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Minimum Wages and the Gender Wage Gap Within Firms

Serife Yasar*

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Abstract

This paper studies how the introduction of a statutory minimum wage affects the gender wage gap within firms. I compare the residual gender wage gap in firms with and without minimum wage exposure before and after the reform using linked employer–employee data from Germany and employ a difference-in-differences approach in an event-study style with firm- and year-fixed effects. My results show that the introduction of the minimum wage led to a modest decline in the within-firm gender wage gap, with the clearest effects among incumbent workers and in the lower to middle part of the wage distribution. Effects differ across employment status, industries, and firm size. I find small wage changes in full-time jobs, a positive and significant post-reform effect for part-time workers, and no precise post-reform effects for marginal employment. These findings suggest that the effects of the minimum wage on the gender wage gap vary across worker groups and firm environments. A rich set of robustness checks, including alternative exposure thresholds and gender gap definitions, support my main findings.

Keywords: minimum wages, gender wage gap, within-firm wage inequality
JEL Codes: J23, J31, J38, J71

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1 Introduction

Although over the past decades progress has been made toward wage equality between men and women, significant disparities still persist up to nowadays (Blau and Kahn, 2017).[1] The gender wage gap is partly explained by observable factors, including occupational segregation, differences in educational attainment, and variations in academic discipline choices (Blau and Kahn, 2017; Sparreboom, 2018; Graham et al., 2000; Ransmayr and Weichselbaumer, 2024; Chevalier, 2007); and partly by unobservable factors, such as divergent negotiation strategies or women's lower tendency to enter competitive fields (Niederle and Vesterlund, 2007; Babcock and Laschever, 2003; Bowles et al., 2007). Moreover, discrimination further exacerbates these disparities, with women often earning lower wages despite equivalent qualifications (Altonji and Blank, 1999; Neumark, 1996). In addition, a recent wave of literature highlights the role of firms in explaining the gender gap, as women are more frequently employed in organizations that offer lower wage premia (Card et al., 2016; Bruns, 2019).

On the other hand, minimum wage policies are a prominent policy tool for promoting pay equity in the labor market (Bossler and Schank, 2023; Autor et al., 2016) and have already been effective in narrowing the gender wage gap (Caliendo and Wittbrodt, 2022; Bargain et al., 2019; Schmid, 2025).[2] However, in frictional labor markets, firms retain considerable wage-setting power, which they can exploit to suppress wages for both high- and low-wage earners even in the presence of a minimum wage. Evidence suggests that some employers continue to pay below the statutory minimum wage threshold (Bachmann et al., 2017; Caliendo et al., 2023) or implement wage cuts for unaffected workers, through lowering wages for highly skilled employees (Gregory and Zierahn, 2022) or by adjusting wage scales to preserve internal wage hierarchies (Dube et al., 2019; Falk et al., 2006).[3] Given unintended wage adjustments and firms' cost-minimizing incentives, it is crucial to examine whether firms respond to minimum wage policies by gender-specific wage adjustments.

[1]The term *gender* refers to socially constructed identities, whereas *sex* denotes biological attributes. This paper focuses solely on men and women, as gender-diverse individuals cannot be identified in the underlying data.

[2]Bargain et al. (2019) examine changes in the gender wage gap following the introduction of minimum wages in Ireland and the United Kingdom. They find a significant reduction in the gender wage gap at the lower end of the wage distribution in Ireland, whereas no substantial change is observed in the UK. Schmid (2025) analyzes how the minimum wage influences the gender wage gap and the wage distribution on a regional level. She shows that the minimum wage reduced the gender wage gap by 60% to 95%.

[3]Antonczyk et al. (2010) investigate the decline of collective bargaining in Germany, which acted as a de facto minimum wage. They show that this decline had unintended positive effects on women's wages in the upper part of the wage distribution.

This paper is the first that analyzes the impact of minimum wages on the gender wage gap within firms. I exploit Germany's introduction of a statutory minimum wage in 2015, set at 8.50 Euros, as a quasi-experimental setting. I employ an event-study approach to identify the causal effects of the policy on the gender wage gap by comparing wages in establishments exposed to the minimum wage reform with those in establishments that are not directly exposed to the reform. Of course, a policy reform necessarily affects all firms in the country, doing so through spillover effects (Demir, 2023) or reallocations of workers across firms (Dustmann et al., 2022). Nevertheless, the treatment indicator *exposure to the minimum wage* is defined as the share of employees who are affected by the minimum wage in the year 2014 prior to the introduction.

My outcomes of interest are the raw log gender wage gap (men-women) and a residual-adjusted wage gap that nets out observable worker characteristics and firm- and year-specific wage components. The analysis is based on administrative Social Security data from the Sample of Integrated Employee Data (SIEED) (Schmidtlein et al., 2025), augmented with information on working hours imputed from the Socio-Economic Panel (SOEP) (SOEP, 2023).

My results show that in firms exposed to the minimum wage, the gender wage gap narrowed modestly after the reform. This reduction occurred mainly among incumbent workers who remain at the same firm over time, rather than among new hires or workers switching firms. To further identify potential spillover effects of the minimum wage on other parts of the wage distribution, I examine gender wage gaps across different wage levels. My results suggest that the reduction is strongest in the lower to middle part of the wage distribution, where the minimum wage is more likely to bind, while effects at the top of the distribution are small and statistically not significant.

Further, I examine the heterogeneity of these effects along employment status (full-time, part-time, and marginal employment), industry sectors based on the German WZ classifications (Destatis, 1994), and firm size. I show that the reduction in the gender wage gap is most visible among workers in smaller firms, while the patterns differ across employment groups and sectors. My results suggest substantial variation in responses across sectors and firm-size categories. This indicates that institutional and organizational factors shape how firms adjust gender-specific wages in response to the minimum wage.

Finally, I conduct a rich set of robustness checks, including (i) alternative exposure to minimum wage thresholds, (ii) using log daily wages as outcome, (iii) leaving out sectoral minimum wage sectors, (iv) keeping only firms subject to the dismissal protection law, and (v) a pre-reform anchored adjusted gap. The pre-reform anchored adjusted gap conceptually relates to counterfactual pre-treatment reweighting approaches (DiNardo et al., 1996), but instead of using the full approach, it uses the relationship between the adjusted gender wage gap and the raw gender wage gap. My results remain stable when treatment intensity is defined using alternative exposure thresholds. Potential bias from the working hours imputation does not seem to drive the results, as the main pattern remains when I construct a pre-reform anchored adjusted gap based on the pre-reform wage gap relationship or when I use directly observed daily wages, which are not affected by the imputed working hours, as alternative outcomes.

Germany introduced a statutory minimum wage on January 1, 2015 at the level of 8.50 euro gross per hour.[4] The policy had extensive coverage, affecting 4 million workers (equivalent to 10.7% of all jobs), of whom 2.5 million were women and 1.5 million were men (Destatis, 2016). The minimum wage was nearly universally binding. Exemptions apply to workers younger than 18 years, apprentices, interns (for a maximum of 3 months), and the long-term unemployed during the first 6 months of their new employment. Sectors with pre-existing minimum wages below 8.50 euros were granted a transition period until the end of 2016 and accounted for only 5% of total employment (Dustmann et al., 2022). At the time of its introduction, the minimum wage was considered high, leading to a significant compression of the wage distribution. When adjusted for purchasing power, it was among the highest minimum wages in Europe (Caliendo et al., 2019). Although non-compliance can result in fines of up to 500,000 euros (see §21 MiLoG, 2024), evidence suggests significant non-compliance in the short-run (Bachmann et al., 2017; Caliendo et al., 2023; Mindestlohnkommission, 2018). Destatis (2018) estimated that in 2017, despite being eligible, approximately 800,000 employees still earned less than the statutory minimum wage.

This paper contributes twofold to the literature. First, this study contributes to the broad literature on how firms respond to minimum wage policies. This literature already shows that firms adjust through various channels, including passing costs to consumers via higher prices (Harasztosi and Lindner, 2019; Allegretto et al., 2018), altering job security and working conditions (MaCurdy, 2015; Clemens and Wither, 2019), substituting lower-skilled workers with higher-skilled ones (Schmitt, 2013; Clemens and Wither, 2019), investing in automation (Meer and West, 2015; Aaronson et al., 2018), raising hiring standards, and favoring higher-productivity workers (Butschek, 2022; Clemens et al., 2021), reconstructing internal wage hierarchies for maintaining perceptions of fairness, motivation, and effort among higher-paid employees (Dube et al., 2019; Falk et al., 2006), or wage reductions for above-minimum wage earners (Gregory and Zierahn, 2022). Dustmann et al. (2022) show that these adjustments

[4]After its introduction, the minimum wage was gradually increased in several stages: on January 1, 2017 to 8.84 euro; on January 1, 2019 to 9.19 euro; on January 1, 2020 to 9.35 euro; on January 1, 2021, to 9.50 euro; on July 1, 2021 to 9.60 euro; on January 1, 2022 to 9.82 euro; on July 1, 2022 to 10.45 euro; on October 1, 2022 to 12.00 euro; on January 1, 2024 to 12.41 euro. It reached 13.90 euros on January 1, 2026 (Mindestlohnkommission, 2026).

can reallocate low-wage workers toward larger, higher-paying, and more productive firms.[5] My paper adds to this literature by introducing the firm-level gender gap as a new result through which firms adjust to minimum wages.

Second, this paper adds to the literature on within- and between-firm wage inequality (Card et al., 2013; Barth et al., 2016; Helpman et al., 2017; Song et al., 2019). This literature documents the growing importance of firms in shaping individual wages (Abowd et al., 1999; Card et al., 2018) and the rising segregation of workers by skill and wage levels (Bruns, 2019; Cortes and Salvatori, 2019), but the drivers behind these patterns are less explored (Cortes et al., 2023). The literature shows that within-firm wage inequality has increased, particularly in large firms, where wages in the upper part of the distribution have risen sharply while wages below the median have declined (Song et al., 2019). Within-firm wage inequality grows with firm size (Mueller et al., 2017), and skill-biased technological change has further widened within-firm wage inequality (Cortes et al., 2023). Trade and globalization have also reshaped within-firm wage structures, increasing wage dispersion (Frías et al., 2012), performance-based compensation (Pupato, 2017), and top income shares (Ma and Ruzic, 2020). My paper contributes to this literature by demonstrating that minimum wages reduce the within-firm wage inequality for women.

For Germany, existing analyses of the introduction of the minimum wage indicate significant positive effects on hourly wages below the minimum wage threshold (Bachmann et al., 2020; Bossler and Gerner, 2020; Burauel et al., 2020; Caliendo et al., 2023). Bossler and Gerner (2020) and Bossler et al. (2020) find that overall employment declined, primarily due to reduced hiring rather than job displacements. Bonin et al. (2018) report significant but small employment effects, with regular employment showing minimal to no reduction, while marginal employment declined (Bonin et al., 2018; Schmitz, 2017).

Caliendo and Wittbrodt (2022) and Schmid (2025) analyze the impact of the German minimum wage on aggregate or regional changes in the gender wage gap. In contrast to these, my paper focuses on the within-firm gender wage gap, as it enables a more detailed view of the mechanisms underlying these effects. Looking at aggregate effects on the gender wage gap may reflect the sorting of men and women across firms (Card et al., 2013) or the reallocation of workers across firms after the introduction of the minimum wage (Dustmann et al., 2022). The within-firm gap, however, captures gender differences in pay within the same workplace. This perspective is particularly relevant in frictional labor markets, where firms have wage-setting power and gender inequality may persist even among workers employed by the same firm.

[5] For a recent overview, see Neumark (2024). For a review of firm responses, see Clemens (2021).

The remainder of this paper is organized as follows. Section 2 describes the data and the construction of the gender wage gap, while section 3 illustrates the empirical strategy. Section 4 presents the main results, discusses robustness and extensions. Section 5 concludes.

2 Data and Measurements

2.1 Data Sources, Sample Construction and Working Hours

This paper uses the SIEED *(Stichprobe integrierter Employer-Employee Daten)* data, a linked employer–employee data provided by the Institute for Employment Research (IAB).[6] The SIEED data combines administrative records on all employees in Germany who are subject to social security contributions with their full employment biographies. The sample is drawn in two steps. In the first step, a 1.5% sample of all firms in Germany is drawn. In the second step, the full employment biography of all individuals who worked at least one day in the respective establishment is merged. This procedure ensures that the dataset is representative on the firm level, which is necessary for analyzing the impact of the minimum wage introduction on within-firm outcomes. For more information on the SIEED, see Schmidtlein et al. (2025).[7]

As the minimum wage was introduced in 2015, I restrict the sample to the years 2012 to 2018. For each year between 2012 and 2018, I observe individual daily wages, employment status (full-time, part-time, marginal employment), demographic characteristics (gender, education, tenure, and employment status), and firm identifiers. I restrict the sample to employees with valid information on hourly wages, employment status, schooling, and other variables needed to construct experience and treatment-exposure variables. I only keep full-time, part-time, and marginal employment in my sample and exclude apprentices, interns, and minors, as these groups were legally exempt from the 2015 Minimum Wage Act and their wage-setting mechanisms differ substantially from those of standard employment. I also drop firms from a agriculture-related industries as these are strongly subject to seasonal workers.

I focus on a balanced panel of firms that are present in all years from 2012 to 2018. During this period, some sectoral minimum wages were already in effect, which I later exclude from my analysis as a robustness check. Specifically, I start with the full data and keep only firms with at least one male and one female employee in each year of the period. This reduces the dataset by around 18%. The resulting dataset is a balanced firm-year panel. Lastly, I perform

[6] Dustmann et al. (2022) use the data source on which the SIEED is drawn from. Instead of the sample, they use the full Social Security records.

[7] Please note that the data provides establishment IDs, which I define as “firms”.

additional data preparation steps, as recommended by Dauth and Eppelsheimer (2020), such as defining the highest-wage job as the main job in case of parallel spell data, constructing an education variable, and using 30 June in each year as the observation day.

Because the SIEED data only include average daily wages[8] but no information on average daily working hours, I augment my data with working hours from the German Socio-Economic Panel (SOEP), a representative household survey.[9] For more information, see SOEP (2023). Here, I calculate average daily working hours by year, gender, and employment status (full-time, part-time, marginal employment) and merge this with the SIEED. I obtain individual hourly wages by dividing gross daily earnings in the SIEED by the SOEP-based working hours. The main outcome variable in the analysis is the log hourly wage.

Table A.1 reports the SOEP-based working-hours divisors used in the hourly-wage imputation. In line with the literature (Bachmann et al., 2020), the table shows that working hours for marginally employed workers decrease, while they remain relatively constant for full-time employment. I construct working time from the individual SOEP working-time variables. As my main measure, I use contractual weekly hours because this is the most stable concept and less sensitive to short-run shocks, temporary overtime, or reporting noise. This matters because I later average hours within year × gender × employment-status cells and want these cell means to reflect typical working time rather than idiosyncratic weeks. Finally, I convert weekly hours into hours per calendar day by dividing by seven (rather than five), because the SIEED daily earnings measure is defined per calendar day of the employment spell, so the hours divisor should be on the same calendar-day scale to remain consistent with the numerator.

Table 1 reports mean hourly wages which I get through dividing the SIEED wages through the SOEP working hours. In 2014, women earn 16.96 euro and men 20.20 euro, which implies an unadjusted gender gap of about 16.0% (computed as $(\bar{w}_m - \bar{w}_f)/\bar{w}_m$). In 2018, women earn 18.43 euro and men 21.57 euro, implying a gap of about 14.6%. For external validation, I compare these levels to official statistics from Eurostat (2026), the so-called Structure of earnings survey (SES). According to Eurostat (2026) hourly earnings in Germany are 15.44 euro for women and 19.87 euro for men in 2014 (gap 22.3%) and in 2018, 17.33 euro for women and 21.70 euro for men (gap 20.1%).

[8]The pay variable in the SIEED is the gross daily earnings from the German social-security notification system (DEÜV) and is constructed from the reported spell earnings divided by the spell duration in calendar days. Earnings are censored at the pension insurance contribution ceiling, and one-off payments may be included in some reports or recorded separately, a practice that becomes more common from 2013 onwards.

[9]Dustmann et al. (2022) harmonize establishment-reported hours from administrative records with overtime corrections, I orient on this approach by imputing typical daily hours from the SOEP.

Table 1: Mean hourly wages by gender

Year	N_F	N_M	$N_{F,<8.5}$	$N_{M,<8.5}$	$\bar{w}_f$	$\bar{w}_m$	$g\bar{a}p^{raw}$
2012	96,992	104,821	22,282	12,123	16.34	19.56	3.22
2013	100,047	107,390	20,593	12,313	16.52	19.71	3.18
2014	101,723	110,158	18,924	12,143	16.96	20.20	3.24
2015	102,727	111,477	15,691	10,819	17.44	20.73	3.29
2016	104,372	113,994	13,896	9,254	17.84	20.98	3.15
2017	105,481	115,125	12,494	9,754	18.21	21.38	3.17
2018	106,443	117,357	12,478	7,926	18.43	21.57	3.14

Notes: N_F and N_M denote the numbers of women and men in the SIEED after my sample adjustments. $N_{F,<8.5}$ and $N_{M,<8.5}$ denote the number of women and men with hourly wages below 8.50 euro in the respective year. $\bar{w}_f$ and $\bar{w}_m$ represent the average hourly wages in euros for women and men, respectively. Hourly wages are based on imputed daily wages divided by SOEP-based working-hours divisors. Wages are trimmed at the 1st and 99th percentiles of the yearly distribution. The raw gender gap is defined as the difference between the mean hourly wages of men and women.

The lower official wage levels and the larger official gender wage gaps are mainly due to different measure and sampling structures. In contrast to me, the SES uses *paid hours* in the reference month (including paid overtime and paid hours not worked, such as paid leave) and a regular-earnings concept that excludes extraordinary payments (Eurostat, 2025). My sample is a balanced establishment panel (firms observed in all years with at least one man and one woman), includes only full-time, part-time, and marginal employment, and trims wages at the 1st and 99th percentiles. This trimming reduces the influence of remaining outliers (e.g., due to reporting errors, very short spells, or unusual earnings–hours combinations after constructing hourly wages), which can otherwise shift mean wage levels and the implied gender gap relative to SES benchmarks.

This procedure inevitably introduces measurement error because true working hours vary within each year × gender × status cell. Additionally, SOEP working-time information is self-reported and therefore noisy, and the external mapping cannot capture firm-specific schedules or overtime for a given worker. The hours-imputation error can affect two aspects of my design: the outcome (log hourly wage / firm-year gender gap) and the treatment definition. In general, noise in the outcome variable mainly increases the residual variance and reduces precision, but it does not bias regression coefficients under classical measurement-error assumptions (Wooldridge, 2010; Angrist and Pischke, 2009). A potential concern in my setting is that the imputed hourly wage is also used for treatment classification, since minimum wage exposure is defined as the share of workers below the minimum wage. However, exposure is measured once in the pre-reform year 2014, so any misclassification is time-invariant. This should mainly weaken the treated–control contrast and attenuate estimated effects rather than generate false post-reform effects. Moreover, my main specifications include firm- and year-fixed effects and focus on changes over time. As a robustness check, I show that the event-study patterns are very similar when using gender gaps in log daily wages (which do not rely on imputed hours), suggesting that the core results are not driven by the SOEP-based hours imputation.

2.2 Measures for the Gender Wage Gap

I use mainly two definitions of the gender wage gap as my main outcome: (i) the raw gender wage gap and (ii) the adjusted gender wage gap. First, I construct the raw gender wage gap. For each firm and year, I compute the average hourly wage separately for men and women and define the raw gender wage gap as the log difference between these two averages (men minus women). This measure is descriptive and reflects the average wage difference between men and women within the same firm and year, without controlling for worker characteristics or composition. Nevertheless, the raw gap still remains informative because it summarizes the observed within-firm gender wage differential and thus reflects the reform's total effect as experienced in firms.

The construction of the adjusted gender wage gap is defined as the residual difference between men and women (Oaxaca, 1973; Blinder, 1973; Blau and Kahn, 2017). In my analysis, I employ a two-step residual-based event study, motivated by the bargaining framework of Card et al. (2016) and the empirical design of Biasi and Sarsons (2022).[10] Motivated by Card et al. (2016), I construct an adjusted gender wage gap that nets out observable worker characteristics and common firm- and year-specific wage components. To this end, I estimate the following pooled log-wage model at the worker level:

$$\ln(w_{ijt}) \;=\; X'_{ijt}\beta \;+\; \alpha_j \;+\; \gamma_t \;+\; u_{ijt}, \tag{1}$$

where w_{ijt} denotes the hourly wage of individual i employed at firm j in year t, X_{ijt} is a vector of controls and α_j, γ_t are firm- and year-fixed effects, respectively. The vector X_{ijt} includes employment-status categories, a quadratic in experience, and an education measure. Table A.2 shows the estimation results for equation 1.

[10] Card et al. (2016) estimate an AKM-style log-wage model with worker and firm effects and allow firm-specific wage premia to differ by gender. They quantify a *within-firm* (pay-setting/bargaining) component using differences between male and female firm wage premia, alongside a *between-firm* sorting component. In contrast, because my outcome of interest is a time-varying firm–year gap around the 2015 reform, I residualize worker-level log wages using a pooled specification with observable controls and firm and year fixed effects (without individual fixed effects) and then define the adjusted within-firm gender wage gap as the difference in average residual log wages between men and women within each firm–year. This two-step residual-based construction is in the spirit of Biasi and Sarsons (2022), who similarly obtain conditional wages as residuals from a pooled regression and study event-time dynamics in these conditional outcomes. See Section 3 for details.

I do not include a female indicator in equation (1), so that residuals are defined relative to a common wage-setting structure and fixed-effects normalization for all workers. After estimating equation 1, I compute the within firm–year mean residual separately for men and women:

$$\bar{u}_{jt}^{M} = \frac{1}{N_{jt}^{M}} \sum_{i \in (j,t):\, g_i = M} u_{ijt}, \qquad \bar{u}_{jt}^{F} = \frac{1}{N_{jt}^{F}} \sum_{i \in (j,t):\, g_i = F} u_{ijt}, \tag{2}$$

and define my main outcome, the residual-adjusted gender wage gap at the firm–year level as

$$\text{gap}_{jt} \equiv \bar{u}_{jt}^{M} - \bar{u}_{jt}^{F}. \tag{3}$$

The residual-adjusted wage gap gap_{jt} can be interpreted as the within firm–year difference in average log wages after controlling for observed characteristics X_{ijt} and additive firm and year components. The gap_{jt} is per construction an "unexplained" residual wage gap and traditionally among labor economists interpreted as discrimination, unmeasured productivity, bargaining, and many other unobservables. gap_{jt} isolates gender differences in wages net of observed worker characteristics and common firm/year wage components, and provides the residual object whose dynamics I study in the event-study design.

In the following empirical analysis, I embed the residualized adjusted-gap gap_{jt} into a matched employer–employee setting with firm- and year-fixed effects and decompose the effect of the minimum wage policy. My approach of residualization and decomposition is a widely used method in the literature to separate the contribution of observables from "unexplained" components of wage differentials, as in Abowd et al. (1999), Oaxaca (1973), Blinder (1973), and Fortin et al. (2011). Most similar to my approach, Abowd et al. (1999) partials out observables and fixed effects before analyzing the remaining variation. They motivate interpreting $\widehat{\alpha}_j$ as a firm-level wage component and $\widehat{u}_{ijt}$ as residual wage variation.

2.3 Definition of Treatment and Control Group

I define treatment status at the firm level based on each firm's exposure to the statutory minimum wage prior to its introduction in 2014. In the first step, I compute, for each firm, the share of its employees whose hourly wages are below the minimum wage threshold of 8.50 euros. I define firms *with* exposure to minimum wage, i. e., all above $> 0\%$ share of employees, as the treatment group, while firms with *no* exposure to the minimum wage are defined as the control group. This treatment definition is time-invariant in my setting, so that subsequent changes in wages and within-firm gender gaps can be interpreted relative to this fixed baseline classification in my later difference-in-differences strategy.

Table 2 reports firm-level summary statistics for the year 2014, separately for treatment and control firms. Several systematic differences appear between treatment and control firms. We see that firms in the treatment group employ a larger share of part-time workers (0.216 vs. 0.181) and a higher share of women (0.499 vs. 0.407). Average log hourly wages are substantially lower in treatment firms for both men and women. This is consistent with the definition of treatment firms as low-wage firms and with their greater exposure to the introduction of the minimum wage. The treatment firms also show a larger raw firm-level gender log wage gap (0.197 vs. 0.154) and residual-adjusted wage gap (0.118 vs. 0.097). Finally, treatment firms are on average larger than the control firms (about 39 employees vs. 21). Differences in means are statistically significant for all variables reported, although the difference in the residual-adjusted wage gap is only weakly significant. This implies that treatment and control firms are not comparable in levels, so a cross-sectional comparison would not be possible. My difference-in-differences design with firm- and year-fixed effects, therefore, relies on within-firm changes over time. As the workforce composition differed already prior to the minimum wage introduction in 2014, I run robustness checks that hold the workforce constant (e.g., stayer samples or controls for time-varying composition).

Table 2: Firm-level summary statistics

	Treatment firms			Control firms			t-test	
	N	Mean	SD	N	Mean	SD	Diff (C–T)	p-value
Share part-time workers	4,774	0.216	0.223	1,297	0.181	0.218	-0.035	0.0000
Share female workers	4,774	0.499	0.249	1,297	0.407	0.234	-0.092	0.0000
Log hourly wage, men	4,774	2.433	0.522	1,297	2.938	0.349	0.505	0.0000
Log hourly wage, women	4,774	2.237	0.480	1,297	2.785	0.338	0.548	0.0000
Raw log gender wage gap	4,774	0.197	0.495	1,297	0.154	0.303	-0.043	0.0029
Residual-adjusted wage gap	4,718	0.118	0.429	1,291	0.097	0.281	-0.021	0.0951
Number of employees	4,774	38.73	142.25	1,297	20.81	40.98	-17.91	0.0000

Notes: Firm-level means for 2014 based on the balanced-firm panel (2012–2018). Treatment firms are defined by having a positive low-wage exposure in 2014 ($exposure > 0$). Selected WZ93 industries, including agriculture-related codes, are excluded. Shares are computed over all employees in the firm. The raw log gender gap is defined as $\ln(\bar{w}_m) - \ln(\bar{w}_f)$ at the firm level. The residual-adjusted wage gap uses individual residuals from a wage regression with firm- and year-fixed effects and no gender dummy. *Diff (C–T)* reports mean(Control) − mean(Treat). The p-values are from two-sample t-tests with equal variances.

Additionally, table 3 presents an unconditional 2×2 difference-in-differences comparison for hourly wages. The table compares treated and control firms before and after the introduction of the statutory minimum wage. In line with the treatment definition, wage levels are lower in treated firms already before the reform. After 2015, wages increase in both groups, with stronger increases in treated firms. Since this comparison is unconditional, it mainly serves as descriptive evidence and does not replace the regression-based analysis below.

Table 3: Unconditional 2×2 Difference-in-Differences for hourly wages

Period	Group	N_f	N_m	w_f	w_m
Pre (2012–2014)	Control	30,005	49,709	20.45	21.82
Pre (2012–2014)	Treat	268,757	272,660	16.18	19.46
Post (2015–2018)	Control	42,426	71,257	21.48	22.68
Post (2015–2018)	Treat	376,597	386,696	17.59	20.90
Difference (Treat − Control), Pre				-4.27	-2.36
Difference (Treat − Control), Post				-3.89	-1.78
Difference-in-Differences				0.38	0.58

Notes: Pre denotes 2012–2014 and post denotes 2015–2018. Cell entries report pooled averages by treatment status and period. N_f and N_m denote the total number of female and male worker-year observations. w_f and w_m are pooled mean hourly wages in euros for women and men, respectively. The unconditional difference-in-differences is computed as the post-treatment difference between treated and control firms minus the corresponding pre-treatment difference. These descriptive comparisons do not adjust for compositional differences between treatment and control firms.

2.4 Between and Within-Decomposition

Table 4 decomposes the mean log gender wage gap in each year into a within-firm and a between-firm component. It shows that the raw gap declines from about 0.19 log points in 2012 to about 0.16 in 2018. Throughout the years, the within-firm component accounts for roughly three-quarters to four-fifths of the total raw gap, while the between-firm component is around 0.04 to 0.05 log points and slightly decreases over time. For the residual-adjusted wage gap, the total difference is smaller (around 0.06 to 0.07 log points), and the within-firm component is larger than the total gap. This yields a negative between-firm component of roughly -0.02 log points. A negative between-firm component does not imply a negative overall gender gap. Rather, it means that cross-firm sorting reduces the aggregate residual-adjusted gap relative to what would be observed from within-firm differences alone (Card et al., 2016). Overall, the raw gap is larger than the residual gap. Both the raw and residual gap show that the within-firm component is more relevant in the gender wage gap; the raw gap shows a positive between-firm component, implying women mostly work in low-wage firms, and the residual gap implies a negative between-firm component, indicating that the net out observable component is mainly due to within-firm differences.

Table 4: Within- vs. between-firm decomposition of gender wage gap

(1) Year	(2) raw^{total}	(3) raw^{within}	(4) $raw^{between}$	(5) $residual^{total}$	(6) $residual^{within}$	(7) $residual^{between}$	(8) N_{indv}	(9) N_{firms}
2012	0.1872	0.1323	0.0548	0.0612	0.0787	-0.0176	201,813	6,071
2013	0.1849	0.1360	0.0489	0.0688	0.0856	-0.0169	207,437	6,071
2014	0.1777	0.1302	0.0475	0.0669	0.0843	-0.0174	211,881	6,071
2015	0.1752	0.1324	0.0428	0.0718	0.0899	-0.0180	214,204	6,071
2016	0.1682	0.1246	0.0436	0.0650	0.0854	-0.0204	218,366	6,071
2017	0.1586	0.1175	0.0412	0.0591	0.0799	-0.0207	220,606	6,071
2018	0.1627	0.1243	0.0384	0.0695	0.0908	-0.0213	223,800	6,071

Notes: This table decomposes the total gender wage gap into components occurring within the same firm and between different firms. The results show that the total raw gap declines over time, largely driven by a reduction in the within-firm component. In the residual-based decomposition, the within-firm component remains larger than the total gap, implying a negative between-firm component throughout.

In Table 4, most of the gender wage gap is within firms, and the between-firm part is about 0.04 to 0.05 log points. This is consistent with the minimum wage changing the gap primarily through wage adjustments inside firms rather than through large gender-specific changes in sorting across firms. The raw between-firm component may also partly reflect that women are overrepresented in firms with higher exposure to the minimum wage, making that wage adjustments after the introduction of the minimum wage are more likely. The between-firm component becomes slightly smaller over time. This is consistent with a small decline in sorting differences after 2015. The between-firm share of the raw gap is roughly 20 to 29% across years,[11] which is close (in order of magnitude, though not directly comparable) to the 10-30% range for the firm-wage premium reported in the cross-country meta-study by Palladino et al. (2025). For the residual wage gap, the negative between-firm component implies that, conditional on observables, differences in worker's sorting offset part of the overall residual wage gap (i.e., the within-firm residual wage gap exceeds the total residual wage gap), so the remaining inequality is predominantly within firms.

Dustmann et al. (2022) show that the German minimum wage induced reallocation across firms and jobs, so composition and sorting can matter for post-reform outcomes. In my data, the gender gap is mainly within firms, suggesting that gender differences in sorting across firms account for only a limited share of the average gap—even though minimum wage induced reallocation may still matter for post-reform outcomes. Consistent with this, Dustmann et al. (2022) document reallocation of low-wage workers to higher-quality establishments and show that these reallocation effects are more pronounced for women, even though their wage gains are not larger than for men. Even when workers switch firms, women may still earn less than men

[11] The between-firm share is computed as $raw^{between}/raw^{total}$ (and the within-firm share as raw^{within}/raw^{total}). Using Table 4, the between-firm share equals 29.3% (2012), 26.4% (2013), 26.7% (2014), 24.4% (2015), 25.9% (2016), 25.9% (2017), and 23.6% (2018), implying within-firm shares of 70.7%, 73.6%, 73.3%, 75.6%, 74.1%, 74.1%, and 76.4%, respectively.

within the destination firm because wage growth, task assignment, and promotion opportunities differ by gender. Palladino et al. (2025) also report that women receive about 90% of the rents that men receive from firm surplus gains, so firm-level shocks like the minimum wage can raise women's pay without fully closing the within-firm gap.

3 Empirical strategy

To estimate the effect of the statutory minimum wage on the gender wage gap within firms, I employ an event-study difference-in-differences design at the firm-by-year level. For this, let Y_{jt} denote an outcome for firm j in year t. My main outcome of interest are the raw log gender wage gap and the residual-adjusted gender wage gap. I also use gender-specific mean log wages within the firm. Specifically, I estimate:

$$Y_{jt} \;=\; \alpha_j \;+\; \lambda_t \;+\; \sum_{k\in\mathcal{K}} \beta_k \left(\mathbb{1}\{t - 2014 = k\} \times \text{Treat}_j\right) \;+\; \varepsilon_{jt}, \tag{4}$$

where α_j are firm fixed effects and λ_t are year fixed effects. The indicator Treat_j equals one if the firm is in the treatment group and zero otherwise. The event time k is measured relative to 2014 as the reference year. As I look at 2012 to 2018, $k \in \{-2, -1, +1, +2, +3, +4\}$, which corresponds to 2012–2013 and 2015–2018, so that 2014 is the omitted reference year. Standard errors are clustered at the firm level.

The coefficients of interest are the β_k. These are the dynamic difference-in-differences estimators. For the years after the minimum wage introduction ($k > 0$), β_k measures the change in the gender wage gap in treated firms relative to control firms, compared to the corresponding treated-control difference in 2014. A negative β_k implies that the gender wage gap within treated firms declines relative to the control group after the introduction of the minimum wage.

The identification of the causal effect of the policy relies on the parallel-trends assumption: absent the introduction of the minimum wage, the gender wage gap in treated and control firms would have the same trend (conditional on the fixed effects). In this setting, any post-2014 divergence can be attributed to the minimum wage. I test the parallel trend assumption using coefficients for the years before the minimum wage introduction ($k < 0$). If these are not statistically significant, this implies that treatment and control firms would follow parallel trends absent the introduction of the minimum wage.

The second identification assumption in this setting is the *Stable Unit Treatment Value Assumption (SUTVA)*. A potential bias arises from potential spillover effects in the economy and potential general equilibrium effects. My estimation strategy requires that control firms not be directly affected by the reform. However, control firms may still be indirectly affected by the reform through labor-market spillovers, driven by wage pressure from outside options (Demir, 2023) or worker reallocation across firms (Dustmann et al., 2022). To address these issues, I rely on the fact that any such spillovers would likely lead to a convergence of wages between treatment and control groups. Therefore, this would bias my results toward zero. Consequently, my findings can be interpreted as a conservative estimate of the minimum wage's true effect on the within-firm gender wage gap.

However, my empirical strategy faces other potential threats that could undermine the identification of the causal effect of the minimum wage policy. As with the previous claim, there may be anticipation effects within firms. If firms adjusted wages or employment in anticipation of the reform (e.g., in late 2014), part of the policy response could appear prior to 2015. This would bias post-2014 estimates toward zero and potentially generate non-zero lead effects. The event-study design helps detect anticipation through deviations in the $k = -1$ coefficient. However, if anticipation occurred in 2014, it may not be fully captured because 2014 is the reference year. Bossler (2017) argues that in 2014 uncertainty in expectations existed. However, given the evidence on non-compliance in the short run (Destatis, 2018; Caliendo et al., 2018; Bachmann et al., 2020), systematic anticipation effects and employment and wage effects remain unlikely.

Employment, hours, and the composition of the workforce (e.g., entry/exit of low-wage workers, substitution between part-time and mini-jobs, or differential retention of men and women) may all be affected by the minimum wage. Changes in composition may change the measured gender gap even if individual wages remain unchanged because my results are based on within-firm worker wages, and inversely. I partially address this by reporting complementary outcomes like firm composition measures (share female, share part-time) and analyzing both overall firm-year gaps and subgroup-specific gaps (e.g., stayers, hires, movers; full-time/part-time/mini-jobs). However, the estimates should be viewed as *equilibrium effects on within-firm gender wage gaps*, which may incorporate both composition and wage channels.

Other potential biases could arise from sectoral minimum wages and developments in these sectors. In Germany, some sectors were covered by sectoral minimum wages prior to the national reform. If treated firms are disproportionately located in such sectors, estimated effects might partly reflect the impact of sectoral minimum wages or sectoral changes. To overcome this, I implement robustness checks that exclude sectors with sectoral minimum wages during 2012–2018, based on WZ-2008 classifications. Any contemporaneous policy or macroeconomic shock is controlled for by year-fixed effects.

Most importantly, my research design may be biased by measurement errors in approximating working hours and in the subsequent definition of the treatment and control groups. I define the treatment and control groups using the share of minimum wage workers in the pre-reform year 2014. This *per se* is measured with a measurement error, as these are not the real working hours and henceforth real hourly wages. Misclassifications of firms around zero exposure can attenuate estimates. I mitigate extreme sensitivity by trimming the upper tail of exposure, and I conduct robustness checks using alternative exposure thresholds.

4 Results

4.1 Baseline results

Table 5 presents the baseline estimation results from equation 4. In all specifications, I include firm- and year-fixed effects, and cluster the standard errors at the firm level. As my reference year is 2014, the results can be interpreted as differential changes between exposed and non-exposed firms relative to 2014. In Panel A, I use the full sample of firm–year cells, and in Panel B I restrict the sample to stayers. The stayers are defined as workers who stay in the same firm from 2012 to 2018. Focusing on the stayers allows me to net out the *pure wage effects* of the minimum wage from compositional effects arising from workers moving and hiring. The specifications in Panel A include stayers, hirings, and movers in each year.[12]

Panel A shows that the wages increase after the introduction of the minimum wage for both genders. By 2018, log wages were higher by about 0.118 for men and 0.120 for women, corresponding to roughly a 12% increase relative to control firms and relative to the pre-reform baseline year 2014. The results suggest that women's wages rise slightly more in the later post-reform years, mainly reflecting the higher exposure to the minimum wage than men's. Turning to the within-firm gender wage gap, I observe only modest changes. In most years, the raw and residual wage gaps remain largely unchanged. In 2017, both estimates turn negative, with the raw gap declining by about 0.014 log points and the residual wage gap by about 0.012 log

[12] I do not show separate results for new hires or movers because many firms do not hire every year. The hire-and-mover individuals change from year to year, so the construction of firm-year gender gaps is noisy and not comparable over time.

points, relative to control firms and the pre-reform baseline year 2014. In contrast to the wage outcomes, the two gender wage gap outcomes show no evidence of differential pre-trends in the full sample. However, the stayer sample exhibits a negative and significant 2012 lead.

Table 5: Baseline Results

	Outcome			
	(1)	(2)	(3)	(4)
	$\ln \bar{w}_m$	$\ln \bar{w}_f$	raw gap	residual gap
Panel A: Full sample				
Treat × 2012	0.0458***	0.0508***	-0.0050	-0.0067
	(0.0061)	(0.0068)	(0.0087)	(0.0078)
Treat × 2013	0.0333***	0.0267***	0.0066	0.0057
	(0.0049)	(0.0052)	(0.0068)	(0.0060)
Treat × 2015	0.0782***	0.0652***	0.0130*	0.0040
	(0.0049)	(0.0051)	(0.0069)	(0.0063)
Treat × 2016	0.0985***	0.0948***	0.0036	0.0035
	(0.0058)	(0.0061)	(0.0082)	(0.0076)
Treat × 2017	0.1005***	0.1141***	-0.0135	-0.0118
	(0.0064)	(0.0070)	(0.0091)	(0.0084)
Treat × 2018	0.1179***	0.1204***	-0.0025	-0.0002
	(0.0069)	(0.0075)	(0.0097)	(0.0090)
Observations	42,497	42,497	42,497	42,013
Firms	6,071	6,071	6,071	6,042
Adj. R^2	0.8821	0.8837	0.7279	0.7122
Panel B: Stayer				
Treat × 2012	0.0000	0.0161***	-0.0194***	-0.0165**
	(0.0043)	(0.0050)	(0.0065)	(0.0066)
Treat × 2013	0.0002	0.0012	-0.0010	0.0034
	(0.0031)	(0.0036)	(0.0047)	(0.0047)
Treat × 2015	0.0289***	0.0260***	-0.0012	-0.0023
	(0.0033)	(0.0035)	(0.0046)	(0.0051)
Treat × 2016	0.0355***	0.0351***	-0.0017	-0.0016
	(0.0043)	(0.0049)	(0.0064)	(0.0066)
Treat × 2017	0.0279***	0.0472***	-0.0218***	-0.0210***
	(0.0047)	(0.0058)	(0.0072)	(0.0072)
Treat × 2018	0.0397***	0.0497***	-0.0137	-0.0121
	(0.0054)	(0.0067)	(0.0085)	(0.0083)
Observations	35,882	37,107	32,123	31,560
Firms	5,126	5,301	4,589	4,511
Adj. R^2	0.9429	0.9363	0.8941	0.8577

Notes: This table reports the baseline estimates from equation 4. Standard errors are clustered at firm-level. Panel A shows the full sample and Panel B shows stayers. 2014 is the reference year. $\ln \bar{w}_m$ and $\ln \bar{w}_f$ are the average male and female wages, respectively. Raw gap and residual wage gap are the raw gender wage gap and residual-adjusted gender wage gap as described in subsection 2.2. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In Panel B, I repeat the analysis for stayers only. Here, male wages increase modestly, while female wages rise more strongly in exposed firms. The effects grow with time. I interpret this as a greater relative improvement for women among incumbent workers. Consistent with the divergent wage developments, I observe reductions in within-firm gender wage gaps. Relative to control firms and relative to the pre-reform baseline year 2014, the raw gap falls by about 0.022 log points in 2017 and 0.014 log points in 2018. The residual wage gap estimates are very similar in size, with a decline of about 0.021 log points in 2017 and 0.012 log points in 2018.

As already discussed in section 2, I restrict the sample to firms that employ at least one woman and one man every year from 2012 to 2018. The number of observations can still differ across the outcomes and sample definitions. Also, differences between the raw and residual wage gaps are possible, as the residual wage gap requires non-missing covariates to be constructed. Also note that in the stayer subsample, a firm-year-cell may contain only male or only female stayers.

Overall, the baseline results demonstrate that the minimum wage led to sizeable wage gains for men and women and modest reductions in within-firm gender wage gaps in more exposed firms. The reduction in the gender wage gap is concentrated among stayers and is not driven by changes in a firm's workforce. This is in line with existing evidence that the German minimum wage raised wages and compressed the wage distribution (Caliendo et al., 2019; Bossler and Schank, 2023; Schmid, 2025) and that the minimum wage can reduce gender wage gaps (Bargain et al., 2019; Caliendo and Wittbrodt, 2022). My baseline results add to this literature by providing a new within-firm perspective: I show that the reduction in the gender wage gap within firm-year cells is stronger for incumbent workers.

My results also show that the wage gains through firm reallocation documented for the German minimum wage by Dustmann et al. (2022) do not necessarily imply a reduction in the gender wage gap. Even if workers move to high-wage firms, the gender wage gap still depends on whether women receive the same firm premium as men, since women capture a smaller share of the firm wage premium (Card et al., 2016). In the following analysis, I examine effects across the wage distribution and decompose how stayers, movers, and hires contribute to the observed changes in the gender wage gap.

In table 6, I examine whether the minimum wage affects within-firm gender gaps differently across the wage distribution. For this analysis, I compute male and female wage percentiles for each firm-year and use the log difference between them as the outcome. Similar to the baseline results, a negative coefficient implies a decline of the gender gap in more exposed firms. I run for each sub-sample equation 4 and therefore control for firm- and year-fixed effects, cluster standard errors at the firm level, and the reference year is 2014. The distributional results are based on raw hourly wages, as the purpose is solely to see where in the wage distribution the within-firm gender gap changes. Since the residual-adjusted wage gap is a constructed measure, I keep the quantile analysis on raw wages to avoid that possible measurement or specification issues from the residualization step affect the distributional patterns.

Table 6: Distributional Effects on the gender wage gaps

	Percentile of hourly wage distribution				
	(1) p10	(2) p25	(3) p50	(4) p75	(5) p90
Panel A: All					
Treat × 2012	-0.0059	-0.0245*	-0.0076	0.0035	0.0009
	(0.0166)	(0.0137)	(0.0085)	(0.0090)	(0.0099)
Treat × 2013	0.0135	-0.0054	0.0049	0.0083	0.0068
	(0.0138)	(0.0111)	(0.0067)	(0.0071)	(0.0080)
Treat × 2015	0.0209	0.0052	0.0079	0.0155**	0.0107
	(0.0135)	(0.0106)	(0.0069)	(0.0072)	(0.0081)
Treat × 2016	0.0113	-0.0013	-0.0055	0.0029	0.0029
	(0.0157)	(0.0128)	(0.0081)	(0.0088)	(0.0096)
Treat × 2017	-0.0325**	-0.0362***	-0.0133	0.0039	-0.0022
	(0.0164)	(0.0137)	(0.0090)	(0.0100)	(0.0109)
Treat × 2018	-0.0093	-0.0135	-0.0055	-0.0031	0.0093
	(0.0169)	(0.0141)	(0.0098)	(0.0106)	(0.0117)
Adj. R^2	0.5510	0.5980	0.7226	0.7312	0.7461
Obs. (firm×year)	42,497	42,497	42,497	42,497	42,497
Firms (clusters)	6,071	6,071	6,071	6,071	6,071
Panel B: Stayers					
Treat × 2012	-0.0273***	-0.0213***	-0.0197***	-0.0139**	-0.0148*
	(0.0084)	(0.0074)	(0.0067)	(0.0069)	(0.0076)
Treat × 2013	-0.0006	-0.0039	-0.0028	0.0026	0.0021
	(0.0057)	(0.0051)	(0.0048)	(0.0053)	(0.0059)
Treat × 2015	-0.0030	-0.0020	-0.0030	0.0033	-0.0022
	(0.0061)	(0.0051)	(0.0048)	(0.0053)	(0.0058)
Treat × 2016	0.0008	0.0010	-0.0066	-0.0035	-0.0008
	(0.0079)	(0.0071)	(0.0065)	(0.0072)	(0.0077)
Treat × 2017	-0.0289***	-0.0233***	-0.0212***	-0.0178**	-0.0175**
	(0.0089)	(0.0081)	(0.0073)	(0.0079)	(0.0085)
Treat × 2018	-0.0163	-0.0137	-0.0173**	-0.0115	-0.0126
	(0.0103)	(0.0094)	(0.0086)	(0.0094)	(0.0099)
Adj. R^2	0.8785	0.8874	0.8905	0.8979	0.8984
Obs. (firm×year)	32,123	32,123	32,123	32,123	32,123
Firms (clusters)	4,589	4,589	4,589	4,589	4,589

Notes: The outcome is the gender wage gap at each percentile, measured as the difference between the male and female hourly wage percentiles in logs. Each column reports a separate regression of equation 4, with firm- and year-fixed effects. Standard errors are clustered at the firm level. The reference year is 2014. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Panel A shows the results for the full sample. Here, I observe that the effects are mainly concentrated at the lower part of the distribution. In 2017, relative to control firms and relative to the pre-reform baseline year 2014, the gender gap fell by about 0.033 at the 10th percentile and 0.036 at the 25th percentile, while the coefficient at the median is also negative (around 0.013) but not statistically significant. In contrast, effects at the 75th and 90th percentiles are small and close to zero. This pattern is consistent with the main goal of a minimum wage policy, which targets low-wage jobs, and therefore, the effects on the gender wage gap are mostly as expected.

Panel B focuses on the stayer sample. Here, the gender gap declines significantly across almost all percentiles in 2017 (roughly from 0.018 to 0.029). The reductions are still evident in 2018, particularly at the 50th percentile. Compared to all workers in Panel A, the stayer results suggest that within-firm wage adjustments for stayers contribute strongly to the compression of the distributional gap, while changes in hires and movers may partly weaken the effects in Panel A. This means that the minimum wage affected the entire wage distribution, consistent with the evidence of Gregory and Zierahn (2022).

Overall, the distributional results explain the baseline effects: minimum wages reduce gender gaps at the bottom of the wage distribution within firms, and this is strongest among stayers. This interpretation aligns with the evidence that the German minimum wage increased wages and compressed the lower tail (Caliendo et al., 2019; Bossler and Schank, 2023; Schmid, 2025), and with the findings that minimum wages can reduce gender wage differences primarily where the wage floor binds (Caliendo and Wittbrodt, 2022; Autor et al., 2016). While prior work documents overall wage compression on the aggregate level, my results identify where the gender gap adjusts within firms and which workers drive the effects. Additionally, the results show that wage compression does not solely occur through reallocation across firms (Dustmann et al., 2022), but also through wage adjustments within existing worker-firm matches.

4.2 Decomposition by worker types

Building on the previous results, which reveal modest effects on the aggregate but stronger effects among incumbents and at the lower tail, I examine in the following which worker types account for the changes in the gender wage gap. For identifying this, I decompose year-to-year changes in the firm-level log gender wage gap Δgap_{all} into contributions from three types of workers ω: stayers (S), movers (M), and hires (H). I classify each worker into one of these worker types ω relative to year $t-1$. In my definition, *stayers* are employed in the same firm in $t-1$ and t, *movers* are new to the firm in t but were employed at a different firm in $t-1$, and *hires* enter the firm from non-employment (or are otherwise not observed as employed) in $t-1$. For each worker type ω the contribution is defined as $c_\omega = (g_\omega^t - g_\omega^{t-1}) \cdot e_\omega^t$ and shares as $s_\omega = 100 \cdot c_\omega/(c_S + c_M + c_H)$, where g_ω^t is the within-group log wage gap and e_ω^t the employment share of type ω in year t.

The shares might not add up to exactly 100% for two reasons. First, some components may be very small, so rounding can create minor deviations. Second, because the shares are calculated as $s_\omega = 100 \cdot (c_\omega/\Delta gap_{all})$, they are very sensitive when the denominator, Δgap_{all}, is close to zero or changes sign. This can yield very large or negative percentages even if the actual changes c_ω are small.

Table 7: Contributions of stayers, movers, and hires on the gender wage gap

From→To	Δgap_{all}	c_S	s_S (%)	c_M	s_M (%)	c_H	s_H (%)	N_{firms}
2014→2015	−0.0025	−0.0003	8.00	−0.0000	0.79	−0.0038	91.21	6,071
2015→2016	−0.0069	−0.0069	125.36	−0.0000	0.23	0.0014	−25.59	6,071
2016→2017	−0.0096	−0.0033	30.74	−0.0001	1.08	−0.0072	68.18	6,071
2017→2018	0.0040	0.0054	77.52	0.0002	2.24	0.0014	20.24	6,071

Notes: This table shows the contributions of stayers, movers, and hires to changes in the gender wage gap. It reports year-to-year changes in the firm-level gender log wage gap, Δgap_{all}, and the corresponding contributions c_ω and percentage shares s_ω of stayers (S), movers (M), and hires (H). Contributions are defined as $c_\omega = (g_\omega^t - g_\omega^{t-1}) \cdot e_\omega^t$ and shares as $s_\omega = 100 \cdot c_\omega/(c_S + c_M + c_H)$, where g_ω^t is the within-group log wage gap and e_ω^t the employment share of worker type ω in year t. N_{firms} refers to the number of firms in the destination year.

Table 7 decomposes year-to-year changes in the firm-level gender gap into contributions from stayers, movers, and hires. The results show that the relative importance of these groups differs across transitions. Between 2014 and 2015, the aggregate gap declines by 0.0025 log points. Most of this decline is accounted for by hires, which contribute about 0.0038 log points or roughly 91% of the total change, while the contribution of stayers is very small and movers are negligible.

Between 2015 and 2016, the aggregate gap decreased by 0.0069 log points. This decrease is fully explained by stayers, whose contribution amounts to 0.0069 log points. The mover component is again close to zero. The negative share for hires reflects that hires work in the opposite direction and partly offset the increase in the aggregate gap. From 2016 to 2017, the aggregate gap declines by 0.0096 log points. Here, both stayers and hires contribute to the compression, but hires account for the larger part of the decline, with about 68% compared to around 31% for stayers. Movers again play almost no role. From 2017 to 2018, the aggregate gap increases by 0.0040 log points. This increase is driven mainly by stayers, who account for around 78% of the total change, while hires explain about 20% and movers remain negligible.

Table 7 can also be interpreted as an extension of table 4. Table 4 shows, for each year, the overall raw gender wage gap in the data and, in a second step, decomposes the total wage gap into its within- and between-components. In contrast, table 7 gives for each year the difference in gap from year $t-1$ to year t with Δgap_{all} and then decomposes it into the contribution by worker type. For example, in column (1) of table 4, the rounded raw gender wage gap for 2015 is given as 0.1752 and for 2016 as 0.1682. The difference between these two rounded values is $-0.007(= 0.1682 - 0.1752)$ and before rounding -0.0069 which is the Δgap_{all} for the year-to-year change difference from 2015 to 2016 in table 7.

Overall, the decomposition confirms the interpretation from the baseline and distributional results: changes in the firm-level gender gap are driven mainly by stayers and hires, while movers contribute very little in all transitions. While Dustmann et al. (2022) highlights that worker reallocation causes wage increases following the introduction of the minimum wage, particularly for female workers, my study shifts the focus to the internal wage structure of firms. Specifically, I examine whether the policy led to a significant narrowing of the gender wage gap among employees staying within the same establishment.

4.3 Heterogeneity

In this section, I investigate whether the impact of the minimum wage on the gender wage gap varies significantly across different segments of the labor market. To this end, I examine the heterogeneity of the main results along three dimensions. First, I analyze heterogeneity by employment status, distinguishing between full-time, part-time, and marginally employed workers. Second, I test for differential effects by firm size, and third, by industry sector. For each specification, I re-estimate the baseline event-study on the respective subsamples for the two outcomes, the raw log wage gap and the residual-adjusted gender wage gap.

Employment status. Table 8 examines the heterogeneity by employment status, distinguishing between full-time, part-time, and marginally employed workers. It shows that the heterogeneity pattern differs sharply across employment types. The sample consists of all individuals in the firm. Panel A shows in column 1 that for full-time jobs, the effects on the raw gap are small and not significant throughout. In Panel B, the residual-adjusted wage gap is also negative after 2015, with coefficients between about -0.010 and -0.012 log points relative to control firms and the pre-reform baseline year 2014, but none of these estimates is statistically significant. For part-time, I do not find any evidence that the gender wage gap closes. In contrast, the gap widens in 2017, with positive coefficients of about 0.056 and 0.063

log points for the raw and residual wage gaps, respectively, where the residual-adjusted gap is statistically significant at the 5% level and the raw gap at the 10% level. This implies that the total compression effects observed in the previous sections are not due to part-time jobs.

Table 8: Heterogeneity by employment status

	Employment Category		
	(1) Full-time	(2) Part-time	(3) Marginal employment
Panel A: Raw gender wage gap			
Treat × 2012	0.0018	-0.0384	-0.4809***
	(0.0081)	(0.0294)	(0.1175)
Treat × 2013	0.0025	-0.0098	-0.5208***
	(0.0063)	(0.0239)	(0.1369)
Treat × 2015	-0.0068	0.0057	-0.4527***
	(0.0065)	(0.0220)	(0.0750)
Treat × 2016	-0.0071	0.0263	-0.6118***
	(0.0077)	(0.0263)	(0.0108)
Treat × 2017	-0.0100	0.0558*	-0.5500***
	(0.0084)	(0.0299)	(0.0732)
Treat × 2018	-0.0120	0.0238	-0.5538***
	(0.0095)	(0.0305)	(0.0886)
Adj. R^2	0.7716	0.6861	0.4949
Obs. (firm×year)	28,495	11,687	11,828
Firms (clusters)	4,466	2,298	2,492
Panel B: Residual-adjusted gender wage gap			
Treat × 2012	0.0012	-0.0402	0.1034
	(0.0078)	(0.0282)	(0.1684)
Treat × 2013	0.0053	-0.0167	0.1016
	(0.0062)	(0.0228)	(0.1893)
Treat × 2015	-0.0102	0.0127	0.0881
	(0.0063)	(0.0213)	(0.1223)
Treat × 2016	-0.0117	0.0312	-0.0977
	(0.0073)	(0.0248)	(0.1031)
Treat × 2017	-0.0109	0.0633**	-0.0188
	(0.0081)	(0.0289)	(0.0931)
Treat × 2018	-0.0100	0.0325	–
	(0.0093)	(0.0299)	(omitted)
Adj. R^2	0.7790	0.6956	0.4873
Obs. (firm×year)	28,240	11,264	10,982
Firms (clusters)	4,429	2,233	2,346

Notes: Each column reports a separate event-study regression at the firm×year level with firm and year fixed effects. Standard errors are clustered at the firm level. The reference year is 2014. For marginal employment in Panel B, Treat × 2018 is omitted due to collinearity. Significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The most interesting results appear in column 3 for the marginally employed. In Panel A for the raw gender wage gap, we see that there are very significant negative coefficients already in 2012 and 2013 before the reform. For this reason, I cannot assume parallel trends, as exposed firms were probably already moving differently relative to 2014 even before the minimum wage. However, for the residual-adjusted wage gap in Panel B, the pre-trends disappear and are not statistically significant. However, in this case, the last year is omitted due to collinearity.[13] One reason is that the raw gap is likely affected by changes in worker characteristics, such as education or age, that occurred before the reform. Another reason is that the adjusted gap controls for these factors, removing the pre-trends and showing that the actual wage-setting was more stable.

My findings for different employment types are consistent with recent studies on the German minimum wage. The fact that the largest but also unstable raw effects appear for marginally employed individuals aligns with evidence that marginal employment had the largest bite and the largest wage increases after 2015 (Caliendo et al., 2019). However, the pre-trends I observe in the raw gap suggest that these workers are highly sensitive to compositional shifts, a known issue in the literature on how firms restructured marginal jobs into regular ones (Bachmann et al., 2017). Furthermore, my result that the narrowing of the gap is somewhat more visible for full-time workers than for part-time workers aligns with the idea that the minimum wage improved wage structures more effectively in standard employment relationships (Dustmann et al., 2022). Finally, the lack of a significant reduction in the gender gap in part-time jobs may be due to the complex adjustments in working hours that firms made to offset higher hourly costs, as discussed by Bonin et al. (2018).

Firm size. Table 9 examines heterogeneity by firm size. For this, I divide firms by their 2014 employee count into quartiles. Panel B shows that, for firms in the smallest quartile, the residual wage gap is negative throughout the post-reform period. The coefficient amounts to about -0.021 log points in 2015, -0.013 in 2016, -0.029 in 2017, and -0.027 in 2018 relative to control firms and relative to the pre-reform baseline year 2014. In Panel A, the corresponding raw-gap coefficients are also negative in the later post period, for example about -0.018 in 2017, though less precisely estimated. In the middle quartiles, I observe weaker and less systematic patterns. Some coefficients are positive and significant, as in Q3 for 2015 in Panels A and B. Additionally, for the fourth quartile of firms, the coefficients are small and not statistically significant, suggesting limited average within-firm gap responses among the largest employers.

[13] In the marginal employment subsample, the residual wage gap outcome is available for comparatively few firm×year cells (e.g., because some firms have only male or only female marginally employed workers in a given year, so the firm-year gap cannot be computed). In 2018, this can leave too little independent variation in the Treat×2018 indicator once firm and year fixed effects are absorbed, so the regressor becomes (near-)perfectly collinear and is omitted by Stata.

Overall, my results show that the strongest results are in the smallest firms. This is expected because the minimum wage has a much larger effect on small firms with many low-wage workers (Bossler and Gerner, 2020). This explains why the gap narrows mostly in the Q1 group. On the other hand, large firms often pay higher wages anyway, so the new law does not change their internal pay as much (Dustmann et al., 2022). Other studies also find that the bite of the minimum wage is not the same for everyone. Small firms often have to adjust wages more directly when the law changes (Caliendo et al., 2018). This is consistent with my finding that the largest employers (Q4) were not substantially affected by the 2015 reform.

Table 9: Heterogeneity by firm size

	Firm Size quantile			
	(1) Q1 (smallest)	(2) Q2	(3) Q3	(4) Q4 (largest)
Panel A: Raw log wage gap				
Treat × 2012	-0.0205	0.0073	-0.0057	0.0005
	(0.0186)	(0.0183)	(0.0159)	(0.0111)
Treat × 2013	-0.0045	0.0066	0.0098	0.0177**
	(0.0140)	(0.0160)	(0.0129)	(0.0084)
Treat × 2015	0.0029	0.0080	0.0366***	0.0063
	(0.0144)	(0.0155)	(0.0131)	(0.0074)
Treat × 2016	0.0015	0.0034	0.0037	-0.0035
	(0.0177)	(0.0179)	(0.0144)	(0.0092)
Treat × 2017	-0.0178	-0.0231	-0.0069	-0.0168
	(0.0194)	(0.0205)	(0.0161)	(0.0103)
Treat × 2018	-0.0055	-0.0170	0.0164	-0.0108
	(0.0206)	(0.0221)	(0.0178)	(0.0108)
Obs. (firm×year)	13,216	8,771	9,940	10,570
Firms (clusters)	1,888	1,253	1,420	1,510
Adj. R^2	0.7494	0.6929	0.6742	0.7359
Panel B: Residual-adjusted wage gap				
Treat × 2012	-0.0287*	0.0033	0.0094	-0.0079
	(0.0173)	(0.0157)	(0.0138)	(0.0096)
Treat × 2013	-0.0064	0.0070	0.0153	0.0067
	(0.0128)	(0.0138)	(0.0105)	(0.0074)
Treat × 2015	-0.0208	0.0224	0.0218*	0.0032
	(0.0138)	(0.0144)	(0.0116)	(0.0063)
Treat × 2016	-0.0133	0.0259	0.0074	-0.0037
	(0.0167)	(0.0164)	(0.0129)	(0.0080)
Treat × 2017	-0.0290	0.0014	-0.0072	-0.0133
	(0.0186)	(0.0186)	(0.0143)	(0.0090)
Treat × 2018	-0.0268	0.0169	0.0149	0.0010
	(0.0199)	(0.0195)	(0.0158)	(0.0095)
Obs. (firm×year)	12,863	8,674	9,907	10,569
Firms (clusters)	1,864	1,248	1,420	1,510
Adj. R^2	0.7445	0.6590	0.6369	0.7111

Notes: Event-study estimates by firm-size quartiles based on firm employment in 2014. All models include firm- and year-fixed effects. Standard errors are clustered at the firm level. Standard errors in parentheses. Significance level: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Industry. To analyse heterogeneity by sector, I use the German Classification of Economic Activities 1993 (*Klassifikation der Wirtschaftszweige, Ausgabe 1993 (WZ 93)*). For more information, see Destatis (1994). For the heterogeneity analysis, I construct a coarse industry indicator based on the first digit of the classification. This groups firms into broad industry blocks such as manufacturing, utilities and construction, trade and hospitality, transport and finance, business services and public administration, education and health, and other services. This level of aggregation is large enough to identify sectoral differences while keeping the subsamples large and comparable. Table A.3 shows the corresponding industry definitions.[14]

Table 10 investigates whether within-firm gender gap responses differ across broad industries. The results show that gender wage gap compression is concentrated in a subset of industries and is strongest where the minimum wage is likely to bind. Specifically, utilities and construction show consistent and sizeable reductions, around −0.07 to −0.11 log points in 2016 to 2018, relative to control firms and to the pre-reform baseline year 2014, in Panel B. I also observe a persistent post-reform reduction in one of the manufacturing blocks, namely Manufacturing II, where the coefficients are negative and significant in 2016 to 2018 in Panel B and in 2017 to 2018 in Panel A. Trade and hospitality show a weaker but still notable reduction, with negative and significant coefficients in Panel A in 2017 and 2018.

By contrast, some industries show positive coefficients, indicating that the gender wage gap even increased. For example, education and health, as well as other services, display positive and often significant estimates in several post-reform years in both panels. In addition, some industries show significant pre-trends. This is the case for utilities and construction and education and health in Panel A, and also for utilities and construction and education and health in Panel B. This cautions against a causal interpretation of every single industry coefficient and suggests that pre-existing differential trends, measurement, or composition changes may matter in some sectors. The contrast between Panels A and B is informative here: when post-reform patterns persist after residual adjustment, as in utilities and construction and in Manufacturing II, they are more consistent with within-firm wage-setting changes.

Overall, my heterogeneity results align with the distributional evidence from the previous section. As the minimum wage primarily affects the lower tail, industries with a larger low-wage share and a stronger bite should have greater scope for compression, whereas industries with fewer affected jobs may show weaker responses, or even widening if other adjustments dominate

[14] Industry heterogeneity is based on a coarse aggregation of the WZ 93 code available in the data (w93_3_gen), constructed as $WZ1 = w93\_3\_gen/100$ and assigned to firms by the modal value over worker-year observations. This yields ten broad bins (0xx–9xx) and keeps subsamples sufficiently large, but some bins combine conceptually different activities. For example, WZ1 =6 may mix transport/telecommunications (60–64) with financial/insurance activities (65–67), and WZ1 =7 may mix real estate and business services (70–74) with public administration (75). Therefore, results for these mixed-service bins (and the residual category WZ1 = 9) should be interpreted as broad-sector evidence rather than as finely defined industry effects. Overall, WZ1=0 represents mainly agriculture, forestry, fishing; WZ1=1 to WZ1=3 manufacturing; WZ1=4 utilities and construction; WZ1=5 trade and accommodation/food services; WZ1=6 transport/communication and financial intermediation; WZ1=7 real estate/business services and public administration; WZ1=8 education and health/social work and WZ1=9 other services, households, extraterritorial organisations. Please note that, as I dropped agriculture-related industries, WZ1=0 is no longer included in my sample.

(Autor et al., 2016; Manning, 2021). In sectors where many workers are affected, the gap closes more within the firm. However, in other sectors, the gap may even widen because of different job types or wage-setting practices. This matches other studies showing that firms and their pay rules are very important for wage inequality (Card et al., 2016, 2018) and the gender gap (Blau and Kahn, 2017).

Table 10: Heterogeneity by firm industry

	Firm Industry								
	(1) Mfg. I	(2) Mfg. II	(3) Mfg. III	(4) Utilities/ Construction	(5) Trade/ Hospitality	(6) Transport/ Finance	(7) Business services/ Public admin.	(8) Education/ Health	(9) Other services
Panel A: Raw log wage gap									
Treat × 2012	-0.0693	-0.0092	0.0615	-0.0664***	0.0043	-0.0179	-0.0008	0.0726**	-0.0205
	(0.0636)	(0.0261)	(0.0420)	(0.0235)	(0.0185)	(0.0243)	(0.0191)	(0.0332)	(0.0373)
Treat × 2013	-0.0585	0.0179	0.0367	-0.0385**	-0.0106	0.0321	0.0171	0.0392	0.0367
	(0.0431)	(0.0181)	(0.0300)	(0.0196)	(0.0134)	(0.0207)	(0.0152)	(0.0262)	(0.0344)
Treat × 2015	-0.0121	-0.0098	-0.0958**	-0.0457**	0.0049	0.0317	0.0314**	0.0900***	0.0872**
	(0.0448)	(0.0190)	(0.0382)	(0.0185)	(0.0125)	(0.0242)	(0.0151)	(0.0258)	(0.0366)
Treat × 2016	-0.0124	-0.0440*	-0.0265	-0.0671***	0.0042	0.0086	0.0251	0.0319	0.1025***
	(0.0325)	(0.0254)	(0.0387)	(0.0222)	(0.0157)	(0.0234)	(0.0191)	(0.0294)	(0.0391)
Treat × 2017	0.0010	-0.0959***	0.0088	-0.0903***	-0.0356**	-0.0037	0.0265	0.0452	0.1027**
	(0.0329)	(0.0270)	(0.0422)	(0.0249)	(0.0176)	(0.0260)	(0.0207)	(0.0336)	(0.0448)
Treat × 2018	0.0077	-0.0796***	0.0102	-0.0962***	-0.0337*	-0.0091	0.0463**	0.0853***	0.1288***
	(0.0537)	(0.0288)	(0.0414)	(0.0260)	(0.0200)	(0.0302)	(0.0222)	(0.0326)	(0.0427)
Obs. (firm×year)	1,274	4,081	1,487	4,880	12,224	3,252	8,173	4,298	2,826
Firms (clusters)	182	583	213	699	1,748	465	1,171	614	404
Adj. R^2	0.6228	0.7464	0.7382	0.7893	0.6708	0.7502	0.7172	0.7699	0.6410
Panel B: Residual-adjusted wage gap									
Treat × 2012	-0.0713*	-0.0221	0.0032	-0.0610***	0.0004	-0.0225	-0.0026	0.0813***	0.0289
	(0.0406)	(0.0223)	(0.0355)	(0.0221)	(0.0158)	(0.0241)	(0.0169)	(0.0310)	(0.0364)
Treat × 2013	-0.0572	0.0064	0.0018	-0.0267	-0.0027	0.0209	0.0118	0.0329	0.0558
	(0.0364)	(0.0148)	(0.0256)	(0.0164)	(0.0123)	(0.0195)	(0.0129)	(0.0230)	(0.0355)
Treat × 2015	-0.0033	-0.0072	-0.0708**	-0.0510***	-0.0093	0.0394*	0.0230*	0.0635***	0.0429
	(0.0336)	(0.0166)	(0.0346)	(0.0167)	(0.0124)	(0.0221)	(0.0135)	(0.0244)	(0.0336)
Treat × 2016	0.0190	-0.0396*	-0.0189	-0.0745***	0.0019	0.0246	0.0264	0.0358	0.0779**
	(0.0431)	(0.0206)	(0.0354)	(0.0206)	(0.0149)	(0.0233)	(0.0174)	(0.0290)	(0.0365)
Treat × 2017	0.0773**	-0.0723***	0.0155	-0.1105***	-0.0270	0.0159	0.0078	0.0498*	0.0917**
	(0.0388)	(0.0226)	(0.0382)	(0.0246)	(0.0167)	(0.0257)	(0.0183)	(0.0301)	(0.0426)
Treat × 2018	0.0825*	-0.0632***	0.0201	-0.1014***	-0.0199	0.0185	0.0174	0.0798***	0.1125***
	(0.0423)	(0.0236)	(0.0398)	(0.0261)	(0.0188)	(0.0287)	(0.0203)	(0.0299)	(0.0387)
Obs. (firm×year)	1,271	4,074	1,486	4,842	11,996	3,225	8,109	4,274	2,734
Firms (clusters)	182	583	213	697	1,736	463	1,166	613	397
Adj. R^2	0.6394	0.7728	0.7409	0.8176	0.6223	0.7015	0.6952	0.7463	0.6099

Notes: Event-study estimates by industry. All models include firm- and year-fixed effects. Standard errors are clustered at the firm level. The reference year is 2014. Standard errors in parentheses. Industry groups correspond to the dataset-defined WZ1 aggregation based on WZ93 3-digit codes. Agriculture-related sectors are excluded from the current sample by construction and therefore do not appear as a separate category. Significance level: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.4 Robustness

In this subsection, I conduct five robustness checks to validate my baseline findings. First, I test the sensitivity of the exposure threshold by comparing firms with and without minimum wage exposure. Second, I address potential measurement error in the working hour approximation by using an alternative mapping from the SOEP (German Socio-Economic Panel) to the SIEED data. Third, to avoid approximating hours entirely, I construct a pre-reform anchored adjusted gender wage gap based on daily wages, which are directly observed in the SIEED. Then, I verify that the results are not driven by pre-existing sectoral minimum wages by excluding these specific industries from the estimation sample. Finally, I test how the results change if I only keep firms with more than 10 employees, as these firms are subject to the resignation protection law.

Alternative exposure thresholds. A potential concern with the baseline approach is that results might be sensitive to how I define treatment and control firms. To assess this, I test sensitivity at different treatment threshold definitions. I use three alternative binary definitions of firm exposure to the minimum wage. In my baseline estimations, exposure is measured as the share of a firm's workforce earning less than 8.50 euros per hour in 2014. Treated firms are defined as those with exposure to the minimum wage above zero, while control firms are those with zero exposure, i.e., no workers below 8.50 euros in 2014. I check three alternative cutoffs: (i) a high-exposure threshold of 50%, (ii) a threshold at the sample mean of positive exposure (13.9%), and (iii) a threshold at the 75th percentile of positive exposure (19.5%). For each cutoff, I redefine treatment firms as those with exposure at or above the respective threshold and restrict the comparison group to firms with zero exposure, dropping firms with intermediate exposure levels (0 < exposure < cutoff). For each definition, I report effects on both the raw and residual-adjusted firm-level gender gap. Table 11 presents results for all workers in Panel A and for the stayer sample in Panel B.

Across thresholds, the qualitative pattern from the baseline results in section 4.1 persists. In the full sample in Panel A, the estimated effects are mostly small and imprecise. In 2017, the coefficients turn negative across all threshold definitions, but they are not statistically significant. The point estimates range from about −0.007 to −0.015 log points for the raw and residual-adjusted wage gap measures. Under the stricter high-exposure cutoff in column (1) to column (2), the 2017 estimates are likewise negative, but again imprecisely estimated. Overall, the message remains that average gap compression in the full sample is limited and not very robust across alternative treatment thresholds. In Panel B, the stayer results are more pronounced and more stable across thresholds. Using the mean and 75th percentile cutoffs, the gender gap narrows relative to control firms and the pre-reform baseline year 2014 by roughly 0.024 to 0.027 log points in 2017. The magnitudes for the raw and residual measures are similar. In 2018, the coefficients remain negative under the mean and 75th percentile cutoffs, at around 0.007 to 0.013 log points, but they are no longer statistically significant. The high-exposure cutoff yields the same general sign pattern in 2017, but the estimates are much less precise and no longer statistically significant, consistent with the smaller sample size under the stricter definition.

Table 11: Alternative exposure thresholds

	exposure ≥ 50%		exposure ≥ mean		exposure ≥ p75	
	(1)	(2)	(3)	(4)	(5)	(6)
	Raw gap	Residual gap	Raw gap	Residual gap	Raw gap	Residual gap
Panel A: All						
Treat × 2012	-0.0181	-0.0162	-0.0074	-0.0104	-0.0063	-0.0107
	(0.0131)	(0.0125)	(0.0100)	(0.0091)	(0.0103)	(0.0094)
Treat × 2013	0.0013	-0.0018	0.0092	0.0052	0.0110	0.0063
	(0.0107)	(0.0098)	(0.0079)	(0.0070)	(0.0083)	(0.0073)
Treat × 2015	0.0236**	0.0077	0.0196**	0.0068	0.0212**	0.0096
	(0.0104)	(0.0103)	(0.0080)	(0.0074)	(0.0083)	(0.0078)
Treat × 2016	0.0152	0.0086	0.0083	0.0081	0.0088	0.0100
	(0.0124)	(0.0121)	(0.0094)	(0.0088)	(0.0097)	(0.0092)
Treat × 2017	-0.0072	-0.0147	-0.0116	-0.0148	-0.0142	-0.0139
	(0.0138)	(0.0135)	(0.0105)	(0.0098)	(0.0109)	(0.0102)
Treat × 2018	0.0223	0.0130	0.0097	0.0062	0.0084	0.0073
	(0.0146)	(0.0146)	(0.0112)	(0.0105)	(0.0116)	(0.0110)
Observations	19,271	18,875	30,590	30,118	28,140	27,677
Firms	2,753	2,727	4,370	4,342	4,020	3,992
Adj. R^2	0.7042	0.6908	0.7243	0.7069	0.7229	0.7052
Panel B: Stayer						
Treat × 2012	-0.0422***	-0.0314**	-0.0277***	-0.0258***	-0.0310***	-0.0305***
	(0.0116)	(0.0124)	(0.0078)	(0.0081)	(0.0083)	(0.0086)
Treat × 2013	-0.0060	0.0024	0.0004	0.0034	0.0006	0.0037
	(0.0091)	(0.0088)	(0.0057)	(0.0057)	(0.0061)	(0.0061)
Treat × 2015	0.0175*	0.0187*	0.0024	0.0000	0.0057	0.0033
	(0.0094)	(0.0106)	(0.0059)	(0.0064)	(0.0063)	(0.0069)
Treat × 2016	0.0246**	0.0256**	0.0014	0.0008	0.0056	0.0052
	(0.0113)	(0.0125)	(0.0077)	(0.0080)	(0.0081)	(0.0085)
Treat × 2017	-0.0101	-0.0086	-0.0269***	-0.0253***	-0.0253***	-0.0238**
	(0.0130)	(0.0140)	(0.0087)	(0.0090)	(0.0091)	(0.0095)
Treat × 2018	0.0076	0.0105	-0.0126	-0.0103	-0.0102	-0.0072
	(0.0144)	(0.0153)	(0.0101)	(0.0101)	(0.0105)	(0.0107)
Observations	13,272	12,920	21,735	21,214	19,810	19,332
Firms	1,896	1,848	3,105	3,033	2,830	2,764
Adj. R^2	0.8721	0.8395	0.8898	0.8534	0.8883	0.8508

Notes: Event-study coefficients from firm×year regressions with firm and year fixed effects. Exposure is measured as the 2014 share of a firm's workers earning less than 8.50 euros per hour. For each cutoff, treated firms are those with exposure at or above the threshold, whereas the control group comprises firms with zero exposure. Firms with intermediate exposure (0 < exposure < cutoff) are excluded. Panel A uses the full sample, and Panel B is restricted to the stayer sample. Standard errors are clustered at the firm level, and the reference year is 2014. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Pre-reform anchored adjusted gender wage gap. Another limitation of my analysis is that the residual-adjusted gender wage gap is constructed from log wage regressions which is based on econometric and measurement assumptions embedded in that specification. To address potential bias, I additionally construct a more robust outcome measure, which I call the *pre-reform anchored adjusted gap.* This outcome keeps the core idea of constructing an adjusted gender wage gap, but relies only on the raw gap and a pre-reform scaling factor. Specifically, the pre-reform anchored adjusted gap rescales the raw gap by each firm's pre-reform ratio in the year 2012, of residual-adjusted to raw gaps, holding this ratio fixed over time. This approach is motivated by concerns that some covariates used in the wage regressions (e.g., the returns to schooling and experience) may themselves respond to the minimum wage reform. In this case, potential post-treatment changes could constitute bad controls (Angrist and Pischke, 2009) and mechanically bias the residual-adjusted wage gap. This pre-reform anchored adjusted gap is conceptually related to counterfactual reweighting approaches that use fixed pre-treatment mappings/weights to construct adjusted outcomes, as in DiNardo et al. (1996). But it does not directly reweight the underlying wage distribution. Specifically, I compute in the pre-reform year 2012 the firm-specific ratio between the residual-adjusted and the raw gap,

$$\text{share}_{j,2012} = \frac{\text{gap}^{\text{resid}}_{j,2012}}{\text{gap}^{\text{raw}}_{j,2012}}, \qquad \text{gap}^{\text{synth}}_{j,t} = \text{gap}^{\text{raw}}_{j,t} \times \text{share}_{j,2012}.$$

Here, for each year t the raw gap is rescaled by a firm-specific adjustment share that is fixed at each firm j's 2012 level. If the post-reform dynamics documented in section 4.1 give the real causal effect of the minimum wage on the adjusted gender wage gap within firms, the pre-reform anchored adjusted gap analysis should reproduce the main pattern of results.

Table 12 confirms this prediction. In Panel A, column (1) for the full sample, the pre-reform anchored adjusted gap is close to zero in most years but turns negative in 2017 at about -0.004 log points. This mirrors the timing of the baseline residual wage gap results, although the effect is smaller and not statistically significant. In column (2) for stayers, the pre-reform anchored adjusted gap shows a stronger narrowing, with a decline in 2017 of around -0.009 log points relative to control firms and the pre-reform baseline year 2014, and continued negative effects in 2018 of about -0.005 log points. This is consistent with the baseline evidence that gap compression is most robust among incumbent workers.

Table 12: Pre-reform anchored adjusted gap and daily wages

	Sample	
	(1) Full sample	(2) Stayer sample
Panel A: Pre-reform adj. residual wage gap		
Treat × 2012	-0.0016	-0.0076***
	(0.0028)	(0.0025)
Treat × 2013	0.0022	-0.0004
	(0.0022)	(0.0018)
Treat × 2015	0.0042	-0.0005
	(0.0022)	(0.0018)
Treat × 2016	0.0012	-0.0007
	(0.0027)	(0.0025)
Treat × 2017	-0.0044	-0.0085***
	(0.0030)	(0.0028)
Treat × 2018	-0.0008	-0.0053
	(0.0032)	(0.0033)
Observations	42,497	32,123
Firms	6,071	4,589
Adj.R^2	0.7279	0.8941
Panel B: Daily mean gap		
Treat × 2012	-0.0041	-0.0260***
	(0.0131)	(0.0080)
Treat × 2013	-0.0065	-0.0141**
	(0.0106)	(0.0062)
Treat × 2015	0.0063	-0.0058
	(0.0099)	(0.0055)
Treat × 2016	-0.0107	-0.0065
	(0.0123)	(0.0080)
Treat × 2017	-0.0082	-0.0136
	(0.0134)	(0.0100)
Treat × 2018	-0.0195	-0.0203**
	(0.0147)	(0.0120)
Observations	42,497	32,123
Firms	6,071	4,589
Adj.R^2	0.7484	0.9273

Notes: The table reports event-study estimates with firm and year fixed effects. Standard errors are clustered at the firm level. The reference year is 2014. Column (1) uses the full sample, while column (2) restricts the sample to stayers. Panel A reports effects on the pre-reform anchored adjusted gap within-firm gender gap, constructed by scaling the firm-year raw log gap by the baseline adjustment share from 2012 (i.e., $\widehat{\text{gap}}_{jt}^{\text{syn}} = \text{raw\_log\_gap}_{jt} \times \text{share12}$, with $\text{share12} = \text{adj\_gap}_{2012}/\text{raw\_gap}_{2012}$ computed in the corresponding sample). Panel B reports effects on the firm-year gender gap in log daily wages (log of daily earnings) constructed in the spirit of Dauth and Eppelsheimer (2020). Standard errors are in parentheses. Significance level:$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In table 13, I repeat the distributional analysis using pre-reform anchored adjusted quantile gaps. Consistent with section 4.1, compression is concentrated toward the bottom of the distribution. In the all-firm sample in Panel A, effects are most pronounced in 2017 at the 10th and 25th percentiles and, to a lesser extent, at the median, while higher percentiles show little response. Among stayers in Panel B, reduction is wider and more persistent: the estimates are negative and statistically significant across all percentiles in 2017, including the upper percentiles. They remain negative in 2018, but with less precision and statistical significance outside the median.

Overall, the pre-reform anchored adjusted gap approach provides a relevant robustness check for validating the main findings. The results remain broadly unchanged regarding both the timing of the effects and the concentration in the lower part of the wage distribution. This confirms that the main conclusions remain the same, regardless of how the residualization is specifically calculated.

Table 13: Pre-reform anchored adjusted quantile gaps

	Percentiles				
	(1) p10	(2) p25	(3) p50	(4) p75	(5) p90
Panel A: Full sample					
Treat × 2012	-0.0019	-0.0080*	-0.0025	0.0012	0.0003
	(0.0054)	(0.0045)	(0.0028)	(0.0029)	(0.0032)
Treat × 2013	0.0044	-0.0018	0.0016	0.0027	0.0022
	(0.0045)	(0.0036)	(0.0022)	(0.0023)	(0.0026)
Treat × 2015	0.0068	0.0017	0.0026	0.0051**	0.0035
	(0.0044)	(0.0035)	(0.0023)	(0.0023)	(0.0026)
Treat × 2016	0.0037	-0.0004	-0.0018	0.0009	0.0010
	(0.0051)	(0.0042)	(0.0026)	(0.0029)	(0.0031)
Treat × 2017	-0.0106**	-0.0118***	-0.0043	0.0013	-0.0007
	(0.0054)	(0.0045)	(0.0029)	(0.0033)	(0.0036)
Treat × 2018	-0.0030	-0.0044	-0.0018	-0.0010	0.0031
	(0.0055)	(0.0046)	(0.0032)	(0.0035)	(0.0038)
Observations	42,497	42,497	42,497	42,497	42,497
Firms	6,071	6,071	6,071	6,071	6,071
Adj. R^2	0.5510	0.5980	0.7226	0.7312	0.7461
Panel B: Stayer sample					
Treat × 2012	-0.0106***	-0.0083***	-0.0077***	-0.0054**	-0.0058*
	(0.0033)	(0.0029)	(0.0026)	(0.0027)	(0.0030)
Treat × 2013	-0.0002	-0.0015	-0.0011	0.0010	0.0008
	(0.0022)	(0.0020)	(0.0019)	(0.0021)	(0.0023)
Treat × 2015	-0.0012	-0.0008	-0.0012	0.0013	-0.0009
	(0.0024)	(0.0020)	(0.0019)	(0.0021)	(0.0023)
Treat × 2016	0.0003	0.0004	-0.0026	-0.0014	-0.0003
	(0.0031)	(0.0028)	(0.0025)	(0.0028)	(0.0030)
Treat × 2017	-0.0113***	-0.0091***	-0.0083***	-0.0069**	-0.0068**
	(0.0035)	(0.0032)	(0.0029)	(0.0031)	(0.0033)
Treat × 2018	-0.0064	-0.0053	-0.0068**	-0.0045	-0.0049
	(0.0040)	(0.0037)	(0.0034)	(0.0037)	(0.0039)
Observations	32,123	32,123	32,123	32,123	32,123
Firms	4,589	4,589	4,589	4,589	4,589
Adj. R^2	0.8785	0.8874	0.8905	0.8979	0.8984

Notes: Coefficients from firm×year regressions with firm and year fixed effects. Standard errors clustered at the firm level. Outcomes are pre-reform-anchored adjusted gaps for the 10th, 25th, 50th, 75th, and 90th percentiles. The reference year is 2014. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Daily wages. A further limitation of my analysis is that the administrative data do not report working hours. Therefore, a similar pattern across hourly- and daily-wage gaps suggests that the main conclusions are not biased by the working hours imputation from the SOEP (2023). To assess whether my baseline and distributional results are sensitive to this potential source of measurement error, I construct an alternative wage measure at the daily level that does not rely on imputed hours, as it is directly observed in the SIEED data. I then replicate the distributional event-study using within-firm gender gaps in log daily wages as outcomes. While calculating the hourly wage involves dividing daily earnings by imputed hours, the daily wage measure does not rely on this method. Therefore, the presence of similar patterns in both hourly and daily wage gaps indicates that my main conclusions remain valid despite the hours imputation. This robustness check helps to reduce the bias of the hours approximation in the outcome while there is still noise in the regressor, as the exposure to the minimum wage is necessarily still defined at the hourly wage level with the working hours imputation.

Table 14: Distributional effects of daily log wage gender wage gap

	Percentiles				
	(1)	(2)	(3)	(4)	(5)
	p10	p25	p50	p75	p90
Panel A: Full sample					
Treat × 2012	0.0018	-0.0249	-0.0197	-0.0012	0.0007
	(0.0268)	(0.0227)	(0.0138)	(0.0120)	(0.0125)
Treat × 2013	0.0005	-0.0255	-0.0153	-0.0029	-0.0020
	(0.0222)	(0.0185)	(0.0112)	(0.0098)	(0.0104)
Treat × 2015	0.0351	0.0017	-0.0132	-0.0022	0.0020
	(0.0225)	(0.0172)	(0.0106)	(0.0091)	(0.0095)
Treat × 2016	0.0104	-0.0191	-0.0245^{*}	-0.0206^{*}	-0.0134
	(0.0269)	(0.0218)	(0.0126)	(0.0112)	(0.0115)
Treat × 2017	0.0021	-0.0214	-0.0205	-0.0108	-0.0110
	(0.0273)	(0.0227)	(0.0141)	(0.0126)	(0.0131)
Treat × 2018	-0.0062	-0.0287	-0.0321^{**}	-0.0290^{**}	-0.0076
	(0.0292)	(0.0241)	(0.0154)	(0.0139)	(0.0146)
Observations	42,497	42,497	42,497	42,497	42,497
Firms (clusters)	6,071	6,071	6,071	6,071	6,071
Adj. R^2	0.5628	0.6211	0.7348	0.7585	0.7771
Panel B: Stayer sample					
Treat × 2012	-0.0330^{***}	-0.0262^{***}	-0.0266^{***}	-0.0204^{**}	-0.0241^{***}
	(0.0109)	(0.0098)	(0.0084)	(0.0080)	(0.0086)
Treat × 2013	-0.0202^{***}	-0.0201^{***}	-0.0166^{***}	-0.0060	-0.0084
	(0.0076)	(0.0069)	(0.0064)	(0.0066)	(0.0071)
Treat × 2015	-0.0094	-0.0057	-0.0073	-0.0031	-0.0050
	(0.0075)	(0.0064)	(0.0056)	(0.0058)	(0.0064)
Treat × 2016	-0.0051	-0.0005	-0.0124	-0.0104	-0.0057
	(0.0103)	(0.0092)	(0.0081)	(0.0083)	(0.0088)
Treat × 2017	-0.0175	-0.0078	-0.0152	-0.0155	-0.0163
	(0.0131)	(0.0115)	(0.0102)	(0.0101)	(0.0107)
Treat × 2018	-0.0285^{*}	-0.0162	-0.0246^{**}	-0.0196	-0.0164
	(0.0161)	(0.0139)	(0.0121)	(0.0122)	(0.0127)
Observations	32,123	32,123	32,123	32,123	32,123
Firms (clusters)	4,589	4,589	4,589	4,589	4,589
Adj. R^2	0.9108	0.9220	0.9240	0.9310	0.9311

Notes: Event-study coefficients from firm×year regressions with firm and year fixed effects. Standard errors are clustered at the firm level. Columns report gender gaps in imputed daily log wages at the 10th, 25th, median, 75th, and 90th percentiles. The reference year is 2014. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In table 12 Panel B, the gender gap becomes more negative after the reform. This is most visible for stayers, where the effect reaches about -0.020 log points in 2018. This matches my baseline results in table 5, where stayers also show a significant narrowing of the gap by about -0.022 log points in 2017. The distributional results in table 14 show a similar pattern: in the full sample, the gap closes mostly in the middle of the distribution, with weakly significant effects at the median and the 75th percentile in 2016 and stronger significant effects at the median and the 75th percentile in 2018. This is broadly similar to my baseline results, where the compression is strongest in the lower and middle parts of the distribution, while the effects at the top are small and not significant.

Overall, the daily wage results largely mirror the patterns observed in the baseline hourly wage analysis. Therefore, this suggests that my core findings in this paper are not a result of the external approximated working hours.

Sectoral minimum wages. In my last robustness check, I investigate whether my results are affected by minimum wages that already existed in certain sectors (Demir, 2023). To ensure to capture only the effect of the 2015 national minimum wage, I rerun the analysis excluding industries with sector-specific minimum wage agreements. Specifically, I exclude all firms operating in sectors that were subject to sectoral minimum wages at any point between 2012 and 2018. I define these sectors using the WZ2008 (3-digit) classification Destatis (2010). Table A.4 summarizes the mapping used to classify sectors with sectoral minimum wages. This restriction ensures that the estimated effects are not confounded by pre-existing wage floors that could mechanically compress wages independently of the 2015 statutory minimum wage.

Table 15 shows that my results remain broadly unchanged even when excluding industries that already had their own minimum wages. In the full sample, the effects are small and not significant. However, for stayers, the gap narrows clearly after 2015. For this group, the raw gap decreases significantly in 2017 by -0.0222 and in 2018 by -0.0164 relative to control firms and relative to the pre-reform baseline year 2014. The residual wage gap also shows a significant drop in 2017 by -0.0162. This aligns with my main findings, indicating that the narrowing of the gap is most pronounced for workers who remain in their firms and primarily occurs in the later years. Because these patterns are evident even in sectors without previous agreements, they confirm that the national minimum wage is the main driver of my results.

Table 15: Sectors without sectoral minimum wages

	(1) Raw gap	(2) Residual gap
Panel A: Full sample		
Treat × 2012	0.0041	0.0021
	(0.0097)	(0.0086)
Treat × 2013	0.0149**	0.0129*
	(0.0076)	(0.0067)
Treat × 2015	0.0138*	0.0077
	(0.0078)	(0.0072)
Treat × 2016	0.0081	0.0120
	(0.0090)	(0.0083)
Treat × 2017	-0.0122	-0.0035
	(0.0100)	(0.0092)
Treat × 2018	-0.0011	0.0069
	(0.0109)	(0.0100)
Observations	32,711	32,306
Firms	4,673	4,647
Panel B: Stayers		
Treat × 2012	-0.0110	-0.0127*
	(0.0072)	(0.0073)
Treat × 2013	0.0013	0.0049
	(0.0054)	(0.0054)
Treat × 2015	0.0047	0.0052
	(0.0051)	(0.0055)
Treat × 2016	0.0035	0.0064
	(0.0069)	(0.0072)
Treat × 2017	-0.0222***	-0.0162**
	(0.0075)	(0.0076)
Treat × 2018	-0.0164*	-0.0078
	(0.0089)	(0.0087)
Observations	24,640	24,140
Firms	3,520	3,451

Notes: Event-study estimates for firms in sectors without sectoral minimum wages. All models include firm- and year-fixed effects. Standard errors are clustered at the firm level. The omitted reference year is 2014. Standard errors are reported in parentheses. Panel A uses the full sample, and Panel B restricts to the stayer sample. Column (1) reports the raw log wage gap, and column (2) reports the residual-adjusted wage gap. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Dismissal protection law. As an additional robustness check, I restrict the sample to firms with more than ten employees in each year. The motivation for this is the German dismissal protection law *(Kündigungsschutzgesetz)*, which applies in establishments that employ more than ten workers. In firms above this threshold separations are subject to more regulation, and therefore dismissal costs are likely to be higher. This condition can influence minimum wage adjustments, as firms may depend less on dismissals and more on internal wage-setting, wage compression, or other internal adjustments. If my main results are really driven by wage-setting effects of the statutory minimum wage, they should also appear in this subsample of firms that are more strongly covered by dismissal protection.

Table 16 shows that the main pattern remains very similar in this subsample. In the full sample, the coefficients are mostly small and not statistically significant. In the stayer sample, however, both the raw and the residual-adjusted gender wage gaps decline in 2017 by about -0.017 log points relative to control firms and relative to the pre-reform baseline year 2014. In 2018, the coefficients are close to zero. Thus, even among firms above the dismissal protection law threshold, gap compression is concentrated among incumbent workers and is most pronounced in the later post-reform period. This supports my interpretation that the minimum wage mainly changed the within-firm wage structure rather than only inducing composition changes through worker turnover.

Table 16: Firms subject to the dismissal protection law

	(1) Raw gap	(2) Residual gap
Panel A: Full sample		
Treat × 2012	-0.0022	-0.0043
	(0.0096)	(0.0086)
Treat × 2013	0.0197***	0.0125*
	(0.0074)	(0.0065)
Treat × 2015	0.0181**	0.0094
	(0.0074)	(0.0066)
Treat × 2016	0.0026	0.0057
	(0.0086)	(0.0076)
Treat × 2017	-0.0143	-0.0078
	(0.0096)	(0.0084)
Treat × 2018	0.0018	0.0112
	(0.0100)	(0.0089)
Observations	18,116	18,091
Firms	2,588	2,588
Adj. R^2	0.7306	0.6920
Panel B: Stayers		
Treat × 2012	-0.0133*	-0.0175**
	(0.0070)	(0.0070)
Treat × 2013	0.0026	0.0005
	(0.0052)	(0.0052)
Treat × 2015	-0.0004	0.0001
	(0.0056)	(0.0058)
Treat × 2016	-0.0040	-0.0031
	(0.0071)	(0.0072)
Treat × 2017	-0.0167**	-0.0163**
	(0.0079)	(0.0080)
Treat × 2018	-0.0002	0.0016
	(0.0091)	(0.0090)
Observations	16,191	16,093
Firms	2,313	2,299
Adj. R^2	0.8938	0.8359

Notes: Event-study estimates for the subsample of firms with more than 10 employees in every year from 2012 to 2018. All models include firm- and year-fixed effects. Standard errors are clustered at the firm level. The omitted reference year is 2014. Standard errors are reported in parentheses. Panel A uses the full sample, and Panel B restricts the sample to stayers. Column (1) reports the raw log wage gap, and column (2) reports the residual-adjusted wage gap. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5 Conclusion

This paper studies how the introduction of the German statutory minimum wage in 2015 affected the gender wage gap within firms. Using the Sample of Integrated Employer–Employee Data (SIEED), I employ an event study design with firm and year fixed effects and compare firms exposed to the minimum wage with firms that are *not*. The main outcomes of interest are (i) first, the raw gender wage gap, and (ii) second, a residual wage gap accounting for differences in worker characteristics, such as age or education. To identify where wage effects occur, I additionally examine the effects along the wage distribution and across worker type definitions. Additionally, I conduct several robustness checks and analyze heterogeneity across various dimensions.

The results show that, on average, treated firms experienced significant wage increases for both men and women, with women experiencing an increase of around 10% in 2017 and men by 11% in 2017, and larger gains in the years following the introduction. Women who stay within the same firm experience wage gains of 4% while men who stay experience 2%. These wage gains contributed to a modest reduction in the within-firm gender wage gap, particularly noticeable in 2017 among employees who stayed with the firm, with a reduction of around 2%. Estimates across the wage distribution indicate that the strongest effects are observed around the median and in the lower-middle percentiles. A decomposition of changes in the gender wage gap shows that the compression of the gap is mainly due to employees who remain at the same firm, rather than to new hires or movers to other firms. Several robustness checks, such as using log daily wages (which do not rely on imputed hours) or excluding sectors with sectoral minimum wages, support the baseline estimates.

Overall, my findings are in line with the German minimum wage literature, which documents wage gains (Bachmann et al., 2020; Caliendo et al., 2018; Bossler and Gerner, 2020), reduction of the gender wage gap (Caliendo and Wittbrodt, 2022; Schmid, 2025), and reallocation across firms and jobs (Dustmann et al., 2022). I contribute to this extensive body of research by demonstrating that these adjustment processes are associated with the compression of the gender gap within firms. Since the effects are concentrated among incumbents in the lower- to middle part of the wage distribution, these are the areas where the reform appears to promote gender equity within firms. Some limitations still remain. As working hours are not recorded in the administrative data, I approximate them with data from the Socio-economic Panel (SOEP, 2023). Second, the adjusted gender gap measures are constructed using the approximated hours. Mechanisms such as working hour adjustments or promotions still cannot be tested directly. From a policy perspective, my results suggest that although minimum wage policies mainly aim to reduce labor market inequality, they unintentionally reduce gender-based inequality and bring pay equity within firms.

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Appendix

Table A.1: Imputation of Hourly Wages

(1) Year	(2) Gender	(3) Status	(4) Avg.hours/day
2012	Male	Full-time	5.82
2012	Male	Part-time	3.59
2012	Male	Marginal	1.91
2012	Female	Full-time	5.63
2012	Female	Part-time	3.34
2012	Female	Marginal	1.62
2013	Male	Full-time	5.81
2013	Male	Part-time	3.55
2013	Male	Marginal	1.90
2013	Female	Full-time	5.63
2013	Female	Part-time	3.37
2013	Female	Marginal	1.67
2014	Male	Full-time	5.81
2014	Male	Part-time	3.62
2014	Male	Marginal	1.97
2014	Female	Full-time	5.64
2014	Female	Part-time	3.42
2014	Female	Marginal	1.62
2015	Male	Full-time	5.82
2015	Male	Part-time	3.65
2015	Male	Marginal	1.78
2015	Female	Full-time	5.65
2015	Female	Part-time	3.43
2015	Female	Marginal	1.59
2016	Male	Full-time	5.82
2016	Male	Part-time	3.68
2016	Male	Marginal	1.71
2016	Female	Full-time	5.66
2016	Female	Part-time	3.44
2016	Female	Marginal	1.53
2017	Male	Full-time	5.81
2017	Male	Part-time	3.56
2017	Male	Marginal	1.87
2017	Female	Full-time	5.65
2017	Female	Part-time	3.46
2017	Female	Marginal	1.47
2018	Male	Full-time	5.81
2018	Male	Part-time	3.52
2018	Male	Marginal	1.64
2018	Female	Full-time	5.64
2018	Female	Part-time	3.48
2018	Female	Marginal	1.48

Notes: This table reports average working hours per calendar day by year, gender, and employment status. The cell means are used to impute working hours for observations with missing working-time information when constructing gross hourly wages from monthly earnings. Source: Own calculations based on SOEP.

Table A.2: Worker-level wage regression used to construct residuals

	(1) ln(hourly wage)
Marginal part-time workers	-0.7106***
	(0.0134)
Marginal part-time workers (household cheque)	-0.3926***
	(0.0061)
Experience	0.0192***
	(0.0006)
Experience squared	-0.0003***
	(0.0000)
Part-time indicator	-0.0657***
	(0.0078)
Schooling	0.0424***
	(0.0013)
Firms (clusters)	6,070
Adj. R^2	0.6572
Observations	1,462,240
Firm fixed effects	Yes
Year fixed effects	Yes
SE clustered at firm level	Yes

Notes: The table reports the worker-level regression used to construct residual wages. The dependent variable is log hourly wages. Controls include employment status indicators, experience and experience squared, a part-time indicator, and schooling. Firm and year fixed effects are absorbed. Standard errors are clustered at the firm level. Residuals from this regression are used to construct the residual-adjusted within-firm gender wage gap. Significance level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.3: Interpretation of the industry groups

(1) WZ1	(2) Approx. WZ93 divisions	(3) Description (coarse industry block)
0	01–09	Agriculture, forestry, fishing
1	10–19	Manufacturing (e.g. food, textiles, apparel)
2	20–29	Manufacturing (e.g. wood/paper, chemicals, plastics)
3	30–39	Manufacturing (e.g. machinery, electrical equipment, transport equipment)
4	40–45	Utilities and construction
5	50–55	Trade and accommodation/food services
6	60–67	Transport/communication and financial intermediation
7	70–75	Real estate/business services and public administration
8	80–85	Education and health/social work
9	90–99	Other services, households, extraterritorial organisations

Notes: These groups are not official WZ 1993 sections. They are a dataset-defined aggregation, defined as $wz1 = w93\_3\_gen/100$, and then assigned to each firm as its modal value across worker-year observations. *Source:* Destatis (1994).

Table A.4: Sectors with sectoral minimum wages with WZ mapping indicative at 3-digit level

(1) Sector (English / German)	(2) WZ 2008 (3-digit)
Waste management incl. street cleaning & winter service / Abfallwirtschaft inkl. Straßenreinigung, Winterdienst	381–383; 812
Building cleaning / Gebäudereinigung	812
Construction (main & finishing trades) / Bauhaupt- und Ausbaugewerbe	411–433, 439
Roofing trade / Dachdeckerhandwerk	439
Painting & varnishing / Maler- und Lackiererhandwerk	433
Scaffolding / Gerüstbauerhandwerk	439
Electrical installation (assembly) / Elektrohandwerk (Montage/Installation)	432
Security services & cash/valuables transport / Wach- & Sicherheitsdienste, Geld- & Wertdienste	801–802
Temporary agency work (statutory floor) / Arbeitnehmerüberlassung (Lohnuntergrenze)	782
Care sector (in-/outpatient) / Pflegebranche (PflegeArbbV)	869, 871, 873, 881
Stonemasonry & stone sculpting / Steinmetz- & Steinbildhauerhandwerk	237
Chimney sweeping / Schornsteinfegerhandwerk	812
Adult & continuing education / Aus- und Weiterbildungsdienstleistungen	855
Textiles & clothing industry / Textil- und Bekleidungsindustrie	13–14
Laundry services (object customers) / Wäschereidienstleistungen (Objektkundengeschäft)	960
Hairdressing / Friseurhandwerk	960
Meat industry / Fleischwirtschaft	101
Agriculture, forestry & horticulture / Land-, Forstwirtschaft, Gartenbau	01–03
Special workers in hard coal mining (expired 03/2015) / Bergbauspezialarbeiter Steinkohlebergwerke	051

Notes: This table shows sectors with sectoral minimum wages at any point between 2012 and 2018. Sources: Deutscher Bundestag (2012, p. 33), Bundesregierung (2015, pp. 2–4), and Gemeinsames Tarifregister Berlin und Brandenburg (2021); Sozialpolitik (2018).