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DISCUSSION PAPER SERIES

IZA DP No. 15534

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SEPTEMBER 2022

ISSN: 2365-9793

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ABSTRACT

Using Distribution Regression Difference-In-Differences to Evaluate the Effects of a Minimum Wage Introduction on the Distribution of Hourly Wages and Hours Worked[1]

This paper evaluates the effects of the newly introduced German minimum wage on the distribution of hourly wages and hours worked. The study is based on the German Structure of Earnings Survey (GSES), the only large scale data set for Germany that includes information on hourly wages and hours worked. We provide a full distributional analysis based on counterfactual distributions that would have prevailed, had the minimum wage not been introduced. Our results suggest that its introduction almost eliminated wage rates below its threshold and, depending on the specification considered, led to spill-over effects up to 20 percent above it. We show that inequality in hourly wages fell between 2014 and 2018, but that the long-term trend of rising inequality would already have been stopped after 2014 without the minimum wage. We demonstrate that the existence of pre-trends leads to an upward bias for the estimation of the minimum wage effect. We do not find any significant shifts in the distribution of weekly working hours. As a methodological contribution, we provide a transparent treatment of distribution regression difference-in-differences (DR DiD) based on bite measures.

JEL Classification: D31, J31, J38

Keywords: minimum wage, distribution regression, difference-in-differences, inequality

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[1] We are grateful to Ronald Bachmann, Fabien Postel-Vinay, Regina Riphahn, Christoph M. Schmidt, Uta Schönberg, Stephan Thomsen, Christina Vonnahme, as well as seminar participants at the RWI Essen, the 6th international conference of the DFG Priority Programme 1764, ESPE 2021, IAAE 2021, the Annual Meeting of the German Economic Society 2021 and EEA-ESEM 2022 for many valuable comments and suggestions. Financial support through the DFG Priority Program 1764 and DFG projects BI 767/3-1 and FI 692/16-1 is gratefully acknowledged. This paper uses the German Structure of Earnings Survey (GSES) 2014 and 2018 provided by the German Federal Statistical Office as well as administrative social insurance data provided by the Institute of Employment Research (IAB). We thank Stefan Seitz, Kerstin Stockmayer (FDZ Stuttgart) and Miriam Schirmer (IAB) for their generous support. We also thank Maximiliane Sick for valuable research assistance.

1 Introduction

Against the backdrop of a stark increase in wage inequality from the mid-1990s onwards (Dustmann et al., 2009; Antonczyk et al., 2010; Card et al., 2013; Biewen and Seckler, 2019), a national statutory minimum wage was introduced in Germany in 2015. Reducing wage inequality and helping the 'working poor' were the main arguments put forward in favor of a general, economy-wide minimum wage (Bundesministerium für Arbeit und Soziales, 2014). Even though there existed a number of sector-specific minimum wages before (Fitzenberger and Doerr, 2016), Germany was one of the few countries without a national minimum wage in the years prior to 2015. The introduction of a general minimum wage in 2015 (at the level of 8.50 euros per hour) implied a considerable 'bite': nationwide, around 4 million workers (or roughly 11% of the workforce) were eligible for it (Mindestlohnkommission, 2020).[2]

Although there have been a number of recent contributions addressing various aspects of the German minimum wage (Caliendo et al., 2018, 2019; Burauel et al., 2019; Dustmann et al., 2022; Bossler and Schank, 2022, and literature review below), it is an open question to what extent the introduction of the minimum wage causally changed the quantity at which it is targeted, the distribution of hourly wage rates. The main aim of a minimum wage is to shift distributional mass from below its threshold to points above it, but because of potential non-compliance and spillover effects, it is an empirical question to what extent this is accomplished. A further challenge is to separate the causal effects of the minimum wage from changes in the pay structure that would have happened anyway, i.e., from trends in wage setting policies of employers, workers and trade-unions which may have been visible before the minimum wage introduction.

Studying the effects of the minimum wage introduction on the distribution of hourly wages faces a number of challenges. First, precise information on hourly wages and hours worked is often hard to obtain. Perhaps surprisingly, this kind of information is particularly scarce in Germany. The well-known administrative data bases provided by the Institute of Employment Research (IAB) cover, by construction, monthly earnings which are used to calculate social security contributions. On the other hand, information on hourly wages and working hours in survey data such as the *German Socio-Economic Panel (GSOEP)* suffer from relatively small sample size and potentially large

[2]See Caliendo et al. (2019) for a comprehensive overview of research on the German minimum wage and its institutional details.

measurement error in self-reported wages and working hours possibly leading to the estimation of noisy and attenuated minimum wage effects (Autor et al., 2016). Second, popular tools for distributional analysis such as RIF-regressions (Firpo et al., 2009, 2018) may be unsuitable for the evaluation of changes in the distribution of hourly wages as the minimum wage introduction is targeted at nominal rather than at relative wage levels (quantiles) and introduces discrete mass points which may pose a problem for methods based on continuous distributions.

This study aims at the following contributions. First, while there is a considerable number of contributions on various aspects of the German minimum wage (see literature review below), this is the first study to make use of the scarce information on hourly wages and working hours in Germany from administrative and large-scale data bases. We use an innovative two-sample strategy to combine data from the *German Structure of Earnings Survey (GSES)* – which is the only German large scale data base that includes information on hourly wages and working hours for periods after the minimum wage introduction – and from the administrative *Deutsche Gesetzliche Unfallversicherung (DGUV-IAB)* data base, which includes information on wages and working hours, but only for a number of years before the minimum wage introduction. Both data bases are considered highly reliable as firms participation is compulsory, and as wage and hours information are in most cases derived from firms' internal accounting systems.[3] We show how to combine the two data bases GSES and DGUV-IAB for a difference-in-differences analysis without physically combining them (which would be forbidden under German data protection laws). Apart from its effects on the hourly wage, we study the potential effects of the minimum wage on the distribution of hours worked, which may be another important consequence of a minimum wage introduction. In particular, firms may reduce working hours for low-wage employees to keep overall wage bills constant (Stewart and Swaffield, 2008; Bossler and Schank, 2022), or there may be shifts between part-time and full-time employment as a reaction to changed constraints on low-wage employment (Garloff, 2019).

As a second contribution, we explore the use of a distribution regression difference-in-differences

[3] The DGUV-IAB data were also used by Dustmann et al. (2022). Administrative IAB data on monthly earnings were used to study minimum wage effects by Bossler and Schank (2022). Caliendo et al. (2018), Burauel et al. (2019), Burauel et al. (2020) studied minimum wage effects based on survey data from the German Socio-Economic Panel (GSOEP). Caliendo and Wittbrodt (2021) also use data from the most recent 2018 wave of the GSES but their focus is quite different from ours (they investigate the effects of the minimum wage on the regional gender gap).

strategy (DR-DiD). A small number of previous contributions have carried out calculations analogous to the ones we present below (Almond et al., 2011; Dube, 2019b; Cengiz et al., 2019), but we have not seen a full statement of the approach along with its identifying assumptions. In particular, we show that viewing the problem as a distribution regression naturally leads to an identification condition for distributional treatment effects recently shown by Roth and Sant'Anna (2021) to be equivalent to a parallel-trends assumption being independent of the functional form of the outcome variable. The distribution regression approach (DR, Chernozhukov et al., 2013) appears particularly suited to study effects of the minimum wage on the distribution of hourly wages and hours worked as it can easily deal with discrete distributions and distributions with discrete mass points. We offer a full distributional analysis of the effects of the German minimum wage in the sense that we construct counterfactual wage structures and hours distributions that would have prevailed, had the minimum wage not been introduced. Among other things, we can estimate in this way by how much inequality in hourly wages as measured by, say, the Gini coefficient was reduced by the introduction of the minimum wage. By contrast, most previous contributions focused either on effects on the mean or on particular points of the distribution (e.g., Caliendo et al., 2017; Bossler and Gerner, 2020; Burauel et al., 2019; Dustmann et al., 2022). An exception is the study by Bossler and Schank (2022) who also conduct a full distributional analysis but for monthly earnings and based on an alternative methodology (RIF-regressions). As a final contribution, we present a comparison of alternative bite measures (based on regions, occupations and industries, respectively) which adds to the robustness of our findings.

The rest of this paper is structured as follows. Section 2 provides a brief review of related literature. In section 3 and 4, we describe our data and econometric method. Section 5 presents empirical results, while section 6 concludes.

2 Related literature

The literature on the effects of minimum wages is huge (e.g., Neumark and Wascher, 2008). In the following, we provide a selective review of contributions dealing specifically with minimum wage effects on the wage distribution, wage inequality and hours worked.

A seminal contribution aimed at distributional effects of minimum wages is DiNardo et al. (1996).

They used a 'tail-pasting' approach to construct counterfactual wage distributions in the absence of the minimum wage for the US from 1973 to 1992. The 'tail-pasting' approach rules out spill-over effects of the minimum wage, which were found in an important contribution by Lee (1999). Lee (1999) exploited between-states variability in the minimum wage 'bite' in order to describe its effects on wage levels far above its threshold. His findings were later challenged by Autor et al. (2016) who used an instrumental variables approach to suggest that the spill-over effects found by Lee (1999) might be 'measurement artifacts' stemming from imprecise wage and hours data. More recently, Cengiz et al. (2019) studied the impact of minimum wage changes on the wage distribution in the US. They find that minimum wage increases boosted average earnings in low-wage jobs which was amplified by modest spill-over effects. Using the same method as Cengiz et al. (2019), Cribb et al. (2021) find that the introduction and subsequent increases of the UK National Living Wage from 2016 to 2019 led to substantial wage effects for workers at the lower tail of the distribution. Beyond this, the policy led to substantial spill-over effects up to the 20th percentile, while no significant effects on employment were found. Based on reliable administrative payroll data, Gopalan et al. (2022) also find spill-over effects up to 2.50 dollars above the minimum wage level accruing to incumbent as well as to newly hired workers, but only in firms with a significant fraction of low-wage workers. In an update to their DiNardo et al. (1996) contribution, Fortin et al. (2021) explicitly allow for spill-over effects. They find significant evidence for spill-over effects and show that allowing for spill-over effects substantially increases the contribution of minimum wage effects on changes in the wage distribution.

A number of previous contributions deal with the effects of the German minimum wage. An important general finding is that the introduction of the minimum wage did not have significant employment effects (Caliendo et al., 2019; Dustmann et al., 2022; Bossler and Schank, 2022). As to the wage effects of the minimum wage introduction, Caliendo et al. (2017) conduct a difference-in-differences analysis based on data from the German-Socio Economic Panel (GSOEP) exploiting a regional bite measure. They find positive wage effects for the bottom hourly wage quintile but no effects on monthly earnings. Burauel et al. (2019) present further evidence based on the GSOEP suggesting excess hourly wage growth for low-wage workers. Based on adminstrative data, Dustmann et al. (2022) investigate wage, employment, and reallocation effects of the German minimum wage. They find that the minimum wage raised wages but did not reduce employment. It also implied reallocation effects to better paying firms accounting for around 17% of the wage increases. Caliendo et al. (2017), Burauel et al. (2019) and Dustmann et al.

(2022) focus on particular wage groups but do not provide a full distributional analysis aimed at measuring the impact of the minimum wage on the overall wage structure and wage inequality. Also based on administrative IAB data and using a Recentered-Influence-Function (RIF) approach, Bossler and Schank (2022) provide such a full distributional analysis but for the distribution of monthly earnings. By contrast, our study focusses on the direct effect of the minimum wage on the pay structure as represented by hourly wage rates.

A smaller number of studies has focussed on the potential effects of the minimum wage on working hours. For example, Neumark et al. (2004) found that the minimum wage reduces hours worked for those paid at the minimum wage level with an elasticity of -0.3, but no effects for workers receiving wages above the minimum wage. Stewart and Swaffield (2008) examined the effect of the British minimum wage on working hours and found a small total effect (including immediate as well as lagged effects) on weekly hours amounting to one to two hours per week. Dube (2019a) also found a small negative effect on working hours due to the introduction of the 2016 national living wage in the UK.

For Germany, Bonin et al. (2018) present a difference-in-differences analysis of working hours for workers affected by the minimum wage based on the German Socio-Economic Panel. They conclude that contractual working hours fell by 4.5 percent relative to the control group of workers unaffected by the minimum wage in the first two years after the minimum wage introduction but that the fall of actual working hours was smaller and statistically insignificant. Based on a similar difference-in-differences approach, Burauel et al. (2020) find a significant decline in contractual working hours relative to unaffected workers but smaller and statistically insignificant effects on actual hours. Bachmann et al. (2020) present a comprehensive study of wage and hours effects of the minimum wage up to the year 2017 based on survey data (apart from the GSOEP, they exploit the so-called *Verdiensterhebung (VE)* which is similar in structure to the GSES but smaller and without compulsory participation). They conclude that there was a decline in hours in the year after the minimum wage introduction but find evidence that it was reversed later. Similarly, Bossler and Gerner (2020) exploit firm panel data to study, among other things, firms' behavioral responses to the introduction of the minimum wage. They find that firms reduced average working hours at the establishment level by 0.15 hours one year after its introduction (representing a 0.4 percent decrease in contractual working hours), but there were no significant shifts two years after its introduction. Taken together, the existing evidence on the effects of the German minimum wage on working hours is quite mixed, based on relatively small survey data and concentrates on the short-term effects in the first years after the introduction.

3 Data

The main part of our analysis is based on the *German Structure of Earnings Survey* (GSES) for the years 2014 (before the minimum wage introduction) and 2018 (after the minimum wage introduction). As mentioned above, the GSES is the only large-scale data base for Germany that includes information on hours worked and thus hourly wages after the introduction of the minimum wage. The fact that the GSES is only carried out every four years makes an analysis of pre-trends difficult, especially given that there were major changes in the GSES sample design between 2014 and the preceding wave 2010. However, a pre-trend analysis is a necessary requirement to any credible DiD analysis. We resort for this purpose to a specific administrative data base from the *German Social Accident Insurance (DGUV)* whose working hours information can be linked to IAB data on employment histories, but only for the years 2011 to 2014.[4]

3.1 The German Structure of Earnings Survey (GSES)

We exploit the two most recent minimally anonymized waves of the GSES (2014 and 2018), which are only available on-site at the German statistical offices (see Forschungsdatenzentrum der Statistischen Ämter des Bundes und der Länder, 2019). The GSES is a linked employer-employee dataset in which firms are legally obliged to participate and whose results are used for official statistical purposes. This ensures extremely low non-response rates of 2.3% in 2014 (Statistisches Bundesamt, 2016) and 3.2% in 2018 (Statistisches Bundesamt, 2020). The data included in the GSES can be considered highly accurate as most of them stem from firms' internal accounting systems which are transmitted electronically to the statistical agencies in the course of the survey. The GSES follows a two-stage sampling design. In the first stage, the statistical agencies draw from the full population of German firms (as listed in the official business registers). The second stage comprises the employees reported by a given firm, where the number of employees a firm has to report depends on the number of workers they employ.

[4]This data base is not publicly available and was provided to us by the Institute for Employment Research (IAB). It was also used in the study by Dustmann et al. (2022).

Sample weights ensuring the representativness of the survey for the German dependent worker population are used by us throughout the analysis.

We impose a number of sample selection restrictions in order to address eligibility rules for the minimum wage as well as data limitations such as the missing regional information for particular groups of individuals (see supplementary appendix for details). Enforcing these sample selection restrictions yields our working sample covering 708,081 worker observations from 55,579 firms in 2014 and 693,827 worker observations from 55,722 firms in 2018, respectively.

3.2 Variables

Our wage information is monthly gross earnings including overtime remuneration. Our data on hours worked refer to individuals' regular weekly working hours in the reporting month, including overtime hours. In GSES waves prior to 2014, the reporting month was October, but it was moved to April from the 2014 wave onwards to rule out anticipation effects of the newly introduced minimum wage. We follow the convention of transforming weekly working hours into monthly working hours by multiplying the former by the factor 4.345. The hourly wage measure is computed by dividing monthly gross earnings including remuneration for overtime hours by monthly hours worked including overtime hours. We do not adjust hourly wages by inflation as the minimum wage is likely to have an effect around its nominal level whose estimation would be blurred if inflation factors were taken into account.

As individual characteristics, we consider sex, age, education, tenure, occupational position and occupation (KldB10, 2 digits). At the firm level, we include information on the federal state, individual information on remuneration according to collective agreements, firm size, whether the firm was associated to the public sector, industry (WZ08, Statistisches Bundesamt, 2008), as well as an indicator whether the firm was covered by a sectoral minimum wage (such sectors existed before the general minimum wage was introduced and continued to exist afterwards). The large size of our data set allows us to include all of this information in a very detailed way in our main analysis (see table A.1).

3.3 Bite measures

Within our difference-in-differences approach, we rely on 'bite' measures reflecting the extent to which the minimum wage was going to affect certain population subgroups from the perspective of the pre-policy period. The seminal work by Card (1992) paved the way for a large body of contributions exploiting the bite measure derived from regional or other characteristics. The minimum wage bite in a particular population subgroup is defined as the fraction of individuals in this group with hourly wages below the minimum wage level before its introduction. This continuous group-level variation can be used to identify the effect of the minimum wage as wage adjustments are expected to be the stronger, the more workers in the respective group were below the minimum level before it was enacted. As our post-policy period is 2018, we set the relevant minimum wage level to 8.84 euros per hour (on 1 January 2017, the minimum wage was increased from the original level of 8.50 to 8.84 euros per hour). A particular feature of our study is that we use the following alternative bite measures which we compute in the pre-reform 2014 wave of our data base.

Local labor markets A bite definition which has been used extensively in the literature is based on the relative impact of the minimum wage in different local labor markets. We use a definition of 96 German regions ('*Raumordnungsregionen*') as described in Bundesinstitut für Bau-, Stadt- und Raumforschung (2019). Figure 1 provides an overview of minimum wage bites across regions.

Augmented occupations An alternative bite measure can be defined at the level of the occupations (e.g., Friedrich, 2020). Given the obvious importance of East-West differences, we augment the categorization according to 2-digit occupation codes (KldB10) by the information of whether the person worked in East or in West Germany. This yields a total number of 72 different groups. Defining bite measures at the occupation level is appealing because of anecdotic evidence on pay shifts in certain professions following the introduction of the minimum wage (hairdressers, cleaners, waiters etc.). Pursuing this strategy follows the intuitive approach of studying whether, and to what extend, wages changed differentially in occupations that were affected to a higher or lesser extent by the minimum wage introduction.

Augmented industries In a similar way, we define a bite measure for differences in the exposure to the new minimum wage across finely defined industries (WZ08). As in the case of occupations, we augment this categorization by information on whether the given person worked in East or in West Germany. Our industry bite measure augmented with East/West information comprises 152 different groups.

An overview of our alternative bite measures is given in table 1. Interestingly, we observe considerably more variation in the bite definition based on augmented occupations or industries compared to that across regions. This suggests potential gains in statistical precision for the impact estimates of the minimum wage on wages and hours worked. Using alternative bite definitions is also a way to accomodate alternative spill-over mechanisms at the regional, occupational or industry level. At the same time, the use of alternative bite definitions ensures that results do not depend on the specific properties of the characteristic on which a given bite definition is based thus capturing the common component of the minimum wage introduction rather than idiosyncratic developments of the variables used to define the bite.

— Table 1 around here —

3.4 Supplementary data base for pre-trend analysis

Due to exceptional circumstances, the working hours information typically recorded by the *German Social Accident Insurance (DGUV)* can be linked to administrative employment data (Beschäftigenhistorik, BeH) provided by the Institute for Employment Research (IAB) for the years 2011 to 2014. We use a 3.75 % sample of the BeH that was augmented with this working hours information for our pre-trend analysis. With some exceptions (see supplementary appendix), the DGUV-IAB data include the same covariate information as we use in the GSES. After applying the same sample selection criteria as in the GSES, our DGUV-IAB working sample covers 642,738 worker observations in 2011, 817,770 worker observations in 2012, 824,770 worker observations in 2013, and 831,304 worker observations in 2014, respectively. The use of the DGUV-IAB and its working hours information requires some pre-processing steps (see Dustmann et al., 2022; Vom Berge et al., 2014, and supplementary appendix). As the wage data in the administrative employment data is censored, we only consider hourly wages up to a threshold of around 30 euros per hour for these data.

4 Econometric methods

Our aim is a full distributional analysis of the effects of the minimum wage introduction on the distribution of hourly wages and hours worked. A possibility would be to use recentered influence functions (Firpo et al., 2009, 2018) in combination with a difference-in-differences setup (DiD). A small number of previous contributions have used such a RIF-DiD approach, see Havnes and Mogstad (2015), Dube (2019b) and Bossler and Schank (2022). However, we think that – in contrast to the applications pursued in these contributions – a RIF-DiD approach would be ill-suited for an analysis of minimum wage effects on the *hourly wage structure* as the introduction of a minimum wage is likely to introduce discrete mass points around its threshold which is in conflict with the assumption of continuous distributions underlying the RIF approach.[5] Moreover, the RIF approach is most easily applied to quantities such as quantiles and quantile ratios rather than to an analysis of changes in *nominal wage levels* at which the minimum wage is targeted. The same arguments apply to the distribution of weekly working hours which is known to be highly discontinuous.

In order to address these aspects, we explore in this study the use of a distribution regression difference-in-differences approach (DR-DiD). The distribution regression approach (DR) as fully developed by Chernozhukov et al. (2013) models effects on conditional and unconditional cumulative distribution functions by applying binary regressions to a range of thresholds of an outcome. A small number of previous contributions have carried out calculations analogous to the ones we present below (Almond et al., 2011; Dube, 2019b; Cengiz et al., 2019), but we have not seen a full statement of the approach along with an identification analysis. In particular, we show in appendix A that viewing the problem as a distribution regression and applying standard difference-in-differences assumptions to all thresholds naturally leads to an identification condition for distributional treatment effects recently identified by Roth and Sant'Anna (2021). Roth and Sant'Anna (2021) derived this condition as a characterization of the assumption that the parallel trends assumption on the outcome is insensitive to functional form. The statement of the problem as a distribution regression also directly opens up possibilities for pre-trend analysis and correction which we consider in section 5.4. In the appendix, we provide a discussion of

[5] By contrast, Dube (2019b) consider minimum wage effects on the distribution *family incomes*, while Bossler and Schank (2022) focus on the distribution of *monthly earnings*. Both distributions are close to continuous as minimum wage earners are spread over wide regions in these distributions.

further differences between the RIF-DiD and the DR-DiD approach, which may be of interest to practitioners who wonder which method is best suited in their own applications.

4.1 Distribution regression difference-in-differences

We estimate the causal effect of the minimum wage using the continuous treatment measure $Bite_g$ (the minimum wage bite in group g) by estimating a large set of linear probability models for the cumulative distribution function (cdf) of the variable of interest based on the DiD model

$$\begin{aligned} P(y_{igt} \leq z|Bite_g, D_g, D_t, X_{igt}) &\equiv F(z|Bite_g, D_g, D_t, X_{igt}) \\ &= \alpha_z + D_g\gamma_z + \lambda_z D_t + \beta_z(Bite_g \times D_t) + X_{igt}\delta_z + (X_{igt} \times D_t)\eta_z, \end{aligned} \quad (1)$$

where y_{igt} represents the observed wage of individiual i in bite group g at time t (we explain everything in terms of hourly wages, analogous interpretations apply to hours worked). The values z refer to thresholds for either the distribution of hourly wages or hours worked. For the case of hourly wages, we define the set of thresholds as $z \in \mathcal{W} = \{3.49, 4.49, \dots, 49.49\}$ (euros per hour) leading to a set of wage bins $[0; 3.49), [3.50; 4.49), \dots, [48.50, 49.49)$ (see below). In this way, equation (1) describes the fraction of individuals with characteristics $(Bite_g, D_g, D_t, X_{igt})$ whose wage is below or equal to threshold z. For the case of weekly hours worked, we define eight thresholds $z \in \mathcal{H} = \{7, 12, 20, 25, 30, 35, 42, 51\}$ (hours per week) implying eight hours categories $[0; 7), [7, 12), \dots, [42; 51)$ that correspond to different forms of marginal part time, part-time and full-time working arrangements (see below).

The variable D_g is a vector of dummies indicating to which bite group g individual i belongs. The term $D_g\gamma_z$ controls for time-constant differences in the fraction of individuals with hourly wages below z between the different bite groups g. For example, if the bite is defined in terms of regions, we control in this term for the full set of regions. D_t indicates the pre-treatment ($t = 0$) and post-treatment period ($t = 1$), i.e. the term $\lambda_z D_t$ represents differences between periods 1 and 0 that are common to all individuals. Finally, we control for observed characteristics X_{igt} which may affect productivity or hours choices and whose impact may be different in periods 1 and 0.[6] The parameters in (1) are estimated by weighted least squares using the sample weights.

We assume an approximately linear impact of $Bite_g$ on the cdf of y_{igt}, i.e. β_z describes by how much the fraction of individuals below z was higher or lower in the treatment period $t = 1$ per unit of $Bite_g$ after controlling for all other observables characteristics. It is the part of changes that can solely be attributed to the degree of exposure to the newly introduced minimum wage but not to other determinants. The case $Bite_g = 0$ corresponds to the counterfactual situation with no minimum wage exposure. Consequently, the fraction of wages below z in period 1 in the absence of the minimum wage is given by

$$F(z|Bite_g = 0, D_g, D_t = 1, X_{igt}) = F(z|Bite_g, D_g, D_t = 1, X_{igt}) - \beta_z Bite_g, \tag{2}$$

i.e. effects on the fraction of wages below threshold z solely due to the minimum wage are subtracted.

Identification of this minimum wage effect is achieved under the assumption that $Bite_g$ is unrelated to factors influencing the wage distribution that are not captured by (D_g, D_t, X_{igt}). In particular, there must not be differential time trends between groups g not captured by X_{igt}. This has to hold at each threshold z of the wage distribution. In section 5.4, we investigate potential violations of this assumption in periods before the minimum wage introduction and use these observations to correct for pre-trends by augmenting (1) with a trend component estimated in the pre-period.

By the law of iterated expectations, the unconditional *factual* wage distribution in target year $t = 1$ is given by

$$F(z \mid D_t = 1) = \int F(z \mid Bite_g, D_g, D_t = 1, X_{igt})\; dF(Bite_g, D_g, X_{igt} \mid D_t = 1). \tag{3}$$

By contrast, the unconditional *counterfactual* wage distribution in the absence of minimum wage

[6] We account for various characteristics in the two periods separately in order to avoid an omitted variable bias when estimating the minimum wage effect. The characteristics considered are basically those shown in table A.1 in the appendix. In order to save degrees of freedom, we do not interact all characteristics with the time period. Naturally, for a given bite specification, the characteristic on which it is based (region/occupation/industry) is not included in X_{igt} due to collinearity. Furthermore, given potential re-allocation effects of the minimum wage as investigated by Dustmann et al. (2022), we omitted in an alternative specification all firm characteristics shown in table A.1 from our distribution regressions. This did not change our results in any substantial way.

effects is given by

$$F^{cf}(z \mid D_t = 1) = \int \left[F(z \mid Bite_g, D_g, D_t = 1, X_{igt}) - \beta_z Bite_g\right] \, dF(Bite_g, D_g, X_{igt} \mid D_t = 1). \tag{4}$$

We show in appendix A how (4) is identified in repeated cross-sections under the assumption that standard parallel trends assumptions conditional on observables hold at each threshold z. This leads to an identification condition identical to that in Roth and Sant'Anna (2021). Roth and Sant'Anna (2021) show that this condition is equivalent to assuming that a parallel trends assumption on the outcome is insensitive to functional form of the outcome. As we argue in appendix A, conditioning on a large number of observables and carefully addressing potential time effects (including those constructed from trends observed in pre-periods) renders the parallel trends assumption credible and secures the identification of the counterfactual distribution (4).

As cumulative distribution functions are more involved to interpret and in order to calculate inequality measures, we construct grouped probability functions based on the increments across the set of thresholds $z \in \{z_0, z_1, \dots, z_J\}$

$$f_{j,t} = F(z_j | D_t) - F(z_{j-1} | D_t), \tag{5}$$

$$f_{j,1}^{cf} = F^{cf}(z_j \mid D_t = 1) - F^{cf}(z_{j-1} \mid D_t = 1). \tag{6}$$

We use the following interpolation formulas for grouped data in order to calculate inequality measures and quantiles (Tillé and Langel, 2012). The formula for the quantiles is

$$Q_t(\tau) = z_j + \frac{\tau - F(z_{j-1} \mid D_t)}{f_{j,t}} \, (z_j - z_{j-1}), \tag{7}$$

for τ such that $F(z_{j-1} \mid D_t) \leq \tau < F(z_j \mid D_t)$ and $t \in \{0, 1\}$. The one for the Gini coefficient is given by

$$Gini_t = \frac{1}{2\bar{z}} \frac{N_t}{N_t - 1} \sum_{j=1}^{J} \sum_{k=1}^{J} f_{j,t} f_{k,t} |z_j^c - z_k^c| + \frac{1}{\bar{z}} \sum_{j=1}^{J} \frac{\left(N_t f_{j,t}^2 - f_{j,t}\right) L_{j,t}}{6(N_t - 1)}, \tag{8}$$

where N_t is the sample size, $z_j^c = (z_j + z_{j-1})/2$ the center of group j, $\bar{z} = \sum_{j=1}^{J} f_{j,t} z_j^c$ the group-implied estimator for the mean, and $L_j = z_j - z_{j-1}$ the length of the jth group. For the right-open top group $j = J$, we make the following choices. Its length is chosen to be $L_{J,t} = z^{max} - z_{J-1}$, where z^{max} is the highest value observed in the sample. Its probability mass is given by $f_{J,t} = 1 - F(z_{J-1} \mid D_t)$ by the definition of the cdf. As the center of the last group, we always take the average value of y_{igt} in that group as observed in the factual distribution. We find that the grouped formulas lead to values that are very close to the ones coming from the usual nonparametric formulas.

We report the ceteris paribus effects of the introduction of the minimum wage on the distribution and on inequality measures as

$$\Delta_j^{cf} := f_{j,1} - f_{j,1}^{cf}, \quad j = 1, \dots, J \tag{9}$$
$$\Delta^{cf}(v(\cdot)) := v(F(z \mid D_t = 1)) - v(F^{cf}(z \mid D_t = 1)), \tag{10}$$

where $v(\cdot)$ denotes either quantiles or inequality measures (Gini and quantile ratios) computed from the full distribution.

4.2 Pre-trend analysis

The identification of the counterfactual wage distribution (4) is only valid if there are no other time trends in wage developments that are differential across bite groups. For example, if the minimum wage bite is defined for regions, then it must not be the case that low-wage growth (conditional on covariates) was higher in high-bite vs. in low-bite regions as this would make the wage boosting effects of the minimum wage introduction appear higher than they were. Potential differences in wage growth across different bite levels in the years before the minimum wage introduction can be investigated in our pre-trend analysis sample. To identify such potential pre-trends, we run regressions analogous to (1) for the pre-introduction period 2011 to 2014 (e.g., Dobkin et al., 2018; Ahlfeldt et al., 2018; Freyaldenhoven et al., 2021):

$$\begin{aligned} F(z|Bite_g, D_g, year, X_{igt}) = \alpha_z + \sum_{t=2011}^{2014} \lambda_z^t \times 1[year = t] + D_g \gamma_z + \\ \sum_{t=2011}^{2014} \beta_z^t (Bite_g \times 1[year = t]) + X_{igt}\delta_z + \sum_{t=2011}^{2014} (X_{igt} \times 1[year = t])\eta_z^t. \end{aligned} \tag{11}$$

Here, we define the year $t = 2014$ as the reference so that all its coefficients are normalized to zero (i.e. $\lambda^{2014} = 0, \beta_z^{2014} = 0, \eta_z^{2014} = 0$). The coefficients $\beta_z^{2011}, \beta_z^{2012}, \beta_z^{2013}$ represent systematic differences in wage growth for different levels of the minimum wage bite in pre-treatment years. The null hypothesis of no pre-trends can be tested as $H_0: \ \beta_z^{2011} = \beta_z^{2012} = \beta_z^{2013} = 0$. If the coefficients $\beta_z^{2011}, \beta_z^{2012}, \beta_z^{2013}$ display systematic patterns (which they do in our application), we can extrapolate these patterns to the post-treatment period. For example, if the likelihood of falling under the hourly wage threshold of 8.5 euros per hour declined in high-bite regions faster than in low-bite regions before the minimum wage introduction, then one should subtract the extrapolation of this effect from the minimum wage effect in the post-period (because the fraction of wages below 8.5 euros would already have more strongly declined in these regions without the minimum wage). This will become clearer in section 5.4 where we present the estimated patterns of $\beta_z^{2011}, \beta_z^{2012}, \beta_z^{2013}$ in pre-treatment periods. In our application, the pre-trends follow an almost linear time trend, which we will use for counterfactual trend extrapolation.

Let $\bar{\delta}_z$ denote the extrapolated effect of the pre-trend for wage threshold z. Following this approach, the effect of the minimum wage *corrected for pre-trends* is then given by

$$F^{cf,trend}(z \mid D_t = 1) = \int [F(z \mid Bite_g, D_g, D_t = 1, X_{igt}) - \left(\beta_z - \bar{\delta}_z\right) Bite_g\big] \; dF(Bite_g, D_g, X_{igt} \mid D_t = 1), \tag{12}$$

i.e. the effect that can already be explained by the pre-trend has to be subtracted from the measured effect of the minimum wage. In section 5.4, we will consider different scenarios of extrapolating pre-trends, e.g., $\bar{\delta}_z^1$ is the pre-trend effect under the assumption that the pre-trends last up to one year after the minimum wage introduction, and $\bar{\delta}_z^2$ up to two years after the minimum wage introduction.

4.3 Estimation and specification

All factual and counterfactual distribution functions and their derivatives can be estimated by their sample counterparts (i.e. weighted sample averages using the sample weights). We compute bootstrap standard errors for all quantities based on clustering at the treatment level (Bertrand et al., 2004). In principle, it would be desirable to specify the distribution regressions (1) as logit or probit models (Chernozhukov et al., 2013). Unfortunately, the data on which our analysis is based can only be accessed on-site with substantial computational limitations that make the use of logit or probit models infeasible. However, we found in preliminary experiments with logit models that the factual and counterfactual *unconditional* distributions (3) and (4) were to a very large extent insensitive to the use of different models (linear proability vs. logit models) and/or covariate specification choices (inclusion/exclusion of covariates and interactions). This is not surprising given the large amount of averaging involved. We also found that potential problems associated with linear probability models such as predictions outside the unit interval were minor given the rich set of covariates we use for conditioning. Finally, we point out the practical advantages of linear probability models in the given context: computational simplicity, transparency, consistent aggregation due to the law of iterated projections and immediate interpretation of β_z in terms of percentage points probability mass gained/lost per unit of bite.

5 Empirical Results

5.1 Hourly wages

Figures 2 to 4 show the effects of the minimum wage on the distribution of hourly wages as measured by the three alternative bite definitions. The upper panels in each figure compare the factual distribution in 2018 with the counterfactual distribution in the absence of the minimum wage. The lower panels explicitly show these effects of the minimum wage on the frequency of hourly wages in each bin.

The results based on the regional bite definition are presented in figure 2. The dark bars for the factual distribution in the upper panel suggest that in 2018, compliance with the minimum wage level (then 8.87 euros per hour) was achieved to a very large extent, but not completely. The light bars in the upper panel of figure 2 depict the situation that would have prevailed under a hypothetical wage structure without the minimum wage as inferred from the differential behavior of distributional change across regions. The differences between the factual and the counterfactual distribution in the lower panel demonstrate more explicitly that the introduction of the minimum wage almost completely eliminated hourly wages below its nominal level, shifting mass to wage bins above it. There is some evidence for spill-over effects which are measured very imprecisely in the given specification.[7]

[7]Following common practice in difference-in-differences analysis, we cluster standard errors at the treatment level (here at the regional level). Clustering standard errors at the firm level leads to substantially tighter confidence intervals but at the cost of potentially erroneous inference (due to ignoring unmodelled correlations within bite groups).

— Figures 2 to 4 around here —

Figure 3 shows the estimates based on bite differences across occupations augmented by East/West information. The overall pattern looks quite similar to the one in figure 2 but the area of change is much more compressed and statistical precision much higher. In particular, we observe statistically significant spill-over effects up to 11.5 euros per hour. The results for higher bins deliver precise zeros, which was not the case for the bite definition based on regions. Precise zeros for high values of hourly wages can be viewed as a validation check for our identification strategy as direct effects of the minimum wage on very high wages seem unlikely. The higher statistical precision of the results in figure 3 may be related to the much higher variation in its bite variable as shown in table 1 (bite values range between .056 and .32 for the regional bite, whereas their range is between .01 and .634 for the bite measure based on occupations). Employing the bite definition based on industries leads to even sharper results around the minimum wage level as shown in figure 4. This is in line with the higher variation in the industry compared to the occupational bite (table 1). Again, we observe statistically significant spill-over effects up to 11.5 euros along with sharp zeros for higher values of hourly wages.

Summing up, all three bite definitions lead to a qualitatively similar pattern of distributional change showing that the introduction of the minimum wage in 2015 led to the elimination of very low wages but also increased hourly wages for wages up to 30 above the minimum wage.

5.2 Effects on wage inequality

How do these effects translate into changes of inequality measures? Asking this question is important as it is only in this way that one can assess the contribution of the minimum wage to general trends in wage inequality. Table 2 shows that wage inequality as measured by the Gini coefficient fell in a statistically significant way between 2014 and 2018 (by -0.02, see first column in lower panel). Columns 2, 5 and 8 in the lower panel of table 2 show that this fall can be fully explained by the minimum wage effect as estimated on the basis of the regional bite (-0.024), but only partly when the other two bite definitions are used (-0.013 based on occupations and -0.013 based on industries).

— Table 2 around here —

Further results in the lower panel of table 2 indicate that the minimum wage significantly reduced the Q90/Q10 ratio (by -.453, -.288 and -.264, see columns 2, 5 and 8), although other developments counteracted this trend as the factual change was only -.160 (and not statistically significant, see column 1). As a validation check, the minimum wage did not significantly affect inequality in the upper half of the distribution (Q90/Q50 ratio). It did, however, more than fully explain the observed inequality decline of -.060 in the lower half of the distribution (Q50/Q10 ratio) indicating that other developments counteracted these effects (the overall decline is smaller than the contributions by the minimum wage, i.e. -.092, -.154 and -.130, depending on the bite measure).

In summary, the available evidence suggests that wage inequality significantly fell between 2014 and 2018, and that this fall can be largely explained by the introduction of the minimum wage. However, the trend in wage inequality between 2014 and 2018 would already have been flat or even falling without the minimum wage. This can be seen in the upper panel of table 2. For example, the counterfactual Gini in 2018 without the minimum wage would have been .264 (regional bite) or .253 (occupational or industry bite). This is close to or even slightly lower than the inequality level four years before (.259) implying a flat or even slightly falling trend in the absence of the minimum wage. This is an important observation suggesting that the minimum wage was not the main factor responsible for stopping the trend of rising wage inequality in Germany. Rather, this finding is consistent with evidence in Biewen and Seckler (2019) who showed that de-unionization and compositional changes with respect to education and experience were responsible for rising inequality in hourly wages before 2014, but that these inequality drivers did not continue to enhance inequality in the years before 2014. Also, Fitzenberger and Seidlitz (2020) show that earnings inequality among full-time workers stopped rising in Germany after 2010 way before the introduction of the minimum wage.

5.3 Hours worked

We now present the results for hours worked. We first focus on individuals with wages below 12 euros per hour as one would expect that any effects of the minimum wage on hours worked will be strongest in this group of workers. Even though the point estimates exhibit some interesting patterns, the introduction of the minimum wage did not have a significant impact on hours worked or the share of part-time, marginal part-time and full-time employment. In the supplementary appendix, we report the evidence for workers with hourly wages above 12 euros per hour which consists of precisely measured zero effects throughout. Again, this can be interpreted as a validation check for the explanatory variation used by us as one would expect hours changes only for groups whose wages may have been affected by the minimum wage introduction.

For hours worked, we consider grouped categories rather than a fine grid of thresholds (as in the case of hourly wages) for the following reasons. First, grouped categories directly address the question to what extent the minimum wage led to shifts between different forms of part-time, marginal part-time and full-time work. This would not be possible if we used small bins for different values of hours worked. Second, due to small differences in the definitions of hours worked between the GSES and the DGUV-IAB data (see supplementary appendix), we have to define broader hours categories to connect the main GSES analysis to a pre-trend analysis based on the DGUV-IAB. In results available on request, we carried out an analysis based solely on the GSES data but using a fine grid of thresholds for weekly hours worked. This analysis yields the same result of zero effects of the minimum wage introduction throughout the distribution of hours worked.

— Figures 5 to 7 around here —

Figure 5 shows shifts between hours worked categories induced by the minimum wage as modelled by the regional bite. The figure suggests some shifts which, however, are not statistically significant. The shift from the category between 12 and 20 hours per week to the category below 12 hours is particularly suggestive because working hours of 12 hours per week precisely coincide with what a so-called mini-job worker would have to work at the level of the minimum wage rate to reach the nominally fixed mini-job level of 450 euros per month.[8] As mini-jobs are also subject to the minimum wage, a plausible hypothesis would be that working hours in mini-jobs exceeding 12 hours per week before the minimum wage introduction were reduced to under 12 hours to keep the hourly wage rate above 8.87 euros/hour. The pattern shown in 5 points into this direction but, despite the large number of observations, the measured effects are

[8]'Mini-jobs' are a legally privileged form of marginal employment which are exempt from social security contributions and taxes up to the nominal level of 450 euros per month. An individual working 12 hours a week at the minimum wage rate of 8.84 euros/hour would almost exactly reach the nominal mini-job pay of 450 euros per month (4.345 weeks $\times$ 12 hours $\times$ 8.84 euros/hour $\approx$ 450 euros per month).

not statistically significant. Figures 6 and 7 present the results based on the occupational and the industry bite. Exploiting these bite definitions yields statistically precise effects close to zero throughout the hours distribution suggesting that, in the fourth year after the minimum wage introduction, there is no evidence for shifts between working hours categries that we can link to the differential exposure to the minimum wage.

5.4 Pre-trend analysis

The results presented above are not valid if there were differential time trends across the subgroups that define the bite variable in the years preceding the minimum wage introduction. For example, if it was the case that the fraction of low wages decreased in a stronger way in high-bite regions than in low-bite regions even before the minimum wage introduction, one would expect that this trend would have continued after the minimum wage introduction, wrongly attributing part of the wage increases after 2015 to the minimum wage introduction. In this section, we demonstrate that such trends indeed existed and show how to incorporate them into our analysis. However, our conclusion is that our final results are changed only to a minor extent by correcting for these trends.[9]

The estimates of the pre-introduction coefficients of the bite variable are shown in figures 8 to 10 (these are the $\hat{\beta}_z^t$ coefficients in equation (11)). In the absence of pre-trends, it should be the case that $\beta_z^{2011} = \beta_z^{2012} = \beta_z^{2013} = \beta_z^{2014} = 0$, i.e. the likelihood for a wage rate below z should not have been systematically different in high-bite vs. in low-bite groups as the minimum wage had not been introduced yet. Moreover, if the degree by which $\hat{\beta}_z^t$ differed from zero displayed a systematic trend in the years before the minimum wage introduction, this trend can be extrapolated to years after 2014.

— Figures 8 to 10 around here —

For example, take the case of $z = 8.49$ euros/hour in panel (a) of figure 8 (solid line). In the

[9] The pre-trend analysis for the distribution of hours worked is presented in the supplementary appendix. By contrast to the hourly wage, pre-trends show less clear patterns and lower statistical significance for hours worked. Incorporating them into our analysis does not change the finding of zero effects of the minimum wage on the distribution of hours worked.

years before the minimum wage introduction 2011 to 2013, individuals in high-bite regions were more likely to have wages below 8.49 euros/hour than in low-bite regions ($\hat{\beta}_z^t > 0$), but this was less and less the case, i.e. wages in high-bite groups already caught up to those in low-bite groups before the minimum wage introduction. In the area right of the vertical bar, we extrapolate this trend linearly up to 2015 and 2016 (one year extrapolation and two year extrapolation).[10] The values of the extrapolated trend at 2015 and 2016 are therefore the values $\bar{\delta}_z^1$ and $\bar{\delta}_z^2$ we have to subtract from the coefficient of the minimum wage effect after 2014 because these represent by how much the fraction of wages below threshold z would have declined in high-bite vs. in low-bite regions by the differential time trends alone, see equation (12).[11]

It turns out that there are systematic differential time trends years before the minimum wage introduction across all bite definitions (figures 8 to 10). Remarkably, the intensity of differential pre-trends increases smoothly towards the 8.50 euro/hour threshold and smoothly decreases for higher wages. This means that the fraction of wages below 8.50 euros/hour was already declining more strongly in high-bite than in low-bite groups before the minimum wage introduction indicating that the minimum wage effects may be overestimated without subtracting these effects.

The patterns in figures 8 to 10 also represent evidence against anticipation effects as the developments were already systematically linear since at least the year 2012 and did not accelerate towards the year 2014. There appears to be a slight acceleration from 2013 to 2014 in figure 8 based on the regional bite, but there is no acceleration in figures 9 and 10 where the patterns are remarkably linear. For a discussion of potential anticipation effects of the German minimum wage, see Bossler (2017). Note that our GSES wage measure refers to April 2014. The decision about introduction of the minimum wage was made in parliament in July 2014 following parliamentary debates. Recall, however, that the minimum wage did not come into force until 1st January 2015. Generally, it is unclear why employers should pay higher wages in anticipation of a minimum wage if they are not obliged to do so (altruistic employers may always pay wages above the market level independently of a minimum wage). Bossler and Schank (2022) also find little to no evidence for anticipation effects in 2014, although their wage measures are based on IAB employment spell data which mostly rely on end-of-year notifications (i.e. their data cover the

[10] In order to stay conservative, we only use the years 2012 to 2014 to fit the pre-trend and only extrapolate up to two years after 2014 to avoid potential over-extrapolation.

[11] This is equivalent to the approach in, e.g., Dobkin et al. (2018) who only consider the part of the DiD-effect that deviates from a linearly extrapolated time trend.

whole year 2014, whereas our wage measure exactly refers to April 2014).

Figure 11 displays the p-values for the null hypothesis of no differential pre-trends $\beta_z^{2011} = \beta_z^{2012} = \beta_z^{2013} = \beta_z^{2014} = 0$ at different wage thresholds. These results confirm the statistical significance of differential pre-trends at low wage levels and at around 20 euros/hour (extrapolations not shown here). Remarkably, the observed patterns are uniform across the alternative bite definitions suggesting that wage growth was already higher for low-wage workers in high-bite groups before the minimum wage introduction, independently of whether these bite-groups are defined by region, occupation or industry. This points to exceptional wage growth for low-wage workers where ever there were low wages even in the years preceding the minimum wage introduction – and, incidently, the effect is strongest around the minimum wage.

To what extent do these pre-trends change our estimated effects of the minimum wage? Figures A.1 to A.3 in the appendix show that the size of our estimates is reduced by accounting for pre-trends, but to a limited extent. Moreover, confidence intervals become wider due to the pre-trend estimation.[12] A reason for the limited changes induced by the trend-correction is that the original distribution regression refers to the cumulative distribution function, while for the histogram bins the differences of the cumulative distribution function across adjacent thresholds matter (see equation (5)). As long as the trend-correction terms $\bar{\delta}_z$ vary relatively smoothly across thresholds (as they do), their effect on histogram bins will be limited. For the same reason, the inequality reducing effect of the minimum wage is only slightly diminished if the trend-correction is applied and its statistical significance reduced due to trend estimation, see the columns headed by $\bar{\delta}_z^1$ and $\bar{\delta}_z^2$ in table 2. Still, we conclude that the impact of the minimum wage is slightly overestimated if pre-trends are not taken into account.

6 Discussion and conclusion

This paper analyzes the effects of the German statutory minimum wage on the distribution of hourly wages and hours worked. Our analysis is based on the German Structure of Earnings Survey (GSES) and administrative DGUV-IAB data, which are the only large-scale data bases for

[12]These figures show the results based on the assumption that pre-trends continue up to two years after the introduction of the minimum wage. The results based on extrapolating pre-trends for one year are naturally smaller and reported along with the trend-corrected results for working hours in the supplementary appendix.

Germany that include information on hourly wages and working hours. We propose a transparent methodology in terms of difference-in-differences distribution regressions (DR-DiD) based on bite measures providing a full distributional analysis of minimum wage effects on the distribution of hourly wages and working hours, while addressing the problems of discrete mass points and nominal target values in these distributions.

Our results suggest that the introduction of the minimum wage in 2015 shifted low hourly wages over its threshold and produced spill-over effects up to 20 percent above it. Given that we consider our information on wages and hours to be much less prone to rounding and other measurement error than in small-scale survey data with non-compulsory participation, our analysis indicates that such spill-over effects are real. We find that inequality in hourly wages fell between 2014 and 2018, counteracting a long-term trend until 2010 in rising inequality in hourly pay (Antonczyk et al., 2010; Biewen and Seckler, 2019). Our results suggest that the introduction of the minimum wage explains this fall to a large extent, depending on the inequality measure. However, the trend in hourly pay inequality would already have been approximately flat between 2014 and 2018 in the absence of the minimum wage, suggesting that the minimum wage was not the only factor stopping the long-term trend of rising wage inequality. We also demonstrate that low-wage growth was already higher in groups that were later most affected by the minimum wage upward biasing the estimation of minimum wage effects. As to the effect of the minimum wage on the distribution of hours worked, we obtain the remarkable result that, four years after its introduction, there is no evidence for shifts between working hours categories that can be linked to the exposure to the minimum wage at the time of its introduction. Our conclusions are to a large extent insensitive to the use of alternative bite measures, but we find that using a bite measure based on regions leads to less clear and less statisticaly precise results than using bite measures based on occupations or industries.

Our results are in line with effects of the minimum wage on the distribution of *monthly earnings* as estimated by Bossler and Schank (2022). Bossler and Schank (2022) find that the minimum wage boosted monthly earnings in the lower part of the distribution which explains a large part of the reduction in monthly wage inequality between 2014 and 2017. This mirrors our result on the reduction of hourly wage inequality between 2014 and 2018 which can also largely (depending on the inequality measure) be explained by the minimum wage. Compared to Bossler and Schank (2022) a potential limitation of our analysis is that we do not consider compositional changes of the workforce. However, Bossler and Schank (2022) have shown that, over the period under consideration, such changes almost exclusively referred to changing fractions of different employment forms (part-time, marginal part-time, full-time) which are automatically accounted for in our hourly wage measure (i.e., monthly wage divided by hours worked). Our descriptive statistics also provide no evidence for major compositional shifts in observables between 2014 and 2018 (see table 1). This also holds by low-bite vs. high-bite groups (results available on request).

Finally, the result that the minimum wage changed the distribution of *monthly wages* to a similar extent as that of *hourly wages* is consistent with our finding of no effects on working hours. The comparison of our results to earlier studies, some of which found small but significant effects on working hours (Caliendo et al., 2017; Bonin et al., 2018; Bachmann et al., 2020), suggest that such effects were either transitory, or are due to differences in the working hours information in the different data sources.

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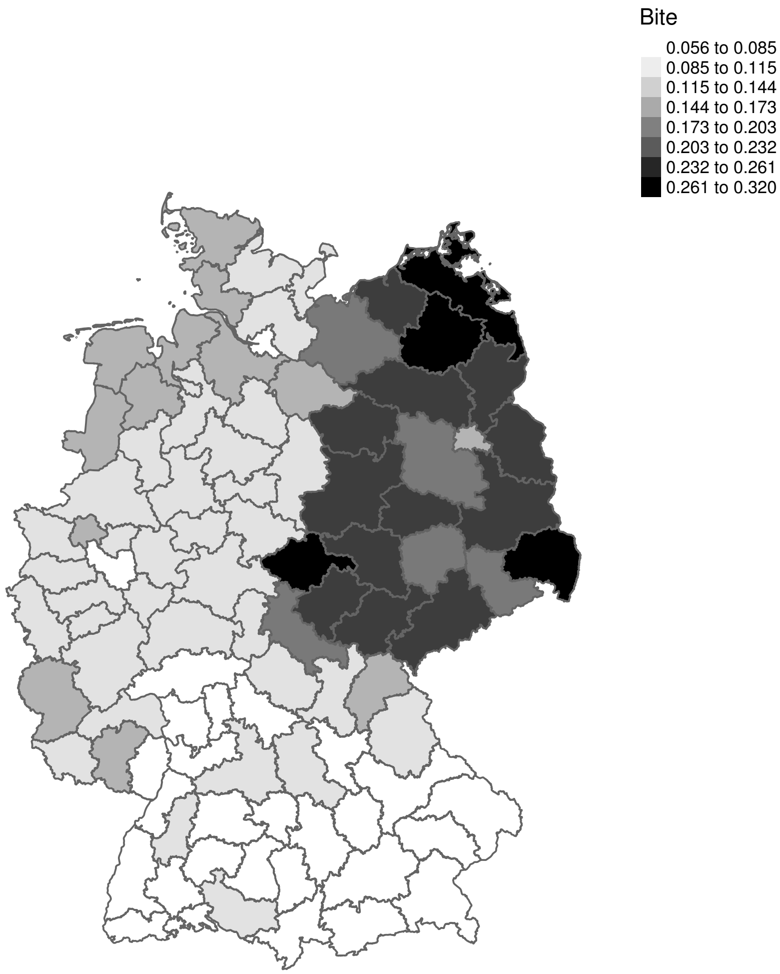
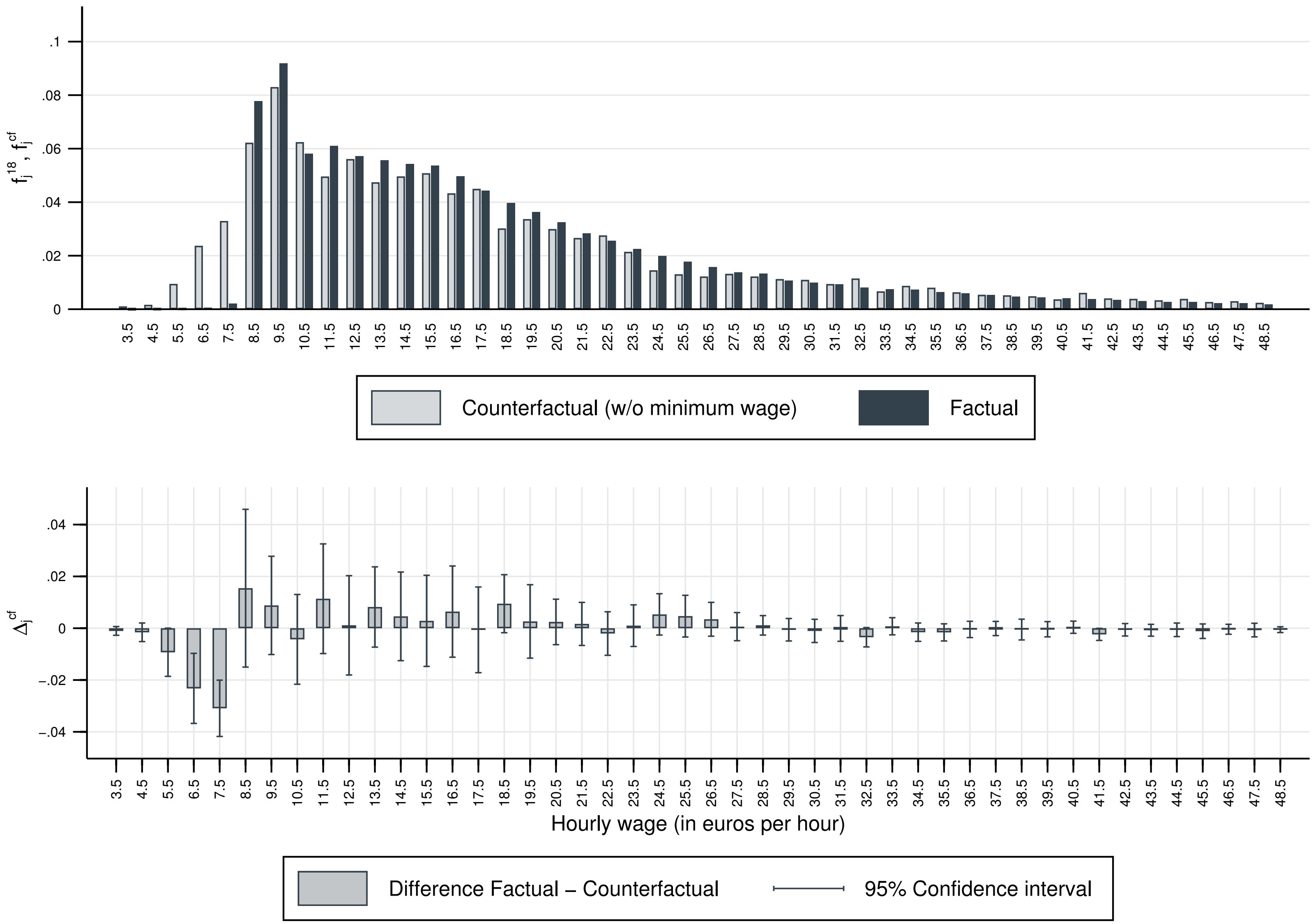
8 Figures

Figure 1 – Bite of the minimum wage across German regions



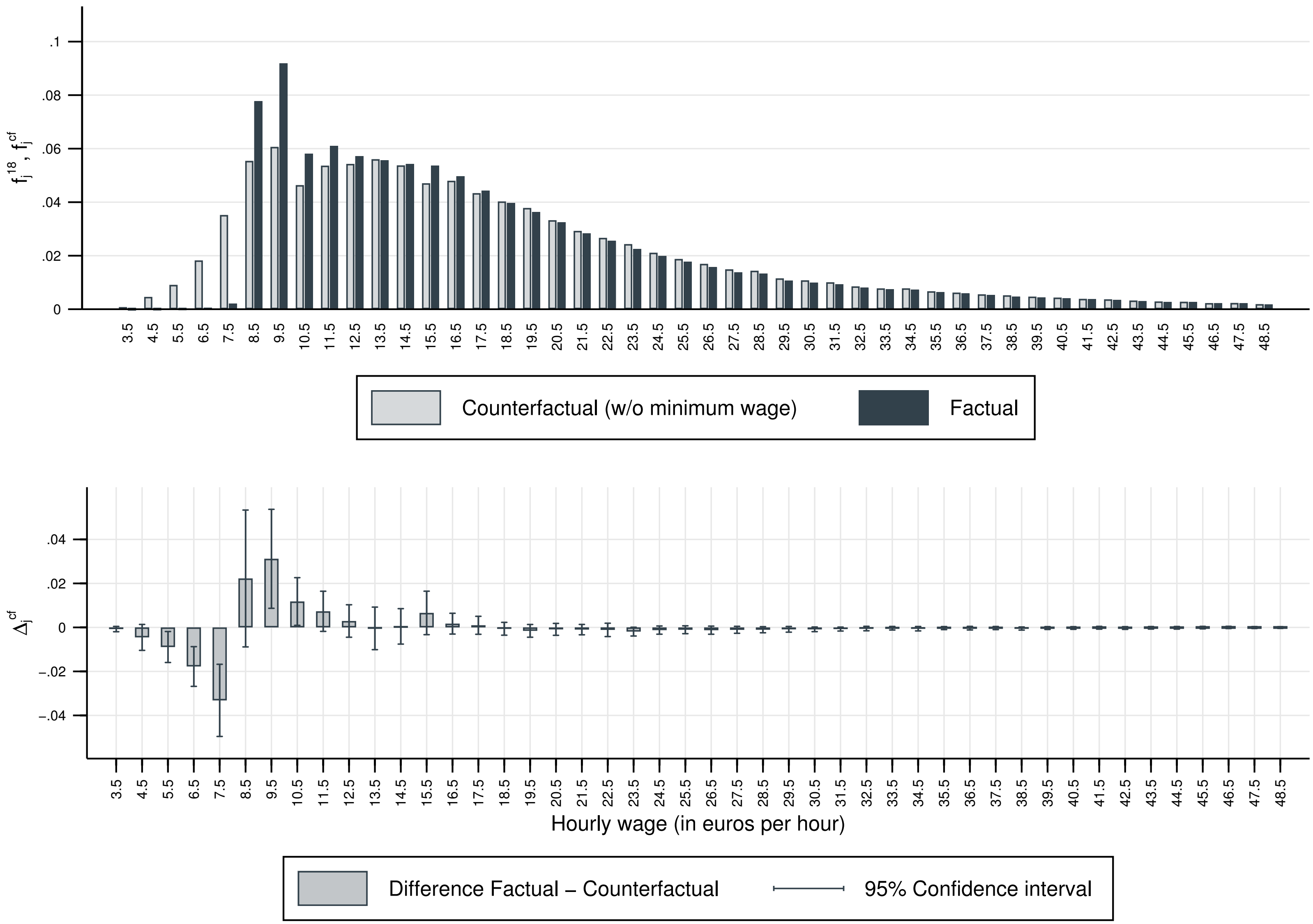
Notes: Graph shows the fraction of individuals with hourly wages less than the 2018 minimum wage (8.84 euros per hour) in the pre-policy period (April, 2014) across German regions (*'Raumordnungsregionen'*) (dark = higher bite). Source: GSES 2014, own calculations.

Figure 2 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 1: Regions. No trend adjustment.



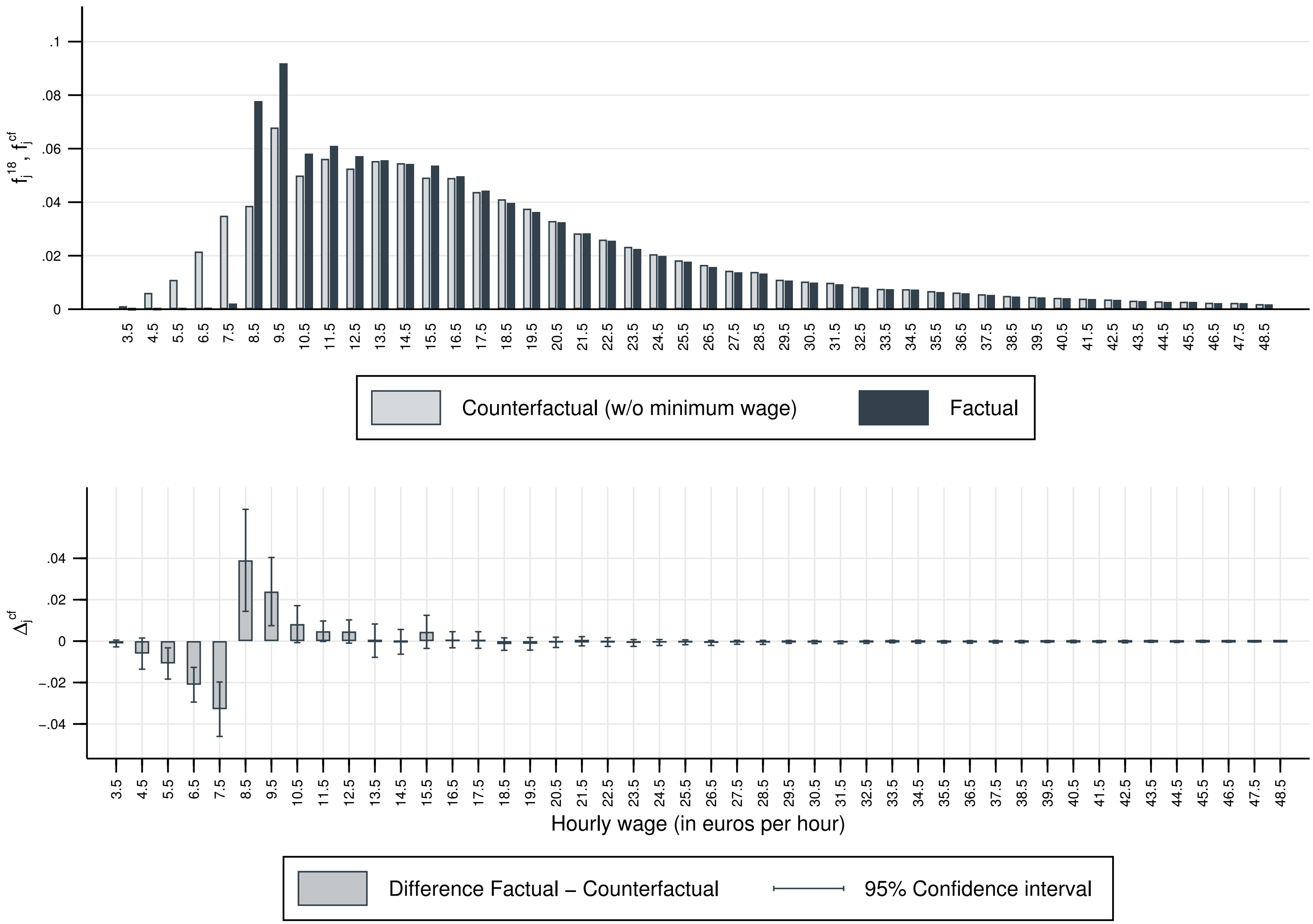
Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018; own calculations.

Figure 3 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 2: Augmented occupations. No trend adjustment.



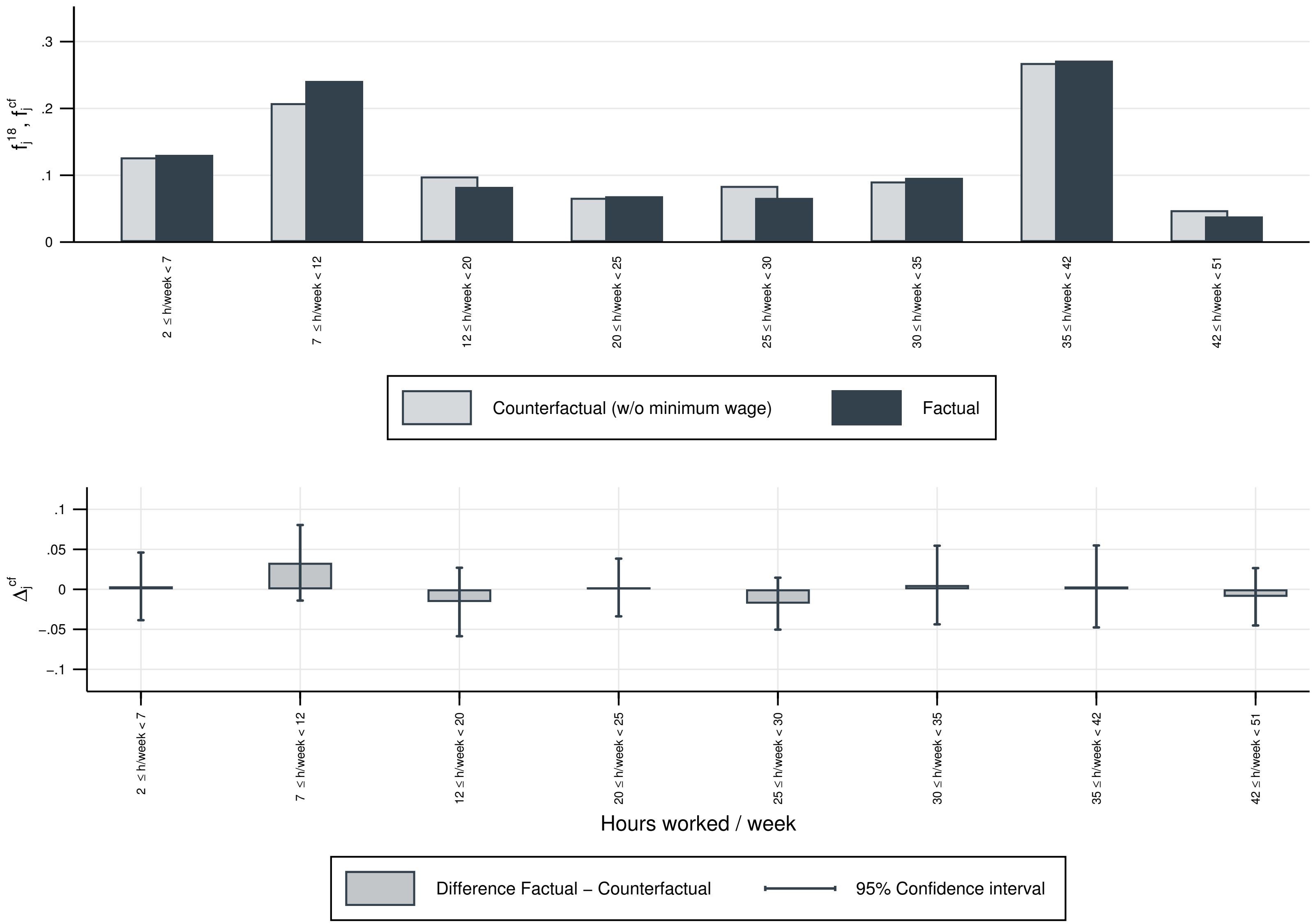
Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018; own calculations.

Figure 4 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 3: Augmented industries. No trend adjustment.



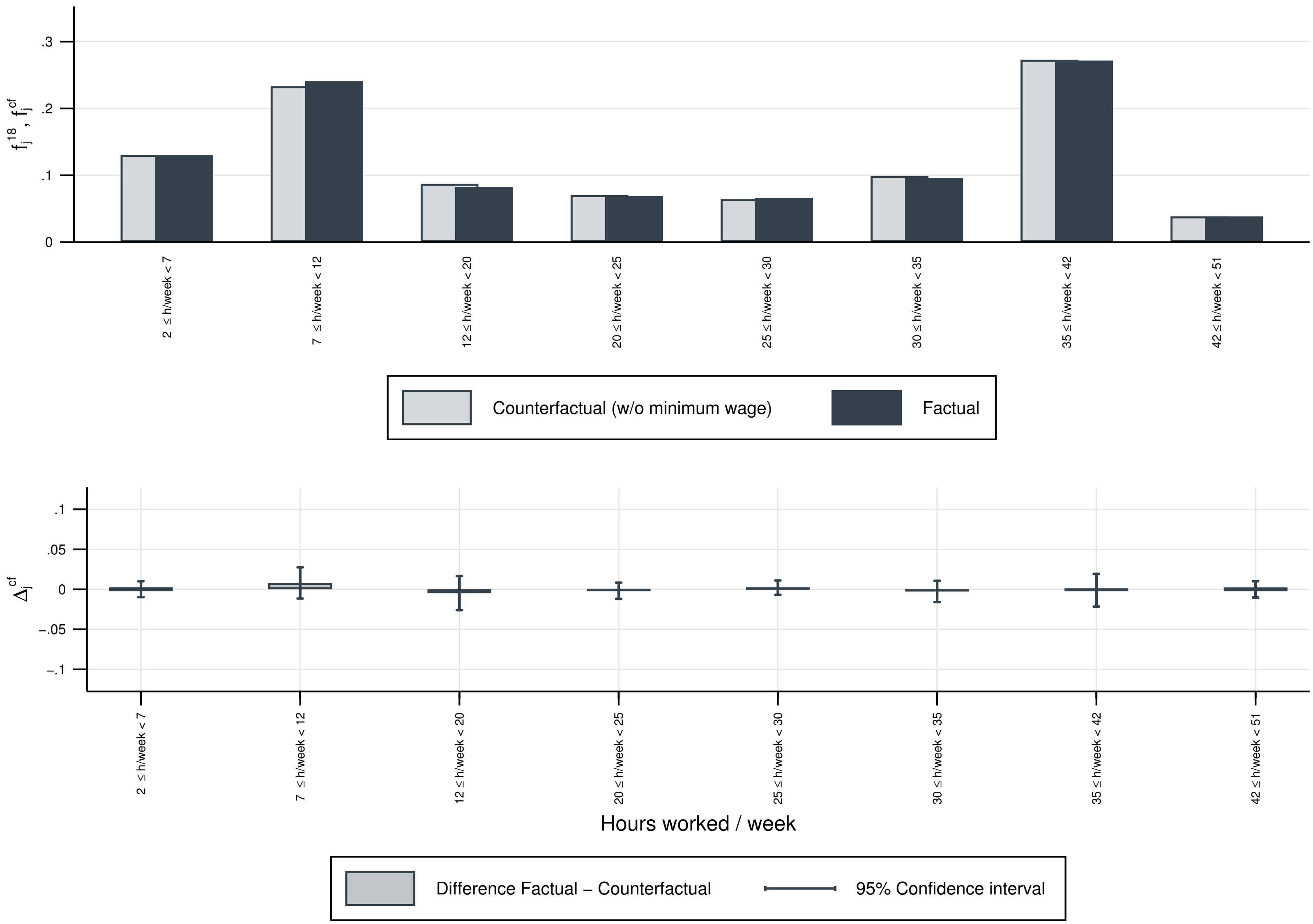
Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018; own calculations.

Figure 5 – 2018 Factual vs. counterfactual distribution of weekly working hours in the absence of minimum wage for individuals with hourly wages below 12 euros per hour. Bite 1: Regions. No trend adjustment.



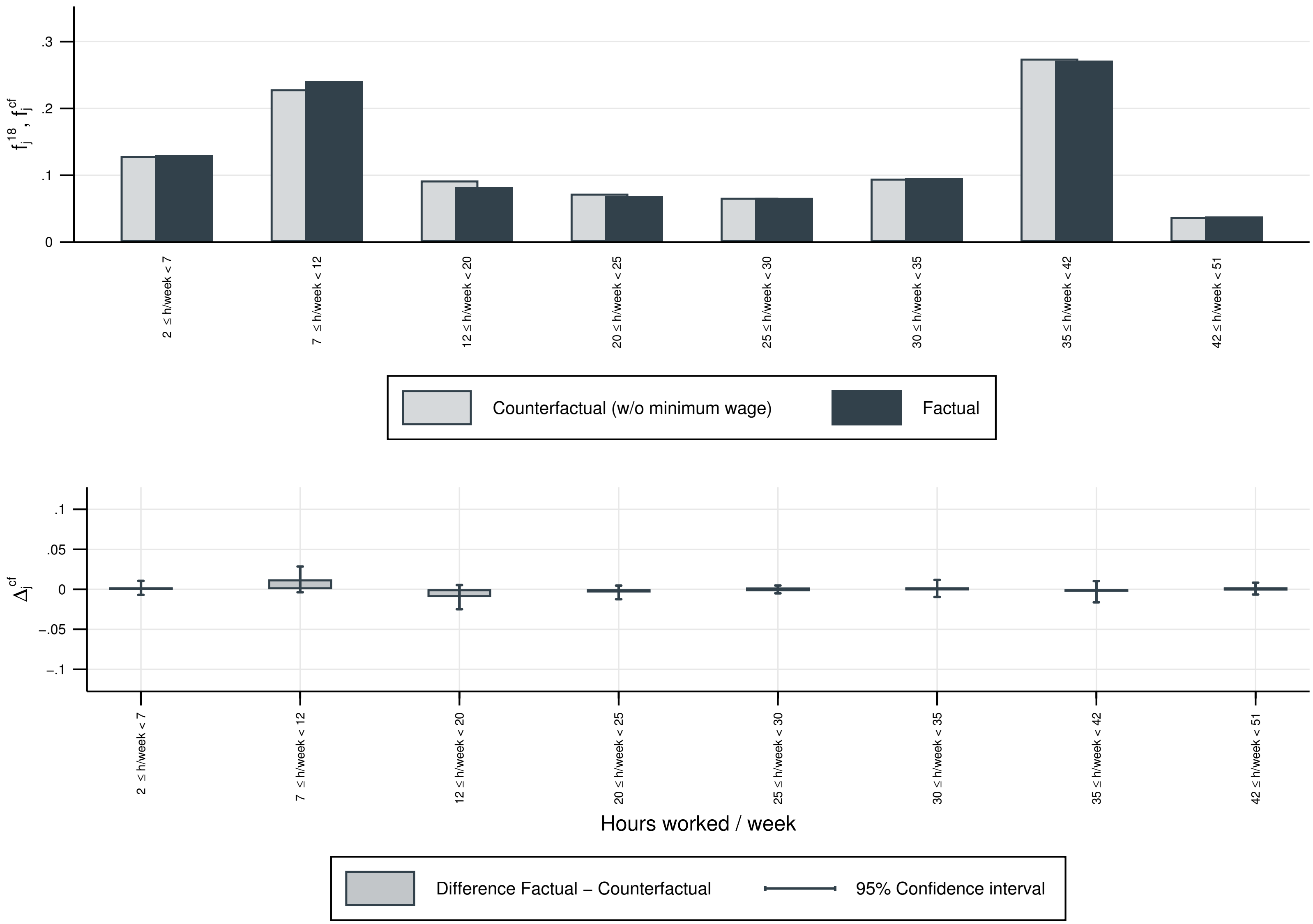
Notes: 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: GSES waves 2014 and 2018, own calculations.

Figure 6 – 2018 Factual vs. counterfactual distribution of weekly working hours in the absence of minimum wage for individuals with hourly wages below 12 euros per hour. Bite 2: Augmented occupations. No trend adjustment.



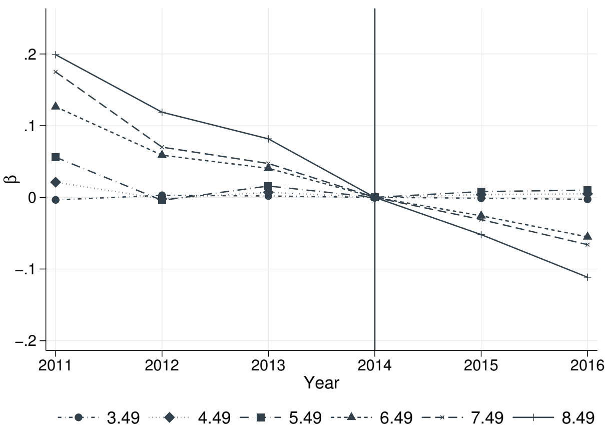
Notes: 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: GSES waves 2014 and 2018, own calculations.

Figure 7 – 2018 Factual vs. counterfactual distribution of weekly working hours in the absence of minimum wage for individuals with hourly wages below 12 euros per hour. Bite 3: Augmented industries. No trend adjustment.

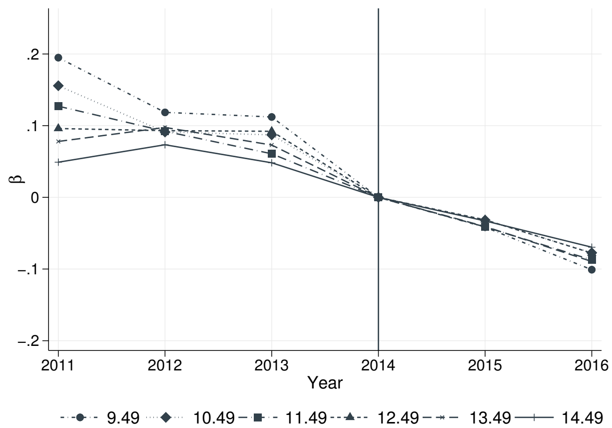


Notes: 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: GSES waves 2014 and 2018, own calculations.

Figure 8 – Pre-treatment estimates of treatment coefficients using *DGUV-IAB* data – Hourly wages, bite 1 (region)



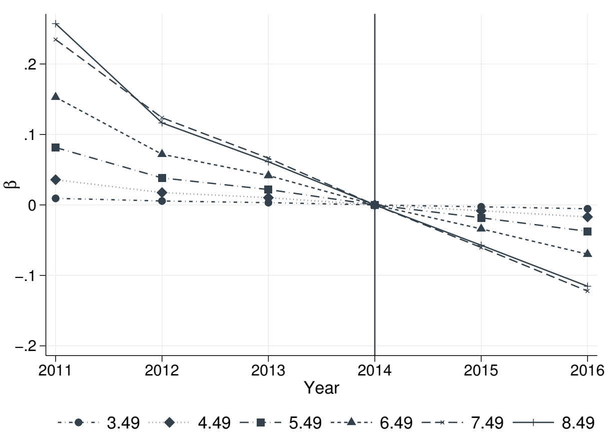
(a) Lower thresholds (3.49 to 8.49)



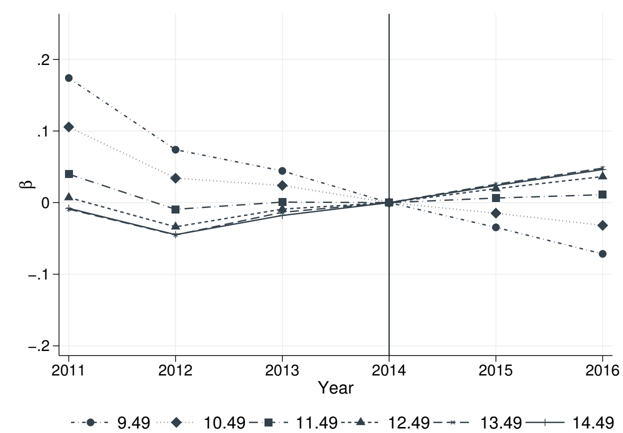
(b) Upper thresholds (9.49 to 14.49)

Notes: Estimates for the treatment effect, $\hat{\beta}_z^t$, in the pre-treatment periods 2011-2014 as specified in (11) for bins below and above the minimum wage level. Specification refers to bite defined by regions. Base period: 2014. Values in 2015 and 2016 refer to linearly extrapolated trends using the estimates from 2012, 2013, and 2014.
Source: *DGUV-IAB* sample covering the years 2011–2014, own calculations.

Figure 9 – Pre-treatment estimates of treatment coefficients using *DGUV-IAB* data – Hourly wages, bite 2 (augmented occupation)



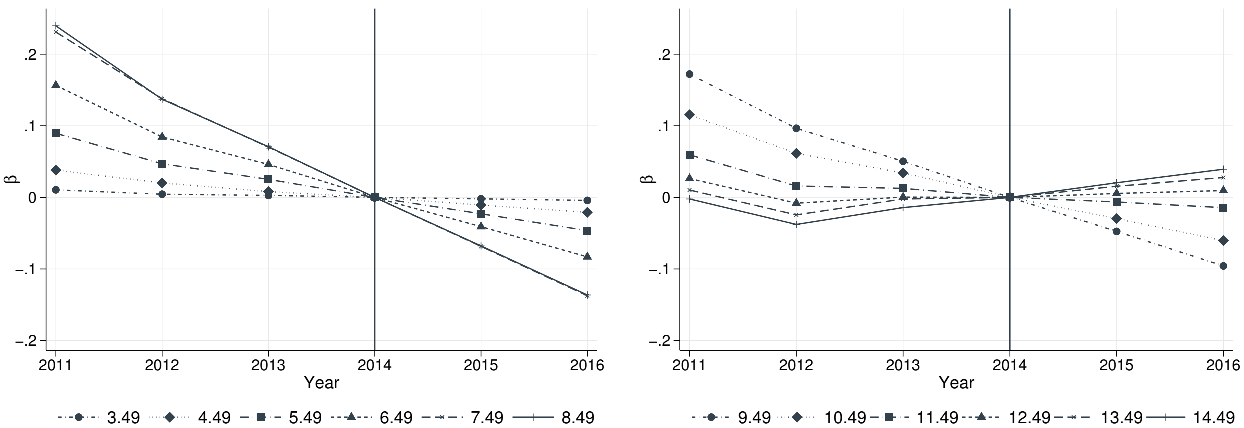
(a) Lower thresholds (3.49 to 8.49)



(b) Upper thresholds (9.49 to 14.49)

Notes: Estimates for the treatment effect, $\hat{\beta}_z^t$, in the pre-treatment periods 2011-2014 as specified in (11) for bins below and above the minimum wage level. Specification refers to bite derived from augmented occupations. Base period: 2014. Values in 2015 and 2016 refer to linearly extrapolated trends using the estimates from 2012, 2013, and 2014.
Source: *DGUV-IAB* sample covering the years 2011–2014, own calculations.

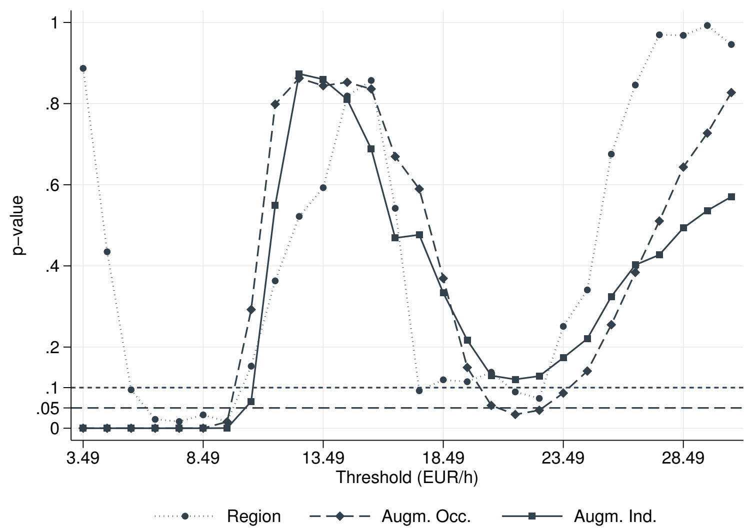
Figure 10 – Pre-treatment estimates of treatment coefficients using *DGUV-IAB* data – Hourly wages, bite 3 (augmented industries)



(a) Lower thresholds (3.49 to 8.49) **(b)** Upper thresholds (9.49 to 14.49)

Notes: Estimates for the treatment effect, $\hat{\beta}_z^t$, in the pre-treatment periods 2011-2014 as specified in (11) for bins below and above the minimum wage level. Specification refers to bite derived from augmented industries. Base period: 2014. Values in 2015 and 2016 refer to linearly extrapolated trends using the estimates from 2012, 2013, and 2014.
Source: *DGUV-IAB* sample covering the years 2011–2014, own calculations.

Figure 11 – P-values of joint significance – Hourly wage specifications



Notes: Values indicate p-values from a Wald test for joint significance of all pre-trend estimates ($\widehat{\beta}_z^{2011}$, $\widehat{\beta}_z^{2012}$, and $\widehat{\beta}_z^{2013}$ from (11)) for a given threshold z based on bootstrap (100 replications, clustered at treatment level).
Source: *DGUV-IAB* sample covering 2011-2014, own calculations.

9 Tables

Table 1 – Bite descriptive statistics

	(1) German regions	(2) Occupations+East/West	(3) Industry+East/West
Descriptives			
# Groups	96	72	146
Minimum bite	0.056	0.010	0.004
Maximum bite	0.320	0.634	0.759
Average bite	0.128	0.128	0.128
Standard deviation	0.062	0.129	0.138
Lowest categories			
Lowest	München	Technical research, development, construction and production control occupations – West	Financial service activities + (Re)Insurance/pension funding – East
2nd lowest	Ingolstadt	Computer science, information and communication technology occupations – West	Manufacture of motor vehicles, trailers and semi-trailers – West
3rd lowest	Oberland (Bavaria)	Professions in financial services, accounting and tax consulting – West	Manufacture of basic metals – West
4th lowest	Nürnberg	Professions in geology, geography and environmental protection – West	Manufacture of basic pharmaceutical products and pharmaceutical preparations – West
5th lowest	Südostoberbayern	Construction planning, architecture and surveying professions – West	Financial service activities + (Re)Insurance/pension funding – West
Highest categories			
Highest	Vorpommern	Tourism, hotel and catering occupations – East	Food and beverage service activities – East
2nd highest	Mecklenburgische Seenplatte	Cleaning professions – East	Gambling and betting activities – East
3rd highest	Oberlausitz-Niederschlesien	Food manufacturing and processing – East	Other personal service activities – East
4th highest	Nordthüringen	Horticultural professions and floristry – East	Accommodation – East
5th highest	Mittleres Mecklenburg/Rostock	Protection, security and surveillance occupations – East	Security and investigation activities – East

Source: GSES 2014, own calculations.

Table 2 – Factual vs. counterfactual quantiles and inequality measures for hourly wages

	2014	2018	CF^{region}			CF^{occup}			CF^{ind}		
			No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$	No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$	No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$
Statistic											
Gini	0.260	0.240	0.264	0.262	0.260	0.253	0.251	0.249	0.253	0.250	0.248
	(0.002)	(0.002)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.006)	(0.007)	(0.007)
Q90	29.592	32.374	34.083	34.083	34.083	32.850	32.850	32.850	32.882	32.882	32.882
	(0.831)	(0.882)	(1.315)	(1.346)	(1.45)	(1.726)	(1.742)	(1.802)	(1.548)	(1.563)	(1.603)
Q50	14.698	16.264	15.901	15.999	16.106	16.585	16.494	16.416	16.499	16.407	16.327
	(0.242)	(0.235)	(0.46)	(0.483)	(0.524)	(0.838)	(0.839)	(0.846)	(0.674)	(0.679)	(0.679)
Q10	8.464	9.703	8.993	9.087	9.206	9.063	9.161	9.253	9.132	9.304	9.458
	(0.100)	(0.039)	(0.199)	(0.187)	(0.192)	(0.328)	(0.331)	(0.338)	(0.326)	(0.309)	(0.299)
Q90/Q10	3.496	3.337	3.790	3.751	3.702	3.624	3.586	3.550	3.601	3.534	3.477
	(0.074)	(0.080)	(0.146)	(0.149)	(0.160)	(0.159)	(0.163)	(0.171)	(0.154)	(0.153)	(0.154)
Q90/Q50	2.013	1.991	2.143	2.130	2.116	1.981	1.992	2.001	1.993	2.004	2.014
	(0.030)	(0.030)	(0.111)	(0.115)	(0.124)	(0.068)	(0.070)	(0.074)	(0.063)	(0.064)	(0.067)
Q50/Q10	1.737	1.676	1.768	1.761	1.749	1.830	1.800	1.774	1.807	1.763	1.726
	(0.013)	(0.018)	(0.052)	(0.051)	(0.052)	(0.067)	(0.065)	(0.063)	(0.067)	(0.064)	(0.062)

	$\widehat{\Delta}^{18-14}$	$\widehat{\Delta}^{cf}_{reg}$			$\widehat{\Delta}^{cf}_{occ}$			$\widehat{\Delta}^{cf}_{ind}$		
		No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$	No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$	No adj.	$\bar{\delta}_1$	$\bar{\delta}_2$
Statistic										
Gini	−0.020***	−0.024***	−0.023***	−0.021***	−0.013**	−0.011*	−0.009	−0.013***	−0.011**	−0.008*
	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.004)	(0.005)	(0.005)
Q90	2.782**	−1.709	−1.709	−1.709	−0.476	−0.476	−0.476	−0.509	−0.509	−0.509
	(1.263)	(1.337)	(1.361)	(1.443)	(0.820)	(0.872)	(0.905)	(0.551)	(0.546)	(0.562)
Q50	1.566***	0.363	0.265	0.158	−0.321**	−0.230	−0.152	−0.235*	−0.143	−0.063
	(0.348)	(0.376)	(0.406)	(0.452)	(0.159)	(0.194)	(0.236)	(0.136)	(0.155)	(0.179)
Q10	1.239***	0.709***	0.615***	0.497***	0.639***	0.542***	0.449**	0.571***	0.398**	0.245
	(0.110)	(0.18)	(0.170)	(0.178)	(0.193)	(0.189)	(0.196)	(0.216)	(0.199)	(0.191)
Q90/Q10	−0.160	−0.453***	−0.414**	−0.366**	−0.288**	−0.249*	−0.213	−0.264**	−0.198*	−0.140
	(0.113)	(0.164)	(0.164)	(0.172)	(0.129)	(0.133)	(0.139)	(0.109)	(0.101)	(0.099)
Q90/Q50	−0.023	−0.153	−0.140	−0.126	0.010	−0.001	−0.011	−0.003	−0.014	−0.023
	(0.045)	(0.115)	(0.119)	(0.128)	(0.057)	(0.062)	(0.066)	(0.042)	(0.043)	(0.046)
Q50/Q10	−0.060***	−0.092*	−0.084*	−0.073	−0.154***	−0.124***	−0.098**	−0.130**	−0.087*	−0.050
	(0.022)	(0.052)	(0.050)	(0.051)	(0.047)	(0.046)	(0.046)	(0.054)	(0.050)	(0.047)

Notes: CF^{region}, CF^{occup}, CF^{ind} refer to the counterfactual measures for 2018 in the absence of the minimum wage (based on the alternative bite measures). For each counterfactual scenario, we distinguish between three adjustment scenarios: No adjustment, one year ($= \bar{\delta}^1$) and two year trend extrapolation ($= \bar{\delta}^2$) as specified in (12). $\widehat{\Delta}^{18-14}$ is the factual difference between 2014 and 2018. $\widehat{\Delta}^{cf}_{reg}$, $\widehat{\Delta}^{cf}_{occ}$, $\widehat{\Delta}^{cf}_{ind}$ and the respective trend adjusted versions thereof are the differences of the factual measure in 2018 and the counterfactual one in the absence of the minimum wage (representing the isolated effect of the minimum wage on the change between 2014 and 2018). Bootstrap standard errors in parentheses. Bootstrap standard errors for factual values (columns 1, 2, in the upper and column 1 in the lower panel) are clustered at the regional level. Bootstrap standard errors for the counterfactual values and differences are clustered at the respective treatment level (region, augmented occupation or augmented industry level). All bootstrap standard errors were obtained using 100 bootstrap replications.

***/**/* indicate statistical significance for the factual/counterfactual differences at the 1%/5%/10% level.

Appendix

A Identification assumptions for DR-DiD

In this section, we show that viewing the distributional treatment effect problem as a distribution regression difference-in-differences model leads to a straightforward identification analysis employing standard difference-in-differences assumptions for repeated cross-sections. In the following, let I^z denote the dummy variable indicating whether or not the observed outcome Y falls below threshold z, i.e. $I^z = 1[Y \leq z]$. The potential outcome under treatment level $Bite = b$ is defined as $Y(b)$, and, correspondingly, $I^z(b) = 1[Y(b) \leq z]$. Recall that there are two time periods $t = 0$ and $t = 1$ represented by the indicator $D_t = 0$ (for $t = 0$) and $D_t = 1$ (for $t = 1$). We assume repeated cross-section sampling, i.e. we observe i.i.d samples from $(I^z, Bite, W)|D_t = 0$ and from $(I^z, Bite, W)|D_t = 1$, where W subsumes individual characteristics and time effects.

Recall that the factual distribution of Y in $D_t = 1$ is given by

$$F(z \mid D_t = 1) = \int E(I^z(b) \mid Bite = b, W, D_t = 1)\, dF(Bite, W \mid D_t = 1). \tag{A-1}$$

The counterfactual distribution under the assumption of no minimum wage is defined as

$$\begin{aligned} F^{cf}(z \mid D_t = 1) = & \int E(I^z(0) \mid Bite = b, W, D_t = 1)\, dF(Bite, W \mid D_t = 1) \qquad \text{(A-2)} \\ = & \int E(I^z(b) \mid Bite = b, W, D_t = 1) \\ & \qquad - \underbrace{[E(I^z(b) - I^z(0) \mid Bite = b, W, D_t = 1)]}_{:=ATT^z(b|b,W)}\, dF(Bite, W \mid D_t = 1) \end{aligned}$$

The parameter $ATT^z(b|b, W)$ is the average treatment effect for $Bite = b$ vs. $Bite = 0$ for individuals with characteristics W who actually receive treatment b, see Callaway et al. (2021). Note that our research question only involves the comparison between treatment level $Bite = b$ and treatment level $Bite = 0$, so that the complications due to comparing different treatment levels discussed in Callaway et al. (2021) do not arise.

Using standard arguments described in Callaway et al. (2021) identify $ATT^z(b|b, W)$:

$$\begin{aligned}
ATT^z(b|b, W) &= E(I^z(b) - I^z(0) \mid Bite = b, W, D_t = 1) \\
&= E(I^z(b) \mid Bite = b, W, D_t = 1) - E(I^z(0) \mid Bite = b, W, D_t = 1) \\
&= E(I^z(b) \mid Bite = b, W, D_t = 1) - E(I^z(0) \mid Bite = b, W, D_t = 0) \\
&- [E(I^z(0) \mid Bite = b, W, D_t = 1) - E(I^z(0) \mid Bite = b, W, D_t = 0)] \\
&= E(I^z(b) \mid Bite = b, W, D_t = 1) - E(I^z(0) \mid Bite = b, W, D_t = 0) \\
&- [E(I^z(0) \mid Bite = 0, W, D_t = 1) - E(I^z(0) \mid Bite = 0, W, D_t = 0)]
\end{aligned}$$

The last equation only contains quantities that can be estimated from the data. In addition to common support conditions and a no anticipation assumption in $E(I^z(0) \mid Bite = b, W, D_t = 0)$ (individuals who would be treated in $t = 1$ show outcome $I^z(0)$ in $t = 0$), the main identification assumption used in the last step is

$$\begin{aligned}
&E(I^z(0) \mid Bite = b, W, D_t = 1) - E(I^z(0) \mid Bite = b, W, D_t = 0) \\
&= E(I^z(0) \mid Bite = 0, W, D_t = 1) - E(I^z(0) \mid Bite = 0, W, D_t = 0),
\end{aligned} \tag{A-3}$$

i.e. in the treated group, wage growth at different points of the distribution in the absence of treatment would be the same as in the untreated group.

Our motivation for this assumption in our application is as follows. Take the case in which the intensity of treatment is defined by the minimum wage bite at the regional level. In this case, W contains productivity characteristics such as education, experience, occupation, industry etc. Then (A-3) amounts to assuming that wage changes for workers in narrow education/experience/occupation/industry etc. cells evolve in a parallel fashion across different regions in the absence of a minimum wage. If systematic deviations from this assumption are observed in pre-treatment periods, then extrapolations of such trends can be incorporated into the different terms such that (A-3) holds (this is what we do in section 5.4).

Condition (A-3) is identical to the condition identified by Roth and Sant'Anna (2021) to characterize the situation that parallel trends are insensitive to functional form (i.e. to strictly monotonic transformations of the outcome). This condition is a 'parallel trends-type assumption for the cumulative distribution function of untreated potential outcomes' and is stated in Roth and Sant'Anna (2021) for the case of two treatment levels and no covariates: $F_{Y_1(0)|treatment=1}(y) - F_{Y_0(0)|treatment=1}(y) = F_{Y_1(0)|treatment=0}(y) - F_{Y_0(0)|treatment=0}(y)$ (proposition

3.1 in Roth and Sant'Anna, 2021). To see the equivalence to (A-3), recall that cumulative distribution functions of Y are defined as $F(z|\cdot) = E(I^z|\cdot)$. This type of identification condition represents a substantial improvement over earlier approaches to find identification assumptions for distributional treatment effects in that it avoids restrictions on the joint distribution of outcomes in $t = 0$ and $t = 1$ (e.g., Callaway and Li, 2019; Fan and Yu, 2012). It thus easily extends to the cross-sectional case. Note the implication that DR-DiD is automatically invariant to functional form of the outcome, which directly follows from the fact that threshold indicators are unchanged by monotonic transformations, e.g., $1[y \leq z] = 1[\log(y) \leq \log(z)] = 1[y^* \leq z^*]$. This applies to both identification and estimation as both only use threshold indicators as dependent variables (of course, for equivalent estimation results a transformed set of thresholds has to be used).

Note that we impose in our actual application the additional assumption that $ATT^z(b|b, W) = ATT^z(b|b) = \beta_z \cdot Bite$. This entails two substantial restrictions, which we impose for practical and statistical reasons. The first restriction is that the treatment effect is independent of W (homogeneity). In principle, this could be relaxed, but we found this to be difficult both practically and statistically given the many covariates in W. Relaxing this restriction would also substantially complicate the pre-trend analysis (which would have to be carried out separately by subgroups characterized by W). The second restriction is that the treatment effect is linear in treatment intensity. In principle, this could be relaxed by discretizing treatment intensity. However, we found in initial experiments that discretizing the bite variable into a non-trivial number of categories quickly introduces a lot of noise into the estimations. It also complicates the pre-trend analysis considerably. Unfortunately, given the computational limitations we face due to the restricted on-site access to our data bases, we have to abstain from pursuing more flexible approaches in our application. Also note that, despite its limitations, the linear DiD specification is still by far the most widely used model DiD designs with continuous treatment variables (Roth and Sant'Anna, 2021).

B Differences between DR-DiD and RIF-DiD

For the following, also see the discussion in Dube (2019b). The main differences between RIF-DiD and DR-DiD that lead us to adopt the DR-DiD approach in our application is that DR-DiD can deal with discrete mass points and nominal values of the outcome variable, while the RIF approach is based on continuous operations on continuous distributions which rule out these cases. In addition, the RIF approach targets aggregate statistics such as quantiles and inequality measures rather than nominal levels of the outcome variable. If one is interested in particular nominal points of the outcome distribution then one could in principle define the quantiles that correspond to these points. However, this is not possible in a DiD setup as there are multiple time periods (e.g., quantiles will correspond to varying nominal points in the distribution in different time periods). Moreover, modeling quantiles in order to target nominal points would involve unnecessary inversions (from nominal points to quantiles and back) which are not nececssary in the DR approach.

Apart from these aspects, we list the following points to highlight the differences between DR and RIF when applied to a DiD setup. In general, recall that the recentered influence function of a statistic θ is defined as $RIF(y, \theta) = IF(y, \theta) + \theta$, where $IF(y, \theta)$ is the influence function of the statistic θ (Firpo et al., 2009). A difficulty of the RIF approach in the context of DiD is that the RIF regression involves different time periods (pre- and post-treatment) raising the question whether the estimate of θ used to recenter the influence function $IF(y, \theta)$ shall be computed only from the pre-treatment period or from the pooled sample (the latter potentially being affected by the treatment effects). However, given that θ is typically a highly aggregated statistic, the difference between the two cases is probably small in most applications. By contrast, the dependent variables of the DiD regression (i.e. the threshold indicators) do not use any distributional information but only information of the observation itself.

In terms of identification, we showed above that, in order to identify the full distributional treatment effect, the DR-DiD approach needs to make a parallel-trends assumption for each threshold indicator of the outcome distribution. The latter is known to be equivalent to the parallel-trends assumption being independent of the functional form of the outcome variable Y (Roth and Sant'Anna, 2021). One might wonder if the RIF-DiD approach is less restrictive as it only needs to invoke a parallel-trends assumption for the chosen form of the RIF-function. However, to recover the full distributional treatment effect, one has to compute the RIF-regression separately for a comprehensive set of quantiles (each quantile having its separate RIF-function). For this, one has to invoke a separate parallel-trends assumption for each quantile, which is equivalent to assuming parallel-trends assumptions for the set of thresholds that correspond to these quantiles. As a consequence, the RIF-DiD approach is as demanding as the DR-DiD approach if the goal is to identify the full distributional treatment effect.

If one is only interested in the treatment effect on a particular functional θ of the counterfactual distribution, then RIF-DiD indeed only requires to make a parallel-trends assumption for the RIF-function of the statistic of interest $RIF(y,\theta)$. This shortcut is not possible in the DR-DiD approach in which one always first has to identify the full distributional treatment effect and then possibly derives results for functionals from that. On the other hand, using this shortcut in the RIF-DiD approach assumes that the usually highly nonlinear object $\theta = E(RIF(Y,\theta))$ can be well-approximated by a DiD regression model. Depending on the application, this may be more restrictive than assuming that regression models for threshold indicators (or influence functions for individual quantiles) follow a DiD regression structure which then identify the full distributional treatment effect from which results for particular functionals can be derived (as in our empirical application).

C Descriptive statistics

Table A.1 – Descriptive statistics (GSES-sample)

	2014		2018	
Variable	mean	sd	mean	sd
Male				
Yes	0.525	0.499	0.531	0.499
Age				
Age 18-25	0.081	0.273	0.079	0.27
Age 26-30	0.106	0.308	0.108	0.311
Age 31-35	0.109	0.311	0.114	0.317
Age 36-40	0.106	0.308	0.113	0.317
Age 41-45	0.128	0.334	0.109	0.312
Age 46-50	0.162	0.368	0.136	0.343
Age 51-55	0.147	0.354	0.154	0.361
Age 56-60	0.109	0.312	0.125	0.331
Age 61-65	0.052	0.222	0.062	0.241
Educational attainment				
No degree, with or w/o voc. training	0.029	0.169	0.030	0.172
Lower or middle secondary, w/o voc. training	0.080	0.272	0.077	0.266
Lower or middle secondary, with voc. training	0.605	0.489	0.584	0.493
Upper secondary (Abitur), w/o voc. training	0.028	0.165	0.029	0.169
Upper secondary (Abitur), with voc. training	0.126	0.331	0.133	0.339
Diploma/Master degree, PhD	0.132	0.338	0.147	0.354
Tenure with current firm				
Tenure $\leq$ 5 yrs	0.501	0.500	0.521	0.500

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Table A.1 – continued from previous page

Variable	2014 mean	2014 sd	2018 mean	2018 sd
Tenure 6-10 yrs	0.170	0.376	0.170	0.376
Tenure 11-15 yrs	0.116	0.320	0.094	0.291
Tenure 16-20 yrs	0.072	0.258	0.081	0.272
Tenure 21-25 yrs	0.063	0.243	0.048	0.214
Tenure > 25 yrs	0.078	0.268	0.088	0.283
Federal State				
Schleswig-Holstein	0.030	0.170	0.030	0.170
Hamburg	0.028	0.165	0.028	0.166
Lower Saxony	0.091	0.288	0.092	0.288
Bremen	0.010	0.100	0.010	0.099
Northrhine-Westphalia	0.215	0.411	0.211	0.408
Hesse	0.080	0.271	0.079	0.270
Rhineland-Palatinate	0.043	0.203	0.044	0.204
Baden-Württemberg	0.147	0.354	0.153	0.360
Bavaria	0.170	0.376	0.171	0.376
Saarland	0.012	0.111	0.012	0.107
Berlin	0.039	0.193	0.041	0.198
Brandenburg	0.025	0.155	0.024	0.154
Mecklenburg Western Pomerania	0.017	0.129	0.016	0.127
Saxony	0.046	0.209	0.045	0.206
Saxony-Anhalt	0.024	0.152	0.022	0.148
Thuringia	0.024	0.153	0.023	0.150
District Type				
Large urban districts	0.354	0.478	0.354	0.478
Urban districts	0.365	0.481	0.366	0.482
Rural districts	0.154	0.361	0.152	0.359
Sparsely populated / rural districts	0.127	0.332	0.128	0.334
Industry (WZ08)				
Agriculture, forestry and fishing	0.009	0.096	0.009	0.092
Mining and quarrying	0.002	0.046	0.002	0.039
Manufacturing	0.221	0.415	0.215	0.411
Electricity, gas, steam and air conditioning supply	0.007	0.085	0.007	0.083
Water supply; sewerage, waste management and remediation activities	0.009	0.092	0.008	0.090
Construction	0.056	0.230	0.054	0.227
Wholesale and retail trade; repair of motor vehicles and motorcycles	0.156	0.363	0.153	0.360
Transportation and storage	0.058	0.234	0.059	0.236
Accommodation and food service activities	0.046	0.210	0.049	0.217
Information and communication	0.032	0.177	0.035	0.184
Financial and insurance activities	0.033	0.177	0.028	0.166
Real estate activities	0.010	0.099	0.010	0.099
Professional, scientific and technical activities	0.066	0.247	0.068	0.252
Administrative and support service activities	0.083	0.275	0.086	0.281
Education	0.020	0.140	0.020	0.139
Human health and social work activities	0.147	0.354	0.152	0.359
Arts, entertainment and recreation	0.013	0.112	0.013	0.114
Other service activities	0.032	0.177	0.031	0.174
Occupation (KldB10, 2-Digit Code)				

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Table A.1 – continued from previous page

Variable	2014 mean	2014 sd	2018 mean	2018 sd
Agriculture, animal husbandry and forestry occupations	0.007	0.084	0.007	0.081
Horticultural and floricultural occupations	0.008	0.086	0.008	0.087
Raw material extraction and processing, glass and ceramics production and processing	0.004	0.064	0.004	0.062
Plastics manufacturing and processing, woodworking and wood processing	0.017	0.128	0.016	0.127
Paper and printing occupations, technical media design	0.009	0.096	0.009	0.092
Metal production and processing, metal construction occupations	0.043	0.203	0.041	0.197
Mechanical and automotive engineering occupations	0.053	0.223	0.053	0.224
Mechatronics, energy and electrical occupations	0.031	0.172	0.029	0.167
Technical research, development, design and production control occupations	0.032	0.176	0.034	0.180
Textile and leather occupations	0.004	0.066	0.004	0.063
Food manufacturing and processing	0.029	0.168	0.029	0.168
Construction planning, architecture and surveying occupations	0.006	0.077	0.007	0.083
Building construction and civil engineering occupations	0.016	0.126	0.017	0.130
(Interior) finishing occupations	0.012	0.108	0.011	0.104
Building and utility engineering occupations	0.021	0.142	0.020	0.141
Mathematics, biology, chemistry and physics occupations	0.012	0.109	0.011	0.105
Geology, geography and environmental protection occupations	0.001	0.037	0.001	0.038
Computer science, information and communication technology occupations	0.021	0.145	0.024	0.153
Transport and logistics occupations (except vehicle driving)	0.066	0.248	0.067	0.250
Vehicle and transport equipment operators	0.038	0.191	0.037	0.188
Protection, security and surveillance occupations	0.010	0.101	0.011	0.102
Cleaning occupations	0.049	0.215	0.045	0.208
Purchasing, distribution and trade occupations	0.030	0.169	0.030	0.170
Sales occupations	0.074	0.261	0.075	0.263
Tourism, hotel and restaurant occupations	0.034	0.181	0.037	0.188
Occupations in business management and organization	0.131	0.337	0.128	0.334
Occupations in financial services, accounting and tax consulting	0.046	0.209	0.042	0.199
Professions in law and administration	0.014	0.118	0.014	0.117
Medical health professions	0.079	0.269	0.080	0.271
Non-medical health, personal care and wellness occupations, medical technology	0.028	0.164	0.030	0.170
Education, social and domestic professions, theology	0.043	0.202	0.046	0.210
Teaching and training occupations	0.011	0.103	0.010	0.100
Linguistic, literary, humanistic, social and economic professions	0.002	0.044	0.003	0.051
Advertising, marketing, commercial and editorial media occupations	0.016	0.126	0.018	0.132
Product design and arts and crafts occupations, fine arts, musical instrument making	0.002	0.044	0.002	0.044
Performing and entertainment occupations	0.004	0.063	0.004	0.062
Union coverage				
No coverage	0.615	0.487	0.636	0.481
Sectoral agreement	0.310	0.462	0.293	0.455
Plant / firm agreement	0.043	0.204	0.042	0.200
Company agreement	0.032	0.177	0.030	0.170
Participation of the public sector				
Yes	0.072	0.259	0.063	0.243
Firmsize				
< 10 empl.	0.152	0.359	0.141	0.348
10 to 49 empl.	0.244	0.430	0.246	0.431
50 to 99 empl	0.106	0.307	0.104	0.305
100 to 249 empl.	0.133	0.340	0.139	0.346

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Table A.1 – continued from previous page

Variable	2014 mean	2014 sd	2018 mean	2018 sd
250 to 499 empl.	0.094	0.291	0.095	0.293
500 to 1,000 empl.	0.075	0.263	0.074	0.262
> 1,000 empl.	0.197	0.398	0.200	0.400
Sectoral minimum wage				
Yes	0.247	0.431	0.244	0.430
No	0.599	0.490	0.612	0.487
Unknown	0.154	0.361	0.144	0.351
Number of observations				
	708,081		693,827	

Source: GSES 2014 and 2018 and own calculations. Survey weights have been used for all calculations.

D Additional figures

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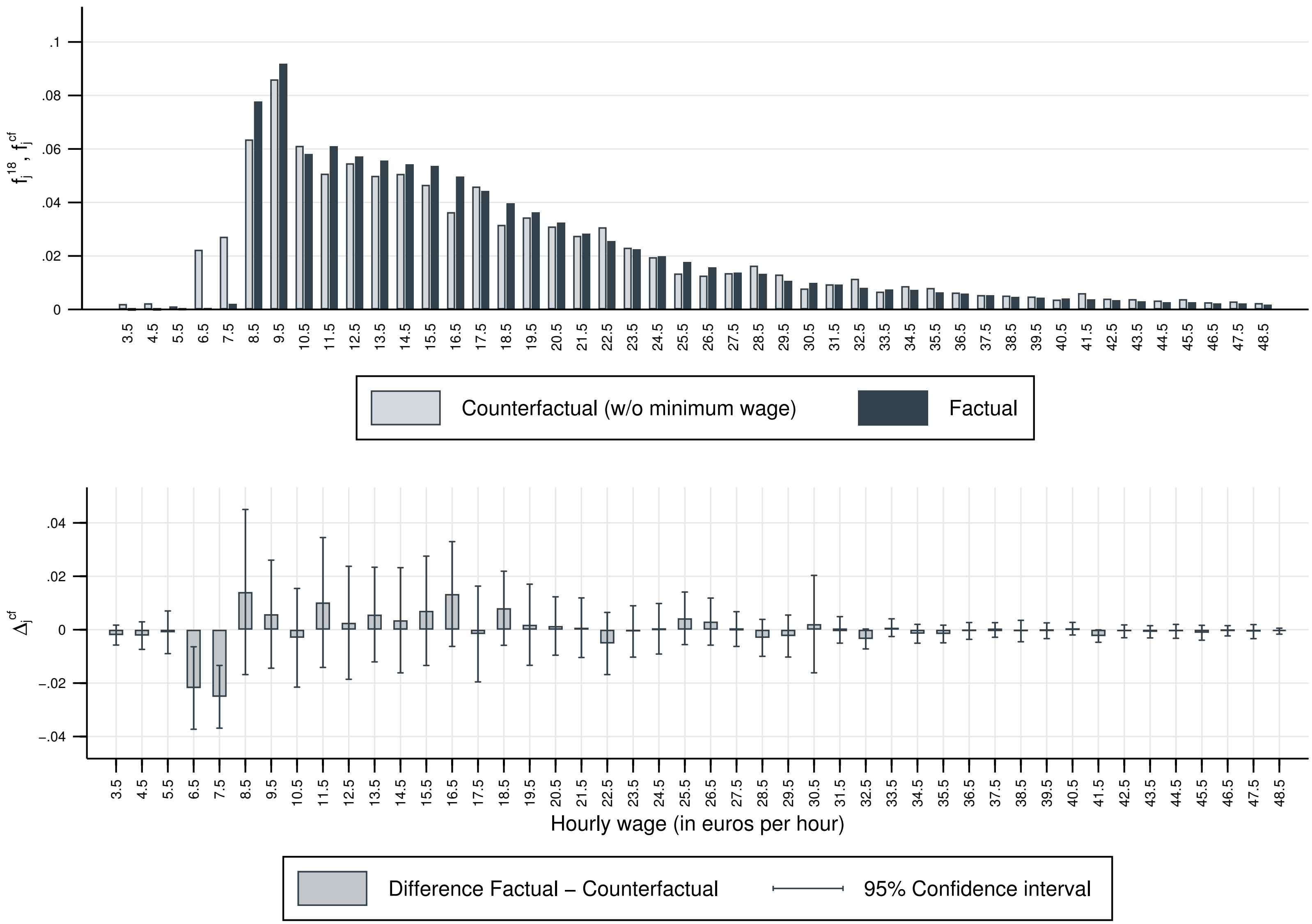
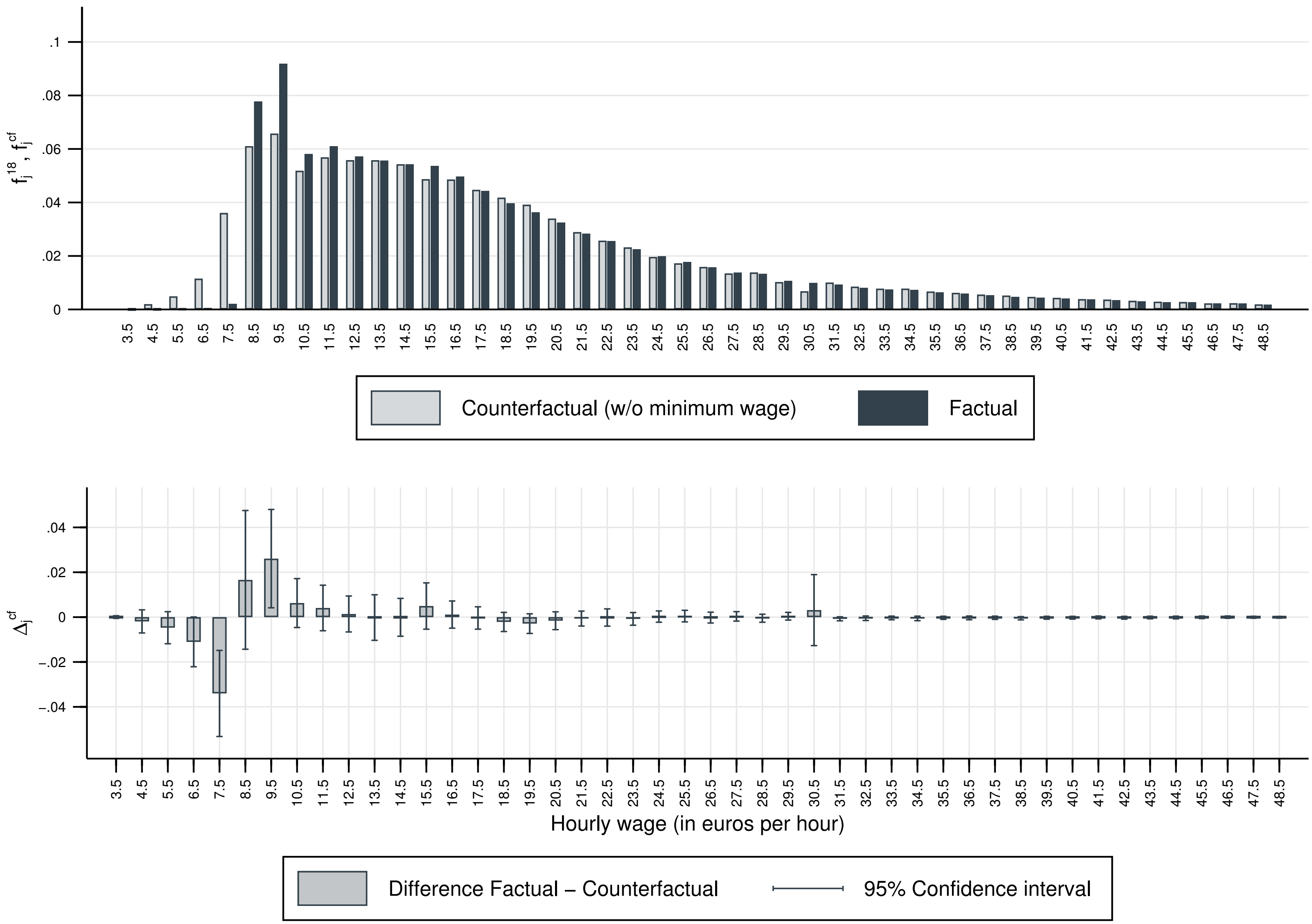
Figure A.1 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 1: Regions. Trend adjustment (two years extrapolation).



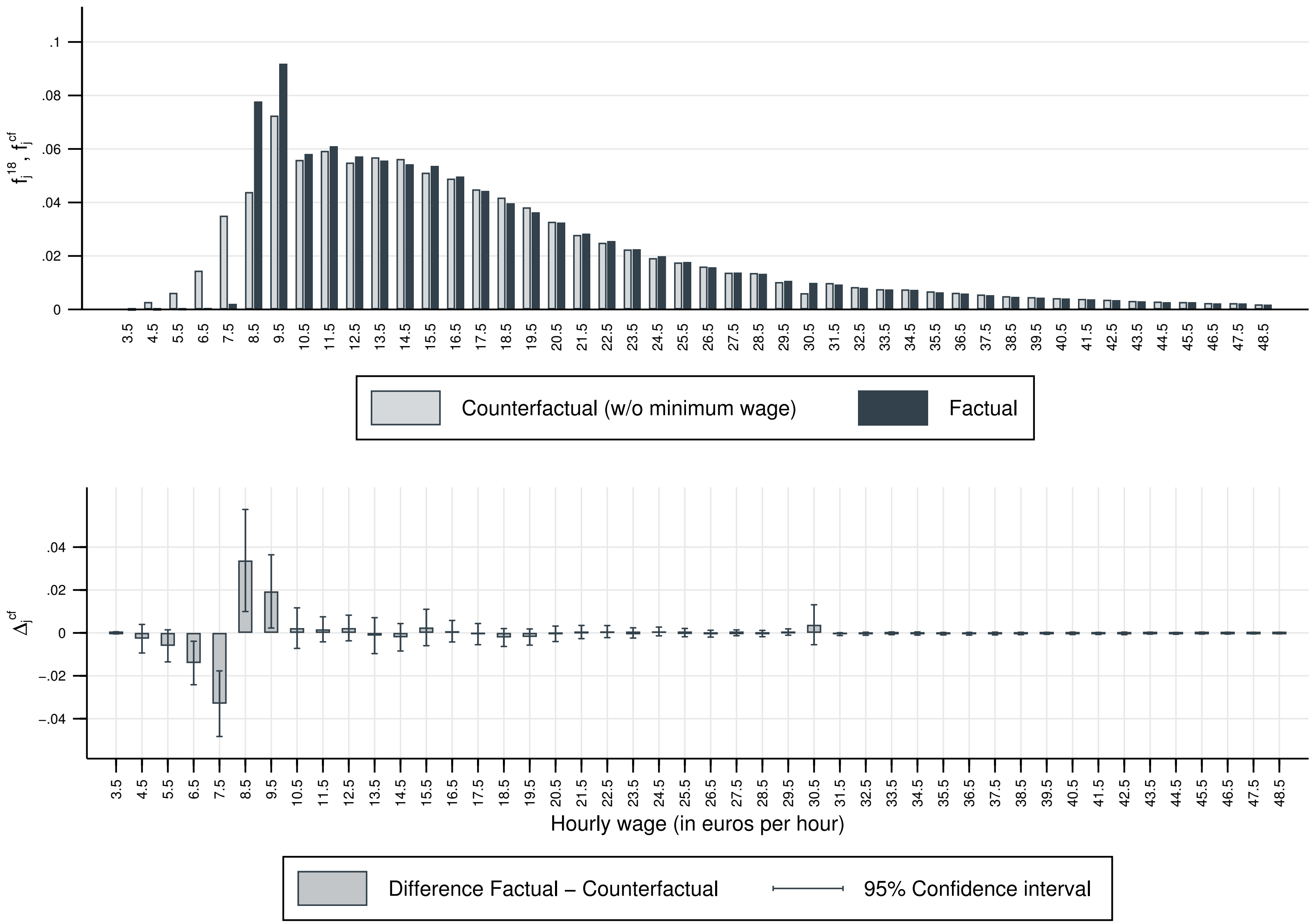
Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. Results are trend-adjusted as specified in the main text. Last bin of adjustment using *DGUV-IAB* data is 29.5 leading to larger confidence interval at the 30.5 due to jump in implied distributional mass in the counterfactual case. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018, and *DGUV-IAB* sample covering the years 2011–2014; own calculations.

Figure A.2 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 2: Augmented occupations. Trend adjustment (two years extrapolation).



Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. Results are trend-adjusted as specified in main text. Last bin of adjustment using *DGUV-IAB* data is 29.5 leading to larger confidence interval at the 30.5 due to jump in implied distributional mass in the counterfactual case. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018, and *DGUV-IAB* sample covering the years 2011–2014; own calculations.

Figure A.3 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 3: Augmented industries. Trend adjustment (two years extrapolation).



Notes: Bins are left-closed and right-open. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.5) euros per hour. Results are trend-adjusted as specified in main text. Last bin of adjustment using *DGUV-IAB* data is 29.5 leading to larger confidence interval at the 30.5 due to jump in implied distributional mass in the counterfactual case. 95% bootstrap confidence intervals (100 replications, clustered at treatment level).
Source: *GSES* waves 2014 and 2018, and *DGUV-IAB* sample covering the years 2011–2014; own calculations.