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Estimating employment effects of the German minimum wage

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Abstract

This thesis uses individual-level data from the Socio-Economic Panel (SOEP) to examine the effect of the introduction of the German minimum wage on the employment retention probabilities of those directly affected: individuals whose wages would have had to be raised to comply with the minimum. The findings suggest significant disemployment effects of the minimum wage on this group. For full-time and part-time employees the estimated reduction in the employment probabilities is roughly between 3 and 5.5 percentage points. The estimates further increase when only those are considered for whom the required wage raises were particularly large. Since there is a substantial degree of non-compliance with the minimum wage, the sample is further restricted to "compliers": the estimated reduction in subsequent employment probabilities increases to up to 13.5 percentage points for individuals in regular employment and up to almost 18 percentage points for the marginally employed. A separate estimation of aggregate employment effects further suggests that the impact of the minimum wage is highly heterogenous across industries.

Keywords: minimum wage, employment retention probabilities, employment effects

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Introduction

Effective January 1, 2015, Germany has introduced a statutory minimum wage of 8.50 €. The introduction of a legal minimum was preceded by a controversial debate. Despite mixed international evidence, economists overwhelmingly predicted sizable employment losses (SVR 2014; Knabe, Schöb, and Thum 2014; Henzel and Engelhardt 2014). Thus far, those pessimistic predictions have not been confirmed by the empirical evidence. While evidence for disemployment effects has been found, these are much smaller than what had been anticipated (Bossler and Gerner 2016; Garloff 2016; Caliendo et al. 2017a).

While these studies rely on aggregate (Caliendo et al.; Bossler and Gerner) or firm-level data (Garloff), I use individual-level data from the Socio-Economic Panel (SOEP) to examine employment effects of the minimum wage. Specifically, I estimate the effect the minimum wage had on the probability of remaining in employment of those individuals for whom wages would have had to be raised to comply with the minimum wage. Thus, I focus my analysis on a group that is of particular interest: individuals whose jobs are directly impacted by the introduction of minimum wage.

I estimate a model of employment transition probabilities that compares employment outcomes of the directly affected to those with a wage marginally above in a probability difference-in-differences specification. Concerns regarding the functional form when the outcome variable is binary are addressed by estimating both linear and non-linear models. In addition, I use fixed effect estimators to control for individual heterogeneity, thereby exploiting the panel nature of the data.

After the main analysis, I present an extension of the recent study of Caliendo et al. (2017a). Following Card (1992b), the authors exploit regional variation in the bite of the minimum wage to estimate its effect on aggregate employment. I construct an alternative bite measure that accounts for the regional shares in employment of a given industry and re-estimate the resulting model separately for four different industry aggregations.

The thesis is organized as follows. Section 2 briefly discusses the economic theory concerning employment effects of minimum wages and reviews the literature most relevant for the analysis. Section 3 describes the institutional background of the German minimum wage and reviews existing evidence on employment effects. Section 4 describes the data and lays out the estimation strategy. Section 5 discusses the main results and robustness checks. Section 6 presents the extension to the study of Caliendo et al. Section 7 concludes.

Theory and evidence: A brief discussion

2.1 Frictions in the market

The neoclassical theory of the labor market leaves no room for interpretation when it comes to the employment effects of a minimum wage. In a complete market where firms are price-takers and information asymmetries are absent, workers are paid according to their marginal productivity. Based on this premise, a minimum wage necessarily causes disemployment effects. All those workers whose wages fall below the minimum wage will inevitably lose their job. Since they were previously paid their marginal product, the minimum wage would require employers to pay them a wage in excess of their productivity. As firms maximize profits, this leads to the termination of the employment contract.

Until the early 1990s it was indeed widely undisputed in the literature that minimum wages have negative employment effects. This has changed with the emergence of the so-called "new minimum wage research". This literature was initiated by a number of studies that exploited state-level variations in the U.S. minimum wage as natural experiments and found insignificant or even positive employment effects of regional minimum wage increases.[1] These findings sparked enormous controversy among economists, many of whom subsequently began to view labor markets through the lens of imperfect competition.

Manning (2011) lists a variety of reasons to demonstrate that it is probably best to think of the labor market as a market with frictions. Importantly, frictions cause the existence of rents in an employment relationship. This implies that both the employer and the worker benefit from the relationship being upheld. Dissolving an employment contract with an existing worker and hiring a new one typically incurs costs for the employer, such as recruitment or training costs. Similarly, workers have to invest time and effort to find a job. Thus, contrary to the assumptions made in the neoclassical theory, employers cannot find an equivalent worker to an equivalent wage at no cost, and vice versa. Frictions prevent matches between workers and firms to occur efficiently.

[1] The probably most influential contribution in this literature is Card and Krueger (1994), who investigate a raise of the minimum wage in New Jersey by comparing employment in fast-food restaurants in New Jersey before and after the intervention to employment in fast-food restaurants in the neighboring state Pennsylvania, where there was no change in the minimum wage. The authors find a positive employment effect of the minimum wage and, en passant, popularized the now standard difference-in-differences approach.

The effect of a minimum wage then crucially depends on how these rents are divided between employers and workers, i.e. on their respective bargaining power. If an employer enjoys rents this implies that the marginal productivity is above the wage. In this case a minimum wage does not lead to the determination of the employment contract as long as it lies at or below the marginal productivity of the worker. It merely redistributes the rents from the firm to the worker. The greater the share of the rents the firm receives, the higher a minimum wage can be without incurring job loss. This likely differs across labor markets, industries and skill-groups. Particularly for low-wage sectors, however, the assumption that much of the rents is enjoyed by the employer does not seem implausible. Low-wage (and often low-skilled) workers are more likely to be insufficiently informed about potential earnings in other firms, or be constraint in moving to another location where earnings would be higher. Moreover, idiosyncratic preferences may lead them to accept wages below their marginal product. Examples would be the atmosphere at the work-place, preferences over working hours or commuting time (cf. Manning 2011).

There are only few empirical applications that seek to estimate the degree of worker and firm bargaining power. Cahuc, Postel-Vinay, and Robin (2006), for example, exploit matched employer-employee data from France to estimate an equilibrium search model where wages are determined according to Nash bargaining.[2] They find bargaining power estimates close to zero for low-skilled workers in France. For Germany, Hirsch, Schank, and Schnabel (2010) follow methods proposed by Manning (2003) and allow for wage-setting power on the side of firms by estimating a dynamic monopsony model[3] with the IAB's Linked-Employer-Employee-Data (LIAB).[4] They estimate that male workers would earn between 27.4 and 40.2 percent more were they paid their marginal product, whereas the gap for female workers is between 38.7 and 53.6 percent.

The purpose of this brief (and certainly incomplete) discussion was to demonstrate that the employment effects of a minimum wage cannot be determined ex-ante and crucially depend on the relationship between productivity and wages. A minimum wage is expected to cause the determination of a given worker-firm match only if its level exceeds the productivity of the match. However, existing evidence suggests that the gap between wages and productivity can be substantial, particularly in low-wage sectors, implying that the minimum wage could potentially redistribute rents to workers without causing significant employment losses.[5]

[2] See Mortensen and Pissarides (1999) for an overview of this literature.

[3] For a survey of the literature on monopsony or monopsonistic competition in the labor market see Bhaskar, Manning, and To (2002).

[4] The LIAB matches the IAB Establishment Panel with individual data from the Integrated Employment Biographies (IEB), which allows tracking the daily employment status of an individual. It is administered by the German Institute for Employment Research. For more information, see http://www.fdz.iab.de/.

[5] A further point that makes the ex-ante prediction of minimum wage effects problematic is that minimum wages also affect labor supply. Specifically, higher wage offers induced by the minimum wage can attract individuals to the labor market whose reservation wage was not met by previous, lower offers. For details, see the the referenced literature on search and monopsony models. When employment retention

2.2 Review of related literature

Overall, the empirical literature on minimum wages remains inconclusive. In the U.K., there is a broad consensus that the introduction and subsequent raises of the national minimum wage has not had adverse employment effects (Manning 2013; Metcalf 2008).[6] In a series of papers by Card and co-authors, no evidence for negative effects of minimum wage raises on teenage employment in the U.S. were found, a group that is considered particularly vulnerable to adverse employment effects given the prevalence of low wages and low skills (Card 1992b; Card 1992a; Card, Katz, and Krueger 1993). By contrast, Neumark and Wascher (2007) provide an extensive review of the literature that had emerged since the 1990s and conclude that a vast majority of the studies still find negative employment effects, albeit not always statistically significant.

Most of the minimum wage literature, however, focuses on aggregate employment effects. A much smaller number of studies is based on individual-level data, as is the case in the main part of my analysis. In the following, I briefly present a selection of these studies.

Currie and Fallick (1996) use National Longitudinal Survey of Youth (NLSY) data from the U.S. to investigate the impact of two increases in federal minimum wage that took place in the early 1980s. They follow a sample of individuals over the period 1979-1987. To estimate the impact the minimum wage raises had on the employment retention probabilities of affected young workers, they create two measures to determine whether an individual was affected by a given raise. The first is a GAP variable that equals the amount by which the wage of a worker in the base year would have to be raised to comply with the minimum. If a worker earned less than, more than or exactly the minimum wage the variable is set to zero. Since employment probabilities based on the GAP measure may simply reflect that individuals with very low wages (and thus a higher gap) are less likely to keep their job for reasons unrelated to minimum wage increases, they also use a binary BOUND variable that is one if an individual is affected by the minimum wage increase and zero otherwise.[7] The comparison group consists of *all* non-affected workers in the sample. To somewhat mitigate the resulting problem of making a simple high-wage versus low-wage comparison, they add individual fixed effects to net out individual heterogeneity, thereby exploiting the entire sample period. Overall, they find that individuals affected by

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probabilities are estimated labor supply effects are no concern.

[6] Solely Machin, Manning, and Rahman (2003) find negative employment effects of the national minimum wage in the residential care home industry. However, the authors argue that the effects are small relative to the high fraction of individuals earning less than the minimum prior to its introduction (30 percent). Moreover, strict regulations prevented employers from resorting to other adjustment mechanisms such as raising prices.

[7] Furthermore, one would expect the GAP variable to be highly correlated with wage growth for those who remain employed. The fact that this is not always the case suggests further problems with this measure. See also the corresponding discussion of this paper in Card and Krueger (1995).

a minimum wage raise were about 3 percent less likely to be employed the following year.

Similarly, Abowd et al. (2000) exploit the size of changes in the real minimum wage to categorize workers as "between" old and new values of the real minimum wage. Using longitudinal data from both France (Enquête Emploi, 1990-1998) and the US (Current Population Survey, 1981-1991), they examine whether workers directly affected by real minimum wage changes have significantly different employment probabilities. In contrast to Currie and Fallick (1996), the control group is restricted to unaffected workers with wages close to the minimum wage. Since they observe both increases and decreases in the real minimum wage, they are able to consider both exit from and entry to employment. That is, they estimate the probability of remaining employed conditional on previous employment for increases in the real minimum wage as well as the probability of previous employment conditional on current employment for decreases in the minimum wage. They find that entry into employment is not sensitive to changes in the real minimum wage in either country. Exit from employment is also insensitive to real minimum wage changes in the U.S., whereas there are strong negative effects in France. The corresponding difference-in-differences elasticity of employment retention with respect to changes in the minimum wage is -2 for men and -1.5 for women.

Most closely related to my own analysis is Stewart (2004).[8] To the best of my knowledge, this is the only study of this kind that examines the *introduction* of a minimum wage. Stewart estimates how employment retention probabilities of low-wage workers in the U.K. have changed with the introduction of the minimum wage in 1999. He uses three different longitudinal data sets, all of which are either panels or matched cross-sections. He estimates his model on data starting several years before the introduction and ending shortly after, where the pre-treatment period serves as a control for waves where there was no minimum. Treatment and control group in each period are defined according to their relative position in the wage distribution, where the control group is limited to individuals with wages slightly above the minimum. Using both a binary indicator and a wage gap measure, he finds no significant effect of the minimum wage on employment.

Finally, Campolieti, Fang, and Gunderson (2005) and Yuen (2003) apply the methodology of Currie and Fallick (1996) to Canadian data. Yuen (2003) refines the approach by restricting the comparison group to low-wage workers in unaffected provinces, exploiting the fact that minimum wages in Canada considerably vary across regions. Moreover, he distinguishes low-wage workers according to their employment history. He finds that teenagers with more than three quarters of low-wage employment between 1988 and 1990 are 7 percent less likely to remain employed after a 8.4 increase in the minimum wage. Using the same approach, but different data in a later time period (1993-1999), Campolieti, Fang, and Gunderson (2005) find that the probability of continued employment decreased in the range of 4 to 8 percentage points for affected youth after minimum wage increases.

[8] The most crucial difference to my approach is that Stewart does not control for individual fixed effects.

The German minimum wage

3.1 Institutional Background

On January 1, 2015 the universal minimum wage in Germany came into effect. The law was passed in August 2014 with the entry level of the minimum wage being set at 8.50 € (Bundestag 2014b). Every two years, the independent minimum wage commission recommends an appropriate adjustment of the minimum wage, which can then be put into effect by the secretary of labor.[1] Effective January 1, 2017 the minimum wage was raised to 8.84 € upon recommendation of the minimum wage commission (Bundestag 2016).

Prior to its introduction there was no legal wage floor with the exception of a number of sectoral minimum wages. These were bilaterally negotiated by employer representatives and unions and then declared legally binding for the entire industry by the German secretary of labor.[2]

The minimum wage applies to almost all employees, with very few exceptions. Exempted are the self-employed, individuals in vocational training, individuals under the age of 18 who have not completed a vocational training, interns if the internship is compulsory as part of a university curriculum or lasts no longer than three months as well as the long-term unemployed for a maximum of six months. Additionally, few sector have been granted an extended transition period during which the minimum wage can be undercut. This applies under the condition that a legally binding sectoral minimum wage is in place and that the level of the universal minimum wage is reached by January 2018 (Bundestag 2014a).[3]

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[1] If the secretary of labor rejects the recommendation, the old minimum wage level continues to apply. Thus, politically motivated minimum wage changes are not possible (Bundestag 2014a).

[2] This stands in contrast to standard sectoral wage agreements which only extend to employers who voluntarily commit to it. See, for example, Bispinck and Schulten (2010) for a a discussion of sectoral wage agreements and minimum wages. Sectoral minimum wages exist, for example, in the construction sector (since 1997), the roofing sector (since 1997) or in the painting and varnishing trade (since 2003) (BMAS 2018).

[3] Sectors that made use of this possibility include, for example, the meat industry or agriculture and forestry (BMAS 2018).

3.2 Previous findings

In 2014, roughly 4.1 million individuals who would have been eligible to the minimum wage earned a wage below 8.50 €. This corresponds to around 13.3 percent of all employees.[4] About half of these potential minimum wage beneficiaries were in marginal employment. Among these so-called mini-jobbers low wages are particularly prevalent. In 2014, more than half earned less than 8.50 € (see section 4.1).[5] To put this in an international context, the share of minimum wage workers in the United Kingdom has been relatively stable at around 5 percent since its introduction in 1999 (Manning 2013). The share of workers with wages at the minimum has been in a similar range in the United States over the past several years (David, Manning, and Smith 2016). In France, 1.65 million workers earned the minimum wage in 2017, which corresponds to about 10.6 percent (Pinel 2017).

Given the relatively high fraction of affected workers in Germany, large employment losses were predicted prior to the introduction of the minimum wage, in particular for marginal employment. Estimates ranged from a predicted reduction in overall employment of at least 200,000 in the short-run (Gemeinschaftsdiagnose 2014) to predicted job losses of 250,000 in regular employment (full-time or part-time) and 650,000 in mini-jobs (Knabe, Schöb, and Thum 2014).

Thus far, employment losses of this magnitude have not been confirmed by the empirical literature. Bossler and Gerner (2016) use data from the IAB establishment survey[6] to compare firms affected by the minimum wage to those that were unaffected in a difference-in-differences specification. They find that roughly 60,000 more workers could be employed absent the minimum wage.

Applying a difference-in-differences specification as well, Garloff (2016) employs administrative data from the Federal Employment Agency to construct region-age-sex cells, which he then compares based on their level of affectedness, or, equivalently, the "bite" of the minimum wage (i.e. the share of workers with a wage below 8.50 €).[7] He finds a *positive* effect of the minimum wage on regular employment in the magnitude of 78,000 jobs, and a reduction in mini-jobs of around 66,000. Garloff interprets this finding as the result of a politically favored shift from marginal to regular employment, which is

[4] Own calculations, based on SOEP data.

[5] In Germany marginal employment is a special form of employment contract with maximum monthly earnings of 450 Euro. These employees are exempted from health and unemployment insurance. Taxes and social security contributions are very low and paid by the employer in a lump sum. For the employee, gross and net earnings are virtually identical.

[6] The IAB Establishment Panel is a representative employer survey covering 16,000 establishments. It is administered by the German Institute for Employment Research. For more information, see http://www.iab.de/en/erhebungen/iab-betriebspanel.aspx.

[7] However, Garloff's bite measure suffers from the problem that it is calculated based only on regular employment. Since marginal employment is much more strongly affected, the true bite is likely underestimated.

subject to regular taxes and social security contribution.[8] Descriptive evidence for such dynamics is provided by Vom Berge et al. (2017). They document that between December 2014 and January 2015 marginal employment decreased by 3.3 percent (160,000 jobs), of which 1.9 percent (92,500 jobs) cannot be explained by seasonal patterns. However, around 50 percent of the lost jobs in marginal employment were transformed into regular employment.

Similarly, Caliendo et al. (2017a) follow Card (1992b) and exploit regional variation in the bite of the minimum wage to identify employment effects. They find that regular employment was reduced by approximately 78,000 jobs. When pre-treatment trends are taken into account, which had not been considered by the studies described above, this effect becomes weakly significant or insignificant. The estimated loss in marginal employment amounts to roughly 183,000. However, the authors are cautious with respect to the magnitude of this finding given that they find evidence for a negative pre-treatment trend in marginal employment. The study of Caliendo et al. (2017a) will be discussed in detail in section 6.

[8] Such as shift can occur, for example, when mini-jobbers, whose monthly earnings exceed 450 € due to a minimum wage induced wage raise, transition into part-time employment.

Estimation strategy

4.1 Data

The data I use is from the German Socio-Economic Panel (SOEP), administered by the German Institute for Economic Research (DIW Berlin). The SOEP is a representative, longitudinal household survey that started in 1984 and covers around 30,000 individuals in almost 11,000 households. The panel is characterized by a high degree of attrition and offers a wide range of information on the individuals in the sample. Interviews are conducted once a year with the majority of interviews taking place in the first months of the year.

For my analysis, I use data from the waves 2011 to 2016, which is the latest available survey year. I restrict my sample to prime age workers between the age of 18 and 65. Furthermore, I exclude all those individuals who are not subject to the minimum wage. This group includes the self-employed, individuals in apprenticeships, the long-term unemployed as well as interns.[1] I also exclude individuals in so called 1-Euro-jobs[2] as well as those who state to work more than 50 hours per week due to possible measurement error. Individuals to whom a sectoral minimum wage already applies are excluded from the sample as well.[3] Furthermore, I exclude individuals interviewed in January. This is because individuals are asked about their earnings in the month preceding the interview to determine their gross monthly income, which corresponds to the previous year for individuals interviewed in January. Finally, I winsorize wages at the first and 99th percentile to prevent outliers biasing the results.[4]

Hourly wages in the SOEP have to be constructed from the individuals' monthly income and their hours worked per week. Respondents are asked to report their weekly hours in two different ways. That is, they are asked to report both their *contractual*

[1] Whether an intern is eligible to the minimum wage depends on the exact duration of the internship. However, as the duration of an internship cannot be unequivocally established with the data at hand, I exclude interns entirely from the analysis.

[2] 1-Euro-jobs were an employment measure by the government with the aim of integrating the long-term unemployed into the labor market.

[3] I do this since sectoral minimum wages are either higher than the minimum wage or subject to an extended transition period as described in the previous section.

[4] Note that this makes no difference to the estimation of employment effects, but only affects descriptive and estimation results of wage effects.

weekly hours as well as the *actual* number of hours they typically work in a given week. The latter is particularly interesting for the analysis since it allows to uncover potential non-compliance. In particular, individuals may be paid in compliance with the minimum wage when contractual hours are referenced, but in reality be paid less when actual hours are considered. That is, they work unpaid overtime. In computing hourly wages, I follow Brenke and Müller (2013). Monthly income in the SOEP includes payments for overtime and excludes special payments (e.g. for vacation and Christmas). To account for this, I use actual hours instead of contractual hours worked unless one of the two following conditions is met: a) the individual is compensated for overtime with additional free time or b) overtime is partly compensated with additional free time and partly paid. While the latter is a borderline case, I follow the suggestion of Brenke and Müller (2013) and use the more conservative method, which is to use contractual hours. Hourly wages are then computed according to the following formula:

$$\text{hourly wage} = \frac{\text{gross monthly income incl. overtime}}{\text{4.33 x weekly hours worked}} \tag{4.1}$$

Hourly wages calculated this way typically lie in between what one would obtain from using contractual and actual hours, respectively.

Affected individuals and non-compliance

Table 4.1 shows the share of all employees eligible to the minimum wage who earned a wage below 8.50 € in 2014. I apply the sample restrictions as described above. Thus, I exclude not only employees for whom exceptions apply, but also those who work in industries where sectoral minimum wages are in place. The share of affected employees is shown for all employees in the sample as well as separately for regular and marginal employment. The left column shows the raw percentages of low-wage earners in the sample, whereas in the right column sample observations were weighted to represent the overall share of affected individuals in the population. Thus, around 12.6 percent of all employees for whom the minimum wage law applies earned a wage below 8.50 € in 2014 and could expect a wage raise to at least 8.50 € in 2015.[5] Among the regularly employed, 9.1 percent earned less than 8.50 € while the proportion of mini-jobbers whose wages would have to be raised to comply with the minimum wage was as high as 57.7 percent. Thus, more than half of the individuals in marginal employment were directly affected by the minimum wage law.[6]

Given the high level of affectedness in this group, negative employment effects of the minimum wage on the marginally employed seem particularly likely. However, the degree

[5]Note that this number is different from the one given in the previous section due to the additional sample restrictions made.

[6]Note that I consider only individuals for whom the mini-job is the primary employment. Therefore, for individuals who hold a mini-job as a secondary job in addition to full-time or part-time regular employment, the latter is used to define their employment status.

Table 4.1: Share of employees win the sample with wages below 8.50 € in 2014

	unweighted	weigthed
all employees	13.9%	12.6%
regular employment	10.1%	9.1%
marginal employment	55.8%	57.7%

Table 4.2: Share of employees in the sample with wages below 8.50 € in 2016

	unweighted	weigthed
all employees	9.4%	8.3%
regular employment	6.9%	6%
marginal employment	43.1%	43.3%

to which employment losses are to be expected also depends on whether employers are in compliance with the new law. It has already been shown that there is, in fact, a significant degree of non-compliance with the minimum wage (Burauel et al. 2017; Caliendo et al. 2017b). This is reflected in my sample and shown in table 4.2. The number of individuals who still earn a wage below the minimum wage in 2016 is strikingly high. In total, more than 8 percent of all eligible employees were paid less per hour than is legally allowed in 2016, 6 percent among the regularly employed and more than 43 percent of mini-jobbers. Compared to 2014, the reduction in the number of workers who earned less than 8.5 € was therefore around one-third for regular employment and only one-fifth for marginal employment. I will further investigate the issue of non-compliance in my robustness checks.

4.2 Econometric specification

In order to identify the effect the minimum wage introduction had on the employment retention probabilities of low-wage individuals, I implement a probability-difference-in-differences approach. Denote e_{it} the employment status of an individual at time t and e_{it+1} his employment status at time $t+1$. Furthermore, define a treatment group $g = 1$ and a control group $g = 0$. Let the binary variable T_{t+1} indicate whether $t+1$ is after (T_{t+1}=1) or before (T_{t+1}=0) the introduction of the minimum wage and let X_{it} be a vector of control variables. An individual belongs to the treated group if he is categorized as a low-wage worker at time t, which I define as having a real wage below 8.50 €, since this is the level at which the minimum wage was introduced in 2015. The control group consists of all individuals with a real wage between 8.50 € and 10.50 € at time t. The causal effect of the minimum wage on the employment retention probability of the treatment group is then given by

$$E\{(e_{it+1}|e_{it}, g = 1, T_{t+1} = 1, X_{it}) - E(e^0_{it+1}|e_{it}, g = 1, T_{t+1} = 1, X_{it}\} \qquad (4.2)$$

Therefore, what I am essentially interested in, is the minimum wage induced change in probability that a low-wage worker transitions *from* employment *to* employment. Note, however, that the right hand side of the equation, the potential expected employment outcome (denoted by the superscript zero) of treated individuals in the absence of treatment is unobserved. For the post-minimum period we obtain estimates for $E\{(e_{it+1}|e_{it}, g = 1, T_{t+1} = 1, X_{it})\}$ and $E\{(e_{it+1}|e_{it}, g = 0, T_{t+1} = 1, X_{it})\}$ for the treatment and control group, respectively. Likewise, we obtain separate estimates for the treatment and control group for the pre-minimum wage period, $E\{(e_{it+1}|e_{it}, g = 1, T_{t+1} = 0, X_{it})\}$ and $E\{(e_{it+1}|e_{it}, g = 0, T_{t+1} = 0, X_{it})\}$. Under the assumption that the employment retention probabilities of both treatment and control group would have evolved similarly absent treatment, that is, with a constant difference in the outcome variable, the average treatment effect on the treated (ATT) in equation (4.2) is identified by:

$$\begin{aligned} DiD =& E\{(e_{it+1}|e_{it}, g = 1, T_{t+1} = 1, X_{it}) - E(e_{it+1}|e_{it}, g = 0, T_{t+1} = 1, X_{it}\} \\ &-(E\{(e_{it+1}|e_{it}, g = 1, T_{t+1} = 0, X_{it}) - E(e_{it+1}|e_{it}, g = 0, T_{t+1} = 0, X_{it}\}) \equiv \delta \end{aligned} \qquad (4.3)$$

Therefore, this specification requires that I observe employment transitions that are entirely before the introduction of the minimum wage and transitions that comprise the introduction of the minimum wage. In my baseline specification, I therefore consider employment to employment transitions from 2013 to 2014 and from 2014 to 2015. This scenario is depicted in table 4.3. I then extend the analysis to my entire sample period, covering the years 2011 to 2016. In total this covers five yearly transitions, beginning with transitions from 2011 to 2012 up to transitions from 2015 to 2016. The minimum wage law was passed in August 2014 and put into effect in January 2015. As most interviews in the SOEP are conducted in the first few months of the year, possible anticipation effects should be covered by the baseline specification. However, delayed adjustments to the new law that have taken place after January 2015 up until the early months of 2016 are then picked up by the specification including all years.

Furthermore, for an individual to be considered in the employment regression, at least one employment transition needs to be observed for this individual. In other words, his employment status has to be known for at least two consecutive years. Moreover, since I am interested in the probability that an individual who is employed in a given year, is still employed in the subsequent year, the individual needs to be employed at least in the first of two consecutive yearly observations. Therefore, I employ an unbalanced panel where a maximum of 5 yearly employment transitions is observed for a given individual.

The difference-in-differences estimator in (4.3) can be estimated with the following

Table 4.3: Baseline specification

	t	t + 1
Minimum wage period	2014	2015
Pre-minimum wage period	2013	2014

linear probability model:

$$Pr[e_{it+1} = 1|e_{it} = 1] = x_{it}^{\intercal}\beta + \alpha g_{it} + \eta T_{t+1} + \delta T_{t+1} g_{it} + \epsilon_{it} \tag{4.4}$$

To estimate this object, the dependent variable is coded 1 when an individual who was employed at time t is still in employment at time $t + 1$ and 0 if he was employed at time t, but non-employed at time $t + 1$. If an individual was not employed at time t, his employment transition is not considered. The interaction between the binary treatment and time indicators g_{it} and T_{t+1} yields the parameter of interest δ. For this model to be identified, the difference-in-differences methodology requires that the group difference α is constant over time and that η, which captures macro time effects between the pre- and post minimum period, is constant across groups.

Given the known drawbacks associated with performing ordinary least squares estimation when the dependent variable is binary[7], it would be desirable to estimate a logit or probit model:

$$Pr[e_{it+1} = 1|e_{it} = 1] = F\{x_{it}^{\intercal}\beta + \alpha g_{it} + \eta T_{t+1} + \gamma T_{t+1} g_{it}\} \tag{4.5}$$

$F(.)$ then corresponds to either the logistic transformation $\Lambda(.)$, or the standard normal $\Phi(.)$ if a probit regression is estimated. γ is the parameter corresponding to the interaction between the binary treatment and time indicators g_{it} and T_{t+1}.

Given the non-linearity of (4.5), however, the standard computation of the marginal effect corresponding to γ does not identify the treatment effect. In the linear model, cross-differencing as in (4.3) eliminates the group and time fixed effects and δ is identified. From (4.3) and (4.4) we get:

$$\beta + \alpha + \eta + \delta - (\beta + \eta) - \{\beta + \alpha - \beta\} = \delta \tag{4.6}$$

As was pointed out by Ai and Norton (2003), the underlying assumption that the treatment effect is constant across the treated population does not hold in the non-linear case. Therefore, the standard computation of the marginal effect corresponding to the interaction term in the non-linear model would not identify δ. Fortunately, however,

[7] Most notably, linear probability models can predict probabilities outside the 0 - 1 interval. See, for example Greene (2003) for details.

Puhani (2012) has shown that, when both the treatment and time indicator are held constant and the interaction term is allowed to vary, the treatment effect is identified by the following non-parametric restriction:

$$\delta(g = 1, T_{t+1} = 1, \bar{x}) = F\{\bar{x}^{\intercal}\beta + \alpha + \eta + \gamma\} - F\{\bar{x}^{\intercal}\beta + \alpha + \eta\} \tag{4.7}$$

Therefore, the treatment effect is correctly identified by calculating the marginal effect of γ for the average individual at $g = 1$ and $T_{t+1} = 1$. This is the strategy I follow throughout this thesis.

Bias in the DiD estimator

As noted previously, the standard (linear) difference-in-differences estimator relies on α and η being constant across time and groups, respectively. However, assuming that α captures the time-constant differences between the treatment and control group also implies that in an unbalanced panel, as is the case in this thesis, the *composition* of treatment and control groups with respect to their individual fixed effects must remain constant. In other words, since α only captures group fixed effects, that is, any heterogeneity between the treatment and control group that is not picked up by the control variables (and is therefore unobserved), it is implicitly assumed that any such heterogeneity is, on *average*, the same before and after treatment.

One way to alleviate the stringency of this assumption is to compare several years before and after the intervention, such that the pre- and post-treatment averages do not just rely on one point in time each. This is also why I employ data from 2011 onwards for the most part of my analysis rather than restricting the analysis to the points in time right around the minimum wage introduction as is done in the baseline specification.

Yet unobserved heterogeneity may still be a confounding factor in the analysis. Thus, even if the composition of treatment and control groups is the same before and after treatment, it may still be the case that individuals within the treatment group are affected differently by the minimum wage due to some characteristics of these individuals that are unobserved and not picked up by either the group fixed effect or the control variables. For instance, individuals with (unobserved) characteristics particularly unfavorable to the labor market may be disproportionally affected by the minimum wage and thus be more likely to lose their job. A typical example would be unobserved ability. To the extent that ability affects the probability of remaining in employment and is not picked up by the controls for educational attainment, for example, it can introduce a bias in the estimate. If individuals within the treatment group with low ability are particularly likely to lose their job, then the standard difference-in-differences estimator would underestimate the true magnitude of the negative employment effect.

To address this concern, I also estimate a version of (4.4) with individual fixed effects in lieu of the group fixed effects term:

$$Pr[e_{it+1} = 1 | e_{it} = 1] = x_{it}^{\intercal}\beta + \eta T_{t+1} + \delta T_{t+1} g_{it} + \theta_i + \epsilon_{it} \tag{4.8}$$

Note that the fixed effects estimation also relaxes the above assumption of a constant group composition. With the inclusion of individual fixed effects, identification of the parameter of interest δ relies only on the set of individuals that is observed both before and after the minimum wage introduction. For this reason, the fixed effects are only estimated in the specification including all years from 2011 to 2016.[8] A potential drawback of the fixed effects estimator is that it is less precise than the ordinary least square estimator as it uses fewer observations. Therefore, if there is no indication for inconsistency of the ordinary least square estimator, it would be preferred (Lechner, Rodriguez-Planas, and Fernández Kranz 2016).

Generally, it would be appealing to estimate a non-linear model with fixed effects. Consistent fixed effect estimators in a non-linear setting can be obtained by estimating a conditional logit model (Chamberlain 1980). However, this would require that all individuals for whom the dependent variable remains unchanged over the sample period are dropped (Allison 2005). This would drastically reduce the sample size. Therefore, I solely rely on a linear model to estimate the fixed effect model.

In the following section, I thus present estimates for the parameter of interest from logit and probit models, pooled ordinary least squares as well as from a linear fixed effects regression.

[8] If I were to include fixed effects in the baseline specification, I would artificially restrict the analysis to the subgroup of individuals that were in employment both in 2013 and 2014, which is the necessary condition for an individual's employment transition to be considered both before and after the intervention (see table 4.3).

Results

5.1 Main results

Wage effects

I begin this section with some descriptive evidence of the effect of the minimum wage introduction on wages. If the minimum wage was indeed effective in raising the wages of low-wage workers, we should see that the lower tail of the wage distribution has been significantly thinned out in 2015. If that was not the case, this would imply that either a) an insignificant number of people earned less than the minimum wage before 2015 and thus wages did not have to be raised, b) there is a substantial amount of non-compliance, or c) all individuals who earned a wage below the minimum before 2015 lost their job. In the previous section, I have documented that there was indeed a significant number of people that earned a wage below 8.50 € prior to its introduction. Furthermore, there was a notable fraction of individuals eligible for the minimum wage that still earned a wage below the legal minimum in both 2015 and 2016.

Figure 5.1 shows the real wage density in 2015 relative to the baseline year 2014.[1] The cross-sectional sample in both years is weighted to allow for inference about the population wage density. The x-axis shows the deciles as defined by the wage distribution in 2014 and the bars represent the density in 2015 relative to the baseline year 2014. The wavy line represents a kernel density estimate. The relative density can thus be interpreted as the percentile rank the data points from 2015 would have in 2014. Bars above the horizontal line where the y-axis equals one imply that there is more density in 2015, and bars below the line imply more density in 2014 in the respective decile. The top horizontal axis represents the real wages that correspond to the deciles shown on the x-axis.

The figure reveals that the relative density in the first decile of the 2014 distribution is between 0.75 and 0.8. This means that there were around 20 to 25 percent fewer individuals in that decile of the wage distribution in 2015 than there were in 2014. The first decile corresponds to individuals with real wages up to around 7.50 €.[2] In the second

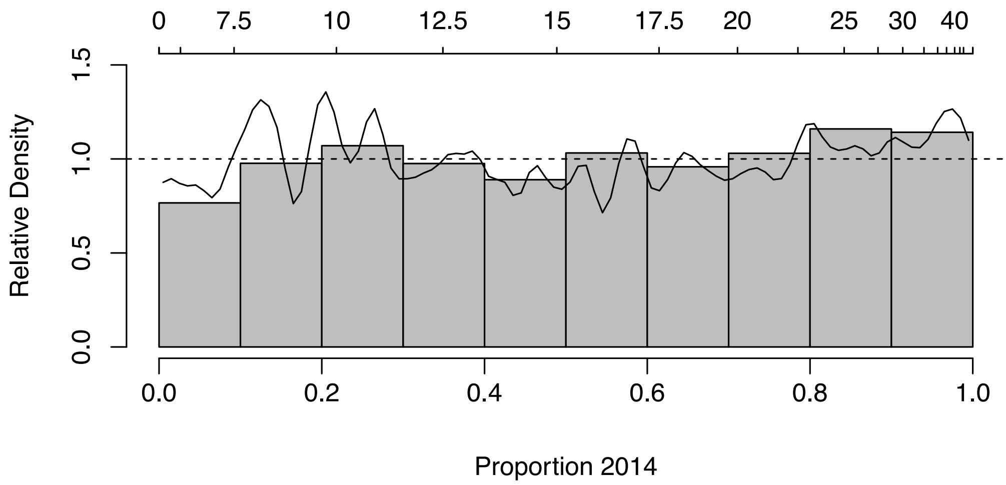
[1] The figure was created using the methodology and R package by Handcock and Morris (1998).

[2] Wages were deflated using Eurostat's Harmonized Index of Consumer Prices (HICP). The base year is 2015. A wage of 7.50 € in 2015 corresponds to a real wage of 7.4925 € in 2014.

decile, which covers wages between approximately 7.50 € and 10 €, the reduction relative to 2014 is significantly smaller. This is unsurprising given that individuals who earned less than the minimum wage in 2014 are likely to have earned wages in the range of 8.50 € and 10 € in 2015. The third decile covers relatively more individuals in 2015, which could similarly be seen as an indicator for minimum wage induced wage increases.

The increase in wages at or above 8.50 € is more clearly seen in figure 5.2, which plots the wage histograms of individuals with real wages below 15 €. The x-axis shows the real wage and the bars represent the real wage density where each bar corresponds to a range of 50 cents. The y-axis shows the percentage of individuals who earn a real wage in the respective wage range. The histograms for 2014 and 2015 are shown mirror-inverted on the same x-axis for easier comparability.[3] The figure reveals that the lower tail of the distribution is thinner in 2015 than in 2014. Furthermore, there is some bunching in 2015 at the 8.50 € mark.[4] While there were only around 2.5 percent of individuals earning between 8.50 € and 9 € in 2014, this figure increases by approximately 1 percentage point in 2015. Yet both figures show what has already been discussed previously: The fraction of individuals earning below the minimum wage after its introduction is considerable.

Figure 5.1: Relative wage density 2014-2015



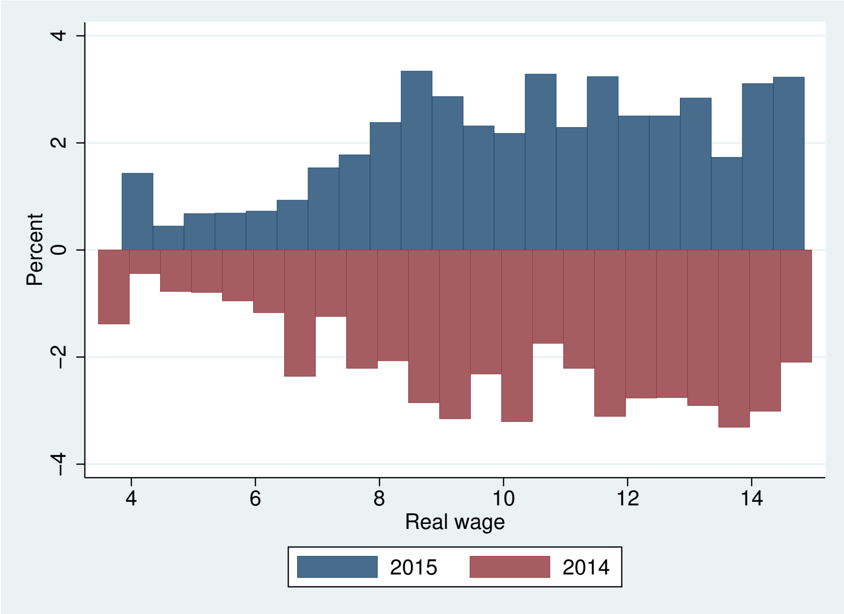
Sources: SOEP v.33; R package by Handcock and Morris (1998)

While figures 5.1 and 5.2 show that the lower tail of the wage distribution has been (somewhat) thinned out after the minimum wage was introduced, this does not yet provide an adequate comparison of the wage growth in the treatment and control group as is required for the econometric analysis that follows. In particular, if we are to expect a

[3]The negative values on the y-axis simply reflect the inverse representation, but are, of course, to be interpreted as positive percentages.

[4]Note that the observed bunching in both years at the 4 € mark is due to winsorizing of the data at the first and last percentile.

Figure 5.2: Wage histogram 2014-2015



Source: SOEP v.33

significantly different impact on employment between both groups, individuals in the treatment group should have experienced a significantly higher wage boost than individuals in the control group. Why? Say, conversely, wages grew in a parallel fashion for both groups post-treatment. This would imply that either a) the minimum wage has not been effective in raising wages of low-wage workers, b) the minimum wage has lead to substantial *wage spillovers* to individuals in the comparison group to the extent that wage growth in both groups was completely symmetric[5] or c) all individuals who earned a wage below the minimum before 2015 lost their job. a) would simply imply that the minimum wage law is not complied with and we would therefore not expect any significant impact on employment to show up in the analysis. In case of considerable wage spillovers on the other hand, the price of labor increases for both treatment and control group. This could lead to job losses in both groups, which would in turn bias the estimated employment effect towards zero.[6]

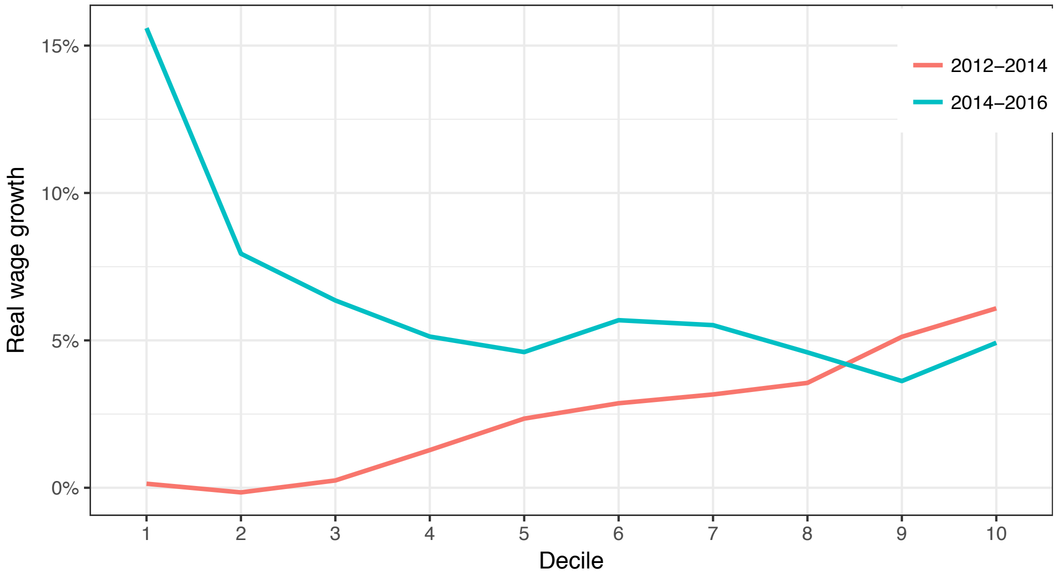
Figure 5.3 compares the average real wage growth for each decile of the wage distribution in the pre- and post minimum period.[7] The figure reveals that the bottom decile has experienced a growth in real wages of around 15.5 percent between 2014 and 2016. Over the same two-year period, real wages in the second decile have grown around 8 percent. Both increases are substantial compared to the wage change between 2012 and 2014,

[5]Wage spillovers occur when the introduction or raise of a minimum wage has caused the wages of those further up the distribution to rise as well. This could, for instance, be a consequence of fairness considerations on the side of the firm or higher wage demands from workers.

[6]I will discuss spillover effects in more detail in the following analysis of employment effects.

[7]Weighting factors were applied to each cross-sectional distribution. Similar findings on wage growth for the bottom deciles are presented in Burauel et al. (2017).

Figure 5.3: Real wage growth by decile



Sources: SOEP v.33; R package by Wickham (2009)

which was close to zero for both groups. Note from figure 5.1 that the first decile in 2014 exclusively consists of individuals in the treatment group, while the second decile comprises both treated individuals and those from the control group (individuals with real wages between 8.50 € and 10.50 €). This suggests that wages in the treatment group indeed rose more sharply than wages in the control group.[8] In order to test this suggestive evidence econometrically, I estimate equation (4.4) with the logarithm of real wages as dependent variable. The results are shown in table 5.1.[9] Between 2014 and 2015, the

Table 5.1: Wage effects

	Baseline		full sample	
Treatment	0.0281***	0.0247***	0.0517***	0.0428***
	(0.00931)	(0.00948)	(0.00648)	(0.00663)
Controls	No	Yes	No	Yes
Observations	5335	4961	17581	16414

Source: SOEP v.33, 2011-2016 .
Cluster-robust standard errors in parentheses .
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[8] Of course, it should be noted that overall growth was smaller in the years 2012 to 2014, which immediately followed the financial crisis, whereas the German economy boomed in the years 2014-2016. However, this does not change the fact that growth was particularly strong in the lowest deciles, which was likely due, at least in part, to the introduction of the minimum wage.

[9] The control variables are the same as those used in the estimation of employment effects (see following section).

additional real wage growth induced by the minimum wage for those directly affected, relative to the control group, was around 2.5 percent. Until 2016, the additional growth in real wages amounts to 4.3 percent. Without the inclusion of control variables the estimates further increase. These effects are roughly in line with what one would expect from a visual inspection of figure 5.3.

Employment effects

I estimate the effect of the minimum wage on the employment retention probability of those for whom wages had to be raised to comply with the legal minimum by comparing their employment outcomes to those of a group with wages slightly above the minimum. Table 5.2 shows the treatment effect, i.e. the estimate corresponding to δ for the estimation of equation (4.4), (4.5) and (4.8), on regular employment. Column (1) to (3) show the estimation results when only the transitions from 2013 to 2014 and 2014 to 2015 are considered. In column (4) to (7) the estimation is performed on the whole sample period 2011-2016. Note that across all specifications, the non-linear models (logit and probit) on the one hand and the linear probability model on the other hand yield very similar results. This is reassuring as it demonstrates robustness of the estimated coefficient to different functional form assumptions. The estimated treatment effect in the baseline specification is between -5.4 and -5.9 percentage points and highly significant. This implies that the minimum wage has reduced the probability for affected individuals to remain in employment after its introduction in 2015 by up to 5.9 percentage points. When the

Table 5.2: Employment effects, regular employment

	Baseline specification			Full sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Logit	Probit	OLS	Logit	Probit	OLS	FE
Treatment	-0.0592***	-0.0560***	-0.0543***	-0.0415***	-0.0388***	-0.0394***	-0.0342**
	(0.0187)	(0.0189)	(0.0201)	(0.0121)	(0.0123)	(0.0129)	(0.0152)
Observations	3449	3449	3451	9336	9336	9340	9340

Source: SOEP v.33, 2011-2016 .

Cluster-robust standard errors in parentheses; standard errors are clustered at the level of individuals .

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The logit and probit model estimates shown are marginal effects, calculated as suggested by Puhani (2012). All models contain a full set of controls. These are: gender, age, age squared, marital status, a regional dummy (to distinguish East and West Germany), dummies for highest educational attainment, labor market experience full-time (quadratic), labor market experience part-time (quadratic), time worked for current employer, dummies for skill requirements of current occupation, and firm size of current employer.

estimation is performed on the full sample period, the estimated coefficient is still highly significant, but smaller in magnitude, hovering around 4 percent. As explained in section

4.2, a longer period is chosen to make the estimation less susceptible to compositional bias. Furthermore, the sample period now includes the year 2016, that is one year after the minimum wage was implemented. As noted, there is still a significant number of individuals who earned a wage less than 8.50 € in early 2015 and are thus still at risk of losing their job as employers make adjustments to the new minimum wage law in 2016. Yet the bulk of these adjustments had occurred already in 2015, which naturally leads to a lower post-treatment estimate if both years are considered.[10] Lastly, the individual fixed effect estimator as well yields very similar results compared to the logit, probit and linear probability model. The estimated coefficient is somewhat less significant, yet still at a 5 percent significance level. As discussed in section 4.2, the linear probability model tends to yield more precise estimates as it uses more observations, given that it is consistent, that is, given that ignoring individual fixed effects does not invalidate the pooled estimator. As the results are not wildly off, I take that as an indication that the linear probability model provides a reliable estimate.

As discussed previously, the marginally employed are considered particularly likely to lose their job as a result of the minimum wage introduction. Existing estimates range from a reduction in marginal employment of 66,000 (Garloff 2016) to a loss of 183,000 mini-jobs (Caliendo et al. 2017a). Given these considerably diverging estimates, it would seem informative to look at the effects the minimum wage had on marginal employment at the individual level rather than the aggregate level. While the total number of mini-jobs does seem to have decreased as a result of the minimum wage, it would be interesting to see whether it was indeed the marginally employed directly affected by the minimum wage (i.e. for whom wages would have had to be raised), who lost their job. Table 5.3 shows the estimated treatment effects for the marginally employed. Note that both treatment and control group are restricted to individuals who were marginally employed at time t. However, an individual is considered as still employed at time $t+1$ if he is either still marginally employed or in regular employment at time $t+1$. This implies that transitions into regular employment are, unlike in the analysis of Garloff (2016) and Caliendo et al. (2017a), not considered as lost mini-jobs.

Neither the estimates for the baseline specification, nor for the full sample period show any statistically significant treatment effect and even have a positive sign in some cases. The estimated coefficient in the fixed effects specification is negative and roughly 10 times larger in magnitude than in the linear probability model, yet not at a statistically significant level either. This surprising result could imply that, while mini-jobs have decreased overall, those directly affected suffered no asymmetrically larger job losses. It may also be possible that increased transitions into regular employment obscured any

[10] I repeated the analysis by excluding the transition from 2015 to 2016 from the full sample (results not shown). As expected, the estimated treatment effect is very close to the effect obtained in the baseline specification.

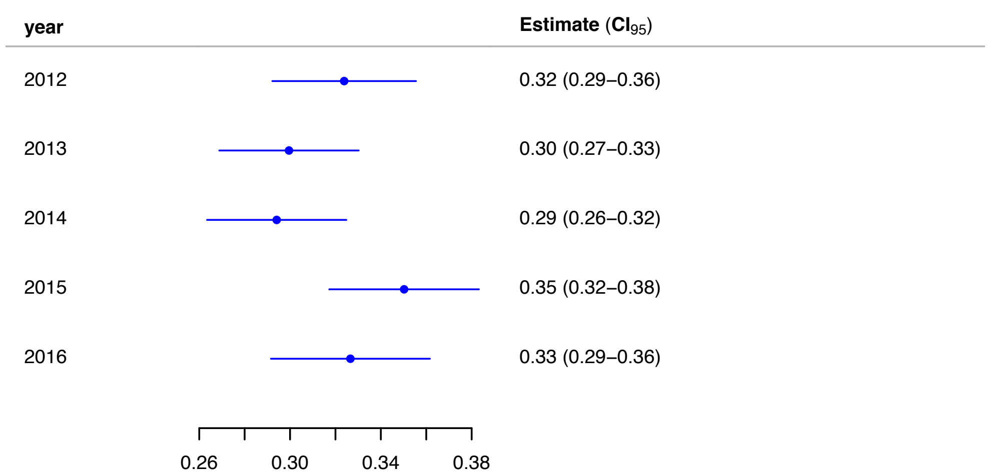
Table 5.3: Employment effects, marginal employment

	Baseline specification			Full sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Logit	Probit	OLS	Logit	Probit	OLS	FE
Treatment	-0.00629	-0.00906	-0.0104	0.0165	0.0112	0.00618	-0.0631
	(0.0548)	(0.0527)	(0.0498)	(0.0387)	(0.0362)	(0.0306)	(0.0412)
Observations	1076	1076	1080	2607	2607	2611	2611

Source: SOEP v.33, 2011-2016 .
Cluster-robust standard errors in parentheses .
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

visible employment effects. Figure 5.4 lends some support to this hypothesis. From 2014 to 2015, the probability of transitioning from marginal into regular employment increased by 6 percentage points and remained higher in 2016 compared to the pre-minimum wage period. Yet the confidence intervals are not tight enough to make reliable inferences. As noted in section 4.1, almost 58 percent of mini-jobbers earned a wage less than 8.50€ in 2014. In this analysis, however, the control group is restricted to individuals earning wage between 8.50€ and 10.50€, resulting in a sample with between 70 and 80 percent of individuals in the treatment group and between 20 and 30 percent in the control group in the years prior to the minimum wage. In 2014 for example, 69 percent in the sample belonged to the treatment group. Therefore, the robustness of these results will have to be investigated in detail in the next section.

Figure 5.4: Transitions from marginal to regular employment



Sources: SOEP v.33; R package by Gerds and Ozenne (2018)

5.2 Robustness Checks

Identifying assumption

The fundamental identifying assumption that needs to hold in this difference-in-differences framework is that employment retention probabilities in both treatment and control group would have evolved in a parallel fashion absent the policy. That is, the group-specific difference in employment retention probabilities is constant over time and both treatment and control group are subject to the same macroeconomic trends. Naturally, this assumption cannot be tested directly, as the counterfactual development of employment absent treatment is unobserved. One way to gain confidence in judging wether the assumption should hold is by observing pre-treatment patterns in the employment retention probabilities of treatment and control groups. This is depicted in figure A.1.

The two graphs show quarterly employment retention probabilities from August 2011 until November 2015. As most interviews in the SOEP are conducted in the first few months of the year, the important period for which pre-trends have to be compared runs from the third quarter in 2011 until the second quarter in 2014. Overwhelmingly, for both regular and marginal employment, treatment and control group follow a similar cyclical pattern.[11] In the second quarter of 2014, the employment probability in the control group in regular employment increases, while it drops in the treatment group. Coincidentally, this is the time when many interviews take place, so a possible violation of the common trend assumption cannot be ruled out at this stage. Note, however, that the standard error is relatively large at this point for the treatment group. It also appears that the employment retention probability in the third quarter of 2014 and the second quarter of 2015 dropped quite significantly in the marginal employment treatment group, relative to the control group. However, both these points in time are after the 2014 interviews took place, hence any effect would be attributed to the post-treatment period.

Table 5.4 presents a further check on the validity of the parallel trend assumption. The post-treatment indicator T_{t+1} in equation (4.4) is replaced by year dummies. These are then interacted with the treatment variable, creating a treatment-year interaction term for every observed transition in the full sample period. The parallel trends assumption is violated if the estimated interaction term at any point in time prior to the treatment is significant.[12] For regular employment, the only significant interaction occurs in 2015, which is in line with the treatment effect found in table 5.2. The effect in 2016 is still negative, but not statistically significant. the interactions with 2013 and 2012 are very small in magnitude and insignificant. It follows that the hypothesis of common trends cannot be rejected at any relevant significance level. For marginal employment no interaction term is

[11] The cyclical pattern can mainly be attributed to seasonal patterns.

[12] Note that the base year is 2014, which is the last year prior to the introduction of the minimum wage.

Table 5.4: Parallel trends

	Regular employment	Marginal employment
treatment x 2016	-0.0222	-0.0147
	(0.0205)	(0.0480)
treatment x 2015	-0.0542***	0.000156
	(0.0201)	(0.0492)
treatment x 2013	0.000926	0.00443
	(0.0191)	(0.0458)
treatment x 2012	-0.00295	-0.0600
	(0.0180)	(0.0451)
N	9340	2611

Source: SOEP v.33, 2011-2016 .

Cluster-robust standard errors in parentheses .

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

significant, which is in line with the above finding of no significant effects. At the same time, there is no indication for a violation of the parallel trends assumption.

Alternative control groups

The employment effects of the minimum wage may also depend on how it affects individuals further up the wage distribution. The minimum wage increases the relative price of low-wage workers from the perspective of the employer. Depending on the substitutability between workers, this may lead to increased demand for workers further up the wage distribution, who are now relatively cheaper. I have argued that choosing a control group with wages just above the minimum wage is appropriate as these individuals are most likely to be similar in observable and unobservable characteristics, making them comparable to the treatment group. At the same time, this similarity in characteristics makes substitution between treatment and control group more likely. Substitution between the treatment and control group would introduce a bias into the estimates. Specifically, the estimated negative effect on regular employment in table 5.2 would be overestimated. The reduction in employment probabilities of the treated would be paralleled by improved employment outcomes in the control group post-treatment, leading to an estimate biased away from zero. As mentioned previously, the minimum wage may also have a causal effect on wages further up the distribution. This is what is known in the literature as *spillover effects*. Induced by fairness considerations, workers with wages above the minimum wage may demand higher wages to restore prior wage differentials. This mitigates substitution pressure, but potentially also worsens the employment probabilities of those workers further up the distribution. Therefore, substitution and wage spillovers typically work in the opposite direction.

The international evidence on spillover effects is mixed. In the United Kingdom, spillover effects were shown to be virtually nonexistent (Dickens and Manning 2004, Stewart 2012). In the United States, wage spillovers have been found up to the 15th wage percentile. For the retail trade sector, which is particularly strongly affected by the minimum wage, Wicks-Lim (2006) finds evidence for wage spillovers up to the 40th percentile. Aeberhardt, Givord, and Marbot (2012) find wage spillovers of minimum wage increases between 2003 and 2005 in France up to the the 7th wage decile, yet they find no evidence for adverse employment effects.

While an analysis of such effects is not subject of this thesis, it is important to check the robustness of the obtained estimates with respect to the choice of the control group. A control group further up the distribution is less susceptible to induce bias through substitution or wage spillovers. At the same time, it likely worsens the comparability of treatment and control group as they become less similar in characteristics. Table 5.5 compares the treatment group to a comparison group with wages between 10 € and 12 €. In addition to making wage spillover and substitution less likely, it makes the estimates also less susceptible to classification bias. Specifically, since wages have to be calculated from information provided by individuals on monthly income and hours worked, measurement errors are likely, as is typical in survey data. When treatment and control group are adjacent, this creates the risk of misclassification. Individuals who are in fact treated may be falsely classified into the control group and vice versa. If misclassifications are not systematic, this does not necessarily introduce a significant bias, but it likely makes the estimates less precise. Choosing an alternative control group further up the distribution mitigates this problem. As in all robustness checks that follow, table 5.5 shows estimates

Table 5.5: Alternartive comparison group (wages between 10 € and 12 €)

	Regular employment				Marginal employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit	Probit	OLS	FE	Logit	Probit	OLS	FE
Treatment	-0.0272**	-0.0254*	-0.0266**	-0.0277*	-0.0417	-0.0421	-0.0342	-0.0691
	(0.0132)	(0.0131)	(0.0127)	(0.0166)	(0.0390)	(0.0382)	(0.0357)	(0.0561)
Observations	9476	9476	9480	9480	2282	2282	2285	2285

Source: SOEP v.33, 2011-2016 .

Cluster-robust standard errors in parentheses .

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

only for the full sample period.[13] For regular employment, the estimated treatment effect is between -2.5 and -2.7 percentage points across specifications. The estimate from the fixed effect specification is again similar in magnitude, but at a smaller significance level. What is striking, however, is that the estimates obtained with this alternative comparison group are notably smaller than those obtained in table 5.2. The difference in magnitude

[13] I estimated all alternative specifications on the baseline sample as well (not reported), but the implications remain unchanged.

ranges from 0.7 percentage points in the fixed effects specification to 1.4 percentage points in the logit model. Moreover, the significance of the estimate decreases. While the former may imply that the original comparison group was affected by the minimum wage as well through the mechanisms described above,[14] the lower estimate obtained with the alternative control group may simply reflect that the treatment and the alternative control group are less comparable in characteristics, impairing the reliability of the estimate. Note as well that the estimates obtained for the original and alternative comparison group from the fixed effects model, which controls for unobserved differences between groups and thus mitigates the comparability problem, are much closer in magnitude than the other estimates. All estimates for marginal employment remain insignificant.

Table 5.6: Alternartive comparison group (full sample)

	Regular employment				Marginal employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit	Probit	OLS	FE	Logit	Probit	OLS	FE
Treatment	-0.0135*	-0.0135*	-0.0198*	-0.0303***	-0.0328	-0.0360	-0.0323	-0.0470
	(0.00773)	(0.00809)	(0.0105)	(0.0117)	(0.0258)	(0.0255)	(0.0262)	(0.0353)
Observations	45270	45270	45270	45270	3218	3218	3223	3223

Source: SOEP v.33, 2011-2016 .
Cluster-robust standard errors in parentheses .
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5.6 compares the treatment group to the entire distribution above the minimum wage. This is generally ill-advised, as treatment and control groups are likely very heterogenous in this case and thus hardly comparable. The probit, logit and OLS estimates for regular employment drop notably in significance. Most of the minimum wage effect seems to have been washed out by heterogeneity. Since the estimation now exploits the full sample, the precision of the fixed effect estimator is unsurprising, as there is now much more within-variation available for estimation. The estimate lies in between what has been obtained in table Table 5.2 and 5.5. Yet the former remains my preferred specification as comparing groups that are as similar as possible is generally more advisable. Once again, all estimates for marginal employment remain insignificant.

Low-wage individuals

Table 5.7 restricts the treatment group to individuals with real wages below 6.50 €, that is, individuals for whom the required wage increase was particularly large. At the same time, misclassifications into the treatment group are less likely. The comparison group is the same as in the main specification in section 5.1. While the estimated treatment effect for

[14]The employment retention probability in the original comparison group in fact slightly *increased* in 2015 while it *decreased* in the treatment group (not shown), which could be caused, for example, by substitution between the treatment and comparison group.

the marginally employed remains insignificant across specifications, the differences in the estimated employment effect for the regularly employed to the main results in table 5.7 are substantial. The estimated treatment effect is very similar across specifications, ranging

Table 5.7: Low-wage individuals (wages below 6.50 €)

	Regular employment				Marginal employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit	Probit	OLS	FE	Logit	Probit	OLS	FE
Treatment	-0.0666***	-0.0633***	-0.0643***	-0.0670**	-0.00153	-0.00857	-0.0159	-0.0536
	(0.0197)	(0.0201)	(0.0216)	(0.0301)	(0.0428)	(0.0403)	(0.0357)	(0.0518)
Observations	6295	6295	6298	6298	1871	1871	1873	1873

Source: SOEP v.33, 2011-2016 .
Cluster-robust standard errors in parentheses .
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

between -6.4 and -6.7 percentage points. It is, however, approximately 3 percentage points larger in magnitude. This suggests substantial heterogeneity in employment effects within the treated group. For those close to the minimum from below the necessary wage raise was significantly smaller, which seems to have positively affected their employment retention probability. Moreover, this group was large enough to push the estimated overall treatment effect downwards.[15]

Compliers

Focusing on individuals with very low wages has shown that employment effects of the minimum wage were particularly large when the necessary wage increase to comply with the new minimum was sizable. Yet thus far, a significant employment effect on the affected marginally employed, *relative* to the marginally employed with higher wages, has not been identified. I have discussed previously that there is a significant fraction of individuals eligible to earn the minimum wage who still earned a lower wage post January 2015. Marginal employment contracts are particularly susceptible to regulatory non-compliance. It is relatively easy for employers to understate the true working hours of a mini-jobber in order to officially comply with the law. The SOEP has the advantage that respondents are asked directly how many hours they work and what they earn on a monthly basis. Since I specifically account for unpaid overtime when calculating hourly wages, this has allowed me to investigate the magnitude of this phenomenon. I have documented that around 43 percent of mini-jobbers still earned a wage below 8.50 € in 2016. The same is true for around 6 percent of individuals in regular employment (see section 4.1).

Table 5.8 shows the usual estimated treatment effects for regular and marginal employment when only those individuals are considered whose wages have been raised to

[15] In the pre-minimum wage years the proportion of individuals in the treatment group with real wages between 6.50 € and 8.50 € was around 60 percent each year.

comply with minimum wage. Individuals with wages below 8.50 € after January 2015 have been excluded from the analysis. Before turning to the results, some remarks are necessary. I exclude individuals that still earned a wage below 8.50 € after the minimum wage introduction from the *entire* sample to avoid selective deletion of observations in the post-minimum wage period only. Obviously, in the pre-minimum wage sample, there are still individuals with low wages for whom there are no observed employment transitions in the post-minimum period, that is, for the transitions from 2014 to 2015 or 2015 to 2016.[16] It remains unclear how the minimum wage would have affected them had they stayed in the sample. By construction of the complier sample, such low-wage individuals cannot be present in the post-treatment sample. Therefore, given this asymmetric exclusion of low-wage individuals pre- and post-treatment, the estimates that follow should be interpreted with caution. Nevertheless, they should at least give some sense of magnitude with respect to the treatment effect on compliers.[17]

Table 5.8: Compliers

	Regular employment				Marginal employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit	Probit	OLS	FE	Logit	Probit	OLS	FE
Treatment	-0.123***	-0.122***	-0.135***	-0.114***	-0.0776	-0.0853*	-0.0902*	-0.177***
	(0.0228)	(0.0232)	(0.0241)	(0.0277)	(0.0522)	(0.0499)	(0.0466)	(0.0619)
Observations	6769	6769	6773	6773	1601	1601	1605	1605

Source: SOEP v.33, 2011-2016 .

Cluster-robust standard errors in parentheses .

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For regular employment, the estimated treatment effect is highly significant and much larger in magnitude than any of the previous estimates. The estimated employment effects indicate a reduction in the employment retention probability between 11.4 and 13.5 percentage points. Given the high degree of non-compliance in marginal employment as reported in A.1, it may be unsurprising that there is now a visible negative effect as well. The linear probability model estimates a negative effect of around 9 percentage points, significant at the 10 percent level. The fixed effects estimate removes enough of the unobserved heterogeneity to reveal a highly significant, negative effect of the minimum wage of almost 18 percentage points. This is in stark contrast to the nonsignificant estimates obtained in all previous specifications. It seems that non-compliance has obscured the visibility of the negative employment effect of the minimum wage on affected mini-jobbers. Again, while I remain cautious with respect to the estimates obtained from this specification, the magnitude of the estimated treatment effect on marginal employment

[16] This is either because they drop out of the SOEP entirely or because the dependent variable is set to NA in the post-treatment period, meaning that they were neither employed in 2014 nor in 2015.

[17] To check the sensitivity of the results I repeat the analysis excluding individuals with very low wage observations (below 3.50 €) from the entire sample, but the results change little (not reported).

seems consistent with previous findings of significantly negative aggregate employment effects of the minimum wage on mini-jobs (Caliendo et al. 2017a; Garloff 2016).

To conclude the discussion on compliers, I present estimates of a simple probit model to investigate who these non-compliers are and where they work. These are shown in tables A.1 and A.2. The dependent variable is a binary indicator that equals one if an individual earned less than the minimum wage *after* its introduction date. In table A.1 the non-compliance indicator is regressed on worker and firm characteristics and in table A.2 non-compliance is regressed on a full set of industry dummies. As expected, being marginally employed increases the probability of earning a wage below the legal minimum significantly. Similarly, being female, living in Eastern Germany and working in a small firm also significantly increases the probability of earning a wage below the legal minimum. Conversely, higher education and more years of full-time experience reduce the likelihood of earning less than the minimum post January 2015.[18] Table A.2 reveals that non-compliance is, perhaps unsurprisingly, particularly prevalent in the food service industry and to a smaller, but still substantial degree in wholesale and retail trade. The food service industry is characterized by irregular working hours and a large proportion of mini-jobbers, making it especially susceptible to minimum wage evasion strategies. Tips can be (illegally) offset against the minimum wage and hours worked can be falsely documented. In my sample one third of employees in the food service industry earned less than 8.50 € in 2016, in retail non-compliance still amounts to around 20 percent.

Non-employment versus unemployment

Thus far, I have considered transitions from employment to employment and from employment to non-employment. However, transitions into non-employment do not necessarily imply that an employment contract was dissolved involuntarily from the perspective of the employee. He might, for example, simply decide to take care of the household rather than continuing in employment. Conversely, not every involuntary departure from a job results into registered unemployment. This is the case, for example, if individuals who are either not eligible for unemployment benefits or who are afraid of being stigmatized do not register as unemployed. Therefore, I investigate the sensitivity of the main results to the definition of non-employment. In particular, I consider transitions from employment into non-employment only when the individuals departing from their job register as unemployed in the following period. In general, this modification is expected to reduce the estimated employment effect. The results are shown in table 5.9. For regular employment, the estimated treatment effect is indeed smaller in magnitude compared to the main results. The fixed effect estimate is now insignificant, but the linear probability model and non-linear specifications yield an estimated treatment effect between -1.9 and -2.2 percentage

[18] Obviously, all these estimates reflect not only the likelihood of non-compliance, but also the probability of earning low wages more generally.

Table 5.9: Registered unemployed

	Regular employment				Marginal employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit	Probit	OLS	FE	Logit	Probit	OLS	FE
Treatment	-0.0199**	-0.0192**	-0.0224**	-0.0154	0.0171	0.0103	-0.00800	-0.0436***
	(0.00854)	(0.00890)	(0.00987)	(0.0114)	(0.0377)	(0.0299)	(0.0178)	(0.0160)
Observations	8860	8860	8874	8874	2199	2199	2309	2309

Source: SOEP v.33, 2011-2016 .
Cluster-robust standard errors in parentheses .
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

points. Somewhat surprisingly, the fixed effect estimate for marginal employment now lets the negative effect of the minimum wage show through, yielding a significant negative treatment effect of -4.4 percentage points. Note however, that this estimate is smaller in magnitude than what has been obtained previously, even though these were imprecisely estimated and thus not significant.

Section summary

In this section, I have estimated the effect of the minimum wage introduction on the employment retention probabilities of individuals in both regular and marginal employment. In the main specification, I compare individuals with real wages below the minimum wage of 8.50 € in a given year to those with real wages between the minimum and 10.50 €. I find that the minimum wage reduced the employment retention probabilities of the regularly employed by around 4 percentage points in the estimation on the full sample period. For the marginally employed no negative treatment effect is detected. Thus, marginally employed individuals who were directly affected by the introduction of the minimum wage seemed to have been no more likely to lose their job than mini-jobbers with wages slightly above the minimum wage.

The sensitivity of these results was checked by refining the definition of both the treatment and control group in a variety of ways. Comparing the treatment group to control groups further up the wage distribution has made the estimates smaller and more imprecise. While this finding may imply that the employment outcomes of the control group were as well affected by the minimum wage, it may simply reflect decreased comparability of the two groups, stemming from reduced similarity in terms of individual characteristics.

Restricting the treatment groups to individuals with particularly low wages increases the estimated negative effect of the minimum wage by almost 3 percentage points, suggesting significant heterogeneity in treatment effects. For mini-jobbers, the estimated effect remains insignificant.

Focusing on the effect on the minimum wage on individuals whose wages were actually raised above the minimum (compliers), changes the picture dramatically. The estimated reduction in the employment retention probability ranges between 11.4 and 13.5 percentage points for the regularly employed. Previous evidence suggests that regular employment has been created at the expense of marginal employment (Garloff 2016). The nevertheless high estimated disemployment effect on the directly affected thus suggests that this job creation has not necccessarily benefitted the most vulnerable workers at the low end of the distribution.[19]

For marginal employment, the standard difference-in-differences specifications yield a treatment effect of around 9 percentage points in the complier sample. The fixed effects estimate is twice as large, suggesting a substantial bias in the standard difference-in-differences estimates when individual-specific unobserved heterogeneity is not controlled for. While these estimates seem to be in line with previous findings on employment effects of the minimum wage on marginal employment, they need to be interpreted with caution, given the asymmetric exclusion of low-wage workers in the pre- and post-minimum wage period.

Lastly, I restrict the definition of non-employment to unemployment, such that only a subset of transitions out of employment is considered. This reduces the estimated impact on regular employment. At the same time, the fixed effects estimator yields significantly negative results for the marginally employed now as well.

[19]Recall that my definition of continued employment does not require employment in the same firm. Hence, an individual who lost his job because of the minimum wage, but finds a job elsewhere thanks to additional jobs being created, would count as remaining in employment.

Estimating employment effects by industry: An extension of Caliendo et al. (2017a)

6.1 Estimation strategy

For this final section I will turn away from an individual-level analysis and investigate employment effects of the minimum wage from a macro perspective. This is in line with what other studies analyzing the German minimum wage have done to date. Specifically, I suggest to extend the analysis of Caliendo et al. (2017a)[1] by estimating industry-specific employment effects of the minimum wage. I begin by replicating the main results of Caliendo et al. using a slightly different combination of data sets and then continue with my proposed extension.

Caliendo et al. apply a methodology developed by Card (1992b) that exploits regional variation in the *bite* of the minimum wage to estimate its employment effects. The general idea is that the variation in treatment intensity allows to identify the treatment effect by comparing the employment outcomes of groups that are relatively more affected to groups that a relatively less affected.

Card investigates the effects of the 1990 increase in the US federal minimum wage on teenage employment and wages by exploiting the variation in treatment effects across states. He estimates wage change and employment change models, which he interprets as reduced-form equations from a simple structural model that explains the wage increase Δw_i between 1989 and 1990 in a given state as a function of the fraction of teenagers (the bite of the minimum wage) in the affected wage range in 1989 (F_i), and the employment change Δe_i as a movement along the teenage employment demand function. This yields the following equations:

$$\Delta w_i = a + bF_i + cX_i + \theta_i \tag{6.1}$$

$$\Delta e_i = \alpha + \beta \Delta w_i + \gamma X_i + \epsilon_i \tag{6.2}$$

[1] Henceforth Caliendo et al.

where β denotes the labor demand elasticity, X_i is a set of control variables and θ_i and ϵ_i are residual terms. The parameter b represents the average effect of the minimum wage on the change in wages. Combining (6.1) and (6.2) yields the reduced-form employment change equation:

$$\Delta e_i = \alpha + b\beta F_i + (\gamma + c\beta)X_i + \beta\theta_i + \epsilon_i \tag{6.3}$$

The causal effect of the minimum wage on employment is then given by the product $b\beta$.

Caliendo et al. implement this methodology by estimating the following regression:[2]

$$lne_{rt} = x_{rt}^{\intercal}\beta + \alpha_t + \gamma_r + bite_{r,2014}\alpha_t^{\intercal}\delta_t + \epsilon_{rt} \tag{6.4}$$

where lne_{rt} denotes the log level of employment at time t in region r, $x_{rt}^{\intercal}$ is a set of regional control variables, α_t and γ_r are time- and region-fixed effects, respectively, and ϵ_{rt} denotes the error term. The parameter of interest is the interaction term δ_{2015} between the year 2015 and the $bite_{r,2014}$ measure. The bite is calculated in 2014, that is, one year before the minimum wage was introduced. The year-vector includes the years 2014 and 2015 in the baseline specification, but is expanded to 2012 and 2013 to control for anticipation effects and in order to test the parallel trends assumption (as was done in my previous analysis of employment retention probabilities). The control variables include the log population level as well as the lagged log level of GDP.

Since I am interested in employment effects by industry, I adjust the above regression in two ways: First, I perform the regression separately for every industry considered. Second, I adapt the bite measure to account for the share of a given industry in overall employment in each region. Specifically, my new bite measure is given by:

$$Z_{rt}^{i} = bite_{r,2014} \text{ x } share_{rt}^{i} \tag{6.5}$$

where Z_{rt}^{i} is the adjusted bite measure for industry i in region r and $share_{rt}^{i}$ is the employment share of industry i in region r in a given year.[3]

The industry-specific regression equation is then given by:

$$lne_{rt}^{i} = x_{rt}^{\intercal}\beta^{i} + \alpha_t^{i} + \gamma_r^{i} + Z_{rt}^{i}\alpha_t^{\intercal}\delta_t^{i} + \epsilon_{rt}^{i} \tag{6.6}$$

[2] Note that my notation slightly differs from that used in the original paper.

[3] I would like to thank my advisor Professor Florian Oswald for pointing out to me that this adjusted bite measure bears resemblance to the well-known Bartik instrument (Bartik 1991), variations of which are frequently used in labor economics.

where the superscripts i denote the respective industry.

6.2 Data

For the estimation of employment effects, I use administrative data from the Federal and Regional Statistical Agencies (Regionaldatenbank 2018). I use information on employment stocks for 401 administrative districts (Landkreise) and four industries. Specifically, the data provides the yearly average employment stock for every administrative district by industry, where employment is defined as dependent employment, thus excluding self-employment. Regular and marginal employment are, however, not distinguished. Information on GDP and population is obtained from the same data source.

Industries are aggregated into six categories and classified according to their respective NACE Rev. 2 industry code:[4]

- Agriculture, forestry and fishing (A)
- Mining, manufacturing, electricity and gas, water supply, sewage and waste (B-E)
- Construction (F)
- Wholesale and retail trade, transportation and storage, accommodation and food service, information and communication (G-J)
- Finance and insurance, real estate, professional, scientific and technical activities, administrative and support services (K-N)
- Public sector, defense and social security, education, health and social work, arts, entertainment and recreation, other services, household production and services, activities of extraterritorial organizations (O-U)

Since there are sectoral minimum wages in place in agriculture, forestry and fishing as well as construction, I exclude these two categories from the analysis. Some of the other categories include industries with sectoral industries as well. However, as those are often relatively small sub-sectors[5], they cannot be excluded separately.

To obtain the bite measure, I again use data from the SOEP. Subject to additional data protection requirements the SOEP allows users to match individuals with regional identifiers. This way, every individual in the sample can be classified into one of 96 planning

[4]NACE Rev.2 is Eurostat's statistical classification of economic activities (Eurostat 2008). The letters in parentheses denote the NACE Rev. 2 codes of the industries included in the respective aggregation.

[5]For example, there exists a sectoral minimum wage for industrial cleaning, which is classified into administrative and support services.

regions (Raumordnungsregionen). These planning regions are defined by the federal states and account for factors such as demographic structure, economic activity, commuter flows and infrastructure (BBSR 2018). Using individual-level SOEP data in combination with the regional identifiers, I compute the fraction of individuals in every of the 96 regions who had a wage below 8.50 € in 2014 to obtain the bite measure $bite_{r,2014}$. Thereby, as in my sample for the estimation of employment retention probabilities, I only consider individuals who are subject to the minimum wage law, exclude respondents interviewed in January and winsorize the data at the first and last wage percentile.[6] However, to maximize the comparability of the results with those obtained by Caliendo et al., I use, as do they, *contractual* hours worked to compute hourly wages. In the next step, I aggregate the 401 administrative districts to match the 96 planning regions and combine the employment figures with the bite measure. This allows me to estimate equation (6.4). The adjusted bite measure Z^i_{rt} needed to estimate equation (6.6) is then simply obtained by multiplying the bite measure with the share of the respective industry in total employment in a given region.

The data I use differs from the data employed in Caliendo et al. along several dimensions. The authors use data from the Statistics Bureau of the Federal Employment Agency (BA 2018), which has the advantage of distinguishing regular and marginal employment on a regional level, but does not offer the appropriate aggregation of employment figures by region *and* industries as needed for my analysis. The wage data in Caliendo et al. draws from the *Verdienststrukturerhebung (VSE) 2014*, which covers around 70,000 firms representatively chosen firms and 1 million workers. Workers' income and hours are reported by employers, who are required by law to provide this information.[7] Caliendo et al. classify regions into 141 regional labor markets as suggested by Kosfeld and Werner (2012) rather than using the 96 planning regions.[8] Finally, they use the Kaitz-index as an additional bite measure. In light of the problems with this index, I refrain from doing the same.[9]

[6]As noted previously, it is only possible to exclude industries with sectoral minimum for the calculation of the bite measure, whereas the employment figures in the administrative data still include some industries with sectoral minimum wages.

[7]Data access to the VSE 2014 has to be specifically requested by scientific institutions and is subject to certain requirements. See http://www.forschungsdatenzentrum.de. Since I have access to SOEP data, I use this instead.

[8]Caliendo et al. do, however, use the planning regions in a robustness check and obtain similar results.

[9]The Kaitz-index sets the level of the minimum wage in relation to the mean. In addition to being susceptible to outliers, the mean wage is directly affected by a change in the minimum wage, which in turn changes the Kaitz-index (Dolado et al. 1996).

Table 6.1: Summary statistics: Bite of the minimum wage in 2014

N	Mean	SE	Min	Max	Range	33th percentile	Median	67th percentile
96	0.136	0.06	0.027	0.317	0.29	0.104	0.131	0.159

Sources: Regionaldatenbank (2018); SOEP v.33; compare to table 2 in Caliendo et al. (2017a)

6.3 Results

Graphical analysis

The comparison of pre- and post-treatment outcomes for groups that are differently affected by the minimum wage requires that employment would have evolved similarly across groups absent treatment. Since the treatment variable is now continuous, there is no strictly defined treatment and comparison group. Therefore, I follow Caliendo et al. and order all regions in ascending order of the bite measure, that is, the fraction of individuals in a given region who earned less than 8.50 € in 2014, and create three groups of equal size. Since I have 96 planning regions, I obtain three groups containing 32 regions each, with the respective thresholds being at the 33th and the 67th percentile.[10] Table 6.1 shows summary statistics for the bite measure. The average bite per region is 13.6 percent.[11] The bite at the 33th and 67th percentile threshold is at 10.4 percent and 15.9 percent, respectively.

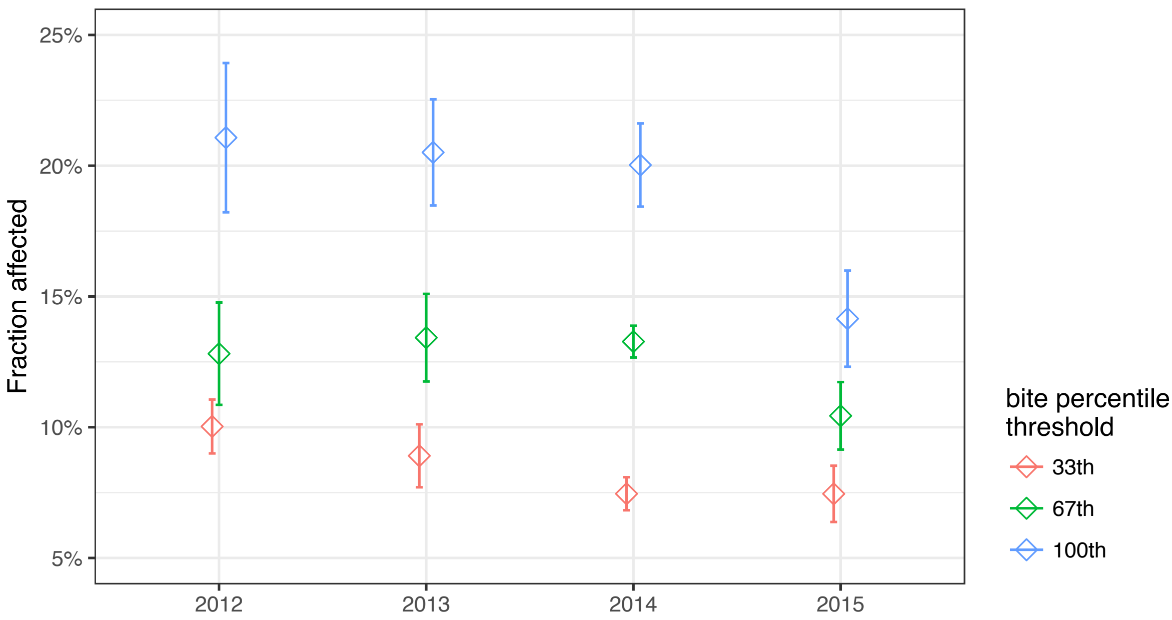
However, similarly to the analysis of employment retention probabilities, I begin by investigating whether wages have increased more substantially in the regions most affected by the minimum wage. Figure 6.1 shows how the bite of the minimum wage for the three impact groups has evolved between 2012 and 2015. The regions have been sorted into the three groups according to their bite level in 2014. The group of regions that are most affected are shown in blue while the least affected regions are depicted in red. The figure clearly shows that the fraction of individuals earning less than 8.50 € substantially decreased for the most affected regions from 2014 to 2015. For the middle group the decline was notable as well, while the least affected regions experienced almost no change in the measured bite. Thus, the more affected a region was by the minimum wage, the greater the impact of its introduction on the bite measure. This is exactly what one would expect if the minimum wage was effective in increasing the wages of low-wage workers.

Next, I investigate graphically whether the parallel trends assumption in employment holds. In order to do so, I examine how employment evolved in the three impact groups. The graph on the left-hand side of figure 6.2 shows the growth in employment between 2012

[10] Henceforth I will call these three groups *impact groups*.

[11] This is slightly bigger than the 12.1 percent Caliendo et al. find when using the SOEP as an alternative data source in a robustness check. However, this is due to the fact that their bite measure is based on a different area classification (see Caliendo et al. for details).

Figure 6.1: The minimum wage bite over time



Sources: Regionaldatenbank (2018); SOEP v.33; R package by Wickham (2009); compare to figure 2 in Caliendo et al. (2017a); whiskers denote the 95 percent confidence interval

and 2015 for dependent employment in Germany. Overall, employment growth evolves in a parallel fashion in all three groups with growth being lower in the most affected regions. From this graphical representation, no indication for a violation of the parallel trends assumption is given.

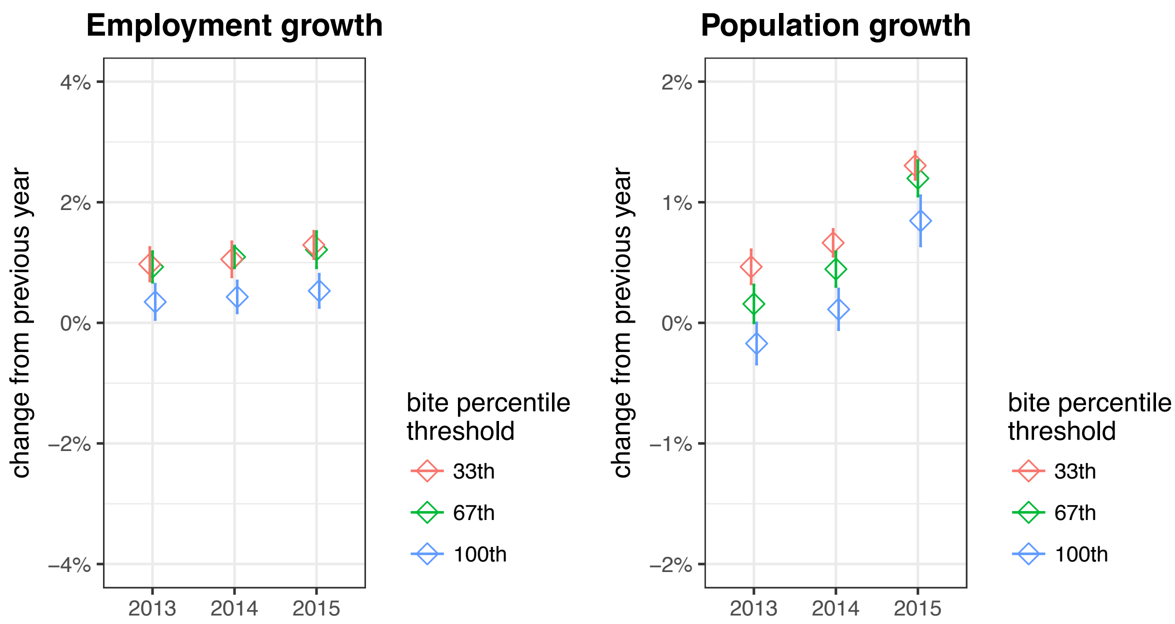
Caliendo et al. rightly argue that it is important to account for the population level in the analysis since employment and population level are clearly interdependent. In addition, there has been large influx of Syrian refugees into Germany in 2015.[12] Trends in population growth are shown in the graph on the right-hand side of figure 6.2.[13] Population growth in strongly affected regions is negative until 2014 and increases notably with the migration inflow in 2015. Therefore, low-wage regions suffered population outflows in the period preceding the minimum wage, which may have also affected employment. This, again, emphasizes that it is crucial to control for the population level.

Figure A.2 shows employment growth for the four industry aggregations used in the analysis. Now I use the adjusted bite measure instead to construct the three impact groups. The industry-specific employment growth rates do not evolve as homogeneously as was the case for overall employment growth. For industries B-E, where manufacturing constitutes

[12]However, since many of the refugees were not allowed to work, employment levels are not necessarily affected.

[13]The population levels, from which I compute the growth rates are are recorded each year with the cutoff date December 31.

Figure 6.2: Employment and population growth, overall



Sources: Regionaldatenbank (2018); SOEP v.33; R package by Wickham (2009); compare to figure 3 in Caliendo et al. (2017a); whiskers denote the 95 percent confidence interval

the biggest group, employment growth is positive, but slows down for the most and the least affected group over the span 2012 to 2015. The middle group, however, seems to experience negative employment growth from 2012 to 2013 and thereafter follows the same trend as the least affected group. Moreover, confidence intervals are relatively large, so it is difficult to draw substantive conclusions from the graph with respect to differences in employment growth. Industries G-J, which include retail trade, transportation and food services, similarly do not paint a clear picture. While the most and the least affected seem to grow more strongly from 2013 to 2014 and then slow down in growth, growth for the middle group seems to have been relatively stronger from 2012 to 2013 and continuously declined thereafter. However, once again the confidence intervals are large. Industries K-N, which include finance and insurance, real estate and administrative and support services, experienced a positive trend in employment growth that seemed particularly strong for the middle group from 2014 to 2015. Yet here, too, standard errors are too large to draw meaningful conclusions. Lastly, in industries O-U, where the public sector, education and health and social work are the largest groups, employment seems to grow roughly in a parallel fashion. Notably, growth rates for all three groups decline from 2013 to 2014 and then increase from 2014 to 2015 when the minimum wage was introduced. However, the *difference* in employment growth seems to have been somewhat increased between the middle and the least affected group on the one hand and the most affected group on the other hand.

In summary, the graphical analysis of employment growth in the four industry aggregations gives no clear pattern. Clear conclusions cannot be drawn, neither with respect to the parallel trend assumption, nor regarding potential effects of the minimum wage. However, confidence intervals are in many cases too large to reject the hypothesis of a zero difference in employment growth between the groups. This favors the common trend assumption since a violation thereof would mean that pre-treatment trends between groups *are significantly different*. Finally, note that growth rates are in a very narrow percentage range across industries and years, making it even more difficult to draw meaningful conclusions from the graphical analysis alone.

Regression results

Table 6.2 shows estimation results for equation (6.4) and thus reproduces the main results of Caliendo et al. with the data used in my analysis.[14]

Table 6.2: employment effects, overall

	(1)	(2)	(3)	(4)
bite x 2015	-0.0554***	-0.0363***	-0.0219*	-0.0154
	(0.0124)	(0.0116)	(0.0117)	(0.0111)
Population		0.585***	0.774***	0.919***
		(0.185)	(0.168)	(0.143)
GDP		-0.0466	0.0556	0.0681
		(0.0510)	(0.0419)	(0.0410)
bite x 2013			0.0186	0.0133
			(0.0165)	(0.0161)
bite x 2012				0.0272
				(0.0246)
Year/region fixed effects	Yes	Yes	Yes	Yes
R^2 within	0.670	0.710	0.761	0.789
R^2 between	0.00716	0.965	0.979	0.979
R^2 overall	0.0685	0.965	0.979	0.979
N	192	192	288	384

Sources: Regionaldatenbank (2018); SOEP v.33; compare to table 3 in Caliendo et al. (2017a)
Cluster-robust standard errors in parentheses; standard errors clustered at the regional level
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column (1) shows the treatment effect of the minimum wage without any control variables. The obtained estimate is highly significant, but decreases notably once population and GDP are controlled for, as depicted in column (2). Like Caliendo et al., I find that population plays a crucial role, while GDP seems to have no effect. The model improves substantially, particularly the between R squared, suggesting that population explains a

[14] see section 6.2 for details on the industries in the aggregation.

Table 6.3: Comparison of estimated employment effects

	My results	Caliendo et al. (2017a)	
	overall employment	regular employment	marginal employment
including controls	191,000***	78,000**	183,000***
controls + pre-treatment trends	114,000*	54,000*	183,000***

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

significant proportion of the variation in employment between regions. The coefficient of -0.0363 implies that, all else equal, an increase in the bite of the minimum wage by one percentage points decreases employment by 0.0363 percent. Multiplying this by the average regional bite of 0.136 results in an average effect of the minimum wage on employment of -0.5 percent. Since the employment level in 2014 corresponded to 38.26 million, this implies a loss of around 191,000 jobs. This is less than the results of Caliendo et al. suggest, who estimate employment losses of 78,000 and 183,000 in regular and marginal employment, respectively (see table 6.3). However, as noted in section 3.2, the authors qualify their finding of such a high negative employment effect on minijobs since their results show a significant negative effect on marginal employment already from 2013 to 2014, suggesting that the common trend assumption does not hold (cf. Caliendo et al.).[15]

Column (3) shows the results when interaction terms of the bite with the years 2012 and 2013 are included.[16] Similarly to my analysis of employment retention probabilities this allows for testing the parallel trends assumption. Moreover, the estimate for 2015 is more precise since pre-treatment trends in employment are controlled for. The interaction terms for 2012 and 2013 are insignificant, suggesting that the parallel trend assumption is not violated. However, as seen in columns (3) and (4), taking pre-treatment trends into account reduces the estimated employment effect to a weakly significant or even insignificant level. This is again in line with the findings of Caliendo et al., who as well obtain weakly significant or even insignificant estimates when the years 2012 and 2013 are included.

In summary, it can be concluded that despite using a different combination of data sets, I was able to replicate the analysis of Caliendo et al. and obtain very similar results. In the following, I will present the results of my extension where I estimate industry-specific employment effects of the minimum wage. These are shown in tables 6.4 to 6.7.

Across columns (1) to (3), the estimated employment effect of the minimum wage is insignificantly different from zero and even positive in sign for industries B-E. Similarly

[15] Recall as well from section 3.2 that this estimate is also larger than what previous studies (Garloff 2016; Vom Berge et al. 2017) find.

[16] Note that 2014 is the reference year, so every interaction term has to be interpreted relatively to this year.

to before, adding controls for population and GDP reduces the estimate and particularly population appears crucial in explaining much of the variation in employment within and between regions.[17] That there is no visible effect of the minimum wage on employment is unsurprising, given that the largest sector in this aggregation is manufacturing, where wages are traditionally high. Furthermore, the interaction terms of the adjusted bite measure with the years prior to the minimum wage are all insignificant. In accordance with the large confidence intervals observed in the graphical analysis, the estimation results as well suggest that the common trend assumption was not violated.

Table 6.4: employment effects, B-E

	(1)	(2)	(3)
adj. bite x 2015	0.0157	0.0106	0.00944
	(0.0972)	(0.0567)	(0.0601)
Population		0.922***	1.020***
		(0.0794)	(0.0438)
GDP		0.0126	0.0124
		(0.0194)	(0.0124)
adj. bite x 2013			-0.0596
			(0.0415)
adj. bite x 2012			-0.0910
			(0.0800)
Year/region fixed effects	Yes	Yes	Yes
R^2 within	0.0755	0.748	0.861
R^2 between	0.0000105	0.980	0.980
R^2 overall	0.0000566	0.980	0.981
N	192	192	384

Sources: Regionaldatenbank (2018); SOEP v.33
Cluster-robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For industries G-J, the estimated employment effect is negative throughout, yet insignificant. As before, population level turns out to be a crucial control and there is no indication for a violation of parallel trends. At first glance, this may be surprising since this industry aggregation is dominated by the sectors wholesale and retail trade, transportation and storage as well as accommodation and food services, all of which are rather low-paying sectors.[18] Solely the information and communication sector is very little affected by the minimum wage in this aggregation. However, recall from the analysis

[17]Information on GDP per region and industry aggregation is readily available in the data obtained from the Regionaldatenbank (2018). For population I use the regional population level and adjust it for the respective industry share.

[18]My SOEP sample shows that the share of individuals earning less than 8.50 € in these three sectors is above the average level of affectedness. However, since the number of observations per sector is small for some industries (overall ranging between 4 and 1,103), I refrain from stating exact figures for the SOEP data with respect to the level of affectedness per industry.

of non-compliance in section 5.2 and A.2 that the share of non-compliers is highest in wholesale and retail trade and accommodation and food services. This may provide an explanation for why no significant employment effects of the minimum wage are visible in sectors G-J.

Table 6.5: employment effects, G-J

	(1)	(2)	(3)
adj. bite x 2015	-0.0451	-0.0667	-0.0599
	(0.100)	(0.0404)	(0.0417)
Population		1.031***	0.982***
		(0.102)	(0.0557)
GDP		-0.00339	0.0300
		(0.0170)	(0.0210)
adj. bite x 2013			0.0434
			(0.0577)
adj. bite x 2012			0.0797
			(0.0814)
Year/region fixed effects	Yes	Yes	Yes
R^2 within	0.0715	0.816	0.851
R^2 between	0.000895	0.975	0.978
R^2 overall	0.0339	0.975	0.978
N	192	192	384

Sources: Regionaldatenbank (2018); SOEP v.33
Cluster-robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For industries K-N, the picture changes notably. When employment level and GDP are controlled for, there is a negative effect of -0.151 percent. Multiplying this with by average adjusted bite measure (which is 0.019) and the 2014 employment level in industries K-N of 6.245 million, implies a loss of roughly 18,000 jobs. Controlling for pre-treatment trends slightly reduces the estimated impact to a reduction of roughly 16,000 jobs. While this industry aggregation does include rather high-paying sectors such as the financial and insurance industry, roughly one-third of it is made up of administrative and support services, according to data from the Federal Employment Agency (BA 2018). These include, for example, the labor leasing industry, where low wages are particularly prevalent.[19]

Industry aggregation O-U includes sectors where low wages are rare, such as the public sector and education, as well as sectors with a high incidence of low pay, such as human

[19]Workers with labor leasing contracts (Leiharbeit) are employed at agencies who rent out their workforce to firms for a limited amount of time. These workers typically earn wages that are substantially lower than those of regular workers. In Germany, there are more than 800,000 individuals under labor leasing contracts (BA 2018).

Table 6.6: employment effects, K-N

	(1)	(2)	(3)
adj. bite x 2015	-0.370	-0.151**	-0.134*
	(0.274)	(0.0688)	(0.0692)
Population		0.992***	1.051***
		(0.0416)	(0.0272)
GDP		-0.0560*	-0.0330
		(0.0310)	(0.0233)
adj. bite x 2013			0.0936
			(0.0840)
adj. bite x 2012			0.163
			(0.117)
Year/region fixed effects	Yes	Yes	Yes
R^2 within	0.445	0.939	0.950
R^2 between	0.000362	0.980	0.982
R^2 overall	0.0313	0.980	0.982
N	192	192	384

Sources: Regionaldatenbank (2018); SOEP v.33
Cluster-robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

health and social work[20] or other services, which includes hairdressers, cleaning services or simple repair services. Taken together, this sectoral aggregation accounted for 12.12 million employees in 2014. The estimated employment effects are highly significant. Multiplying the estimate in column (2) with the average adjusted bite measure (which is 0.045) results in an estimated employment loss of about 55,000. Controlling for pre-treatment trends, the estimated effect reduces to roughly 43,000 lost jobs. While it is impossible to accurately attribute the estimated losses to specific sectors given the data at hand, the minimum wage seems to have reduced employment in these industries. However, it should be noted that the interaction of the adjusted bite measure with the year 2012 is significant. Since this suggests that the common trend assumption is violated, I remain cautious in interpreting the estimate.

Discussion

In this section, I have replicated the analysis of Caliendo et al. and extended it by estimating industry-specific employment effects of the minimum wage. Estimating their model with my data and controlling for population and GDP, I find a highly significant negative overall employment effect of the minimum wage, implying reduced employment in the magnitude of 191,000 jobs. While this effect is smaller than the sum of employment

[20]There exists a sectoral minimum wage in the care sector, which, however, does not extend to all type of employees in the industry (Harsch and Verbeek 2012).

Table 6.7: employment effects, O-U

	(1)	(2)	(3)
adj. bite x 2015	-0.220***	-0.100***	-0.0791***
	(0.0423)	(0.0305)	(0.0296)
Population		0.706***	0.776***
		(0.108)	(0.0818)
GDP		-0.00235	0.101**
		(0.0484)	(0.0479)
adj. bite x 2013			0.0470
			(0.0413)
adj. bite x 2012			0.110**
			(0.0505)
Year/region fixed effects	Yes	Yes	Yes
R^2 within	0.758	0.870	0.882
R^2 between	0.00467	0.969	0.976
R^2 overall	0.0381	0.969	0.977
N	192	192	384

Sources: Regionaldatenbank (2018); SOEP v.33
Cluster-robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

effects Caliendo et al. found for regular and marginal employment, their estimate for marginal employment is likely inflated by the decrease in mini-jobs prior to the introduction of the minimum wage. Subsequently adding interactions with the years 2013 and 2012 reduces my estimated overall employment effect to 114,000 lost jobs and to a non-significant level, respectively. By industry, I find significant employment effects only for the industry aggregations K-N as well as O-U, which include low-wage sectors such as administrative and support services and personal services, but also high-paying industries like the financial sector. An individual disaggregation by sector is unfortunately not possible with the data at hand. Surprisingly, no negative employment effect was detected in the industries G-J, which include retail trade and food services, where the incidence of low pay is high. However, as was shown previously, non-compliance in these sectors is substantial.

One may also notice that the industry-specific estimates do not always add up to the estimate obtained when the model is estimated on overall employment. Partly, this can be attributed to aggregation error. Furthermore, it is likely that disemployment effects that were imprecisely estimated in the industry-specific estimation (i.e. for the retail trade and food services sectors) are likely to contribute to the estimate obtained from the estimation of overall employment effects. However, the difference is particularly striking when one considers the estimates from column (2), that is, when controlling for GDP and population, but not for pre-treatment trends. To investigate this further I re-estimated the industry-specific model on the construction sector.[21] The construction sector is governed by a sectoral minimum wage since 1997, which lies markedly above the national minimum wage.[22] Furthermore, previous studies have found virtually no disemployment effects of this sectoral minimum wage (Möller and König 2008). Hence, the introduction of the national minimum wage should not have any impact on the construction sector. Yet, estimating the model without controlling for pre-treatment trends, I still find a statistically significant (albeit small) disemployment effect of nearly 8,000 jobs in an industry with roughly 1.9 million workers. This effect only begins to vanish once pre-treatment trends are accounted for. Therefore, I argue that controlling for pre-treatment trends is crucial and the estimates obtained by doing so should be given most credibility. This implies that the true employment effects are likely to be found in the lower range of the estimated effects found in Caliendo et al. as well as in my own analysis.

[21] Results not shown.

[22] As of January 2017, the minimum wage in the construction sector is at 11.30 € for both unskilled and skilled workers in East Germany and 11.30 € for unskilled and 14.70 € for skilled workers in West Germany (BMAS 2018).

Conclusion

In this thesis I use individual-level survey data from the Socio-Economic Panel (SOEP) to examine the impact of the introduction on the German minimum wage on the employment retention probabilities of those directly affected: individuals whose wages would have had to be raised to comply with the minimum wage. I find that the probability of remaining in employment for regular employment decreased between about 3 and 5.5 percentage points for individuals in full-time and part-time employment. These estimates further increase when only those are considered for whom the required wage raises were particularly large, by a magnitude of about 3 percentage points.

In contrast to previous studies examining the employment effects of the German minimum wage, the individual-level data used in my analysis allows me to specifically address the observed high degree non-compliance with the minimum wage. Restricting the sample to compliers, the estimated decrease in employment retention probabilities is substantially higher. While treating the estimates obtained from this specification with caution, I find that the probability of non-employment following the introduction of the minimum wage increased by up to 13.5 percentage points for full-time and part-time employees and up to almost 18 percentage points for individuals in marginal employment. Due to a high incidence of low wages, the marginally employed were considered particularly affected by the minimum wage. However, non-compliance has likely prevented the negative employment effect of the minimum wage on this group to show through in the previous specifications.

My findings are consistent with estimates from other countries obtained from minimum wage increases, which typically are in the range of 3 to 8 percentage points (Currie and Fallick 1996; Campolieti, Fang, and Gunderson 2005). The only other study to my knowledge which estimates the impact of the *introduction* of a minimum wage on low-wage employment probabilities finds no effect (Stewart 2004). However, the intervention into the wage distribution was much smaller in the U.K. (roughly 5 percent affected).

Lastly, I estimate the industry-specific impact of the minimum wage on aggregate employment in an extension to Caliendo et al. (2017a). I find significant disemployment effects in only two of the four industry aggregations considered. Surprisingly, no significant effect was found in the retail and food service industry, where low wages are particularly prevalent. However, these sectors are also characterized by a high degree of non-compliance.

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Appendix

Table A.1: Probability of non-compliance A

	By worker and firm characteristics
Minijob	0.0850***
	(0.0103)
Female	0.0394***
	(0.00613)
Age	0.00109**
	(0.000502)
Married	-0.00953*
	(0.00562)
East Germany	0.0844***
	(0.00774)
Education (in school)	
Primary education	0.0715*
	(0.0428)
Lower secondary education	0.00575
	(0.0346)
Upper secondary education	-0.0235
	(0.0337)
Post-secondary non-tertiary education	-0.0536
	(0.0344)
Short-cycle tertiary education	-0.0934***
	(0.0360)
Bachelors or equivalent level	-0.0575*
	(0.0345)
Masters or equivalent level	-0.0883**
	(0.0352)
Doctoral or equivalent level	-0.141***
	(0.0366)
Years at current employer	-0.00460***
	(0.000445)
Experience in years, part time	-0.000600
	(0.000696)
Experience in years, full time	-0.00302***
	(0.000548)
Firm size (less than 20 employees)	
between 20 and 200 employees	-0.0564***
	(0.00745)
more than 200 employees	-0.0861***
	(0.00705)
Required skill level of occupation (unskilled)	
skilled	-0.108***
	(0.00675)
managerial position	-0.141***
	(0.00902)
N	21354

Cluster-robust standard errors in parentheses; I report Average Marginal Effects

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Probability of non-compliance B (NACE Rev. 2)

	By industry
base category: public sector, defense and social security	
Manufacturing	0.0429***
	(0.00818)
Energy and gas	0.000254
	(0.0195)
Wholesale and retail trade	0.122***
	(0.0361)
Transportation and storage	0.0912***
	(0.0224)
Accomodation and food services	0.305***
	(0.0450)
Information and communication	0.0472
	(0.0445)
Financial services and insurance	-0.0240**
	(0.0101)
Real Estate	0.106**
	(0.0415)
Professional, scientific and technical activities	0.0717***
	(0.0253)
Administrative and support services	0.217
	(0.155)
Education	0.0464***
	(0.0106)
Health and social work	0.0690***
	(0.0111)
Arts, entertainment and recreation	0.0260
	(0.0390)
Other services	0.0403*
	(0.0226)
Household production and services	0.438***
	(0.0571)
Activities of extraterritorial organisations	0.00266
	(0.0527)
N	13963

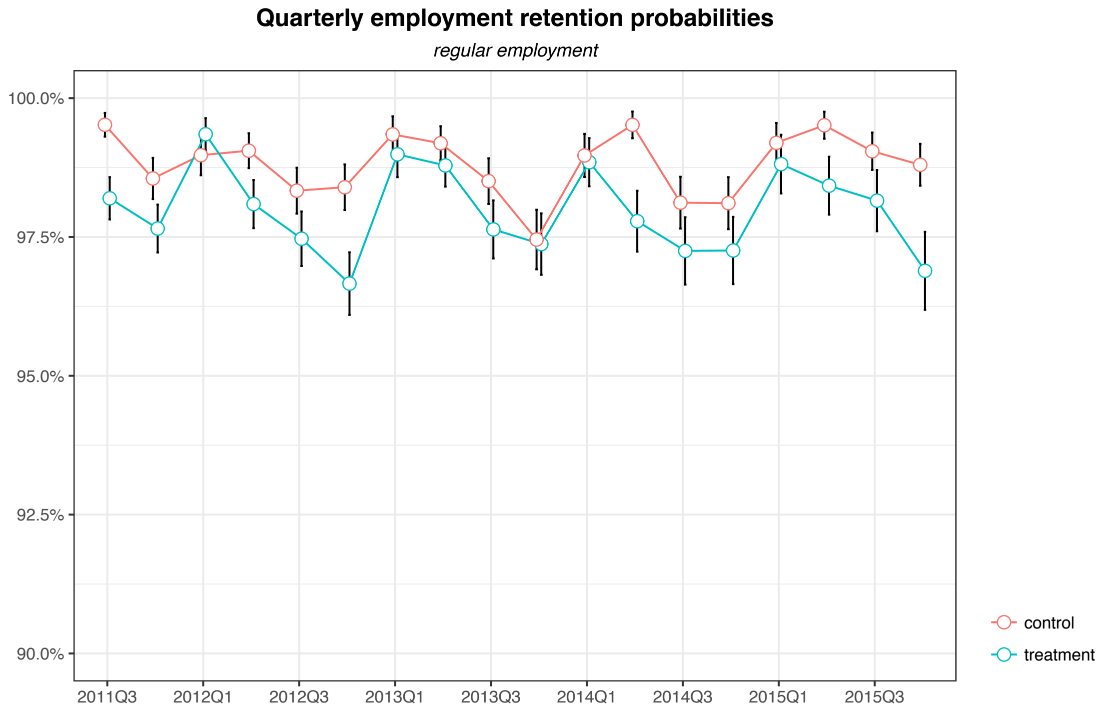
Cluster-robust standard errors in parentheses; I report Average Marginal Effects

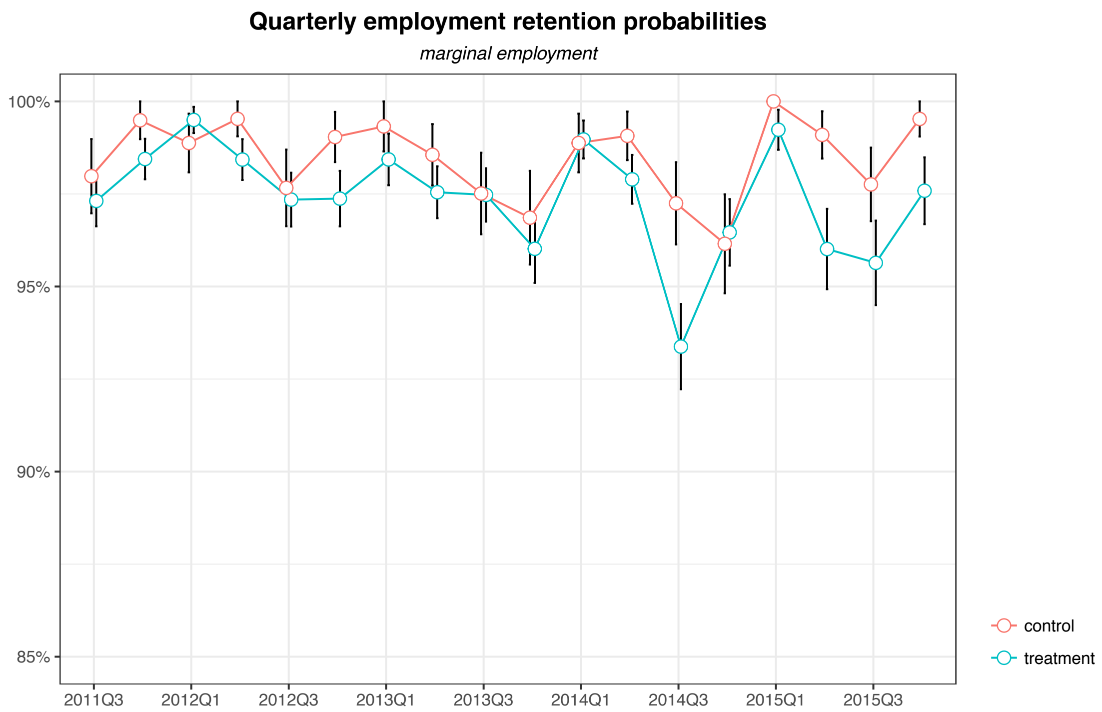
NACE Rev.2 is Eurostat's statistical classification of economic activities (Eurostat 2008)

Industries with very few observations were excluded

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

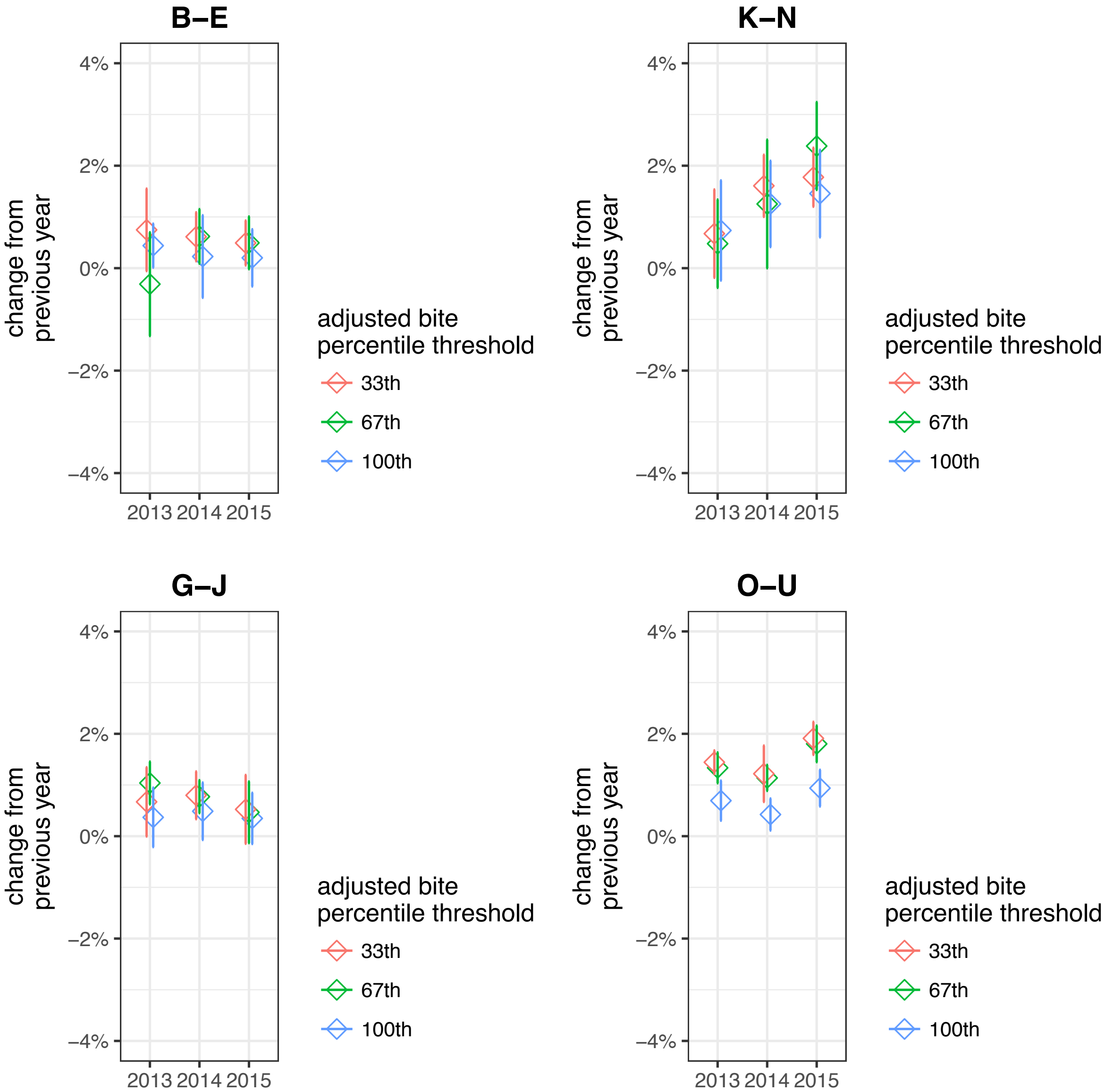
Figure A.1: Employment retention probabilities 2011 - 2015





Sources: SOEP v.33; R package by Wickham (2009); whiskers denote the corresponding standard errors

Figure A.2: Employment growth by industry (NACE Rev. 2)



Sources: Regionaldatenbank (2018); SOEP v.33; R package by Wickham (2009); whiskers denote the 95 percent confidence interval