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 estimate the causal effects of the minimum wage introduction in Germany on the distributions of hourly wages, hours worked, and monthly earnings combining rich administrative data sources. Low hourly wages increase to the level of the minimum wage and there are large spillovers effects up to 50-70% above. Hours change very little. The minimum wage explains a sizeable share of the recent fall in wage and earnings inequality. Significant pre-trends lead to an overestimation of minimum wage effects if not accounted for. We provide a transparent treatment of distribution regression difference-in-differences and we compare alternative bite measures.

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1 Introduction

Against the backdrop of a stark increase in wage inequality (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 minimum wage at the level of 8.50 euros per hour constituted a major policy experiment: over 4 million workers (roughly 11% of the workforce) were eligible (Mindestlohnkommission, 2020).[2] Although there have been a number of recent contributions on its effects (Caliendo et al., 2018, 2019; Burauel et al., 2019; Dustmann et al., 2022; Bossler and Schank, 2023, and literature review below), the causal effect on the actual distribution of hourly wages and hours worked is an open question. The main aim of a minimum wage is to shift distributional mass from below to its level, leading to a spike in the wage distribution at the minimum wage. However, because of potential spillovers, its effects 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 that would have happened anyway, i.e., from trends in wage setting policies of employers, in labor supply, and in wage bargaining, which may have started before.

This study makes the following contributions. First, while a few studies have estimated 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 from large-scale administrative data. This allows us to reliably separate the effects of the minimum wage introduction along the distribution on prices (= hourly wages) from those on quantities (= hours worked). Minimum wages may not only change hourly wages but also working hours. For example, firms may reduce hours for low-wage employees to keep overall wage bills constant (Stewart and Swaffield, 2008), or there may be shifts between part-time and full-time employment because the minimum wage may change the relative price between the two (Garloff, 2019).

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 dataset that includes information on hourly

[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.

wages and working hours both before and after the minimum wage introduction – and from the administrative *Deutsche Gesetzliche Unfallversicherung (DGUV-IAB)* data, which includes information on wages and working hours, but only for a few years before the minimum wage introduction. Both databases are considered highly reliable because a firms' participation is compulsory and the information on wages and hours is typically based on the firm's internal accounting system. In contrast, the studies by Caliendo et al. (2018), Burauel et al. (2019), Burauel et al. (2020), and Caliendo et al. (2023), which find reductions in hours worked in response to the minimum wage introduction, use survey data possibly suffering from relatively small sample size and large measurement error in self-reported wages and working hours, thus possibly leading to noisy estimates and spurious findings of spillovers and noncompliance (Autor et al., 2016). Using GSES and DGUV-IAB data, we complement evidence from Bossler and Schank (2023) and Dustmann et al. (2022) solely based on the DGUV-IAB data, which do not have individual information on hours worked and on hourly wages after the minimum wage introduction. Only reliable data on hourly wages allow to assess whether hourly wages are increased to the level of the minimum wage and whether there are spillover effects above. Evidence on hours worked reveals responses of firms at the intensive margin which may rationalize possible differences between the effects on hourly wages and earnings as found by the aforementioned studies using survey data.

As a second contribution, we develop a distribution regression difference-in-differences approach (DR-DiD) that may be of independent interest for other applications involving the evaluation of distributional effects. A small number of previous contributions have carried out calculations related to the ones we present below (Almond et al., 2011; Dube, 2019b; Cengiz et al., 2019), but until recently, the literature lacked a full statement of the approach for the whole distribution along with its identifying assumptions. In particular, we show that tackling the problem with 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 (Chernozhukov et al., 2013) appears particularly well suited to study effects of the minimum wage on the distribution of hourly wages and hours worked as it directly targets nominal values instead of quantiles. It can also deal with discrete mass points and discontinuous distributions which may pose a problem for methods based on continuous distributions such as the Recentered-Influence-Function (RIF) regression (Firpo et al., 2009, 2018).

As a final contribution, we use alternative bite measures (based on regions, occupations and industries, respectively) as treatment indicators for the minimum wage introduction. This allows us to assess the sensitivity of our findings with respect to alternative channels for the minimum wage effect, e.g., concerning spillover effects, and potential violations 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

A large literature analyzes the effects of minimum wages (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 spillover effects of the minimum wage, evidence for which was 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 spillover 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, which were amplified by modest spillover effects, boosted average earnings in low-wage jobs. 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 spillover 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 spillover 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. Building on DiNardo et al. (1996), Fortin et al. (2021) explicitly allow for spillover effects. They find significant evidence for spillover effects and show that allowing for spillovers substantially increases the contribution of minimum wage effects on changes in the wage distribution.

A number of contributions have analyzed the effects of the minimum wage introduction in Germany. An important general finding is the absence of 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) observe positive wage effects for the bottom hourly wage quintiles, but no significant effects on monthly earnings which they attribute to working hours reductions caused by the minimum wage, possibly reflecting noncompliance with the minimum wage by reducing paid hours but not actual hours. Based on the adminstrative DGUV-IAB data for all workers (full-time, part-time, marginal jobs), Dustmann et al. (2022) and Bossler and Schank (2023) have access to individual information on hours worked up to 2014 which allows them - like our study does - to define the regional bite of the minimum wage as treatment intensity. Dustmann et al. (2022) find that the minimum wage raised earnings in low-wage jobs and that reallocation to better paying firms accounting for around 17% of the earnings increases. Not having access to hours worked after the minimum wage introduction, the study divides earnings by the average hours worked in a labor market cell to proxy hourly wages. This approach does not allow to analyze the effect on the distribution of hourly wages. Moreover, Burauel et al. (2019), Dustmann et al. (2022) and Caliendo et al. (2023) focus on particular points in the distribution 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.

Using a Recentered Influence Function (RIF) approach, Bossler and Schank (2023) provide a full distributional analysis of monthly earnings for all workers. In addition, our study also considers the minimum wage effect on the distribution of hourly wages and of hours worked which cannot be inferred from the administrative DGUV-IAB data after the minimum wage introduction. Estimating the effects on the distribution of hourly wages and hours worked adds to the exisiting evidence on the effects on the distribution of earnings. It yields further insights on the mechanisms behind the estimated minimum wage effects in Germany reported in the literature since the minimum wage targets the nominal hourly wage and its effects should be visible in the the distribution of hourly wages and earnings effects also hinge depend upon whether and to what extent there is an adjustment of hours worked at the intensive margin.

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 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 crucial for a credible difference-in-differences (DiD) analysis. We resort for this purpose to a specific administrative database from the *German Social Accident Insurance (DGUV)* containing information on individual working hours that can be merged with IAB data on employment histories for a few years before the minimum wage introduction (2011 to 2014), a unique dataset also used by Dustmann et al. (2022). Unfortunately, no such information on individual working hours is available from 2015 onward.

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.

3.2 Variables

Our earnings information is based on monthly gross earnings including overtime remuneration for the GSES reporting month April. Using data from April 2014 rules out possible anticipation effects of the newly introduced minimum wage.[3] Our data on hours worked refer to individuals' regular weekly working hours in the reporting month, including overtime hours. 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-

[3] Bossler and Schank (2023) use earnings informations from employment spells including June 30 of the calendar year considered, which include additional earnings components typically paid to the employee during the second half of the calendar year. This explains as to why the earnings for April reported by the GSES are lower than the earnings averaged over a longer employment spell in the IAB, except for the 5%- and the 10%-quantile (see table SA3 in the supplementary appendix).

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). As a particular contribution, we use three different bite measures based on regions, occupations and industries, respectively, based on pre-reform 2014 data. This allows us to investigate the sensitivity of the estimated minimum wage effects based on the treatment intensity as measured in different segments of the labor market, as there is no unambiguous measure of the strength of 'treatment' implied by the minimum wage introduction, and to explore different channels for spillover effects.

Bite 1: 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]

Bite 2: Augmented occupations

An alternative bite measure is 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.

Bite 3: Augmented industries

Finally, 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

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

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.

Our motivation for using alternative bite measures is as follows. Since Card (1992), the most widely used bite measure has been the regional one, the idea being that in a region with a high fraction of individuals below the minimum wage all workers can be considered as being potentially 'treated'. This allows for spillover effects to wages above the minimum wage in the local labor market as the labor market segment considered. Such spillovers can be rationalized both in a competitive labor market model (demand for higher skilled workers increases as lower skilled workers become more expensive, see Neumark and Wascher, 2008) and in a monopsonistic model (higher wage workers are more likely to quit or do not start if wages are not raised by employers, altering the labor supply curve the monopsonistic employer faces, see Manning, 2003). Spillovers are particularly plausible under local monopsonistic competition, because the behavior of workers and employers depends strongest on the outside options offered by rival firms in the same region, whereas outside options in other regions would have to been sufficiently attractive to justify incurring the costs of regional mobility (Bhaskar et al., 2002; Bassier, 2021; Datta, 2021; Ransom, 2022). An additional explanation for spillovers to higher wage rates are fairness concerns, i.e., firms maintain wage differentials to prevent quitting (Dube et al., 2019). This particularly applies to the regional level, where workers can observe each other and also compete in other markets (e.g., housing). Since the minimum wage introduction reduces the monopsony power, one can expect potentially large spillover effects when using a regional bite measure.

In addition to neglecting spillover effects to less treated regions (e.g., by reallocation effects as found in Dustmann et al., 2022), a disadvantage of a very broad regional bite measure is that it may miss minimum wage effects for strongly exposed subgroups. Suppose, for example, that regional differences in exposure to the minimum wage are small. Nevertheless, it may be the case that individual subgroups such as certain occupations are strongly affected by the minimum wage. Defining bite measures at the level of, say, occupations, is appealing because of anecdotal evidence of pay shifts in certain occupations following the introduction of the minimum wage (hairdressers, cleaners, waiters etc.). This strategy follows the intuitive approach of studying to what extent wages changed differentially in occupations that were affected to a higher or lower extent by the minimum wage. Given a potential lack of explanatory power in the regional bite, these effects may not be picked up when using this bite. Indeed, the summary statistics for the different bite measures in table 1 reveal stark differences in explanatory variation provided by the alternative bite measures, with the regional bite showing the smallest variation.

In addition to the occupational bite, we consider a bite measure defined across industries. This also turns out to contain more explanatory variation than the bite measure defined across regions. An additional advantage of a bite measure defined at the industry level is that a large part of wage bargaining in Germany takes place at this level (Jäger et al., 2022). This means that, in addition to target industries that are particularly affected by the minimum wage, an industry bite is able to pick up minimum wage spillovers within industries. Given the persistent labor market differences between East and West Germany, we augment both the occupation bite and the industry bite by accounting for the differences in these between the two parts of the country.

A final motivation for considering alternative bite measures is that different bite measures may be differentially susceptible to violating the no pre-trend assumption in our difference-in-differences analyses. In particular, our results suggest that idiosyncratic developments at the regional level may produce irregular pre-trend patterns in some cases, while pre-trends at the occupation or industry level appear more stable, adding credibility to our estimation approach. Taken together, we view the use of alternative bite measures as complementary, allowing for a more complete picture of the available evidence and to assess the sensitivity of our results with respect to different aspects of exposure to the newly introduced minimum wage.

— Table 1 around here —

3.4 Supplementary database for pre-trend analysis

By coincidence, the working hours information typically recorded by the *German Social Accident Insurance (DGUV)* can be merged with administrative employment data (Beschäftigenhistorik, BeH) provided by the Institute for Employment Research (IAB) just for the years 2011 to 2014 and for no other years. These data were also used by Dustmann et al. (2022). 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. The use of the DGUV-IAB working 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 4,050 euros per month and hourly wages up to 30 euros/hour.

4 Econometric method

Our aim is to determine the causal effects of the minimum wage introduction on the distributions of hourly wages, hours worked, and monthly earnings. One possibility would be to estimate a difference-in-differences (DiD) version of a recentered influence function (RIF) (Firpo et al., 2009, 2018). Some existing contributions have used such an 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.[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. Both arguments also apply to the distribution of weekly working hours which is highly discrete and discontinuous.[6]

[5]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. Supplementary appendix SA2 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.

[6]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

In order to address these aspects, we use 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 related to the ones we present below (Almond et al., 2011; Dube, 2019b; Cengiz et al., 2019), but the previous literature has not produced a full statement of the approach along with its identifying assumptions. In particular, we show in the appendix 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 as recently pointed out by Roth and Sant'Anna (2023) 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 naturally allows for the estimation of possible pre-trends, which we correct for as described below in section 4.2.

The identification assumption used by us has also been employed in a recent contribution by Kim and Wooldridge (2024) who develop a framework for evaluating quantile treatment effects. By contrast, our approach targets nominal points in distributions. We also point out a recent working paper by Fernandez-Val et al. (2024) (published after the first draft of our paper Biewen et al., 2022), which provides a more detailed theoretical analysis of DR-DiD. In particular, Fernandez-Val et al. (2024) advocate for the use of non-linear distribution regression models for DR-DiD. We fully agree with the arguments presented by Fernandez-Val et al. (2024), but, for reasons explained in section 4.3, we have to use simpler linear probability models in our application.

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

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

for the cumulative distribution function (cdf) of the variable of interest based on the DiD model

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

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 bins $[0; 3.49], [3.50; 4.49], \dots, [48.50, 49.49]$ (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 $[0; 6], [7, 11], \dots, [42; 50]$.[7] 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

[7] In principle, it would be possible to define finer bins for working hours when using the GSES data only. However, it would be hard to connect such an analysis to potential pre-trends observed in the DGUV-IAB data whose working hours information is slightly different. In order to avoid potential misalignment, we define coarser hours categories.

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

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

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

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

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 $z \in \{z_0, z_1, \ldots, z_J\}$ 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.

The ceteris paribus effects of the minimum wage introduction on the distribution and on inequality measures are given by

$$\Delta_j^{cf} := f_{j,1} - f_{j,1}^{cf}, \quad j = 1, \dots, J \tag{9}$$

$$\Delta^{cf}(v(\cdot)) := v(F(z \mid D_t = 1)) - v(F^{cf}(z \mid D_t = 1)), \tag{10}$$

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/hour would already have more strongly declined in these regions without the minimum wage). Section 5 discusses the estimated patterns of $\beta_z^{2011}, \beta_z^{2012}, \beta_z^{2013}$ in the pre-treatment period. For hourly wages, the pre-trends follow linear time trends almost exactly, which we then use for counterfactual trend extrapolation.[8] For hours of work and monthly earnings, the findings are more ambiguous with the pre-trends showing a less clear and often nonlinear pattern.

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 given by

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

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 in two separate research data centers. This has important consequences for our estimation strategy. First, we face substantial computational limitations on-site that make the use of the computationally more involved logit or probit models, as suggested by Fernandez-Val et al. (2024), for (1)

[8] 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.

infeasible. Second, in a non-linear distribution regression model it would not easily be possible to combine our main analysis with a pre-trend analysis based on a *separate* data set, while this is straightforward in a linear probability model (see equation (12)). As a robustness check, preliminary experiments show that the estimated factual and counterfactual *unconditional* distribution functions (3) and (4) are 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 unsurprising given the large amount of averaging involved. We point out the further practical advantages of linear probability models in the given context: (i) computational efficiency (the large number of parallel regressions for the different distributional thresholds can be efficiently parallelized as in Chernozhukov et al., 2020), (ii) computational simplicity (avoidance of convergence problems for logit/probit models in case of near-perfect-prediction problems which may easily arise at extreme distributional thresholds), (iii) transparency, (iv) consistent aggregation due to the law of iterated projections, and (v) immediate interpretation of β_z in terms of percentage points probability mass gained/lost per unit of bite.

Still, one might be concerned that the 'rigid' nature of the linear probability model may lead to ill-defined treatment effects as described in Fernandez-Val et al. (2024). Following Roth and Sant'Anna (2023) and Kim and Wooldridge (2024), we therefore subject our resulting counterfactual distributions (4) and (12) to the test whether they are proper distribution functions. There are basically no indications of specification problems, with the exception of the two lowest threshold values for the specific case of hourly wages with the two-year trend-correction (indicating trend over-correction in (12)). For details, see supplementary appendix SA1.

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 eliminating hourly wages below its nominal level (8.84 euros/hour in 2018). 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 spillover effects up to 16.5 euros/hour which is more than 80% above the nominal level of the minimum wage.[9] 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.

— 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 the significant spillover 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 spillover effects are larger for the regional bite suggests spillover effects within regions that are not 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

[9] We point out that 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 Mindestlohnkommission (2020), Bachmann et al. (2020), and Burauel et al. (2019). Such a behavior would suggest that hours worked for some low-wage workers are reduced in a given job, a point we will address in section 5.2.

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 regions already caught up to those in low-bite regions 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 **is** 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? Addressing 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 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 quantile gaps 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 spillovers are stronger and even reach beyond the median hourly wage (around 16 euros/hour in 2018). We conclude that the minimum wage introduction explains a very large share of the decline in the inequality of hourly wages, a share which is larger than the share explained in the reduction in monthly earnings as found by Bossler and Schank (2023).

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 above 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 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 figure 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, there are some more pronounced but nonlinear and concave patterns. For wages up to 12 euros/hour, high-bite regions show an increase in the distribution function for the low- and medium-hours bins - the effect being strongest for the 7 to 11 hours bin, i.e., the share of workers with lower hours of work is increasing between 2011 and 2014 with a particular strong increase between 2011 and 2012. For wages between 12 and 16 euros/hour, high-bite regions show concave pre-trends with positive trends in various bins between 2011 and 2012 and falling pre-trends between 2012 and 2014 for medium-hours bins but not in the lowest hours bin. Due to the concavity of the pre-trends, the case for a linear trend extrapolation is weaker for hours of work compared to hourly wages. Still correcting for pre-trends regarding the regional bite does not change the significant effects in two bins for wages up to 12 euros/hour and the zero (insignificant) effects in all other bins (figures SA17 and SA32). To complete the discussion, 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).

Our findings suggest that the bite definition makes a difference for hours of work. For the regional bite, the minimum wage introduction did increase the share of workers in the 12 to 19 hours bin and reduce the share of those working 42 hours and more among workers making at most 12 euros/hour. Thus, within a region, low-wage workers shifted away from low paid jobs with long hours to jobs with lower hours that became comparatively better paid after the introduction of the minimum wage. However, this shift did not occur within industries and occupations suggesting that the effect found for the regional bite is due to reallocation effects within regions but across occupations and industries (Dustmann et al., 2022).

In sum, our results on the effects on hours worked are more mixed than those for hourly wages. We find no statistically significant effects of the minimum wage on the distribution of working hours based on the occupational and the industry bite. By contrast, there are significant effects for two specific hours bins (12 to 19 hours, 42 to 50 hours) for the regional bite. These suggest a reduction in working hours due to the minimum wage introduction. These findings suggest that the shift from full-time work with overtime hours to part-time work with 12 to 19 hours is caused by reallocation effects at the regional level across industries and occupations, not by hours reductions in a given job to keep total wage costs constant.

5.3 Effects on monthly earnings

In this section, we consider the effects of the minimum wage introduction on monthly earnings, which have also been studied by Bossler and Schank (2023). The effects of the minimum wage on monthly earnings reflects the joint effect of changes in hourly wages and in hours worked. Given that we observe little evidence for systematic changes of working hours, we would expect significant changes in the distribution of monthly earnings. However, the question is how strong these changes are and how they vary along the earnings distribution.

Our 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 the strongest spillover effects at the regional level.

— Figures 7 to 9 around here —

Compared to hourly wages, we find less clear and often nonlinear pre-trends suggesting higher earnings growth in high-bite groups for moderately low and medium earnings levels (see figures SA65 to SA71 in the supplementary appendix). Similar to hours of work, the pre-trends turn out concave for the regional bite. In contrast, they are convex for the two other bites. A linear pre-trend specification between 2012 and 2024 seems a good approximation. 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), see Bossler and Schank (2023) for similar findings. 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 spillovers 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 concerning the regional bite 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. However, compared to the strong minimum wage effects found by us on hourly wages, the effects on monthly earnings are somewhat muted, especially after the adjustment for pre-trends and when considering the occupation and industry bite definitions. This may be due to the fact that minimum wage earners are a smaller share of workers at each point of the overall distribution of monthly earnings, thus the impact of the minimum wage introduction is weaker at each point of the overall distribution and more dispersed along the distribution in contrast to the distribution of hourly wages where the direct impact can be pinned down exactly at a certain value of the hourly wage where one expects a spike in the post minimum wage distribution. This could also explain, why based on survey data with smaller sample size, Caliendo et al. (2023) did not find significant effects on monthly earnings.

— 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 alone. 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 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 datasets for Germany including information on hourly wages and working hours both before and after the minimum wage introduction. Providing a transparent treatment of the method, we suggest to use difference-in-differences distribution regressions (DR-DiD) for a full distributional analysis, while accounting for discrete mass points and changing nominal target values in these distributions. Using different bite measures allows to investigate the sensitivity of the estimated minimum wage effects based on the treatment intensity as measured in different segments of the labor market and to explore different channels for spillover effects.

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 spillover 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 spillover 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. 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. This leads to an overestimation of minimum wage effects if these pretrends are not corrected for. This suggests 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 education and work experience were responsible for rising inequality before 2014, but that the effect of these on inequality flattened out before 2014.

Our comparison of alternative bite measures suggests the existence of substantiall spillovers for hourly wages and monthly earnings in all cases but the effects are much larger at the regional level. Our results for working hours are more mixed with no working hours effects for the occupational and the industry bite but signficant effects for individual hours bins when using the regional bite, suggesting a slight shift from full-time work to part-time work which would be consistent with reallocation effects at the regional level as found by Dustmann et al. (2022).

Our overall conclusion is that the minimum wage changed prices (= hourly wages) while having only a small effects on quantities (= hours worked) so that the hourly wage increases – which we show to have affected hourly wages substantially above the minimum wage – also changed 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 those on the distribution of hourly wages, especially after adjustment for pre-trends. Our results thus help 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). There is concern that non-compliance with the minimum wage may have led firms to reduce paid hours of work and not actual hours worked while keeping monthly earnings constant (Mindestlohnkommission, 2020; Burauel et al., 2019). Our data involve paid hours and our findings of no hours of work effects for the industry and occupation bite speak against this concern because one would have expected a shift towards lower hours of works, especially in the low-hours bins.

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Conflict of Interest Statement

There are no sources of conflict of interest for the authors of this paper.

Tables and figures

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

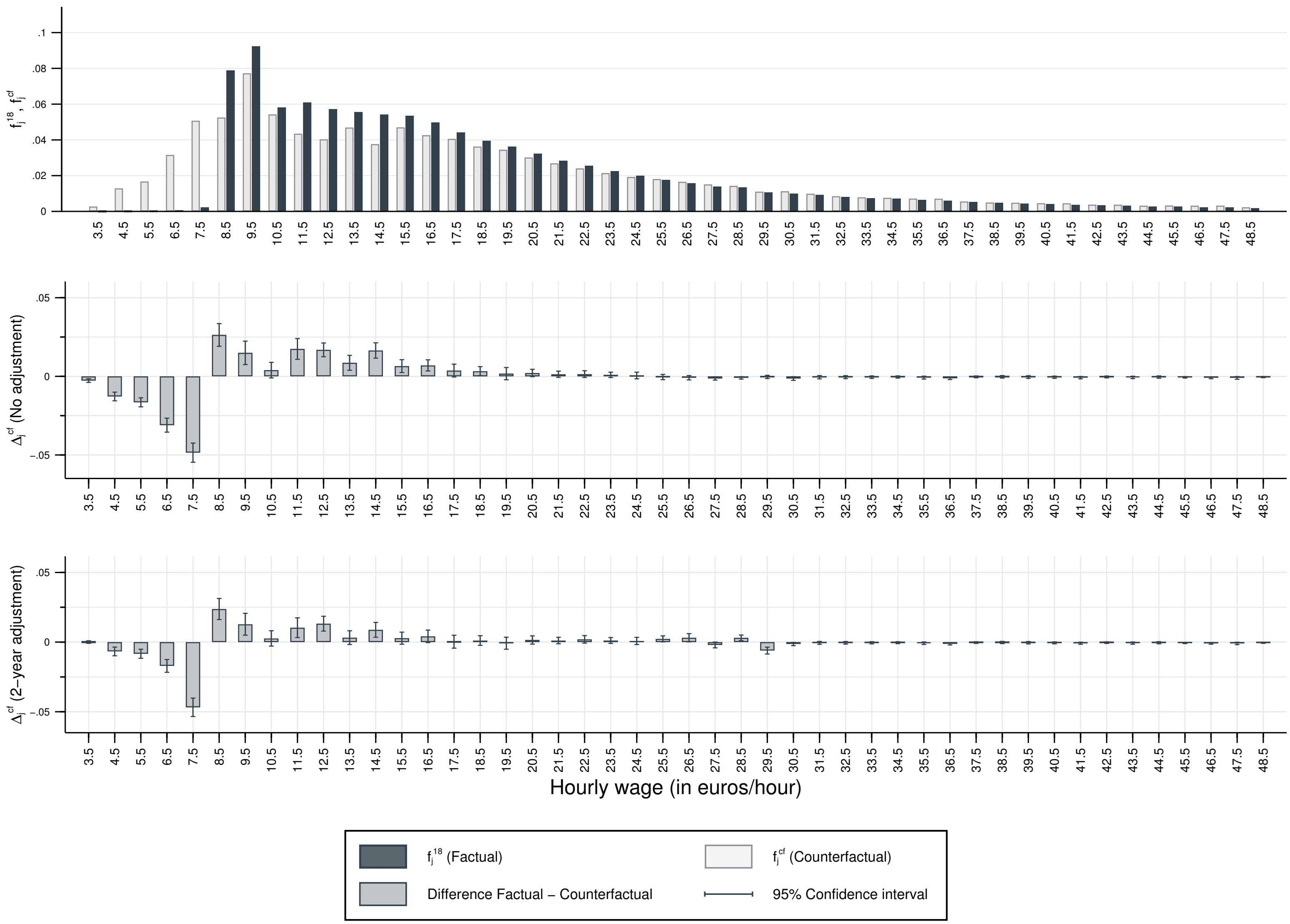
Sources: GSES 2014/18, DGUV-IAB 2011-14, own calculations. *Notes*: Estimates in rows four to twelve refer to eq. (10). Bootstrap standard errors (100 replications) in parentheses. Bootstrap standard errors for factual values (rows one to three) are clustered at the regional level. Bootstrap standard 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)

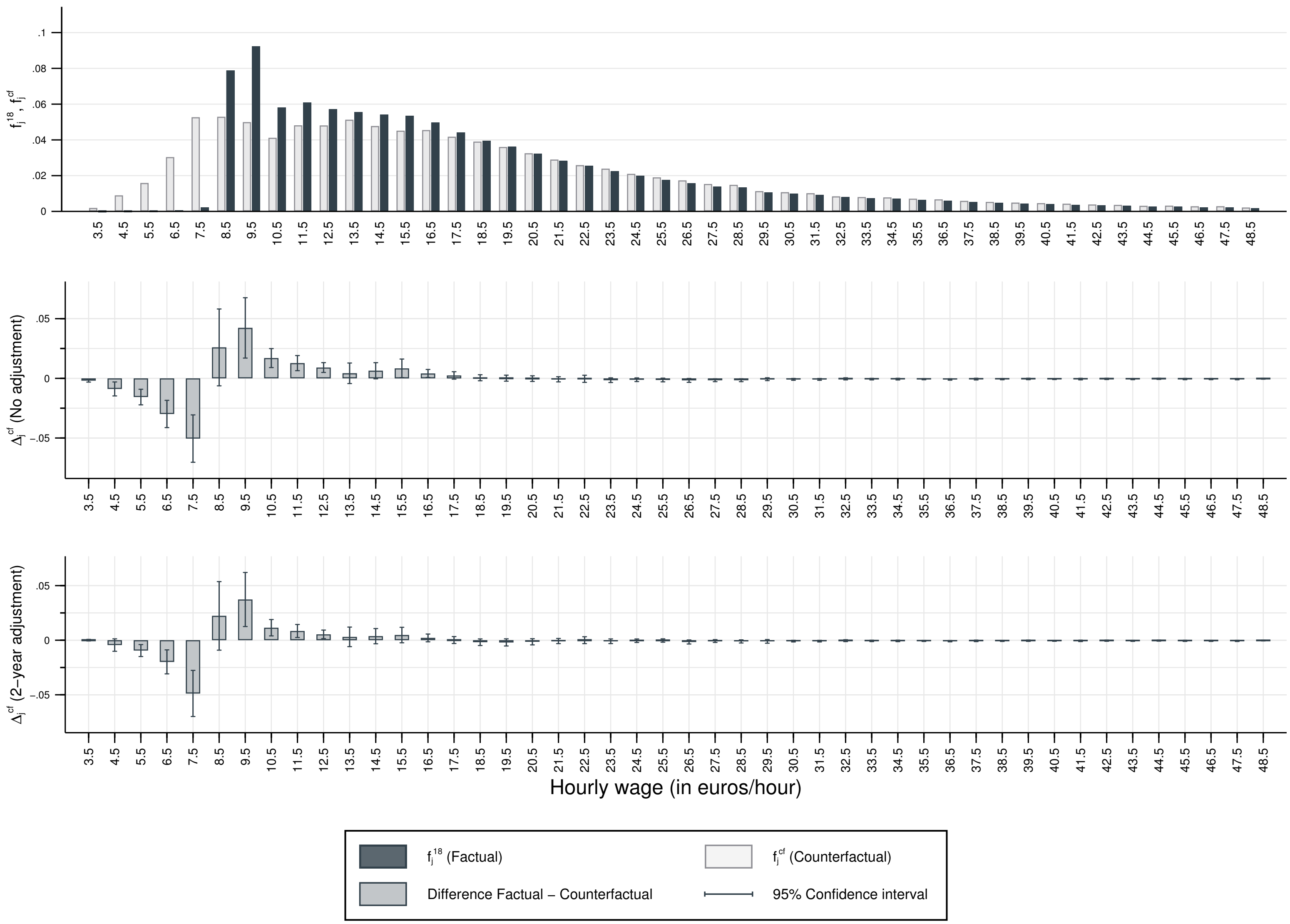
Sources: GSES 2014/18, DGUV-IAB 2011-14, own calculations. *Notes*: Estimates in rows four to twelve refer to eq. (10). Bootstrap standard errors (100 replications) in parentheses. Bootstrap standard errors for factual values (rows one to three) are clustered at the regional level. Bootstrap standard 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.

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



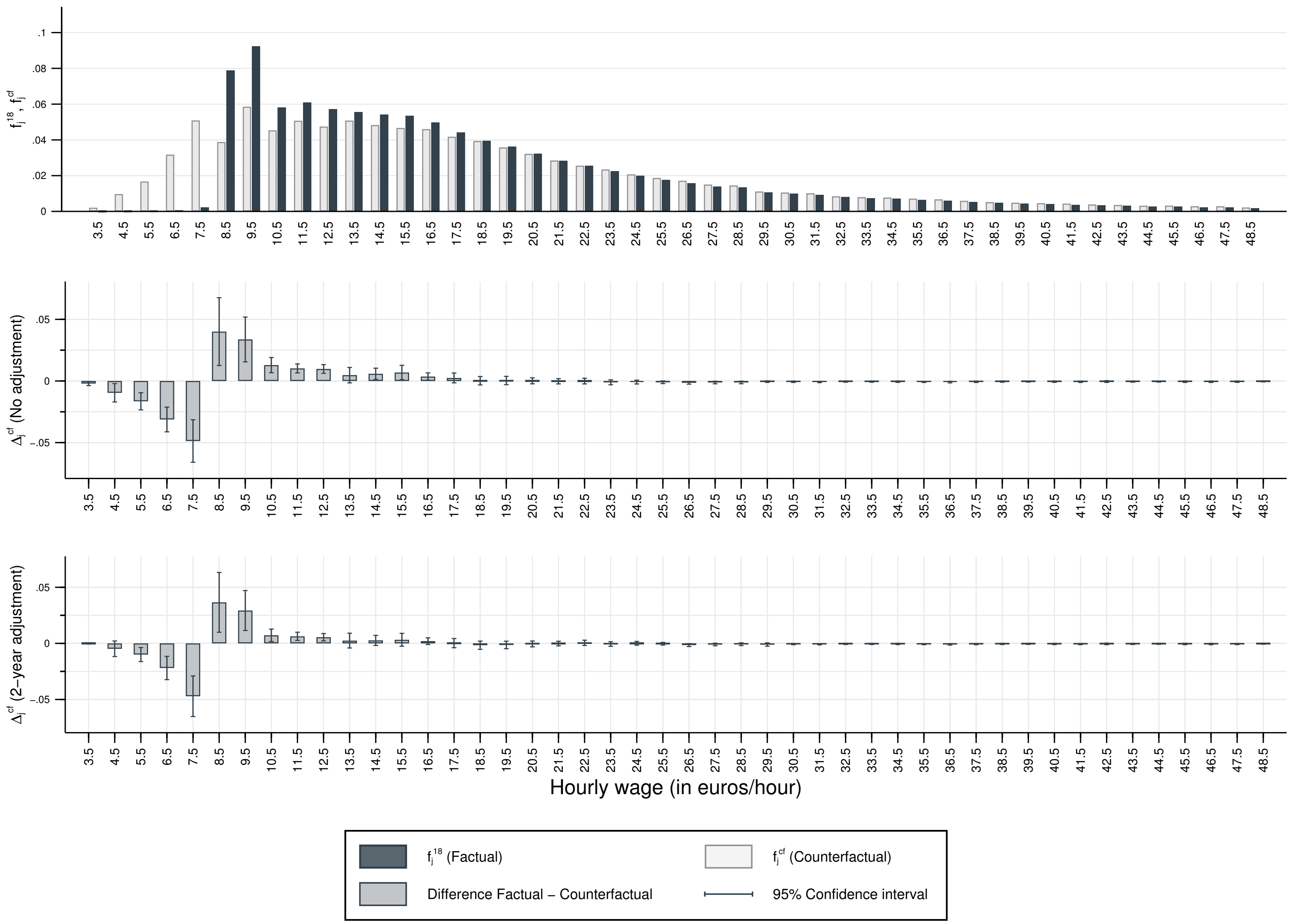
Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without 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



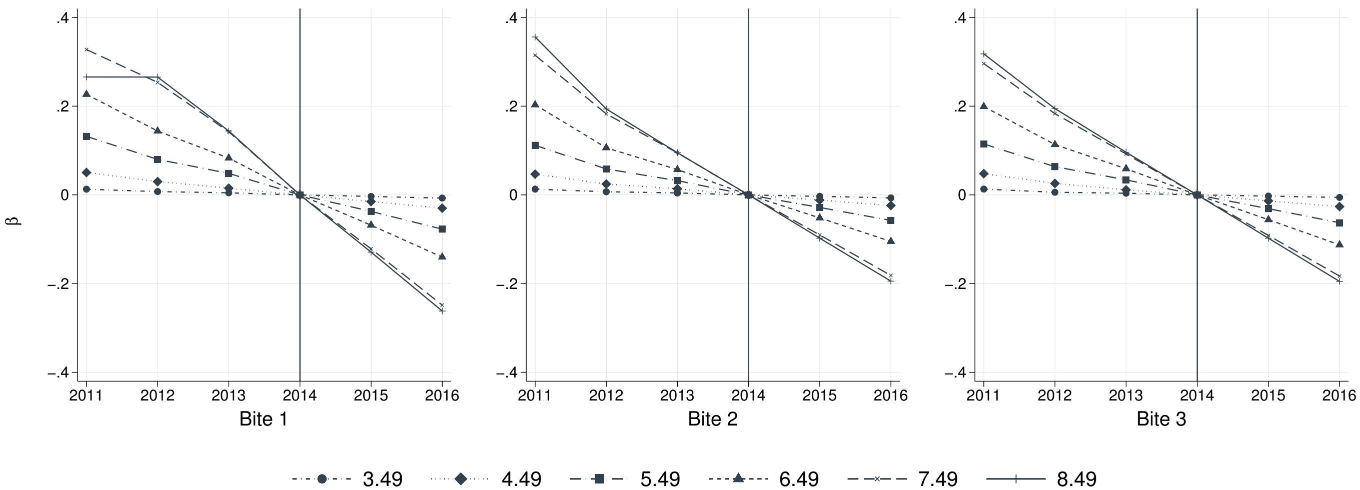
Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without 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

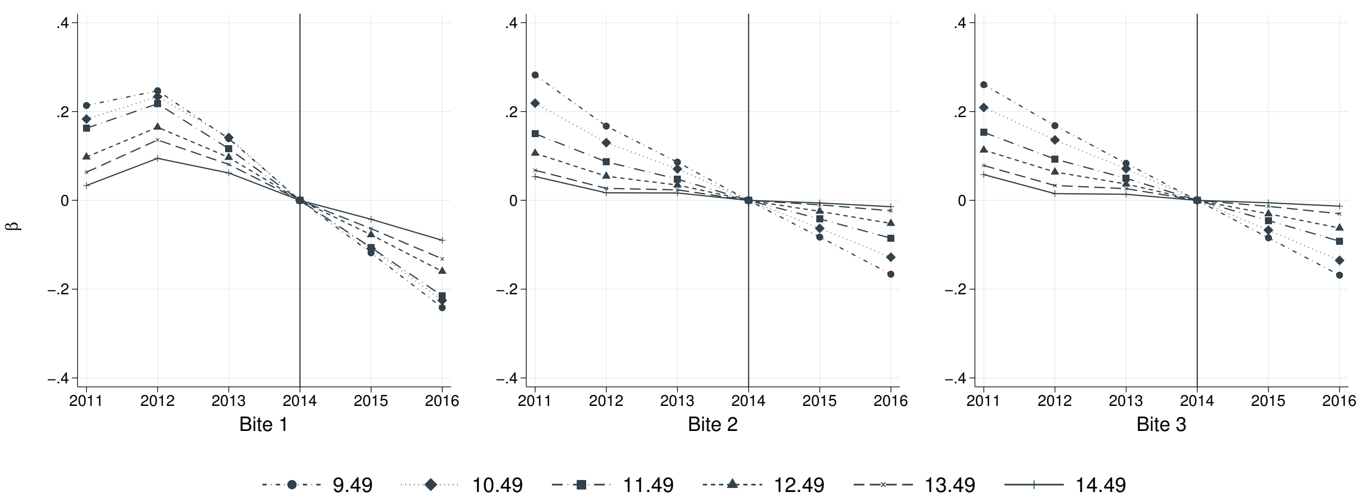


Notes: The x-axis shows hourly wage bins. For example, the '10.50' bin comprises hourly wages in the interval [10.50; 11.49] euros/hour. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without 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.



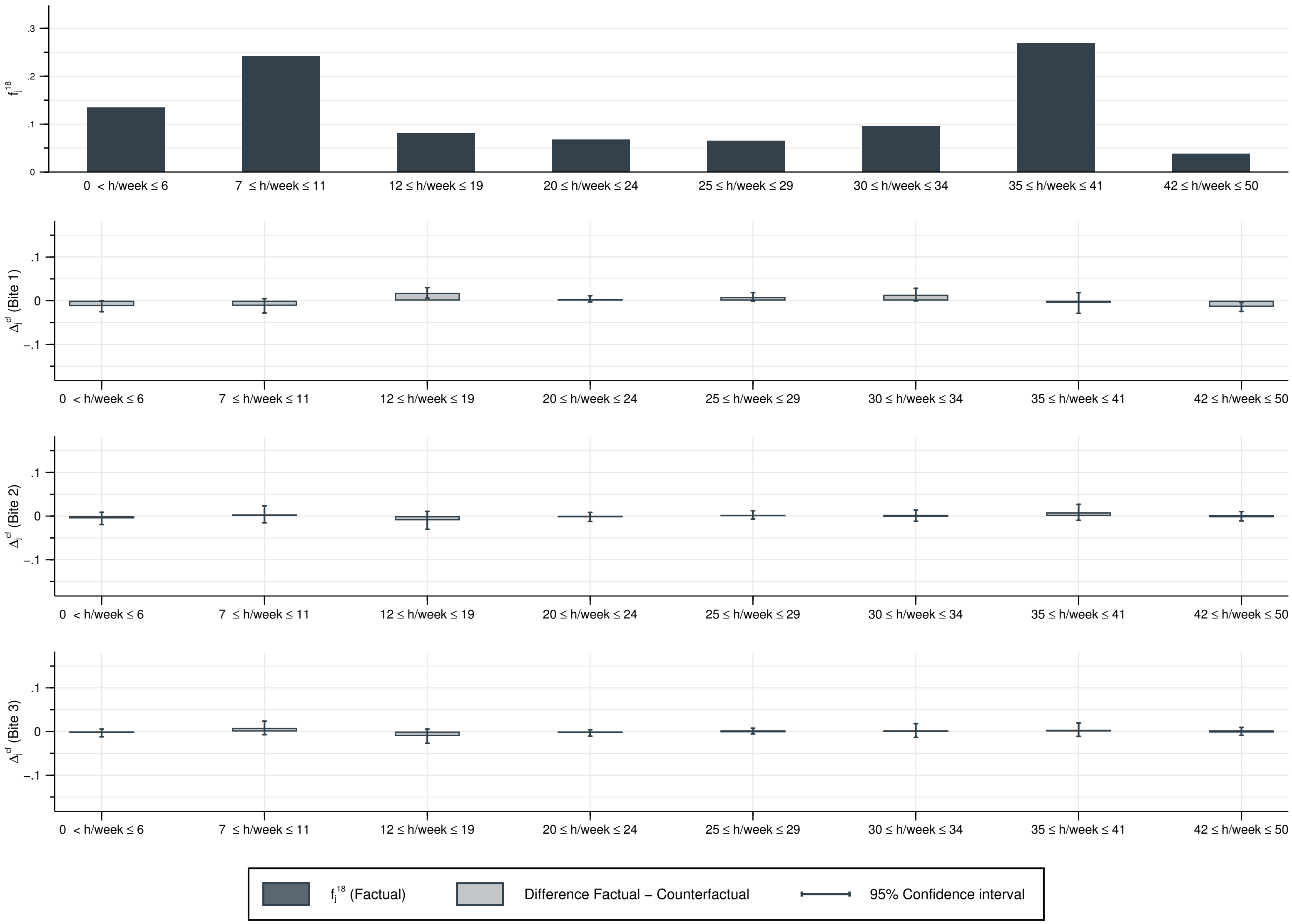
(a) Lower thresholds (3.49 to 8.49)



(b) Upper thresholds (9.49 to 14.49)

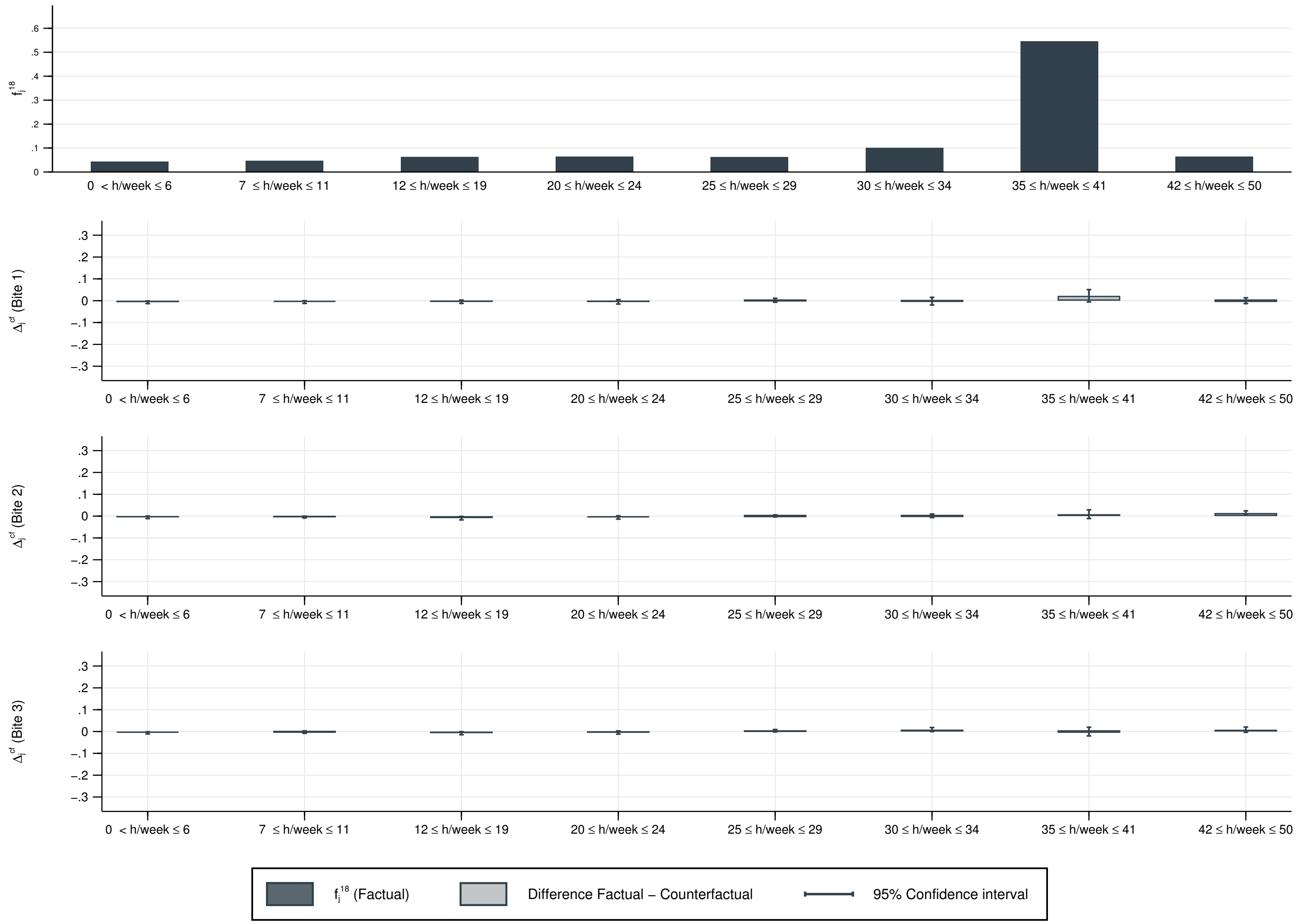
Notes: Estimates for the treatment effect, $\hat{\beta}_z^t$, in the pre-treatment periods 2011-2014 as specified in (11) for bins below and above the minimum wage level. Base period: 2014. Values in 2015 and 2016 refer to linearly extrapolated trends using the estimates from 2012, 2013, and 2014. *Source*: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



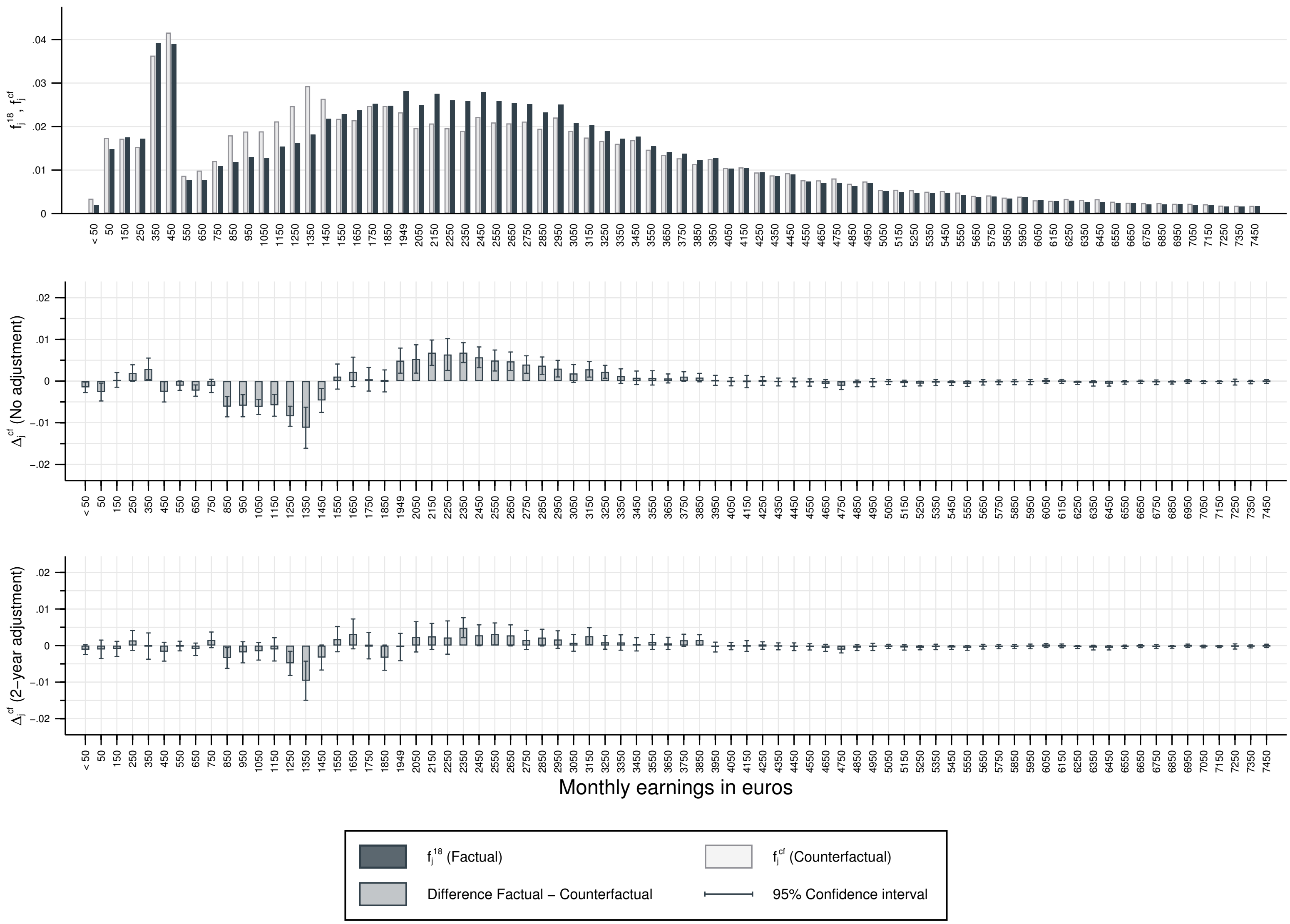
Notes: The bars in the first panel show the factual distributional mass in 2018. The three lower panels show differences between the factual and counterfactual bin frequencies for different bite specifications. 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: 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.



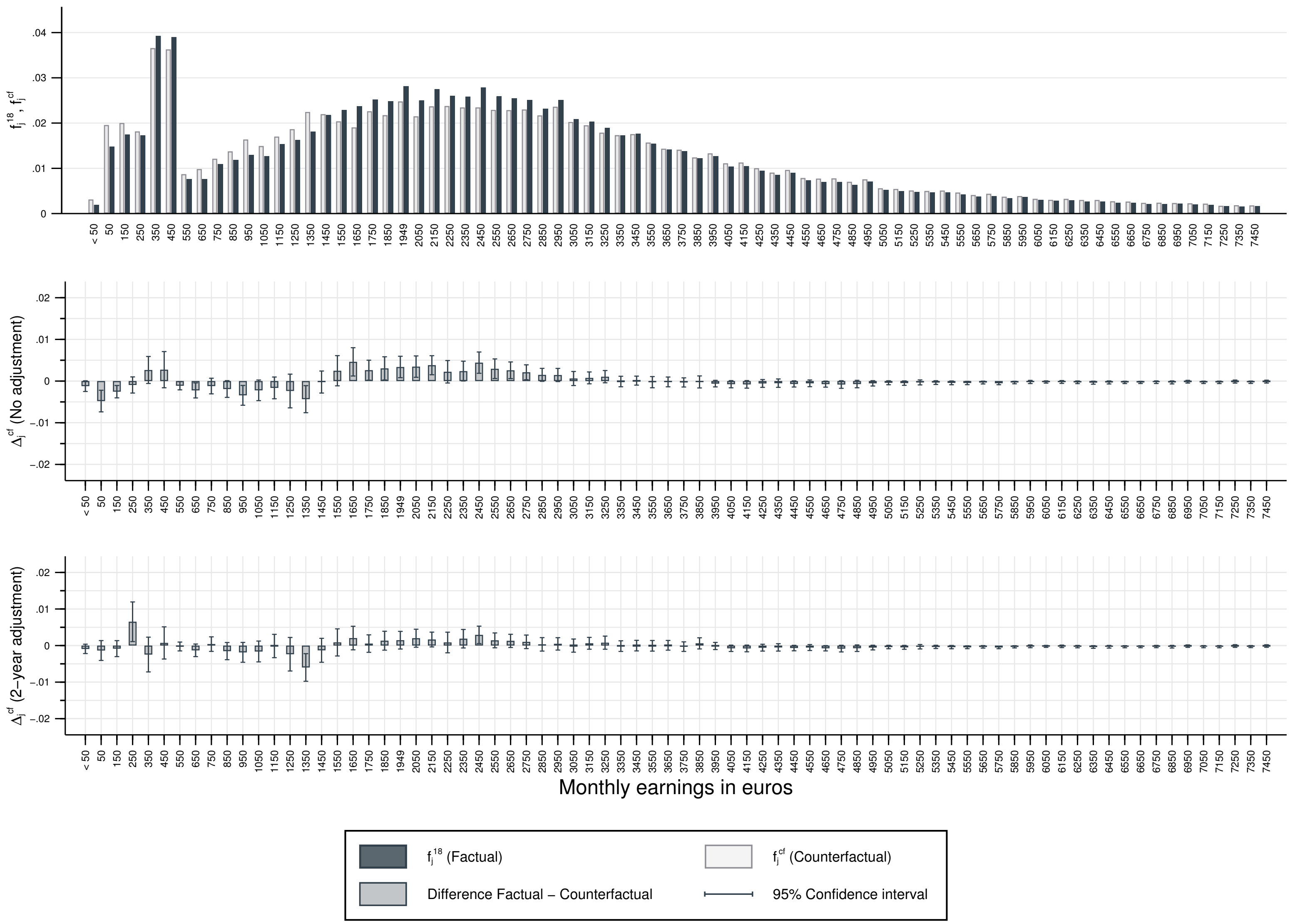
Notes: The bars in the first panel show the factual distributional mass in 2018. The three lower panels show differences between the factual and counterfactual bin frequencies for different bite specifications. 95% bootstrap confidence intervals (100 replications, clustered at treatment level). *Source*: 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.



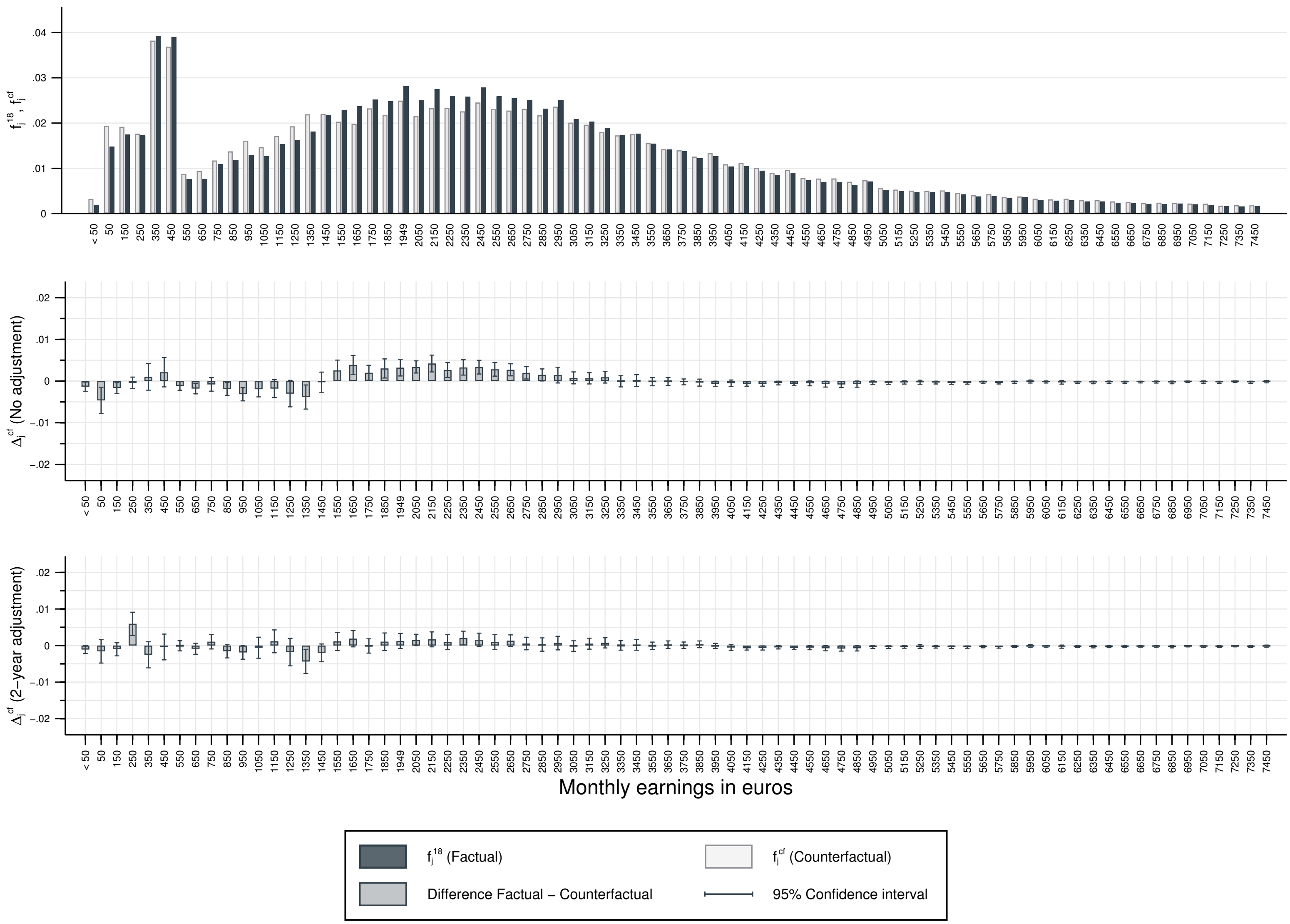
Notes: The x-axis shows monthly wage bins. For example, the '1050' bin comprises monthly earnings in the interval [1,050; 1,149] euros. The counterfactual bins in the first row of the figure correspond to the model-implied counterfactual distributional mass in the absence of the minimum wage *without 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

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

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) :

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

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

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

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 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., $1[y \leq z] = 1[\log(y) \leq \log(z)] = 1[y^* \leq z^*]$.

The original contribution by Roth and Sant'Anna (2023) provides an interpretation of the 'parallel trends assumption for the cumulative distribution function of untreated potential outcomes', implying that the underlying data generating process involves a mixture of random assignment and stationary potential outcomes. This strong interpretation has been questioned by Kim and Wooldridge (2024) who show that the condition only requires that distributional change in the treatment and control group has a common component absent treatment. Note that we only use a conditional version of the original condition which is substantially weaker irrespective of its exact interpretation.

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 .