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Ronald Bachmann
Marion König
Sandra Schaffner

Lost in Transition? – Minimum Wage Effects on German Construction Workers

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

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

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

Editors

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

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

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

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

Editorial Office

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

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Abstract

Using a linked employer-employee data set on the German construction industry, we analyse the effects of the introduction of minimum wages in this sector on labour market dynamics. In doing so, we focus on accessions and separations, as well as the underlying labour market flows, at the establishment level. The fact that minimum wages in Germany are sector-specific enables us to use other industries as control groups within a difference-in-differences framework. We find that both accessions and separations rise in East Germany as a result of the minimum wage introduction. The evidence on detailed worker flows suggests that this is mainly due to increased recalls. Furthermore, the minimum wage introduction lowered job-to-job transitions in East Germany, which can be explained by a more compressed wage distribution making on-the-job search less worthwhile. No clear effects on labour market dynamics in West Germany arise.

JEL Classification: J23, J38, J42, J63

Keywords: Minimum wage; labour market flows; difference-in-differences, linked employer-employee

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1 Ronald Bachmann, RWI and IZA; Marion König, Institut für Arbeitsmarkt- und Berufsforschung; Sandra Schaffner, RWI. *– We thank participants of the ESPE Annual Conference and of seminars at RWI and ZEW for helpful comments and suggestions. We are grateful to Marvin Deversi and Barbara Treude for excellent research assistance..* *– All correspondence to Sandra Schaffner, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, E-Mail: sandra.schaffner@rwi-essen.de.*

1 Introduction

The labour market effects of minimum wage legislation have been a very active research area in labour economics during the last decades. In this context, the impact of minimum wages on employment levels has taken centre stage, without a general consensus emerging yet (e.g. Card and Krueger, 1994, Neumark and Wascher, 2008, Dube et al., 2010). However, even without strong effects on employment levels, minimum wages may have an impact on gross worker flows. This is important for the efficiency of the economy, as well as for individual and social welfare for several reasons. First, changes in job separation rates directly affect employment security, which is usually highly valued by workers. Second, altered hiring rates are likely to affect the duration of unemployment if some of the hirings come from unemployment. Third, worker turnover is associated with costs for firms and workers, e.g. for job search and vacancy posting. Finally, studying the impact on gross worker flows is important because it may provide an explanation for the (lack of) effects of minimum wages on employment levels.

From a theoretical point of view, there are two potential channels through which one can expect effects of minimum wages on labour market dynamics. On the one hand, the introduction of minimum wages can lead to transitional labour market flows. Within a search-and-matching framework with endogenous job destruction (Mortensen and Pissarides, 1994), job matches with productivity below a certain level at the time of the introduction of minimum wages are destroyed. Furthermore, with two-sided heterogeneity, there may be an increase in churning flows (Burgess et al., 2001) as the introduction of minimum wages may change the optimal combination of firm and worker characteristics. This channel thus unambiguously leads to an increase in worker flows.

On the other hand, the introduction of minimum wages may have an effect on equilibrium outcomes. First, with stochastic match productivity and a binding minimum wage – i.e. a minimum wage above the reservation wage before the introduction of the minimum wage –, job destruction rises. Second, if the minimum wage leads to a compression of the wage distribution, this will *ceteris paribus* lead to less on-the-job search and thus a lower level of direct job-to-job transitions (van den Berg and Ridder, 1998). Third, the effects on hirings depend on the reaction of both workers and firms, i.e. the elasticities of job search and of vacancy creation. The ultimate effect of minimum wages on equilibrium labour market dynamics is thus not clear ex ante and boils down to an empirical question.

In this paper, we examine the effects of the introduction of minimum wages on labour market dynamics in the German main construction industry (*Bauhauptgewerbe*) in 1997.[1] This industry is of particular interest because for Germany, it constitutes one of the largest economic sectors (1.3 million

[1] The minimum wage was €8 per hour for blue collar workers in East Germany and €8.69 in West Germany when the regulations came into force on January 1, 1997.

workers in 1997), it is the sector where minimum wages were introduced first, and it is by far the largest sector covered by minimum wage legislation. As explained in more detail in the next section, the German regulation concerning minimum wages is special in that there is no statutory minimum wage at the national level. Instead, minimum wages may be introduced at the level of the sector through the Posting of Workers Law (*Arbeitnehmerentsendegesetz*). This institutional set-up provides a unique opportunity for the study of the causal effects of minimum wages because economic sectors which are not affected by the minimum wage legislation in the main construction industry can be used as control groups.

Our analysis focuses on the effects of the aforementioned minimum wage introduction on worker flows. In particular, we examine hiring and separation rates, as well as individual worker transitions to and from employment, at the firm level taking into account different worker origin and destination states. In order to identify the causal effects of the minimum wage introduction, we conduct a difference-in-differences analysis with firms and workers from other economic sectors as the control groups. We are able to do so using a unique linked employer-employee data set on the German construction industry. This data set is derived from administrative sources and contains all the firms and workers who were in dependent-status employment in the construction industry, as well as in the industries chosen as control groups, during the time period under investigation.

We thus contribute to the literature on minimum wage effects in two ways. First, we add to the international evidence on the effects of minimum wages on labour market dynamics. To the best of our knowledge, this issue has up to now only been investigated for Canada (Brochu and Green, 2011), Portugal (Portugal and Cardoso, 2006), and the US (Dube et al., 2011). All three studies find hiring and separation rates to be lower due to minimum wage increases. In this context, the combination of having ideal control groups and of disposing of an outstanding data set allows us to identify a truly causal effect.

Second, we complement the evidence on the effects of minimum wages in Germany. König and Möller (2009) analyze the minimum wage effects on wage growth and the individual employment retention probability in the main construction sector. Rattenhuber (2011) focuses on the consequences in wage distribution and Müller (2010) on the employment effects in the same sector. In contrast, Frings (2012) concentrates on employment effects in other subsectors of the construction industry where different minimum wage rates were introduced. Bachmann et al. (2008) and Bachmann et al. (2012) use establishment surveys in order to examine the attitude of employers towards minimum wages and possible employment impacts for postal services and other sectors.[2] With our analysis, we go one step further and do not only look at employment effects. Instead, we split up employment

[2] A second body of the German minimum wage literature consists of "ex-ante" studies simulating the minimum wage effects (cf. Bauer et al., 2009, and Müller and Steiner, 2008).

changes into gross worker flows. These dynamics may give more detailed insight into the effects of minimum wages.

The paper is structured as follows. In the next section, we describe the institutional background of the German minimum wage regulations. In the third section, we present the data set used in the analysis. The fourth section contains a description of our empirical methodology, and the fifth section presents the descriptive and econometric evidence. The final section summarizes and concludes the discussion.

2 Institutional Background

Germany is one of the few countries in the European Union without a generally binding statutory minimum wage. Instead, minimum wages may be introduced at the industry level. The main reason for the introduction of the first sectoral minimum wage in Germany was that many workers from different countries of the European Union as well as third countries were posted to the German construction sites. These workers were mostly paid according the regulations of their home country, which often implied wages below the level of wages paid to German workers.

The legal framework for the introduction of the minimum wage is the Posting of Workers Law, which allows the extension of specific collective agreements to all firms and workers in an industry, independently of their membership in an employer association or a trade union. Minimum wages can thus be introduced by extending a collective agreement which includes a specific wage floor. The latter applies to domestic and foreign workers alike. Therefore, after the introduction of the minimum wage, it was also binding for posted foreign workers in the construction sector.

The Posting of Workers Law specifies strict requirements for a collective agreement to be declared generally binding. First, the initial collective agreement must be representative, implying that no additional collective agreement exists in the respective industry that covers more workers or union members. Second, the extension of the collective agreement should be in the public interest. Third, the social partners need to apply jointly for an extension, which requires a high degree of consensus. If these conditions are met, the Federal Ministry of Labour and Social Affairs usually declares the collective agreement generally binding without consulting any additional governmental bodies or institutions. Only when the application is filed for the first time, a committee consisting of three representatives of the respective trade union and employer association has to give its consent.

The collective bargaining agreement which lead to the introduction of minimum wages in the German construction industry was concluded on September 2, 1996 and declared generally binding on November 12, 1996. The minimum wage became effective on January 1, 1997 at a level of DM 17 (€8.69) for West Germany and DM 15.64 (€8.00) for East Germany. In September 1997, the

minimum wage was lowered to DM 16 (€8.18) and DM 15.14 (€7.74) for West and East Germany, respectively. Generally, workers are paid the minimum wage according to whether their place of work is in East or West Germany. West German workers who work in East Germany, however, receive the West German rate.

At the time of its introduction, the minimum wage was binding for 4 percent of the workers in West Germany and for 24 percent of the workers in East Germany (Apel et al., 2012). The Kaitz index – the ratio of the minimum wage to the median wage – was also at a higher level in East Germany (85 percent) than in West Germany (64 percent).

3 Data

The data set used in the empirical analysis is based on an extraction from the Integrated Employment Biographies (IEB) of the Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung - IAB*). The IEB data cover all individuals who are employed subject to social security, recipients of social security benefits, or registered with the Federal Employment Agency (*Bundesagentur für Arbeit - BA*) as a jobseeker. Given that the data are derived from administrative sources, the data quality is very high and the information contained very precise.[3] The structure of the IEB is described in more detail in Dorner et al. (2010).

From the IEB, we extract individual information for workers who are either in employment covered by social security or who are unemployed. For both labour market states, we only choose those individuals who were employed at least once in the main construction sector or one of the industries chosen as control groups for the time period between 1993 and 1999, which includes the introduction of the minimum wage in 1997 (for the empirical method and the control groups chosen, see Section 4).

The data set consists of employment and unemployment spells and provides several variables describing workers' characteristics such as year of birth, level of education, sex, job status, occupation, nationality, daily gross wage, and unemployment benefits. Given the administrative nature of our data set, parallel employment spells may occur for individuals. We therefore restrict our sample to the main employment spell of blue and white collar employees working full-time, as well as trainees. Marginal and part-time workers and spells with the duration of one day are excluded from our sample. On the employer side, the data include a unique establishment identifier as well as information on industry

[3]The German social security system requires firms to record the stock of workers at the beginning and the end of each year as well as all changes in employment relationships within the year. So the exact date for hirings, quits or dismissals of employees eligible for social security benefits during the year is reported. Civil servants, self-employed workers and retired persons are not included in the data.

affiliation and the employer's regional location.

The establishment identifier and the information on the universe of employees in the individual data set allows us to aggregate the information at the establishment level and thus to create a panel data set which contains gross worker flows for every establishment.[4] We thus obtain a unique linked employer-employee data set for the German construction sector and the control industries.

The main variables of interest in our analysis are the accession and separation flows of the blue collar workers for each establishment, which we compute using the underlying information on the employment histories of the individual workers. Note that the accession and separation rates are based only on blue collar workers as the minimum wage regulations are binding only for this group. We count all hirings and separations per establishment in a time period of eight months – between January 1 and August 31 of each year. The denominator of the different flow definitions is composed of all blue collar workers being employed in the corresponding establishment on the first day of the observation period (January 1). Section 4 contains the detailed definition of the different flow variables.[5]

Additionally, we generate establishment-specific variables which serve as control variables in the following multivariate analyses. The median hourly wage of the male blue collar workers controls for different wage levels in the establishments.[6] The variable "winter employment" indicates the average number of days worked at the establishment during the preceding winter season. This variable accounts for a special feature of the construction sector, namely the winter time from November to March, where special rules apply concerning canceled working hours due to bad weather. Additionally, we include dummies for nine different types of regions to control for different regional labour market conditions.

Finally, one important constraint of the data set should be mentioned. The information on posted workers from other countries is not included in the data set. This is unfortunate given that the Posting of Workers Law was introduced to protect the domestic workers in the German main construction sector from competition from posted workers.[7] Hence, the data do not allow us to investigate the

[4] In the data set, the observation unit on the employer side is the establishment, firms cannot be identified. In the following we use the terms "establishment" and "firm" synonymously for our observation unit.

[5] In order to separate new or exiting establishments from ID-changes or spin-off we apply the indicator of Hethey and Schmieder (2010).

[6] Note that the (daily) wages in the IEB are censored at the social security contribution limit. This does not constitute a problem in our case because we use the median wage for the computation of average wages within the firm, and because censoring for blue collar workers is low. Additionally, the underlying individual data set used does not contain hourly wages. As described above, it contains only information on daily wages as well as a qualitative variable on working time. Hence, we extract the information of another micro data set, the Mikrozensus, to impute the daily working hours in our sample. With this information we calculate the individual hourly wages for blue collar workers in our data set.

[7] Unfortunately, there is also no other data set which allows a causal analysis of the effects of minimum wages on posted workers in our context.

effects of the minimum wage introduction on posted workers.

4 Empirical Strategy

4.1 Identifying labour market flows

Our aim is to investigate the effect of the minimum wage introduction on worker flows in the construction industry in Germany. In order to do so, we distinguish between two labour market states, employment and non-employment (see Section 3 for details about the data set). The state of non-employment is defined as non-participation, i.e. not being observed in the data, or unemployment, i.e. receiving unemployment benefits.

Transitions of workers who change from one job to another job in a different firm within a seven-day period are counted as job-to-job flows (S_{EE}); if employment spells at the same firm are interrupted by a period of inactivity (i.e. the individual is not observed in the data set) of seven days or less, this does not count as a transition. Transitions from employment to unemployment are counted as employment-to-nonemployment flows (S_{EN}), as are transitions out of employment which are not followed by an employment or unemployment spell within the next seven days (i.e. the individual is not observed for more than seven days after the employment spell). Therefore the number of separations (S) consists of the sum of the two underlying separation flows:

$$S = S_{EE} + S_{EN}. \tag{1}$$

Similarly to the number of separations, the number of hirings (H) in one firm can be derived by the number of hirings from another job (H_{EE}) and from non-employment (H_{NE}).

The construction industry is characterized by a high share of workers that are unemployed during the winter and re-employed in the same firm in spring. Therefore, we also calculate the number of "recalls". We define "recalls" as transitions of workers who leave one firm to unemployment or to non-participation and re-enter employment in the same firm within three months without being employed by another firm during this period.

We thus compute two measures for hirings and separations, respectively. For the first measure, all hirings and separations are counted as described above. For the second measure, we do not take into account recalls. On the one hand, this means that the hiring of a worker does not count as an accession if the worker was with the same employer at some point during the previous three months, and has had no other intervening employment spell since then. On the other hand, a job separation is not counted if the worker returns to the same establishment within the next three months. Therefore, it is possible to calculate the number of hirings and separations without recalls by subtracting the

hirings and separations that are recalls:

$$S^{norecall} = S_{EE} + S_{EN} - S_{EN}^{recall}. \tag{2}$$

$$H^{norecall} = H_{EE} + H_{NE} - H_{NE}^{recall}. \tag{3}$$

The separation and hiring rates in each firm are derived by dividing the figure for the respective flow by the number of blue collar workers (E) employed on the first day of the observation period (January 1 of the years under investigation).

4.2 Difference-in-Differences Approach

To identify the causal effects of the minimum wage introduction on the separation and hiring rates, we apply a difference-in-differences (DiD) approach. All establishments in the main construction sector serve as treatment group. The control group consists of the establishments in the control industries. Based on this differentiation between treatment and control groups, we specify the following difference-in-differences model:

$$S_{it} = \lambda_0 + \lambda_1 Constr_i + \lambda_2 After_t + \lambda_3 Constr_i * After_t + \beta \mathbf{X}_{it} + \varepsilon_{it}, \tag{4}$$

where S_{it} is the separation rate of an establishment i at time t. $Constr_i$ is a dummy variable that indicates whether firm i is part of the main construction industry, $After_t$ takes the value 1 if t is after the introduction of the minimum wage and zero otherwise. $\mathbf{X}_{it}$ is a matrix of different additional control variables as described in Section 3. We analyse the hiring rate as well as the different flows into and out of employment in the same way.

The coefficient of the difference-in-differences operator λ_3 measures the causal effect of the minimum wage if two underlying assumptions are fulfilled. First, the minimum wage introduction does not affect the control group. Hence, spill-over effects from the main construction industry to the control branches should be as small as possible. This is particularly important because the main construction industry is characterized by interdependencies with many other industries. Second, the evolution of the variables of interest over time would not differ between the treatment and the control group in case no minimum wage was introduced. This assumption clearly cannot be tested as we cannot observe the counterfactual situation, i.e. no minimum wage in the main construction sector after 1997. The comparison of the time trends between the two groups before the minimum wage introduction as well as similar characteristics of both groups give an indication of the quality of the control groups. For a comprehensive overview of the difference-in-differences approach see Lechner (2010).

In our analysis of labour market transitions, we focus on the short-run effects of the minimum-wage introduction in 1997. We do so for two reasons. First, the assumption of a common trend is

more likely to be fulfilled in the short run than in the longer run, i.e. a DiD analysis over a longer time horizon is unlikely to identify a causal effect. Second, there was a strong minimum-wage hike in September 1999 (from €7.74 to €8.32 in East Germany, and from €8.81 to €9.46 in West Germany). Therefore, it would not be possible to disentangle the effects of the minimum wage introduction from the effects of the rise in minimum wages. For these reasons, we analyse the effects of the minimum wage introduction for the years 1997 and 1998, which is obtained by comparing the differences between the treatment and control industries in 1996 with the same differences in 1997 and 1998.

4.3 Two-part analysis

In the main construction sector, there is a relatively large number of firms who do not display any hirings or separations. Therefore, we observe a substantial share of observations with 0 as dependent variable. Furthermore, it is possible that the distribution of hirings and separations is governed by two separate processes. The first process determines the discrete decision of whether a firm realises no hirings (or separations) or whether it realises any positive number of hirings (or separations). The second process determines the intensity of hirings (or separations) given that the firm displays a positive number of hirings (or separations). In order to allow for the possibility that the introduction of minimum wages has a different impact on these two processes, we apply a two-part model. We prefer this model to a selection model for two reasons. On the one hand, the potential distribution (in this case, the potential transition probabilities of the firms who do not display any transitions) is not of interest in our context. On the other hand, our data set does not provide us with a variable which satisfies the exclusion restriction required by the Heckman selection model.

In the first stage, we estimate the discrete process no separations (or hirings) vs. a positive number of separations (or hirings):

$$S_{it}^{*} = \alpha_0 + \alpha_1 Constr_i + \alpha_2 After_t + \alpha_3 Constr_i * After_t + \beta \mathbf{X}_{it} + \epsilon_{it} \tag{5}$$

applying a probit model for all firms, with the dependent variable being one if we observe a positive number of transitions and zero if there are not transitions at the respective firm in the observation period. In the second stage, we estimate the transition rate for all firms with a positive number of transitions:

$$S_{it} = \gamma_0 + \gamma_1 Constr_i + \gamma_2 After_t + \gamma_3 Constr_i * After_t + \delta \mathbf{Z}_{it} + u_{it} \quad \text{if: } S_{it}^{*} = 1. \tag{6}$$

In non-linear models such as the two-part model, the coefficient of the interaction term differs from the marginal effect. Therefore, we adopt the derivation of marginal effects by Frondel and Vance

(2012)[8]:

$$\frac{\Delta^2 E(S_{it})}{\Delta After \Delta Constr} = \Phi\left(\alpha_0 + \alpha_1 + \alpha_2 + \alpha_3 + \beta \mathbf{X}_{it}\right) * \left(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3 + \delta \mathbf{Z}_{it}\right) - \Phi\left(\alpha_0 + \alpha_1 + \alpha_2 + \beta \mathbf{X}_{it}\right) * \left(\gamma_0 + \gamma_1 + \gamma_2 + \delta \mathbf{Z}_{it}\right). \quad (7)$$

4.4 Selection of Control Groups

The choice of a good control group is essential in order to identify the true minimum wage effect. For the analysis at hand, we choose different industries as control groups where no minimum wage regulations were in force during the time period analyzed. Nevertheless, there is generally a trade-off between the two assumptions mentioned above: An industry which is very similar to the main construction industry – as an indication for a common time trend – also has a higher likelihood of being affected by spill-overs from this industry.

In order to find the best control industries, we thus compare key figures of the construction sector with those of all potential control industries in the years before the introduction of the minimum wage in 1997. As key figures, we choose the growth rates of the first and fifth quartiles of the wage distributions as well as the employment growth rate. The mean square deviation serves as statistical similarity index and selection criterion for the potential control groups.

From the industries which feature key figures of similar size as the main construction industry, we identify four industries as potential control groups: (i) an industry which is closely related to the main construction sector in the sense that the evolution of production over time is similar; (ii) an industry which is not strongly connected to the main construction industry, i.e. its input-output interlinkages with main construction are relatively weak; (iii) a downstream industry, (iv) an upstream industry, i.e. an industry which produces primary products for the construction sector. Based on the results of placebo experiments, we finally use the industry which produces primary products for the construction sector (upstream industry) and the industry with weak linkages to main construction ("construction-unrelated") as control groups for our application here. Table 1 gives an overview of the definition of the main construction sector as treatment group and of the selection of the different control groups.

As a test for the validity of the control groups used, we perform placebo tests with hypothetical minimum wage introductions for the years 1995 and 1996. This means that we conduct the DiD regression analysis described above for the year 1995 (compared to 1994) and 1996 (compared to 1995), although a minimum wage was only introduced in 1997. Significant results for these placebo

[8] In contrast to Frondel and Vance (2012) we base our calculation on the formula of Puhani (2012). The standard errors are calculated using the delta method.

tests indicate that the treatment group and the control groups did not experience common trends in the two years before the minimum wage introduction.

The results of this exercise show that in several cases, the validity of the control groups cannot be taken for granted (Tables 7, 8 and 9). This is particularly the case for the upstream industries, which are apparently no good control groups in West Germany for overall hirings and for hirings from non-employment. Here, the placebo tests are significant at the 5% level for both 1995 and 1996. The same is true for the East German construction-unrelated control industries for overall separations, and for separations to non-employment. For the latter control industries, the placebos are significant for either 1995 or 1996 for overall hirings both with and without recalls in both parts of the country, for job-to-job hirings in East Germany, for hirings from non-employment without recalls in East Germany, and hirings from non-employment with and without recalls in West Germany, as well as for job-to-job separations in West Germany. Finally, placebos which are significant at the 5% level can be observed in West Germany for either 1995 or 1996 for separations without recalls and separations to non-employment without recalls based on the upstream industries as control group. In the cases mentioned above, the regression results should therefore be interpreted cautiously.

5 Results

5.1 Descriptive evidence

The mean hiring rates for the firms in the treatment group and in the control groups are displayed in Figures 1 and 2 for East and West Germany, respectively. While hiring rates in the construction firms in East Germany are higher than in West Germany, the evolution of hiring rates in construction in East and West Germany are similar for the time period under investigation: From 1996 to 1997, one can observe a relatively strong increase of these hiring rates. In the following year, hiring rates decline, virtually undoing the preceding increase. By contrast, the hiring rates in both control industries in East Germany feature a steady decline from 1996 to 1998. In West Germany, the hiring rates increase monotonously in the construction-unrelated industries, while in the upstream industries they slightly rise in 1997 and decline relatively strongly in 1998.

Looking at the hiring sources, it becomes evident that the increase of the hiring rate in the East German construction industry in 1997 is due to a rise in hirings from non-employment, especially recalls, which undoes the fall in hirings from employment. The decline in the following year is due to both hiring transitions falling. For West Germany, a similar pattern can be observed. Here, hirings from employment are virtually unchanged over time, i.e. hirings from non-employment, and especially recalls, drive the evolution of overall hirings.

The separation rate (Figure 3) behaves very similarly to the hiring rate in East Germany. In particular, one can observe an increase in 1997 and a decline in 1998, which brings the separation rate virtually back to its 1996 level. This evolution is again driven by the transitions between employment and non-employment as well as recalls. The separation rate in the West German construction industry is also very similar to the one of the hiring rate. The separation rates in the West German control industries, by contrast, display a decline (upstream industries) and a slight increase in 1997 and 1998 relative to 1996 (construction-unrelated). Table 2 shows the descriptives for our sample used in the subsequent analysis.

5.2 Econometric evidence

In the following, we present the results from the difference-in-differences estimation explained in Section 4. We do so for the overall levels of hirings and separations, for the underlying hirings flow rates (hirings from another job, and hirings from non-employment), and for the underlying separation flows (separations to another job, and separations to non-employment). Furthermore, we conduct separate estimations for East and West Germany, as well as for the year 1997 and 1998 (both compared to the year 1996).

The results for hirings and separations are displayed in Table 3.[9] For East German hirings including recalls, one can observe a positive effect for the construction industry compared to the construction-unrelated control industry in 1997, but no effect compared to the upstream control industry. This gives some indication for a positive effect, which however is not robust. For the year 1998, by contrast, a clear positive effect becomes apparent. Without recalls, a positive effect emerges for the unrelated control industries, but no effect for both years for the upstream control industries. Taken together, this yields a positive effect for hirings in the main construction sector in 1998 only.

For separations in East Germany (lower panel of Table 3), the results for both control industries in 1997 and 1998 indicate an unambiguously positive effect for hirings with recalls; for hirings without recalls, the effect is insignificant when comparing the main construction sector to the upstream control industries, while the effect using the unrelated control industries is still positively significant, but smaller than for separations without recalls.

Taken together, these results imply a significant impact of between 3 and 7.6 percentage points for hirings in East Germany for the year 1998, which corresponds to between 5 and 14 percent of the overall hiring rate (about 55 percent in 1996), i.e. the effect is relatively large. This effect seems to be mostly due to an increase in recalls in the main construction industry relative to the control

[9] The results tables generally only feature the marginal effects of the coefficient of the difference-in-differences operator, i.e. λ_3 in equation 4. For the calculation method see Section 4. A full set of coefficients for the estimation of the hiring rate is contained in Table 4.

industries since it becomes smaller or insignificant if recalls are excluded. For separations, a similar feature emerges. The effect lies between 5.2 and 6.6 percentage points including recalls, which equals an increase between 9 and 11 percent of the overall separation rate, and virtually disappears for separations without recalls.

For West Germany, no causal effect of the minimum wage introduction on hirings can be established (upper panel of Table 3), as the comparison with the two control groups yields effects which are of opposite sign (positive for the upstream industries, negative for the construction-unrelated industries), which are partly insignificant. The picture for separations is similar in that the results for the two control groups are opposite in sign (for separations including recalls) or insignificant (for separations excluding recalls). This means that overall, we are not able to identify a clear effect of the minimum wage neither for the hiring rate nor for the separation rate in the West German construction sector.

In the second step of the causal analysis, we examine the effect of the minimum wage introduction on hirings in more detail, distinguishing between the two sources of hirings, i.e. hirings from employment (job-to-job transitions leading to hirings) and hirings from non-employment (hirings from unemployment and non-participation). For East Germany, hirings out of employment are unaffected by the minimum wage introduction in both 1997 and 1998 (Table 5). For hirings from non-employment including recalls, we observe a significantly positive effect of between 1.6 and 9 percent. The latter effect disappears or becomes smaller when recalls are excluded. This implies that the positive effect on hirings can at least to some extent be explained by an increase in the hiring of workers which have been employed by the same firm before, i.e. recalls.

Looking at the different hiring flows in West Germany, it becomes apparent that the minimum wage introduction has no significant effect on job-to-job hirings (Table 5). In particular, the coefficients of interest are mostly insignificant for job-to-job hirings. As for hirings from non-employment, the upstream control industries yield insignificant results for the year 1997, and the two control groups display opposite signs for the year 1998 which does not give a clear indication of a minimum wage effect.

Finally, we scrutinize the effect of the minimum wage introduction on separations leading to a new job match (job-to-job separations) and separations to non-employment separately. For East Germany, the regression results show that separations to another job in 1997 were unaffected by the introduction of the minimum wage (upper panel of Table 6). For the year 1998, however, a robust negative effect on job-to-job separations becomes apparent. For separations into non-employment, relatively large effects can be observed in East Germany. The minimum wage increased the rates by 5.2 to 8.3 percentage points which corresponds to an increase of between 10.7 and 17 percent of the employment to non-employment rate. However, compared to this result, the effects on the transition

rate without recalls become smaller or even insignificant.

As for separations leading to another job, the coefficients for both years and both control industries are insignificant. It thus becomes apparent that separations leading to another job are not affected by the minimum wage introduction in West Germany (Table 6). Regarding West German separations to non-employment (including recalls), the coefficients for the construction sector compared to the upstream industries are positive and significant, but the coefficients for the construction-unrelated control groups are negative and insignificant; excluding recalls, the results for both control groups are insignificant.

Summarising, it becomes apparent that the introduction of minimum wages had effects in East Germany: Both hirings and separations were positively affected, which seems to be due to an increase in recalls, i.e. temporary transitions from employment to non-employment followed by transitions from non-employment back to the same firm. Furthermore, we find some evidence that job-to-job transitions fell after the introduction of minimum wages in East Germany. By contrast, we do not find any (clear) evidence that the minimum wage introduction in West Germany had an effect on either overall hirings and separations or on their underlying flows. Note that, with the exception of separations in East Germany, these results are robust to the placebo-testing of the control groups (see Section 4.4). The interpretation and the implications of these findings are discussed in the next section.

6 Summary and Conclusions

In this paper, we analyse the effects of the introduction of minimum wages on labour market flows in the German main construction industry in 1997. In particular, we examine overall hirings and separations, as well as job-to-job transitions and the flows between employment and non-employment at the establishment level. The fact that minimum wages in Germany are sector-specific allows us to use comparable industries as control groups within a difference-in-differences framework and thus to identify a truly causal effect.

Our analysis shows that the introduction of minimum wages had significant and quantitatively important effects in East Germany. In particular, overall hirings and separations were both positively affected. This seems to be mainly due to an increase in recalls, which we define as transitions from employment to non-employment followed by transitions from non-employment back to the same firm within a three-month period. One interpretation of this finding is that, after the introduction of minimum wages, firms resort more often to temporarily laying off workers in order to save on increased wage costs.

A second minimum wage effect we identify for East Germany is a reduction of job-to-job transi-

tions. This effect can be explained by the evolution of the wage distribution in East Germany, which became more compressed after the introduction of minimum wages in 1997. A more compressed wage distribution reduces the incentives of employed workers to engage in on-the-job search, as the expected gains from searching while employed are reduced, which in turn lowers job-to-job transitions (van den Berg and Ridder, 1998).

In contrast to the results for East Germany, we do not find clear evidence that the minimum wage introduction had an effect on labour market dynamics in West Germany. This result is not surprising given that the minimum wage was binding for relatively few workers in West Germany when it was introduced.

Our results, especially the ones for East Germany where minimum wages in the main construction sector are binding, thus add interesting insights into the effects of minimum wages. First, our results suggest that, by compressing the wage structure, minimum wages reduce job-to-job transitions.

Second, we show that the introduction of minimum wages may well increase labour market flows. This stands in contrast to the previous literature which has found a dampening effect of minimum wages on accessions and separations for Canada (Brochu and Green, 2011), Portugal (Portugal and Cardoso, 2006) and the US (Dube et al., 2011). It seems likely that our result is due to our focus on short-run effects, where transitory dynamics, rather than equilibrium effects, dominate. In addition, the result may be partly due to the specificities of the main construction industry, which is generally characterised by high turnover rates and a high prevalence of recalls.

Third, our results complement and add to the literature on minimum wages in Germany. In particular, we qualify the result by König and Möller (2009) who found relatively small effects of the minimum wage in the German main construction industry on employment security, especially in East Germany. The much larger effects uncovered by our analysis are likely to be due to the relatively long time period (January 1 to August 31) that is used for identifying labour market flows both in the year before the minimum wage introduction (1997) as well as for the two years after the minimum wage introduction. Apparently, our analysis is thus able to capture important effects which take place early in the calendar year.

Finally, a word of caution regarding further interpretation of the results is in order. While the industry-specificity of minimum wages allows us to identify a causal effect by using industries not affected by minimum wages as control groups for the main construction industry, the downside of our approach is that the results cannot easily be generalised. In particular, our results apply to the main construction industry only; whether other industries would experience similar effects upon the introduction of minimum wages is thus not clear at all. This is all the more important because the main construction industry displays several peculiarities, in particular a large number of foreign, posted

workers before the introduction of minimum wages. Given that minimum wages were introduced in this sector for mainly protectionist reasons, this group of workers may have experienced negative labour market effects. Unfortunately, there is no data to identify a causal effect of minimum wages on this group. For these reasons, our results should not be generalised to the effects of minimum wages in other sectors, or to the effects of a generally binding minimum wage.

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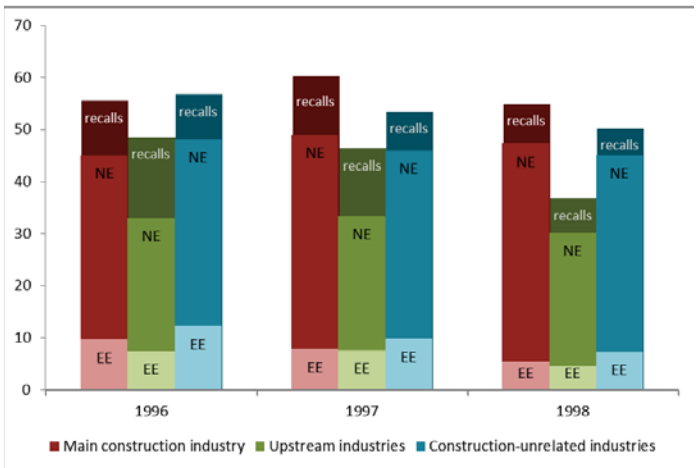
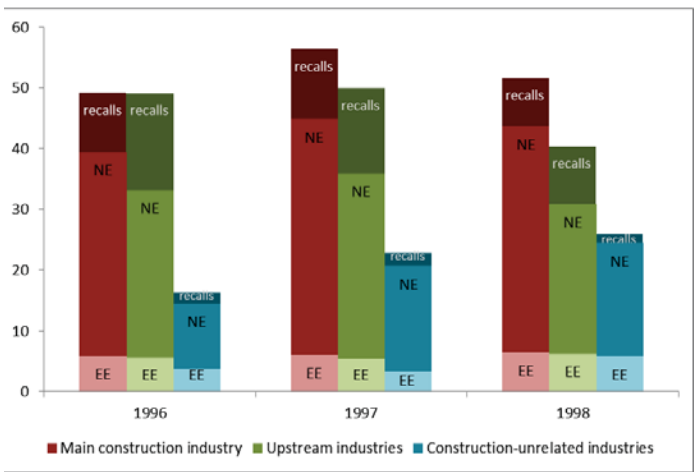
Appendix

Figure 1: Hiring rates East Germany



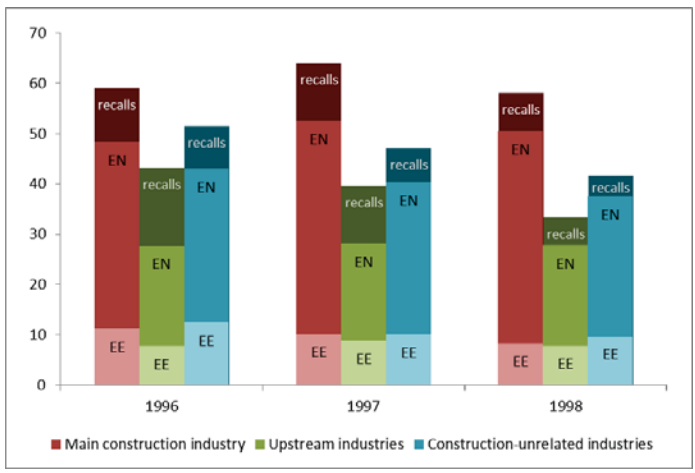
Notes: Mean hiring rates on establishment level. NE: transition rate from non-employment to employment. EE: job-to-job transition rate. Recall: transition from employment to employment with the same employer within 3 month without any other employment period in this time.

Figure 2: Hiring rates West Germany



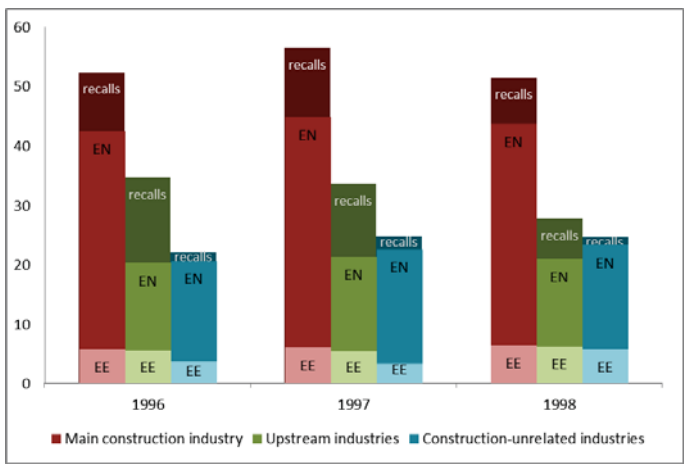
Notes: Mean hiring rates on establishment level. NE: transition rate from non-employment to employment. EE: job-to-job transition rate. Recall: transition from employment to employment with the same employer within 3 month without any other employment period in this time.

Figure 3: Separation rates East Germany



Notes: Mean separation rates on establishment level. EN: transition rate from employment to non-employment. EE: job-to-job transition rate. Recall: transition from employment to employment with the same employer within 3 month without any other employment period in this time.

Figure 4: Separation rates West Germany



Notes: Mean separation rates on establishment level. EN: transition rate from employment to non-employment. EE: job-to-job transition rate. Recall: transition from employment to employment with the same employer within 3 month without any other employment period in this time.

Table 1: Definition of Treatment and Control Groups

Treatment Group	Industry Code	Sector
Main Construction	590	General civil engineering activities
	591	Building construction and civil engineering
	592	Civil and underground
	593	Construction of chimneys and furnaces
	594	Plasterers and foundry dressing shops
	600	Carpentry and timber construction
	614	Floor tilers and paviors
Control Groups	Industry Code	Sector
(ii) West	431	Processing of paper and paperboard
(ii) East	651	Carriage of goods by motor vehicles
(iv) West and East	146	Manufacture of sand-lime brick, concrete and mortar

Notes:
Industry codes according to the Classification of Economic Activities of the German Federal Employment Agency 1973 (Wirtschaftszweige nach BA-Klassifikation 1973), (WZ 73).
For information on the selection procedure of the control groups please see text.

Table 2: Sample descriptives, averages for 1996-1998

Sample descriptives		East Germany	West Germany
Number of firms	Main construction industry	85,767	230,127
	Upstream industries	3,692	9,730
	Construction-unrelated industries	24,375	2,404
Number of employees	Main construction industry	1,739,001	888,417
	Upstream industries	127,458	46,515
	Construction-unrelated industries	48,523	103,248
Firm characteristics (averages)			
Number of employees	Main construction industry	10.36	7.56
	Upstream industries	12.60	13.10
	Construction-unrelated industries	4.24	20.18
Worker age	Main construction industry		
	Upstream industries		
	Construction-unrelated industries		
Wage level	Main construction industry	9.01	12.18
	Upstream industries	9.93	14.07
	Construction-unrelated industries	7.38	12.49
Number of employees below MW at introduction (share in %)	Main construction industry	26.67	13.18
	Upstream industries	18.11	3.22
	Construction-unrelated industries	63.04	8.39

Table 3: Difference-in-differences results transitions

			upstream		unrelated		
Hirings	with recalls	1997	0.033		0.040	**	East
		1998	0.076	**	0.030	**	
	without recalls	1997	0.018		0.032	**	
		1998	0.027		0.026	**	
	with recalls	1997	0.033	**	-0.051		West
		1998	0.085	**	-0.116	**	
	without recalls	1997	0.006		-0.056	*	
		1998	0.045	**	-0.101	**	
Separations	with recalls	1997	0.052	**	0.064	**	East
		1998	0.066	**	0.057	**	
	without recalls	1997	0.018		0.047	**	
		1998	0.002		0.045	**	
	with recalls	1997	0.022	*	-0.010		West
		1998	0.051	**	-0.045		
	without recalls	1997	-0.007		-0.015		
		1998	-0.006		-0.034		

Notes: Marginal effects, * significance at the 10% level, ** significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.

Table 4: Estimates for Hiring rate

	East Germany		West Germany	
	Coeff.	z/t-value	Coeff.	z/t-value
	First stage			
DiD	-0.0072	(-0.41)	0.0467**	(4.45)
Treatment Group	0.1796**	(10.63)	0.1129**	(9.19)
After introduction	-0.0156	(-1.19)	-0.0328**	(-3.67)
Hourly wage	0.0803**	(20.39)	0.0950**	(34.77)
Share of winter employment	0.0067**	(28.56)	0.0053**	(31.58)
District types	yes		yes	
Constant	-1.2591**	(-30.72)	-1.5379**	(-29.06)
	Second stage			
DiD	0.0414**	(3.44)	0.0197**	(2.20)
Treatment Group	0.0017	(0.13)	-0.0366**	(-3.80)
After introduction	-0.0212**	(-2.39)	0.0116	(1.56)
Hourly wage	-0.0462**	(-15.72)	-0.0666**	(-41.30)
Share of unskilled blue collar workers	0.0003	(0.81)	-0.0008**	(-2.58)
Share of skilled blue collar workers	-0.0039**	(-6.22)	-0.0031**	(-6.35)
Share of crafts master	0.0099**	(2.26)	0.0060**	(2.13)
Share of winter employment	-0.0162**	(-60.59)	-0.0159**	(-88.42)
District types	yes		yes	
Constant	3.3382**	(61.99)	3.6700**	(101.63)
N		81,322		190,229

Notes:
Z/t-values in parentheses. The standard errors are clustered on the level of districts.
For the description of the variables and the estimation method see text.

Table 5: Difference-in-differences detailed hiring rates

				upstream		unrelated		
Job-to-job			1997	-0.016		-0.002		East
			1998	-0.005		-0.003		
			1997	0.003		-0.002		West
			1998	-0.002		-0.020	**	
unemployment/ non-participation		with recalls	1997	0.035		0.037	**	East
			1998	0.090	**	0.016	*	
		without recalls	1997	0.018		0.029	**	
			1998	0.043	*	0.012		
		with recalls	1997	0.020		-0.063	*	West
			1998	0.084	**	-0.096	**	
		without recalls	1997	-0.005		-0.066	**	
			1998	0.044	**	-0.081	**	

Notes: Marginal effects, * significance at the 10% level, *∗ significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.

Table 6: Difference-in-differences detailed separations

				upstream		unrelated		
Job-to-job			1997	-0.016		0.006		East
			1998	-0.022	**	-0.010	**	
			1997	0.003		-0.003		West
			1998	-0.004		-0.014		
unemployment non-participation		with recalls	1997	0.062	**	0.052	**	East
			1998	0.083	**	0.060	**	
		without recalls	1997	0.027		0.036	**	
			1998	0.011		0.047	**	
		with recalls	1997	0.023	**	-0.017		West
			1998	0.068	**	-0.032		
		without recalls	1997	-0.008		-0.018		
			1998	0.011		-0.023		

Notes: Marginal effects, * significance at the 10% level, *∗ significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.

Table 7: Placebo-Tests transitions

			upstream		unrelated to construction		
		Placebo	ME		ME		
Hirings	with recalls	1995	-0.010		-0.01		
		1996	-0.029		-0.03	**	East
	without recalls	1995	0.014		-0.01		
		1996	-0.005		-0.05	**	
	with recalls	1995	0.032	**	-0.07	**	
		1996	-0.080	**	0.04		West
	without recalls	1995	0.039	**	-0.09	**	
		1996	-0.043	**	0.03		
Separations	with recalls	1995	0.021		0.04	**	
		1996	-0.017		0.04	**	East
	without recalls	1995	0.037	*	0.04	**	
		1996	0.017		0.03	**	
	with recalls	1995	0.009		0.00		
		1996	-0.007		0.04		West
	without recalls	1995	0.017		-0.01		
		1996	0.023	**	0.02		

Notes: Marginal effects, * significance at the 10% level, *∗ significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.

Table 8: Placebo-Tests detailed hiring rate

				upstream		unrelated to construction		
			Placebo	ME		ME		
Job-to-Job			1995	-0.009		-0.01		East
			1996	0.007		-0.03	**	
			1995	0.005		-0.02		West
			1996	-0.003		0.00		
unemployment/ non-participation		with recalls	1995	0.012		-0.01		East
			1996	-0.046	*	-0.02		
		without recalls	1995	0.027		-0.01		
			1996	-0.028		-0.03	**	
		with recalls	1995	0.027	**	-0.06	**	West
			1996	-0.072	**	0.05		
		without recalls	1995	0.032	**	-0.08	**	
			1996	-0.039	**	0.03		

Notes: Marginal effects, * significance at the 10% level, *∗ significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.

Table 9: Placebo-tests detailed separation rates

				upstream		unrelated to construction		
			Placebo	ME		ME		
Job-to-Job			1995	0.010		0.00		East
			1996	0.010		0.00		
			1995	0.001		-0.03	**	West
			1996	0.001		0.01		
unemployment/ non-participation		with recalls	1995	0.015		0.04	**	East
			1996	-0.033		0.04	**	
		without recalls	1995	0.030		0.05	**	
			1996	0.001		0.02	**	
		with recalls	1995	0.007		0.03		West
			1996	-0.010		0.04		
		without recalls	1995	0.012		0.02		
			1996	0.022	**	0.02		

Notes: Marginal effects, * significance at the 10% level, *∗ significance at the 5% level. The standard errors are calculated based on the delta method.
For the description of the additional covariates and the estimation method see text.