The short- and medium-term distributional effects of the German minimum wage reform

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Abstract

This study quantifies the distributional effects of the minimum wage introduced in Germany in 2015. Using detailed Socio-Economic Panel survey data, we assess changes in the hourly wages, working hours, and monthly wages of employees who were entitled to be paid the minimum wage. We employ a difference-in-differences analysis, exploiting regional variation in the "bite" of the minimum wage. At the bottom of the hourly wage distribution, we document wage growth of 9% in the short term and 21% in the medium term. At the same time, we find a reduction in working hours, such that the increase in hourly wages does not lead to a subortionate increase in monthly wages. We conclude that working hours adjustments play an important role in the distributional effects of minimum wages.

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1 Introduction

As social policy instrument, minimum wages aim to reduce wage inequality. By increasing earnings at the lower end of the wage distribution, minimum wages have been shown to achieve this goal in the United States (DiNardo et al. 1996; Lee 1999; Teulings 2003; Neumark et al. 2004; Autor et al. 2016) and the United Kingdom (Dickens and Manning 2004a, b; Dolton et al. 2012). However, most of the corresponding literature refers to incremental changes in federal minimum wage levels.[1] Against this backdrop, the present article examines a major labor market reform in Germany that introduced a high statutory national minimum wage in 2015. The reform directly affected 10–15% of the employed population, corresponding to between 4 and 6 million working people (Amlinger et al. 2016; Brenke 2014; Falck et al. 2013; Kalina and Weinkopf 2014; Lesch et al. 2014). In the following, we provide an in-depth analysis of the reform's distributional effects at the individual level in the short and medium term: We test how much hourly wages increased, who benefited from the increase in hourly wages, and what happened to working hours and monthly wages.

Our analysis is based on data from the German Socio-Economic Panel (SOEP). It allows us, on the one hand, to identify eligible workers within a representative longitudinal sample. On the other hand, it includes detailed information about employees' earnings and working hours, which is its major advantage compared to alternative datasets for Germany. In administrative data, for example, working hours are not available throughout the period of interest (Bossler and Schank 2020) so they have to be approximated—for instance, by keeping working hours fixed across time (Dustmann et al. 2021) or by imputing individuals' working hours using the average hours of full- and part-time employees (Ahlfeldt et al. 2018). The SOEP, in contrast, allows us to construct and distinguish two hourly wage concepts: an hourly wage based on *contractual* working hours and an hourly wage based on *actual* hours worked. Whereas contractual working hours are set out in black and white in employment contracts, *actual* hours reflect the effective workload of the employed person—that is, contractual hours plus paid and unpaid overtime. Since the national minimum wage applies to overtime as well, the actual hourly wage concept is the policy variable of interest.[2]

Methodologically, our analysis uses a difference-in-differences (DiD) regression framework with continuous treatment (Card 1992b). The DiD framework builds on regional variation in treatment intensity, measured by the share of eligible employees in 96 different regions (*Raumordnungsregionen*, ROR) who were paid below the minimum wage in a pre-reform period. Even though the statutory minimum wage is fixed at a uniform level across all German regions, the wage structure exhibits significant regional differences. Ahlfeldt et al. (2018) and Caliendo et al. (2018) have used this variation to identify wage and employment effects on an aggregate level. We take this approach one step further and apply it to individuals. This stands in contrast to the previous literature that identifies the effect of the policy reform on individual

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[1] Changes in regional minimum wages, for instance in Seattle, Washington (United States) by 16 and 18% in 2011 and 2015, respectively, are notable exceptions (Jardim et al. 2018).

[2] Unfortunately, introduction of a more precise question on paid and unpaid time in the SOEP in 2015 was accompanied by a structural break in these variables which limits the analysis of unpaid overtime. In a previous version of this paper (see IZA DP No 11246), we discuss these issues in more detail.

wages, working hours, or earnings by exploiting the individuals' pre-reform wage levels as a quasi-natural experiment (Burauel et al. 2020a, b; Dustmann et al. 2021). Here, employees with hourly wages below (above) the minimum wage level serve as the treatment group (control group). However, potential spillover effects (Dickens and Manning 2004b) put the stable unit treatment values assumption (SUTVA) in this type of identification at risk. By using the regional variation in ROR, which are separated by economic structure and commuter flows (BBSR 2016), we are able to incorporate spillover effects without affecting our identification strategy. This allows us to analyze the reform's effects on the whole wage distribution.

The results of our analysis can be summarized as follows. The descriptive evidence indicates that, in the post-reform period (2015–2018), hourly wages in the bottom wage segment increased at an above-average rate. However, this increase did not translate into higher monthly wages. Our DiD analysis confirms these results and documents a negative effect on working hours that prevented the minimum wage from affecting monthly wages. We further compare our individual-level results to regional-level results (Ahlfeldt et al. 2018) and conclude that the positive effect on monthly wages in the regional-level estimation does not stem from the direct effect on monthly wages of the exposed individuals, but rather from intensified upward wage mobility in the exposed regions. To test the validity of our findings with respect to misreporting of monthly wages and working hours, we compare the wage distribution in the SOEP with the Structure of Earnings Survey (SES), a repeated cross-sectional survey of employers about their workers' wages. Here, we find that they are very close.

Our findings complement the existing literature in three ways. First, using regional variation in wage levels, we apply a robust method to identify the distributional effects of the minimum wage reform on hourly wages. Previous studies using employees' pre-reform wage levels as a source of variation have been compelled to make strong assumptions with respect to potential spillover-effects (Burauel et al. 2020a, b; Dustmann et al. 2021). Our method is not constrained in this respect. We confirm the previous results and show that spillover effects of the minimum wage are modest. Second, by looking at monthly wages, hourly wages and working hours, we shed more light on the findings of Ahlfeldt et al. (2018), Caliendo et al. (2018), and Bossler and Schank (2020), who look at isolated variables only. We are thus able to paint a complete picture of the key policy variables by analyzing wages, earnings, and working hours within a unified framework. Third, we extend previous findings on the distributional effects with respect to the time horizon. Looking at the effects from 2015 to 2018, we show that the findings of Burauel et al. (2020a, b) are not morning-after effects only: Changes in working hours dampen the positive effect of the minimum wage reform on monthly wages in both 2017 and 2018.

The documented working hours reductions are important for social policy: They imply that the hourly wage increase do not translate to a full extent in higher gross monthly wages. Due to income taxes, social security contributions and cuts in transfer payments, the effect on households' disposable income is even smaller (Backhaus and Müller 2019; Dube 2019). Furthermore, the reduction in working hours might be the reason, why the minimum wage did not affect employment in Germany as predicted (e.g., Knabe et al. 2014).

The paper proceeds as follows. Section 2 gives an overview of the institutional details and reviews the literature on distributional effects of minimum wages. Section 3 introduces our data source and provides descriptive evidence. Section 4 introduces the identification strategy, discusses its validity, and provides results for hourly wages, monthly wages, and hours worked. Section 5 provides various sensitivity tests, and Section 6 concludes.

2 Minimum wage reform

2.1 Institutional background

On January 1, 2015, a general statutory minimum wage of €8.50 per hour became effective in Germany. It is codified in the Minimum Wage Law (*Mindestlohngesetz*, MiLoG). Before its introduction, there were only sector-specific wage floors set by collective agreements. These had been introduced starting in the 1990s in several sectors, including construction and roofing (in 1997), hairdressing (in 2013), and security services (in 2011).[3] In addition, some federal states (*Länder*) introduced minimum wages in the framework of public procurement law (see Sack and Sarter 2018). In the following, we focus on regulations pertaining to the statutory minimum wage.

With the introduction of the Minimum Wage Law, the German Minimum Wage Commission also recommended future adjustments of the minimum wage level. In light of the negligible employment effects in the first year after the reform, the minimum wage was raised by €0.34 per hour effective January 1, 2017. The minimum wage was gradually raised to €9.82 in January 2022 (Mindestlohnkommission 2020a).

As of January 2015, almost all employed people in Germany became eligible for the statutory gross minimum wage of €8.50. Some sectors with pre-existing sector-specific minimum wages were exempt for a transitional period ending in January 2017. Permanent exemptions apply to minors (persons below the age of 18) as well as trainees and interns (e.g., students or apprentices completing required or elective internships of up to three months). Unemployed people who have been registered as such for at least twelve months may be employed below €8.50 for up to six months. However, vom Berge et al. (2016) show that this exemption is rarely used. The exemption for trainees and minors, in contrast, reduces the number of eligible individuals substantially. In 2014, about 5.5 million employed people earned less than €8.50 per hour, 4 million (72 percent) of whom were eligible for the minimum wage (Destatis 2016). Three groups of employed people were over-represented in this figure: East German residents, people in marginal employment, and women. While the first group results from the continuing structural differences between East and West Germany, as evident in different price levels, the third group results from the gender wage gap and the higher proportion of part-time work carried out by women. The second group, people in marginal employment, are not subject to social security contributions. Thus,

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[3] Most sector-specific minimum wages were higher than the statutory minimum wage and were increased further after the minimum wage reform (Amlinger et al. 2016). An overview of sector-specific minimum wages is given in Schröder (2014).

in many cases, their gross income is equal to their net income, meaning that their wages are not entirely comparable with regular wages.

2.2 Literature on distributional effects

Soon after the introduction of the minimum wage in 2015, aggregated data began showing that wages of low-wage groups—such as unskilled workers, women, part-time employees in small firms, and employees in East Germany—were growing at above-average rates (Amlinger et al. 2016; Mindestlohnkommission 2016). However, since multiple factors besides the minimum wage may affect wage growth, the observed changes are not necessarily attributable to the minimum wage reform in a unicausal way. For this reason, several studies implemented causal designs to identify the effect of the German policy reform on wages, earnings and inequality. In the following, we summarize these studies, organized by their type of identification, and give an overview of factors which can impede the wage effects of minimum wages.

2.3 Identification using variation between employees' wage levels

In the absence of variation between sectors,[4] individuals' wage levels can be used for separating a control and treatment group of employees and for implementing a quasi-experimental setting. The wage growth of individuals with wages just above the threshold before the reform is used as control group for those below the threshold, the directly affected. Stewart and Swaffield (2008) as well as Stewart (2012) make use of this design to identify wage and employment effects in the United Kingdom. For the German case, Burauel et al. (2020b) use the SOEP and apply a differential trend-adjusted difference-in-differences model that compares hourly and monthly wage *growth* of individuals below and just above the minimum wage threshold. They identify positive effects on hourly and monthly wages in the first two years after the introduction of the minimum wage. In a follow-up study, Bachmann et al. (2020) use the same method and find positive but smaller wage effects for 2017. However, the model comes with stringent data demands since changes in trends (second differences) are analyzed. Sample attrition is thus a potential threat. Dustmann et al. (2021) also make use of variation by individual wage levels and identify positive wage effects in the first two years after the reform. However, their data—a combination of administrative data and the Employee History (*Beschäftigtenhistorik*) data provided by the Institute for Employment Research—include information on hours worked up to 2014 only. After that point, they make the assumption that employers' working hours are fixed over time. Burauel et al. (2020a) call this assumption partially into question:

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[4] The effects of sector-specific minimum wages have not only been evaluated by using the variation of individuals wage level (e.g., König and Möller 2009) or by regions (e.g., vom Berge and Frings 2019). Since the group of affected employed is precisely defined, several studies have used non-affected but comparable sectors to model the counterfactual (Fitzenberger and Doerr 2016). A study by Frings (2013) evaluates the effect of minimum wages on painters and electricians, using the transport and communication industry and the wholesale and retail sectors as control groups. Aretz et al. (2013) as well as Gregory and Zierahn (2022) analyze the minimum wage in the roofing sector and use not-affected sub-sectors of the building industry as counterfactual. In case of the statutory uniform minimum wage, however, this approach is not applicable.

Using the same identification strategy as Stewart and Swaffield (2008) and using the SOEP data, they find negative effects of the minimum wage reform on working hours in 2015. However, the effects are no longer significant in 2016.

Studies of this kind face the problem of potential spillover effects—in this case, the possibility that employees above the minimum wage are affected by the policy reform. These effects might arise, for instance, if employers want to maintain the wage structure within their firm. If spillover effects occur, the stable unit treatment values assumption (SUTVA) of the DiD approach is at risk. Estimators will be biased and a causal interpretation of the results will not be possible. The literature on spillover effects for the United States and the United Kingdom is inconclusive (Dickens and Manning 2004a; Stewart 2012; Neumark and Wascher 2008). Autor et al. (2016) argues that the nature of spillovers is not fully understood and might be at least partly attributable to errors in measuring wages in survey data. For Germany, evidence is mixed. Bossler and Schank (2020) as well as Dustmann et al. (2021) find evidence for spillover effects, while Burauel et al. (2020b) do not. If these spillover effects did truly occur, the wage effects identified by the literature summarized above are underestimated. Alternative identification strategies will be necessary to identify the causal effect of the minimum wage correctly.

2.3.1 Identification using variation between regions

Regional heterogeneities can arise from legislative differences in minimum wage settings and/or variation in wage structures. Then, variation in the regional "bite" of the minimum wage can be exploited for identification (Card and Krueger 1992; Card 1992b; Dube et al. 2010). Lee (1999) and Dolton et al. (2012) exploit regional heterogeneities to analyze the wage inequality in the United States and the United Kingdom. For Germany, Bossler and Schank (2020) use differences in wage levels across the country to analyze changes in monthly wages. Using a decomposition, they show that the minimum wage was responsible for 40–60% of the observed reduction in earnings inequality between 2015 and 2017. Furthermore, Ahlfeldt et al. (2018) and Caliendo et al. (2018) analyze regional wage growth as part of their analysis. As proposed by Card (1992b), they first examine wage effects before analyzing employment effects. Ahlfeldt et al. (2018) finds positive effects at the tenth percentile of the wage distribution, Caliendo et al. (2018) a reduction in the average fraction of employed people earning less than the minimum wage. However, neither of the latter studies look at wage effects in more detail.

2.3.2 Negative employment effects and non-compliance

When minimum wages are accompanied by job losses, overall wage inequality may decline as the population of interest shrinks at the lower end of the wage distribution. Accordingly, when assessing the effect of minimum wages on inequality, (potential) negative effects on disposable income must be taken into account (Backhaus and Müller 2019; Neumark et al. 2004). While the discussion on employment effects is still ongoing (e.g., Manning 2021; Neumark and Shirley 2021), the existing literature

on the German case does not identify short-run employment losses on the extensive margin (Caliendo et al. 2019; Mindestlohnkommission 2020b).

Another potential factor that can prevent minimum wages from achieving their full intended impact on wages is non-compliance. If companies violate the new regulations, whether unintentionally or intentionally—for instance, through a lack of timekeeping or the replacement of employee positions with false self-employment – hourly wages and, thus, monthly wages will not increase as expected (Brown 1999; Metcalf 2008; Mindestlohnkommission 2020b). Using the SOEP, Burauel et al. (2017) identify 1.8 million employees earning less than the statutory threshold in 2016. In principle, the German customs administration is responsible for enforcing compliance with social security and minimum wage regulations. In case of violations, it can impose fines up to €500,000. However, inspections are relatively rare and generally focus on high-risk sectors such as construction (Mindestlohnkommission 2020b).

3 Data and descriptive evidence

3.1 Socio-economic panel

In the subsequent empirical analysis, we investigate hourly wages, working hours, and monthly wages before and after the introduction of the minimum wage reform. Our analysis relies on data from the German Socio-Economic Panel (SOEP), an ongoing representative longitudinal panel survey of around 30,000 individuals in 15,000 households per year (see Goebel et al. 2019). We use SOEP version 35 and consider individual-level data from 2012 to 2018 (see SOEP v35 2019). In the following, we describe the core variables and the composition of our working sample.

3.2 Core variables for the empirical analysis

The SOEP routinely allows for computation of hourly wages as the ratio of gross monthly wages and weekly working hours adjusted by average weeks per month and by the information on overtime compensation.[5] Hours are stated in the SOEP as *actual* and *contractual* weekly working hours. While the latter are the number of hours determined in the employment contract, actual hours include overtime. Thus, hourly wages can be constructed with either measure of working hours, producing two different wage concepts: actual and contractual hourly wages. Each of these wage concepts has its own advantages. From a legal perspective, minimum wage regulations are binding for any number of working hours, including overtime. Thus, actual wages are the target of minimum wage policy and the focus of interest, but they are also the blind spot in administrative data. Arguably, contractual working hours are less affected by measurement errors. For the sake of brevity, we present evidence based on actual hourly wages. Results based on contractual hourly wages provide a qualitatively similar picture and are available from the authors upon request.

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[5] https://www.diw.de/de/diw_02.c.222729.de/instrumente___feldarbeit.html, last accessed on May 25, 2020.

A key advantage of the SOEP data is that they contain hourly wages, hours worked, and monthly wages in addition to detailed socio-demographic information. Administrative data usually cannot provide the same density of information on a regular basis, especially with respect to working hours. Survey data are, however, prone to imprecision. Item non-response or rounded answers may bias hourly wage computations. In Sect. 5, we discuss potential sources of imprecise measurement. In "Online Appendix C", we provide an external validation of the information on wages and working hours from the SOEP data using the Structure of Earnings Survey (SES) and the Earnings Survey (ES), which are based on companies' payroll accounting records and one of the most widely used sources to quantify the labor earnings distribution of the Federal Statistical Office. The most important comparison for our purposes is between SOEP 2014 and SES 2014, a year in which both surveys were representative. In 2015, the ES was run on a voluntary basis, which resulted in a substantially lower response rates from the participating firms and their potentially high selectivity.[6] In sum, wages and hours from SES 2014 and SOEP 2014 are relatively similar, and we do not see any structural differences that could invalidate our estimates. The second report of the Minimum Wage Commission shares this assessment (paragraph 83 Mindestlohnkommission 2018). At the same time, the distributions of the same variables in 2015–2016 in SOEP and ES differ to a greater extent, which may stem from the selectivity issues of the latter.

3.3 Working sample

In our analysis, we focus on those employees in Germany who are eligible for the minimum wage. Hence, we exclude individuals belonging to the groups and sectors that were initially exempted from the reform.[7] Further, our data contain only employees aged 18 or older for whom we have wage information.[8] In order to prevent outliers in hourly wages from biasing our results, we winsorize the data by setting the top and bottom 1 percent of hourly wages to the value of the first and 99th percentiles, respectively.

Table 1 presents the losses in observations for each imposed restriction and the resulting sample sizes of our working sample. Our empirical design relies on the pre-reform levels of regional exposure to the minimum wage from 2013, which is why our sample contains only respondents for whom we can identify their regional location in 2013. We further restrict our sample to have only valid information on basic socio-demographics (gender, age, German citizenship, presence of children in the household below the age of 16, and marital status). The DiD analysis relies on the wage distributions of Germany's planning regions to infer the region-specific treatment intensity. Germany is subdivided into 96 planning regions. The treatment intensity is

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[6] Given that administrative data from the Federal Employment Office do not feature information on working hours, it is currently not possible to compare their distribution of hourly wages with the SOEP. Moreover, these data are not yet available to the broad research community for the post-2015 period.

[7] See "Online Appendix B" for the construction of the corresponding restrictions.

[8] The main analysis relies on the wage information available directly from the respondents. In a robustness check, we additionally employ imputed values for the missing wages.

Table 1 Observations per year under imposition of sample restrictions

	2013	2014	2015	2016	2017	2018	Total
SOEP v35	30,776	27,238	25,287	23,338	22,032	20,337	149,008
Eligible in exposed sectors	13,853	12,335	11,201	10,666	10,145	9,593	67,793
With wage info	12,825	11,460	10,206	9,847	9,332	8,765	62,435
With bite 2013	12,825	10,869	9,498	8,916	8,247	7,576	57,931
With ind info	12,517	10,610	9,279	8,736	8,086	7,430	56,658
Exclude small regions	12,189	10,336	9,065	8,546	7,909	7,265	55,310
Working sample	12,189	10,336	9,065	8,546	7,909	7,265	55,310

SOEP v35, own calculations

derived from the region-specific wage distributions according to the SOEP. In some planning regions, sample sizes are relatively small, calling the precision of the derived treatment intensity into question. We decided to discard regions with fewer than 30 observations in order to guarantee valid descriptions of the included regional wage distributions while not losing too many regions. This leaves us with 88 regions for the DiD analysis. By limiting the restrictions on the SOEP to a minimum, we aim to preserve the representative character of the data.

After all restrictions, our working sample contains 55,310 observations in 2013–2018. As expected from the imposed restrictions, the annual number of observations falls from year to year, with the attrition rate of up to 15%. By construction, as our working sample conditions on employment, analyses using this sample say nothing about the reform's effects on those who entered or left the labor force between 2014 and 2018. However, the short-term effects of the reform on employment are shown to be minor and, thus, negligible for our analysis (Mindestlohnkommission 2016; Bossler and Gerner 2019; Caliendo et al. 2018). Table A.1 contains average values for the socio-demographic variables in individual survey years, documenting that most of the characteristics remains same over the analyzed time span. Over time, we document a slight increase in the share of women, the average age, and the share of married respondents. In the regression analysis, we explicitly control for these characteristics.

3.4 Wage distribution

First, we compare the dynamics in the hourly wage distributions between 2013 and 2018 based on cross-sectional samples. This means that all eligible employees in a year are considered. Hence, wages of persons entering the labor market in, say, 2014 are not included in 2013, and wages of persons leaving the labor market in 2014 and remaining non-employed thereafter are not included in 2015.

As a graphical description of the change in annual wage distribution, Fig. 1a shows annual densities of hourly wages and illustrates that the low-wage sector remained stable in 2013–2014 and then shifted rightwards starting in 2015, when the minimum wage was introduced. Figure 1b additionally depicts average growth in (year-specific)
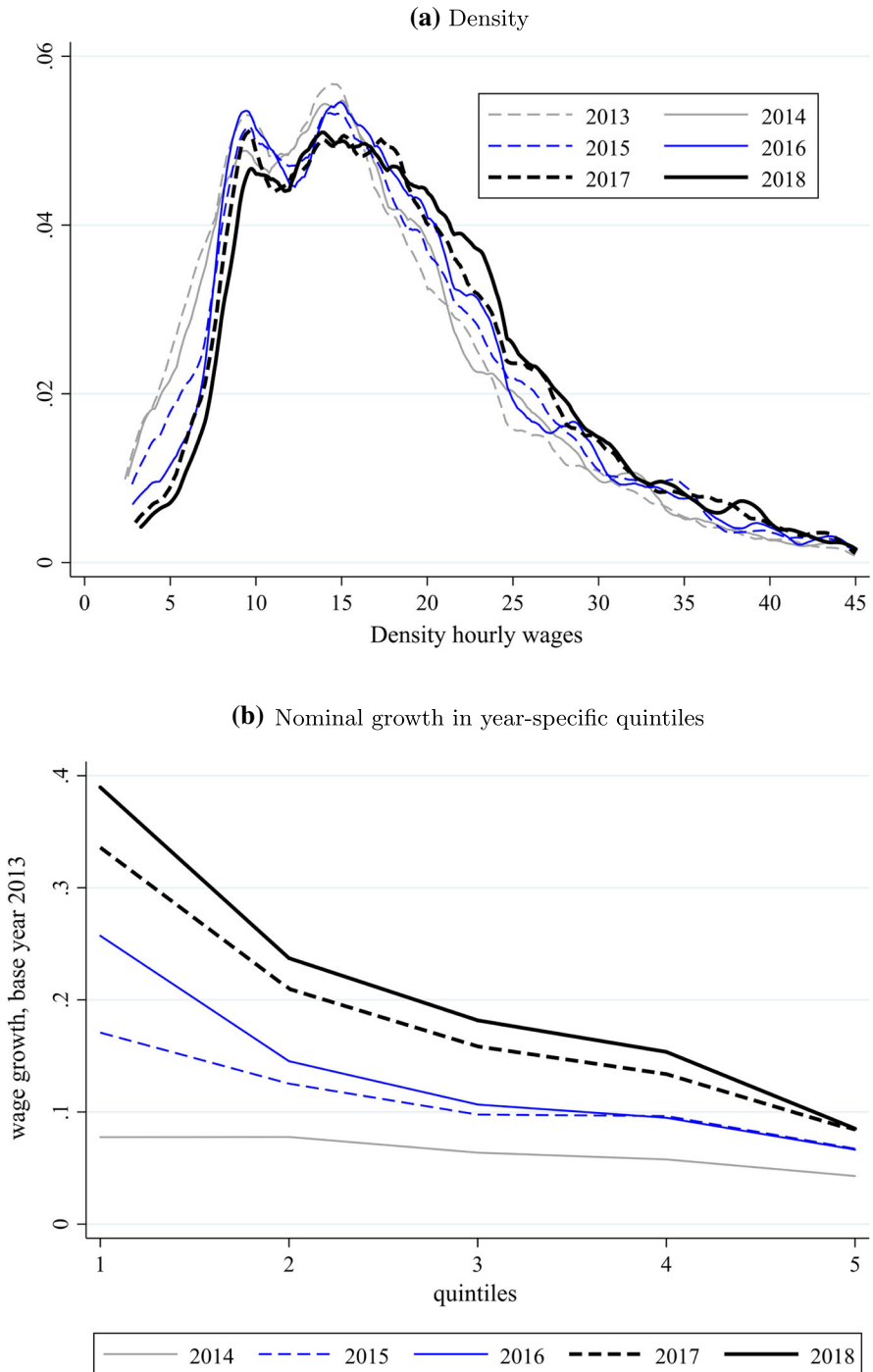
(a) Density

(b) Nominal growth in year-specific quintiles

Fig. 1 Change in hourly wage distribution. Notes: SOEP v35 (Sample 1), authors' calculations.

quintiles of annual distributions of hourly wages relative to the distribution of 2013.[9] The figure documents that, in 2014, all quintiles experienced about the same growth rates. Starting in 2015, the lowest quintile experiences over-proportional growth, which becomes even more pronounced in 2016–2018. A possible explanation is that the SOEP 2015 data were predominantly gathered in the first half of the year and that companies needed a short-term phase to adapt employment contracts to the law.

An important goal of the minimum wage reform is to prevent in-work poverty. However, in-work poverty depends on several factors, such as the transfer system, household income, and individual monthly wages. The introduction of a minimum wage is aimed at hourly wages, but it is also expected to improve individual monthly wages. In order to illustrate this aspect, we depict growth in monthly wages relative to 2013, in the respective annual quintiles (Fig. 2a) and in the quintiles of the hourly wage distribution (Fig. 2b). The growth in hourly wages (Fig. 1b) does not translate into comparable growth in monthly wages. Figure 2b is especially illustrative of the restricted growth of monthly wages in the lowest quintile of the hourly wage distribution, where mini-jobs are concentrated. This may be explained, first, by the spread of minimum wage earners across the distribution of monthly wages depending on their number of working hours, meaning that affected workers are not necessarily located in the lowest segment of the monthly wage distribution. Figure A.1 depicts the high spread of working hours within monthly wages deciles, which is especially pronounced below the median. Second, the reform might induce additional adjustment channels, such as adjustments of working hours, which would also lead to less growth in monthly wages. In order to document the channels at work in detail, we switch from the *aggregate* perspective to the level of *individual* data and to study changes in hourly wages, monthly wages, and hours worked of the affected workers (Clemens and Wither 2019).

4 Difference-in-differences analysis

4.1 General framework

Because the statutory minimum wage in Germany is uniform across all regions and basically all employees, identifying the reform's effect on wages is not straightforward. In the following, we apply the identification strategy suggested by Card (1992b). It relies on regional differences in relative treatment intensity. In Germany, the treatment intensity differs because of sizeable regional heterogeneities of wages. This gives rise to variation in the bite (treatment intensity) of the reform, measured by the regional shares of employees paid below the minimum wage in the years prior to the reform. For the reform to be effective, it should have a larger wage changes in higher-treated regions. However, below average productivity and profitability of the resident firms in highly treated regions may weaken this effect.

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[9] Henceforth, we rely on the division of the distribution into quintiles, as it provides a reasonable trade-off between the number of analyzed quantiles and their size. Figure 1b confirms that such a division is meaningful, as most of the wage growth since the minimum wage introduction happens in the first quintile.

(a) in quintiles of monthly wages

(b) in quintiles of hourly wages
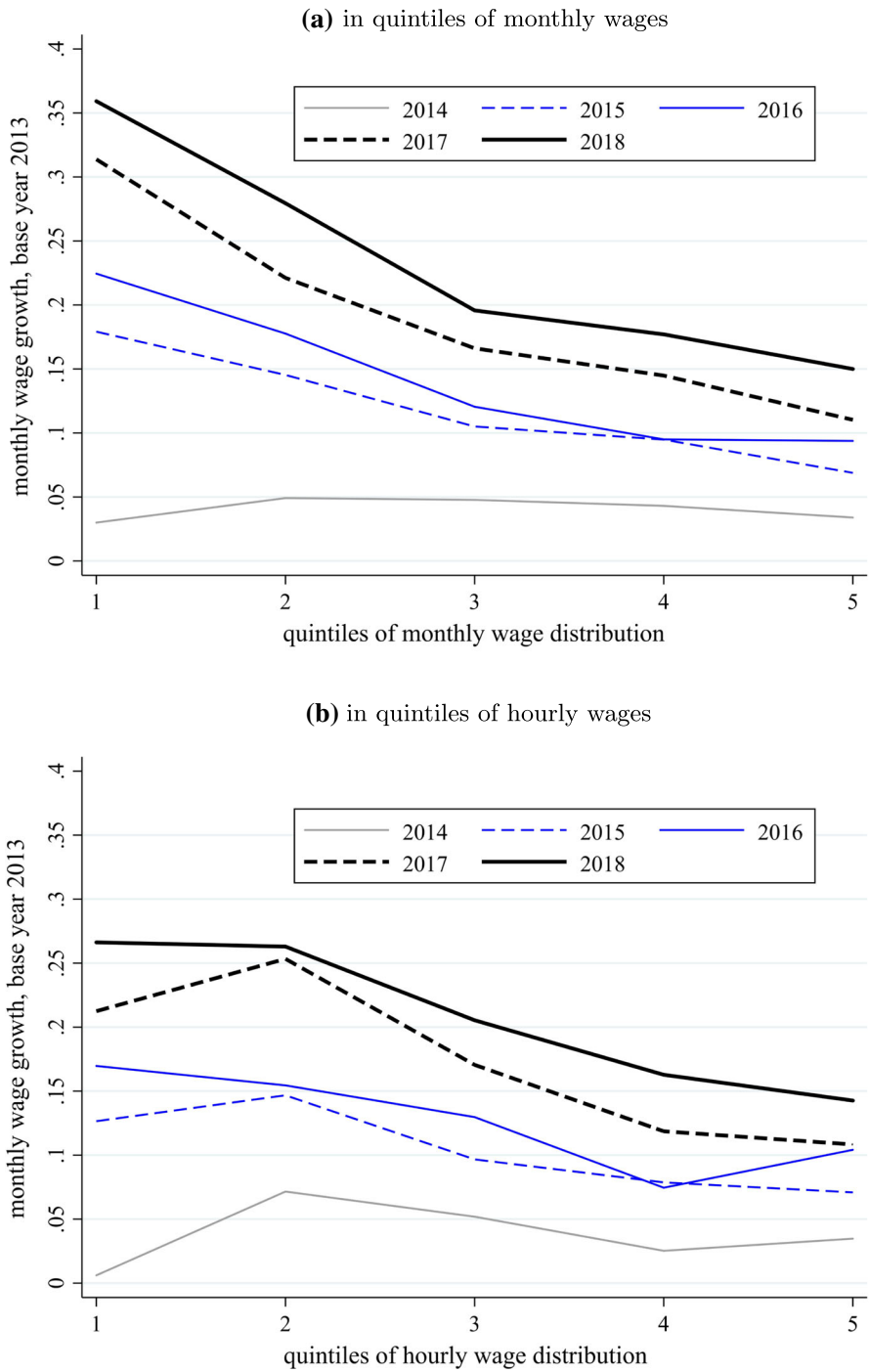
Fig. 2 Nominal growth of monthly wages in year-specific quintiles, relative to 2013. Notes: SOEP v35, authors' calculations

One threat to the identification strategy is the potential existence of influences that correlate with the regional bite and unfold parallel to the introduction of the minimum wage. We are not aware of any other new policies or factors that might drive our estimates.[10] A potential threat to the region-based identification is the spatial dependency of regions, which creates a bias in the regional effects of the minimum wage reform.[11] Another threat is that the regional bite is correlated with regional economic performance. For example, if the reform's bite in economically weak regions is high, these regions should exhibit the highest wage adjustment. Therefore, as mentioned by Dolton et al. (2015), the underlying regression equation should include controls for economic performance, such as, for example, lagged region-specific GDP per capita. Following this rationale, our basic regression equation takes the form,

$$
\log(w_{irt}) = \delta^{2015}\left(P^{2015} \times Bite_r^{2013}\right) + \delta^{2016-2018}\left(P^{2016-2018} \times Bite_r^{2013}\right)
$$
$$
+ \mu \mathbf{X}_{irt} + e_r + \alpha_i + \nu_t + \epsilon_{irt}. \tag{1}
$$

The dependent variable is the log of hourly gross wages of individual i at time $t \in (2014, 2018)$ residing in region r. The treatment effect is captured by an interaction of the bite measure with the post-reform dummies. $Bite_r^{2013}$ captures the treatment intensity measured by the regional fractions of eligible employees with contractual hourly wages below €8.50 normalized by the average regional bite. Because of the possibility of anticipation effects and in order to avoid endogeneity, we use the bite for 2013. We differentiate between the effect in the first post-reform year (short term, 2015) and the subsequent three post-reform years (medium term, 2016–2018). The associated regression coefficients (δ^{2015} and $\delta^{2016-2018}$) capture the treatment effect, that is, the differential changes in wages dependent on the regional treatment intensity in the short and medium term. These coefficients can be interpreted as average *individual* wage growth.

Additionally, the model includes a set of explanatory variables, $\mathbf{X}_{irt}$, encompassing age, marital status, German citizenship, presence of children aged below 16 in the household, as well as two-period lagged regional GDP per capita (inclusion of pre-reform controls for regional economic condition is suggested by Dolton et al. 2015). Finally, e_r represents region-specific fixed effects, α_i includes individual-level fixed effects (both observable and unobservable effects such as motivation, ability, and bargaining power), ν_t captures year-specific dummies, and ϵ_{irt} represents the remaining error term.

We first estimate the average treatment effect at the mean. Additionally, to infer the effects of the reform along different segments of the wage distribution pre-reform, we estimate Eq. (1) separately by quintiles of the regional wage distributions, with individuals being assigned to their position in the (unweighted) regional distribution

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[10] The arrival of large numbers of refugees starting in summer 2015 did not affect the 2015–2016 field phase of the SOEP, and these refugees were not able to start entering the labor market until 2016 due to administrative hurdles. In 2017–2018, refugees might have created additional labor supply for low-wage jobs, which may have caused a downward bias in our estimates.

[11] Dolton et al. (2015) show that controlling for region-specific gross domestic product or gross value added removes the main estimation bias stemming from spatial dependency of regions.

in 2013. The upper panel of Table A.2 in the "Online Appendix A" summarizes the number of observations and mean wages by quintiles. By construction, the number of observations is evenly distributed between the quintiles. Furthermore, we also use the framework described in Eq. (1) to study the treatment effects on log monthly wages and log hours worked as dependent variables.

The regional treatment intensity is defined as the share of eligible employees paid less than the initial minimum wage level (€8.50) prior to the reform in 2013. These shares are derived from the SOEP. As explained above, we assign the employed to 96 "planning regions" (*Raumordnungregionen* (ROR), see BBSR 2016), a concept that is frequently used in the regional analysis of infrastructure, economic situations, and investments (e.g., see Funke and Niebuhr 2005).[12] On average, we rely on about 140 individuals per region in 2013. As seen in Fig. 3, the bite varies considerably between regions. Many regions with high treatment are located in the former East; many regions with low treatment are in the Southwest. Although the number of observations in each region is small in the SOEP, Caliendo et al. (2018) show that SOEP-based bite indicators are highly correlated with bite indicators constructed from the more comprehensive Structure of Earnings Survey (SES).

4.2 Parallel trend assumption

Crucial for our design is that the parallel trend assumption holds for the treatment and control groups. In the following, we provide a visual representation of parallel trends of the mean and percentiles of the unconditional distribution of contractual gross hourly wages. While treatment intensity is a continuous variable, we conduct a graphical inspection by distinguishing regions with "low," "medium," and "high" treatment intensity following Card (1992b). The three types of regions are distinguished by sorting regions by increasing order of bite in 2013 and then splitting the sorted regions into thirds.

Figure 4a provides means of contractual gross hourly wages for the three types of regions for the 2012 to 2018 period. The visual indications for the pre-reform period support the parallel trend assumption within the 95% confidence intervals: while mean wages across "low," "medium," and "high" treatment regions differ by definition, the slopes of the time trends for the three types of regions are similar. The graph also suggests no reform-induced changes in average wage growth. Figure 4b plots the time trends for the three bottom wage quintiles. Before the reform (2012–2014), wages decrease between 2012 and 2013 and start growing steadily after 2013.[13] However, these changes are insignificant within the 95% confidence bands. After the reform, we observe a convergence of wages in the first quintiles of the three groups of regions.

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[12] For our regional assignment of the employed, we use the SOEP variable *region of residence*. Please note that a higher level of disaggregation seems unfeasible due to the reduction of the number of observations within regional units.

[13] The slight reduction in wages observed between 2012 and 2013 stems from the inclusion of a new migration sample in the SOEP in 2013. In the regression design, the year 2012 is involved only for the computation of the bite in the placebo estimation, which is highly correlated with the bite computed for survey years using information on migrants. Starting from 2013, we rely on the same sample composition for the individual-level regression analyses.
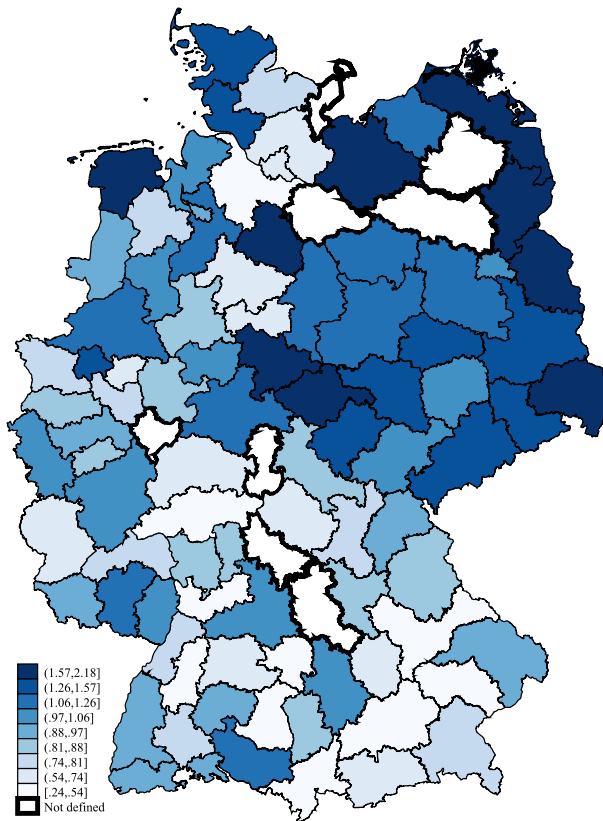
Fig. 3 Regional intensity of treatment, SOEP 2013. Notes: SOEP v35 (Sample 1), authors' calculations

4.3 Effect on hourly wages

Table 2 provides the coefficients of the treatment effect, δ, from the regression Equation (1).[14] As discussed above, we report one coefficient for the first post-reform year (short term, 2015) and one for the three subsequent years (medium term, 2016–2018). The upper panel of Table 2 reports the results of estimations of hourly wages, first on average and then separately for individuals in specific quintiles, that is, Q1 to Q5. All regressions include socio-demographic controls as well as fixed effects at the level of individuals, survey years, and regional units (ROR).

The results show that the estimates of δ at the mean are insignificant in 2015 but positive in the medium term, with average growth in 2016–2018 being about 4%. Focusing on the subgroup at whom the reform was aimed, employees with low wages, we now turn to regressions by quintiles of the regional wage distribution. For the first quintile, we find highly significant and positive treatment effects of about 9% in

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[14] Tables with details on all estimated coefficients are available upon request.

(a) Mean hourly wages

(b) Quintiles 1-3 of hourly wages
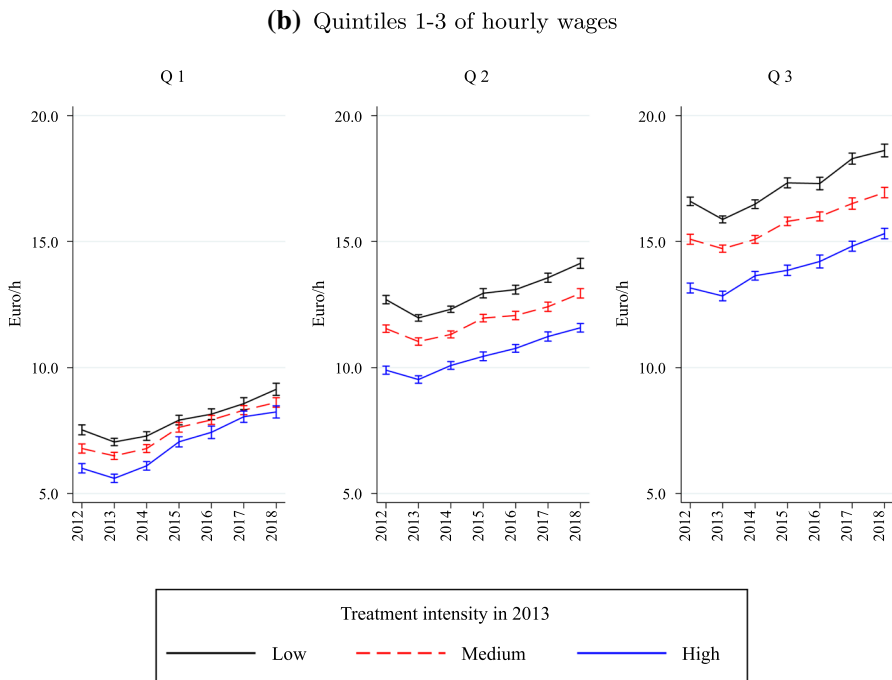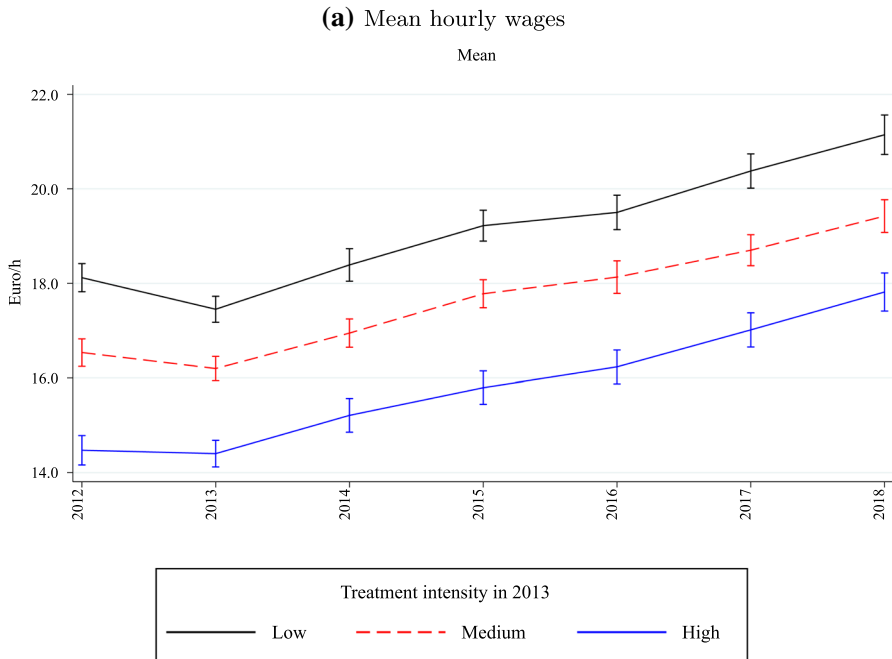
Fig. 4 Evolution of hourly wages by year and regional treatment intensity . Notes: SOEP v35, authors' calculations. Whiskers denote 95% bootstrap confidence intervals (200 repetitions)

2015, which increases to 21 percent in 2016–2018.[15] In 2016–2018, we also document significant wage growth of 4.8% in the second quintile. There are two possible reasons for the wage growth in the second quintile. First, in some regions, the minimum wage reform affected not only the first but also the second wage quintile (see Fig. 3). Second, the minimum wage reform may also induce growth above the directly affected wage segment; this phenomenon is known as "spillover effects."

Taken together, average wages within the bottom quintile have been growing faster in highly affected areas, with an indication of a spillover effect on the second quintile of the regional wage distribution.[16]

4.4 Effect on monthly wages and hours worked

The above DiD framework also allows for estimating the effect of the minimum wage reform on monthly wages and hours worked. In the following, we re-estimate Eq. (1), replacing the left-hand variable with the logarithm of monthly gross wages and log *actual* hours worked. The middle and the lowest panels of Table 2 summarize the DiD results.

For actual hours worked, we find a negative treatment effect for the bottom quintile of the hourly wage distribution of about 6% in 2015 and 10% in 2016–2018. This means that the reform reduced working hours more among low-paid employees in highly treated regions. Taking these results together—an increase in hourly wages and a decrease in working hours—we find a positive but statistically insignificant result for log monthly wages in the lowest wage segments. We conclude that the decrease in working hours plays a substantial role in the observation that the increase in hourly wages does not translate into an equiproportionate increase in monthly wages (Burauel et al. 2020a, b).

Although this wage segment is not directly affected by the minimum wage reform, the effect of the reduction in working hours is also observed in the fourth quintile of the wage distribution in both 2015 and 2016–2018. For monthly wages, we observe a negative effect in the fourth quintile only in 2015. One plausible explanation for the negative treatment effect is that employers reduce hours worked in an attempt to reduce labor costs at the intensive margin. Another possible explanation is that employees lower their own labor supply in response to the reform in order to not exceed earnings limits that secure access to specific social security or tax advantages. The potential underlying mechanism of spillovers of minimum wage regulations on high-skilled workers is described in Gregory and Zierahn (2022).

4.5 Placebo regressions

This section addresses the validity of the common trend assumption for hourly wages, monthly wages, and working hours. The graphical representations in Sect. 4.2 provide

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[15] A coefficient of 0.085 means that, in a region with the average treatment intensity (normalized to be 1.0), wages in the first quintile grew by 8.5%.

[16] We also run the estimation with the treatment intensity defined by the contractual hourly wage and obtained qualitatively similar results. These are available from the authors upon request.

Table 2 Effect on hourly wages, gross monthly wages and working hours, 2015–2018 versus 2014

	Post-Reform				Placebo	
	2015		2016–2018		2014	
	Coef	p-value	Coef	p-value	Coef	p-value
Hourly wages						
Mean	0.012	0.259	0.036***	0.003	0.006	0.643
By quintiles						
Q1	0.085**	0.044	0.206***	0.000	−0.005	0.936
Q2	0.009	0.689	0.048*	0.069	0.017	0.589
Q3	0.003	0.857	0.006	0.774	−0.004	0.837
Q4	−0.021	0.197	0.008	0.627	−0.001	0.967
Q5	−0.002	0.903	−0.006	0.769	−0.009	0.716
Monthly wages						
Mean	−0.008	0.496	0.008	0.582	−0.005	0.703
By quintiles						
Q1	0.015	0.720	0.091	0.116	0.000	0.994
Q2	−0.005	0.862	0.041	0.221	−0.018	0.547
Q3	0.004	0.867	−0.016	0.529	−0.006	0.750
Q4	−0.046***	0.006	−0.024	0.264	−0.006	0.749
Q5	−0.008	0.543	−0.007	0.738	−0.022	0.495
Actual hours						
Mean	−0.013	0.161	−0.023**	0.044	−0.009	0.435
By quintiles						
Q1	−0.056*	0.096	−0.099**	0.032	0.004	0.924
Q2	−0.007	0.767	0.002	0.933	−0.025	0.445
Q3	0.000	0.979	−0.022	0.260	0.006	0.783
Q4	−0.025*	0.053	−0.041**	0.016	−0.007	0.694
Q5	0.004	0.805	0.008	0.715	−0.010	0.681

SOEP v35 (Sample 2), own calculations.
The tables contains results of two regression specifications: First, one DiD estimation for the post-reform period, with separate estimation for the first post-reform year 2015 and the three subsequent years 2016–2018. Second, a placebo estimation for the pre-reform period.
All specifications contain individual, year and regional fixed effects.
All specifications also include controls for age, German citizenship, presence of children aged below 16 in the household, marital status, two-periods lagged regional GDP. Standard errors are robust.
Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

supportive descriptive evidence, but do not reproduce the DiD regression design in detail. Here, we explicitly check the common trend assumption by means of placebo regressions. The minimum wage was introduced on January 1, 2015, and, for this reason, the treatment effects above are derived by comparing the wage distributions from 2014 and 2015–2018 using regional bites from 2013. In the placebo regressions, we estimate a model with lagged variables, that is, with wage distributions from 2013

and 2014 and the regional bite from 2012. If the common trend assumption holds, we would expect to find no treatment effect in this placebo regression. The regression equation takes the form,

$$\log\left(y_{irt}\right) = \delta\left(P^{2014} \times Bite_r^{2012}\right) + \mu\mathbf{X}_{irt} + e_r + \alpha_i + \nu_t + \epsilon_{irt}, \qquad (2)$$

with y_{irt} denoting one of the three aforementioned dependent variables and $t \in$ (2013, 2014).

In sum, the placebo estimations (see the two right columns of Table 2) give evidence supporting the parallel trend assumption, thus lending credibility to our identification strategy. The results suggest that no systematic anticipation of the reform took place. Most importantly, we detect neither growth in hourly wages nor a decrease in working hours that correlates with the identifying variation in treatment exposure prior to the reform.

4.6 Dynamic treatment

In order to illustrate how the reform's effect unfolds during the post-reform period and to justify our results differentiating between 2015 and the subsequent post-reform years, we adjust the Eq. (1) to include year dummies and respective interaction terms for the years 2015–2018.

$$\log(w_{irt}) = \sum_{y=2015}^{2018} \delta^y\left(P^y \times Bite_r^{2013}\right) + \mu\mathbf{X}_{irt} + \alpha_i + \nu_t + e_r + \epsilon_{irt},$$
$$t \in (2014, 2018). \qquad (3)$$

As in the main specification, we conduct separate estimations by quintiles of the regional distribution of hourly wages in 2013. Figure 5 presents the coefficients δ^y, $y \in$ (2015, 2018) for the first and second quintiles, where the main effect of the reform is detected, and for the three dependent variables—log hourly wage, log monthly wages, and log working hours. The figure corroborates the main results for the bottom quintile, the wage segment that is the focus of the reform: In all post-reform years, we observe an increase in hourly wages, a decrease in working hours, and unchanging monthly wages. For the second quintile, the effects on all three dependent variables slowly unfold and are the highest in 2018.

4.7 Unconditional quantile regression

In order to underline the difference between the perspective of individual-level wage development due to the minimum wage reform and the reform's effect on the overall distribution of wages, we adapt the empirical approach of Dube (2019) and conduct additional estimations in the spirit of Eq. (1) by the means of an unconditional quantile regression (Firpo et al. 2009) for 2015–2018. Table 3 documents the reform's effect on the distribution of hourly wages (upper panel) and monthly wages (lower panel) on the
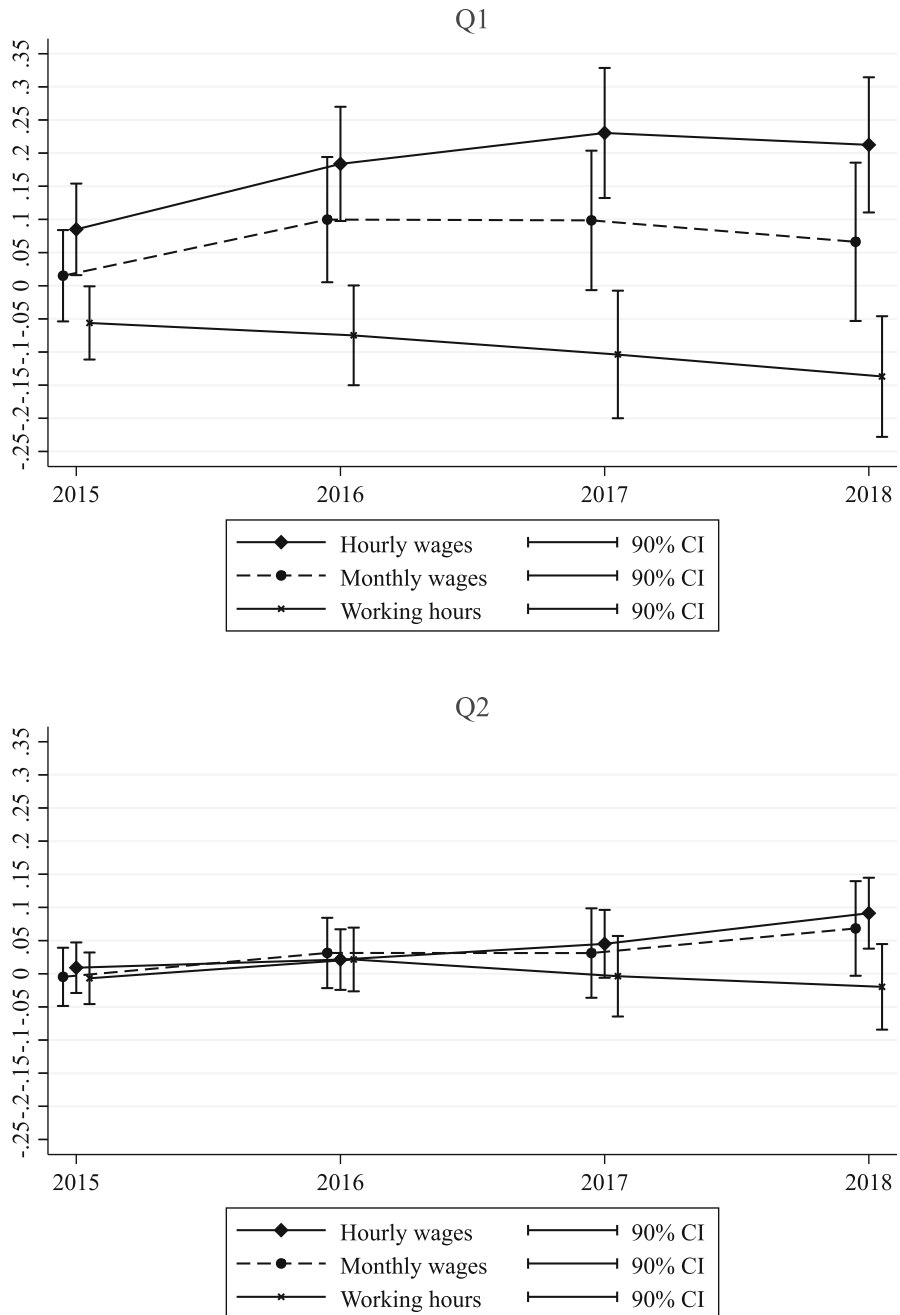
Fig. 5 Dynamic treatment effect. Notes: SOEP v35, authors' calculations

Table 3 Unconditional quantile regression (2014–2018)

	Q10	Q20	Q30	Q40	Q50	Q60	Q70	Q80
Log hourly wages	0.157***	0.079***	0.035	−0.005	−0.014	−0.017	−0.009	0.010
	(0.032)	(0.028)	(0.025)	(0.019)	(0.019)	(0.019)	(0.017)	(0.020)
Log monthly wages	−0.213	0.061	0.048	0.032	0.005	−0.015	−0.015	0.003
	(0.131)	(0.046)	(0.036)	(0.031)	(0.023)	(0.021)	(0.022)	(0.023)

SOEP v35, authors' calculations.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Specifications include controls for regional averages of gender, age, German citizenship, presence of children aged below 16 in the household, marital status, two-period lagged regional GDP, and regional fixed effects

quantiles of the respective distributions. We detect an effect of the reform of 16 percent on the 10th percentile and 8 percent on the 20th percentile of hourly wages, which is generally in line with our individual-level estimates. For monthly wages, we do not find a statistically significant effect. In general, this finding corroborates the previous observation that the minimum wage reform is inducing growth in hourly wages for low-wage earners, but not improving their monthly wages (see Fig. 2). This evidence underlines that the effect of minimum wages on monthly wages is highly dependent on the number of hours that the affected people work (Bossler and Schank 2020). Comparing these results to the individual-level, we can conclude that the positive effect on monthly wages (suggested by Fig. 2a) is driven by minimum-wage workers with longer working hours and who are less likely to experience a reduction of their working time.

5 Robustness checks

Due to its panel character and the wealth of information it contains on socio-demographics and job characteristics, the SOEP offers several advantages over other datasets. However, there may be measurement error in reported monthly wages and working hours. For our estimation design and results, a regionally equiproportionate bias is innocuous. If the error is randomly distributed, variation increases, which solely diminishes the standard deviation of the coefficients. Measurement error is problematic if it is systematic and correlated with the treatment intensity. In order to evaluate whether these and other concerns affect our results, we perform a series of robustness checks.

5.1 Pooled model with and without individual fixed effects

For comparability of our results with other studies, we re-estimate the DiD model without differentiating between short- and medium-term coefficients. The upper panel of Table A.3 provides coefficients on the pooled effect for 2015–2018 with individual-level fixed effects (left) and without them (right). It documents that the FE estimates are higher in the first quintile, which points at the existing negative correlation between

wage growth and time-invariant characteristics of the eligible employees in this wage segment. At the same time, we do not find substantial structural differences between the OLS and the FE estimations.

5.2 Item non-response

Not all respondents provide answers to all SOEP questions. If non-response is systematic, this creates another element of uncertainty.[17] For concerns like this, some missing values are statistically imputed. This is also the case for monthly gross wages but not for working hours. Because imputations are accompanied by additional uncertainty, following Autor et al. (2016), we discard observations with imputed monthly wages in our main results.

In order to check whether inclusion of the imputed values affects the results, we redo the DiD estimation for hourly wages, integrating the previously discarded imputed wages. The second panel of Table A.3 summarizes the results. The table reconfirms our previous evidence. The reform has a small effect on the mean wage: the strongest positive effect in the first quintile, a positive effect in the second quintile, and no effect in the upper quintiles.

5.3 Inclusion on non-exposed economic sectors

As presented in Table 1, our working sample does not contain the economic sectors that were temporarily exempted from the minimum wage regulation. These sectors had to adjust their sector-specific minimum wage levels to the national threshold within two years, which makes the wage adjustment for all employed people a relevant question. The third panel of Table A.3 shows the estimation coefficients based on the sample including all economic sectors—both those exposed and those not exposed to the minimum wage regulation. For this extended sample, we find pronounced wage growth in hourly wages in the first and second quintiles.

5.4 Alternative bite measure

The analysis presented here relies on a bite measure constructed from the SOEP data. In some regions, numbers of observations in SOEP are small, calling the validity of the derived bite into question. This is a threat to our identification strategy, particularly if the measurement error is systematic. For this reason, we re-run our analysis using a bite constructed from large-scale company data, the Structure of Earnings Survey (SES).[18] Unfortunately, the data are available only for 2014, but not for our preferred period, 2013, meaning that anticipatory effects may already have taken effect on the bite. However, Caliendo et al. (2018) show no anticipation effects on wages. The fourth panel of Table A.3 replicates the main analyses for hourly wages, except that instead

[17] For the structure of item non-response on income and wealth questions in SOEP, see Frick and Grabka (2005); Frick et al. (2007).

[18] Source: FDZ der Statistischen Ämter des Bundes und der Länder, Verdienststrukturerhebung, 2014.

of the SOEP-based bite for 2013 we use the SES-based bite for 2014. In general, the results using the SES-based bite confirm the presence of significant wage growth in the first quintile. However, the magnitude of the coefficient is lower, and no effect on the second quintile is detected.

5.5 Regional-level aggregation

Our main results rely on individual-level data combined with the regional-level bite information. We believe the individual-level measurement of the outcome variable is appropriate as it enables us to control for regional differences in employee characteristics while also considering within-subject changes over time in characteristics that are relevant for the outcome. In order to compare these mixed-level results to a pure regional-level estimation (as conducted by Card 1992a), we re-estimate Eq. 1 at the level of regional units. The dependent variable is mean wages in a particular year measured in the overall regional distribution or within region-specific quintiles. Results are consistent with the individual-level estimation: There is a sizable effect on wages in the first quintile (see the lowest panel of Table A.3). However, the magnitude of the effect is lower and there is no significant spillover effect on the second quintile.

6 Conclusion

This paper assesses the short- and medium-term effects of Germany's minimum wage reform on the distribution of hourly wages, monthly wages, and working hours. In January 2015, Germany introduced a high statutory minimum wage with only a few legal exemptions. The new minimum wage was set at €8.50, which exceeded the hourly wages of more than 10% of all eligible employees in 2014. Two years later, the minimum wage level was increased to €8.84. We analyze the implications of this major labor market intervention, which is of interest both for the German context and for other developed countries.

   With respect to the reform's main goal, which was to increase hourly wages at the bottom of the wage distribution, our empirical data suggest that it was effective. In the low-wage segment, the descriptive analyses show an acceleration of wage growth starting in the first year following the reform's implementation. A difference-in-differences analysis relying on the regional variation in treatment intensity as a source of identification corroborates this evidence. We find sizable positive treatment effects for the bottom quintile of the region-specific wage distributions: In the first post-reform year (2015), the average wage growth in the first quintile was about 9% and increased to 21% in subsequent years, 2016–2018. However, most of the affected employees simultaneously experienced a reduction in working hours. For this reason, we cannot identify a statistically significant improvement in monthly wages for minimum-wage employees. This result echoes findings from studies carried out in the United States (Neumark et al. 2004), but is somewhat contrary to findings from studies using estimation designs based on regional-level distributions of hourly wages and monthly wages, which show an increase at the low end of both distributions (Ahlfeldt et al. 2018). Our

results imply that the effect of the minimum wage reform on exposed individuals may be different from the effect on exposed regions. Data aggregation at the regional level disregards the individual location of exposed populations in the distribution of monthly wages and the change in these results should be seen as complementary in this respect.

Our analysis provides evidence that—because of downward adjustments in working hours—an hourly minimum wage does not necessarily improve monthly wages for employees with low pay. If the policy goal of a minimum wage is the prevention of poverty, our results imply that not only the interaction with the social security system and its means-tested income support must be considered (Müller and Steiner 2011). Changes in working time can also contradict with the political intentions. Simultaneously, this stresses the importance of the intensive margin when employment effects are analyzed: Employers seem to use changes in working hours as a means to cope with increasing labor costs without reducing jobs. This might explain why effects on the extensive margin have not been identified in Germany so far.

Declaration

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