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DISCUSSION PAPER SERIES

IZA DP No. 11190

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# ABSTRACT

## The Short-Run Employment Effects of the German Minimum Wage Reform*

We assess the short-term employment effects of the introduction of a national statutory minimum wage in Germany in 2015. For this purpose, we exploit variation in the regional treatment intensity, assuming that the stronger a minimum wage "bites" into the regional wage distribution, the stronger the regional labour market will be affected. In contrast to previous studies, we draw upon detailed individual wage data from the Structure of Earnings Survey (SES) 2014 and combine it with administrative information on regional employment. Moreover, using the Socio-Economic Panel (SOEP), we are able to affirm the absence of anticipation effects and verify the assumption of a common trend in wages before the reform. Based on hourly wages, we compute two regional bite indicators – the share of affected employees and the Kaitz index – for 141 regional labour markets. In order to get a broader picture, we construct and compare a variety of these measures, including a bite based on full-time workers only. All of these display a considerably strong correlation. Overall, we do not find a pronounced significant effect on regular (full- and part-time) employment in most specifications, although some estimations yield a small significant reduction amounting to 78,000 (roughly 0.3% of all regular jobs). The results concerning marginal employment are more pronounced. We find evidence that mini-jobs dropped substantially from 2014 to 2015, making for a reduction of about 180,000 jobs (about 2.4% of all mini-jobs). This result is robust to a variety of sensitivity tests.

**JEL Classification:** J23, J31, J38

**Keywords:** minimum wage, regional bite, employment effects

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# 1 Introduction

In January 2015, the German labour market was exposed to a massive intervention in its wage structure with the introduction of a national statutory gross minimum wage of €8.50 per hour applying to nearly all employees. The introduction of the minimum wage was preceded by a long debate among German economists and policy-makers about its potential risks and benefits. Advocates emphasised the primary policy targets of poverty prevention and inequality reduction (Bosch, 2007; Kalina and Weinkopf, 2014; BMAS, 2014), while opponents stressed the economic burden of the reform. Due to its high level and – with only a few exemptions – universal character, it was expected to affect more than one in ten employees in Germany, potentially leading to extensive job destruction (SVR, 2013, 2014; Müller and Steiner, 2010, 2011, 2013; Knabe *et al.*, 2014). Accordingly, the aim of our paper is to examine whether these earlier expectations have actually proven to be true in the short run.

In theory, the potential effects of minimum wages on labour demand depend on the market structure. While negative employment effects are expected in a competitive price-taker setting, a monopsonistic labour demand does not imply negative effects in general. Depending on the minimum wage level, the demand for labour may increase when employees are paid below the marginal product of labour. Moreover, the time frame in which employment effects should arise has not been determined and other adjustment channels might be used in the short run to postpone displacements, e.g. working hours (Stewart and Swaffield, 2008), profits (Draca *et al.*, 2011) or simply non-compliance (Metcalf, 2008). Identifying employment effects is therefore an empirical question that has been addressed with a variety of strategies and – in most cases – has come up with no or weak negative employment effects (Neumark and Wascher, 2007; Card and Krueger, 1995). Unfortunately, due to the universal validity of the reform, the set of empirical identification strategies is considerably restricted in the German case. We base our analysis on the approach suggested by Card (1992), which relies on the degree to which regional labour markets are affected by the minimum wage. Between regions, earnings and wages differ due to structural and environmental differences. This variation implies that a nominal minimum wage affects regions to different intensities. The stronger that a minimum wage 'bites' into the regional wage distribution, the stronger the regional labour market is affected. We adapt this approach to the German case and apply a difference-in-difference framework to analyse short-term effects of the minimum wage on employment for the first year after the policy reform.

Since the definition of the treatment indicator – i.e. the degree to which a region is affected by the minimum wage – is crucial to our identification strategy, we construct two commonly-used bite measures: the *Fraction* and the *Kaitz index*. The Fraction reflects the share of affected eligible employees per region, while the Kaitz index displays the relation of the minimum wage to the regional mean wage. Moreover, the construction of the regional bite also calls for a definition of a suitable area classification. We rely on 141 distinct regional labour markets (RLMs) as proposed by Kosfeld and Werner (2012). Since this approach aggregates areas according to economic performance and commuter flows even across federal states, it allows constructing credible RLMs. We draw upon data from the comprehensive Structure of Earnings Survey (SES) 2014, which contains detailed individual information on wages and working hours. In contrast to previous studies (e.g Garloff, 2016) focusing on full-time workers and their monthly earnings, this enables us to compute a more precise bite measure based on individual *hourly* wages. Therein, we can include not only full-time employed individuals but also part-time workers and marginally employed (in Germany also called 'mini-jobbers')[1], which is especially important as the latter two groups were most strongly affected by the minimum wage introduction.

The minimum wage introduction was preceded by a legislative process that allows for potential anticipation effects, which have largely been neglected by previous studies on Germany. Since our identification strategy depends on the assumption that wages would have developed equally among low- and high-bite regions in the absence of the minimum wage reform, we have to test the notion that wages were not adapted in anticipation. For this purpose, we need to make use of data on the pre-treatment period. Unfortunately, the SES is only available every four years. Therefore, we additionally employ the German Socio-Economic Panel (SOEP), which is smaller in sample size but conducted every year. With the annual SOEP data, we can consider hourly wages in a time frame before the minimum wage was decided upon, allowing us to explore wage effects and their potential anticipation. The bite measures constructed with SES and SOEP display strong positive correlations. Accordingly, despite the SOEP's smaller sample size, its measures identify a similar variation in regional treatment intensity compared with the SES indicators. The pre-treatment analysis reveals no anticipatory effects, meaning that prior to the reform wages followed the same trend across regions with different treatment intensities. However, with the policy reform, the share of individuals earning less than the minimum wage

[1]Marginal employment (throughout the paper also called mini-jobs) is a specific type of job in Germany with an income of up to €450 per month, which requires (almost) no employee-sided social security contributions. By contrast, regular employment refers to employment that entails social security contributions, i.e. full- and part-time employment.

substantially decreased in affected areas, while the mean wage – and thus the Kaitz – has hardly been affected.

For our estimation of employment effects, we combine our bite measures with administrative data on employment stocks from the Federal Employment Agency ('Bundesagentur für Arbeit', FEA), measuring the development of employment from 2012 onwards. Overall, we do not find a pronounced significant effect on regular (full- and part-time) employment in most specifications, although some estimations yield a small significant reduction amounting to 78,000 jobs (corresponding to roughly 0.3% of all regular jobs). However, marginal employment was significantly reduced after the minimum wage reform. Our results identify a loss of about 180,000 mini-jobs (corresponding to about 2.3% of all mini-jobs). These results are robust across all specifications and a variety of sensitivity tests. Our results are roughly in line with the previous literature on short-term employment effects in Germany, indicating that adaptation within the extensive margin of labour demand was less strong than expected. Using employer survey data, Bossler and Gerner (2016) find that the minimum wage led to the absence of about 60,000 new hirings, while Garloff (2016) – also applying the identification strategy by Card (1992) – identifies no effect on regular employment, but finds evidence of a shift from marginal to regular employment.

The remainder of this paper proceeds as follows. Section 2 introduces the legal framework of the German minimum wage and discusses expectations and previous findings. Section 3 considers the identification strategy, its implementation and our data sources. Subsequently, Section 4 displays the descriptive results for our bite measures and the employment data, while Section 5 presents the main analysis of employment effects as well as robustness analyses. Finally, Section 6 concludes.

## 2 Institutional Details and Expectations

**Institutional Details** On January 1, 2015, the Minimum Wage law ('Mindestlohngesetz') entered into force, introducing a minimum wage of €8.50 gross per hour. Until then, wage floors were set by collective, voluntary agreements within specific sectors.[2] The formal decisions about future adjustments to the minimum wage are to be made by the German Minimum Wage Commission ('Mindestlohnkommission'). In light of minor short-term employment effects, the

[2]Sector-specific minimum wages had been introduced over the last two decades in several sectors, including the construction sector or the roofing sector (in 1997), hair dressing (in 2013) and security services (in 2011). Most sector-specific minimum wages are higher than the statutory minimum wage and have been increased after the uniform minimum wage (Amlinger *et al.*, 2016). See Fitzenberger and Doerr (2016) for an overview.

Minimum Wage Commission has recommended raising the statutory minimum wage by 34 cents per hour starting from January 1, 2017 (see Mindestlohnkommission, 2016a).

With the 2015 regulations, almost any employee in Germany is eligible for the minimum wage. Restrictions have only been introduced with respect to two dimensions. First, specific groups are excluded, namely the self-employed, trainees, specific types of interns[3], minors without vocational training, volunteers and the long-term unemployed. Second, albeit temporarily, sector-specific minimum wages under the national level of €8.50 remained valid until December 2016 and had to be adjusted afterwards. The exemption for the long-term unemployed is rarely drawn upon (vom Berge *et al.*, 2016c) and only few sector-specific minimum wages have been below the minimum wage (Mindestlohnkommission, 2016b; Amlinger *et al.*, 2016). Nonetheless, the exception for trainees and adolescents reduces the number of eligible individuals to a great extent. Table 1 summarises the number of beneficiaries. In 2014, about 5.5 million employees earned less than €8.50 per hour, of which 4.0 million (72 percent) were eligible for the minimum wage (Destatis, 2016a).

[Include Table 1 here]

The timeline of the minimum wage introduction allows for potential anticipation effects, which previous studies on Germany have largely neglected. In September 2013, the German parliament ('Bundestag') was elected. Given that the major centre-left wing party (SPD) announced their uncompromising stance for a universal, nationwide minimum wage of €8.50 per hour, the inclusion of such a policy in the coalition contract with the major centre-right wing party (CDU/CSU) in December 2013 was commonly expected. Announced as a high-priority project in January, the law was then passed by the two German parliaments in July 2014. These decisions may have had instantaneous effects. According to Bossler (2017), employers affected by the minimum wage reported greater employment uncertainty in summer 2014. The potential anticipation of the new regulations could thus have affected employers' behaviour even before the minimum wage introduction. Therefore, in Section 4 we examine the bite indicators in the pre-treatment period.

**Expectations and Previous Findings** The wage floor of €8.50 places Germany in the middle of the international minimum wage ranking (OECD, 2015, p. 37). Expectations on its

[3] Interns are excluded if the internship is compulsory ('Pflichtpraktikum'), is either a voluntary accompanying or voluntary orientation internship ('freiwilliges Orientierungspraktikum' or 'freiwilliges ausbildungsbegleitendes Praktikum') that lasts less than three months or if it is an entry-level qualification ('Einstiegsqualifizierung').

effects on employment were predominantly negative, even though both theory and empirics are not conclusive on this topic (see Neumark and Wascher, 2007, 2008). For the long run, ex-ante simulations predicted a reduction of 500,000 to 900,000 jobs, while positive effects on poverty prevention are small due to the German in-work benefits regulations and withdrawal rates from 80 to 100 percent for households with low income (Müller and Steiner, 2013; Knabe *et al.*, 2014). Then the reform has little impact on the budgets of employees but relaxes the public budget constraints. The largest impacts on earnings are to be expected in East Germany, for women and mini-jobbers since a great share of the beneficiaries belong to one of these groups (see Table 1). In fact, 2.2 million of the 4 million beneficiaries were marginally employed. Moreover, one in five East-German residents earned less than the new wage floor in 2014, whereas only 9 percent within West Germany were affected. Differences in earnings also arise with respect to gender, since over 60 percent of all eligible employees who earned less than €8.50 were women (Destatis, 2016a).

Due to its high share of low hourly wages, mini-jobs hold special interest within the discussion about the introduction of the minimum wage. They require almost no employee-sided social security contributions, which is why gross income is nearly equivalent to net income up to monthly earnings of €450.[4] However, with the 451st euro of monthly income, a worker's status changes from marginal to part-time employment, which then entails employee-sided social security contributions.[5] Consequently, it is optimal for many marginally-employed individuals to work exactly at the kink point as it maximises their net earnings while avoiding social security payments. However, with the minimum wage introduction, these beneficiaries could easily exceed the threshold if their working hours are not reduced simultaneously. In this case, the minimum wage would lift a mini-jobber into part-time employment, which is considered regular work with respect to social security contributions. A reduction in mini-jobs thus does not necessarily result in rising unemployment, but may actually be associated with an increase in regular work, resulting in the lack of an overall employment effect. The first evidence on such a substitution is discussed by vom Berge *et al.* (2016a,b).

Thus far, the literature does not substantiate the huge negative employment effects that were foretold in ex-ante predictions (see Garloff, 2016; Bossler and Gerner, 2016; Schmitz, 2017).

[4]Mini-jobbers are required to pay contributions to the social pension fund but can be liberated from that obligation upon request.

[5]More precisely, employment situations with monthly income between €451 and €850 are considered 'Midi-Jobs.' Here, the rate of social security contributions follows a progressive design until the standard flat rate is reached.

Garloff (2016) does not find any overall employment effects, but a politically-favoured shift from mini-jobs to part-time employment. He calculates that about 66,000 mini-jobs were lost after the reform. Using employer survey data, Bossler and Gerner (2016) identify that 60,000 fewer jobs were created within firms affected by the minimum wage between 2014 and 2015. Although these results are short-run effects, they are vastly below the long-term predictions of 500,000 to 900,000 job losses. We will return to potential explanations for these diverging findings in the conclusions in Section 6.

# 3 Identification Strategies, Empirical Approach and Data Sources

## 3.1 Identification Strategies

The vast literature on minimum wage evaluation has used and discussed a variety of identification strategies for the causal evaluation of aggregated short- and long-term employment effects. In the following, we will provide an overview of these strategies and discuss their potential applicability for the German case.

**Legislative Variation** Minimum wage regulation can differ between certain sectors, allowing for the use of this exogenous setting as a quasi-experiment. This has been used intensively for the sector-specific regulations in Germany before 2015 (see Fitzenberger and Doerr, 2016). However, only a few sectors are exempted from the national minimum wage, and those who are, have been chosen by the law-makers because of specific characteristics (i.e. many low-paid employees).[6] Therefore, the assumption that exempted and non-exempted sectors would have developed equally in the absence of the reform and thus would share a common trend is likely violated. The causal identification strategy would then be at risk and the external validity for other (all) sectors would also be questionable. Alternatively, exemptions for individuals could also be used as exogenous variation, although since employers could substitute minimum wage eligible employees by non-eligible workers, the latter could have also have been affected. More importantly, these groups are not randomly exempted but differ structurally from the eligible employees, and thus they cannot be used as a control group in a difference-in-difference setting. Another idea is to use variation in legislation between regions, if there is any. In the US, differences in state-level minimum wages are used to evaluate diverging wage and employment

[6] The exempted sectors are the meat processing industry, hairdressers, agriculture and forestry sectors and – in East Germany only – temporary employment ('Leiharbeit') and textile producers.

trends (see Card and Krueger, 1994, 1995; Neumark and Wascher, 2008; Dube *et al.*, 2010; Meer and West, 2015). This approach is appealing, especially if federal state labour markets only marginally differ with respect to their regulations and structure. However, in Germany, the minimum wage is set at the national level and all federal states are subjected equally to the wage floor.

**Regional Variation** Card (1992) proposes an alternative identification approach using regional variation that does not depend on differences in legislation (for applications in the UK see Stewart, 2002; Dolton *et al.*, 2010). Conceptually, the intensity with which wages need to change in accordance with a minimum wage introduction is heterogeneous between areas. Where minimum wages 'bite' hard, adaptations in wages will be stronger and so should be the adaptation of labour demand. This approach is applicable to the German context and will be pursued in the subsequent analysis. With 2015 as the year of the minimum wage introduction, the following structural model summarises this relationship:

$$\Delta W_{j,2015} = \alpha + \beta Bite_{j,2014} + u_{1,j} \tag{1a}$$

$$\Delta E_{j,2015} = \gamma + \eta \Delta W_{j,2015} + u_{2,j}, \tag{1b}$$

where $\Delta W_{j,2015}$ describes the changes in aggregated wages for region $j$ between 2014 and 2015, i.e. $\Delta W_{j,2015} = W_{j,2015} - W_{j,2014}$. The wage change during the minimum wage introduction depends on three elements: the average change ($\alpha$), the lagged minimum wage bite in area $j$ ($Bite_{j,2014}$) and an error term ($u_{1,j}$). Following Card (1992), $\beta$ then describes the average effect of the minimum wage on wages. However, $Bite_{j,2014}$ does not affect employment ($E_j$) directly. Given a labour demand elasticity of $\eta$, only $\Delta W_j$ is transferred to employment changes. Substituting Equation (1a) into (1b) emphasises this relation, i.e.

$$\Delta E_{j,2015} = \gamma_0 + \eta\beta Bite_{j,2014} + \epsilon_j, \tag{2}$$

with $\epsilon_j = \eta u_{1j} + u_{2j}$ and $\gamma_0 = \gamma + \eta\alpha$. The product $\kappa = \eta\beta$ can then be interpreted as the causal effect of the minimum wage on employment.

## 3.2 Empirical Approach

Based on Equation (2), we estimate employment effects in accordance with Card (1992) and Stewart (2002). However, while they look at changes in the employment-to-population ratio, we decided to use the log employment level as the dependent variable. This is because the employment-to-population ratio not only reflects changes in employment levels, but also changes in the population, which held particular relevance in 2015 due to a large inflow of migrants. We thus analyse the log employment level but include population levels as a control variable in our specifications below. Furthermore and in advantage of the stylized model from above, estimations do not need to be restricted to the year of introduction. Additional years may be included in order to control for anticipation and contradiction with the common trend assumption. Then, instead of using the change in employment as left hand side variable, fixed effect estimation on employment levels are a more appropriate choice as they control for time persistent characteristics best. Accordingly, the annual log employment level is estimated by:

$$E_{j,t} = \gamma_j + \gamma' T_t + \theta_1' T_t \times Bite_{j,2014} + \delta X_{j,t} + \upsilon_{j,t}, \tag{3}$$

where $E_{j,t}$ denotes the log employment level (either for regular or marginal employment as will be explained in Section 5.1) in period $t$, $\gamma_j$ a region-fixed effect, $X_{j,t}$ a set of regional controls and $\upsilon_{j,t}$ the error term. $T_t$ denotes a year vector, which we expand from the years around the minimum wage (2014 and 2015) to 2013 and 2012 to evaluate the common trend assumption. The model estimates the reform's effect based on the pre-treatment bite in year 2014.

The definition of $Bite_j$ is crucial to the analysis, which is why the literature discusses several alternatives. Most prominent is the Kaitz index (*'Kaitz'*), which measures the ratio between the minimum wage and the regional mean wage. The higher the Kaitz, the stronger that the minimum wage bites. However, its development is not exclusively determined by changes caused by the minimum wage. Movements in other parts of the wage distribution also affect this indicator. Card (1992), Stewart (2002) and Dolton *et al.* (2010, 2012, 2015) rely on the share of the employed population earning less than the minimum wage (*'Fraction'*). In contrast to the Kaitz, this definition rather focuses on the group of affected individuals. It depicts how many of the working population eligible for the minimum wage are actually affected by it. However, please note that the fraction neglects the density below the wage floor since the low-wage employed affect the indicator independently of their distance to the threshold. Therefore, the suitability of Fraction and Kaitz as a treatment intensity indicator hinges on the assumption that the relative wage distribution below €8.50 per hour or below the mean wage, respectively, is similar among all regions. We construct both bite measures on a scale from 0 to 1 and include them in our analysis. To evaluate the general robustness of our approach, we will also test binary specifications of the bite measures as well as different bite definitions in the later sensitivity analysis in Section 5.2.

### 3.3 Data Sources

To identify the minimum wage effects on the basis of regional variation, we need comprehensive wage data on the eligible population as well as employment stocks on the regional level. Moreover, to control for pre-treatment trends we need data from the years 2012 to 2015. Thus, our analysis combines different data sources, taking advantage of the differing scopes of the data sets. For our employment measures, we rely on administrative information provided by the FEA (2016). However, the wage data is drawn from two different sources, given that we need extensive data both in terms of the time frame and concerning the number of observations. The longitudinal design of the SOEP allows us to test our identifying assumptions for the pre-treatment period. However, our identification strategy requires computing the regional average wage for a multitude of regions and the number of participants in the SOEP does not ensure a sufficient sample size per region. This is why we use the SES data for our analysis, which is considerably larger in sample size but only takes place at a four-year interval.

**Wage Data** To evaluate the minimum wage introduction, the availability of comprehensive information on earnings and working hours is crucial. Since the policy reform targets hourly wages, corresponding data is needed for the calculation of the bite. Additionally, marginally and part-time employed persons should be identifiable in addition to full-time employed, since they compose the most affected groups. Moreover, the proposed difference-in-difference approach relies on a common trend assumption with respect to wages. Information on the bite is thus not only needed during the reform, but also before any potential anticipation took place, which is why longitudinal data is needed. In the German case, one can rely on different alternatives with respect to regional wage data. In previous studies (see Garloff, 2016), administrative data such as the remuneration statistic ('Entgeltstatistik') has been used to derive regional bite levels.[7] Unfortunately, these statistics do not include detailed information about working hours. Hence, either full-time employment with the same number of working hours has to be assumed for

[7] The remuneration statistic is released by the Federal Employment Agency and reports information on regular employment and mini-jobs relationships, including both employment information such as average monthly payments and individual sociodemographic information. For further information, see www.statistik.arbeitsagentur.de/Navigation/Statistik/Statistik-nach-Themen/Beschaeftigung/Entgeltstatistik/Entgeltstatistik-Nav.

everybody or working hours have to be imputed. However, neither solution has been shown to provide a precise measure of hourly wages.

An alternative source of individual wage data is the SES 2014.[8] It is an employer data set with more than 70,000 firms with one million workers overall. In April 2014, representatively-chosen firms were legally obliged to provide detailed information on the income and working hours of their employees (and thus the data does not suffer from systematic bias caused by the sampling process or non-response). Due to its scope, the SES is perfectly suited to aggregate individual wage data at any regional level to derive precise bite measures. Although the SES is considered only representative at the level of federal states, it still contains considerably large sample sizes in our classification of 141 RLMs.[9] Unfortunately, the SES only takes place at a four-year interval. Thus, only data from the year prior to the minimum wage introduction is available, which makes it impossible to analyse short-term changes in bites or wages or to test for anticipation.[10]

For this reason, we rely on an additional data set, namely the annual Socio-Economic Panel (SOEP, 2016, v32). The SOEP is an ongoing panel survey with currently about 30,000 survey participants per year, conducted since 1984 (see Wagner *et al.*, 2007). Similar to the SES, the SOEP allows us to retrieve individual information about employment, earnings and working time. However, the SOEP has its own limitations since surveys are typically prone to measurement issues. Participants may refuse answers or misreport income or working hours, which can potentially lead to measurement errors. Moreover, the division of the data into small-scale areas results in small sample sizes, questioning the precision of the regional indicator. We will therefore first evaluate whether the SOEP is actually suited to derive bite measures on small-scale levels by comparing them to the SES indicators. Subsequently, we can use it to evaluate whether wages have adapted in anticipation of the reform and how they changed after its introduction. Finding no anticipation and no contradiction with the common trend assumption within the SOEP, we can make use of the more comprehensive SES bite measures from April 2014 for our regression analysis of employment dynamics.

[8]Source: FDZ der Statistischen Ämter des Bundes und der Länder, Verdienststrukturerhebung, 2014.

[9]However, note that the lack of representativeness at the RLM level might lead to measurement error and thus possibly to attenuation bias in our later analyses.

[10]In light of the minimum wage introduction, an additional, voluntary survey was conducted in 2015 ('Verdiensterhebung 2015'). However, only 6,000 firms among the original sample participated. The representative character of the SES 2014 is therefore at risk (Destatis, 2017) and we refrain from using it for our analysis.

**Hourly Wages** We derive hourly wages in the SES by dividing information about gross monthly wages excluding compensation for overtime and surcharges by monthly paid hours without overtime for workers who are paid based on their working hours. For all others, the income is divided by regular weekly working hours multiplied by average weeks per month. As the SES does not provide regional information of civil servants at a smaller level than federal states, the overall sample size reduces to 780,000 observations. Therefore, mean wages could be underestimated. In order to prevent outliers in hourly wages from biasing our results, we winsorise the data and set the first and last percentile of the overall hourly wage distribution to the value of the corresponding percentiles. The sample is restricted to eligible employees only. The SES provides weights for both firms and employees. However, they are constructed to weight observations at the level of federal states. Since we conduct our estimations at a smaller regional level, we refrain from weighting our estimations. Nonetheless, we include weights as a sensitivity check in Section 5.2.

Similar to the SES, hourly wages are not retrieved directly in the SOEP but can be computed as the ratio of gross monthly wages and weekly working hours adjusted by average weeks per month. As the SOEP not only includes contractual weekly working hours but also the actual working time – which includes paid and unpaid overtime – we derive the bite measures for both concepts and compare them with each other. In principle, using actual working time is the more accurate measure as minimum wages need to be paid for any working time. However, the SOEP asks for working hours in general and for income for the previous month. Since both measures thus do not necessarily match each other, this possibly leads to measurement errors. Nonetheless, since the contractual wages are closer to the hours concept of the SES, our analysis focuses on contractual wages. Again, we restrict the SOEP sample to eligible employees reporting all necessary wage information, and apply a top and bottom recoding at the first and last percentile to avoid measurement errors. Moreover, the sample is restricted to individuals participating after February, since the income question refers to the previous month and thus to the previous year in case of January.

**Classification of Regions** We use the two wage data sets to compute annual characteristics of regional wage distributions. For this purpose, each individual is assigned to one region. Nonetheless, the choice of the regional level comes with a trade-off: the smaller the area classification, the better that heterogeneous labour market performances and their variation can be captured, although the more likely it is that the economic structure of a region is not picked up accurately, e.g. in areas with high commuter flows. In this sense, the sixteen federal states might be considered too broad a classification, since there is a substantial amount of heterogeneity within each state. The 401 administrative districts ('Landkreise') account for this dissimilitude more accurately, although they are also more prone to high commuter flows, which is why measures like GDP per capita may not reflect the actual economic performance of a district. Moreover, while the SOEP data provides information on the place of residence, the SES identifies the place of work of each respondent. The classification should therefore unite both concepts.

A common solution to these problems is using area classifications that account for commuter flows and general economic performance. In the following, we rely on 141 RLMs defined by Kosfeld and Werner (2012) as they consider commuter flows and regional labour market seclusion. Moreover, they do not coincide with the federal states, which is especially expedient for metropolitan areas and city states. As an additional sensitivity check, in Section 5.2 we replicate our estimation based on another area classification, the 96 planning regions ('Raumordnungsregionen', ROR). They divide Germany into segregated regions by commuter flows and economic structure and are defined by the federal states according to their own regulations (for details see BBSR, 2016).

Since the SOEP sample is not uniformly distributed in Germany, some regions have only few observations. Thus, in addition to the previous restrictions to the SOEP, we discard regions with fewer than 30 observations per year as they are strongly dependent on single individuals and thus are relatively volatile in their bite measures. For this reason, we omit 48 RLMs in the analysis with the SOEP. After this restriction, we observe 89 individuals on average per region and year. When using the SES 2014, we rely on average on 5,345 individuals per RLM in 2014 with the smallest (largest) region containing 366 (46,202) observations. Based on these regional wage distributions, we compute the previously-introduced Kaitz and Fraction bite measures for both data sets.[11]

**Employment Data** Following our theoretical considerations, heterogeneous wage effects between regions can – depending on the labour demand elasticity – cause varying adaptation in

[11]Please note that the SOEP and SES hourly wage data slightly differ. The SOEP only captures the main occupation, while SES also entails secondary employment. Moreover, SOEP respondents are registered according to the residence principle, whereas SES data is collected in compliance with the place of work principle. This is especially crucial for the regional classification in our analysis. However, we account for this by allowing for commuter flows. For a summary of differences between the two data sets and a review of the consequential computation of hourly wages, see Dütsch *et al.* (2017).

employment. This will be analysed in our regression analysis in Section 5. For this purpose, we combine our regional bite measures with administrative data on employment stocks. We examine regular and marginal employment, relying on administrative information provided by the FEA (2016), where marginal employment includes jobs carried out as sole employment as well as add-on mini-jobs. Data on regular employment is available from 2012 onwards and information on mini-jobs is at hand from 2013 onwards.[12] The labour market data are available for administrative districts ('Landkreise'), although for the aforementioned reasons we aggregate it to our RLM by summation. Although the data at hand are on a quarterly basis, we will focus on annual effects to abstract from any seasonal effects. As a reference point in time, we choose the second quarter of each year (June 30th), as the points of the legislative process (parliament election in September 2013, law passing in July 2014, see Section 2) lie around that date.

In our regression analysis, we will make use of additional control variables, namely the regional population level and GDP, taken from Destatis (2016b). Like the outcome variables, they are only available for administrative districts. Hence, we also aggregate them by addition. Note that the indicators are measured at the end of each respective year. We impute the population level for each quarter by geometric weighting and assume a constant flow of migration within a year. As the population, we employ the working age population between 15 and 65 years. GDP per capita is manually computed as the GDP-population-ratio.

# 4 Descriptive Results for Bite Measures and Employment

**Regional Variation** First, we evaluate the geographical structure of the bite as its variation is crucial for the identification strategy. Only if areas are affected differently can wage effects and thus employment effects be identified by the suggested model. Figure 1 presents both Fraction and Kaitz from the SES for the 141 RLMs.[13]

[Include Figure 1 here]

Both bite measures reflect the long-lasting and still significant structural differences between East and West Germany, since the bite is considerably higher in the eastern part. However, there is not only a substantial variation between East and West, but also within. Figure 1a shows that

[12] The definition of marginal employment changed in 2013. Until 2012, employment was considered a 'Mini-Job' up to a threshold income of €400. The FDE does not provide coherent information concerning marginal employment for administrative districts before 2013.

[13] For reference, a histogram of the distributions of Fraction and Kaitz is presented in Figure A.1 (see Appendix).

there are RLMs in the West that display a high Fraction. This holds true for some areas close to East Germany, such as Göttingen and Goslar. However, regions closer to the German borders – such as Cham to the east, the Volcanic Eifel, Bitburg and Cleves to the West and Emden and Wilhelmshaven to the North – also display a high Fraction. On the other hand, cities and their surrounding areas like Munich, Düsseldorf or Hanover, as well as highly-industrialised areas, such as east Lower Saxony or Rhine-Main report a relatively lower bite compared with other parts of the corresponding federal state. Moreover, Figure 1b shows that the Kaitz and the Fraction do not always yield the same ranking of regions by bite. While the overall picture remains roughly the same, the Kaitz index displays more variation within Bavaria than the Fraction. There are also some differences for Baden-Württemberg and Rhineland-Palatinate. This is due to the fact that the Kaitz index measures the ratio between the minimum wage and the average wage. It is therefore not necessarily affected in the same way as the fraction of affected workers, since it only decreases when the average wage is in fact increased. Nonetheless, it can also be influenced by movements in other parts of the wage distribution. To get the full picture, we will therefore look at both measures in our following analysis. Overall, the graphical analysis shows that we observe considerable regional variation in the bite indicators, which can be used for the upcoming analysis.

**Correlations between SES and SOEP** Before analysing changes in bite indicators, we elaborate on whether the SOEP is suited to replicate the SES measures, despite its smaller sample size per region. For this reason Table 2 displays summary statistics for various indicators derived from the SES and SOEP for 2014. As the SOEP offers two wage concepts, bite indicators for contractual wages (SOEP$_{con}$) and actual wages (SOEP$_{act}$) are listed.

[Include Table 2 here]

Table 2 shows that according to the SES data on average 17.1 percent of eligible workers earned less than €8.50 in 2014. In the SOEP, this amounts to 12.1 and 15.5 percent, respectively. This is roughly in line with the considerations of the German low-wage commission (Mindestlohnkommission, 2016b). The differences in levels between both dataets arise because the SES does not include any public sector employees at our regional level. Since only 0.9 percent of them are affected by the wage floor, excluding them increases the bite measures. This is also the reason why the table does not replicate the fractions of affected workers from Table 1.

The lower panel of the table displays the correlations between bite measures. The correlations between indicators from different data sets are considerably high. The correlation between SES and SOEP Fraction is 0.657 or 0.7, respectively. The Kaitz indices derived from the SES and the SOEP display a correlation of at least 76 percent. Due to these large correspondences, the identification of pre-treatment indicators with the SOEP is reasonable. We can attest that regardless of the known shortcomings of survey data, both SOEP and SES provide similar bite measures.

To relate our results to previous studies and make an additional contribution to the literature, we calculate one additional bite measure (Fraction $\text{SES}_{month}$) based on the monthly earnings of full-time employed.[14] Since many data sources lack information on working hours, using monthly wages of full-time employees only is common practice (see Garloff, 2016; Schmitz, 2017). However, the validity of this approach is strongly discussed. By neglecting the fact that especially part-time and marginally employed earned below the minimum wage, the fraction of affected workers could be strongly underestimated. Table 2 shows that the monthly bite for full-time employees amounts on average to only 3.8 percent, which is substantially lower than the bite based on hourly wages. This poses a problem for general statements about the degree to which regions were – or still are – affected. However, assuming that there are no systematical differences between regions, it is less crucial when the bite is used as a treatment indicator. Subsequently, it should only reflect a relative degree to which regions are affected, irrespective of its amount. The Fraction $\text{SES}_{month}$ displays a very strong positive correlation with the SES Fraction derived by hourly wages ($corr$ = 0.918). Accordingly, although the monthly bite is possibly less precise, both indicators measure the same relative treatment intensity. We will include the measure in our sensitivity anlysis in Section 5.2 to test whether it yields similar results when used as a treatment indicator.

**Anticipation and Changes in Bite** As already discussed, the SES does not allow evaluating wages in years other than 2014. However, this is possible with the longitudinal SOEP data. For the analysis, this is relevant for two reasons: first, as proposed by Equations 1a and 1b, we only expect diverging changes in employment if wages adapt in light of the minimum wage reform; and second, the difference-in-differences framework also implies a common trend assumption

[14]We follow Garloff (2016) and calculate the fraction of workers who earn less than €1,400 per month, are full-time employed and between 30 and 54 years old in proportion to all full-time employees of that age category.

concerning wages, namely that in the absence of the policy reform, wages would have developed equally in all areas. This assumption can be evaluated by analysing a pre-treatment time frame.

For the following descriptive analysis, all considered regions are sorted into one of three groups – namely low-, medium- and high-bite areas – according to their bite level estimated with the SOEP in 2014. We follow Card (1992) and set the cut points in the respective bite such that each group comprises the same number of RLMs. See Table 2 for the corresponding thresholds, i.e. the 33th and 67th percentile.

[INCLUDE FIGURE 2 HERE]

Figures 2a and 2b present the annual average change in Fraction $\text{SOEP}_{con}$ and Kaitz $\text{SOEP}_{con}$ of high-, medium-, and low-bite areas. Between groups, no significantly diverging trends can be observed in the pre-minimum wage periods (2012 to 2013 and from 2013 to 2014). Accordingly, before the reform the regional wages followed the same trend in high-, medium- and low-bite areas. After the introduction of the minimum wage (2014 to 2015), Fraction changed in accordance with expectations: while no significant change in low-bite areas can be identified, medium- and highly-affected regions report a significant decrease in the presented bite indicator, implying wage increases at the bottom of the wage distribution. However, note that 12 percent of all employees had been paid below the minimum wage in 2014 (see Table 2); therefore, the average reduction in Fraction by six percentage points in high bite areas was far below expectations. In accordance with Caliendo *et al.* (2017), these results stress that the adaptation of wages – and thus Fraction – had not been executed completely in early 2015. Focusing on Kaitz in Figure 2b shows a slightly different picture. Similarly, there are no significant differences in the pre-treatment period, implying that regional bite indicators followed a common trend. However, effects after the reform are less pronounced than within Fraction. There is only a small and weakly significant decrease in high-bite regions. However, this is not very concerning, since the Kaitz depends on the mean wages, which are not as likely to react strongly if only the bottom part of the wage distribution was affected.

Overall, our results do not imply any anticipation effects or contradictions with the common trend assumption in wages. However, with the minimum wage introduction, the wage structure changed at the bottom in dependence of the minimum wage bite. Although recent literature shows that there were still people earning less than the minimum wage in 2015 (see Caliendo *et al.*, 2017), our analysis of the changes in the regional bite implies heterogeneous wage effects.

**Employment** As an initial analysis, we focus on descriptive evidence on employment dynamics. Figure 3 summarises the average annual changes in selected statistics for three commensurate groups, i.e. low-, medium- and high-bite areas. Again, regions are assigned into one of the three groups by their Fraction obtained from the SES, with the 33rd and 67th percentile of the bite's distribution posing as a threshold.

[Include Figure 3 here]

Figure 3a displays annual percentage changes of the population. Obviously, highly-affected areas underlie a significant reduction in population until 2014, while population grows in high-wage areas. In light of the increasing number of refugees, the negative trend in low-wage areas stopped, although the differences between areas prevail from 2014 to 2015.[15] They are likely a result of the migration from the countryside to urban areas, as well as the migration from East to West Germany. Nonetheless, not controlling for these diverging trends may lead to wrong presumptions regarding the employment effects, which is why further analysis needs to control for population size. Figures 3b and 3c therefore present the average annual changes in employment-population ratios by quarters. Both figures indicate no differences between groups before the minimum wage introduction. Accordingly, before the minimum wage was introduced, the three groups of regions had a similar development of the employment-to-population ratio. From this perspective, our second underlying common trend assumption – i.e. in the absence of the minimum wage reform, employment would have developed equally – is not rejected. After the reform, regular employment appears to be unaffected and does not leave its previous, positive trend. This is substantiated by the absolute numbers: while in the second quarter of 2014 there were 30 million regularly employed workers, this amount increased to 30.6 million in 2015. However, the employment-to-population ratio of marginal employment decreased on average after the minimum wage introduction, and especially in highly-affected areas. Overall, the number of mini-jobs declined from 7.72 million in the second quarter of 2014 to 7.65 million in the following year. This gives a first indication that mainly marginal employment was affected by the reform.

[15] Although the inflow of migrants increases the population size, most of them were not allowed to work yet, such that the absolute employment level is most likely not as affected.

## 5 Employment Effects

### 5.1 Main Effects

After looking at the employment dynamics descriptively, we will now apply a fixed-effects estimation to evaluate employment effects of the new minimum wage in a difference-in-difference framework. We derived our estimation equation (3) in Section 3.2:

$$E_{j,t} = \gamma_j + \gamma' T_t + \theta_1' T_t \times Bite_{j,2014} + \delta_1 GDP_{j,t-1} + \delta_2 POP_{j,t} + \upsilon_{j,t},$$

where $E_{j,t}$ denotes the log employment level and is measured on 30th June in each year.[16] Since regular and marginal employment differ by their social security contributions and therefore are only partly comparable (see Section 2), they are analysed separately and will be presented in Panels A and B of the following tables. The treatment effect is identified by the vector of the coefficients of the interaction term between bite and year ($\theta_1$). We are especially interested in the coefficient of the interaction term between the bite and the year 2015 (denoted as Bite × D2015 in the following tables), which identifies the employment change in the year of the minimum wage introduction. However, to control for pre-treatment trends and test the common trend assumption, we also include pre-treatment years, such that $t \in \{2012, 2015\}$.[17] Besides the inclusion of time- and region-fixed effects[18], we also control for regional differences in logarithmic GDP in $t-1$, assuming that a region's economic power will have an impact on its employment. As employment is additionally strongly affected by the population size (see also discussion in Section 3.2) and Figure 3a (see Section 4) displays differing trends in population, we also include the logarithmic population size in $t$.[19]

**Changes in Regular Employment** First, we focus on short-term effects on regular – i.e. full- and part-time – employment. Panel A in Table 3 presents the corresponding results. Columns

[16] Including not only one point in time per year but rather all quarters yields similar results. However, variation between quarters indicates strong seasonal dependency, which can be ruled out easily by focusing on annual changes only. The results are available on request from the authors.

[17] Please note that for comparability the reference year remains fixed at 2014, such that the employment effects of the interaction terms between the bite and previous years (Bite × D2013 and Bite × D2012, respectively) have to be interpreted in relation to the year 2014.

[18] Using the change in employment as dependent and neglecting regional fixed effects yield the same results. The results are available on request from the authors.

[19] We thus assume that the current population level is not affected by the minimum wage. Although this might not be the case – i.e. the population in $t$ is potentially endogenous – we chose this specification over the population in $t-1$. This is because the population changed especially in 2015, due to the large inflow of migrants, which would not be captured by the population in $t-1$. Moreover, since we only look at the first six month after the reform, adaptations in the current population caused by the minimum wage are not very likely.

(1) to (4) use Fraction, adding control variables as well as pre-treatment periods iteratively. Columns (5) and (6) repeat the main specifications for the Kaitz index. All specifications use 2014 as the reference year such that treatment effects in 2015 can be compared between different specifications.

[Include Table 3 here]

Columns (1) and (2) present the employment effects from 2014 to 2015 without and with controls for population and GDP per capita. While GDP's effect is negligible, controls for current population dynamics appear to be crucial. Due to the direct interrelation between population and the employment level, the population in $t$ explains a great share of within and between variation in employment. This holds major importance for the upcoming analysis. Because high-bite regions underlie a different population dynamic (see Figure 3a), variation in employment growth due to diverging changes in population will be attributed to the minimum wage when these differences are not controlled for. Since population would have affected employment in the absence of the policy reform, controlling for the current population level is obligatory. By contrast, differences with respect to GDP seem to be controlled for by regional fixed effects.

Accordingly, controlling for population effects reduces the highly significant treatment effect from -0.080 in Column (1) to -0.017 in Column (2), which means that an increase of Fraction by one percentage point is associated with a reduction in employment by 0.017 percent ceteris paribus. In combination with the mean regional Fraction of 17.1 percent as displayed in Table 2, Column (2) implies an average employment effect of 0.3 percent. This is equivalent to a reduction of approximately 78,000 jobs, which is roughly in line with the estimations by Bossler and Gerner (2016). Columns (3) and (4) include the years 2013 and 2012, respectively. On the one hand, this allows us to revisit the common trend assumption. On the other hand, we can thus control for pre-treatment trends, giving us a more precise effect for the year 2015. Accordingly, the effect diminishes to a weakly significant or even insignificant level. This means that if employment trends are taken into account, the minimum wage affected employment only weakly. Column (4) yields a treatment effect of -0.012, which implies a reduction of regular employment by about 52.000 jobs. Moreover, the pre-treatment interaction terms (Bite $\times$ D2013 and Bite $\times$ D2012) do not display significant coefficients. Thus, in accordance with the descriptive analysis from Section 4, we can substantiate the conclusion that the regions followed the same trend before the reform. Repeating the specifications from Column (2) and (4) with the the Kaitz index in Columns (5) and (6) doe not yield any significant effects.

**Changes in Marginal Employment** Panel B of Table 3 sheds light on changes in mini-jobs, the type of employment that is especially characterised by low wages. We follow the same structure as in Panel A. However, due to the fact that coherent information on this employment status is only available after 2012, Column (4) is missing in Panel B. All columns of Panel B show that high-bite regions report a highly significant reduction in mini-jobs after the introduction of the minimum wage. Comparing Columns (1) and (2) shows that – very similar to regular employment – population size has a large impact on marginal employment throughout all specifications, while a region's GDP does not have a significant effect. Following Column (2) in Panel B, an increase in Fraction by one percentage point is associated with a reduction in marginal employment by 0.17 percent. Multiplying this effect with the observed Fraction of 17.1 percent reveals that on average mini-jobs diminished by 2.9 percent, which is equivalent to an overall reduction of roughly 183,000 mini-jobs from June 2014 to June 2015. By comparison, vom Berge *et al.* (2016a) report a reduction of 94,000 mini-jobs between December 2014 and January 2015 in addition to any seasonal trends. We thus find an even larger negative effect on marginal employment than previous studies (see also Garloff, 2016). However, since Column (3) of Panel B in Table 3 identifies a slight decrease in mini-jobs already from 2013 to 2014, we need to be cautious with the interpretation. With the reference year 2014, a one percentage point increase in Fraction translates to 0.05 percent more mini-jobs in 2013, pointing to the fact that mini-jobs already declined in highly-affected regions even before the minimum wage introduction. However, its magnitude is not as large as the effect from 2014 to 2015, which indicates that the additional reduction is likely due to the reform. Using the Kaitz index does not show any such anticipation effect.

### 5.2 Robustness Analysis

Thus far, we have found strong effects of the minimum wage on mini-jobs, while regular employment has only been slightly affected. We will now test the robustness of our results in different directions, i.e. we employ another area classification, a weighted SES bite measure, the SES bite based on monthly wages, a bite measure constructed with the SOEP and two binary indicators. The results of the robustness tests are depicted in Table 4. The following tables use the Fraction indicator and include all pre-treatment years, since this is the most comprehensive specification.

[Include Table 4 here]

**Area Classification** As discussed in Section 3.3, we apply another area classification to test the extent to which our results are dependent on the chosen definition of regions. Therefore, we re-estimate the effects based on the 96 planning regions. The results are displayed in Column (2) of Table 4. For reference, our baseline estimation from above is presented in Column (1). Overall, the magnitude of the effects are very similar to the results obtained using the RLMs. However, in contrast to the baseline results (see Column (1)), the Fraction yields a weakly significant effect for regular employment. This might be due to the fact that the ROR divide the labour markets more strictly and more artificially, since they are subject to the borders of the federal states. They could thus pick up an effect that is rather due to an erroneous aggregation of the regional labour market. However, the strongly significant results for marginal employment are all replicated.

**Weighting of the SES Bite Measure** The SES provides weights for firms and employees. However, they are constructed to be representative at the level of federal states. Since we use a smaller area classification, we have used unweighted data thus far. Nonetheless, since this decision is arbitrary, we conduct a sensitivity test by including weighted data. Column (3) of Table 4 provides the corresponding results. The results show a weakly significant negative employment effect of 0.014 for regular employment. Following this, the average treatment effect obtained by multiplying with the mean weighted Fraction amounts to -0.2 percent.[20] This translates into an employment loss of about 55,000 jobs. When looking at marginal employment, the results also become more pronounced. However, due to the slightly smaller average Fraction in the weighted case, the highly significant coefficient of -0.186 means a reduction of marginal employment by 177,000 jobs, which is slightly below our baseline effect of 183,000.

**Bite Measure Based on Monthly Earnings** As discussed in Section 4, previous studies use information on monthly earnings only, thus relying on critical assumptions on working hours (see e.g. Garloff, 2016). We can review this alternative approach with our data and rely on a bite based on the share of full-time employed earning less than €1,400 per month. Column (4) of Table 4 displays a highly significant coefficient of -0.042. While this effect is much higher in magnitude than our baseline result, it has to be evaluated in relation to the average Fraction. Given the lower level in the monthly Fraction (on average 3.8 percent, see Table 2), this translates

[20] Note that the mean Fraction of 17.1 percent in the unweighted case changes when using weighted data, and thus now amounts to 15.2 percent.

into an average treatment effect of -0.16 percent and thus an employment loss of only 37,000 jobs. While this is close to our baseline estimation, differences with respect to mini-job employment are higher. Here, the estimation in Column (4) shows a coefficient of -0.34, which means an average treatment effect of -1.28 percent and thus a reduction in marginal employment of 66,000 only. While this is exactly the effect obtained by Garloff (2016), it amounts to only one third of our previously-derived results. The difference could be explained by the lack of information on part-time employed and mini-jobbers within the computation of the bite, which appears to be crucial for the estimation of corresponding employment effects.

**Bite Measure Based on SOEP** Thus far, we have used SOEP data only to evaluate changes in bite over time, while we refrained from using it in our analysis of employment effects since it contains only little data for some RLMs and is somewhat prone to measurement error. However, we will employ the bite derived from contractual hours as an alternative data source for the employment effects estimation as a sensitivity test. The corresponding results are presented in Column (5) of Table 4. RLMs with fewer than 30 observation in the SOEP are not considered in the estimation, which is why the number of observations decreases. The results show that effects of Fraction are similar to the SES results. The effects on regular employment, on the one hand, are not significantly different from zero. However, there are weakly significant negative effects even before the introduction, implying that the common trend assumption does not hold. Marginal employment, on the other hand, significantly declines in dependence of the bite. However, the effects obtained are considerably lower, since the average treatment effect amounts to only -1.3 percent, compared to -2.8 percent in our baseline estimation in Section 5.1. This could be due to the fact that the dropped 48 RLMs are foremost those with a low population density, typically also being areas with relative high bite levels. Moreover, measurement error could lead to attenuation bias, which reduces the effects.

**Binary Treatment Indicator** The lack of representativeness at the RLM level within the SES might cause a measurement error on the bite level, which could bias our estimates if the error correlates with bite or the outcome variable. To test the robustness regarding errors at the bite level, we additionally derive two binary treatment indicators, sorting regions into treatment and control groups based on the Fraction's distribution. Accordingly, the exact level is not decisive for the estimation, but rather the distinction between low- and high-wage regions only. In Column (6), regions in the highest quarter of the distribution are considered treated and the lowest quarter are sorted into the control group. Regions in between are discarded.[21] Column (7) alters the cut-off to the median, such that no RLMs are discarded. Using binary indicators yields similar results compared with our baseline estimation. Column (6) identifies a significant effect for regular employment. This means that regions in the highest quarter of the bite distribution had a significant reduction of 0.4 percent in regular employment compared to regions in the lowest quarter. As for mini-jobs, the results are again highly significant. Column (6) yields a treatment effect of 3 percent, which is nearly the effect of our baseline estimation. The softening of the treatment assignment in Column (7) diminishes the average treatment effect for both employment types. Given the less disjunctive definition, this is to be expected and the results substantiate our baseline estimations.[22]

## 6 Conclusions

The introduction of the minimum wage in Germany was preceded by many – predominantly negative – expectations for the development of employment. In worst-case scenarios, a loss of 500,000 to 900,000 jobs was predicted (Müller and Steiner, 2013; Knabe *et al.*, 2014). The evaluation of the effects of the minimum wage, however, is complicated considerably by the universality of the policy, which reduces the set of potential identification strategies. The application of the minimum wage on almost any employee in every region does not allow for the use of the law for a quasi-experimental setting. For this reason, our analysis relies on regional differences in wage levels as a source of variation, as proposed by Card (1992). The approach exploits the assumptions that the higher the impact of the minimum wage on the regional wage distribution, the more strongly that the regional labour market is affected.

Therefore, we identify two commonly-used regional bite measures – the Fraction and the Kaitz index – which give us a broader picture of the degree to which regions are affected by the minimum wage reform. Using 141 RLMs, we are able to account for regional dependencies in economic performance and commuter flows. To assure a valid assessment of the minimum wage effects, we combine different datasets. Our main data source is the SES 2014, which entails comprehensive wage data from the year prior to the reform. However, since it is only available

[21] This leaves us with 36 RLMs in each group. What needs to be kept in mind with this separation is, that 32 of the high-bite regions in the treatment group are in East Germany, whereas all of the low-bite regions are in West Germany.

[22] Using a binary specification that divides the treatment and control group at p33/p66 as well as constructing binary indicators with the Kaitz yield essentally the same results. The results are available on request from the authors.

every four years, we extend our analysis by adopting the longitudinal Socio-Economic Panel (SOEP), allowing testing our assumptions about anticipation in more pre-treatment periods. When examining the bite measures obtained by different concepts and different data sources more closely, we establish strong correlations between these concepts, thus declaring their application valid. This is substantiated by our estimation results, which do not yield considerably different results. Despite certainly being less precise, we can even validate the employment of the treatment intensity constructed with the monthly earnings of full-time employees only, if hourly wage data is not available in future research.

We use the SOEP to analyse potential anticipation effects in wages and show that overall there were no anticipation effects before the introduction of the reform, such that we can assume that regional wages would have followed the same trend in the absence of the minimum wage. Our estimation results imply that effects on employment have only marginally materialised. In our baseline estimation, we find a weakly significant effect on regular employment in only one specification, although it is levelled out when including pre-treatment periods. Throughout various sensitivity tests, our main results are merely substantiated. However, some specifications produce a reoccurring weakly significant negative effect. Accordingly, the lack of an effect on regular employment is not particularly robust. Where we find an effect, it amounts to a loss of up to 78,000 jobs (approximately 0.3% of regular employment). However, the effects on marginal employment are considerably stronger: we find a strongly significant reduction of marginal employment, which remains observable throughout all specifications and robust to all sensitivity tests. In our main estimation, it translates into a decrease of about 180,000 mini-jobs (about 2.4 percent of all mini-jobs), thus being even higher than the effect found in previous studies (see vom Berge *et al.*, 2016a; Garloff, 2016). One limitation with our study is, that the SES data only ensures representativeness at the level of federal states. Thus, we induce possible measurement error by disaggregating to RLMs, which could bias our results towards zero. However, we address this issue by using weighted data as well as testing a binary indicator in our robustness analysis and the results do not hint at any major problems.

Although our results lie above the effects found in previous studies, they are still well below the predictions of 500,000 to 900,000 job losses. One possible explanation for this is that we only identify short-term effects, while the effects predicted in ex-ante studies are catered to the long run. However, job losses could have also failed to appear because other channels of adjustment were chosen. Correspondingly, employer surveys show that alternatives to displacements such as the reduction in working hours or an increase in prices, have been preferred to date (Bellmann *et al.*, 2016; Mindestlohnkommission, 2016b; Sauer and Wojciechowski, 2016). An additional explanation could be the existence of a monopsony, where the current minimum wage lies below the marginal product of labour, in which case theory would not predict any job losses. Moreover, in a companion paper (see Caliendo *et al.*, 2017), we show that wages had not adapted fully in 2015, and about 7 percent of the eligible population were still earning less than the minimum wage per hour shortly after the introduction. This points to a generally-postponed adjustment, in which case labour demand would not have fully adjusted in 2015 either. The near absence of an effect on regular employment could then also be explained by non-compliance. In this case, it might be too early to derive policy conclusions. However, the reduction in the number of marginally employed – which was the most affected group with relatively easy adjustment possibilities – might serve as a precursor for more severe effects on regular employment in the long run once labour demand has fully adjusted and the minimum wage is fully enforced. Thus, the effects of the minimum wage will have to be re-evaluated when sufficient long-term information is available. Moreover, the adaptation of the wage floor from €8.50 to €8.84 introduces new movement and a possible route for further evaluations.

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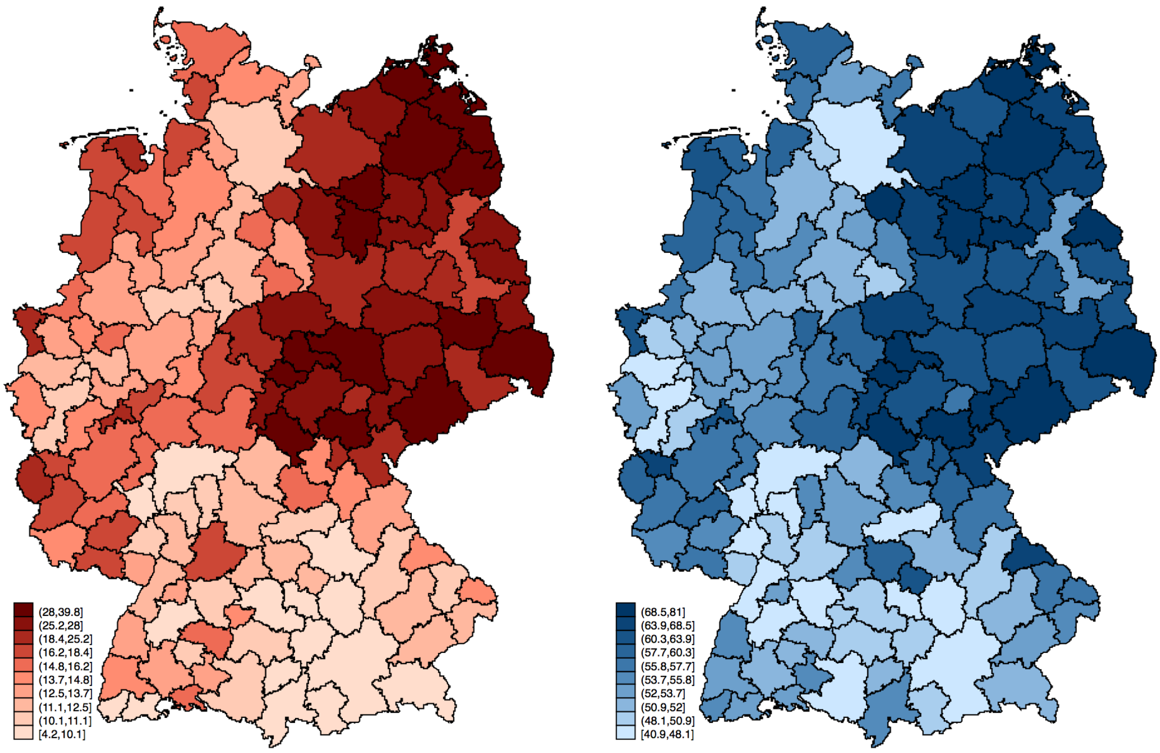
# Figures and Tables

Figure 1: Degree to Which Regional Labour Markets Are Affected in 2014

(a) Fraction

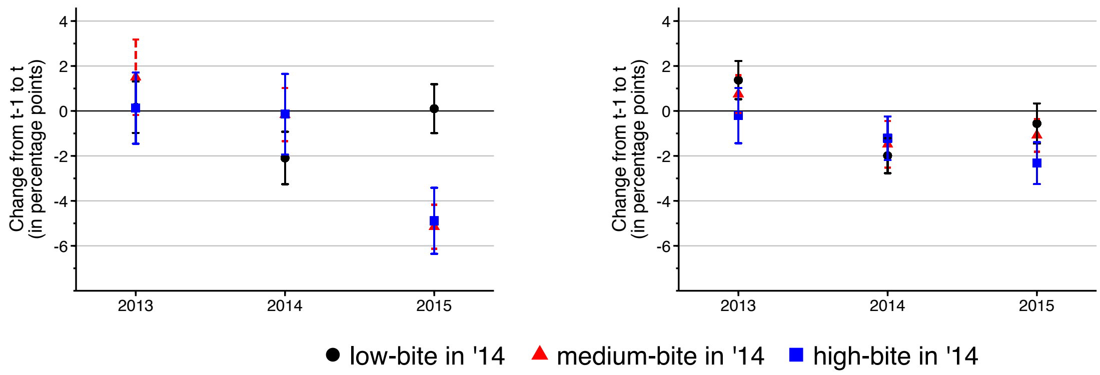
(b) Kaitz




Source: SES 2014, own calculations.
Notes: Bite measures are divided into deciles, such that each category contains about the same number of regions in 2014.

Figure 2: Annual Percentage Change in Bite Within High-, Mid-, and Low-Wage Regions

(a) Fraction (b) Kaitz

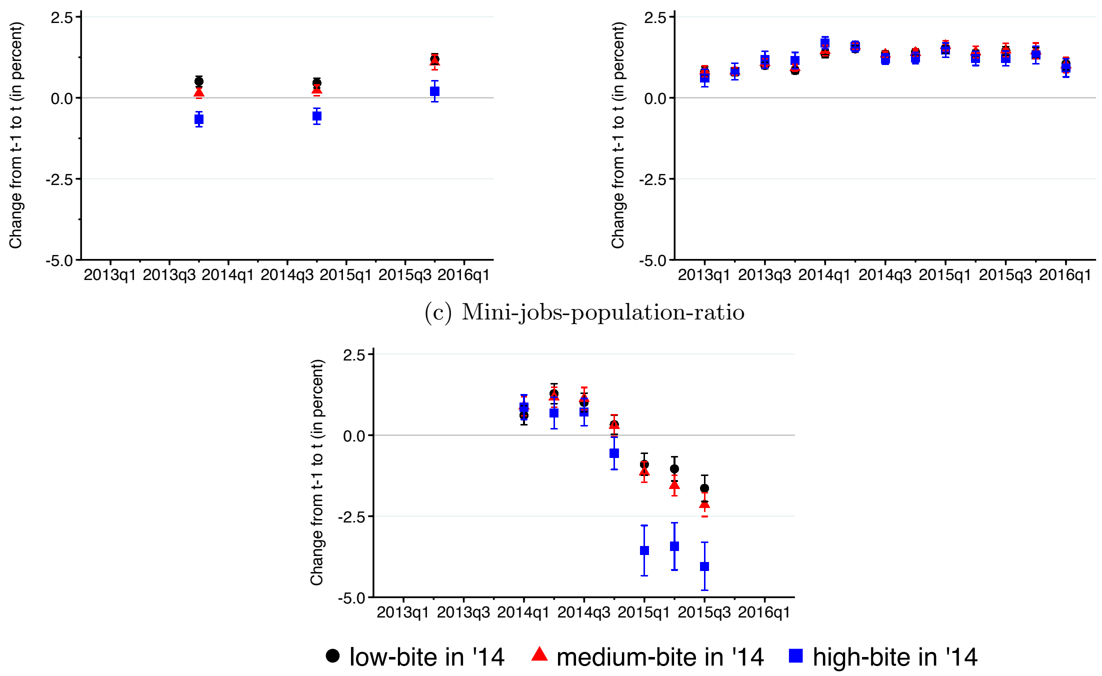



Source: SOEP 2012-2015, own calculations.
Notes: Each point represents the average change in bite (in percentage points). Whiskers denote the corresponding 95 percent confidence interval. Each region has been sorted by its level of bite (*Fraction* or *Kaitz*) in 2014 into one group.

Figure 3: Annual Percentage Change in Population and Employment Within High-, Mid-, and Low-Bite Regions by Quarter

(a) Population (b) Regular employment-population-ratio

(c) Mini-jobs-population-ratio




Source: SES 2014, FEA 2012-2015, Destatis 2012-2015, own calculations.
Notes: Each point represent the annual change in the denoted indicator. All values in percent. The x-axis denotes $q$, such that each point represents the change from $q - 4$ to $q$. Whiskers represent the corresponding 95 percent confidence interval. Each region has been sorted by its level of affected individuals in 2014 into groups (Fraction derived by contractual wages). Information on mini-jobs is only available from 2013 onwards, information on population only once a year.

Table 1: Minimum Wage Beneficiaries in 2014

| | Absolute (in mio.) | Share of employed | Share of affected |
|---|---|---|---|
| Employed | 37.4 | 100% | - |
| Wage <€8.50 | 5.5 | 14.7% | - |
| Wage <€8.50 and eligible | 4.0 | 10.7% | 100% |
| thereof | | | |
| West-German residents | 2.9 | 7.8% | 72.9% |
| East-German residents | 1.1 | 2.9% | 27.1% |
| Full-time employment | 0.9 | 2.4% | 22.4% |
| Part-time employment | 0.9 | 2.4% | 22.4% |
| Mini-jobs | 2.2 | 5.9% | 55.1% |
| Women | 2.5 | 6.6% | 61.7% |
| Men | 1.5 | 4.0% | 38.3% |

Source: Destatis (2016a).
Note: Numbers base on SES 2014 and include public sector employees.

Table 2: Summary Statistics of Minimum Wage Bite Measures in 2014

| | Fraction | | | | Kaitz | | |
|---|---|---|---|---|---|---|---|
| | SES | SES$_{month}$ | SOEP$_{con}$ | SOEP$_{act}$ | SES | SOEP$_{con}$ | SOEP$_{act}$ |
| $N$ | 141 | 141 | 72 | 72 | 141 | 72 | 72 |
| Mean | 0.171 | 0.038 | 0.121 | 0.155 | 0.569 | 0.485 | 0.531 |
| Sd | 0.073 | 0.035 | 0.048 | 0.058 | 0.079 | 0.060 | 0.064 |
| Min | 0.042 | 0.002 | 0.029 | 0.044 | 0.409 | 0.368 | 0.406 |
| 33rd percentile | 0.131 | 0.017 | 0.093 | 0.125 | 0.527 | 0.450 | 0.495 |
| 50th percentile | 0.148 | 0.022 | 0.113 | 0.146 | 0.558 | 0.480 | 0.523 |
| 67th percentile | 0.176 | 0.031 | 0.154 | 0.175 | 0.593 | 0.512 | 0.556 |
| Max | 0.398 | 0.157 | 0.238 | 0.276 | 0.810 | 0.640 | 0.716 |
| Correlation matrix | | | | | | | |
| Fraction SES | 1.000 | | | | | | |
| Fraction SES$_{month}$ | 0.918 | 1.000 | | | | | |
| Fraction SOEP$_{con}$ | 0.657 | 0.564 | 1.000 | | | | |
| Fraction SOEP$_{act}$ | 0.700 | 0.610 | 0.913 | 1.000 | | | |
| Kaitz SES | 0.896 | 0.847 | 0.616 | 0.679 | 1.000 | | |
| Kaitz SOEP$_{con}$ | 0.692 | 0.637 | 0.766 | 0.798 | 0.762 | 1.000 | |
| Kaitz SOEP$_{act}$ | 0.704 | 0.653 | 0.759 | 0.815 | 0.775 | 0.988 | 1.000 |

Source: SOEP v32, SES 2014.
Note: Table presents bite measures divided by different definitions for Kaitz and Fraction. *SES* and *SOEP* denote measures based on corresponding datasets. $SOEP_{con}$ and $SOEP_{act}$ display measures based on contractual and actual working hours, respectively, while $SES_{month}$ denotes the Fraction calculated with monthly wages of full-time employees only.

Table 3: Employment Effects

| | Fraction | | | | Kaitz | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Panel A: Regular Employment | | | | | |
| Bite × D2015 | -0.080*** | -0.017** | -0.011 | -0.012* | -0.008 | -0.004 |
| | (0.009) | (0.008) | (0.007) | (0.007) | (0.008) | (0.007) |
| Bite × D2013 | | | -0.005 | -0.003 | | -0.006 |
| | | | (0.007) | (0.007) | | (0.006) |
| Bite × D2012 | | | | -0.001 | | -0.010 |
| | | | | (0.014) | | (0.012) |
| GDP (log, t-1) | | -0.023 | -0.028 | 0.021 | -0.023 | 0.017 |
| | | (0.020) | (0.019) | (0.018) | (0.021) | (0.019) |
| Population (log, t) | | 0.907*** | 0.997*** | 0.982*** | 0.971*** | 1.025*** |
| | | (0.086) | (0.065) | (0.057) | (0.089) | (0.060) |
| Constant | 11.880*** | 0.807 | -0.275 | -0.539 | 0.015 | -1.039 |
| | (0.000) | (1.070) | (0.770) | (0.680) | (1.080) | (0.709) |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 282 | 282 | 423 | 564 | 282 | 564 |
| $R^2$ within | 0.886 | 0.941 | 0.962 | 0.955 | 0.940 | 0.955 |
| $R^2$ between | 0.120 | 0.994 | 0.994 | 0.995 | 0.994 | 0.995 |
| $R^2$ overall | 0.013 | 0.994 | 0.994 | 0.995 | 0.994 | 0.995 |
| | Panel B: Marginal Employment | | | | | |
| Bite × D2015 | -0.233*** | -0.168*** | -0.177*** | | -0.109*** | -0.115*** |
| | (0.022) | (0.028) | (0.025) | | (0.026) | (0.023) |
| Bite × D2013 | | | 0.049*** | | | 0.004 |
| | | | (0.018) | | | (0.019) |
| GDP (log, t-1) | | 0.069 | 0.034 | | 0.081 | 0.016 |
| | | (0.061) | (0.055) | | (0.067) | (0.059) |
| Population (log, t) | | 0.925*** | 0.807*** | | 1.277*** | 1.187*** |
| | | (0.261) | (0.193) | | (0.280) | (0.219) |
| Constant | 10.480*** | -1.688 | 0.119 | | -6.168* | -4.446 |
| | (0.001) | (3.292) | (2.411) | | (3.566) | (2.795) |
| Region FE | Yes | Yes | Yes | | Yes | Yes |
| Year FE | Yes | Yes | Yes | | Yes | Yes |
| Observations | 282 | 282 | 423 | | 282 | 423 |
| $R^2$ within | 0.727 | 0.761 | 0.704 | | 0.709 | 0.642 |
| $R^2$ between | 0.310 | 0.924 | 0.923 | | 0.922 | 0.920 |
| $R^2$ overall | 0.113 | 0.924 | 0.923 | | 0.922 | 0.920 |

Source: SES 2014, Destatis 2012-2015, FDE 2012-2015, own calculations.
Note: Robust standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is regular employment (Panel A) and marginal employment (Panel B) in logarithmic terms, annually measured on June 30th. Bite measure is denoted by the first row. Reference year in all specifications 2014.

Table 4: Robustness Analysis

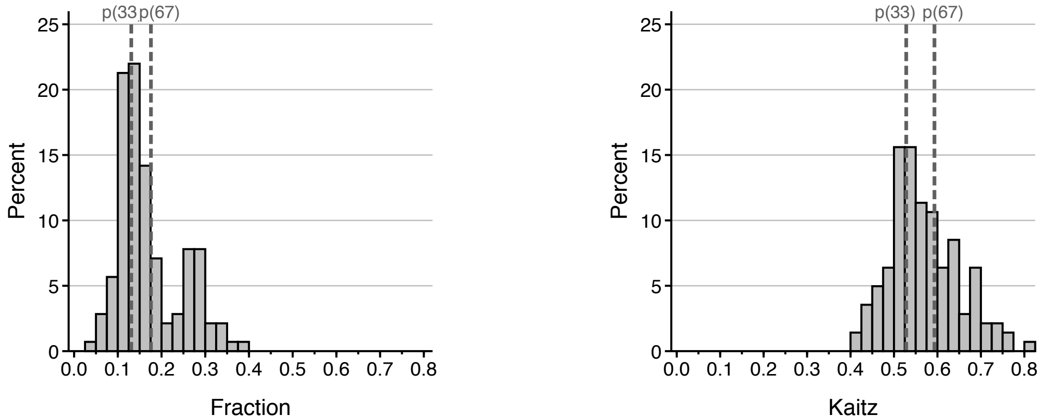
| | (1) Full | (2) ROR | (3) Weights | (4) Month | (5) SOEP | (6) p25/p75 | (7) p50/p50 |
|---|---|---|---|---|---|---|---|
| | Panel A: Regular Employment | | | | | | |
| Bite × D2015 | -0.012* | -0.012* | -0.014* | -0.042*** | 0.003 | -0.004** | 0.000 |
| | (0.007) | (0.007) | (0.007) | (0.015) | (0.012) | (0.001) | (0.001) |
| Bite × D2013 | -0.003 | -0.010 | -0.002 | 0.005 | -0.016* | 0.000 | -0.001 |
| | (0.007) | (0.007) | (0.007) | (0.013) | (0.009) | (0.001) | (0.001) |
| Bite × D2012 | -0.001 | -0.017 | 0.000 | 0.019 | -0.030* | 0.002 | -0.001 |
| | (0.014) | (0.017) | (0.014) | (0.027) | (0.017) | (0.003) | (0.001) |
| GDP (log, t-1) | 0.021 | 0.007 | 0.022 | 0.028 | -0.002 | 0.028 | 0.018 |
| | (0.018) | (0.017) | (0.019) | (0.018) | (0.024) | (0.025) | (0.019) |
| Population (log, t) | 0.982*** | 1.067*** | 0.972*** | 0.936*** | 1.089*** | 0.912*** | 1.021*** |
| | (0.057) | (0.071) | (0.063) | (0.053) | (0.064) | (0.082) | (0.049) |
| Constant | -0.539 | -1.508* | -0.424 | -0.035 | -1.714** | 0.280 | -0.997* |
| | (0.680) | (0.864) | (0.743) | (0.628) | (0.760) | (0.959) | (0.586) |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 564 | 384 | 564 | 564 | 288 | 288 | 564 |
| $R^2$ within | 0.955 | 0.967 | 0.955 | 0.956 | 0.976 | 0.944 | 0.955 |
| $R^2$ between | 0.995 | 0.990 | 0.995 | 0.995 | 0.991 | 0.996 | 0.995 |
| $R^2$ overall | 0.995 | 0.990 | 0.995 | 0.995 | 0.991 | 0.996 | 0.995 |
| | Panel B: Marginal Employment | | | | | | |
| Bite × D2015 | -0.177*** | -0.187*** | -0.186*** | -0.340*** | -0.147*** | -0.030*** | -0.015*** |
| | (0.025) | (0.030) | (0.025) | (0.048) | (0.043) | (0.005) | (0.003) |
| Bite × D2013 | 0.049*** | 0.064*** | 0.075*** | 0.094*** | 0.017 | 0.006 | 0.001 |
| | (0.018) | (0.024) | (0.016) | (0.033) | (0.031) | (0.005) | (0.002) |
| GDP (log, t-1) | 0.034 | 0.044 | 0.037 | 0.053 | -0.057 | 0.070 | -0.012 |
| | (0.055) | (0.072) | (0.053) | (0.056) | (0.082) | (0.094) | (0.056) |
| Population (log, t) | 0.807*** | 0.677*** | 0.692*** | 0.924*** | 1.387*** | 1.039*** | 1.408*** |
| | (0.193) | (0.243) | (0.152) | (0.179) | (0.256) | (0.281) | (0.157) |
| Constant | 0.119 | 1.802 | 1.526 | -1.524 | -6.436* | -3.217 | -6.926*** |
| | (2.411) | (3.176) | (1.949) | (2.294) | (3.573) | (3.260) | (2.111) |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 423 | 288 | 423 | 423 | 216 | 216 | 423 |
| $R^2$ within | 0.704 | 0.745 | 0.731 | 0.698 | 0.710 | 0.764 | 0.640 |
| $R^2$ between | 0.923 | 0.862 | 0.923 | 0.923 | 0.879 | 0.929 | 0.918 |
| $R^2$ overall | 0.923 | 0.862 | 0.923 | 0.923 | 0.879 | 0.929 | 0.918 |

Source: SES 2014, Destatis 2012-2015, FDE 2012-2015, own calculations.
Note: Robust standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is regular employment (Panel A) or marginal employment (Panel B) in logarithmic terms, annually measured on June 30th. Fraction as continuous bite measure used in all specifications. Reference year in all specifications 2014. Regional and time fixed effects included. Controls are GDP in $t-1$ and Population in $t$. Specification (1) is the baseline estimation, (2) applies the regional concept of ROR, (3) relies on weights to compute level of Fraction, (4) applies Fraction of full time employed earning less than €1,400 per month, bite indicators computed with SOEP 2014 are used in (5). Specifications (6) and (7) include the Fraction as a binary measure, with cut-off points at first and last quarter of the distribution and at the median, respectively.

# A Supplementary Appendix

Figure A.1: Fraction and Kaitz Distribution

(a) Fraction (b) Kaitz




Source: SES 2014, own calculations.
Notes: Band width set to 0.025. $p(33)$ and $p(67)$ denote the 33th and 67th percentile of the distribution.