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

Gökay Demir*†

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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. In case of the first sectoral minimum wage in Germany, I find an increase in the wages of sub-minimum wage workers outside the minimum wage sector and reallocation of these workers to better-paying establishments. Horizontal fairness concerns and the reduction of information frictions appear to be the main mechanisms for these effects. By analyzing other sectoral minimum wages in Germany, I find positive wage spillover effects only if the minimum wage sector provides a credible outside option for sub-minimum wage workers.

JEL codes: J31, J38, J42, J62

Keywords: spillover, labor market frictions, minimum wages

*RWI - Leibniz Institute for Economic Research and Heinrich-Heine-Universität Düsseldorf. Email: demir@rwi-essen.de.

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This paper uses confidential data from the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB). The data can be obtained by submitting an application to the Research Data Centre (FDZ). Details on applying for the dataset and possibilities for data processing can be found on the FDZ homepage (https://fdz.iab.de/en.aspx).

1 Introduction

One of the most relevant 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 (Slichter, 1950; Abowd et al., 1999; Card et al., 2013). The difference arises due to labor market frictions. For example, workers have incomplete information about the wages offered by different employers because of search and information frictions (Diamond, 1982; Mortensen, 1982; Pissarides, 1985; Burdett and Mortensen, 1998; Mortensen, 2003). In a labor market with frictions, minimum wages in one sector can lead to wage increases and reallocation in other sectors and thereby equalize the wages of good and bad jobs. Such positive spillover effects can result from strategic interaction among employers (Bhaskar and To, 1999; Bhaskar et al., 2002; Berger et al., 2021) or from the reduction of labor market frictions such as search and information frictions when workers learn about other employers' wages. Without labor market frictions, minimum wages in one sector may also cause workers to lose their jobs and seek employment in other sectors, which could lower wages in those other sectors.

Direct empirical evidence on spillover effects of unilateral wage increases is relatively scarce (Staiger et al., 2010; Bassier, 2021; Derenoncourt et al., 2021). The existing empirical evidence focuses primarily on demand side responses to unilateral wage increases. However, supply side responses may reveal reallocation effects and mechanisms that are not otherwise revealed, although this is precisely what is important for policy.

In this paper, I analyze the wage spillover and reallocation effects of publicly announced sectoral (sector-specific) minimum wages in Germany. By focusing on spillover effects at the individual level, comparing these effects with establishment-level spillover effects, and analyzing spillover effects of multiple sectoral minimum wages, I provide a complete picture of spillover effects and its mechanisms. I argue that the publicly announced introduction of minimum wages in one sector can reduce labor market frictions and raise wages in low-wage jobs outside the targeted sector. Horizontal fairness concerns imply that workers with similar tasks should be paid similar wages (Falk et al., 2006). My results suggest that the public disclosure of sectoral minimum wages reduces information frictions and triggers horizontal fairness concerns, leading to spillover effects.

Germany had only sectoral minimum wages before the introduction of the nationwide minimum wage in 2015. In response to increasing wage competition from firms in other EU countries that were able to send their workers to Germany at conditions in their home country, Germany introduced binding minimum wages for sectors affected by foreign wage pressure (Eichhorst, 2005). Thus, intra-sector concerns led to the introduction of these minimum wages, so that it represented an exogenous change in external wages for firms and workers that were not part of the minimum wage sectors. I use administrative linked-employer-employee data in the analysis, which allows to fully exploit this exogenous change in external wages from the perspective of firms and individuals who did not work in the minimum wage sectors. My identification strategy utilizes difference-in-differences and triple-differences models together with worker fixed effects. In these models, I exploit three dimensions of the data. First, I exploit the longitudinal panel dimension of the data. Second, I compare sub-minimum wage workers to workers who had higher wages. Third, I compare industries[1] that had had high worker outflows to the minimum wage sector ("outside option industries") to industries that had had lower outflows to the minimum wage sector ("non-outside option industries") prior to the introduction of the minimum wage. I expect that sub-minimum wage workers in industries with higher outflows to the minimum wage sector, for whom the minimum wage sector constitutes an outside option and who perform tasks similar to those in the minimum wage sector, also experienced higher spillover effects than sub-minimum wage workers in industries with lower outflows to the minimum wage sector. In this way, my identification strategy eliminates biases stemming from macroeconomic shocks, mean reversion, worker-specific unobserved heterogeneity, or group-specific time shocks such as shocks to the low-wage labor market.

I focus in particular on the spillover effects from the main construction sector minimum wage, because of its relatively high number of employees and because it was the first sectoral minimum wage ever introduced. Thus, in this context, I expect spillover effects to be more likely and information shocks to be more pronounced as a result of the public announcement of the minimum wage. In addition, because firms covered by collective bargaining agreements in the main construction sector had had higher wages than the minimum wage prior to its introduction, I can contrast the spillover effects for workers who were more likely aware of these wages with the spillover effects for workers who were less likely aware of these wages. In this way, I provide direct evidence on whether the removal of information frictions, by the public announcement of the minimum wage, was an important mechanisms for spillover effects. To understand which economic conditions and mechanisms favor spillover effects more generally, I additionally analyze spillover effects from other sectoral minimum wages.

Employing the triple-differences model on the level of the individual worker, I find that the main construction sector minimum wage led to an increase in wage growth by up to 2.7 percent for sub-minimum wage workers in outside option industries. Furthermore, the main construction sector minimum wage induced sub-minimum wage workers in outside option industries to switch establishments, upgrade to establishments which have a better composition of workers in general, but also to establishments that pay a higher wage premium to the same type of worker. The results are robust to controlling for region-specific, industry-

[1] "Industries" refer to individual entries in the German Classification of Economic Activities, while "sectors" refer to multiple industries that are collectively covered by a minimum wage regulation.

specific, and occupation-specific shocks, all of which were relevant around the time of the introduction of the minimum wage in the main construction sector. In addition, the results are also robust to different definitions of the key independent variables which indicate the exposure to the main construction sector minimum wage.

To understand the mechanisms that led to wage spillovers, I perform a set of heterogeneity analyses. I find positive wage spillover effects only for sub-minimum wage female workers and sub-minimum wage workers in labor market regions with low presence of the main construction sector. In contrast, sub-minimum wage male workers and sub-minimum wage workers in labor market regions with high presence of the main construction sector experienced negative wage spillover effects in the post-introduction period. Thus, workers with less points of contact with the main construction sector, and therefore less points of contact with the wage setting in this sector, were more likely to experience positive spillover effects. Furthermore, the wage spillover effects were larger for sub-minimum wage younger and non-German workers, who in general have higher information frictions in the labor market than older and German workers. These results suggest that the reduction of information differences through public disclosure of the minimum wage in the main construction sector was an important mechanism for the positive spillover effects. As recent research by Jäger et al. (2021) shows, low-wage workers have biased beliefs about wages paid by other firms. Therefore, the public disclosure of minimum wages could have updated low-wage workers' biased beliefs and in turn led to an increase in their wages. However, since the introduction of the minimum wage also caused some workers to lose their jobs (e.g. Popp, 2021), a potential increase in labor supply from predominantly male workers may explain the negative wage spillover effects for men and in regions with a high presence of the main construction sector in outside option industries.

Moreover, whereas sub-minimum wage workers in larger establishments did not experience positive wage spillover effects, I find that especially sub-minimum wage workers in very small establishments experienced positive wage spillover effects. Because there are fewer points of comparison within the establishment, workers in very small establishments are more likely to compare their wage with the external labor market. In contrast, with more points of contact within the establishment, workers in larger establishments are more likely to compare their wages with other incumbent workers. As fairness models would predict (see e.g. Fehr et al., 2009), due to the publicly announced introduction of the main construction sector minimum wage, a shift in the perception of the fairness of the current employment situation most likely led to positive wage spillover effects in very small establishments for sub-minimum wage workers. In other words, my results point to horizontal fairness concerns triggered by the reduction of information frictions as the main mechanisms for the positive spillover effects. Also, sub-minimum wage workers in high-wage establishments were more likely to experience an increase in wages in the post-introduction period, likely due to rent-sharing triggered by horizontal fairness concerns and the reduction of information frictions. Thus, the heterogeneity analyses suggest that the main reason for the positive spillover effects were not so much the wage increase for certain establishments in the main construction sector due to the minimum wage, but that the reduction of labor market frictions due to the public announcement of the minimum wage played a decisive role in this regard. This mechanism for wage spillovers is theorized in labor market models with frictions (Bhaskar et al., 2002; Burdett and Mortensen, 1998; Manning, 2021).

I additionally analyze spillover effects at the establishment level. I find that establishments which were more exposed to the minimum wage in the main construction sector, due to a higher number of low-wage workers before its introduction, increased average wages in their establishment after the introduction of the minimum wage. More exposed establishments increased average wages especially for new entrants to the establishment. Fairness models of the labor market predict that incumbent workers are more likely to assess the fairness of their wage relative to other workers in the same establishment. In contrast, entering workers assess the fairness of the offered wage relative to offers in other establishments (Fehr et al., 2009). Thus, my establishment-level results are also consistent with fairness models, as they predict a stronger response of entry-level wages to changing labor market conditions. I find that the number of full-time employees in more exposed establishments did not change after the introduction of the minimum wage in the main construction sector, confirming my initial hypothesis that establishment- or firm-level employment effects mask large reallocation effects on the individual-level.

Analyzing the wage spillover effects from other sectoral minimum wages in Germany by using the same identification strategy at the individual level, I find positive wage spillover effects from minimum wages in sectors with a relatively high share of full-time workers. In contrast, I find only negative wage spillover effects if the the minimum wage sector has a relatively low share of full-time workers. Since I focus on full-time workers in my sample, this suggests that positive wage spillover effects prevail only if the minimum wage sector is also a *credible* outside option for workers in my sample.

This paper mainly contributes to an emerging literature on cross-employer spillover effects of wage-setting changes at major employers.[2] Staiger et al. (2010) study the effects of a wage policy change at Department of Veterans Affairs (VA) hospitals in the US. They find that wages at non-VA hospitals rose in response to the VA wage change with only very little changes in employment. The wage increase was largest among non-VA hospitals located close to a VA hospital. Derenoncourt et al. (2021) study the self-imposed voluntary minimum wage introductions at large retailers in the US using online job postings data. They find that other employers in the same commuting zone also increased their wages in response to these voluntary minimum

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

wages. Furthermore, using the CPS, Derenoncourt et al. (2021) find small declines in employment. In a paper written concurrently with mine, Bassier (2021) studies the effects of prescribed wage changes at bargaining council firms on the broader wage structure in South Africa. More relevant for this paper, he also studies the cross-firm wage spillovers of these wage changes at other firms which are connected through worker flows to the bargaining council firms. He observes positive wage spillover effects in firms that are more connected to the bargaining council firms. Moreover, firm separations decrease strongly after the prescribed wage increase.

In contrast to these papers, my main research design and analysis is on the level of the individual worker. By using the complete employment biographies of sampled individuals from administrative social security data, I am able to exploit the longitudinal information on these workers and thereby depict the supply side spillover response to the introduction of sectoral minimum wages in addition to demand side spillover responses. This approach offers four main advantages. First, I explore how the publicly announced introduction of minimum wages in outside sectors affected the career trajectories of individual workers. I exploit the *introduction* of sectoral minimum wages, control for unobserved fixed worker characteristics and for possible shocks to the low-wage labor market, regions, industries and occupations, which speaks for a causal interpretation of my results. Second, I demonstrate how workers who were more exposed to the spillover effects reallocated to new jobs in higher-paying establishments. Defining exposure to spillovers at the firm level hides these highly informative and policy-relevant individual reallocation effects. Third, by performing a set of heterogeneity analyses and by exploiting the contextual details of the different sectoral minimum wages, I am able to provide evidence for the mechanisms that support positive wage spillover effects. In particular, by showing that horizontal fairness concerns triggered by the removal of information frictions appear to be the main source of positive wage spillover effects, I am able to fill a gap in the literature for which only conjecture but no empirical evidence has been provided so far. Fourth, I perform additional analyses on the establishment level and thereby depict the demand side response to the introduction of sectoral minimum wages. By showing that establishments responded to the sectoral minimum wages by strongly adjusting wages for new entrants to the establishment, I further add to our understanding of all mechanisms at play for spillover effects.

My paper is related to several other strands of the literature. The literature on the ”union threat hypothesis” or spillover effects of union wage setting shows how unionization can have either a positive or negative effect on non-union wages in the same industry (Freeman and Medoff, 1981; Moore et al., 1985; Podgursky, 1986; Neumark and Wachter, 1995; Fortin et al., 2021; Farber et al., 2021). Unionization can positively affect the wages of non-unionized firms within the same industry due to a ”threat effect”, or negatively affect those wages because of a labor supply shock from workers of the unionized firms reallocating to the non-unionized firms (Lewis, 1963). Whereas the former mechanism is not viable in the institutional context I analyze, the latter mechanism is also relevant for my setting. Thus, I build on this literature by contrasting the mechanisms of positive wage spillover effects with the mechanisms of negative wage spillover effects.

I draw from the theoretical literature on imperfect labor markets and monopsony power to understand the mechanisms for the spillover effects in the context of my paper. Robinson (1933) first coined the term monopsony and argued that firms may face an upward-sloping labor supply curve. Manning (2003) applied the Burdett and Mortensen (1998) model to show how theoretically search frictions can lead to monopsony power of firms even with many firms in the labor market. Furthermore, Manning (2003) provided an estimation framework to measure the wage elasticity of labor supply to the individual firm, sparking an empirical literature on monopsony power summarized in Sokolova and Sorensen (2021).[3] This literature shows that the wage elasticity of labor supply for the individual firm is indeed not infinite, which implies that firms have potential monopsony power over their workers. Bhaskar and To (1999) and Bhaskar et al. (2002) provide useful models of monopsonistic competition and oligopsony showing how unilateral wage increases of single firms/ sectors can spill over and lead to wage increases and reallocation in other firms/ sectors. Berger et al. (2021) develop a more sophisticated but tractable quantitative, general equilibrium, oligopsony model of the labor market. With their model, Berger et al. (2021) are able to replicate spillover effects of unilateral wage increases in the labor market, such as of Staiger et al. (2010).

A growing literature studies the role of workers' outside options and their impact on wages (Caldwell and Danieli, 2018; Caldwell and Harmon, 2019; Schubert et al., 2021; Jäger et al., 2021). Methodologically, I use this literature to define industries for which the minimum wage sectors are potential outside options. Empirically, I show that positive wage spillover effects are higher in outside option industries and thereby add to this literature.

This paper also relates to the literature on the role of labor market institutions in disrupting the coexistence of good and bad jobs. Acemoglu (2001) develops an extension of a standard search model in which, due to search frictions, good and bad jobs coexist. He argues that labor market institutions, such as e.g. the minimum wage, can disrupt the coexistence of good and bad jobs and shift the composition of employment towards good jobs. Dustmann et al. (2021) study the wage, employment and reallocation effects of the introduction of the German federal minimum wage in 2015. The German federal minimum wage increased wages and did not lower employment. In response to the federal minimum wage, low-paid workers reallocated to better paying firms. I show how government-imposed sectoral minimum wages affect the broader labor market in general and low-wage jobs in particular. Without directly targeting the workers most exposed to

[3] More recent examples include Bassier et al. (2021) and Bachmann et al. (2021).

spillover effects, sectoral minimum wages can increase these workers' wage growth and induce them to look for higher-paying jobs. As a labor market institution, sectoral minimum wages have the advantage over the federal minimum wage in that the wage level is high, but generally led to no or modest employment effects in the targeted industries (Vom Berge and Frings, 2020; Möller, 2012), albeit with heterogeneous effects by labor market concentration (Popp, 2021). Thus, government-imposed and collectively bargained sectoral minimum wages can act as standard bearers in the labor market, as called for in, for example, Derenoncourt et al. (2021).

Finally, this paper relates to the literature on pay transparency (Card et al., 2012; Baker et al., 2019; Mas, 2017; Perez-Truglia, 2020) and fairness concerns at the workplace (Dube et al., 2019; Breza et al., 2018). By exploiting the public announcement of sectoral minimum wages and showing the impact of this pay disclosure on workers who were less likely aware of the already relatively high existing wages in these sectors, I add to this literature.

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 results. Section 6 discusses the findings and concludes.

2 Institutional Background

Due to European trade integration, sectors in Germany that have been largely spared from international trade up to the beginning of the 1990's were then facing fierce wage competition. European firms could essentially 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). The main construction sector in particular was affected by foreign wage competition. Although there were of course profiteers from cheaper construction products in Germany, an opposition 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[4]. Sectoral minimum wages can be extended to foreign firms through the Posting of

[4]§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.

Workers Law[5].

The main construction sector already had a relatively high collective bargaining coverage in 1995 of approximately 80 percent in West Germany and 40 percent in East Germany (Möller et al., 2011). While the unions proposed to set the minimum wage at the level of the lowest wage group of the existing collective bargaining agreement (10.35 Euro), construction employers demanded to introduce a new wage level below the existing one (Eichhorst, 2005). The two sides of collective bargaining parties (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 BMAS 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 (Tagesschau)[6] 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 with a set of heterogeneity analyses.

In the years following the introduction of the minimum wage in the main construction sector, other sectoral minimum wages were also introduced.[8] The Temporary Work Law (*Arbeitnehmerüberlassungsgesetz*) is another piece of legislation which, since changes in the law in 2011, allowed to enact 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

[5]The Posting of Workers Law came into force in March 1996. This law allows to establish mandatory working conditions for employees posted to Germany by employers based abroad.

[6]Zubayr and Gerhard (2005)

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

[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.

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 high quality 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 in 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 spells are not included in it.

The SIEED is constructed in a three-step procedure. In the first step, a 1.5% random sample from the population of establishments in the BeH is drawn. In the second step, all persons are drawn who worked at least one day in one of these establishments between 1975–2018. In the third step, for these persons, the complete employment biographies are drawn from the BeH. The employment biographies include the time period 1975–2018 and employment times in the sampled establishments, from the first step, as well as 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[10], detailed occupation codes, the daily wage[11] and type of job (e.g. part-time vs. full-time). To

[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.

[10] The variable on education changes frequently with establishment transitions, because the new establishment recompiles the reporting data. E.g. status changes in education (e.g. due to further education) will probably only be recorded in the new establishment (Meinken and Koch, 2004). To circumvent this issue I use the imputed education variable which is based on Fitzenberger et al. (2006); Thomsen et al. (2018) and is already included in the data.

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

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).[12] 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 full-time workers and composition of workforce.

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 from 5 years before the introduction of its minimum wage.[13] To include East Germany in the data, I restrict the observation period to start from the year 1992 onward. I create a yearly panel and select all employment spells that include the June 30^{th} as a cutoff date. 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. In the data preparation, I

[12] The first four digits in the WZ are based on the Statistical Classification of Economic Activities in the European Community (NACE).

[13] The commercial cleaning sector has already decided on a generally binding collective bargaining agreement on 04/2004. In estimating the spillover effects from this sector, however, I focus on the inclusion of this sector to the Posting of Workers Law, as this was the first time that a binding minimum wage could be enforced for employers based abroad in this sector. To ensure that the generally binding collective bargaining agreement from 04/2004 in the commercial cleaning sector does not distort the spillover effects from other minimum wages, I exclude the commercial cleaning sector from the sample as early as 1999.

largely follow the guide in Dauth and Eppelsheimer (2020).[14] I trim extremely low daily wages of full-time workers by dropping observations with real daily wages below the mean real daily wage of the first percentile of real daily wages.

For the heterogeneity analyses, I calculate the presence 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.

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). 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. The estimation strategy of AKM requires a connected set of establishments linked by worker mobility to identify the fixed effects. Due to this restriction, for most very small establishments an AKM establishment fixed effect value is missing. Bellmann et al. (2020) also provide a ranking of the establishment fixed effects according to size and grouped into 20 quantiles indicating the position in the overall distribution. I split this ranking further into terciles and classify establishments in the lowest part of the distribution as "low AKM", in the middle as "middle AKM" and in the upper tercile of the distribution as "high AKM".

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.

[14] I take full responsibility for any errors in my code.

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 wage in West Germany (including Berlin) as a threshold, I define the groups in the following way:[15]

Definition of Wage Groups

	Treated Group	**Partially Treated Group**	**Control Group**
Hourly Wage (in Euro)	$h_{i,t} < 8.69$	$8.69 \leq h_{i,t} < 12.69$	$12.69 \leq h_{i,t} < 20.69$

The variable $h_{i,t}$ refers to the nominal hourly wage of worker i at year t. Using data on the years prior to its introduction (1992–96), Table 2 illustrates descriptive statistics for worker groups affected by the minimum wage in 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. Noticeable is also the higher level of log (daily) two-year wage growth for treated groups, especially in the Treated Group, compared to the Control Group, probably due to mean reversion. 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., 2021) and has recently been used by Derenoncourt et al. (2021) to analyze exposure to firm-level minimum wages across employer-by-occupation-by-commuting-zone cells. Formally, I define the exposure $D_{j(i)}$ of an establishment j to the main construction minimum wage as follows:

$$D_{j(i)} = \frac{\sum_{i \in j(i)} \sum_{t \in [1992,1996]} \mathbb{1}(h_{i,t} < 12.69\,\text{Euro})}{N_{j(i),t \in [1992,1996]}}. \tag{1}$$

[15] The minimum wage in the main construction sector for East Germany is 8 Euro per hour (see Table 1). Therefore, for all individuals working in an East German establishment outside of the main construction sector I define: Treated Group ($h_{i,t} < 8.00$), Partially Treated Group ($8.00 \leq h_{i,t} < 12.00$) and Control Group ($12.00 \leq h_{it} < 20.00$).

In Equation 1, I define exposure of an establishment to the main construction sector minimum wage as the fraction of workers paid a nominal hourly wage below 12.69 Euro in the five years prior to the introduction of the minimum wage. To also include workers that might have been only partially treated from the main construction sector minimum wage, I use the nominal hourly wage of 12.69 Euro as the cutoff.

Figure 7 shows the distribution of the exposure measure across establishments. Many establishments pay all of their workers an hourly wage below the cutoff of 12.69 Euro. 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.[16] Apart from this, a continuous and relatively uniform distribution across exposure bins can be observed.

Industries

Furthermore, I also classify industries for whose workers 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 the outcomes of workers in industries for whose workers the main construction sector was not considered as an outside option (herein: non-outside option industries). Using an employment flows approach to define outside options, as in Schubert et al. (2021), I construct the share of separations from a 3-digit industry[17] k to the main construction sector as follows

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

I define separations as any employer transition.[18] To construct $\pi_{k\rightarrow \text{main construction}}$, I only use separations of workers who are in the Treated or Partially Treated Group at year t. Moreover, I only use the 5 years prior to the introduction to the minimum wage in the main construction sector (1992–96). I proceed by classifying industries in the top 10^{th} percentiles of the employment weighted distribution 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,

[16] Results are available upon request.

[17] Here I use the time-consistent version of the 3-digit German equivalent of NACE Rev. 1 (Eberle et al., 2014).

[18] 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 overstate the role of some industries for workers' job choice.

whereas industries which are more service-oriented (e.g. ”Telecommunications”) are classified as non-outside option industries for the main construction sector.[19]

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. (2021); 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, around the time of the introduction of the main construction sector minimum wage:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{\tau=1993}^{1997} \beta_\tau Treated_{i,t} + \sum_{\tau=1993}^{1997} \gamma_\tau Partial_{i,t} + \delta X_{i,t} + \epsilon_{i,t}. \tag{3}$$

Here $w_{i,t}$ refers to the log (deflated daily) wage of worker i in 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 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_τ and γ_τ 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 1992 to 1994. I estimate the DiD specifications including two pre-introduction periods $\tau < 1995$ and three post-introduction periods $\tau \geq 1995$. 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.[20] 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.

[19] 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.

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

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.[21]

Triple differences

To further validate that the estimated DiD effects in Equation 3 are indeed spillover effects from the main construction sector minimum wage, I estimate a triple differences (DiDiD) specification. Using the outside option and non-outside option industry classifications, I estimate the DiDiD specification as follows:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{\tau=1993}^{1997} \beta_\tau Treated_{i,t} \times Option_{i,t} + \sum_{\tau=1993}^{1997} \gamma_\tau Partial_{i,t} \times Option_{i,t} + \delta X_{i,t} + \epsilon_{i,t}. \quad (4)$$

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 in $X_{i,t}$. The coefficients of interest, β_τ and γ_τ, 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 validates the working hypothesis that, after the introduction of the minimum wage, workers who are "closer" to the main construction sector (outside option industries) should also experience a higher change in their wage growth compared to workers who are "farther" to the main construction sector (non-outside option industries). Second, the DiDiD estimates also remove any group-specific time shocks. The parallel growth assumption here is that the spillover effect can be isolated if the wage growth gap between outside option and non-outside option industries would have evolved similarly in the treatment groups counterfactual as it did in the pre-introduction control group (Cunningham, 2021). In other words, any contemporaneous shock to the outcome variable, which affects workers in the treated groups but not workers

[21] 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, industry-specific and occupation-specific shocks.

in the control group or vice versa, should be similar in outside option industries and in non-outside option industries. Statistically and/or economically insignificant effects for β_τ and γ_τ in the pre-introduction period indicate that this 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.

Establishment-level analysis

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

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{\tau=1992,\tau\neq 1996}^{1999} \gamma_\tau D_{j(i)} + \epsilon_{j,t}. \tag{5}$$

$y_{j,t}$ denotes the outcome of interest, α_j are establishment fixed effects and ζ_t are time fixed effects. The coefficients γ_τ 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 1996. For the years $\tau > 1996$, the coefficients estimates γ_τ 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 5 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. I provide suggestive evidence of this parallel trends assumption by visualizing the coefficient estimates for γ_τ for the years prior to the minimum wage introduction $\tau < 1996$. Coefficient estimates of $\tau < 1996$ 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:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{\tau=1992,\tau\neq 1996}^{1999} \gamma_\tau D_{j(i)} \times Option_{j(i),t} + X_{j,t} + \epsilon_{j,t}. \tag{6}$$

I estimate a triple interaction and include all respective double interactions as well as the $Option_{j(i),t}$

variable in $X_{j,t}$.[22] The DiDiD specification in Equation 6 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 6 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, not induced by the minimum wage, to the outcome variable, 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, I provide suggestive evidence for this assumption in an event-study figure, by showing that the coefficient estimates of $\gamma_\tau < 1996$ are statistically insignificant. If this assumption holds, $\gamma_\tau > 1996$ identifies the causal spillover effect of the main construction sector minimum wage on establishments in outside option industries with higher exposure.

5 Results

5.1 Wages and Reallocation

Figure 1 illustrates the individual-level results using the DiD estimator of Equation 3 with the change in wage growth as the outcome variable.[23] The y-axis shows the DiD coefficient estimates and the x-axis shows the time period. The time period 1992–94 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 and partially treated workers right at 1994–96 and therefore indicate this with the vertical line at this time period. Because treated (partially treated) workers have a slightly lower (higher) relative two-year wage growth compared to the control group in 1993–95, the parallel growth assumption is not perfectly fulfilled. Nevertheless, I interpret the sharp growth in coefficients right at the public announcement of the main construction sector minimum wage in 1996 as a first indication for spillover effects of this minimum wage. The positive excess wage growth effects are as high as 1.7 percent in 1994–96 for treated workers and

[22] To be more specific, $X_{j,t}$ includes: $Option_{j(i),t}$, $\zeta_t \times Option_{j(i),t}$, $D_{j(i)} \times Option_{j(i),t}$ and $\sum_{\tau=1992,\tau\neq1996}^{1999} \gamma_\tau D_{j(i)}$.

[23] In Table A5, I estimate various specifications of Equation 3 in which I subsequently add control variables. Without the usage of person fixed effects in columns 1–4, 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 the person fixed effects in column 5, 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 1992–94 and relative to the control group. Figure 1 illustrates the coefficients estimates of column 5 in Table A5.

increase sharply to 3.3 percent for the time period 1995–97, to 4.2 percent in 1996–98, and 4.9 percent in 1997–99. I also find a similar pattern for the partially treated group, however with effects that are smaller in the amount.

In Figure 2, I estimate the DiDiD specification from Equation 4 using the change in wage growth as the outcome variable.[24] 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). The coefficients are statistically insignificant for the time periods of 1994–96 and 1995–97. In 1996–98, following the public announcement of the main construction sector minimum wage, the wage growth of treated workers relative to the control group increased by 1.2 percent more for workers within outside option industries than in non-outside option industries. In 1997–99, after the introduction of the main construction sector minimum wage, this change in wage growth for treated workers in outside option industries increased even more to 2.7 percent. Again, despite the negative and statistically significant coefficient in the pre-period (1993–95), I interpret the sharply rising and positive coefficients in the post-introduction period for treated workers as spillover effects from the sectoral minimum wage in the main construction sector. Assuming that in the counterfactual the coefficient in the 1993–95 period can also be extrapolated into subsequent periods, I would underestimate the true effect for treated workers. Therefore, my results can be viewed as lower bound. For the partially treated group, I find either only very small or insignificant effects throughout.[25]

Dustmann et al. (2021) highlight the reallocation effects of the federal minimum wage, which was introduced in the year 2015 in Germany, and rationalize their results with a model of monopsonistic competition with heterogeneous firms. Similarly, I argue that the publicly announced introduction of sectoral minimum wages can also lead to reallocation effects in frictional labor markets. For example, Bhaskar et al. (2002) predict that in response to an increase in wages of competing firms, workers in other firms will reallocate to the competing firms. I test these predictions in Figure 3.[26]

[24] Without the usage of worker fixed effects in column 1 of Table A6, I find that the DiDiD wage effect for treated workers in outside option industries is statistically insignificant until 1994–96, negative in 1995-97 and 1996-98, and turns positive in 1997–99. With the inclusion of worker fixed effects in column 2, I again detect a statistically significant difference in the wage growth of treated relative to control group workers in outside option versus non-outside option industries in the pre-period 1993-95. Figure 2 illustrates the coefficient estimates of column 2 in Table A6.

[25] Table A7 shows that workers in the treated group from non-outside option industries and outside option industries experienced higher 2-year wage growth over time relative to the control group. However, the coefficient estimates for workers in the outside option industries are higher compared to workers in the non-outside option industries, further validating the hypothesis that workers who work in "closer" industries should experience a higher wage growth after the introduction of the main construction sector minimum wage.

[26] In Table A8, I illustrate the results in table form and additionally include the number of observations and standard errors.

Figure 3 shows the results of DiDiD specifications (see Equation 4) using different outcome variables and illustrating the reallocation effects of the main construction sector minimum wage. In the first panel, I use the change in employment status of an individual as the outcome variable. The variable takes the value 0 if the individual is employed according to my sample selection criteria (see Section 3.2) and -1 if the individual drops out of the sample.[27] The coefficient of the triple interaction term is positive and statistically significant in the pre-introduction period of 1993–95, meaning that treated workers differ from control group workers in outside option industries relative to non-outside option industries in dropping out of the sample over time. Thus, my empirical approach for this outcome variable is only partially valid and most likely leads to an overestimation of the true effect, assuming that the positive effects of the 1993–95 period can be carried forward to the subsequent periods. Given this drawback, I find that following the public announcement of the main construction sector minimum wage in 1996, treated workers in outside option industries were more likely to drop out of the sample and thus out of regular full-time employment covered by social security. The sectoral minimum wage in the main construction sector has thus likely led to significant upheavals in the working lives of more exposed individuals. Individuals may have entered a period of unemployment, e.g., to find a better match in the labor market, or they may have started their own firm. Since the data does not include information on individuals' unemployment spells or information on self-employment, a more in-depth analysis is not possible here.

In the second panel of Figure 3, I use the change of jobs as the outcome variable.[28] I find insignificant effects for the pre-period of 1993–95, which reassures that treated and control group workers in outside option versus non-outside option industries are comparable with respect to the outcome variable. 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, these workers had a 12.6 and 12.2 percentage points higher likelihood of switching jobs in 1994–96 and 1995–97 respectively, compared to treated group workers in non-outside option industries relative to the control group and reference period (1992–94). For the subsequent periods, the DiDiD coefficient is insignificant in 1996–98 and increases again to 3.2 percentage points in 1997–99 when the minimum wage in the main construction sector was increased. For the partially treated workers, I also find an immediate increase right at the public announcement and introduction of the main construction sector minimum wage, albeit smaller in the amount, in the likelihood of more exposed workers switching jobs for the periods 1994–96 and 1995–97. The coefficient turns negative in 1996–98 and 1997–99. Overall, immediately after the public announcement of the minimum wage in the

[27] Thus, if the variable takes the value -1 the individual transitions either from full-time to part-time work, from employment to retirement, from employment to unemployment, from employment to non-employment, to civil service, to self-employment, or begins a study or vocational training.

[28] The 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 $t + 2$.

main construction sector, more exposed workers decided to leave their job to find a new and better match, especially in terms of pay, as I show below.

In the third and fourth panel of Figure 3, I follow Dustmann et al. (2021) in estimating the reallocation to higher paying establishments over a two-year period. To do so, 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 third panel of Figure 3, I show the results for the change in average establishment (daily) wages. I find that treated workers relative to control group workers in outside option industries had a higher likelihood of changing to establishments which pay a higher average wage already in the pre-period of 1993-95 compared to treated workers relative to control group workers in non-outside option industries. This DiDiD coefficient amounts to 0.6 percent in 1993-95 and increases slightly to 1.1 percent in 1994-96 and 1995-97, 1.2 percent in 1996-98 and 1.5 percent in 1997-99. Given that the coefficient does not increase sharply right at the public announcement of the main construction sector minimum wage, I interpret the coefficients only cautiously as suggestive evidence that more exposed workers were moving to establishments which pay higher average daily wages.

In the fourth panel of Figure 3, I show the results for the change in AKM establishment fixed effects. 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. The DiDiD coefficients are statistically insignificant until including 1996-98 and become positive and statistically significant in 1996-98 and 1997-99 where the coefficients amount to 0.5 percent and 0.9 percent, respectively. For partially treated workers, I find a similar pattern with coefficient estimates that are positive and statistically significant in 1996-98 and 1997-99 and amount to 0.3 percent and 0.4 percent, respectively. Thus, low-wage workers who are more exposed to spillovers from the main construction sector minimum wage moved to establishments which pay a higher pay premium to the same worker type after the introduction of the minimum wage. The coefficient estimates for treated workers in 1997-99 and after the introduction of the main construction sector minimum wage are three times as high compared to the coefficients estimates after the introduction of the federal minimum wage found in Dustmann et al. (2021) and therefore highlight the magnitude of the spillover effects from the main construction sector minimum wage. Comparing the reallocation estimates for establishment's average daily wages in 1997-99 (1.5 percent) with the reallocation estimates for establishment's AKM fixed effects (0.9 percent), I find that about 40 percent ($(1-\frac{0.9\%}{1.5\%})\times 100$) of the increase in establishment's average daily wage was due to more exposed treated workers moving to establishments with a better composition of workers (e.g. more high-skilled workers) and about 60 percent due to these workers moving to establishments that pay a higher wage premium to the same worker type.

5.2 Robustness Checks

The triple differences specification of Equation 4 and estimated in Figures 2 and 3 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 3, 4, 5, 6 and 7 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. For the wage spillover effects in Table 3, the coefficient on the pre-period of 1993-95 is now statistically insignificant and larger in the amount for the subsequent periods. For the reallocation effects in Tables 4, 5, 6 and 7, I find no or only relatively small changes in the coefficients. Thus, the results suggest that the positive wage spillover and reallocation effects that I estimate are not driven by unrelated 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. With the exemption of the job-to-job change probability, I find that the inclusion of industry times year fixed effects does not change the results qualitatively. For the job-to-job change probability in Table 5, I find that controlling for industry-specific shocks leads to a negative and statistically significant negative coefficient in the pre-period 1993-95 and dramatically decreases the coefficients for 1994-96 and 1995-97 with negative coefficients in 1996-98 and 1997-99. Through the inclusion of industry times year fixed effects, the estimations rely on comparisons of (partially) treated versus control group workers and outside option versus non-outside option industries within 1-digit industry cells. As indicated by the negative coefficient in 1993-95, this makes individuals less comparable with respect to job-to-job changes and results in a worse depiction of the counterfactual scenario. Nevertheless, because the coefficient estimate for the DiDiD in Table 5 is statistically significant and positive right at the public announcement of the minimum wage in 1994-96 and as all other results do not change qualitatively, I conclude that the results are robust to controlling for industry-specific shocks.

Third, I include both, labor market region times year and 1-digit industry times year fixed effects. In the most restrictive estimation, I additionally include occupation times year fixed effects to capture shocks which affect some type of tasks differently than others (e.g. technological change).[29] The results in Tables 3, 4, 5, 6 and 7 are also robust to these specifications.

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 3, 4, 5, 6 and 7. 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.[30] 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, I define the $Option_{i,t}$ variable based on tasks instead of employment flows. Specifically, I assume that the main construction sector mainly relies on manual tasks. Thus, occupations with manual tasks outside the main construction sector should be more likely to be affected by spillover effects than other occupations. In the robustness check, I compare manual occupations with agricultural occupations as these occupations are relatively similar to the manual occupations and therefore capture shocks to the low-wage labor market common to these two occupational groups, but still different enough so that spillover effects can be estimated and do not get differenced out.[31] I find positive wage spillover and reallocation effects also with this definition of the $Option_{i,t}$ variable.

[29] I use the widely used occupational classification of Blossfeld (1987) and the codes provided for this in Dauth and Eppelsheimer (2020). This classification uses the detailed 3-digit occupations and creates the 13 broad groups of occupation: agricultural occupations, unskilled manual occupations, skilled manual occupations, technicians, engineers, unskilled services, skilled services, semiprofessions, professions, unskilled commercial and administrational occupations, skilled commercial and administrational occupations, managers, and unassignable occupations.

[30] To do so, I classify an individual as belonging to the (partially) treated group if one observation between 1992 and 1996 of the individual is classified as (partially) treated. I proceed similarly for control group workers.

[31] Again, I use the broad occupational groups in Blossfeld (1987) for this.

5.3 Mechanisms

To understand the mechanisms that led to wage spillovers from the main construction sector minimum wage, I conduct heterogeneity analyses. In the following estimates, I only show the results of the DiDiD estimates, as they are empirically more valid. In particular, controlling for group-specific time shocks in the period of the main construction sector minimum wage is particularly important, which is done by the DiDiD specification. I present results from the DiDiD specification (Equation 4) separately estimated by socio-demographic characteristics, place of work and establishment characteristics measured in t.

Figures 4, 5, and 6 show three key findings.[32] First, I find positive wage spillover effects only for treated women and for treated workers in labor market regions with a lower presence of the main construction sector. Table A2 shows, for example, that women rarely work in the main construction sector and therefore have less contact with its wage setting. Therefore, these workers were probably less aware of the already high collectively bargained wages in some main construction establishments, and the public announcement of the minimum wage probably had the effect of an information shock on them. Second, I find larger effects for treated non-German and very young workers. These workers are more likely to have higher information frictions about their outside options in the labor market in general, and the public announcement and introduction of the minimum wage in the main construction sector therefore probably acted as an information shock for them as well. Third, I find negative wage growth effects for men and in labor market regions with a high presence of the main construction sector. An increase in labor supply due to predominantly male workers losing their job in the main construction sector can rationalize these negative wage spillover effects for men and in labor market regions with a high presence of the main construction sector. This mechanism has been emphasized in other contexts (e.g. Lewis, 1963). Fourth, I find positive wage spillover effects for treated and partially treated workers only in very small establishments and treated workers in high AKM establishments.[33] Because there are fewer points of comparison within the own establishment, workers in very small establishments are more likely to compare the fairness of their wage with the ongoing wage in the external labor market, whereas workers in larger establishments are more likely to compare the fairness of their wage with other incumbent workers (Fehr et al., 2009). Overall, the results point to the reduction of information frictions, which then triggered horizontal fairness concerns, as the main mechanisms for the positive wage spillover effects from the main construction sector minimum wage.

[32] I illustrate the results also in table form in Tables A9, A10, and A11 with standard errors, observation numbers and p-values.

[33] I use AKM establishment fixed effects for 1993–99. Unfortunately, due to this I lose the reference period of 1992–94 which I have used in the estimations up to this point. However, I find that also using the period of 1993–95 as the reference period does not change the baseline results qualitatively. Results are available upon request.

5.4 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 8 plots the coefficient estimates γ_τ for the DiD specification from Equation 5 as well as the coefficient estimates γ_τ for the DiDiD specification from Equation 6. The outcome variable in these figures are log (daily) average wages of an establishment. The panel for the DiD estimates shows that wages grew already by 2.2 percent in the year 1992 and 1.3 percent in the year 1993 for establishments with a higher exposure relative to establishments with lower exposure, relative to the base year 1996. This estimate becomes statistically insignificant in 1994 and 1995. After the introduction of the main construction minimum wage in 1997, wages grew by 2.5 percent in 1997, 3.3 percent in 1998 and 3.7 percent in 1999 for establishments with higher exposure relative to establishments with lower exposure, relative to the base year 1996. To interpret these results as causal spillover effects from the main construction sector minimum wage, I need to assume that the evolution of the wage growth for high exposure versus low exposure establishments would have followed the path starting in 1994. Namely, in the absence of the main construction sector minimum wage, the coefficient estimates for $\gamma_\tau > 1996$ would have been statistically and economically insignificant. However, the coefficient estimates for 1992 and 1993 might cast doubt on this assumption.

In line with previous research on cross-employer wage spillovers, the DiDiD estimates in Figure 8 more convincingly show that more exposed 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 for $\gamma_\tau < 1996$ are statistically insignificant and increase after the introduction of the main construction sector minimum wage to up to 7.7 percent in 1999.

In Figure 9, I estimate the same specification as in Figure 8, but use only new entrants to the establishment in the respective year and then collapse the data to the establishment level. Because all establishments without any entrants within a year do not appear in this sample, this reduces the number of establishments in the estimation. Using only new entrants to an establishment, I also define exposure as in Equation 1 and illustrate it in Figure A.1. The exposure variable using only new entrants to an establishment has a very similar distribution to Figure 7.

For the DiD specification in Figure 9, I find almost no pre-trend for establishments with higher levels of exposure versus establishments with lower levels of exposure in the years prior to the introduction of the main construction sector minimum wage and a large spike in the growth of average wages for establishments with higher levels of exposure after the introduction. Specifically, relative to the year 1996 and relative to establishments with lower levels of exposure, establishments with higher levels of exposure increased average wages for new entrants by 20.5 percent in 1997. This effect decreased slightly in the following years to 15.7 percent in 1999. Unfortunately, most likely due to the low number of observations, I find only insignificant effects using the DiDiD specification for entrants after the introduction of the main construction sector minimum wage. Nevertheless, this analysis shows that more exposed establishments are increasing their average wages, especially for entry-level employees. This finding provides additional evidence that horizontal fairness concerns seem to be an important mechanism, as especially entrants compare the fairness of their wage with the external labor market, while e.g. incumbents compare the fairness of their wage with other incumbents (Fehr et al., 2009).

In Figure 10, I estimate the DiD and DiDiD specifications on the establishment level using the log number of full-time employed workers as the outcome variable. I find that the main construction sector minimum wage did not have an effect on the number of full-time employed workers in more exposed establishments. However, as I have demonstrated in Section 5.1, this aggregate employment effect hides reallocation effects that took place on the individual level.

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.[34]

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. Specifically, I use a generalized version of Equation 4:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{\tau=-4}^{0} \beta_\tau Treated_{i,t} \times Option_{i,t} + \sum_{\tau=-4}^{0} \gamma_\tau Partial_{i,t} \times Option_{i,t} + \delta X_{i,t} + \epsilon_{i,t}, \quad (7)$$

[34] Because I do not want to capture possible effects of the federal minimum wage, I do not analyze the spillover effects from the textile & clothing sector, the agriculture, forestry & gardening sector, the chimney sweeping sector, and the slaughtering & meat processing sector. These sectoral minimum wages were either introduced right at or only months before the federal minimum wage was introduced (see Table 1). Furthermore, due to sparse indicator variables I cannot analyze the spillover effects from the electrical trade sector.

where $\tau = -4$ are now 4 years prior to the introduction of the sectoral minimum wage and $\tau = 0$ is the year in which the sectoral minimum wage was introduced. The reference year is $\tau = -5$. I define treated (sub-minimum wage) workers as workers with an hourly wage below the respective minimum wage, partially treated workers earn an hourly wage with up to 4 Euro above the respective minimum wage and control group workers earn an hourly wage between 4 Euro and 12 Euro above the respective minimum wage. I define outside option and non-outside option industries by using the procedure outlined in Section 3.3.

Figure 11 illustrates the results. The y-axis displays the coefficient estimates of the triple interaction and the x-axis indicates the time period. The vertical line in each figure marks the time period in which anticipation effects, e.g. due to the public announcement, are likely. As information treatments seem to be important in the context of spillover effects (Section 5.3), I expect to find spillover effects right at the vertical line of each panel. I find positive wage spillover effects on sub-minimum wage workers in outside option industries from the roofing minimum wage, the painting & varnishing minimum wage, the waste removal minimum wage, the security minimum wage, the temporary work minimum wage, the scaffolding minimum wage, and the stonemasonry minimum wage. I find negative wage spillover effects on sub-minimum wage workers in outside option industries from the commercial cleaning minimum wage, the nursing care minimum wage and hairdressing minimum wage. Positive wage spillover effects range from 1.1 percent from the minimum wage in the stonemasonry sector to 5.96 percent from the minimum wage in the painting & varnishing sector, each measured in the last time period. Negative wage spillover effects range from 2.6 percent from the minimum wage in the nursing care sector to 4.7 percent from the minimum wage in the hairdressing sector, each measured in the last time period. 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. Similar for the scaffolding, stonemasonry, and hairdressing minimum wages. This provides additional evidence that my identification strategy does not capture year-specific shocks to low-wage earners, but rather spillover shocks that affect only specific industries.

Two characteristics distinguish sectors with negative wage spillover effects from those with positive wage spillover effects (see Table A2). First, the share of women in these sectors is significantly higher. Second, the share of full-time employees is significantly lower. However, an explanation of the spillover effects based on the gender difference is unlikely, as estimates on this separately by gender show that women and men experience negative spillover effects from the commercial cleaning and hairdressing minimum wages.[35] An explanation of the difference in spillover effects based on the share of full-time employees in the respective minimum wage sectors is more likely. Because my sample includes only full-time workers, I interpret the

[35] Results available upon request. I find negative wage spillover effects for men and no statistically significant effect for females from the nursing care minimum wage.

different signs on spillover effects as suggestive evidence that positive wage spillover effects can only arise if the minimum wage sector is a credible outside option for workers. For example, 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, since switching to part-time might be associated with substantial wage declines.[36]

6 Discussion and Conclusion

This paper investigates the spillover effects of publicly announced sectoral minmum wages on workers and establishments outside the minimum wage sector. The main reasons for the introduction of sectoral minimum wages were intra-sector concerns, which made it an exogenous variation in wages for workers and firms outside the sector.

I find large wage spillover and reallocation effects from the main construction sector minimum wage for more exposed workers, i.e. sub-minimum wage workers in outside option industries. Sub-minimum wage workers in outside option industries experienced an increase in their wage growth and reallocated to 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, and horizontal fairness concerns are the most likely mechanisms for the positive wage spillover effects. In particular, I find that groups of workers who have less points of contact with the main construction sector, and workers who are more likely to compare the fairness of their wages with the external labor market, experienced positive wage spillover effects. I find negative wage spillover effects for workers who are more likely to be affected by a labor supply shock resulting from the minimum wage in the main construction sector. Furthermore, when analyzing the wage spillover effects of other sectoral minimum wages, I only find positive wage spillover effects if the minimum wage sector is also a credible outside option. In the case where the minimum wage sector is not a credible outside option, I find only negative wage spillover effects from the introduction of the respective minimum wage.

The results are in line with labor market models which incorporate frictions (Bhaskar and To, 1999; Bhaskar et al., 2002; Berger et al., 2021; Manning, 2003, 2021). In these models, unilateral wage increases in one sector can lead to wage increases and reallocation in other sectors. These models also show that a reduction in labor market frictions can lead to wage increases, and my results suggest that this is the main mechanism by which positive wage spillover effects have emerged from the main construction sector. The results are also in line with findings in Jäger et al. (2021) who show that workers in Germany have biased

[36]Since my identification strategy is based on comparing industries with already high outflows to industries with low outflows to the minimum wage sector, an estimate that uses outflows to the minimum wage sector as an outcome variable could lead to biases. Therefore, I do not analyze the probability of switching to the minimum wage sector here.

beliefs about outside wages with low-paid workers being particularly overpessimistic. My results indicate that the public nature of the main construction sector minimum wage together with its media coverage most likely updated low-paid workers biased beliefs which then led to positive wage spillover effects.

The results have two important implications for policy. First, sectoral minimum wages can act as standard bearers in the low-wage labor market. As a policy instrument, it has the key advantage over the federal minimum wage in that it is based on negotiations between employee and employer representatives on the minimum wage in a sector and is therefore likely to have a rather modest impact on employment despite a high wage level (Vom Berge and Frings, 2020; Möller, 2012). Second, my results highlight the importance of pay transparency in the low-wage labor market to update low-wage workers' biased beliefs (Jäger et al., 2021) and disrupt the coexistence of good and bad jobs. Pay transparency has been shown to be important in different settings as well (Card et al., 2012; Baker et al., 2019; Mas, 2017). The results of this study suggest that information about existing wages in the low-wage sector can help increase the wages of low-wage workers.

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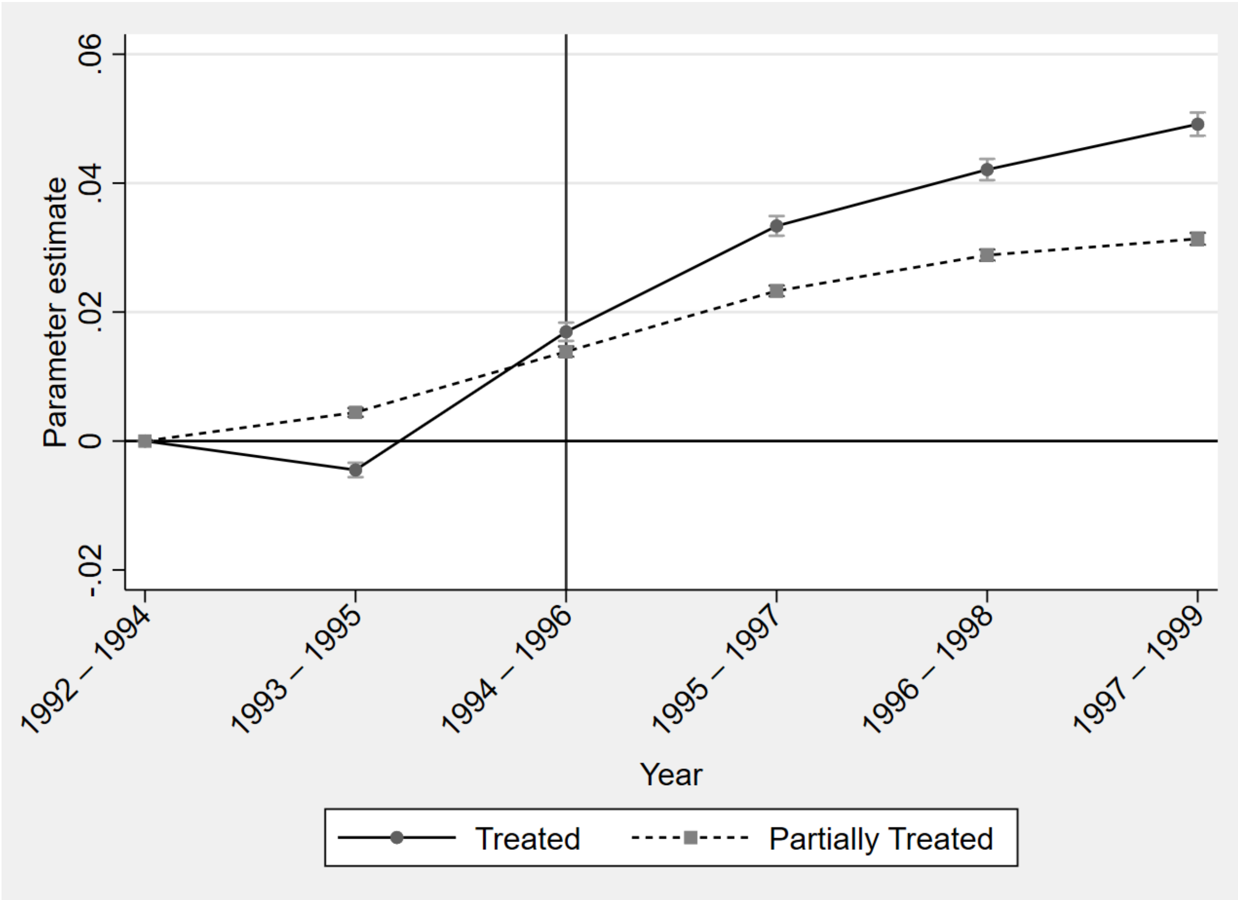
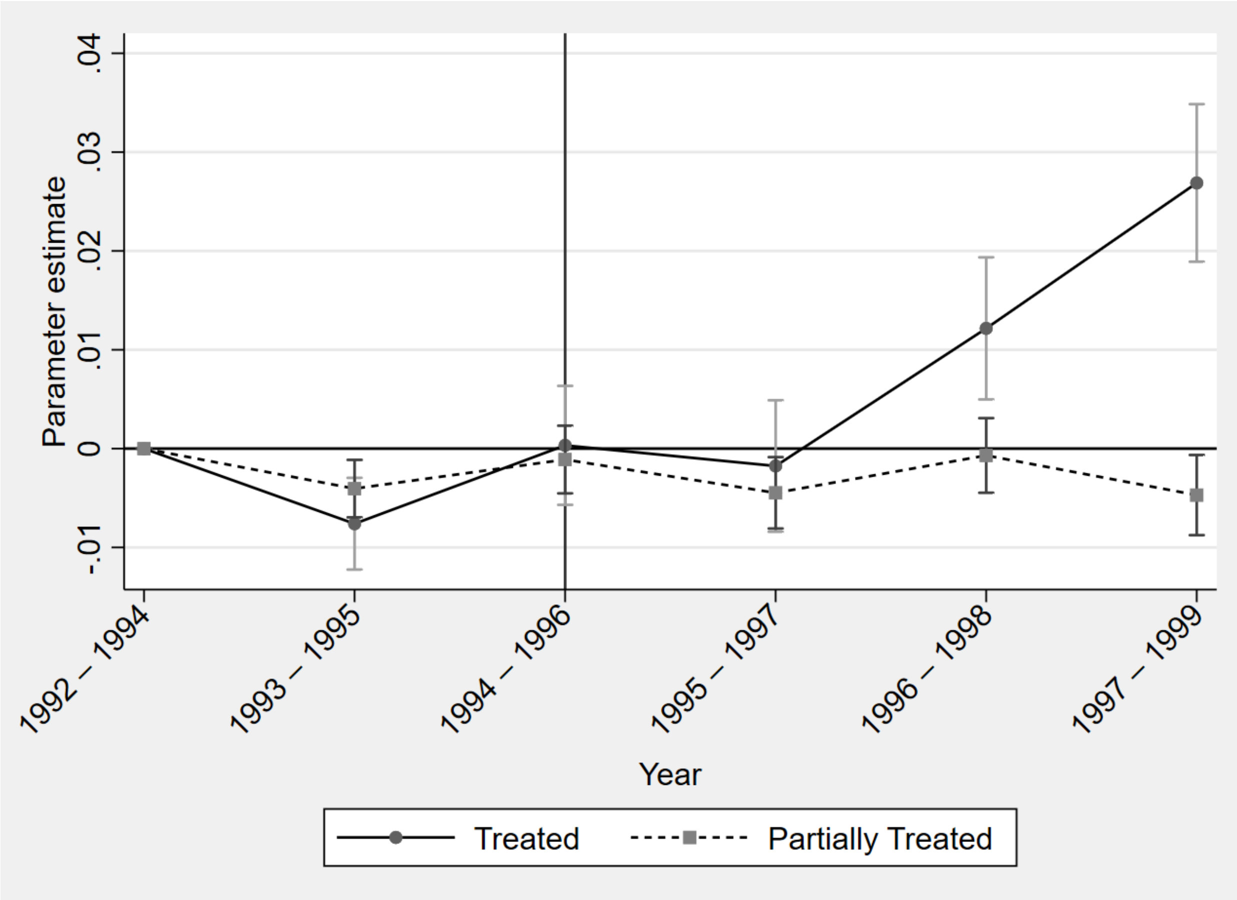
Figure 1: Difference-in-differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage



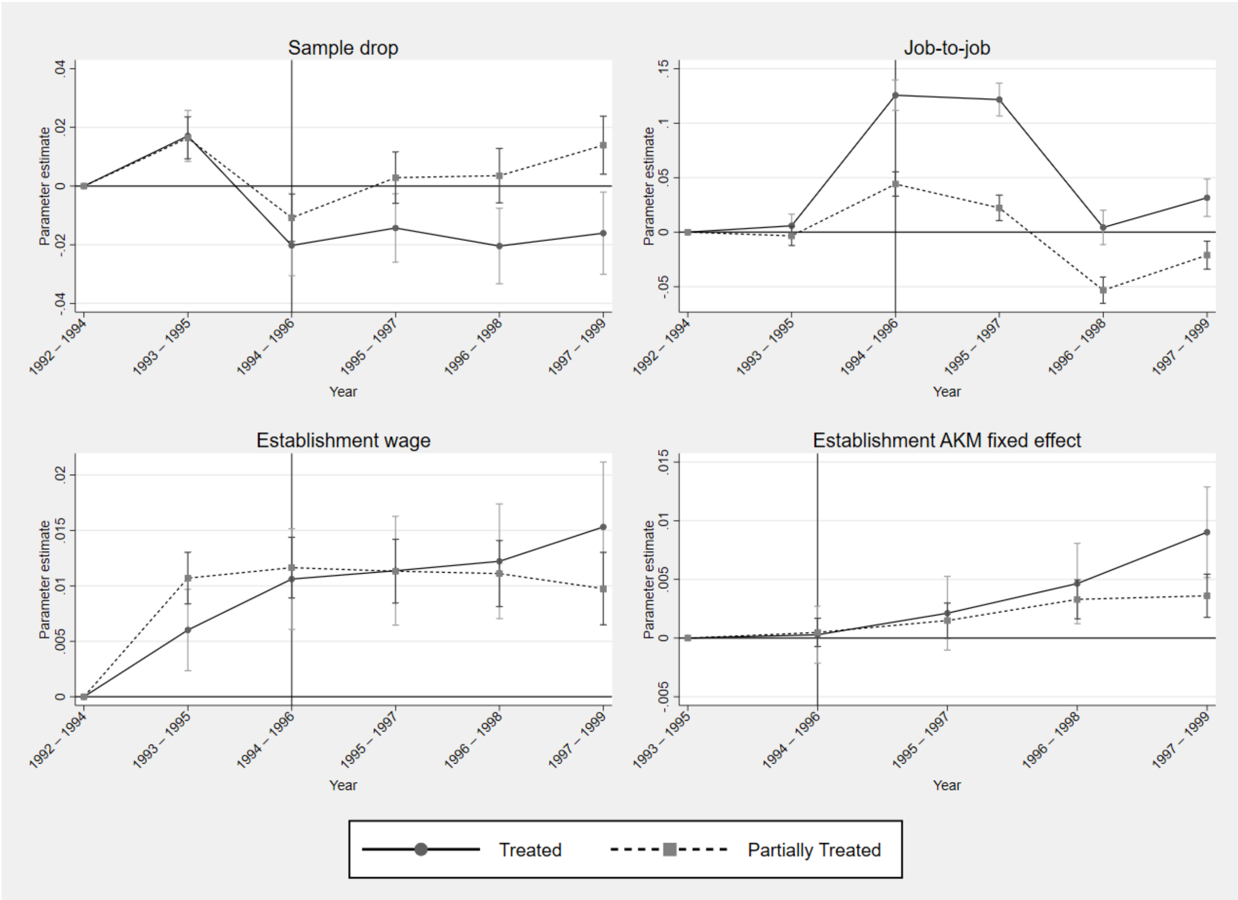
Notes: This figure illustrates the results of the difference-in-differences specification with the two-year change in log daily wages as the outcome (see Equation 3). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1992–94. **Source:** SIEED and BHP. Authors' calculations.

Figure 2: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage



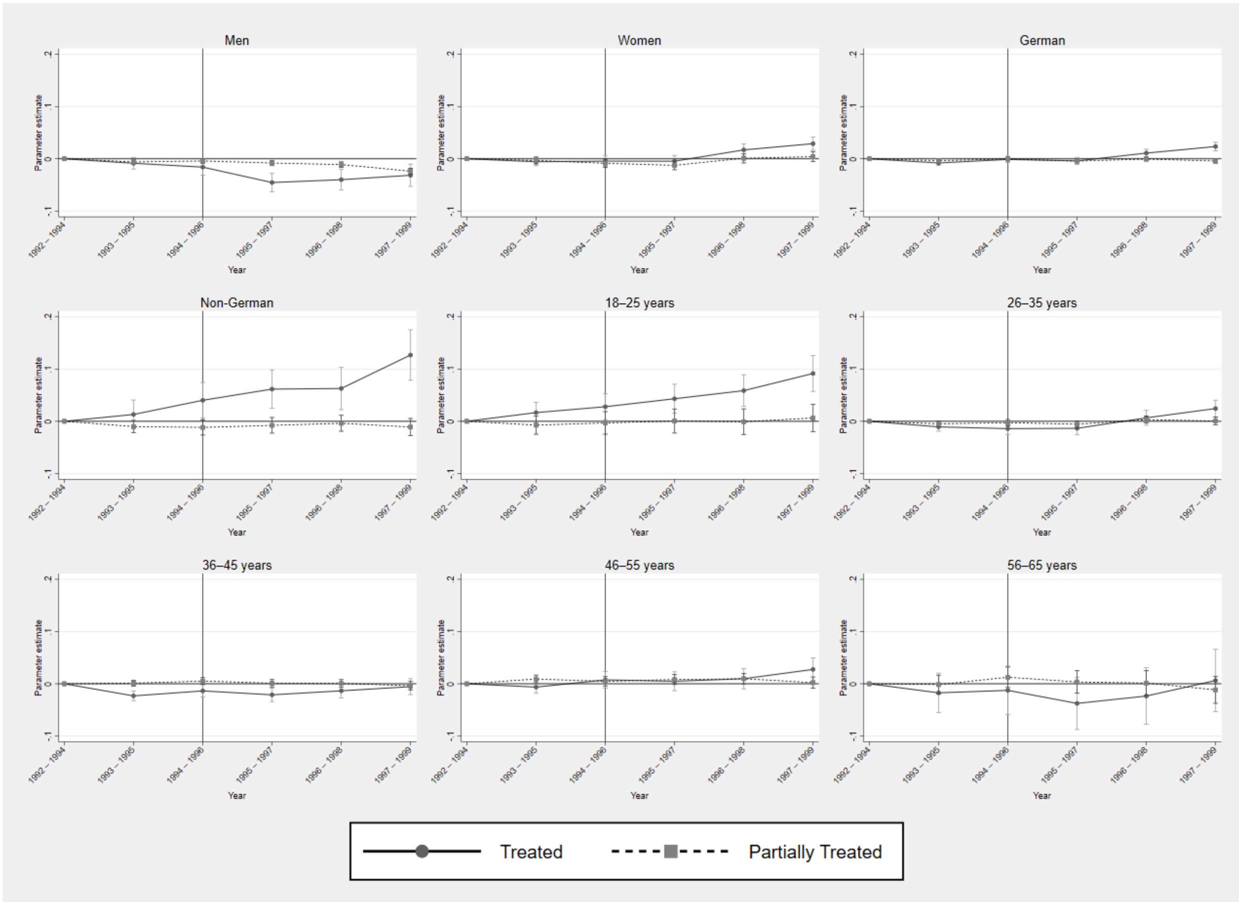
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1992–94. **Source:** SIEED and BHP. Authors' calculations.

Figure 3: Triple differences: Reallocation Spillover Effects of the Main Construction Sector Minimum Wage



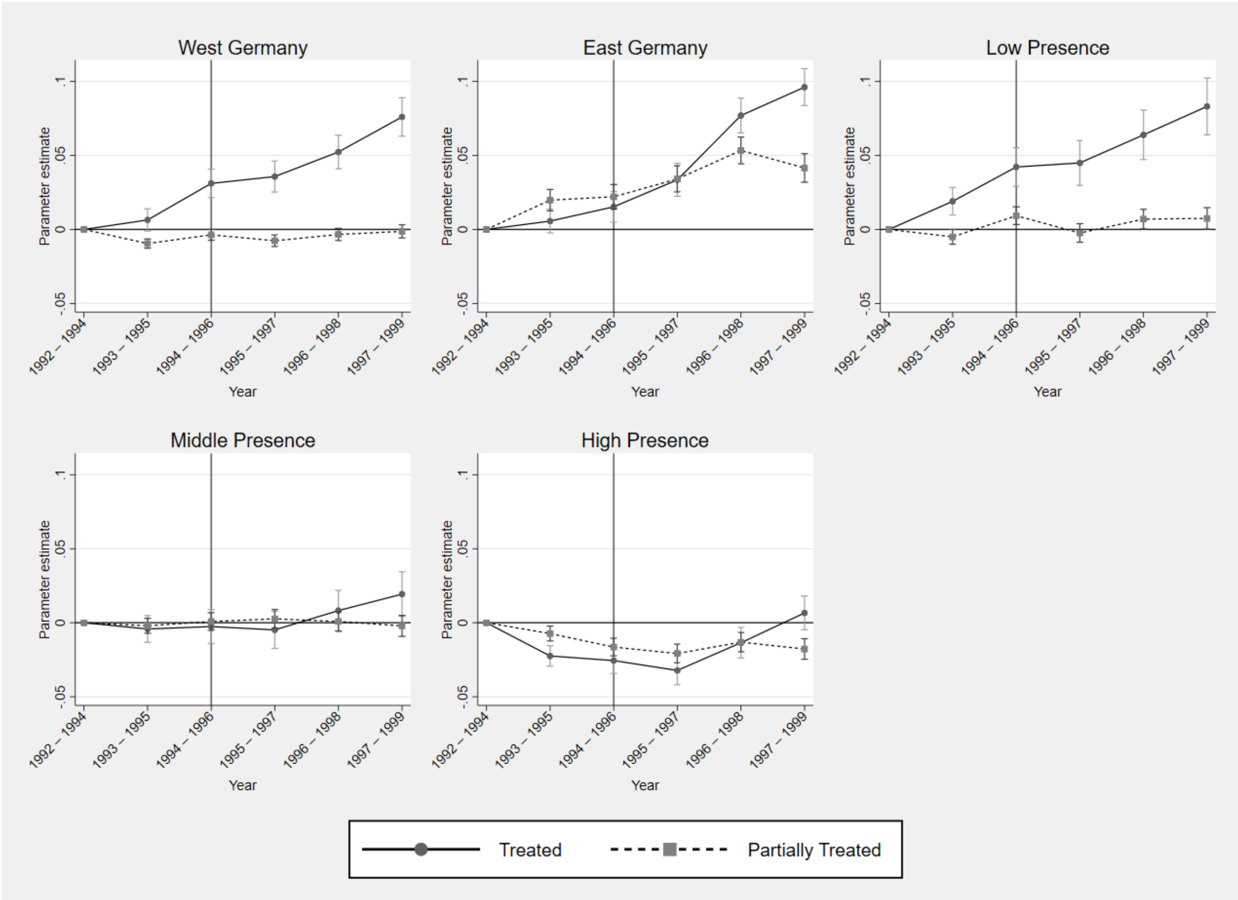
Notes: This figure shows the results of several triple differences specifications using different outcome variables (see Equation 4). I use 95% confidence intervals. In the first panel, I use the change in full-time employment status of an individual as the outcome variable, which takes the value -1 if the individual drops out of the sample in $t + 2$ and 0 if she is employed according to my sample criteria. In the second column, 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. In the third and fourth columns, I use the change in log establishment average wages and change in establishment AKM fixed effects as outcome variables, respectively. 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 1992–94. **Source:** SIEED and BHP. Author's calculations.

Figure 4: Triple differences: Wage Spillover Effects by Socio-Demographic Characteristics



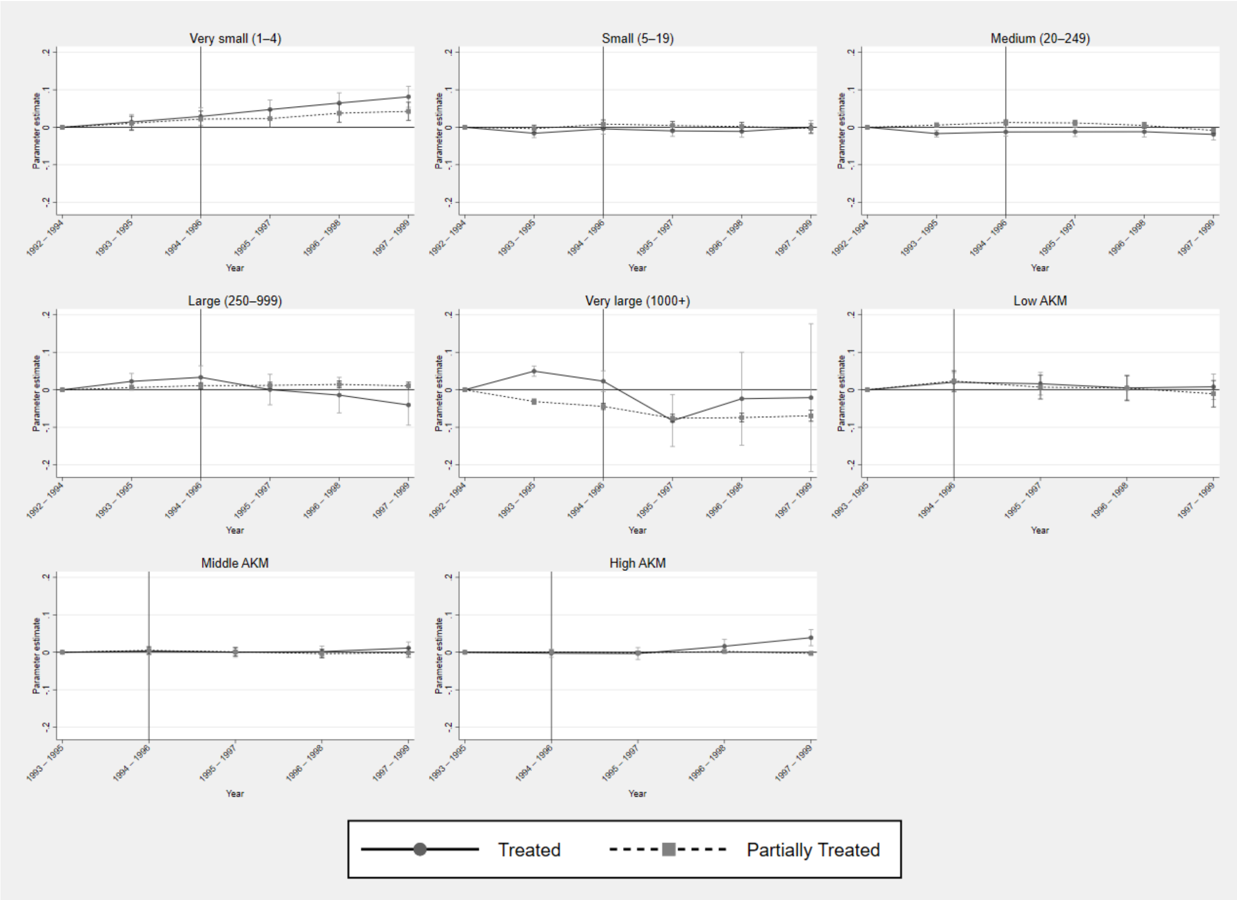
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. In each panel, I present the results of the DiDiD specification separately estimated for men, women, workers with German nationality, workers with non-German nationality, workers who were in either age group 18–25, 26–35, 36–45, 46–55 or 56–65, measured at 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 1992–94. **Source:** SIEED and BHP. Authors' calculations.

Figure 5: Triple differences: Wage Spillover Effects by Place of Work



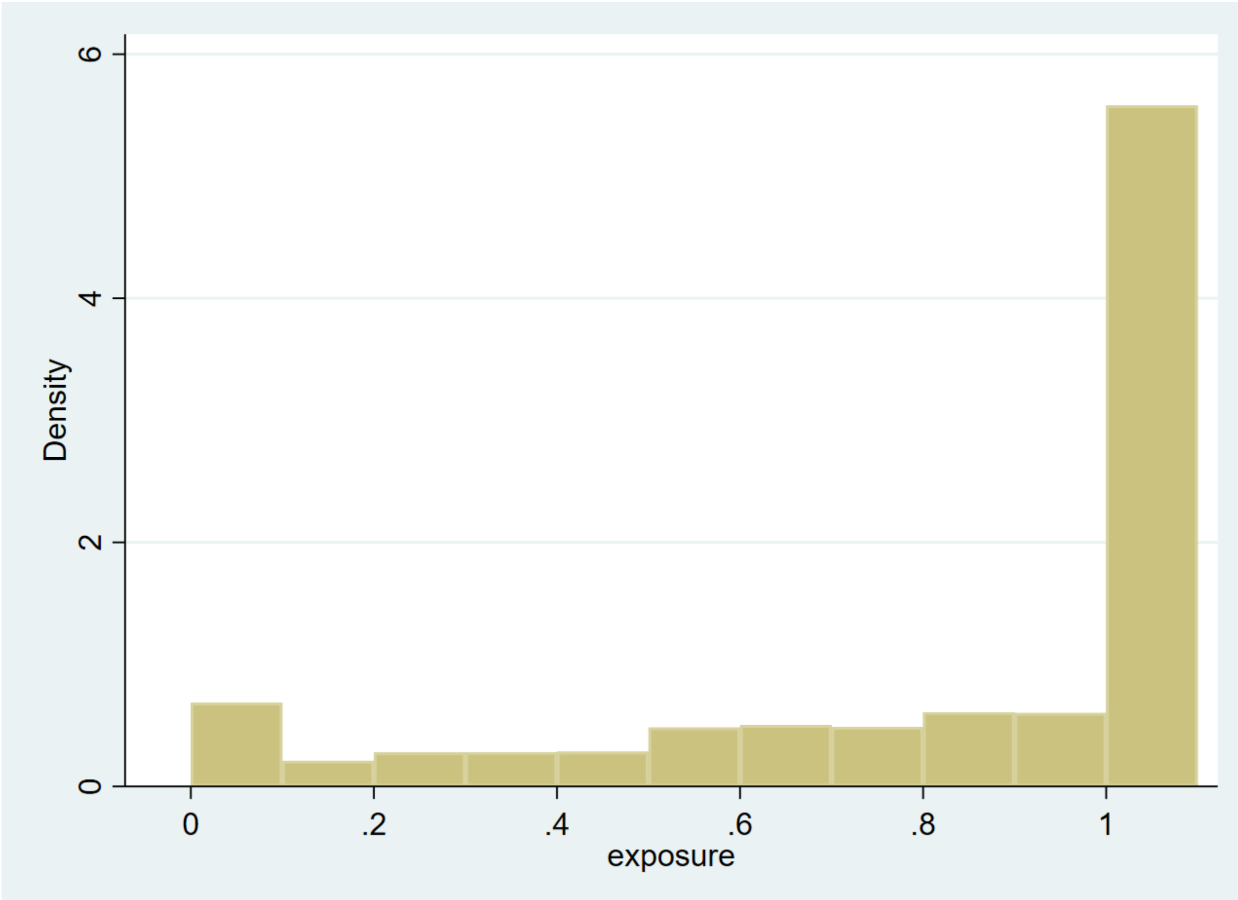
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. In each panel, I present the results of the DiDiD specification separately estimated for individuals working in an establishment in West Germany, East Germany, a labor market region with low presence of the main construction sector, middle presence of the main construction sector or high presence of the main construction sector in the pre-introduction period. I measure the establishment region characteristics for a worker 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 1992–94.
Source: SIEED and BHP. Authors' calculations.

Figure 6: Triple differences: Wage Spillover Effects by Establishment Characteristics



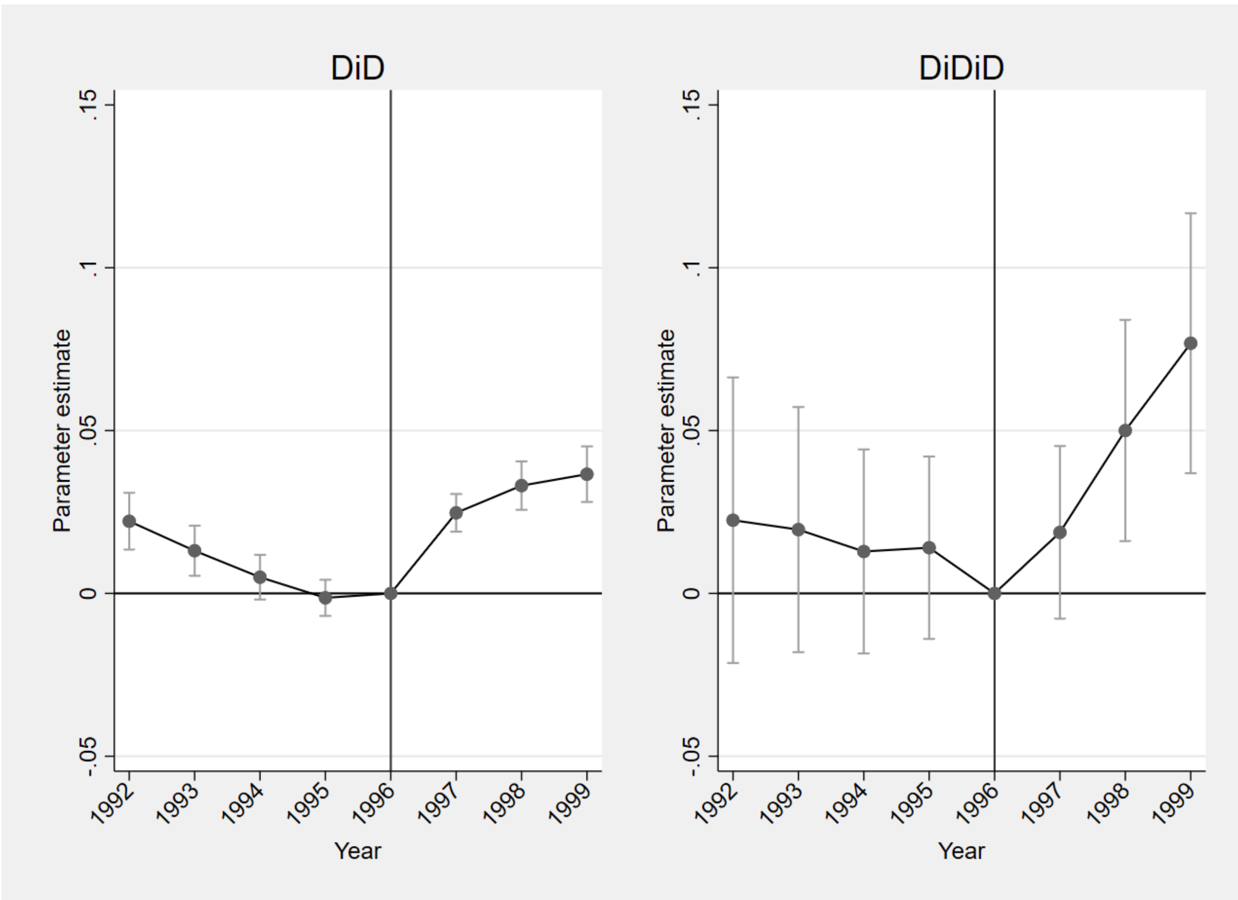
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 4). I use 95% confidence intervals. In each panel, I present the results of the DiDiD specification separately estimated for individuals working in a very small establishment (1–4 employees), small establishment (5–19 employees), medium establishment (20–250 employees), large establishment (250–999 employees) or very large establishment (1000+ employees) measured in t. The reference period is 1992–94. Furthermore, I use the position of the establishment in the distribution of the establishment-specific wage premia provided by Bellmann et al. (2020) using the estimation strategy in Abowd et al. (1999) and Card et al. (2013). I split this distribution into terciles and estimate the DiDiD specification separately for workers in an establishment in t with low establishment fixed effects (low AKM), middle establishment fixed effects (middle AKM) and high establishment fixed effects (high AKM). Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. Because I use the establishment fixed effects for 1993–99, the reference period here is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure 7: Density of Continuous Exposure Measure



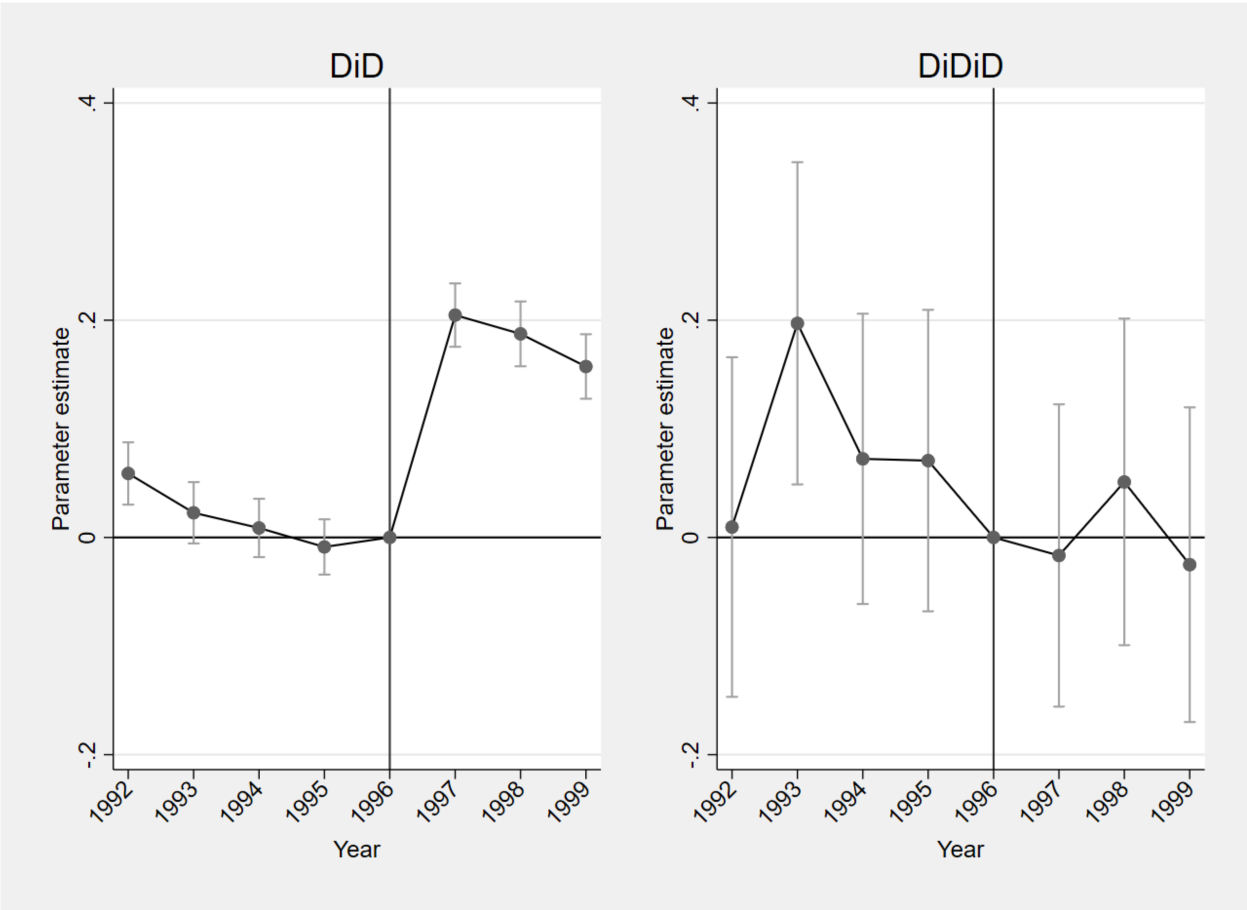
Notes: For this figure, I keep only one observation per establishment in the period 1992–96. **Source:** SIEED and BHP 1992–96. Authors' calculations.

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



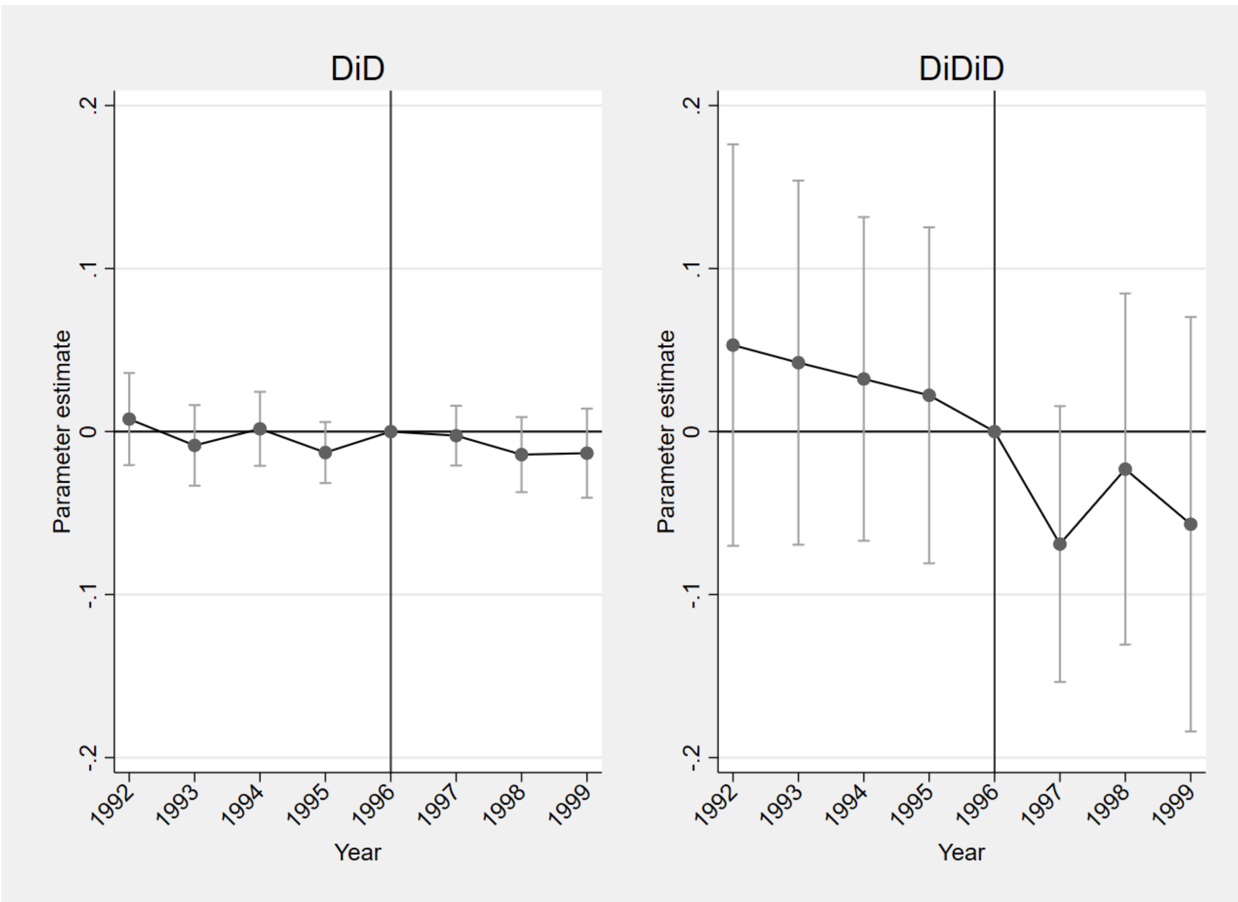
Notes: The DiD estimation uses 177,324 establishment-year observations and the DiDiD estimation uses 47,081 establishment-year observations. The outcome variable is the log (daily) average wage. In the panel DiD I estimate Equation 5 and in the panel DiDiD I estimate Equation 6. **Source:** SIEED and BHP 1992–99. Author's calculations.

Figure 9: Establishment-Level: Wage Spillovers from the Main Construction Sector Minimum Wage Using only Entrants



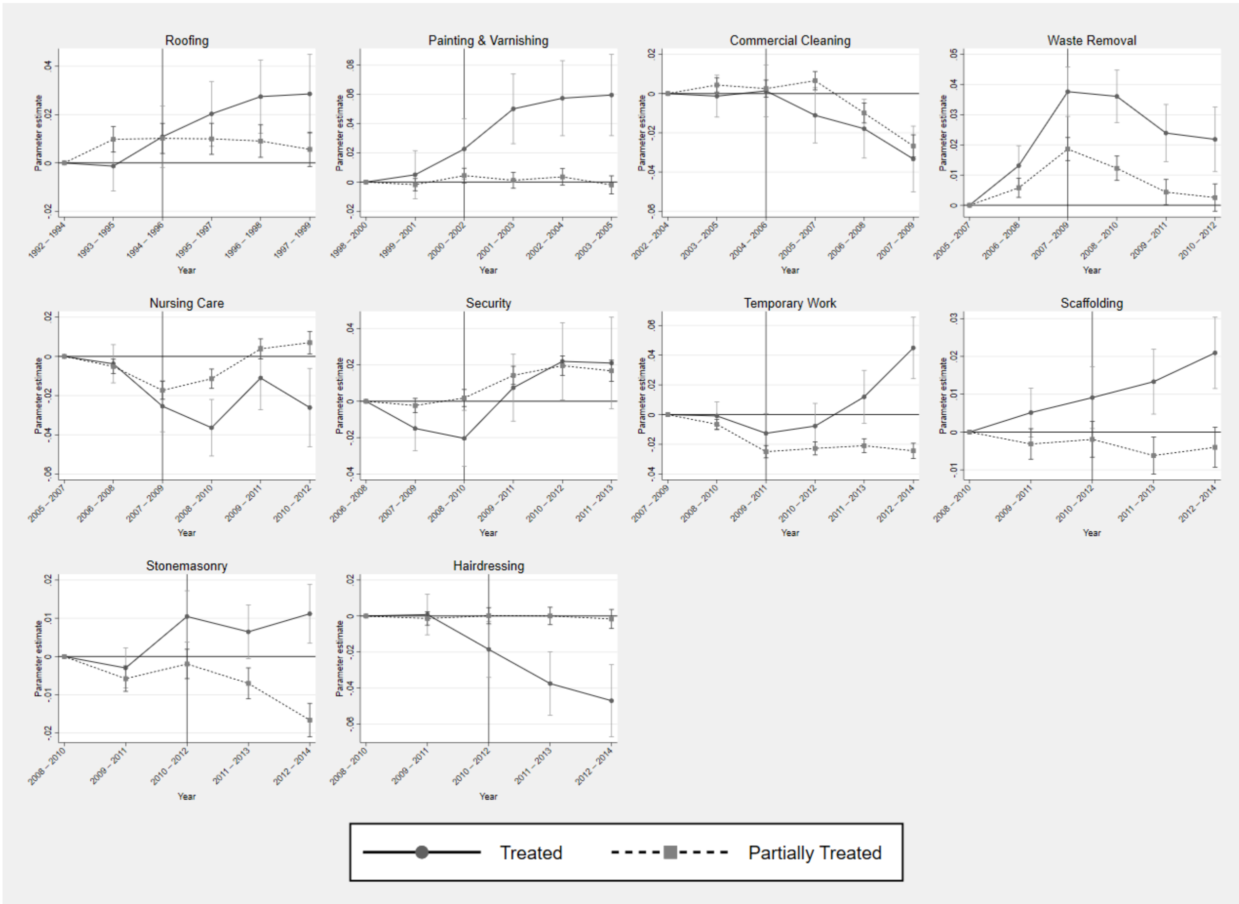
Notes: These estimations use only workers who entered the establishment in the respective year and collapse the data to the establishment level. The DiD estimation uses 55,386 establishment-year observations and the DiDiD estimation uses 12,240 establishment-year observations. The outcome variable is the log (daily) average wage. In the panel DiD I estimate Equation 5 and in the panel DiDiD I estimate Equation 6. **Source:** SIEED and BHP 1992–99. Author's calculations.

Figure 10: Establishment-Level: Employment Effects from the Main Construction Sector Minimum Wage



Notes: The DiD estimation uses 177,324 establishment-year observations and the DiDiD estimation uses 47,081 establishment-year observations. The outcome variable is the log number of full-time employed workers (according to sample restrictions). In the panel DiD I estimate Equation 5 and in the panel DiDiD I estimate Equation 6. **Source:** SIEED and BHP 1992–99. Author's calculations.

Figure 11: 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 7). 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. The reference period in each panel is the first time period in each figure. **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 (1992–96)

	Treated Group		**Partially Treated Group**		**Control Group**	
No. of observations	1,154,470		2,286,276		2,237,980	
Share	20.33		40.26		39.41	
Averages						
Daily wage	52.00	(11.35)	83.67	(9.70)	121.40	(17.96)
Log (daily) wage	3.92	(0.25)	4.42	(0.12)	4.79	(0.14)
Log (daily) two-year wage growth	0.11	(0.24)	0.03	(0.15)	0.01	(0.13)
Shares within group (in percent)						
Women	58.70		37.69		23.17	
Non-German nationality	8.82		8.97		6.84	
By age						
18-25 years old	25.70		18.20		4.51	
26-35 years old	34.63		42.68		40.32	
36-45 years old	25.10		24.14		33.44	
46-55 years old	12.74		12.61		18.22	
56-65 years old	1.83		2.36		3.51	
By education						
No vocational training	13.39		11.43		6.63	
Vocational training	83.69		84.03		78.80	
University or university of applied sciences	2.17		4.15		14.24	
Missing education	0.75		0.39		0.33	
By industry						
Agriculture and Forestry	3.44		1.21		0.38	
Fishing and Fish Farming	0.03		0.01		0.02	
Mining	0.32		1.66		2.15	
Manufacturing	22.51		30.47		38.18	
Energy and Water Supply	0.19		0.84		2.11	
Construction	3.32		4.73		2.44	
Trade and Repair	22.24		18.56		12.99	
Catering	10.11		1.97		0.69	
Transport and News	6.40		9.70		8.31	
Finance and Insurance	0.62		2.04		4.99	
Real Estate and Housing	12.49		7.17		7.91	
Public Services	3.12		8.14		6.54	
Education	0.96		2.05		3.07	
Health	7.95		8.34		6.55	
Other Services	5.60		2.78		3.55	
Private Household	0.38		0.27		0.13	
Missing industry	0.34		0.05		0.01	
By plant size						
Very small (1-4 workers)	21.13		7.32		3.47	
Small (5-19 workers)	28.65		19.87		12.55	
Medium (20-249 workers)	37.96		41.53		38.03	
Large (250-999 workers)	8.38		18.08		22.23	
Very large (1000+ workers)	3.88		13.20		23.71	
By region type						
District-free cities	30.84		36.64		45.51	
Urban districts	26.79		33.48		36.34	
Rural districts, some densely populated areas	20.40		15.37		10.25	
Rural districts, sparsely populated	21.98		14.51		7.89	

Notes: 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, 1992–1996. Authors' calculations.

Table 3: Triple differences: Robustness Checks on Wage Spillovers

	Baseline	Region-specific shocks	Industry-specific shocks	Region + Industry shocks	Region + Industry + Occupation shocks	Different Treated	Different Option
Treated x Option							
x 1993-95	-0.008***	0.003	-0.006**	0.002	-0.001	0.000	-0.014***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
x 1994-96	0.000	0.021***	-0.001	0.015***	0.005	0.005	-0.007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
x 1995-97	-0.002	0.025***	-0.002	0.021***	0.011***	0.005	0.011**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
x 1996-98	0.012***	0.040***	0.006*	0.031***	0.018***	0.009*	0.009
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
x 1997-99	0.027***	0.058***	0.015***	0.042***	0.024***	0.010**	0.023***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Partial x Option							
x 1993-95	-0.004***	-0.002	-0.001	0.001	0.000	-0.001	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.004)
x 1994-96	-0.001	0.003	0.003*	0.006***	0.002	-0.006***	0.008*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
x 1995-97	-0.004**	0.002	0.002	0.008***	0.003	-0.008***	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
x 1996-98	-0.001	0.006***	0.004*	0.010***	0.004*	-0.012***	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
x 1997-99	-0.005**	0.004*	-0.002	0.006***	-0.001	-0.016***	0.027***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
No. of observations	1,188,109	1,175,828	1,188,109	1,175,828	1,175,828	1,188,109	2,301,796
No. of individuals	293,542	290,601	293,542	290,601	290,601	293,542	526,444
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	yes	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no
Occupation x year fixed effects	no	no	no	no	yes	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 2 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 add occupation times year fixed effects to the specification of the fourth column. 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 unskilled or skilled manual occupation (outside the main construction sector) at year t and equal to 0 if an individual i is working in an agricultural occupation at year t. The reference period is 1992–94. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table 4: Triple differences: Robustness Checks on Probability To Drop Out of Sample

	Baseline	Region-specific shocks	Industry-specific shocks	Region + Industry shocks	Region + Industry + Occupation shocks	Different Treated	Different Option
Treated x Option							
x 1993-95	0.017***	0.012**	0.015***	0.011**	0.010*	0.014**	-0.006
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.008)
x 1994-96	-0.020***	-0.031***	-0.042***	-0.047***	-0.037***	-0.029***	-0.010
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)
x 1995-97	-0.014**	-0.023***	-0.032	-0.036***	-0.034***	-0.019**	-0.006
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.010)
x 1996-98	-0.020***	-0.027***	-0.024***	-0.028***	-0.028***	-0.025***	0.031***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.011)
x 1997-99	-0.016**	-0.024***	-0.017**	-0.024***	-0.020**	-0.012	0.034***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.012)
Partial x Option							
x 1993-95	0.016***	0.018***	0.017***	0.019***	0.019***	0.018***	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.009)
x 1994-96	-0.011***	-0.010**	-0.017***	-0.015***	-0.001	-0.016***	-0.013
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.009)
x 1995-97	0.003	0.005	-0.004	-0.001	0.008	0.001	-0.010
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.010)
x 1996-98	0.004	0.010**	0.003	0.009*	0.013**	0.010**	-0.012
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.011)
x 1997-99	0.014***	0.020***	0.012**	0.016***	0.021***	0.026***	-0.012
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.011)
No. of observations	1,509,255	1,493,275	1,509,255	1,493,275	1,493,275	1,509,255	2,873,761
No. of individuals	376,398	372,547	376,398	372,547	372,547	376,398	672,019
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	yes	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no
Occupation x year fixed effects	no	no	no	no	yes	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year probability to drop out of the sample 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 2 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 add occupation times year fixed effects to the specification of the fourth column. 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 unskilled or skilled manual occupation (outside the main construction sector) at year t and equal to 0 if an individual i is working in an agricultural occupation at year t. The reference period is 1992–94. 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-specific shocks	Industry-specific shocks	Region + Industry shocks	Region + Industry + Occupation shocks	Different Treated	Different Option
Treated x Option							
x 1993-95	0.006	-0.008	-0.011*	-0.021***	-0.022***	0.055***	0.020*
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.011)
x 1994-96	0.126***	0.117***	0.024***	0.034***	0.031***	0.190***	0.044***
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.014)
x 1995-97	0.122***	0.108***	0.012	0.021**	0.011	0.161***	0.018
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.014)
x 1996-98	0.004	0.011	-0.042***	-0.024***	-0.038***	0.016*	0.003
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.014)
x 1997-99	0.032***	0.037***	-0.020**	-0.004	-0.015	0.056***	0.000
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.015)
Partial x Option							
x 1993-95	-0.003	-0.005	-0.008*	-0.011**	-0.007	0.009**	0.013
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.011)
x 1994-96	0.044***	0.044***	0.019***	0.022***	0.030***	0.079***	0.027**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.014)
x 1995-97	0.022***	0.022***	-0.012**	-0.006	-0.001	0.038***	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.013)
x 1996-98	-0.053***	-0.039***	-0.067***	-0.050***	-0.040***	-0.052***	-0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.013)
x 1997-99	-0.021***	-0.008	-0.038***	-0.022***	-0.007	-0.013**	0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.014)
No. of observations	1,188,109	1,175,828	1,188,109	1,175,828	1,175,828	1,188,109	2,301,796
No. of individuals	293,542	290,601	293,542	290,601	290,601	293,542	526,444
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	yes	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no
Occupation x year fixed effects	no	no	no	no	yes	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year probability to switch establishments 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 2 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 add occupation times year fixed effects to the specification of the fourth column. 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 unskilled or skilled manual occupation (outside the main construction sector) at year t and equal to 0 if an individual i is working in an agricultural occupation at year t. The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table 6: Triple differences: Robustness Checks on Change in Average Establishment Wages

	Baseline	Region-specific shocks	Industry-specific shocks	Region + Industry shocks	Region + Industry + Occupation shocks	Different Treated	Different Option
Treated x Option							
x 1993-95	0.006***	0.008***	0.003	0.004**	0.002	0.015***	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
x 1994-96	0.011***	0.011***	0.004*	0.005*	0.003	0.015***	0.006
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
x 1995-97	0.011***	0.014***	0.005**	0.008***	0.006**	0.015***	0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
x 1996-98	0.012***	0.016***	0.007***	0.011***	0.008***	0.013*	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
x 1997-99	0.015***	0.020***	0.007**	0.012***	0.009***	0.014***	0.025***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Partial x Option							
x 1993-95	0.011***	0.012***	0.009***	0.011***	0.009***	0.007**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
x 1994-96	0.012***	0.013***	0.009***	0.011***	0.010***	0.009***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
x 1995-97	0.011***	0.013***	0.009***	0.011***	0.009***	0.009***	0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
x 1996-98	0.011***	0.014***	0.009***	0.012***	0.010***	0.008***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
x 1997-99	0.010***	0.013***	0.007***	0.010***	0.009***	0.006**	0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
No. of observations	1,023,712	1,013,012	1,023,712	1,013,012	1,013,012	1,023,712	2,005,781
No. of individuals	260,930	258,293	260,930	258,293	258,293	260,930	477,514
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	yes	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no
Occupation x year fixed effects	no	no	no	no	yes	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in the establishments' average wage as the outcome variable (see Equation 4). The outcome variable equals 0 by definition for establishment stayers and changes only for establishment switchers. Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 2 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 add occupation times year fixed effects to the specification of the fourth column. 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 unskilled or skilled manual occupation (outside the main construction sector) at year t and equal to 0 if an individual i is working in an agricultural occupation at year t. The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

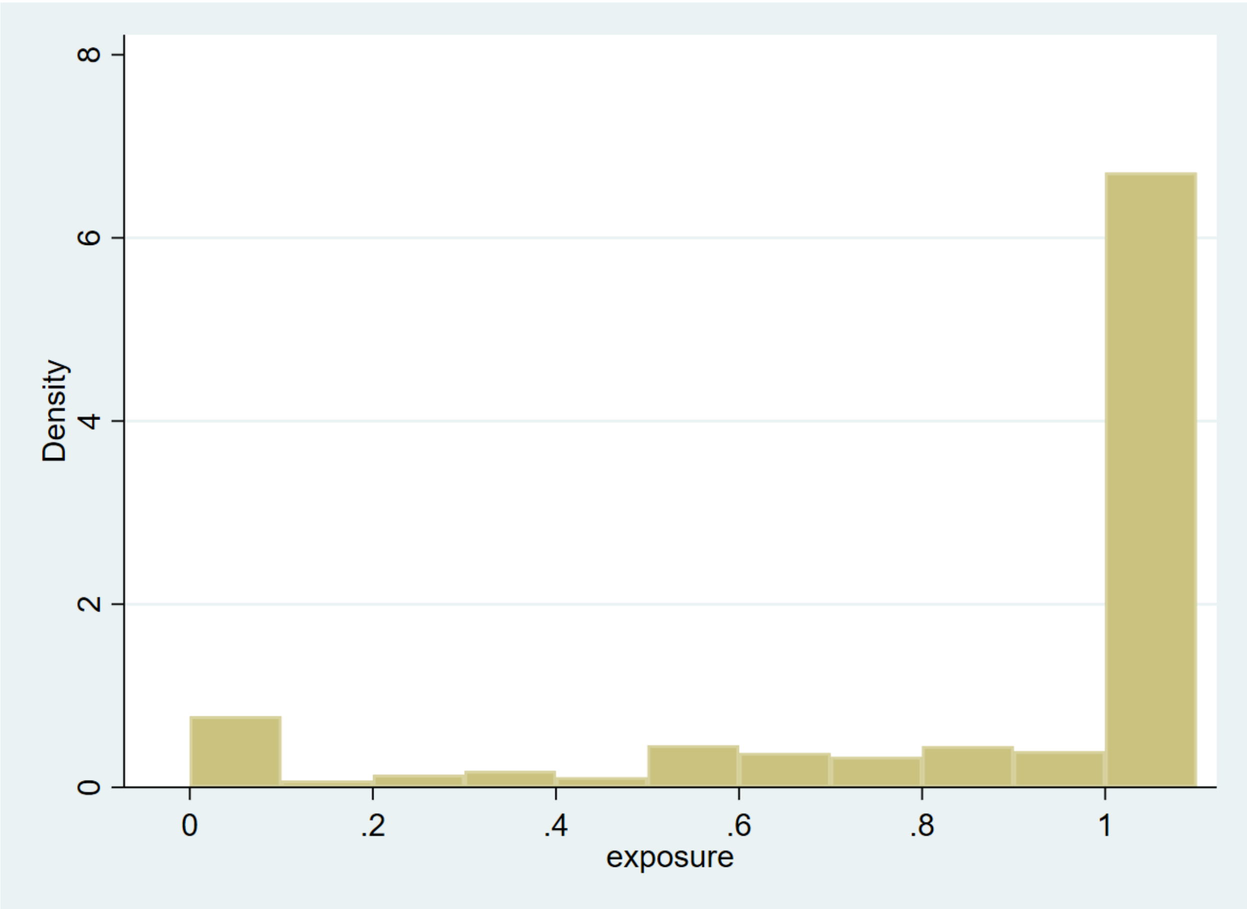
Table 7: Triple differences: Robustness Checks on Change in Establishment AKM Fixed Effects

	Baseline	Region-specific shocks	Industry-specific shocks	Region + Industry shocks	Region + Industry + Occupation shocks	Different Treated	Different Option
Treated x Option							
x 1993-95							
x 1994-96	0.000	0.000	0.001	0.001	0.000	-0.002	0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
x 1995-97	0.002	0.002	0.003*	0.003*	0.002	0.000	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1996-98	0.005***	0.005***	0.004**	0.005**	0.003*	0.002	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
x 1997-99	0.009***	0.010***	0.008***	0.009***	0.008***	0.004**	0.017***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Partial x Option							
x 1993-95							
x 1994-96	0.000	0.000	0.001	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1995-97	0.001*	0.001*	0.003***	0.003***	0.002**	0.002***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1996-98	0.003***	0.004***	0.004***	0.004***	0.003***	0.004***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
x 1997-99	0.004***	0.004***	0.004***	0.005***	0.004***	0.002***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
No. of observations	810,963	802,559	810,963	802,559	802,559	810,963	1,611,758
No. of individuals	227,494	225,194	227,494	225,194	225,194	227,494	428,853
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	yes	no	no
Industry x year fixed effects	no	no	yes	yes	yes	no	no
Occupation x year fixed effects	no	no	no	no	yes	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in the establishments' AKM fixed effect as the outcome variable (see Equation 4). The outcome variable equals 0 by definition for establishment stayers and changes only for establishment switchers. Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 2 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 add occupation times year fixed effects to the specification of the fourth column. 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 unskilled or skilled manual occupation (outside the main construction sector) at year t and equal to 0 if an individual i is working in an agricultural occupation at year t. The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

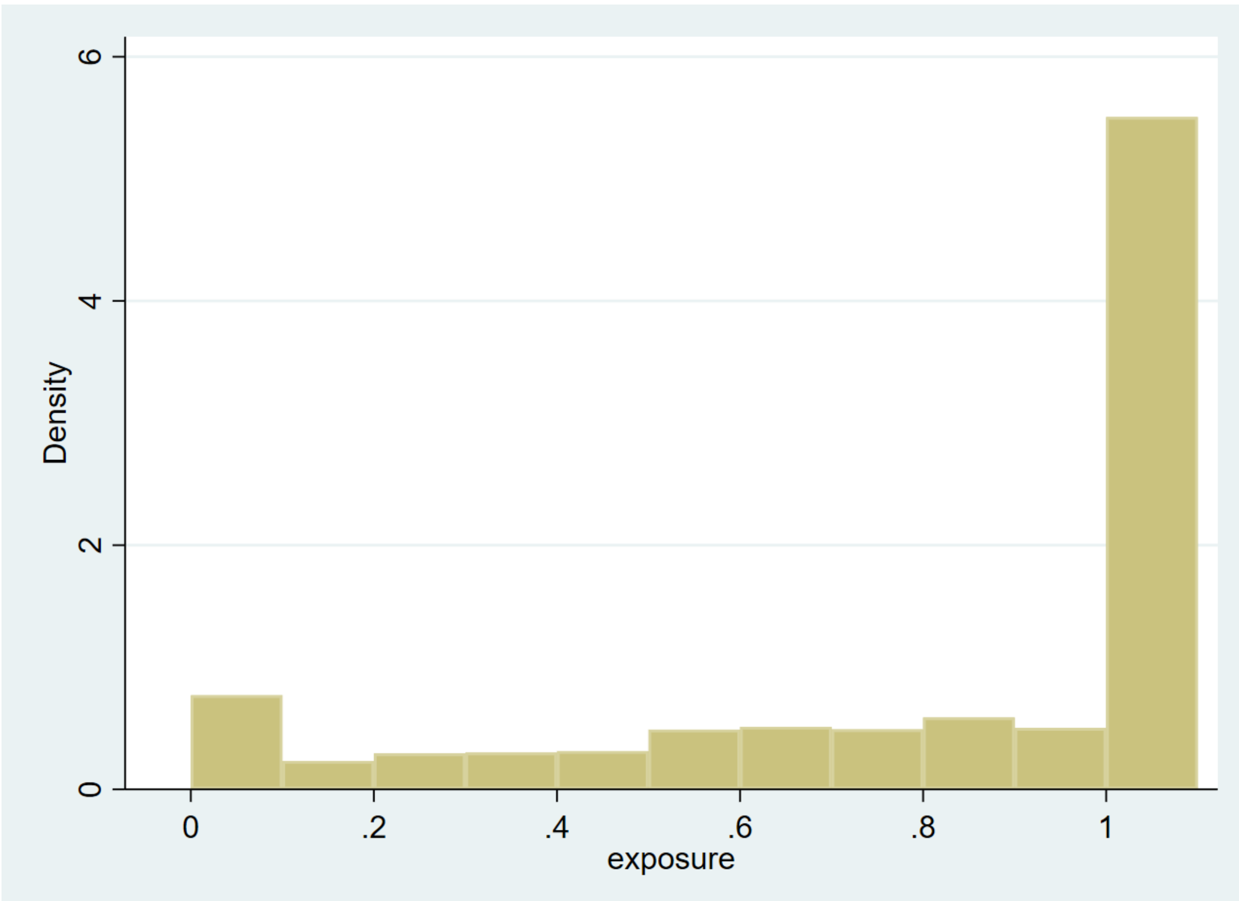
A Appendix

Figure A.1: Density of Continuous Exposure Measure Using only Entrants



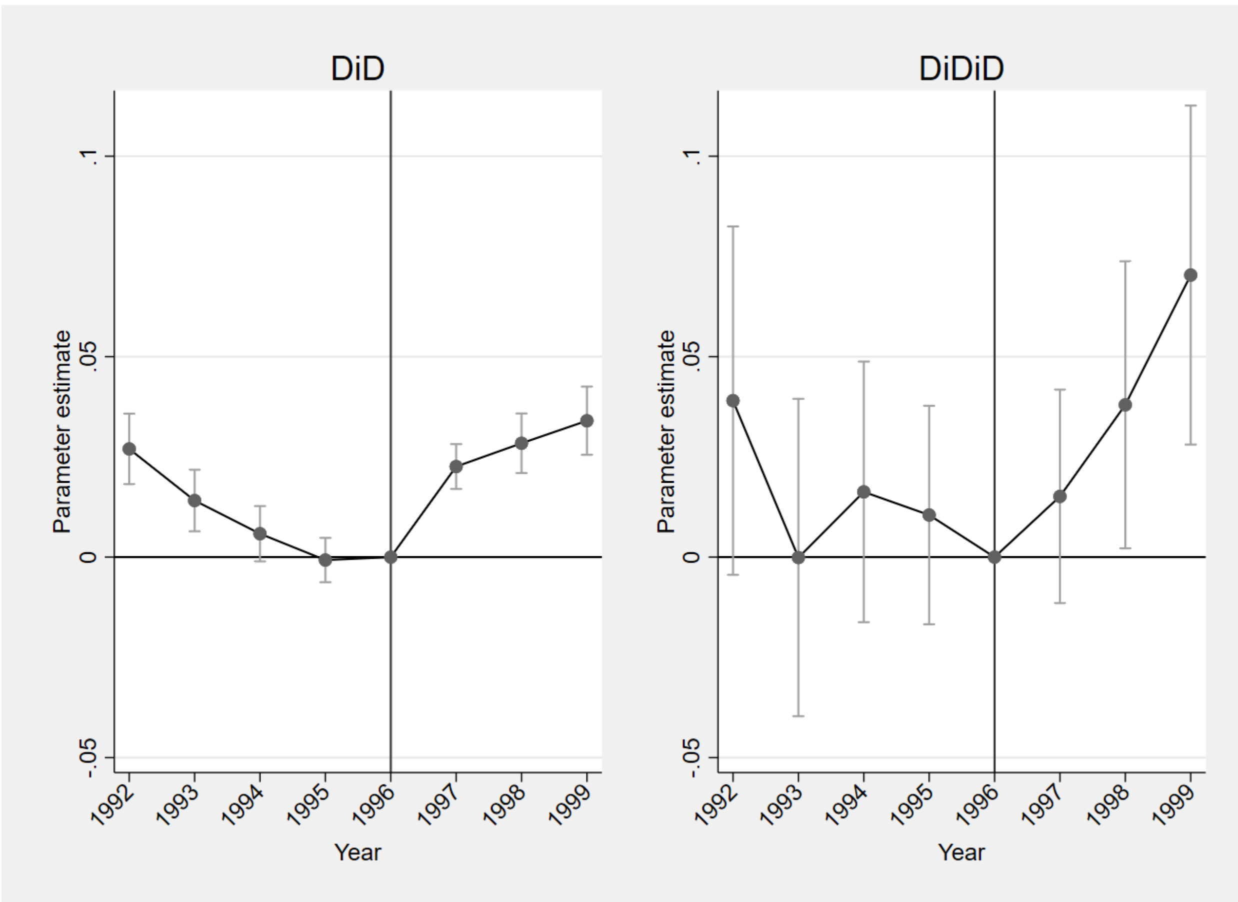
Notes: For this figure, I keep only one observation per establishment in the period 1992–96. Furthermore, I use only new entrants to the establishment in a respective year. **Source:** SIEED and BHP 1992–96. Authors' calculations.

Figure A.2: Density of Continuous Exposure Measure Using only Incumbents



Notes: For this figure, I keep only one observation per establishment in the period 1992–96. Furthermore, I use only incumbent workers in an establishment-year combination. **Source:** SIEED and BHP 1992–96. Authors' calculations.

Figure A.3: Wage Spillovers from the Main Construction Sector Minimum Wage Using only Incumbents



Notes: These estimations use only incumbent workers in an establishment-year combination and then collapse the data to the establishment level. Incumbent workers are defined as individuals who were already employed in the year before. The DiD estimation uses 161,865 establishment-year observations and the DiDiD estimation uses 43,293 establishment-year observations. The outcome variable are log (daily) average wages. In the panel DiD I estimate Equation 5 and in the panel DiDiD I estimate Equation 6. **Source:** SIEED and BHP. Authors' 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
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
203	Manufacture of builders' carpentry and joinery
204	Manufacture of wooden containers
232	Manufacture of refined petroleum products
241	Manufacture of basic chemicals
264	Manufacture of bricks, tiles and construction products, in baked clay
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
352	Manufacture of railway and tramway locomotives and rolling stock
361	Manufacture of furniture
372	Recycling of non-metal waste and scrap
401	Production and distribution of electricity
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
712	Renting of other transport equipment
713	Renting of other machinery and equipment
742	Architectural and engineering activities and related technical consultancy

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

Industry	
No.	Description
15	Hunting, trapping and game propagation, including related service activities
132	Mining of non-ferrous metal ores, except uranium and thorium ores
233	Processing of nuclear fuel
242	Manufacture of pesticides and other agro-chemical products
334	Manufacture of optical instruments and photographic equipment
523	Retail sale of pharmaceutical and medical goods, cosmetic and toilet articles
603	Transport via pipelines
621	Scheduled air transport
622	Non-scheduled air transport
623	Space transport
633	Activities of travel agencies and tour operators; tourist assistance activities n.e.c.
641	Post and courier activities
642	Telecommunications
721	Hardware consultancy
724	Database activities
726	Other computer related activities
801	Primary education
851	Human health activities
922	Radio and television activities
930	Other service activities

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

	(1)	(2)	(3)	(4)	(5)
Treated					
x 1993–95	-0.028***	-0.031***	-0.029***	-0.029***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1994–96	-0.040***	-0.042***	-0.041***	-0.039***	0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1995–97	-0.046***	-0.049***	-0.047***	-0.046***	0.033***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1996–98	-0.050***	-0.053***	-0.051***	-0.050***	0.042***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1997–99	-0.042***	-0.045***	-0.044***	-0.043***	0.049***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Partial					
x 1993–95	0.000	-0.003***	-0.002***	-0.002***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
x 1994–96	-0.002***	-0.005***	-0.004***	-0.004***	0.014***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
x 1995–97	-0.006***	-0.009***	-0.008***	-0.007***	0.023***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
x 1996–98	-0.009***	-0.012***	-0.011***	-0.010***	0.029***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
x 1997–99	-0.009***	-0.012***	-0.011***	-0.011***	0.031***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No. of observations	6,744,756	6,744,756	6,744,756	6,744,756	6,333,652
No. of workers	1,845,526	1,845,526	1,845,526	1,845,526	1,434,422
Year fixed effects	yes	yes	yes	yes	yes
Demographic controls	no	yes	yes	yes	no
1-digit industry fixed effects	no	no	yes	yes	yes
Federal state fixed effects	no	no	no	yes	yes
Region type fixed effects	no	no	no	yes	yes
Worker fixed effects	no	no	no	no	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 and add age group dummies, education dummies, gender and nationality in column (2). In column (3), I add 1-digit industry fixed effects and in column (4) I add federal state as well as region type fixed effects. Finally, column (5) uses worker fixed effects. The reference period is 1992–94. 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)
Treated x Option		
x 1993–95	-0.003	-0.008***
	(0.003)	(0.002)
x 1994–96	-0.004	0.000
	(0.003)	(0.003)
x 1995–97	-0.016***	-0.002
	(0.003)	(0.003)
x 1996–98	-0.006*	0.012***
	(0.003)	(0.004)
x 1997–99	0.009***	0.027***
	(0.003)	(0.004)
Partial x Option		
x 1993–95	0.000	-0.004***
	(0.002)	(0.001)
x 1994–96	0.006***	-0.001
	(0.002)	(0.002)
x 1995–97	-0.000	-0.004**
	(0.002)	(0.002)
x 1996–98	0.003	-0.001
	(0.002)	(0.002)
x 1997–99	0.001	-0.005**
	(0.002)	(0.002)
No. of observations	1,327,599	1,188,109
No. of workers	433,032	293,542
Year fixed effects	yes	yes
Demographic controls	yes	no
1-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	no	yes

Notes: This table shows the results of two triple differences specifications with the two-year change in log daily wages as the outcome (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 use year fixed effects, age group dummies, education dummies, gender, nationality, 1-digit industry fixed effects, federal state as well as region type fixed effects. Column (2) uses worker fixed effects. The reference period is 1992–94. 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 1993–95	-0.001	-0.009***
	(0.002)	(0.002)
x 1994–96	0.007***	0.007***
	(0.002)	(0.002)
x 1995–97	0.020***	0.017***
	(0.002)	(0.003)
x 1996–98	0.017***	0.028***
	(0.002)	(0.003)
x 1997–99	0.011***	0.037***
	(0.003)	(0.003)
Partial		
x 1993–95	0.005***	0.001
	(0.001)	(0.001)
x 1994–96	0.009***	0.008***
	(0.001)	(0.001)
x 1995–97	0.017***	0.012***
	(0.001)	(0.001)
x 1996–98	0.019***	0.018***
	(0.001)	(0.001)
x 1997–99	0.021***	0.017***
	(0.001)	(0.001)
No. of observations	580,427	604,974
No. of workers	141,683	151,635
Year fixed effects	yes	yes
Demographic controls	no	no
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 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Authors’ calculations.

Table A8: Triple differences: Reallocation Spillover Effects of the Main Construction Sector Minimum Wage

	Sample drop	**Job-to-job**	**Establishment wage**	**Establishment AKM fixed effect**
Treated x Option				
x 1993–95	0.017***	0.006	0.006***	
	(0.004)	(0.005)	(0.002)	
x 1994–96	-0.020***	0.126***	0.011***	0.000
	(0.005)	(0.007)	(0.002)	(0.001)
x 1995–97	-0.014**	0.122***	0.011***	0.002
	(0.006)	(0.008)	(0.003)	(0.002)
x 1996–98	-0.020***	0.004	0.012***	0.005***
	(0.007)	(0.008)	(0.003)	(0.002)
x 1997–99	-0.016**	0.032***	0.015***	0.009***
	(0.007)	(0.009)	(0.003)	(0.002)
Partial x Option				
x 1993–95	0.016***	-0.003	0.011***	
	(0.004)	(0.005)	(0.001)	
x 1994–96	-0.011***	0.044***	0.012***	0.000
	(0.004)	(0.006)	(0.001)	(0.001)
x 1995–97	0.003	0.022***	0.011***	0.001*
	(0.004)	(0.006)	(0.001)	(0.001)
x 1996–98	0.004	-0.053***	0.011***	0.003***
	(0.005)	(0.006)	(0.002)	(0.001)
x 1997–99	0.014***	-0.021***	0.010***	0.004***
	(0.005)	(0.007)	(0.002)	(0.001)
No. of observations	1,509,255	1,188,109	1,023,712	810,963
No. of individuals	376,398	293,542	260,930	227,494
Year fixed effects	yes	yes	yes	yes
Demographic controls	no	no	no	no
1-digit Industry fixed effects	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes

Notes: This table shows the results of several 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 employment status of an individual as the outcome variable, which takes the value -1 if the individual is not employed in $t+2$ and 0 if she is employed. In the second column, 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. In the third and fourth columns, I use the change in log establishment average wages and change in establishment AKM fixed effects as outcome variables, respectively. 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 A9: Triple differences: Wage Spillover Effects by Socio-Demographic Characteristics

	Men	**Women**	**German**	**Non-German**	**18–25 years**	**26–35 years**	**36–45 years**	**46–55 years**	**56–65 years**
Treated x Option									
x 1993–95	-0.009	-0.006	-0.008***	0.013	0.017*	-0.011**	-0.023***	-0.006	-0.017
	(0.006)	(0.004)	(0.002)	(0.014)	(0.010)	(0.004)	(0.005)	(0.006)	(0.019)
x 1994–96	-0.016**	-0.004	-0.001	0.040**	0.028**	-0.014**	-0.014**	0.008	-0.013
	(0.008)	(0.005)	(0.003)	(0.017)	(0.013)	(0.006)	(0.006)	(0.008)	(0.024)
x 1995–97	-0.045***	-0.005	-0.004	0.062***	0.043***	-0.013**	-0.021***	0.005	-0.038
	(0.009)	(0.006)	(0.003)	(0.019)	(0.014)	(0.006)	(0.007)	(0.009)	(0.026)
x 1996–98	-0.040***	0.017***	0.011***	0.063***	0.059***	0.007	-0.013*	0.010	-0.024
	(0.010)	(0.006)	(0.004)	(0.021)	(0.015)	(0.007)	(0.007)	(0.010)	(0.028)
x 1997–99	-0.032***	0.029***	0.024***	0.127***	0.092***	0.024***	-0.006	0.027**	0.006
	(0.011)	(0.006)	(0.004)	(0.025)	(0.018)	(0.008)	(0.008)	(0.011)	(0.030)
Partial x Option									
x 1993–95	-0.006***	-0.003	-0.003**	-0.010*	-0.007	-0.005*	0.001	0.009**	-0.002
	(0.002)	(0.003)	(0.002)	(0.006)	(0.009)	(0.003)	(0.003)	(0.004)	(0.009)
x 1994–96	-0.004*	-0.009**	0.000	-0.012	-0.003	-0.003	0.005	0.005	0.012
	(0.002)	(0.004)	(0.002)	(0.007)	(0.011)	(0.003)	(0.003)	(0.005)	(0.010)
x 1995–97	-0.008***	-0.012***	-0.004**	-0.008	0.001	-0.006*	0.001	0.008*	0.004
	(0.003)	(0.004)	(0.002)	(0.008)	(0.012)	(0.003)	(0.004)	(0.005)	(0.011)
x 1996–98	-0.011***	0.001	0.000	-0.004	-0.001	0.003	0.001	0.010*	0.001
	(0.003)	(0.005)	(0.002)	(0.008)	(0.012)	(0.003)	(0.004)	(0.005)	(0.012)
x 1997–99	-0.024***	0.004	-0.004*	-0.011	0.006	0.001	-0.004	0.002	-0.012
	(0.003)	(0.005)	(0.002)	(0.008)	(0.013)	(0.004)	(0.004)	(0.006)	(0.013)
No. of observations	676,627	511,482	1,117,742	68,488	165,316	459,809	319,562	151,343	24,920
No. of individuals	165,490	128,052	276,038	17,594	56,242	130,954	87,694	42,452	8,509
Year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Demographic controls	no	no	no	no	no	no	no	no	no
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: This table shows the results of several triple differences specifications with the two-year change in log daily wages as the outcome (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 each column, I present the results of the DiDiD specification separately estimated for men, women, workers with German nationality, workers with non-German nationality, workers who were in either age group 18–25, 26–35, 36–45, 46–55 or 56–65, measured at t. 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: Triple differences: Wage Spillover Effects by Place of Work

	West Germany	**East Germany**	**Low Presence**	**Middle Presence**	**High Presence**
Treated x Option					
x 1993–95	0.006*	0.006	0.019***	-0.004	-0.022***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
x 1994–96	0.031***	0.015***	0.042***	-0.003	-0.026***
	(0.005)	(0.005)	(0.007)	(0.006)	(0.004)
x 1995–97	0.036***	0.034***	0.045***	-0.005	-0.032***
	(0.005)	(0.006)	(0.008)	(0.006)	(0.005)
x 1996–98	0.052***	0.077***	0.064***	0.008	-0.014**
	(0.006)	(0.006)	(0.009)	(0.007)	(0.005)
x 1997–99	0.076***	0.096***	0.083***	0.019**	0.007
	(0.007)	(0.006)	(0.010)	(0.008)	(0.006)
Partial x Option					
x 1993–95	-0.010***	0.020***	-0.005**	-0.002	-0.007***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
x 1994–96	-0.004*	0.022***	0.009***	0.001	-0.016***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
x 1995–97	-0.008***	0.034***	-0.002	0.003	-0.021***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
x 1996–98	-0.003	0.053***	0.007**	0.001	-0.013***
	(0.002)	(0.005)	(0.003)	(0.003)	(0.003)
x 1997–99	-0.001	0.042***	0.007**	-0.002	-0.018***
	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)
No. of observations	886,583	298,025	341,029	367,170	451,225
No. of individuals	217,692	75,897	85,374	92,523	115,025
Year fixed effects	yes	yes	yes	yes	yes
Demographic controls	no	no	no	no	no
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 several triple differences specifications with the two-year change in log daily wages as the outcome (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 each column, I present the results of the DiDiD specification separately estimated for individuals working in an establishment in West Germany, East Germany, a labor market region with low presence of the main construction sector, middle presence of the main construction sector or high presence of the main construction sector in the pre-introduction period. I measure the establishment region characteristics for a worker in t. The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table A11: Triple differences: Wage Spillover Effects by Establishment Characteristics

	Very small (1–4)	Small (5–19)	Medium (20–249)	Large (250–999)	Very large (1000+)	Low AKM	Middle AKM	High AKM
Treated x Option								
x 1993–95	0.014	-0.016***	-0.017***	0.023**	0.050***			
	(0.010)	(0.006)	(0.005)	(0.011)	(0.007)			
x 1994–96	0.029**	-0.004	-0.012**	0.033**	0.023	0.020	0.004	-0.003
	(0.012)	(0.007)	(0.006)	(0.016)	(0.014)	(0.013)	(0.005)	(0.006)
x 1995–97	0.047***	-0.009	-0.012*	0.001	-0.082**	0.016	0.000	-0.003
	(0.013)	(0.008)	(0.006)	(0.021)	(0.035)	(0.015)	(0.007)	(0.008)
x 1996–98	0.065***	-0.011	-0.012*	-0.014	-0.024	0.005	0.002	0.016*
	(0.014)	(0.008)	(0.007)	(0.024)	(0.063)	(0.016)	(0.008)	(0.009)
x 1997–99	0.081***	0.000	-0.019**	-0.040	-0.021	0.008	0.011	0.039***
	(0.014)	(0.009)	(0.008)	(0.028)	(0.101)	(0.017)	(0.008)	(0.011)
Partial x Option								
x 1993–95	0.011	-0.004	0.006**	0.006*	-0.031***			
	(0.010)	(0.005)	(0.003)	(0.003)	(0.003)			
x 1994–96	0.022**	0.008	0.013***	0.011***	-0.044***	0.023*	0.005	0.001
	(0.011)	(0.006)	(0.003)	(0.004)	(0.004)	(0.014)	(0.005)	(0.002)
x 1995–97	0.023*	0.005	0.012**	0.012***	-0.076***	0.007	0.001	-0.001
	(0.012)	(0.006)	(0.004)	(0.004)	(0.005)	(0.016)	(0.005)	(0.002)
x 1996–98	0.038***	0.002	0.005	0.015***	-0.074***	0.005	-0.004	0.002
	(0.012)	(0.006)	(0.004)	(0.005)	(0.006)	(0.017)	(0.006)	(0.002)
x 1997–99	0.043***	-0.003	-0.008**	0.010**	-0.069***	-0.011	-0.001	-0.003
	(0.012)	(0.006)	(0.004)	(0.005)	(0.007)	(0.018)	(0.006)	(0.002)
No. of observations	134,430	241,364	350,697	207,676	177,546	90,149	191,041	641,673
No. of individuals	39,823	70,572	96,141	55,112	44,845	26,529	56,823	172,328
Year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Demographic controls	no	no	no	no	no	no	no	no
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes	yes

Notes: This table shows the results of several triple differences specifications with the two-year change in log daily wages as the outcome (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 each column, I present the results of the DiDiD specification separately estimated for individuals working in a very small establishment (1–4 employees), small establishment (5–19 employees), medium establishment (20–250 employees), large establishment (250–999 employees) or very large establishment (1000+ employees) measured in t. Furthermore, I use the position of the establishment in the distribution of the establishment-specific wage premia provided by Bellmann et al. (2020) using the estimation strategy in Abowd et al. (1999) and Card et al. (2013). I split this distribution into terciles and estimate the DiDiD specification separately for workers in an establishment in t with low establishment fixed effects (low AKM), middle establishment fixed effects (middle AKM) and high establishment fixed effects (high AKM). The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.