

Boockmann, Bernhard; Krumm, Raimund; Neumann, Michael; Rattenhuber, Pia

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Turning the Switch: An Evaluation of the Minimum Wage in the German Electrical Trade Using Repeated Natural Experiments

Bernhard Boockmann

Raimund Krumm

Michael Neumann

Pia Rattenhuber

Institut für Angewandte Wirtschaftsforschung e.V.
Ob dem Himmelreich 1 | 72074 Tübingen | Germany
Tel.: +49 7071 98960 | Fax: +49 7071 989699

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Turning the Switch: An Evaluation of the Minimum Wage in the German Electrical Trade Using Repeated Natural Experiments

Bernhard Boockmann^{* ¶ #}
Raimund Krumm^{*}
Michael Neumann[§]
Pia Rattenhuber[§]

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Abstract: The introduction, abolition and subsequent re-introduction of the minimum wage in the German electrical trade gave rise to series of natural experiments, which are used to study minimum wage effects. We find similar impacts in all three cases on wages, employment and the receipt of public welfare benefits. Average wages are raised by the minimum wage in East Germany, but there is almost no evidence for employment effects. The results also show that the wage effect is quickly undone after the abolition of the minimum wage.

JEL-Codes: J38, J31

Key Words: Minimum wage, labor market regulation, employment

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* Institute for Applied Economic Research (Institut für Angewandte Wirtschaftsforschung, IAW), Tübingen ¶ University of Tübingen # IZA, Bonn § German Institute for Economic Research (Deutsches Institut für Wirtschaftsforschung, DIW), Berlin

1. Introduction

The natural experiment approach has been used in many empirical studies estimating the effects of minimum wages on labor market outcomes.¹ Variation in minimum wage provisions originates from differences over geographic areas, industries, or groups of workers. In this paper, we use the introduction, expiration and re-introduction of an industry-specific minimum wage in the German electrical trade industry to identify minimum wage impacts.

Using successive natural experiments has at least two different advantages. First, repeated natural experiments may be used to validate the estimated treatment effects. Validation requires that all circumstances influencing minimum wage impacts remain the same. A large literature argues that the effects of labor market institutions such as minimum wages vary with changes in economic circumstances (Blanchard and Wolfers 2000, Burkhauser et al. 2000) or the regulatory ecosystem (Amable and Gatti 2006, Bassanini and Duval 2006, Belot and van Ours 2004, Boockmann 2010). In this context, repeated evaluations may be used to check whether differences in treatment effects exist and, if they do, whether these differences are in line with theoretical predictions.

Second, by observing the introduction and abolition of minimum wages, one is also able to provide evidence on whether the effects of these events are symmetric. Many authors suggest the presence of long-lasting effects of minimum wages that are not reversed once the minimum wage is abolished. According to the insider-outsider argument, a minimum wage may cause the workers with the lowest skills to exit permanently from the labor market. If the minimum wage is abolished, the needs of these workers are no longer taken into account in wage negotiations (Hamermesh, 1994). Experimental results by Falk et al. (2006) suggest that the introduction of a minimum wage increases workers' reservation wages because it changes their perception of a fair wage. If the minimum wage is removed, reservation wages do not decrease to the level before the minimum

¹ For recent overviews, see Neumark and Wascher (2007, 2008).

wage introduction. Thus, fairness concerns lead to a kind of ratchet effect. Furthermore, there is empirical evidence for substantial wage rigidity (for Germany, see e.g. Franz and Pfeiffer, 2006). Mechanisms such as these suggest that the effects of the introduction of minimum wages on the wage distribution and other outcomes will not simply be undone if the minimum wage is no longer applied.

The abolition of minimum wages has been a rare event in history and there is very little literature on the effects of a removal of the lower wage floor, none of it from Germany. The end of the Wages Councils in the UK in 1993, when the last 26 minimum wages in place were annulled by the “Trade Union Reform and Employment Rights Act”, is an exception. Evidence of the effects of the abolition is collected in the report of the Low Pay Commission (1998, p. 225ff.). The general consensus voiced by the Commission was that the abolition showed that minimum wages had “no significant damaging impact on the labor market” (Low Pay Commission 1998, p. 96), as no positive employment effects could be observed in consequence of their removal. Dolado et al. (1996) consider the wage distribution half a year after the abolition and find that the spike around the former minimum wage had already decreased. New contracts had been partly signed at rates lower than the minimum wage lastly in place. In the short run they find no negative employment effects. Neumark and Wascher (2007, p. 114) oppose this view; they point out that compared to other industries without minimum wages (and their subsequent abolition) the last sectors of the Wages Councils saw a relatively greater increase in hiring rates than in exit rates after the abolition. They interpret this change in employment dynamics as supportive of the beneficial employment effects of the abolition.

The minimum wage in the electrical trade in Germany has been introduced, similar as in other branches of the construction sector, in 1997. It is based on a framework agreement of the social partners, which is made generally binding (“erga omnes”) by legal decree. It covers all production workers in the electrical trade with the exception of apprentices and some other groups

of employees. The minimum wage sets a uniform wage floor, which is lower in East Germany (i.e., the federal states joining the Federal Republic of Germany after re-unification in 1990 including Berlin) than in West Germany. In 2003, the employers declined the union's invitation to negotiate a new minimum wage, and there was no minimum wage until a new agreement was signed in 2007.

The electrical trade is the only industry with repeated changes in the existence of a minimum wage in Germany. In other industries, such as the roofing industry and the painters and varnishers trade, there have been short interruptions in minimum wage coverage of several weeks, but it is unlikely that wages and employments adjusted to these interruptions, in particular since the re-introduction may have been anticipated. Indeed, identification of the treatment effect using the difference-in-difference-estimator requires that the effect of a previous minimum wage is completely undone, a fact that will be checked in the course of the present paper.

There is a small number of previous studies on the effects of minimum wages in Germany, using a similar methodology as in this paper but focusing on different industries and issues. König and Möller (2008) look at the construction industry and use the introduction of the minimum wage in 1997 as a natural experiment. They find significantly positive wage effects and significantly negative employment effects for East Germany, while the effect on employment in West Germany is positive, although only marginally significant. The study has been challenged due to the choice of individuals earning slightly higher wages than the minimum wage before the minimum wage introduction (Bachmann et al. 2008). Indeed, if there are spillover effects between different parts of the wage distribution, the use of a different group of wage earners from the same industry violates the "stable unit treatment assumption" (SUTVA) required for identification. Rattenhuber (2011) examines the effects of the minimum wage in the main construction industry on the whole wage distribution using a difference-in-differences-in-differences approach for conditional quantile effects. She does find strong evidence for effects of the minimum wage on workers earning higher wages than the minimum and, thus, not directly affected by them. In West Germany, these effects

even dominate the direct effects. More recently, a number of studies have emerged from the evaluation of the minimum wages commissioned by the German government. Where sufficient data was available, they used the difference-in-differences methodology. In general, these studies found positive wage effects for East Germany but little evidence was obtained for employment effects.² An exception to this is the study by Aretz et al. (2012) for the roofing industry. Bachmann et al. (2012) focus on labor market flows rather than employment levels; their findings suggest that the job-to-job transition rate was lowered as an effect of the introduction of minimum wages in the East German construction industry.

The remainder of this paper is structured as follows. In the next section, we describe the minimum wage in the electrical trade and provide some information about the structure and development of the industry. In section 3, we introduce our data and provide some descriptive analysis of the wage distribution and the strength of the minimum wage regulations. The estimation approach, including the definition of outcome variables and control groups, is contained in section 4. Our preferred estimates are presented in section 5 while section 6 reports the results of further robustness analysis. Section 7 concludes.

2. Minimum wages in the electrical trade

The minimum wage in the electrical trade belongs to the first generation of industry-specific minimum wages introduced in Germany in 1997.³ The legal scope of the minimum wage extends to

2 The results are published in a special issue of the *Journal for Labour Market Research*, Vol. 45, No. 3-4 (December).

3 Legally, minimum wages are implemented as a collective agreement which is then declared generally binding according to the procedures of the Posted Workers Act of 1996. The primary goal of the Act was to limit the presence of posted workers from other EU countries earning substantially lower wages than German construction workers. However, it was clear from the outset that the Act also had an impact on domestic workers. For details and further references, see König and Möller (2008).

all establishments or autonomous parts of establishments in the electrical trade, i.e. establishments engaged in the installation of electrical appliances and equipment, including e.g. wiring and the installation of electrical communication networks. All production employees with the exception of apprentices and short-term helpers, such as students working during school vacancies and interns, are covered by the minimum wage, provided that they work outside of the premises of the establishment (which usually means work on construction sites). Temporary agency workers hired to establishments in the electrical trade must also receive at least the minimum wage.

Figure 1 shows the development of the minimum wage over time. When first coming into force on 1 April 1997, the minimum wage amounted to an equivalent of 6.41 € for workers in East Germany and 8.03 € in the West. The difference in minimum wage rates within Germany gives rise to subtle problems of implementation; for instance, East German workers on West German construction sites must be paid the West German minimum wage. After 1997, the minimum wage was increased in several steps up to 7.40 € (East) and 8.90 € (West). After the existing agreement had expired on 30 April 2003, the employer side in the (transsectoral) wage committee at the German Labor Ministry refused to agree to a further “*erga omnes*” extension in May 2003, although both sides of the industry were in favor of the minimum wage.⁴ After the re-introduction in 2007, the minimum wage started at a higher level as compared to 2003 (7.70 € in the East and 9.20 € in the West). The reintroduction had been agreed upon earlier in 2007 but could be implemented only

4 The reason given was that the minimum wage was higher than the lowest wages specified by several regional collective agreements. For instance, the minimum wage in East Germany was 7.40 € in 2003, while the lowest pay groups in Saxony-Anhalt were remunerated at 5.92 €, 6.77 € and 7.19 €, respectively. This differential had existed in several federal states since 1997, but the issue had not been raised before 2003 (for the following, see Bosch et al. 2011, p. 162ff). In principle, unions and employers could have quickly amended the minimum wage agreement, but due to disputes over issues unrelated to the minimum wage (such as working time and paid holidays) there was a stalemate in collective bargaining between one of the industry unions (IG Metall) and the industry employer association (ZVEH) after 2003. Only in 2007 were both sides able to agree on an additional clause stipulating that existing collectively agreed wages below the minimum wage would automatically be lifted to the level of the minimum wage, which formally satisfied the requirements of the wage committee.

after concerns by the employers in the wage committee had been taken into account. By 2011, the minimum wage had risen to 8.40 € / 9.70 €.

Apart from the minimum wage, there also exist collective agreements on wages and other subjects. According to a recent survey, 42 % (48 %) of all establishments in the electrical trade were legally bound to collective agreements in West (East) Germany (IAW 2011). The actual coverage rate of collective agreements is higher, because many establishments apply collective norms voluntarily. With the exception of the federal states of Baden-Württemberg, Hesse and Rhineland-Palatinate, the workers' side in collective bargaining is represented by the Christian Metalworkers Federation (CGM) and not the IG Metall, the industry union belonging to the German Trade Union Confederation (DGB). Thus, there is competition in worker representation in the electrical trade. Overall, the evolution of the wages in the collective agreements follows a similar path as the minimum wage.

Sales in the electrical trade have evolved rather discontinuously in the period from 1996, the year before the initial introduction of the minimum wage. During the period from 1997 to 2000, sales were increasing before plummeting in 2002 and 2003 to the level of 1997 (see Figure 2). Since then, sales have been increasing, with an interruption only in the crisis year of 2009. The development of sales in the electrical trade was overall slightly more favourable than in construction as a whole, but the drop in sales 2002 to 2003 was deeper than in the whole economy.

The electrical trade is characterized by a high proportion of small businesses. In 2008, three quarters of all companies (74.9 %) had less than 5 employees, and 33.2% of all businesses had no employees at all (not taking into account unpaid family members). Only 4.5% of all firms had 20 employees or more. Overall, there are only minor differences in establishment size between East and West Germany.

In 2008, approximately 233,000 employees were working in the electrical trade, 176,000 in West German and 57,000 in East German establishments (see Figure 3). After a strong increase in

the second half of the 1990s, employment numbers decreased by 2 % in West Germany from 1999 to 2008. In East German establishments, however, substantial employment losses of about 32 % (26,000 in absolute numbers) were incurred. Workers are overwhelmingly male and mostly employed on a full-time basis. In West (East) Germany, 18 % (11 %) of employees in the electrical trades were working part-time in 2008. This number includes non-production workers, where part-time work is more prevalent. Finally, there has been an increase in the share of non-skilled workers and a declining share of skilled workers, masters and foremen between 1999 and 2008. At the same time, the number of casual employees earning less than 400 € has increased. The share of temporary workers is also high; according to a survey, 52 % (41 %) of all establishments in the West (East) German electrical trade with at least one employee used temporary agency work at some time during the year 2010 (IAW 2011). The share of temporary employment in the total volume of work was in the range of 10 to 11%. As noted earlier, temporary agency workers hired by establishments in the electrical trade also receive the minimum wage.

In the same survey, 68 % of establishments have experienced an increase in competition for skilled workers during the five years prior to the survey (75 % in the East, 66% in the West). Regarding the recruitment of unskilled labor, by contrast, only 14 % of firms in the West and 18 % of East German establishments report that competition for workers among firms has increased.

Overall there is a clear sense that developments in East and West Germany diverged in the electrical trade during the late 1990s and the 2000s. Employment in East German establishments declined substantially, while little changes were observed in West Germany. Moreover, almost a fifth (19 %) of the revenues of East German firms originated from activities in West Germany, while West German firms offer their services almost exclusively in the West (IAW 2011).

3. Data and descriptive analysis

A sample of 4,000 establishments in the electrical trade and in the control industries was drawn from the Establishment History Panel (Betriebs-Historik-Panel) of the Institute for Employment Research (IAB) specifically for this project. The data originate from the employment statistics of the Federal Employment Agency, which in turn are derived from employers' reports to the pension system and contain all establishments with at least one employee. The data were drawn for a base year (2008) for the industry electrical installation.⁵ To account for firms entering and leaving the market, repeated refreshments of the sample were drawn. The sample disproportionately over-samples large establishments (IAW 2011, p. 18ff.).

The data of the establishment sample were merged to the social security records of all workers employed by sample establishments (Integrated Employment Biographies, IEB). This means that we have information about working biographies, income and characteristics of all employees of a sample establishment for the period 1998 to 2009.⁶ Employees contain all employees subject to paying social security contributions, including marginal employment but excluding freelancers and temporary agency workers (the assignment of the latter to a particular sector cannot be observed in the data).

A major difficulty is a lack of information on working hours in the employment data. To complement the IEB data, we use the Microcensus, an annual survey carried out among one percent of all households resident in Germany by the Federal Statistical Office. All households are legally required to answer the questionnaire. In the Microcensus, working time is measured as normal

5 Utmost care was taken to align the definition of the minimum wage with the industry definition according to the national accounts. In particular, using another database with information on adherence to collective agreements, the Salaries Structure Survey of the Federal Statistical Office, several neighboring industries were checked whether their collective agreements are the same as in electrical installation. It turned out that the industry definition in the national accounts well approximates the scope of the minimum wage in the electrical trade.

6 The IEB is a standard data source for evaluation projects in Germany. More information about the data can be obtained from Oberschachtsiek et al. (2009).

weekly working time and actual working hours in the week preceding the survey. We use the average usual working hours within a cell, defined by occupation, year and qualification, to calculate hourly wages of full-time employed workers based on our measure of daily wages obtained from the IEB. The cell averages are then merged to the IEB data. There is relatively little variation in usual working hours among full-time employees, with cell averages ranging from 39.2 for low-qualified electricians in 2005 to 40.6 for employees in other occupations in 2008, the most recent year available. In this study, we use wage information only for individuals in full-time employment. Hence, variations in usual working hours are mainly due to overtime.⁷ In addition, we exclude all workers not covered by the minimum wage, such as apprentices and interns. Moreover, we exclude highly skilled workers (workers with a master craftsman's degree and foremen). In addition, we drop observations with wages less than 3 € and more than 50 €, because these are likely to be due to measurement error. The evolution of average wages of the just defined group of employees is shown in figures A3 and A4 in the Appendix.

In the remainder of this section, we use these data and definitions for a descriptive analysis of wage distributions and indicators for the minimum wage bite. Table 1 contains the Kaitz index and the percentage of individuals directly affected by the minimum wage. In the latter case, we follow Lemos (2004) and define worker “at the minimum wage” as workers earning between 0.98

7 Usual working hours include regular overtime but not irregular overtime. The difference of regular hours to hours worked last week may be used as an indication of the amount of irregular overtime. Correcting for absences from the workplace due to sickness, maternity, vacations and strikes, hours last week exceed regular hours by 0.5 to 1.2 hours per week (depending on the year). According to this calculation, we overestimate hourly wages by 1.2 % to 3.0 %. Given that the amount of irregular overtime is relatively small, any bias resulting from these measurement problems will not influence our estimation results substantially. To check the validity of the wage data, we compare the information with another data source, the Earnings Structure Survey conducted in 1995, 2001 and 2006 in enterprises with 10 or more employees. The shape of the wage distribution looks similar for workers in the electrical trade (see IAW 2011, p. 123ff.). However, the populations of the two data sets are slightly different (e.g., companies with less than 10 employees are not included in the Earnings Structure Survey) so the results are not perfectly comparable.

and 1.02 of the minimum wage. Workers affected are all workers with a wage less or equal to 1.02 of the minimum wage. For the upper half of the table, hourly wages are measured in the years of the introduction (1997) or re-introduction (2007) of the minimum wage. The lower half shows the measures during 2001, i.e. in a year in which the minimum wage was operational.

The size of the Kaitz index, which we define here as the relation of the minimum wage to the median wage, is between 0.74 and 0.79 in East Germany and between 0.62 and 0.64 in West Germany. The index is increasing by 5 percentage points in the East and 2 percentage points in the West between 1997 and 2007, suggesting that minimum wages increased slightly faster than median wages. There is only a slight change in the Kaitz index between 1997 and 2001 (i.e. before and after the minimum wage introduction). This reflects the fact that, while the median wage has increased following the minimum wage introduction in 1997, the minimum wage has also been gradually increased (see figure 1), keeping the Kaitz index roughly at the same level.⁸

The stronger bite of the minimum wage in East Germany is also evident from the percentage of workers directly affected by the minimum wage. In East Germany, slightly less than 6 % of workers in 1997 and less than 7 % in 2007 had wages at or below the new minimum wage. In West Germany, we see a relatively strong increase by 2 to 3 percentage points from 1997 to 2007, but starting from a very low level. According to our data, about 2 % of workers were remunerated at a lower level than the minimum wage in 2001. This reflects noncompliance with the minimum wage, of which there is some qualitative evidence (IAW 2011), and possibly measurement error regarding hourly wages.

The share of workers directly affected is also visible from the kernel density estimates of the wage distributions depicted in figure 4. For West Germany (right column), there are small rightward

⁸ One must be careful comparing the ex ante and contemporary Kaitz indexes. While the ex ante Kaitz index is unaffected by the minimum wage introduction, the contemporary measure incorporates the minimum wage not only through the numerator but potentially also through the denominator if the minimum wage affects the median wage.

shifts in the wage distribution in 1997 and 2007 but no changes in the shape of the wage distribution. In East Germany, the left part of the wage distribution steepened noticeably between 1995 and 1999. In the other years, the changes in the wage distribution appear to be minor. In 2007, the shift in the wage distributions was larger in East Germany than in West Germany.

All in all, the strength of the minimum wage appears to be moderate, in particular in West Germany, but tightening slightly between 1997 and 2007.⁹ We interpret the latter finding as support for our use of the re-introduction of the minimum wage as a natural experiment to estimate the treatment effect: compared to the average wage, the bite of the minimum wage in 2007 was not lower than at the original introduction in 1997.

4. Empirical model

To estimate the effects of minimum wages on wages and employment, we use a conditional difference-in-differences methodology, based on the “natural experiments” of the introduction, abolishment and re-introduction of the minimum wage in 1997, 2003 and 2007, respectively.¹⁰ The control groups are workers from neighboring industries and/or professions, as explained below.

The introduction of the minimum wage in 1997 leads to the usual estimator for the average treatment effect on the treated (ATT). Identification rests crucially on the assumptions of (1) common trends between the treatment and control group and (2) the assumption of that the minimum wage had no pre-treatment effect, such as anticipation effects. To avoid the influence of

9 All the results reported here were replicated with the Earnings Structure Survey. Although the levels of the Kaitz index and the other measures of the minimum wage bites are somewhat higher (reflecting the different population which also includes non-production workers), the changes of these measure over time and the East-West differences are very similar to the results obtained on the basis of the IEB and the Microcensus (IAW 2011).

10 We have tried other approaches, in particular using the gap measure of Card and Krueger (1994) as a continuous treatment variable. The fact that the gap measure was uniformly negative for employment in all estimations (introduction, abolition or placebo treatment) points to the potential endogeneity of this measure. This is why these results are not presented here.

the latter, we use 1 June 1996 as the pre-treatment observation date closest to the experiment for all outcomes¹¹, i.e. a date exactly one year before the treatment occurred. At this time, there was uncertainty about whether and when social partners would close an agreement to implement the minimum wage provisions.

We define the treatment occurring due to the change in 2003 as abolition of the minimum wage, and we estimate the ATT of the minimum wage abolition.¹² The DiD effect of the third natural experiment, the re-introduction of the minimum wage in 2007, can be interpreted as the ATT of the minimum wage provided that the effect of the previous minimum wage had evaporated by 2007. Otherwise, the minimum wage would impact on the pre-treatment outcome. On the basis of the estimated treatment effects of the minimum wage abolishment reported below, as well as on the basis of our descriptive evidence on the Kaitz index and percent of workers affected, we argue that this is indeed the case, so that the effects estimated for 1997 and 2007 can be compared directly.

The usual way to define a control group in difference-in-differences estimation is to use a group of individuals who have not been exposed to treatment (Lechner, 2011). Following this approach, the control group in the 1997 and 2007 changes consists of individuals not covered by a minimum wage, while the control group of the 2003 change is a group of individuals who have been exposed to a minimum wage before and after 2003.

We use two different ways to obtain a control group from outside the electrical trade. The first is to use observations from similar industries. These industries are chosen from the wider building industry, due to the fact that the building industry has followed a distinct demand pattern

11 For the probability to stay employed we use time periods as observations, see below.

12 In principle, one could interpret the estimated effects as an average treatment effect on the non-treated (ATNT) of the minimum wage (Lechner, 2011). However, one cannot be sure that continuing with the minimum wage is a valid counterfactual for those industries that had abolished the minimum wage. This would require that the minimum wage did not influence the outcome after treatment.

during this time. The similarity in the development of demand is due to the fact that the products from these industries are complementary, although some parts of construction are related to new buildings and others to the refurbishment of existing ones.

Our principle was to include all industries within the building industry. To apply the difference-in-differences method, we need to distinguish between similar industries with and without a minimum wage. The industries used for the control groups are listed in table A1. For some industries it was not clear whether a minimum wage is applied and, therefore, they were not included.

An analysis of common trends, as explained below, was performed for the individual industries to uncover industries with wage and employment evolutions vastly different from the electrical trade. In the end, we did not exclude industries on the ground of uncommon trends. We use information on all production workers from these industries, using the same exclusion rules as for the treatment group (see section 3).

The second approach is to use observations on persons in the same occupation, but working for a company with a different sector affiliation. This could, for instance, be an electrician employed by a company in the main construction industry. We still require that these workers work in the wider construction sector (45.2 in the WZ 2003). Because the number of electricians working in industries without a minimum wage is very small, we use only information from industries covered by a minimum wage. Hence, the alternative control group is available only for the 2003 natural experiment.

The advantage of the second approach is that the common trends assumption is more likely to be fulfilled if the time pattern of the outcome variables is mainly influenced by the supply and demand for certain qualifications. The disadvantage is that substitution effects due to the minimum wage cannot be ruled out. Indeed, firms in other industries covered by the minimum wage before may have gained market share after the introduction of the minimum wage in the electrical trade.

The appropriateness of the control groups with respect to the common trends assumption can be judged by comparing the composition of the treatment and control groups with respect to characteristics that could give rise to unequal trends (apart from the obvious fact that the industries produce different services). In the following, we give an overview of the results available in more detail from the project report (IAW 2011a). The composition of the treatment group and the control groups with and without a minimum wage was checked for 79 personal, establishment and regional characteristics and for the most recent year available. Two-sample t-tests show statistically significant differences in most cases (38 variables for the control group with minimum wages and 67 cases for the control group without a minimum wage). However, the magnitude of the difference is small and statistical significance is easily attained due to the large sample size (see Table A2 in the Appendix).

A second check consists in comparing the evolution of the outcome variables during time periods unaffected by the treatment. Due to a break in industry classification, consistent measures of the probability of remaining employed are available from 1995 to 2001 and from 2001 to 2008 (see Figures A1 and A2 in the appendix). Figure A1 shows the evolution of the employment probabilities (as defined below) in the treatment group and the control group of industries without a minimum wage. The pre-treatment development (1995 to 1996, shaded area of the graph) is slightly negative in both groups. In Figure A2, common trends in the years 2001 and 2002 can be checked to assess the validity of the natural experiment of 2003, and common trends between 2004 and 2006 in case of 2007. In East Germany, the time pattern in the treatment group and the control groups are similar, while there is more heterogeneity in West Germany, where a drop in the employment probability occurred in the treatment group but not in the control group in 2005.

Figures A3 and A4 in the appendix allow for a similar analysis regarding the development of wages. They show that wages increased more rapidly in the electrical trade than in the control group of industries without a minimum wage, but almost all of this difference originated in the period

from 1997 and 2003 and could, therefore, be a result of the minimum wage. From 2003 to 2007, the development was quite similar. The wage trends in the electrical trade and in the comparison industries with a minimum wage are very similar from 2000 to 2003, which are the most relevant years for this comparison group.

In addition to these visual checks, statistical tests are performed to check the validity of common trends using so-called placebo experiments, as explained in section 5 below.

In the econometric analysis, we use a number of different outcome variables. Regarding wages, we use hourly wages as obtained from our administrative data source. Concerning employment, we focus on the probability of remaining in employment (either with the same employer or with any employer). In addition, we use establishment-level outcomes (hiring and separation rates, as well as the number of employees measured at a particular date) and the probability of receiving public welfare benefits in addition to labor earnings.

The different employment outcomes capture different subsets of employees that could be affected by the minimum wage. The employment probability concerns only the effect of job separations and not accessions, and only the effect on separations of workers employed at the pre-treatment measurement date. The hirings rate reflects whether minimum wages stopped establishments from hiring new workers. The separation rate takes into account all quits and dismissals within a given period, regardless of the job duration. Hence, it is not confined to workers employed at the time of treatment.

The stock of employees indicates the net changes in employment at the company, while the other measures relate to gross changes. This measure is in spirit of the Card and Krueger (1994) studies, where stocks of employees in fast food stores were used as a dependent variable. All measures relate to existing establishments, i.e. they are not a measure of overall employment in the

industry.¹³ Since we do not have individual working hours in the data, we do not estimate the effect on hours of work or the total volume of employment.

A final outcome is a dummy indicating whether the individual is receiving additional public welfare benefits to top up earnings in order to reach a social minimum income. These benefits, called Unemployment Benefit II in Germany, were introduced in 2005 and the number of individuals working and receiving benefits at the same time has steadily increased since then. The use of this outcome variable measure is motivated by the fact that moving workers out of public welfare dependency and providing them a self-sufficient income was one of the main political reasons for the introduction of minimum wages in Germany.

For the continuously measured outcomes, we use a linear regression for average wages and the stock of employees and a Tobit model for hirings and separation rates. Wages are measured as average hourly wages during the time interval around 1 June of the years before and after the treatment years. For instance, we use average wages measured at 1 June 1996 and at 1 June 1998 for the analysis of the minimum wage introduction in 1997. Stocks of employees are measured at the same dates but just one year before and after an experiment. Hirings and separation rates are defined as the number of hirings and separations in 1996, 1998, 2002, 2004, 2006 and 2008 over the stocks measured at the dates just mentioned.

For the re-employment probability, we specify a probit model. Two different variants are specified: a narrow definition defined according to whether the worker continued in his or her previous establishment and a wide definition where employment counts as employment anywhere in the economy. We compare the probability of remaining in employment between two periods: a period before the minimum wage was introduced or abolished, and another period after the change

13 To know these, effects on the number of establishments would need to be estimated. Unfortunately, exact numbers on the stock of establishments are only available for the years 2002 to 2005. They show a modest decline, while the numbers have increased slightly in the control industries.

occurred. For instance, we look at the stock of employees on 30 January 2001 and estimate their employment probability on 30 January 2003, i.e. three months before the minimum wage expired. This probability is then compared to the corresponding probability for the stock of workers as of 30 January 2003, measured on 30 January 2005. Thus, the time periods are chosen long enough to allow for substantial lags in employment responses to the minimum wage.

The independent variables consist of individual characteristics such as gender, education, age group dummies, job tenure, nationality and (in case of employment as a dependent variable) part-time or full-time employment status. Regional characteristics include dummies indicating rural, urban and metropolitan areas, the unemployment rate measured at county level and dummies for federal states. Establishment-level variables include the number of employees (in groups) and the employment structure (share of white-collar and blue-collar workers, qualification structure, share of part-time and marginal workers, share of women and age structure). Means of these variables are contained in Table A2 in the Appendix.

5. Estimation results

In this section, we report difference-in-differences results for wages, employment, establishment-level outcomes and the receipt of public welfare benefits. In each of the tables, we display the treatment effect and the main effects for the treatment group and the post-treatment period.

Regression output for the control variables is not displayed.

Effects on wages

Table 2 contains results for log hourly wages of production workers in the electrical trade. The comparison is made between the year before and after the change (for instance, 1996 versus 1998) due to the fact that wage information is not available for periods shorter than one year for many individuals in the data. The treatment effect is the interaction of the two main effects. We display

specifications for one control group for the introduction of the minimum wage in 1997 and 2007 and two control groups for the suspension in 2003. Results of further robustness analysis are discussed below.

In East Germany, we find evidence that the introduction of a minimum wages in 1997 increased the average wage in the electrical trade by slightly more than 1 %; however, the effect is only marginally significant. The suspension of minimum wages in 2003 reduced average wages substantially (by about 4 %) according to the results for both control groups. From the 2007 natural experiment, the minimum wage effect on average wages is estimated as 3 %. For West Germany, most results point in a similar direction but the magnitudes of the effects are substantially lower. Only the effect of the minimum wage abolition, when estimated with a control group from other occupations, is statistically significant. These findings are in line with the descriptive evidence, which points to a greater strength of the minimum wage in East as compared to West Germany.

Placebo effects are estimated by taking two observation dates in which none of the observation units have been exposed to the treatment. They indicate treatment effects without a treatment, which would suggest that the common trends assumption must be violated. The following years have been chosen as placebo treatment dates: 1996, 2001 and 2006. As can be seen from the p-values for the placebo treatment effects, placebos are all insignificant. This supports the claim that the estimated treatment effects are valid.

The finding of a negative treatment effect of the minimum wage abolition in 2003 is highly interesting as it indicates that the effects of minimum wages may not be permanent. Given the positive wage development in the control sector (as indicated by the coefficient of the time dummy, a 5.3 % pay rise in this period), the negative treatment effect does not translate into a nominal cut in average wages. This accords well with the findings of Dolado et al. (1996) for the UK which suggest that “employers may have been reluctant to cut nominal wages for their current employees, and relative wage concerns may have limited the willingness of employers to bring in new workers

at wage rates below the minimums previously set by the Wages Councils” (quoted from Neumark and Wascher 2007). In East Germany, wages in the control sector with minimum wages were at a significantly higher level. Otherwise, there is little indication of consistent sectoral differences.

Since the effects of minimum wages may be heterogenous, we repeated the analysis for certain sub-groups of employees (see table 3). First, we checked whether newly (within 10 months before measurement date) hired employees are more strongly affected by the minimum wage (see table 3). The results of this analysis are then compared to a sample restricted to employees in continuous employment before and after the treatment dates. In East Germany, the suspension of the minimum wage in 2003 and its reintroduction in 2007 resulted in more pronounced effects for newly employed workers, which could be due to the fact that these workers were hired on lower rates than the existing workforce. In West Germany, the effects of the original introduction in 1997 were limited to new hires. Otherwise, the sizes of the coefficients are similar between new hires and employees with longer job tenure.

Since unskilled workers (workers without a vocational training degree as an electrician or in a related occupation) are more likely to belong to the group directly affected by the minimum wage, we re-estimated the treatment effects for this group only. The results show that, after the introduction in 1997, wages for low-skilled workers in East Germany were clearly more positively affected as compared to all blue-collar workers. In the other groups, the effects are not more pronounced and even become insignificant for some treatment dates, which could be due to a substantial loss of observations.

In addition to the results displayed here, we conducted a number of robustness checks. In particular, we estimated models with outcomes measured in greater distance from the treatment

period and including more than one pre-treatment and post-treatment period. The results of these multiperiod models yielded similar coefficient magnitudes as the ones presented here.¹⁴

Effects on the probability of remaining in employment

Table 4 shows the results for the outcome variable of remaining in employment within the same company (i.e., one minus the probability of a separation).¹⁵ Results from dynamic labor demand estimation for Germany (e.g. Hunt 2000) suggest that the full effect of a wage rise on employment is realised only after considerable time. Therefore, we use long-term employment responses measured about two years after the treatment (e.g., employment in 1999 for the 1997 treatment).

The treatment effect is calculated using Puhani's (2008) formula for the average treatment of the treated effect (ATT). As the results show, none of the estimated treatment effects is statistically significant at the 5 percent level. The effect of abolishing the minimum wage in 2003 has a borderline significant coefficient for West Germany and the treatment effect suggests that the employment probability was influenced positively by the treatment. However, the placebo test, performed analogously to the placebo tests for wages, indicates a violation of common trends in this case. We also found significant placebo effects for 1997 in East Germany and for 2007 in West Germany; hence, one must abstain from interpreting results for these treatments.

In addition, we also looked at short-run effects on the probability of employment, measured within one year after treatment. The estimated effects are similar to the ones presented in the table. An exception is a significantly negative employment effect in East Germany obtained for 2003.

14 Results of these robustness checks and the ones mentioned further below are available on request from the authors.

15 In the estimations shown in table 4, the probability of remaining in employment is defined as the likelihood of being employed by the same employer a certain period after the sampling date. As an alternative, we also considered employment to be defined as any employment relationship; since we observe the work histories of the workers in our sample, this variable could be easily constructed from the data. For none of the years did we find qualitative differences between the narrow and broad definition of the employment probability.

This could perhaps indicate a short-term response of voluntary mobility out of the electrical trade, if workers were seeking jobs in other industries after the suspension of the minimum wage.

Effects on establishment-level outcomes

Next, we turn to results at the level of the establishment (see Table 5). In the first two columns for each treatment period, we present results for the hiring and separation rates using tobit estimation. We report short-term rates measured within one year after treatment, which takes into account that the effect on hirings (and separation of newly hired workers) may occur with a shorter lag than the termination of employment relationships with longer durations. Entries in the table are the interaction effects of the post-treatment and the second period dummies. From the results, we observe that there is a negative effect of the separation rate from the abolition of the minimum wage in 2003 both in East and West Germany. This could indicate that, employment seems to have been stabilised by the minimum wage. Yet the placebo test again indicates a violation of common trends, similar to the results for individual employment stability in Table 4. There are no significant effects on job separations from other periods.

The introduction of the minimum wage apparently had little effects on hirings. Only in West Germany, a negative effect can be found for the 1997 natural experiment. The post-treatment dummies indicate that, due to the positive business climate for construction in general, hirings increased between 1996 and 1998. After 2007, hirings declined and separations increased in East Germany; this may be an indication of the economic crisis setting in in 2008.

The third column for each year displays results for the number of employees measured at the level of the establishment. Apart from a weakly significant and positive effect of the minimum wage abolition in 2003 estimated for East Germany and countervailing a negative employment trend, there are no statistically significant findings for the level of employment from any other years or for West Germany. The fact (mentioned earlier) that the control group with minimum wages

consists of larger establishments on average than the electrical trade shows in the negative treatment group dummy for 2003.

Taking these results together with the results estimated at the level of the individual, we do not find any significant treatment effects on the level or movement of employment for the natural events in 1997, 2003 and 2007. The evidence that the abolishment of the minimum wage during the recession of 2003 helped companies to retain some of their employment is invalidated by the placebo treatments. The only significant effect remaining concerns the reduction of hirings in West Germany after the initial minimum wage introduction in 1997, a finding which is difficult to explain given the absence of significant wage effects.

Effects for welfare benefit receipt

Results for our final outcome, receipt of public welfare benefits on top of earnings, show no reduction of welfare dependency; the estimated treatment effects are far from statistical significance. There is a negative time effect, contrary to the increase in the aggregate number of workers on benefits (see Bundesagentur für Arbeit, 2011), which indicates the presence of a sector-specific trend in the receipt of additional benefits.

6. Results for a different control group

To compare our results with those obtained for a control group from the same industry, we replicated the approach by Möller and König (2008) and used a treatment group of individuals earning less than the minimum wage before its introduction. In line with their definitions, the control group includes individuals with earnings higher than the minimum wage, but not more than 15 % above the minimum wage before its introduction. As regards the suspension of the minimum wage in 2003, this definition of the two groups cannot be reproduced because this would require

identifying those individuals who would have earned less than minimum wage before 2003 in case the minimum wage had not existed.

In the analysis of wages, the dependent variable is the change in log wages in the year of treatment and a control period. This required three periods of measurement (in the case of the first treatment, 1995, 1996 and 1997). The employment probability is estimated as before. All the specifications, including definition of measurement, choice of covariates and estimation models, are the same as in the approach using a control group from different industries.

Table 7 displays the results both for wages and the probability of remaining in employment. Since the definition of treatment and control groups confines analysis to the lowest part of the wage distribution, the number of observations is much lower than before. This holds in particular in West Germany, where the minimum wage is less binding (see figure 4). The treatment group dummy is always highly significant; in case of wages as outcomes, this is mechanically due to the definition of the control group as workers earning above-minimum wages.

According to the estimations, the minimum wage has increased effective wages in the treatment group by 2.1 percent in East Germany in 1997. The effect, however, is significant only at the ten percent level. In 2007, the treatment effect is insignificant and, with 1.5 percent, small in magnitude. No significant wage effects are found in the West. Since 1995 is the earliest period for which data could be obtained, placebo tests can be performed only for the reintroduction. They do not suggest that common trends are violated.

As regards employment, treatment effects are zero for West Germany. For the East, there is evidence of a small negative effect in 2007, with the parameter almost reaching statistical significance at the 5 percent level. The marginal effect suggests that the probability to remain with one's current employer was reduced by three percentage points due to the reintroduction of the minimum wage. There is no effect at the time of the first introduction of the minimum wage in 1997. Again, the placebo tests do not indicate uncommon trends.

In our interpretation, the smaller size (and insignificance) of the treatment effects on wages in East Germany as compared to our previous results are due to wage spillovers into higher parts of the wage distribution. The fact that there is more (although not particularly strong) evidence of a wage effect in 1997 than in 2007 is consistent with the change in the shape of the distribution in 1997 (see figure 4), which did not occur in 2007. A further look at the wage distribution suggests that in 2003 and 2007, the minimum wage treatments resulted in parallel shifts of the whole wage distribution into opposite directions. This interpretation is given support by Rattenhuber (2011), who finds that the minimum wage in construction has affected the whole wage distribution.

The negative treatment effect on employment could indicate that there has been a small loss in employment due to the minimum wage in the group of workers directly affected. It is possible that this loss is not found in other estimations because these are not restricted to the directly affected group. However, if wages also drive up wages of workers in other parts of the wage distribution, one might expect to find employment losses among these workers as well. Moreover, there is no clear explanation for the fact that there is an employment effect in 2007 but not in 1997.

7. Interpretation and conclusions

This paper investigates the effects of the successive introduction, expiry and re-introduction of minimum wages in the German electrical trade. The descriptive results suggest that all three events can be considered as treatments with sufficient strength to make analysis interesting, at least in the case of East Germany. Looking at measures of the minimum wage strength, the re-introduction in 2007 was as strong a treatment as the original introduction in 1997.

The estimation results for the minimum wage abolition in 2003 show that this event lowered wages in East Germany, while the introduction and re-introduction increased them. Wages appear to respond symmetrically to the introduction and abolition of minimum wages. This suggests that there is not much wage rigidity. Hence, the view that a minimum wage shifts the wage distribution

permanently (e.g., due to fairness perceptions as in Falk et al. 2006) does not seem to be very relevant, at least for this industry. One of the possible reasons why our results diverge from the experimental evidence by Falk et al. (2006) is that wages do not have to decline nominally to adjust to the pre-minimum wage level; as our results show, nominal wages in the treatment group remained roughly constant after the minimum wage introduction, while wages in the control industries increased.

The strength of the minimum wage is weaker in West Germany and, hence, the effects are less pronounced than in East Germany, both with respect to magnitude and statistical significance. The point estimates do show the same pattern as in East Germany but only the positive effect of the introduction and the negative effect of the abolishment appear statistically significant.

Concerning employment, there are little indications for robust treatment effects. The only finding that might indicate employment losses is the reduction in the employment probability by three percentage points in East Germany using the alternative control group. Given the size of the treatment group of workers directly affected in the population, this estimate would mean that a total of about 100 jobs were ended as a consequence of the re-introduction of the minimum wage. However, this finding is not particularly robust since the coefficient is only at the border of statistical significance. Moreover, there is no corresponding finding for the original introduction of the minimum wage in 1997.

The absence of an employment effect of minimum wages in the electrical trade is in line with the perception of businesses in this industry. According to a survey among almost 900 companies in electrical trade which we conducted in 2011 (IAW 2011), only 5 % of establishments in West Germany and 14 % in East Germany believed that the minimum wage in this industry has had negative employment effects.

From an economic point of view, the question remains how the increased production costs due to increasing wages in East Germany were financed if labor demand did not decline. Price

increases for the services of the industry are a possible explanation. Unfortunately, price information is not available at the company level and, hence, econometric analysis is not feasible. In our survey, however, only 10 % of East German establishment responded that competitive pressure was low, which makes it unlikely that much of the additional labor costs can be shifted into higher prices. For the same reason, it is unlikely that economic rents are earned in the electrical trade and rising costs are financed by lower profits, as the monopsony argument would have it. Productivity gains due to increases in technical efficiency are another possible explanation, yet also difficult to ascertain. Another potential reason is the potential gains in market share due to a more than proportional reduction in the amount of posted workers from other EU countries, in particular the New Member States. Last but not least, non-compliance with the minimum wage provisions, such as unofficially working extra hours, may also be a candidate for explaining why we do not find employment effects. However, the effect of non-compliance with minimum wage provision on employment is theoretically ambiguous; Danziger (2010) argues that non-compliance in small firms could turn them into monopsonists, lowering the level of employment.

It is likely that a combination of these factors have contributed to the stable employment performance in this industry, but how much explanatory power rests on each of them is a matter for further research.

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Table 1: Descriptive measures of the minimum wage bite

Ex ante measures	1997		2007	
	East	West	East	West
Kaitz index ex ante (before MW introduction)	74.	62.	79.	64.
	2	1	2	4
	7	4	6	1
Per cent affected (employees with wage ≤ 1.02 of the minimum wage)	5.84	1.60	6.72	4.29
Per cent at minimum wage (employees with wages $0.98 \leq \text{wage} \leq 1.02$ of the minimum wage)	2.18	0.30	3.25	0.98
Contemporary measures	2001			
	East	West		
Kaitz index	77.34	62.53		
Per cent affected (employees with wage ≤ 1.02 of the minimum wage)	3.99	2.46		
Per cent at minimum wage (employees with wages $0.98 \leq \text{wage} \leq 1.02$ of the minimum wage)	2.19	0.49		

Notes: All numbers in percent. Data sources: IAB Integrated Employment Biographies, Microcensus.

Table 2: Estimated effects of the minimum wage on average wages

	East Germany				West Germany			
	Introduction 1997	Abolition 2003	Introduction 2007	Introduction 2007	Introduction 1997	Abolition 2003	Introduction 2007	Introduction 2007
Treatment group	0.001 (0.06)	-0.059*** (5.87)	-0.088*** (3.22)	0.001 (0.09)	0.002 (0.12)	-0.055*** (6.59)	-0.028 (0.92)	-0.024*** (2.83)
Post policy	-0.011 (1.07)	0.053*** (7.55)	0.055*** (3.34)	0.034*** (5.28)	-0.018** (2.56)	0.006 (1.61)	0.001 (0.06)	0.031*** (5.25)
Treatment effect	0.013 (1.64)	-0.039*** (-5.48)	-0.037** (-2.32)	0.032*** (4.66)	0.005 (0.78)	-0.021*** (-4.42)	-0.016 (-1.10)	0.009 (1.36)
Control group	without MW	with MW	similar occupation	without MW	without MW	with MW	similar occupation	without MW
R²	0.27	0.38	0.35	0.35	0.38	0.38	0.43	0.41
Placebo test	0.22	0.91	0.24	0.84	0.15	0.51	0.35	0.20
Observations	36,665	48,485	25,435	40,344	54,203	80,280	44,987	64,506

Notes: t-statistics (based on standard errors clustered at establishment level) in parentheses. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. The placebo estimate (as explained in the text) is represented by the p-value of a t-test for the estimated pseudo-treatment effect. All estimations contain further control variables, as described in the text.

Table 3: Estimated effects of the minimum wage on average wages of particular sub-groups

	Introduction 1997	Abolition 2003	Introduction 2007
East Germany			
New hires	-0.018 (0.88)	-0.058*** (3.49)	0.043*** (2.72)
Long-term employees	0.019** (2.48)	-0.033*** (6.85)	0.019*** (3.57)
Low skilled workers	0.054* (1.67)	-0.053*** (3.28)	0.007 (0.33)
West Germany			
New hires	0.030** (2.08)	-0.023* (1.84)	0.022 (1.63)
Long-term employees	0.000 (0.04)	-0.016*** (4.79)	0.018*** (4.77)
Low skilled workers	0.012 (0.77)	0.004 (0.41)	-0.011 (0.80)
	without MW	with MW	without MW

Notes: The columns contain treatment effects with t-statistics in parentheses. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. All estimations include main effects for treatment group and post-treatment period as well as further control variables.

Table 4: Estimated effects of the minimum wage on the probability of remaining in employment

	East Germany				West Germany			
	Introduction 1997	Abolition 2003	Introduction 2007		Introduction 1997	Abolition 2003	Introduction 2007	
Treatment group	0.006 (1.35)	-0.001 (0.30)	0.042 (1.10)	-0.002 (0.80)	0.004 (1.48)	-0.002 (0.68)	0.005 (1.22)	0.001 (0.92)
Post policy	0.007 (1.03)	0.002 (0.71)	0.005 (0.13)	0.003 (0.84)	0.019*** (4.86)	-0.006 (1.53)	0.002 (0.21)	0.001 (0.95)
Treatment effect	0.003 (0.46)	-0.004 (0.99)	0.007 (0.18)	-0.005 (0.80)	-0.004 (0.79)	0.008 (1.60)	0.001 (0.13)	0.000 (0.04)
Control group	without MW	with MW	similar occupation ^a	without MW	without MW	with MW	similar occupation	without MW
Placebo test	0.05*	0.59	0.70	0.92	0.87	0.01***	0.57	0.04**
Observations	34,617	51,073	25,672	40,416	52,376	81,805	45,979	65,946

Notes: t-statistics in parentheses. All estimations contain further control variables, as described in the text. Estimation is based on probit models. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. The treatment effects are marginal effects calculated according to Puhani (2008). The placebo estimate (as explained in the text) is represented by the p-value of a t-test for the estimated pseudo-treatment effect. ^a short-run effects (1 year) reported instead of long-run effects (2 years) due to insufficient data.

Table 5: Estimated effects of the minimum wage on establishment-level outcomes

	MW introduction 1997			MW abolition 2003			MW introduction 2007		
	Hirings	Separations	Stocks	Hirings	Separations	Stocks	Hirings	Separations	Stocks
East Germany									
Treatment group	-0.015 (0.70)	-0.022 (0.90)	0.408 (0.38)	-0.010 (0.61)	-0.068*** (3.36)	-9.417*** (10.04)	-0.029 (1.68)	-0.021 (1.12)	-1.213 (1.33)
Post policy	0.237*** (6.12)	0.005 (0.13)	-2.991 (1.61)	0.019 (1.10)	-0.007 (0.32)	-4.133*** (3.63)	-0.044** (2.32)	0.061*** (3.13)	1.994* (1.82)
Treatment effect	0.043 (1.36)	0.004 (0.11)	-0.955 (0.60)	0.001 (0.06)	-0.088*** (3.28)	2.226* (1.64)	0.020 (0.84)	-0.007 (0.30)	-0.350 (0.27)
Control group	without MW			with MW			without MW		
Placebo test	0.15	0.39	0.73	0.85	0.00***	0.10	0.45	0.75	0.42
Observations	3,546	3,546	3,452	3,960	3,960	3,729	3,814	3,814	3,605
West Germany									
Treatment group	0.018 (1.14)	0.003 (0.19)	0.138 (0.15)	0.005 (0.37)	-0.068*** (4.24)	-4.580*** (5.91)	-0.039*** (3.01)	0.004 (0.29)	-1.780** (2.04)
Post policy	0.348*** (12.52)	0.117*** (3.39)	0.393 (0.25)	0.028** (2.29)	0.001 (0.07)	-0.951 (1.04)	-0.018 (1.35)	0.102*** (6.67)	-0.090 (0.09)
Treatment effect	-0.049** (2.28)	-0.021 (0.87)	-0.898 (0.66)	-0.021 (1.31)	-0.041** (2.06)	1.147 (1.09)	0.026 (1.49)	-0.014 (0.72)	1.271 (1.05)
Control group	without MW			with MW			without MW		
Placebo test	0.29	0.65	0.76	0.17	0.00***	0.35	0.95	0.11	0.90
Observations	5,625	5,625	5,580	7,235	7,235	6,962	6,347	6,347	5,997

Notes: t-statistics in parentheses. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. All estimations contain further control variables, as described in the text. Estimation of hirings and separation is based on Tobit models, estimations for employment stocks on OLS.

Table 6: Estimated effects of the minimum wage on the receipt of public welfare benefits (2007)

	East Germany	West Germany
Treatment group	0.001 (0.58)	0.000 (0.29)
Post policy	-0.009*** (4.38)	-0.001*** (2.81)
Treatment effect	0.001 (0.34)	0.001 (1.11)
Control group	without MW	
Placebo test	0.08	0.22
Observations	40,759	65,937

Notes: t-statistics in parentheses. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. All estimations contain further control variables, as described in the text. Estimation is based on probit models. The treatment effect is calculated using the methodology by Puhani (2008).

Table 7: Estimated effects of the minimum wage on average wages and employment using a different control group

	Wages		Employment	
	Introduction 1997	Introduction 2007	Introduction 1997	Introduction 2007
East Germany				
Treatment group	0.064*** (7.47)	0.059*** (7.78)	-0.121*** (-5.80)	-0.029** (-2.76)
Post policy	0.002 (0.22)	0.039 (11.13)	-0.020 (-0.94)	0.038*** (5.87)
Treatment effect	0.021* (1.78)	0.015 (1.50)	0.001 (0.05)	-0.031* (-1.93)
Placebo test	-	0.62	-	0.52
Observations	5,395	8,754	6,764	9,797
West Germany				
Treatment group	0.122*** (5.44)	0.0854 (6.47)	-0.083** (-2.45)	-0.077*** (-4.00)
Post policy	0.007 (0.56)	0.026 (4.13)	-0.063** (-2.22)	0.033** (3.18)
Treatment effect	0.028 (1.01)	0.008 (0.53)	-0.012 (-0.25)	0.003 (0.17)
Placebo test	-	0.55	-	0.15
Observation	1,937	3,864	2,644	4,458
Control group	employees with earnings exceeding the minimum wage			

Notes: t-statistics in parentheses. ***, ** and * refer to statistical significance at the 1%, 5% and 10% levels. All estimations contain further control variables, as described in the text.

Figure 1: Development of minimum wages in the electrical trade

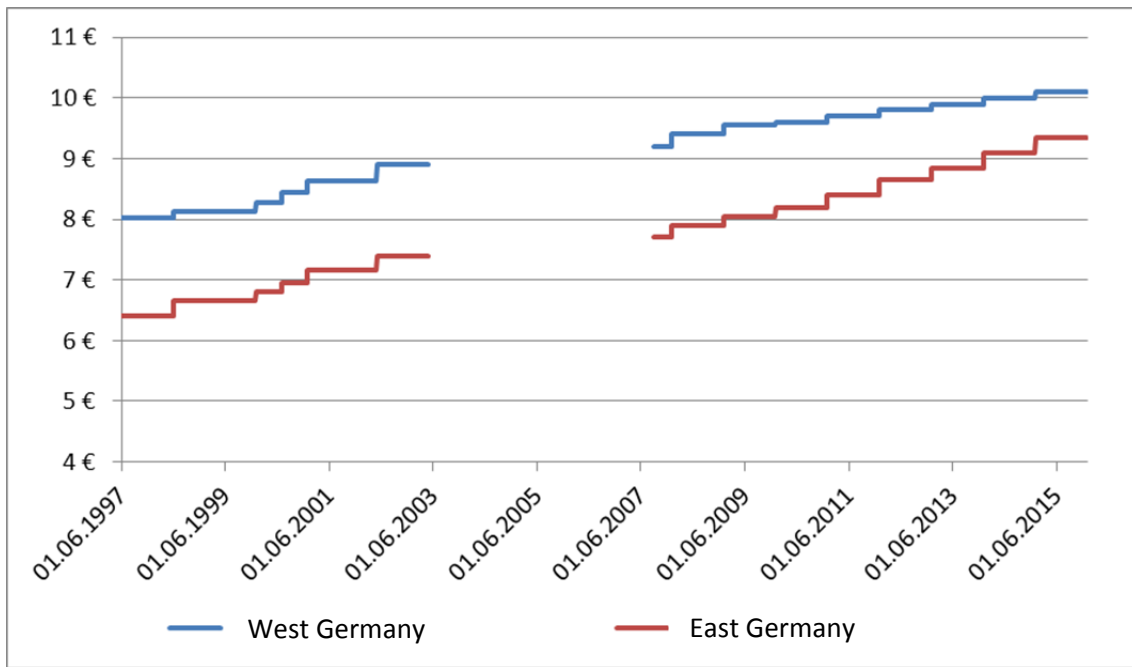
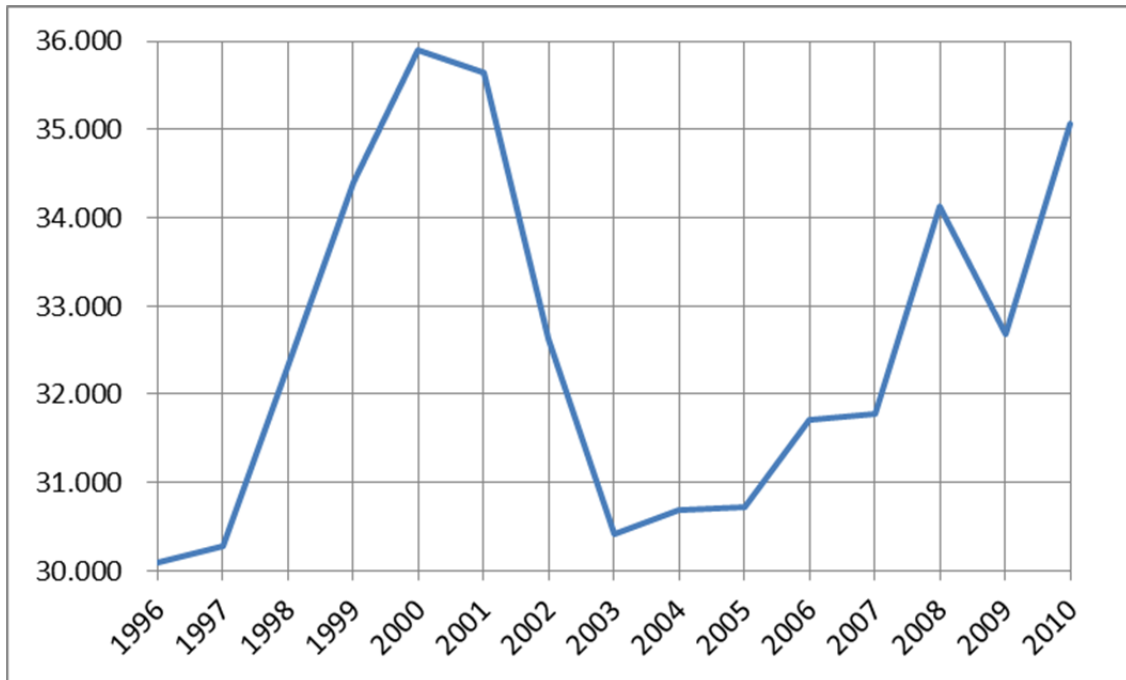
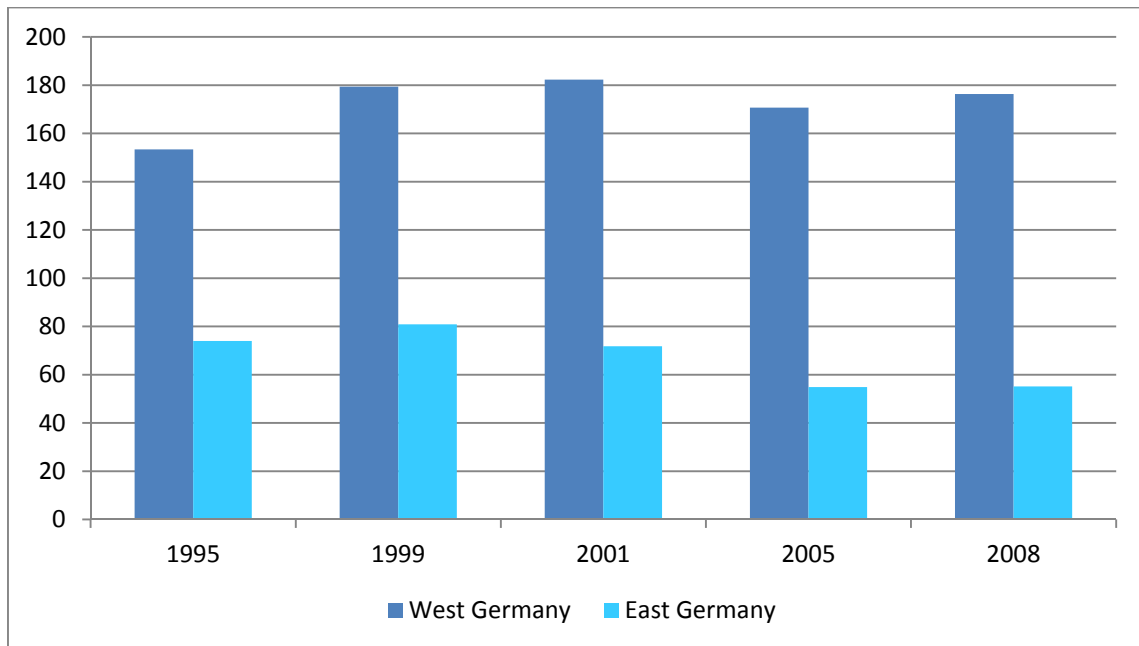


Figure 2: Total nominal sales in the electrical trade (in million €)



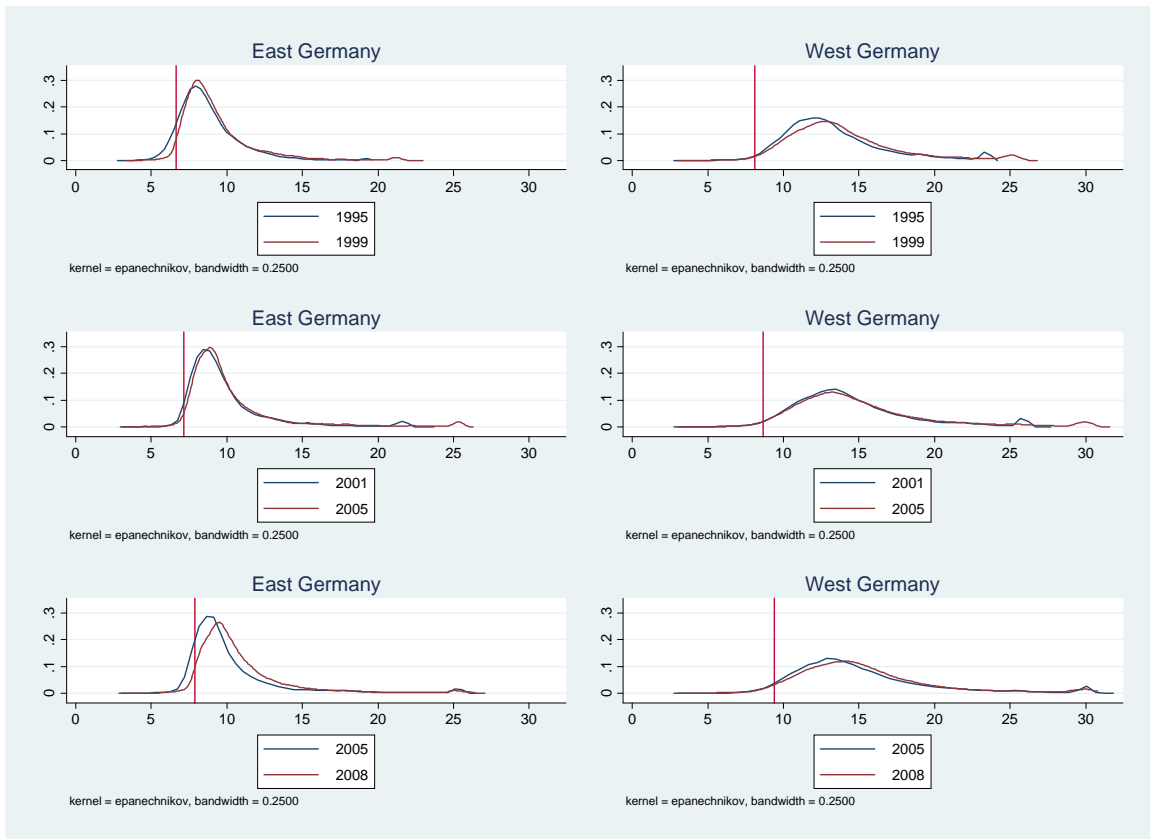
Source: ZVEH (Central Association of the Electrical Trade) Annual Reports.

Figure 3: Employees in the electrical trade (in 1,000s)



Source: IAB Establishment History Panel.

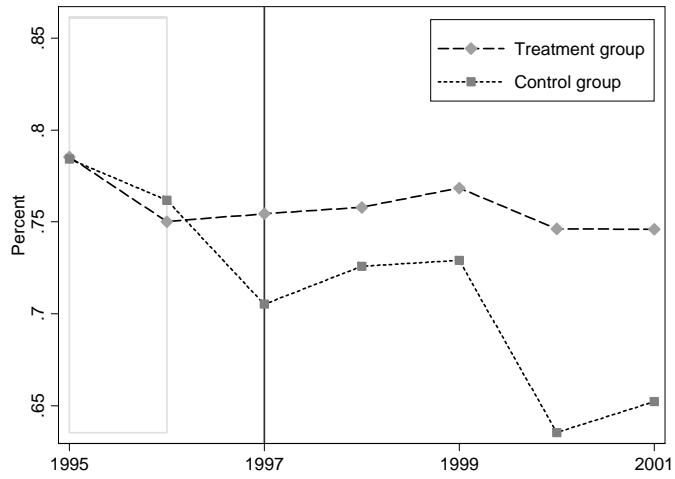
Figure 4: Distribution of hourly wages in the electrical trade



Appendix

Figure A1: Evolution of the employment probability – introduction

East Germany



West Germany

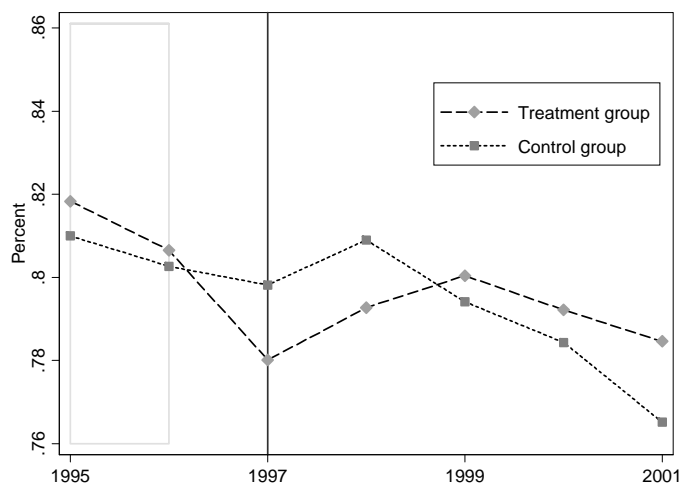
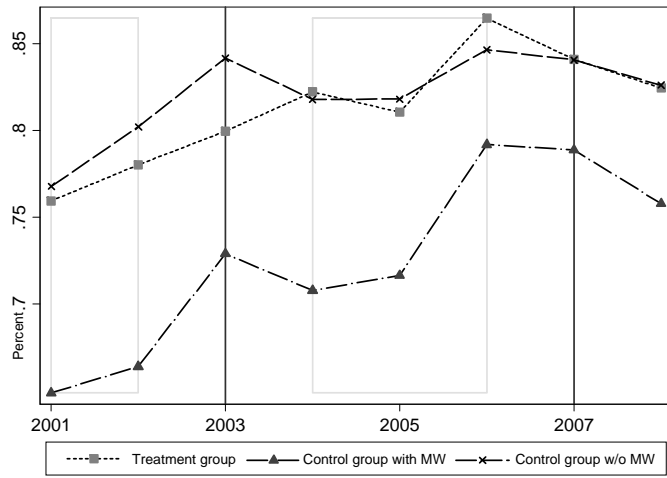


Figure A2: Evolution of the employment probability – abolishment and reintroduction
 East Germany



West Germany

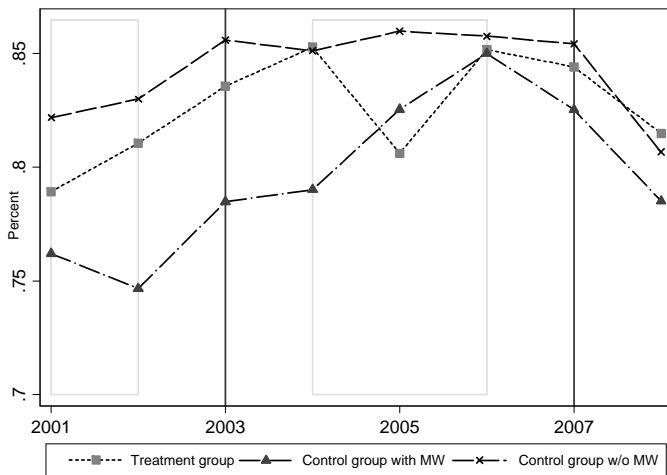


Figure A3: Evolution of the average hourly wage – introduction 1997

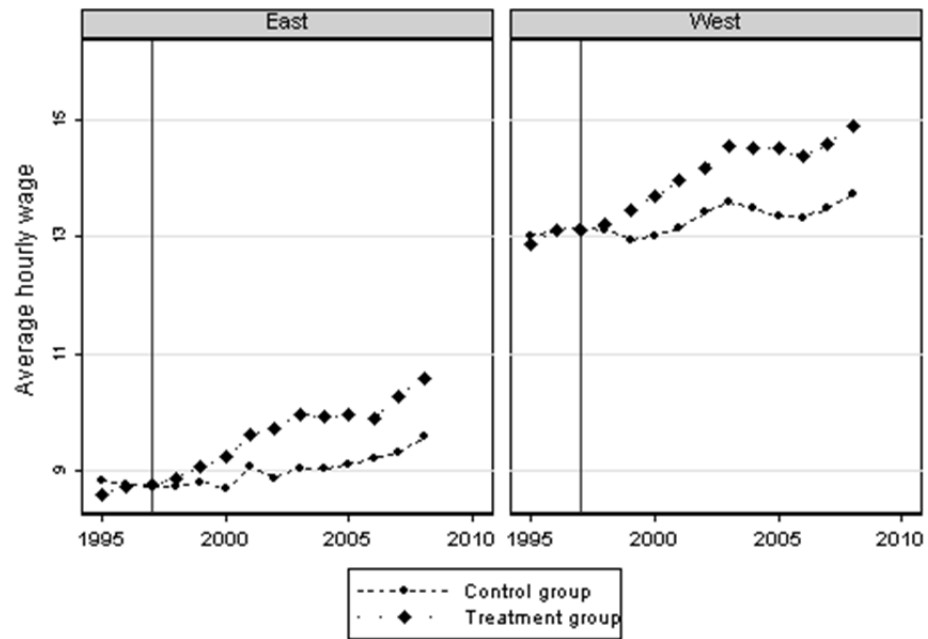


Figure A4: Evolution of the average hourly wage – abolishment and re-introduction

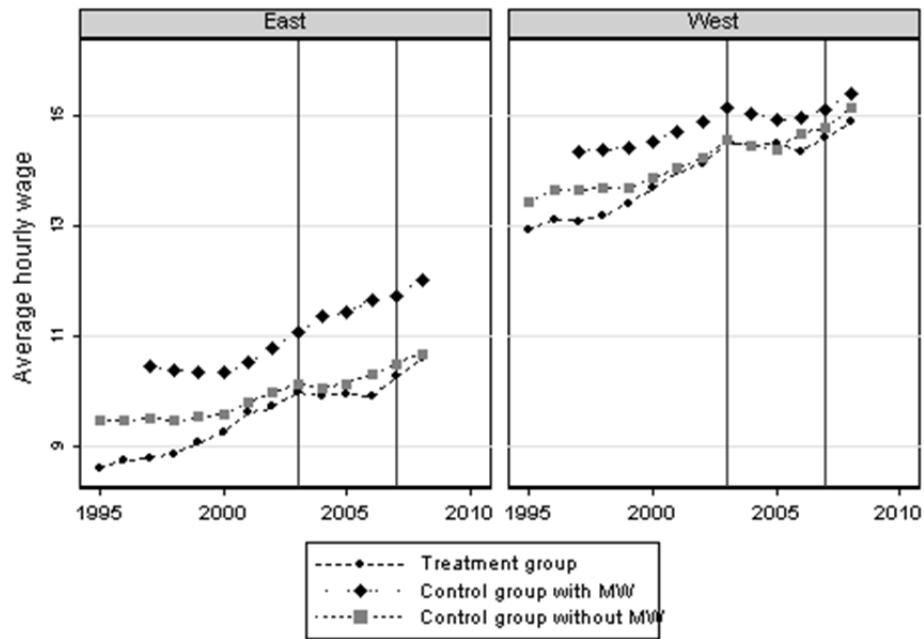


Table A1: Control group industries

WZ 2003 ^a	Industry	Used as control group for
45.21.1	general civil engineering	2003
45.21.2	building construction	2003
45.21.3 to 45.21.5	erection of prefabricated constructions	2003
45.21.6	bridge and tunnel construction	2003
45.22.3	carpentry and timber engineering	2003
45.23.1	road construction	2003
45.23.2	rail construction	2003
45.43.2	tiling and moulding	2003
45.43.3	screed laying	2003
45.33.0	plumbing, heating and gas and water installation	1997 and 2007
45.42.0	joinery installation	1997 and 2007
45.43.1	laying of parquet flooring	1997 and 2007
45.44.2	glazing	1997 and 2007
45.45.3	other finishing trade	1997 and 2007
45.50.2	leasing of building equipment	1997 and 2007

Notes: ^a WZ 2003 classification of the Statistical Office; The WZ 2003 industry classification has been applied from 2001. For earlier years, a different classification (WS73) is used, which means that time series of variables for the control group are unavailable for the whole observation period from 1995.

Table A2: Means of the independent variables

	Treatment group	Control group with MW	Control group w/o MW	Control group similar occupation
Male	0.988	0.987	0.958	0.998
Without vocation education	0.031	0.112	0.047	0.025
Completed vocation education	0.943	0.865	0.933	0.948
A-levels	0.003	0.003	0.003	0.000
A-levels and voc. Education	0.022	0.020	0.017	0.027
< 30 years	0.245	0.158	0.211	0.083
30 - 39 years	0.318	0.298	0.327	0.296
40 - 49 years	0.274	0.325	0.283	0.324
50 - 59 years	0.146	0.182	0.144	0.257
> 60 years	0.016	0.036	0.035	0.039
Nationality: German	0.962	0.933	0.955	0.947
Nationality: EU	0.013	0.026	0.015	0.034
Nationality: Eastern Europe	0.004	0.004	0.003	0.004
Nationality: other	0.022	0.037	0.026	0.016
Job tenure 1st quantile	0.231	0.285	0.230	0.243
Job tenure 2nd quantile	0.268	0.225	0.255	0.232
Job tenure 3rd quantile	0.243	0.259	0.250	0.261
Job tenure 4th quantile	0.258	0.231	0.265	0.264
Apprentice	0.002	0.002	0.003	0.002
Fulltime	0.994	0.968	0.961	0.998
Parttime (\geq 15 hours/week)	0.002	0.008	0.005	0.000
Parttime (< 15 hours/week)	0.001	0.001	0.001	0.000
Marginal employment	0.000	0.021	0.030	0.000
Second job	0.001	0.004	0.004	0.000
Rural area	0.659	0.717	0.666	0.644
City area	0.126	0.108	0.116	0.133
Metropolitan area	0.215	0.174	0.218	0.223
Age of establishment	15.203	16.486	17.242	16.539
Schleswig-Holstein	0.028	0.019	0.026	0.009
Hamburg	0.017	0.020	0.018	0.023
Niedersachsen	0.066	0.088	0.077	0.135
Bremen	0.008	0.003	0.005	0.000
Nordrhein-Westfalen	0.149	0.143	0.136	0.147
Hessen	0.060	0.047	0.101	0.030
Rheinland-Pfalz	0.022	0.040	0.035	0.041
Baden-Württemberg	0.101	0.091	0.112	0.124
Bayern	0.136	0.118	0.137	0.128
Saarland	0.011	0.012	0.006	0.002
Berlin	0.046	0.029	0.056	0.048
Brandenburg	0.058	0.068	0.037	0.051

	Treatment group	Control group with MW	Control group w/o MW	Control group similar occupation
Mecklenburg-Vorpommern	0.035	0.052	0.051	0.030
Sachsen	0.137	0.134	0.081	0.053
Sachsen-Anhalt	0.057	0.078	0.054	0.090
Thüringen	0.070	0.058	0.069	0.089
Unemployment rate (county level)	13.231	13.500	12.547	13.018
Share fulltime workers	0.811	0.873	0.810	0.897
Share marginal workers	0.054	0.047	0.068	0.034
Share of non-marginal workers	0.946	0.953	0.932	0.966
Share of women	0.130	0.095	0.158	0.114
Share parttime (< 15 hours/week)	0.050	0.044	0.062	0.039
Share parttime (≥ 15 hours/week)	0.018	0.022	0.022	0.017
Share w/o voc. Education	0.134	0.153	0.130	0.144
Share with voc. education	0.751	0.672	0.754	0.657
Share with higher education	0.028	0.047	0.029	0.079
Share qualification unknown	0.087	0.128	0.086	0.120
Share apprentices	0.117	0.052	0.100	0.042
Share unskilled	0.070	0.171	0.112	0.167
Share semi-skilled	0.593	0.586	0.559	0.514
Share non-production	0.189	0.155	0.198	0.238
Share master craftsmen/foremen	0.030	0.034	0.029	0.036
Share menial occupations (manual)	0.023	0.376	0.101	0.352
Share menial occupations (services)	0.025	0.045	0.033	0.063
Share primary job	0.991	0.993	0.989	0.997
Share aged 15 to 19	0.080	0.039	0.070	0.030
Share aged 20 to 24	0.118	0.074	0.104	0.051
Share aged 25 to 29	0.105	0.084	0.095	0.066
Share aged 30 to 34	0.130	0.119	0.130	0.109
Share aged 35 to 39	0.145	0.167	0.164	0.167
Share aged 40 to 44	0.132	0.166	0.144	0.166
Share aged 45 to 49	0.112	0.134	0.107	0.140
Share aged 50 to 54	0.093	0.110	0.088	0.130
Share aged 55 to 59	0.052	0.064	0.052	0.090
Share aged 60 to 64	0.023	0.034	0.031	0.043
Share aged 65 and above	0.010	0.010	0.015	0.007
1 - 5 employees	0.047	0.032	0.041	0.009
6 - 10 employees	0.104	0.070	0.096	0.021
11 - 20 employees	0.171	0.169	0.176	0.071
21 - 100 employees	0.473	0.473	0.423	0.434
> 100 employees	0.205	0.256	0.263	0.465

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