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The Effects of Germany's New Minimum Wage on Employment and Welfare Dependency

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Abstract

In January 2015, Germany introduced a federal, statutory minimum wage of 8.50€ per hour. This study evaluates the effects of this policy on regular and marginal employment and on welfare dependency. Based on county-level administrative data, this study uses the difference-in-differences technique, exploiting regional variation in the *bite* of the minimum wage, i.e. the county-specific share of employees paid less than 8.50€ before the introduction of the minimum wage. The minimum wage had a considerable negative effect on marginal employment. There is also some indication that regular employment was slightly reduced. Concerning welfare dependency, the minimum wage reduced the number of working welfare recipients, with some indication that about one half of them left welfare receipt due to the minimum wage.

Keywords Minimum Wages · Welfare Dependency · Labor Supply · Germany

JEL Classification I38 · J22 · J30

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1 Motivation

On the 1st of January 2015, Germany introduced a federal, statutory minimum wage of 8.50€ per hour. There were some fears about negative employment effects of this new policy. However, proponents of the minimum wage, such as the German Social Democrats (SPD, 2013), argued that apprehensions regarding job losses were ungrounded. In fact, a minimum wage would be necessary to supplement earnings in the growing low-wage sector and to cushion the large-scale labor market reforms of the early 2000s, the so-called Hartz reforms. A key target group of the minimum wage are households that receive supplementary welfare benefits (unemployment benefit II, *UBII*) while working, the so-called *Aufstocker*. The proponents of the minimum wage argued that increasing the labor earnings via the minimum wage would reduce welfare spending and help households to end their welfare dependency.

At first glance, the labor market outcomes since the reform seem rather comforting. The Federal Ministry of Labor and Social Affairs (BMAS, 2015) reports that after the introduction of the minimum wage unemployment actually fell and that regular employment is at an all time high, although there was some loss of marginal employment.[1] The ministry estimates that 3.7 million employees profited from higher labor earnings. The number of the *Aufstocker* dropped by 50,000 and related welfare expenditures on UBII were expected to decrease by 0.9bn€. The ministry concludes that the "minimum wage works" (BMAS, 2015).

Without a doubt, Germany introduced the minimum wage in times of a healthy labor market and solid economic circumstances. However, it is impossible to determine the *causal* effects of the minimum wage just based on the aggregate employment and social security statistics. Accordingly, this study evaluates the effects of the statutory minimum wage on employment and welfare dependency using county-level administrative data. In order to construct a credible counter-factual, i.e., the development of the labor market under the absence of the minimum wage, identification is based on variations in the relative depth of the intervention on regional level.

The analysis for employment effects will focus on the impact on both, regular and marginal employment, where the latter refers to so-called *Minijobs*, which are fully social-security exempt (up to 450€ per month). Regarding welfare dependency, there will be a distinction between those who are capable of working, but do not work while receiving UBII payments and those who are working, but top-up their labor earnings with UBII (*Aufstocker*). Also the composition of the *Aufstocker* will be considered: I separate the analysis according to labor income brackets[2] and employment status (self-employed or

[1] Regular employment (sozialversicherungspflichtige Beschäftigung) refers to jobs subject to social security contributions, i.e. with an average monthly income of more than 450€. Jobs below or just paying 450€ are exempt from these contributions and constitute marginal employment (geringfügige Beschäftigung.).

[2] There are three different categories, based on the rules regarding social security contributions: First, fully social-security exempt (*Minijobs*, up to 450€ p.m); second, the phase-in zone for social security

dependently employed). Furthermore I will evaluate the effects of the minimum wage on the regional wage distribution.

The results suggest that the minimum wage had a considerable negative effect on marginal employment. A back-off-the-envelope calculation indicates that in 2015 150,000-200,000 marginal jobs have been lost, due to the minimum wage. Concerning regular employment, the results indicate a rather small (short-run) negative effect of the minimum wage. Concerning welfare dependency, the minimum wage reduced the number of working welfare recipients, with some indication that about one half of them left welfare receipt due to the minimum wage. The effect on welfare reduction in absolute terms is rather small.

The remainder of this paper is structured as follows: Section 2 introduces the institutional background and briefly summarizes the previous studies on employment and welfare effects. Section 3 describes the identification strategy, the different outcome variables, and presents the data. Section 4 discusses the results. Section 5 concludes.

2 Minimum Wages in Germany

Background and Institutional Factors Germany introduced a statutory, federal minimum wage as a response to a large variety of economic and political trends. Traditionally, the majority of wages in West Germany have been determined by collective wage bargaining. Hence, trade unions and employers associations alike opposed minimum wages as interference with their autonomous wage- setting. However, with declining coverage of collective agreements starting in the 1990s, and increasing dispersion of gross labor income and equivalized net income[3], trade unions began to favor broadly applicable, legal wage floors.

Germany started to introduce sectoral minimum wages from 1997 onwards, using the legislation on the posting of workers (*Arbeitnehmer-Entsendegesetz*). The majority of the covered branches are in the crafts and construction sector and were introduced in order to shield domestic firms against foreign competitors who are also subject to the minimum wages if they operate in Germany.

Minimum wages became also politically more desirable as a means to supplement and cushion the large-scale labor market reforms of the early 2000s, the so-called Hartz reforms.[4] The reforms attempted to lower reservation wages and introduced new forms of marginal employment, exempt from social security contributions (so-called *Minijobs*).

contributions (*Midijob*, between 450€ and 850€ p.m.) and third, regular employment (more than 850€ p.m., fully subject to payroll taxes)

[3] See among others Gernandt and Pfeiffer (2007); Antonczyk et al. (2010, 2011); Biewen and Juhasz (2012); Card et al. (2013). This upward trend in inequality was most pronounced from German unification in the early 1990s till the mid 2000s. Income inequality stabilized in more recent years (c.f. Grabka and Goebel, 2014; Möller, 2016).

[4] For an overview, see Ochel (2005).

It is often argued that the reform package stimulated the expansion of the low-wage sector.[5] The Hartz reforms also encouraged the use of welfare payments as an implicit combination wage. Households with low labor earnings can supplement their income with the *unemployment benefit II* (UBII) to reach subsistence level. People making use of this provision are commonly referred to as *Aufstocker*, literally "those who top-up". Some researchers argue that these reforms are (at least partially) responsible for the success of the German labor market and the German economy in the last decade (Carlin and Soskice, 2009; Boysen-Hogrefe and Groll, 2010; Gartner and Klinger, 2010; Burda and Hunt, 2011). Yet, there is disagreement of its relative importance compared to other factors, such as wage moderation (Akyol et al., 2013; Dustmann et al., 2014).

Proponents of the minimum wage, such as Rürup and Heilmann (2012) or the Social Democrats (SPD, 2013) argue that the introduction of a general minimum wage is necessary to counter the negative effects of the new low-pay sector and will increase the efficiency of the combination wage scheme, since the *Aufstocker* will obtain a larger share of their income from work and not from transfers. Assuming the absence of detrimental employment effects,[6] a modestly set minimum wage would be beneficial for the public budget, due to the reduction in supplementary welfare payments for the *Aufstocker* and increases in payroll and income taxes. The Social Democrats, the driving force in introducing the minimum wage, argued that the minimum wage would generate a fiscal surplus of 7bn€ per year (SPD, 2013, pg. 69).[7]

After the federal election in Autumn 2013 and the change of government, the political climate shifted in favor of a statutory minimum wage.[8] In January 2015 a minimum wage of 8.50€ per hour was intoduced. The minimum wage passed into law in the summer of 2014[9], only few exemptions apply.[10] Sectoral minimum wages remain unaffected.

[5]The low-wage sector is usually defined as wages below 2/3 of the median wage. The share of jobs considered to be in the low-wage sector increased between the 1997 and 2007 from ca 16 to 22% and remained constant afterwards. However, most of that increase took place already *before* the Hartz reforms (Schäfer and Schmidt, 2012; Brenke, 2012).

[6]Standard economic theory about minimum wages predicts unambiguous negative effects and involuntary unemployment. However, as for instance argued in Manning (2003); Garloff (2010), a minimum wage does not necessarily need to reduce employment because of some monopsony power of the employers, for instance due to search frictions (Card and Krueger, 2015).

[7]This claim is based on Ehrentraut et al. (2011) who calculate the fiscal effects of a minimum wage, assuming the absence of negative emplyoment effects.

[8]Chancellor Merkel (Christian Democrats) remained in office, but the Social Democrats replaced the Liberals as the coalition partner.

[9]The German *Bundestag* voted for the minimum wage on 3 July 2014, the second chamber *Bundesrat* confirmed the law on 11 July 2014. The law became effective on 16 August 2014.

[10]Apprentices, compulsory internships, long-term unemployed for the first six month.

The following literature review will present the evidence on the economic effects of minimum wages in Germany.[11] I will focus on the empirical insights from ex-ante and ex-post studies concerning employment effects and welfare dependency.[12]

Employment Effects There are various ex-ante studies on the effects of a statutory minimum wage.[13] These simulations generally point to rather substantial employment losses, but the variation of potential effects is large (for a comparisson, see Müller, 2009). Concerning the sectoral minimum wages, there is also a large body of ex-post studies.[14] Not surprisingly, these studies indicate that the bite, i.e. the share of directly affected workers matters a lot for the effects of minimum wages. In West Germany, the sectoral minimum wages were usually comparatively low. Hence, employment effects have been very small or not statistically significant. Moreover, spillover effects might be an issues, i.e. minimum wages can affect the wage distribution above the level of the wage floor.

There are also several studies that analyze descriptively or with ex-post evaluations the employment effects of minimum wage. Based on aggregate employment statistics Groll (2015) shows descriptively that there is a striking reduction of marginal employment in the beginning of 2015. Vom Berge et al. (2016) study data on individual transitions and show that there is no large flow into unemployment, but indeed transitions from marginal to regular employment. Based on the IAB Establishment Panel, Bossler (2016) and Bossler and Gerner (2016) exploit the self-declared affectedness of establishments by the minimum wage and detect a small negative effect on employers' employment expectations (before the reform) and estimate a reduction in employment growth of 60,000 jobs due to the minimum wage. Garloff (2016) uses regional data of the Federal Employment Agency and does not find any evidence for a decrease of employment growth or an increase in unemployment growth. His results suggest that there was a transformation from marginal into regular employment. Knabe et al. (2016) argue that a simple East/West comparison can already detect effects of the minimum wages, since East Germany is much more exposed to the minimum wage than most of the West. They consider aggregate employment statistics and argue that the minimum wage had an effect on labor market dynamics. In 2015, overall employment grew by 300,000 jobs; however, only by 0.2% in the East compared tp 0.9% in the West. Additionally, they conclude that even if there was some transformation of marginal into regular employment, not all marginal jobs have been upgraded.

[11] For a general overview about the empirics of minimum wages see Brown (1999); Neumark and Wascher (2008), for a European focus Dolado et al. (1996).

[12] For a discussion about the theoretical arguments concerning minimum wages in the German context see Fitzenberger (2009).

[13] Among others Bachmann et al. (2008); Bauer et al. (2009); Knabe and Schöb (2009); Müller and Steiner (2011); Knabe et al. (2014); Arni et al. (2014); Henzel and Engelhardt (2014).

[14] Among others Möller and König (2008); Müller (2010); Boockmann et al. (2013); Frings (2013); Aretz et al. (2012, 2013); Gregory (2014); Rattenhuber (2014).

Effects on Welfare Dependency Proponents of the minimum wage argue that it could lift poor households, such as the *Aufstocker* out of welfare. Some of the ex-ante studies (e.g. Bachmann et al., 2008; Müller and Steiner, 2009; Bauer et al., 2009; Knabe and Schöb, 2009; Knabe et al., 2014; Arni et al., 2014) do not only analyze the employment, but also the fiscal effects of a minimum wage. These studies commonly find that the effects on the *Aufstocker* and welfare dependency are very small or negligible and usually offset by negative employment effects, which are especially severe for this group. The vast majority of the *Aufstocker* is found to remain in welfare receipt, either because of the household context (single parents, many children), hours constraints (disabilities, child care), or both of it. Additionally, Müller and Steiner (2009) estimate that only 25% of the gross income increase due to a minimum wage sticks with households and further argue that minimum wages are not well-targeted for poverty reduction, since they also affect secondary earners in households above the poverty line. In a more recent account, Bruckmeier and Wiemers (2014) argue along similar lines. However, even if the disposable income of the households would not change much due to high transfer withdrawal rates, reduced welfare stigma could improve well-being considerably (c.f. Hetschko et al., 2016).

Bruckmeier and Wiemers (2016) describe the developments for the *Aufstocker* after the introduction of the minimum wage and report that their numbers decreased from December 2014 to January 2015 by 2% (-23,000). This reductions is larger than at previous turns of the years, mostly driven by former marginally employed *Aufstocker*. They provide evidence that in the following months more *Aufstocker* than before managed to leave welfare dependency.

Summing Up So far, no evidence for substantial employment losses due to the new minimum wage exists. There is a loss of marginal employment that seems to be (partially) offset by transformations into regular employment; however, there might be some reductions in employment dynamics. Since the introduction of the minimum wage there was a reduction in the number of the *Aufstocker*, especially those with a marginal employment. There is tentative evidence that more *Aufstocker* than before left welfare receipt due to higher labor earnings.

3 Method

3.1 Identification Strategy

In order to identify the *causal* effects of the minimum wage on employment and welfare dependency, this study will exploit regional differences (county level, N=402) in the *bite* of the minimum wage as the source of exogenous variation. I will define the bite of the minimum wage as the county-specific share of workers with wages less than 8.50€ per

hour before the introduction of the minimum wage.[15] Unlike in the United States, where states can set their own wage floor above the federal minimum wage, in Germany this kind of regional variation does not exist. However, a uniformly set minimum wage of 8.50€ per hour has rather different repercussions across the country. In prosperous economic regions, such as Munich or Frankfurt, the vast majority of workers already receives a wage rate well above 8.50€. On the other hand, 8.50€ is a relatively high wage rate in most parts of East Germany and also in rural, economically struggling regions in the West. In that sense, even though the minimum wage is nominally the same in all regions, the effective strength of the treatment differs considerably. Card (1992) uses this type of variation in order to study the effects of minimum wages on teenage employment, Garloff (2016) uses the same data set as this study, but considers employment outcomes only.

The estimation will make use of observations before and after the policy change, hence a difference-in-differences (DID) estimator is appropriate. The effect of the minimum wage is recovered as the difference between strongly and only mildly "treated" regions before and after the turn of the year 2014/15. Instead of a binary treatment, the regional bite of the minimum wage functions as an indicator for the strength of the treatment. Given that we have a monthly panel running from January 2012 to December 2015 (T=48) of the 402 counties, the difference-in-differences estimator in log-levels can be implemented as

$$\log(y_{it}) = bite_i \cdot D_t^{\text{MW}} \cdot \beta_L + \sum_t D_t^{\text{month}} \cdot \gamma_t + \theta_i + \varepsilon_{it} \tag{1}$$

where y_{it} is one of the outcomes of interest[16], measured in period t in county i, the $bite_i$ is the county-specific (but not time-varying) depth of the intervention, interacted with an indicator variable D_t^{MW} which is equal to one for all periods after the introduction of the minimum wage, i.e. the entire year 2015. Furthermore, Equation 1 features time fixed effects γ_t and county fixed effects θ_i. Standard errors are clustered on county level as advocated by Bertrand et al. (2004). Thus, β_L is the parameter of interest. In order to ease interpretation of β_L, the bite will be normalized by one standard deviation and divided by 100. Hence, the estimate of β_L from Equation 1 corresponds to the *percentage change* of the outcome variable, due to one additional standard deviation of the county-specific bite.

Alternatively, one could specify a model of growth rates instead of levels, where only the left-hand side of Equation 1 is modified, yielding

$$\triangle_{12}\log(y_{it}) = \log(y_{it}) - \log(y_{i,t-12}) = bite_i \cdot D_t^{\text{MW}} \cdot \beta + \sum_t D_t^{\text{month}} \cdot \gamma_t + \theta_i + \varepsilon_{it} \tag{2}$$

[15] The details will be presented in the following Section 3.2.

[16] Regular and marginal employment, working and non-working recipients of UBII. Information on wages is only available on an annual basis. The specification for the impact on the wage structure will be presented in Section 4.

Note that Equation 1 and 2 are two distinct models and the estimated coefficients of interest, β and β_L have entirely different interpretations. For the specification in growth rates (Equation 2), β provides an estimate for the *percentage point change* of the annual month-specific growth rate of the outcome variable, due to one additional standard deviation of the county-specific bite.

Since the difference-in-differences identification is scale dependent, at best only one of the two specifications is valid. I will argue graphically in Section 3.4 that the developments of the outcome variables follow a process modeled more appropriately by growth rates as in Equation 2 than in log-levels, as specified in Equation 1. The advantage of the specification in growth rates is that stochastic patterns of seasonality are accounted for by having the month t-specific seasonally adjusted annual growth rate of y_{it} on the left-hand side of the equation.

As in any difference-in-differences estimator, identification rests on the validity of the comparison between the treatment and control groups. When the treatment is continuous, all regions are affected by the policy change, but the intensity of the treatment differs. Regions with a high bite were affected more than those with a low bite. The standard DID framework with an unambiguously defined binary treatment requires that the comparison group is unaffected by the treatment. When the treatment is continuous, the requirement is that regions with a lower bite are *proportionally* less affected by the policy. Given this setting, identification rests on two canonical difference-in-differences assumptions, the common trend and the stable unit treatment value assumption.

The common trend assumption (CTA) states that in the absence of the policy, the development of the outcomes of interest should have been *parallel* in regions which are highly affected and regions which are only mildly affected by the new policy. For the specification in log-levels, this implies that percentage changes of the outcome over time should be unrelated to bite if there would be no minimum wage. For the alternative specification from Equation 2, the CTA implies that changes in growth rates would not be systematically related to the bite, if there was no minimum wage.

It is conceivable that regions with a high bite exhibit different trend (or growth rate) behavior for the outcomes of interest than those with a low bite. This issue will be addressed with an alternative specification, in which $bite_i \cdot t \cdot \delta$ is introduced as an additional regressor. The term t is a linear time trend and δ a bite-specific linear trend differential. If the differences in the pre-treatment trend behavior can be adequately captured with a deterministic linear trend proportional to the bite, β will provide a valid estimator for the ATT. The causal effects are then recovered as deviations from a pre-treatment trend differential due to the larger impact of the minimum wage. Section 3.4 compares the trend behavior of the treated and the control groups graphically, in order to decide whether this alternative specification is appropriate or not.

The stable unit treatment value assumption (SUTVA) stipulates that more or less treated observations do not interfere, so that for example a stronger treatment in one region does not affect the outcome in another region. It is very likely that the SUTVA does

not hold in the medium to long run, for instance through firms investment decisions. A firm with two factories, one in a high wage and one in a low wage region might shift its investments to the high wage region, due to the change in relative wages between the two regions. However, these investments and relocation decisions take time and I assume that they are negligible in the narrow time frame of this study. Also labor mobility and migration could invalidate the SUTVA; however, as for the decisions of the firms, I assume that the effects of the minimum wage on labor mobility and migration are negligible in the first year of the new policy.

Having multiple time periods does not only allow to argue graphically for the validity of the CTA, but also to study how the effect evolves over time. This is done by specifying multiple pre- and post-treatment periods, as for instance in Autor (2003). One obtains a more flexible specification for the annual growth rate in which D_t^{MW} is replaced by a series of indicators:

$$\triangle_{12} \log(y_{it}) = bite_i \cdot \sum_{\tau} D_{\tau}^{\text{month}} \cdot \beta_{\tau} + \sum_{t} D_t^{\text{month}} \cdot \gamma_t + \theta_i + \varepsilon_{it} \tag{3}$$

where τ is an indicator for the periods of interest. Of course, also the (log-)levels specification from Equation 1 could be respecified in the fashion of Equation 3. In the analysis, τ will take seven post-treatment values for lagged adjustment[17] and six pre-treatment values (July - December 2014) for anticipation effects after the minimum wage law was passed. Also Equation 3 can be supplemented with $bite_i \cdot t \cdot \delta$, if the alternative CTA (deviations from trend differential) is more appropriate.

Figure 1 provides evidence that shortly already after the passing of the minimum wage bill, some anticipation effects arose. The figure plots the weekly relative search intensity for the Google search query *"Mindestlohn"* originating from Germany for the years 2013 to 2015 (Google Trends, 2017). The search intensity index is set to 100 for the week with the highest number of queries relative to all search queries. The graph shows a first large spike in the first week of July 2014, when the minimum wage was voted for (light blue vertical line) and a second large spike at the turn of the year when the minimum wage took effect (dark blue vertical line) Hence, it is likely that the majority of people was already well-informed about the new minimum wage, half a year before the official start and that anticipation effects are possible.

[17] Six month adjustment after introduction (January 2015, February 2015, ..., June 2015) and a joint medium-run effect, i.e. the time between July and December 2015.

Note: Measured between January 2013 and December 2015, Own Graph, Data: Google Trends (2017)

Figure 1: Google Trends for Search Query *"Mindestlohn"* over Time

3.2 Measurement of the Bite and Treatments

The bite is calculated based on the wage statistics of the Federal Employment Agency, an administrative dataset, aggregated on county level, containing the distribution of gross labor earnings. The statistic is based on social security notifications and refers to regular, full-time employment (Statistik der Bundesagentur für Arbeit, 2016c). As in Garloff (2016), all earnings up to 1400€ per month are supposed to be subject to the minimum wage.[18] The bite is calculated as the county specific share of these earnings in relation to all recorded full-time employees. The wage statistics are available only once per year, namely in December. In order to avoid anticipation effects, the bite is calculated based on data from December 2013, hence one year before the introduction of the new minimum wage.

This measure of the bite has some important caveats. First, there is no information on hours worked and the calculation is only based on the social security records of full-time employees. If this lack of information results in a classic symmetric measurement error, the estimated coefficients and the ATT $\hat{\beta}$ would suffer from a downward attenuation bias. The problem would be exacerbated if there are systematic differences across counties,

[18] Assuming 4.35 workweeks per month and reasonable 38 hours work week implies a gross hourly wage rate of 8.47€. As a robustness check, the main results are also replicated with bite measures based on income thresholds of 1500 and 2000€ per months. The bite measures are highly correlated, hence the results remain largely unaffected.

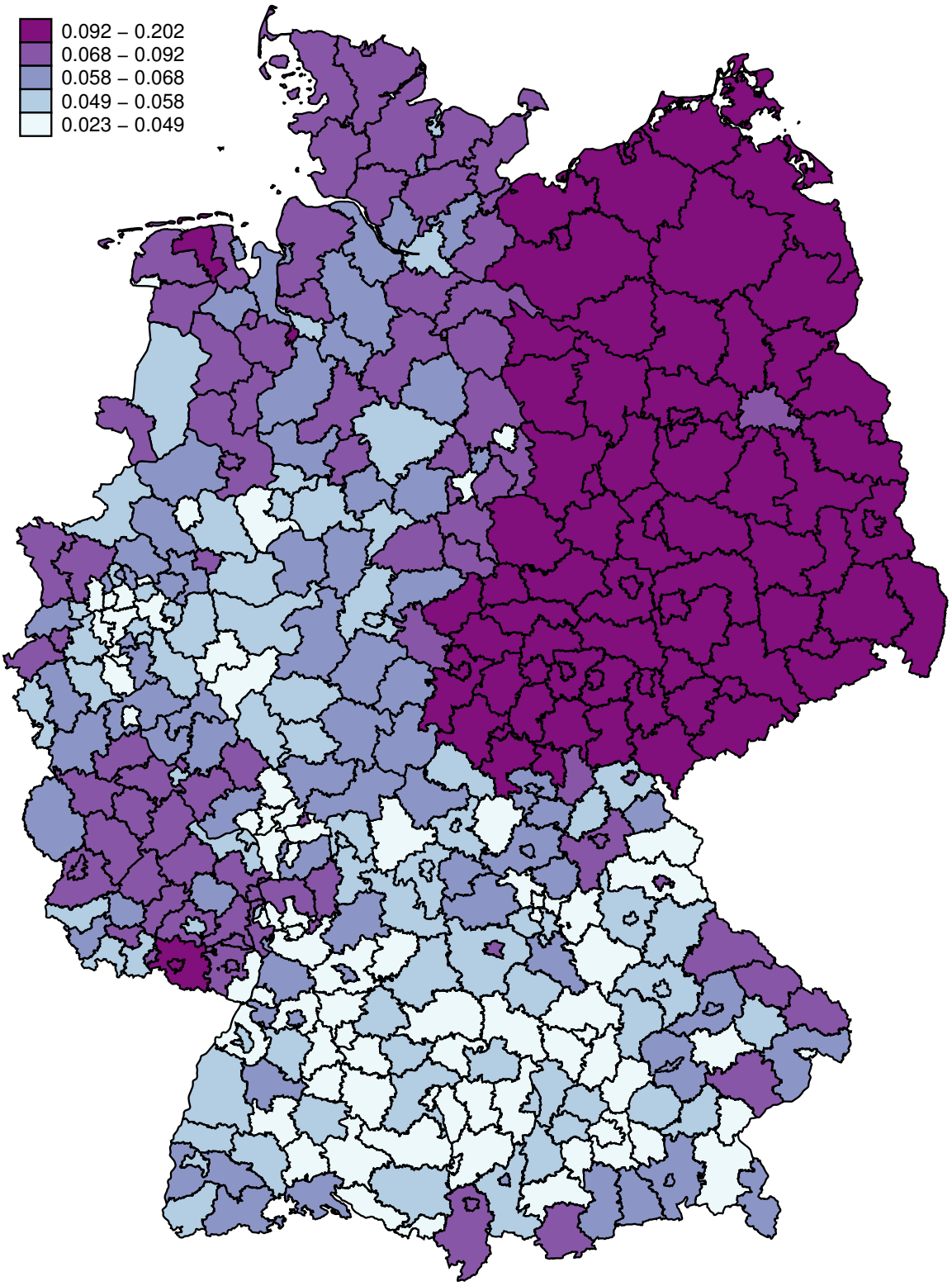
related to the size of the bite, for instance due to differences in the prevalence of part-time employment. Second, recipients of minimum wages are frequently working in marginal employment, i.e. jobs not subject to social security contributions and hence not covered in the wage statistics and in the calculation of the bite. This feature might actually be desirable for the effects on regular employment, but could potentially be misleading for marginal employment and for *Aufstocker* and welfare dependency. Given that there are neither alternative wage statistics, nor credible instrumental variables available, I will abstract from these issues and

Figure 2 maps the distribution of the bite across counties. It shows five different quintiles of the bite in ascending darkness (from light blue to dark purple). The bite ranges from 2.3% to more than 20%; so variation across regions is substantial. The most striking pattern is that the entire former GDR - except for Berlin - is in the highest quintile. This observation matches expectations and echoes the simple East West comparison used by Knabe et al. (2016), however, as displayed in Figure A1 there is also considerable variation within East German regions. The variation within West Germany confirms intuitions: The prosperous regions in Bavaria and Baden-Württemberg in the South of Germany and other metropolitan regions (e.g. Frankfurt Rhine-Main are, Düsseldorf Rhine-Ruhr, Hamburg) are less affected than more rural and less prosperous areas such as the south of Rhineland-Palatinate or East Frisia.

Accordingly, I generate two alternative binary treatments. First, by splitting the counties into those with a bite above and those with a bite below the median bite. This alternative treatment assumes that only the counties with a large bite -above the median- are treated, while the other half is not affected. The resulting map is shown in the Appendix (Figure A2 - left panel). The second alternative binary treatment ignores counties close to the median on either side, since they are very similar in their exposure to the new policy. In this *robust* binary treatment, counties are *treated* if their bite lies above the 60th percentile of the bite distribution, counties below the 40th percentile belong to the control group, and counties close to the median (above 40th and below 6oth percentile) are excluded. The resulting map is also shown in the Appendix (Figure A2, right panel).

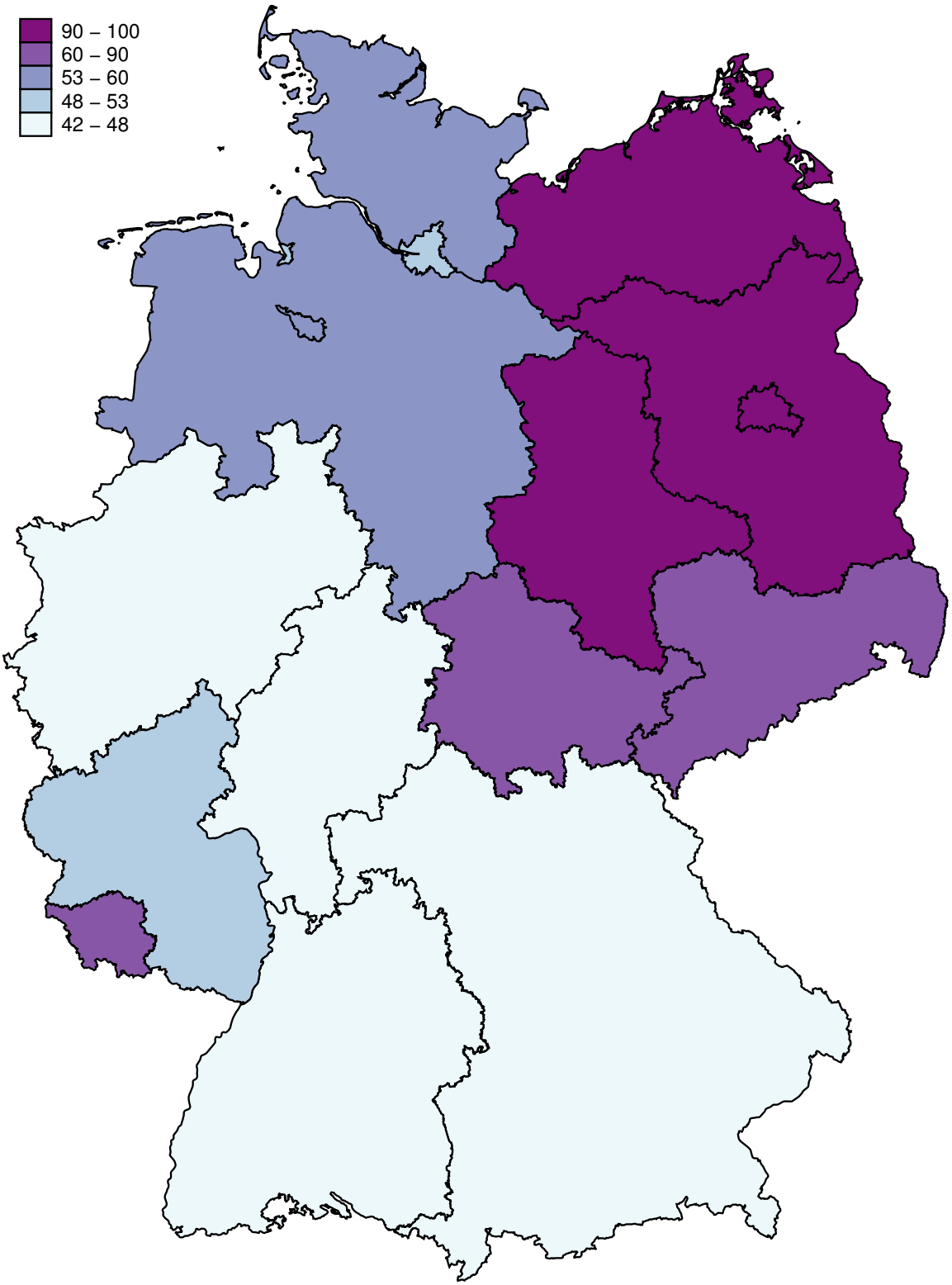
One can exploit the regional variation in Google search queries to validate the bite measure. Figure 3 shows the relative search intensity for the term *"Mindestlohn"* across federal states in the years 2013 to 2015. The search intensity index is set to 100 in the state in which it was most popular relative to all search queries (Mecklenburg-Vorpommern). A value of 50 implies that the term was only half as popular than in the reference region. The lowest value (42) is observed for Baden-Württemberg (Google Trends, 2017). The map also uses quintiles of the search intensity distribution. Although the data is only available at the coarser state level, the resulting map shows that the bite is highly correlated with interest in the new policy. Regions with a higher bite also have a higher relative search intensity for the minimum wage. As argued by Askitas and Zimmermann (2009), Google search queries can be a powerful predictor for the analysis of labor market outcomes.

Hence, the bite measure appears to be a reasonable indicator for the strength of the treatment.



Note: Measured in December 2013

Figure 2: Average Bite of the Minimum Wage across Counties ,



Note: Measured between January 2013 and December 2015, Own Graph, Data: Google Trends (2017)

Figure 3: Relative Search Intensity *"Mindestlohn"* across Federal States

3.3 Data

The impact of the minimum wage will be evaluated using the results from four main outcomes: Concerning the employment effects, regular and marginal employment are considered. Concerning welfare dependency, the stock of recipients of unemployment benefit II, who are deemed to be able to work, will be decomposed into those working (*Aufstocker*) and those not working (*NW UBII*).[19] In order to get an in-depth look at the effects for the *Aufstocker*, I will decompose them based on monthly income (Minijobs, Midijobs and Maxijobs) and employment status (self-employed or dependently employed). The county-specific, monthly time series for all four outcomes are provided by the Federal Employment Agency (Statistik der Bundesagentur für Arbeit, 2016a,b) for the years 2012 to 2015.

Table 1: Totals for December 2013

Main Outcomes		*Composition of Aufstocker*	
Regular Employment	29.88m	Self-employed	118,584
Marginal Employment	7.44m	Employees	1,189,417
Able to Work - UBII	4.31m	Minijobs	639,942
Aufstocker	1.30m	Midijobs	233,757
Non-working UBII	3.01m	Maxijobs	315,718

Sum over all 402 counties

Table 1 displays the aggregate values of the outcomes of interest for December 2013. The left column contains the employment and welfare outcomes, the right column the composition of the *Aufstocker*. The data contains just short of 30 million regular jobs and 7.5 million marginal jobs. Out of the 4.3 million recipients of UBII who are deemed to be able to work, about 30% (1.3m) top-up their labor income with welfare payments. In the public debate, this group was considered a core target group of the minimum wage, even though economic research (Müller and Steiner, 2009; Bruckmeier and Wiemers, 2014) dampened expectations about the effectiveness of minimum wages. Figure A3 in the Appendix illustrates the composition of the *Aufstocker* graphically. Circa 9% of the *Aufstocker* are self-employed, but do not earn enough. Almost 50% of the *Aufstocker* only have a marginal job, paying up to 450€ per month. Table 1 highlights that the vast majority of marginal jobs are not held by people depending on welfare benefits, but by people from households that do not receive welfare payments. Marginal jobs are often held by secondary earners, due to the favorable tax treatment.

[19] There are also recipients of UBII that are not deemed to be able to work, namely the unfit household members of those who are able to work and receive UBII. Thus, the overall number of UBII recipients in Germany is larger.

Table 2 shows the most important variables for December 2013, i.e. the reference period for the bite measure. The average bite from the wage statistics is 6.8% and ranges from 2 to 20%. All outcomes display considerable variations across counties, which is not surprising, given that these administrative units are very heterogeneous. Recall that county fixed effects will be included in the estimation and identification is based on inter-temporal differences. The table does not only feature the outcomes of interest, but also the average labor earnings in 2013 and 2015 from the wage statistics (Statistik der Bundesagentur für Arbeit, 2016c).[20]

Table 2: Summary Statistics for December 2013

	Mean	**Std. Dev.**	**Min.**	**Max.**
Bite	0.068	0.034	0.023	0.202
Regular Employment	211,742	292,078	12,023	1,250,649
Marginal Employment	44,379	50,972	2835	210,496
Aufstocker	11,185	24,996	215	127,939
Non-working UBII	26,183	55,993	550	283,446
Avg Income 2013	3556	628	2220	5082
Avg Income 2015	3723	649	2361	5386
Composition of Aufstocker				
Employees	9868	21,018	205	107,192
Self-employed	1420	4297	8	22,128
Minijobs	4951	9955	105	50,849
Midijobs	2174	5004	40	25,309
Maxijobs	2744	6086	42	31,034

N=402 counties, weighted with county-specific employment.

As outlined in Section 3.1, the DID framework will make use of inter-temporal variation in order to identify the effects of the minimum wage. Figure 4 shows the development of the four outcomes (in logs) over time. The graphs run from January 2012 to December 2015 showing time series normalized by the value of January 2012. Hence, all lines start at zero and are growth rates with respect to January 2012. A light blue vertical line indicates the passing of the minimum wage bill in July 2014, the dark blue vertical line the turn of the year 2014/15, the introduction of the minimum wage. start of the new policy. Note

[20] Recall from Section 3.2 that the wage statistics only feature full-time regular employment. The data is available in 50€ brackets. For each bracket, the mean value is assumed. The data is top-coded. Average income in the highest bracket is imputed, using a Pareto distribution with $\alpha = 2.6$. The imputation for the top income bracket does not affect the bite, given that the bite is defined only as the share of monthly labor income below 1400 over all full-time employees.

that the series are not yet separated by treatment or intensity of treatment, which will be delegated to the following subsection.

In the upper graph, the two employment outcomes are shown. First, the upward trend for both employment outcomes indicates that Germany introduced the minimum wage into a very robust and expanding labor market. Not surprisingly, the series exhibits a stochastic pattern of seasonality. Strikingly, there is a pronounced reduction in marginal employment exactly after the turn of the year 2014/15. This observation matches the descriptive evidence concerning marginal employment reported for instance by Groll (2015). There does not seem to be a comparable movement in the series of regular employment; however, the relatively small seasonal decrease in the winter 2014/15 could indicate that at least some of the marginal jobs have been converted into regular employment. The stock of regular employment is about four times as large as the stock of marginal employment; hence, in a graph displaying growths rates, such a transformation is certainly difficult to spot.

The lower graph shows the development for the working and non-working recipients of unemployment benefit II. As in the upper graph, there are strong seasonal patterns; however, contrarily to the employment outcomes, the patterns appear to be shifted. This observation indicates that people frequently shift from one category to the other. The non-working series seems to be pretty stable in its average level prior to the introduction of the minimum wage. There seems to be a small upward trend in the first months of 2015. Concerning the working UBII recipients (*Aufstocker*), there seems to be a slight downward trend before the introduction of the policy; however, this downward trend is amplified after the passing of the minimum wage bill in July 2014 with a very pronounced drop in the first months of 2015.

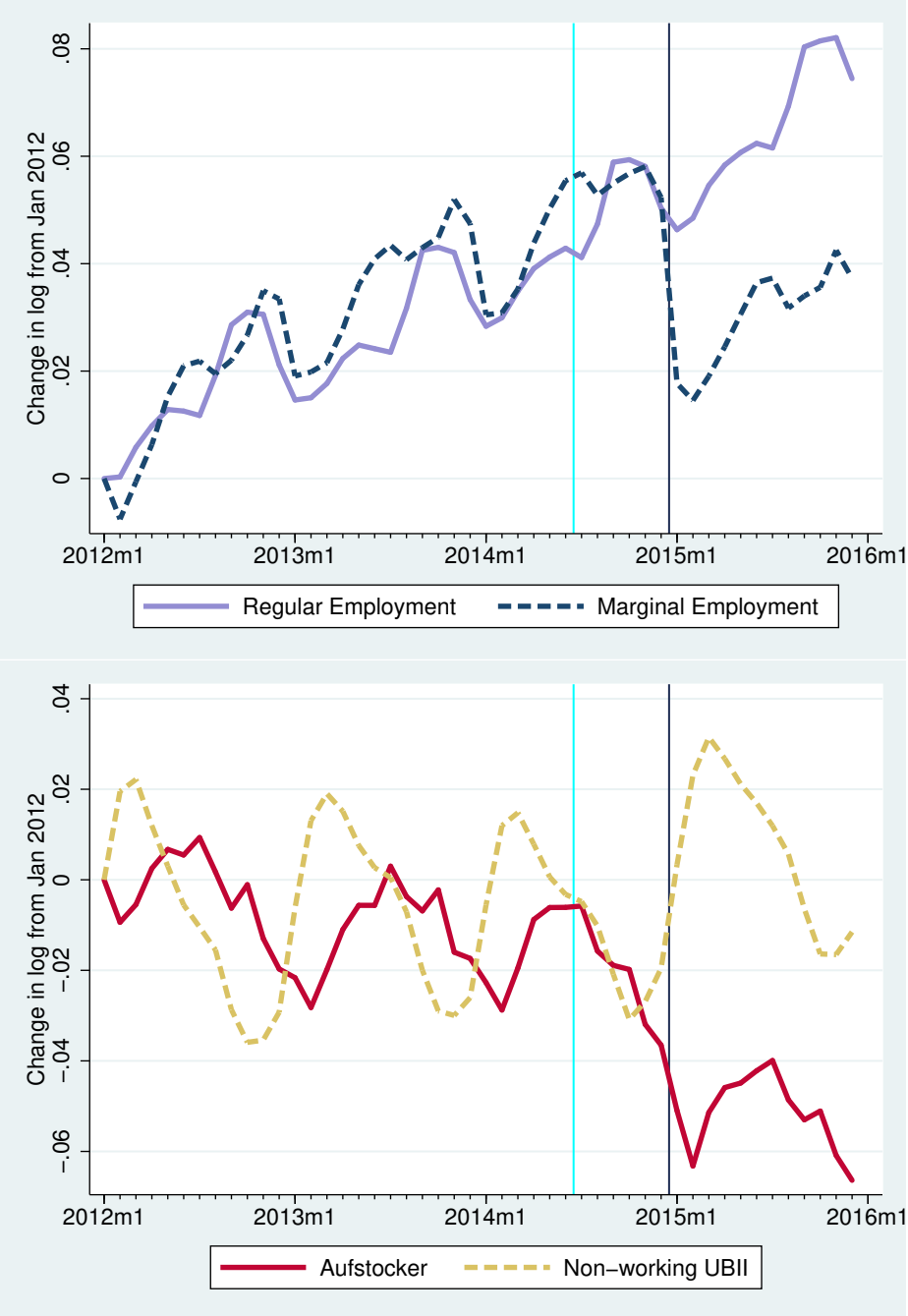


Figure 4: Employment and Welfare Outcomes over Time

3.4 Graphical Evidence & Trend Assumptions

The graphs in Figure 4 provide a descriptive account of the events; nevertheless, they suggest that the minimum wage might had a negative effect at least on marginal employment and on *Aufstocker*. However, these graphs do not account for the differences in the strength of the treatment and possible pre-trend differences in deterministic trends. Additionally, the series are noisy, due to the presence of seasonality. In this section, these concerns will be addressed.

Another goal of this section is to answer two important questions concerning the regression specification. First, whether the left-hand side of the equation should be specified in log-levels (Equation 1) or in growth rates (Equation 2), and second, whether to include a pre-treatment trend differential or not. As outlined in Section 3.1, identification hinges on the so-called common trend assumption (CTA), which is not directly testable. However, if for a certain specification more and less heavily treated counties move parallel *before* the policy change, one would be more confident hat under the absence of the new policy, the parallel movement would have continued also *after* the policy change. Thus, this section compares graphically the different specifications.[21]

In order to ease the graphical exposition, treatment won't be based on the continuous bite, but on the binary treatment indicator.[22] The resulting two time series are normalized by the average value in January 2012 (for levels specification) or 2013 (for the growth rate specification) respectively.[23] As in Figure 4, a vertical dark blue line indicates the official start of the minimum wage in January 2015; a light-blue vertical line six months before indicates the the passing of the law in July 2014.

Concerning the first question, the specification of the left-hand side, the time series for the levels specification is based on the residuals of a regression on time and county dummies. For the growth rate specification, the time series is differenced. As it is evident from Figure 4, all four outcomes exhibit strong seasonal patterns. The levels approach treats these patterns deterministically with a set of time-specific fixed effects, while the growth rate approach removes the seasonality *stochastically* by differencing.

For the second question, the pros and cons for the inclusion of a pre-treatment trend differential, the resulting time series can also be adjusted by the interaction of a trend

[21] Besides this graphical exposition, I will report in Section 4 the estimated coefficients of the pre-treatment trend differentials. Note that a test of their statistical significance cannot directly test the CTA, since the assumption refers to the hypothetical behavior *after* the policy change.

[22] Conveniently, one can compare the movements of two distinct groups, rather than 402 different counties. Treatment and control group are averages of their respective counties, weighted by the county-specific regular employment in December 2013. Recall that in the binary treatment counties are *treated* if the bite is above the median bite; otherwise, counties belong to the control group. The resulting binary treatment is displayed in a map (Figure A2, left panel) in the Appendix.

[23] Hence, values can be interpreted as percentage changes relative to January 2013 net of seasonal effects. The structure of the graph is similar to Figure 1 in Angrist and Krueger (1999) referring to the Mariel Boatlift study by Card (1990).

term (linear, quadratic, or in logs) with the region-specific bite.[24] Due to this adjustment, the values for this graph can no longer be read as a seasonally adjusted growth rate relative to the first period. The resulting series are the ones to be compared for the plausibility of the CTA, conditional on a deterministic pre-treatment trend differential.[25]

Concerning the first question, the graphical analysis suggests that the log-levels specification is not appropriate to distinguish seasonality and trend behavior. The graphs without trend differential and a linear trend differential for the four outcome variables are shown in the Appendix in Tables A4 to A7. The seasonal pattern is not entirely removed and there remain important differences between counties above and below the median. Hence, I will not consider the log-levels specification except for a robustness check and focus on the specification in growth rates (Equation 2). Thus, all remaining graphs in this section are seasonally adjusted by taking the 12-month difference. Additionally, the graphs are centered relative to the value for January 2013.

Figure 5 shows the two relevant graphs for the growth rates of regular employment. The upper panel shows seasonally adjusted data, the lower panel additionally corrects for a deterministic trend differential. In the upper panel, the two lines move parallel almost everywhere, except a short period in Spring 2014. A common trend appears plausible, and there seems to be no striking effect after the introduction of the minimum wage. In the graphs controlling for a trend differential, the movements before treatment are similar but less congruent. Thus, the graphical analysis speaks in favor of a specification *without* an additional trend differential.

For marginal employment, the two corresponding graphs are displayed in Figure 6. The upper and the lower panel appear to be very similar. In both cases, treatment and control group move parallel and almost horizontal until the minimum wage comes into effect. Both series experience a drop at the turn of the year; however, the one in the treatment group is much more pronounced. Recall that the smaller drop in the control regions is likely also related to the minimum wage, given that also the control group is partially treated, but only to a lesser extent.

Figure 7 displays the development of the growth rates of the *Aufstocker*. Contrary to the employment outcomes, there seems to be a clear discrepancy between the treatment and the control group before the introduction of the minimum wage. If one does not control for a trend differential (upper panel), the CTA appears implausible. Fortunately, the picture changes, once one controls for a simple linear trend differential. The movements of the control and the treatment are parallel until the introduction of the minimum wage. In 2015 a large discrepancy appears between the two groups.

The last of the four outcomes in growth rates, the non-working UBII recipients, are displayed in Figure 8. There are apparent similarities to the graphs for the *Aufstocker* in

[24] Technically, the seasonally adjusted values are regressed on the bite-trend interaction for the sample from January 2013 till December 2014. The remainder of the procedure described above is performed on the predicted (out of sample) residuals from this regression.

[25] I will focus on a linear trend specification. The resulting graphs for the growth rate specification with a quadratic polynomial or a logarithmic trend differential are shown in the Appendix (Figures A8 to A11)

Figure 7. Without any trend correction, the CTA seems implausible. However, as soon as one controls for a deterministic linear trend differential, the movements align. Unlike the *Aufstocker*, there is no strong indication for a diverging movement of the treatment group after the policy change.

Summing Up The graphical inspection of the binary treatment and control groups indicates that the growth rates specification is more appropriate than the specification in log-levels. Additionally, the CTA should be satisfied for the two employment outcomes without any inclusion of pre-treatment trend differentials. For the two outcomes studying welfare dependency however, a specification with a deterministic trend differential seems more plausible. The graphical analysis suggests the presence of effects for marginal employment and the *Aufstocker*. For regular employment and non-working UBII the graphs do not reveal any striking impact.

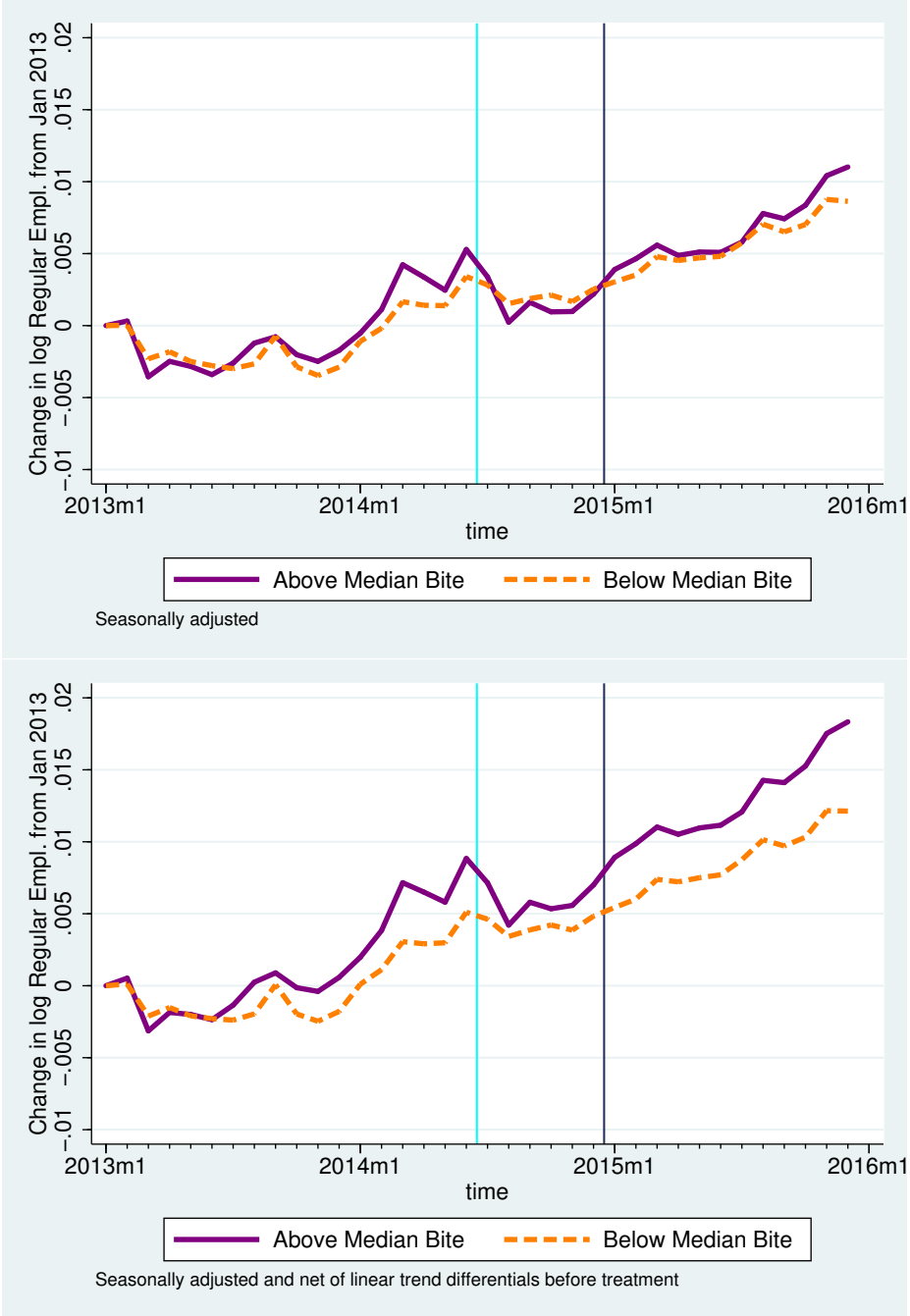


Figure 5: Regular Employment - Comparing Different Specifications

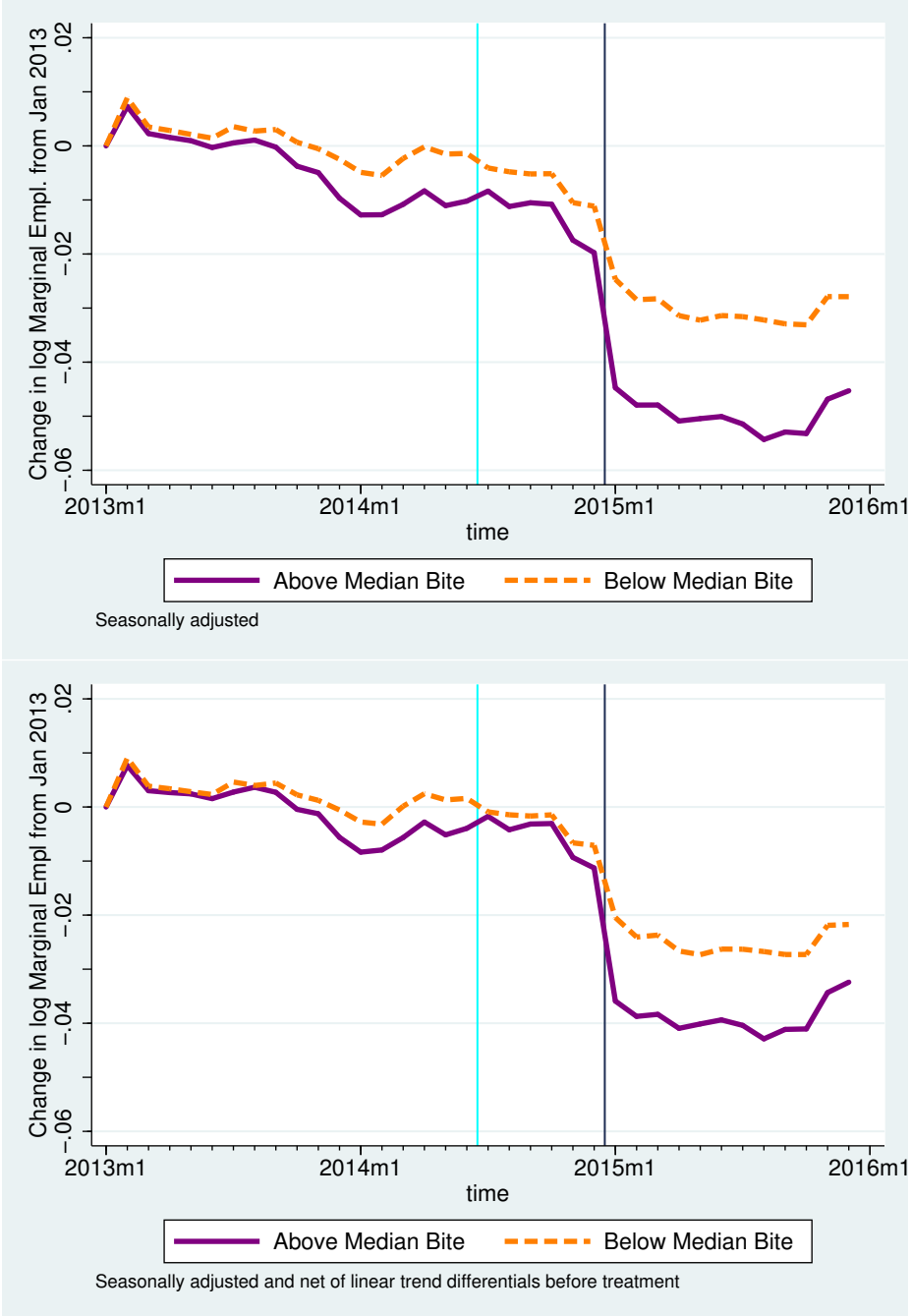


Figure 6: Marginal Employment - Comparing Different Specifications

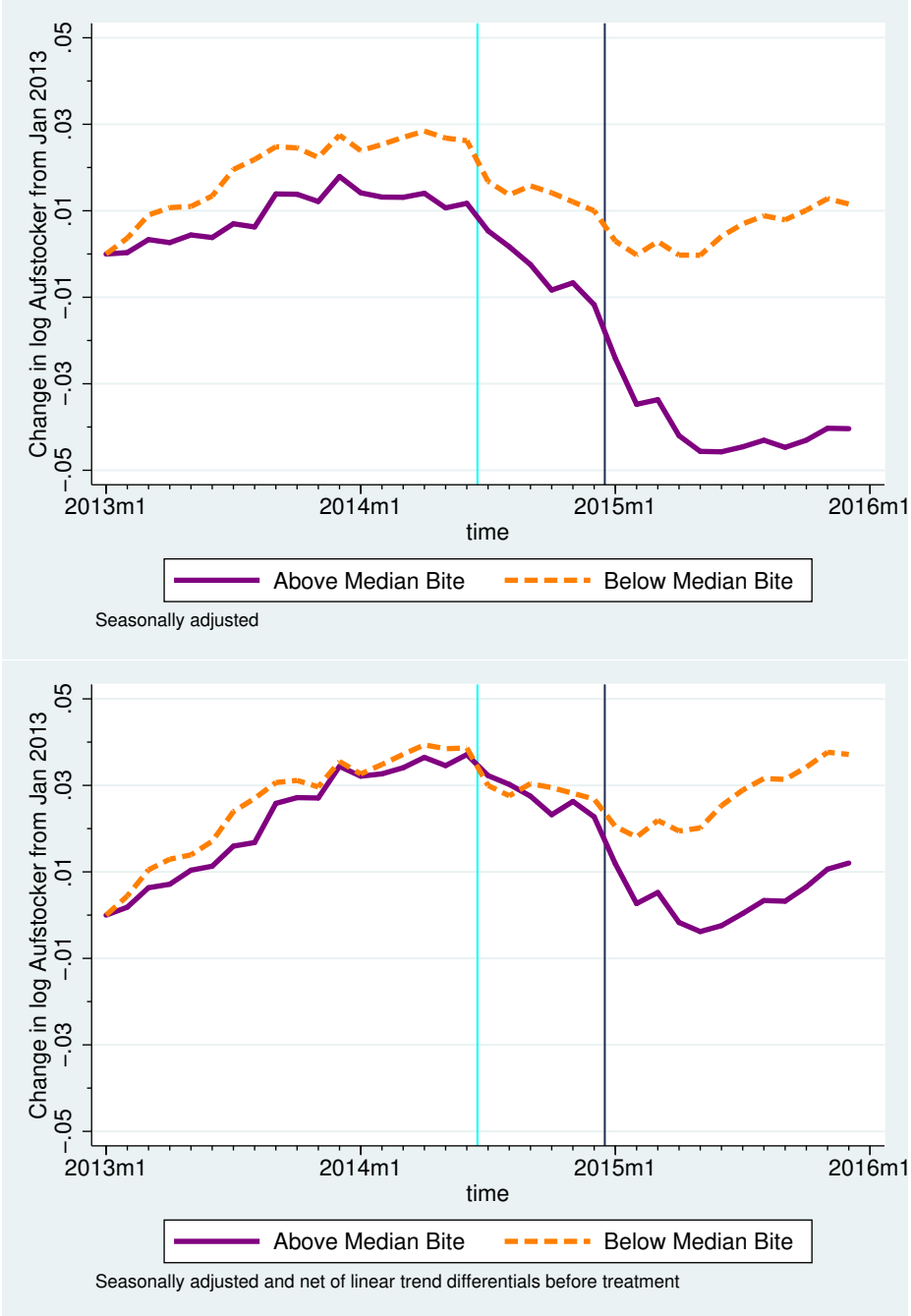


Figure 7: *Aufstocker* - Comparing Different Specifications

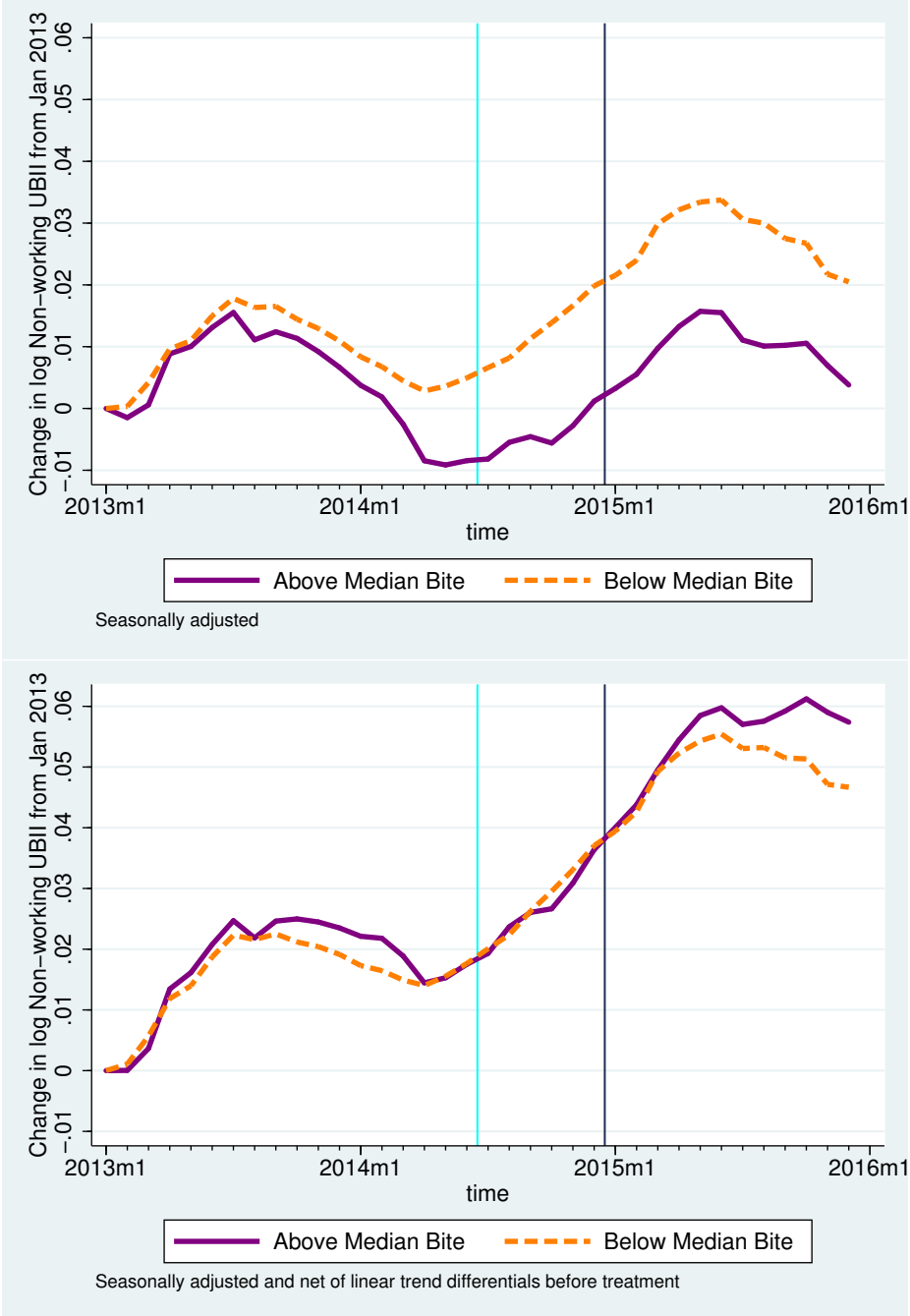


Figure 8: Non-working UBII - Comparing Different Specifications

4 Results

This section presents the results from fixed effects regressions as described in Section 3. As argued in the previous Section 3.4, the main specification will be in growth rates of the outcomes of interest (Equation 2). Note that in order to ease interpretation of the estimated coefficients, the county-specific bite of the minimum wage is normalized by its standard deviation and divided by 100. Thus, $\hat{\beta}$ gives the *percentage point change* of the growth rate of the respective outcome (regular or marginal employment, *Aufstocker*, non-working UBII) due to one additional standard deviation of the bite.[26] All tables will also report the overall, as well as the between and the within R2 measure. The between R2 represents the explained variation if the time-dimension would be collapsed on county level. The within R2 provides the goodness of fit for the mean detrended data, neglecting all variation across counties.

Employment Table 3 shows the effects of the minimum wage on employment outcomes. The upper panel displays the results for regular employment, the lower panel for marginal employment. Column (1) is the standard model from Equation 2, including time and county fixed effects, but no trend differential. As it was argued in the graphical analysis in Section 3.4, column (1) is the preferred specification for both, regular and marginal employment. The corresponding graphs are the upper panels in Figure 5 and 6. The specification in column (2) additionally includes $bite_i \cdot t \cdot \delta$, i.e. a linear trend differential proportional to the bite. This specification corresponds to the lower panels in Figure 5 and 6. As additional robustness checks, column (3) specifies a polynomial quadratic trend differential, column (4) a logarithmic one.[27]

For regular employment, all four specifications have a negative point estimate and thus indicate that the minimum wage reduced the growth rate of regular employment. The graphical analysis spoke in favor of the specification in column (1). The estimated coefficient implies that one standard deviation of the bite decreases the growth rate of employment by about 0.1 percentage points, significant at the 10% level. Given that the average bite is about two times the standard deviation, one could argue that such an effect does not appear to be economically significant. However, if the minimum wage would *permanently* depresses the growth rate of employment, the long-run effect could potentially be very large.

The choice of the functional form of the pre-treatment trend behavior appears to be influential, even though the differences across the bite coefficients are not always significant. Table 3 also reports the estimated trend coefficients; the linear and the log-trend differential are not statistically significant, while the quadratic trend differential

[26] In a robustness check, also the log-levels specification will be estimated. Here, $\hat{\beta}_L$ corresponds to the *percentage change* in the outcome variable due to one additional standard deviation of the bite.

[27] The corresponding graphs are shown in the Appendix in Figure A8 and A9.

is jointly highly significant.[28] Thus, on statistical grounds, one could argue that the specification from column (3) with a quadratic trend should be the preferred one. In that case, the effect of one standard deviation becomes very small and is no longer statistically different from zero at conventional levels of significance.

For marginal employment, the lower panel of Table 3 shows that effect on growth rate of marginal employment is more pronounced than the one on regular employment. This is in line with Figure 4 and previous evidence on the employment effects of the minimum wage. In the preferred specification, an additional standard deviation of the bite reduces the growth rate by 1.4 percentage points. This estimate is reduced to about 1 percentage point, if one controls for a pre-trend differential, regardless of the trend's functional form. Regarding the significance of the estimated trend differentials, all three trend differentials are statistically significant at least at the 5% level.[29] All three specifications including a trend differential indicate more or less the same effect of a 1 percentage point reduction of the growth rate. Thus, there is strong evidence that the minimum wage had a negative effect on marginal employment. Given that all point estimates for regular employment are negative, the results so far do not suggest that marginal jobs have been upgraded to regular jobs on a massive scale.

[28] The F-test of joint significance of the linear and quadratic trend differential has a test statistic of 6.0881 with an associated p-value of 0.0025.

[29] The F-test of joint significance of the linear and quadratic trend differential has a test statistic of 3.3733 with an associated p-value of 0.0353.

Table 3: Effects on Employment Outcomes

	(1)	(2)	(3)	(4)
		Regular Employment		
Bite·2015	-0.096*	-0.160***	-0.040	-0.170***
	(0.050)	(0.060)	(0.041)	(0.057)
$t \times$ Bite		0.001	0.009***	
		(0.001)	(0.003)	
$t^2 \times$ Bite			-0.000***	
			(0.000)	
$\log(t) \times$ Bite				0.035
				(0.022)
Observations	14472	14472	14472	14472
R2 within	0.171	0.171	0.173	0.172
R2 between	0.177	0.177	0.177	0.177
R2 overall	0.097	0.058	0.001	0.000
		Marginal Employment		
Bite·2015	-1.359***	-0.995***	-1.133***	-1.065***
	(0.206)	(0.245)	(0.229)	(0.240)
$t \times$ Bite		-0.006***	-0.015	
		(0.002)	(0.010)	
$t^2 \times$ Bite			0.000	
			(0.000)	
$\log(t) \times$ Bite				-0.138**
				(0.055)
Observations	14472	14472	14472	14472
R2 within	0.306	0.308	0.308	0.308
R2 between	0.110	0.110	0.110	0.110
R2 overall	0.249	0.249	0.231	0.196

Bite measured in December 2013 and normalized by one SD

Estimation including Time and County Fixed Effects

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Welfare Dependency While there already exist several studies about the employment effects, ex-post evidence on the effects of the minimum wage on the *Aufstocker* and welfare dependency is scarce. At the time of writing, there is only the descriptive study by Bruckmeier and Wiemers (2016), reporting an unusually large reduction of the *Aufstocker* at the turn of the year 2014/15 which is also present in the descriptive Figure 4 in Section 3.3.

Table 4 follows the same structure as Table 3, but this time for the two welfare-related outcomes. The number of observations is slightly smaller, since the number of welfare recipients are sometimes missing in the original data. Recall from the graphical exposition in Section 3.4 that for both welfare outcomes column (2) (linear trend differential) was preferred over the specification without any trend differential. For the *Aufstocker*, all four point estimates are negative and statistically significant at the 1% level. The preferred specification in column (2) indicates that one standard deviation of the bite reduces the growth rate of the *Aufstocker* by 1.4 percentage points. The point estimate from column (1) without and trend differential is substantially larger with 2.6 percentage points. This discrepancy was already indicated in the graphical exposition in Section 3.4, as the difference between the upper and lower panel of Figure 7. Concerning the estimated trend differentials, all three trend specifications are (jointly) statistically significant In sum, the results point without any doubt to a reduction of the the growth rate of the *Aufstocker*.

For the other welfare-related outcome, the growth rate of non-working UBII recipients, the results do not draw such a clear picture. The sign of the point estimate switches from negative to positive, if one includes any form of trend differential. The preferred estimate in column (2) with a linear trend differential indicates a small and significant increase in the growth rate of non-working UBII recipients by 0.3 percentage points due to one additional standard deviation of the bite. The other two trend-corrected specifications result in smaller and insignificant point estimates. All trend terms are highly significant. Thus, it is difficult to conclude which of the specifications is the most credible and consequently, whether there is a significant increase in the non-working welfare recipients. In any case, given that the strong effect for the *Aufstocker* is not matched by an equally striking effect on the non-working UBII recipients, the results from Table 4 suggest that the reduction in the *Aufstocker* is not entirely due to lost supplementary jobs, but potentially also due to increased labor earnings of the *Aufstocker*.

Table 4: Effects on Welfare Outcomes

	(1)	**(2)**	(3)	(4)
		Aufstocker		
Bite·2015	-2.617***	-1.460***	-1.249***	-1.745***
	(0.156)	(0.209)	(0.155)	(0.198)
$t \times$ Bite		-0.018***	-0.003	
		(0.003)	(0.012)	
$t^2 \times$ Bite			-0.000	
			(0.000)	
$\log(t) \times$ Bite				-0.411***
				(0.074)
Observations	14298	14298	14298	14298
R2 within	0.238	0.248	0.249	0.247
R2 between	0.530	0.533	0.532	0.534
R2 overall	0.283	0.382	0.368	0.352
		Non-working UBII		
Bite·2015	-0.635***	0.321**	0.091	0.143
	(0.109)	(0.139)	(0.108)	(0.131)
$t \times$ Bite		-0.015***	-0.031***	
		(0.002)	(0.008)	
$t^2 \times$ Bite			0.000**	
			(0.000)	
$\log(t) \times$ Bite				-0.367***
				(0.051)
Observations	14298	14298	14298	14298
R2 within	0.105	0.120	0.121	0.121
R2 between	0.446	0.446	0.446	0.446
R2 overall	0.111	0.294	0.296	0.279

Bite measured in December 2013 and normalized by one SD
Estimation including Time and County Fixed Effects
Cluster-robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Back-of-the-envelope The specification in growth rates and with a continuous treatment has the important drawback that it is difficult to translate the results into an easily understandable effect size. It is for instance not clear for which time horizon the minimum wage will affect the growth rates. It is unlikely that the minimum wage only has an impact in 2015. On the other hand, if the measured impact was *permanent*, it would be implausibly high in the long run. Concerning the treatment, it is not clear how many standard deviations should be the yardstick for the effect of the policy. Table 5 presents back-of-the-envelope calculations for the implied size of the short-run effect in 2015, using two standard deviations - which is about the size of the mean bite - as the preferred multiplier. The estimates should not be taken as *the* definite *treatment effect* of the minimum wage, but illustrate the order of magnitude of the implied effects. The calculations in Table 5 use the stock of the outcomes in December 2013. The $\hat{\beta}$-coefficients are those without adjustment for a trend-differential for employment outcomes (Column (1) in Table 3) and those with a linear trend-differential for the two welfare outcomes (Column (2) in Table 4).[30]

Table 5: Effects of Preferred Specifications

	Employment		Welfare	
	Regular	Marginal	*Aufstocker*	NW-UBII
Stock in 2013	29,883,573	7,438,102	1,298,297	3,016,337
$\hat{\beta}$	-0.096	-1.359	-1.460	0.321
Effect +1 SD	-28,688	-101,084	-18,955	9,682
Effect +2 SD	-57,376	-202,168	-37,910	19,365

Back-of-the-envelope calculation, short-run effects in 2015

For regular employment, the effect of two standard deviations corresponds to about 60,000 less jobs due to the minimum wage. Compared to the stock of employment of about 30 million employees, and the large predicted long-run effect of some of the ex-ante studies, this *short-run* effect is comparatively small. Additionally, the effect does not appear to be robust to the inclusion of pre-treatment trend differentials.[31] Note that this calculation does not imply that existing jobs are lost, but that the job creation dynamics are hampered. As it is displayed in the employment graphs in Figure 4, regular employment followed an upward trajectory in recent years. Knabe et al. (2016) also argue that the minimum wage did not destroy existing jobs but did reduce job creation. For

[30] Given that tests on the statistical significance of the pre-treatment trend differentials in Tables 3 and 4 did not provide a clear guidance, this choice is somewhat arbitrary. Especially the results for regular employment and non-working welfare recipients have to be taken with a pinch of salt. I decided to stick to the preferred specifications of the graphical analysis, since these are credible, but also parsimonious.

[31] For the specification with a quadratic trend differential, the effect would essentially zero.

marginal employment, the effect of two standard deviations of the bite is about 200,000 lost marginal jobs or about 150,000 for the robustness check including a trend differential For the level of marginal employment, the figures and the estimation results indicate an actual (and not only counter-factual) reduction due to the minimum wage, which is also in line with previous research, such as Groll (2015) or Garloff (2016).

For the welfare outcomes, the two standard deviations imply a reduction of about 38,000 *Aufstocker* and an partially offsetting increase in the number of non-working UBII recipients by about 19,000. This calculations suggest that roughly one half of the reduction in the *Aufstocker* was due to the loss of a supplementary job, instead of an increase in the household income. However, the effect on non-working UBII has to be taken with a pinch of salt, because the estimated coefficients fluctuate considerably. The very small absolute reduction of the *Aufstocker* due to the minimum wage also confirms the previous literature, which pointed to the limited effectiveness of minimum wages for reducing welfare dependency.

Differences between East and West Germany Table 6 uses only variation in the bite *within* East and West Germany.[32] The table presents only the preferred specification, i.e. without any pre-treatment trend differential for the two employment outcomes (column 1 and 2), but with a linear trend differential for the two welfare related outcomes (column 3 and 4). The Upper panel shows the results for West Germany, the lower panel for the East.

Concerning employment outcomes within West Germany, the result suggest positive job dynamics in counties more heavily affected by the minimum wage: The growth rate of marginal employment is reduced by about 0.6 percentage points, however, this reduction appears to be offset by an increase of the growth rate of regular employment of almost 0.3 percentage points. Give the relative magnitudes of the two types of employment, this pattern can be seen as evidence for upgrading of marginal into regular jobs due to the minimum wage. For East Germany however, both point estimates for the employment outcomes are negative and relatively large, even though they are not statistically significant at conventional levels.

For the welfare outcomes within West Germany, the point estimate for the *Aufstocker* is positive but not significant. For the non-working UBII recipients, the results indicate a statistically significant *reduction* of the growth by about 1 percentage point. The welfare effects in the West are difficult to reconcile. For East Germany however, the growth of the *Aufstocker* is reduced by 2.3 percentage points, while the growth of non-working UBII recipients increases by 0.9 percentage points. Thus, the overall effects on welfare dependency shown in Table 4 seem to be driven by changes in the East. Given the relative magnitudes of working and non-working welfare recipients, the estimates suggest that most of the *Aufstocker* in the East ended up in non-working welfare dependency.

[32]The corresponding maps of the bite are shown in the Appendix in Figure A1.

On balance, the results from this sample split paint a rather positive picture of the short-run effects of the minimum wage in West Germany and a negative for the East. In the West, some marginal jobs seem to be upgraded due to the minimum wage. This dynamic is not present in the East, where some of the *Aufstocker* appear to have lost their supplementary jobs. These differences echo the findings of Knabe et al. (2014) and the concerns in the public debate whether a universally set minimum wage could be workable in the west, but too high for the east of Germany.

Table 6: Variation within West and East Germany

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
			West Germany	
Bite·2015	0.280**	-0.659**	0.796	-1.010**
	(0.124)	(0.262)	(0.717)	(0.471)
Observations	11700	11700	11558	11558
R2 within	0.183	0.279	0.078	0.103
R2 between	0.003	0.017	0.050	0.034
R2 overall	0.095	0.208	0.066	0.068
			East Germany	
Bite·2015	-0.287	-0.955	-2.330***	0.900*
	(0.188)	(0.971)	(0.495)	(0.463)
Observations	2772	2772	2740	2740
R2 within	0.190	0.347	0.735	0.299
R2 between	0.088	0.002	0.057	0.169
R2 overall	0.117	0.264	0.639	0.224
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Composition of the *Aufstocker* While there seems to be an overall reduction of the growth rate of the *Aufstocker*, it remains to be seen which group was affected most

severely by the minimum wage. Table 7 shows the effects on four different *Aufstocker* subgroups, namely self-employed and Mini-, Midi- and Maxijobs. As for the parent category - *Aufstocker* - a linear trend differential is included. Note that the number of observations differs slightly for the self-employed due to data availability.

The effect on the self-employed is not statistically significant, which is not surprising, since a minimum wage should not directly affect self-employed *Aufstocker*. The three groups of dependently employed *Aufstocker* however, all feature statistically significant reductions of their growth rates. The relative effect is the largest for midi and maxi jobs. Especially for those with already relatively high earnings, it is plausible that some have left welfare dependency entirely.

Table 7: Composition of *Aufstocker*

	Self-empl. (1)	Mini Job (2)	Midi Job (3)	Maxi Job (4)
Bite·2015	-0.834	-0.786***	-2.676***	-2.357***
	(0.733)	(0.278)	(0.455)	(0.401)
Linear Trend	✓	✓	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓
Observations	14294	14298	14298	14298
R2 within	0.012	0.296	0.108	0.068
R2 between	0.126	0.485	0.061	0.419
R2 overall	0.018	0.366	0.083	0.168

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Wages This section analyzes whether the introduction of the minimum wage actually affected labor earnings. Table 8 shows the results from OLS regressions where the bite measured in 2013 is related to the county-specific growth rate of labor earnings between 2013 and 2015 (upper panel) or to the level of monthly earnings in 2015, controlling for the average level in the county in 2013. Hence, the estimated coefficients in the upper panel can be read as the effect on the growth rate of earnings in percentage points, due to one additional standard deviation of the bite. In the lower panel, the coefficients provides the income change in Euro due to one additional standard deviation of the bite. All earnings information are taken from the wage statistics (Statistik der Bundesagentur für Arbeit, 2016c).

Table 8: Earnings and Earnings Growth between 2013 and 2015

	Avg income (1)	Up to 1400 (2)	Up to 1500 (3)	1500 to 2000 (4)	Above 2000 (5)
			Earnings Growth 2015-2013		
Bite	0.716***	0.419***	1.423***	-0.027	-0.891***
	(0.045)	(0.067)	(0.064)	(0.019)	(0.044)
Observations	402	402	402	402	402
R2	0.319	0.090	0.556	0.005	0.515
			Monthly Earnings 2015		
Bite	22.77***	8.27***	18.52***	-2.82***	-27.22***
	(2.761)	(1.032)	(1.013)	(0.498)	(1.911)
Observations	402	402	402	402	402
R2	0.996	0.847	0.896	0.683	0.995

Bite measured in December 2013 and normalized by one SD. Robust standard errors in parentheses.
Earnings Growth is $\frac{\text{Income 2015}-\text{Income 2013}}{\text{Income 2013}}$.
The effect on monthly earnings 2015 is conditional on average monthly earnings in 2013
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column (1) shows specifications which do not condition on the income level, in column (2) the outcomes are based on the average income of those earning up to 1400€, in column (3) the threshold is raised to 1500€, in column (4) of the average income between 1500 and 2000€ per month. Column (5) shows the outcomes based on the average income of those earning more than 2000€ per month. There are positive statistically significant effects for the first three columns, i.e. overall earnings and earnings that are likely affected by the minimum wage The largest relative effect is found column (3), i.e. for those earning slightly more than the minimum wage. This pattern provides strong evidence for a positive effect of the policy on wages. There is no significant earnings growth for those earning between 1500 and 2000€ per month. Also the absolute change for this group does not seem to be *economically* significant. Thus, there is no evidence for strong positive spillover effects across the wage distribution. The average income of those earning above 2000€, a group which is likely not affected by the minimum wage, is negatively related to the bite.

Robustness In the following, I will discuss the results from various robustness checks. All related tables are delegated to the Appendix. Tables A1 contains the employment effects for the levels specification (Equation 1) and follows the same structure as Table 3,

hence compares the four different specifications of the pre-treatment trend differential. Remember that in this specification, the estimated coefficients do not provide an effect on the growth rate in percentage points, but a percentage change of the outcomes. Concerning regular employment, the preferred specification implies a reduction of employment by about 0.8 percent due to an additional standard deviation of the bite. All four specifications show a negative effect, but controlling for a trend differential reduces the estimated coefficients considerably. The relative effect on marginal employment is more pronounced: In the preferred specification, an additional standard deviation of the bite reduces marginal employment by 1.8%. Recall from the graphical analysis in Section 3.4 that the common trend assumption did not appear to hold and thus might be misleading. Nevertheless, the levels specifications would point to similar conclusions as the specification in growth rates.

Tables A2 provides the welfare counterpart in levels to Table A1. The specification in levels with a linear trend differential indicates that for the *Aufstocker* one additional standard deviation of the bite reduces the stock by 2.7% . Also all other point estimates are negative and statistically significant, even though their validity is questionable, since the common trend assumption is likely not to hold. For non-working UBII recipients, not all point estimates have the same sign, even though the standard errors point to rather precise estimates. For non-working UBII, also in the log-levels specification, the point estimates oscillate wildly with the chosen specification of the pre-treatment trend differential.

Table A3 summarizes the results from the alternative specification of the growth rate (Equation 3) with anticipation effects and adjustment over time. The employment outcomes are displayed in column (1) and (2). The two specifications do include any pre-treatment differential. The welfare-related outcomes are displayed in column (3) and (4) and are estimated including a linear trend differential. The anticipation period starts with July 2014, after the minimum wage bill was passed and hence consists of the six month in the second half of 2014. The adjustment period starts in January 2015 and includes all months until June 2015. The last six months of 2015 are grouped together. For regular employment, the first statistically significant effects arise form October 2014 onwards, for marginal employment from November 2014 onwards. For the welfare outcomes, there is no striking significant anticipation apart from small positive and significant effects just in the month of August 2014. On Balance, the found patterns do not point to any considerable anticipation effects. If one redefines the start of the treatment from January 2015 to October 2014 (the month with the first significant employment effects), and ignores the adjustment procedure over time, the effects remain largely unaffected.

Given that the identification rests on a difference-in-differences framework and that there are multiple time periods, it is natural to test the validity of the identification strategy using a placebo treatment. Table A4 reports the preferred estimates for all four outcomes (no trend differential for the employment and a linear trend differential for the welfare outcomes) on a treatment that starts in January 2014 and ends in December 2014. The information from 2015 is discarded. Ideally, the estimated effects of this

pseudo treatment would be close to zero and not statistically significant. Indeed, this is the case for regular employment and non-working UBII recipients. The coefficient for the *Aufstocker* indicates a change of the growth rate by 0.2 percentage points, even though it is not statstically significant at conventional levels. The growth rate of marginal employment is reduced by 0.3 percentage points and significant at the one percent level. This casts some doubts at the identification strategy. Nevertheless, the estimated pseudo-effects are much smaller than the preferred estimates and would be considered not economically significant.

The main analysis so far did not distinguish between female and male employees. Table A5 shows variation within men and women and also another variant in which both gender types are used together. The latter corresponds to the "gender cell" specification in Garloff (2016) and only shows the preferred specifications of the pre-treatment trend behavior. In all three gender specification, there is no effect on regular employment, but always a reduction of the growth rate of marginal employment by about 1 percentage point. For the growth rate of the *Aufstocker* all three gender specifications find negative effects. However, the effect sizes differ considerably across samples. Concerning non-working UBII recipients, the effects vary from zero for the male sample to a positive effect of about 0.4 for women and -0.4 percentage points for the gender cells approach. Taken together, the results from Table A5 confirm the discussion about the emplyoment welfare effects estimated in Table 3 and 4: The minimum wage has a negligible effect on regular, but a very robust negative effect on marginal employment and on the *Aufstocker*. The effect on the growth rate of non-working UBII is rather sensitive to the chosen estimation method and thus not very reliable.

Table A6 shows the effects for alternative binary treatments, used for instance in the graphs assessing the validity of the common trend assumption. Treated and not-treated counties are displayed in Figure A2. These specifications show strong significant negative effects on the growth rate of marginal employment and on the *Aufstocker*. The other two outcomes are not significant in this specification. Table A7 repeats the same exercise, but this time with alternative definitions of the bite, namely with 1400€ or 2000€ as the threshold of monthly gross earnings. Changing the threshold for the bite only mildly affect the estimated coefficients and leaves the detected patterns unchanged.

Table A8 uses a coarser level of aggregation, namely labor market regions instead of counties. The advantage of this approach is that it rests on variation across labor market regions which might be more relevant for the impact of the minimum wage. The regions are defined following Eckey et al. (2007), based on observed commuting patterns. Also this robustness check confirms strong significant negative effects on the growth rate of marginal employment and on the *Aufstocker*, but does not find significant effects for the other two outcomes.

To sum up, the vast majority of robustness checks confirm the existence of negative effects on marginal employment and on the *Aufstocker*. The effect on regular employment, which is quite small in the preferred specification frequently vanishes entirely, if one modifies the estimation strategy. Thus, one can conclude that there are only very

small or even no considerable effects on regular employment. The effect on non-working UBII recipients does not appear to be robust.

5 Discussion

This study examines the effects of the German statutory minimum Wage on employment and welfare dependency, using a difference-in-differences framework. The German labor market remained in a seemingly strong position after the introduction of the minimum wage with no striking *immediate* negative repercussions. However, this study finds evidence for a comparatively large reduction in marginal employment. Concerning regular employment, there is some evidence for an overall small negative effect, even though it does not appear to be very robust. For West Germany, there is some evidence that the loss of marginal employment is offset by conversions into regular employment. In general, the results confirm the findings of previous ex-post studies on the (modest) short-run employment implications of the statutory minimum wage.

Concerning welfare dependency, there is a reduction in the number of the *Aufstocker*, i.e. recipients of unemployment benefit II while working. However, as already argued by Müller and Steiner (2009); Bruckmeier and Wiemers (2014) this effect does not need to imply an improvement in the economic situation of the affected households, since withdrawal rates of the supplementary welfare payments are high. Nevertheless, there might be strong effects on subjective well-being, due to the elimination of welfare stigma (Hetschko et al., 2016) and a partial relief for social spendings. For West Germany there is no indication that the reduction in the growth rate of the *Aufstocker* was caused by them, loosing their job and ending up in non-working welfare receipts. For East Germany on the other hand, there is evidence that a considerable share of the *Aufstocker* did so.

The analysis only considers the *short-run* effects in the first year after the introduction of the minimum wage. Thus, the results cannot give a proper indication of the total effect or the impact of the minimum wage during the next economic recession and recovery. Additionally, the minimum wage might have some harmful medium to long-run effects in strongly affected regions due to location and investment decisions which have yet to take effect. Firms could invest in new machines which are less labor intensive or firms could decide to relocate to other areas due to a change in the relative prices for labor among regions. Nevertheless, it is worthwhile to study the *immediate* effects of the minimum wage directly after its introduction. The short-run loss of about 200,000 jobs in marginal employment is substantial. This finding at least casts some doubts at the sentiment that the minimum wage was free from side effects. Last but not least, if the detected reduction in the growth rate of regular employment turns out to be *permanent*, the resulting long-run effect on employment will be substantial.

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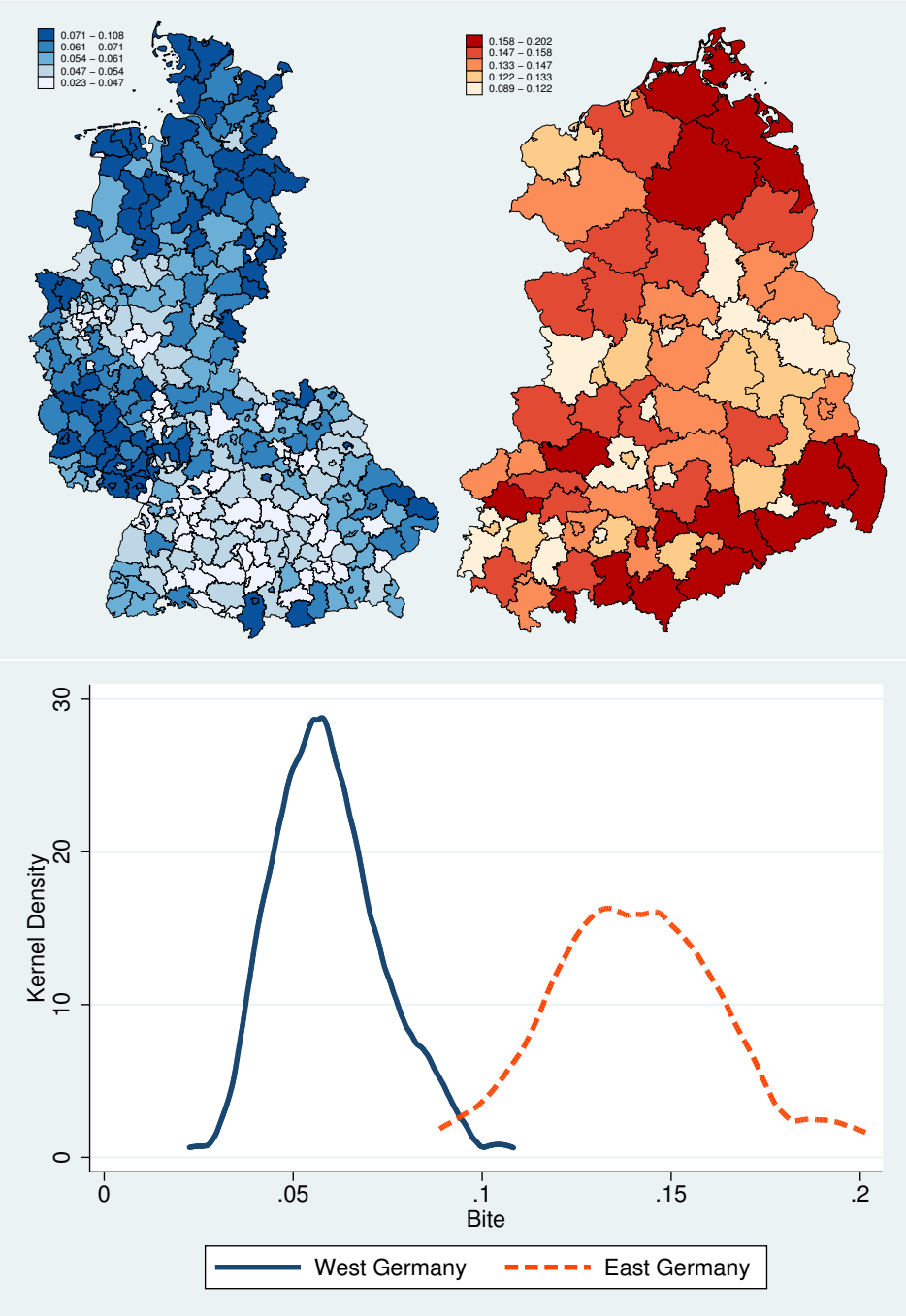
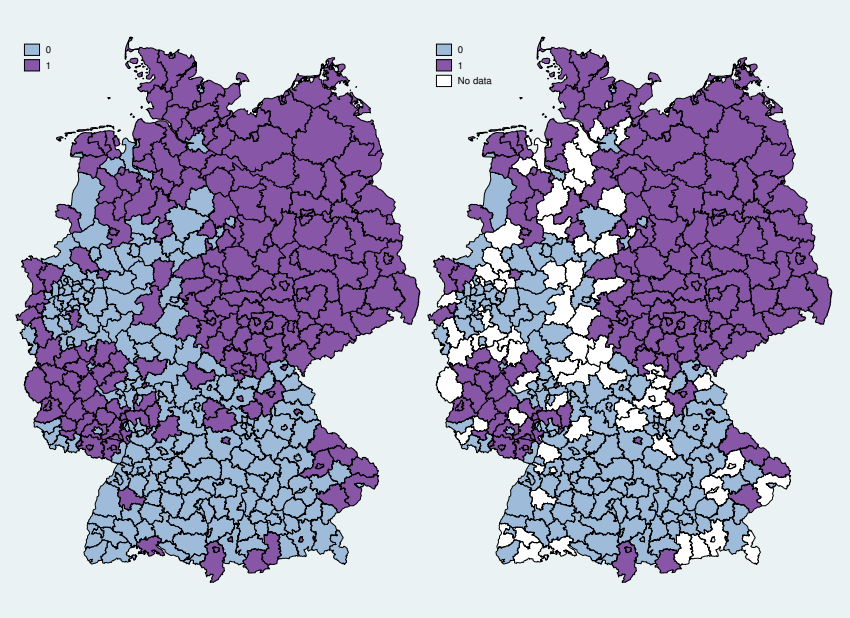
Appendix



Note: Measured in December 2013

Figure A1: Bite within West and East Germany - Map and Kernel Density

Figure A2: Alternative Binary Treatments



Note: 1: Treatment; 0: Control; Left: Above/below p50; Right: Above p60 - Below p40

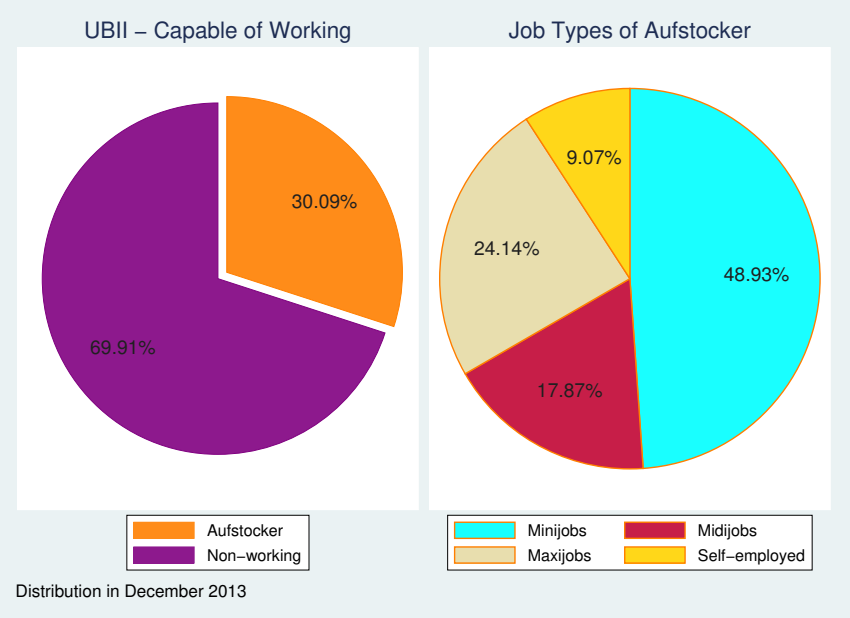


Figure A3: Composition of UBII Recipients and Job Types of *Aufstocker*

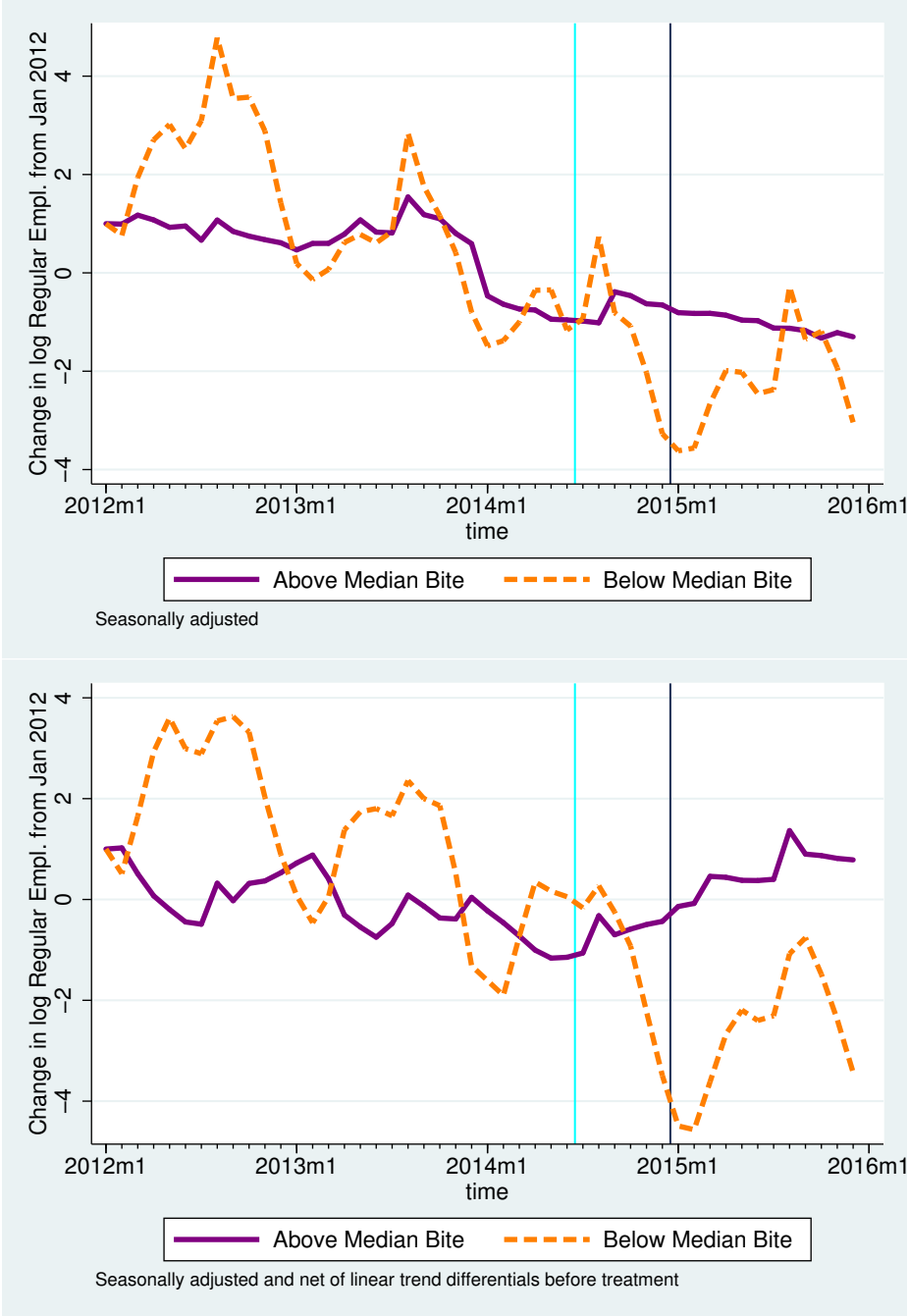


Figure A4: Regular Employment - Deterministic Seasonality, With and Without Linear Trend Differential

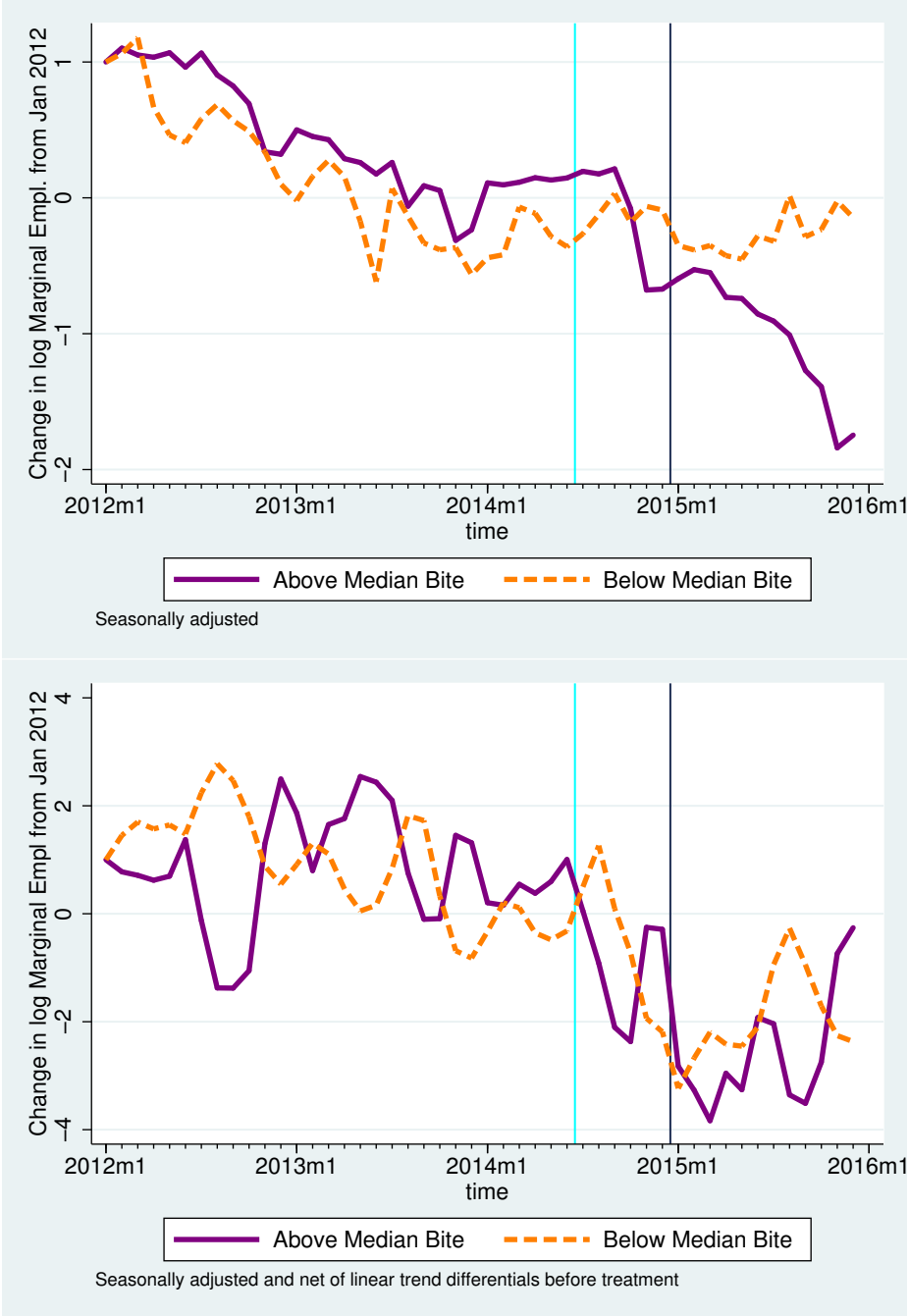


Figure A5: Marginal Employment - Deterministic Seasonality, With and Without Linear Trend Differential

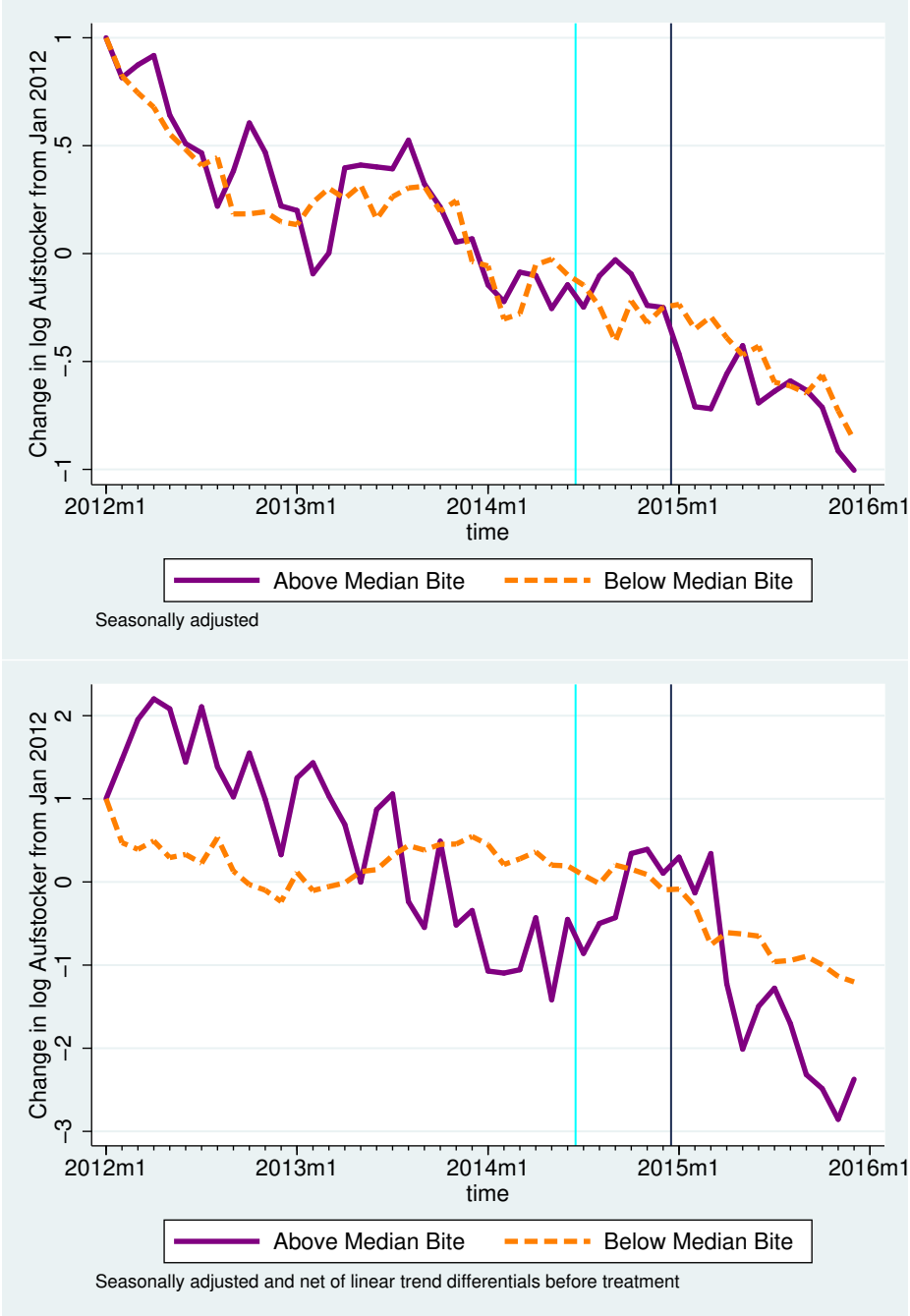


Figure A6: *Aufstocker* - Deterministic Seasonality, With and Without Linear Trend Differential

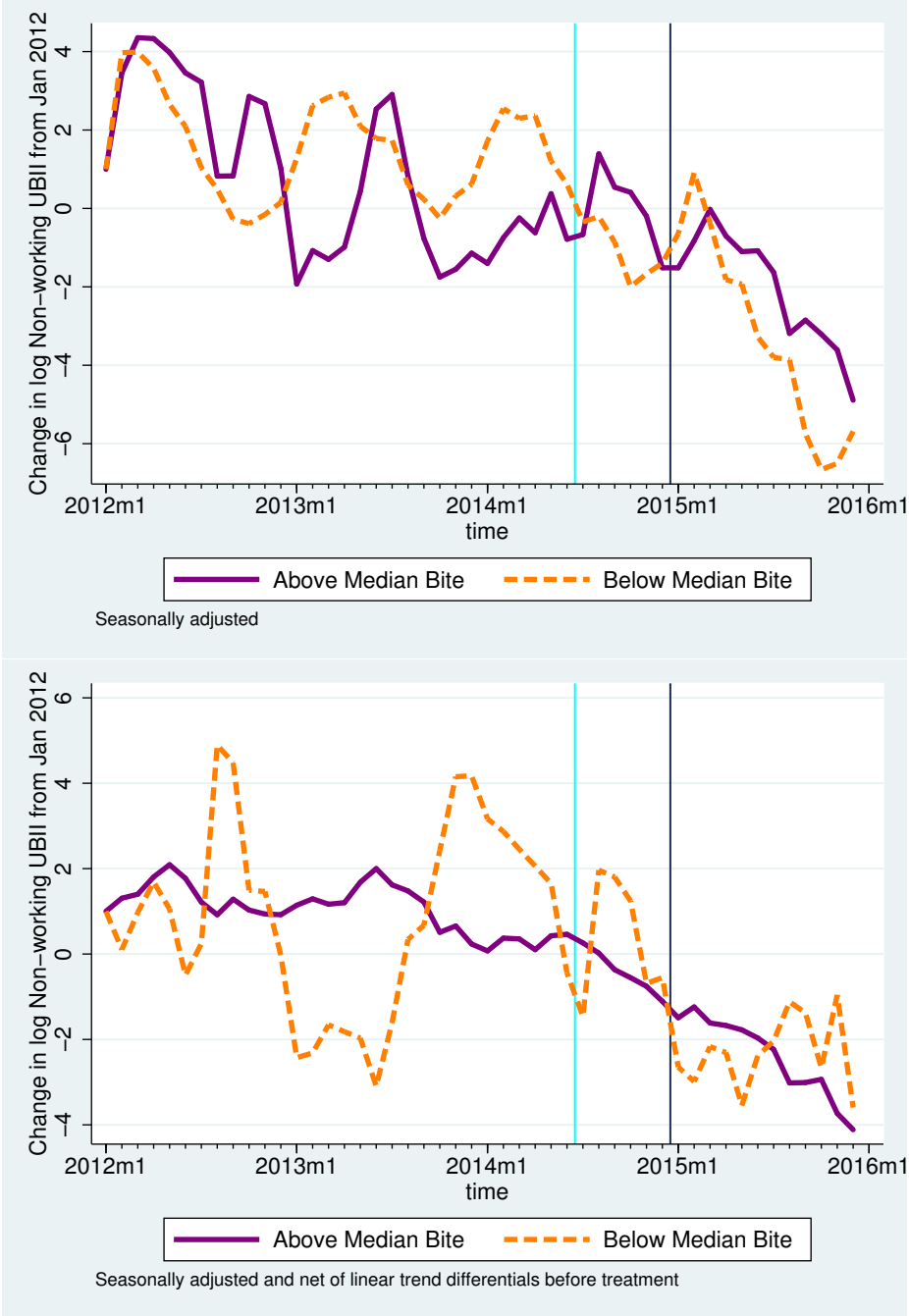


Figure A7: Non-working UBII - Deterministic Seasonality, With and Without Linear Trend Differential

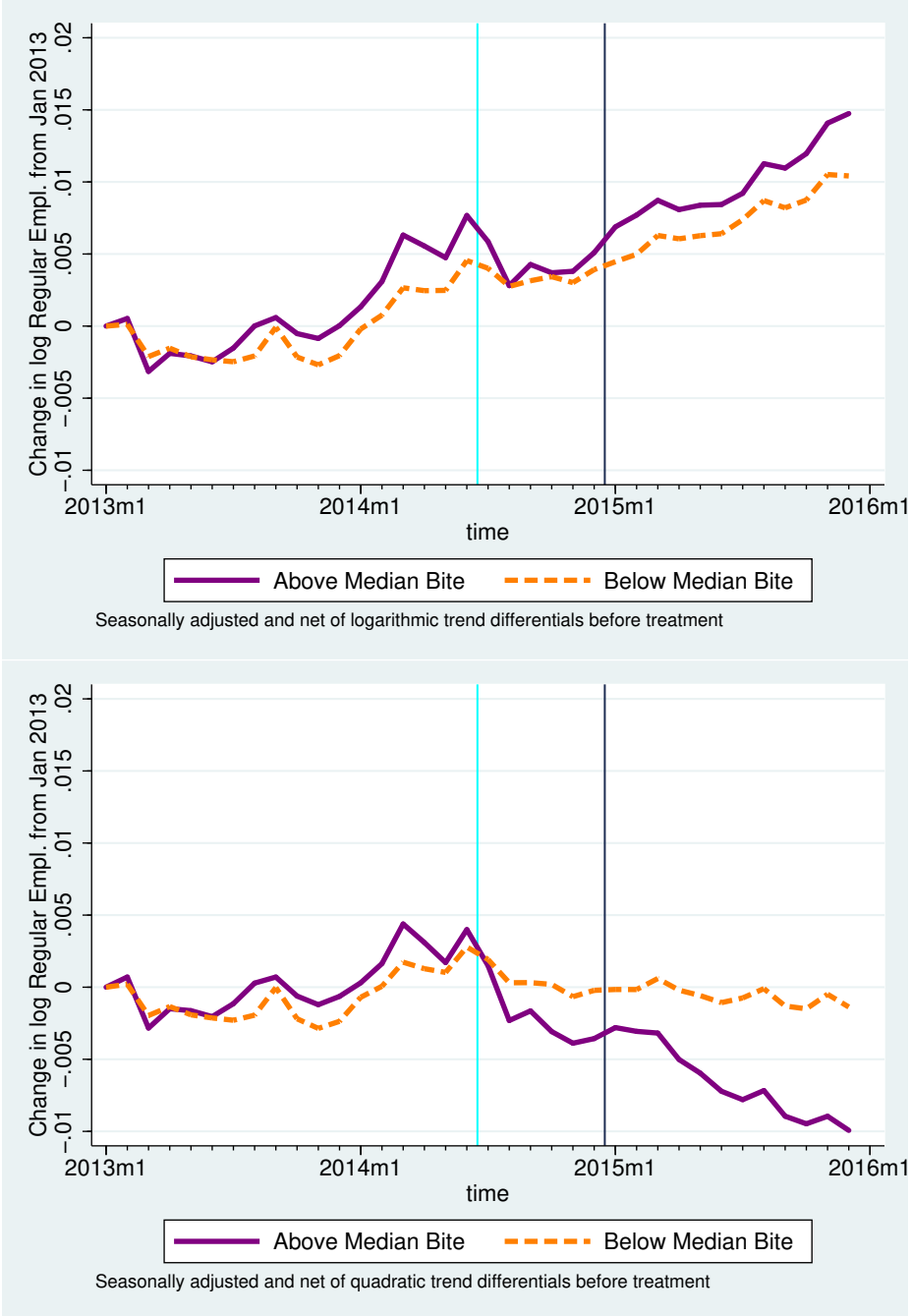


Figure A8: Regular Employment - Further Trend Specifications

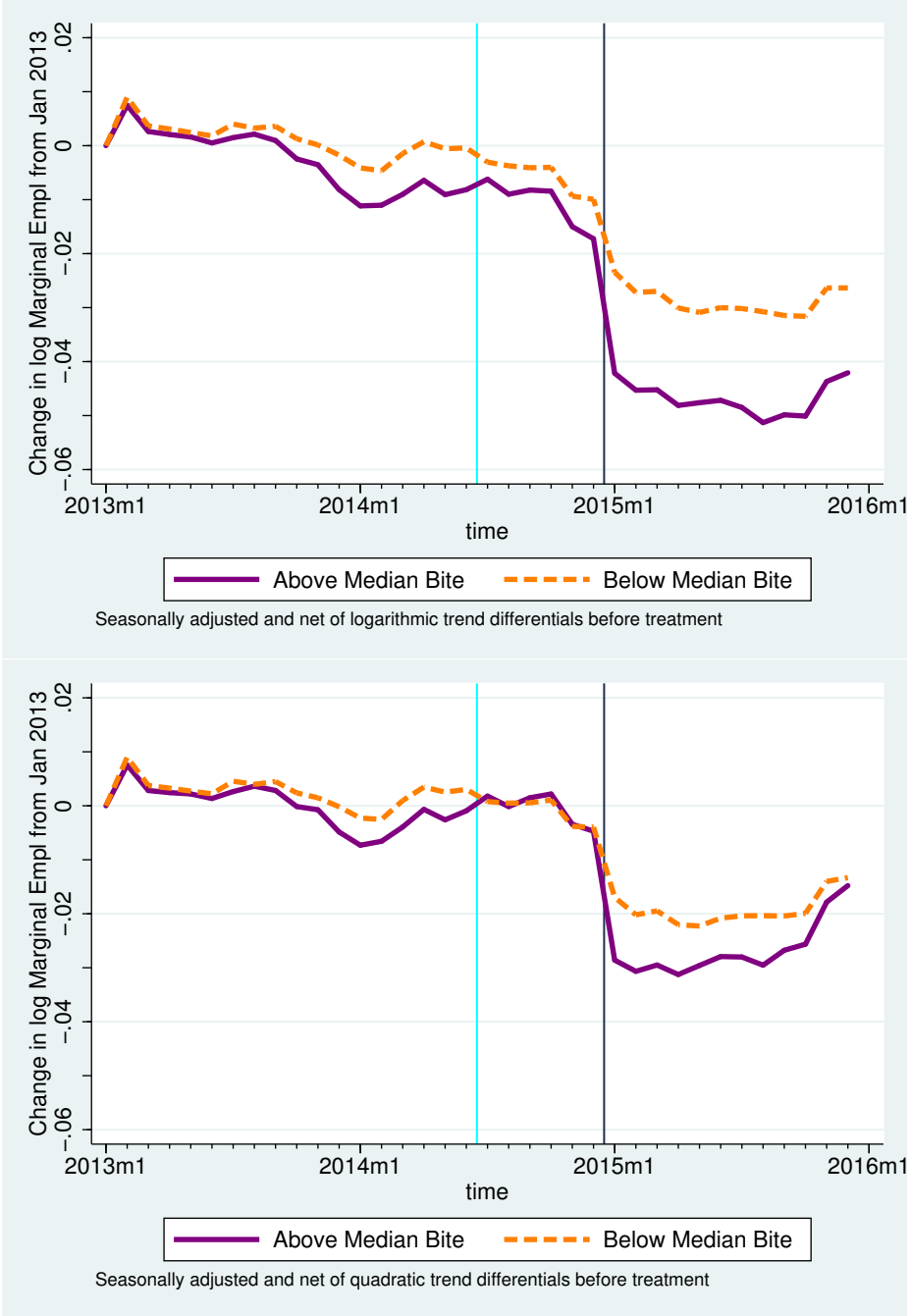


Figure A9: Marginal Employment - Further Trend Specifications

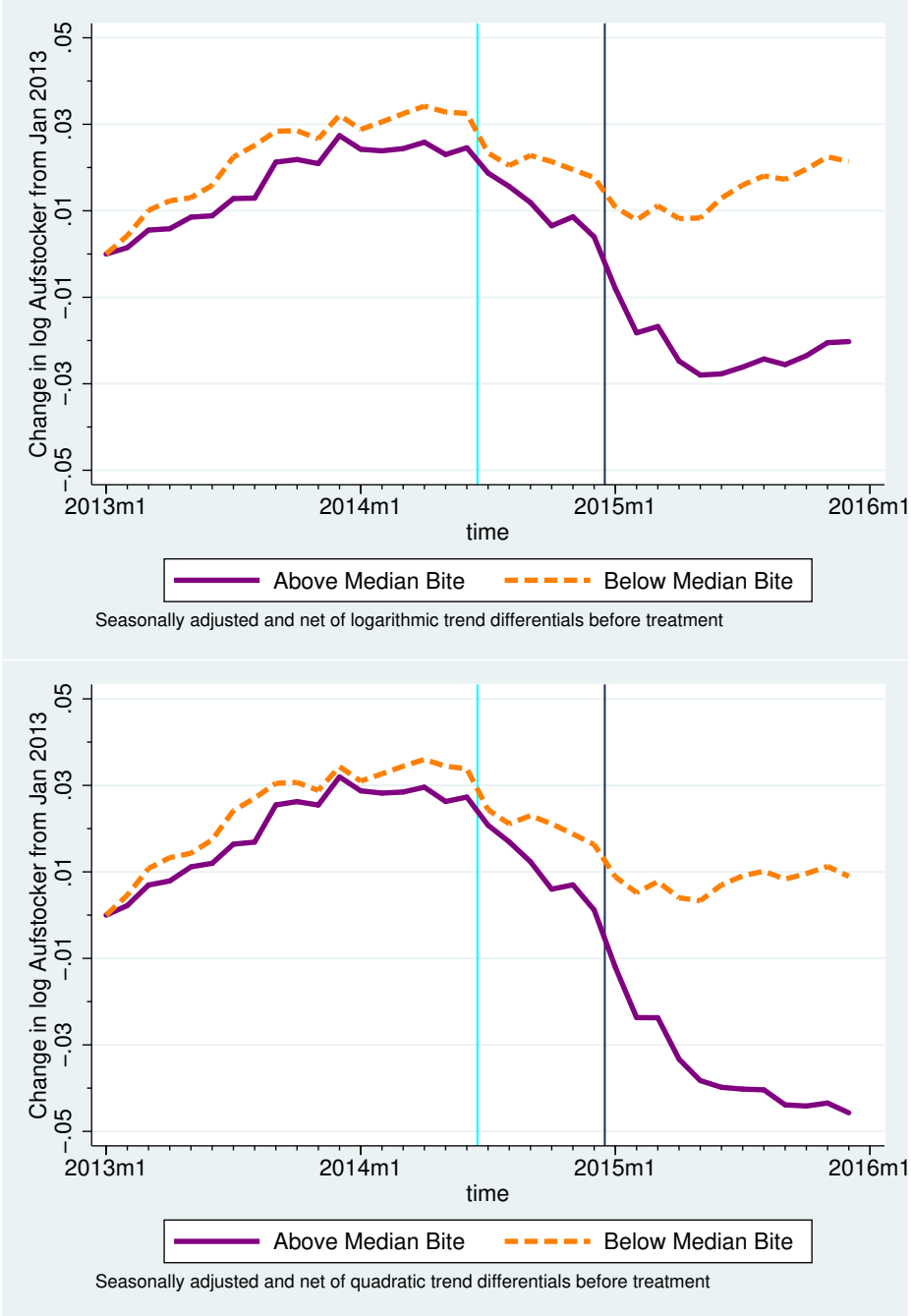


Figure A10: *Aufstocker* - Further Trend Specifications

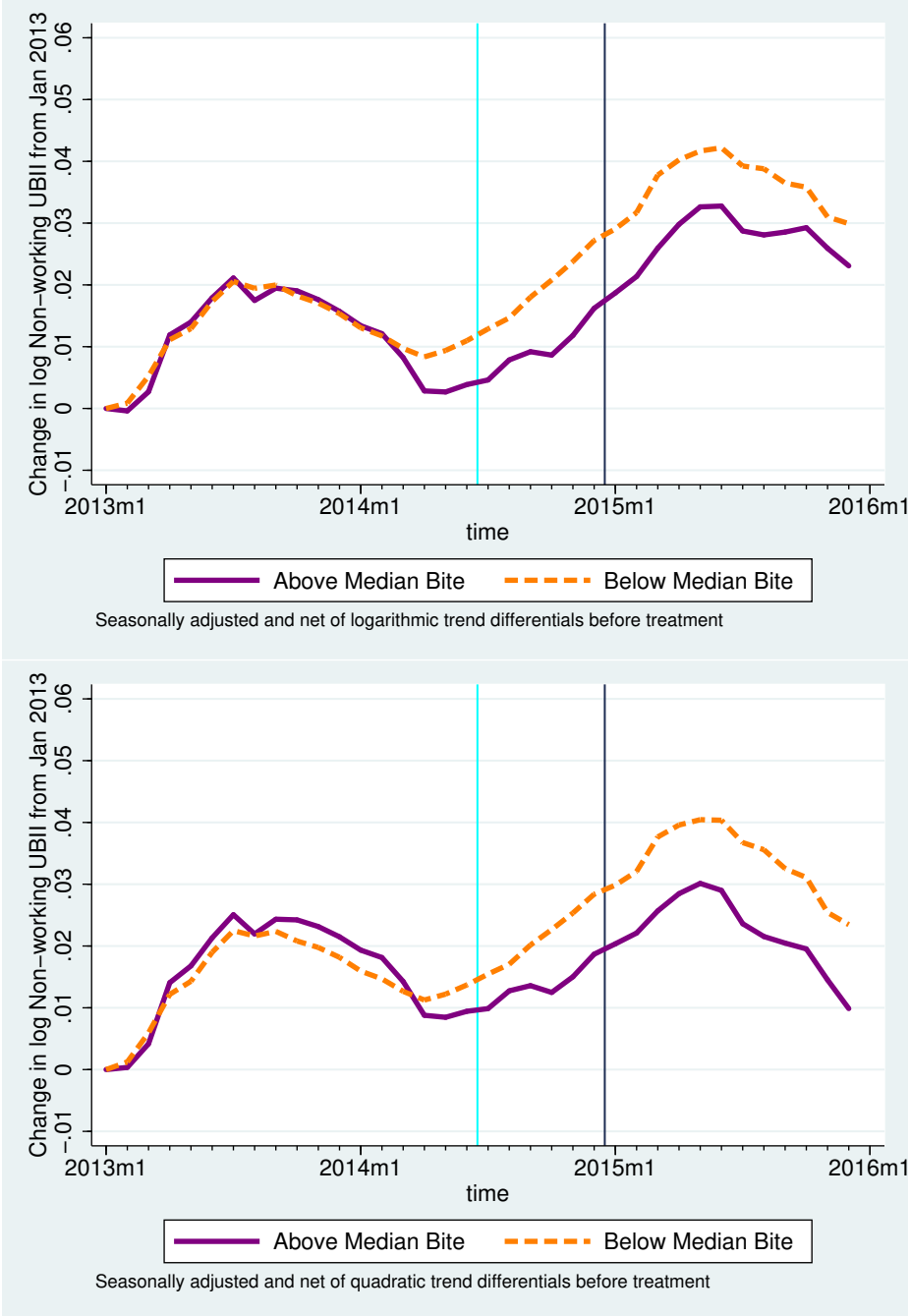


Figure A11: Non-working UBII - Further Trend Specifications

Table A1: Effects on Employment Outcomes - Levels Specification

	(1)	(2)	(3)	(4)
		Regular Employment		
Bite·2015	-0.826***	-0.186***	-0.222***	-0.519***
	(0.079)	(0.048)	(0.030)	(0.050)
$t \times$ Bite		-0.007***	-0.008***	
		(0.001)	(0.002)	
$t^2 \times$ Bite			0.000	
			(0.000)	
$\log(t) \times$ Bite				-0.078***
				(0.012)
Observations	19296	19296	19296	19296
R2 within	0.707	0.718	0.718	0.715
R2 between	0.044	0.044	0.044	0.044
R2 overall	0.003	0.009	0.010	0.014
		Marginal Employment		
Bite·2015	-1.825***	-1.343***	-1.057***	-1.604***
	(0.265)	(0.195)	(0.171)	(0.208)
$t \times$ Bite		-0.006**	0.001	
		(0.003)	(0.004)	
$t^2 \times$ Bite			-0.000***	
			(0.000)	
$\log(t) \times$ Bite				-0.056*
				(0.029)
Observations	19296	19296	19296	19296
R2 within	0.291	0.294	0.295	0.293
R2 between	0.201	0.201	0.201	0.201
R2 overall	0.018	0.045	0.032	0.058

Bite measured in December 2013 and normalized by one SD

Estimation including Time and County Fixed Effects

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Effects on Welfare Outcomes - Levels Specification

	(1)	**(2)**	(3)	(4)
		Aufstocker		
Bite·2015	-6.347***	-2.699***	-1.452***	-4.662***
	(0.234)	(0.149)	(0.107)	(0.172)
$t \times$ Bite		-0.042***	-0.012**	
		(0.003)	(0.005)	
$t^2 \times$ Bite			-0.001***	
			(0.000)	
$\log(t) \times$ Bite				-0.433***
				(0.030)
Observations	19177	19177	19177	19177
R2 within	0.498	0.560	0.564	0.540
R2 between	0.071	0.071	0.071	0.071
R2 overall	0.004	0.027	0.021	0.037
		Non-working UBII		
Bite·2015	-4.069***	-0.650***	0.292***	-2.460***
	(0.203)	(0.113)	(0.096)	(0.145)
$t \times$ Bite		-0.040***	-0.016***	
		(0.002)	(0.003)	
$t^2 \times$ Bite			-0.001***	
			(0.000)	
$\log(t) \times$ Bite				-0.413***
				(0.023)
Observations	19177	19177	19177	19177
R2 within	0.410	0.504	0.507	0.476
R2 between	0.044	0.044	0.044	0.044
R2 overall	0.002	0.020	0.016	0.027

Bite measured in December 2013 and normalized by one SD

Estimation including Time and County Fixed Effects

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Anticipation and Adjustment Effects

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
Bite·07/2014	0.025	-0.145	0.218	0.003
	(0.041)	(0.153)	(0.137)	(0.090)
Bite·08/2014	-0.044	-0.242	0.370*	0.233**
	(0.044)	(0.153)	(0.195)	(0.111)
Bite·09/2014	-0.055	-0.212	0.229	0.196
	(0.047)	(0.155)	(0.230)	(0.130)
Bite·10/2014	-0.119**	-0.235	0.123	0.113
	(0.048)	(0.159)	(0.244)	(0.151)
Bite·11/2014	-0.111**	-0.395*	0.056	0.181
	(0.049)	(0.210)	(0.273)	(0.170)
Bite·12/2014	-0.105**	-0.525**	-0.067	0.293
	(0.052)	(0.215)	(0.281)	(0.186)
Bite·01/2015	-0.065	-1.484***	-0.462	0.340*
	(0.054)	(0.235)	(0.320)	(0.198)
Bite·02/2015	-0.042	-1.479***	-1.040***	0.409*
	(0.057)	(0.241)	(0.343)	(0.218)
Bite·03/2015	-0.077	-1.510***	-1.223***	0.320
	(0.060)	(0.245)	(0.381)	(0.232)
Bite·04/2015	-0.119**	-1.501***	-1.530***	0.460*
	(0.058)	(0.252)	(0.407)	(0.240)
Bite·05/2015	-0.154**	-1.430***	-1.636***	0.579**
	(0.062)	(0.257)	(0.430)	(0.255)
Bite·06/2015	-0.149**	-1.397***	-1.832***	0.607**
	(0.062)	(0.256)	(0.437)	(0.265)
Bite·HY2/2015	-0.126**	-1.398***	-1.664***	0.702**
	(0.058)	(0.223)	(0.474)	(0.291)
Observations	14472	14472	14298	14298
R2 within	0.172	0.308	0.252	0.121
R2 between	0.177	0.110	0.533	0.446
R2 overall	0.105	0.252	0.382	0.297
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Placebo

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
Bite· 2014	0.072	-0.329***	0.222	0.084
	(0.049)	(0.125)	(0.146)	(0.085)
Observations	9648	9648	9533	9533
R2 within	0.092	0.039	0.078	0.109
R2 between	0.128	0.012	0.322	0.356
R2 overall	0.021	0.028	0.197	0.253
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Variation within Gender

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
			Men	
Bite·2015	0.013	-1.184***	-0.858**	0.050
	(0.078)	(0.293)	(0.334)	(0.195)
Observations	14472	14472	14298	14298
R2 within	0.116	0.258	0.103	0.067
R2 between	0.103	0.121	0.394	0.393
R2 overall	0.060	0.214	0.189	0.160
			Women	
Bite·2015	-0.050	-1.301***	-1.418***	0.388***
	(0.051)	(0.181)	(0.230)	(0.140)
Observations	14472	14472	14297	14297
R2 within	0.203	0.277	0.198	0.055
R2 between	0.104	0.085	0.426	0.347
R2 overall	0.104	0.220	0.288	0.198
			Gender Cells	
Bite·2015	0.035	-0.968***	-1.608***	-0.445***
	(0.044)	(0.166)	(0.211)	(0.122)
Observations	28944	28944	28595	28595
R2 within	0.132	0.258	0.107	0.053
R2 between	0.017	0.200	0.211	0.274
R2 overall	0.062	0.211	0.140	0.126
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Binary Treatment

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
		Binary Treatment		
Treated·2015	0.022	-1.320***	-1.398***	0.117
	(0.101)	(0.383)	(0.492)	(0.349)
Observations	14472	14472	14298	14298
R2 within	0.168	0.276	0.177	0.107
R2 between	0.050	0.032	0.235	0.173
R2 overall	0.080	0.207	0.204	0.142
		Robust Binary Treatment		
Treated·2015	0.037	-1.932***	-1.711***	-0.346
	(0.117)	(0.392)	(0.549)	(0.400)
Observations	11592	11592	11455	11455
R2 within	0.156	0.313	0.219	0.112
R2 between	0.072	0.084	0.289	0.239
R2 overall	0.072	0.247	0.253	0.177
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Alternative Bite Definitions

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
			Bite 1500	
Bite·2015	-0.098*	-1.371***	-1.463***	0.334**
	(0.050)	(0.219)	(0.206)	(0.138)
Observations	14472	14472	14298	14298
R2 within	0.171	0.307	0.249	0.121
R2 between	0.182	0.114	0.546	0.456
R2 overall	0.097	0.251	0.388	0.301
			Bite 2000	
Bite·2015	-0.126**	-1.381***	-1.495***	0.343**
	(0.050)	(0.253)	(0.202)	(0.137)
Observations	14472	14472	14298	14298
R2 within	0.172	0.308	0.247	0.123
R2 between	0.168	0.102	0.571	0.479
R2 overall	0.102	0.249	0.397	0.314
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Labor Market Regions

	Employment		**Welfare**	
	Regular	Marginal	*Aufstocker*	NW UBII
	(1)	(2)	(3)	(4)
Bite·2015	-0.044	-1.799***	-1.689***	0.211
	(0.097)	(0.457)	(0.253)	(0.225)
Observations	5076	5076	5033	5033
R2 within	0.191	0.283	0.316	0.205
R2 between	0.122	0.141	0.458	0.355
R2 overall	0.106	0.239	0.391	0.278
Linear Trend	-	-	✓	✓
Time Fixed Effects	✓	✓	✓	✓
Labor Market Region FE	✓	✓	✓	✓

Bite measured in December 2013 and normalized by one SD

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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