. Note that, even though the waste removal sector and nursing care sector minimum wages were introduced in the same year, their spillover effects on the respective outside option industries differ greatly. This provides additional evidence that my identification strategy does not capture year-specific common shocks to low-wage earners, but rather spillover shocks that affect only low-wage earners in specific industries.

The sectors with minimum wages that had negative spillover effects clearly differ from the other minimum wage sectors in that they employ a high proportion of women in part-time or mini-jobs (see Table A2). Since my sample only includes full-time workers, I interpret the different signs of the spillover effects for the commercial cleaning and nursing care sector as an indication that positive wage spillover effects can only occur when workers in the minimum wage sector are in a similar employment relationship. For example, because switching from full-time to part-time is associated with substantial earnings declines (for workers with similar hourly wages), full-time workers might compare their wages only with other full-time jobs or switch to the minimum wage sector only if it also offers sufficient full-time jobs.

6 Conclusion

Firms differ in the wages they pay to equally skilled workers even if they are in similar jobs. Wage and information shocks related to potential outside options for workers currently in bad jobs could shed light on why workers stay in those bad jobs in the first place. In this paper, I investigate whether and why publicly announced sectoral minmum wages had spillover effects on sub-minimum wage workers outside the targeted sectors in similar jobs. I find that sub-minimum wage workers in outside option industries experienced an increase in their wage growth that was driven by switching to new jobs in establishments with better average pay and higher wage premium to the same type of worker. I find that the reduction of information frictions, due to the public announcement and media coverage of the main construction sector minimum wage, seems to have been the most likely mechanism for the positive wage spillover effects. Thus, the public announcement of sectoral minimum wages had an unexpected benefit, informing workers with bad jobs of their possible outside options and encouraging them to look for new and better-paying jobs. The unsolicited public disclosure of the minimum wages, along with its prominent placement in the media, set them apart from other wage transparency laws and may account for their effectiveness (Brütt and Yuan, 2022).

Using the same data and identification strategy, I find that the spillover effects are about one-third of the wage effects within the main construction sector. A back-of-the-envelope calculation suggests that those exposed to the spillover effects earned on average 383 Euro more every year after the public announcement of the minimum wage than they would have earned without the public announcement.[29] If we take into account that sub-minimum wage workers earned an average of 19,188 Euro annually before the minimum wage was announced, this shows that the spillover effects have led to a substantial improvement in the income situation of low-wage employees. Moreover, because low-paying establishments are less productive than high-paying establishments (Abowd et al., 1999), the reallocation of employment from low-paying establishments to high-paying establishments may have increased the welfare of the economy as a whole. Furthermore, when analyzing the wage spillover effects of other sectoral minimum wages, I only find positive wage spillover effects for sub-minimum wage workers in outside option industries if workers in the minimum wage sector are in a similar employment relationship compared to them. In the case where the typical employment relationship in the minimum wage sector is not similar, I find only negative wage spillover effects from the introduction of the respective minimum wage.

The current German government is again increasingly thinking in the direction of generally binding collective agreements in order to set sectoral minimum wages. Two of the three governing parties have announced in their government programs that they will facilitate the introduction of generally binding collective agreements (SPD, 2017, 2021; Greens, 2021). In the coalition agreement, the government parties agreed to tie public payments to compliance with a representative collective agreement for the respective sector (SPD et al., 2021). In this context, the current German government has already passed the *Gesundheitsversorgungsweiterentwicklungsgesetz* (Health Care Advancement Act), which will restrict public payments to care facilities that pay their employees according to collective agreements. In this paper, I have shown that publicly disclosed sectoral collective agreements can have a significant signaling effect on the low-wage labor market and thus have positive wage and reallocation effects far beyond the boundaries of the sector actually affected.

[29] On average, sub-minimum wage workers in my sample earned 52.57 Euro daily before the public announcement of the minimum wage (Table 2). Two-year wage growth was 11% before the public announcement. Thus, daily wages grew by an average of 5.78 Euro every two years. After the public announcement, the daily wage grew by 13% every two years and thus by an average of 6.83 Euro every two years. For a continuously employed person this means on average (1.05 Euro × 365 days) 383 Euro more every year.

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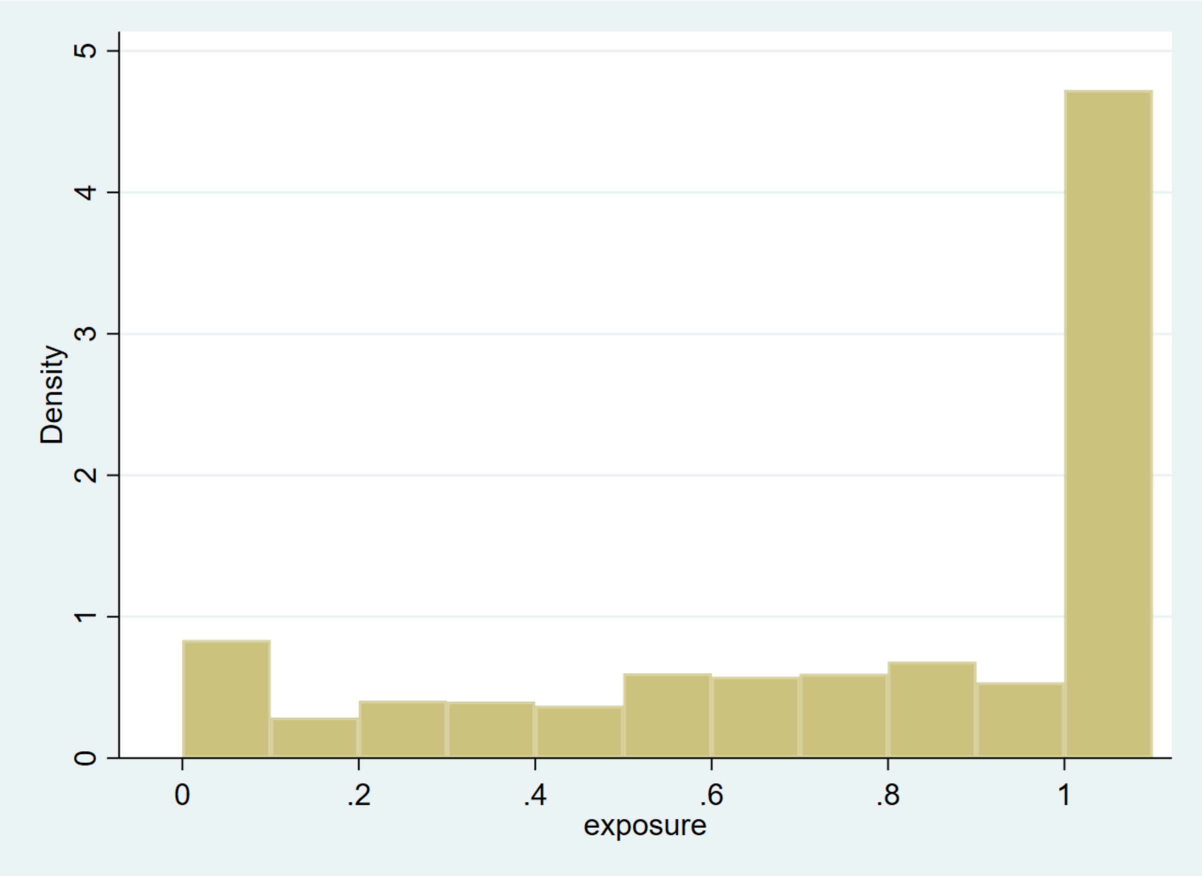
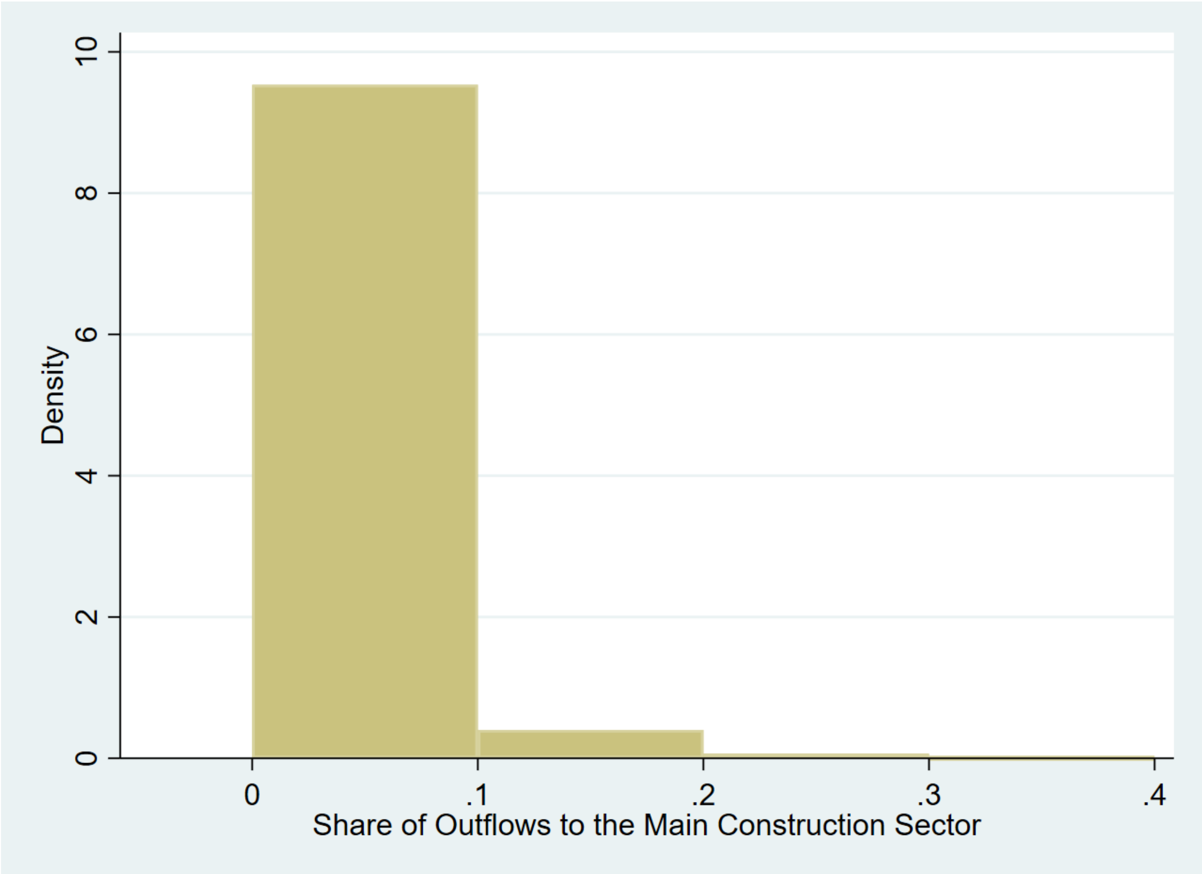
Figure 1: Density of the Continuous Establishment Exposure Measure



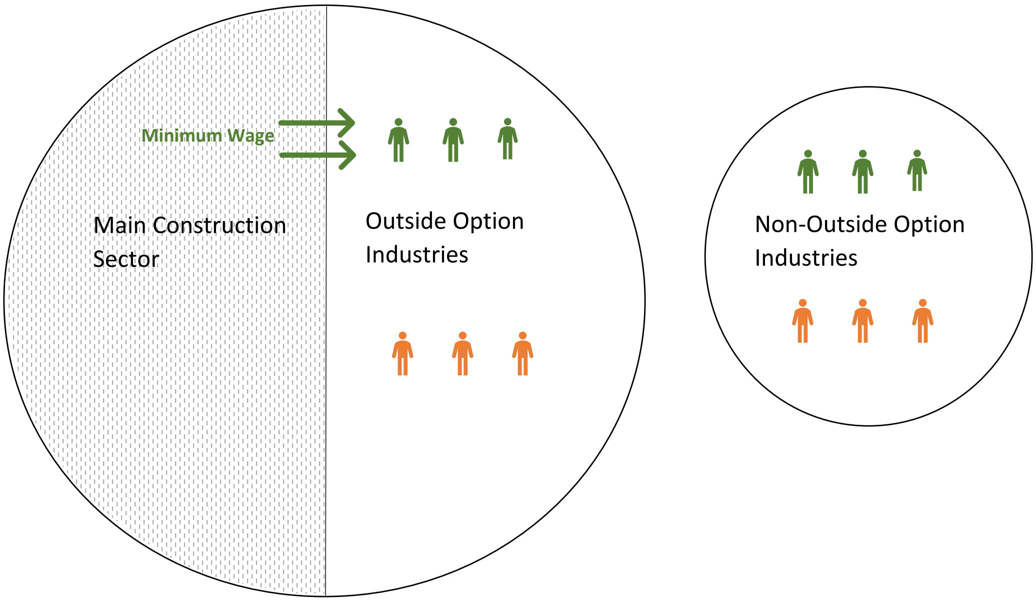
Notes: For this figure, I keep only one observation per establishment in the period 1992–95. **Source:** SIEED and BHP 1992–95. Authors' calculations.

Figure 2: Density of the Share of Outflows to the Main Construction Sector by 3-digit Industries



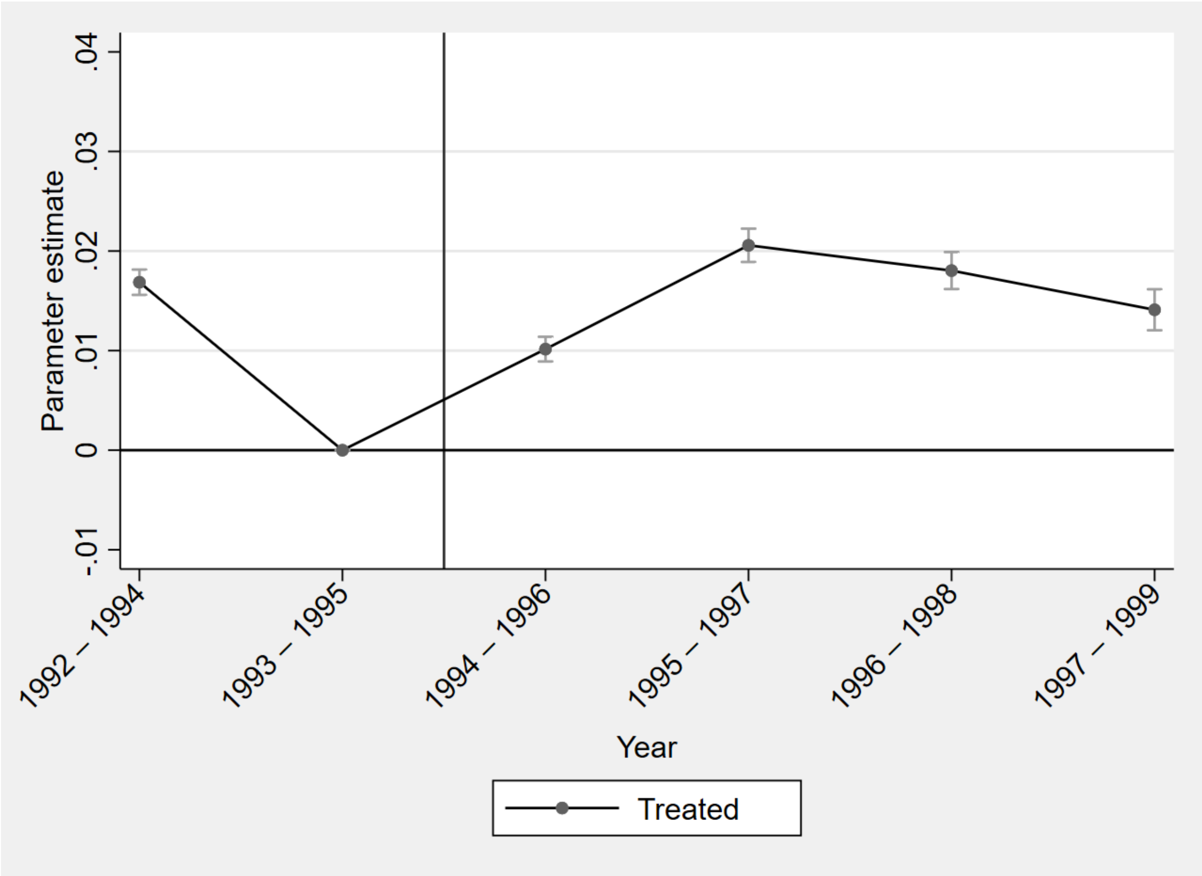
Notes: For this figure, I only keep observations from the period 1992–95 and drop all observations with missing two-year wage growth or treatment assignment. The figure shows the share of outflows to the main construction sector by 3-digit industries weighted by the number of workers in each industry from 1992–95. **Source:** SIEED and BHP 1992–95. Authors' calculations.

Figure 3: Illustration of the Triple Differences Identification Strategy



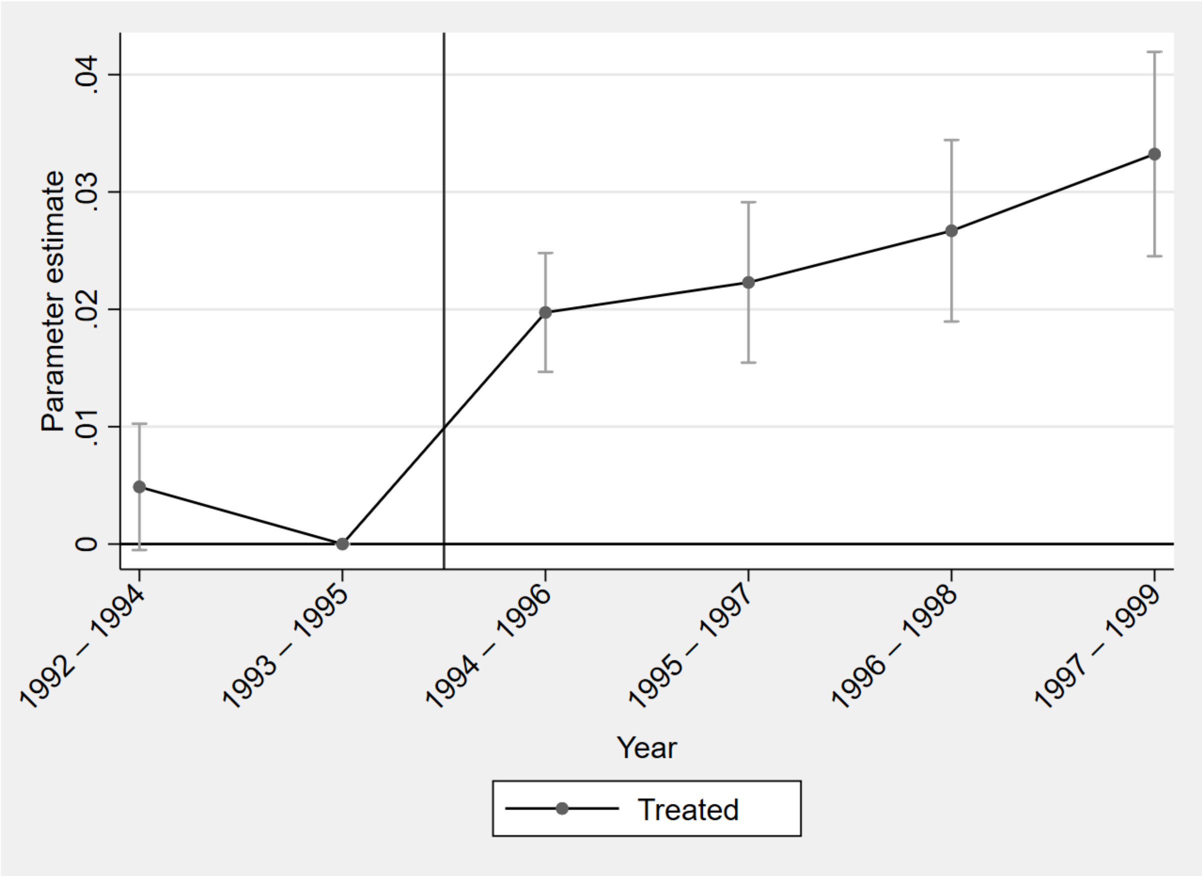
Notes: This figure illustrates the triple differences identification strategy from Equation 4. The green individuals in the top half of the figure represent the treated workers, while the orange individuals in the bottom half of the image represent the control group. The main construction sector and outside option industries share one common labor market. However, the minimum wage was only introduced in the main construction sector. I expect this minimum wage to have spillover effects on treated workers in outside option industries. Non-outside option industries are outside this common labor market and serve as an additional control group.

Figure 4: Difference-in-differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage



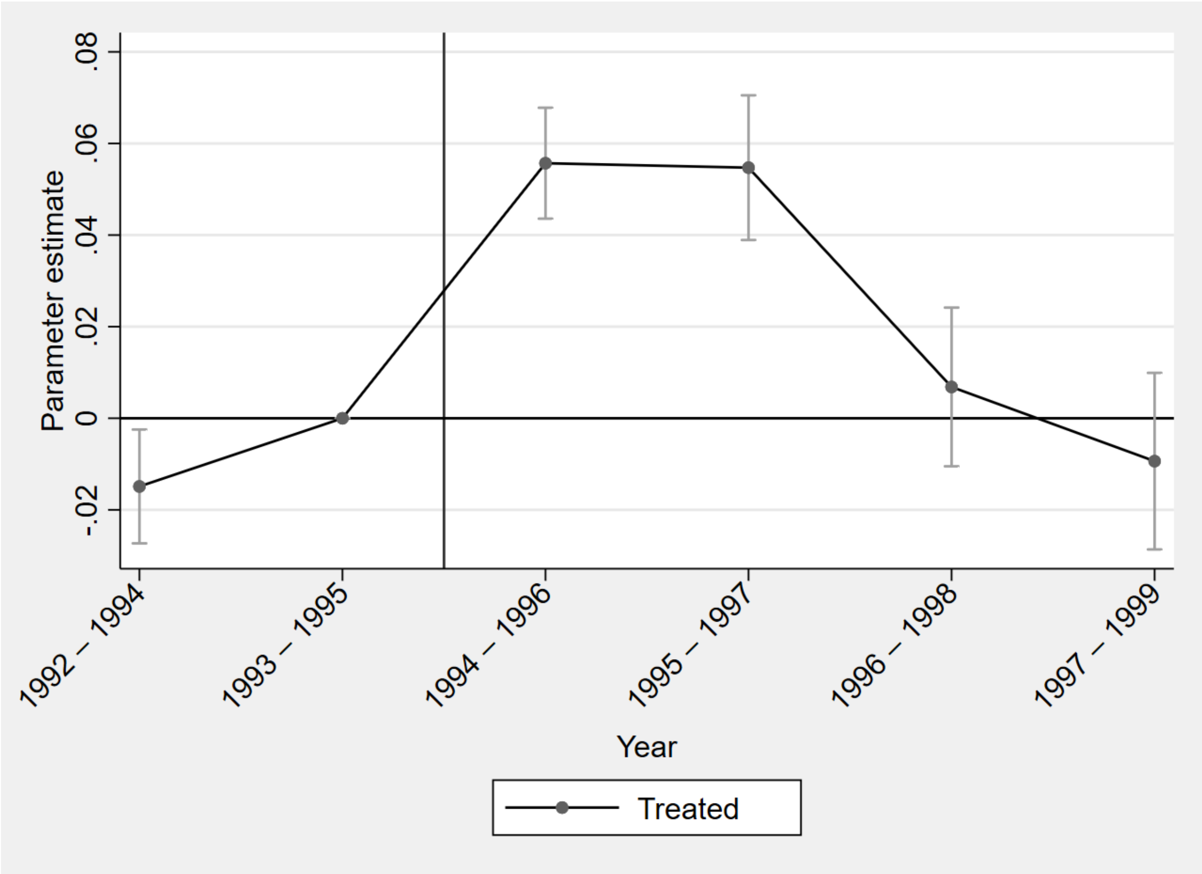
Notes: This figure illustrates the results of the difference-in-differences specification with the two-year change in log daily wages as the outcome (see Equation 3). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Column 3 of Table A5 illustrates this result in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Authors' calculations.

Figure 5: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage



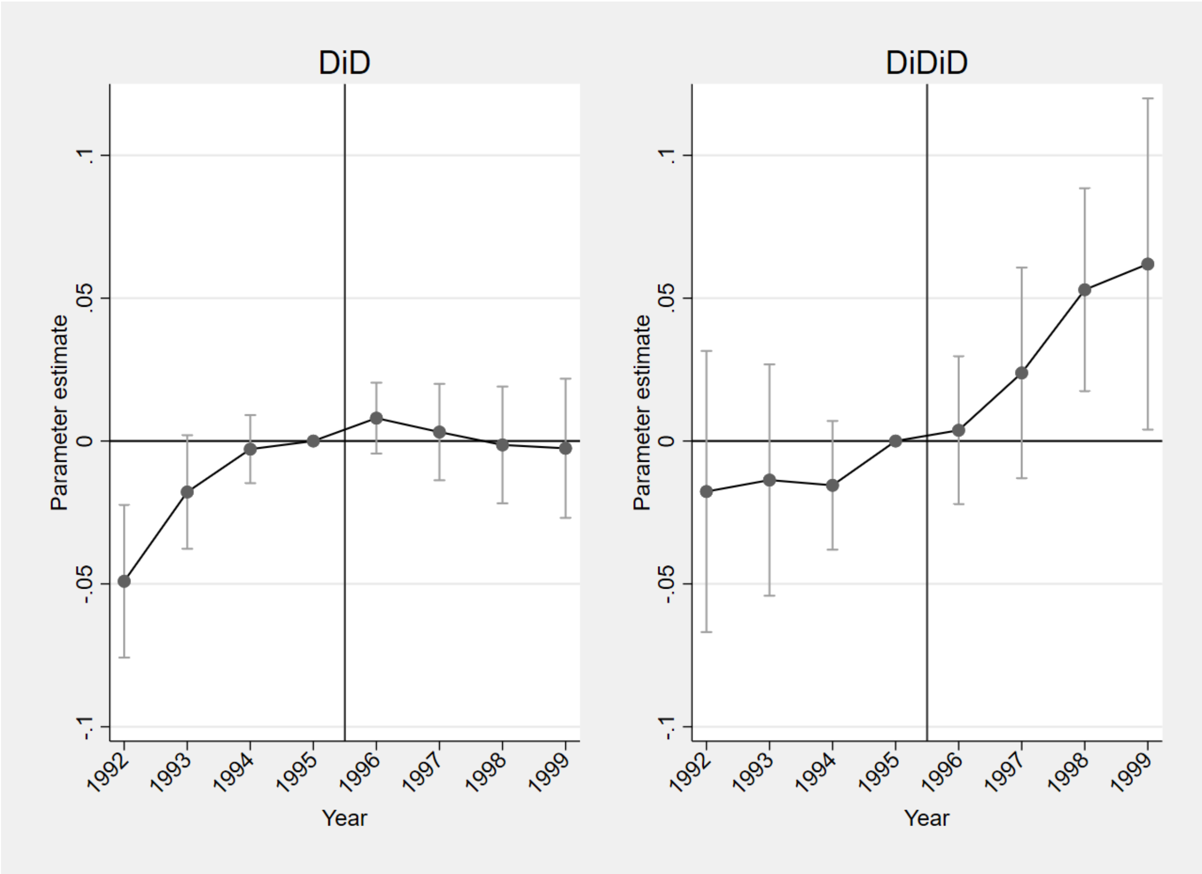
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Column 4 of Table A6 illustrates this result in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Authors' calculations.

Figure 6: Triple Differences: Probability to Switch Establishments



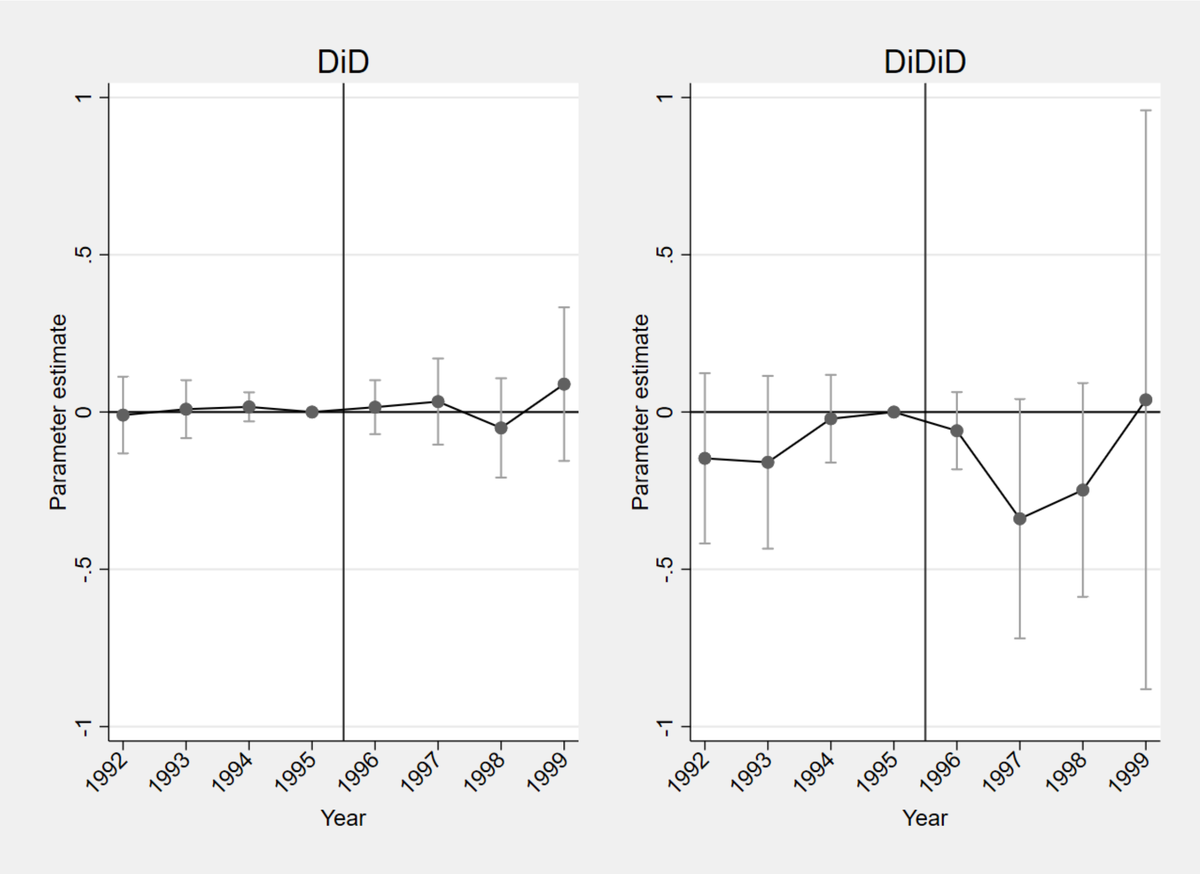
Notes: This figure shows the result of a triple differences specifications using the probability to switch establishments as the outcome variable (see Equation 4). I use 95% confidence intervals. The variable takes the value 1 if the individual switched establishments from t to $t + 2$ and 0 if she did not. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Table A9 illustrates these results in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Author's calculations.

Figure 7: Establishment-Level: Wage Spillovers from the Main Construction Sector Minimum Wage



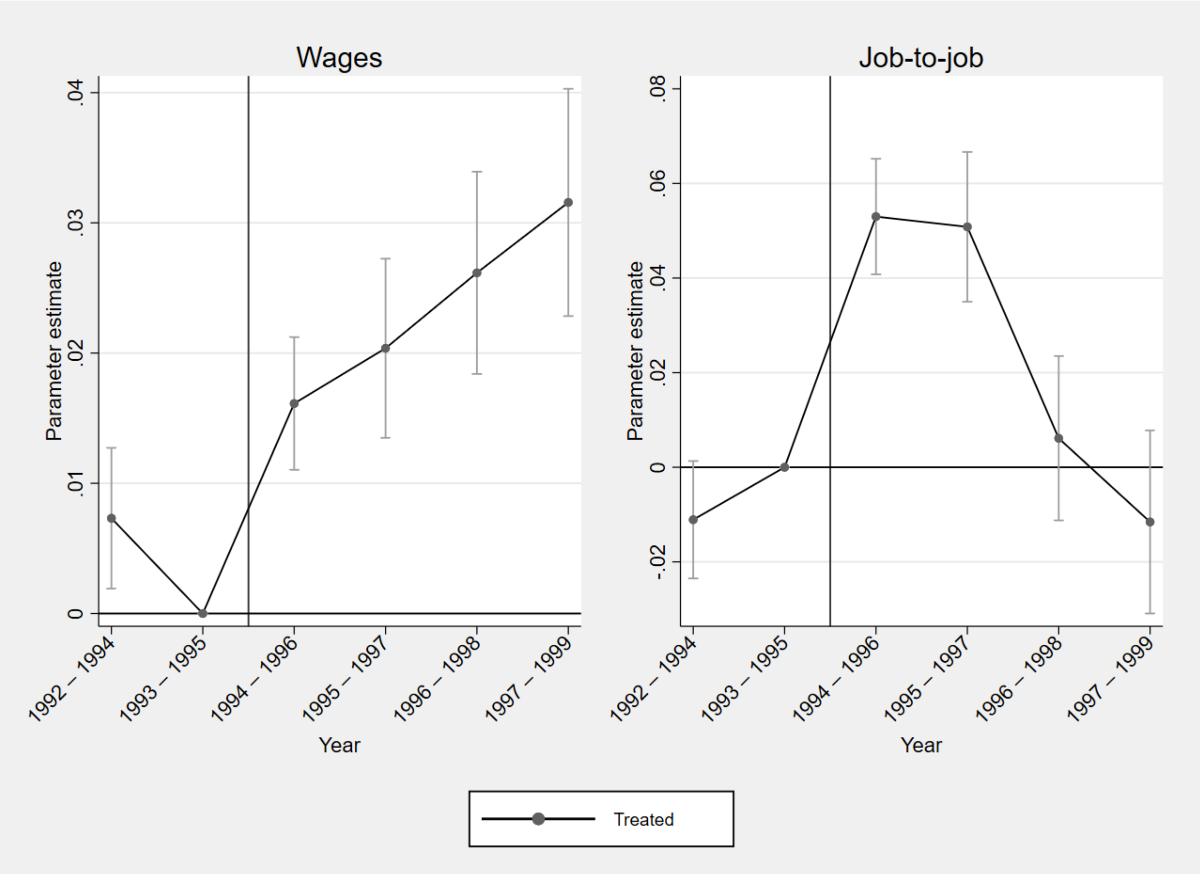
Notes: The outcome variable is the log (daily) average wage. In the panel DiD, I estimate Equation 6 and in the panel DiDiD, I estimate Equation 7. Both estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Tables A10 and A11 illustrate these results in table form including the number of observations and standard errors. **Source:** SIEED and BHP 1992–99. Author's calculations.

Figure 8: Establishment-Level: Employment Effects from the Main Construction Sector Minimum Wage



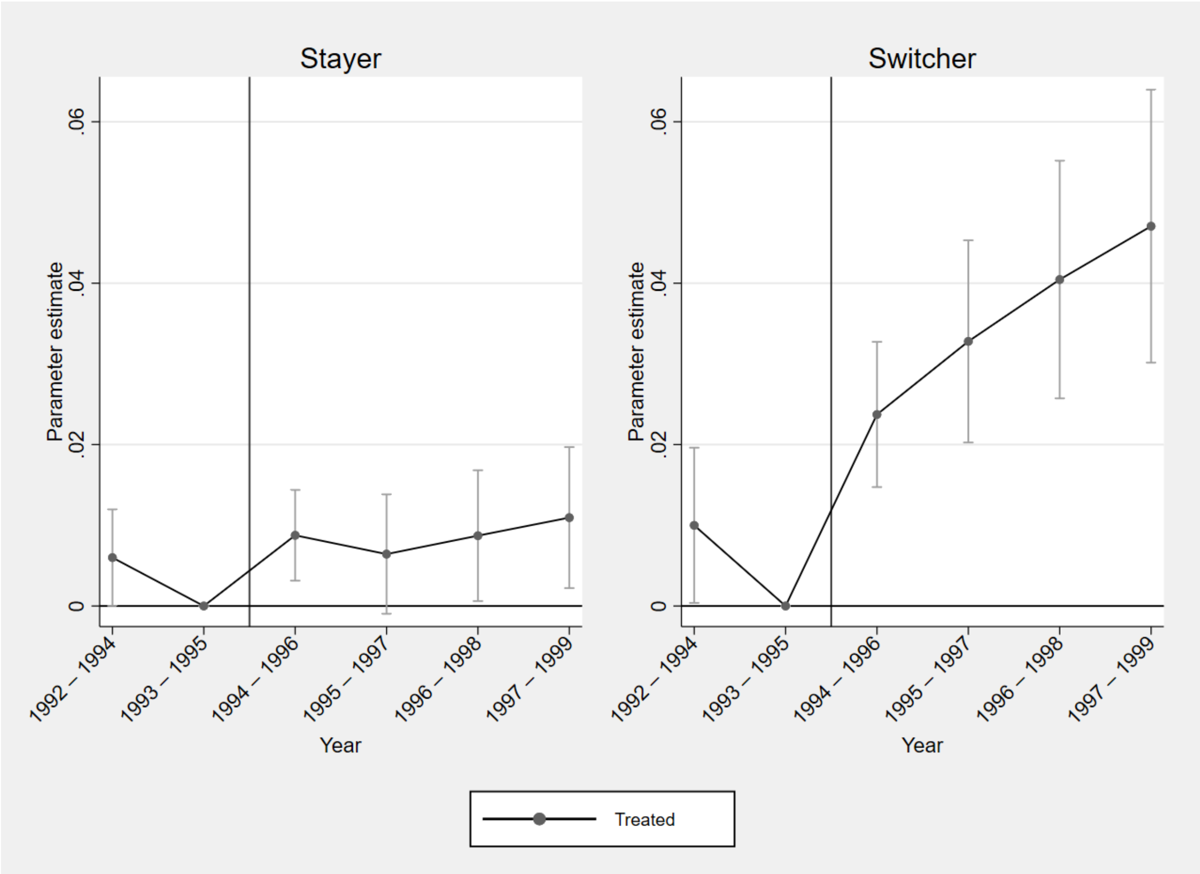
Notes: The outcome variable is the log number of full-time employed workers (according to sample restrictions). In the panel DiD, I estimate Equation 6 and in the panel DiDiD, I estimate Equation 7. Both estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Tables A10 and A11 illustrate these results in table form including the number of observations and standard errors. **Source:** SIEED and BHP 1992–99. Author's calculations.

Figure 9: Triple Differences: Wage Spillover and Reallocation Excluding Switches to Main Construction



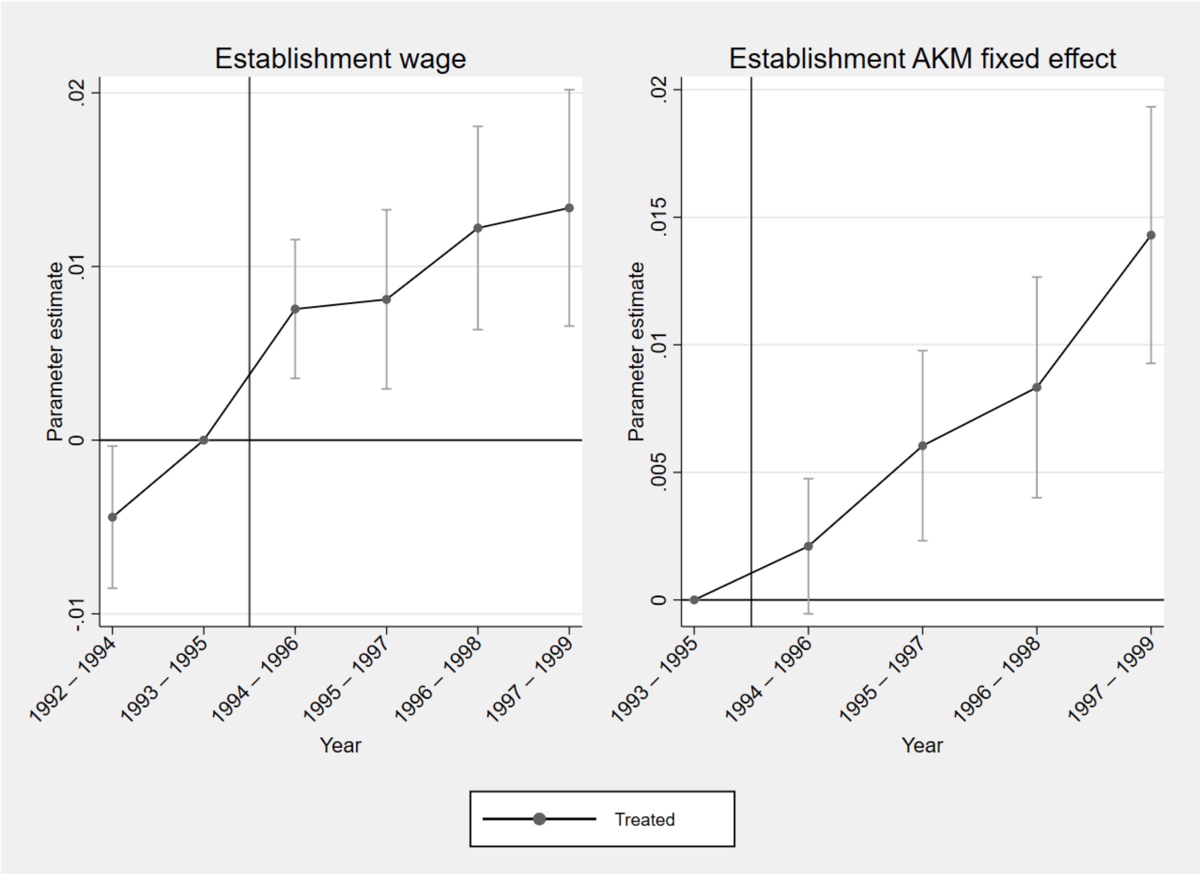
Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 4) and excluding switchers to the main construction sector from t to $t + 2$. I use 95% confidence intervals. In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable, which takes the value 1 if the individual switched establishments from t to $t + 2$ and 0 if she did not. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 10: Triple Differences: Wage Spillover for Stayers vs. Switchers



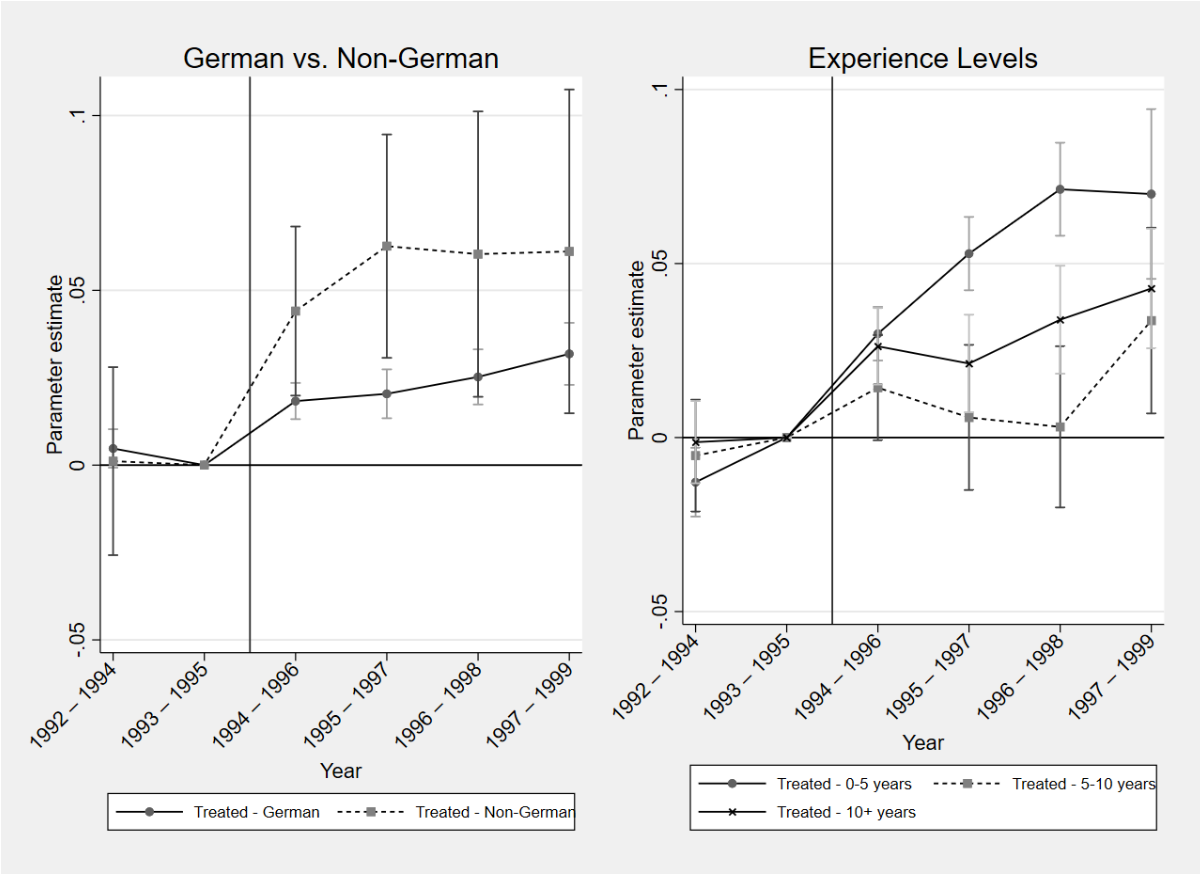
Notes: This figure shows the results of two triple differences specifications using the two-year change in log daily wages as the outcome (see Equation 4). I define Stayers as workers who stayed within the same establishment during the 1994–97 period. Switchers are workers who changed establishments at least once from t to $t+2$ during 1994–97. For the left panel, I use a sub-sample of Stayers. For the right panel, I use a sub-sample of Switchers. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 11: Triple Differences: Reallocation to Higher-Paying Establishments



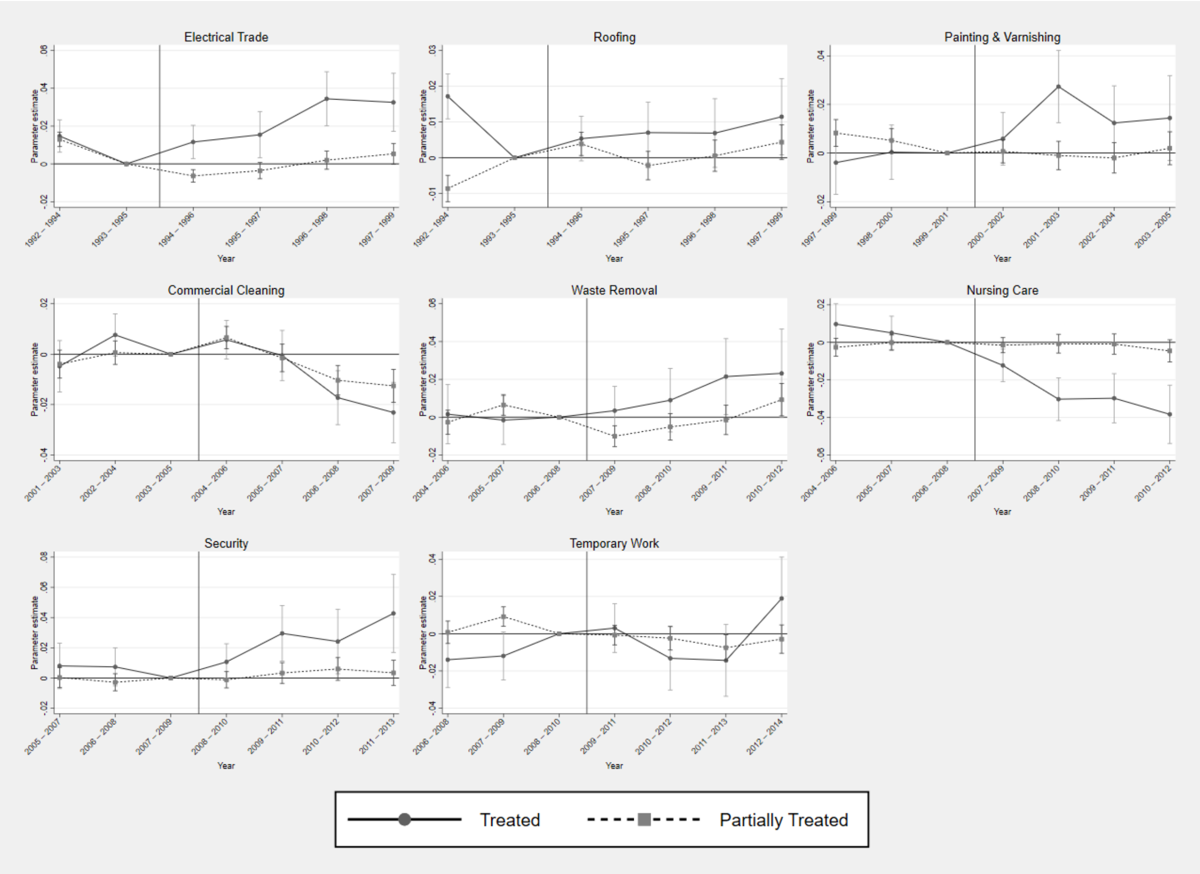
Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 4). I use 95% confidence intervals. In the first panel, I use the change in log establishment average imputed wages as the outcome variable. Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In the second panel, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Table A12 illustrates these results in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Author's calculations.

Figure 12: Triple Differences: Heterogeneity in Wage Spillover Effects



Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. The figure illustrates the coefficients only for treated workers. In the first panel, I present the results separately for sub-samples of workers with German nationality and workers with non-German nationality. In the second panel, I present the results separately for sub-samples of workers with 0 to 5 years, 5 to 10 years, and 10+ years of labor market experience. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95.
Source: SIEED and BHP. Authors' calculations.

Figure 13: Triple Differences: Wage Spillover Effects from Other Sectoral Minimum Wages



Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 16). I use 95% confidence intervals. In each panel, I present the wage spillover effects from the minimum wages of different sectors. Thus, I compare the wage growth of (partially) treated versus control group workers in outside option versus non-outside option industries. The definition of (partially) treated, control group, outside option and non-outside option industries changes for the analysis of spillover effects from each minimum wage sector. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. **Source:** SIEED and BHP. Authors' calculations.

Table 1: Sectoral Minimum Wages in Germany

Sector	First MW	Hourly Wage (in Euro)
Main Construction	01/1997	West (incl. Berlin) 8.69; East 8.00
Electrical Trade	06/1997	West 8.03; East (incl. Berlin) 6.41
Roofing	10/1997	West (incl. Berlin) 8.18; East 7.74
Painting & Varnishing	12/2003	West (incl. Berlin) 7.69; East 7.00
Commercial Cleaning	07/2007	West (incl. Berlin) 7.87; East 6.36
Waste Removal	01/2010	8.02
Nursing Care	08/2010	West (incl. Berlin) 8.50; East 7.50
Security	06/2011	Federal states: ranges from 6.53 to 8.60
Temporary Work	01/2012	West 7.89; East (incl. Berlin) 7.01
Scaffolding	08/2013	10.00
Stonemasonry	10/2013	West (incl. Berlin) 11.00; East 10.13
Hairdressing	11/2013	West 7.5; East (incl. Berlin) 6.5
Chimney Sweeping	04/2014	12.78
Slaughtering & Meat Processing	08/2014	7.75
Textile & Clothing	01/2015	West 8.5; East (incl. Berlin) 7.5
Agriculture, Forestry & Gardening	01/2015	West 7.4; East (incl. Berlin) 7.2

Table 2: Descriptives for Main Construction Sector Spillover Groups (1992–95)

	Treated Group		**Partially Treated Group**		**Control Group**	
No. of observations	878,392		1,502,064		1,203,169	
Share	24.51		41.91		33.57	
Averages						
Daily wage	52.57	(11.38)	82.27	(8.69)	107.30	(8.71)
Log (daily) wage	3.93	(0.25)	4.40	(0.11)	4.67	(0.08)
Log (daily) two-year wage growth	0.11	(0.24)	0.03	(0.15)	0.01	(0.14)
Shares within group (in percent)						
Women	59.47		39.58		25.93	
Non-German nationality	8.37		8.83		8.33	
By age						
18-25 years old	26.75		20.02		7.67	
26-35 years old	34.81		42.67		43.26	
36-45 years old	24.42		23.10		29.94	
46-55 years old	12.36		12.05		16.14	
56-65 years old	1.66		2.15		3.00	
By education						
No vocational training	12.98		11.68		9.04	
Vocational training	84.01		83.82		82.59	
University or university of applied sciences	2.25		4.11		8.03	
Missing education	0.75		0.39		0.35	
By industry						
Agriculture and Forestry	2.47		1.05		0.42	
Fishing and Fish Farming	0.02		0.01		0.01	
Mining	0.39		1.65		2.86	
Manufacturing	23.55		30.80		37.72	
Energy and Water Supply	0.23		0.88		1.65	
Construction	2.77		3.35		2.50	
Trade and Repair	24.64		20.24		13.06	
Catering	10.77		2.18		0.84	
Transport and News	7.15		10.27		10.73	
Finance and Insurance	0.69		2.12		3.96	
Real Estate and Housing	8.80		6.14		6.15	
Public Services	3.78		8.87		7.99	
Education	1.08		2.15		2.80	
Health	7.70		7.53		6.45	
Other Services	5.11		2.39		2.65	
Private Household	0.42		0.33		0.19	
Missing industry	0.41		0.05		0.02	
By plant size						
Very small (1-4 workers)	21.88		7.50		3.84	
Small (5-19 workers)	29.21		19.93		13.44	
Medium (20-249 workers)	35.57		40.31		37.01	
Large (250-999 workers)	8.66		18.30		22.49	
Very large (1000+ workers)	4.68		13.96		23.22	
By region type						
District-free cities	30.28		36.84		43.08	
Urban districts	27.05		33.40		36.65	
Rural districts, some densely populated areas	20.49		15.21		11.30	
Rural districts, sparsely populated	22.19		14.56		8.98	

Notes: Observations are worker-year combinations. Standard deviation in parentheses. The groups are defined by using the nominal hourly wage of a worker at year *t*. Daily wages are deflated using the consumer price index of the Federal Statistical Office. For workers in West Germany, I use the nominal main construction minimum wage of 8.69 Euro and for workers in East Germany 8.00 Euro as a threshold (see Table 1).
Source: SIEED and BHP, 1992–1995. Authors' calculations.

Table 3: Triple Differences: Pre- vs. Post-Period Specifications

	2-year wage growth	Job-to-job
Treated x Option x Post	0.021***	0.037***
	(0.003)	(0.006)
Partial x Option x Post	0.011***	0.037***
	(0.001)	(0.005)
No. of observations	761,276	796,763
No. of workers	177,647	194,574
Year fixed effects	yes	yes
1-digit industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: Standard errors in parentheses. The table shows specifications of Equation 5 with different outcome variables. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table 4: Triple Differences: Robustness Checks on Wage Spillovers

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Treated x Option							
x 1992-94	0.005*	-0.008***	0.006*	-0.003	0.006**	-0.007*	0.006***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
x 1994-96	0.020***	0.024***	0.014***	0.017***	0.019***	0.020***	0.025***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
x 1995-97	0.022***	0.029***	0.007*	0.013***	0.020***	0.022***	0.024***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
x 1996-98	0.027***	0.037***	0.010**	0.019***	0.024***	0.011**	0.036***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
x 1997-99	0.033***	0.046***	0.016***	0.028***	0.031***	0.015***	0.044***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)
Partial x Option							
x 1992-94	-0.006***	-0.006***	-0.009***	-0.007***	-0.006***	-0.005***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1994-96	0.009***	0.009***	0.007***	0.008***	0.008***	0.003**	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1995-97	0.007***	0.010***	0.001	0.003	0.006***	0.004*	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1996-98	0.009***	0.015***	0.002	0.006***	0.009***	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1997-99	0.008***	0.015***	0.000	0.006**	0.007***	-0.001	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
No. of observations	761,276	752,408	761,276	752,408	754,698	761,276	2,117,788
No. of workers	177,647	175,700	177,647	175,700	176,157	177,647	481,939
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes
LMR x year fixed effects	no	yes	no	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	no	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in log daily wages as the outcome variable (see Equation 4). Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 5 and Table A6. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the sixth column, I use a time-constant treatment variable. In the seventh column, I change the $Option_{it}$ variable to be equal to 1 if an individual i is working in an occupation that had large outflows to the main construction sector at year t and equal to 0 if an individual i is working in an occupation that had low outflows to the main construction sector at year t. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 5: Triple Differences: Robustness Checks on Job-to-Job Probability

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Treated x Option							
x 1992-94	-0.015**	0.001	0.008	0.018***	-0.014**	-0.028***	0.042***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1994-96	0.056***	0.069***	0.044***	0.057***	0.054***	0.078***	0.106***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1995-97	0.055***	0.058***	0.031***	0.040***	0.052***	0.079***	0.071***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.010)	(0.006)
x 1996-98	0.007	0.013	0.007	0.013	0.005	-0.016	0.044***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.006)
x 1997-99	-0.009	0.005	-0.024**	-0.009	-0.011	-0.044***	0.016**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.007)
Partial x Option							
x 1992-94	-0.004	0.002	0.013**	0.017***	-0.004	-0.011**	0.038***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
x 1994-96	0.059***	0.062***	0.052***	0.055***	0.058***	0.073***	0.029***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
x 1995-97	0.064***	0.064***	0.049***	0.052***	0.064***	0.088***	0.011**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)
x 1996-98	0.015**	0.018***	0.014**	0.017**	0.016**	0.005	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.005)
x 1997-99	0.009	0.019**	-0.003	0.007	0.010	-0.022***	-0.015***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.005)
No. of observations	796,763	787,452	796,763	787,452	789,906	796,763	2,207,206
No. of workers	194,574	192,416	194,574	192,416	192,959	194,574	524,356
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes
LMR x year fixed effects	no	yes	no	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	no	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in job-to-job transition as the outcome variable (see Equation 4). Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 6 and Table A9. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the sixth column, I use a time-constant treatment variable. In the seventh column, I change the $Option_{it}$ variable to be equal to 1 if an individual i is working in an occupation that had large outflows to the main construction sector at year t and equal to 0 if an individual i is working in an occupation that had low outflows to the main construction sector at year t. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

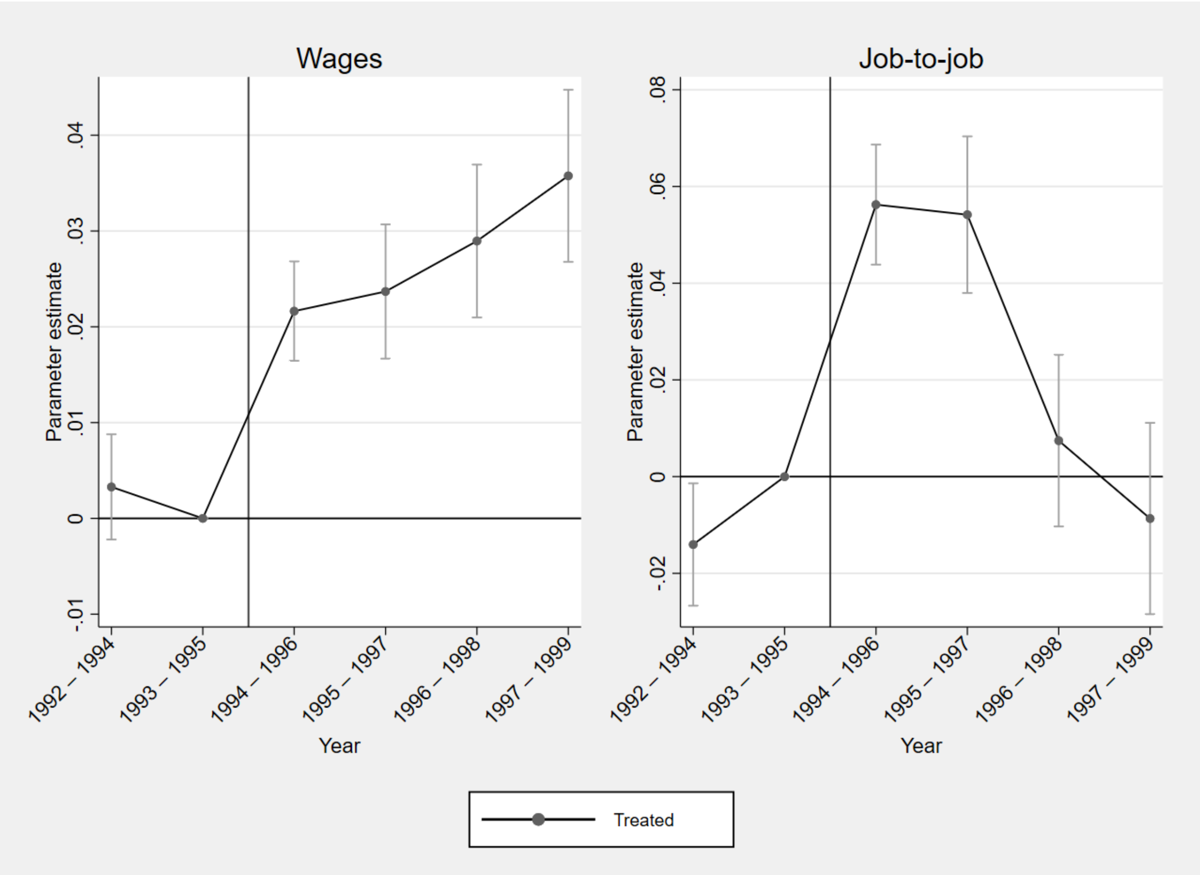
Table 6: Tests of Strategic Complementarity Model Predictions

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
Treated x Option x Post	0.021***	0.046***	0.055***	0.054***	0.006**	0.006**
	(0.003)	(0.005)	(0.003)	(0.006)	(0.003)	(0.003)
Partial x Option x Post	0.011***	0.018***	0.011***	0.032***	0.005***	0.004***
	(0.001)	(0.003)	(0.001)	(0.005)	(0.001)	(0.001)
Treated x Option x Middle x Post		-0.037***				
		(0.007)				
Treated x Option x High x Post		-0.025***				
		(0.007)				
Partial x Option x Middle x Post		-0.011***				
		(0.004)				
Partial x Option x High x Post		-0.010***				
		(0.004)				
Treated x Option x Bite x Post			-0.004	0.018***		
			(0.003)	(0.006)		
Partial x Option x Bite x Post			-0.001	0.013***		
			(0.001)	(0.005)		
Treated x Option x Switch x Post					0.021***	0.021***
					(0.005)	(0.005)
Partial x Option x Switch x Post					0.012***	0.016***
					(0.003)	(0.003)
No. of observations	761,276	752,408	817,826	176,319	761,276	746,624
No. of workers	177,647	175,700	150,801	42,836	177,647	173,237
Year fixed effects	yes	yes	yes	yes	yes	yes
1-digit industry fixed effects	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes
LMR fixed effects	no	yes	yes	yes	no	no
Excluding mcs switchers?	no	no	no	no	no	yes

Notes: Standard errors in parentheses. The table shows specifications using Equation 5 with the 2-year change in log (daily) wages as the outcome. The first column shows the baseline estimation, also illustrated in Table 3. In the second column, I interact the baseline triple interaction additionally with the terciles of the share of the main construction sector within a LMR. Where ”Middle” indicates workers who live in LMR in the middle tercile of the employment weighted distribution of shares of the main construction sector and ”High” indicates workers in the highest tercile of this distribution. In the third and fourth column, I interact the baseline triple interaction additionally with the bite of the main construction sector minimum wage in each labor market region. I calculate the bite as the share of employees who earned below the first minimum wage threshold in the pre-period within the main construction sector. I split the sample to West and East Germany. For West Germany, I additionally use the years 1989–91 in the analysis. I standardize the bite measure, separately for West and East Germany, to have mean zero and standard deviation 1. In the last two columns, I interact the baseline triple interaction additionally with a dummy variable for switching during the post-period. Specifically, this variable takes the value 1 if the worker changed establishments from t to $t+2$ in 1994-97, and 0 otherwise. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

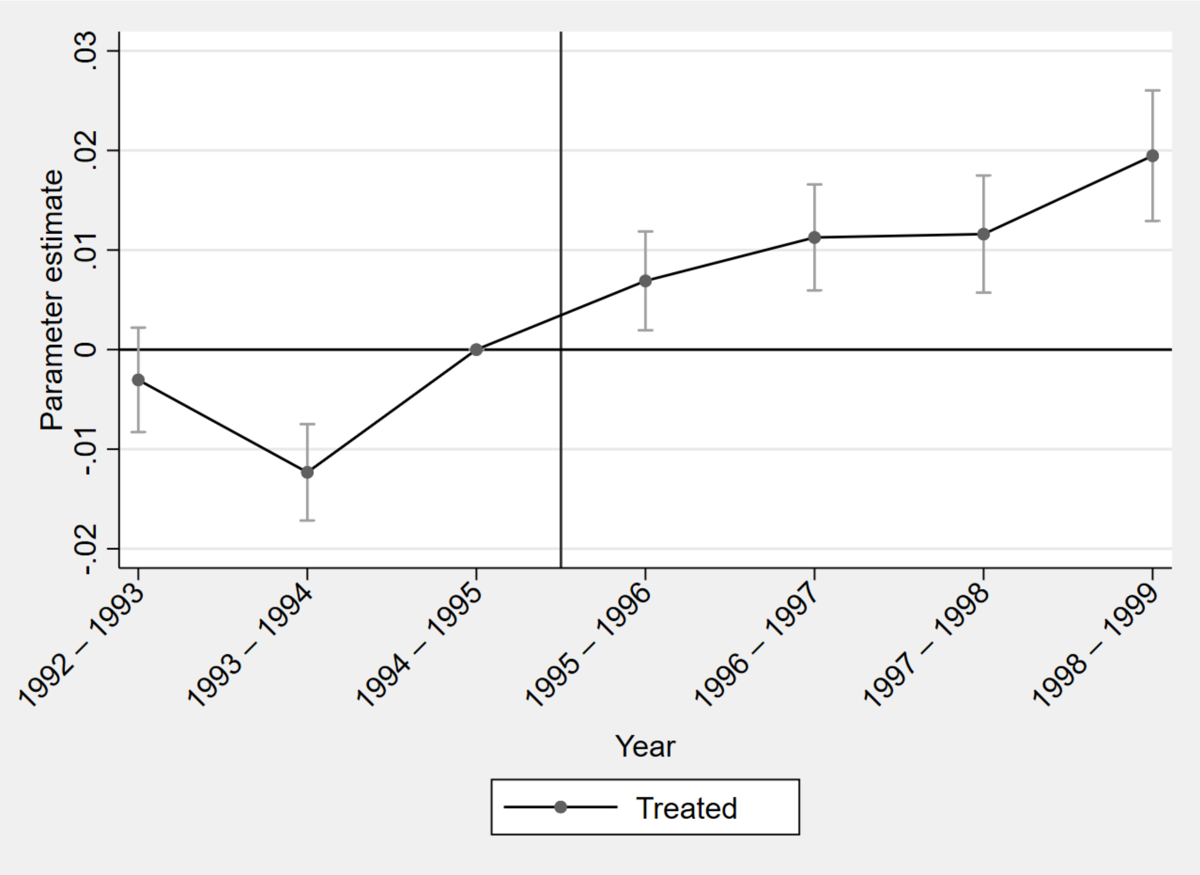
A Appendix

Figure A.1: Triple Differences: Excluding other Construction Industries from Outside Option Industries Classification



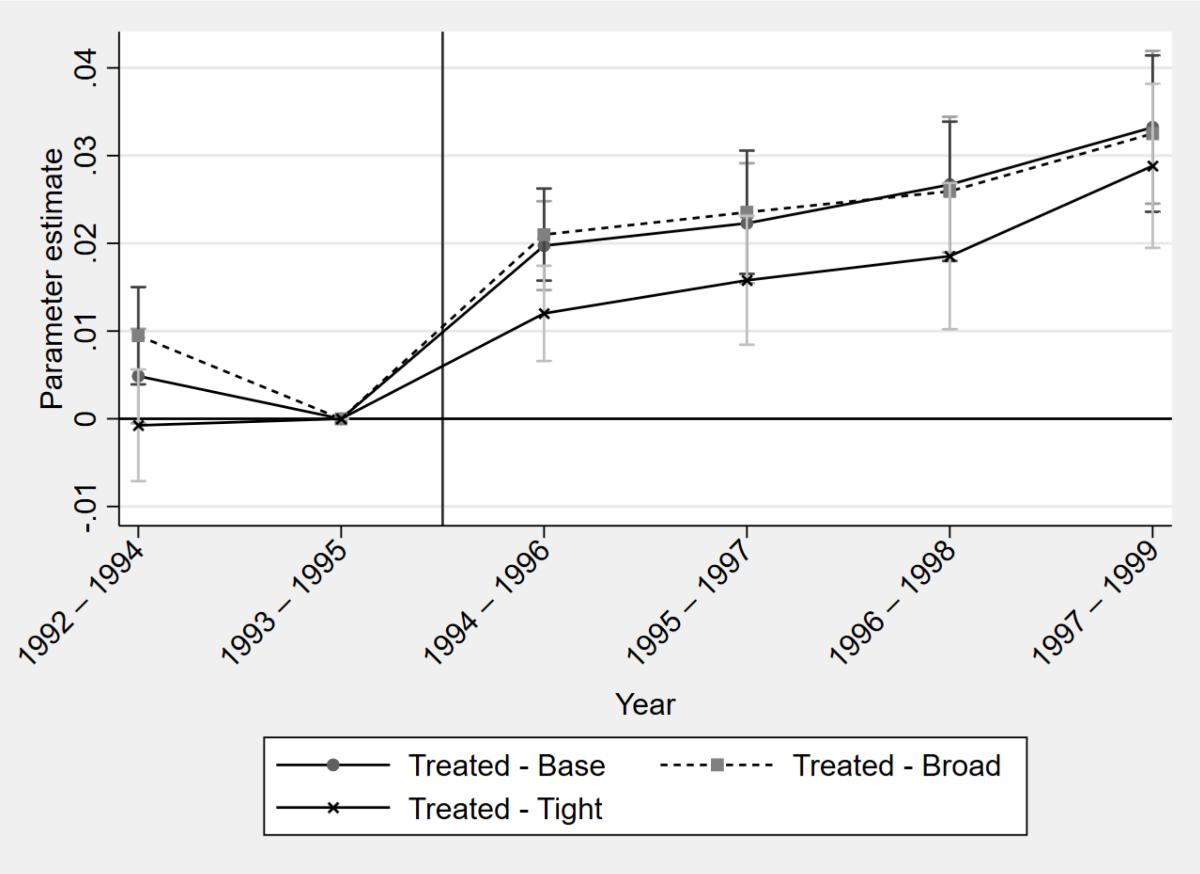
Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 4) and excluding other construction industries from the outside option industries classification in Table A3. Specifically, I drop the 3-digit industries 451, 452, 454, and 455. I use 95% confidence intervals. In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure A.2: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage. 1-Year Wage Growth Changes



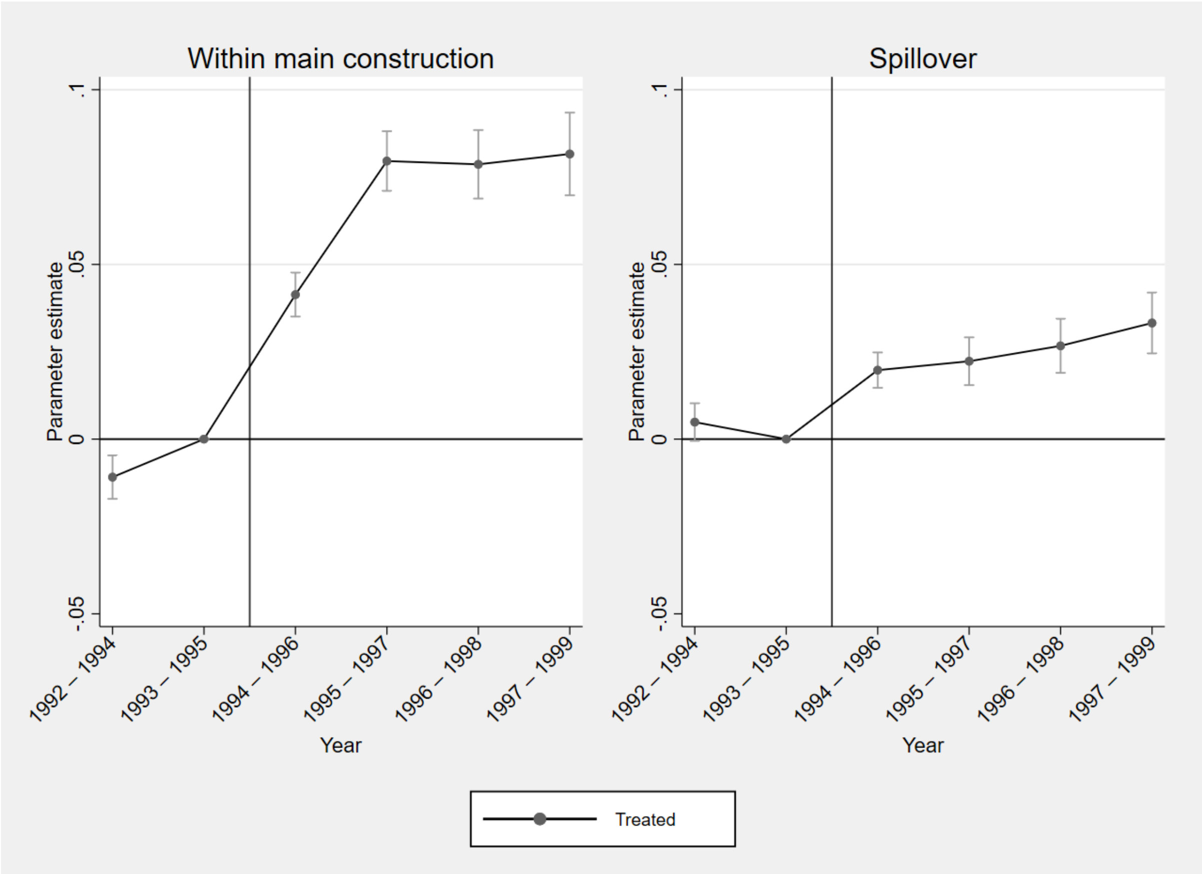
Notes: This figure illustrates the results of the triple differences specification with the one-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1994–95. **Source:** SIEED and BHP. Authors' calculations.

Figure A.3: Triple Differences: Different Bandwidths on Control Group



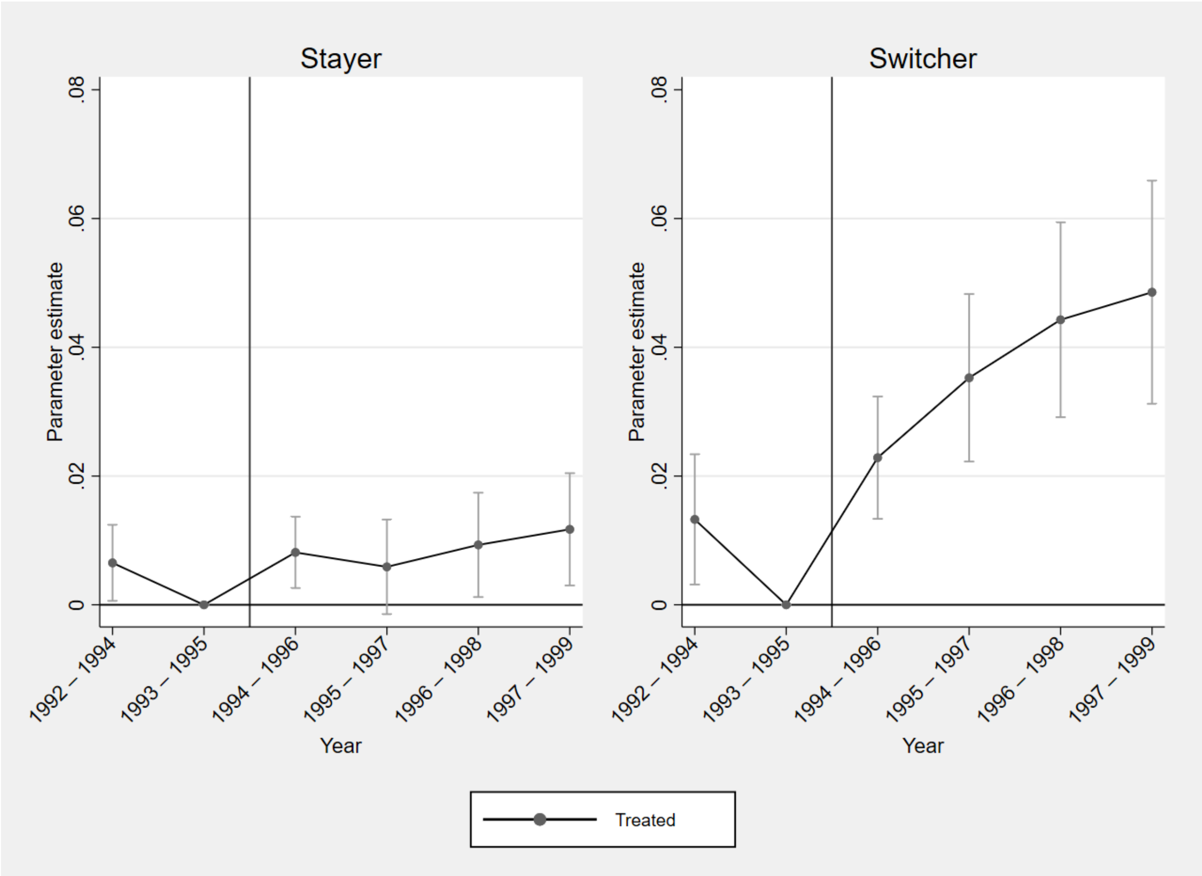
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. "Treated - Base" refers to the baseline approach in which the control group is defined with $MW + 40\% \leq h_{i,t} < MW + 80\%$, where MW refers to the minimum wage. In "Treated - Broad" I use $MW + 60\% \leq h_{i,t} < MW + 120\%$ to define the control group and in "Treated - Tight" I use $MW + 20\% \leq h_{i,t} < MW + 40\%$. In all three cases, I use the outside option industries and non-outside option industries in Tables A3 and A4. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure A.4: Triple Differences: Wage Growth Effects within the Main Construction Sector



Notes: In the first panel of this figure, I estimate the within-effects of the minimum wage in the main construction sector by using a similar triple differences specification as in Equation 4. The only difference is that I compare the DiD in the main construction sector itself with the non-outside option industries. For comparison, the second panel shows the baseline specification with triple differences to estimate spillover effects. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure A.5: Triple Differences: Wage Spillover for Stayers vs. Switchers. Excluding Switchers to the Main Construction Sector.



Notes: This figure shows the results of two triple differences specifications using the two-year change in log daily wages as the outcome (see Equation 4). I define Stayers as workers who stayed within the same establishment during the 1994–97 period. Switchers are workers who changed establishments at least once from t to $t+2$ during 1994–97. For the left panel, I use a sub-sample of Stayers. For the right panel, I use a sub-sample of Switchers. In both panels, I exclude switchers to the main construction sector in any period during the observation window. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Table A1: Classification of Sectoral Minimum Wages

Sector	WZ73 (1975–2002)	WZ93 (1999–2003)	WZ03 (2003–2008)	WZ08 (from 2008)	First MW
Main Construction	590/ 591/ 592/ 593/ 594/ 600/ 614	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1/ 45.23.2-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.0	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.1/ 45.50.2	41.20.1-42.99.0/ 43.12.0/ 43.13.0/ 43.29.1/ 43.31.0/ 43.33.0/ 43.91.2-43.99.9	01/1997
Electrical Trade	611	45.31.0	45.31.0	43.21.0	06/1997
Roofing	601	45.22.1	45.22.1	43.91.1	10/1997
Painting & Varnishing	211/ 613	28.51.0/ 45.44.1	28.51.0/ 45.44.1	25.61.0/ 43.34.1	12/2003
Commercial Cleaning		74.70.1/ 74.70.3/ 74.70.4	74.70.1/ 74.70.3/ 74.70.4	81.21.0/ 81.22.9-81.29.9	04/2004
Waste Removal			37.10.1/ 37.10.2/ 37.20.1-37.20.5/ 90.02.1-90.02.5/ 90.03.0	38.11.0-39.00.0	01/2010
Nursing Care			85.31.5/ 85.31.7/ 85.32.6	87.10.0/ 88.10.1	08/2010
Security			74.60.2	80.10.0/ 80.20.0	06/2011
Temporary Work			74.50.2	78.20.0/ 78.30.0	01/2012
Scaffolding				43.99.1	08/2013
Stonemasonry				23.70.0	10/2013
Hairdressing				96.02.1	11/2013
Chimney Sweeping				81.22.1	04/2014
Slaughtering & Meat Processing				10.11.0-10.13.0	08/2014
Textile & Clothing				13.10.0-14.39.0	01/2015
Agriculture, Forestry & Gardening				01.11.0-02.40.0/ 03.12.0-03.22.0	01/2015

Table A2: Descriptives for Minimum Wage Sectors ($t-5$ to $t-1$)

	Main Construction	Electrical Trade	Roofing	Painting & Varnishing	Commercial Cleaning	Waste Removal	Nursing Care	Security	Temporary Work	Scaffolding	Stonemasonry	Hairdressing	Chimney Sweeping	Slaughtering & Meat Processing
Panel A: West Germany														
Bite (for main sample restrictions)	5.82	9.38	5.73	6.89	26.81	2.18	15.24	13.86	28.55	39.35	10.27	46.22	5.27	11.78
Share in the economy	5.59	0.78	0.42	0.79	0.69	0.54	1.48	0.33	4.55	0.14	0.08	0.42	0.04	0.51
Share of full-time workers	93.35	79.78	89.31	76.00	19.05	82.79	38.98	55.93	72.38	74.62	62.15	39.97	50.00	58.78
Share of part-time workers	2.30	3.83	2.72	4.87	22.78	4.53	34.86	5.57	14.22	6.10	5.69	13.73	7.14	13.84
Share of women	9.11	16.29	8.07	21.45	69.05	16.81	80.57	19.03	43.62	8.87	19.85	91.87	36.67	56.33
Share of full-time women (full-time)	7.22	14.42	6.12	13.26	34.46	11.17	71.90	13.65	29.71	4.40	8.46	90.76	8.96	41.06
Share of full-time entrants	88.39	71.18	85.09	59.52	15.90	73.60	31.68	48.45	69.99	71.23	54.69	34.57	37.17	52.72
Share low-skill (full-time)	13.44	4.52	14.00	12.26	33.20	16.27	7.84	10.38	17.88	27.67	6.47	4.63	2.56	12.67
Share middle-skill (full-time)	79.33	93.16	83.32	84.51	57.90	77.10	81.50	84.53	70.82	63.98	88.56	93.51	96.38	80.39
Share high-skill (full-time)	5.88	1.77	2.44	2.55	4.62	5.43	10.00	3.80	10.23	4.12	2.89	1.11	0.64	5.94
Share non-German nationality (full-time)	15.32	8.70	10.46	10.90	32.12	5.40	3.92	8.89	16.67	34.76	26.07	9.33	0.43	10.04
Panel B: East Germany														
Bite (for main sample restrictions)	25.15	14.84	20.05	9.17	56.81	12.99	20.30	61.52	43.52	39.93	59.22	73.28	12.82	51.03
Share in the economy	10.04	1.48	0.39	1.06	1.47	1.32	2.57	0.63	3.82	0.11	0.09	0.67	0.03	0.64
Share of full-time workers	92.96	89.14	88.53	81.78	27.94	80.15	37.98	69.27	78.89	79.84	81.03	47.32	42.39	70.39
Share of part-time workers	1.24	2.53	1.33	1.83	33.94	3.76	48.51	4.15	7.97	5.94	6.32	31.31	16.85	11.74
Share of women	8.28	11.24	6.86	11.15	68.04	19.56	82.82	19.03	28.42	10.08	26.88	93.65	42.39	54.48
Share of full-time women (full-time)	7.68	9.72	6.45	8.76	59.93	14.86	77.84	15.48	17.90	8.09	22.44	93.13	3.85	54.27
Share of full-time entries	91.49	87.00	87.38	74.04	21.49	63.11	29.41	53.11	77.45	72.83	77.92	36.41	37.50	47.61
Share low-skill (full-time)	3.80	2.18	4.75	3.29	17.56	6.40	3.27	1.86	4.04	2.59	2.93	1.85	6.41	9.61
Share middle-skill (full-time)	89.66	92.12	92.90	94.17	75.34	85.34	82.36	91.39	92.14	93.20	88.78	97.16	88.46	75.41
Share high-skill (full-time)	5.70	4.56	1.11	2.15	3.82	7.78	13.52	6.52	3.61	3.56	5.37	0.87	0.00	4.52
Share non-German nationality (full-time)	3.32	1.28	1.24	1.15	9.89	0.51	1.48	0.47	0.78	0.65	4.88	0.38	0.00	12.72

Notes: This table shows descriptive statistics for the minimum wage sectors. The bite is calculated for the sample restrictions mentioned in Section 3.2. All other descriptives are calculated in each case in t-5 to t-1 before the introduction of the respective minimum wage using the full SIEED and BHP data. For example, the descriptives in column ”Main Construction” are calculated from 1992 to 1996. All rows followed by the parentheses ”(full-time)” are calculated by using the number of all full-time workers in the respective minimum wage sector as the denominator.

Source: SIEED and BHP. Authors' calculations.

Table A3: List of Outside Option Industries (Main Construction Sector)

Industry	
No.	Description
11	Growing of crops; market gardening; horticulture
12	Farming of animals
13	Growing of crops combined with farming of animals (mixed farming)
14	Agricultural and animal husbandry service activities, except veterinary activities
20	Forestry, logging and related service activities
102	Mining and agglomeration of lignite
103	Extraction and agglomeration of peat
111	Extraction of crude petroleum and natural gas
112	Service activities incidental to oil and gas extraction, excluding surveying
131	Mining of iron ores
141	Quarrying of stone
142	Quarrying of sand and clay
143	Mining of chemical and fertilizer minerals
144	Production of salt
145	Other mining and quarrying n.e.c.
201	Sawmilling and planing of wood; impregnation of wood
202	Manufacture of veneer sheets; manufacture of plywood, laminboard, particle board, fibre board and other panels and boards
203	Manufacture of builders' carpentry and joinery
204	Manufacture of wooden containers
261	Manufacture of glass and glass products
264	Manufacture of bricks, tiles and construction products, in baked clay
265	Manufacture of cement, lime and plaster
266	Manufacture of articles of concrete, plaster and cement
267	Cutting, shaping and finishing of stone
281	Manufacture of structural metal products
282	Manufacture of tanks, reservoirs and containers of metal; manufacture of central heating radiators and boilers
283	Manufacture of steam generators, except central heating hot water boilers
285	Treatment and coating of metals; general mechanical engineering
355	Manufacture of other transport equipment n.e.c.
361	Manufacture of furniture
364	Manufacture of sports goods
371	Recycling of metal waste and scrap
372	Recycling of non-metal waste and scrap
451	Site preparation
452	Building of complete constructions or parts thereof; civil engineering
454	Building completion
455	Renting of construction or demolition equipment with operator
701	Real estate activities with own property
703	Real estate activities on a fee or contract basis
713	Renting of other machinery and equipment
742	Architectural and engineering activities and related technical consultancy
900	Sewage and refuse disposal, sanitation and similar activities

Table A4: List of Non-Outside Option Industries (Main Construction Sector)

Industry	
No.	Description
15	Hunting, trapping and game propagation, including related service activities
233	Processing of nuclear fuel
242	Manufacture of pesticides and other agro-chemical products
403	Steam and hot water supply
523	Retail sale of pharmaceutical and medical goods, cosmetic and toilet articles
603	Transport via pipelines
621	Scheduled air transport
623	Space transport
642	Telecommunications
651	Monetary intermediation
724	Database activities
726	Other computer related activities
732	Research and experimental development on social sciences and humanities
801	Primary education
851	Human health activities
912	Activities of trade unions
924	News agency activities
930	Other service activities

Table A5: Difference-in-Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage

	(1)	(2)	(3)
Treated			
x 1992-94	0.054***	0.017***	0.017***
	(0.001)	(0.001)	(0.001)
x 1994-96	-0.035***	0.010***	0.010***
	(0.001)	(0.001)	(0.001)
x 1995-97	-0.041***	0.021***	0.021***
	(0.001)	(0.001)	(0.001)
x 1996-98	-0.050***	0.018***	0.018***
	(0.001)	(0.001)	(0.001)
x 1997-99	-0.051***	0.015***	0.014***
	(0.001)	(0.001)	(0.001)
Partial			
x 1992-94	0.024***	0.004***	0.004***
	(0.000)	(0.000)	(0.000)
x 1994-96	-0.014***	0.005***	0.004***
	(0.000)	(0.000)	(0.000)
x 1995-97	-0.016***	0.008***	0.007***
	(0.000)	(0.000)	(0.000)
x 1996-98	-0.021***	0.007***	0.006***
	(0.000)	(0.000)	(0.000)
x 1997-99	-0.021***	0.007***	0.007***
	(0.001)	(0.001)	(0.001)
No. of observations	4,052,448	4,052,448	4,052,448
No. of workers	862,374	862,374	862,374
Year fixed effects	yes	yes	yes
Demographic controls	yes	no	no
1-digit industry fixed effects	yes	no	yes
Federal state fixed effects	yes	no	yes
Region type fixed effects	yes	no	yes
Worker fixed effects	no	yes	yes

Notes: This table shows the results of varying difference-in-differences specifications with the two-year change in log daily wages as the outcome (see Equation 3). Standard errors (in parentheses) are clustered at the worker level. In column (1), I use year fixed effects, age group dummies, education dummies, a gender dummy, a nationality dummy, 1-digit industry fixed effects, federal state as well as region type fixed effects. In column (2), I use worker and year fixed effects. In column (3), I present the baseline DiD specification with worker fixed effects and all control variables, excluding demographic controls. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A6: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage

	(1)	(2)	(3)	(4)	(5)
Treated x Option					
x 1992-94	-0.004	0.000	0.005*	0.005*	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
x 1994-96	0.012***	0.010***	0.020***	0.020***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
x 1995-97	0.010***	0.007*	0.022***	0.022***	0.022***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
x 1996-98	0.011***	0.007*	0.026***	0.027***	0.025***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
x 1997-99	0.012***	0.007*	0.033***	0.033***	0.032***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Partial x Option					
x 1992-94	-0.009***	-0.008***	-0.006***	-0.006***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1994-96	0.012***	0.009***	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1995-97	0.013***	0.006***	0.008***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1996-98	0.015***	0.007***	0.010***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1997-99	0.012***	0.002	0.008***	0.008***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
No. of observations	796,968	796,968	761,276	761,276	757,763
No. of workers	213,339	213,339	177,647	177,647	176,786
Year fixed effects	yes	yes	yes	yes	yes
Demographic controls	no	yes	no	no	no
1-digit industry fixed effects	no	yes	no	yes	no
3-digit industry fixed effects	no	no	no	no	yes
Federal state fixed effects	no	yes	no	yes	yes
Region type fixed effects	no	yes	no	yes	yes
Worker fixed effects	no	no	yes	yes	yes

Notes: This table shows the results of different triple differences specifications with the two-year change in log daily wages as the outcome using different controls (see Equation 4). Intuitively, the estimator compares the DiD of workers in industries listed in Table A3 with workers in industries listed in Table A4. Standard errors (in parentheses) are clustered at the worker level. In column (1), I only use year fixed effects. In column (2), I add demographic controls, 1-digit industry, federal state and region type fixed effects. In column (3), I use worker fixed effects with only the year fixed effects. In column (4), I present my baseline specification by using worker fixed effects and all controls, excluding demographic controls. In column (5), I use a similar specification as column (4) but with 3-digit industry fixed effects instead of 1-digit industry fixed effects. The reference period is 1993–95. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A7: Difference-in-Differences: Spillover Effects of the Main Construction Sector Minimum Wage, Separately by Non-Outside vs. Outside Option Industries

	Non-outside option	Outside option
Treated		
x 1992-94	0.010***	0.016***
	(0.002)	(0.002)
x 1994-96	0.000	0.020***
	(0.002)	(0.002)
x 1995-97	0.001	0.024***
	(0.002)	(0.003)
x 1996-98	-0.004*	0.022***
	(0.002)	(0.003)
x 1997-99	-0.010***	0.023***
	(0.003)	(0.003)
Partial		
x 1992-94	0.004***	-0.001
	(0.001)	(0.001)
x 1994-96	0.002*	0.010***
	(0.001)	(0.001)
x 1995-97	0.003**	0.010***
	(0.001)	(0.002)
x 1996-98	-0.001	0.008***
	(0.001)	(0.002)
x 1997-99	-0.001	0.007***
	(0.002)	(0.002)
No. of observations	394,299	364,929
No. of workers	88,739	88,947
Year fixed effects	yes	yes
1-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: This table shows the results of two difference-in-differences specifications (see Equation 3). In the column "non-outside option" the table shows the DiD estimates for the industries listed in Table A4 and in column "outside option" the estimator shows the DiD estimates for the industries listed in Table A3. Standard errors (in parentheses) are clustered at the worker level. In both columns I use year fixed effects, 1-digit industry fixed effects, federal state fixed effects, region type fixed effects and worker fixed effects. The reference period is 1993–95. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table A8: Triple Differences: Change in Wage Growth within the Main Construction Sector

	Within main construction	Spillover outside main construction
Treated x Option x Post	0.066***	0.021***
	(0.003)	(0.003)
Partial x Option x Post	0.011***	0.011***
	(0.001)	(0.001)
No. of observations	738,117	761,276
No. of workers	163,189	177,647
Year fixed effects	yes	yes
1-digit industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: Standard errors in parentheses. The table shows specifications using different versions of Equation 5. In the first column, I compare treated workers to control group workers in the main construction sector with the same comparison in non-outside option industries. For comparison, I show the pre-post spillover specification for outside option industries vs. non-outside option industries in the second column. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table A9: Triple Differences: Probability to Switch Establishments

	Job-to-job
Treated x Option	
x 1992-94	-0.015**
	(0.006)
x 1994-96	0.056***
	(0.006)
x 1995-97	0.055***
	(0.008)
x 1996-98	0.007
	(0.009)
x 1997-99	-0.009
	(0.010)
Partial x Option	
x 1992-94	-0.004
	(0.006)
x 1994-96	0.059***
	(0.005)
x 1995-97	0.064***
	(0.006)
x 1996-98	0.015**
	(0.007)
x 1997-99	0.009
	(0.007)
No. of observations	796,763
No. of workers	194,574
Year fixed effects	yes
1-digit Industry fixed effects	yes
Federal state fixed effects	yes
Region type fixed effects	yes
Worker fixed effects	yes

Notes: This table shows the results of a triple differences specifications using the probability of switching establishments as the outcome variable (see Equation 4). The variable takes the value 1 if the individual switched establishments from t to $t+2$ and 0 if she did not. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1992–94. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A10: Establishment-Level: Difference-in-Differences Estimations on Wages and Employment

	Log average wage	Log number of employment
Exposure		
x 1992	-0.049***	-0.010
	(0.014)	(0.062)
x 1993	-0.018*	0.009
	(0.010)	(0.047)
x 1994	-0.003	0.016
	(0.006)	(0.024)
x 1996	0.008	0.015
	(0.006)	(0.044)
x 1997	0.003	0.033
	(0.009)	(0.070)
x 1998	-0.001	-0.051
	(0.010)	(0.081)
x 1999	-0.003	0.089
	(0.012)	(0.125)
No. of observations	146,826	146,826
No. of establishments	21,649	21,649

Notes: This table shows the results of two difference-in-differences estimations on the establishment level using Equation 6. The outcome variable in the first column is the log average wage in an establishment. The outcome variable in the second column is the log number of full-time employees in an establishment. Standard errors (in parentheses) are clustered at the establishment level. The reference period is 1995. All estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A11: Establishment-Level: Triple Differences Estimations on Wages and Employment

	Log average wage	Log number of employment
Exposure x Option		
x 1992	-0.018	-0.147
	(0.025)	(0.138)
x 1993	-0.014	-0.160
	(0.021)	(0.140)
x 1994	-0.016	-0.021
	(0.011)	(0.071)
x 1996	0.004	-0.059
	(0.013)	(0.063)
x 1997	0.024	-0.339*
	(0.019)	(0.194)
x 1998	0.053***	-0.248
	(0.018)	(0.173)
x 1999	0.062**	0.039
	(0.030)	(0.469)
No. of observations	43,237	43,237
No. of establishments	6,303	6,303

Notes: This table shows the results of two triple estimations on the establishment level using Equation 7. Intuitively, the estimator compares the DiD of establishments in industries listed in Table A3 with the DiD of establishment in industries listed in Table A4. The outcome variable in the first column is the log average wage in an establishment. The outcome variable in the second column is the log number of full-time employees in an establishment. Standard errors (in parentheses) are clustered at the establishment level. The reference period is 1995. All estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A12: Triple Differences: Reallocation to Higher-Paying Establishments

	Establishment mean wage	Establishment AKM fixed effect
Treated x Option		
x 1992-94	-0.004**	
	(0.002)	
x 1994-96	0.008***	0.002
	(0.002)	(0.001)
x 1995-97	0.008***	0.006***
	(0.003)	(0.002)
x 1996-98	0.012***	0.008***
	(0.003)	(0.002)
x 1997-99	0.013***	0.014***
	(0.003)	(0.003)
Partial x Option		
x 1992-94	-0.006***	
	(0.001)	
x 1994-96	0.004***	0.002***
	(0.001)	(0.001)
x 1995-97	0.007***	0.005***
	(0.002)	(0.001)
x 1996-98	0.008***	0.007***
	(0.002)	(0.001)
x 1997-99	0.005***	0.006***
	(0.002)	(0.001)
No. of observations	693,303	509,298
No. of workers	174,569	140,934
Year fixed effects	yes	yes
1-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: This table shows the results of two triple differences specifications using different outcome variables (see Equation 4). Intuitively, the estimator compares the DiD of workers in industries listed in Table A3 with workers in industries listed in Table A4. In the first column, I use the change in log establishment average wages as the outcome variable. Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In the second column, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A13: Triple Differences: Reallocation to Higher-Paying Establishments

	Establishment mean wage			**Establishment AKM fixed effect**		
	Baseline	Excluding main construction switchers	No closing plants	Baseline	Excluding main construction switchers	No closing plants
Treated x Option						
x 1992-94	-0.004**	-0.003	-0.004**			
	(0.002)	(0.002)	(0.002)			
x 1994-96	0.008***	0.005**	0.006***	0.002	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1995-97	0.008***	0.006**	0.007***	0.006***	0.004**	0.005***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
x 1996-98	0.012***	0.010***	0.010***	0.008***	0.006**	0.007***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
x 1997-99	0.013***	0.009***	0.011***	0.014***	0.011***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Partial x Option						
x 1992-94	-0.006***	-0.005***	-0.006***			
	(0.001)	(0.001)	(0.001)			
x 1994-96	0.004***	0.003**	0.004***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1995-97	0.007***	0.006***	0.007***	0.005***	0.004***	0.005***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1996-98	0.008***	0.007***	0.007***	0.007***	0.006***	0.007***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1997-99	0.005***	0.004**	0.005***	0.006***	0.005***	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
No. of observations	693,303	686,013	690,064	509,298	504,386	505,617
No. of workers	174,569	172,173	173,619	140,934	139,259	139,759
Year fixed effects	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes

Notes: This table shows the results of several triple differences specifications (see Equation 4). Intuitively, the estimator compares the DiD of workers in industries listed in Table A3 with workers in industries listed in Table A4. In the first three columns, I use the change in log establishment average wages as the outcome variable. In the last three columns, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. I present the baseline results for each outcome, change in establishment average wages and change in establishment AKM fixed effects, without switchers to the main construction sector and excluding workers in establishments during their closing year (from the baseline). Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A14: Triple Differences: Wage Spillover Effects by Socio-Demographic Characteristics

	German	Foreign	0 - 5 years exp.	5 - 10 years exp.	10+ years exp.
Treated x Option					
x 1992-94	0.005*	0.001	-0.013**	-0.005	-0.001
	(0.003)	(0.014)	(0.005)	(0.008)	(0.006)
x 1994-96	0.018***	0.044***	0.030***	0.014*	0.026***
	(0.003)	(0.012)	(0.004)	(0.008)	(0.006)
x 1995-97	0.020***	0.063***	0.053***	0.006	0.021***
	(0.004)	(0.016)	(0.005)	(0.011)	(0.007)
x 1996-98	0.025***	0.060***	0.071***	0.003	0.034***
	(0.004)	(0.021)	(0.007)	(0.012)	(0.008)
x 1997-99	0.032***	0.061**	0.070***	0.034**	0.043***
	(0.005)	(0.024)	(0.012)	(0.014)	(0.009)
Partial x Option					
x 1992-94	-0.006***	-0.005	-0.015***	-0.006	0.003
	(0.002)	(0.007)	(0.005)	(0.004)	(0.002)
x 1994-96	0.008***	0.013**	0.009***	0.007*	0.012***
	(0.002)	(0.006)	(0.003)	(0.004)	(0.002)
x 1995-97	0.006***	0.014*	0.012***	0.007	0.017***
	(0.002)	(0.007)	(0.004)	(0.005)	(0.003)
x 1996-98	0.009***	0.014*	0.026***	0.016***	0.019***
	(0.002)	(0.008)	(0.005)	(0.005)	(0.003)
x 1997-99	0.007***	0.014*	0.007	0.026***	0.025***
	(0.002)	(0.009)	(0.009)	(0.006)	(0.003)
No. of observations	713,851	46,503	285,336	164,313	261,290
No. of workers	166,763	11,014	84,866	55,516	62,428
Year fixed effects	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes

Notes: This table shows the results of multiple triple differences specifications with the two-year change in log daily wages as the outcome separately for workers with different nationality and workers with different levels of labor market experience (see Equation 4). Intuitively, the estimator compares the DiD of workers in industries listed in Table A3 with workers in industries listed in Table A4. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Author's calculations.