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The devil is in the details: Heterogeneous effects of the German minimum wage on working hours and minijobs☆

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HIGHLIGHTS

- Following the introduction of the minimum wage in Germany in 2015, minijobbers experienced significant reductions in working hours due to institutional constraints.
- While employment in regular jobs remained largely unaffected, the number of minijobs declined, driven by transitions into both regular employment and non-employment.
- The estimated wage elasticity of minijob employment is −0.16.
- After the minimum wage hike in 2022, the reduction in working hours is not limited to minijobs, corresponding to an employment volume elasticity of −0.38.

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ABSTRACT

Germany introduced a national minimum wage in 2015. While prior studies find limited effects on overall employment, we go into detail and examine its impact on working hours and minijobs. The minimum wage significantly reduces inequality in hourly and monthly wages. While average working hours remain stable, minijobbers experience notable cuts in working hours, which can be explained by the institutional context shaping the effects of the minimum wage. Employment in regular jobs remains unaffected, but minijobs decline, driven by transitions into both regular jobs and non-employment. The latter implies an employment elasticity of −0.16 for minijob employment. Following the first major minimum wage increase in 2022, we reveal a reduction in working hours that is not limited to minijobs, corresponding to an employment volume elasticity of −0.38.

1. Introduction

Minimum wages are a popular policy to improve the situation of the working poor, but also to address rising wage inequality and the declining bargaining power of workers. A critical and detailed evaluation of minimum wages is essential to inform policymakers about potential adverse labor market effects, such as employment declines. We analyze heterogeneous effects of the German minimum wage, which was introduced at a level of €8.50 on 1 January 2015 with a major increase to €12 in October 2022. Germany provides an interesting case to study since it was one of the few Western economies that did not have a nationwide minimum wage before 2015. It offers a natural experiment with a substantial magnitude of the minimum wage. The introduction of the minimum wage directly affected around 10–14 % of the workforce, who had earned on average 26 % below the threshold in 2014.

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The group of minijobbers — workers earning less than €450 per month and not fully covered by the social security system — was particularly affected, with a fraction of 45 % paid below the minimum wage.

We examine the impact of both the 2015 introduction of the minimum wage and the 2022 hike by exploiting variation in the *bite* of the minimum wage (i.e., the share of workers affected) across regions in Germany. Thus, we identify minimum wage effects from difference-in-differences specifications, as first proposed in Card (1992). While our analysis provides a special focus on the adjustments in working hours, we also offer a comprehensive analysis of how the minimum wage affects hourly and monthly wages, as well as the number of employees. In particular, we are interested in whether the institutional setting leads to heterogeneous effects of the minimum wage, leading us to investigate heterogeneities associated with the €450-earnings threshold for minijobs.

Several important studies have investigated the labor market effects of the German minimum wage. Bossler and Gerner (2020) analyze employment effects using firm-level data and find that they are non-zero but relatively small in size. Caliendo et al. (2018, 2025) study regional-level employment effects, which appear insignificant in the aggregate but reveal a significant decline among minijobbers. Dustmann et al. (2022) offer an explanation for why disemployment effects remain relatively limited. While some firms lay off workers or even shut down, many affected employees are reallocated to higher-paying, potentially more productive establishments. A further explanation is provided by Link (2024) who finds that price adjustments play an important role and businesses with scope to raise prices are able to retain their workforce.[1] Finally, Bossler and Schank (2023) document that the minimum wage increases monthly wages, thereby playing a significant role in the decline in Germany's wage inequality. For a comprehensive review of the effects of the German minimum wage policy, see Dütsch et al. (2025). We make seven distinct contributions to this growing literature.

First, we examine the effects of the minimum wage introduction on hourly and monthly wages. The analysis of Bossler and Schank (2023) is limited to monthly wages. While monthly wages are more relevant for improving the situation of the working poor, minimum wages are an hourly wage policy that constitutes a decisive factor in labor demand and supply decisions. We provide novel and comprehensive evidence regarding the reduction of inequality in hourly wages. Using the German Structure of Earnings Survey, we can directly compare hourly and monthly wage adjustments using the same data and the same source of identifying variation. Our results show that the average wage effect is strikingly similar between hourly and monthly wages, suggesting the absence of working hours adjustments on the average. However, the percentage inequality reduction is larger for hourly than for monthly wages, which is consistent with heterogeneous responses in working hours, particularly at the lower end of the distribution. Still, in line with Bossler and Schank (2023), monthly wage inequality significantly declined, despite heterogeneous adjustments in hours worked. That is, working time adjustments did not offset the impact of the minimum wage on hourly wages. Further, while the hourly wage effect of the minimum wage is monotonically decreasing along the hourly wage distribution, we observe significant spillovers up to the median.

Second, we contribute to the literature on minimum wage effects and working hours. While there is considerable international evidence on the extensive margin of employment adjustments in heads (see Neumark and Wascher (2008) for a review), evidence of adjustments at the intensive margin of hours worked is relatively scarce and not conclusive. For the United Kingdom, for example, Stewart and Swaffield (2008) find a reduction in hours, while a more recent study by Datta and Machin (2021) finds only a redistribution of hours, but no overall reduction. For the U.S., the debate is even more controversial. For hours worked by teenage workers, who are strongly affected by the minimum wage, Couch and Wittenburg (2001) find a negative effect, while Zavodny (2000) finds no adverse hours effects. Examining the impact of two recent minimum wage hikes in the city of Seattle in 2015 and 2016, Jardim et al. (2022) report significant hours reductions, particularly in the short run and for less experienced workers. For Germany, Bossler and Gerner (2020) and Ohlert (2025) analyze firm-level average hours from establishment-level survey data, and Burauel et al. (2020) examine individual-level survey information. These studies obtain a short-run reduction in working hours in the first year after the minimum wage introduction, but they cannot identify an effect in the second year. Most similar to our study, Caliendo et al. (2023) and Biewen et al. (2022) evaluate the impact of the minimum wage on working hours and combine it with the analysis of monthly and hourly wages. Caliendo et al. (2023) report a negative hours effect, while Biewen et al. (2022) find no effect on hours. We contribute to the controversy by highlighting that there may not be a reduction in working hours on average, but that the impact varies by establishment size and employment type, with negative effects among small firms and minijobbers.

Third, we contribute to the literature on strongly affected groups of workers. While in the U.S., aggregate employment effects of minimum wages are typically modest, most of the literature concentrates on particular groups that are heavily affected by the minimum wage, including fast food workers (Card and Krueger, 1994), teenagers (Allegretto et al., 2011), individuals without a high school degree in low-wage occupations (Clemens et al., 2021), or highly affected workers identified through a data-driven approach (Cengiz et al., 2022). By contrast, in Germany, the fast-food sector is not as prominent, and teenagers (due to the apprenticeship system) rarely enter the labor market. Hence, in our analyses of the national minimum wage in Germany, we focus on minijobs as they are often considered a precarious segment in the German labor market. Minijobs are low-paid jobs with monthly earnings up to €450 and accounted for about 16 % of all jobs in the German labor market in 2014. There are marked differences between the characteristics of minijobbers and regular employees. Minijobbers are more likely to work in services than in the industrial sector. They are significantly more likely to be female, younger than 20 or older than 65, and less educated. Hence, minijobbers tend to have characteristics that are associated with disadvantages in the labor market.[2] Furthermore, minijobs are characterized by considerable job dynamics.[3] In fact, our results show that the group of minijobbers experiences more severe reductions in hours and job losses.

Fourth, we show that the institutional context in which the minimum wage applies crucially shapes its labor market impact. For Germany, the group of minijobs provides a particular institutional setting. For employers and employees, minijobs may be an attractive alternative compared to regular social security jobs, where the latter start at a salary of €451 per month.[4] The advantage for the employee is that no social security contributions or taxes are generally due. For the employer, although they still have to pay a flat-rate social security contribution for an employed minijobber, these employment types offer a low bureaucratic burden, no income tax processing, and often minijobbers are willing to accept a lower wage (since their net income is equal to their gross income) than regular employees.[5] However, when the minimum wage was introduced,

[1] Against the backdrop of international evidence on minimum wages, Clemens (2021) argues that the lack of substantial effects on employment is due to firms adjusting margins other than the size of their workforce.

[2] See Table A1 in Appendix A.

[3] One year later, 34 % of minijobbers from 2013 had left their positions. Of those who left, two-thirds moved to regular jobs, and one-third became unemployed (own calculations based on the IEB).

[4] Outside our main period of investigation, in 2022 and 2024, the minijob threshold increased in two steps to €538.

[5] Appendix B provides a summary of tax regulations and social security contributions for minijobs and for regular jobs.

the resulting rise in monthly wages pushed some minijobbers above the €450 threshold, in which case the advantages associated with the minijob status were lost. In our empirical analysis, we observe two responses. First, working hours were reduced to comply with increased hourly wages while preserving the minijobs. Second, some minijobs were terminated, in which case the affected workers were either upgraded to regular social security jobs or moved into non-employment. Both effects underline the hypothesis that institutional earnings thresholds can interact with minimum wages, resulting in potentially adverse labor market effects.

Fifth, we investigate the minimum wage effect on employment. There is broad consensus in the literature that the minimum wage has no economically significant effect on overall employment (Bruttel, 2019; Dütsch et al., 2025). We confirm this non-negative effect from a difference-in-differences specification that controls for county-level population size as a proxy for labor supply. Thus, we are the first to provide an economic mechanism to control for the pre-trend without relying on the relatively strict assumptions of a de-trended difference-in-differences specification.

In our employment analysis, we are able to distinguish between regular employment and minijobs. In their descriptive report to the German government, Vom Berge et al. (2018) document a reduction in the number of minijobs in January 2015, when the minimum wage was introduced. This reduction has been referred to as one of the most remarkable consequences of the minimum wage policy on the German labor market (German Minimum Wage Commission, 2023). The finding of a reduction in minijobs is corroborated in causal analyses by Caliendo et al. (2018) and Caliendo et al. (2025). Consistent with the literature, we show that the employment effect of minijobs is significantly negative. In absolute terms, 133,500 minijobs vanished due to the minimum wage. Contributing to the articles by Caliendo et al., we examine a hitherto unresolved question, namely, whether the respective minijobbers were upgraded to regular jobs or went into non-employment. Using the Integrated Employment Biographies (IEB), which is the universe of administrative employment data, as an additional data source, we find that 62,800 minijobbers (which is about half of the vanished minijobs) entered non-employment, while the other half was upgraded to regular employment (mostly within the same establishment). The non-employment transition can be interpreted as an employment elasticity for minijobbers with respect to wages of -0.16, while the employment elasticity for regular jobs is virtually zero.

Sixth, we contribute to the literature by presenting novel results on the 2022 minimum wage hike. In 2022, the minimum wage was raised from €9.82 to €12 in only a few months. This 22 % raise made the German minimum wage the highest in Europe in terms of purchasing power. Interestingly, the minijob threshold was harmonized with the minimum wage in 2022, such that there is no longer a need to adjust the minijobs. Hence, we no longer observe effects that are particular to the subgroup of minijobs. However, we observe an overall decline in working hours. The size can be interpreted as an elasticity of -0.38, which is the percentage change in the implied employment volume relative to the percentage wage change.

The literature highlights the relevance of the economic context in which a minimum wage is implemented. Addison et al. (2013) shows a more negative employment effect in states that were more severely hit by the financial crisis. Similarly, Clemens and Wither (2019) find that the increases in minimum wage during the financial crisis contributed to the overall employment decline during the recession.[6] For Germany, Caliendo et al. (2025) provide evidence that employment effects are more strongly pronounced in regions with low economic growth. Likewise, firms' employment adjustments are found to correlate with reports of poor business conditions (Link, 2024). Our finding of a more pronounced hours adjustment, and thereby, a more severe disemployment elasticity in response to the 2022/23 minimum wage hike than in response to the 2015 minimum wage introduction, adds to this literature. The economic conditions under which the minimum wage was raised in 2022 were significantly worse: while the German economy was steadily growing in the mid-2010s, it has been stagnating since 2019.

Seventh, we contribute to quantifying non-compliance — that is, the extent of sub-minimum-wage payments. Internationally, payments below the minimum wage are observed in both developed and developing countries, although the incidence is notably higher in the latter (see Goraus-Tańska and Lewandowski (2019) for a survey of international evidence on minimum wage violations). Clemens and Strain (2022) present evidence for the U.S. that higher minimum wages lead to a higher prevalence of sub-minimum wages.[7] Similarly, using European country-level data, Garnero et al. (2015) show that higher Kaitz indices (i.e., higher ratios of the minimum wage to the average wage) are associated with a higher proportion of individuals paid below the minimum wage. For Germany, the descriptive prevalence of non-compliance remains highly controversial (German Minimum Wage Commission, 2023). The Structure of Earnings Survey provides detailed and precise information on hours worked and monthly wages, thus defining the hourly wage that can be compared with the required minimum wage. Moreover, we can compare the initial and remaining wage gaps with the size of the wage effect to indirectly infer how much of the wage gap was closed by the minimum wage. The size of our wage effects indicates that more than the initial wage gap has been closed by the minimum wage. This is only possible if there are significant spillovers, which is consistent with our observed wage effects along the wage distribution (up to the median). However, we still observe a significant descriptive number of non-compliant wages, pointing to an important policy area.

The remainder of the article is structured as follows. Section 2 presents the data, which comprise the Structure of Earnings Survey, the Administrative Employment Data, and the Earnings Survey. The section also provides descriptive statistics of the main variables of interest and documents changes at the lower end of the wage distribution. Section 3 describes the empirical strategy, which is a difference-in-differences approach that identifies minimum wage effects from regional variation in the bite. Section 4 reports and discusses the difference-in-differences effects on hourly wages, monthly wages, and hours worked. Section 5 presents heterogeneities in the hours effect across establishment size, along the hours distribution, and between minijobs and regular jobs. Section 6 investigates employment effects with a special focus on minijobbers, where we first analyze aggregate employment effects and then examine individual transitions out of minijobs. Section 7 analyzes the effects of the minimum wage increase to €12 in 2022. Finally, Section 8 concludes.

2. Data

For our comprehensive analysis of minimum wage effects, we use various data sources on the German labor market: First, we employ the Structure of Earnings Survey (SES) to analyze wage and hours effects of the minimum wage introduction, as presented in Section 4, and to examine effect heterogeneities, as discussed in Section 5. Second, we draw on the Administrative Employment Data (BHP and IEB) to analyze employment effects, as presented in Section 6 for the minimum wage introduction and in Section 7 for the 2022 minimum wage hike. Third,

[6] More recently, Karabarbounis et al. (2022) suggests that minimum wage hikes in Minneapolis resulted in stronger job losses during the Covid-19 pandemic.

[7] Non-compliance shares are similar across age groups but highest for young African American workers (Clemens and Strain, 2023). In Italy, non-compliance trades off with employment losses (Garnero and Lucifora, 2022).

we utilize the Earnings Survey (ES) to analyze wage and hours effects of the 2022 minimum wage hike, as presented in Section 7.

Structure of Earnings Survey (SES). Most importantly, for the analysis of wages and working hours, we use the years 2010, 2014, and 2018 of the SES, collected by the Federal Statistical Office of Germany every four years until 2018. It is a linked employer–employee data set consisting of repeated cross-sections. For each employee, it covers information on employment characteristics, gross monthly wages (deflated by the CPI to 2014 €), contractual working hours, and paid hours (which also include paid overtime hours). The sampled establishments are legally obliged to provide information on a random sample of employees directly from the payroll accounting for April of the respective years.[8] Therefore, the information in the SES is highly reliable. We analyze the data of the 2010, 2014, and 2018 waves, covering two pre-periods and one post-period of the introduction of the minimum wage policy.

The sample of the SES varies between 2010 and 2014/2018. To ensure time consistency of the sample, we drop establishments in agriculture. We also need to restrict the sample to establishments with at least 10 employees when we estimate treatment effects together with pre-trend placebo effects, since small establishments are not included in the 2010 data. This restriction applies to the results of Section 4, where we analyze the effects of the minimum wage introduction on wages and hours. However, the restriction is relaxed in Section 5 when examining effect heterogeneities by establishment size, and is also removed in Sections 6 and 7 when analyzing employment effects and the impact of the 2022/23 minimum wage hike, respectively. Finally, we exclude the public sector due to the missing regional information, and we exclude apprentices as they are exempt from the minimum wage legislation.

To ensure representativeness, we apply weights targeting the employment population. We calculate the weights based on a stratification including three establishment size categories, regular jobs and minijobs, manufacturing and services, and 400 counties where the latter is important for our regional identification of treatment effects.[9] In a robustness check presented in Appendix E, we use an alternative weighting factor using nine industries to better reflect a changing industry composition in the data. While it comes at the cost of somewhat higher variability in the weighting factors, the results remain fully robust.

Table 1 provides an overview of the employee-level data that we analyze. While the number of observations shrank from 2010 to 2014 and again slightly in 2018, the weighted number of employees in establishments with at least 10 employees grew from 23.27 million in 2010 to 25.24 million in 2014 and 27.40 million in 2018. These figures are consistent with relatively strong overall employment growth at that time. At the same time, the fraction of minijobs fell. One year before the minimum wage was introduced (in 2014), 11.8 % of the workers were still paid below the initial minimum wage of €8.50, highlighting the relevance of the policy. For 2018, we observe that only 0.4 % of the workers were paid below €8.50, while 2.5 % were paid below the new minimum wage level, which was increased to €8.84 in 2017. While this shows a significant impact of the minimum wage on reducing low-wage employment, the remaining employees below the minimum wage suggest some degree of non-compliance.[10]

Regarding wage growth, Table 1 shows stagnating average real wages in the early 2010s, while wages (hourly and monthly) started to grow after the introduction of the minimum wage. Looking at the standard deviation of the respective variables, we observe a continuous decrease in log wage dispersion. This decline in descriptive wage inequality concerning log monthly wages, which started to emerge before the introduction of the minimum wage, is consistent with findings in Bossler and Schank (2023). At the same time, the average number of working hours (with and without paid overtime) remained fairly constant during the period of analysis.

Fig. 1 provides a more precise description of changes along the wage distribution when the minimum wage was introduced. Applying the illustration from Cengiz et al. (2019), we examine the mass of employees along the real hourly wage distribution. The bars show, for each wage bin, the difference in the weighted number of jobs between 2018 and 2014, i.e., after and before the minimum wage introduction in 2015. The figure clearly documents a reduction in the number of jobs up to the 2018 minimum wage level of €8.84 (expressed in 2014 €), which is in line with the intention of the policy to reduce the number of jobs below the minimum wage. We also observe a spike to the right of the minimum wage, indicating an increased mass of jobs that are paid the minimum wage (or slightly above). These observations are consistent with workers receiving a wage increase and thereby moving to the right on the x-axis.

The solid line in Fig. 1 illustrates the cumulative change in the mass of jobs along the wage distribution. It shows that not the entire mass of workers paid below the minimum wage in 2014 received a wage rise exactly to the level of the minimum wage. Rather, the line crosses the horizontal axis (indicating unchanged total employment) not before the €18 mark. This may be because minimum wage workers moved further up the wage distribution.[11]

Administrative employment data. We use the German Administrative Employment Data hosted by the Institute for Employment Research (IAB). In particular, we use the Establishment History Panel (BHP), which contains the number of employees (also by subgroup) of each establishment in the German labor market. After aggregating the establishments' employment to stratification cells defined above, we employ this data to construct weighting factors. Moreover, we use the data to create a county-by-year panel data set for all employees, regular employees, and minijobs, to estimate region-level employment effects.[12]

In addition, we use the individual-level administrative employment spells of the Integrated Employment Biographies. The IEB covers the full population of employment spells except for those of civil servants and the self-employed, which are, by and large, irrelevant to the minimum wage policy.[13] From these data, we construct a yearly panel based on employment spells covering June 30th for the years 2012 to 2015.[14] The data allow us to trace transitions between employment statuses, to analyze transitions out of minijobs, as well as transitions of minijobbers into non-employment and into regular employment.

[8] Establishments with fewer than ten employees are obligated to report information on all employees. The proportion of sampled employees then decreases with establishment size. For example, in 2014 and 2018, establishments with at least 100 and fewer than 250 employees were obliged to report information on every sixth employee.

[9] The weights are based on establishment size categories of 1–9, 10–99, and more than 100 employees. While the first category is not used in the main specifications, it is included in further analyses.

[10] According to the descriptive figures, the fraction of wages below the initial minimum wage decreased already between 2010 and 2014 (from 17.5 to 11.8 %). However, in real terms, the fraction of workers below €8.50 remained fairly constant (12.8 % in 2010 compared with 11.8 % in 2014).

[11] The monthly wage distributions of 2010, 2014, and 2018 are shown in Appendix D. For all three years, the mode at the minijob threshold is clearly visible. Moreover, between 2014 and 2018 there is a rightward shift in the monthly wage distribution of full-time workers, which is not the case when comparing the 2010 and 2014 distributions, i.e., before the minimum wage was introduced.

[12] See Appendix C.1 and Ganzer et al. (2020) for more details on the BHP.

[13] A description of the data is presented in Appendix C.2 and in Müller and Wolter (2020).

[14] We exclude apprentices, interns, and (for tractability of the transitions) individuals with multiple jobs. The exclusion of multiple jobs focuses our analysis on the group of low-income minijobbers for whom the minijob is the only source of labor income. Additional analyses in Appendix N show that the restriction leads to a more vulnerable target group of low-educated minijobbers. The relevance of multiple jobs for individuals affected by the minimum wage is addressed in Vom Berge and Umkehrer (2023).

Table 1
Weighted summary statistics, establishments (≥ 10 employees).

	2010		2014		2018	
	mean	(s.d.)	mean	(s.d.)	mean	(s.d.)
Gross hourly wage (2014 €)	18.08	(12.57)	17.59	(12.51)	19.15	(14.26)
Gross monthly wage (2014 €)	2626.45	(2262.89)	2590.70	(2275.51)	2804.76	(2539.74)
Log gross hourly wage (2014 €)	2.73	(0.56)	2.72	(0.52)	2.82	(0.49)
Log gross monthly wage (2014 €)	7.48	(1.05)	7.48	(1.02)	7.57	(1.01)
Contractual monthly hours	132.82	(53.95)	135.15	(53.67)	134.88	(53.14)
Paid monthly hours	134.64	(55.41)	136.77	(54.85)	136.32	(54.11)
Log contractual monthly hours	4.732	(0.678)	4.750	(0.686)	4.744	(0.704)
Log paid monthly hours	4.742	(0.683)	4.760	(0.689)	4.753	(0.706)
East Germany	0.165		0.164		0.164	
Female	0.434		0.445		0.436	
Low education	0.190		0.121		0.110	
Medium education	0.693		0.699		0.684	
High education	0.118		0.180		0.207	
Regular job (weekly hours)						
at least 30	0.684		0.697		0.705	
between 18 and 30	0.104		0.106		0.108	
less than 18	0.038		0.033		0.037	
Minijob	0.173		0.165		0.150	
Bite: Nominal gross hourly wage below the 2015 min. wage (€8.50)	0.175		0.118		0.004	
…their relative wage gap to €8.50	0.399	(0.844)	0.256	(0.477)	0.354	(0.800)
Nominal gross hourly wage below the 2018 min. wage (€8.84)	0.196		0.144		0.025	
…their relative wage gap to €8.84	0.409	(0.840)	0.254	(0.461)	0.077	(0.375)
Weighted no. of observations	23,269,170		25,236,570		27,403,608	
Actual no. of observations	1,493,904		605,352		568,337	

Notes: Weighted sample averages and standard deviations by year of observation. No standard deviations are reported for dummy variables. All wages are deflated to 2014 €. Low education denotes neither *Abitur* nor vocational training; medium education denotes *Abitur* and/or vocational training; high education denotes master craftsman/technician or university degree. The relative gap is calculated as (8.50 – nominal gross hourly wage)/nominal gross hourly wage. *Data:* SES, 2010, 2014, and 2018, weighted analysis sample, establishments with at least 10 regular employees.

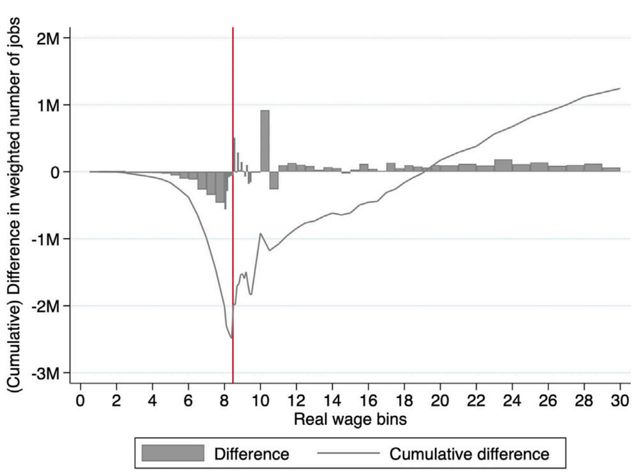


Fig. 1. Changing mass of jobs along the wage distribution between 2014 and 2018, establishments (≥ 10 employees). *Notes:* Changes in the number of employees in millions (between 2018 and 2014) by real hourly wages (in 2014 €), as in Cengiz et al. (2019). Employees with real hourly wages above €30 not included. These amount to 10.2 % (12.7 %) of all employees in 2014 (2018). The vertical line is the initial minimum wage of €8.50, which corresponds with the real minimum wage of 2018 (see Footnote 15). *Data:* SES, 2014 and 2018, weighted analysis sample, establishments with at least 10 regular employees.

Earnings Survey (ES). For the analysis of the 2022 minimum wage increase, we make use of the ES. The ES is the successor to the SES, which was discontinued after 2018. The ES is surveyed annually, as a panel data set, and establishments are required by law to report data on all of their employees when selected to participate. We use the first two data waves collected in April 2022 (before the minimum wage increased in July and October of that year) and in April 2023 (after the minimum wage had increased to €12). The data cover the entire economy, including all establishment size categories and industries, and it collects worker-level information on the type of job, allowing for a distinction between minijobs and regular jobs, as well as information on wages and contractual working hours. Descriptive statistics are presented in Appendix Table R1.

3. Empirical strategy

We adopt a difference-in-differences framework exploiting regional variation in the minimum-wage bite to identify the policy's effect, as first proposed by Card (1992). The bite is calculated as the share of workers in a county paid below the minimum wage threshold (€8.50 per hour) in 2014, i.e., before the minimum wage came into force.[15] Fig. 2 clearly demonstrates significant variation in the regional treatment intensity of the minimum wage policy across Germany. While the bite exceeds 20 % in eastern German regions, it is particularly low in southern Germany, falling below 5 % in some of its counties.

[15] We calculate the bite from nominal wages paid in 2014 evaluated at the nominal minimum wage level of €8.50 at its introduction in January 2015. However, a bite based on the real minimum wage of 2018 would be largely equivalent to the bite of the nominal minimum wage of 2015. Our definition of the bite assumes that workers paid between 8.50 and 8.84 in 2014 are not treated because they would have received a pay rise above 8.84 until 2018, even in the absence of the minimum wage. This conjecture is supported by the positive development of collectively bargained wages in these years.

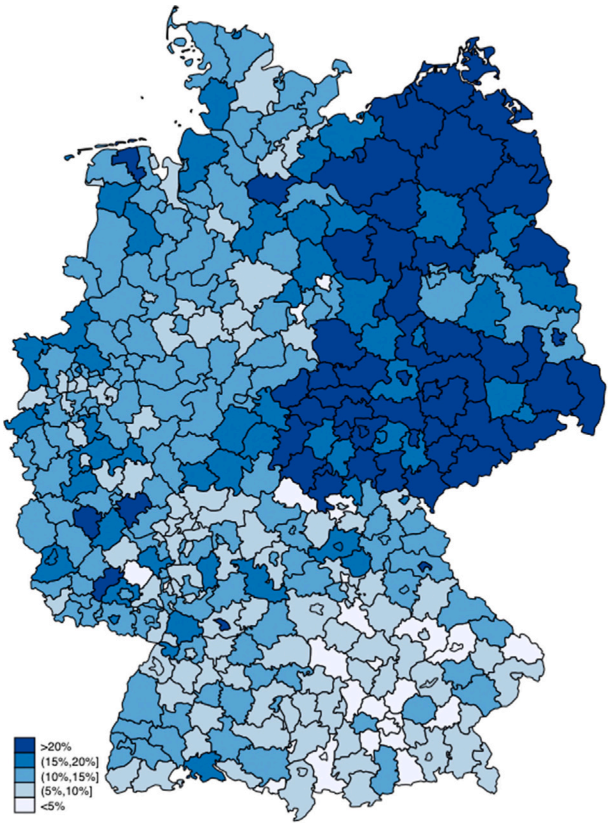


Fig. 2. Distribution of the bite across counties in Germany, establishments (≥ 10 employees). *Notes:* The map displays the distribution of the bite across German counties, where the bite is defined as the weighted share of all workers paid below the €8.50 minimum wage in 2014. *Data:* SES 2014, establishments with at least 10 regular employees.

Based on the SES data, we estimate the following regression specification for employee i in year t working in county r:

$$y_{it} = \delta_0 * Bite_r + \delta_{2010} * Bite_r * Year_{2010,t} + \delta_{2018} * Bite_r * Year_{2018,t} + \boldsymbol{X}_{it}\boldsymbol{\beta} + \mu_{Year\times Industry} + \epsilon_{it} \quad t = 2010, 2014, 2018 \tag{1}$$

where y_{it} denotes the logarithm of the respective dependent variable (hourly wage, monthly wage, working hours). We estimate not only conditional mean specifications but also unconditional quantile and variance regressions, where y_{it} is the RIF (re-centered influence function) at quantile q. $Bite_r$ is the (regression sample) weighted share of workers in region r earning in 2014 below the minimum wage of €8.50. The bite is recalculated if we analyze a different sample, e.g., minijobbers or small establishments. The coefficient δ_{2018} captures the treatment effect of the minimum wage policy, whereas δ_{2010} serves as a placebo test concerning the plausibility of the parallel trends assumption. $\boldsymbol{X}_{it}$ denotes a set of control variables including age and dummies for gender and education. In addition, the covariates include the county-level population size in order to control for pre-trends, as discussed below in Section 6. To allow for potentially heterogeneous developments of the dependent variable across industries, we include year–industry fixed effects ($\mu_{Year\times Industry}$), capturing time effects for 82 industries according to the two-digit NACE Rev. 2 classification. All regressions are weighted as described in Section 2, and standard errors are clustered at the county level.

4. Results on wage and hours adjustments

We begin our regression analysis with the level and variance of log hourly wages as dependent variables. The estimates of the interaction effects are reported in Table 2. The first column shows a significant minimum wage effect on hourly wages. A 10-percentage-point increase in the regional bite causes wages to rise by 4.26 %. As reported in Table 1, on average (real) hourly wages increased by about 10 % between 2014 and 2018. Given the average bite of 11.8 %, roughly half of the hourly wage increase can be attributed to the minimum wage introduction.[16] Note that the placebo interaction of the bite and the year 2010 is insignificant and small, supporting the internal validity of the reported minimum wage effect. As depicted in the second column, the minimum wage reduced the dispersion in hourly wages. A 10-percentage point increase in the regional bite implies a *ceteris paribus* reduction in the hourly wage variance of 0.061. This represents a meaningful effect size, as the variance was 0.31 in 2010. Again, the respective placebo interaction effect is small and insignificant.

Table 2
Minimum wage effects on the mean and the variance of log hourly and monthly wages, establishments (≥ 10 employees).

	log hourly wage		log monthly wage	
	mean	variance	mean	variance
$Bite * Year_{2010}$	−0.065	−0.071	−0.041	−0.072
	(0.042)	(0.044)	(0.070)	(0.193)
$Bite * Year_{2014}$	Reference	Reference	Reference	Reference
$Bite * Year_{2018}$	0.426***	−0.613***	0.428***	−1.025***
	(0.030)	(0.029)	(0.059)	(0.195)
Average *Bite*	0.118	0.118	0.118	0.118
Clusters	400	400	400	400
Observations	2,667,593	2,667,593	2,667,593	2,667,593

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (1). The dependent variable is either the log hourly wage, the log monthly wage, or the RIF of the variance of log hourly or log monthly wages, as indicated by column titles. *Bite* is the weighted share of all workers (in establishments with at least 10 regular employees) earning below the €8.50 minimum wage in 2014 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* SES, 2010, 2014, and 2018, weighted analysis sample, establishments with at least 10 regular employees.

A more comprehensive picture is obtained by integrating the results on monthly wages (third and fourth columns of Table 2). We find that monthly wages increase on average by 4.28 % if the bite increases by 10 percentage points. This magnitude is close to the hourly wage effect, already providing evidence that, on average, working hours did not decrease by a significant margin. The last column shows that also for log monthly wages, the variance decreases after the introduction of the minimum wage. A 10-percentage point increase in the regional bite leads to a reduction in the variance of about 0.103. Since the variance of log monthly wages is at a higher level (1.1 in 2010), the reduction in relative terms is not as pronounced as for the variance in hourly wages. This difference in the reduction of the variance is consistent with heterogeneous responses in working hours, although not at the average. However, while there might be hours adjustments along the distribution, they clearly did not offset the positive hourly wage gains induced by the minimum wage. Again, the placebo interactions with the year 2010 are small and insignificant, supporting the causal interpretation of the baseline estimates also for monthly wages.

Notably, the baseline hourly wage effect already points to significant spillovers. If we extrapolate and compare the average hourly wage effect of 42.6 % (for affected workers) with the average initial wage gap of affected workers of 26 %, it is evident that the wage increase caused by the minimum wage is larger than the initial wage gap. This result aligns with regressions using the bite gap (Appendix F), which demonstrates

[16] The average percentage hourly wage increase does not allow us to infer the change in the overall wage bill — even for a given level of employment — because the minimum wage predominantly affects workers with low hours.

Table 3
Minimum wage effects along the hourly and the monthly wage distribution, RIF regression estimates, establishments (≥ 10 employees).

	q4	q8	q12	q16	q20	q30	q50	q70	q90
A: Log hourly wages									
$Bite * Year_{2010}$	−0.264	0.169***	0.166***	0.118*	0.092*	0.009	−0.127*	−0.212***	−0.084
	(0.147)	(0.049)	(0.046)	(0.047)	(0.045)	(0.062)	(0.049)	(0.050)	(0.057)
$Bite * Year_{2014}$	Reference	Reference	Reference	Reference	Reference	Reference	Reference	Reference	Reference
$Bite * Year_{2018}$	2.654***	1.450***	1.053***	0.815***	0.726***	0.641***	0.181***	−0.017	−0.199***
	(0.085)	(0.031)	(0.031)	(0.037)	(0.040)	(0.054)	(0.046)	(0.040)	(0.053)
Average *Bite*	0.118	0.118	0.118	0.118	0.118	0.118	0.118	0.118	0.118
Clusters	400	400	400	400	400	400	400	400	400
Observations	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593
B: Log monthly wages									
$Bite * Year_{2010}$	−0.171	0.095	0.004	0.122	0.198	0.012	−0.040	−0.139**	−0.069
	(0.316)	(0.164)	(0.055)	(0.085)	(0.289)	(0.096)	(0.055)	(0.052)	(0.060)
$Bite * Year_{2014}$	Reference	Reference	Reference	Reference	Reference	Reference	Reference	Reference	Reference
$Bite * Year_{2018}$	1.849***	0.684***	0.146***	0.186**	1.027***	1.091***	0.274***	0.003	−0.184**
	(0.319)	(0.158)	(0.044)	(0.065)	(0.240)	(0.081)	(0.049)	(0.038)	(0.057)
Average *Bite*	0.118	0.118	0.118	0.118	0.118	0.118	0.118	0.118	0.118
Clusters	400	400	400	400	400	400	400	400	400
Observations	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593	2,667,593

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (1). The dependent variable is the RIF of the log hourly wage (upper panel), and the log monthly wage (lower panel), defined for various percentiles, as indicated by column titles. *Bite* is the weighted share of all workers (in establishments with at least 10 regular employees) earning below the €8.50 minimum wage in 2014 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* SES, 2010, 2014, and 2018, weighted analysis sample, establishments with at least 10 regular employees.

that the effect exceeds the initial gap to be closed, thereby suggesting spillovers.

In the presence of spillovers, positive wage effects can coexist with significant non-compliance. A descriptive assessment of the 2018 data shows that 2.5 % of workers were still paid below minimum wage in 2018. Among those workers, the remaining wage gap is 7.7 %, which is 30 % of the initial wage gap in 2014 (see Table 1). While the magnitude of non-compliance with the German minimum wage is still controversial in the literature (Caliendo et al., 2019; German Minimum Wage Commission, 2023), our share of non-compliant wages is meaningful given that it is employer-reported data. It points to the importance of institutions and inspections to monitor compliance (Bossler et al., 2022; Goerke and Pannenberg, 2024). However, it is possible that effects on wages (but also adverse employment effects) could be stronger if non-compliance were reduced (Garnero and Lucifora, 2022; Yaniv, 2001). Note that it does not pose a threat to our identification because we estimate unconditional reduced form effects which reflect ex-post realized outcomes after the minimum wage introduction.

Table 3 reports difference-in-differences effects of the minimum wage along the wage distribution from unconditional quantile regressions. As expected, the minimum wage effect is largest at the bottom of the distribution and then monotonically decreases. This matches the observed reduction in the variance discussed above. Interestingly, the treatment effect is statistically and economically significant up to the median, implying considerable spillover effects in the hourly wage. This is also consistent with Fig. 1, which shows significant employment increases in wage bins that are far above the minimum wage threshold.

When looking at the effects of the minimum wage introduction along the monthly wage distribution, we observe spillover effects up to the median. Moreover, similar to the findings of Bossler and Schank (2023), there is an interesting hump-shaped pattern between the 4th and the 30th percentile, with the smallest treatment effect at the 12th percentile. This is exactly where subsidized minijobbers are located in the monthly wage distribution. The small treatment effect is consistent with many minijobbers not being promoted to regular employees after the minimum wage increase. Hence, the results clearly demonstrate that the minijob institution dampened the minimum wage effect on monthly wages for the respective workers. Note that for both hourly and monthly wages, there is a negative treatment effect at the 90th percentile.[17] This negative effect is consistent with Gregory and Zierahn (2022), who examine the minimum wage introduced since 1997 in parts of the German construction sector. While the authors also obtain positive spillovers above the minimum wage, they find that the minimum wage caused a reduction in wages for the highest quantiles.

The results of estimations with working hours as the dependent variable are presented in Table 4. When using the logarithm either of contractual or paid monthly working hours, the minimum wage effect turns out to be statistically and economically insignificant on average. This confirms the finding above that the minimum wage affected average hourly and monthly wages similarly. The effects on the level of both hours variables are slightly negative, i.e., the coefficient of −6.316 for contractual hours multiplied by the bite of 0.118 yields a reduction of 0.75 h compared to the monthly average of 135 hours in 2014. Although the placebo coefficient on paid hours may call into question the validity of these findings, the differing results from the log and level specifications nonetheless suggest heterogeneous adjustment along the hours distribution.

Robustness checks. We perform a series of robustness checks, summarized in Appendix G.

(i) One may argue that wages would have also grown in the counterfactual scenario, i.e., even in the absence of the minimum wage introduction, in which case the effective bite may have been uniformly less than the bite implied by holding fixed the nominal

[17] More detailed RIF regressions for each percentile (untabulated) already show a negative (but smaller) coefficient at the 80th percentile (−0.112 for hourly wages, respectively −0.087 for monthly wages). Furthermore, the statistically significant negative effect is (in absolute terms) monotonically increasing until the top of the distribution. At the 97th percentile, the coefficient is −0.313 for hourly wages, respectively −0.254 for monthly wages.

Table 4
Minimum wage effects on monthly working hours, establishments (≥ 10 employees).

	Contractual hours		Paid hours	
	level	log	level	log
$Bite * Year_{2010}$	0.826	0.011	3.120	0.025
	(3.629)	(0.052)	(3.626)	(0.052)
$Bite * Year_{2014}$	Reference	Reference	Reference	Reference
$Bite * Year_{2018}$	−6.316*	0.006	−6.661*	0.002
	(2.995)	(0.049)	(3.058)	(0.049)
Average *Bite*	0.118	0.118	0.118	0.118
Cluster	400	400	400	400
Observations	2,667,593	2,667,593	2,667,593	2,667,593

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (1). The dependent variable is either the (monthly) contractual or the (monthly) paid working hours in levels or in logarithms, as indicated by column titles. *Bite* is the weighted share of all workers (in establishments with at least 10 regular employees) in the data earning below the €8.50 minimum wage in 2014 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* SES, 2010, 2014, and 2018, weighted analysis sample, establishments with at least 10 regular employees.

wage distribution at €8.50. We examine the sensitivity of constructing the bite measure using two variants: we have redefined the bite as the share of workers in 2014 earning below €8.40, and, respectively, the share of workers earning below €8.00. The first variant assumes that wages would have grown by 1.2 %, while the second variant assumes that wages would have grown by 6.25 % in any case. Note that actual inflation between 2014 and 2015 amounted to 0.6 %, while the actual inflation between 2014 and 2018 was 4.3 %. The effect on hourly and monthly wages slightly increases for the second alternative bite, capturing only workers paid below €8.00 (with a change in coefficients from −0.43 to −0.51), which seems perfectly plausible.

(ii) Since we do not know the actual bite per county, throughout our analysis, we use an estimate of the regional bite by taking the weighted average of the observed employees within a county. Hence, the variable will by definition include some measurement error, particularly if there are only a few observations in a county. To examine its importance, we run regressions without those 32 counties with fewer than 500 observations in 2014, and in addition, we run regressions without those 91 counties with fewer than 30 surveyed establishments in 2014. Excluding these counties has only a minor effect on the baseline estimates of the minimum wage impact on hourly and monthly wages.

(iii) We add county fixed effects to account for the possibility that the unbalancedness of the sample across regions might influence the results, but these remain literally unaffected.

(iv) The inclusion of covariates in a difference-in-differences specification is often a contentious issue. Since they may be affected themselves by the treatment, such that controlling for them may capture a genuine treatment effect. Therefore, we repeat the analysis above without covariates, but our findings do not change.

(v) We censor hours worked above 250 h per month to examine whether these 0.2 % outlier observations in hours worked influence the results, but this is not the case.

(vi) So far, the analysis has been based on establishments with at least 10 regular employees since small establishments are not included in the 2010 wave. To examine the severity of this restriction, we rerun the wage regressions using the waves 2014 and 2018 only, thereby allowing us to also include establishments with fewer than 10 regular employees. Hence, a placebo term for 2010 cannot be included. The minimum wage effect on hourly wages for all establishments is very similar to the baseline results. In contrast, the coefficient of the treatment variable in the monthly wage specification has been reduced (0.367 for all establishments versus 0.428 in the baseline). This indicates that the hours effect may differ by establishment size which we will examine in the next section.

(vii) A threat to our regional identification of minimum wage effects is that estimates could be driven by different trends between Eastern and Western Germany, because Eastern German regions are more severely affected than the West. We test this conjecture by repeating our analysis with data only for Western Germany, which comprises 81 % of all counties. It turns out that our baseline results do not simply reflect different trends between Western and Eastern Germany since they also hold within the West.

(viii) An additional comprehensive robustness check (presented in Appendix F) concerns the definition of our treatment variable. In our baseline regressions, the treatment variable measures the share of workers paid below the minimum wage before it was introduced. In an alternative specification, we instead use the county-level average bite gap. For workers initially paid below the minimum wage, the bite gap defines the percentage gap to be closed by the minimum wage (while it is set to zero for those already paid above the minimum wage). The results are quantitatively similar when using the bite gap. Again, the hourly wage effect closely aligns with the monthly wage effect, and the hours effect (in logs) is insignificant on average. Again, the monthly wage effect is much smaller at the 16th percentile of the distribution, where the institutional minijob threshold slows down the minimum wage effect.

5. Heterogeneities in the hours adjustment

According to our baseline results, average hours worked remain unaffected after the minimum wage introduction. In this section, we examine whether there are heterogeneous effects by establishment size or along the hours distribution. Thereafter, we focus on minijobbers.

5.1. Hours adjustments in small establishments

Our baseline sample is restricted to establishments with at least 10 regular employees subject to social security. The exclusion of small establishments ensures a consistent sample over time, as establishments with fewer than 10 regular employees were excluded from the 2010 SES wave. However, the wave of 2010 is crucial to test for the existence of a bite-specific trend between 2010 and 2014, i.e., before the minimum wage was introduced. The rejection of a bite-specific trend between 2010 and 2014 supports the plausibility of the parallel trends assumption between 2014 and 2018, implying that in the absence of the minimum wage introduction, we would not identify any treatment effect.

To analyze the effect of the minimum wage on working hours at small establishments, we can only use data from the 2014 and 2018 waves. Consequently, we cannot check for a pre-treatment bite-specific trend in working hours.[18] Since Table 4 shows no evidence of such a trend for establishments with at least 10 regular employees, we now have to assume that this is also the case for small establishments with fewer than 10 regular employees.

The first two columns of Table 5 report hours regressions for workers of medium-sized and large establishments. The results closely align with the baseline results of Table 4 since the only difference is the exclusion of 2010 data.[19] When we instead analyze the contractual hours

[18] Biewen et al. (2022) correct the hours trend in the SES data using an alternative data source, the German Social Accident Insurance, but find hardly any trend divergence in the years before 2015.

[19] The slightly negative level effect in the first column of Table 5 is driven by the relatively smaller establishments within the group of medium-to-large

Table 5
Minimum wage effects on monthly working hours; small (< 10 employees) versus medium-sized and large (≥ 10 employees) establishments.

	establishments (≥ 10 employees)		establishments (< 10 employees)	
	hours	log hours	hours	log hours
$Bite * Year_{2014}$	Reference	Reference	Reference	Reference
$Bite * Year_{2018}$	−6.969*	−0.002	−13.202***	−0.144***
	(3.030)	(0.049)	(2.776)	(0.041)
Average $Bite$	0.118	0.118	0.273	0.273
Clusters	400	400	400	400
Observations	1,173,689	1,173,689	277,791	277,791

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (1). The dependent variable is the (monthly) contractual working hours in levels or in logarithms, as indicated by column titles. $Bite$ is the weighted share of all workers in establishments with at least 10 regular employees (first two columns), respectively in establishments with less than 10 regular employees (last two columns) earning below the €8.50 minimum wage in 2014 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* SES, 2014 and 2018, weighted analysis sample.

worked (in levels and logs) of small establishments in columns three and four, the treatment effect becomes economically and statistically significant. It is twice as large as for the hours in levels, and it also becomes substantially negative for log working hours. Since the average bite in small establishments is 0.273, the percentage hours reduction amounts to 3.9 % ($=-0.144 * 0.273$), which is a meaningful effect size.[20]

These findings suggest that the working hours adjustments in the course of the minimum wage introduction are heterogeneous with respect to establishment size. As documented in Appendices F and C, this finding is empirically robust for all robustness checks discussed at the end of Section 4. It is also economically very intuitive since the bite of the minimum wage is larger at small establishments (German Minimum Wage Commission, 2016). In addition, small establishments have less leeway for wage increases compared to medium-sized and large establishments. The latter are ascribed to paying significant wage premiums, possibly due to higher productivity and perhaps also high market power (Oi and Idson, 1999).

To examine the impact of market concentration on the difference in the hours response by establishment size, we leverage market concentration at the two-digit sectoral level and include interaction terms with $Bite$ and $Bite * Year_{2018}$ in the hours regressions (for similar approaches in analyses of employment effects, see Azar et al. (2024) and Popp (2024)).[21] Moreover, rather than running separate regressions for small and medium-to-large establishments, we specify one regression where the small establishment size dummy was interacted with $Bite$, $Year_{2018}$, $Bite * Year_{2018}$, and the $\boldsymbol{X}$-vector. The results are reported in Appendix H1. It turns out that the difference between small and medium-to-large establishments is reduced by 25 % after including the market concentration variables.[22]

The hours estimates translate into an aggregate effect of 22.8 million working hours lost in small establishments due to the minimum wage. While this is less than 1 % of total hours across all establishments, the lost hours volume is equivalent to 234 thousand workers in small establishments.[23]

Finally, note that the share of minijobbers is considerably larger in establishments with fewer than 10 employees and amounted to 40.8 % in 2014 compared to 16.5 % in establishments with at least 10 employees. In the following sections, we examine whether the hours' response after the minimum wage introduction differs between minijobbers and regular employees and also whether the heterogeneous effects across plant size are solely due to a different share of minijobbers.

One concern of a separate analysis of working hours by establishment size might be that the minimum wage led to a change in the establishment size composition, which would imply an endogenous sample split. In Appendix I, we examine whether the minimum wage affects the establishment size composition at the 10-employee threshold. We do not find any indication of such an effect, supporting the causal claim that the hours effect differs by establishment size. We also test whether minijobs moved to establishments with at least 10 employees in high-bite regions. However, between 2014 and 2018 we do not find any statistical difference (p-value of 0.529).

5.2. Hours adjustments along the unconditional hours distribution

To examine further heterogeneities in the impact of the minimum wage policy on hours worked, we estimate effects along the unconditional distribution of working hours. Although the differences are not statistically significant, from Table 4 it is already apparent that the hours effect slightly differs when looking at working hours in levels compared with working hours in logarithms. This finding suggests a closer look at effects along the (unconditional) hours distribution. For this purpose, we estimate RIF regressions of the difference-in-differences specification at each percentile. Given the different working hours adjustments of small and medium-to-large establishments presented in Section 5.1, we again stratify by establishment size.[24]

Fig. 3 visualizes the treatment effect at each percentile of the respective sample. For the medium-sized and large establishments in panel (a), the results show negative treatment effects in the upper half of the hours distribution, indicating a negative hours effect among full-time workers. However, the effects are small in size, and moreover, some of these estimates come with a significant placebo interaction for the year 2010 (see

establishments. Hence, it can still be interpreted as a heterogeneous effect by establishment size.

[20] Note that the bite variable is adjusted to the sample under investigation. In the first two columns of Table 5, the bite is defined as the weighted share of all workers in establishments with at least 10 employees earning below the €8.50 minimum wage in 2014, while in the remaining two columns, the bite is the respective share in establishments with fewer than 10 employees.

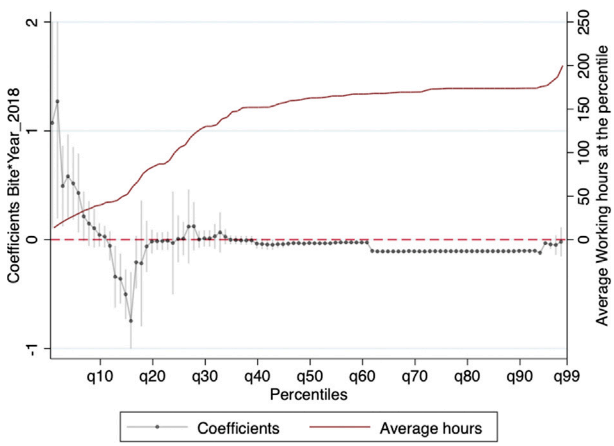
[21] The market concentration index variable is calculated by the sum of squared revenue share of all firms in a sector and is obtained from the German Monopoly Commission (see Heidorn and Weche (2021) for a description). Note that due to the inclusion of time-industry fixed effects, the level of the market concentration, as well as an interaction variable between market concentration and year, could not be included because of perfect multicollinearity.

[22] According to Table H1, the bite in 2018 has no effect on log hours for establishments with at least ten employees, while the effect for small establishments is -0.224. The latter coefficient is larger than the coefficient of -0.144 reported in Table H1 for small establishments when using separate samples. However, these results are consistent since different bite variables are used. In Table H1, the bite is based on all establishments and has a mean of 0.147, while for the small establishment regressions reported in Table 5 the bite is based on small establishments and has a mean of 0.273.

[23] The lost total hours volume is obtained as: $-0.144\times$ *bite in small establishments* × *average contractual hours in small establishments (in 2014)* × *total employment in small establishments (in 2014)*. Plugging in yields: $-0.144 * 0.273 * 97.4 * 5{,}959{,}086 = 22{,}817{,}274 \text{hours per month}$. Dividing the lost total hours volume by *average contractual working hours in all establishments (in 2014)* × *total employment in all establishments (in 2014)*, yields the share of total hours lost: $22{,}817{,}274/(127.5 * 31{,}195{,}656) = 0.006$. Dividing the lost total hours volume by the average contractual hours in small establishments yields the worker-equivalent that is lost in small establishments: $22{,}817{,}274/97.4 = 234{,}264$.

[24] While for small establishments we are restricted to the years 2014 and 2018, for medium-sized and large establishments we also utilize the year 2010, which allows us to test the parallel-trends assumption. Therefore, Appendix J reports the regression coefficients on the interaction terms of bite and year, including the placebo effects, for the sample of medium-sized and large establishments.

(a) Establishments ($\geq$ 10 employees)



(b) Establishments (< 10 employees)

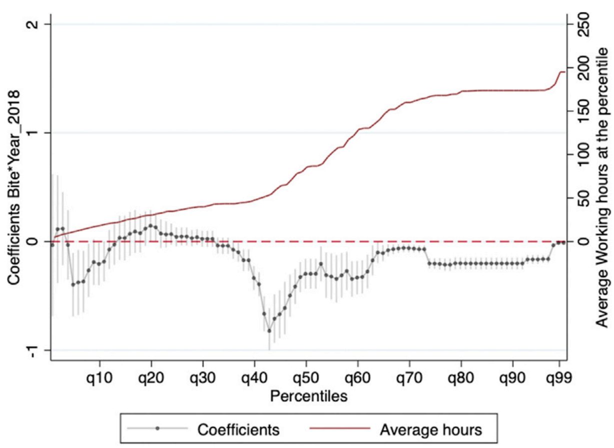


Fig. 3. Minimum wage effects on log monthly working hours along the hours distribution, RIF regression estimates. *Notes:* Black dots represent estimates of the treatment effect (the coefficient estimate of $Bite * Year_{2018}$) from difference-in-differences, as specified in Eq (1), where $Bite$ is the county-level weighted share of workers in establishments with at least 10 regular employees (panel (a)) respectively in establishments with less than 10 regular employees (panel (b)) earning below the €8.50 minimum wage in 2014. The average bite is 0.118 in panel (a) and 0.273 in panel (b). The dependent variable is the RIF of log (monthly) contractual working hours, defined for the respective percentiles on the x-axis. The coefficient estimate of the first percentile of panel (a) (3.22 with s.e. 1.54) is not displayed. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. 95 % confidence intervals are based on standard errors clustered at the county level. The upward-sloping line represents the monthly contractual working hours at the respective percentile. *Data:* SES, 2010, 2014, and 2018, weighted analysis sample. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Appendix Table J1). This casts some doubt that the coefficient estimates of the bite interacted with the year 2018 are genuine causal effects of the minimum wage on hours worked. Instead, they may reflect spurious trends in working hours.

However, the hours effect is significantly positive and monotonically decreases at the very bottom of the distribution between the 1st and 7th percentiles. By contrast, we observe a negative hours effect between the 15th and 17th percentiles. For these effects, the causal claim is much stronger since the respective placebo effects remain statistically insignificant and are smaller in size relative to the treatment effect after the minimum wage introduction.

The negative hours effect between the 14th and the 17th percentile occurs where employees are working between 45 and 60 hours per month. Given a nominal minimum wage of €8.84 per hour, the interval comprises the minijob threshold. Minijobs are a particularly interesting group of low-income jobs, as they are defined by a maximum gross monthly salary of €450. Hence, in the presence of the €8.84 minimum wage, minijobbers can no longer work more than 50.9 h (€450/ €8.84 per hour). Upgrading the respective minijob to a regular job, which has to be paid above €450 can only be avoided by reducing its working hours. According to the regression results, this seems to be the case for a significant number of minijobs. Hence, there is a very intuitive economic explanation for the negative hours effect at this point of the hours distribution which is to preserve the benefits of minijobs.[25]

Regarding the positive hours effect at the very bottom of the hours distribution, where 96 % of jobs are minijobs, we can only speculate about the mechanism underlying this observation. First, it may be less attractive to hire workers for only very few hours if they have to be paid the minimum wage, causing these jobs to move up the hours distribution.[26] Second, an increase in hours of other minijobs could compensate for the decreasing working hours between the 14th and the 17th percentiles, i.e., hours reallocation among minijobs may explain this effect. Third, jobs at the very bottom of the hours distribution may have been terminated. Therefore, the positive hours effect may have been caused by job destruction rather than increasing working hours within the same jobs.

Given that the analysis for medium-sized and large establishments reveals a negative hours effect of the minimum wage introduction at the minijob threshold, one might speculate that the difference in the hours response across plant size documented in Section 5.1 is due to a different share of minijobbers. If a different share of minijobbers solely drives differences across plant size, then the RIF regressions for small establishments should again only show negative effects at those percentiles where the minijob threshold is located. The results are displayed in Panel (b) of Fig. 3.

While there are no more significant positive effects at the bottom of the distribution, we still observe large negative effects further up the distribution. However, they do not occur between the 15th and the 17th percentile (as in Panel (a) of Fig. 3) but beyond the 40th percentile. This is perfectly plausible since 50.9 hours (the minijob threshold at the €8.84 minimum wage) are for small establishments very close to the 42nd percentile. Moreover, Panel (b) of Fig. 3 displays a significantly negative hours effect up to the 62nd percentile, where working time amounts to 130 h. While the share of minijobbers in small establishments is 61 % at the 42nd percentile, it has already fallen to 2.5 % at the 49th percentile. This suggests that the difference in the hours response of the minimum wage effect across plant size is *not* solely driven by a different share of minijobbers. We rather observe a negative hours effect among regular (part-time) employees in small establishments.

5.3. Hours adjustment of minijobs

The analysis along the unconditional hours distribution reported in Section 5.2 already suggests that working hours of minijobs were

[25] Note that minijobs are highly controversial in the public debate (see, for example, *Frankfurter Allgemeine Zeitung*, 22.11.2021, *Der Makel der Minijobs*). While no social security contributions are due from the employee side, minijobs offer little protection from the social security insurances, with little coverage by the unemployment insurance and no mandatory savings in the retirement pension plans. Hence, minijobs are criticized as being precarious. They are defined by a low monthly salary and also experience other disadvantages such as low tenure, low social recognition, and little scope to climb up the job ladder. For a discussion of the pros and cons of minijobs, see also Walwei (2019).

[26] If a new hire has to be paid the minimum wage, the hiring cost may increase because the employers intensify the screening to ensure that the respective hire fulfills the required productivity. To compensate for the increased hiring costs, the firms may wish to extend the respective minijobbers' working hours.

Table 6
Minimum wage effects on monthly working hours of exact minijobs (monthly wage between €350 and €450), establishments (≥ 10 employees).

	hours	log hours
$Bite * Year_{2010}$	−2.467	−0.063
	(1.633)	(0.034)
$Bite * Year_{2014}$	Reference	Reference
$Bite * Year_{2018}$	−15.092***	−0.268***
	(1.286)	(0.028)
Average *Bite*	0.445	0.445
Clusters	400	400
Observations	112,802	112,802

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (1). The dependent variable is the (monthly) contractual working hours in levels or in logarithms, as indicated by column titles. *Bite* is the weighted share of minijobbers (in establishments with at least 10 regular employees) earning below the €8.50 minimum wage in 2014 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* SES, 2010, 2014, and 2018, only minijobs with a monthly wage between €350 and €450, weighted analysis sample, establishments with at least 10 regular employees.

affected by the minimum wage introduction. However, the reported results refer to all employees, including minijobs as well as regular social security jobs. Hence, the significant negative effects displayed in Figs. 3, for example, could also result from changes in working hours of (well-paid) part-time jobs. To check more directly whether minijobs at the threshold of €450 were affected by the working hours reduction discussed in Section 5.2, we run our baseline difference-in-differences specification on this particular group of minijobs, i.e., we restrict the sample to jobs with monthly wages between 350 and 450€.

Table 6 shows that for minijobbers with a monthly wage between 350 and 450€ there is a significant reduction in monthly contractual working hours, both in levels and in logarithms. An increase in the average bite by 44.5 percentage points (the average bite for minijobbers) reduces contractual working time by 6.7 hours per month, which is a substantial relative effect size given that the average for minijobs is 36.3 h per month (see Appendix A). While the estimates document a significant effect, it should be noted that they could include a bias due to selection into and out of this specific group of jobs.

However, despite potential biases, the finding of reduced working hours for minijobbers is also corroborated by a descriptive inspection of the hours distribution of minijobbers and part-time workers. These are illustrated in Fig. 4 for 2014 and 2018. For minijobbers, it is evident that jobs above 55 working hours disappeared entirely. The change in the hours distribution implies that about 600,000 minijobbers with hours beyond 50 per month are no longer observed in 2018.[27] Of these, 200,000 jobs may have been downgraded to 45–49 h. Consequently, the other 400,000 jobs must have been either terminated or upgraded to regular (unsubsidized) jobs. The latter possibility, however, is not supported by the working hours distribution of part-time jobs which does not show a significant increase in the respective bins, i.e., between 50 and 70 h there is no significant increase in regular part-time jobs. Of course, the descriptive distributions can only provide suggestive evidence rather than a decisive answer to the question of whether a significant number of minijobs have been terminated.

[27] For ease of quantitative comparison with the individual-level analysis of minijobber transitions in Section 6, Fig. 4 is based on all establishments. However, we obtain a very similar figure when restricting the sample to establishments with at least 10 employees (see Appendix Figure K1).

6. Results on employment effects of minijobs

We inspect employment effects by looking at the aggregate and then stratifying into the subgroups of regular jobs and minijobs to further examine the possibility of job destruction among minijobs. In addition, we investigate individual transitions of minijobbers to inspect whether aggregate effects are due to compositional changes in and out of employment or due to individual transitions between labor market statuses. Hence, the analysis of labor market transitions helps us answer the question of where all the minijobs went, i.e., whether they were promoted to regular jobs or whether they entered non-employment.

6.1. Aggregate employment effects

We start by analyzing the employment effects of the minimum wage in a difference-in-differences regression at the regional level, using the administrative employment data described in Section 2. As in the individual-level wage and hours regressions, we use identifying variation of the minimum wage bite across 400 counties r in Germany. To inspect the full bite-specific development over the years of analysis, including the crucial pre-treatment trend, we estimate a conditional difference-in-differences specification that includes interactions of the bite with all years separately, only leaving out 2014:

$$y_{rt} = \alpha + \delta_0 * Bite_r + \sum_{k\neq 2014} \delta_k * Bite_r * Year_{k,t} + \sum_{k\neq 2014} \gamma_k * Year_{k,t} + \boldsymbol{X}_{rt}\boldsymbol{\beta} + \epsilon_{rt} \quad t = 2010, \dots, 2018 \tag{2}$$

Fig. 5 displays the estimates for all three employment variables (total employment, regular jobs, and minijobs) and shows a declining bite-specific pre-treatment trend in the raw unconditional estimation (grey estimates). These declining trends violate the parallel trends assumption. Consequently, most of the literature on the German minimum wage introduction relies on estimating employment effects from a pre-trend adjusted difference-in-differences (Ahlfeldt et al., 2018; Bossler and Gerner, 2020; Bossler and Schank, 2023; Dustmann et al., 2022). That is, the literature uses a two-step procedure, which first subtracts an extrapolated bite-specific trend from the outcome variable that is identified from pre-treatment data. Then, it estimates a difference-in-differences specification on the trend-adjusted outcome variable.

While we apply this two-step approach in Appendix L, for our baseline estimation we use a conditional difference-in-differences approach, which includes region and time-varying covariates $\boldsymbol{X}_{rt}$ to control for the bite-specific pre-trend. Thereby, we can explain the trend-divergence from underlying economic factors without relying on the extrapolation of a linear trend and without relying on estimates that essentially capture divergences from the extrapolated trend. It turns out that the county-level population (as a proxy for supply-side forces) is a good predictor of the pre-trend, and after its inclusion, the parallel trends assumption is plausibly justified (dark estimates of Fig. 5).

Fig. 5 also illustrates the employment effects for the years following the minimum wage introduction in 2015. The corresponding (average) coefficient estimates are reported in Table 7, which aggregates the effect over all post-treatment years and compares it with the pre-treatment period. The effect on total employment reported in the first column is insignificant. This insignificant effect, which is also small in magnitude, aligns with other minimum wage evaluations at the regional level (Ahlfeldt et al., 2018; Bossler and Schank, 2023; Dustmann et al., 2022). The second column reports the effect on the subgroup of regular jobs, which appears to be unaffected by the minimum wage, both in terms of statistical and economic significance. In contrast, the third column points to significant and relatively large employment effects for the subgroup of minijobs, which aligns with the graphical illustrations in panel (c) of Fig. 5 and is also consistent with previous results presented in the literature (Caliendo et al., 2018, 2025). As documented in Appendices F and G, the negative effect on the number of minijobs holds

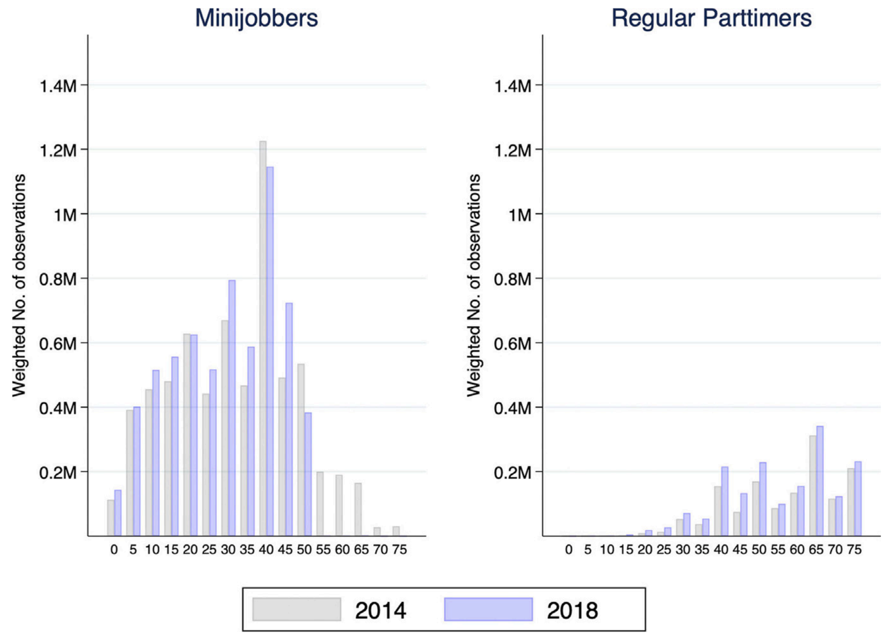


Fig. 4. Histograms of total monthly working hours, 5-hour bins. *Notes:* Histogram of monthly working hours of minijobs and part-time jobs, bin width is 5 h. *Data:* SES, 2014 and 2018, weighted analysis sample, all establishments.

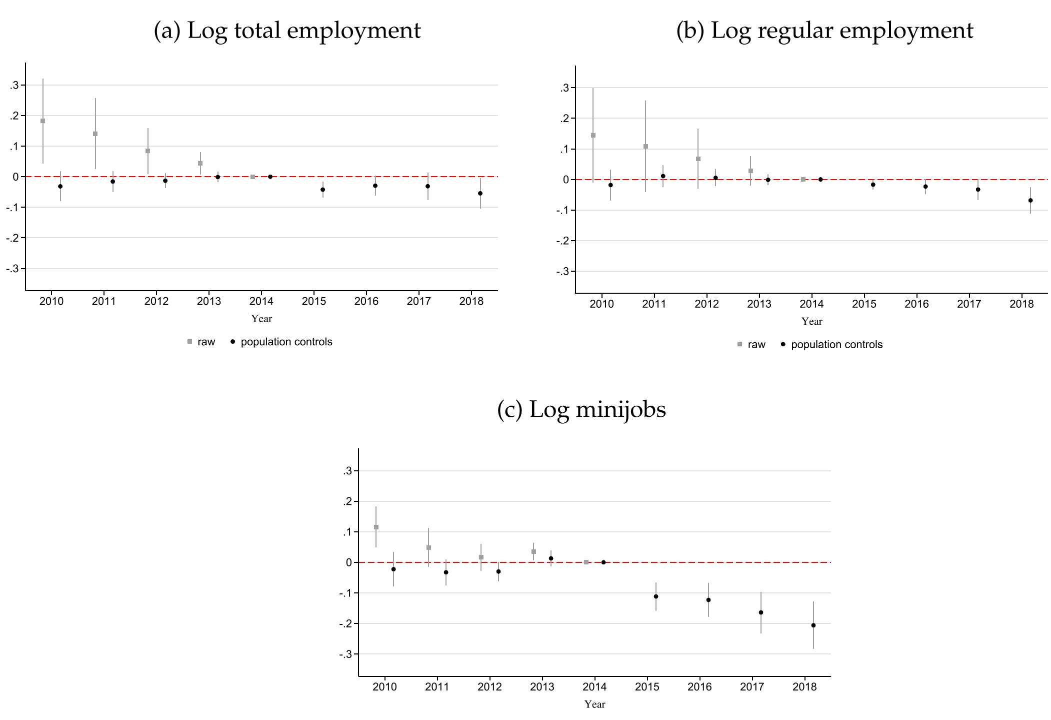


Fig. 5. Inspection of employment trends. *Notes:* Grey squares and black dots represent estimates of $Bite * Year_k$ as specified in Eq (2). The regressions are run at the county level and weighted by county-level population. The dependent variable is the logarithm of employment of either total employment, regular employment, or minijobs, as indicated by sub-titles. $Bite$ is calculated from the SES data and represents the weighted share of all workers, regular workers, respectively minijobbers earning below the €8.50 minimum wage in 2014 at the county level. The average bites are reported in Table 7. The regressions presented by the black dots also control for the county-level population size variables, namely log total population in panel (a), log female population in panels (b), and log total population as well as log young (age 15–24) and old (60–74) population in panel (c). All coefficient estimates of these regressions are reported in Appendix M. Inference is based on clustered standard errors at the county level. Spikes display 95 % confidence intervals. *Data:* BHP, 2010–2018; full population aggregated to counties. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Table 7
Minimum wage effects on employment.

	Log total employment	Log regular employment	Log minijobs
$Bite * Year_{pre}$	Reference	Reference	Reference
$Bite * Year_{post}$	−0.027	−0.035	−0.137***
	(0.022)	(0.020)	(0.028)
Average *Bite*	0.163	0.083	0.453
Clusters	400	400	400
Observations	3600	3600	3600

Notes: Treatment effect interactions from difference-in-differences. The regressions are run at the county level and weighted by county-level population. The dependent variable is the logarithm of employment of either total employment, regular employment, or minijobs, as indicated by column titles. *Bite* is calculated from the SES data and represents the weighted share of all workers, regular workers, or minijobbers earning below the €8.50 minimum wage in 2014 at the county level. The regressions also include year dummies and the county-level population size as in Fig. 5. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* BHP, 2010–2018; full population aggregated to counties.

for all robustness checks discussed at the end of Section 4. Moreover, the employment effects are also qualitatively similar when restricting the sample to medium-sized and large establishments (see Appendix M).

6.2. Transitions out of minijobs

To address the negative effect on the number of minijobs, we further investigate what happened to the respective individuals who exited the minijob. Some studies suggest that minijobs may have been upgraded to relatively better-paid regular jobs (Garloff, 2019) or, alternatively, that individuals affected by the minimum wage were even reallocated to better-paying establishments (Dustmann et al., 2022). Other studies, however, do not find evidence for such an upgrading of jobs (Caliendo et al., 2018). To contribute to the debate on the potential upgrading of jobs, we investigate individual-level labor market transitions of the minijobbers. Since the SES does not allow for following individuals over time, we rely on the German Administrative Employment Data, i.e., the IEB described in Section 2.

We estimate the following difference-in-differences specification:

$$T(status_t \rightarrow status_{t+1})_{it} = \psi_0 * Bite_r + \sum_{k=2013,2014} \psi_k * Bite_r * Year_{k,t} + \boldsymbol{X}_{it}\beta + \mu_{Year\times Industry} + v_{rt} \quad t = 2012, 2013, 2014 \tag{3}$$

The dependent variable $T(status_t \rightarrow status_{t+1})_{it}$ indicates whether or not individual i has transitioned between two exclusively defined labor market statuses between periods t and $t+1$. If T_{it} denotes transitions out of minijobs, it is defined for all minijobbers in period t and takes the value 0 if an individual remains in a minijob in $t+1$, while it takes the value 1 if an individual is no longer in a minijob in $t+1$.[28] In the latter case, the individual may have entered non-employment, a regular social security job, or in rare cases may have entered one of the excluded labor market statuses.[29] Note that the coefficient ψ_{2014} of the interaction term $Bite_r * Year_{2014,t}$ denotes the treatment effect, since observations of T_{it}

[28] In our transition analysis, we look at yearly changes because tenure in minijobs is typically low. Hence, the transition between 2014 and 2015 should be relevant to capture the treatment effect. However, in a robustness check, presented in Appendix O.4, we also look at two-year changes. For the pre-treatment period, we restrict our sample to the years 2012–2014 due to a major break in the definitions of the administrative data.

[29] The status non-employment captures individuals who are registered as unemployed or individuals who are non-employed but who have not registered as unemployed at the local employment agency either because they do not fulfill the eligibility criteria to receive benefits or because they do not want to register as unemployed for other reasons. In fact, the literature documents far from complete take-up (Bruckmeier et al., 2021) of benefits, which is why we prefer to analyze transitions in non-employment rather than transitions in registered unemployment. However, in rare cases, transitions to non-employment could also imply transitions into retirement or to an education program.

Table 8
Minimum wage effects on forward-looking transitions out of minijobs.

	Transitions between years t and $t+1$		
	out of minijob	minijob to regular job	minijob to non-employment
$Bite * Year_{2012}$	Reference	Reference	Reference
$Bite * Year_{2013}$	−0.007*	−0.002	−0.005
	(0.003)	(0.002)	(0.003)
$Bite * Year_{2014}$	0.068***	0.037***	0.032***
	(0.009)	(0.003)	(0.009)
Average *Bite*	0.432	0.432	0.432
Clusters	400	400	400
Observations	13,678,421	13,678,421	13,678,421
Minijobs in 2014	4,542,156	4,542,156	4,542,156

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (3). The dependent variable captures forward-looking transitions out of minijobs between years t and $t+1$, as indicated by column titles. *Bite* is calculated from SES data of all establishments and represents the weighted share of minijobbers earning below the €8.50 minimum wage in 2014 at the county level. The regressions include year × two-digit industry fixed effects and the county-level population size as in Fig. 5. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* IEB, 2012–2015, population of all minijobbers, multiple jobs excluded.

in 2014 capture transitions between 2014 and 2015 and therefore the date of the minimum wage introduction (1 January 2015). Standard errors are clustered at the county level because the bite again varies by county.

The regression results are presented in Table 8.[30] According to the first column, the treatment effect presents a positive and significant increase in the probability of leaving a minijob.[31] In absolute size, the coefficient closely matches the coefficient of the last column of Table 7. Also, note that the placebo coefficient for the year before the treatment is close to zero. Given the average regional bite of 0.432 (weighted by the sample used in Table 8), the treatment effect of 0.068 implies that almost 3 % of minijobs entered another labor market status between 2014 and 2015, corresponding to 133,500 minijobs that disappeared. This number of destroyed minijobs falls short of the descriptively calculated 400,000 minijobs that disappeared from the hours distributions, as illustrated by Fig. 4.[32] There are several possible explanations for why the descriptive decline in the number of minijobs (with large hours) is much larger than the causal effect on minijobs. First, the minijobs (with long hours) may have vanished as part of the regular minijob dynamics, i.e., minijobbers are exiting their jobs irrespective of the minimum wage

[30] For our transition analyses, we utilize the full population of all minijobs, allowing us to infer absolute numbers of transitions into regular employment and non-employment. However, the estimates remain fully robust when analyzing the subsample of establishments with at least 10 employees, as reported in Appendix O.1.

[31] Appendix P shows that the minimum wage effects on transitions out of minijobs are not offset by excess transitions into minijobs.

[32] The discrepancy is even larger since we checked whether the 133,500 causal transitions out of minijobs are entirely driven by those who worked long hours (i.e., ≥ 55hours, which is—by visual inspection—the group that disappeared from the histogram in Fig. 4). In fact, we also observe significant transitions from minijobs to non-employment even among minijobbers who worked fewer hours, suggesting that there is also a genuine labor demand effect (see Appendix O.2).

introduction. Second, a reallocation within the group of minijobs could lead to fewer minijobs with large hours.

In the last two columns of Table 8, we disentangle the transitions out of minijobs into transitions to regular jobs (second column) and into transitions to non-employment (third column). It shows that about half of the employees who moved out of minijobs entered regular jobs, i.e., given the bite (for minijobbers) of 0.432 %, 1.6 % of all minijobbers got promoted. Hence, these employees experienced a promotion to jobs subject to social security. Note, however, that the promotions from minijobs to regular jobs are partially offset by evidence of increased demotions from regular jobs to minijobs, as presented in Appendix Table P1. Most interestingly, as indicated in the third column, the treatment coefficient for transitions from minijobs to non-employment is 0.032. Given the average bite of 0.432 in the sample of minijobbers, about 1.4 % of all minijobbers entered non-employment as a result of the minimum wage introduction. In absolute terms, it implies that 62,800 minijobs were destroyed.[33] To convert this number into an economic parameter, we calculate an employment elasticity with respect to wages by dividing the minimum wage effect on log employment by the minimum wage effect on log wages. Both parts of the ratio can be inferred from our regression results, multiplied by the average bite used in the respective regressions. The numerator is the disemployment effect of minijobs of 0.032 times their average bite of 0.432, and the denominator can be obtained from Panel B of Table 3, where the estimates at percentiles 4 to 16 correspond to the wage effect of minijobs. These average to 0.716 which has to be multiplied by 0.118, i.e., with the average bite of all workers used in the RIF regressions. Together, these figures result in an employment elasticity of -0.16. The corresponding elasticity for regular jobs is zero, given the zero employment effect reported in Table 7.

6.3. Heterogeneities in the transitions out of minijobs

For those employees who were upgraded from minijobs to regular jobs (second column of Table 8), the question arises whether they were promoted internally (within the same employer) or externally (by switching employers).[34] At first sight, it may seem intuitive that minijobbers are promoted internally, simply because of a monthly wage increase due to the minimum wage which causes the respective employees to exceed the minijob threshold. However, external promotions of minijobbers are certainly possible because minijobs are typically characterized by low job tenure and frequent job changes.[35] Due to turnover, it could be possible that some minijobs are terminated and the respective employees may find a regular job at another employer, especially since the level of unsatisfied labor demand, as measured by vacancies was growing in the years after the minimum wage introduction (Bossler and Popp, 2024).

The possibility of external upgrading of jobs is also closely linked to the literature. Dustmann et al. (2022) show that employees affected by the German minimum wage introduction were reallocated from lower-paying employers to higher-paying employers. It is interesting to see whether this upgrading of jobs through reallocation corresponds with the observed promotions of employees from minijobs to regular jobs.

Based on the reasoning above, we decompose upgrading to a regular job into the outcome variables (1) promotion to regular jobs within the establishment and (2) promotion to regular jobs across establishments. The results of these regressions are reported in Table 9. Interestingly,

Table 9
Minimum wage effects on minijobber promotions within and across establishments.

	Transitions from minijob to regular job between years t and $t+1$	
	within establishments	across establishments
$Bite * Year_{2012}$	Reference	Reference
$Bite * Year_{2013}$	-0.0004	-0.001
	(0.001)	(0.003)
$Bite * Year_{2014}$	0.034***	0.003
	(0.003)	(0.002)
Average $Bite$	0.432	0.432
Clusters	400	400
Observations	13,678,421	13,678,421
Minijobs in 2014	4,542,156	4,542,156

Notes: Treatment effect interactions from difference-in-differences, as specified in Eq (3). The dependent variable captures forward-looking transitions out of minijobs between years t and $t+1$, as indicated by column titles. $Bite$ is calculated from SES data of all establishments and represents the weighted share of minijobbers earning below the €8.50 minimum wage in 2014. The regressions include year × two-digit industry fixed effects and the county-level population size as in Fig. 5. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* IEB, 2012–2015, population of all minijobbers, multiple jobs excluded.

all promotions to regular jobs due to the minimum wage introduction are observed within establishments.[36] Hence, the upgrading of minijobs is unrelated to the reallocation effect detected in Dustmann et al. (2022). However, transitions across establishments may involve delays (e.g., relocation). Hence, previous minijobbers could officially be in non-employment for some time before starting at a new employer. Therefore, we additionally examined the transitions from minijobs in t to regular jobs in $t+2$. Even over this prolonged period of two years, there are still no transitions to regular employment across establishments due to the minimum wage (see Appendix Table O7). The positive effect on internal promotions of minijobs is in line with results in Bossler and Schank (2023), which show a significant positive wage effect for existing jobs. Note, however, that this monthly wage effect is much smaller (although still meaningful and statistically significant) at the minijob threshold, as implied by the estimate for the 12th percentile reported in Table 3.

Finally, we assess whether upgrading to a regular job, respectively, whether the transitions of minijobbers to non-employment after the minimum wage introduction depend on sociodemographic characteristics and job tasks. Therefore, we run regressions where $Bite$, $Year_k$, and $Bite * Year_k$ are fully interacted with employee and job characteristics. The results in Fig. 6 illustrate predictions for the transitions of the respective minijobbers (at the mean of all other characteristics). This subgroup analysis helps us characterize the 62,800 minijobbers who lost their jobs due to the minimum wage.[37] One may have expected gender to be a relevant factor, given that the minimum wage disproportionately affected the wages of females to increase (Caliendo and Wittbrodt, 2022). Interestingly, we do not observe a significant gender difference for transitions to non-employment, but females are more likely to receive promotions to regular jobs, which is in line with the

[33] These results remain fully robust when we add covariates to the regression specification, as presented in Appendix O.3. If anything, the treatment effect on transitions out of minijobs increases slightly.

[34] The Administrative Employment Data contain a unique identifier for establishments, but no information on the employing enterprise. Hence, internal promotions to regular jobs are defined within establishments.

[35] Using the IEB data between 2012 and 2015, we observe that 48 % of the minijobbers left their jobs each year, which is defined as either leaving the respective establishment or leaving the minijob within the establishment.

[36] Plausibly, these promotions to regular jobs are mostly observed among minijobbers who worked long hours initially (see Appendix O.2). This is in line with the institutional need to promote these individuals since long minijob hours can no longer exist after the minimum wage was introduced.

[37] Appendix Q presents further results on the effect heterogeneities: Figure Q1 shows the bite of the subgroups of analysis, Figure Q2 replicates the analysis from separate regressions instead of a single fully interacted specification, and Figure Q3 presents heterogeneities by establishment size, the establishments' AKM wage effect, and industry.

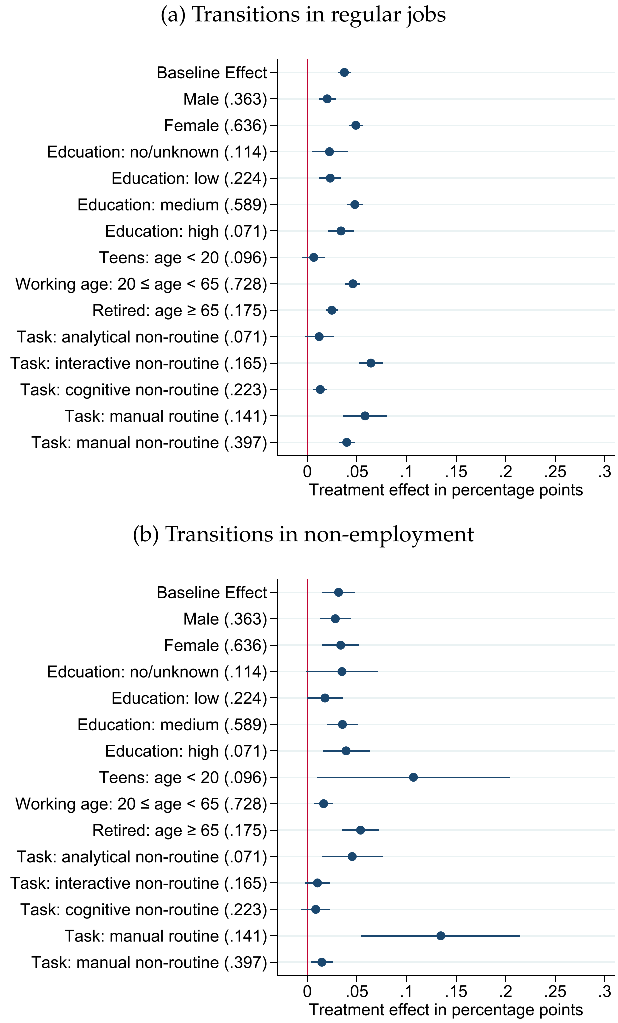


Fig. 6. Effect heterogeneities of transitions out of minijobs. *Notes:* The heterogeneities are estimated from a single specification where the variables $Bite$, $Year_k$, and $Bite * Year_k$ are fully interacted with the respective worker and job characteristics. The illustrated effects are predicted effects for the respective group at the averages of all other groups. The relative size of the groups (as of all minijobbers) is reported in parentheses. The average region-level bite is 0.432 for the full sample of minijobbers, and by subgroups, we provide the average bite in Appendix Q. No/unknown education denotes no secondary or unknown education; low education denotes neither post-secondary education nor vocational training; medium education denotes vocational training; high education denotes master craftsman/technician or university degree. The regressions include year × two-digit industry fixed effects and the county-level population size as in Fig. 5. Inference based on clustered standard errors at the county level. Spikes display 95 % confidence intervals. *Data:* IEB, 2012–2015, population of all minijobbers, multiple jobs excluded.

disproportionate female effect documented in the literature (Caliendo and Wittbrodt, 2022).

Concerning education, the results indicate no significant heterogeneity with respect to transitions in non-employment due to the minimum wage. At the same time, upgrades to regular jobs are more likely among the medium-skilled workers. When looking at the age of minijobbers, the results show that workers in the main working age are most likely promoted to regular jobs. By contrast, teenagers and workers above retirement age, i.e., the groups that typically use the minijob as supplementary income, face a much higher risk of transitioning to non-employment. However, the coefficient for teenagers is imprecisely estimated. The working-age population of minijobbers, who most likely have to rely on the minijob as their main income source, has good chances of receiving a promotion due to the minimum wage. The age-specific non-employment transitions closely match the descriptive findings of Vom Berge and Weber (2017), who show that the termination of minijobs was relatively more likely among young and old workers.

According to the task-based job categorization, manual routine minijobs are substantially more likely than other types to transition into non-employment. This finding is in line with the assumption that manual routine jobs face the highest risk of being replaced (potentially by other input factors).

Another interesting heterogeneity of the minijobs transitioning into non-employment, of which the results are presented in Appendix Q.4, is a differentiation between Eastern and Western Germany. It shows that Eastern Germany almost entirely drives the effect, as the effect is insignificant in the West. However, promotions to regular jobs are observed in both Eastern and Western Germany.

7. Effects of the 2022 minimum wage hike

Up until recently, the German minimum wage remained largely constant in real terms. While the minimum wage has been increased according to the suggested steps by the Minimum Wage Commission in 2017, 2019, 2020, and 2021, these increases were largely adjustments to the development of the price level. In the aftermath of the general election of 2021, however, the new government overruled the Minimum Wage Commission and implemented a real increase from €9.82 to €10.45 in July 2022 and to €12 in October 2022, creating a new quasi-experimental minimum wage variation, which we analyze in the following. So far, only one other study by Bossler et al. (2024) analyzes the 2022 increase using monthly data from 2022 to estimate very short-run effects on incumbent employees.

For our analyses, we make use of the ES as described in Section 2. Descriptive statistics of the ES data (presented in Appendix Table R1) show that the average fraction of workers paid below €12 is 0.171 in 2022 and shrinks to 0.044 in 2023. We cannot combine the ES with the SES, mainly because of the changes in the design of the data, but also due to structural changes in the number of minijobs during the Covid pandemic, and the intermediate nominal minimum wage increases. Hence, it is not possible to construct a consistent pre-treatment panel. However, we can compare April 2023 with the pre-treatment information of April 2022. Since the ES does not allow us to run a pre-treatment placebo test, we have to interpret the results cautiously. However, the striking similarity of effect size in hourly wage effects compared with the 2015 minimum wage introduction and the absence of effects higher up in the wage and hours distribution make us confident that we capture causal treatment effects.

Table 10 presents effects of the minimum wage hike on the averages of log hourly wages, log monthly wages, and log contractual hours. The effect in the hourly wage regression is 0.23. Since the treated workers, on average, are less severely affected by the minimum wage increase (with an average wage gap of 0.137) than they were by the minimum wage introduction (with an average wage gap of 0.256, see Table 1), it is plausible that this effect falls short of the 0.426 log-points effect of the minimum wage introduction (see Table 2). When comparing the effect size of the bite gap (instead of the bite), as shown in Appendix Tables F2 and F10, the effect sizes perfectly align. Hence, two separate minimum wage increases exert the same hourly wage elasticity. The effect on working hours is −0.09, implying that the monthly wage response falls short of the hourly wage effect. The negative hours effect relative to the hourly wage effect corresponds to a wage elasticity of the employment volume of −0.38. Hence, the working hours reduction is stronger than for the minimum wage introduction. Further results in Table 10 show that the negative hours effect is not restricted to small establishments. Fig. 7 shows that the effect is larger at lower percentiles, while the effect is virtually zero at the top of the hours distribution, where there are barely any minimum wage workers. Although not statistically

Table 10
Effects of the 2022 minimum wage increase on wages and monthly working hours.

	Wage effects		Working hours effects		
	Log hourly wage	Log monthly wage	Log hours	Log hours, establishments (<10 employees)	Log hours, establishments (≥10 employees)
$Bite * Year_{2022}$	Reference	Reference	Reference	Reference	Reference
$Bite * Year_{2023}$	0.228***	0.132*	−0.087*	−0.147	−0.143***
	(0.042)	(0.062)	(0.043)	(0.083)	(0.040)
Average *Bite*	0.171	0.171	0.171	0.349	0.137
Clusters	400	400	400	400	400
Obs.	16,769,582	16,769,582	16,769,582	244,196	16,525,386

Notes: Treatment effect interactions from difference-in-differences. The dependent variable is either the log hourly wage, the log monthly wage, or the log contractual working hours. *Bite* is the weighted share of all workers in the data earning below the €12 minimum wage in 2022 at the county level. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the logs of the county-level population sizes of all persons and of children aged 3–5 years and their respective squares. All regressions are weighted. Standard errors in parentheses are clustered at the county level. Asterisks indicate significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. *Data:* ES, 2022 and 2023, weighted analysis sample, all establishments.

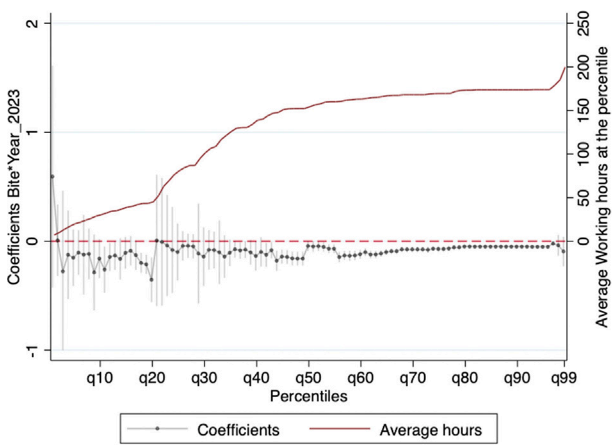


Fig. 7. Effect of the 2022 minimum wage increase on along the hours distribution, RIF regression estimates. *Notes:* Black dots represent estimates of the treatment effect (the coefficient estimate of $Bite * Year_{2023}$) from difference-in-differences, where *Bite* is the weighted share of all workers in the data earning below the €12 minimum wage in 2022 at the county level. The average *Bite* is 0.171. The dependent variable is the RIF of the log hourly wage (left panel), respectively of the log (monthly) contractual working hours, defined for the respective percentiles on the x-axis. Year × two-digit industry fixed effects are included. The $\boldsymbol{X}$-vector includes age, dummy variables for gender and education, and the county-level population size of all persons and of children aged 3–5 years and their respective squares. All regressions are weighted. Inference based on clustered standard errors at the county level. Spikes display 95 % confidence intervals. The upward-sloping line represents the hourly wage, respectively the monthly contractual working hours, at the respective percentile. *Data:* ES, 2022 and 2023, weighted analysis sample, all establishments. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

significant and not as marked as for the minimum wage introduction, the hours effect is slightly larger at the minijob threshold (at the 20th percentile).[38]

Turning to the extensive margin of employment, we again analyze the effects of the minimum wage on levels of total employment, regular employment, and minijobs. Using BHP data, Fig. 8 illustrates whether the development in the respective regional employment figures between 2018 and 2023 is associated with the regional bite. The grey squares represent raw coefficient estimates on interactions between the bite variable and each year between 2018 and 2023. Again, they are unlikely to justify the parallel trends assumption. When adding controls for county-level population, the pre-trends again disappear, as illustrated by the black dots, supporting the parallel trends assumption for the post-treatment period. Although the raw estimates point to a negative development of regular employees after the minimum wage introduction, the estimates including the population controls no longer show negative employment effects for regular employment or minijobs.

In contrast to the minimum wage introduction, the minimum wage hike barely exerted an effect heterogeneity on the group of minijobbers, which is plausible because the minijob threshold has been adjusted. Still, for the first (smaller) step from €9.82 to €10.45 the minijob threshold remained fixed at €450, such that maximum hours needed to be adjusted from 45.8 hours to 43.1 hours per month if the minijobs were to be preserved. However, the minijob threshold was harmonized with the second (larger) step of the minimum wage, i.e., it increased from €450 to €520 which is the same percentage increase as the minimum wage rise from €10.45 to €12.[39] Hence, following the second and larger minimum wage hike there is no longer a need to cut hours of minijobs (or even terminate them) when they require a higher hourly wage by means of the minimum wage. Instead, the results suggest that there is a severe hours effect across both types of jobs, minijobs and regular jobs. Hence, it is rather a genuine labor demand effect driving the hours reduction than a reduction that is linked to the institutional setting of minijobs. However, such hours adjustments may occur rather in the short run, as hours are more easily adjusted than heads; hence, it remains open whether the effects continue to persist in the longer run.

Nevertheless, our results suggest that the genuine reduction in employment volume was stronger in 2022/23 than in 2015. This increased disemployment effect likely reflects the adverse economic conditions at the time. Business conditions deteriorated in 2022, as indicated by the ifo Business Climate Index, which fell about 15 % below its level at the time of the minimum wage introduction (see Appendix S). The finding that minimum wages exert stronger disemployment effects during economic downturns contributes to a growing literature documenting this

[38] In Appendix R3, we show that the working hours effects along the hours distribution, as presented in Fig. 7, are not significantly different across establishment sizes.

[39] The descriptive Appendix Figure R2 indicates a shift in the distribution of minijobs from above 45 hours to just below. This is consistent with the required reduction of maximum working hours of exact minijobs after the first minimum wage rise.

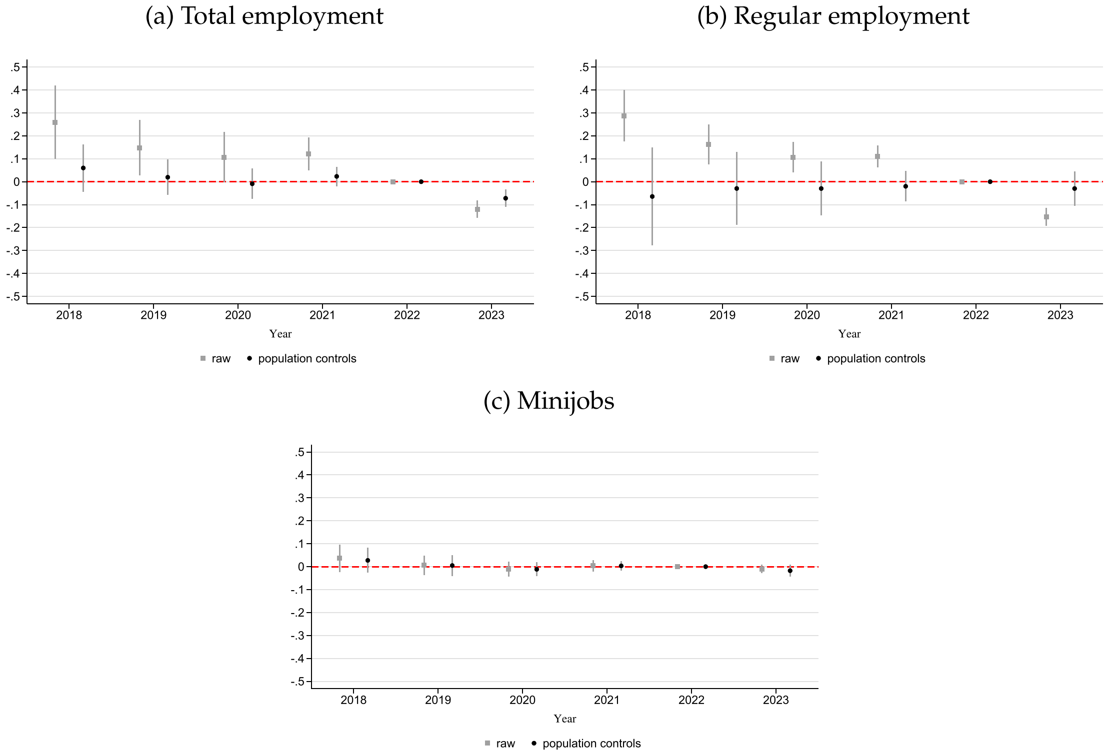


Fig. 8. Inspection of employment trends before and after the 2022 minimum wage increase. *Notes:* Grey squares and black dots represent $Bite * Year$ interactions from difference-in-differences. The regressions are run at the county level and weighted by county-level population. The dependent variable is the logarithm of employment of either total employment, regular employment, or minijobs, as indicated by column titles. $Bite$ is calculated from the SES data and represents the weighted share of all workers, regular workers, respectively, minijobbers earning below the €12 minimum wage in 2022 at the county level. The average $Bite$ is 0.186 in panel (a), 0.094 in panel (b), and 0.593 in panel (c). The regressions presented by the dots also control for the county-level population size variables, namely log total population, log kindergarten population (age 3–5), and in panel (b) log teen population (age 15–19). Inference based on clustered standard errors at the county level. Spikes display 95 % confidence intervals. *Data:* BHP, 2018–2023; full population aggregated to counties. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

heterogeneity (e.g., Addison et al. (2013), Link (2024), and Caliendo et al. (2025)).

8. Conclusion

Using identifying variation from a regional bite variable in a difference-in-differences analysis, we document compelling evidence that the 2015 introduction of the minimum wage in Germany induced significant wage increases in hourly and monthly wages. Thereby, the minimum wage contributed to a significant reduction in both hourly and monthly wage inequality. The wage effects are most pronounced at the bottom of the respective distributions but extend up to the median, implying significant spillover effects beyond the workers directly affected by the policy. Furthermore, the minimum wage effect on monthly (but not hourly) wages follows a hump-shaped pattern along the distribution, with a local minimum at the 12th percentile. This is where the minijob threshold of €450 is located, demonstrating that this institutional threshold suppresses the effect of the minimum wage. All jobs below €450 are minijobs, which are exempt from income taxation and only require reduced social security contributions. There are supply and demand-side incentives to keep jobs below this threshold, which clearly hampers the effect of the minimum wage.

On average, contractual and paid working hours are hardly affected by the minimum wage introduction, corroborating the findings by Biewen et al. (2022). However, we document two important heterogeneities in the hours response. First, we show a negative hours effect among small establishments, which are disproportionately affected by the minimum wage. Second, we observe a negative hours effect for the severely affected minijobs.

We observe clear indications that minijobs with more than 50 hours were eliminated. This observation is very intuitive in the presence of an hourly minimum wage and the definition of minijobs (by a monthly wage up to €450). Hence, the number of working hours for minijobs was effectively limited by the minimum wage legislation. We investigate the hitherto unresolved question, namely, whether the respective minijobs were terminated or whether they were upgraded to regular social security jobs. While Caliendo et al. (2018, 2025) demonstrate a decreased number of minijobs, we causally examine individual-level transitions of minijobbers using social security data. Our results show that about half of the terminated minijobs were upgraded to regular social security jobs, while half of the individuals entered non-employment. This corresponds to 62,800 minijobbers entering non-employment, implying an employment elasticity with respect to wages of −0.16 for minijobs but a zero elasticity for regular jobs.

Our results clearly suggest that the institutional setting leads to heterogeneous effects of the minimum wage. Effects on working hours and transitions into non-employment may not be detected when analyzing the aggregate workforce. However, significant non-negligible effects are uncovered when looking at the particular subgroup of minijobs.

In 2022, the institutional context changed, allowing for monthly wage increases within minijobs. Correspondingly, when we estimate effects of the 2022 minimum wage hike, we no longer obtain any negative employment effects, neither for regular jobs nor for minijobs. However, we do observe a negative effect on average working hours among all employees. Accordingly, the estimates indicate a zero employment elasticity with respect to wages at the extensive margin, but they indicate a negative elasticity of −0.38 at the intensive margin of hours adjustments.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Online Appendix

Online Appendix for this article can be found online at doi:10.1016/j.jpubeco.2025.105540.

Data availability

Upon application; access through research data centers of the Statistical Office (regarding SES and ES) and the Institute of Employment Research (regarding BHP and IEB); we make all do-files available upon request.

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