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Side effects of the new German minimum wage on (un-)employment:

First evidence from regional data

Alfred Garloff

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Alfred Garloff, Institute for Employment Research (IAB); (Federal Ministry of Economic Affairs and Energy)

Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

The “IAB-Discussion Paper” is published by the research institute of the German Federal Employment Agency in order to intensify the dialogue with the scientific community. The prompt publication of the latest research results via the internet intends to stimulate criticism and to ensure research quality at an early stage before printing.

Contents

Abstract

In Germany, decreasing collective bargaining coverage and rising wage inequality led to the introduction of a new statutory minimum wage of € 8.50 per hour of work. We analyze the relationship between the bite of the minimum wage and employment/ unemployment growth using regional data of the Federal Employment Agency for prime age individuals. We use difference-in-differences type of specifications using a panel of region-age-sex cells. The results do not provide evidence in favor of a reduced employment growth for the analysed groups, nor do they provide evidence for an increase in unemployment growth due to the minimum wage. However, we find an increase in growth of regular employment at the expense of marginal employment.

Zusammenfassung

Seit 1.1.2015 gilt in Deutschland ein allgemeinverbindlicher Mindestlohn. Eine wichtige Frage ist, ob der Mindestlohn, neben seinem Hauptzweck, Arbeitnehmer vor niedrigen Löhnen zu schützen, auch weitere, ggf. unerwünschte Nebenwirkungen hat. Die ökonomische Theorie erlaubt keine eindeutige Aussage, ob negative Wirkungen auf Beschäftigung und Arbeitslosigkeit auftreten.

Ziel dieses Papieres ist es zu analysieren, ob zum derzeitigen Zeitpunkt bereits negative Wirkungen des Mindestlohnes auf Beschäftigung und Arbeitslosigkeit nachweisbar sind. Dazu nutzen wir die regionale Variation der Eingriffstiefe des Mindestlohnes in die Lohnverteilung und deren Zusammenhang zur regionalen Beschäftigungs- und Arbeitslosenentwicklung in einem Panel von Regionen, Altersgruppen und Geschlecht.

Für die analysierten Gruppen zwischen 30 und 54 Jahre alter Personen, finden wir, dass Zellen die stark vom Mindestlohn betroffen waren, kein langsameres Wachstum der Gesamtbeschäftigung (sozialversicherungspflichtig und geringfügig Beschäftigte) aufweisen als Zellen die weniger stark betroffen waren. Allerdings sind in Zellen die stark vom Mindestlohn betroffen waren, Minijobs verloren gegangen. Gleichzeitig sind aber in diesen Zellen auch besonders viele sozialversicherungspflichtige Beschäftigungsverhältnisse entstanden. Dies legt eine Umwandlung nahe. Für die Arbeitslosigkeit können wir, ähnlich wie für die Gesamtbeschäftigung, keinen signifikanten Zusammenhang zum Eingriff des Mindestlohnes messen.

Wir schlussfolgern, dass die vorgelegte Evidenz dafür spricht, dass der Mindestlohn bisher weder zu Rückgängen der Gesamtbeschäftigung bei den analysierten Gruppen noch zu einem Anstieg der Arbeitslosigkeit geführt hat.

JEL classification: C23, J38, R10

Keywords: Minimum wage, employment effects, difference-in-differences, regional variation, Germany

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1 Introduction

On 1 January 2015 a new statutory minimum wage of € 8.50 per hour of work was introduced in Germany. While the minimum wage legislation experienced extensive support from the public, many economists are still sceptical and predict negative effects of the minimum wage on employment (ifo Ökonomenpanel 2016). The scepticism is grounded on the theoretical consideration that minimum wages restrict competitive labour markets to hourly wages of at least € 8.50. According to standard neo-classical theory, wages equal the value of marginal product. If wages are forced to exceed the competitive equilibrium wage, employers will not have them and therefore reduce employment. However, this negative prediction for employment can be relaxed when labour markets are monopsonistic (Dickens/Machin/Manning 1994, 1999; Garloff 2010). Moreover, empirical studies addressing minimum wages often fail to detect negative employment effects (e.g. Card/Krueger 1994; Dube/Lester/Reich 2010; Doerr/Fitzenberger 2015). This theoretical and empirical ambiguity calls for scientific ex-post evaluations of the new German minimum wage (Arni et al. 2014; Möller 2014; Zimmermann 2014). We present an early attempt of an ex-post evaluation by comparing regional data (further differentiated according to age groups and sex) cells that are differently affected by the introduction of the minimum wage in difference-in-differences type of specifications.

For many decades the empirical literature has discussed potential employment effects of minimum wages. The controversy gained momentum after the famous case study on relative minimum wage increase in New Jersey compared with Pennsylvania (Card/Krueger 1994). This study failed to detect a negative employment effect. The authors even show positive effects on employment of fast food restaurants in New Jersey, which were affected by an increase in the state minimum wage, compared to otherwise similar fast food restaurants across the border in Pennsylvania. These results have been challenged by Neumark and Wascher (2000), who claim to detect negative effects in the same institutional setting. This controversy lasted for a couple of years with numerous well published articles (Card/Krueger 2000; Neumark/Wascher 1992, 2000). More recently, the literature converged to a point where even the opposing studies find only small employment elasticities (Addison/Blackburn/Cotti 2015; Neumark/Salas/Wascher 2014). Along with this development new methods such as the synthetic control method, border discontinuities, and interactive fixed effects were applied to minimum wages in the US and mostly fail to detect negative employment effects (Dube/Lester/Reich 2010; Allegretto et al. 2015). All these studies on US minimum wages use regional variation for identification. This is in line with the approach that we present in this article, as we exploit regional variation in the bite of the minimum wage.

The German experience of minimum wages is restricted to sector specific minimum wages such as in the construction sector or the roofing sector, which were introduced in 1997. Some other sectoral minimum wages were introduced before 2015 including hair dressing (in 2013) or security services (in 2011). But these more re-

cently introduced sectoral minimum wages have not entered the economic discussion yet.

König and Möller (2009) analyse the introduction effects of the minimum wage in the construction sector by comparing affected workers with unaffected workers of the same sector and find negligible employment effects. Analyzing the minimum wage for electricians and painters Frings (2013) does not find negative effects. However, Aretz, Arntz, and Gregory (2013) find a considerable negative effect for the employment retention in the roofing sector. The only study that uses regional variation is vom Berge, Frings, and Paloyo (2013), who find negative effects for the minimum wage in the construction sector.

So far, there is little evidence on the new statutory minimum wage in Germany. Groll (2015) descriptively compares aggregated employment trends and claims to find a negative effect on employment. By contrast, the descriptive governmental monitoring ("Arbeitsmarktspiegel"), which describes individual transitions in the months around the minimum wage introduction, does not detect a large flow into unemployment (vom Berge et al. 2016a, 2016b). However, it shows an increase in the transitions from highly affected marginal employment, which is defined by a monthly salary of no more than € 450, into regular employment. Bossler (2016) uses establishment-level micro data from before of the minimum wage introduction and shows a modest negative effect on the affected employers' employment expectations. Bossler and Gerner (2016) are the first paper to show ex-post effects of the statutory minimum wage on employment. They use the establishment level affectedness by the minimum wage, based on survey data (the IAB establishment panel) and show that employment growth was lower because of the minimum wage. In their estimates, employment growth was 60,000 individuals below the value that would have been expected without the introduction of the minimum wage. Although the size of the effect is modest, it is still a negative effect. In the conclusion, we discuss why their results may differ from ours.

We contribute to the literature by providing first evidence on the relationship between employment and unemployment growth and the bite of the minimum wage using the variation over regions, age groups, and sex using data of the Federal Employment Agency. We can track employment and unemployment growth over time which provides us with the possibility of constructing a panel of cells. This allows for difference-in-differences type of specifications. The relationship is identified by comparing the employment growth in cells, which were heavily affected (e.g. middle-aged women in the North of Mecklenburg-Vorpommern) by the introduction of the minimum wage with cells that were not strongly affected by the introduction of the minimum wage (e.g. middle-aged men in the labour market region Frankfurt). While the cell data provide less detail than micro data, it comes with the advantage that the estimation is robust towards spill-over effects within cells. This is because employment changes are only identified across cells, but not within such cells.

The article proceeds as follows: Section 2 describes the economic background of the minimum wage introduction, which is the decreasing collective bargaining coverage and the rising wage inequality. Section 3 derives the econometric approach of our analysis. Section 4 describes the publicly available data source. Section 5 presents the estimation results including effects on employment and unemployment and, finally, Section 6 concludes.

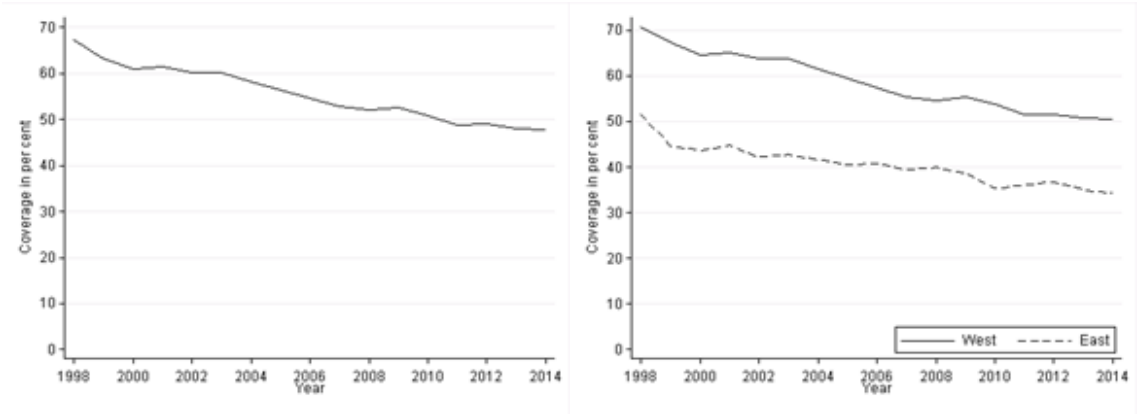
2 The new German statutory minimum wage

There is a political and an economic way to look at the introduction of the minimum wage in Germany. Politically, the introduction of the minimum wage was a part of the coalition agreement in December 2013 of the Grand coalition (social democrats, SPD, and conservatives, CDU/CSU) in Germany. To this date, only a minority of employees was covered by sectoral minimum wage regulations. Up to 1996, there were no minimum wages in Germany. Starting in 1996, minimum wages were introduced in the construction sector as a prolongation of collective bargaining outcomes, opening the principal possibility to extend this to other sectors as well. At the time, this was predominantly done to protect the German construction sector against Eastern European competitors in the context of the enlargement of the European Union (Apel et al. 2012). In the 2013 coalition negotiations, the SPD was able to negotiate a statutory minimum wage in the coalition agreement by making their participation in the Grand Coalition dependent on a party vote over the coalition agreement. This gave them an exceptionally strong position in the negotiations. In mid 2014, the minimum wage law ("Tarifautonomiestärkungsgesetz") passed the two chambers of the Parliament and became effective as of August 2014. It contained the introduction of a minimum wage of € 8.50 from January, 1st, 2015, plus extended possibilities for sectoral minimum wages. There are few exemptions of the minimum wage: it does not apply to those under 18 years of age, to individuals in vocational training, to interns, to the former long-term unemployed (more precisely those long-term unemployed without interruptions within the last year) in the first 6 months of new employment, and individuals who work in sectors where a binding minimum wage under € 8.50 exists (they are exempted from the minimum wage until 2017).

The traditional and most important wage setting institution in Germany is collective bargaining. In the process of collective bargaining employer associations representing their member firms bargain with a union (sometimes with several union), representing the employees of the firms, over wages and other working conditions, where collective bargaining is mostly characterized by a sectoral and a regional dimension. These agreements are typically for one to two years. Then, the process restarts. Typically, the collective bargaining agreements are extended to all employees of the firms, not only union members. Since 1996, there exists a possibility to extend these negotiated working conditions from the collective bargaining agreement to all firms and employees within the sector and region. By the end of 2014, around 4 million or under 12 percent of total employment is covered by extended collective agreements.

Figure 1:
Collective bargaining coverage, 1998 to 2014

Panel A: Overall Germany Panel B: Eastern and Western Germany



Notes: Collective bargaining coverage in Germany (Panel A) and collective bargaining coverage in Eastern and Western Germany (Panel B). The yearly averages are weight using non-random sampling weights and, additionally, we weight by establishment-level employment, which yields an interpretation in terms of covered employees.

Source: IAB-Establishment Panel 1998–2014.

In the empirical literature, collective bargaining is often attributed economically relevant effects on outcomes such as wages (Addison et al. 2014), productivity (Hübler/Jirjahn 2003), and labour turnover (Pfeifer 2011). While collective bargaining has a long tradition, recent data illustrates a decreasing coverage (Figure 1). The fraction of employees covered by collective bargaining steadily decreased since the late 1990s. Moreover, Panel B of Figure 1 shows that collective bargaining coverage is at a lower level in the East than in the West, but the decreasing pattern evolves very similar. This falling employee-level coverage is mostly driven by a lower fraction of firms participating in their respective employer associations' bargaining.[1] As firms that are not participating in collective bargaining agreements typically pay lower wages than firms that do participate (Fitzenberger/Kohn/Lembcke 2013), a decreasing coverage likely leads to lower average wages and might also lead to problems at the very bottom of the wage distribution; wages that are typically looked after by unions in the wage negotiations.[2] Thus, a decreasing importance of collective bargaining may provide reasons to introduce minimum wages.

Most importantly, this development reflects that participation in collective bargaining and application of its outcomes is voluntary for firms that do not participate in collective bargaining. Minimum wages, however, are legally binding for all employers. Corresponding with the idea of sectoral bargaining, the first minimum wages were set by extending collective bargaining agreements on a sectoral level, before the federal minimum wage was introduced on 1 January 2015.

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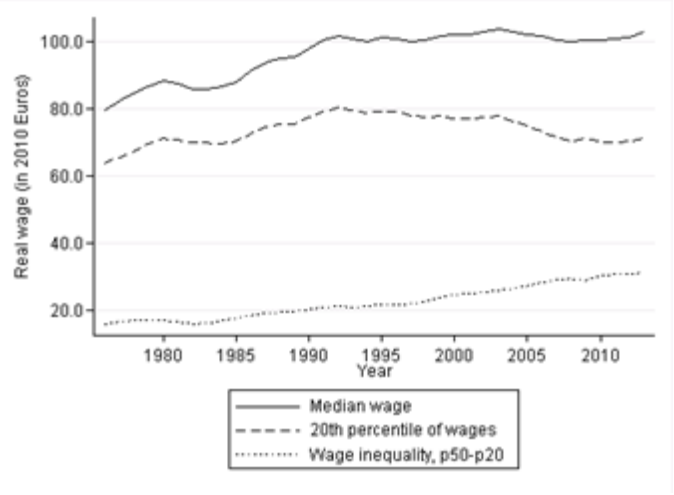
1 This can be because fewer firms are members of employer associations, or they are members without tariff commitment.

2 It is to be noted however, that the cited paper finds that union density rather than firm-level coverage affects wage dispersion negatively (ibid.).

Figure 2:
Wage inequality in Western Germany, 1976 to 2013



Notes: Development of the median wage, the 20th percentile of wages, and the wage distribution's lower tail inequality defined by the 50th minus the 20th percentile of the wages. Wages are deflated to 2010 real Euros.

Source: IAB integrated employment biographies: 50-percent sample of all male employees in Western Germany liable to social security contributions.

Other developments advancing the introduction of the minimum wage were the stagnating real wage development and the increasing wage inequality. Figure 2 illustrates that the real median wage remained stagnant since the early 1990, and the 20th percentile of the real wage distribution even started to fall within the same time span. Together these developments cause an increase in wage inequality, particularly in the lower tail of the wage distribution depicted by the dotted line. (see, e.g. Kohn 2006; Möller 2008; Dustmann/Ludsteck/Schönberg 2009; Fitzenberger 2012; Card/Heining/Kline 2013; Burda/Seele 2016). More recently, Möller (2016) shows that there is no more increase in wage inequality after 2010 and for some groups even a decline of wage inequality until 2014.

Minimum wages are a potential policy tool to tackle wage inequality and to foster wage growth along the lower tail of the wage distribution (Gregory 2014). The impact on wages and a modest effect on wage inequality are mostly approved in the economic literature (e.g. Dickens and Manning 2004). However, employment effects – as side effects – are much more uncertain and the focus of the following sections.

Theoretically, in competitive labour markets binding minimum wages would give rise to decreasing employment and to increasing unemployment. In a production function framework, given that there is diminishing marginal productivity in the use of the affected individuals, the employment decreases generally by less than the number of affected. If marginal productivity was constant, employment would decrease by the number of persons affected through it. With increasing marginal productivity, the employment loss could even be larger. When there are other production factors that are not affected by the minimum wage (e.g. high-skilled individuals), they could profit from the employment loss of the affected factor, when production is complementary, they could also lose when production is substitutive. When employment de-

creases, unemployment normally increases. The effect of the minimum wage on unemployment generally also depends on participation. The effect on unemployment could be more pronounced, when participation increases due to the increased wage expectation. On the other side, it is also possible that people retreat from unemployment as discouraged workers, because search costs are higher than the expected utility from job search. They may also revise their participation decision because they do not have unemployment claims (for example, pensioners or students). If labour markets are not competitive, for example because there are search or matching frictions, effects on employment and unemployment can be anything: negative, zero, or positive (see e.g. Garloff 2010).

3 Estimation method

In the empirical part of the paper, we explain the employment development ΔL_{ijkt}, which is the growth rate of employment in region i, age group j, and sex k relative to previous year's employment in the same month, i.e. $\Delta L = \frac{L_{ijkt} - L_{ijkt-1}}{L_{ijkt-1}}$. We identify the relationship between the minimum wage bite and the employment development using a standard (unweighted) fixed effect panel specification:

$$\Delta L_{ijkt} = B_{ijk}\beta_1 + D_t^{MW}\beta_2 + B_{ijk}D_t^{MW}\alpha + \sum_t D_t^{month} * \gamma^t + \theta_{ijk} + \varepsilon_{ijkt} \qquad (1)$$

The subscript *ijk* represents the units of observation, which are labour market regions-- age group—sex cells in our baseline sample. However, we test different aggregations in our robustness checks (see below). Subscript *t* represents monthly time observations, which characterize our panel structure. B_{ijk} is the measure for the bite, i.e. the share of persons whose wage is below the minimum wage before the introduction of the minimum wage. The parameter of interest in this specification is α, the relationship between the bite after the introduction of the minimum wage ($D_t^{MW} = 1$) and employment growth. The parameter is identified over the time variation because the bite is constant over time. Clearly, β_1 is not identified in equation (1). β_2 describes whether average employment growth was different in 2015, conditional on the other covariates, particularly the bite. γ^t captures aggregate time specific effects for each month ($D_t^{month} = 1$) in the analysis sample. The specification further controls for a unit fixed effect θ_{ijk}, i.e. a cell specific employment growth over the time period is controlled for. ε_{ijkt} is assumed to satisfy strict exogeneity in the sense of Wooldridge's (2002) equation (10.12) (p.253). I.e. the conditional expectation of the error term for all t conditional on all explanatory variables, including the fixed effects, is zero.

In a second step, we slightly change the specification and allow for flexible interactions between the bite and time for each month before and after the minimum wage introduction. Thus, for every month the bite is allowed to have a separate relationship with employment growth:

$$\Delta L_{ijkt} = B_{ijk}\beta_1 + \sum_t D_t^{month} * B_{ijk} * \alpha^t + \sum_t D_t^{month} * \gamma^t + \theta_{ijk} + \varepsilon_{ijkt} \qquad (2)$$

α^t captures the relationship between the bite and employment growth in each month included in the sample (we choose the 4 month preceding and following the introduction of the minimum wage). After the minimum wage introduction this allows for effect differences in each month, which is January to April 2015. Additionally, it allows for anticipation effects in the months before the minimum wage was introduced. Or, if there was no anticipation, it allows for checking whether the bite measure really measures the impact of the minimum wage, or whether there are other unobserved factors causing low-wage cells to grow at a different pace than others.

An underlying assumption of our approach is the homogeneity of the treatment effect across cells, which we use as observations. As we use labor market region-age group-sex cells, we compare male cells with female cells. Moreover, we compare different age groups, i.e. the age group 30–34 years with the group that is 50–54. We also compare labour market regions across Germany. If the cells are not homogenous, i.e. males and females face different effects from the minimum wage, our results can be biased. In our robustness checks, we test two crucial differentiations and estimate separate effects by gender and for East and West Germany. We also test whether the relationship is similar across age groups.

Further, below we interpret the results as meaning that there is no significant relationship between employment and the minimum wage bite for the age groups considered. Whether these results do also carry over to the young and the old age groups is an open question. When the effect of the minimum wage is homogenous across age groups, the results carry over. Although the young age groups are more heavily affected than the prime agers, it is by no means clear that the effects are heterogenous. It is to be noted however, that the literature addresses minimum wage effects on youth employment separately from the effects on total employment (e.g., Abowd et. al., 1997)

As we use differences and not levels as dependent variable, the approach is of use only for the first year after the introduction of the minimum wage. This clearly means that our only interest is the estimation of short-term effects of the minimum wage. We use differences instead of levels, because levels are affected by a variety of reasons which we would have to model when explaining them. As far as those factors do not vary over time, the use of the difference allows us to ignore them. In that sense our approach allows to take into account fixed effects both in levels and in differences. A caveat of doing this with monthly observations and 12 months differences is that the autocorrelation of the observations is high.[3] To account for this, we also estimate a dynamic version of the panel (see below). We regard this setup as a generalized differences in differences specification since we do not have a treatment

[3] Using month-to-month differences instead would imply to identify the effect from one observation only, namely the difference between December and January. In our view, the use of the 12 months difference for the 12 months is 2015 conveys more information on the effect of the minimum wage than just looking at one observation.

group and a control group at our disposal but many groups that are affected by the minimum wage differently.

4 Data

We use publicly available data of the statistical office of the Federal Employment Agency (henceforth BA).[4] The BA publishes official employment and unemployment statistics for each month. This data can be ordered on customized levels of aggregation. From this data source, we hold a monthly data panel of prime age workers of age between 30 and 54. The data is disaggregated by sex, five age groups (30–34, 35–39, 40–44, 45–49, 50–54), and 141 labour market regions.[5] The labour market regions can be constructed sharply from districts ("Kreise") in Germany and are defined by commuting so that labour market regions show little overlap in terms of flows of employees (Eckey/Kosfeld/Türck 2006).

In total the disaggregation leads to a data panel with 5*2*141=1,410 data cells which are our units of analysis. The panel covers the time period from January 2013 to April 2015. Since we look at the 12-month employment development as dependent variable, we yield an analysis sample of 18 months from January 2014 to April 2015.

In the robustness checks, we present results from two alternative levels of disaggregation. First we use data for each of the 403 districts instead of labour market regions, again disaggregated by sex and age groups. Second, we use the employment projections of the BA which are available with a waiting period of 2 months, which cannot be disaggregated into age, sex or full-time/part-time but can be disaggregated by region ("Bundesländer")-industry cells comprising all states and 20 different industries.

The major outcome variable of interest is the employment growth in each unit of observation. We look at regular employment, which is subject to social security contributions, and (exclusive) marginal employment, which is not subject to social insurance contributions.[6] We also consider total employment, the sum of both. Further, we distinguish between full-time and part-time employment. In the period of analysis marginal employment is legally defined by an employment contract that pays no more that € 450 a month.[7] Our second outcome variable is regional unemployment growth, which is also available regionally, by sex and age groups. So far, we do not

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4 The data are available online at http://statistik.arbeitsagentur.de/

5 These data are available with a waiting period of 7 months.

6 For a robustness check we also consider total marginal employment that consists of exclusive marginal employment (about two thirds of all marginal employment) and marginal employment of second earners (one third). The results are generally similar but smaller than for exclusively marginally employed individuals.

7 The threshold of € 450 which defines marginal employment was raised from € 400 on 1 January 2013. This limits the length of the data panel to the months starting from January 2013.

have unemployment data disaggregated by age groups. So the unemployment regressions are based on 282 cells only. For obvious reasons the unemployment growth is not available for a disaggregation by industry.[8]

We define the bite of the minimum wage from two kinds of data. First, we construct a measure from the remuneration statistic ("Entgeltstatistik") of December 31st, 2013 provided by the Federal Employment Agency. More precisely, for each cell of interest we calculate the share of full-time employed individuals whose monthly wages are below € 1,400 (€ 1,500/€ 2,000 for robustness). We do this since there is no information on hours worked in the data. In this respect we share the difficulties of other studies addressing the effects of the sectoral minimum wages in Germany (Doerr/Fitzenberger 2015). Thus, our bite measure measures the approximate bite for full-time employees only[9]: € 1,400 corresponds to about 38 hours of work per week, € 1,500 to slightly less than 41 hours. This is probably the range into which full-time employment falls for most full-time employees. Further, the bite is approximate in the sense that between December, 31st 2013 and December, 31st 2014 the wage distribution did not remain constant.[10] This might be seen as an advantage because in 2013 anticipation effects are unlikely whereas anticipation has been shown to exist in 2014 (see Kubis/Rebien/Weber 2015). But this clearly makes our bite measure less precise. If on the left hand-side we use measures other than full-time employment, the bite measure is even harder to justify. Its use can be justified if the bite measures for the different groups considered are highly correlated across cells. In this case, again, there remains a problem of (hopefully uncorrelated) measurement error in the dependent variable, which leads to a bias towards zero (Wooldridge 2002: 73 f.).[11]

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8 The reason is that, an unemployed person cannot be uniquely assigned to a sector, as is it generally unknown and uncertain in which sector a particular unemployed individual is searching for a job.

9 Further measurement errors might stem from minimum wage exemptions. As we do not consider people under the age of 18 or individuals in vocational training, however, there should not be a problem for our analysis with these groups. More problematic is the exemption of individuals whose extended collective bargaining agreement is below € 8.50. However, not many sectors have undercut the minimum wage: There are around 600,000 employees in sectors and regions where sectoral minimum wages are allowed to be below € 8.50, but even in these sectors the share of individuals whose wage is below € 8.50 is relatively small (over 20 percent only in agriculture and floristics, see Amlinger et al 2016). In addition, these wages generally are only slightly below the minimum wage. What we also cannot control for is the exemption for long-term unemployed in the first 6 month of new employment. But it seems that this exemption has not been applied very often. This might be because the access to this exemption is restricted to a sub group of the around 1 million long-term unemployed, namely, those who did not have any interruption of their unemployment spell in the last year.

10 The remuneration statistics for 2014 will become available at the end of April 2016.

11 We plan to construct, for robustness reasons, a measure from the 2014 IAB-Establishment Panel, which includes a direct question on the number of affected employees within each establishment in the sample. While the Entgeltstatistik allows for a disaggregation of all our samples, the IAB-Establishment Panel only allows constructing a measure of the bite by state and industry.

In our aggregated data and in the fixed effect model there is only little scope for time-varying control variables which can be measured on a monthly basis. For all employment specific variables, which are available and reported by the BA, we cannot claim exogeneity. Therefore, we restrict our analysis to an (almost) unconditional generalized difference-in-differences

Table 1:
Sample description, unweighted averages

	(1) All units of observation	(2) Above median bite	(3) Below median bite
Avg. number of employees	12,041	9,936	17,851
Avg. growth rate of employees	1.22	1.55	0.31
Avg. number of regular employees	10,586	8,361	16,725
Avg. growth rate of regular employees	1.73	2.18	0.50
Avg. number of marginal employed	1,456	1,575	1,126
Avg. growth rate of marginal employees	-1.27	-1.11	-1.71
Avg. bite of the minimum wage (%) (region*age*sex)	8.85	14.47	3.90
Avg. number of unemployed	4,665	4,042	5,245
Avg. growth rate of unemployed	-0.02	-1.13	1.00
Avg. bite of the minimum wage (%) (region*sex)	19.57	30.21	9.66

Source: BA employment statistic and BA earnings statistics, analysis sample. All averages refer to cells, which are defined by region, age and sex for employment and by region and sex for unemployment. The bite of the minimum wage refers to region, age, sex. Time period used 12/2012–04/2015.

A description of the baseline analysis sample is presented in table (1), which displays averages by bite of the minimum wage for total employment, regular employment, marginal employment and unemployment in levels and growth rates. We observe that labour market region-age group-sex cells are smaller when the bite is above the median but they have a more dynamic employment growth on average. The same patterns for employment levels and growth hold true for regular employ-

ment subject to social security contributions. However, the number of marginal employees is on average larger in cells for which we calculated a pebite above the median. This corresponds with the notion that marginal employees are by definition of their monthly pay more severely affected. The average bite of the minimum wage is 8.8 percent, which is 14.5 for units above the median and only 3.9 percent below the median, indicating that there is sufficiently large variation in the data to study the effects of the minimum wage.

5 Regression results

The baseline regression results on employment growth are displayed in table (2).[12] Panel A shows the relationship between the bite in 2015 and employment growth, which aggregates the relationship for the months January to April. The relationship between the bite in 2015 and total employment is virtually zero. I.e. from our regression, we cannot detect negative consequences of the minimum wage on the employment of the age groups considered. However, the estimates on the development of regular employment show a significant positive relationship between the growth of regular employment and the bite of the minimum wage. Although theoretically possible, this is a surprising sign result. At the same time, and less surprising, there is a strong(er) negative relationship between the bite of the minimum wage and the growth of marginal employment.

[12] The baseline specifications are unweighted regressions. Using approximate weights in the fixed effects model (approximated weights = relative cell size in the beginning of the observation period) does not change the results qualitatively, nor does the use of exact weights in the GMM estimations (exact weights = relative cell size at the moment of every observation).

Table 2:
Employment effects of the minimum wage

	(1) Total employment	(2) Employment subject to social insurance contributions	(3) Marginal employment
Panel A: Coefficient 2015			
Bite*2015	-0.003	0.045***	-0.247***
	(0.006)	(0.007)	(0.030)
Panel B: Separate coefficients for the months before after the minimum wage introduction			
Bite*09/2014	-0.007	0.006	-0.107***
	(0.005)	(0.005)	(0.030)
Bite*10/2014	-0.010*	0.004	-0.111***
	(0.006)	(0.006)	(0.031)
Bite*11/2014	-0.011*	0.007	-0.128***
	(0.007)	(0.007)	(0.033)
Bite*12/2014	-0.016**	0.006	-0.176***
	(0.007)	(0.007)	(0.034)
Bite*01/2015	-0.012*	0.034***	-0.270***
	(0.007)	(0.008)	(0.036)
Bite*02/2015	-0.005	0.047***	-0.274***
	(0.008)	(0.008)	(0.038)
Bite*03/2015	0.000	0.056***	-0.300***
	(0.008)	(0.008)	(0.038)
Bite*04/2014	-0.006	0.050***	-0.282***
	(0.008)	(0.009)	(0.041)
Observations	26,790	26,790	26,790
Cluster	1,410	1,410	1,410

Notes: Labour market regions, 12/2012–04/2015; standard errors are robust.
Source: BA employment statistic and BA earnings statistics.

Panel B displays the relationship for each month before and after the minimum wage introduction. This allows an assessment of when such effects occurred. While there is some room for anticipatory adjustments, an effect which is due to the minimum wage should be observed in all months after December 2014 (until December 2015 in our setting). Column (1) shows that there is some variation in total employment over the months of observation, which is also correlated with the bite. However, we do not observe any clear pattern indicating a minimum wage effect on overall employment growth. While it is true that most monthly interactions show an (insignificant) negative sign, giving rise to the possibility that the measurement error obscures a negative relationship, our view on the entity of results is that this is not consistently the case over specifications (see the robustness part). Thus, we do not

think, but cannot exclude either, that a negative effect of total employment is obscured by insignificant coefficients.

Looking at the two other employment outcomes, we observe an increase of regular employment and a drop in marginal employment in January 2015, which is shortly after the minimum wage was introduced. For regular employment the patterns are relatively coherent: while coefficients are small and insignificant from September to December 2014, the size of the coefficients is about 6 times larger in 2015 than in 2014 and they are significant. For marginal employment the coefficients are about double the height in 2015 than in 2014, but they are significant already before the introduction of the minimum wage, implying either anticipation or measurement problems of the bite for this group.

We conclude that from the relationship between the bite of the minimum wage and employment growth, there is not much evidence in favour of negative effects of the minimum wage on total employment. Instead, our interpretation of the findings, which is also confirmed by a dataset that has been assembled for purpose of tracing changes through the minimum wage (vom Berge 2016a, 2016b), is that the minimum wage created regular employment at the expense of marginal employment. This can be viewed as a politically desirable result since changing marginal employment into regular employment is advantageous to both, the welfare of the individual employee and the Germanys social security system.

6 Heterogeneities across dimensions

As a first check for heterogeneities we estimate the relationships separately for regions in East and West Germany. We do this on the basis of districts rather than labour market regions because not all labour market regions can be uniquely assigned to the East or West. As the bite of the minimum wage is significantly larger in the East (Bellmann et al. 2015), one might expect potentially larger disemployment effects in the East. However, the results on the growth of total employment do not show significant relationships for East Germany. For regular employment, a positive relationship is detected in East Germany whereas marginal employment is negatively correlated with the minimum wage bite in 2015. The results for West Germany show a slightly different pattern: Regular employment is positively related to the bite of the minimum wage and so is, surprisingly, total employment and marginal employment. Taking these results seriously would imply that the minimum wage in West Germany did not even destroy marginal employment.

Table 3:
Heterogeneities for Eastern and Western Germany

	(1) Total employment	(2) Employment subject to social insurance contributions	(3) Marginal employment
Panel A: Coefficient for Western Germany			
Bite*2015	0.017**	0.082***	0.059**
	(0.007)	(0.007)	(0.027)
Panel B: Coefficient for Eastern Germany			
Bite*2015	-0.000	0.031***	-0.171***
	(0.010)	(0.010)	(0.048)
Observations	61,940/14,440	61,940/14,440	61,940/14,440
Cluster	3,260/760	3,260/760	3,260/760

Notes: Districts; 12/2012–04/2015, standard errors are robust.
Source: BA employment statistic and BA earnings statistics, analysis sample.

When we separate the relationship by sex, we find negative coefficients on total employment, which are not significantly different from zero. Moreover, we observe strong negative partial correlations with marginal employment while the relationship with regular employment is not significant

Table 4:
Heterogeneities for males and females

	(1) Total employment	(2) Employment subject to social insurance contributions	(3) Marginal employment
Panel A: Coefficient for males			
Bite*2015	-0.026	0.028	-0.743***
	(0.025)	(0.026)	(0.103)
Panel B: Coefficient for females			
Bite*2015	-0.013	0.018	-0.496***
	(0.011)	(0.012)	(0.043)
Observations	13,395	13,395	13,395
Cluster	705	705	705

Notes: Labour market regions, 12/2012–04/2015, standard errors are robust.
Source: BA employment statistic and BA earnings statistics, analysis sample.

Although mathematically it is by no means necessary that the results for Germany as a whole represent a weighted average for East and West and for women and men, it is of concern that this does not seem to be the case. E.g. the absolute size of the coefficient for marginal employment for Germany is larger than the values for East and West.

7 Results for regional unemployment

Next, we estimate equation (1), replacing ⟦ΔL⟧_ijkt by ⟦ΔU⟧_ikt , the growth rate of unemployment for each labour market region and for men and women. As discussed above, a decline in employment would not necessarily result in an equal increase in unemployment because the participation decision might play a role.

We measure unemployment growth from labour market regions-sex groups. Again, we use the fraction of affected full-time workers as the measure of the bite in the respective unit of observation.[13]

Table 5:
Minimum wages effect on unemployment

	(1)
	Unemployment
Panel A: Coefficient 2015	
Bite*2015	0.014
	(0.026)
Panel B: Separate Coefficients for the months before after the minimum wage introduction	
Bite*09/2014	0.059**
	(0.029)
Bite*10/2014	0.072**
	(0.030)
Bite*11/2014	0.042
	(0.030)
Bite*12/2014	0.062**
	(0.030)
Bite*01/2015	0.055**
	(0.027)
Bite*02/2015	0.023
	(0.025)
Bite*03/2015	-0.023
	(0.025)
Bite*04/2015	0.009
	(0.027)

Notes: Dependent variable is the growth rate of unemployed individuals. Estimates as specified in Table (2), standard errors are robust.

Source: Aggregate data of the Federal employment agency. Observations are labour market region-gender cells.

The result for the development of unemployment is displayed in Table (5). Panel A shows no significant relationship between the bite of the minimum wage in 2015 and the growth rate of unemployment in the months after the minimum wage was intro-

[13] Doing this, we have similar measurement problems as above.

duced. Panel B displays separate effects for the month before and after the minimum wage introduction and also shows no clear structure of the relationship between the bite of the minimum wage and unemployment across regions and months. The results do not imply anticipation because in this case the positive coefficients should persist throughout 2015. Because the relationship between our bite measure and unemployment growth is not strongly related to the time of the introduction of the minimum wage, one should be careful in interpreting it. Taken together, our view is that the displayed coefficients do not provide evidence for the view that the minimum wage has lead to higher unemployment growth.

8 Results on Robustness

We test different dimensions of aggregation in our robustness checks. For example, we estimate table (2) also for districts instead of labour market regions. Including the size of the coefficients the results are very similar (see table 6). Another level of aggregation are industry-State (Bundeslaender or NUTS 1 regions) data for which we have a longer time series (until July 2015) but which is less detailed in the sense that we do only have the time series of regular employment (without further possible distinctions) at our disposal. The results are surprisingly similar: the interaction term of equation (1) estimated for industry-State cells rather than for labour market region-age group-sex cells and for the period from June 2013 up to July 2015 is 0,056***, i.e. slightly larger than the 0,045 from table (2). From this, we conclude that further accounting for spatial dependence seems unnecessary as the aggregation that takes the strongest spatial correlations out yield similar results to the not aggregated version. In addition, wo conclude that at least the effect on total employment seems to carry over from the prime-age age groups to the average of all age groups.

Table 6:
Bite of the minimum wage and employment, for districts

	(1) Total employment	(2) Employment subject to social insurance contributions	(3) Marginal employment
	Coefficient January to April 2015		
Bite*2015	-0.003	0.042***	-0.187***
	(0,005)	(0.005)	(0,024)
Observations	76380	76380	76380
Cluster	4020	4020	4020

Notes: Labour market regions, 12/2012–04/2015, standard errors are robust.
Source: BA employment statistic and BA earnings statistics, analysis sample

We also estimate the relationship that we are interested in with monthly data in a nonpanel setting (i.e. a cross section regression for each month), to get a better understanding of the time structure of the relationship between the bite and the em-

ployment changes. Table (7) demonstrates these. The estimated equation is similar to equation (1) without time index, estimated separately for each month and using all possible dummies but no fixed effects and including a cell trend growth (change of employment between 2006 and 2013) as explanatory variable. The results show that for employment variables the relationship seems to grow a bit over time and the coefficients for regular employment are somewhat larger than in the panel specification, while the results for marginal employment are somewhat smaller (not necessarily in a statistical sense). In these specifications, there is positive significant coefficient for total employment as well. Results for the longer time series for regular employment only, on industry-State cells, shows that the relationship is insignificant after May 2015 (size of about 0.08 from January to April 2015, 0.04 in May and 0.02 in June), pointing to anticipation effects in 2014.

Table 7:
Cross section regressions for each month of the observation period

	January 15	February 15	March 15	April 15
Reg. Empl.	0.116***	0.128***	0.139***	0.133***
Marg. Empl.	-0.083*	-0.123***	-0.190***	-0.176***
Total empl.	0.049***	0.054***	0.055***	0.047***
N	1,410	1,410	1,410	1,410

Notes: Labour market regions, standard errors are robust.
Source: BA employment statistic and BA earnings statistics, analysis sample.

Back in the panel, we also run Placebo-type regressions. That is, we delete the information for 2015 from the sample, and run the regression by arbitrarily assigning the treatment to October 2014. We expect the interaction term to be close to zero for this treatment assignment because there is no treatment at the time. The results can be found in table (8). They show that, while the bite measure seems to perform good for regular employment, the results for marginal employment and total employment are somewhat more dubious. Clearly, this makes sense because that's what the bite is supposed to measure. Still, for marginal employment the coefficient is considerably larger after 2014, pointing to the fact that the bite measure also measures something that is related to the minimum wage introduction.

Table 8:
Placebo panel regression

	(1) Total employment	(2) Employment subject to social insurance contributions	(3) Marginal employment
	Coefficient October to December 2014		
Bite*10-12/2014	-0.012**	0.005	-0.129***
	(0.006)	(0.006)	(0.029)
Observations	21,150	21,150	21,150
Cluster	1,410	1,410	1,410

Notes: Labour market regions, 12/2012–12/2014, standard errors are robust.
Source: BA employment statistic and BA earnings statistics, analysis sample.

As described above, we use time series for regular employment differentiated by working time. Regular employment is split into three categories: full-time, part-time and unknown. Unfortunately this time series has a structural break: In September 2014 the number with unknown working time drops from about 350,000 to about 80,000 because of changes in the reporting process. Unfortunately, those with unknown working time predominantly work in establishments that pay low wages so that the structural break and the bite of the minimum wage are related. If we do not correct for this structural break, a part of the structural break is in the estimates for the minimum wage coefficient that grow large for both full-time and part-time.[14] To account for the structural break, we add two terms to equation (1): a dummy variable which equals 1 if time is after August 2014 and a term for the interaction between the bite and this dummy. The estimates for full-time and part time yield 0.015** and 0.132*** respectively. This can be easily interpreted as a transformation of marginal employment (Minijobs) to regular employment, where this transformation will take place mostly in part-time. It is reassuring that we receive positive and significant result also for full-time employees, because this is where our bite measure is supposed to be the best fit to the left-hand side variable and it is also reassuring that the coefficient is relatively small for full-time employees.

In a further robustness check, we cast our panel regression in a dynamic framework. This makes sense with monthly growth rates because they largely overlap. For the Difference GMM and for the System GMM estimators, we get consistently positive significant results for regular employment (magnitude of the short term relationship 0.01 to 0.02), negative significant results for marginal employment (magnitude -0.03 to -0.06) and a positive, but mostly insignificant coefficient for total employment. Not all specification tests for the dynamic panel setting have been successful. We often

[14] The coefficients for equation (1) are 0.081*** for full-time employment and 0.598*** for part-time employment.

fail the overidentifying restrictions tests. Thus, these results must be read with caution and we do only present them as robustness checks and not as main results. [15]

We also run robustness checks for the results on unemployment. If we estimate equation (1) for districts rather than labour market regions, the coefficient is again not significant (0.014). When we omit sex and calculate coefficients on the aggregate level the picture changes for districts: While for labour market regions the coefficient is larger but still insignificant (0.088), for districts the coefficient grows to 0.163 and is significant on the 1 percent level. This difference could mirror spatial dependence.

Further, we perform Placebo tests similar to the one above for the four specifications from above.[16] In three of four cases, the results are significant and positive. This implies that there is either anticipation or that the bite measure is not fit to measure the relationship between minimum wage introduction and unemployment growth.[17]

In the data section the possibility of measurement error in the bite variable was discussed. When regressing the growth of full-time employment on the bite, measurement error stems from two sources. First, we measure the bite end of 2013. Second, we do not have valid information on hours worked. If the change in the bite between 2013 and 2014 (the measurement error) and the imprecision in the bite caused by the missing hours information are unrelated to (un-)employment growth, the measurement error inflates the variance of the independent variable and the estimator is biased towards zero. Otherwise the direction of the effect is unknown. When we use the measure for regular employment instead of full-time employment, the measurement error gets larger because we measure the bite only for a subgroup, albeit a large subgroup, of the left-hand side variable. When the measurement is unrelated, this "only" biases the coefficient towards zero. For marginal employment, clearly the bite does not measure anything that is directly relevant for the left hand side of the equation. However, we think that the bites over cells are strongly related for the subgroups considered: regions where the full-time employees are heavily affected by the minimum wage are expected to be regions where part-time and marginal employment are heavily affected by the minimum wage. If this is the case, and if the measurement error is uncorrelated, we can still interpret the results. A similar rea-

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15 We also experiment with non-linear relationships between the bite and employment growth, i.e. we do add a squared bite interaction in the fixed effects model. In fact, the squared terms are significant and negative for regular employment and marginal employment, whereas the linear term is positive and significant in the former case and insignificant in the latter. For regular employment the total effect of the bite in 2015 is positive for bites smaller than 42.4 percent and negative for larger values. For total employment both terms are insignificant.

16 I.e. we delete observations after 2015 from the data and assign the Placebo treatment to either June or October 2014.

17 For the dynamic panel setup, we cannot conclude consistently as the results depend heavily on the specifications chosen and we can get positive, negative and insignificant results.

soning applies to total employment, being the sum of regular and marginal employment. For unemployment, the ideal bite measure would probably be the one for total employment and thus the same reasoning also applies here.

9 Conclusion

In this paper, we provide an attempt to analyze side effects of the minimum wage on employment and unemployment based on administrative data. For regular employment, and particularly for full-time employment, we provide a credible measure of the bite and the results uniformly confirm a positive relationship between the bite of the minimum wage and growth of regular employment for cells that are defined by region, age, and sex for the age groups cosidered. This implies that cells that were heavily affected by the minimum wage introduction haven been growing faster after the minimum wage introduction. The size of this growth is of a magnitude of 78,000 additional regular employees. This is probably related to a decrease in marginal employment, itself related to the bite measure. We provide evidence that the decrease in marginal employment is strongly and negatively related to this same bite measure. Here, the employment loss is 66,000 Minijobs. We interpret the shift from decreasing marginal employment to increasing regular employment as the result of a development which favours regular employment at the expense of marginal employment, caused for example by the minimum wage. This can be – but must not – by a direct transformation of Minijobs to regular employment. It is also possible that there is a labour supply effect that is related to the bite measure.[18] A third and less optimistic possibility is that the growing regular employment is due to fact that black market work has been turned into regular employment. This might be due to the fact that with the introduction of the minimum wage, controls of working time etc. have been intensified.

If we consider the sum of regular and marginal employment (total employment), we fail to detect a significant relationship between the bite measure and employment growth (magnitude: -6,000). The same holds true for unemployment growth (magnitude: +2,000). We cannot conclude that there is no effect on total employment (for the age groups considered) or on total unemployment, but it fails to show up in our setting, which gives a first sign that it might be indeed absent so far.

That we do not find a significant relationship between minimum wage bite and total employment, whereas Bossler/Gerner (2016) do, might stem from the fact that they use survey data (the IAB-Establishment panel) while we use administrative data. It is likely that low wage establishments do answer less frequently to surveys than others. Therefore the minimum wage bite is considerably lower in the establishment panel than in in the earnings statistics or other data (e.g. the German socioeconomic

[18] Interestingly, recently Gürtzgen et al. (2016), analysing the difficulties to fill vacancies, showed that the difficulties to fill vacancies at the minimum wage level increased with the introduction of the minimum wage. According to the authors a reason for this is that the standards for new employment have increased.

panel). It might be the case that the IAB-Establishment panel is not representative with respect to changes in employment in low-wage establishments.

An open question is that even if our preliminary conclusion of the absence of (un-) employment effects is correct, negative side effects of the minimum wage could show up later, for example in the next recession. Also, it might be the case that there are some groups in the labour market that are negatively affected (e.g. the low-skilled) but that we fail to detect this effect since it is offset by labour supply effects for other groups (e.g. migrants).

The transformation of marginal employment to regular employment has clearly a positive effect on the welfare of employees and the social security system. So, one might argue, when the minimum wage has destroyed a few Minijobs but transformed most of them to regular employment, this is desirable. A good question, then, is how higher wages and the social security plus are financed: Are profit margins of the affected firms getting smaller, or, are they able to pass along higher wages to higher prices? There is some evidence of price increases in affected sectors (see Sachverständigenrat 2015; Amlinger/Bispinck/Schulten 2016).

Another open question is how minimum wages should develop or how further developments will change these outcomes. In the middle of 2016 the German Minimum Wage Commission will present its first report on the development after the introduction of the minimum wage and – most likely – suggest an increase of the minimum wage. By law, the commission is supposed to consider the employment development and the increase shall also be oriented at the development of average wages in the period considered. Clearly, the refugee wave to Germany will be affecting the labour market development especially for the low-skilled. Simulations imply that up to 1.5 Million low-skilled refugees may enter the labour market until the year 2020. This is a large number when compared to the about 3.6 Million low-skilled employees in Germany at the moment. It is not unrealistic that the wages of the low-skilled will be considerably affected and that the minimum wage could lead to increasing unemployment of this group. The decision to base the minimum wage development on these considerations might depend on the question, whether the immigrants strongly substitute natives or whether they will work in different occupations and industries. If they are substitutes, this could clearly affect the wage for all low-skilled and this would mean that exemptions from the minimum wage for refugees (as suggested for example by Sachverständigenrat 2015) would be counter-productive as these would give the refugees an advantage over natives in a race about jobs. However, if there is no strong competition between natives and migrants, an exemption could be considered.

Methodologically, the use of other measures for the bite of the minimum wage has to be considered, for example from the IAB-Establishment panel and the new earn-

ings statistics once available. Other measures for the cell-specific effect of the minimum wage (instead of the bite) could be considered (e.g. the Kaitz-Index).[19] Further robustness checks for all possible disaggregations would strengthen the arguments presented as regards the transformation of marginal to regular employment in the aftermath of the introduction of the minimum wage. To check whether our bite measure can be used not only for individuals employed full-time but also for other employees we want to check if the share of low-wage employees is strongly related over all working time categories with data from previous waves of the so-called Verdienststrukturerhebung. Further, we want to check whether there is time varying information from the INKAR-database that we can use as additional covariates for our main equation.[20]

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[19] There is also a bite measure for West German men in marginal employment that could be considered.

[20] INKAR is a database with regional content provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

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Institute for Employment Research
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Technical completion
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For further inquiries contact the authors:

Alfred Garloff
Phone +49.911.179 3035
E-mail alfred.garloff@bmwi.bund.de