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ABSTRACT

Wage Inequality in Germany after the Minimum Wage Introduction*

We revisit the development of monthly wages in Germany between 2000 and 2017. While wage inequality strongly increased during the first years of this period, it recently returned to its initial level, raising the question what the role of the German minimum wage introduction for this reversal is. We identify effects of the minimum wage from difference-in-differences based on unconditional quantile regressions applied to German administrative employment data. The results show significant wage effects of varying magnitudes along the lower half of the wage distribution. Employment dynamics do not explain effects along the wage distribution, implying strong wage increases among the existing workforce. The increased individual labor income is not offset by decreasing social benefits. Overall, the introduction of the minimum wage can account for about half of the recent decrease in wage inequality.

JEL Classification: J31, J38
Keywords: minimum wage, inequality, wages, Germany

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1. Introduction

The development of wage inequality between 1980 and 2010 has been investigated for various countries. The US, for example, experienced a continuous rise in the upper tail wage inequality (measured by the gap between the 90th and the 50th percentile), while the lower tail wage inequality (measured by the wage gap between the 50th and the 10th percentile) increased in the 1980s, but did not later on; see, for example, Acemoglu and Autor (2011). There has been some debate on the importance of the decline in the real value of the minimum wage for the rise in the lower tail wage inequality in the 1980s (Autor, Katz, & Kearney, 2008; Fortin & Lemieux, 1997; Lee, 1999). In a recent study, Autor, Manning, and Smith (2016) attributed 30 to 40 percent of the rise in lower wage inequality to the minimum wage decline. For the UK, the wage inequality appeared to be only moderately affected by the minimum wage introduction in 1999, which can be explained by the fact that the minimum wage was introduced below the 10th percentile (Dickens & Manning, 2004; Stewart, 2012).

Dustmann, Ludsteck, and Schönberg (2009) documented a substantial increase in wage inequality for West Germany between 1980 and 2004, contradicting the view that a rising wage inequality is mainly a phenomenon observed in the US or in Anglo-Saxon countries. In contrast to the US, lower tail wage inequality increased in Germany since the 1990s; and wages at the 15th percentile even declined since the mid-nineties. Explanations that have been suggested for the increase in the lower tail wage inequality in Germany are supply shocks and de-unionization (Dustmann et al., 2009; Biewen & Seckler, 2019), a growing heterogeneity in firm-level wage policies (Antonczyk, Fitzenberger, & Sommerfeld, 2010; Hirsch & Mueller, 2020), increasing positive assortative matching, i.e. low-skilled workers being employed in low-paying firms (Card, Heining, & Kline, 2013), domestic outsourcing of service personnel (Goldschmidt & Schmieder, 2017), and changes in working hours (Biewen & Plötze, 2019). Biewen, Fitzenberger, and de Lazzer (2018) present a survey of the literature. There is some evidence from studies looking at data until 2014 suggesting that the rise in wage inequality has not proceeded or even decreased after 2010 (Brüll & Gathmann, 2018; Möller, 2016). Bruell & Gathmann link the declining wage dispersion (at the bottom of the distribution) in Eastern Germany to the expansion of sector-specific minimum wages that were introduced in some industries.

On January 1st 2015, a nation-wide hourly minimum wage of €8.50 was introduced in Germany in order to increase low wages, which directly affected about 10 to 14% of the workforce who were paid below the new threshold before it came into force (Caliendo, Schröder, & Wittbrodt, 2019). On average, affected employees were paid €6.01 before the minimum wage was introduced.[1] If successful, the policy would lead inter alia to a reduction in the well-documented wage inequality. However, it is by no means clear to which extent this has been achieved. Monthly wage effects may be dampened by non-compliance with the law or by a downward adjustment of hours worked (Caliendo, Fedorets, Preuss, Schröder, & Wittbrodt, 2017; Burauel et al., 2019). Unintended reductions in wage equality may arise if minimum wage workers lose their jobs, although such effects were found to be moderate (Bossler &

[1]On January 01, 2017 the minimum wage was increased to €8.84. However, in real terms this amounts only to an increase to 8.65 such that we do not expect a sizeable effect on wage inequality.

Gerner, 2019; Caliendo, Fedorets, Preuss, Schröder, & Wittbrodt, 2018; Garloff, 2019; Schmitz, 2019). Moreover, there may be wage-spillovers to workers not directly affected by the minimum wage with an ambiguous effect on wage inequality.[2] Such spillover effects may arise due to a demand shift from minimum wage workers to other employees or due to employers which want to maintain wage differentials between different skill groups. Neumark and Wascher (2008) review evidence of minimum wage spillovers in the US and the UK. When investigating how the minimum wage introduced in the roofing sector in 1997 affected the monthly wage distribution, Gregory (2014) obtained positive effects up to the 60th percentile and negative effects beyond.

Rattenhuber (2014) investigated the minimum wage introduction in the German construction sub-sectors in 1997 and found a compression in the lower part of the wage distribution in East Germany. Regarding the introduction of the nation-wide minimum wage, there is some evidence of increasing hourly wages of low-wage workers (Ahlfeldt, Roth, & Seidel, 2018; Bossler & Broszeit, 2017; Burauel et al., 2019), but it is not yet clear whether monthly wages increased. Therefore, the impact of the nation-wide minimum wage on wage inequality in Germany remains an open issue.

Using German register data from the social security system, we revisit the development of wage inequality after the turn of the century. We contribute to the existing literature on wage inequality in various dimensions. First, compared to earlier studies, we expand the time window until 2017 such that we are the first to investigate the effect of the introduction of the new minimum wage in Germany on wage inequality. Second, while previous evidence mainly focused on full-time employees or even full-time employed males, we deliberately include all employment types (including part-time and minijob) and do not impose restrictions on gender or age. Thereby, we ensure that those groups which should be affected the most by the minimum wage are not dropped. Third, we analyze monthly earnings rather than hourly wages. Monthly earnings are less prone to measurement errors than hourly wages, which are typically calculated by dividing earnings by hours worked. Moreover, monthly earnings combine two dimensions (hourly wages and hours worked) which may be affected by the minimum wage. Note that we investigate inequality in individual gross labor market income and do not consider family income, other income sources or re-distributional tax effects. However, Biewen, Ungerer, and Löffler (2019) show that changes in labor income have been the driving factor for changing total income inequality over the last two decades.

Our descriptive analysis confirms – for the lower tail of the wage distribution – a rising wage inequality in the 2000s but a declining trend from 2010 onwards. As expected, we observe a pronounced dip in wage inequality after 2014. We estimate the (causal) effect of the minimum wage introduction on wage deciles of the unconditional wage distribution by combining unconditional quantile regressions with difference-in-differences that exploit variation in the share of workers directly affected by the minimum wage at the level of regional labor markets. We distinguish between changes within the existing workforce and selection effects

[2] It depends on the inequality measure used. Since workers at the 10% percentile have been directly affected by the minimum wage in Germany, the gap between the 50th and the 10th percentile may fall less compared to a situation where spillovers were absent. Conversely, the gap between the 90th and the 50th percentile may be reduced.

through employment effects. The results show that monthly wages significantly increased at the bottom of the wage distribution but also up to the median. This result implies a meaningful reduction of overall wage inequality measured by the variance of log wages.

Further, investigations demonstrate that the baseline result is robust with respect to (i) restricting the sample to prime age male full-time workers as applied in previous studies, (ii) employment dynamics through entries and exits along the wage distribution, (iii) fixed effect estimation identifying an effect for incumbent workers, (iv) a specification that excludes the treatment group-specific trend and (v) alternative definitions of regional labor markets and calculations of the bite variable (i.e. the share of workers paid below the minimum wage before its introduction).

Counterfactual predictions based on the regression results show that the minimum wage is responsible for 40-60 percent of the recent decline of wage inequality, depending on the year of reference. If the minimum wage was introduced at a lower level, say €5, the effect on wage inequality would have been much lower. Finally, we do not observe a noteworthy role of spillovers for the variance of log wages.

The remainder of the paper is organized as follows. Section 2 describes the data source and the wage variable used in this study, Section 3 presents descriptive evidence on the development of wage inequality in Germany between 2000 and 2017. Section 4 investigates the importance of compositional changes for the reported wage inequality development. Section 5 presents unconditional quantile regressions to identify the causal effect of the minimum wage on different wage deciles and on the variance of log wages. Section 6 assesses the importance of employment dynamics for the effect of the minimum wage at different parts of the distribution. Section 7 investigates the robustness of our results after adding social benefits as an additional income source of employees. Section 8 discusses the importance of the effect of the minimum wage on wage inequality. Section 9 concludes.

2. Data

The data of our investigations are the administratively collected social security data of the German labor market, also known as the Integrated Employment Biographies (IEB), see Dorner, Heining, Jacobebbinghaus, and Seth (2010) and Jacobebbinghaus and Seth (2007). The data is collected by the Federal Employment Agency and it can be merged to information about social benefit receipt, as applied in Section 7. The IEB are prepared and provided for scientific purposes by the Institute for Employment Research (IAB). The employment and wage information, which are most relevant for our purposes, stems from compulsory reports of employers to the social security insurance.[3] Each employer in Germany is obliged to report employment information for each employee at least once a year. The data have a spell format, where the reported information covers a period that is defined by a start and an end date for each spell. The employment data cover almost the entire German labor market, only excluding the self-employed and civil servants.

For our analysis, we use a random two percent sample of the individuals in the IEB, covering

[3]The data on social benefits is collected by the Federal Employment Agency.

the period from 2000 to 2017.[4] We select all employment spells that cover June 30th of a given year in order to create a yearly panel. An individual identifier allows us to track the individuals included in the sample over time as long as they stay in the labor market, and an establishment identifier allows us to identify employer changes. The identifiers are used in Section 6.3, when estimating effects on wages of the incumbent workers and within existing jobs. In our baseline analysis, we only include observations of individuals with a single job as this provides an interpretation at the job-level and at the individual-level. This sample restriction will be relaxed in the robustness checks.

Finally, we exclude observations with zero wages, as these individuals are not working for pay and are most likely volunteering in the social (non-profit) sector, which is exempted from the minimum wage. Further, we also exclude apprentices and internships.

2.1. The measurement of wages

The wage variable in the data provides the most relevant information for the analysis of inequality. Wages in the IEB should be highly accurate, since they are used to calculate claims from the unemployment insurance. The wage is reported for each employment spell, defined by start and end dates. Even though the wage information is not updated daily, it is accurate for the dates of the respective employment spell. From the spell-specific wage information, we calculate gross monthly wages and deflate wages with the consumer price index such that all results can be interpreted in 2014 Euro values.[5] We analyze monthly wages, the development of which incorporates changes in hourly wages and in hours worked. Since the IEB data does not include working hours information after 2014 (and before 2011), we cannot investigate hourly wages. The existing evidence concerning the impact of the minimum wage on working hours draws on individual survey data (Caliendo et al., 2017; Burauel et al., 2019) and establishment survey data (Bossler & Gerner, 2019) and concludes that working hours slightly decreased in course of the minimum wage introduction, but only during the first year after the law came in force. Consequently, the minimum wage effect on monthly gross wages should be smaller than the (unknown) effect on the hourly wage distribution. From the individuals' perspective, reduced working hours increase leisure time, such that the change in monthly wages captures a lower bound of the individual benefits of the minimum wage introduction.

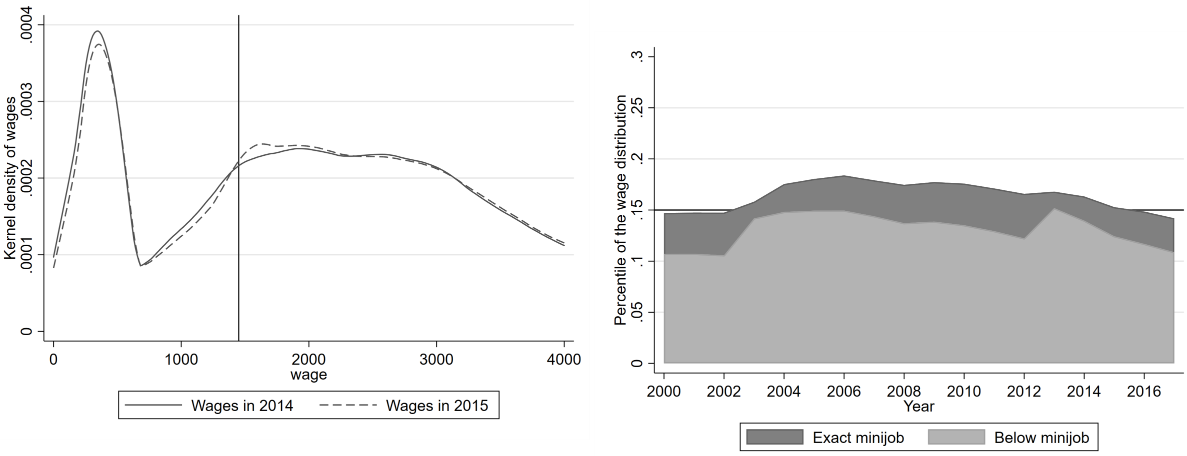
We argue that for the development of income inequality, investigating monthly wages is a relevant measure. One has to bear in mind, however, some institutional specifics when analyzing the monthly wage distribution. As it is apparent from Figure 1a, monthly wages follow a bi-modal distribution. The first mode is due to the existence of minijobs which define employment below a certain income threshold, currently set to €450 per month.[6] These minijobbers are exempted from taxes and social security contributions and their employers pay a flat rate of

[4] The analysis can be replicated using the SIAB data that is provided through the Research Data Center of the Institute for Employment Research.

[5] The wage information is top-coded as the upper end of the wage distribution is not social security relevant. This affects 6.7 percent of the wages (at the top) in our period of analysis. However, the estimated effect of the minimum wage on the variance of log monthly wages (discussed in Sections 5 – 8) remains unaffected when imputing the censored wages.

[6] During our period of analysis, the minijob threshold has been €325 until 2002, €400 between 2003 and 2012, and €450 from 2013 onwards.

social security contributions. Figure 1b displays the fraction of workers directly at and below the minijob threshold and its development over time. Since there is a severe bunching of workers at the minijob threshold, effects of the minimum wage between the 10th and 20th percentile of the wage distribution may be very specific to this institutional threshold of minijobs.



(a) Real monthly wage distributions, 2014 and 2015; vertical line indicates the full-time minimum wage threshold of €1440

(b) Share of minijobs, 2000-2017

Figure 1: The wage distribution and the share of employees below and at the minijob threshold

Figure 1(a) compares the wage distributions between 2014 and 2015. Both distributions are more distinct than the comparisons between the years 2013/2014, 2015/2016 and 2016/2017 (see Appendix Figure A1). Nevertheless, between 2014 and 2015 only a small share of the mass shifted across the full-time minimum wage threshold indicated by the vertical line. Hence, one might reason that the impact of the minimum wage has been only modest.

A different picture about the impact of the minimum wage emerges from Table 1 which displays average real monthly wage growth (measured by the difference in log points) by bins of the initial hourly wage distribution, following the approach of Stewart (2012).[7] Individuals that were initially located very low in the wage distribution experience the strongest wage growth on average. Strong wage growth at the bottom of the wage distribution, however, is not very surprising due to mean-reversion. For the group of directly affected minimum wage workers (in the first row), we also see that wage growth picks up sharply from 2014 to 2015. In fact, wage growth already increases slightly beforehand, which can be due to a positive wage trend at the bottom of the wage distribution or even a small anticipation effect. The second row shows an increasing annual wage growth even for individuals that received between 100 and 110 % of the minimum wage. While this effect shrinks further up in the wage distribution (rows 3 and beyond), we still observe slight increases in wage growth along the wage distribution, suggesting an upward shift in wages due to the minimum wage that goes beyond the directly affected employees.

Figure 2 demonstrates the location in the monthly wage distribution of workers paid exactly

[7] Using bins of the hourly wage distribution allows directly identifying workers paid below the minimum wage in 2014. To calculate hourly wages, we use working hours information from the compulsory injury insurance, which has been merged to the administrative employment data. Note that we cannot use the hourly wage as the dependent variable because we observe hours worked only between 2011 and 2014.

Table 1: Real monthly wage growth by bins of the initial hourly wage distribution

	(1)	(2)	(3)	(4)
	wage growth in the periods:			
Initial wage bin	2011-2012	2012-2013	2013-2014	2014-2015
below minimum wage	0.137	0.143	0.155	0.234
100% − 110% minimum wage	0.055	0.051	0.071	0.107
110% − 120% minimum wage	0.050	0.043	0.064	0.095
120% − 130% minimum wage	0.042	0.036	0.048	0.060
130% − 140% minimum wage	0.025	0.026	0.041	0.050
140% − 150% minimum wage	0.024	0.021	0.033	0.048
150% − 200% minimum wage	0.010	0.006	0.023	0.025
≥ 200% minimum wage	0.011	0.011	0.025	0.027

Notes: The figures are growth rates of real monthly wages, measured by the log-point differences between the respective years indicated by column titles. Samples are split by the individuals' real hourly wage in the respective baseline year, where 100% minimum wage is exactly the minimum wage threshold €8.50, and for example, 200% minimum wage represents an initial real hourly wage of €17. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded. To calculate real hourly wages that are used to classify workers into wage bins in the initial year, we use working hours information from the compulsory injury insurance (available from 2011 until 2014), which has been merged to the administrative employment data.

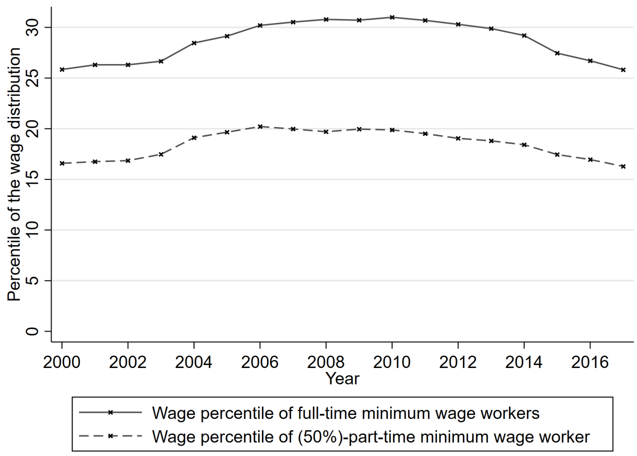


Figure 2: Percentile (within the monthly wage distribution) of full-time and of 50-percent-part-time minimum wage workers in 2014-Euros, 2000-2017. The real monthly wage of a full-time minimum wage worker is equal to 1,440 Euros (8.50€ $\times$ 39$hours/week$ $\times$ 4.345$weeks$); the real monthly wage of a 50 percent part-time minimum wage worker is equal to 720 Euros (8.50€ $\times$ 19.5$hours/week$ $\times$ 4.345$weeks$).

the minimum wage of €8.50 (and its evolvement over time). Assuming a weekly working time of 39 hours and 4.345 weeks per month, a full-time minimum wage worker is located between the 25th and the 30th percentile and only exceeds the 30th percentile in the mid-2000s when the unemployment rate was particularly high in Germany. After 2014, when the minimum wage was introduced, we observe a slight drop suggesting that workers being paid previously at or below the minimum wage have been crossing the full-time minimum wage threshold or, alternatively, even have become unemployed.

A very similar movement in the monthly wage distribution is observed for part-time minimum wage workers, who are assumed to work 19.5 hours per week (i.e. exactly half of the working time of a full-time worker). Again, such a person moves up the wage distribution during the economic downturn, but in recent years the threshold dropped down in the distribution.

3. Trends in wage inequality in Germany in the 21st century

To analyze the wage structure descriptively, the upper panels of Figure 3 display the real wage growth at different percentiles of the unconditional wage distribution (and the corresponding development of these percentiles is presented in Appendix Figure A3). It shows a striking fall of the 5th and (even more) of the 20th percentile until 2006, the latter falling by almost 50 percent between 2000 and 2006. Interestingly, 2006 also marks the turning point for an initially rising unemployment rate. Thereafter, both wage series rise again and the 20th percentile reaches its 2000-value in 2016. For both percentiles, the sharpest increase is observed between 2014 and 2015 which coincides with the introduction of the minimum wage. The development of the other percentiles at the bottom of the distribution is less pronounced, but real wages at the 10th (15th) percentile also have been rising since 2009 (2012). For the last five years of the time window, we observe that wages have increased the least at the 50th and the 80th percentile implying a fall in wage inequality (see top right panel of Figure 3).

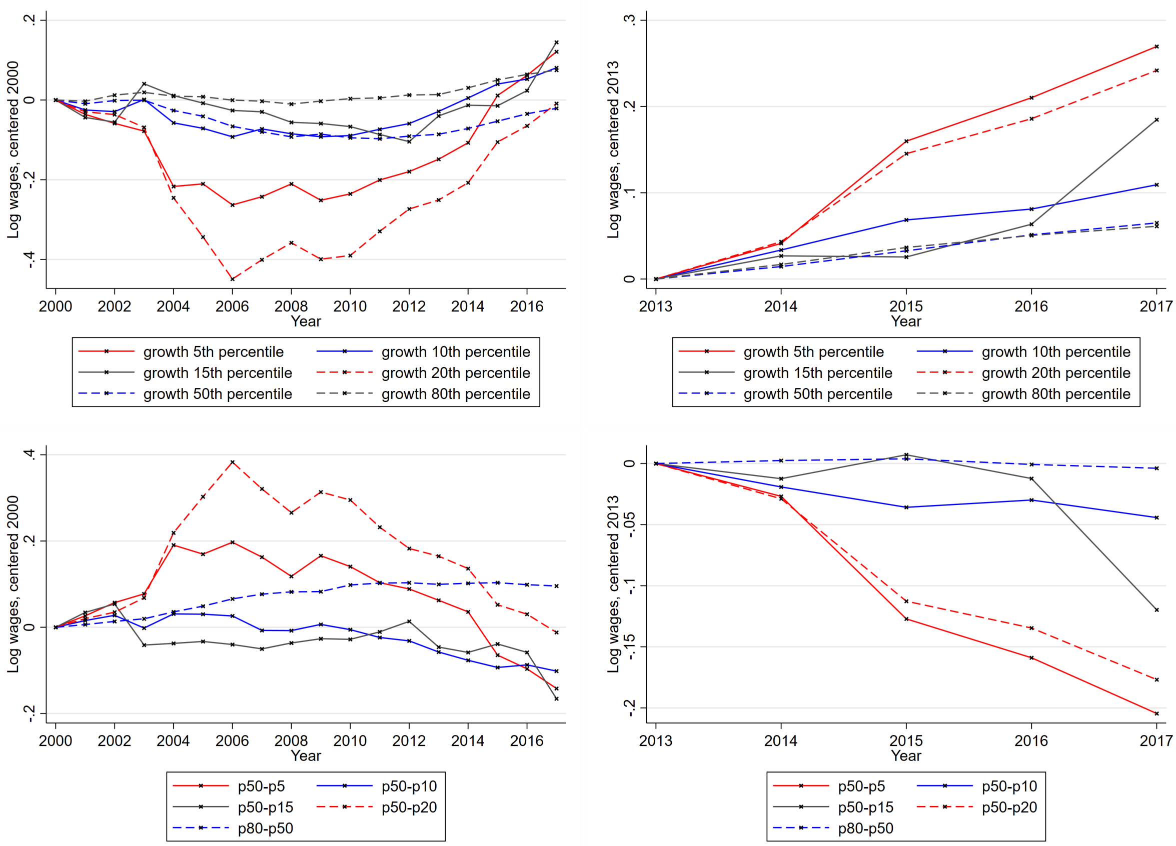


Figure 3: Real growth of log monthly wage percentiles and log-differences of monthly wage percentiles, 2000-2017 and 2013-2017, where the initial year is normalized to zero.

The corresponding development of the log wage differences between various percentiles is depicted in the lower panels of Figure 3, where all differences are indexed to zero in the first year (the actual log wage difference is presented in Appendix Figure A3). Wage inequality at the upper part of the distribution (measured by the gap between the 80th and the 50th percentile) was rising until 2010, but has remained constant thereafter, confirming the findings of Brüll and Gathmann (2018). The picture is different for the wage inequality at the lower part of the distribution, measured by the 50-20, 50-15, 50-10 and 50-5 gaps, where wage inequality has been falling since 2009 (respectively 2012 in the case of the 50-15 gap). Note that the development in the first part of the observation window crucially depends on the choice of percentile at the bottom: while the 50-20 and 50-5 gaps increased rapidly until 2006, there is no observable trend for the 50-15 as well as for the 50-10 percentile comparisons. This heterogeneity in developments across the wage distribution clearly demonstrates that looking at just one particular percentile within the bottom part of the distribution may not provide a complete picture of the development in wage inequality. Relatedly, the bottom right panel clearly suggests that the minimum wage introduced in 2015 has been effective in reducing the 50-20 and 50-5 wage gaps, while the difference between the 50th and the 10th percentile does not seem to be affected and the 50-15 gap has experienced a sharp fall not before two years after the introduction of the minimum wage.

The absence of meaningful changes at the 10th and 15th percentile may be due to the prevalence of minijobs. This conjecture is supported by a descriptive comparison of the wage distributions between 2014 and 2015 (Table A2), which demonstrates an overall rightward shift of the wage distribution after the minimum wage has been introduced, while the number of individuals exactly at the minijob threshold increased. It suggests an incentive (for employers or employees) to stay in subsidized minijobs even in the presence of a minimum wage induced hourly wage increase (which should affect most minijobber).

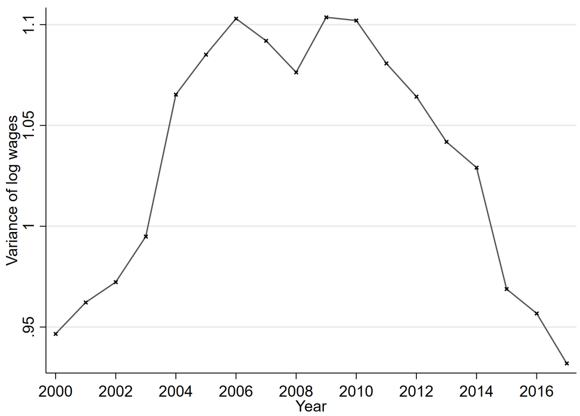


Figure 4: Variance of log real monthly wages, 2000-2017

Given that the various percentiles at the bottom part of the distribution do not evolve uniformly, we also inspect overall wage inequality measured by the variance of log wages throughout this study. Figure 4 depicts the development of the variance during the sample period.[8] The

[8]The variance of log wages displayed in Figure 4 is substantially above the respective numbers typically found in the literature. Dustmann et al. (2009), for example, report the log wage variance in the year 2000 to be just

variance of log wages initially rises, peaks between 2006 and 2010 and falls thereafter, reaching the level of 2000 once more in 2016. Again, we observe a pronounced drop between 2014 and 2015, when the minimum wage was introduced.

In summary, descriptive developments of the log wage variance and the log wage gaps of the 50-20 and the 50-5 percentiles suggest that the nationwide minimum wage may have reduced wage inequality considerably. We will investigate in the following section whether this result holds when taking changes in the workforce composition into account before we proceed in Section 5 with a difference-in-differences analysis to identify a causal effect.

4. The impact of compositional changes on the wage structure

During the last twenty years, the structure of the workforce experienced some notable changes, for example the share of university or college graduates and the share of elderly employees increased considerably (see Appendix Figure B4). In this section, we examine whether the development in wage inequality discussed in the previous section is affected at least partially by such compositional changes in the workforce. We apply the decomposition method by DiNardo, Fortin, and Lemieux (1996). The idea is to hold the workforce characteristics constant at a reference period, defined to be the year 2000, to obtain the density that would have prevailed if individual attributes had remained at their 2000-level while being paid according to the wage schedule observed in the years thereafter. Thereby, it is assumed that there is no impact of changes in the distribution of observable characteristics on the structure of wages.

The observed density of log wages w in 2001 (and analogously for any year thereafter) and 2000 are decomposed into a wage function $g(.)$ and a composition function $h(.)$, see also Autor et al. (2008) or Dustmann et al. (2009):

$$\begin{aligned} f(w|T=2001) &= \int g(w|x,T=2001)h(x|T=2001)dx \\ f(w|T=2000) &= \int g(w|x,T=2000)h(x|T=2000)dx \end{aligned} \tag{1}$$

where $g(w|x, T = t)$ is the density of log wages in year t for observable characteristics x and $h(x|T = t)$ is the density of characteristics in year t. To obtain the counterfactual wage distribution $f_c(.)$ in year 2001, the wage function $g_{2001}(.)$ of year 2001 is re-weighted with the ratio $h_{2000}(.)/h_{2001}(.)$ of the densities of characteristics in years 2001 and 2000.

$$f_c(w|T=2000) = \int g(w|x,T=2001)\phi(x)h(x|T=2001)dx \tag{2}$$

where $\phi(x)$ represents the re-weighting function. Applying the Bayes rule, $\phi(x)$ can be computed as the following:

$$\phi(x) = \frac{h(x|T=2000)}{h(x|T=2001)} = \frac{Pr(T=2000|x)}{1-Pr(T=2000|x)} \times \frac{1-Pr(T=2000)}{Pr(T=2000)} \tag{3}$$

above 0.2. The difference is due to the fact that we do not restrict the sample to full-time employees resulting in a bi-modal distribution (as discussed in the previous section).

The first term of the fraction on the right-hand-side of (3) can be computed from the propensity score of being observed in the year 2000, conditional on observed characteristics. Since the second term of the fraction on the right-hand-side of (3) does not depend on x, it is the same for all observations and can therefore be omitted from the weights. Hence, constructing the counterfactual is equal to inverse probability weighting. The weights could also be obtained from alternative procedures such as matching or entropy balancing; the latter is applied in a robustness check reported in Appendix B.

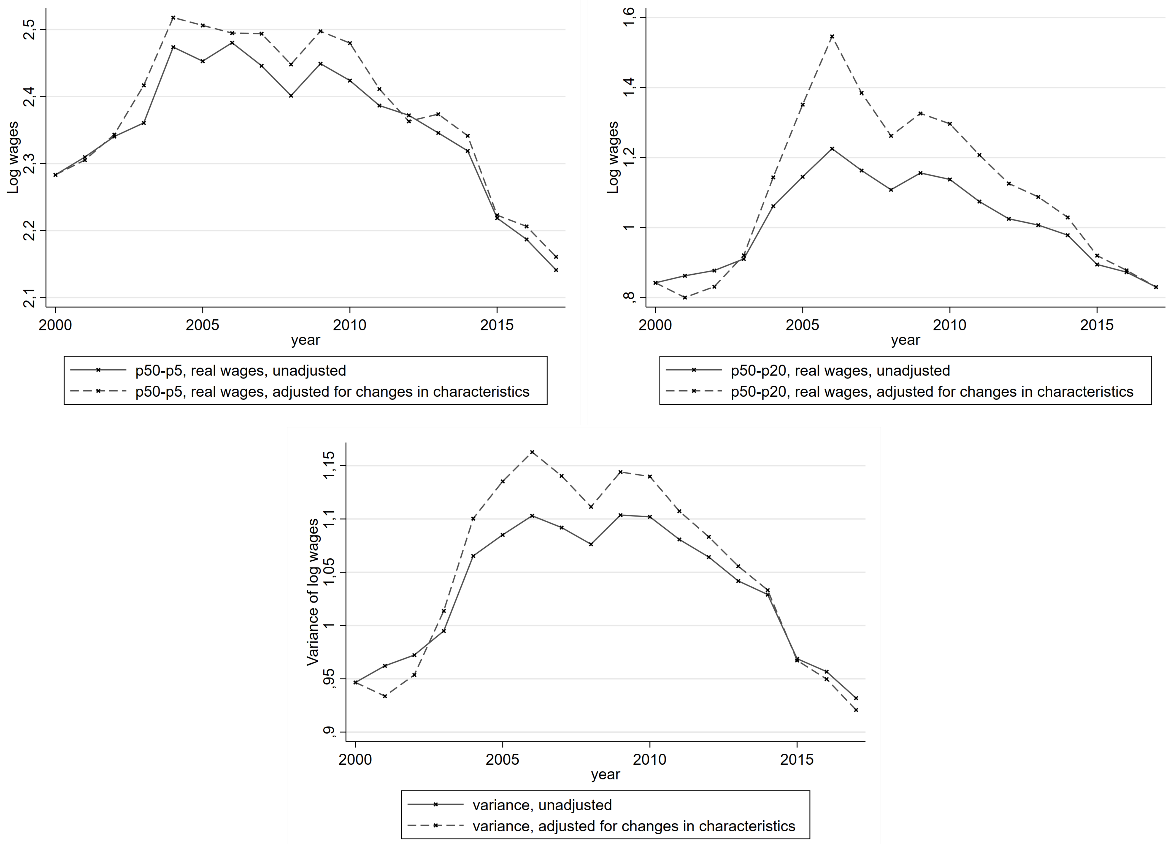


Figure 5: Decomposition of the percentile differences and of the variance of log real monthly wages using inverse probability weighting based on the propensity scores for being observed in the year 2000; propensity scores obtained from separate logit estimations, each using pooled data from the year 2000 and one of the years thereafter (2001–2017); logit models include the following observable characteristics, fully interacted with gender: age (12 categories), experience (8 categories), tenure (8 categories), post-secondary degree (4 categories), foreign citizenship (dummy).

We estimate the propensity score with a logit model with the following covariates, fully interacted with gender: twelve age categories, eight categories for experience and tenure, four post-secondary degree categories and a dummy for foreign citizenship.[9] Appendix Figure B5 confirms that the difference in the propensity scores between the base year 2000 and the years thereafter increases in time, implying increasing compositional differences over time.

Figure 5 contrasts the actual and the counterfactual of the 50-5 (top left) and the 50-15 log

[9]In a robustness check, we additionally control for 38 occupations and 89 industries, but the decomposition results remain virtually unchanged for the period of the minimum wage introduction. We do not include these variables in our main specification because both variables have structural breaks in the 2000s.

wage gaps (top right), as well as the actual and counterfactual variance of log wages (bottom). It shows that the reported wage inequality would have developed remarkably similar if the composition of the workforce remained constant at its 2000-values. If anything, the counterfactual distribution even exhibits slightly higher peaks in wage inequality during the previous decade. Moreover, the drop of wage inequality between 2014 and 2015, which is of the most interest for our study, would have been even slightly steeper if workforce characteristics were held fixed. We conclude that the decline in wage inequality at the bottom of the distribution at the time of the minimum wage introduction is by no means due to changes in the workforce composition.

5. Difference-in-differences analysis

After having explored the development of wage inequality in the German labor market descriptively, we estimate the effect of the minimum wage on different quantiles of the wage distribution, allowing us to quantify the decline in wage inequality that can be ascribed to the minimum wage. We apply a difference-in-differences specification that uses regional labor market variation in the bite of the minimum wage introduction for identification. We calculate the bite as the fraction of workers paid below the (hourly) minimum wage threshold before the law came into force.[10] Regional labor markets are obtained from Kropp and Schwengler (2016), who use the graph approach to construct functional labor markets maximizing worker commuting within markets while minimizing commuting across markets.[11] Hence, the identification strategy compares mostly independent labor markets within Germany that are differentially treated by the minimum wage. Figure 6 illustrates the variation of the bite measure across Germany, demonstrating that the most severely treated regions are located in the east while the least affected regions are located in the south.

By using regional variation in the bite for identification of the minimum wage effect, we follow previous studies which mainly analyze effects on employment; e.g. Card (1992) for the US and Caliendo et al. (2018) for Germany. The major advantage of regional variation is capturing spillover effects induced by the minimum wage that occur within a regional labor market. If, for example, an individual benefits from increased wages while another individual's wage is cut in compensation, the average wage effect in the labor market is zero irrespective of who of the two individuals is treated. Individual based treatment assignments would show a treatment effect if only one of these individuals is assigned to be treated.

The difference-in-differences specification of interest is specified as follows:

$$RIF(y_{it},.) = \sum_{t=2014}^{2017} \delta_t * Bite_r * Year_t + \sum_{t=2012}^{2017} \gamma_t * Year_t + \phi * Bite_r + \pi * Bite_r * Trend_t + \epsilon_{it}, \quad (4)$$

[10] To calculate hourly wages for our main specifications, we use working hours information from the compulsory injury insurance, which has been merged to the administrative employment data. The bite variable is available upon request. In addition, we conduct several robustness checks in which the bite is calculated from full-time workers' monthly wages and from survey information of the IAB Establishment Panel; see Appendix C.

[11] When we compared different labor market definitions presented in the literature, the assignment by Kropp and Schwengler (2016) performed best in minimizing commuting as well as job-mobility across regions. Even so, all presented results are robust with respect to other regional definitions; see Apendix D.

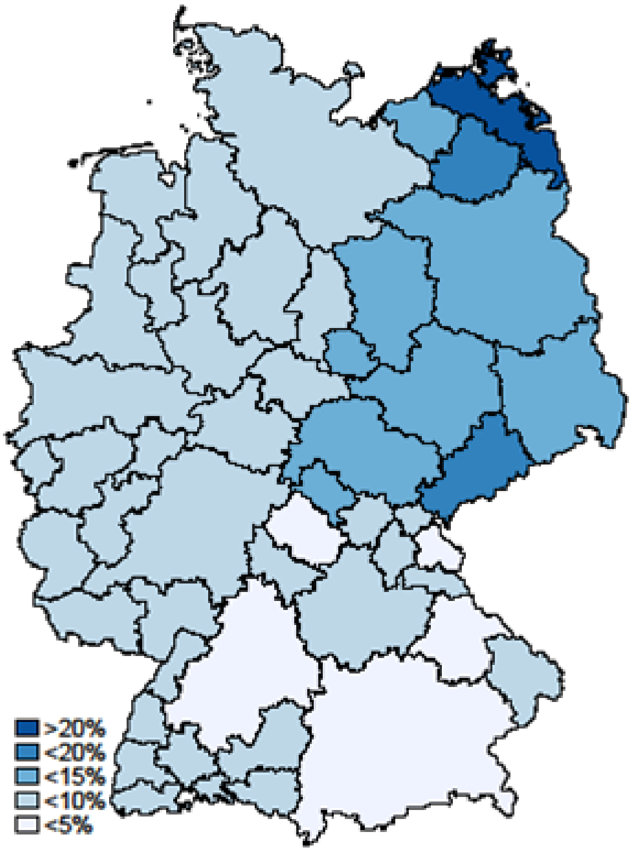


Figure 6: Distribution of the bite across labor market regions in Germany. The employment-weighted average of the bite is equal to 12.6 percent.

where y_{it} denotes log monthly wages of individual i at time t. The dependent variable of interest is the RIF (re-centered influence function) of log monthly wages, calculated for different deciles of the wage distribution, τ, and the variance of log wages, σ^2. The specification includes treatment effect interactions for the years after the minimum wage introduction and for 2014, which is the last year before the minimum wage came into force and because the law was already debated by the parliament at that time, capturing potential anticipation effects. Equation (4) also includes common time effects for each year in the sample; the effect of the bite itself captures level differences which are constant across all years. Finally, the specification includes an interaction between the bite variable and a time trend, capturing a pre-existing bite-specific trend.[12] We estimate this equation for a sample covering the years 2011–2017, where 2011 is a natural starting year since it is after the great recession and after the turnaround in the development of wage inequality described in Section 3.[13] Standard errors are clustered at the level of regional labor markets, which is the level variation of the minimum wage bite. The calculation of standard errors is based on a block cluster bootstrap with 50 replications.

Firpo, Fortin, and Lemieux (2009, 2018) described that the RIF of y_{it} (in our case of log

[12] In a robustness check, we exclude this bite-specific trend; see Appendix E.

[13] In 2011, the administrative data contain a break in the part-time variable (Fitzenberger & Seidlitz, 2019). Our results are not affected by this break since we do not condition on part-time. Yet, our results are robust when we exclude 2011 from the analysis sample.

monthly wages) is defined for various deciles, τ, and for the variance σ^2 as follows:

$$RIF(y_{it}, \tau) = y_\tau - \frac{\tau - I[y <= y_\tau]}{f_Y(y_\tau)} \tag{5}$$

$$RIF(y_{it}, \sigma^2) = (y_{it} - \mu)^2 \tag{6}$$

As derived in Firpo et al. (2009), using the RIF of the variable of interest (here log wages) as the dependent variable in a linear regression yields the unconditional quantile regression. The results can be interpreted as average marginal effects on log wages at the location of percentile τ (or as the average marginal effect on the variance of log wages). Hence, our specification identifies the average effect of the minimum wage bite at different locations of the unconditional distribution of log monthly wages after the minimum wage was introduced.

5.1. Graphical inspection

A central question regarding our empirical specification is whether or not to control for a bite-specific trend $(Bite_r * Trend_t)$ which can only be identified by the variation of the time trend before the minimum wage introduction. We justify our specification from a graphical inspection, but t-tests also favor the inclusion of such a bite-specific time trend.

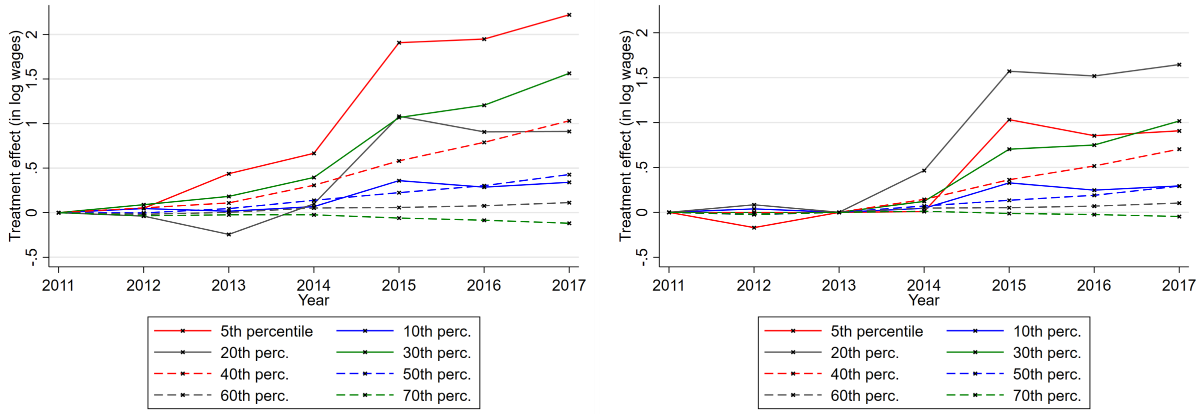
In the graphical inspection, we compare trends of the unadjusted variable with a time trend-adjusted variable, where the bite-specific time trend is eliminated. In other words, we inspect $RIF(y_{it}, .)$ against $RIF(y_{it}, \tau) - \hat{\pi} * Bite_r * Trend_t$, where the latter is net of the bite-specific time-trend which has been obtained from estimating Equation (4). We use these two variables, unadjusted and adjusted, and estimate the following difference-in-differences equation, which captures bite-specific treatment effects for each year in the data:

$$RIF(y_{it}, \tau, (un\text{-})adjusted) = \sum_{t=2012}^{2017} \delta_t * Bite_r * Year_t + \sum_{t=2012}^{2017} \gamma_t * Year_t + \phi * Bite_r + \epsilon_{it} \tag{7}$$

Figure 7 displays the estimates of δ_t for various deciles. The unadjusted graph (on the left) shows substantially positive time trends even before 2014, especially at the bottom of the wage distribution (i.e. at the 5th percentile depicted by the solid red line). This implies a steeper growth of low wages in high-bite regions (compared to low wages in low-bite regions). By contrast, the adjusted graph (on the right) no longer shows a trend divergence by bite before 2014. This parallelism of time trends is also suggested by statistical t-tests on the pre-treatment placebo interactions, which are insignificant throughout. Therefore, in the following we present estimates of treatment effects that are adjusted for a linear bite-specific time trend, as specified in Equation (4).

5.2. Results

The baseline results are presented in Table 2. Column (1) displays the effects of the minimum wage on average log monthly wages. The coefficient of 0.35 on the interaction term of the bite variable and the year dummy for 2015 implies that a 10-percentage point increase in the bite



(a) Unadjusted

(b) Trend-adjusted

Figure 7: Effect plot of the treatment effect (i.e. the interaction effects of bite with each year) for various percentiles as specified in Equation (7), where the outcomes are log real monthly wages unadjusted (a) and log real monthly wages adjusted by a linear bite-specific trend (b). The reference year is 2011.

leads to a 3.5 percent increase in monthly wages. In 2016, the effect is virtually unchanged, but it slightly increases in size in 2017, when the minimum wage was raised for the first time.

The respective interaction term for 2014 has a small and weakly significant positive effect, which can be interpreted as anticipation. As demonstrated in Bossler (2017), affected employers already changed their expectations in 2014, when the minimum wage was already discussed in the public and the parliamentary debate was ongoing. The coefficients of the additional covariates are in line with our expectations, showing a lower level of monthly wages and a slightly more positive time trend in high bite regions, as well as a positive common time trend in wages (being consistent with the descriptive evidence in Section 3).

The subsequent columns report the minimum wage effects on the RIF of log wages at the 5th percentile and at all deciles up to the 70th percentile of the wage distribution. The effects are interpreted as average effects at different locations of the unconditional wage distribution. At the 5th percentile the treatment effect interactions are close to one, implying that a 10-percentage point increase in the bite leads to a 10 percent increase in monthly wages. At the 10th percentile, the effect is only weakly significant and ranges in size between 0.2 und 0.3. This is because the 10th percentile is located right at the minijob threshold (see Figure 1b). While some of the minijobs may be upgraded to regular jobs in course of the minimum wage introduction, most of the minijobs remain unaffected by the minimum wage. Hence, albeit the number reduces slightly between 2014 and 2015, a mass of the workers remains in minijobs as already suggested descriptively in Figure 1a.

At the 20th percentile (column 4), the wage effect sharply rises to about 1.6. The 20th percentile is located at the lower end of the distribution of regular jobs. Hence, a large effect is very plausible since there is no other meaningful institutional threshold that would attenuate wage growth induced by the minimum wage, and mostly these workers should be directly affected by the minimum wage, as illustrated in Figure 2.

Table 2: Minimum wage effect on the unconditional distribution of log real monthly wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite	-1.954***	-2.273***	0.040	1.842*	-2.155***	-3.331***	-3.495***	-2.970***	-2.781***	-1.432***
	(0.195)	(0.531)	(0.276)	(1.057)	(0.329)	(0.247)	(0.165)	(0.175)	(0.181)	(0.489)
Bite * trend	0.007	0.229***	0.004	-0.153	0.092***	0.053*	0.020	0.001	-0.012	-0.115**
	(0.028)	(0.076)	(0.026)	(0.107)	(0.034)	(0.030)	(0.021)	(0.025)	(0.018)	(0.045)
D2012 ($year = 2012$)	0.011***	0.010	0.010**	0.062***	0.001	0.002	0.003	0.006	0.006**	-0.003
	(0.004)	(0.010)	(0.005)	(0.018)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.006)
D2013	0.022***	0.015	0.034***	0.099***	0.002	0.002	0.004	0.009	0.011**	-0.012
	(0.008)	(0.020)	(0.010)	(0.031)	(0.009)	(0.008)	(0.006)	(0.007)	(0.005)	(0.012)
D2014	0.031***	0.011	0.052***	0.092**	-0.010	-0.010	0.005	0.018**	0.028***	0.000
	(0.008)	(0.018)	(0.010)	(0.037)	(0.012)	(0.011)	(0.008)	(0.007)	(0.006)	(0.013)
D2015	0.037***	-0.010	0.061***	0.072	-0.055***	-0.022	0.011	0.034***	0.051***	0.016
	(0.014)	(0.045)	(0.011)	(0.055)	(0.021)	(0.014)	(0.011)	(0.010)	(0.007)	(0.029)
D2016	0.059***	0.029	0.085***	0.140**	-0.050**	-0.026	0.020*	0.048***	0.070***	0.011
	(0.016)	(0.048)	(0.013)	(0.065)	(0.023)	(0.017)	(0.012)	(0.010)	(0.008)	(0.030)
D2017	0.070***	0.055	0.093***	0.198***	-0.070***	-0.038*	0.018	0.054***	0.084***	0.001
	(0.016)	(0.044)	(0.022)	(0.067)	(0.026)	(0.021)	(0.015)	(0.012)	(0.009)	(0.028)
Bite * D2014	0.091*	0.113	0.040	0.511***	0.130**	0.167***	0.084**	0.049	0.007	-0.095
	(0.048)	(0.165)	(0.051)	(0.187)	(0.063)	(0.049)	(0.034)	(0.038)	(0.024)	(0.085)
Bite * D2015	0.354***	1.010***	0.288**	1.659***	0.740***	0.394***	0.161***	0.053	-0.023	-0.576***
	(0.095)	(0.373)	(0.142)	(0.358)	(0.123)	(0.087)	(0.058)	(0.068)	(0.043)	(0.188)
Bite * D2016	0.355***	0.809**	0.211	1.655***	0.800***	0.558***	0.220***	0.073	-0.034	-0.505***
	(0.114)	(0.382)	(0.150)	(0.383)	(0.134)	(0.110)	(0.075)	(0.092)	(0.060)	(0.186)
Bite * D2017	0.438***	0.813**	0.273*	1.852***	1.111***	0.761***	0.332***	0.110	-0.053	-0.490***
	(0.134)	(0.339)	(0.152)	(0.491)	(0.181)	(0.146)	(0.102)	(0.113)	(0.074)	(0.183)
Constant	7.672***	5.588***	5.892***	6.459***	7.516***	7.936***	8.171***	8.278***	8.413***	1.281***
	(0.028)	(0.074)	(0.037)	(0.145)	(0.042)	(0.033)	(0.025)	(0.026)	(0.029)	(0.063)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF of various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

The minimum wage effect on the deciles above the 20th percentile are displayed in columns (5) to (9). We still observe positive treatment effects of the minimum wage introduction which, however, are diminishing the further we move up the wage distribution. Although the effect is much smaller in size than for lower percentiles, there still is a positive average effect at the median wage. Since full-time minimum wage workers are located at the 30th percentile (if working 39 hours per week), a direct effect up to the median is very unlikely even in the presence of overtime hours. This implies spillover effects of the minimum wage introduction along the wage distribution. Further analysis in this respect will be presented below when we quantify the impact of such spillovers for the overall development of wage inequality (Section 8.2).

Column (10) reports the effect of the minimum wage introduction on the variance of log wages. The respective treatment effect is about 0.5 in the years after the minimum wage introduction, implying that an increase in the regional bite by 10 percentage points reduces the variance of log wages of the unconditional wage distribution by 0.05. This estimate is meaningful in size, given that the unconditional variance of log monthly wages is about 1 in our period of analysis. The variance of log wages is a very relevant measure for our analysis because it summarizes effects along the wage distribution. Since we observe varying effect sizes at the bottom of the wage distribution, e.g. due to the minijob threshold, such a measure is much more informative than specific percentile differences.

5.3. Full-time prime age men

As a major check of robustness, we test the sensitivity of our results with respect to the sample definition. Most of the previous literature has focused on male employees in full-time jobs who are in their prime age (between 25 and 55 years). Hence, we check whether our results can be transferred to this stream of literature. It is entirely possible that our results do not hold for prime age males in regular full-time jobs, as these may be less likely affected by the minimum wage, which is why we have used a much broader sample in our baseline analysis. It is also interesting to restrict the sample to regular jobs (i.e. excluding minijobs) because minijobs may not represent the poorest individuals in society. Minijobs are often held by young individuals (e.g., students), pensioners, or along with a well-paid job. Hence, it is interesting to see how the prime-age males' wages in regular jobs are affected by the minimum wage.

Table 3 reports the respective results from much narrower samples with the (log) monthly wage variance as the dependent variable. The first column repeats the baseline result from Table 2, while the sample of analysis is more and more restricted in subsequent columns. Note that from the first (baseline) to the last column (prime-age males with regular full-time jobs) the sample size reduces by as much as sixty percent. Nevertheless, the effect of the minimum wage introduction on overall wage inequality remains surprisingly robust with very similar variance reductions after the introduction of the minimum wage across all columns of Table 3. If anything, the treatment effect slightly increases when looking at males only (column 2) instead of the full sample, but it decreases back to its initial size when further sample size restrictions are considered.

Table 3: Minimum wage effect on the variance of log real monthly wages for a restricted sample

	(1)	(2)	(3)	(4)	(5)
Dependent variable	$RIF(\sigma^2)$	$RIF(\sigma^2)$	$RIF(\sigma^2)$	$RIF(\sigma^2)$	$RIF(\sigma^2)$
Explanatory variables					
Bite	-1.432***	0.419	0.884***	1.095***	1.130***
	(0.385)	(0.416)	(0.169)	(0.104)	(0.116)
Bite * trend	-0.115**	-0.135*	-0.080***	-0.089***	-0.070**
	(0.045)	(0.070)	(0.034)	(0.022)	(0.030)
D2012 ($year = 2012$)	-0.003	0.003	0.011	0.029***	0.024***
	(0.006)	(0.000)	(0.004)	(0.003)	(0.004)
D2013	-0.012	0.012	0.023	0.033***	0.029***
	(0.011)	(0.001)	(0.008)	(0.005)	(0.007)
D2014	0.000	0.056	0.050	0.060***	0.059***
	(0.014)	(0.000)	(0.007)	(0.006)	(0.006)
D2015	0.016	0.079	0.085	0.088***	0.088***
	(0.025)	(0.000)	(0.007)	(0.007)	(0.008)
D2016	0.011	0.085	0.103	0.105***	0.106***
	(0.024)	(0.000)	(0.007)	(0.008)	(0.007)
D2017	0.001	0.094	0.114	0.119***	0.120***
	(0.025)	(0.000)	(0.008)	(0.009)	(0.009)
Bite * D2014	-0.095	-0.275	-0.100	-0.120	-0.165***
	(0.084)	(0.171)	(0.081)	(0.035)	(0.047)
Bite * D2015	-0.576***	-0.731***	-0.344***	-0.317***	-0.388***
	(0.223)	(0.294)	(0.124)	(0.067)	(0.095)
Bite * D2016	-0.505**	-0.711**	-0.368***	-0.365***	-0.458***
	(0.233)	(0.330)	(0.154)	(0.083)	(0.121)
Bite * D2017	-0.490*	-0.779**	-0.395**	-0.393***	-0.520***
	(0.271)	(0.366)	(0.189)	(0.098)	(0.146)
Constant	1.281***	0.886***	0.197***	0.072***	0.056***
	(0.048)	(0.051)	(0.019)	(0.013)	(0.013)
Sample restrictions					
Males only		yes	yes	yes	yes
Regular jobs only			yes	yes	yes
Full-time only				yes	yes
Prime age only					yes
Observations	4,154,228	2,180,611	1,961,213	1,744,753	1,337,726
Cluster	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In all columns the dependent variable is the RIF of the variance of log real monthly wages. Estimation samples are restricted as stated in the bottom rows of the table. "*Regular jobs only*" implies that minijobs are excluded and "*Prime age only*" implies that only employees of age between 25 to 55 years are included. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

5.4. Further robustness checks

In order to check the robustness of the inequality reducing effect of the minimum wage, we conducted several sensitivity tests when estimating Specification (4):[14] (i) The results remain fully robust when we apply weighted regressions to control for compositional changes over

[14]If not explicitly stated to be reported in a specific appendix section, the robustness tests are available upon request.

time, using the weights derived in Section 4. (ii) We use alternative data sources to calculate our main variable of interest, namely the bite variable, exploiting monthly wages of full-time workers in the IEB (following Garloff (2019)) and an employer-reported fraction of affected workers in the IAB Establishment Panel (following Bossler and Gerner (2019)). Appendix C demonstrates the robustness of our results regarding these alternative bite variables. (iii) We use alternative definitions of the regional labor market exploiting 105 sub-labor market regions (Kropp & Schwengler, 2016) and 401 German administrative counties (*Kreise*), see Appendix D. The respective results remain qualitatively unchanged. (iv) We assess the linearity of the treatment effect using dummies of the regional (county-specific) bite, where each dummy comprises a quarter of all observations. The estimated coefficients are monotonically increasing in these dummy variables, largely confirming linearity. (v) We conduct estimations with the RIF for the gini coefficient of log monthly wages instead of the variance, but the results remain qualitatively unchanged. (vi) We exclude the bite-specific time trend and re-estimate the difference-in-differences specification without accounting for this additional time- and treatment-specific heterogeneity, see Appendix E. The treatment effects slightly increase in size. However, these increases are due to a general upward trend in wages at the very bottom of the distribution within the period of the minimum wage introduction. (vii) We also allowed for separate region-specific trends instead of a bite-specific trend, which captures trend differences between labor market regions more flexibly, but the results remain virtually unchanged. (viii) We included observations of individuals with multiple jobs, but the results remain qualitatively unchanged, see Appendix F.

6. Selection through employment dynamics

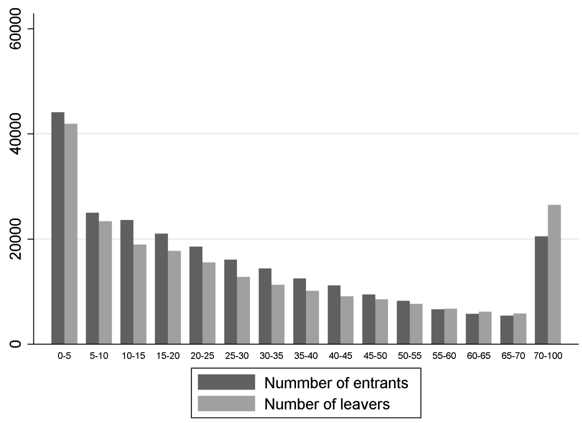
Employment effects could have influenced the presented inequality reducing effect of the minimum wage. If individuals at the bottom of the wage distribution become unemployed, wage inequality decreases among the contemporary workforce even if they have not received a rise in payments. In surveys of the existing empirical literature, the employment effect of the German minimum wage is assessed, if anything, to be modest (Bruttel, 2019; Caliendo et al., 2019).[15] In our context, however, the assessment that aggregate employment effects are only modest may not be sufficient to rule out that employment changes impacted wage inequality for two reasons: First, the results vary to some extent across the presented studies, which report employment effects between zero (Ahlfeldt et al., 2018; Garloff, 2019) and an employment reduction of 200,000 jobs (Schmitz, 2019). While the former suggests that employment did not respond at all, the latter may be substantial if concentrated at specific parts of the wage distribution. Second, even in the absence of overall employment effects, employment dynamics could influence our results if individuals enter and leave employment at different points of the wage distribution. In fact, the literature suggests that there are such employment dynamics since the number of minijobs was slightly reduced while regular employment experienced an expansion (Garloff, 2019).

[15] Also for our regression sample, the employment effect obtained from a regional-level difference-in-differences regression is economically and statistically insignificant (results available upon request).

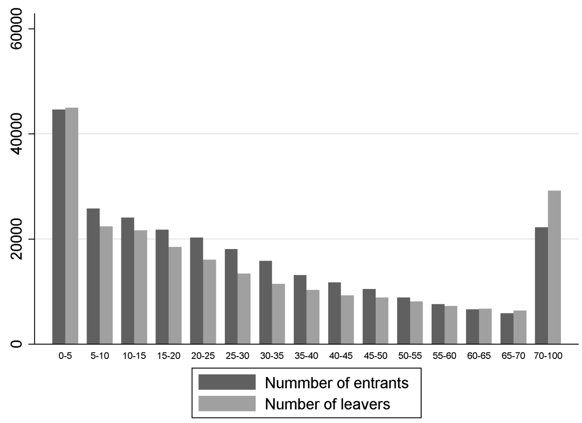
In this section, we address the role of employment dynamics. We descriptively look at the position of entrants into employment and leavers out of employment along the wage distribution, separately for before and after the minimum wage was introduced. We also estimate effects for the hypothetical scenario that all entrants and leavers stayed in employment during our period of analysis by imputing the respective individuals' wages. Finally, we identify effects on the wage distribution of the existing workers applying fixed effects estimation.

6.1. Entrants and leavers across the wage distribution

We first report the entrants and leavers along the wage distribution before and after the minimum wage was introduced. Thereby, we assess whether employment dynamics, i.e. entering and leaving employment, is selective across the wage distribution. To locate entrants and leavers in the wage distribution, we classify these individuals by bins covering five percentage points of the unconditional wage distribution. To ensure that these five-percentage point bins are not influenced by entrants and leavers themselves, we use bins that are calculated for each year separately using the individuals who stayed in employment. Hence, we evaluate the location of entrants and leavers at the unconditional distribution of stayers. To classify the leavers of a particular year, we set the wage of leavers one year forward, allowing us to compare individuals that entered employment in the last year (entrants) with individuals that leave employment in the next year (leavers). Hence, we compare entrants with leavers if they would have stayed in the same job.



(a) Before the minimum wage introduction, 2012–2014



(b) After the minimum wage introduction, 2015-2017

Figure 8: Number of entrants and leavers along five-percentage point bins of the monthly wage distribution of stayers. The number of stayers in each bin was 75,750 before the minimum wage was introduced and 78,310 after the minimum wage was introduced.

Figure 8 illustrates the number of entrants and leavers in each bin, where Figure 8a includes the last three years prior to the minimum wage introduction and Figure 8b includes the years after the minimum wage introduction. In both periods, the numbers of entrants and leavers are much larger at the bottom of the wage distribution, indicating higher employment dynamics at the bottom of the wage distribution, where the first bin covering the 5 percent lowest wages shows particularly strong employment dynamics. The graphs also illustrate that entries in employment (on average) tend to be lower in the wage distribution than exits. This pattern could be a life-cycle effect implying that individuals receive relatively lower entry wages and leave employment at a higher wage.

Most importantly for our analysis, we do not observe a visible difference in employment dynamics between both figures, implying that the patterns of entries and exits remain constant before and after the minimum wage introduction. This is indicative that the minimum wage did not have a substantial impact on employment dynamics along the wage distribution. Nevertheless, this aggregate time-invariance in employment dynamics might mask regional variation, which we investigate below.

6.2. Imputation of counterfactuals for entrants and leavers

Even in the absence of aggregate changes in employment dynamics during the years of analysis, the minimum wage could still affect employment dynamics differently through regional variation. Therefore, we return to the difference-in-differences framework to analyze the effect of the minimum wage on the wage distribution in scenarios in which entrants and leavers would have been employed throughout the whole analysis window. We follow Brüll and Gathmann (2018) and impute wages of all leavers out of employment and entrants in employment to construct a hypothetical wage distribution absent of dynamics in and out of employment.

We use two approaches to impute wages of entrants and leavers. First, we use a simple myopic procedure writing back the first observed (real) wage of entrants and writing forward the last observed (real) wage of leavers. Second, we impute the wages of leavers and entrants from the wage development of all stayers from within the same sub-labor market region.

Regarding the second procedure, we use the following predicting equations to impute log wage w of leaver i who has been working in labor market region r:

$$\hat{w}_{i(r),t} = \hat{\alpha}_{L1,r} + \hat{\beta}_{L1,r} * w_{i,t-1} + \hat{e}_{i(r),t} \quad \textit{if } i \textit{ left in } t \tag{8}$$

$$\hat{w}_{i(r),t} = \hat{\alpha}_{L2,r} + \hat{\beta}_{L2,r} * w_{i,t-2} + \hat{e}_{i(r),t} \quad \textit{if } i \textit{ left in } t-1 \tag{9}$$

$$\vdots$$

$$\hat{w}_{i(r),t} = \hat{\alpha}_{L6,r} + \hat{\beta}_{L6,r} * w_{i,t-6} + \hat{e}_{i(r),t} \quad \textit{if } i \textit{ left in } t-5 \tag{10}$$

where $\hat{\alpha}_{L1,r} \dots \hat{\alpha}_{L6,r}$ and $\hat{\beta}_{L1,r} \dots \hat{\beta}_{L6,r}$ are obtained from the respective autoregressive regressions (8)–(10) using stayers only. The $\hat{e}_{i(r),t}$ are draws from a normal distribution with mean zero and variance equal to the residual variance from the respective stayers regression. We mirror this imputation procedure for entrants' wages, i.e. we use an imputation model conditional on future wages (instead of lagged wages) where the parameter estimates are again obtained from analogous regressions using stayers only.

The results when adding imputed wages are presented in Table 4, where the first three columns report parameter estimates for the mean of log wages, and the last three columns report parameter estimates for the variance of log wages. Columns (1) and (4) repeat our baseline specification without imputing entrants and leavers (from Table 2). Columns (2) and (5) report estimations that use data of the simple imputation, and columns (3) and (6) display

Table 4: Minimum wage effect on the unconditional distribution of log real monthly wages, entrants and leavers imputed

	(1)	(2)	(3)	(4)	(5)	(6)
Imputation	no	simple	modelled	no	simple	modelled
Dependent variable	ln(wage)	ln(wage)	ln(wage)	$RIF(\sigma^2)$	$RIF(\sigma^2)$	$RIF(\sigma^2)$
Explanatory variables						
Bite	-1.954***	-1.494***	-1.550***	-1.432***	-1.715***	-1.358**
	(0.195)	(0.298)	(0.289)	(0.489)	(0.627)	(0.678)
Bite * trend	0.007	-0.012	-0.010	-0.115**	-0.088***	-0.166***
	(0.028)	(0.008)	(0.014)	(0.045)	(0.017)	(0.041)
D2012 ($year = 2012$)	0.011***	0.017***	0.036***	-0.003	0.010***	0.013***
	(0.004)	(0.001)	(0.002)	(0.006)	(0.002)	(0.005)
D2013	0.022***	0.035***	0.078***	-0.012	0.013***	0.008
	(0.008)	(0.002)	(0.004)	(0.012)	(0.004)	(0.010)
D2014	0.031***	0.056***	0.115***	0.000	0.031***	0.022
	(0.008)	(0.004)	(0.006)	(0.013)	(0.008)	(0.017)
D2015	0.037***	0.076***	0.146***	0.016	0.042***	0.040
	(0.014)	(0.006)	(0.010)	(0.029)	(0.013)	(0.027)
D2016	0.059***	0.111***	0.203***	0.011	0.039**	0.013
	(0.016)	(0.008)	(0.013)	(0.030)	(0.018)	(0.033)
D2017	0.070***	0.143***	0.263***	0.001	0.026	-0.008
	(0.016)	(0.010)	(0.014)	(0.028)	(0.025)	(0.039)
Bite * D2014	0.091*	0.057***	0.077***	-0.095	-0.088**	-0.145*
	(0.048)	(0.012)	(0.019)	(0.085)	(0.035)	(0.078)
Bite * D2015	0.354***	0.212***	0.349***	-0.576***	-0.284***	-0.688***
	(0.095)	(0.027)	(0.035)	(0.188)	(0.067)	(0.116)
Bite * D2016	0.355***	0.230***	0.325***	-0.505***	-0.269***	-0.543***
	(0.114)	(0.041)	(0.051)	(0.186)	(0.104)	(0.150)
Bite * D2017	0.438***	0.254***	0.337***	-0.490***	-0.215	-0.471**
	(0.134)	(0.059)	(0.071)	(0.183)	(0.154)	(0.212)
Observations	4,154,228	6,144,838	6,144,838	4,154,228	6,144,838	6,144,838
Cluster	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (4)-(6), the dependent variable is the RIF of the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * $p<0.1$, ** $p<0.05$, and *** $p<0.01$. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

estimates using data in which wages of entrants and leavers are imputed based on the wage development of stayers. Note that the sample size has increased by 50% through adding the imputed wages of joiners and leavers.

The average minimum wage effects based on the simple imputation (column 2) are significantly smaller than the baseline wage effects (column 1). This difference in average wage effects is not surprising since the simple imputation provides a wage effect which is mechanically downward biased. It assumes that leavers would not have received a wage effect if they stayed in employment. Relatedly, according to the first imputation procedure entrants would also not have received a wage effect if they entered employment at the beginning of the period. Therefore, a considerable number of individual observations has been added for which wages did not change when the minimum wage was introduced, implying an attenuation of the wage effect. Nevertheless, even this extreme imputation procedure yields a considerable positive minimum wage effect resulting in a reduction of wage inequality (column 5). In 2017, the inequality reduction falls short of (statistical) significance, which could be due to a reduction in wage variation over time since wages are written forward until 2017, i.e. for some workers by as much as six years.

The average wage effect of the regression-based imputation (column 3) is very similar compared with the baseline, i.e. the wage increase would have been similar if entrants in employment and leavers out of employment would have remained throughout while experiencing the same wage development as stayers. The result is also very similar when looking at the variance of log wages (column 6). Since the number of entrants and leavers may vary across regions and time, the number of imputed wages may vary and could thereby affect the results. Instead, the similarity of findings suggests that entrants and leavers are not selectively different from stayers (when comparing different labor market regions).

6.3. Fixed effect estimation

We finally address employment dynamics by adding worker-level fixed effects to our baseline specification (4). In a second step, we allow for match effects by including job-cell fixed effects for unique worker-plant combinations (instead of worker-level fixed effects).

Applying worker fixed effects estimation identifies an effect of the minimum wage on inequality only from variation within individuals. Hence, it presents an effect of the minimum wage on the existing employees and excludes relative effects between two years that are due to a changing employee composition. However, wage changes of movers between employers add to the identification of the effects. These wage changes do not play a role when we control for job-cell fixed effects, which identify the parameters only from wage developments within existing jobs, i.e. within worker-firm-matches.

The treatment effects of the worker fixed effect estimations are presented in columns (1)–(10) of Table 5 and the effects of the job-cell fixed effect estimations are presented in columns (11)–(20) of Table 5.[16] The worker fixed effect estimations yield parameter estimates of similar effect size as the baseline, reported in Table 2. However, when controlling for time-invariant worker heterogeneity the minimum wage effect in 2016 and 2017 on the variance is slightly larger than for the baseline (column 10). This is consistent with a less beneficial treatment effect at the upper part of the distribution. For the fixed effects specification, we observe zero effects at the 60% percentile (column 8) and negative effects at the 70% percentile of the wage distribution (column 9), whereas in Table 2 the respective coefficients are zero (column 8) respectively less negative (column 9).

[16]Full regression results including all the control variables are presented in Appendix H.

Table 5: Minimum wage effect on the unconditional distribution of log real monthly wages, controlling for fixed effects

	Worker fixed effects									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite * D2014 ($year = 2014$)	0.061***	0.176	0.033	0.429***	0.096*	0.151***	0.056**	0.013	-0.038**	-0.144***
	(0.015)	(0.167)	(0.043)	(0.098)	(0.051)	(0.030)	(0.023)	(0.026)	(0.016)	(0.052)
Bite * D2015	0.288***	1.002***	0.243***	1.415***	0.718***	0.382***	0.133***	0.006	-0.093***	-0.586***
	(0.025)	(0.192)	(0.073)	(0.192)	(0.107)	(0.054)	(0.039)	(0.050)	(0.030)	(0.065)
Bite * D2016	0.304***	0.913***	0.186**	1.495***	0.827***	0.591***	0.201***	0.008	-0.140***	-0.630***
	(0.037)	(0.254)	(0.082)	(0.248)	(0.114)	(0.070)	(0.056)	(0.072)	(0.045)	(0.087)
Bite * D2017	0.342***	0.842**	0.197**	1.619***	1.099***	0.766***	0.294***	0.004	-0.201***	-0.616***
	(0.054)	(0.366)	(0.100)	(0.331)	(0.145)	(0.099)	(0.079)	(0.093)	(0.059)	(0.129)
	Job-cell fixed effects									
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite * D2014 ($year = 2014$)	0.040***	0.081	0.012	0.348***	0.086**	0.127***	0.053**	0.029	-0.034**	-0.093**
	(0.014)	(0.114)	(0.042)	(0.085)	(0.039)	(0.033)	(0.023)	(0.025)	(0.014)	(0.037)
Bite * D2015	0.258***	0.691***	0.180**	1.227***	0.692***	0.366***	0.148***	0.047	-0.062**	-0.470***
	(0.020)	(0.165)	(0.075)	(0.147)	(0.096)	(0.048)	(0.032)	(0.045)	(0.025)	(0.053)
Bite * D2016	0.278***	0.492**	0.102	1.235***	0.789***	0.583***	0.247***	0.090	-0.068**	-0.466***
	(0.027)	(0.231)	(0.090)	(0.189)	(0.096)	(0.054)	(0.045)	(0.062)	(0.034)	(0.063)
Bite * D2017	0.358***	0.589**	0.128	1.454***	1.082***	0.776***	0.367***	0.123	-0.093**	-0.494***
	(0.032)	(0.269)	(0.110)	(0.232)	(0.122)	(0.072)	(0.062)	(0.080)	(0.043)	(0.086)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50	50	50	50	50	50	50

Notes: Treatment effects from treatment effect interactions controlling for worker fixed effects in specifications (1)-(10) and controlling for job-cell fixed effects in specifications (11)-(20). Time effects, time-constant group effect and group-specific trend are included in all specifications. Dependent variables are depicted by column titles. In columns (2)-(10) as well as (12)-(20), the dependent variable is the RIF calculated for various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

The job-cell fixed effect estimation yields very similar effects compared to the baseline and a lower inequality reduction compared with the worker fixed effects estimation. The minimum wage effects are more pronounced at the bottom part and less beneficial at the top part of the distribution when controlling for worker, but not for job-cell fixed effects, leading to a smaller (but still considerable) variance reduction of the latter.

The results of this section corroborate a meaningful positive effect of the minimum wage effect on wages at the bottom half of the distribution even among existing workers, leading to a reduction of the variance of log wages. The effect of the minimum wage is not explained by employment dynamics.

7. Social benefits as an additional source of income

In this section, we add social benefits as an additional income source of employees. It addresses the hypothesis that social benefits could be crowded out by wage effects of the minimum wage.

In Germany, the working poor are eligible to claim social benefits even if they are in regular employment, but eligibility and the total value of benefits depend on the household context (Bruckmeier & Wiemers, 2018). First, it depends on the type of household, i.e., the number of individuals that have to make a living from the labor income. Second, it depends on the total income of the household including wages and other non-labor income of all household members. Third, it depends on the household's endowments, i.e., poor households have to consume previous savings before they become eligible for social benefits.

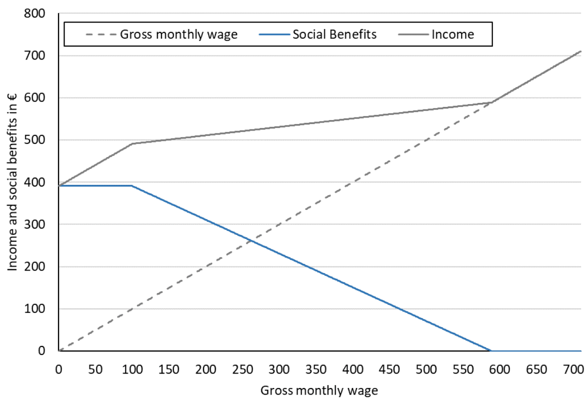


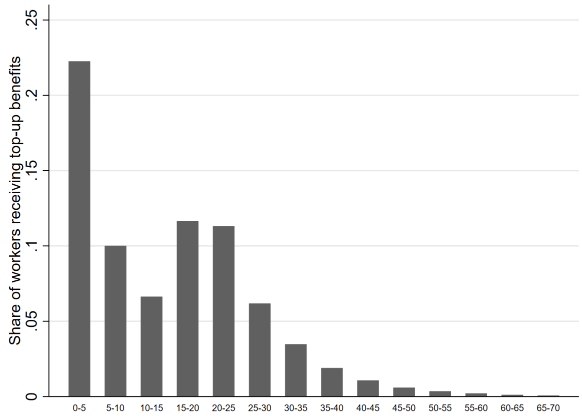
Figure 9: Schematic illustration of social benefit eligibility conditional on the gross monthly wage of a one-person household without children.

Figure 9 shows, for a single person household without children, a very schematic relationship between gross monthly wages, social benefits and income, defined as the sum of wages and benefits.[17] It is very schematic because it ignores housing-related benefits, and by definition of single person households, it also neglects child-specific benefits. Since the administrative data provide information on gross wages before taxes and social security contributions, the illustration also abstracts from these two income reducing dues.[18] For our analysis, the figure

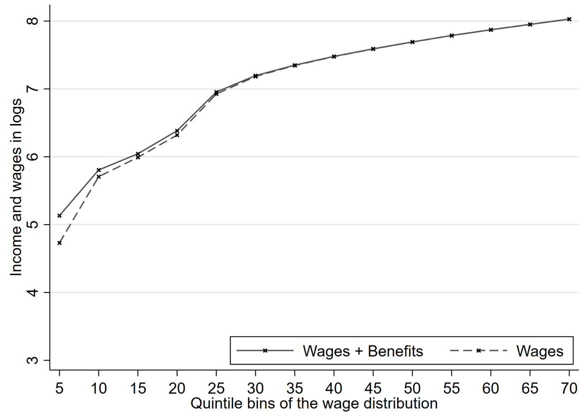
[17]The first €100 of wages are unaffected by any benefit deductions. For each additional Euro of wages, €0.8 are subtracted from social benefits.

[18]In 2017, social security contributions phase in at the minijob threshold of €450 per month and unmarried persons

demonstrates that individual incomes at the bottom of the wage distribution rely much more heavily on social benefits than incomes further up in the wage distribution.



(a) Likelihood of benefit recipiency by five-percentage point wage bins



(b) Value of benefits and wages by five-percentage point wage bins

Figure 10: Benefit recipiency by (gross) real monthly wage before the minimum wage was introduced, 2011-2014.

Figure 10 illustrates benefit recipiency in the observed IEB data. It shows the likelihood of receiving benefits (10a) and the income from wages and benefits (10b) by five-percentage point bins of the wage distribution. As intended, it demonstrates that benefit recipiency is negatively correlated with wages. Moreover, the likelihood to receive benefits largely mirrors the treatment effect pattern in regard to wage inequality (see Table 2), suggesting that the wage effect of the minimum wage could indeed crowd out benefit receipt instead of exerting an effect on the individuals' income.

Table 6 contrasts the minimum wage effect on log wages (thereby replicating the baseline results reported in Table 2) with those on income (wages plus benefits). Columns (1) and (2) report the respective results for the mean, while columns (3) and (4) report the respective results for the variance. While the point estimates of minimum wage effects slightly decrease when moving from log wages to log income, the findings for log income are still very close to the baseline, suggesting that benefits barely interfere with the positive wage effect of the minimum wage.

While it might be surprising at first sight that results hardly change when looking at income including social benefits, it is still plausible since not all low wage earners received benefits before the minimum wage introduction (Figure 10). This can be due to low take-up of social benefits (Bruckmeier & Wiemers, 2012; Riphahn, 2001) or household structures that make benefit recipients different from individuals that receive the minimum wage. This latter argument indirectly confirms that individuals in poverty are only indirectly addressed by the minimum wage, thereby confirming previous literature that discusses the effectiveness of the minimum wage to reduce poverty (Backhaus & Müller, 2019).

have to pay income taxes on wages starting slightly above €1,000 per month.

Table 6: Minimum wage effect on the unconditional distribution of log real monthly income including benefits

Income definition	(1) wages	(2) income (=wages+benefits)	(3) wages	(4) income (=wages+benefits)
Dependent variable	ln(.)		$RIF(\sigma^2)$	
Explanatory variables				
Bite	-1.954***	-1.492***	-1.432***	-2.936***
	(0.195)	(0.209)	(0.489)	(0.517)
Bite * trend	0.007	-0.025	-0.115**	0.027
	(0.028)	(0.026)	(0.045)	(0.047)
D2012 ($year = 2012$)	0.011***	0.013***	-0.003	-0.012**
	(0.004)	(0.004)	(0.006)	(0.005)
D2013	0.022***	0.027***	-0.012	-0.035***
	(0.008)	(0.007)	(0.012)	(0.012)
D2014	0.031***	0.037***	0.000	-0.031***
	(0.008)	(0.007)	(0.013)	(0.011)
D2015	0.037***	0.051***	0.016	-0.045**
	(0.014)	(0.012)	(0.029)	(0.023)
D2016	0.059***	0.075***	0.011	-0.056**
	(0.016)	(0.014)	(0.030)	(0.023)
D2017	0.070***	0.089***	0.001	-0.076***
	(0.016)	(0.014)	(0.028)	(0.023)
Bite * D2014	0.091*	0.106**	-0.095	-0.162*
	(0.048)	(0.047)	(0.085)	(0.087)
Bite * D2015	0.354***	0.312***	-0.576***	-0.469***
	(0.095)	(0.081)	(0.188)	(0.170)
Bite * D2016	0.355***	0.325***	-0.505***	-0.472**
	(0.114)	(0.101)	(0.186)	(0.192)
Bite * D2017	0.438***	0.395***	-0.490***	-0.461**
	(0.134)	(0.118)	(0.183)	(0.219)
Constant	7.672***	7.653***	1.281***	1.347***
	(0.028)	(0.028)	(0.063)	(0.063)
Observations	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

8. Prediction of counterfactuals and effect size

In this section, we discuss the effect size of the estimates and assess the importance of the minimum wage for the development of wage inequality. Using the parameter estimates of Equation (4), we calculate counterfactuals for the scenario without minimum wage introduction and compare it with the observed wage distribution (with minimum wage). This exercise allows us to calculate the relative decrease of the variance (of log wages) that is due to the minimum wage introduction and, therefore, to quantify the contribution of the minimum wage to the overall decline in wage inequality. Moreover, it allows us to present counterfactual predictions regarding the development of wage inequality if the minimum wage was introduced at a different level. Finally, we can discriminate between different effects along the wage distribution, allowing us to calculate the contribution of direct minimum wage effects and the contribution of spillovers that are paid beyond the required minimum wage to the observed decline in wage inequality.

We start by comparing the minimum wage scenario with the counterfactual absent of the minimum wage introduction. Hence, we use parameter estimates reported in Table 2 and calculate two predictions for the variance of log wages. The following equation predicts the minimum wage scenario, where the bite of the treatment effect interaction is the (average) observed bite:

$$\hat{\sigma}_t^2(with\ mw) = \hat{\delta}_t * bite_r * year_t + \hat{\gamma}_t * year_t + \hat{\phi} * bite_r + \hat{\pi} * bite_r * trend_t \tag{11}$$

The scenario absent of the minimum wage is predicted as follows:

$$\hat{\sigma}_t^2(w/o\ mw) = 0 + \hat{\gamma}_t * year_t + \hat{\phi} * bite_r + \hat{\pi} * bite_r * trend_t \tag{12}$$

While the treatment effect interaction is set to be zero, this equation still conditions on the bite-specific time trend as well as the bite-specific level effect.[19]

Figure 11 shows the predictions $\hat{\sigma}^2$ of the scenarios with and without the minimum wage introduction as well as the observed variance of log wages for each year. It is evident that using the estimates of a difference-in-differences specification for the RIF of the variance yields an accurate prediction of the unconditional variance, as both lines almost overlap. Corresponding to the estimation results, after the law came into force, the predicted variance for the minimum wage scenario is lower than the variance of the counterfactual absence of the minimum wage introduction. The graph also illustrates that even in the counterfactual situation absent a minimum wage introduction, the variance of log wages would have decreased between 2014 and 2017, but the decline would have been much smaller in size.

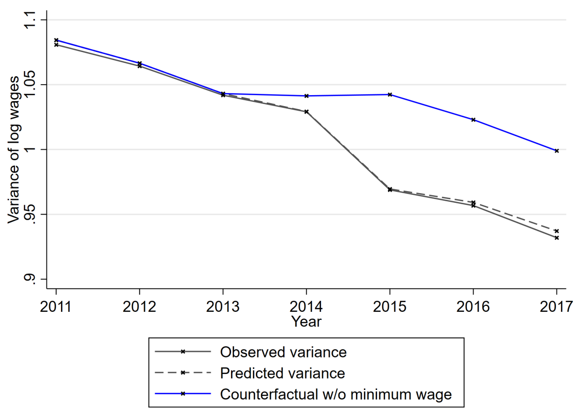


Figure 11: Development of the observed variance of log real monthly wages and of the predicted variances of log real monthly wages. The predicted variances have been calculated according to Equations (11) and (12), i.e. with and without the introduction of the minimum wage.

The corresponding numbers of the predicted scenarios are presented in Table 7. The upper

[19] For the definition of counterfactuals based on potential outcomes of the difference-in-differences, see also Puhani (2012).

part of the table reports the predicted scenarios with and without the minimum wage (columns 1 and 2 respectively) and the relative difference between both scenarios in column (3). It can be seen that the relative decline of the variance due to the minimum wage ranges between 6.6 and 7.5 percent in the years 2015-2017.

Table 7: Size of explained decrease in variance of log real monthly wages

	(1) predicted scenario with mw	(2) counterfactual scenario absent mw	(3) relative difference between prediction and counterfactual [(2)-(1)]/(2)
Variance of log wages (levels):			
2011 (first year in sample)	1.084	1.084	0
2012 (pre mw legislation)	1.067	1.067	0
2013 (pre mw legislation)	1.043	1.043	0
2014 (pre mw introduction)	1.029	1.041	1.2 %
2015 (post mw introduction)	0.970	1.042	7.5 %
2016 (post mw introduction)	0.959	1.023	6.6 %
2017 (post mw introduction)	0.937	0.999	6.6 %
Variance of log wages (differences):			
2017-2011 difference	-0.147	-0.085	42.0 %
2017-2012 difference	-0.129	-0.067	47.8 %
2017-2013 difference	-0.106	-0.044	58.4 %
2017-2014 difference	-0.092	-0.042	54.1 %

Notes: The first column displays the predicted scenario (with minimum wage) as specified in Equation (11). The second column displays the counterfactual scenario (absent minimum wage) as specified in Equation (12). The third column displays the relative differences, which is the relative effects size evaluated at the counterfactual scenario absent of the minimum wage introduction. The upper part of the table describes predicted and counterfactual levels of the log real monthly wage variance and the lower part describes predicted and counterfactual developments of the log real monthly wage variance.

The lower part of Table 7 reports differences through time of the predicted variances, where the years before the minimum wage introduction are compared to the year 2017. In the observed minimum wage scenario, the variance has been reduced between 0.092 and 0.147. In the counterfactual scenario absent of the minimum wage introduction, the variance would have declined by much less. Column (3) shows that the relative contribution of the minimum wage to the decline of the minimum wage ranges between 42 and 58 percent, depending on the year of reference. Since wage inequality has already decreased before the minimum wage introduction, the relative contribution of the policy is the largest when we use 2013 or 2014 as a reference, which is when the pre-treatment decrease in the variance of log wages has already materialized.

8.1. Prediction of inequality effects if the minimum wage was set at a higher level

We next conduct counterfactual predictions if the minimum wage was introduced at a different level. Again, we use the difference-in-differences results to predict various scenarios for minimum wages (hypothetically) set between 0 and €10:

$$\hat{\sigma}_t^2(bite_r|minimum\ wage{=}€x) = bite_r * year_t * \hat{\delta}_t + year_t * \hat{\gamma}_t + bite_r * \hat{\phi} + bite_r * trend_t * \hat{\pi} \quad (13)$$

Equation (13) yields a prediction of the variance when inserting the bite that would have applied if the minimum wage had been set at another level. For validity of these predictions, we have to assume homogeneity in the minimum wage effect (i.e. the coefficient estimates obtained from the actual minimum wage of 8.50€ remain unchanged when the minimum wage increases to a higher hypothetical level), which in turn allows us to extrapolate the effect to different bite levels. Moreover, we have to assume that the employment effects would not differ from the observed scenario, i.e., the inequality effect of the hypothetical minimum wages would not be compensated by a different employment response.

Table 8: Predicted variance of log real monthly wages in 2017 if the minimum wage was set at a different level, absent of employment adjustments that are different to the baseline

(1) Minimum wage	(2) Respective bite	(3) Predicted variance	(4) Implied relative decrease
€0	0	0.999	0 (*reference*)
€1	0.003	0.998	-0.1 %
€2	0.008	0.995	-0.4 %
€3	0.016	0.991	-0.8 %
€4	0.025	0.987	-1.2 %
€5	0.035	0.982	-1.7 %
€6	0.049	0.975	-2.4 %
€7	0.072	0.964	-3.5 %
€8	0.105	0.948	-5.1 %
€8.50 (baseline)	0.126	0.937	-6.6 %
€9	0.147	0.927	-7.2 %
€10	0.195	0.904	-9.5 %

Notes: The first column displays the level of the minimum wage (in 2014 €) that is used to predict the implied effect on the variance. The second column displays the bite of the respective minimum wage level. The third column displays the predicted variance. The fourth column displays the implied relative decrease of the variance relative to the counterfactual scenario in the absence of any minimum wage.

According to Table 8, the actual minimum wage introduction at the level of €8.50 decreased the variance by 6.6 percent. If the minimum wage was introduced at a much lower level of €5, wage inequality would have decreased by a much smaller margin. This is due to the fact that only 3.5 percent of workers received wages below €5 in 2014 (column 2). The bite picks up just below the implemented minimum wage level (€8.50). Hence, this is also when the inequality reducing effect accelerates. In turn, if the minimum wage was introduced at a higher level of €10, the inequality reducing effect would have almost doubled. This is because lots of workers are located in this part of the wage distribution. However, this scenario does not account for potential (negative) employment effects of a higher minimum wage. In fact, the literature is skeptical that employment effects would remain modest (Bossler, Oberfichtner, & Schnabel, 2018).

8.2. Prediction of inequality effects in the absence of spillovers

According to the baseline estimations, the minimum wage has affected the wage distribution up to the 50th percentile, although a full-time minimum wage worker has been located at the 30th percentile (see Figure 2). This points at spillover effects along the wage distribution. In a final counterfactual exercise, we aim to quantify the role of these spillovers for the decline in wage inequality, measured by the variance of log wages. For this purpose, we define spillovers as wage effects that go beyond the 30th percentile of the monthly wage distribution.[20] Hence, we assess the size of the overall inequality decline that is explained by effects above the 30th percentile of the wage distribution.

We estimate difference-in-differences based treatment effects for each percentile in the unconditional wage distribution from RIF-regressions (separately for each percentile). Using these estimates, we predict four counterfactuals:

1. *Scenario absent of minimum wage*: The scenario absent of any minimum wage effect is calculated by subtracting the treatment effect of all percentiles from the unconditional wage distribution.

2. *Observed scenario with minimum wage introduction*: For each year, the variance is simply calculated from observed data.

3. *Scenario with direct effects, but absent of spillovers*: The scenario absent of any spillovers is calculated by subtracting the treatment effects for all percentiles above the 30th percentile from the unconditional (observed) wage distribution.

4. *Scenario without direct effects, but spillovers only*: The scenario that captures only spillovers is calculated by subtracting the treatment effect at or below the 30th percentile from the observed data, leaving only effects beyond the 30th percentile in the unconditional wage distribution.

The variance of log wages in 2017 is calculated for each of the four scenarios after adjusting the wage distribution respectively.

The first two rows of Table 9 display the variance of log wages for the two scenarios with and without minimum wage. It demonstrates that the percentile-by-percentile adjustment of the wage distributions yields very similar results as presented in Tables 7 and 8, i.e. the minimum wage reduced the variance by about 7 percent.

Rows three and four of Table 9 display the contribution of direct minimum wage effects and of wage spillovers beyond the 30th percentile. The minimum wage introduction reduced the variance by 7 percent through effects that affect the wage distribution at and below the 30th percentile. Surprisingly, effects of the minimum wage further up in the wage distribution do not contribute to the declining variance in log wages. This finding may be explained by the fact that the variance is not responsive to wage changes in the middle of the wage distribution.

[20] This definition of spillovers neglects on the one hand that part-time workers could be paid spillovers below the 30th percentile. On the other hand overtime hours could (to some extent) lead to direct minimum wage effects above the 30th percentile.

Table 9: Predicted variance of real monthly wages with and without effects beyond the 30th percentile, 2017

(1) Scenario	(2) Variance	(3) Implied relative decrease
Absent minimum wage	1.002	*reference*
Observed (including all minimum wage effects)	0.932	-7.0 %
With direct effects, but absent spillovers	0.931	-7.1 %
Without direct effects, but spillovers only	1.005	+0.2 %

Notes: The first column describes the counterfactual scenario. The second column displays the predicted variance. The third column displays the implied relative decrease of the variance relative to the counterfactual scenario in the absence of any minimum wage.

9. Conclusion

We analyze the development of monthly wages in Germany between 2000 and 2017. Thereby, we expand on previous literature by adding an interesting period of labor market recovery in the 2010s and by accounting for different kinds of employment, including females and part-time workers. Most importantly, we are the first to analyze the effect of the national minimum wage introduction in 2015 on the wage distribution, while assessing a potential channel through employment dynamics along the distribution.

In line with the literature on the development of wage inequality in Germany (Dustmann et al., 2009; Card et al., 2013), we observe an increasing wage dispersion during the early 2000s. However, wage inequality has been falling since 2010 and in 2017 it is even below the level of 2000. The recent fall in wage inequality is due to rising wages at the bottom part of the wage distribution. Moreover, wage growth at the bottom varies in size across different percentiles, where wage growth is most pronounced at the 5th and the 20th percentile but it is much smaller at the 10th percentile. The latter coincides with the minijob threshold, at which social security contributions phase in. Hence, the minijob threshold may have counteracted a further wage increase in the lower part of the wage distribution. We show that the development of wage inequality is not driven by compositional changes of observable worker characteristics such as the educational expansion or an increasing labor market participation of the elderly. Hence, recent wage growth at the bottom of the distribution seems to be a genuine wage effect.

We identify the influence of the 2015 minimum wage introduction for the recent decrease in wage inequality from difference-in-differences estimation exploiting variation in the bite of the minimum wage across labor market regions. When applying our specification to the unconditional distribution of social security-relevant wages, we find that the minimum wage introduction has an impact on monthly wages up to the 50th percentile, illustrating the existence of spillovers. Thereby, the results also show a substantial reduction of the variance of log wages, which decreased by 14.7 percent after the minimum wage had been introduced and which would have only decreased by 8.5 percent in the absence of the minimum wage. We conducted numerous robustness checks that support our findings. Additional analyses show that this decrease in the variance is not driven by employment dynamics along the wage distribution. In fact, wage inequality decreased by a similar degree among incumbent employees and among employees who stayed with their employing plant. Moreover, the observed increase in labor income of individuals at the bottom of the distribution is not compensated by a reduction in social benefits. Interestingly, our counterfactual predictions demonstrate that spillovers beyond the 30th percentiles of the distribution do not contribute to the decreasing variance in log wages.

Our results are in line with previous evidence from the US that shows an increased inequality in the 1980s along with decreasing real minimum wages (Fortin & Lemieux, 1997). Our results confirm the importance of minimum wages for wage inequality exploiting variation from the German minimum wage introduction. While minimum wages may not be the first-best solution to reduce inequality within a given workforce, they are effective if set at a level that does not cause non-minor job losses. Therefore, we contribute to the literature that suggests that institutions matter for inequality in labor income (Dustmann et al., 2009; Biewen & Seckler, 2019; Brüll & Gathmann, 2018). Our conclusions should be interpreted with respect to in-market wage inequality. Of course, minimum wages do not directly address any sources of pre-market inequality (e.g., education), and we do not account for post-market redistribution through income taxes.

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Appendix A Additional description of the wage distribution

In this appendix, we show additional descriptions of the real wage distribution. Figure A1 shows yearly comparisons of the wage distribution for the years 2013/14, 2014/15, 2015/16, and 2016/17 using kernel densities. It demonstrates that the wage distribution does not follow the usual form of a normal or log-normal distribution. The shape of the wage distribution is most likely due to institutional peculiarities, in particular the existence of minijobs (and possibly high tax rates of labor income for social benefit recipients). This points to the importance of aggregated inequality measures (like the variance) which average across such institutional thresholds.

Figure A2 shows the 2014/15 comparison of the wage distribution separately for minijobs and regular jobs using histograms. It shows that the bin including the minijob threshold increases in size, while the shares of all bins below and of the first three bins above the minijob threshold have been reduced. It again demonstrates the importance of the minijob threshold.

Figure A3 displays the development of log real wages and of selected percentile differences. This replicates Figure 3 without adjusting for level differences. Level differences are relatively small between the 10th and 15th percentile, which is where the minijob threshold is located in the wage distribution.

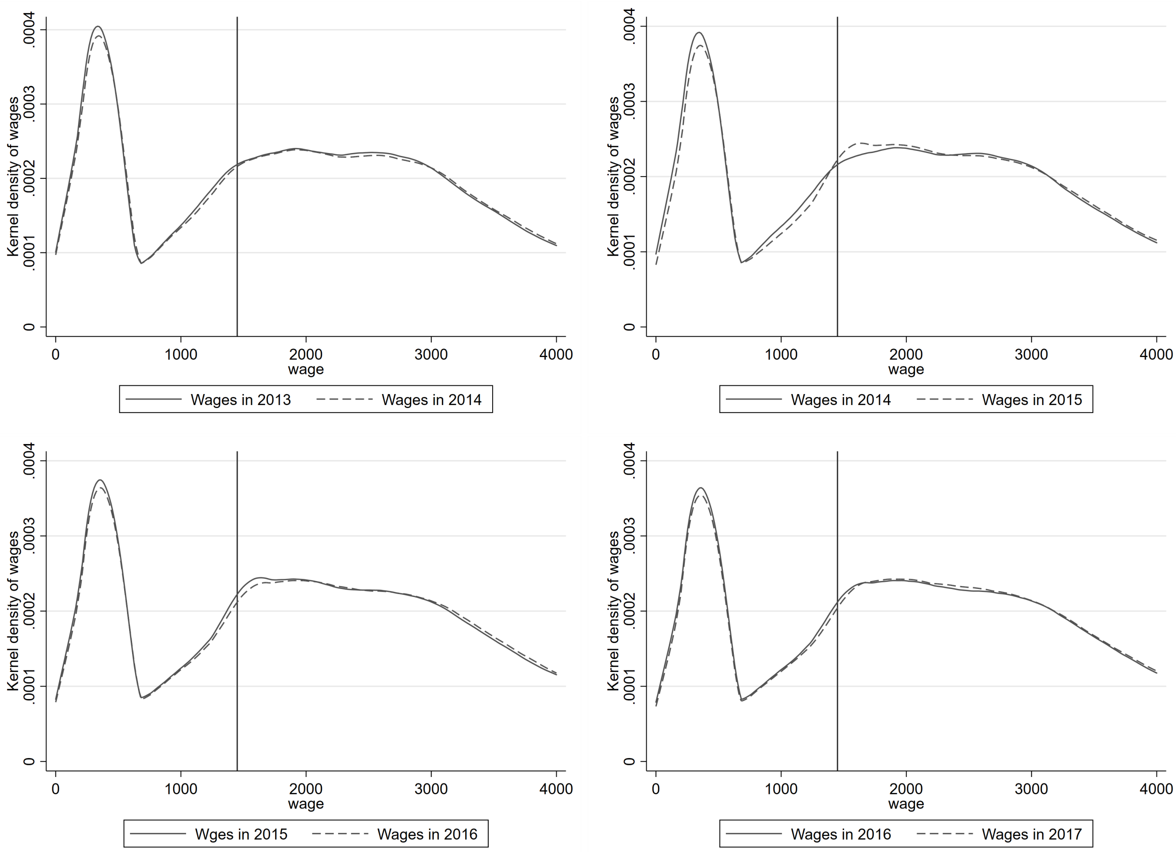

Figure A1: Pairwise yearly comparison of real monthly wage distributions of the years 2013-2017.

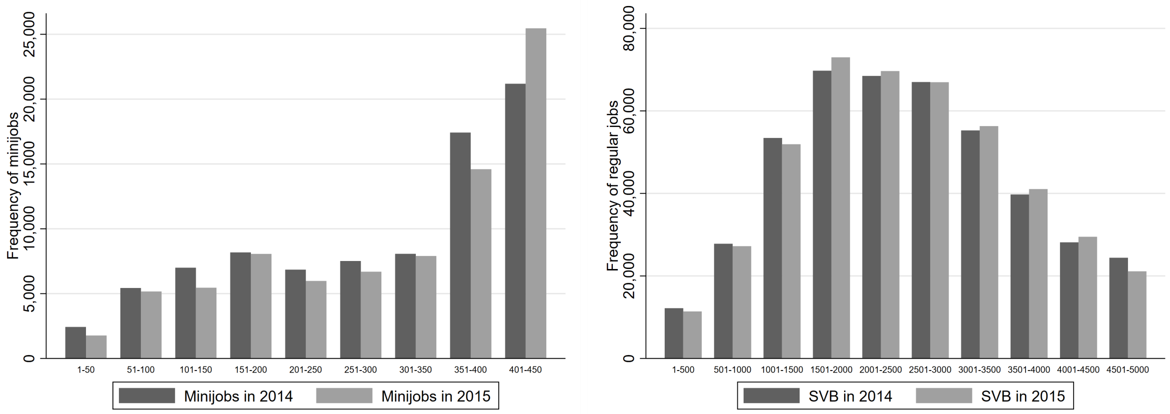

Figure A2: Histograms of real monthly wages of minijobs and regular jobs, 2014 and 2015.

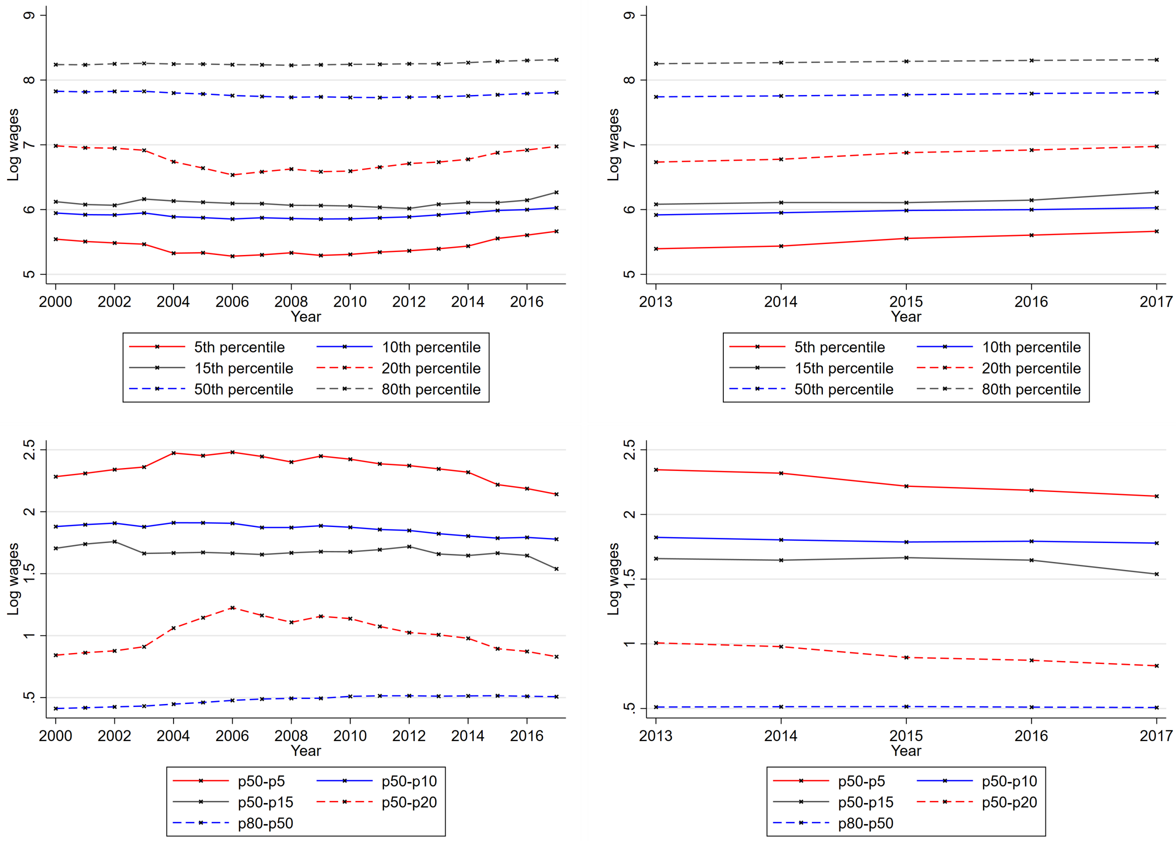

Figure A3: Levels of log real monthly wage percentiles and log-differences of real monthly wage percentiles, 2000-2017 and 2013-2017

Appendix B Decomposition details

In this appendix section, we provide additional details about the decomposition analysis presented in Section 4. Figure B4 shows the development over time of mean values of covariates that are held constant in the decomposition (note that in the decomposition, we also hold constant the interactions of the female dummy with all other covariates). While the female share of all employees remains fairly constant at about 48 percent during the period of analysis, the shares of foreigners and of elderly employees rises considerably between 2000 and 2017. Consistent with the latter, there is a slight increase in the average labor market experience. Finally, we observe an educational expansion in the workforce. These changes suggest that changes in wage inequality could be partly due to time-variation in covariates.

Figures B5 and B6 include pairwise comparisons of the densities of the propensity scores of being observed in the year 2000. The propensity scores are obtained from separate logit estimations, each based on pooled data from the year 2000 and from one of the years thereafter (2001–2017) using the following covariates, fully interacted with gender: twelve age categories, eight categories for experience and eight categories for tenure, four post-secondary degree categories and a dummy for foreign citizenship. The propensity scores have been used to compute inverse probability weights in order to control for differences in covariates between the base year 2000 and the consecutive years when constructing Figure 5. The propensity scores become increasingly different the longer the time span between the year of observation and the base year 2000. Again, this indicates that changes in observables could partly explain changes in wage inequality.

Finally, Figure B7 replicates the decomposition analysis from Section 4 using weights constructed from entropy balancing instead of inverse probability weights obtained from logit propensity score estimation. The results are very similar to those from Figure 5, which indicates that the findings of the decomposition analysis are not sensitive with respect to the methodology used to compute weights.

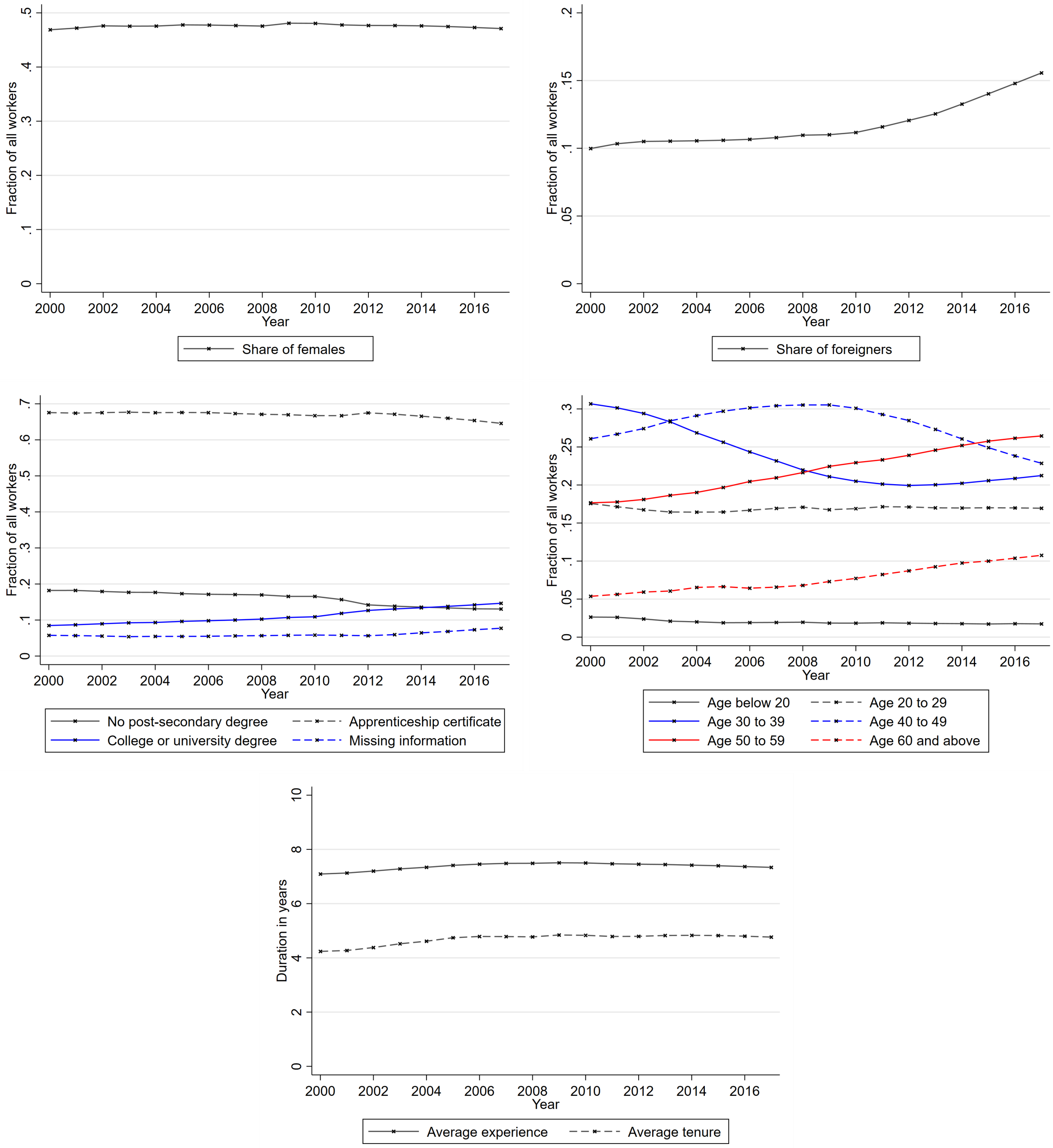

Figure B4: Covariates which are held constant in the decomposition analysis: development of mean values over time

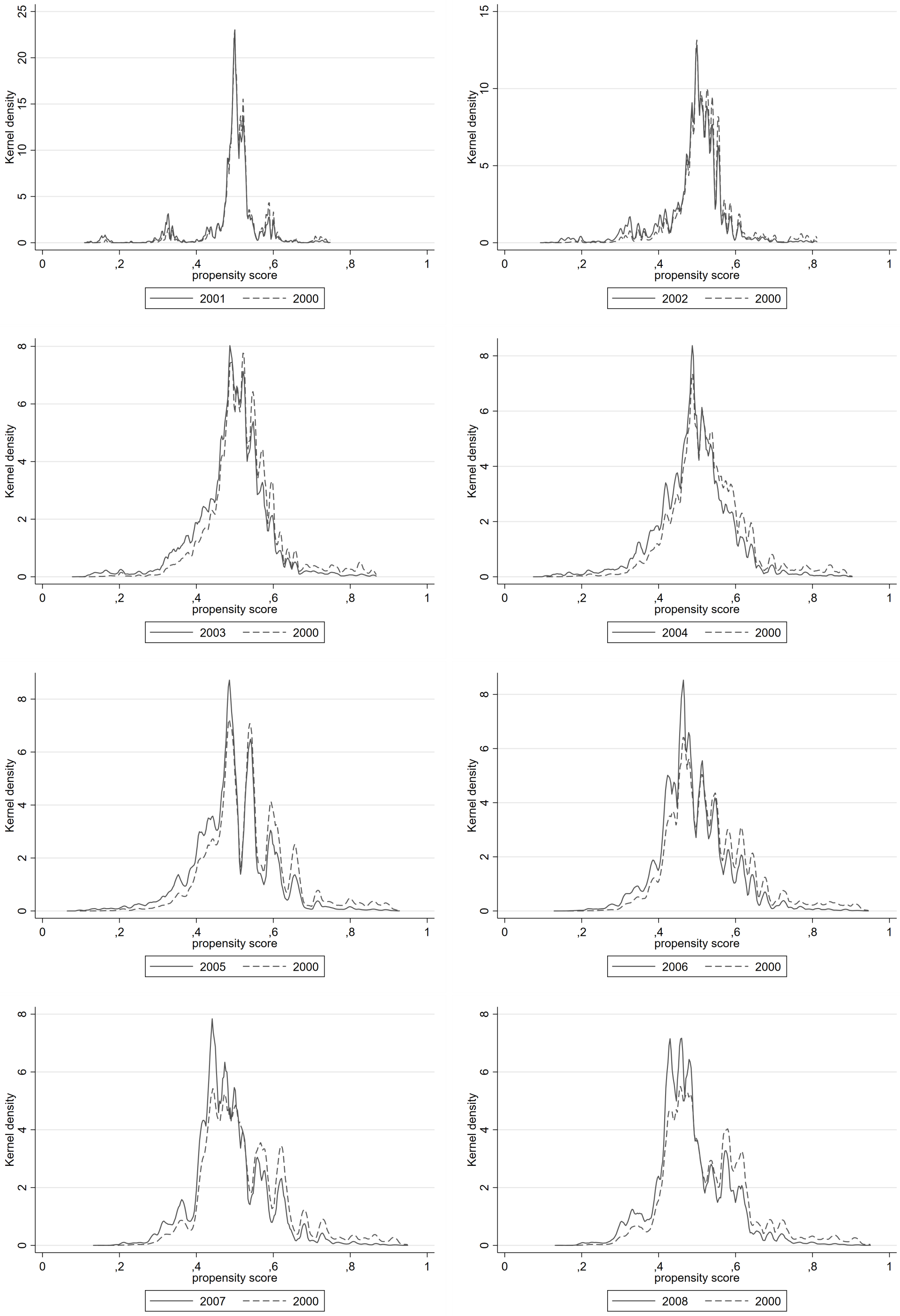

Figure B5: Propensity score densities for being observed in the year 2000, obtained from logit estimations using pooled data from the year 2000 and (for each separate estimation) one of the years thereafter (2001–2008); propensity scores are used in the decomposition analysis to construct the inverse probability weights.

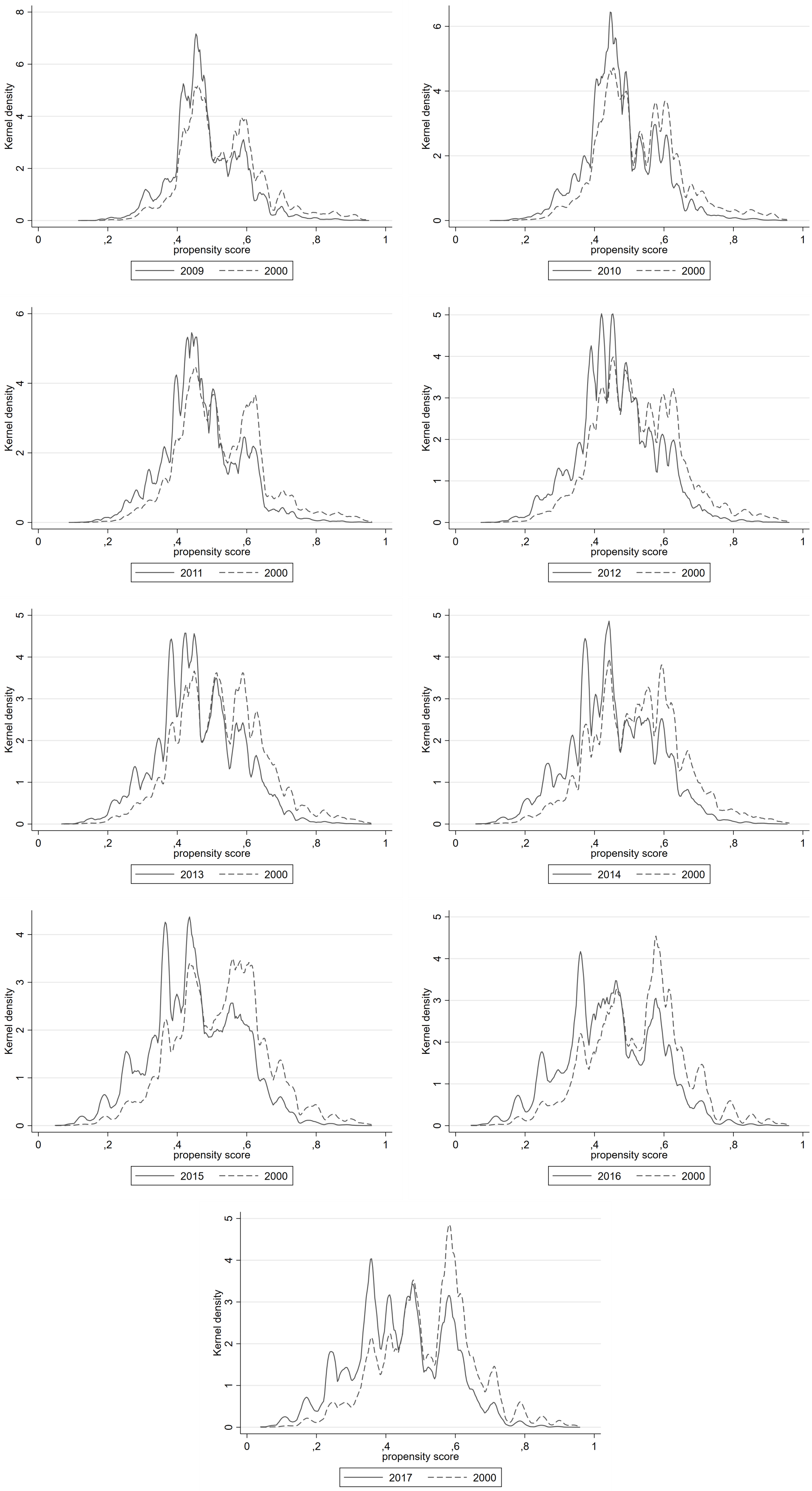

Figure B6: Propensity score densities for being observed in the year 2000, obtained from logit estimations using pooled data from the year 2000 and (for each separate estimation) one of the years thereafter (2009–2017); propensity scores are used in the decomposition analysis to construct the inverse probability weights.

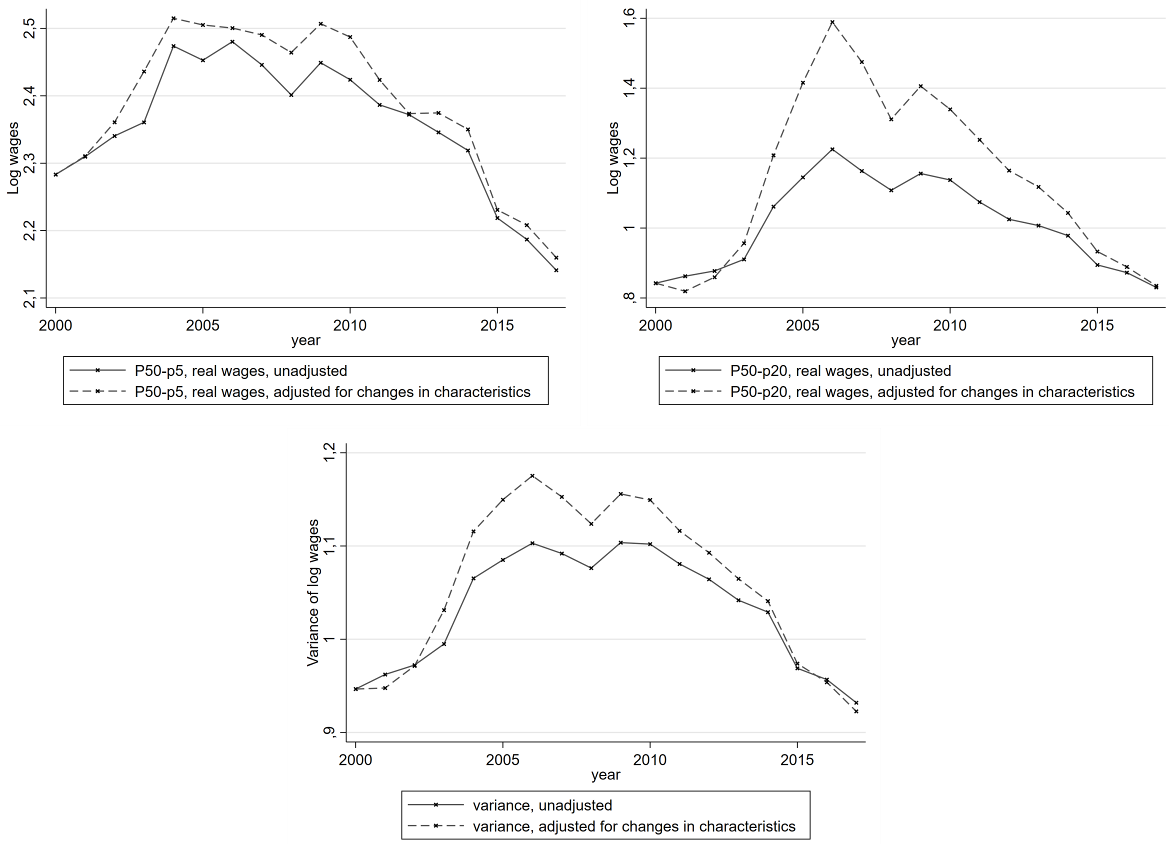

Figure B7: Decomposition of the percentile differences and the variance of log real monthly wages using entropy balancing weights instead of logit-based inverse probability weighting.

Appendix C Varying the data source to calculate the bite variable

In this appendix, we address the data source to calculate the bite variable that is used as treatment variation across labor market regions. In the baseline estimation, we use the hourly wage information available in the IEB for the period before the minimum wage was introduced. The hourly wage is calculated from information on monthly wages for each employee in the administrative employment register and from information on working hours that are reported to the obligatory injury insurance for the very same employees. It was possible to merge both data sources for the period from 2011 to 2014. Unfortunately, the hours information was no longer collected by the obligatory injury insurance after 2014.

Table C1: Comparison of wage percentiles IEB and VSE

	(1)	(2)	(3)	(4)	(5)	(6)
	monthly wage		monthly wage of FT		hourly wage	
Percentile	SES	IEB	SES	IEB	SES	IEB
1	75	81	1200	787	5.22	2.43
2	107	119	1306	1027	6.09	3.56
3	150	160	1388	1182	6.58	4.86
4	180	192	1447	1279	6.99	5.70
5	215	230	1500	1350	7.32	6.30
6	255	272	1550	1407	7.50	6.84
7	300	308	1600	1466	7.78	7.22
8	328	339	1644	1512	8.00	7.57
9	363	360	1692	1563	8.09	7.92
10	396	384	1728	1607	8.34	8.12
11	400	400	1769	1653	8.50	8.43
12	400	401	1800	1700	8.63	8.66
13	420	417	1844	1741	8.78	8.94
14	447	440	1882	1785	8.98	9.15
15	450	450	1908	1822	9.09	9.32
16	450	451	1948	1862	9.21	9.55
17	464	533	1985	1900	9.30	9.80
18	560	663	2000	1939	9.36	10.00
19	676	787	2039	1977	9.52	10.15
20	780	878	2074	2011	9.72	10.38

Notes: Comparison of wage information in the 2014 Integrated Employment Biographies (IEB) and the 2014 Structure of Earnings Survey (SES). Wages from the 1st to 20th percentile of the 2014 unconditional wage distribution. Data: IEB 2014, 2 percent sample, apprentices and internships excluded, and the SES 2014 wage information is retrieved from Destatis (2016).

Table C1 shows a comparison of the wage information included in the IEB with wage information of the Structure of Earnings Survey (SES). Columns (1) and (2) shows that the percentiles of monthly wages largely correspond at the lower end of the wage distribution. However, there is some discrepancy in monthly wages of full-time employees. Very low full-time wages seem to be slightly under-represented in the SES. Most importantly, when we compare the lowest percentiles in hourly wages, the minimum wage threshold of €8.50 is at the 11th percentile in both data sources.

We test the robustness of the results by calculating the bite from two additional data sources:

1. The full-time employees' monthly wages in the IEB
2. The employer-reported number of treated employees of the IAB Establishment Panel

Tables C2 and C3 re-estimate the baseline specification of Table 2 for the two alternative bite measures. The effects of the bite that is calculated based on full-time workers' monthly wages show virtually unchanged results (Table C2). The results based on the regionally aggregated bite of the IAB Establishment Panel are again qualitatively similar (Table C3): The wage effects are again economically meaningful and significant from the 20% percentile up to the median. In contrast to the baseline results, there is no significant effect on wages at the 5th percentile, leading to insignificant albeit negative effects on the variance of log wages. The absence of a significant effect at the 5th percentile may be explained by the exclusion of establishments that solely provide minijobs from the survey's sampling. These jobs are by definition located at the lowest end of the gross monthly wage distribution and may therefore affect the treatment effect at the lowest end of the wage distribution.

Table C2: Minimum wage effect on the unconditional distribution of log real monthly wages, full-time employees' bite

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Bite	-1.662***	-1.546***	0.516**	3.861***	-1.929***	-3.415***	-3.630***	-3.097***	-2.832***	-2.101***
	(0.255)	(0.541)	(0.233)	(1.025)	(0.401)	(0.383)	(0.232)	(0.209)	(0.253)	(0.427)
Bite * trend	0.010	0.269***	-0.006	-0.170	0.105***	0.068**	0.025	0.011	-0.010	-0.114**
	(0.026)	(0.079)	(0.026)	(0.108)	(0.038)	(0.029)	(0.022)	(0.023)	(0.017)	(0.046)
D2012 ($year = 2012$)	0.012***	0.018**	0.011***	0.057***	0.004	0.004	0.004*	0.005**	0.006***	-0.009**
	(0.003)	(0.008)	(0.004)	(0.013)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.004)
D2013	0.023***	0.031**	0.036***	0.088***	0.009	0.005	0.005	0.008*	0.010***	-0.023***
	(0.005)	(0.013)	(0.008)	(0.021)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.008)
D2014	0.036***	0.048***	0.058***	0.096***	0.006	0.002	0.011**	0.018***	0.026***	-0.021**
	(0.006)	(0.014)	(0.009)	(0.028)	(0.008)	(0.007)	(0.006)	(0.005)	(0.004)	(0.009)
D2015	0.053***	0.084***	0.083***	0.118***	-0.014	-0.000	0.019***	0.034***	0.047***	-0.037*
	(0.009)	(0.032)	(0.013)	(0.042)	(0.011)	(0.008)	(0.007)	(0.007)	(0.005)	(0.020)
D2016	0.074***	0.122***	0.103***	0.182***	-0.005	0.002	0.031***	0.048***	0.065***	-0.042**
	(0.010)	(0.034)	(0.014)	(0.052)	(0.012)	(0.010)	(0.008)	(0.007)	(0.006)	(0.020)
D2017	0.087***	0.164***	0.116***	0.239***	-0.012	-0.002	0.032***	0.053***	0.076***	-0.059***
	(0.010)	(0.034)	(0.024)	(0.056)	(0.011)	(0.011)	(0.009)	(0.008)	(0.007)	(0.020)
Bite * D2014	0.094**	0.003	0.022	0.553***	0.128*	0.175***	0.083**	0.045	0.005	-0.090
	(0.046)	(0.167)	(0.042)	(0.184)	(0.073)	(0.054)	(0.037)	(0.037)	(0.022)	(0.084)
Bite * D2015	0.367***	0.802**	0.226*	1.767***	0.832***	0.418***	0.176***	0.048	-0.020	-0.534**
	(0.095)	(0.404)	(0.135)	(0.368)	(0.118)	(0.094)	(0.059)	(0.062)	(0.036)	(0.211)
Bite * D2016	0.386***	0.594	0.175	1.751***	0.920***	0.618***	0.253***	0.072	-0.029	-0.487**
	(0.111)	(0.377)	(0.137)	(0.398)	(0.137)	(0.121)	(0.078)	(0.082)	(0.051)	(0.201)
Bite * D2017	0.489***	0.505	0.228	2.003***	1.284***	0.861***	0.402***	0.129	-0.035	-0.434**
	(0.137)	(0.355)	(0.145)	(0.510)	(0.200)	(0.163)	(0.108)	(0.107)	(0.069)	(0.208)
Constant	7.557***	5.431***	5.857***	6.379***	7.400***	7.788***	8.018***	8.148***	8.286***	1.261***
	(0.031)	(0.060)	(0.031)	(0.118)	(0.039)	(0.036)	(0.025)	(0.023)	(0.029)	(0.038)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * $p<0.1$, ** $p<0.05$, and *** $p<0.01$. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Table C3: Minimum wage effect on the unconditional distribution of log real monthly wages, IAB-EP bite

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
EP-Bite	-1.187***	-1.080	0.463	3.408**	-1.303***	-2.542***	-2.719***	-2.343***	-2.147***	-1.696***
	(0.371)	(0.678)	(0.327)	(1.611)	(0.446)	(0.593)	(0.546)	(0.490)	(0.482)	(0.496)
EP-Bite * trend	0.019	0.245***	0.001	-0.081	0.074**	0.062**	0.021	0.011	-0.005	-0.115***
	(0.022)	(0.080)	(0.027)	(0.100)	(0.036)	(0.026)	(0.019)	(0.020)	(0.009)	(0.039)
D2012($year = 2012$)	0.012***	0.029***	0.011***	0.047***	0.010***	0.007***	0.005***	0.005***	0.005***	-0.013***
	(0.002)	(0.006)	(0.003)	(0.013)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
D2013	0.023***	0.053***	0.035***	0.068***	0.019***	0.010***	0.008***	0.008***	0.009***	-0.032***
	(0.003)	(0.009)	(0.006)	(0.016)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)
D2014	0.042***	0.089***	0.059***	0.098***	0.028***	0.019***	0.019***	0.022***	0.025***	-0.042***
	(0.003)	(0.011)	(0.008)	(0.022)	(0.006)	(0.003)	(0.003)	(0.003)	(0.002)	(0.007)
D2015	0.074***	0.178***	0.094***	0.172***	0.047***	0.033***	0.033***	0.038***	0.045***	-0.085***
	(0.005)	(0.018)	(0.013)	(0.036)	(0.009)	(0.004)	(0.005)	(0.004)	(0.003)	(0.011)
D2016	0.096***	0.220***	0.111***	0.224***	0.065***	0.048***	0.049***	0.054***	0.061***	-0.093***
	(0.005)	(0.018)	(0.014)	(0.045)	(0.010)	(0.005)	(0.005)	(0.005)	(0.004)	(0.012)
D2017	0.114***	0.266***	0.125***	0.285***	0.079***	0.057***	0.058***	0.063***	0.073***	-0.111***
	(0.006)	(0.023)	(0.023)	(0.054)	(0.011)	(0.006)	(0.007)	(0.006)	(0.005)	(0.012)
EP-Bite * D2014	0.035	-0.166	0.000	0.277	0.093	0.124*	0.061*	0.036	-0.013	0.032
	(0.053)	(0.180)	(0.050)	(0.249)	(0.091)	(0.066)	(0.032)	(0.030)	(0.018)	(0.087)
EP-Bite * D2015	0.212*	0.325	0.119	1.100**	0.614***	0.270**	0.121*	0.028	-0.037	-0.257
	(0.129)	(0.435)	(0.122)	(0.457)	(0.187)	(0.122)	(0.064)	(0.055)	(0.024)	(0.231)
EP-Bite * D2016	0.217*	0.103	0.092	1.099**	0.697***	0.410***	0.176**	0.038	-0.050	-0.212
	(0.132)	(0.360)	(0.116)	(0.433)	(0.211)	(0.157)	(0.081)	(0.076)	(0.033)	(0.195)
EP-Bite * D2017	0.292*	0.097	0.132	1.252**	1.016***	0.607***	0.284**	0.068	-0.066	-0.187
	(0.163)	(0.407)	(0.128)	(0.527)	(0.246)	(0.192)	(0.112)	(0.095)	(0.045)	(0.227)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Appendix D Varying the definition of regions

In the main body of the paper we use labor market regions as defined in Kropp and Schwengler (2016). The regional definition is used to calculate the regional bite in the difference-in-differences analysis. Since the identifying variation is at the level of such labor market regions, standard errors are also clustered at that level.

The major idea to use labor market regions is to compare mostly independent labor markets that are differently treated by the minimum wage. Table D4 shows that the definition of 50 labor markets within Germany provides the best definition in this respect. Mobility across regions as well as commuting across regions is minimized in this definition compared with two alternatives, which are:

1. 105 sub-labor markets defined in Kropp and Schwengler (2016)

2. 401 "Kreise", which is an administrative definition of counties in Germany

Table D4: Description on the fractions of movers and commuters across regions

	(1) movers	(2) commuters
Across **labor markets** (50)	0.024	0.111
Across **sub-labor markets** (105)	0.031	0.168
Across **counties** (401)	0.051	0.379
Overall job mobility (across establishments)	0.097	-

Notes: Fraction of job movers across establishments (as a reference) and across the three regional definitions in column (1). Fraction of commuters across the three regional definitions in column (2).

Nevertheless, we want to test the robustness of the results w.r.t. to the two alternative regional definitions. We re-estimate our baseline regressions (as in Table 2) and calculate the bite, which is a major explanatory variable, at these regional levels. Moreover, standard errors are clustered respectively. The respective results in Tables D5 and D6 show virtually unchanged results.

Table D5: Minimum wage effect on the unconditional distribution, 105 sub-labor market regions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Bite	-2.253***	-2.878***	-0.227	0.761	-2.392***	-3.445***	-3.588***	-3.086***	-2.943***	-1.070**
	(0.313)	(0.519)	(0.305)	(1.235)	(0.432)	(0.330)	(0.178)	(0.165)	(0.227)	(0.415)
Bite * trend	0.011	0.219**	0.008	-0.122	0.091***	0.055**	0.023	0.002	-0.012	-0.115***
	(0.017)	(0.109)	(0.023)	(0.075)	(0.027)	(0.021)	(0.020)	(0.020)	(0.017)	(0.041)
D2012 ($year = 2012$)	0.011***	0.011	0.010***	0.058***	0.001	0.002	0.003	0.005*	0.006**	-0.004
	(0.003)	(0.015)	(0.004)	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)
D2013	0.021***	0.018	0.033***	0.092***	0.002	0.001	0.003	0.009	0.011**	-0.013
	(0.005)	(0.027)	(0.007)	(0.020)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)
D2014	0.032***	0.021	0.051***	0.090***	-0.009	-0.008	0.005	0.017***	0.026***	-0.005
	(0.006)	(0.027)	(0.008)	(0.028)	(0.010)	(0.009)	(0.007)	(0.006)	(0.005)	(0.012)
D2015	0.036***	-0.016	0.055***	0.071*	-0.050***	-0.020*	0.011	0.033***	0.048***	0.021
	(0.008)	(0.033)	(0.009)	(0.038)	(0.015)	(0.011)	(0.009)	(0.008)	(0.006)	(0.019)
D2016	0.059***	0.022	0.080***	0.141***	-0.043***	-0.022*	0.021*	0.046***	0.067***	0.017
	(0.009)	(0.033)	(0.010)	(0.043)	(0.014)	(0.013)	(0.011)	(0.009)	(0.007)	(0.018)
D2017	0.071***	0.043	0.089***	0.204***	-0.058***	-0.033**	0.020	0.053***	0.082***	0.008
	(0.011)	(0.033)	(0.018)	(0.052)	(0.020)	(0.015)	(0.014)	(0.010)	(0.008)	(0.019)
Bite * D2014	0.072*	0.066	0.031	0.438**	0.122**	0.144***	0.079***	0.054**	0.020	-0.053
	(0.043)	(0.211)	(0.053)	(0.203)	(0.060)	(0.043)	(0.028)	(0.025)	(0.024)	(0.085)
Bite * D2015	0.344***	1.089***	0.315***	1.543***	0.704***	0.363***	0.142***	0.058	-0.005	-0.623***
	(0.069)	(0.365)	(0.099)	(0.306)	(0.126)	(0.073)	(0.052)	(0.043)	(0.038)	(0.165)
Bite * D2016	0.335***	0.910**	0.234**	1.490***	0.750***	0.516***	0.198***	0.075	-0.017	-0.557***
	(0.082)	(0.414)	(0.106)	(0.351)	(0.128)	(0.083)	(0.071)	(0.059)	(0.053)	(0.183)
Bite * D2017	0.398***	0.964*	0.281**	1.617***	1.017***	0.703***	0.300***	0.110	-0.039	-0.545**
	(0.107)	(0.493)	(0.115)	(0.460)	(0.176)	(0.113)	(0.098)	(0.071)	(0.065)	(0.218)
Constant	7.709***	5.666***	5.925***	6.593***	7.546***	7.951***	8.183***	8.293***	8.434***	1.235***
	(0.042)	(0.066)	(0.037)	(0.150)	(0.053)	(0.044)	(0.026)	(0.025)	(0.035)	(0.048)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	105	105	105	105	105	105	105	105	105	105

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log monthly wages. Bootstrap cluster robust standard errors are in parentheses (Cluster=sub-labor market regions). Asterisks indicate the respective significance levels: * $p<0.1$, ** $p<0.05$, and *** $p<0.01$. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Table D6: Minimum wage effect on the unconditional distribution, 401 counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\sigma^2)$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Bite	-3.011***	-4.352***	-0.857***	-2.227**	-3.050***	-3.794***	-3.876***	-3.401***	-3.334***	0.021
	(0.268)	(0.406)	(0.188)	(0.900)	(0.288)	(0.229)	(0.188)	(0.187)	(0.231)	(0.306)
Bite * trend	0.022	0.247***	0.037	-0.019	0.078***	0.043**	0.012	-0.009	-0.018	-0.167***
	(0.019)	(0.090)	(0.023)	(0.074)	(0.021)	(0.019)	(0.014)	(0.018)	(0.018)	(0.043)
D2012 ($year = 2012$)	0.009***	0.007	0.006*	0.045***	0.002	0.003	0.004*	0.007**	0.007**	0.003
	(0.003)	(0.014)	(0.003)	(0.010)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.006)
D2013	0.018***	0.010	0.026***	0.065***	0.005	0.004	0.006	0.011**	0.012**	0.001
	(0.005)	(0.026)	(0.006)	(0.018)	(0.006)	(0.005)	(0.004)	(0.005)	(0.005)	(0.012)
D2014	0.029***	0.007	0.040***	0.066***	-0.004	-0.003	0.008	0.019***	0.027***	0.014
	(0.006)	(0.028)	(0.009)	(0.024)	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)	(0.014)
D2015	0.031***	-0.045	0.038***	0.046	-0.038***	-0.012*	0.014**	0.033***	0.048***	0.057***
	(0.008)	(0.036)	(0.009)	(0.029)	(0.010)	(0.007)	(0.005)	(0.006)	(0.006)	(0.020)
D2016	0.054***	-0.011	0.060***	0.110***	-0.031***	-0.012*	0.024***	0.046***	0.067***	0.054***
	(0.007)	(0.032)	(0.010)	(0.030)	(0.010)	(0.007)	(0.006)	(0.006)	(0.006)	(0.017)
D2017	0.065***	0.000	0.067***	0.158***	-0.044***	-0.020***	0.024***	0.053***	0.081***	0.052***
	(0.008)	(0.030)	(0.014)	(0.035)	(0.011)	(0.008)	(0.007)	(0.007)	(0.007)	(0.018)
Bite * D2014	0.060*	0.080	0.036	0.312*	0.123**	0.141***	0.086***	0.067**	0.024	-0.041
	(0.036)	(0.195)	(0.040)	(0.184)	(0.052)	(0.033)	(0.028)	(0.028)	(0.025)	(0.081)
Bite * D2015	0.332***	1.196***	0.332***	1.311***	0.654***	0.347***	0.161***	0.089*	0.011	-0.691***
	(0.072)	(0.340)	(0.084)	(0.321)	(0.096)	(0.055)	(0.037)	(0.047)	(0.043)	(0.203)
Bite * D2016	0.310***	1.015***	0.239***	1.206***	0.713***	0.491***	0.220***	0.125**	0.005	-0.578***
	(0.082)	(0.367)	(0.087)	(0.361)	(0.100)	(0.072)	(0.045)	(0.064)	(0.059)	(0.218)
Bite * D2017	0.374***	1.124**	0.276**	1.345***	0.974***	0.670***	0.329***	0.165**	-0.002	-0.574**
	(0.098)	(0.463)	(0.112)	(0.451)	(0.118)	(0.083)	(0.055)	(0.075)	(0.074)	(0.259)
Constant	7.805***	5.850***	6.002***	6.959***	7.632***	7.998***	8.222***	8.335***	8.485***	1.103***
	(0.040)	(0.052)	(0.026)	(0.128)	(0.041)	(0.033)	(0.028)	(0.028)	(0.036)	(0.036)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	401	401	401	401	401	401	401	401	401	401

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log monthly wages. Bootstrap cluster robust standard errors are in parentheses (Cluster=counties). Asterisks indicate the respective significance levels: * $p<0.1$, ** $p<0.05$, and *** $p<0.01$. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Appendix E Omitting the bite-specific trend

In this appendix, we check the results when omitting the (pre-determined) treatment-specific trend from the baseline specification. The effects are qualitatively similar, but somewhat larger in size, compared with the baseline estimation. The difference in effect size is plausible given we omit a positive (treatment-specific) trend.

Table E7: Minimum wage effect on the unconditional distribution of log real monthly wages, omitting a bite-specific trend

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite	-1.940***	-1.814***	0.048	1.535	-1.970***	-3.224***	-3.455***	-2.969***	-2.805***	-1.663***
	(0.196)	(0.490)	(0.261)	(1.035)	(0.343)	(0.256)	(0.157)	(0.167)	(0.181)	(0.448)
D2012 (*year* = 2012)	0.012***	0.039***	0.011***	0.043***	0.013***	0.009***	0.006***	0.006***	0.005***	-0.018***
	(0.001)	(0.006)	(0.003)	(0.007)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
D2013	0.024***	0.073***	0.035***	0.061***	0.025***	0.016***	0.009***	0.009***	0.008***	-0.042***
	(0.002)	(0.009)	(0.005)	(0.011)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
D2014	0.032***	0.041***	0.052***	0.073**	0.002	-0.003	0.008	0.018***	0.026***	-0.014
	(0.006)	(0.014)	(0.007)	(0.029)	(0.009)	(0.008)	(0.006)	(0.005)	(0.004)	(0.010)
D2015	0.038***	0.019	0.061***	0.052	-0.043**	-0.016	0.013	0.034***	0.049***	0.001
	(0.012)	(0.042)	(0.011)	(0.049)	(0.018)	(0.012)	(0.009)	(0.008)	(0.005)	(0.026)
D2016	0.060***	0.058	0.086***	0.121**	-0.038*	-0.019	0.023**	0.048***	0.068***	-0.004
	(0.014)	(0.044)	(0.012)	(0.058)	(0.020)	(0.015)	(0.010)	(0.008)	(0.006)	(0.027)
D2017	0.071***	0.085**	0.094***	0.178***	-0.059**	-0.032*	0.021	0.054***	0.083***	-0.014
	(0.014)	(0.038)	(0.020)	(0.060)	(0.024)	(0.019)	(0.014)	(0.010)	(0.008)	(0.024)
Bite * D2014	0.106***	0.570***	0.048	0.206	0.314***	0.273***	0.124***	0.050	-0.017	-0.325***
	(0.035)	(0.117)	(0.043)	(0.140)	(0.059)	(0.058)	(0.044)	(0.035)	(0.028)	(0.066)
Bite * D2015	0.375***	1.696***	0.300***	1.201***	1.016***	0.553***	0.220***	0.056	-0.060	-0.921***
	(0.082)	(0.336)	(0.098)	(0.270)	(0.137)	(0.081)	(0.071)	(0.057)	(0.036)	(0.189)
Bite * D2016	0.384***	1.725***	0.227**	1.044***	1.168***	0.771***	0.299***	0.076	-0.083*	-0.965***
	(0.096)	(0.351)	(0.092)	(0.293)	(0.137)	(0.103)	(0.083)	(0.062)	(0.048)	(0.186)
Bite * D2017	0.474***	1.957***	0.293***	1.089***	1.571***	1.027***	0.431***	0.114	-0.114*	-1.065***
	(0.098)	(0.291)	(0.079)	(0.288)	(0.158)	(0.135)	(0.116)	(0.080)	(0.061)	(0.166)
Constant	7.671***	5.559***	5.892***	6.479***	7.504***	7.930***	8.169***	8.278***	8.415***	1.295***
	(0.028)	(0.072)	(0.036)	(0.142)	(0.043)	(0.034)	(0.025)	(0.026)	(0.029)	(0.061)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Cluster	50	50	50	50	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF of various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Appendix F Adding individuals with multiple jobs

In this appendix, we check the robustness of the results regarding alternative data structures. While the baseline presented in the main text restricts the sample to individuals with a single job, observations of individuals with multiple jobs are now added. We use two alternative sample structure:

1. A sample of all jobs, which includes each job spell covering June 30th of the respective year. Thereby, jobs which are held by the same individual are treated as separate observations.

2. A sample at the individual level, where wages are summed over all jobs of a particular individual.

The first alternative has the advantage to cover all jobs as units of observations, which is exactly the level that is targeted by the minimum wage, but it neglects the individual income dimension. The second alternative focuses on labor income at the individual level irrespective of the number of jobs from which an individual receives a wage. By contrast, the baseline in the main text restricts the sample to individuals with a single job, thereby simultaneously providing results for the job and the individual level.

Table F8 presents the effects of the minimum wage introduction on average wages in columns (1)–(3) and on the variance of log wages in columns (4)–(6). Regarding average wages, we observe slightly larger effects at the job level. The baseline and the individual level specifications show very similar results. This heterogeneity seems plausible since the job level is explicitly targeted by the minimum wage.

The effects on the variance in log wages in 2015 show a very similar picture. The reduction in wage inequality is most pronounced at the job level, and the effect is slightly smaller at the individual level compared with the baseline sample. At the individual level the effect size slightly decreases over time and turns insignificant in 2016 and 2017. This effect pattern is simply due to the strong negative bite-specific trend which limits the size of the treatment effect over time. If we omit the bite-specific trend, the effect stays significant and constant over time (not tabulated). However, omitting the bite-specific trend may over-estimate the true effect size.

Table F8: Minimum wage effect on the unconditional distribution of log real monthly wages, side jobs included

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Baseline	Job-level	Individual-level	Baseline	Job-level	Individual-level
Dependent variable		ln(w)			$RIF(\sigma^2)$	
Explanatory variables						
Bite	-1.954***	-1.232***	-2.143***	-1.432***	-2.573***	-0.874*
	(0.195)	(0.218)	(0.192)	(0.489)	(0.575)	(0.512)
Bite * trend	0.007	0.009	0.024	-0.115**	-0.059	-0.205***
	(0.028)	(0.025)	(0.024)	(0.045)	(0.057)	(0.047)
D2012 ($year = 2012$)	0.011**	0.008*	0.025***	-0.003	-0.007	0.010
	(0.004)	(0.004)	(0.004)	(0.006)	(0.008)	(0.007)
D2013	0.022***	0.015*	0.051***	-0.012	-0.014	0.016
	(0.008)	(0.008)	(0.008)	(0.012)	(0.016)	(0.014)
D2014	0.031***	0.016	0.065***	0.000	0.008	0.041**
	(0.008)	(0.010)	(0.010)	(0.013)	(0.019)	(0.016)
D2015	0.037**	0.007	0.067***	0.016	0.047	0.075**
	(0.014)	(0.019)	(0.017)	(0.029)	(0.042)	(0.034)
D2016	0.059***	0.030	0.092***	0.011	0.032	0.078**
	(0.016)	(0.020)	(0.019)	(0.030)	(0.041)	(0.033)
D2017	0.070***	0.042**	0.114***	0.001	0.013	0.081***
	(0.016)	(0.021)	(0.018)	(0.028)	(0.043)	(0.030)
Bite * D2014	0.091*	0.115***	0.078*	-0.095	-0.166*	-0.054
	(0.048)	(0.041)	(0.043)	(0.097)	(0.085)	(0.087)
Bite * D2015	0.354***	0.450***	0.344***	-0.576***	-0.813**	-0.489**
	(0.095)	(0.129)	(0.101)	(0.188)	(0.323)	(0.222)
Bite * D2016	0.355***	0.435***	0.333***	-0.505**	-0.709**	-0.366
	(0.114)	(0.133)	(0.113)	(0.186)	(0.323)	(0.231)
Bite * D2017	0.438***	0.499***	0.403***	-0.490***	-0.674*	-0.304
	(0.134)	(0.151)	(0.131)	(0.183)	(0.354)	(0.224)
Observations	4,154,228	4,838,072	4,499,656	4,154,228	4,838,072	4,499,656
Cluster	50	50	50	50	50	50

Notes: OLS regression coefficients from difference-in-differences specifications. In columns (4)–(6), the dependent variable is the RIF of the variance of log real monthly wages. Bootstrap cluster robust standard errors reported in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Appendix G Stayers, leavers and entrants across the wage distribution

In this appendix, Table G9 includes the exact numbers of individuals plotted in Figure 8. It also includes the number of incumbents from which the quintile bins are calculated as a reference.

Table G9: Entrants and leavers in the unconditional distribution of real monthly wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-minimum wage (2012-2014)			Post-minimum wage (2015-2017)		
	Incumbents	Leavers	Entrants	Incumbents	Leavers	Entrants
Wage bins						
$0 < w_{it} \leq w(\tau_{5\%})$	75,432	42,084	44,167	77,989	44,764	44,551
	46.65	26.03	27.32	46.62	26.76	26.63
$w(\tau_{5\%}) < w_{it} \leq w(\tau_{10\%})$	75,437	22,640	24,308	78,005	21,203	25,517
	61.64	18.50	19.86	62.54	17.00	20.46
$w(\tau_{10\%}) < w_{it} \leq w(\tau_{15\%})$	75,417	19,277	23,974	77,968	22,336	24,104
	63.55	16.24	20.20	62.67	17.95	19.37
$w(\tau_{15\%}) < w_{it} \leq w(\tau_{20\%})$	75,432	17,504	20,699	77,986	18,133	21,479
	66.38	15.40	18.22	66.32	15.42	18.26
$w(\tau_{20\%}) < w_{it} \leq w(\tau_{25\%})$	75,426	15,190	18,444	77,988	16,096	20,262
	69.16	13.93	16.91	68.20	14.08	17.72
$w(\tau_{25\%}) < w_{it} \leq w(\tau_{30\%})$	75,428	12,745	15,813	77,987	13,043	17,933
	72.54	12.26	15.21	71.57	11.97	16.46
$w(\tau_{30\%}) < w_{it} \leq w(\tau_{35\%})$	75,430	11,100	14,255	77.989	11,339	15,669
	74.84	11.01	14.14	74.28	10.80	14.92
$w(\tau_{35\%}) < w_{it} \leq w(\tau_{40\%})$	75,428	10,054	12,379	77,984	10,270	13,026
	77.08	10.27	12.65	77.00	10.14	12.86
$w(\tau_{40\%}) < w_{it} \leq w(\tau_{45\%})$	75,427	9,105	11,034	77,988	9,245	11,677
	78.93	9.53	11.55	78.85	9.35	11.81
$w(\tau_{45\%}) < w_{it} \leq w(\tau_{50\%})$	75,429	8,501	9,323	77,986	8,815	10,338
	80.89	9.12	10.00	80.28	9.07	10.64
$w(\tau_{50\%}) < w_{it} \leq w(\tau_{55\%})$	75,431	7,613	8,235	77,987	8,061	8,795
	82.64	8.34	9.02	82.23	8.50	9.27
$w(\tau_{55\%}) < w_{it} \leq w(\tau_{60\%})$	75,426	6,756	6,568	77.987	7,277	7,600
	84.99	7.61	7.40	83.98	7.84	8.18
$w(\tau_{60\%}) < w_{it} \leq w(\tau_{65\%})$	75,429	6,109	5,715	77,988	6,686	6,532
	86.45	7.00	6.55	85.51	7.33	7.16
$w(\tau_{65\%}) < w_{it} \leq w(\tau_{70\%})$	75,431	5,833	5,414	77,986	6,398	5,909
	87.02	6.73	6.25	86.37	7.09	6.54
$w(\tau_{70\%}) < w_{it}$	452,567	26,344	20,408	467,920	29,058	22,088
	90.64	5.28	4.09	90.25	5.60	4.26

Notes: Shares of incumbents, leavers, and entrants within quintile bins of the unconditional real monthly wage distribution, where the quintile bins are defined from the unconditional distribution of incumbents. Hence the number of incumbents is the same in each bin (rows), but the shares of entrants and leavers vary, as displayed in columns (1) and (2) as well as (5) and (6). Real monthly wages of leavers are moved one year forward to be comparable with their successors who entered employment. Columns (1)-(3) display results for three years directly before the minimum wage introduction (2012-2014) and columns (4)-(6) display results for three years directly after the minimum wage introduction (2015-2017). Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded. Sample restricted to the period 2012-2017 in order to define entrants and leavers from backward- looking year-to-year transitions.

Appendix H Full tables of fixed effect estimation

In this Appendix, we present the full results of the fixed effect estimations presented in Table 5 of Subsection 6.3. Table H10 presents the full results of models (1)-(10) of Table 5, which are estimations including worker-fixed effects, and Table H10 displays the full results of models (11)-(20) of Table 5, which includes job-cell-fixed effects.

Changes in the level effect on *Bite*, the bite-specific trend effect on *Bite * trend*, and the year effects can explain different treatment effects compared with the baseline results presented in Table 2. Hence, it can be helpful to look at these coefficients in addition to the treatment effects presented in the main body of the paper.

Theoretically, the level effect should change compared with the baseline results, because a lot of the regional bite's own effect (level effect) should already be captured by the fixed effects. Also, the bite-specific trends may change when applying a within worker (or job-cell) transformation. Hence, it is interesting that – despite of the differences in the level effect – the treatment effects still remain very similar compared with the baseline results in Table 2.

Table H10: Minimum wage effect on the unconditional distribution of log real monthly wages, including individual-fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite	-2.035***	-4.097***	-1.298***	-4.570***	-3.214***	-3.131***	-2.340***	-1.445***	-0.940***	1.713***
	(0.224)	(0.945)	(0.245)	(0.935)	(0.275)	(0.244)	(0.213)	(0.141)	(0.104)	(0.533)
Bite * trend	-0.023***	0.032	-0.014	-0.167***	0.110***	0.074***	0.016	-0.014	-0.039***	-0.053*
	(0.008)	(0.066)	(0.023)	(0.061)	(0.022)	(0.018)	(0.020)	(0.017)	(0.010)	(0.030)
D2012 ($year = 2012$)	0.022***	0.030***	0.013***	0.075***	0.006**	0.009***	0.014***	0.016***	0.018***	-0.006
	(0.001)	(0.009)	(0.004)	(0.011)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)
D2013	0.045***	0.063***	0.040***	0.132***	0.013***	0.016***	0.026***	0.031***	0.035***	-0.018**
	(0.002)	(0.017)	(0.008)	(0.018)	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	(0.008)
D2014	0.077***	0.093***	0.067***	0.171***	0.020***	0.020**	0.048***	0.061***	0.074***	-0.009
	(0.004)	(0.023)	(0.011)	(0.025)	(0.006)	(0.009)	(0.008)	(0.006)	(0.004)	(0.013)
D2015	0.102***	0.096***	0.085***	0.190***	-0.017	0.021	0.071***	0.096***	0.119***	0.007
	(0.006)	(0.030)	(0.013)	(0.036)	(0.016)	(0.014)	(0.011)	(0.008)	(0.004)	(0.016)
D2016	0.146***	0.176***	0.119***	0.294***	0.001	0.031*	0.103***	0.133***	0.163***	0.001
	(0.007)	(0.039)	(0.017)	(0.048)	(0.017)	(0.018)	(0.014)	(0.009)	(0.006)	(0.019)
D2017	0.187***	0.267***	0.148***	0.411***	0.005	0.043*	0.126***	0.165***	0.203***	-0.024
	(0.009)	(0.048)	(0.029)	(0.064)	(0.023)	(0.024)	(0.017)	(0.011)	(0.008)	(0.022)
Bite * D2014	0.061***	0.176	0.033	0.429***	0.096*	0.151***	0.056**	0.013	-0.038**	-0.144***
	(0.015)	(0.167)	(0.043)	(0.098)	(0.051)	(0.030)	(0.023)	(0.026)	(0.016)	(0.052)
Bite * D2015	0.288***	1.002***	0.243***	1.415***	0.718***	0.382***	0.133***	0.006	-0.093***	-0.586***
	(0.025)	(0.192)	(0.073)	(0.192)	(0.107)	(0.054)	(0.039)	(0.050)	(0.030)	(0.065)
Bite * D2016	0.304***	0.913***	0.186**	1.495***	0.827***	0.591***	0.201***	0.008	-0.140***	-0.630***
	(0.037)	(0.254)	(0.082)	(0.248)	(0.114)	(0.070)	(0.056)	(0.072)	(0.045)	(0.087)
Bite * D2017	0.342***	0.842**	0.197**	1.619***	1.099***	0.766***	0.294***	0.004	-0.201***	-0.616***
	(0.054)	(0.366)	(0.100)	(0.331)	(0.145)	(0.099)	(0.079)	(0.093)	(0.059)	(0.129)
Constant	7.651***	5.826***	6.054***	7.202***	7.610***	7.867***	7.982***	8.049***	8.148***	0.866***
	(0.030)	(0.128)	(0.030)	(0.097)	(0.032)	(0.031)	(0.026)	(0.018)	(0.019)	(0.067)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Number of workers	877,834	877,834	877,834	877,834	877,834	877,834	877,834	877,834	877,834	877,834
Cluster	105	105	105	105	105	105	105	105	105	105

Notes: Individual-fixed effects regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log real monthly wages. Bootstrap cluster robust standard errors are in parentheses (where clusters are labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.

Table H11: Cell-fixed effect difference-in-differences on the RIFs, including job-cell-fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	ln(w)	$RIF(\tau_{5\%})$	$RIF(\tau_{10\%})$	$RIF(\tau_{20\%})$	$RIF(\tau_{30\%})$	$RIF(\tau_{40\%})$	$RIF(\tau_{50\%})$	$RIF(\tau_{60\%})$	$RIF(\tau_{70\%})$	$RIF(\sigma^2)$
Explanatory variables										
Bite	0.084	0.417	0.430	-0.348	-0.730	-0.089	-0.126	-0.172	0.335	-0.369
	(0.251)	(2.270)	(0.534)	(1.282)	(0.494)	(0.386)	(0.581)	(0.248)	(0.294)	(0.711)
Bite * trend	-0.006	0.089*	0.005	-0.129***	0.078***	0.096***	0.065***	0.024	-0.018*	-0.064***
	(0.007)	(0.051)	(0.016)	(0.044)	(0.016)	(0.018)	(0.018)	(0.019)	(0.010)	(0.014)
D2012 ($year = 2012$)	0.007***	-0.022***	-0.004	0.031***	-0.004**	-0.005**	-0.001	0.007***	0.013***	0.022***
	(0.001)	(0.007)	(0.003)	(0.006)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
D2013	0.017***	-0.037***	0.007	0.050***	-0.006*	-0.010**	-0.001	0.014***	0.027***	0.037***
	(0.002)	(0.013)	(0.005)	(0.011)	(0.003)	(0.004)	(0.005)	(0.005)	(0.003)	(0.005)
D2014	0.035***	-0.050***	0.017***	0.050***	-0.011***	-0.017**	0.008	0.033***	0.062***	0.069***
	(0.003)	(0.012)	(0.006)	(0.013)	(0.004)	(0.007)	(0.007)	(0.007)	(0.004)	(0.005)
D2015	0.042***	-0.084***	0.018***	0.014	-0.061***	-0.035***	0.012	0.056***	0.100***	0.113***
	(0.004)	(0.014)	(0.007)	(0.019)	(0.012)	(0.010)	(0.011)	(0.009)	(0.005)	(0.007)
D2016	0.066***	-0.063***	0.033***	0.056**	-0.061***	-0.046***	0.021	0.076***	0.135***	0.139***
	(0.005)	(0.016)	(0.007)	(0.024)	(0.012)	(0.013)	(0.013)	(0.010)	(0.006)	(0.007)
D2017	0.079***	-0.068***	0.035**	0.082***	-0.082***	-0.058***	0.021	0.092***	0.165***	0.160***
	(0.006)	(0.012)	(0.017)	(0.029)	(0.016)	(0.017)	(0.017)	(0.013)	(0.008)	(0.007)
Bite * D2014	0.040***	0.081	0.012	0.348***	0.086**	0.127***	0.053**	0.029	-0.034**	-0.093**
	(0.014)	(0.114)	(0.042)	(0.085)	(0.039)	(0.033)	(0.023)	(0.025)	(0.014)	(0.037)
Bite * D2015	0.258***	0.691***	0.180**	1.227***	0.692***	0.366***	0.148***	0.047	-0.062**	-0.470***
	(0.020)	(0.165)	(0.075)	(0.147)	(0.096)	(0.048)	(0.032)	(0.045)	(0.025)	(0.053)
Bite * D2016	0.278***	0.492**	0.102	1.235***	0.789***	0.583***	0.247***	0.090	-0.068**	-0.466***
	(0.027)	(0.231)	(0.090)	(0.189)	(0.096)	(0.054)	(0.045)	(0.062)	(0.034)	(0.063)
Bite * D2017	0.358***	0.589**	0.128	1.454***	1.082***	0.776***	0.367***	0.123	-0.093**	-0.494***
	(0.032)	(0.269)	(0.110)	(0.232)	(0.122)	(0.072)	(0.062)	(0.080)	(0.043)	(0.086)
Constant	7.424***	5.399***	5.883***	6.806***	7.352***	7.516***	7.722***	7.896***	7.989***	1.040***
	(0.032)	(0.285)	(0.069)	(0.157)	(0.062)	(0.058)	(0.073)	(0.036)	(0.036)	(0.090)
Observations	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228	4,154,228
Number of cells	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986	1,344,986
Cluster	105	105	105	105	105	105	105	105	105	105

Notes: Cell-fixed effects regression coefficients from difference-in-differences specifications. Dependent variables are depicted by column titles. In columns (2)-(10), the dependent variable is the RIF calculated for various quantiles as well as the variance of log monthly wages. Bootstrap cluster robust standard errors are in parentheses (Cluster=labor market regions). Asterisks indicate the respective significance levels: * p<0.1, ** p<0.05, and *** p<0.01. Data: IEB 2011-2017, 2 percent sample, apprentices and internships excluded.