Using distribution regression difference-in-differences to evaluate the effects of a minimum wage introduction on the distributions of wages, hours and earnings[1]

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Abstract. We evaluate the causal effects of the introduction of the German national minimum wage on the distributions of hourly wages, hours worked and monthly earnings. Our results show that the minimum wage led to large spill-overs up to 50-70% above its nominal level, but to no significant changes in working hours. The minimum wage explains most of the recent fall in wage and earnings inequality in Germany, but inequality would have stagnated or even fallen without its introduction. We document significant pre-trends that lead to an overestimation of minimum wage effects if not accounted for. As a methodological contribution, we provide a transparent treatment of distribution regression difference-in-differences (DR DiD) as well as a comparison of alternative bite measures.

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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. The introduction of the German minimum wage at the level of 8.50 euros/hour constituted a major policy experiment: over 4 million workers (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, 2023, and literature review below), it is an open question to what extent the introduction of the minimum hourly wage had a causal effect on the actual distribution of hourly wages. The main aim of a minimum wage is to shift distributional mass from below to its level, leading to a spike of the wage distribution at the minimum wage. However, because of potential spill-overs, its effects on the wage distribution may go beyond also shifting the wage distribution above the minimum wage (Brochu et al., 2023). A key challenge is to separate the causal effect of the minimum wage on the wage distribution from changes in the wage distribution that would have happened anyway, i.e., from trends in wage setting policies of employers, in labor supply, and in individual-level or collective wage bargaining all of which may have started before the minimum wage introduction.

The estimation of the effects of the minimum wage on the distribution of hourly wages has to address a number of issues. First, precise information on hourly wages and hours worked is often hard to obtain. Such information is particularly scarce in Germany. The well-known administrative databases 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 measurement error in self-reported wages and working hours, possibly leading to the estimation of noisy effects, or spurious spill-overs that may be artifacts of measurement error (Autor et al.,

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

2016). Second, popular methods 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 makes the following contributions. First, while there exists a considerable literature on the effects of the German minimum wage on various outcomes (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 databases. 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 database that includes information on hourly wages and working hours both before and after the minimum wage introduction – and from the administrative *Deutsche Gesetzliche Unfallversicherung (DGUV-IAB)* database, which includes information on wages and working hours, but only for a number of years before the minimum wage introduction. Both databases are considered highly reliable because a firm's participation in the GSES is compulsory and the information on wages and hours is typically based on the firm's internal accounting system.[3]

We show how to combine the two databases GSES and DGUV-IAB for a difference-in-differences analysis without physically combining them (which would be forbidden under German data protection laws). Compared to previous contributions, this allows us to reliably separate the effects of the minimum wage introduction on prices (= hourly wages) from those on quantities (= hours worked). It is well known that minimum wages may potentially not only change hourly wages but also working hours. For example, firms may reduce working hours for low-wage employees to keep overall wage bills constant (Stewart and Swaffield, 2008; Bossler and Schank, 2023), or there may be shifts between part-time and full-time employment as a reaction to changed constraints on low-wage employment (Garloff, 2019). Our results thus address conflicting findings

[3]The DGUV-IAB data were also used by Dustmann et al. (2022), also see vom Berge et al. (2023). Administrative IAB data on monthly earnings were used to study minimum wage effects by Bossler and Schank (2023). Caliendo et al. (2018), Burauel et al. (2019), Burauel et al. (2020) and Caliendo et al. (2023) studied minimum wage effects based on survey data from the German Socio-Economic Panel (GSOEP). Caliendo and Wittbrodt (2022) 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).

from German administrative data (Bossler and Schank, 2023, who find significant effects of the minimum wage on monthly earnings), and German survey data (Caliendo et al., 2023, who argue that hourly wage increases did not translate into significant changes in monthly earnings because working hours were adjusted downwards).

As a second methodological contribution, we use a distribution regression difference-in-differences strategy (DR-DiD). A small number of previous contributions have carried out calculations similar 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 for the whole distribution 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 (2023) 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 discontinuous 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. This allows us to estimate how much inequality in hourly wages has fallen, as measured by, say, the reduction in the Gini coefficient. By contrast, most previous contributions focused either on effects on the mean or on particular points of the distribution (e.g., Bossler and Gerner, 2020; Burauel et al., 2019; Dustmann et al., 2022; Caliendo et al., 2023). An exception is the study by Bossler and Schank (2023) 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 allows us to assess the robustness of our findings with respect to alternative spill-over channels and violation of the no-pre-trends assumption.

The remainder of this paper is structured as follows. Section 2 provides a brief literature review. 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. (2021) 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-overs 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, 2023). Burauel et al. (2019) present evidence based on the German-Socio Economic Panel (GSOEP) suggesting excess hourly wage growth for low-wage workers. Also based on survey data from the GSOEP and using a regional bite measure, Caliendo et al. (2023) find positive wage effects for the bottom hourly wage quintiles, but no significant effects on monthly earnings which they attribute to working hours adjustments. 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. Burauel et al. (2019), Dustmann et al. (2022) and Caliendo et al. (2023), 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 (2023) provide such a full distributional analysis but for the distribution of monthly earnings. In contrast to Bossler and Schank (2023), our study focusses on the direct effect of the minimum wage on the distribution of hourly wages which cannot be inferred from the administrative data.

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 U.S. minimum wage reduces hours worked for those paid at the minimum wage level with an elasticity of -0.3, but has no effect 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, 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 database 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 database from the *German Social Accident Insurance (DGUV)* whose working hours information can be linked to IAB data on employment histories for a number of years before the minimum wage introduction (2011 to 2014).

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), respectively. 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 agency. 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. Sample weights ensuring the representativeness 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.

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 part of 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 SA2 in the supplementary appendix).

3.3 Bite measures

Our difference-in-differences approach relies on 'bite' measures reflecting the extent to which the minimum wage was going to affect certain subgroups of workers 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 the post-policy observation period is 2018, we compute the bite based on the minimum wage level of 8.84 euros/hour in that year (in 2017, the minimum wage was increased from the original level of 8.50 to 8.84 euros/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 database.

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).[4]

Augmented occupations

An alternative bite measure can be defined at the level of the occupations (e.g., Friedrich, 2020). 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 extent, wages changed differentially in occupations that were affected to a higher or lesser extent by the minimum wage introduction. Given the obvious importance of East-West differences, we augment the categorization according to 2-digit occupation codes (KldB10) by

[4]Figure SA1 in the supplementary appendix provides an overview of minimum wage bites across regions.

the information of whether the person worked in East or in West Germany. This yields a total number of 72 different groups.

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 the East/West information comprises 146 different groups.[5]

An overview of our alternative bite measures is given in table 1. Using alternative bite definitions is 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 database 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 working

[5] In supplementary appendix SA1, we provide an overview of the regions, occupations and industries with the highest/lowest bite values.

hours information requires some pre-processing steps (see Dustmann et al., 2022; vom Berge et al., 2023, and supplementary appendix). As the wage data in the administrative employment data are top-coded, we only consider monthly earnings up to 4050 euros per month and hourly wages up to 30 euros/hour.

4 Econometric method

Our aim is a full distributional analysis of the effects of the minimum wage introduction on the distributions of hourly wages, hours worked, and monthly earnings. 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 (2023). 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 distribution of hourly wages 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.[6] 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. Both arguments equally apply to the distribution of weekly working hours which is known to be highly discrete and discontinuous.[7]

[6] By contrast, Dube (2019b) considers minimum wage effects on the distribution of *family incomes*, while Bossler and Schank (2023) focus on the distribution of *monthly earnings*. Both distributions are close to be continuous as minimum wage earners are spread over wide regions in these distributions.

[7] The method proposed by Brochu et al. (2023) is an alternative to our distributional analysis of the impact of a minimum wage on the distribution of hourly wages. The method uses standard flexible econometric models for the specification of the hazard rate of a distribution (usually applied to duration analysis) to estimate the effect on the wage distribution. By using a flexible specification of the baseline hazard in the wage dimension and applying the model for discretized wage bins, the method allows to estimate the shifts in the distribution at various levels wages (below the minimum wage, at the minimum wage and slightly above, and at higher wage levels) similar to our method. The method is implemented by Brochu et al. (2023) to estimate 'triple-differences' estimates of the minimum wage effects relative around the minimum wage. The method provides a simple comprehensive model of the conditional wage distribution in a very flexible way. Our modelling approach is more flexible in modelling the impact of the covariates on the distribution function in a distinct way at each wage level. However, the two

In order to address these aspects, we explore the use of a distribution regression difference-in-differences approach (DR-DiD). The distribution regression approach (DR) as 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 similar 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 the appendix to this paper 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 stated by Roth and Sant'Anna (2023). Roth and Sant'Anna (2023) derived this condition as a characterization of the assumption that the parallel trends assumption on the outcome is insensitive to functional form and that the underlying data generating process involves random assignment and stationary potential outcomes. The statement of the problem as a distribution regression naturally allows for the estimation of possible pre-trends, which we will correct for as described below in section 4.2.[8]

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

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

where y_{igt} represents the observed outcome of interest of individiual i in bite group g at time t. The outcomes of interest in our case are either hourly wages, hours worked, or monthly earnings. The values z refer to a fine set of thresholds in the outcome distribution. For the case of hourly wages, we define the set $z \in \mathcal{W}$ such that we obtain wage

modelling approaches are not nested and a careful specification analysis would be necessary to investigate which model fits the data in a better way.

[8]Supplementary appendix SA1 includes a discussion of further differences between the RIF-DiD and the DR-DiD approach, which may be of interest when deciding which method is best suited for an application.

bins [0; 3.49], [3.50; 4.49], ... , [48.50, 49.49] (after rounding hourly wages to the next integer cent value). Equation (1) describes the fraction of individuals with characteristics $(Bite_g, D_g, D_t, X_{igt})$ whose wage is less than or equal to threshold z. For the case of weekly hours worked, we first round hours to the largest integer below or equal and then define thresholds such that we obtain eight hours categories [0; 6], [7, 11], ... , [42; 50]. For monthly earnings we define an equally spaced set of thresholds ranging from 50 to 7,450 euros with a stepsize of 100 euros. For simplicity, the subsequent description of the econometric approach focuses on the hourly wage as the outcome of interest.

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 up to z between the different bite groups g. For example, if the bite is defined in terms of regions, D_g controls 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 include a large set of observed characteristics X_{igt} which are also strong determinants of whether the observed wage does not exceed the threshold z. The characteristics considered are those shown in table SA2 in the supplementary appendix (naturally, for a given bite specification, the characteristic on which it is based, i.e., region, occupation or industry, is not included in X_{igt} as it is already included in D_g). In a sensitivity analysis, we will vary the set of characteristics X_{igt} used for conditioning, including the case in which we only specify the DiD terms $D_g\gamma_z$, $\lambda_z D_t$ and $\beta_z(Bite_g \times D_t)$, but no extra conditioning variables X_{igt}. The parameters in (1) are estimated by weighted least squares using the sample weights.

We model a 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 observable 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 up to z in period 1 in the absence of the minimum wage is given by

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

i.e., the effects on the fraction of wages up to z, that are solely due to the differential exposure 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 our linear separable specification. 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 4.2, we will 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 period $t = 1$ 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 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 the appendix 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 the conditional analogue of an identification condition recently studied by Roth and Sant'Anna (2023), who show that this condition is equivalent to assuming that a parallel trends assumption is insensitive to functional form of the outcome and that the data generating process is a combination of random assignment and stationary potential outcomes. As we argue in more detail in the appendix, conditioning on a large number of observables and carefully addressing potential time effects (including those constructed from trends observed in pre-periods) make these conditions credible, thus securing 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 ordered thresholds $z \in \{z_0, z_1, \dots, z_J\}$ defining J intervals $(z_{j-1}, z_j]$ $(j = 1, ..., J)$ by

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

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

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

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

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 wage interval. 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. Reassuringly, these formulae based on the group information 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-trends: Estimation and Correction

The identification of the counterfactual wage distribution (4) is only valid if there are no other time trends in wages that differ 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 than in low-bite regions as this would make the wage boosting effect of the minimum wage introduction appear higher than it was. To estimate potential differences in wage growth across different bite levels before the minimum wage introduction, we run regressions as in (1) for the pre-introduction period 2011 to 2014

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

(compare Dobkin et al., 2018; Ahlfeldt et al., 2018; Freyaldenhoven et al., 2021, for the non-distributional case).

Here, we define the year $t = 2014$ as the reference period so that all coefficients concerning 2014 are normalized to zero (i.e., $\lambda^{2014} = 0, \beta_z^{2014} = 0$). 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 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/hour declined in high-bite regions in a systematic way 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). Section 5.1 shows the estimated patterns of $\beta_z^{2011}, \beta_z^{2012}, \beta_z^{2013}$ in the pre-treatment period. In our application, the pre-trends follow linear time trends almost exactly, which we then use for counterfactual trend extrapolation.[9]

Formally, let $\bar{\Delta}_z$ denote the extrapolated effect of the pre-trend for wage threshold z. Then, the counterfactual wage distribution in the absence of the minimum wage *corrected for pre-trends* is

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

given by

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

i.e., the extrapolation of the pre-trend has to be subtracted from the estimated effect of the bite. In section 5.1, 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-trend lasts up to one year after the minimum wage introduction, and $\bar{\Delta}_z^2$ up to two years afterwards.

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 for the bite groups defining the treatment units (Bertrand et al., 2004). Unfortunately, the data on which our analysis are based upon can only be accessed on-site with substantial computational limitations that make the use of the computationally more involved logit or probit models, as suggested by Chernozhukov et al. (2013), for (1) infeasible. However, we found in preliminary experiments with logit models that the factual and counterfactual *unconditional* distributions (3) and (4) were basically insensitive to the use of different models (linear proability vs. logit models) and/or covariate specification choices (inclusion/exclusion of covariates and interaction terms). This is not surprising given the large amount of averaging involved. 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 Effects on hourly wages

Figures 1 to 3 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 that would have prevailed had the minimum wage not been introduced. The middle panels display the differences in the factual and the counterfactual frequencies of hourly wages in each bin of the upper panels as defined in eq. (9) without correcting for pre-trends. The lower panels display the differences when including the pre-trend correction.

The results based on the regional bite definition are presented in figure 1. The dark bars for the factual distribution in the upper panel suggest that the minimum wage was highly effective in elminiating hourly wages below its nominal level (then 8.84 euros/hour). The light bars in the upper panel of figure 1 depict the situation that would have prevailed under a hypothetical hourly 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 shown in the middle panel visually demonstrates how the minimum wage shifted wages from below its level to wage bins above it. Apart from the fact that very low hourly wages were effectively eliminated, the results imply sizeable and significant spill-over effects up to 16.5 euros/hour which is more than 80% above the nominal level of the minimum wage.[10] Also note the precisely measured zero effects for higher wage bins which can be interpreted as a validation check for our method because we would not expect causal effects of the minimum wage on very high wages.

[10]Some authors have noted that the GSES might overstate compliance with the minimum wage as employers may be hesitant to report non-compliant hourly wages, see the discussion in Mindestlohnkommission (2020) and Bachmann et al. (2020). While we cannot completely rule out such a possibility, we would expect that such employers would adjust reported working hours rather than manipulate the legally more relevant monthly pay figures. In this case, one would observe significant changes in reported working hours in high bite groups, which is not the case (see section 5.2).

— Figures 1 to 3 around here —

Figures 2 and 3 show the corresponding estimates based on bite differences across occupations (bite 2) or industries (bite 3), each augmented by the East/West distinction. The overall pattern looks quite similar to the one in figure 1, but significant spill-over effects are less spread out, ranging only to 12.5 euros/hour (there are positive point estimates also for higher wage bins, but these are not or only marginally significant). The fact that measured spill-over effects are larger for the regional bite definition suggests that there may be spill-over effects within regions which are not being picked up by the other two bite definitions.

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 the fraction of low wages had fallen more strongly in high-bite regions than in low-bite regions even before the minimum wage introduction, this trend would have been likely to continue after the minimum wage introduction. Then, one would incorrectly attribute part of the wage increases after 2015 in the lower tail of the distribution to the minimum wage. In this section, we demonstrate that such pre-trends indeed existed and we correct for them.

The estimates of the pre-introduction coefficients of the bite variable are shown in figure 4 (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 trend in the likelihood for a wage up to z before the minimum wage introduction should not have been systematically different in high-bite compared to low-bite groups. 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.

— Figure 4 around here —

For example, take the case of $z = 8.49$ euros/hour in the upper panel of figure 4 (solid line). In the 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). In a conservative approach focusing on the local behavior around the minimum wage introduction, we only use the years 2012 to 2014 to fit the pre-trend and extrapolate up to two years after 2014. The values of the extrapolated trend at 2015 and 2016 are therefore the values $\bar{\Delta}_z^1$ and $\bar{\Delta}_z^2$, which we have to subtract from the coefficient of the bite effect after 2014, because these represent by how much the fraction of wages up to z would have declined in high-bite compared to low-bite regions by the differential time trends alone (eq. (12)).

As figure 4 shows, there were systematic differential time trends years before the minimum wage introduction that are *uniform across all bite definitions*. Incidentally, the strength of the pre-trends is increasing constantly up to the 8.50 euro/hour threshold and then decreasing above. 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 fact that the observed patterns are uniform across the alternative bite definitions suggests 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 even in the years preceding the minimum wage introduction – and, incidentally, the effect was strongest around the minimum wage. Note that this pattern is not driven by East/West differences (detailed results are available upon request).

To what extent does the existence of these pre-trends change our estimated effects of the minimum wage? The lower panels of figures 1 to 3 show that the magnitude of the measured minimum wage effects is reduced by accounting for pre-trends, but only to a limited extent. 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 are limited. Still, we conclude that the impact of the minimum wage is somewhat overestimated if pre-trends are not accounted for.

The patterns in figures 4 can be interpreted as evidence against anticipation effects as there existed basically linear trends since at least the year 2012, which did not accelerate in the year 2014. For a discussion of potential anticipation effects of the German minimum wage, see Bossler (2017). As mentioned above, our GSES wage measure refers to April 2014. The parliament decided about the introduction of the minimum wage in July 2014 after intensive political debates earlier in the year. Recall, however, that the minimum wage did only come into force on January 1, 2015. Generally, it is unclear why employers should pay higher wages long before the introduction 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). Based on IAB data for daily earnings averaged over employment spells until the end of 2014, Bossler and Schank (2023) also find little evidence for anticipation effects in 2014. Note that their data cover the whole year 2014, whereas we consider wages reported for April 2014 only.

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 -0.020, see column two). Note that this was the first decline in hourly wage inequality after a long period of substantial increase (Biewen and Seckler, 2019).

— Table 2 around here —

Further results in column two of table 2 show that, depending on the bite measure, the drop by -0.020 Gini points was more than fully explained by the minimum wage if one does not apply the trend adjustment (-0.035 for the regional, -0.027/-0.026 for the occupational/industry bite). As already suggested by the graphical analysis, the pre-trend correction results in lower minimum wage effects. Still, applying the two-year trend adjustment suggests that the minimum wage either fully or largely explained the drop in hourly wage inequality between 2014 and 2018 (-0.022 for the regional bite, -0.017 for the occupational/industry bite). These results suggest that, while the introduction of the minimum wage causally reduced wage inequality, the inequality trend 2014 to 2018 would already have been flat without its introduction, implying that the minimum wage was not the only factor breaking the long-term trend of increasing wage inequality.

Note however, that this conclusion depends on the inequality measure chosen. For the Q90/Q10 and the Q50/Q10 ratio (columns three to five of table 2), the inequality reducing effect of the minimum wage exceeds the actual fall in inequality, suggesting that inequality as measured by these indices would have risen without the minimum wage. Columns three to five of table 2 also indicate that the introduction of the minimum wage specifically reduced inequality in the lower half of the hourly wages distribution as measured by the Q50/Q10 ratio. For the regional bite definition, there is also a significant effect for the upper half (as measured by the Q90/Q50 ratio) owing to the fact that measured spill-overs are stronger and even reach beyond the median hourly wage (around 16 euros/hour in 2018).

5.2 Effects on hours worked

We consider three different subgroups reflecting potential differences in how working hours might react to the minimum wage introduction: i) workers with hourly wages below 12 euros/hour, ii) workers with hourly wages between 12 and 16 euros/hour, and iii) workers with hourly wages over 16 euros/hour. In the following, we will only discuss selected results for groups i) and ii). For group iii), we obtain sharply measured zero effects throughout, which we document in full in the supplementary appendix. Again, the minimum wage effects being zero for high-wage earners can be interpreted as a validity check for our estimation procedure.

— Figure 5 around here —

Figure 5 presents the results for the worker group with hourly wages below 12 euros/hour. For the regional bite definition, there are marginally significant positive effects for the fraction of hours worked in the interval 12 to 19 hours per week, and marginally significant negative effects for 42 to 50 hours per week (second panel of figure 5). By contrast, the effects are insignificant and very close to zero in all other remaining cases for the regional bite and in all cases for the other two bite definitions used (third and fourth panel of table 5). The corresponding results for the group with hourly wages between 12 and 16 euros/hour shown in figure 6 also shows insignificant zero effects across all hours bins and bite definitions.

There are less pronounced pre-trends for hours worked than for hourly wages (see supplementary appendix figures SA20/SA35 as well as figures SA21/SA36). They are almost flat and largely insignificant for the occupational and industry bite. For the regional bite, some more pronounced but less regular patterns emerge and correcting for pre-trends results in zero effects for almost all hours bins but in conspiciously large effects for some bins (figures SA17 and SA32) which follow no plausible pattern. By contrast, correcting for pre-trends in the cases of the occupational bite and the industry bite confirms the previous finding of no effects of the minimum wage (lower panels of figures SA18/SA33 and SA19/SA34).

We assign more credibility to the results based on the occupational and industry bite as there are essentially no pre-trends for these two bite definitions in the three years before the minimum wage introduction (figures SA20/SA35), making these results credible even without pre-trend correction. Presumably, the visible but not very regular trend patterns for the regional bite represent differential hours developments across regions that are unrelated to the introduction of the minimum wage and that may be hard to represent by a simple trend-adjustment.

In sum, our results suggest no effects of the minimum wage on the distribution of working hours four years after its introduction based on the occupational and the industry bite. While we find statistically significant effects for two specific hours bins (12 to 19 hours, 42 to 50 hours) for the regional bite, the results correcting for pre-trends are more volatile and implausible for this case. These would be hard to rationalize and do not show up (neither in sign nor in magnitude) in the other two bite definitions, which provide a consistent picture of insignficant effects across all hours categories and worker groups. If there were causal effects of the minimum wage on the distribution of working hours, then these should also show up if the exposure to the minimum wage is measured at the occupation or industry level (which feature some particularly highly affected groups, see table 1).

5.3 Effects on monthly earnings

The results for monthly earnings are shown in figures 7 to 9. The upper panels show factual and counterfactual monthly earnings distributions for 2018, while the two lower panels display the causal effects of the minimum wage introduction on the earnings distribution in terms of differences at each bin (with and without pre-trend correction). Figure 7 for the regional bite suggests that the introduction of the minimum wage benefitted workers with very low monthly earnings (up to 450 euros per month, marginal part-time) but to a limited extent. The main effect was for workers in the lower middle part of the distribution (850 to 1,450 euros per month), whose earnings shifted to levels around and above the median (2,472 euros per month in 2018, compare middle panel of figure 7). The pattern looks very similar when using the occupational and industry bite but the measured effects are noticeably weaker, again suggesting that spill-over mechanisms may be stronger at the regional than at the occupational or the industry level.

— Figures 7 to 9 around here —

Similar to the hourly wage case, we find clear and significant pre-trends that are basically linear in the three years before the introduction of the minimum wage (see figures SA65 to SA71 in the supplementary appendix). Accounting for these pre-trends significantly weakens the observed effects on the earnings distribution, see lower panels of figures 7 to 9.

Table 3 summarizes the effects of the minimum wage introduction on earnings inequality. Ignoring pre-trends, the fall in the Gini by -0.020 is fully explained by the minimum wage when using the regional bite (-0.020) but only half explained when using the occupational or industry bite (-0.012, see second column of table 3). After pre-trend correction, these contributions shrink to -0.012 for the regional bite and to -0.006 for the occupational and the industry bite. Columns three and four of table 3 show that the minimum wage significantly reduced both the Q90/Q10 and the Q90/Q50 ratio as both median and very low earnings increased. The last columns of table 3, suggest an *increase* of the Q50/Q10 ratio when using the regional bite, but a *decrease* based on the other two bite definitions. The explanation for this finding is that the stronger spill-overs in the case of the regional bite led to a more pronounced increase in median earnings compared to the two other bite definitions. The opposing effects lose statistical significance when correcting for pre-trends. Taken together, our results for monthly earnings are very much in line with those in Bossler and Schank (2023) who also find some but limited effects at the bottom of the distribution, no effects in the range between the bottom and the lower middle of the distribution, and the strongest effects of the minimum wage in the lower middle of the earnings distribution up to the median.

— Table 3 around here —

5.4 Sensitivity analysis

Given the possibility that the minimum wage potentially also changed worker characteristics on which we condition in our analysis, we investigate the sensitivity of our results with respect to varying sets of conditioning variables (see supplementary appendix for full set of results). A point in case are re-allocation effects between firms as studied by Dustmann et al. (2022). In a first sensitivity analysis, we therefore omitted all firm variables from our conditioning set. This led to almost identical estimates in all scenarios. We therefore conclude that our distributional results are robust with respect to re-allocation effects between firms. In a second sensitivity analysis, we omitted *all* conditioning variables from our distribution regressions (i.e., we included only the time and group effects as well as the interactive difference-in-differences terms). This led to small changes in estimated effects, but left all our qualitative conclusions unchanged. Omitting conditioning variables led to somewhat more pronounced inequality reducing effects of the minimum wage on the distributions of hourly wages (compare table 2 to table SA7 in the supplementary appendix) and monthly earnings (compare table 3 to table SA10 in the supplementary appendix). Another natural consequence of not conditioning on covariates was the noticeably lower precision of the point estimates (conditioning reduces the error variance). Note that conditioning on covariates also eliminates effects of compositional changes in workforce characteristics on the outcome distribution. Given that there were only few compositional changes between 2014 and 2018 (see table SA2 in the supplementary appendix), it is not surprising that results change only little when not conditioning on characteristics.

6 Discussion and conclusion

This paper analyzes the effects of the German statutory minimum wage on the distributions of hourly wages, hours worked and monthly earnings. Our analysis is based on the German Structure of Earnings Survey (GSES) and administrative DGUV-IAB data, which are the only large-scale databases for Germany including information on hourly wages and working hours both before and after the minimum wage introduction. As a transparent methodological approach, we suggest to use difference-in-differences distribution regressions (DR-DiD) based on different bite measures for a full distributional analysis of minimum wage effects, while accounting for discrete mass points and changing nominal target values in these distributions.

Our results imply that the introduction of the minimum wage in 2015 caused low hourly wages to rise above its value and that it resulted in significant spill-over effects up to 50-70 percent above. 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 wage inequality fell between 2014 and 2018, counteracting a long-term trend until 2010 in rising inequality in hourly pay and monthly earnings (Antonczyk et al., 2010; Biewen and Seckler, 2019; Bossler and Schank, 2023). Our results suggest that the introduction of the minimum wage explains a large part of the fall in inequality, depending on the inequality measure used. However, inequality as measured by the Gini would not have increased between 2014 and 2018 in the absence of the minimum wage and, for the lower part of the distribution, we demonstrate that wage growth was already higher in groups that were later most affected by the minimum wage. The latter effect leads to an overestimation of minimum wage effects if these pretrends are not corrected for. All this may suggest that the minimum wage was not the only factor stopping the long-term trend of rising wage inequality. Such an interpretation is consistent with evidence in Biewen and Seckler (2019) who showed that de-unionization and compositional changes with respect to educational qualifications and work experience were responsible for rising inequality before 2014, but that the effect of these inequality drivers flattened out in the years before 2014.

Our comparison of alternative bite measures suggests the existence of substantial regional spill-overs in wage determination as distributional effects on hourly wages and monthly earnings follow exactly the same patterns when a bite definition based on occupation or industry is used but lead to noticeably larger distributional impacts when the bite is defined at the regional level. On the other hand, our pre-trend analysis for working hours suggests that using a regional bite may pick up unsystematic and volatile idiosyncratic differences between regions that are unrelated to the minimum wage. As to hours worked, the evidence points to no systematic effects of the minimum wage introduction on the distribution of weekly working hours that can be causally linked to the exposure to the minimum wage at the time of its introduction.

Our conclusion is that the minimum wage changed prices (= hourly wages) but not quantities (= hours worked) so that the hourly wage increases – which we show to have affected hourly wages substantially above the minimum wage – directly fed into the monthly earnings of workers. We show that these effects were strongest for workers not located at the very bottom, but in the lower middle and the middle of the earnings distribution, ranging to levels substantially above the median. However, effects on the distribution of monthly earnings also look weaker than the direct effects on the distribution of hourly wages, especially after trend-adjustment. Our results thus reconcile conflicting findings in the previous literature based on German administrative data (Bossler and Schank, 2023, who find significant effects of the minimum wage on monthly earnings), and German survey data (Caliendo et al., 2023, who do not find significant effects on monthly earnings, arguing that hourly wage increases did not translate into changes in monthly earnings because working hours were adjusted downwards).

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8 Tables

Table 1 – Bite descriptive statistics

	Bite 1: German regions	Bite 2 Occupations+East/West	Bite 3: Industry+East/West
# 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

Source: GSES 2014, own calculations.

Table 2 – Minimum wage effects on inequality in hourly wages, 2014 vs. 2018

	Mean	Gini	Q90/Q10	Q90/Q50	Q50/Q10
2014	16.247 (0.241)	0.260 (0.002)	3.504 (0.073)	2.015 (0.031)	1.739 (0.012)
2018	17.740 (0.208)	0.240 (0.002)	3.339 (0.080)	1.992 (0.030)	1.676 (0.018)
$\hat{\Delta}^{18-14}$	1.493*** (0.329)	−0.020*** (0.003)	−0.165 (0.113)	−0.023 (0.045)	−0.063*** (0.022)
Bite 1 (Regions)					
No trend adjustment	0.371*** (0.064)	−0.035*** (0.002)	−0.829*** (0.066)	−0.118*** (0.032)	−0.299*** (0.023)
1-year trend adjustment	0.309*** (0.066)	−0.029*** (0.002)	−0.670*** (0.063)	−0.116*** (0.032)	−0.225*** (0.023)
2-year trend adjustment	0.233*** (0.074)	−0.022*** (0.002)	−0.529*** (0.060)	−0.114*** (0.033)	−0.160*** (0.022)
Bite 2 (Augmented occupations)					
No trend adjustment	0.032 (0.071)	−0.027*** (0.006)	−0.750*** (0.109)	−0.050 (0.038)	−0.326*** (0.041)
1-year trend adjustment	0.053 (0.071)	−0.022*** (0.005)	−0.635*** (0.102)	−0.057 (0.038)	−0.263*** (0.037)
2-year trend adjustment	0.056 (0.112)	−0.017*** (0.005)	−0.534*** (0.106)	−0.062 (0.045)	−0.209*** (0.036)
Bite 3 (Augmented industries)					
No trend adjustment	0.075* (0.043)	−0.026*** (0.004)	−0.762*** (0.118)	−0.057 (0.042)	−0.325*** (0.052)
1-year trend adjustment	0.085* (0.047)	−0.021*** (0.003)	−0.638*** (0.119)	−0.064 (0.045)	−0.258*** (0.051)
2-year trend adjustment	0.088 (0.067)	−0.017*** (0.003)	−0.502*** (0.118)	−0.070 (0.053)	−0.186*** (0.051)

Sources: GSES 2014/18, DGUV-IAB 2011-14, own calculations. *Notes*: Estimates in rows four to twelve refer to eq. (10). Bootstrap standard errors (100 replications) in parentheses. Bootstrap standard errors for factual values (rows one to three) 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). ***/**/* indicate statistical significance for the factual/counterfactual differences at the 1%/5%/10% level.

Table 3 – Minimum wage effects on inequality in monthly earnings, 2014 vs. 2018

	Mean	Gini	Q90/Q10	Q90/Q50	Q50/Q10
2014	2305.181	0.355	11.566	2.131	5.427
	(32.207)	(0.002)	(0.342)	(0.032)	(0.117)
2018	2483.971	0.336	10.991	2.108	5.215
	(27.681)	(0.002)	(0.302)	(0.031)	(0.101)
$\hat{\Delta}^{18-14}$	178.791***	−0.020***	−0.575	−0.024	−0.212
	(43.967)	(0.002)	(0.465)	(0.046)	(0.157)
Bite 1 (Regions)					
No trend adjustment	56.948***	−0.020***	−0.519***	−0.194***	0.215***
	(9.801)	(0.002)	(0.137)	(0.035)	(0.072)
1-year trend adjustment	48.948***	−0.016***	−0.615***	−0.179***	0.139*
	(10.268)	(0.002)	(0.165)	(0.032)	(0.078)
2-year trend adjustment	39.683***	−0.012***	−0.676***	−0.165***	0.081
	(10.088)	(0.002)	(0.211)	(0.030)	(0.095)
Bite 2 (Augmented occupations)					
No trend adjustment	−2.521	−0.012***	−0.906***	−0.076	−0.233*
	(14.507)	(0.003)	(0.334)	(0.050)	(0.130)
1-year trend adjustment	−0.410	−0.009***	−0.644**	−0.087*	−0.088
	(14.976)	(0.003)	(0.256)	(0.050)	(0.102)
2-year trend adjustment	−1.768	−0.006**	−0.409	−0.096*	0.041
	(19.378)	(0.003)	(0.296)	(0.050)	(0.127)
Bite 3 (Augmented industries)					
No trend adjustment	2.222	−0.012***	−0.917***	−0.075*	−0.242***
	(8.641)	(0.002)	(0.233)	(0.043)	(0.083)
1-year trend adjustment	2.835	−0.009***	−0.695***	−0.083**	−0.121*
	(8.602)	(0.002)	(0.205)	(0.041)	(0.072)
2-year trend adjustment	2.920	−0.006***	−0.492**	−0.089**	−0.012
	(12.785)	(0.002)	(0.205)	(0.040)	(0.078)

Sources: GSES 2014/18, DGUV-IAB 2011-14, own calculations. *Notes*: Estimates in rows four to twelve refer to eq. (10). Bootstrap standard errors (100 replications) in parentheses. Bootstrap standard errors for factual values (rows one to three) 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). ***/**/* indicate statistical significance for the factual/counterfactual differences at the 1%/5%/10% level.

9 Figures

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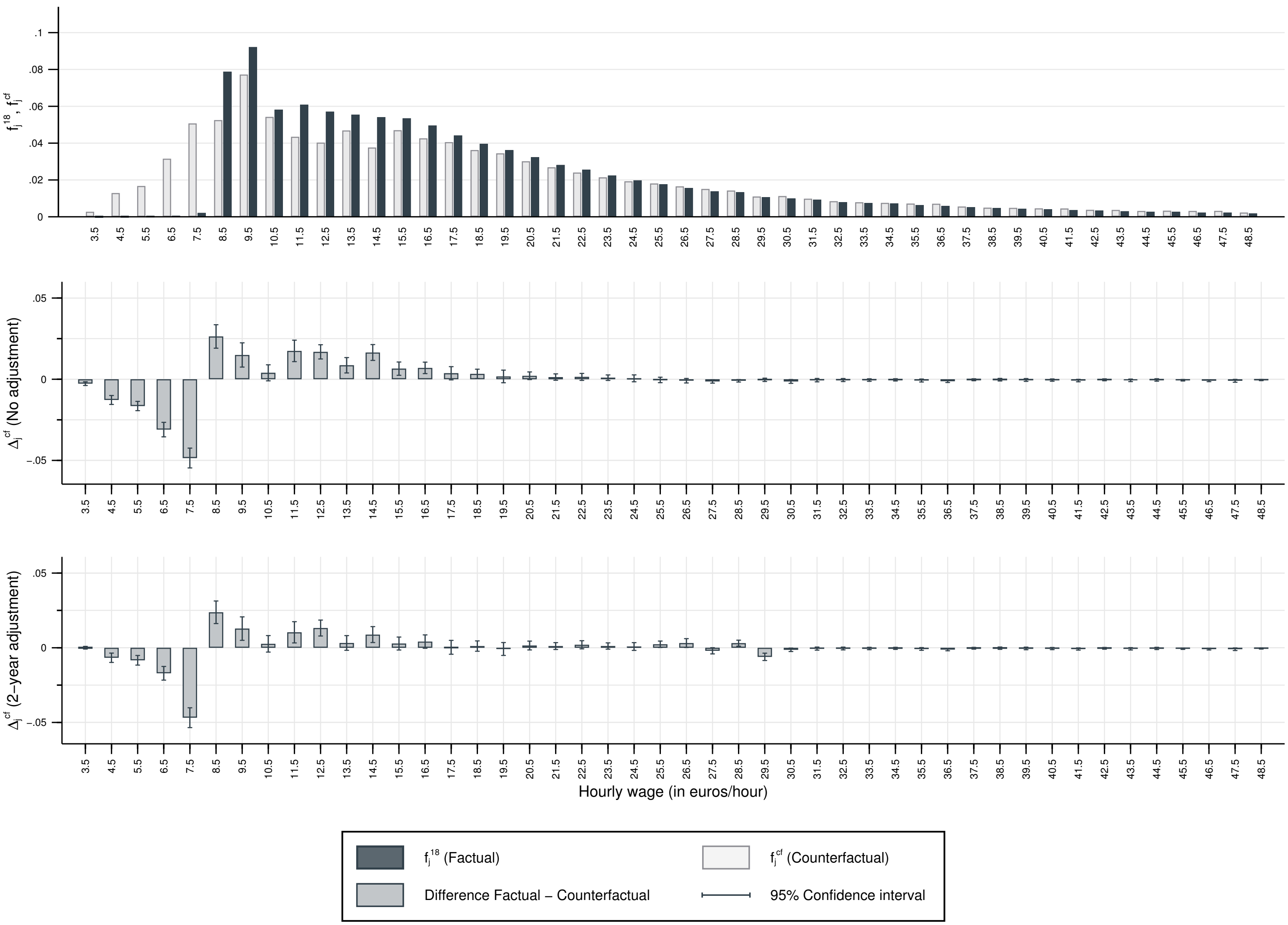
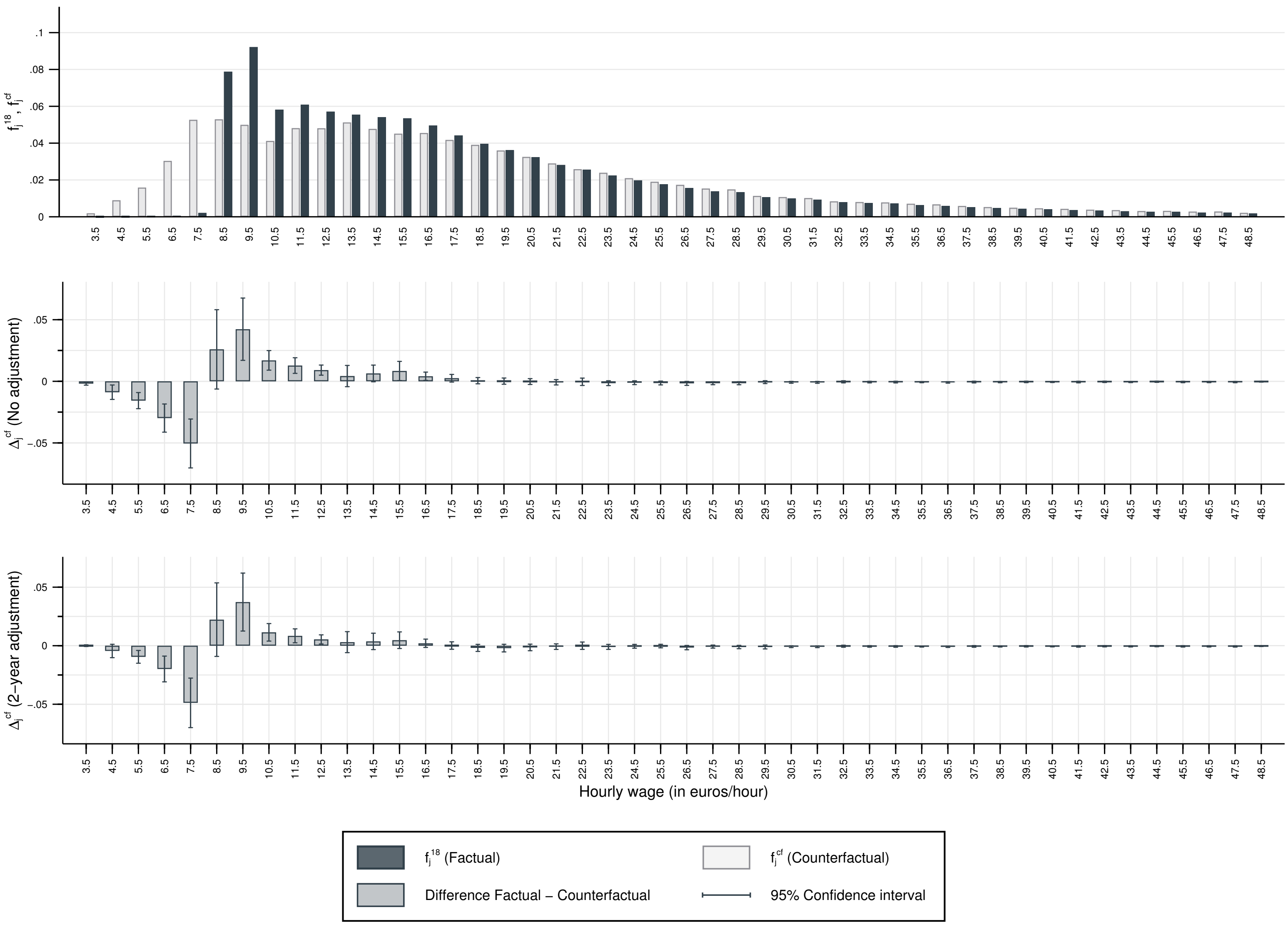
Figure 1 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 1: Regions



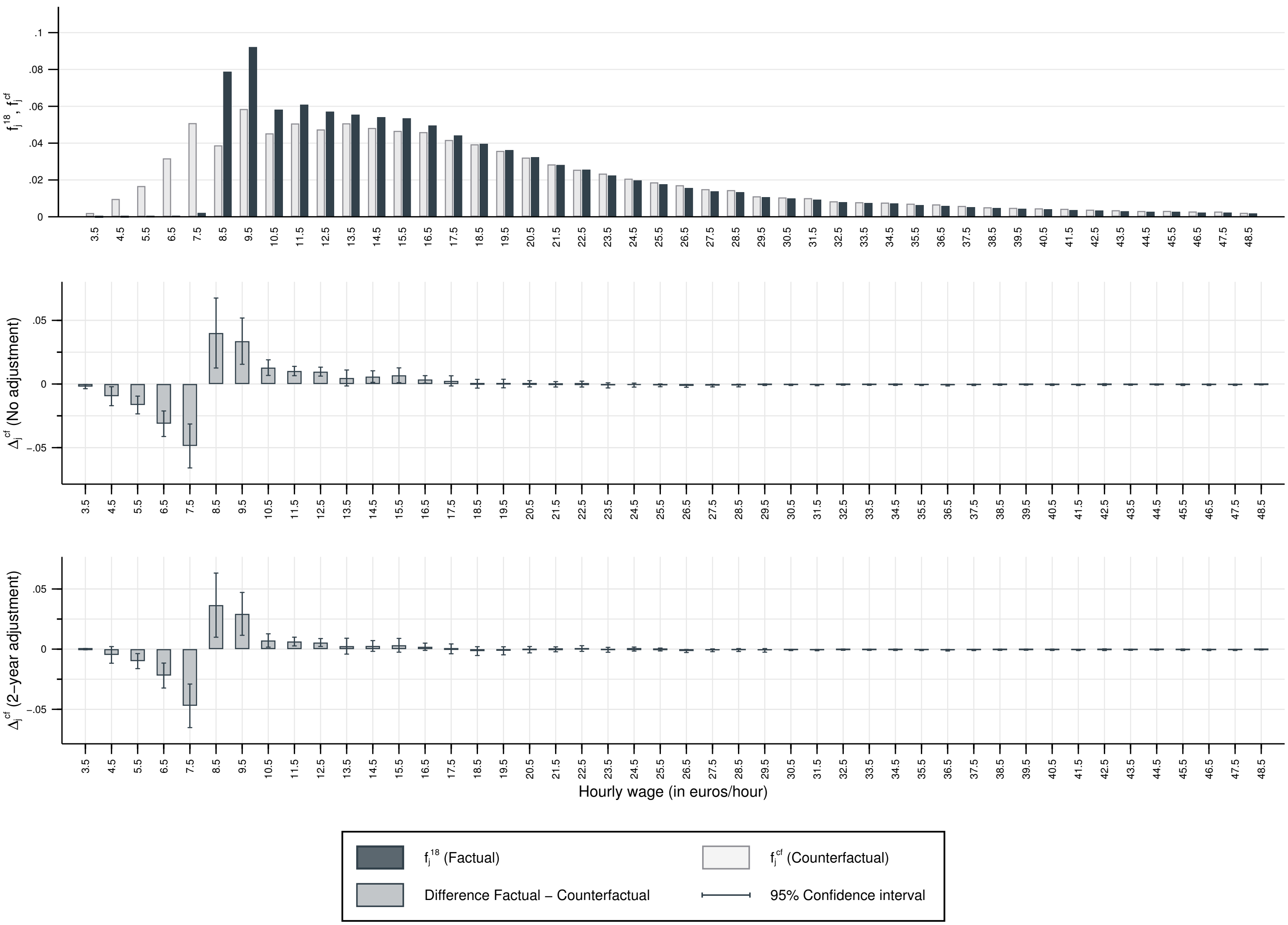
Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 2 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 2: Augmented occupations



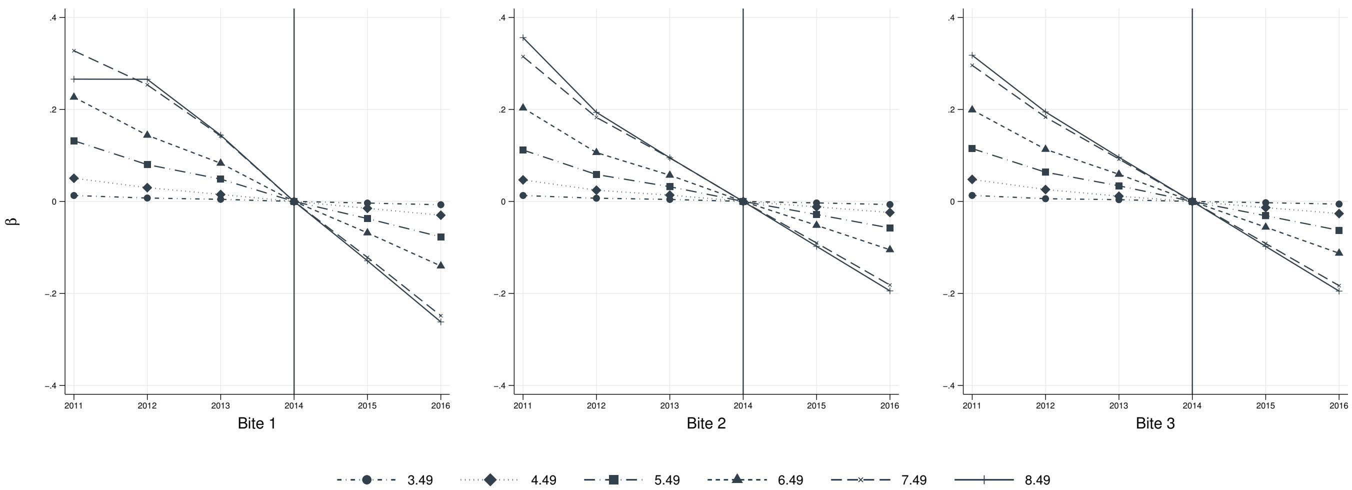
Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 3 – 2018 Factual vs. counterfactual distribution of hourly wages in the absence of minimum wage.
Bite 3: Augmented industries

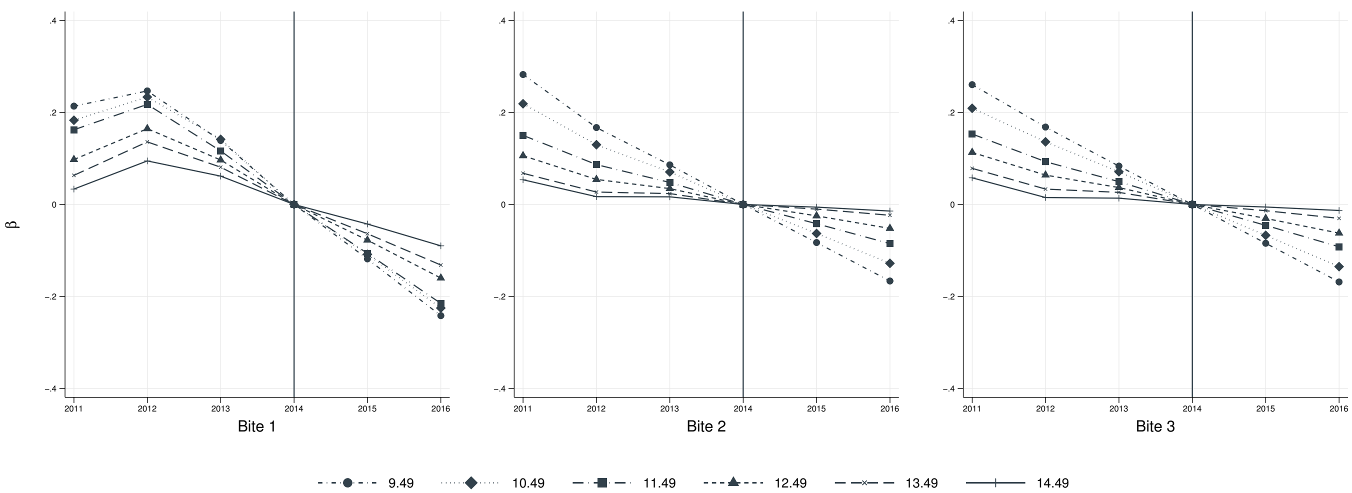


Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 4 – Pre-treatment estimates of treatment coefficients using *DGUV-IAB* data – Hourly wages, all bites.



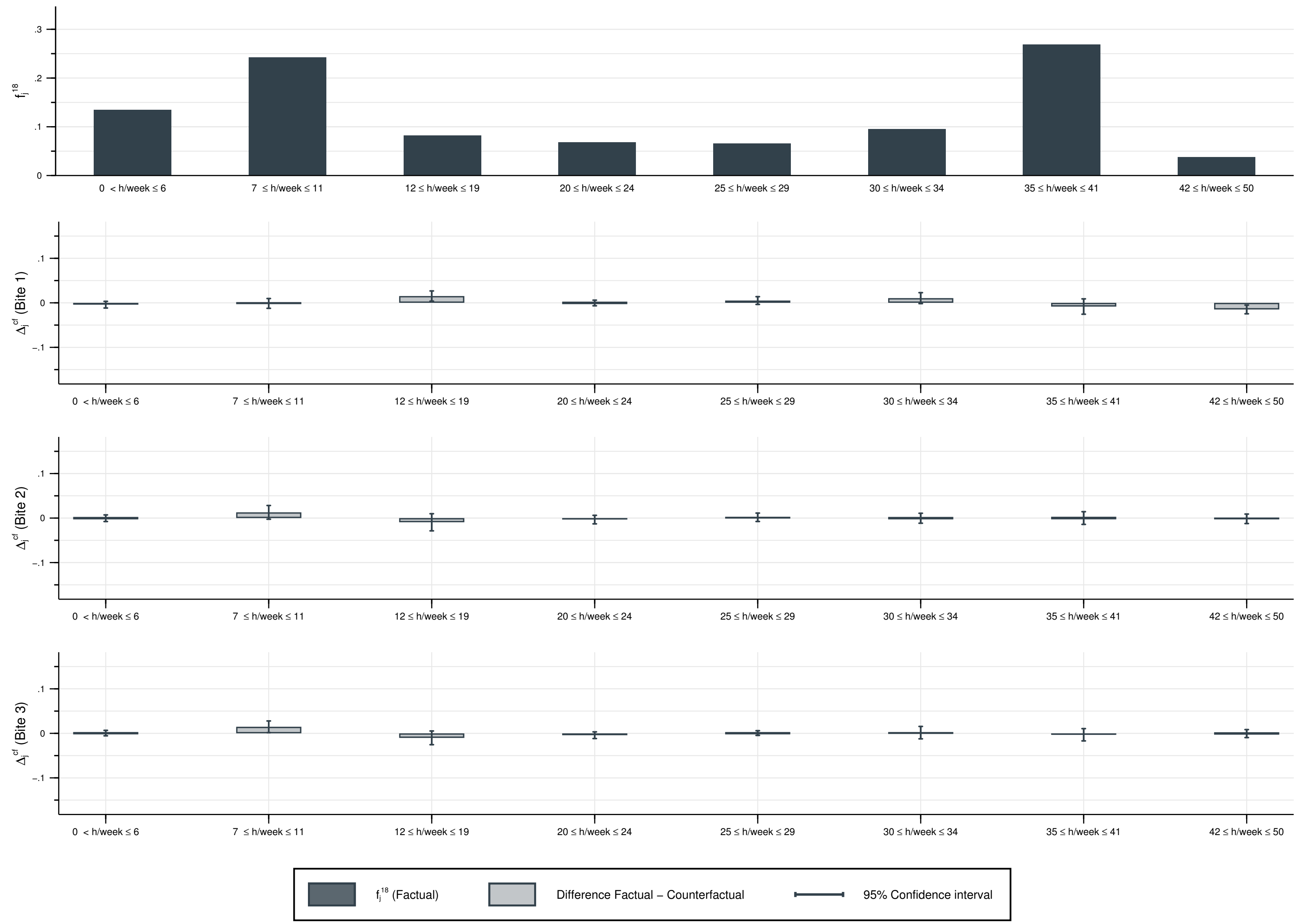
(a) Lower thresholds (3.49 to 8.49)



(b) Upper thresholds (9.49 to 14.49)

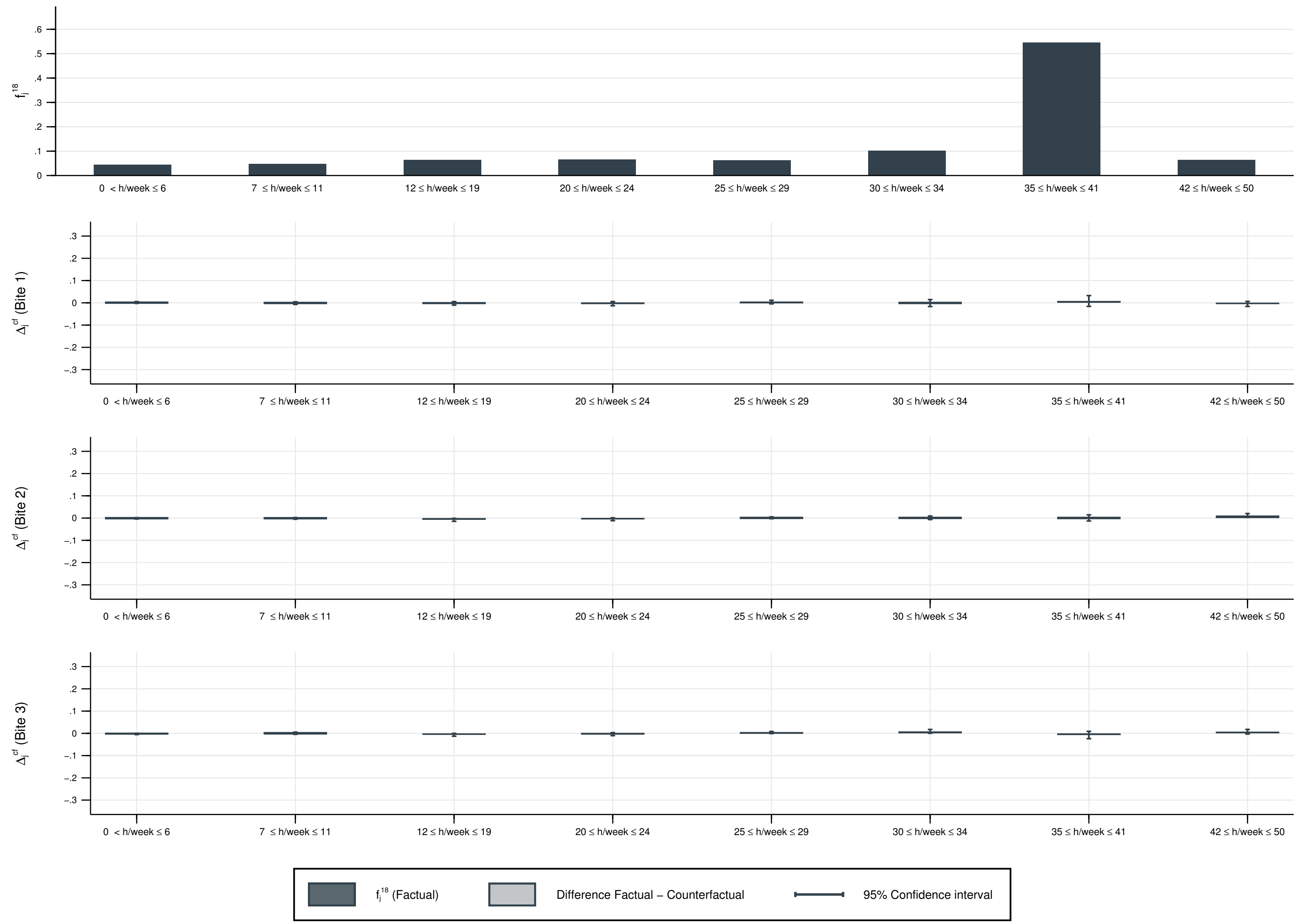
Notes: Estimates for the treatment effect, $\hat{\beta}_z^t$, in the pre-treatment periods 2011-2014 as specified in (11) for bins below and above the minimum wage level. Base period: 2014. Values in 2015 and 2016 refer to linearly extrapolated trends using the estimates from 2012, 2013, and 2014. *Source*: DGUV-IAB 2011-14, own calculations.

Figure 5 – 2018 Factual distribution of weekly working hours and treatment effect due to minimum wage for individuals with hourly wages $\leq$ 12 euros/hour.



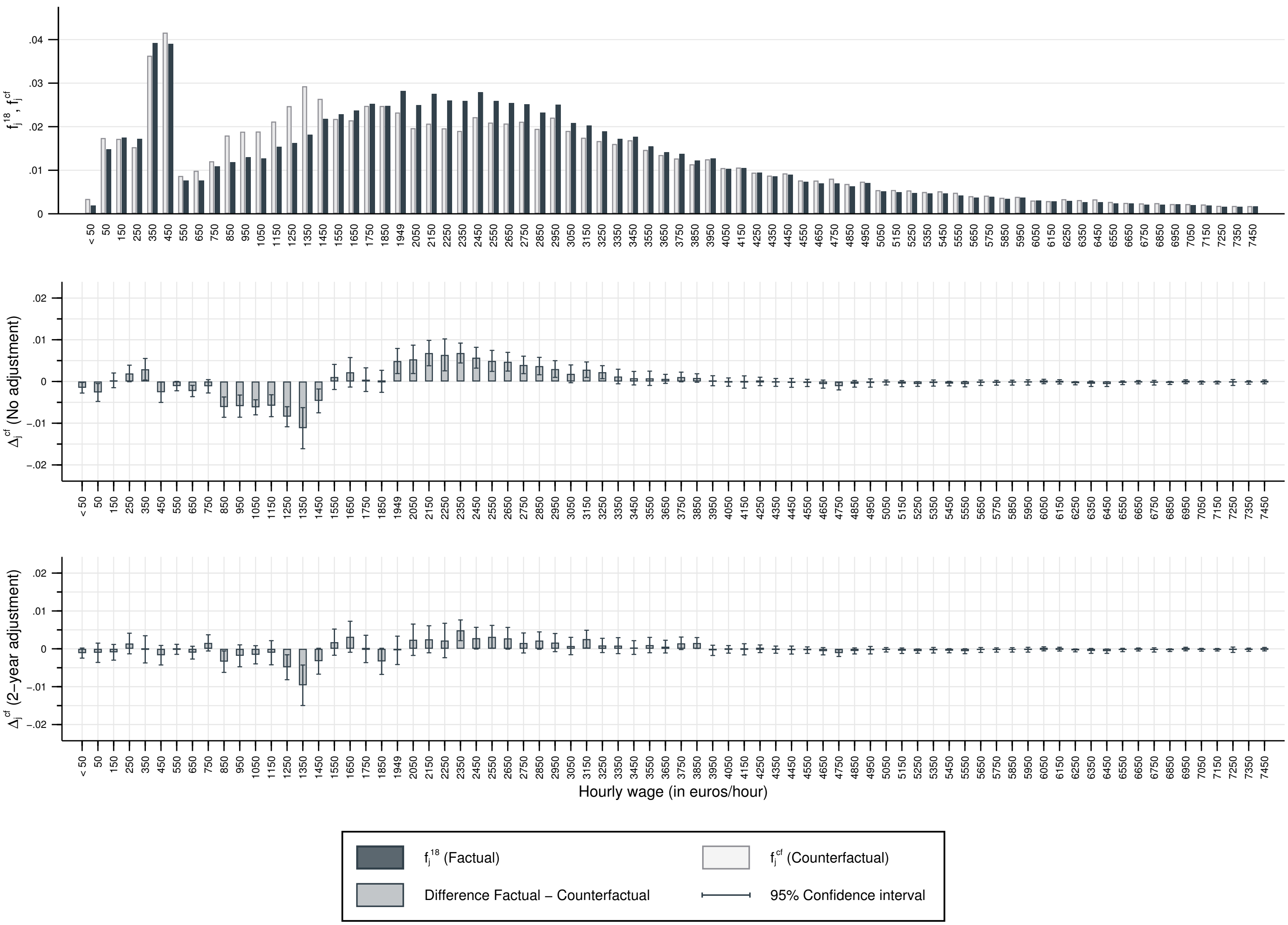
Notes: The bars in the first panel show the factual distributional mass in 2018. The three lower panels show differences between the factual and counterfactual bin frequencies for different bite specifications. 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 6 – 2018 Factual distribution of weekly working hours and treatment effect due to minimum wage for individuals with hourly between 12 and 16 euros/hour.



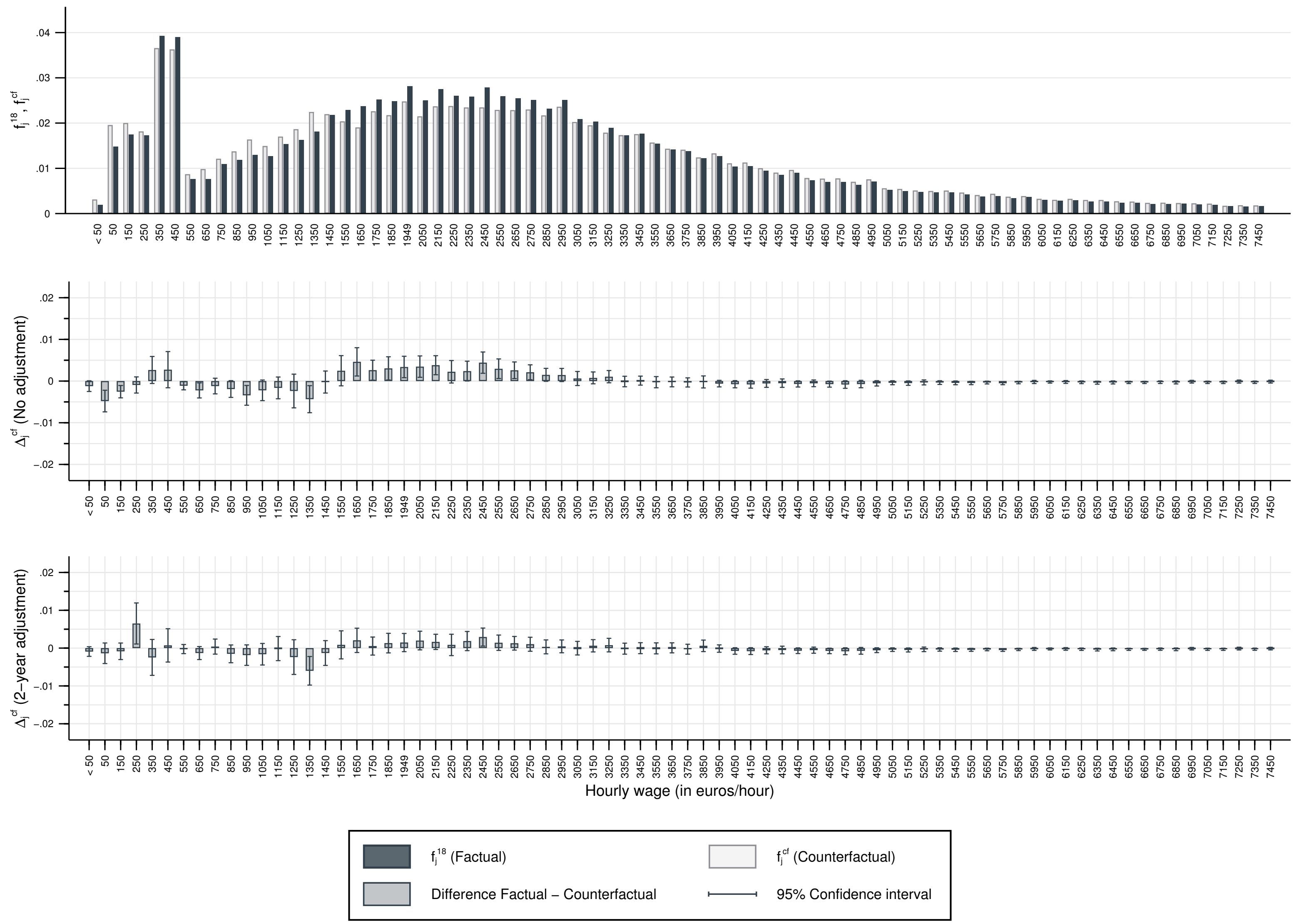
Notes: The bars in the first panel show the factual distributional mass in 2018. The three lower panels show differences between the factual and counterfactual bin frequencies for different bite specifications. 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 7 – 2018 Factual vs. counterfactual distribution of monthly earnings in the absence of minimum wage.
Bite 1: Regions.



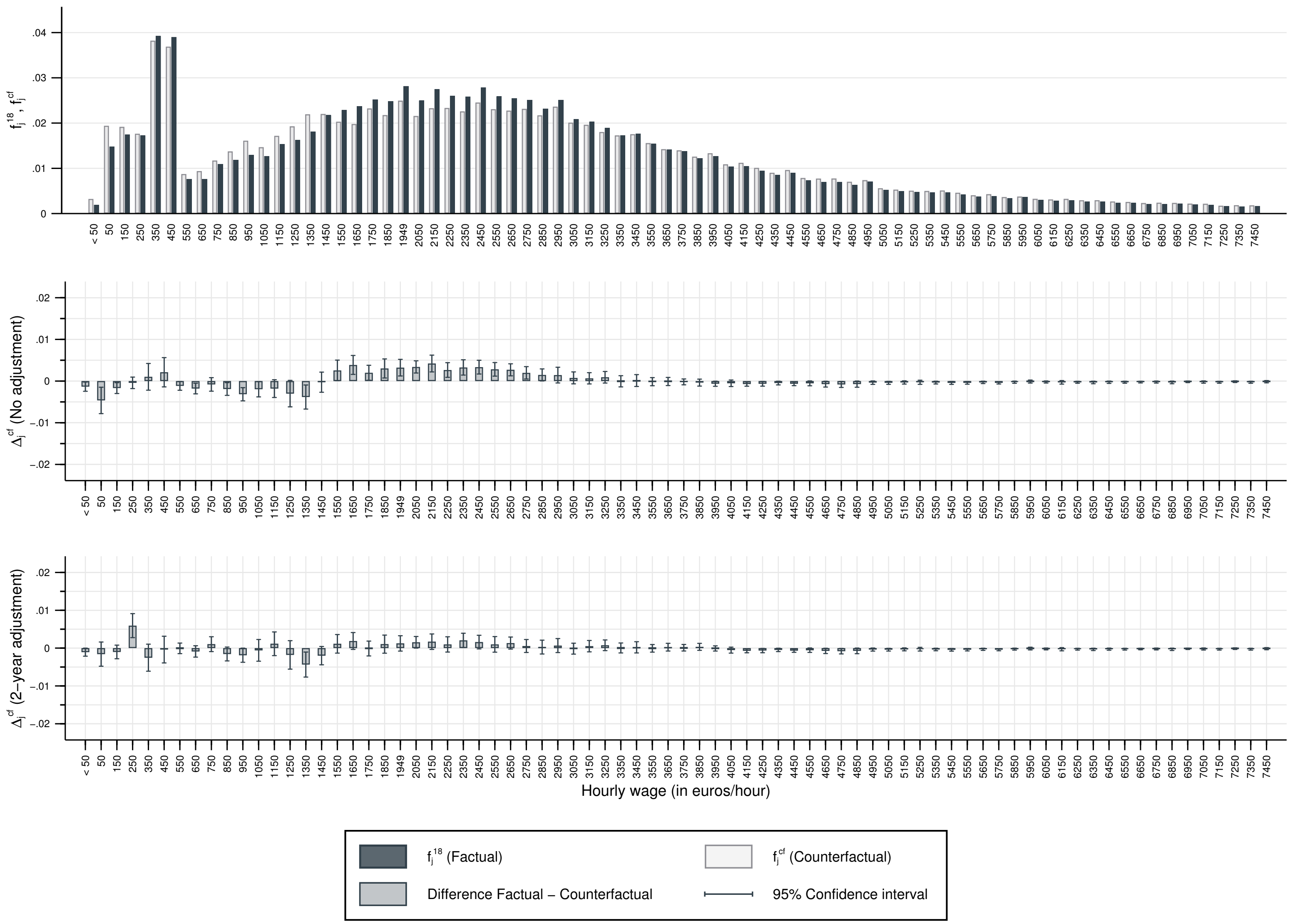
Notes: The x-axis shows monthly wage bins. For example, the '1050' bin comprises monthly earnings in the interval [1,050; 1,149] euros. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 8 – 2018 Factual vs. counterfactual distribution of monthly earnings in the absence of minimum wage. Bite 2: Augmented occupations.



Notes: The x-axis shows monthly wage bins. For example, the '1050' bin comprises monthly earnings in the interval [1,050; 1,149] euros. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Figure 9 – 2018 Factual vs. counterfactual distribution of monthly earnings in the absence of minimum wage.
Bite 3: Augmented industries.



Notes: The x-axis shows monthly wage bins. For example, the '1050' bin comprises monthly earnings in the interval [1,050; 1,149] euros. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without trend adjustment*. The second and third panel show differences in bin frequencies (second panel: no trend adjustment, third panel: 2-year trend adjustment). 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: GSES 2014/18, DGUV-IAB 2011-14, own calculations.

Appendix: Identification assumptions for DR-DiD

In this section, we show that, when conceptualizing the distributional treatment effect problem as a distribution regression difference-in-differences model, identification is implied by the standard difference-in-differences assumptions for repeated cross-sections. Let I^z denote the dummy variable indicating whether or not the observed outcome Y is less than or equal to the threshold z, i.e., $\mathrm{I}^z = 1[Y \leq z]$. The potential outcome under treatment level $Bite = b$ is defined as $Y(b)$, and, correspondingly, $\mathrm{I}^z(b) = 1[Y(b) \leq z]$. 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 $(\mathrm{I}^z, Bite, W)|D_t = 0$ and from $(\mathrm{I}^z, Bite, W)|D_t = 1$, where W includes individual characteristics and time effects.

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

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

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

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

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 involves only the comparison between treatment level $Bite = b$ and treatment level $Bite = 0$, so that the complications due to comparing different treatment levels (with nonzero bites) discussed in Callaway et al. (2021) do not arise.

The following arguments identify $ATT^z(b|b, W)$, analogous to Callaway et al. (2021) :

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

The last line in the above expression can not be estimated directly from the data. In addition to common support conditions and a no anticipation assumption in $E(\mathrm{I}^z(0) \mid Bite = b, W, D_t = 0)$ (individuals who would be treated in $t = 1$ show outcome $\mathrm{I}^z(0)$ in $t = 0$), the key assumption used to identify the last line above is

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

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. Replacing the last line for $ATT^z(b|b, W)$ by (A-3), allows to estimate $ATT^z(b|b, W)$.

Our motivation for assumption (A-3) 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 the extrapolations of such a trend be incorporated into the above expressions (this is what we do in section 4.2, analogous to Dobkin et al. (2018); Ahlfeldt et al. (2018); Freyaldenhoven et al. (2021) for the non-distributional case).

Condition (A-3) is the conditional version of the condition identified by Roth and Sant'Anna (2023) 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 (2023) for the case of two treatment levels and no covariates as $F_{Y_1(0)|treatment=1}(y) - F_{Y_0(0)|treatment=1}(y) = F_{Y_1(0)|treatment=0}(y) - F_{Y_0(0)|treatment=0}(y)$ (proposition 3.1 in Roth and Sant'Anna, 2023). To see the equivalence to (A-3), recall that cumulative distribution functions of Y are defined as $F(z|\cdot) = E(\mathrm{I}^z|\cdot)$. 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). Hence, it 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., $1[y \leq z] = 1[\log(y) \leq \log(z)] = 1[y^* \leq z^*]$. Roth and Sant'Anna (2023) not only show that this condition is equivalent to assuming that parallel trends are invariant with respect to transformations of the outcome variable, but that it forces the population to be a mixture of two aspects: (as-if) random assignment and stationary potential outcomes. The first aspect is addressed in our analysis by conditioning on a large number of observable covariates making the exposure to the minimum wage conditionally independent. The second aspect is addressed by our pre-trend correction, i.e., trends in potential outcomes do not differ systematically by the value of the bite after differential time trends are subtracted.

Note that we impose in our actual application the additional assumption $ATT^z(b|b, W) = ATT^z(b|b) = \beta_z \cdot Bite$. 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, when experimenting with different ways to do this, we found 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 databases, we have to abstain from pursuing more flexible approaches in our application. In line with Roth and Sant'Anna (2023), we also point out that, despite its potential limitations, the linear DiD specification is still by far the most widely used model DiD design with continuous treatment variables .