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Employment Effects of a Statutory Minimum Wage: Evidence from a National Reform of the German Apprenticeship Market

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Employment Effects of a Statutory Minimum Wage: Evidence from a National Reform of the German Apprenticeship Market

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September 22, 2025

Abstract

To enhance the attractiveness of vocational education and training and to secure an adequate supply of skilled labour, the German government introduced a statutory minimum apprenticeship wage. Since January 1, 2020, apprentices who start their training have been entitled to a minimum wage that increases annually. Using administrative register data on apprenticeship contracts, we estimate the causal effect of this legislation on apprentice employment. Exploiting regional and occupational variation in the share of apprenticeships paid at the minimum wage, we apply standard difference-in-differences, triple-difference, and synthetic difference-in-differences models. Our results indicate that the minimum apprenticeship wage increased the number of apprenticeship contracts while reducing the contract termination rate in low-wage occupations. We also find considerable heterogeneity across occupations, which may be best explained by differences in exposure to skilled labour shortages and changes in apprentices' educational attainment.

JEL-Code: J08, J24, J38, C23

Keywords: minimum wage, apprenticeship market, employment effects, difference-in-differences, triple difference, synthetic difference-in-differences

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1 Introduction

The dual vocational training system is a cornerstone of the German education framework and plays a crucial role in ensuring a skilled workforce, maintaining the competitiveness of the German economy, and keeping youth unemployment rates low. However, this system has faced significant challenges in recent years, including declining numbers of newly commenced apprenticeship contracts and relatively high contract termination rates (Bundesinstitut für Berufsbildung [2025]). Against this backdrop, the German government introduced, among other measures, a national minimum apprenticeship wage effective January 1, 2020, to ensure that apprentices are fairly remunerated. The aim of this reform is to enhance the attractiveness of vocational education and training (VET) and to ensure a long-term adequate supply of skilled labour (Bundesministerium für Bildung und Forschung [2019]). It was expected to affect about 10 to 15% of all training firms (Wenzelmann and Pfeifer [2018]), with an above-average exposure in East Germany and among small and medium-sized firms (Dietrich [2019]).

From a labour economics perspective, the introduction of a minimum apprenticeship wage raises critical questions about its intended and unintended effects. While the effect of minimum wages on overall employment has been extensively studied (for a comprehensive meta-analysis, see Dube [2019]), there is much less evidence on how wage floors affect the employment of apprentices. On the one hand, the minimum wage could improve the attractiveness of VET, particularly in low-paid occupations, leading to a greater supply of apprentices. On the other hand, a legally mandated wage floor might lead to a reduction in available apprenticeship positions if firms are unable or unwilling to bear the associated higher costs. Furthermore, higher wages could alter selection criteria on both the employer and the apprentice side, potentially influencing contract stability and separation rates.

Thus, theoretical predictions about the net effects of a minimum apprenticeship wage on apprenticeship employment and contract stability are ambiguous and highlight the need for robust empirical evidence. Our study addresses this gap by providing causal evidence on the effects of Germany's minimum apprenticeship wage reform on employment and contract stability, using variation in exposure across regions as a quasi-experimental setting. Specifically, we address four research questions: (1) Did the minimum apprenticeship wage affect the number of newly commenced apprenticeship contracts? (2) Did it affect separation rates? (3) Are there heterogeneous effects across occupations? (4) If so, what explains these differences?

To answer these questions, we use administrative register data covering all newly commenced apprenticeship contracts between 2015 and 2023. By aggregating the data at the district and district×occupation level, we obtain a panel structure that enables us to isolate the reform effect from cohort-, district-, and occupation-specific factors using difference-in-differences (DiD) methods. Besides standard DiD estimations, we apply a synthetic difference-in-differences (sDiD) approach and a triple difference approach (DDD) to estimate causal effects. In the standard DiD approach, we compare changes in outcomes between districts with different levels of exposure to the minimum wage to control for time-invariant, unobserved heterogeneity, through district-specific fixed effects. The sDiD extends this approach by constructing a weighted combination of untreated districts to closely approximate the counterfactual scenario for treated districts. To address potential violations of the conditional common trends assumption arising from general differences in trends in the treatment and control group, we estimate DDD models, where we add the difference in outcomes between treatment and control districts for high-wage apprenticeship occupations, which are unaffected by the minimum wage legislation.

Our baseline DiD estimates suggest that the minimum apprenticeship wage increased the number of new apprenticeship contracts in low-wage occupations by about 8%, while it reduced the average contract termination rate by about 15%. The majority of robustness checks, including sDiD estimates, confirm these baseline results. However, when we estimate the DDD models, both effects – on new contracts and separation rates – are smaller and statistically insignificant. Thus, the DDD estimates suggest that our baseline and sDiD models overestimate the average treatment effect on the treated (ATT) in low-wage occupations, potentially due to differential impacts of the COVID-19 pandemic on the treatment and control groups.

Analysing the low-wage occupations most affected by the reform, we find robust evidence of substantial heterogeneity across occupations. While the minimum apprenticeship wage had a positive effect on the number of new apprenticeship contracts in agriculture, wood construction, and metal construction, it had a negative effect in sanitation, heating, ventilating, and air conditioning (HVAC), as well as hairdressing. This heterogeneity appears most plausibly explained by the presence or absence of labour shortages in these occupations. Regarding contract stability, our estimates indicate reductions in contract terminations in farming and in sanitation and HVAC. However, due to large standard errors, the farming estimate is not statistically significant. The reduction in sanitation and HVAC, by contrast, seems best explained by a stronger applicant pool, most likely due to the relatively larger increase in apprenticeship wages in the crafts sector compared to higher-paying apprenticeship occupations.

Our findings provide the first causal evidence on whether the policy objectives of Germany's minimum apprenticeship wage – namely, increasing the attractiveness of vocational training and reducing contract termination rates – have been achieved. Given ongoing skilled labour shortages and the increasing importance of vocational qualifications in the face of technological and demographic changes, our analysis holds significant policy relevance. By providing empirical evidence on the effectiveness of a key labour market policy, we contribute to the academic discourse on minimum wage regulations and offer valuable guidance for future policy decisions on vocational training.

The remainder of the paper is structured as follows: Section 2 provides background information on the institutional setting of the German apprenticeship system and the minimum apprenticeship wage. In Section 3, we review the current state of the literature on minimum wages with a particular focus on empirical studies on employment effects and effects on contract terminations. Section 4 describes the data examined in this study. Section 5 outlines the identification strategy underlying our paper and discusses the assumptions necessary to identify the impact of the apprenticeship minimum wage. Section 6 summarises the results. Section 7 concludes.

2 Institutional Setup

2.1 The German Apprenticeship System

In Germany, apprenticeships are structured within a publicly regulated dual system. An apprenticeship typically spans three to three and a half years and combines practical on-the-job training in firms with theoretical instruction provided by vocational schools. The synergy between these two components ensures that apprentices not only develop specialised skills, but also acquire a broader foundation of knowledge that enhances their employability and career prospects. In 2023, Germany officially recognised 327 different vocational training occupations in a wide range of economic sectors (Bundesinstitut für Berufsbildung [2023]).
To initiate an apprenticeship, firms publish job postings detailing the available training position, and prospective apprentices submit applications. Once a candidate is selected, an apprenticeship contract is established that details key employment conditions such as working hours, training objectives, and remuneration. This legally binding agreement safeguards the rights and responsibilities of both the apprentice and the employer, providing a clear framework throughout the training period. However, under German law, apprenticeships are subject to specific regulations regarding termination: during the probationary period, either party may terminate the contract with minimal notice (Federal Ministry of Justice and Consumer Protection [2005]). After this period, stronger protection for apprentices apply, making contract terminations by employers very difficult, while apprentices may terminate the contract with a notice of four weeks.
As a cornerstone of the German education and labour market system, apprenticeships serve as a vital mechanism to secure a steady supply of skilled workers. Each year, approximately 500,000 people enter apprenticeship programmes in approximately 420,000 firms, representing about 19% of all German companies with employees covered by social insurance (Bundesinstitut für Berufsbildung [2023]). A testament to the programme's success is the high retention rate: approximately 72% of apprentices who complete their training are immediately employed by their training firms, demonstrating the importance of VET in addressing labour market demands (Bundesinstitut für Berufsbildung [2022]).

2.2 The Introduction of the Minimum Apprenticeship Wage

Apprentices in Germany receive remuneration from their training firms as stipulated in their apprenticeship contracts. Until the introduction of the statutory minimum, this remuneration was either negotiated directly between the employer and the apprentice or – depending on the occupation, the region, and whether the firm is subject to a collective bargaining agreement (CBA) – set according to the applicable collective agreement.[1] While both systems remain in place, since January 1, 2020, the minimum apprenticeship wage applies to all apprentices whose training firms are not subject to a CBA.[2]

Apprentices who began their training in 2020 were entitled to at least €515 in their first year. This amount increased to €550 in 2021, €585 in 2022, and €620 in 2023. Furthermore, the minimum wage increases progressively throughout an apprenticeship: by 18% in the second year, 35% in the third year, and 40% in the fourth year, where applicable. This structured increase reflects the growing skill set and contributions of apprentices as they progress in their training.

The introduction of the statutory minimum wage was projected to affect 10–15% of all training firms (Wenzelmann and Pfeifer [2018]). However, the proportion of affected firms varies significantly by region and firm size. Small and medium-sized firms and firms located in East Germany were expected to be particularly affected due to historically lower apprenticeship wages in these contexts (Dietrich [2019]).

3 Related Literature

The effects of minimum wage policies on firm-sponsored training are complex and multifaceted. In perfectly competitive labour markets, (increases in) minimum wages that target the entire workforce can reduce firm-sponsored training, as firms may become less willing to invest in general skills that workers can transfer to other employers (Mincer and Leighton [1980]; Hashimoto [1982]). In contrast, in imperfectly competitive labour markets, where firms have greater wage-setting power, higher minimum wages can encourage employers to invest more in training to enhance worker productivity and justify higher wages (Acemoglu and Pischke [1999]). On the labour supply side, Human Capital Theory predicts that

[1] Germany's system of CBAs is based on negotiations between trade unions and employers' associations, resulting in sector- or firm-level agreements that determine wages, working hours, and working conditions of regular workers and – via separate CBAs – apprentices. These agreements are legally binding for members and may be declared generally applicable. CBA coverage is high in the public sector and in sectors such as metalworking and construction, but significantly lower in sectors such as agriculture, hospitality, and parts of retail. Overall, approximately 50% of employees in Germany are covered by a CBA, though this share varies substantially by region and sector. In rare cases, CBAs have been declared generally binding at the state level for all apprentices within a given occupation.

[2] CBAs may set wages below the statutory minimum. However, cases in which collectively agreed wages fall below the statutory minimum are exceptionally rare. Only about 0.4% of contracts that began in 2020 set wages below the minimum apprenticeship wage, and this fraction declined further to 0.25% in 2023, as our data show.

(higher) minimum wages prevent further skill accumulation as they increase the opportunity cost of education, particularly for individuals at the margin between schooling and low-wage employment (Becker [1964]).

Empirical evidence on firm-side effects of minimum wages is mixed. Examining reforms that target the entire workforce, several studies find no effect – or even slight positive effects – of minimum wages on firm-sponsored training (Acemoglu and Pischke [2003]; Arulampalam et al. [2004]; Cardoso [2019]; Fairris and Pedace [2004]; Grossberg and Sicilian [1999]; Neumark and Wascher [2001]). However, when minimum wages directly target—or exempt—apprentices, some studies report that higher minimum wages reduce employer-provided training, particularly for low-skilled youth and in sectors where firms primarily follow a substitution-orientated rather than an investment-orientated training strategy (Papps [2020]; Schumann [2017]; Linckh et al. [2023]). On the labour supply side, there is no direct empirical evidence regarding the effect of a minimum apprenticeship wage on the decision to pursue an apprenticeship. Nonetheless, numerous studies provide robust evidence that higher minimum wages in the regular labour market increase incentives to enter the labour force rather than to invest in further human capital (see, for example, Hyslop and Stillman [2007]; Neumark and Wascher [1995a]; Neumark and Wascher [1995b]; Neumark and Wascher [2003]).[3] Moreover, a recent youth survey conducted by the Bertelsmann Stiftung and the German Economic Institute finds that apprenticeship pay is among the most important factors influencing secondary school graduates' decisions to pursue an apprenticeship (Bertelsmann Stiftung and German Economic Institute [2025]). Accordingly, we expect that higher minimum wages in the apprenticeship market will increase apprenticeship uptake.

Similarly, there is no direct empirical evidence on the effects of a minimum apprenticeship wage on job separations and turnover. However, evidence from the general workforce suggests that higher wages have no effect or may even reduce turnover by reducing layoffs and job-to-job transitions (Brochu and Green [2013]; Dube et al. [2016]; Brochu et al. [2025]). These findings are consistent with search and matching models of the labour market, where both hiring and firing decisions are endogenous. Thus, higher hiring costs – due to increased wages of both new hires and existing staff who train them – prompt employers to retain current workers rather than incur the expenses of separation and recruitment. At the same time, the increased job value for workers resulting from a (higher) minimum wage can reduce voluntary quits. This effect is particularly relevant in low-wage sector jobs, where mobility is typically higher. Therefore, we expect the net effect of (higher) minimum apprenticeship wages on contract terminations to be negative.

Taken together, research on minimum wages in the regular labour market offers clear mechanisms through which minimum apprenticeship wages could influence training provision, labour supply decisions, and turnover. However, the limited direct evidence – particularly for apprentice-specific policies – underscores the need for further research.

[3] For an extensive review of the literature on the effects of minimum wages on human capital accumulation and the theoretical framework underlying Human Capital Theory, see Kellermann [2017].

4 Data

Our analysis is based on the Statistics of Vocational Training (Berufsbildungsstatistik der Statistischen Ämter des Bundes und der Länder, BBS), an annual register data set on apprenticeship contracts collected by the Federal Statistical Office.[4] The data set encompasses all apprenticeship contracts started, ongoing, successfully completed, or prematurely terminated in Germany each year. It includes, among other variables, information on the contractually agreed monthly apprenticeship remuneration for contracts signed from January 1, 2020, onwards. This enables us to identify newly commenced apprenticeship contracts that specify wages at the respective minimum wage level. Beyond contract details and demographic characteristics of apprentices, the data set also provides information on the location of the training firm and the apprentice's occupation, with occupations classified according to the German Classification of Occupations 2010 (KldB 2010, Bundesagentur für Arbeit [2020]), which groups occupations according to their similarity in terms of tasks, skills and expertise. Its hierarchical structure allows us to distinguish 35 distinct occupational groups and 261 distinct occupational subgroups in our data set.[5]

For all specifications, we restrict the data to newly commenced apprenticeships and only examine contractually agreed-upon wages for the first year of training, as the minimum wage in a given year only applies to newly commenced apprenticeships, and wages and contract information are reported by the firms only once, when a new contract is signed. We narrow the data further down to low-wage occupational subgroups, defined as those with a nationwide median wage at or below the 25th percentile of all 261 occupational subgroup-specific median wages in 2020 to 2023, which is equivalent to €750. We do so because in many occupations (and occupational subgroups) the general level of remuneration for apprentices is substantially higher than the minimum wage, and a negligible number of apprenticeship contracts in these occupational subgroups, if any, were remunerated at the minimum wage in 2020 to 2023 (for simplicity, we will henceforth refer to these occupational subgroups as 'occupations'). We then aggregate the data by district and year to obtain our baseline sample.

Our outcome variables are the number of newly commenced apprenticeship contracts and the share of prematurely terminated apprenticeship contracts of first-year apprentices.[6] To estimate the impact the minimum wage legislation has on these two outcome measures, we employ DiD approaches that leverage regional variations in the share of apprenticeship

[4] Training firms are required to report contract details to the regional chambers, which then forward the information to the Federal Statistical Office. The data are subsequently processed by the Federal Institute for Vocational Education and Training (Bundesinstitut für Berufsbildung, BIBB).

[5] Note that the KldB 2010 is a general, not-apprenticeship-specific classification scheme. Some of the 327 government-recognised apprenticeship occupations may fall into different KldB 2010 categories depending on the sector in which the firm operates and the specialisation it consequently trains its apprentices in. For example, a digital and print media design apprentice may either be assigned to the KldB 2010 category of "occupations in sales" or "occupations in digital and print media design", depending on whether the focus of the training is on consulting and planning or on conception, design and visualization.

[6] The data do not allow us to observe whether termination is initiated by the apprentice or the firm.

contracts with minimum wage compensation. In our baseline specification, we define the treatment group as the 15% of districts with the highest share of apprenticeship contracts in low-wage occupations that paid minimum wage between 2020 and 2023. The control group comprises the 15% of districts with the lowest share of apprenticeship contracts among the low-wage occupations that paid the minimum wage, while the remaining districts are excluded. In the treatment group, the share of apprenticeship contracts with remuneration at the minimum wage level is 34%, compared to 5% in the control group.

To analyse the overall effect in more detail and to be able to refer to the findings of Langen and Dörsam [2024], who investigate the effects of the minimum apprenticeship wage on firms' demand for apprentices, we look closely at the largest and strongly affected low-wage occupations in our baseline sample. These are: (1) occupations in hairdressing, (2) occupations in wood construction, furniture and cabinet making, and interior finishing, (3) occupations in sanitation and HVAC, (4) occupations in farming (without specialisation), and (5) occupations in metal construction, with shares of minimum wage contracts between 21 and 92%, and at least 1,000 newly commenced contracts between 2020 and 2023 (see Table 1).

Occupational Subgroup	4-Digit ID	# Newly Commenced Apprenticeship Contracts	Minimum Wage Contracts [%]
Occupations in hairdressing	8231	1864	92.4
Technical occupations in prosthetic dentistry	8254	673	82.8
Management assistants in the sports and fitness industry, sports administrators	6312	452	60.4
Occupations in wood construction, furniture and cabinet making, and interior finishing	2234	2320	46.3
Occupations in meat processing	2923	441	41.6
Occupations in farming (without specialisation)	1110	2076	35.3
Occupations in livestock farming (without poultry farming)	1121	500	34.3
Occupations in sanitation, heating, ventilating, and air conditioning	3421	3536	32.7
Occupations in digital and print media design	2321	535	25.9
Technical occupations, agricultural and construction machinery	2522	1016	23.1
Occupations in metal constructing	2441	2387	21.3
Occupations in ophthalmic optics	8252	1175	16.5
Vehicle paintwork	2221	700	14.6

Table 1: Occupational Subgroups that Form the Set of Low-Wage Occupations in the Baseline Specification. Only those with at least 100 newly commenced contracts per year and a fraction of at least 15% of minimum wage contracts between 2020 and 2023 are presented. *# Newly Commenced Apprenticeship Contracts* is the total amount of newly commenced contracts in the occupational subgroup between 2020 and 2023. *Minimum Wage Contracts [%]* is the average share of minimum wage contracts within the occupational subgroup between 2020 and 2023.

Finally, to enhance our analysis, we merge our dataset with a set of district×year-level covariates, including GDP per capita, firm counts, and the number of school graduates obtained from the Federal Statistical Office. We also include both the level and the square of the total COVID-19 case rates per 100,000 people at the district level on May 1, 2020, 2021, and 2022, drawn from the Robert Koch Institute (RKI).[7] To examine trends before and after the introduction of the minimum wage in 2020, we use data from 2015 to 2023, allowing for a comprehensive evaluation of pre- and post-policy developments.

[7] We tested alternative dates and time spans for the COVID-19 case rates as covariates, but the level and square of the rates on May 1 exhibited the highest explanatory power in our regressions. This result aligns with our expectations, as most apprenticeships begin in August and September, and if firms did react to COVID-19 developments, they would likely have responded to unexpected changes in the preceding months.

4.1 Descriptive Statistics

Figure 1 presents the distribution of contractually agreed wages for first-year apprentices in low-wage occupations, along with the statutory minimum wage applicable in each corresponding year. The graphs indicate that, even within low-wage occupations, the majority of apprenticeship contracts stipulate wages above the prevailing minimum wage. Most contracts set monthly wages between €600 and €800 during the first year of training, with wages typically increasing in subsequent years. However, a distinct concentration of contracts can be observed around the minimum wage level. Each year, approximately 15 to 18% of the contracts in low-wage occupations specify wages equal to or very close to the minimum wage. These contracts can be considered directly affected by the minimum wage legislation. Accordingly, we define the treatment status based on the share of contracts offering wages at the minimum wage. Contracts with wages substantially below the minimum wage are very rare and occur only if a CBA is in place that sets a wage below the minimum wage (see Section 2.2).

Figure 1: Distribution of Contractually Agreed-Upon Wages for the First Year of Apprenticeship in Low-Wage Occupations in 2020, 2021, 2022 and 2023. The Red Line Indicates the Minimum Wage in Effect in the Respective Year.

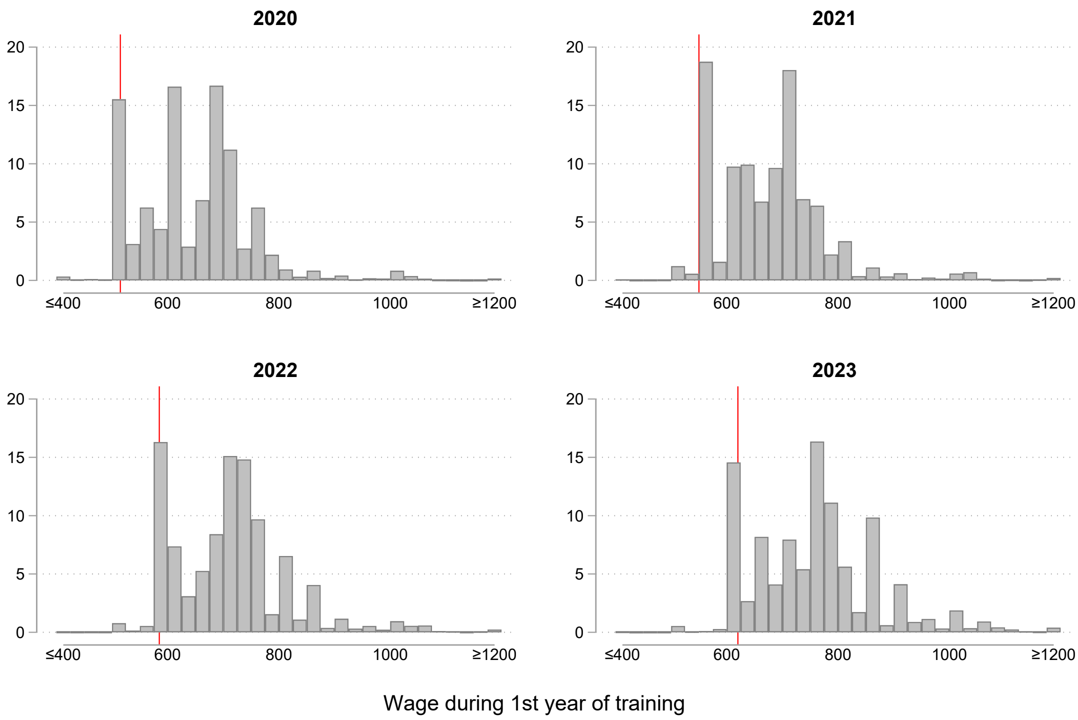
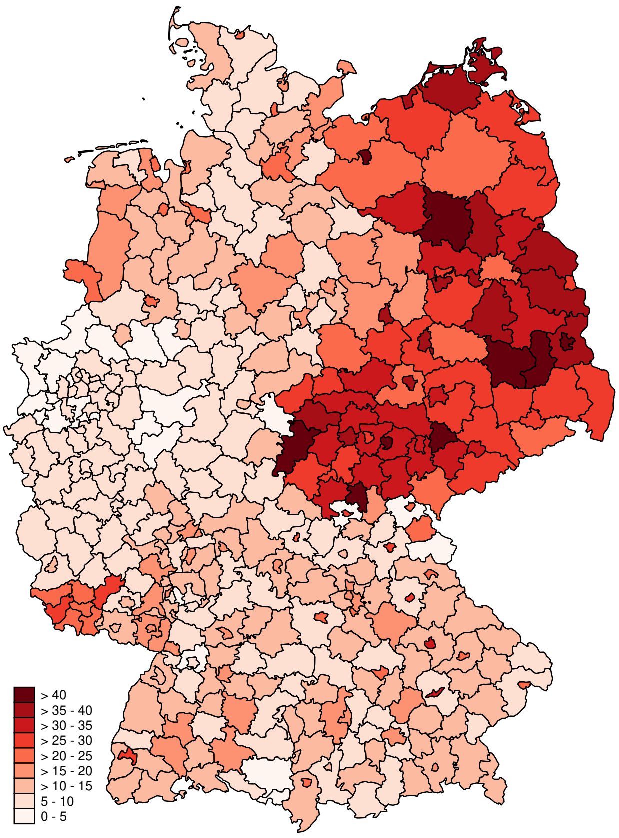


Figure 2 illustrates the district-level shares of apprenticeship contracts in low-wage occupations initiated between 2020 and 2023 that set wages at the statutory minimum. The figure shows that the prevalence of minimum wage contracts is particularly high in East Germany and the Saarland, a small state in southwestern Germany, although some districts in the north-west and in Bavaria also exhibit minimum wage shares of 20% or higher. In addition, the figure suggests that smaller, predominantly urban districts tend to have higher shares of minimum-wage contracts compared to larger, more rural districts.

Figure 2: Geographical Distribution of the Share of Minimum Wage Contracts in Low-Wage Occupations in Germany in 2020 to 2023.



While the relatively high incidence of minimum wage contracts in East Germany and the Saarland can be attributed to lower regional productivity, the relatively high shares observed in certain districts of Bavaria – one of Germany's most prosperous states with the highest GDP per capita, the lowest unemployment rate and the highest wages for skilled workers – are more surprising. In contrast, North Rhine-Westphalia, located in western Germany, shows consistently low shares of minimum wage apprenticeship contracts across all districts, despite including several economically weaker areas in terms of productivity and employment. An explanation for this finding is the presence of sector- and occupation-specific CBAs. While North Rhine-Westphalia had a generally binding collective agreement for hairdresser trainees that mandated wages above the minimum wage between 2020 and 2023, a collective agreement for the same group in Bavaria mandated wages at and below the minimum wage between 2020 and 2023.

A closer look at the development of the district-level shares of minimum wage contracts from 2020 to 2023 shows some modest, but largely homogeneous, nationwide changes (see Figure A.1 in Appendix A). While most districts experienced an increase in the share of contracts at the minimum wage level from 2020 to 2022, the very same districts experienced a decrease in the following year, resulting in a very similar distribution of the district-level shares of minimum wage contracts in 2020 and 2023. This supports the use of the average share of minimum wage contracts in low-wage occupations between 2020 and 2023 as our assignment variable.

	Treatment Group			Control Group		
	'15-'19 Mean	'20-'23 Mean	Diff.	'15-'19 Mean	'20-'23 Mean	Diff.
# Newly Commenced Apprenticeship Contracts	95	93	-2.7	205	186	-19
Fraction Terminated Contracts [%]	14	13	-1.6	14	14	.25
GDP per capita [€]	30,828	35,250	4,422	33,164	37,303	4,139
# Businesses, 0 - 10 Employees	5,587	5,266	-322	9,492	9,076	-417
# Businesses, 10 - 50 Employees	645	738	94	1,003	1,270	268
# Businesses, 50 - 250 Employees	154	171	17	236	283	47
# Lower-Secondary School Graduates	154	160	6.9	427	392	-36
# Upper-Secondary School Graduates	528	549	21	1,130	1,007	-123
East German Districts [%]	88	88	0	0	0	0
Urban Districts [%]	33	33	0	22	22	0
Male [%]	50	49	-.83	54	52	-1.8
German Citizens [%]	92	91	-.66	84	83	-1.6
Minimum Wage Contracts [%]		34			5.1	
N	300	240	540	300	240	540

Table 2: Comparison of Outcome Variables and Covariates in Treatment and Control Group Before and After the Introduction of the Apprenticeship Minimum Wage.

Finally, we examine the composition of the treatment and control groups with respect to the covariates that we include in our estimations. Table 2 shows that the average share of contracts in low-wage apprenticeships that offer remuneration at the minimum wage level is 34% in the treatment group, compared to 5.1% in the control group. As expected, the treatment group is economically weaker than the control group, exhibiting a lower GDP per capita and significantly fewer businesses in all measured dimensions. There are also significant differences in geographical and demographic characteristics: almost 90% of the treated districts are located in eastern Germany, while all control districts are located in Western Germany. The treated districts are also more likely to be urban, while the proportion of apprentices with German citizenship is higher, and the share of male apprentices lower compared to the control group.

Although this comparison reveals notable differences between the treatment and control groups, DiD approaches are designed to account for differing treatment and control groups. Looking at the development of the characteristics of the treatment and control group can be more revealing–here, we do not find strong deviations in the development of the treatment and control group, at least not in the observable covariates.

5 Empirical Strategy

We estimate the ATT using the standard DiD framework, the sDiD (Arkhangelsky et al. [2021]) and a DDD approach (Olden and Møen [2022]). Initially, we assess the effect of the introduction of the minimum apprenticeship wage on both the employment of apprentices and the incidence of premature contract terminations across all low-wage occupations. Additionally, we separately examine the largest occupational subgroups with a particularly high share of low-wage occupations, namely occupations in: (1) farming, (2) wood construction, (3) metal construction, (4) sanitation and HVAC, and (5) hairdressing. This approach allows us to capture potential heterogeneity in the ATT in different occupations and explore different underlying mechanisms.

5.1 Discussion of the Underlying Assumptions

All approaches used in this paper rely on the same three key assumptions: the common trends assumption, the no-anticipation assumption, and the stable unit treatment value assumption (SUTVA).

To address potential violations of the common trends assumption due to differences in economic development and the supply of potential apprentices across districts, we control for annual district-level counts of enterprises with 0 to 10, 11 to 50, and 51 to 250 employees and of lower- and upper-secondary school graduates. In doing so, we relax the common trends assumption to a conditional common trends assumption. We test the validity of the conditional common trends assumption in all models by comparing the development of the outcome in the treatment and control groups in the pre-treatment years both graphically and through statistical tests.

Nevertheless, the simultaneous occurrence of the COVID-19 pandemic and the introduction of the minimum wage in early 2020 may have violated the conditional common trends assumption. To account for the possibility that the pandemic affected the apprenticeship market differently across districts and thus across treatment and control groups, we (1) additionally control for the level and the square of the total COVID-19 case rates per 100,000 people in an additional estimation, and (2) estimate DDD equations that include the development of apprenticeship postings in high-wage occupations as an additional level of differencing. This approach allows treatment and control districts to be differently affected by the pandemic, with potentially distinct consequences for new apprenticeship contracts and contract stability. Under the assumption that the effects of the pandemic on contract formation and stability in low- and high-wage occupations were the same across districts, the DDD estimate yields an unbiased estimate of the ATT.

Regarding the no-anticipation assumption, firms may have accelerated the recruitment of apprentices, shifting – at least partially – the hiring from 2020 to 2019 to avoid higher costs following the introduction of the minimum wage. Following the same rationale, firms may have been less likely to terminate contracts in 2019, since that cohort was the last group not subject to the minimum wage. These potential shifts in hiring and contract terminations could have been prompted by the announcement of the minimum wage in May 2019, earlier public debates, or discussions within the wage-setting committees that include unions and employer associations. However, it is unlikely that this led to significant shifts from 2019 to 2020. Apprentices in different stages of training have varying levels of skills and supervision needs, making them difficult to substitute for each other. This is supported by data from the BIBB cost-benefit survey, which examines the tasks performed by apprentices in a three-year programme. According to the survey, the time that apprentices spend on tasks typically assigned to skilled workers increases from 35 days in the first year to 77 days in the third year – a 120% increase. Meanwhile, the time spent on tasks suitable for unskilled workers drops by 39%, from 61 to 37 days over the same period (Wenzelmann and Schönfeld [2022]). A more pressing concern about the no-anticipation assumption arises from labour unions' involvement in setting the minimum wage level. Unions may have used information about the expected minimum wage – even before May 2019 – to strengthen their bargaining position in collective negotiations. This could have influenced wage levels specified in agreements concluded before the minimum wage took effect, thus indirectly affecting the outcomes of interest. Although we have no evidence that labour unions used foreknowledge of the expected minimum wage in collective bargaining prior to its public announcement, a few CBAs set wages at or near the statutory minimum apprenticeship wage before the law took effect. Among the occupations examined in this study, the hairdressing trade provides a notable case. In three German states (Baden-Württemberg, Hesse and Lower Saxony), collectively bargained wages for hairdressing apprentices were made generally binding in 2018. These wages were approximately equal to the 2020 minimum wage for first-year apprentices and represented a substantial increase over previous wage levels in the three states.[8] Accordingly, in the hairdressing subgroup analyses, we restrict the sample to observations from 2018 onwards.[9]

Finally, one part of the SUTVA, namely that there must not be different versions of treatment, is insofar violated as all DiD specifications we employ rely on grouping districts together into treatment and control groups that are similar but not equal regarding the share of apprenticeship contracts with minimum wage remuneration. VanderWeele and Hernán [2013], however, have developed one relaxed version of the SUTVA that allows for different

[8] In Lower Saxony, the collectively agreed wage for first-year hairdressing apprentices increased from €308 (set in 2008 and unchanged until 2018) to €500 in 2018. In Baden-Württemberg, the CBA wage for first-year apprentices had been €420 since 2006 and was raised to €500 in 2018. In Hesse, it had been €450 since 2016 and was raised to €500 in 2018. In all three states, the collectively agreed wage was scheduled to increase by €10–€20 in 2019.

[9] We also evaluated the impact of these CBAs on our two outcome variables in separate analyses, setting the treatment period to begin in 2018. In some specifications, the estimates closely match those of the main subgroup analyses, reinforcing the robustness of our findings with respect to the common trends assumption and the potential differential impact of COVID-19 on the treatment and control groups. However, the estimates are sensitive to the choice of comparison group, the observation period, and the estimation strategy applied; therefore, we do not report them in detail among the robustness checks.

treatment versions, provided there are no different versions of non-treatment and that the treatment versions are assigned randomly conditionally on covariates. In our case, the variation in the level of non-treatment is relatively small as the share of contracts with minimum wage remuneration is mostly equal to zero or very close to zero. This is especially true in the analysis of individual occupational groups, where, in all but one case, the control group consists exclusively of districts without minimum wage contracts in the respective occupational group (see Section 4.1). The variation in the share of minimum wage contracts in the treatment group can to a large extent be explained by the covariates capturing the economic situation in a district (both in general and in the treatment group). Conditionally on the covariates, there remains only a small apparently random variation in the treatment intensity, so that the modified SUTVA holds approximately.

Apart from the DiD assumptions discussed so far, the treatment definition in all our approaches relies on one more assumption: if the implementation of the minimum wage in 2020 led to reductions in the number of newly signed apprenticeship contracts or contract terminations in the minimum wage sector, these reductions were proportional to the number of hires and contract terminations affected by the minimum wage introduction. The reason we rely on this additional assumption is that the wage agreed in apprenticeship contracts is only available from 2020 onwards. Consequently, we cannot define our treatment based on the share of apprenticeship positions with below-minimum wage compensation in 2019 but rather have to use the share of apprenticeship contracts with minimum wage pay between 2020 and 2023.

5.2 Methodological Approach

In our baseline approach, we estimate the effect of the minimum apprenticeship wage on newly commenced apprenticeship contracts using a fixed-effects (FE) Poisson model. For the rate of premature contract terminations, we employ a linear probability model (LPM).[10] In both estimations, standard errors are clustered at the district level and the treatment variable is defined as described in Section 4. We first estimate a DiD model that includes interactions between the treatment dummy variable and all periods to examine how the effect of the minimum wage evolved over time following its introduction. The estimation approach is applied to data covering all low-wage apprenticeship occupations – our baseline specification – and each of the selected occupational subgroups individually.

To validate our baseline results, we use both sDiD and DDD methods. sDiD is a procedure developed by Arkhangelsky et al. [2021] that combines the strengths of the synthetic control method (Abadie et al. [2010]) and the standard DiD estimator. The sDiD procedure weighs the pre-treatment observations in the control group in such a way that the

[10] As we focus on ATTs, we estimate LPMs, whose treatment effect estimates are directly interpretable as percentage-point changes in the respective contract termination rate. The results are robust to the estimation of fractional logit models (Papke and Wooldridge [1996]); the average marginal effects are virtually identical and lead to the same conclusions as shown in Robustness Section 6.2.1.

respective pre-treatment outcome in control and treatment group follows parallel trends. In addition, it also uses time weights to balance pre- and post-treatment time periods. Compared to the standard DiD approach, the sDiD is more successful in dealing with violations of the (conditional) common trends assumption. Other than the synthetic control method, it only requires the pre-treatment paths to be parallel rather than identical among treated and synthetic control units. The sDiD can serve as a valuable supplementary analysis to the standard DiD as it can help address violations of the common trends assumption and substantiate the credibility of the estimated treatment effects.

In the DDD approach, we include the development of newly commenced apprenticeship contracts in high-wage occupations as an additional level of differencing, where high-wage occupations are defined as those occupations with a nationwide median wage at or above the 75th percentile of all occupation-specific median wages (of €1000). This approach addresses potential violations of the conditional common trends assumption arising from general differences in trends in the treatment and control groups, which cannot be controlled for by means of the considered covariates. The DDD estimator is equal to the difference between the DiD estimators calculated for the high-wage occupations and the low-wage occupations (or the respective low-wage occupation under study), where the treatment and control districts are defined as in the simple DiD setting. The DDD estimator does not necessarily require the common trends assumption to hold in both DiD estimators. Instead, it relies on the assumption that the bias is the same in both DiD estimators (Olden and Møen [2022]).

6 Results

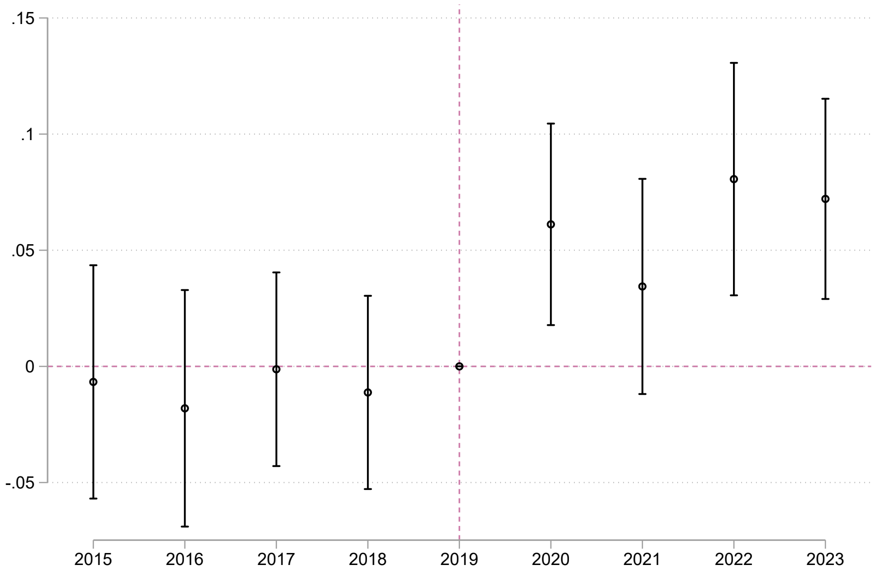
In this section, we present the empirical results on the impact of the statutory minimum apprenticeship wage on (i) the number of newly commenced apprenticeship contracts and (ii) the rate of premature contract terminations. The analysis of both outcomes proceeds in four steps. We first estimate the ATT across all low-wage occupations, then test its robustness across several specifications, examine heterogeneity in treatment effects by occupation, and finally discuss potential mechanisms. All ATT estimates are based on district-level variation in the prevalence of minimum wage contracts.

6.1 The ATT on New Apprenticeship Contracts

We begin by analysing the impact of the statutory minimum apprenticeship wage on the number of newly commenced apprenticeship contracts in low-wage occupations. Figure 3 shows the evolution of the outcome variable between the treatment and control groups from 2015 to 2023. Estimates from the underlying event study model are based on a fixed-effects Poisson regression without covariates and are presented with 95% confidence intervals.

Figure 3 shows that in the pre-treatment years the number of newly commenced apprenticeship contracts in low-wage occupations developed almost identically in the treatment and

Figure 3: FE Effect Estimates From Regression Without Covariates For Low-Wage Occupations



control group, suggesting the absence of divergent pre-trends and thus supporting the common trends assumption. With the introduction of the minimum wage in 2020, significantly more apprenticeship contracts were initiated in the treatment group compared to the control group, and after a dip in 2021, this difference continued to increase in 2022 and 2023.

The estimates presented in Figure 3 are also reported in column (1) of Table 3. In column (2), covariates are added to our baseline model: the annual GDP at the district level, as well as the annual district-level counts of lower- and upper-secondary school graduates and of enterprises with 0–10, 11–50 and 51–250 employees. Column (3) additionally includes the level and square of the total COVID-19 case rate per 100,000 people at the district level as of May 1, 2020, 2021, and 2022.

Across all model specifications and throughout the treatment period from 2020 to 2023, we observe a consistently positive and – except for 2021 – statistically significant reform effect on the number of newly commenced apprenticeship contracts in low-wage occupations. The estimates of the baseline model in column (1) range from 0.061 in 2020 to 0.081 in 2022 ($p < 0.01$ throughout), indicating a higher formation of apprenticeship contracts in low-wage occupations located in districts with a high share of minimum wage contracts (34% on average) compared to districts with a low share of minimum wage contracts (5.1% on average). This finding is robust to the inclusion of our sets of covariates in columns (2) and (3).

6.1.1 Robustness Checks

A broad set of robustness checks, presented in Table B.1, corroborates our main findings. First, in columns (2) to (4), placebo regressions with pseudo-treatment dates set one, two,

Table 3: The Effect of the Minimum Apprenticeship Wage on the Number of Newly Commenced Apprenticeship Contracts in Low-Wage Occupations

	(1)	(2)	(3)
Reform effect 2023	0.072***	0.087***	0.088***
	(0.022)	(0.023)	(0.023)
Reform effect 2022	0.081***	0.095***	0.079**
	(0.026)	(0.030)	(0.031)
Reform effect 2021	0.034	0.050*	0.052*
	(0.024)	(0.028)	(0.028)
Reform effect 2020	0.061***	0.079***	0.079***
	(0.022)	(0.025)	(0.025)
Interaction terms			
Treated x 2018	-0.011	-0.013	-0.013
	(0.021)	(0.021)	(0.021)
Treated x 2017	-0.001	0.001	0.001
	(0.021)	(0.023)	(0.023)
Treated x 2016	-0.018	-0.014	-0.015
	(0.026)	(0.027)	(0.027)
Treated x 2015	-0.007	0.004	0.004
	(0.026)	(0.028)	(0.028)
Year FE	yes	yes	yes
Covariates	no	yes	yes
COVID-19 covariates	no	no	yes
Observations	1,080	1,080	1,080
Groups	120	120	120

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: FE Poisson models are estimated at the district level and are based on 2015-2023 cohorts of apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district level with a compensation at the minimum wage from 2020 to 2023. The treatment group consists of the top 15% of districts with the highest share of contracts with a compensation at the minimum wage. The control group consists of the bottom 15% of districts. *Newly commenced contracts* contains the number of newly commenced contracts in low-wage occupations in the respective district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

and three years before the actual reform – based on the 2015–2019, 2015–2018 and 2015–2017 cohorts, respectively – consistently yield statistically insignificant estimates close to zero. This finding suggests that the observed treatment effects do not stem from spurious time trends or pre-existing differential developments in the treatment and the control group.
Second, we test the sensitivity of the results to changes in the definition of the treatment group. When redefining in columns (5) and (6) the treatment group as the top 10% and the top 20% of districts by minimum wage exposure, with the control group defined accordingly, we continue to observe large and statistically significant positive effects on newly signed apprenticeship contracts, with point estimates close to the baseline estimate.
Two alternative estimation strategies should provide additional robustness. The sDiD approach, designed to correct for possible violations of the common trends assumption, produces a treatment effect of 9.185 ($p < 0.01$; see column (8) of Table B.1). This statistically highly significant estimate corresponds to an approximately 9% increase in the number of newly commenced contracts, thus reinforcing our baseline result. Finally, the triple difference estimator presented in column (9), which includes high-wage occupations serving as an additional control group, yields a positive but statistically insignificant effect. Although the direction of the estimator aligns with the main results, it indicates that the effect estimates from our baseline and the sDiD model may not fully account for the differential effects of the COVID-19 pandemic on our treatment and control observations.

6.1.2 Subgroup Analysis

To explore potential heterogeneity in treatment effects, we estimate separate models for several low-wage occupational subgroups. These subgroups are characterised by a particularly high share of minimum wage contracts with at least 100 newly signed apprenticeship contracts per year (for an overview, see Table 1). To account for the potential differential effects of the COVID-19 pandemic on our treatment and control observations, we again employ triple-difference approaches as a robustness check.[11]
The results of the baseline DiD regressions, summarised in Table 4, reveal considerable variation across occupations.[12] Occupations in farming, wood construction, and metal construction exhibit positive treatment effects, with DiD estimates ranging from 0.066 to 0.117, all significant at the 5% level. These findings suggest that the minimum wage reform increased the number of newly commenced contracts in these occupations by approximately 7 to 12%. In contrast, the estimated effects in sanitation and HVAC, as well as in hairdressing, are both negative, although the effect in sanitation and HVAC is not statistically significant. However, the estimate for hairdressing suggests that the number of new contracts

[11] As the DiD and DDD estimates provide no indication of a violation of the common trends assumption, we do not report the sDiD estimates.

[12] Both the assignment variable and the cohorts are selected such that the DiD and DDD interaction terms in the pre-treatment period are close to zero and show no evidence of violating the common trends assumption. All estimates are robust to alterations of the assignment variable.

in hairdressing has decreased by approximately 8%.

Table 4: Subgroup Analysis: Low-Wage Occupational Subgroups (DiD Estimates)

Dependent variable: Newly commenced contracts	Occ. in farming	Occ. in wood construction	Occ. in metal construction	Occ. in sanitation and HVAC	Occ. in hairdressing
	0.091**	0.117**	0.066**	-0.040	-0.078*
	(0.045)	(0.046)	(0.033)	(0.026)	(0.046)
Year Fixed-Effects	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes
Treated/Control	u/l 25%	u/l 15%	u/l 25%	u/l 20%	u/l 15%
Observations	2,072	1,687	2,373	2,696	720
No. of Groups	259	241	339	337	120
Cohorts	2016-23	2017-23	2017-23	2016-23	2018-23
Minimum wage contracts [%]	47%	52%	24%	35%	99%

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: For each occupational subgroup, FE Poisson models are estimated at the district level and are based on apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district×occupation level with a compensation at the minimum wage from 2020 to 2023. *Newly commenced contracts* contains the number of newly commenced contracts in the respective occupation, district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

Table 5 reports the triple-difference estimates. High-wage occupations are added as a third comparison group, along with the baseline DiD groups, to account for the potential differential effects of COVID-19 between the treatment and control groups. The results are consistent with the baseline DiD estimates. With the exception of wood construction, the estimates become both statistically and economically more significant. Apart from hairdressing – where the ATT turns significantly more negative – we do not find evidence of a substantial differential impact of COVID-19 on the treatment and control groups.

6.1.3 Potential Mechanisms

The evidence from the subgroup analysis highlights the importance of distinguishing between occupational contexts. While the aggregate effect of the reform on the number of newly commenced apprenticeship contracts in the low-wage sector is positive, the patterns differ substantially across occupations.

A straightforward explanation for the observed effect heterogeneity would be that affected firms in different sectors responded differently to the statutory minimum apprenticeship wage, and that in sectors where firms reduce the number of training positions in response to the minimum wage, fewer apprenticeship contracts are concluded. To investigate this

Table 5: Subgroup Analysis: Low-Wage Occupational Subgroups (DDD Estimates)

Dependent variable: Newly commenced contracts	Occ. in farming	Occ. in wood construction	Occ. in metal construction	Occ. in sanitation and HVAC	Occ. in hairdressing
	0.095**	0.096*	0.076**	-0.060*	-0.112**
	(0.045)	(0.050)	(0.038)	(0.033)	(0.054)
Year Fixed-Effects	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes
Treated/Control	u/l 25%	u/l 15%	u/l 25%	u/l 20%	u/l 15%
Observations	26,280	19,866	28,511	32,840	9,594
No. of Groups	3,285	2,838	4,073	4,105	1,599
Cohorts	2016-23	2017-23	2017-23	2016-23	2018-23
Minimum wage contracts [%]	47%	52%	24%	35%	99%

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: For each occupational subgroup, FE Poisson models are estimated at the district level and are based on apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district×occupation level with a compensation at the minimum wage from 2020 to 2023. The third comparison group are high-wage occupations, i.e., occupations with a nationwide average wage at or above the 75th percentile of all occupation-specific median wages. *Newly commenced contracts* contains the number of newly commenced contracts in the respective occupation, district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

relationship, we compare our findings with those of Langen and Dörsam [2024], who provide causal estimates of the effect of the minimum wage on apprenticeship postings in five occupational groups. Three of these groups overlap with the occupational subgroups we consider, namely (1) agriculture, forestry, and farming, (2) plastic-making and processing and wood-working and processing, and (3) non-medical healthcare, body care, wellness and medical technicians. For plastic-making and processing and wood-working and processing, Langen and Dörsam [2024] find a significant negative effect on apprenticeship postings of approximately 9 to 10%. For the other two groups, the estimates are inconclusive. In light of the findings of Langen and Dörsam [2024], the significant positive effects on the number of new apprenticeship contracts in farming and wood construction as well as the significant negative effect in hairdressing are unexpected.

Since the direct link between apprenticeship postings and actual apprentice employment cannot explain the observed heterogeneity across occupational subgroups, additional mechanisms must be considered. First, the introduction of a statutory minimum apprenticeship wage is likely to increase the attractiveness of vocational training on the supply side, particularly in the most affected occupations, potentially increasing both the number and the average quality of applicants (Becker [1964]; Acemoglu and Pischke [1999]). This may have allowed firms – despite higher training costs and a reduction in postings – to maintain or even expand apprentice employment in certain sectors, as observed in farming and wood construction occupations, thanks to an improved pool of applicants and improved matching efficiency. Although direct data on supply-side effects are not available, improvements in matching efficiency are reflected in contract termination rates. We examine this potential channel in more detail in Section 6.2.

Second, persistent skilled labour shortages can put pressure on firms to hire apprentices despite rising training costs (see, e.g. Bellmann et al. [2014]; Mason et al. [2012]). According to labour shortage statistics from the Federal Employment Agency (Bundesagentur für Arbeit [2024]), occupations in farming are among those most affected by shortages, and these shortages have worsened between 2019 and 2023.[13] Occupations in wood and metal construction occupations are somewhat less affected, but also show high shortage scores over the period 2019 to 2023. By contrast, hairdressing occupations have been under observation in 2019 and 2020 but have not been in shortage since 2021. Thus, for these four subgroups, labour shortages may well explain the observed effect heterogeneity. For the fifth group, however, labour shortages do not help explain the reported negative effect, since occupations in sanitation and HVAC experience severe shortages for several years.

Third, sector-specific CBAs may moderate the impact of the minimum apprenticeship wage and contribute to occupational heterogeneity (Neumark and Wascher [2008]), as the statutory minimum wage is more likely to affect apprentice employment in regions and occupations without CBA coverage. However, in the five occupations examined, no consistent pattern emerges. In the three occupations showing a positive treatment effect, CBAs in nearly all treatment and control observations set apprenticeship wages well above the statutory minimum. However, the sources of the positive treatment effects we find vary considerably: In agriculture, the treatment group experienced a substantial increase in apprenticeship contracts, while the numbers in the control group declined slightly; in wood construction, the effect reflects a sharp contraction in the control group alongside stability in the treatment group; and in metal construction, it arises from a comparatively larger decline in the control group. In hairdressing, which exhibits the strongest negative treatment effect, almost all treatment and control observations involve CBAs that establish apprenticeship wages above the statutory minimum. However, the negative ATT results from a stronger decrease in newly commenced contracts in the treatment group relative to the control group. Sanitation and HVAC are the only occupations for which CBAs almost exclusively exist in the control observations (districts of one state belong to both the treatment and control groups and have a CBA stipulating wages above the statutory minimum). Here, the negative ATT results from a weaker increase in newly commenced contracts in the treatment group relative to

[13] The labour shortage statistics of the Federal Employment Agency provide a score indicating the degree of labour shortage in various occupational groups based on six statistical indicators. Scores range from 0 to 3, where a score of 2.0 or higher indicates that an occupation is facing a shortage. Scores below 1.5 indicate no shortage, while scores between 1.5 and 2.0 are classified as "under observation".

the control group. Thus, we find no evidence of a systematic correlation between our ATT estimates and the prevalence of CBAs.

6.2 The ATT on Contract Termination Rates

We next turn to the question of whether the minimum wage had an impact on the stability of apprenticeship relationships as measured by the share of contracts prematurely terminated within the first year. Again, we first estimate the ATT across all low-wage occupations, then perform several robustness checks to validate our baseline result, investigate heterogeneity in treatment effects by occupation, and finally discuss potential mechanisms.

Table 6 presents estimates from linear probability models with clustered standard errors at the district level. In all specifications, the reform is associated with a significant decrease in the contract termination rate across all low-wage occupations, except for 2021. In the fully specified model in column (3), the estimated treatment effect ranges from –0.020 in 2021 ($p < 0.05$) to –0.030 in 2022 ($p < 0.05$), indicating a persistent and meaningful reduction in early contract termination over the four-year post-reform period. Relative to the average contract termination rate of 14.6% in the treatment group during the pre-treatment years, the ATT corresponds to a reduction of approximately 15% on average. Notably, all pre-treatment interaction terms are statistically insignificant, suggesting no diverging pre-trends between treatment and control observations. This supports the identification strategy and mitigates concerns about violations of the common trends assumption. Instead, the estimates suggest that the introduction of the minimum wage improved contract stability in low-wage occupations.

6.2.1 Robustness Checks

The robustness checks reported in Table B.2, however, cast some doubt on the conclusions drawn from our baseline model. While the placebo treatment regressions in columns (2) and (4), which report placebo treatment effects for one and three years before the reform, yield statistically insignificant coefficients close to zero, the regression for two years before the reform in column (3) shows a statistically significant negative effect of similar magnitude to the baseline effect. This suggests that the contract termination rate has evolved differently in treated and control observations from 2017 to 2018.

In contrast, varying the composition of the treatment and control groups in columns (4) and (5) – using the top and bottom 10% or 20% of districts by exposure – produces negative and statistically significant point estimates that closely match the baseline estimate, reaffirming its stability. Similarly, in column (6), the synthetic DiD approach, designed to capture potentially divergent pre-trends between treatment and control observations, yields a highly significant effect of -0.017 ($p < 0.01$), further confirming the baseline result.

Lastly, the DDD estimator in column (7), which uses high-wage occupations as an additional comparison group to account for broader time-specific shocks that may have affected the

Table 6: The Effect of the Minimum Apprenticeship Wage on the Contract Termination Rate in Low-Wage Occupations

	(1)	(2)	(3)
Reform effect 2023	-0.023**	-0.027***	-0.026***
	(0.009)	(0.010)	(0.010)
Reform effect 2022	-0.022**	-0.025**	-0.028**
	(0.010)	(0.011)	(0.012)
Reform effect 2021	-0.005	-0.010	-0.010
	(0.009)	(0.009)	(0.009)
Reform effect 2020	-0.015*	-0.018**	-0.018**
	(0.009)	(0.009)	(0.009)
Interaction terms			
Treated x 2018	-0.013	-0.014	-0.014
	(0.010)	(0.010)	(0.010)
Treated x 2017	0.009	0.007	0.007
	(0.010)	(0.009)	(0.009)
Treated x 2016	0.006	0.005	0.005
	(0.011)	(0.011)	(0.011)
Treated x 2015	0.010	0.010	0.010
	(0.010)	(0.010)	(0.010)
Year FE	yes	yes	yes
Covariates	no	yes	yes
COVID-19 covariates	no	no	yes
Observations	1,080	1,080	1,080
Groups	120	120	120

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Linear probability models are estimated at district level and are based on 2015-2023 cohorts of apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district level with a compensation at the minimum wage from 2020 to 2023. The treatment group consists of the top 15% of districts with the highest share of contracts with a compensation at the minimum wage. The control group consists of the bottom 15% of districts. *Contract Termination Rate* is the fraction of newly commenced contracts in the respective district and year that are terminated within the first year of an apprenticeship. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

treatment and control groups differently, has the same sign as the baseline estimate, but is much smaller in magnitude and statistically insignificant. Thus, similar to the robustness check in Section 6.1.1, the DDD estimator suggests that our baseline and sDiD models overestimate the treatment effect, potentially due to differential effects of the COVID-19 pandemic on the treatment and control observations.

6.2.2 Subgroup Analysis

Next, we investigate whether the ATT on contract termination rates varies between the five low-wage occupational subgroups. To account for a potentially differential impact of COVID-19 on treatment and control observations, we again perform both DiD and DDD estimations.[14]

Table 7: Subgroup Analysis: Low-Wage Occupational Subgroups (DiD Estimates)

Dependent variable: Contract Termination Rate	Occ. in farming	Occ. in wood construction	Occ. in metal construction	Occ. in sanitation and HVAC	Occ. in hairdressing
	-0.037	0.018	-0.011	-0.025***	0.005
	(0.026)	(0.022)	(0.012)	(0.009)	(0.025)
Year Fixed-Effects	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes
Treated/Control	u/l 25%	u/l 15%	u/l 25%	u/l 20%	u/l 15%
Observations	1,202	1,218	2,349	3,015	692
No. of Groups	259	241	339	337	120
Cohorts	2018-23	2018-23	2017-23	2015-23	2018-23
Minimum wage contracts [%]	47%	52%	24%	35%	99%

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: For each occupational subgroup, linear regression models are estimated at the district level and are based on apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district×occupation level with a compensation at the minimum wage from 2020 to 2023. *Newly commenced contracts* contains the number of newly commenced contracts in the respective occupation, district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

The DiD estimates are reported in Table 7. The estimates for farming and sanitation and HVAC are large and negative, but only the estimate for sanitation reaches conventional levels of statistical significance ($p < 0.01$). The other subgroup estimates range from modestly negative to modestly positive and are statistically insignificant.

[14] Both the assignment variable and the cohorts are selected such that the DiD and DDD interaction terms in the pre-treatment period are close to zero and show no evidence of violations of the common trends assumption. All estimates are robust to alterations of treatment assignment.

Table 8: Subgroup Analysis: Low-Wage Occupational Subgroups (DDD Estimates)

Dependent variable: Contract Termination Rate	Occ. in farming	Occ. in wood construction	Occ. in metal construction	Occ. in sanitation and HVAC	Occ. in hairdressing
	-0.031	0.012	-0.017	-0.028***	0.022
	(0.026)	(0.021)	(0.012)	(0.010)	(0.026)
Year Fixed-Effects	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes
Treated/Control	u/l 25%	u/l 15%	u/l 25%	u/l 20%	u/l 15%
Observations	13,666	12,187	20,475	25,907	6,994
No. of Groups	3,194	2,806	4,073	4,150	1,599
Cohorts	2018-23	2018-23	2017-23	2015-23	2018-23
Minimum wage contracts [%]	47%	52%	24%	35%	99%

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: For each occupational subgroup, linear regression models are estimated at the district level and are based on apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district×occupation level with a compensation at the minimum wage from 2020 to 2023. The third comparison group are high-wage occupations, i.e., occupations with a nationwide average wage at or above the 75th percentile of all occupation-specific median wages. *Newly commenced contracts* contains the number of newly commenced contracts in the respective occupation, district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

The DDD estimates in Table 8, where high-wage occupations serve as an additional comparison group, confirm the DiD results. Although the point estimates change slightly, the qualitative conclusion remains the same. Only the estimate for sanitation and HVAC is large and statistically significant at the 1% level. The estimate for farming is also large and negative, but statistically insignificant due to a large standard error. The DDD estimate for hairdressing is substantially larger than the DiD estimate but remains statistically insignificant.

Thus, our DiD and DDD estimates suggest that among the five largest and most affected occupational subgroups within the low-wage sector, the reform had a significant effect only on sanitation and HVAC, where it improved contract stability in the first year of apprenticeships by about 2.5 percentage points.

6.2.3 Potential Mechanisms

In the following, we discuss plausible channels through which the minimum apprenticeship wage may have contributed to greater contract stability in low-wage occupations, and especially in sanitation and HVAC. These channels should be viewed as plausible interpretations rather than definitive causal mechanisms, given the limitations of our data.

One potential explanation concerns the increased attractiveness of apprenticeship training. The minimum wage likely improved the perceived value of vocational education, particularly in occupations previously characterised by very low remuneration. This may have attracted a larger and more qualified applicant pool. Likewise, faced with higher training costs, firms may have adopted more selective hiring practices, for example, by raising expectations about educational background, motivation, or interpersonal skills. Both mechanisms could plausibly reduce early contract terminations, although our data cannot distinguish between supply-side changes in the applicant pool and demand-side changes in firm selectivity. However, such an interpretation is consistent with human capital theory (Becker [1964]) and monopsonistic labour market models (Acemoglu and Pischke [1999]), which posit that higher compensation strengthens labour supply responses and improves matching outcomes. It is also consistent with empirical studies suggesting that higher wages are associated with lower job turnover and job separation rates (Brochu et al. [2025]; Dube et al. [2016]).

Although our data do not allow us to directly examine changes in the applicant pool or firms' hiring practices, we do observe the educational background of apprentices, particularly their highest school degree. Thus, to explore this potential mechanism, we estimate the DDD regression model from Section 6.2.2, using as outcome variables the shares of apprentices with high school, upper-secondary, and lower-secondary degrees in sanitation and HVAC, respectively. The results presented in Table C.1 indicate that the share of high school graduates in the treatment group, relative to the control group and to high-wage occupations, increased significantly after the reform, while the shares of upper-secondary and lower-secondary school graduates declined, respectively. Although educational attainment is an imperfect proxy for applicant quality, this shift is consistent with either a stronger applicant pool or more selective hiring practices, both of which could contribute to better matches and greater contract stability.

A second possible explanation is that firms in low-wage occupations responded to higher training costs by increasing investments in apprentice retention, for example, through enhanced on-boarding, supervision, and mentoring. According to the most recent results of the BIBB cost-benefit survey (CBS), firms in the crafts sector that provide apprenticeship training incur average net costs of €7,700 per apprentice (Wenzelmann et al. [2025]). However, given the persistent shortage of skilled labour and, respectively, high recruitment costs for external skilled workers, these expenditures are more than offset if apprentices remain with the firm after completing their training. For the same reasons, only a small proportion of the training firms in the CBS sample report that they do not intend to retain their apprentices, and this proportion has declined substantially over the past five years (from 10% in 2017/18 to 5% in 2022/23). During the same period, personnel costs for trainers show a slight, albeit statistically insignificant, increase. Taken together, these findings support the explanation that firms in low-wage occupations responded to higher training costs by investing more in apprentice retention.

Lastly, higher pay – particularly in occupations previously characterised by very low wages – may have increased the opportunity costs of changing apprenticeships or leaving the apprenticeship system altogether.[15] Recent analyses of apprenticeship wages show that wage inequality has decreased significantly between 2020 and 2023. The reduction is particularly pronounced in the crafts sector and is also significant in sanitation and HVAC (Langen and Dörsam [2025]). Thus, the financial incentive to switch from sanitation and HVAC, and other low-wage occupations, to higher-paying occupations has decreased. This change may also explain the observed improvement in contract stability.

7 Discussion and Conclusion

This paper provides the first causal evidence on the effects of Germany's statutory minimum apprenticeship wage on apprentice employment and contract stability. Exploiting administrative register data that cover all apprenticeship contracts between 2015 and 2023, we apply DiD, sDiD, and DDD estimators to disentangle the impact of the minimum apprenticeship wage from confounding factors.

Our DiD and sDiD estimates suggest that the reform increased the number of apprenticeship contracts in low-wage occupations and reduced premature terminations, consistent with the stated objectives of the reform, making vocational training more attractive and stable. However, our DDD estimates reveal that these effects are not robust to stricter identification strategies, underscoring the sensitivity of the results to differential shocks – most notably the COVID-19 pandemic. Therefore, the evidence indicates that while the minimum apprenticeship wage likely contributed to positive developments, its effects should be interpreted as heterogeneous and context-dependent rather than universal.

Indeed, our occupation-specific analyses show a pronounced variation. In farming, wood construction, and metal construction, apprentice employment increased significantly, plausibly reflecting strong labour shortages and firms' incentives to secure future skilled workers despite higher training costs. By contrast, in hairdressing – and to a lesser extent also in sanitation – the reform coincided with a reduction in apprentice employment. At the same time, sanitation is the only occupation for which we find a significant and robust improvement in contract stability.

From a policy perspective, our results highlight both the potential and the limitations of statutory wage interventions in vocational training. The minimum apprenticeship wage appears to strengthen participation in certain shortage-affected occupations, thus contributing to long-term labour supply objectives. However, in less- or no shortage-affected occupations,

[15] BBS data indicate that, overall, about 18% of apprenticeship contracts are terminated within the first year (Uhly and Neises [2023]). The fraction of apprentices who switch training firms, change occupations, or leave the apprenticeship system entirely cannot be determined from the BBS data. However, survey data suggest that about two-thirds of apprentices switch training firms and/or occupations, and an additional 8–9 percent pursue university studies (Holtmann and Solga [2022]).

such regulation may instead reduce training opportunities.

More broadly, our analysis contributes to the literature on minimum wage policies by demonstrating that their impact in training markets cannot be reduced to standard employment effects. They also shape match quality, contract stability, and the distribution of training opportunities across occupations. Future research should build on these findings by examining long-term outcomes of apprentices subject to the minimum wage – such as graduation chances, firm retention, and post-completion employment trajectories. Such evidence will be crucial to evaluate whether short-term improvements in apprenticeship uptake and stability translate into sustained gains in human capital formation and labour market performance.

In sum, our findings illustrate both the promise and complexity of regulating apprenticeship compensation. While statutory wage floors can help address concerns about fairness and attractiveness in vocational training, their heterogeneous consequences call for nuanced policy design that is attentive to sector-specific labour market conditions.

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A Descriptive Statistics

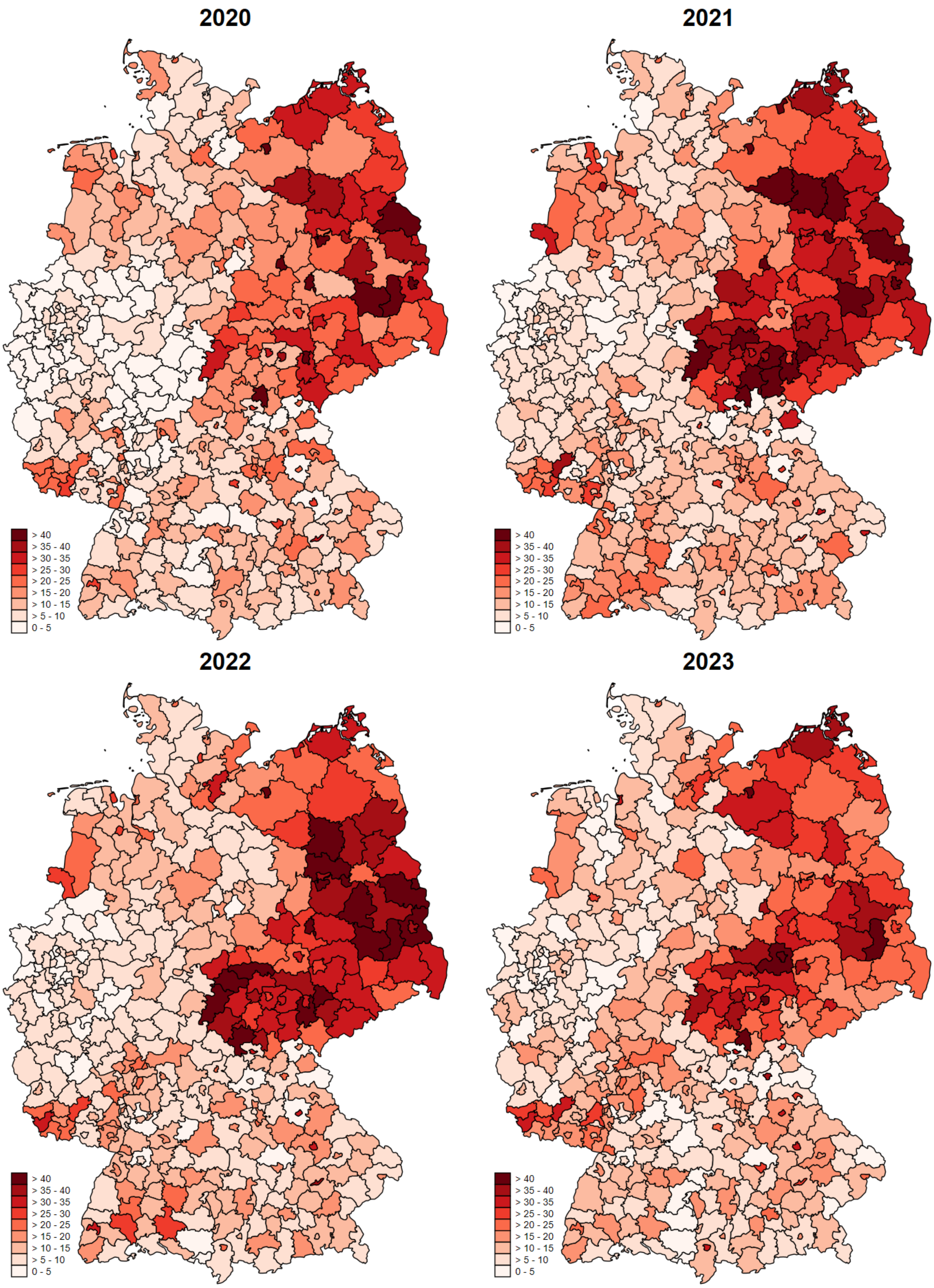

Figure A.1: Geographical Distribution of the Share of Minimum Wage Contracts in Occupations with Low-Apprenticeship Wages in Germany in 2020, 2021, 2022 and 2023 [in %].

B Robustness Checks

Table B.1: Robustness Checks: Newly Commenced Contracts in Low-Wage Occupations

Dependent variable: Newly commenced contracts	Baseline	Placebo -1 Year	Placebo -2 Years	Placebo -3 Years	upper/lower 10%	upper/lower 20%	sDiD	DDD
	0.078***	0.009	-0.005	0.006	0.058**	0.070***	9.185***	0.024
	(0.019)	(0.020)	(0.023)	(0.021)	(0.023)	(0.017)	(2.399)	(0.030)
Year Fixed-Effects	yes	yes	yes	yes	yes	yes	no	yes
Covariates	yes	yes	yes	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes	yes	no	yes
Observations	1,080	600	480	360	720	1,440	1,080	1,920
No. of Groups	120	120	120	120	80	160	120	240

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: FE Poisson models are estimated at the district level and are based on 2015-2023 cohorts of apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. In the DDD estimation, the same restrictions are applied, but the sample includes only the 2016–2023 cohorts. The placebo regressions with pseudo-treatment dates set one, two and three years before the actual reform are based on the 2015-2019, 2015-2018 and 2015-2017 cohorts, respectively. The assignment variable is the share of contracts at the district level with a minimum wage compensation from 2020 to 2023, and varies between upper/lower 15% (*Baseline*) to upper/lower 10% and upper/lower 20%, respectively. *Newly commenced contracts* contain the number of newly signed contracts in the respective district and year. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

Table B.2: Robustness Checks: Contract Termination Rate

Dependent variable: Contract Termination Rate	Baseline	Placebo -1 Year	Placebo -2 Years	Placebo -3 Years	upper/lower 10%	upper/lower 20%	Fractional Logit	sDiD	DDD
	-0.021***	-0.000	-0.019**	0.001	-0.014*	0.022***	-0.021***	-0.017***	-0.007
	(0.006)	(0.008)	(0.008)	(0.009)	(0.007)	(0.005)	(0.006)	(0.005)	(0.007)
Year Fixed-Effects	yes	yes	yes	yes	yes	yes	yes	no	yes
Covariates	yes	yes	yes	yes	yes	yes	yes	yes	yes
COVID-19 covariates	yes	yes	yes	yes	yes	yes	yes	no	yes
Observations	1,080	600	480	360	711	1,440	1,080	1,080	2,160
No. of Groups	120	120	120	120	80	160	120	120	120

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: (RE) Linear regression models are estimated at the district level and are based on 2015-2023 cohorts of apprentices in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district level with a compensation at the minimum wage from 2020 to 2023. *Contract Termination Rate* is the fraction of newly commenced contracts in the respective district and year that are terminated within the first year of an apprenticeship. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. Clustered standard errors at the district level are reported in parentheses.

C Potential Mechanisms

Table C.1: Composition of the Treatment Group in Sanitation and HVAC w.r.t. the Highest School Degree (DDD Estimates)

Dependent variable: Contract Termination Rate	Frac. High-School Graduates	Frac. Upper-Secondary Graduates	Frac. Lower-Secondary Graduates
	0.024**	-0.017	-0.008
	(0.010)	(0.016)	(0.015)
Year Fixed-Effects	yes	yes	yes
Covariates	yes	yes	yes
COVID-19 covariates	yes	yes	yes
Fraction before 2020 [%]	6.1%	51.6%	38.3%

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Linear regression models are estimated at the district level and are based on apprentices in sanitation and HVAC in the first year of their apprenticeship who are not publicly funded or part time employed, and have a reported compensation of more than €10. The assignment variable is the share of contracts at the district level with a compensation at the minimum wage from 2020 to 2023. *Contract Termination Rate* is the fraction of newly commenced contracts in sanitation and HVAC, in the respective district and year, that are terminated within the first year of an apprenticeship. *Covariates* include the annual district-level counts of lower- and upper-secondary school graduates, enterprises with 0 to 10, 11 to 50, and 51 to 250 employees, and GDP per capita. *COVID-19 covariates* include the level and the square of the total case rate per 100,000 people at the district level on May 1, 2020, 2021, and 2022. *Fraction before 2020 [%]* is the fraction of the respective school graduates among the apprentices in the treatment group in the pre-treatment period. Clustered standard errors at the district level are reported in parentheses.