Employment effects of the new German minimum wage:
Evidence from establishment-level micro data[*]

Mario Bossler[†]

(Institute of Employment Research)

Hans-Dieter Gerner

(University of Applied Sciences Koblenz, Institute for Employment Research)

Abstract

We present first evidence on the consequences of the new statutory minimum wage in Germany. Using the IAB Establishment Panel, we identify employment effects from variation in the establishment-level affectedness. Difference-in-differences estimation reveals an increase in average wages by 4.8 percent and an employment loss by about 1.9 percent in affected establishments. These estimates imply an employment elasticity with respect to wages of about -0.3. Looking at the associated labor flows, the employment effect is mostly driven by a reduction in hires and the employment neutral turnover rate decreases. Moreover, we observe a reduction in the typical contracted working hours. [100 words]

Keywords: Minimum wage, employment, turnover, evaluation, difference-in-differences, Germany

JEL-Code: C23, J23, J38

1. Introduction

Internationally, minimum wages are on a rise. Between 2007 and 2009, the US federal minimum wage was raised to $ 7.25 after it decreased in real terms over several years. In early 2014, the US Senate rejected a proposal supported by the Democrats including President Obama, which would have raised the federal minimum wage to $ 10.10 by 2020. At the same time, local authorities have approved city-specific minimum wages in San Francisco, Los Angeles and Seattle, as well as state-specific minimum wages in California and New York that lead to minimum wages of $ 15 within only a few years. In the UK, the conservative chancellor George Osborne announced in July 2015 that the federal minimum wage would be increased from £ 6.50 to £ 9 by 2020 (BBC 2015). The announcement was politically motivated without utilizing the expertise of the Low Wage Commission. This is surprising as the British Low Wage Commission comprising of employers, unions, and academics used to be the leading body in advising the government towards changes in the minimum wages.

We analyze employment effects of the new German minimum wage, which was introduced on 1 January 2015 and requires an hourly wage of at least € 8.50. This new statutory minimum wage is the first federal minimum wage in Germany, where only few sector specific minimum wages have been existing. Traditionally, employer associations and unions collectively bargained over wages in their respective sectors. After collective bargaining coverage steadily decreased over the past two decades and at the same time gross wage inequality increased, the Great Coalition agreed to introduce a new federal minimum. The new minimum wage is certainly the most important labor market legislation in Germany since the Hartz reforms in the early 2000s, which fully reformed the unemployment insurance. Therefore, the minimum wage experiences a high political and public interest and the demand for an independent scientific ex-post evaluation (Zimmermann 2014), which we present here.

Not only politically but also in the public minimum wages are on the rise. In the US, an increase of the federal minimum wage has approval ratings of 76 percent, where even the majority of conservative voters favors an increase (Gallup 2013). Similarly in Germany, 79 percent of the population favored the introduction of a minimum wage in 2014 (IfD Allensbach 2014). However, when looking at the opinion of economists the picture is much more divided. In a recent poll among economists in Germany 56 to 68 percent of the respondents assesses the

minimum wage introduction as a political failure (Jäger, Krause, and Potrafke 2016).[1] This skepticism is mostly because standard microeconomic theory predicts employment to fall if the minimum wage is binding. In competitive markets employers cannot afford paying wages exceeding the value of marginal product, as this would cause a loss. However, monopsonistic labor market theories (Dickens, Machin, and Manning 1999) can relax this pessimistic prediction as a minimum wage could force employers to pay competitive wages. Moreover, a large number of empirical studies, which analyze employment effects of minimum wages in the US or the UK, fail to detect negative employment effects (e.g., Card 1992; Card and Krueger 1994; Dube, Lester, and Reich 2010).

Concerning the new German minimum wage, employment effects are likely for several reasons. The minimum wage legislation is very comprehensive as it only allows very few existing sectoral minimum wages to undercut the minimum wage until the end of 2016. With only few exemptions[2] also on the side of employees, it allows little scope for avoidance strategies leading to an employer-reported applicability of 99 percent.[3] A large fraction of employers affected by the minimum wage and even more important a large intensive margin affectedness of employees within affected establishments makes employment adjustments likely (Bellmann, Bossler, Gerner, and Hübler 2015). Within affected establishments, which are defined by at least one employee with an hourly wage below € 8.50 in 2014, our sample shows that 37 percent of the employees are affected. This concentrated affectedness makes adjustments likely and allows for a comprehensive comparison of affected with unaffected establishments.

A hint on potential employment adjustments is provided in Bossler (2016a), who shows that employers affected by the minimum wage report a slightly lower expected employment growth just a few months ahead of the minimum wage introduction. Such as in Bossler (2016a), we use the IAB Establishment Panel, which is a large-scale establishment-level panel dataset that allows identifying employment effects even if they are small. Using this data, we are the first to provide causal evidence concerning employment effects of the new minimum wage in Germany.

We apply a difference-in-differences comparison of a treatment group of affected establishments with a control group of unaffected establishments and estimate effects on wages, employment,

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[1] A letter article by O'Neill (2015) shows that economists in the US are similarly devided. However, the opposition is significantly weaker among young labour economist.
[2] Employees with exemption clauses are apprentices, internships of college students, young individuals under 18 years of age, and long-term unemployed for the first 6 months after re-employment.
[3] The self-reported applicability is calculated from the 2015 IAB Establishment Panel, where employers are asked whether an exemption clause allows them to undercut the minimum wage.

and an implicit employment elasticity with respect to wages. The latter yields an estimate that is particularly relevant for policymaking, as it allows for a rough prediction of employment effects of future minimum wage increases. We additionally estimate wage and employment effects separately for Eastern and Western Germany and present heterogeneities by product market competition. Furthermore, the data allow us to disentangle the employment effect into a hires and separations margin. Finally, we look at hours of work and the outsourcing of employment into freelance employment as alternative adjustment margins.

The data allow us to address three major economic issues, which can be problematic for micro-econometric evaluations of minimum wages: (1) anticipatory wage adjustments, (2) within establishment wage spillovers, and (3) across establishment spillovers. Anticipation is a particular issue for difference-in-differences estimation, which requires an exogenous treatment event. Since the minimum wage introduction followed a lengthy policy discussion anticipation effects are likely. Moreover, and most importantly, anticipatory wage adjustments due to the minimum wage introduction contaminate the treatment assignment. To receive a sharp treatment assignment, we exclude establishments that report to have adjusted wages in anticipation of the minimum wage introduction and before the affectedness information was collected.

Minimum wages can cause wage spillovers within establishments by increasing wages of workers with an hourly wage already above the required minimum of € 8.50. This is mostly because these employees demand wage increases to preserve the existing wage-productivity differentials (Aretz, Arntz, and Gregory 2013; Dittrich, Knabe, and Leipold 2014). Our data allow us to distinct between establishments that increased wages of workers with initial wages already above € 8.50, establishments that cut extra payments, and establishments without such spillovers. Separate regressions reveal interesting heterogeneities with respect to the ability to further increase wages and the pressure to cut personnel costs.

Finally, minimum wages can cause spillovers across establishments, which are indirect effects via the input and output markets. If there is an upward pressure of prices on the input market or a changed competitive environment, employers may react to the minimum wage differently. To address such spillovers across establishments, the IAB Establishment Panel includes a question on whether the respective establishment was indirectly affected by the minimum wage.

The article proceeds as follows: Section 2 presents a literature review on employment effects of minimum wages in the US, the UK, and of sectoral minimum wages in Germany. Section 3

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describes the IAB Establishment Panel, and Section 4 discusses the treatment assignment including a description of the analysis sample. Section 5 shows a graphical analysis allowing for a visual judgement of the parallel trends assumption and providing a first descriptive hint on the direction of treatment effects. Section 6 presents the econometric specification and the core results of the paper including effects on wages and employment. Section 7 presents robustness checks and effect heterogeneities. Section 8 supplements the effects on employment stocks by an analysis of labor flows. Section 9 presents effects on working hours and freelance employment. Section 10 concludes.

2.   Literature review

After the early literature on minimum wages mostly reported significant employment losses (Brown, Gilroy, and Kohen 1982), the picture became much more divided after Card and Krueger (1994) published their famous article on the impact of the 1992 increase in the New Jersey minimum wage on employment of highly affected fast food restaurants. In a difference-in-differences comparison with unaffected fast food restaurants in the neighboring state Pennsylvania, they do not observe negative employment effects.

This result was heavily debated for the last two decades with proponents claiming that there is no adverse employment effect and opponents claiming of significant employment losses. However, the debate also resulted in a comprehensive discussion on methodological issues of minimum wage evaluations using difference-in-differences-based comparisons, which we apply here. Recent studies started controlling for state and time specific heterogeneity by explicitly modelling state specific trends when analyzing US minimum wages across states (Addison, Blackburn, and Cotti 2015; Allegretto, Dube, and Reich 2011; Neumark, Salas, and Wascher 2014).[4] While these studies agree that employment elasticities with respect to minimum wage increases might in size not exceed -0.2, they still debate on whether there is, or is not, a negative effect.

When assessing the size of employment effects, the literature uses two kinds of elasticities: the employment elasticity with respect to a change in the minimum wage (e.g., Brown, Gilroy, and Kohen 1982; Dube, Naidu, and Reich 2007) and the implied labor demand elasticity of employment with respect to a minimum wage induced change in wages (e.g., Card 1992). While the first has a direct policy implication by relating the height of the minimum wage to

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[4] In a robustness check, we apply this identification strategy and include treatment group specific time trends.

employment changes, it is not possible to calculate such an elasticity for minimum wage introductions. Therefore, we stick to the second elasticity, which is a little more difficult to use as a policy tool. This is because a wage effect has to be estimated before drawing conclusions about subsequent employment changes.

Elasticity estimates are by definition zero whenever no employment effects are detected. Therefore, the studies on the 1992 minimum wage increase in New Jersey by Card and Krueger (1994, 2000) or Michl (2000) show an elasticity which is zero or even slightly positive. The corresponding study by Neumark and Wascher (2000) yields an elasticity of about -0.2. Looking at federal and state level minimum wages in the US, Card (1992), Allegretto, Dube, and Reich (2011), and Dube, Lester, and Reich (2010), do not find employment effects using different periods of time and methods. However, Neumark and Wascher (2004) and Neumark, Salas, and Wascher (2014) find negative employment elasticities of about -0.2 for teens, and more recently, Meer and West (2016) show that minimum wages in the US may not have an effect on employment levels but on employment growth. As we only observe 2015 as a single post treatment year, we look at employment levels and leave this interesting object for future research.

For the 1999 minimum wage introduction in the UK, Machin, Manning, and Rahman (2003) and Machin and Wilson (2004) look at the highly affected care homes sector and present implied labor demand elasticity with respect to wages ranging between -0.1 and -0.4. By contrast, Stewart (2004) does not find any employment effects when comparing individuals at different points of the wage distribution. Dolton, Bondibene, and Stops (2015) contribute by estimating effects of the introduction as well as subsequent changes of the minimum wage, but find little scope for a meaningful employment effect. Additional to the potentially small effect on employment, the UK literature agrees in a small hours reduction (Machin, Manning, and Rahman 2003; Stewart and Swaffield 2008).

Summarizing the international literature, direct elasticities with respect to minimum wage changes are much smaller than implied elasticities with respect to wages. This is because a one percent increase in the minimum wage does not necessarily lead to a one percent increase in average wages. Instead, average wages increase by less than the relative increase in minimum wages. In the same setting, this implies that implied employment elasticities are larger because the denominator is somewhat smaller than the denominator of the respective direct employment elasticity. Thus, elasticity estimates of about -0.3 are interpreted as large when looking at the

direct employment elasticity (Neumark and Wascher 2006), whereas implied elasticities of -0.3 are interpreted as modest effects (Machin, Manning, and Rahman 2003).

Before the introduction of the new minimum wage in Germany, minimum wages were only existent for specific sectors. The Posting of Workers-Law ("Arbeitnehmer-Entsendegesetz") of 1996, allowed unions and employer associations to apply at the federal ministry of labor for a declaration of general application. If approved by the ministry, this implies that bargained wages must be paid to all employees of the same industry even if not covered by collective bargaining. Hence, the declaration of general application implements a sector specific minimum wage. Among others, such sector specific minimum wages were introduced for the construction sector, electricians, roofers, and painters.

König and Möller (2009) were the first who analyzed employment effects of the minimum wage in the construction sector. Using difference-in-differences they compare affected with unaffected workers of the same sector and find sizable effects on wages, but only slightly negative effects on the employment retention in Eastern Germany, where the affectedness was much higher. Very similarly, Frings (2013) studies minimum wages for painters and electricians, but compares affected occupations with unrelated control sectors that were similar with respect to the parallel trends assumption. The results show no effects on full-time employment in either of the two affected sectors. Boockmann et al. (2013) study the minimum wage in the electrical trade sector, where minimum wages were introduced in 1997, abolished in 2003, and re-introduced in 2007 providing extensive variation over time. While the minimum wage effect on wages is consistent and positive across these events, they do not find any employment effects.

As Aretz, Arntz, and Gregory (2013) and vom Berge, Frings, and Paloyo (2013) find meaningful negative employment effects, the German literature is not conclusive either. While Aretz, Arntz, and Gregory (2013) estimate employment retention probabilities before and after the minimum wage introduction in the roofing sector, vom Berge, Frings, and Paloyo (2013) exploit regional variation in the construction sector. Both studies detect sizable effects especially in Eastern Germany and attribute this regional heterogeneity to a relatively larger bite in the east.

A potential criticism of the sector specific minimum wage literature is the endogeneity of the decision to introduce such minimum wages. As sector specific minimum wages need approval from employer associations, it is likely that these minimum wage introductions were to some extent endogenous for protective reasons (Bachmann, Bauer, and Frings 2014). Moreover, it is

likely that at least one of the decisive groups (unions, employer associations, or the federal ministry of labor) would have opposed the respective sector specific minimum wage if negative effects were foreseeable.

Of course, the new statutory minimum wage could also face the criticism of policy endogeneity. Some economists such as the German Council of Economic Experts speculate that the new minimum wage may not have caused an aggregate employment effect because of its timing of introduction in a period of a sound economic development (Sachverständigenrat 2015). However, the economic development was not foreseeable at the end of 2013 when the minimum wage introduction was decided. Moreover, the political arguments in favor of the new minimum were mostly grounded on the steadily increasing wage inequality and the decreasing collective bargaining coverage, but not on the economic development or potential employment effects. Finally, the height and timing of the minimum wage introduction were decided purely political without consent from unions and employer associations.

3.   Data

The dataset of our empirical analyses is the IAB Establishment Panel, which is a large annual survey on firm policies and personnel developments in Germany. The IAB Establishment Panel covers information of about 15,000 establishment observations to the date of June 30 each year. The survey's gross population comprises of all establishments located in Germany with at least one regular employee liable to social security. The sample selection is representative for industries, German states ("Bundesländer"), and establishment size categories. The interviews are conducted face-to-face by professional interviewers, who ensure a high data quality and a yearly continuation response rate of 83 percent. More comprehensive data descriptions of the IAB Establishment Panel can be found in Ellguth, Kohaut, and Möller (2014) or Fischer, Janik, Müller, and Schmucker (2009).

To analyze the employment effects of the minimum wage introduction, we included a module of additional questions to the 2014 and 2015 waves of the IAB Establishment Panel. In 2014, which is the year before the minimum wage came into force, we included questions that provide information on the bite of the minimum wage. The survey includes information on the extensive affectedness by asking whether the respective establishment has at least one employee with an hourly wage below € 8.50. Further, it includes information on the number of currently (in 2014) affected employees with an hourly wage below € 8.50, which we use to construct a fraction of

affected employees. We refer to this fraction as the intensive margin bite or intensive margin affectedness. Furthermore, the 2014 questionnaire includes a question asking whether wages were already adjusted in anticipation of the minimum wage introduction within the last 12 months, which is about the time horizon of the public debate on the minimum wage introduction. We use this latter information to refine the definition of the treatment and control groups as described in the next section.

A unique establishment identifier allows tracking establishments over time if the respective establishments continue to participate in the survey. This allows us to track the outcome variables back and forth, while using the 2014 affectedness by the minimum wage to distinct between a treatment and a control group. This yields an unbalanced panel of establishments, which existed and participated in the survey in 2014.[5]

4. Treatment assignment

We distinguish between a treatment group, which comprises establishments affected by the minimum wage, and a control group, which is unaffected. The group of affected establishments is defined in two alternative ways. First, the extensive margin affectedness includes all establishments with at least one employee with an hourly wage below € 8.50 in 2014. Second, the intensive margin affectedness is defined by the fraction of employees with an hourly wage below € 8.50 in 2014. This yields the same treatment and control groups, but weights the treated establishments by the fraction of affected employees.

A major issue for the exact differentiation between treated and control establishments are establishments that adjusted wages in anticipation of the minimum wage introduction. If the employer adjusted wages before the 2014 survey information was collected, the fraction of affected employees is already contaminated and the true bite is not revealed. In order to construct internally valid and sharp treatment and control groups, we exclude these establishments from the analysis sample.[6]

Another major issue for defining treatment and control groups is the establishment level exemption of the minimum wage law, which allows existing sectoral minimum wages and

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[5] We can replicate the results using a balanced panel. However, the balanced panel comes at the disadvantage of a smaller sample size, as the continuation response rate in the IAB-Establishment Panel is about 83 percent each year. For a balanced panel of multiple years, the sample size reduces by an exponential of the continuation response rate limiting the precision of our treatment effect estimates.
[6] Another reason to exclude anticipating establishments is their selectively positive employment trend (Bossler 2016b).

collective bargaining agreements to undercut the minimum wage until the end of 2016. In the IAB Establishment Panel 2015, employers are directly asked whether this exemption applies to the respective establishment and we exclude these plants from the treatment group. However, only 0.5 percent of the establishments report that this exemption applies.

The final and probably most critical issues for the definition of an unaffected control group are spillover effects, which can be within establishments if wages above € 8.50 are increased,[7] or across establishments if establishments are indirectly affected along the line of the product or labor market.[8] The survey includes questions on both sources of spillovers, which we use to estimate effect heterogeneities.

[Table 1 about here]

Table 1 shows descriptive figures from the analysis sample as of 2014, which is ahead of the minimum wage introduction when the treatment is assigned to establishments. We observe 13,453 establishments in our sample of which 1,599 (11.9 percent) are affected by the minimum wage and the remaining 11,854 establishments are our controls in the baseline sample.[9] The average establishment size shows a median employment of 17 employees in control establishments and 16 employees in treated establishments. The mean establishment size indicates of a few positive outliers in the control group, which are not influential towards our results. Using projection weights, the sample represents 1.9 million establishments and 32.5 million employees in Germany. Most important as we estimate treatment effects on the treated establishments, the treatment group represents 180,000 establishments and 3.1 million employees. The measure of affectedness shows that in total 4.4 percent of all employees had an hourly wage below € 8.50 and within treated establishments a relatively large fraction (37.8 percent) of the employees was affected by the new minimum wage. Moreover, the outcome variables of interest, which are logarithmic (henceforth: log) wages and log employment were on average lower in the treatment group than in the control group.

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[7] The survey question on the first source of spillovers asks whether one of the following wage adjustments were conducted in response to the minimum wage introduction: (a) wages above € 8.50 were reduced, (b) wages above € 8.50 were increased, (c) extra payments were reduced or cut. As only 20 establishments reported to have reduced wages above € 8.50, we combine categories (a) and (c) for our analysis.

[8] The survey question on the second source of spillovers asks whether establishments have been indirectly affected by the minimum wage, e.g., through changes in prices or a change in competition.

[9] Because of item non-response, the sample size is slightly smaller when looking at log wages as the outcome of interest. This could potentially bias the results of the wage effect. However, the treatment effect on wages is very robust, see Sections 5, 6, and 7, such that this potential selectivity bias and its influence on the overall results is only worth a theoretical note.

5. Graphical analysis

Before presenting an econometric analysis, we illustrate the time series of average wages and employment by treatment status. This rather descriptive graphical analysis allows for a first visual inspection of potential treatment effects, and more importantly, it allows inspecting the parallel trends assumption, which is crucial for difference-in-differences analyses.

[Figure 1 about here]

Figure 1 displays the time series of the two outcome variables. We center the time series at the 2013 values, which is before the minimum wage introduction was announced making anticipation effects unlikely (Bossler 2016a). The graphs in Panels A show that the log wages per worker evolve similar for treated and control establishments ahead of the minimum wage intervention in 2015. Both wage patterns are on a positive trend, which reflects increasing nominal wages. In 2015 wages spike for the treatment group of establishments indicating that the minimum wage effectively increased the wages per worker. In 2014, we observe a slight drop in wags among the treated plants. From a visual inspection of the graph, this could reflect a somewhat weaker trend ahead of minimum wage introduction. Panel B displays log employment for the treatment and the control group. Both groups of establishments are on a similar employment trend ahead of the minimum wage introduction. In 2015, treated establishments show a small negative deviation from the trend of unaffected establishments indicating of a small negative employment effect.

6. Econometric analysis

In the econometric analysis, we aim to estimate reduced form treatment effects of the minimum wage on average wages and employment. Furthermore, we are interested in an employment elasticity of a minimum wage induced wage increase. We uncover this elasticity by estimating instrumental variable (IV) regressions, in which the minimum wage effect on log wages per worker serves as the first stage regression.

We start with estimating the reduced form effect on the logarithmic wages per worker, which also serves as first stage regression. We use a difference-in-differences specification

$$Ln(wages/worker)_{it} = treated_i * treatment\ time_t * \beta_{ToT} + X_{it}\beta + \gamma_t + \theta_i + \varepsilon_{it} \quad (1),$$

where β_{ToT} is the treatment effect on the treated, which is the effect on the treatment group and treatment time interaction. It shows whether the minimum wage was effective to increase average wages at affected workplaces. Time-varying control variables in X_{it} comprise of dummies for

collective bargaining coverage and works councils and the shares of full-time and female employees. Specification (1) further includes a vector of year fixed effects γ_t and establishment fixed effects θ_i.

In a second step, we estimate the same reduced form difference-in-difference specification on log employment as the outcome variable of interest:

$$Ln(employment)_{it} = treated_i * treatment\ time_t * \delta_{ToT} + X_{it}\beta + \gamma_t + \theta_i + \varepsilon_{it} \quad (2),$$

where δ_{ToT} is the reduced form policy effect of the minimum wage introduction on employment of treated establishments.

To estimate an employment elasticity as described above, we want to identify the effect of $Ln(wages/worker)_{it}$ on $Ln(employment)_{it}$ using the treatment time and affectedness interaction as exogenous instrument. For this IV estimation, we can apply a moment estimator that divides the reduced form estimate of equation (2) by the reduced form estimate of equation (1), which is the simple instrumental variable Wald estimator:

$$\eta_{moment\ estimator} = \frac{\widehat{\delta_{ToT}}}{\widehat{\beta_{ToT}}} \quad (3)^{10}$$

Table 2 displays the baseline results including effects on wages, employment, as well as the elasticity estimates. Panel A presents the effects from the extensive margin treatment assignment, in which the minimum wage effect on wages per worker at affected establishments is about 4.8 percent. This demonstrates that the minimum wage introduction was binding. The treatment effect on employment is -0.019 log points implying an employment reduction of about 1.9 percent at affected establishments. A closer inspection of the data as well as the graphical analysis show that the negative treatment effect is driven by the fact that control establishments increased employment by about 1.7 percent while the treated establishments held their employment merely constant. The regression based placebo estimates, for which the treatment period is artificially assigned to 2014, are small. While the placebo is slightly negative when looking at wages, this can reflect a weaker time trend, which we address in a robustness check when adding treatment group specific trends. The two elasticity estimates in columns (3) and (4) are between -0.3 and -0.4. This elasticity implies that a 1 percent wage increase from the minimum wage causes an employment reduction by about 0.3 to 0.4 percent.

[Table 2 about here]

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[10] For the moment estimator, we rely on bootstrap based cluster robust inference of all steps combined. We report standard errors from a block clustered bootstrap using 200 replications (Efron and Tibshirani 1994).

The results in Panel B of Table 2 are treatment effects on a treatment group defined by the intensive margin affectedness, i.e. the fraction of affected employees within affected plants. Since the fraction of affected employees within affected plants is about 0.37, the treatment variable is roughly a third of the dummy treatment, and if consistent, the effects should be about three times the effect size of Panel A. The treatment effect on wages per worker is about 11.7 percent and on employment 3.5 percent. Again, both regression based placebo tests, which estimate an effect for 2014 when the minimum wage was not yet introduced, are small. The employment elasticity with respect the minimum wage induced wage increase is about -0.3. Panel C of Table 2 displays separate treatment effects for five different intensities of affectedness. While the size of the treatment effects increases in the intensity of affectedness, the most severely affected establishments, at which 81 to 100 percent of the employees had hourly wages below € 8.50 in 2014, do not show a negative employment effect.

At a first glance, this result seems odd. Why are the most severely affected plants not adjusting employment? However, a closer inspection of the data allows us to provide an intuitive explanation that this may be due to higher rates of establishment closure. While the survey itself only includes information on surviving establishments, the surveying institute TNS Infratest Sozialforschung collects some basic information on the reasons for survey non-response of previously participating panel establishments. We use this information to distinct plant closures from panel continuation and non-response. As in our baseline sample, we look at establishments existing in 2014 and conduct a cross-sectional regression explaining the incidence of closure between 2014 and 2015.

[Table 3 about here]

The results in column (1) show a slightly positive effect on closures for the most severely affected plants. However, this cross-sectional result may be co-determined by other explanatory factors. In column (2), we add control variables for the initial size, the initial profitability, and the initial technological state of the capital stock. However, the respective effects remain similar in size. Only the group of plants indicated by the largest fraction of affected employees shows a meaningful positive effect on closures. This result suggests that closures may be the channel of employment effects in this group of plants, and it serves as an intuitive explanation why we do not see employment adjustments among these most severely affected establishments when they survive.

7.   Robustness checks and heterogeneities

Robustness checks

We test the robustness of our baseline results, with respect to controlling for additional treatment group and time specific heterogeneity: Following Addison, Blackburn, and Cotti (2015), Allegretto, Dube, and Reich (2011) and Neumark, Salas, and Wascher (2014), we include treatment group specific time trends to our baseline specification. We allow for linear trends in columns (1) and (2) and quadratic trends in columns (3) and (4) of Table 4. In both trend specifications, the effect on average wages is slightly larger. This is mostly because the affected plants' trend in average wages was slightly weaker in the years before the minimum wage came in force. After adjusting for this initial difference in trends, the effect on average wages increases. When looking at log employment, the effect is robust towards controlling for treatment group specific trends.

[Table 4 about here]

Effect heterogeneities with respect to spillovers

Next, we address two specific issues of minimum wages, which are spillovers within establishments and spillovers across establishments. By spillovers within establishments, we mean impacts on wages of employees even if they are not directly affected by the minimum wage, and we estimate separate effects (a) for establishments without any wage spillovers, (b) for establishments with minimum wage induced wage increases even above € 8.50, and (c) for establishments with minimum wage induced wage cuts or cuts of extra payments. We retrieve this information from our module of questions that was directly included in the 2015 survey of the IAB Establishment Panel—see section 4.

Table 5 shows the same negative employment effect for establishments without wage spillovers. For the group of establishments with positive wage spillovers, the employment effect shrinks to zero implying that these employers can afford paying higher wages. By contrast, establishments with compensating wage cuts show a much stronger negative employment effect indicting that these cannot afford paying the minimum wage and therefore have to reduce employment levels.

[Table 5 about here]

By spillovers across establishments, we mean indirect impacts on the product market, i.e. by changed prices. Table 6 presents effects with and without such spillovers across establishments. While the wage effect is similar for both types of establishments, the employment effect is more

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pronounced among establishments that are affected by minimum wage induced spillovers across establishments. This suggests that the minimum wage has stronger effects on establishments that are also indirectly affected, i.e., among establishments that operate in an environment in which the minimum wage changed the economic conditions such as market prices.

[Table 6 about here]

East-west heterogeneities and product market competition

We further present heterogeneous effects with respect to differences between Eastern and Western Germany. Moreover, we present separate effects with respect to the employers' reporting to face high product market competition.

In the literature on sectoral minimum wages in Germany, most studies find somewhat larger employment effects in Eastern Germany (Aretz, Arntz, and Gregory 2013; vom Berge, Frings, Paloyo 2013). These studies attribute the relatively larger effects in the east to a larger bite of the respective minimum wages. Two dimensions of a larger bite in the east are possible. First, affected establishments may comprise of a larger fraction of affected employees. Second, affected establishments in the east show a stronger wage effect.

[Figure 2 about here]

To check for the first dimension, Figure 2 illustrates the affectedness across German states. Panel A shows that the number of affected establishments is relatively larger in the east than in the west. However, the severity of affectedness within affected establishments, which is illustrated in Panel B, shows only a modest difference between the east and the west. In our estimation, which identifies a treatment effect on the treated establishments, the intensity of affectedness should be of higher importance. Hence, the similarity in the intensity of affectedness does not help to explain a stronger employment effect in the East.

To check whether a stronger wage effect helps to explain stronger employment effects in the east, we estimate separate wage effects. The results in Table 7 display a wage effect of 5.3 log points in the east and 3.4 log points in the west. Based on this larger minimum wage induced wage increase, we expect a somewhat larger employment effect in the east. In line with this prediction, Table 7 shows that the employment effect in Eastern Germany is negative, while the effect in the west shrinks and lacks statistical significance.

[Table 7 about here]

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We finally estimate separate effects by product market competition. The survey includes a direct self-assessment of the employers' perceived intensity of product market competition. We follow Hirsch, Oberfichtner, and Schnabel (2014) and construct a dummy for high competition. Based on this differentiation Panel B of Table 7 presents results from separate regressions. The wage effect is of similar size irrespective of the competitive pressure. Even though the separate employment effects are imprecise, the employment effect seems slightly larger for the group facing high competition. This is in line with an argumentation that high product market competition leaves little room to pay higher wages such as demanded by the minimum wage.

8. Employment turnover

While the early literature on minimum wages exclusively looks at changes in employment levels, the analysis of labor flows has become more prominent recently. The analysis of labor flows helps to disentangle any labor demand adjustments, but it may also help to detect effects on employee turnover irrespective of employment adjustments. Most of the literature shows that minimum wages reduce labor turnover: Looking at Portuguese data, Portugal and Cardoso (2006) analyze the short run effects of a sharp minimum wage increase and find evidence for reduced hires and reduced separations. Comparing provinces in Canada, Brochu and Green (2013) also show that minimum wages cause decreasing hiring and separation rates resulting in reduced labor turnover. In line with these results, Dube, Lester, and Reich (2016) show an internally valid and robust reduction in labor flows for minimum wages in US states. The only evidence for Germany is presented in Bachmann, Penninger, and Schaffner (2015), who analyze labor flows in response to a sectoral minimum wage in the German construction sector. Their results depend on the choice of the control group.

We first disentangle the employment effect into hires and separations. Both, a reduction in hires and an increase in separations could contribute to the labor demand adjustment, which we observe in Section 6. The data further allow differentiating separations into employee initiated quits and employer initiated layoffs. Looking at the possibilities for separations, layoffs could increase to adjust the total number of employees, but quits may decrease as minimum wages cause a reduction of on-the-job-search through a compressed wage distribution (van den Berg and Ridder 1998).

For the difference-in-differences estimation, we construct a separation rate relative to previous year's employment, i.e., $separation\ rate_{it} = \frac{separations_{it}}{N_{it-1}}$, where $separations_{it}$ is a backward looking flow variable, and the lagged employment level N_{it-1} in the denominator is a stock variable.[11] Correspondingly, we also calculate a hiring rate relative to lagged employment, i.e., $hiring\ rate_{it} = \frac{hires_{it}}{N_{it-1}}$. While the separation, quit, and layoff rates are strictly between zero and one, the hiring rate can be above one if a firm hires more employees than the last year's employment stock. However, our results are not sensitive to these outliers.[12]

[Table 8 about here]

The results in Table 8 show that the negative employment effect can be explained by a reduction in the hiring rate but also by an increase in the separation rate.[13] While the treatment effects are not very precise, the hiring response seems to play a more important role for employment adjustments. However, the placebo tests are fragile when looking at the effects on the hiring and separations rates, i.e. the placebo effects are large compared with the treatment effects. To show some more insights on whether employers use hires or separations to adjust their labor demand, we use some additional descriptive information that we collected in the 2015 wave of the IAB Establishment Panel. The survey asks employers about adjustment measures in response to the minimum wage. Two of these measures directly capture employment adjustments by asking whether the minimum wage caused them to be more cautious in hiring worker or caused them to lay off workers. The survey allows for three different outcome categories (a) carried out (b) intended, and (c) not relevant. We included the second category to capture socially desirable responses of employers who dislike the minimum wage, but have not yet adjusted employment. Of course, this still does not allow for causal conclusions, but we use it to assess the relative importance of the two adjustment channels.

[Table 9 about here]

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[11] In the data at hand the stock variables (i.e., the employment level) are measured to June 30th of each year, while the hiring and separation flow variables are only surveyed for the first six month of the year. We edit the flow variables to yearly measures, which correspond with the total employment change.

[12] In the turnover literature, turnover rates are mostly divided by the average of the contemporary and the previous year's employment stock (e.g., Burgess, Lane and Stevens 2000). However, we avoid using the contemporary employment stock as this endogenously changes due to the minimum wage, see Section 6.

[13] Technically, the hiring and separation responses do not exactly add up to the employment effect identified in Section 6. This is because the dependant variables are rates, while the employment effect is estimated in log points. As there are many zeros in hires and separations logarithmic dependent variables are infeasible.

The descriptive averages are displayed in Table 9. About 10.1 percent of the affected employers report that they have been cautious in hiring new workers and only 4.4 percent reported that they have laid-off workers. The relative importance of these two adjustment channels supports our result that hires are the dominant channel for employment adjustments.

Turning back to the empirical analysis of Table 8, the data allow us to differentiate between different sources of separations, which are employee-initiated quits, employer-initiated layoffs, and a residual category, comprising of retirements, expiring fixed term contracts, and non-takeover of apprentices. We calculate rates for quits, layoffs, and other separations, which sum up to the overall separation rate. Therefore, the coefficients presented in columns (3) to (5) of Table 8 also add up to the separation response in column (2). The estimated treatment effects show no effect on the quit rate for which the coefficient is zero (column 3), but slightly positive coefficients on the layoff rate and the rate of all other separations. While the estimates are not very precisely estimated, it seems that separations are rather driven by employer-initiated layoffs and residual sources than by employee initiated quits.

Besides of employment adjustments through specific channels, minimum wages may affect labor turnover irrespective of adjustments in the employment levels. However, it is not clear whether a reduction or increase of turnover is economically desirable. On the one hand, reduced labor mobility implies a loss in economic efficiency (Hyatt and Spletzer 2013). On the other hand, reduced employee turnover mirrors job security by longer average job retention, which is a desirable job characteristic to most employees.

For our analysis, we first calculate a general turnover rate relative to the previous year's employment:

$$Gross\ turnover_{it} = \frac{hires_{it} + separations_{it}}{N_{it-1}} \qquad (4)$$

Equation (4) is the gross turnover rate (Davis and Haltiwanger 1999), but this rate could also change due to adjustments in the employment level, which may be due to the minimum wage. Therefore, we not only look at the gross turnover rate, but also at an employment neutral turnover rate, which is commonly known and defined as churning (Davis and Haltiwanger 1999):

$$Churning_{it} = \frac{hires_{it} + separations_{it} - |hires_{it} - separations_{it}|}{N_{it-1}} \qquad (5)$$

Table 10 displays the results for both turnover variables. Column (1) shows that the effect on the gross turnover rate is inconclusive and virtually zero. Column (2) shows the effect on the

employment neutral churning rate. The estimated treatment effect shows a reduction of the churning rate by about 2.9 percentage points. This implies that labor turnover would have decreased if there were no employment effects induced by the minimum wage.

The negative effect on churning corresponds with findings in the literature (Brochu and Green 2013; Dube, Lester and Reich 2016; Gittings and Schmutte 2016; Portugal and Cardoso 2006). In size, the negative effect of about 2.9 percentage points on churning corresponds with Bachmann, Penninger, and Schaffner (2015) when they compare treated establishments with an unaffected control group of the same sector.

[Table 10 about here]

9.   Other adjustment margins

Additional to changes in wages and employment, we analyze two additional adjustment margins: hours of work and freelance employment. We estimate separate effects on both of these outcomes. Since hours of work are reported for a typical full-time worker in the employed workforce, it measures an effect that is supplementary to the employment effect. Of course, this is only a crude measure for working time. Nevertheless, it points an interesting margin of adjustments. Since freelance employment is also not included in the number of employees, again any effect would be independent of the employment effects presented in Section 6.

Theoretically, hours of work might fall in response to the minimum wage because of work sharing (Couch and Wittenburg 2001), which implies that a reduced volume of work is shared among the workforce affected by the minimum wage. Additional to the work sharing argumentation, hours of work could be a channel of non-compliance. This is the case if working hours are reduced to increase hourly wages, while unpaid overtime hours are used in compensation. Unfortunately, we cannot identify unpaid overtime work in our data. Therefore, we cannot distinct between the two suggested channels of working hour reductions.

The empirical evidence of a working hours effect is similarly divided as the literature on employment effects. For the famous case study comparison of the New Jersey and Pennsylvania minimum wages, Michl (2000) argues that a negative working hours adjustment could be an explanation for diverging results. Moreover, Neumark, Schweitzer and Wascher (2004) as well as Couch and Wittenburg (2001) find a negative hours adjustment from US state level data. At the same time, Zavodny (2000) does not find effects from micro data of teens. For the UK minimum wage Stewart and Swaffield (2008) detects a negative effect on hours of low wage workers, while

Connolly and Gregory (2002) do not find a negative change for female workers, who are usually due to frequent working hours adjustments.

Another margin of adjustment is freelance employment. Freelancers are self-employed individuals, who receive a contract for a specified service. As they are self-employed without an employment contract, the minimum wage does not apply. We analyze whether the use of freelancers spikes at affected establishments, which would suggest a way to circumvent the minimum wage.

The estimates in columns (1) and (2) of Table 11 show a reduction in the typical contracted working hours at the establishment level of 0.2 hours per week, which corresponds with a 0.6 percent decrease in typical contracted hours. In columns (3) and (4), we present effects on freelance employment. We differentiate between the incidence of freelance employment and the fraction of freelancers among the total number of employees plus freelancers. The treatment effects on both of these outcomes are virtually zero. As freelance employment does not increase, we do not observe hints towards a circumvention of the minimum wage or a substitution of regular employment.

[Table 11 about here]

10. Conclusion

We analyze employment effects of the new statutory minimum wage in Germany, which was introduced on 1 January 2015. We identify employment effects from a difference-in-differences comparison of affected and unaffected establishments. The IAB Establishment Panel allows us to define the minimum wage bite of establishments from the 2014 panel wave and includes outcome variables such as average wages, employment levels, labor flows, typical contracted working hours, and freelance employment.

We observe a treatment effect on the treated establishments, which shows a sharp increase in average wages by about 4.8 percent and a decrease in the affected establishments' employment by about 1.9 percent. In combination, these estimates imply an employment elasticity with respect to wages of about -0.3, which represents a modest negative employment elasticity.

When we relate the negative employment effect of 1.9 percent to the population of represented employees in the treatment group, which are 3,090,626 employees (Table 1), we conclude that about 60,000 additional workers could be employed in the absence of the minimum wage. While

this by far does not correspond with the most pessimistic projections, it still shows a meaningful job loss induced by the minimum wage in Germany. Compared with the descriptive governmental monitoring of transitions between employment states in the months around the minimum wage introduction (vom Berge, et al. 2016) our effect size falls in the range of plausible results. Moreover, we present first causal evidence that the minimum wage may in fact result in a trade-off between benefiting a large number of employees from higher wages at the expense of risking jobs of a much smaller number of employees.

Robustness checks, which control for treatment group specific trends, do not reveal any differences for the presented effects. Effect heterogeneities show a much larger negative employment effect when employers had to conduct compensating cuts of extra payments. By contrast, the employment effect shrinks towards zero when employers were able to increase wages that were initially already above € 8.50.

When we study the minimum wage effect on labor flows, we observe that the employment effect is largely driven by a reduction in hires but also by a slight increase in separations. Moreover, corresponding with recent literature, the employment neutral turnover rate seems to decrease. Additional to the employment effect, we also observe a reduction in the typical contracted working hours. This suggests that the intensive margin of employment is an additional adjustment margin. Finally, we look at freelance employment, as this could be a way to circumvent the minimum wage. However, from the data we do not observe an increase in the incidence or the share of freelance workers.

We admit several limitations of our analysis: First, our data omits black market employment. Therefore, it is possible that the employment reduction led to a compensating increase in black market employment. Second, we only observe short-run effects of the minimum wage introduction to 30 June 2015. Hence, long-run effects might differ and should be addressed in future research. This is of particular relevance because effects of the new minimum wage may differ in an economic downturn. If average productivity decreases during a recession, employment effects are potentially larger. Third, we cannot identify establishment closure in our data. As we only look at employment of surviving establishments, our estimates may be a lower bound of the true effect.

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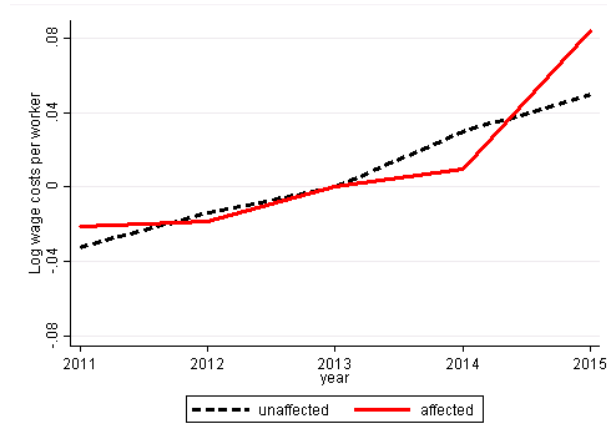
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Figures and Tables

Figure 1: Wage and employment time series by affectedness

Panel A: Log wages per worker   Panel B: Log employment
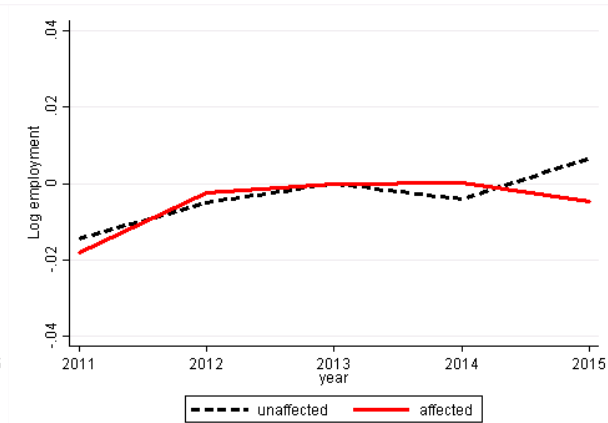
Notes: Panels A displays the time series of the log wages per worker by affectedness of establishments. Panel B displays the aggregated time series of log employment by affectedness. Both time series are centered at the values of 2013.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Figure 2: Bite of the minimum wage across Germany

Panel A: Affected establishments   Panel B: Intensive margin affectedness
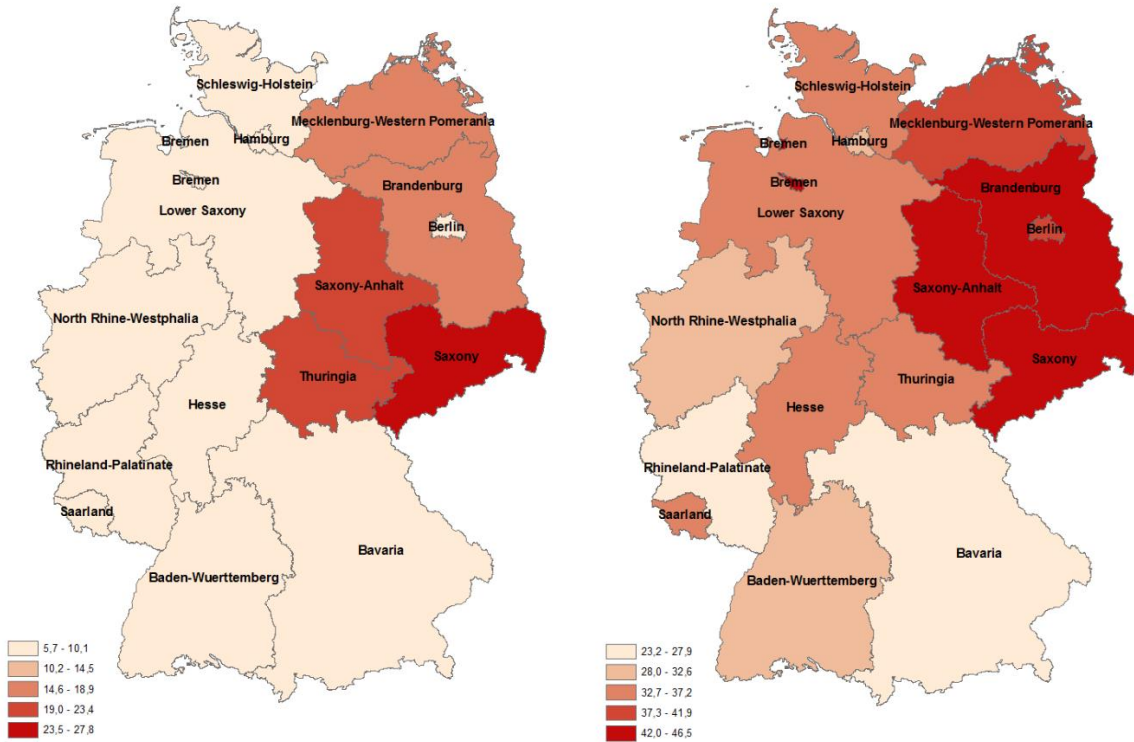
Notes: Panel A illustrates the average establishment-level affectedness by the minimum wage across German states in percent. Affected establishment have at least one employee with an hourly wage below € 8.50 in 2014. Panel B displays the average intensive margin affectedness across German states in 2014, which is the fraction of workers with an hourly wage below € 8.50 at affected establishments, in percent.

Data source: IAB Establishment Panel 2014, analysis sample.

Table 1: Analysis sample description

	(1) Analysis sample	(2) Treatment group	(3) Control group
Establishments and employees in the analysis sample:			
Establishments	13,453	1,599	11,854
Avg. establishment size	123.8	65.3	131.7
Median establishment size	17	16	17
Represented establishments in the population	1,873,200	179,042	1,694,158
Represented employees in the population	32,027,189	3,090,626	28,936,563
Analysis sample averages:			
Extensive margin affectedness	0.119	1	0
Intensive margin affectedness	0.044	0.378	0
Log employment in 2014	3.002	2.872	3.019
Log wages per worker in 2014	7.377	6.931	7.441

Notes: The upper part of the table provides an overview on the number of establishments and the number of employees represented in the sample and the gross population. The lower part shows descriptive sample averages of the major variables for the analysis sample. Column (1) covers the analysis sample, column (2) covers the treatment group, and column (3) covers the control group.

Data source: IAB Establishment Panel 2014, analysis sample.

Table 2: Wage effects, employment effects, and employment elasticities

	Wage effect	Employment effect	Employment elasticity
	(1)	(2)	(3)
	Log wages per worker	Log employment	IV
Panel A: Extensive margin effects (0/1)			
ToT$_{DiD}$	0.048	-0.019	-0.396
	(0.010)	(0.008)	(0.182)
Placebo$_{DiD}$	-0.016	0.0002	
	(0.009)	(0.0072)	
Panel B: Intensive margin effects [0,1]			
ToT$_{DiD}$	0.117	-0.035	-0.299
	(0.024)	(0.021)	(0.194)
Placebo$_{DiD}$	-0.006	-0.006	
	(0.022)	(0.016)	
Panel C: Differing treatment intensities			
ToT$_{DiD}$ *0 < a ≤0.2* *(522 establishments)*	0.030 (0.012)	-0.018 (0.009)	
ToT$_{DiD}$ *0.2< a ≤0.4* *(339 establishments)*	0.048 (0.021)	-0.015 (0.015)	
ToT$_{DiD}$ *0.4< a ≤0.6* *(297 establishments)*	0.083 (0.026)	-0.024 (0.019)	
ToT$_{DiD}$ *0.6< a ≤0.8* *(220 establishments)*	0.059 (0.030)	-0.045 (0.024)	
ToT$_{DiD}$ *0.8< a ≤1* *(156 establishments)*	0.089 (0.034)	0.005 (0.034)	
Observations	41,870	51,381	51,381
Establishments	11,835	13,432	13,432

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. Cluster robust standard errors are in parentheses (cluster=establishment). Control variables are collective bargaining, works councils, female share, part-time share, and dummies for each panel year.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 3: Establishment closure in 20115 by affectedness

	(1)	(2)
	Baseline	Controlling for initial size, profitability and the technical state of the capital stock
0 < a ≤0.2	-0.016	-0.009
(522 establishments)	(0.008)	(0.009)
0.2< a ≤0.4	-0.010	-0.013
(339 establishments)	(0.009)	(0.009)
0.4< a ≤0.6	0.020	0.010
(297 establishments)	(0.010)	(0.010)
0.6< a ≤0.8	0.018	0.010
(220 establishments)	(0.011)	(0.011)
0.8< a ≤1	0.028	0.026
(156 establishments)	(0.014)	(0.013)
Establishments	11,237	11,237

Notes: Coefficients are partial effects from a linear probability model explaining the incidence of firm closure between the 2014 and 2015 survey collection of the IAB Establishment Panel. Controls as in Table 2. Further controls in column (2) are the initial 2014 establishment size measured by 10 categoriesies (0-4, 5-9, 10-19, 20-49, 50-99, 100-199, 200-499, 500-999, 1000-1999, and 2000+ employees), the profitability (5 categories), and the technical state of the capital stock (5 categories).

Data source: IAB Establishment Panel 2014, analysis sample. The sample size shrinks as we include further control variables.

Table 4: Minimum wage effects controlling for group specific trends

	Specification with treatment group specific linear time trends		Specification with treatment group specific linear and quadratic time trends	
	(1)	(2)	(3)	(4)
	Log wages per worker	Log employment	Log wages per worker	Log employment
ToT$_{DiD}$	0.062	-0.018	0.079	-0.019
	(0.013)	(0.009)	(0.021)	(0.013)
Observations	41,870	51,381	41,870	51,381
Establishments	11,835	13,432	11,835	13,432

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. Columns 1 and 2 include a linear treatment group specific time trend. Columns 3 and 4 include a linear and quadratic treatment group specific time trend. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 5: Minimum wage effects controlling for spillovers

	Without spillovers		Plants with wages increases above € 8.50		Plants with wages cuts or cuts of extra payments	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log wages per worker	Log employment	Log wages per worker	Log employment	Log wages per worker	Log employment
ToT_{DiD}	0.047	-0.024	0.029	0.001	0.077	-0.099
	(0.012)	(0.009)	(0.024)	(0.018)	(0.058)	(0.041)
Observations	39,554	48,614	1,973	2,369	419	491
Establishments	11,222	12,760	519	570	115	126

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 6: Minimum wage effects controlling for indirect affectedness

	Not indirectly affected by the minimum wage		Indirectly affected by the minimum wage	
	(1)	(2)	(3)	(4)
	Log wages per worker	Log employment	Log wages per worker	Log employment
ToT_{DiD}	0.048	-0.013	0.039	-0.031
	(0.014)	(0.010)	(0.016)	(0.014)
Observations	29,111	35,557	6,002	7,181
Establishments	7,690	8,540	1,582	1,730

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 7: Effect heterogeneities for Eastern and Western Germany and by competition

	(1)	(2)	(3)	(4)
	Log wages per worker	Log employment	Log wages per worker	Log employment
Panel A: Effects for Eastern and Western Germany				
	Eastern Germany		Western Germany	
ToT$_{DiD}$	0.052	-0.017	0.034	-0.008
	(0.013)	(0.010)	(0.016)	(0.012)
Observations	16,501	19,869	25,369	31,512
Establishments	4,462	5,004	7,373	8,428
Panel B: Effects by product market competition				
	High competition		Medium or low competition	
ToT$_{DiD}$	0.053	-0.024	0.046	-0.014
	(0.029)	(0.022)	(0.011)	(0.008)
Observations	4,769	5,754	36,917	46,384
Establishments	2,586	3,008	11,210	12,808

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 8: Minimum wage effect on hires and separations

	Hires	Separations			
	(1)	(2)	(3)	(4)	(5)
	Hiring rate	Separation rate	Quit rate	Layoff rate	Other separations' rate
ToT_{DiD}	-0.017	0.009	-0.000	0.003	0,007
	(0.011)	(0.009)	(0.004)	(0.005)	(0.004)
$Placebo_{DiD}$	-0.006	-0.008	0.001	-0.002	-0.003
	(0.011)	(0.010)	(0.005)	(0.004)	(0.005)
Observations	51,145	51,145	51,145	51,145	51,145
Establishments	13,420	13,420	13,420	13,420	13,420

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 9: Descriptive use of hires and separations

	Carried out	Intended	Not relevant
Due to the minimum wage, have you been …			
cautious in hiring workers	10.1 %	4.3 %	85.6 %
laid-off workers	4.4 %	1.5 %	94.1 %

Notes: Descriptive average over all affected establishments as defined in the analysis (N=1,240).

Data source: Questions 67 a) and b) of the 2015 IAB Establishment Panel, analysis sample.

Table 10: Minimum wage effect on employee turnover

	Employment turnover	
	(1)	(2)
	Gross turnover	Churning (employment neutral turnover)
ToT$_{DiD}$	-0.004	-0.029
	(0.011)	(0.014)
Placebo$_{DiD}$	0.005	0.001
	(0.013)	(0.019)
Observations	50,932	50,758
Establishments	13,402	13,386

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.

Table 11: Minimum wage effect on contracted working hours and the employment of freelancers

	Hours adjustment		Freelancers	
	(1)	(2)	(4)	(5)
	Contracted working hours	Log contracted working hours	D$_{Freelancers>0}$	Fraction of freelancers
ToT$_{DiD}$	-0.209	-0.006	-0.003	0.0002
	(0.056)	(0.002)	(0.007)	(0.0012)
Placebo$_{DiD}$	-0.024	-0.001	-0.003	-0.0001
	(0.047)	(0.001)	(0.007)	(0.0014)
Observations	50,025	50,025	50,954	50,954
Establishments	13,270	13,270	13,410	13,410

Notes: Coefficients are Treatment effects on the treated from difference-in-difference specifications with establishment-level fixed effects. For further notes, see Panel A of Table 2.

Data source: IAB Establishment Panel 2011-2015, analysis sample.