Does more money help?
The impact of the apprenticeship minimum wage on training offers and take-ups

Tobler, Lina[1*] (ORC-ID 0000-0002-0492-9728); tobler@wiso.uni-koeln.de

Fervers, Lukas[1,2] (ORC-ID 0000-0002-7850-700X); fervers@wiso.uni-koeln.de

Reiff, Annika[3] (ORC-ID 0009-0008-4019-4600); reiff@ifm-bonn.org

[1] University of Cologne, Department of Sociology and Social Psychology, Cologne, Germany

[2] DIE – Leibniz Centre for Lifelong Learning, Bonn, Germany

[3] IfM – Institut für Mittelstandsforschung, Bonn, Germany

Preprint version 30/07/2026

Acknowledgements

This work is part of a project led by Marita Jacob and Lukas Fervers. We would like to thank Marita Jacob for valuable advice in the development of this paper. Moreover, we thank Hans Dietrich. Earlier versions of this research have been presented at FIS-Forum 2024, the ESPAnet PhD Workshop 2025 and ECSR 2025. We thank the discussants and participants for their valuable feedback.

Funding

This work was supported by the Hans Böckler Foundation
[grant number 2023-22-5 “Politische Reformen zur Förderung betrieblicher Ausbildung”].

Data availability

The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access. The data can be requested at the FDZ:
https://fdz.iab.de/en/our-data-products/establishment-data/iab-establishment-panel/

Abstract

We analyse the effect of the German minimum apprenticeship wage that was introduced in 2020 in response to increasing difficulties faced by firms in filling training positions. Theoretically, the impact on training activity is ambiguous: increased wages could potentially increase applicant numbers, while higher costs may reduce firms' incentives to offer apprenticeships. We provide empirical evidence of the policy's impact on offered training positions and actual take-ups. Using a difference-in-differences design with firm-level panel data, we find no evidence of a positive effect on either offered or filled trainings, but (if at all) negative effects.

Keywords

Public policy, evaluation, education, apprenticeship, wages, labour markets

1. Introduction

The apprenticeship system in Germany and other countries has been a success story for many decades. It enhances industry and firm-specific human capital development tailored to the needs of the industry and therefore contributes to both increasing productivity and economic growth as well as access to education and employment (see e.g., Barbieri et al., 2018; Benassi et al., 2022; Busemeyer et al., 2022; Casey, 1991; Goergen et al., 2012; Lüthi, 2025; Neyt et al., 2020). However, the German dual education system has recently been put under pressure. The trend towards tertiary education as well as demographic change has led to a decreasing supply of young adults willing to pursue an apprenticeship in the dual education system. Therefore, it has become increasingly difficult for companies to find qualified applicants for available apprenticeship places (BMBF, 2020; Seeber and Seifried, 2019). Consequently, between 2009 and 2019, a period of solid economic growth and increasing employment rates, the number of new apprenticeships has decreased from 564,307 to 525,081, with an increasing number of unfilled places (BMBF, 2020: 33; see also BIBB, 2020).

Many apprenticeships are covered by tariffs, as the wage setting on the German training market is dominated by collective wage bargaining between employer associations and unions. Some firms, moreover, apply their own tariff system. However, this is not the case for all apprenticeships (Schönfeld and Wenzelmann, 2020). Since educational choices are assumed to be partly driven by monetary considerations, raising apprenticeship pay on the federal level has been considered a promising strategy to increase the number of applicants and thus the number of unfilled places. In 2020, the Federal Government therefore introduced a statutory minimum apprenticeship wage of €515 per month with the aim to increase the attractiveness of apprenticeships (e.g., BMAS, 2020; BMBF, 2019; Deutscher Bundestag, 2019a; Deutscher Bundestag, 2019b).

However, from a theoretical perspective, the effect of the minimum apprenticeship wage (MAW) on actual take-ups in the dual education system is ambiguous. Neoclassical labour market theory (Borjas, 2013) indeed predicts that higher apprenticeship wages should increase the number of applicants. At the same time, higher wages raise training costs for firms, thereby reducing their incentive to offer apprenticeship positions and potentially lowering the number of places that are offered. It is therefore theoretically unclear, which effect dominates in terms of actual take-ups.

In this paper, we provide empirical evidence on the impact of the introduction of the MAW in Germany on offered places and actual take-ups in the dual education system. We combine firm-level panel survey and register data that contain yearly information about the number of offered and filled training places as well as affectedness by the introduction of the MAW, as measured by having paid wages below the minimum wage before 2020. To estimate the causal effect of the MAW on offered places and take-ups, we exploit variation in the affectedness of the MAW between firms and compare the development of affected and unaffected firms in a difference-in-differences (DiD) framework.

Our findings indicate that the reform did not lead to the positive effects intended by policy makers. If anything, the MAW led to a decrease in both the number of offered and the number of filled places (based on the results of some model specifications), which would suggest that the negative impact on offers dominates. We conducted a wide range of robustness and sensitivity analyses, including different model specifications (with and without covariates) and estimation techniques, placebo tests, post-treatment sample selection checks as well as different sample definitions.

This paper contributes to the literature in several ways. First, we contribute to the vast literature on the role and impact of minimum wages in general (e.g., Addison et al., 2012; Allegretto et al., 2011; Backhaus and Müller, 2023; Böckerman and Uusitalo, 2009; Bruttel et al., 2018; Card and Krueger, 1994; de Linde Leonard et al., 2014; Dütsch et al., 2025; Garnero et al., 2015; Sabia, 2008; Sabia, 2009). Second and more specifically, we add to the still rather scarce evidence on the impact of apprenticeship (minimum) wages on training activity (Higton et al., 2012; Langen and Dörsam, 2025; Linckh et al., 2023; Papps, 2020). Finally, we build on and extend previous literature on institutional conditions and policy measures concerned with the attractiveness of vocational education (cf. Busemeyer and Jensen, 2012; Cedefop, 2009; Di Stasio, 2017; Gospel, 1998; Ryan et al., 2013; Smith, 2023).

The remainder of this article is organised as follows. In section 2, we outline the theoretical arguments on the expected effect of the MAW in more detail and provide a brief overview of the related literature. Section 3 describes the dataset and variables used and outlines our research design and estimation technique. Section 4 presents our main results and robustness checks before section 5 concludes with a short discussion of policy implications and directions for future research.

2. Theoretical background and related literature

2.1 Theoretical background

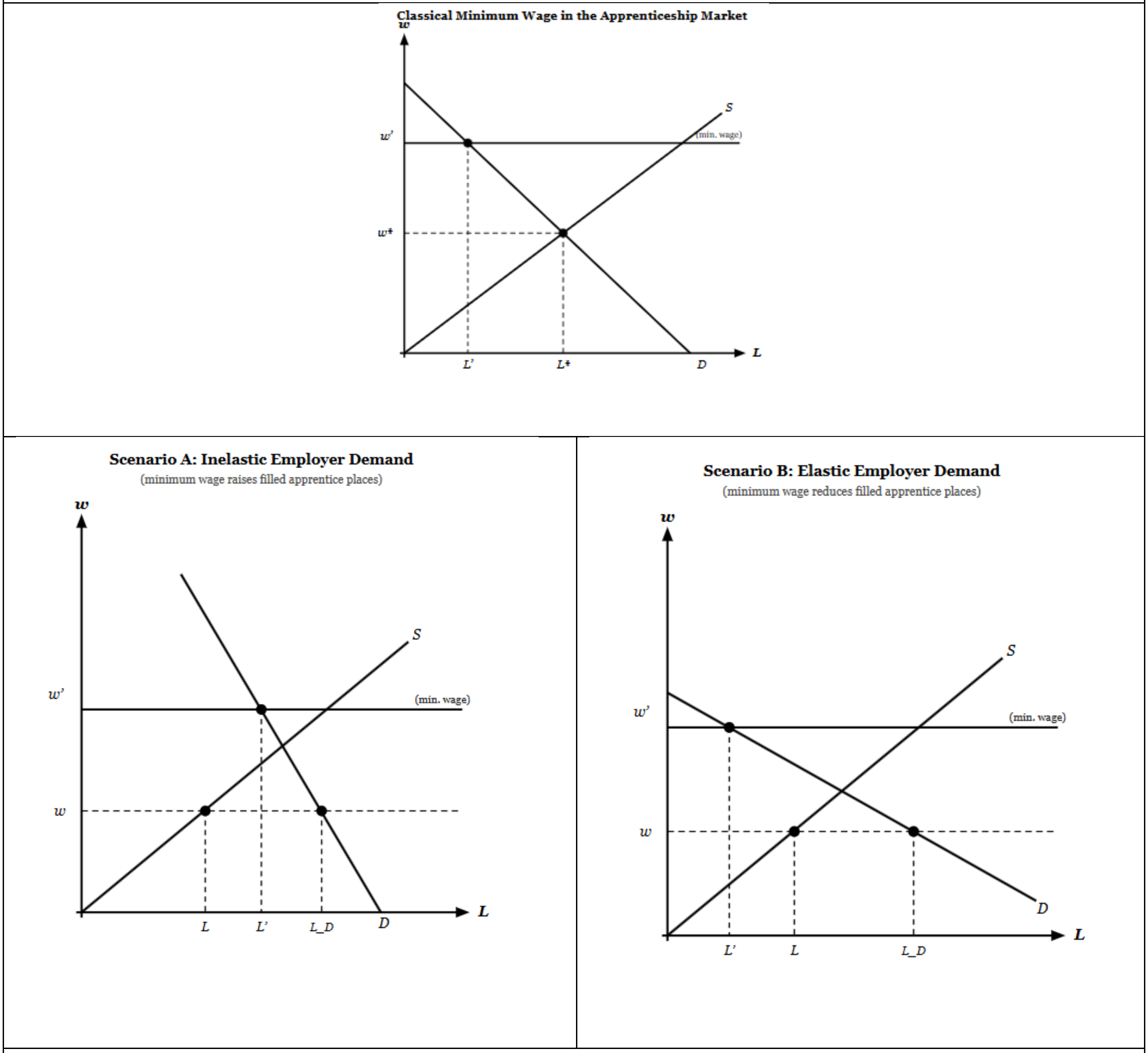
The theoretical expectations regarding the effect of minimum wages for apprentices are related to those for regular employees. Following standard neo-classical theory (Borjas, 2013), the minimum wage would push wages away from the market clearing wage. Starting from a market with zero unemployment (i.e., labour supply and labour demand meet at the market clearing wage), the introduction of a minimum wage would lead to increasing labour supply but decreasing labour demand by employers, resulting in an over-supply of labour (i.e., unemployment, Borjas, 2013; see our illustration in the upper panel of Figure 1). This standard model has been criticised by different lines of reasoning. One prominent argument is modern monopsony that assumes market power of employers due to search frictions in the labour market, incomplete competition or mobility constraints for workers, which challenge the assumptions of the neo-classical model already concerning regular employment (for an in-depth discussion, see Manning, 2021).

While modern monopsony provides one explanation for why wages may remain below competitive levels, the apprenticeship market exhibits an additional feature: prospective apprentices may choose tertiary education as an alternative (see Boeri and van Ours, 2013: chapter 8), potentially creating labour shortages even in the absence of monopsony. In recent years, Germany has experienced an increase of tertiary education with more and more young adults entering universities, but fewer and fewer new apprenticeships (see BIBB, 2020; BMBF, 2020). If apprenticeship wages are low and adjust slowly to a change in the supply/demand situation, young adults may choose not to enter the apprenticeship market but pursue a university education instead. In some regions and sectors, this has resulted in a shortage of apprentice supply at the given wages (e.g., BIBB, 2020; BMBF, 2020). This undermines the argument of the neo-classical model as labour supply and demand do not meet at the market clearing wage without regulation.

In this context, the effect of the MAW is unclear from a theoretical point of view. As illustrated in the lower panel of Figure 1, the effects of a minimum wage in this scenario on take-ups (i.e., filled places) can be either positive or negative depending on the degree of labour (apprentice) demand elasticity. In any case, the introduction of a minimum wage will lead to an increase of wages from w to w', an increase of labour supply and a decrease of labour demand. In case of rather inelastic labour demand (scenario A, bottom left panel of Figure 1), the reduction of labour demand can be over-compensated by the increase of labour supply, leading to a new level of employment L' that is still larger than the starting value L. In contrast, if demand elasticity is higher (scenario B, bottom right panel of Figure 1), the reduction of labour supply is so strong that the resulting level of employment (L') is below the level without the minimum wage. We illustrate this by a simple numerical example. Assume that a firm has offered ten apprenticeship places before the introduction of the MAW but could only fill eight places (starting level L). After the introduction of the MAW w', the firm might reduce its supply due to the increased costs to, say, nine places. If labour supply increases accordingly, this might lead to nine rather than eight filled places, i.e., $L' > L$ (scenario A). In contrast, in scenario B, the firm would show a stronger reaction and reduce labour demand to seven apprenticeship places. Even if all these seven places could be filled, this would still imply that $L' < L$.

This argument has three implications for the expected results with regard to offered and filled training places. First, we would expect a negative effect on offered places. Second, the effect on filled places can be either positive or negative depending on the elasticity of labour demand. Third, it has to be considered that there could also be a co-existence of scenario A and B in different sectors or regions of the economy. If the intended effect on labour supply exists at least in some regions or sectors with a shortage of labour supply, we would therefore expect that the negative effect on offered places would not translate one-to-one into a negative effect on filled places. In other words, if the outlined theoretical argument holds, the effect on filled places should at least be weaker than the one on offered places.

Figure 1: Theoretical expectations regarding the effects of the MAW wage on offered and filled places



Source: Illustration by authors. Note: This figure was generated with the use of Claude AI (version Sonnet 5). The authors reviewed the content and take full responsibility for the figure.

2.2 Related literature

Most of the literature on minimum wages focusses on regular employees. This literature is vast, though no consensus has been reached. The majority of research focuses on minimum wages in the United States (e.g., Addison et al., 2012; Allegretto et al., 2011; Card and Krueger, 1994; Neumark and Wascher, 2000; Neumark et al., 2014; Sabia, 2009) and the United Kingdom (e.g., de Linde Leonard et al., 2014; Dolton et al., 2012; Dolton et al., 2015; Metcalf, 2008; Stewart, 2002; Stewart and Swaffield, 2008). Most of these studies report no or only small negative overall employment effects. Some indicate that employment opportunities are primarily reduced for less skilled workers (Neumark and Wascher, 2008). Meer and West (2016) argue that minimum wages are more likely to reduce job growth over several years than to cause an instantaneous drop in employment.

In the given context, research on minimum wages in Germany might be particularly important as effects of minimum wages might vary with the institutional setting. The earliest studies on minimum wages in Germany examine their impact within specific industries and also deliver mixed results. Evidence on the introduction and increase of a minimum wage in the construction sector indicates negative employment effects in East Germany, while no effects are observed in West Germany (König and Möller, 2009; vom Berge and Frings, 2020). Boockmann et al. (2013) find no employment effects in the electrical trade industry. Similarly, Frings (2013) reports no employment effects in the construction industry, as well as among painters and electricians. In contrast, Aretz et al. (2013) identify negative employment effects in the roofing sector. Overall, the literature also finds no or negative employment effects. In 2015, Germany implemented the first comprehensive statutory minimum wage. Many studies examined the employment effects of this introduction. Overall, they find small negative employment effects (see e.g., Dütsch et al. (2025) for an overview of the literature) though this overall negative effect is mainly driven by a decrease in marginal employment (see Bruttel et al., 2018; Caliendo et al., 2018; Holtemöller and Pohle, 2017; Schmitz, 2017). At the same time, Holtemöller and Pohle (2017) find a positive effect on socially ensured employment (see also Bruttel et al., 2018), which would be in line with the results by Garloff (2019), who argues that marginal jobs transferred to regular jobs. In contrast, Schmitz (2017) does not only find evidence for a reduction in marginal employment but also for a reduction in regular employment. Further, Bossler and Gerner (2020) reveal that the reduction in employment is not driven by a rise in dismissals but rather by a reduction in new hires, while further analyses point to positive effects on wages and decreasing wage inequality (Bossler and Schank, 2023).

In contrast to the literature on minimum wages for regular employees, evidence on minimum wages for apprentices is rather scarce. Although some countries (e.g., Belgium, France, Ireland, UK) have implemented minimum wages for apprentices (Adema et al., 2018; Linckh et al., 2023), there is little evidence on its impact on the vocational training system. Higton et al. (2012) conducted a qualitative study on the introduction of a minimum wage for apprentices in the UK in 2010 and report no evidence of a reduction in apprenticeship offers by employers. Further related studies analyse the impact of changes of existing regulations rather than the newly introduced minimum wages. Papps (2020) analyses the increase in the age-specific minimum wage for apprentices in the UK and find a reduction in the amount of training after an increase in the minimum wage. Linckh et al. (2023) analyse the effects of aligning apprenticeship minimum wages across age groups in Germany, mandated by a 2006 antidiscrimination law that eliminated lower wage levels for apprentices under 18. They find that this policy led to significant declines in apprenticeship contracts for underage apprentices, particularly for low-qualified applicants, with stronger effects observed for larger wage adjustments.

With regard to the newly introduced German MAW, Langen and Dörsam (2025) analyzed its effect on apprenticeship postings in low-wage occupations and found no overall effects (one reason for this may be violations of the parallel trends assumption in the DiD design). However, for some production sectors, the authors reported negative effects on apprenticeship postings (Langen and Dörsam, 2025)

In sum, the impact of minimum wages is ambiguous for both regular employees as well as apprentices. While the literature on minimum wages on regular employees is large and growing, evidence on the impact of minimum wages for apprentices is still scarce and therefore constitutes an important gap in the literature.

3. Methodology

3.1 Research design

To estimate the effect of the reform, we compare firms that are affected by the introduction of the MAW (treatment group) to firms that are unaffected by this reform (control group) in a DiD framework. In brief, the DiD compares the development of both groups from before to after the reform. In its basic form, the effect of treatment d, δ_{ATT}, is estimated by comparing average outcomes (difference between treatment and control group) over time, i.e.,

$$(1)\ \widehat{\delta_{ATT}} = (\overline{y_{T,1}} - \overline{y_{T,0}}) - (\overline{y_{C,1}} - \overline{y_{C,0}}),$$

with subscript T and C denoting treatment and control group, and 0 and 1 before and after treatment. In the absence of variation in treatment timing, the parameter can be estimated by means of a classical two-way fixed effects estimator (TWFE, Wooldridge, 2021), i.e.,

$$(2)\ y_{it} = \mu_i + \theta_t + \boldsymbol{\delta_1} * \boldsymbol{treat_{it}} + u_{it}$$

where y_{it} is the number of offered or filled trainings, μ_i is the individual firm fixed effect, θ_t is the time fixed effect, $\boldsymbol{\delta}_1$ is the estimated treatment effect and $\boldsymbol{treat_{it}}$ indicates the treatment status of firm i at time t. As covariates are measured pre-treatment and are therefore time-invariant, they are not included in the TWFE equation (we elaborate on the inclusion of covariates later in this section).

In our context, firms that have paid apprenticeship wages below 515€ before the introduction of the MAW are defined as treated firms, while firms that have already paid higher wages before the introduction of the MAW are defined as the control group. The treatment came into effect for all new apprentices from 2020 onwards, so apprenticeships starting in autumn 2020 or later are defined as treated. Using the DiD approach, our estimates are neither biased due to structural differences between groups (that are quite likely) nor by general time effects (e.g., due to general COVID-19 trends). At the same time, it has to be acknowledged that the design assumes common trends: treated ($d = 1$) and non-treated ($d = 0$) firms would have developed in the same way if no treatment had taken place. In the terminology of the potential outcome framework this can be stated as follows:

(3) $E(Y_1^0|d = 1) - E(Y_0^0|d = 1) = E(Y_1^0|d = 0) - E(Y_0^0|d = 0)$

(common trends assumption; note that superscript 0 denotes the (hypothetical) outcome in case of no treatment, the subscripts 0 and 1 denote the time periods before and after the treatment and d denotes the actual treatment status).

In the given context, this assumption is controversial for two reasons. First, the treatment status may be correlated with several firm characteristics (e.g., industry, region, firm size) which might cause not only differences in outcome levels but also outcome trends. This implies that the groups might have developed differently before the introduction of the MAW as well as in the counterfactual scenario of no treatment. Second and more specific to this context, treated firms might be affected more strongly by the COVID-19 pandemic than non-treated firms. During the COVID-19 pandemic, certain businesses with direct human contact such as hairdressing, hotels or restaurants were closed to prevent the spread of the virus. As these sectors have often paid low wages, it is possible that they are overrepresented in the treatment group, implying that the latter was affected more heavily by COVID-19. Consequently, a parallel development over time before the treatment may be insufficient to justify the validity of the research design. We assess this problem in two steps. First, we extend the original TWFE specification to a doubly-robust combination of DiD with pre-matching as suggested by Callaway and Sant'Anna (2021). This approach follows a similar idea as earlier work by Abadie (2005) on semi-parametric DiD, but is more easily extended to the case of multiple time periods as well as unbalanced panels (the latter is the case in our dataset). Moreover, it employs a double robust estimation technique (Sant'Anna and Zhao, 2020). The basic idea is to first employ a matching/weighting approach that ensures covariate balance between treatment and control group in the weighted sample. This relaxes the common-trends assumption to the conditional common-trends assumption, i.e., it only has to hold conditional on observable covariates. As discussed above, this is particularly important in our case as differences in covariates could cause diverging trends. We include a wide range of control variables that consider both structural differences in more general terms as well as affectedness by COVID-19. Doing so, we ensure that our treatment effect estimates are not biased due to either structural differences or different affectedness by COVID-19. Second, we extend the common approach to assess diverging trends before treatment by means of placebo tests in time and conduct a series of post-treatment placebo tests on other outcomes, namely the effect of the reform on new hirings of other groups of employees. These groups are likely to be unaffected by the reform itself but if, for example, treated firms are generally more affected by COVID-19 than control firms, this might show up in negative placebo effects on other groups of employees. In contrast, if the placebo tests are insignificant, this would substantiate the credibility that firms would have developed similarly in case of no treatment.

3.2 Data, variables and sample

To analyse the effect of the introduction of the MAW, we use data from the IAB Establishment Panel (BP) (Bächmann et al., 2023a; Bächmann et al., 2023b), in which units are firms. As the IAB data is available in yearly data files, we used a syntax file provided by the Data Research Centre (FDZ) of the German Federal Employment Agency to combine the yearly data files into a panel dataset (Umkehrer, 2017). We combined the data with additional mergeable modules of the IAB Establishment History Panel (BHP) (Ganzer et al., 2023b; Ganzer et al., 2023a). The BHP is based on registry data and collects information on firms independent of their participation in the BP survey. Moreover, we enriched the data with additional regional data on (pre-treatment) unemployment (Bundesagentur für Arbeit, 2024), GDP (Statistische Ämter des Bundes und der Länder, 2023) and COVID-19 incidence rates (Robert Koch-Institut, 2024) on the district level, which we merged using district codes (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2024), that are also available in the BP. It should be noted that while this final dataset provides detailed firm-level information, it does not offer any employee or apprentice information.

As the new MAW was introduced in early 2020 and training cycles start in September, the policy came into effect in the training cycle of 2020/2021. Therefore, our observation period starts pre-treatment in the survey year 2019 (covering the training cycle of 2018/2019) and ends in 2022 (covering the training cycle of 2021/22). Since new firms enter the BP survey regularly to ensure large-enough sample sizes, we do not use earlier pre-treatment periods as this would leave us with only a comparatively small share of firms that meet all necessary sample restrictions (see below) and that remain in the survey until the post-treatment period. Figure 2 provides a timeline of survey years, training cycles and the timing of the new policy.

Figure 2: Timeline of survey years, training cycles and the policy introduction

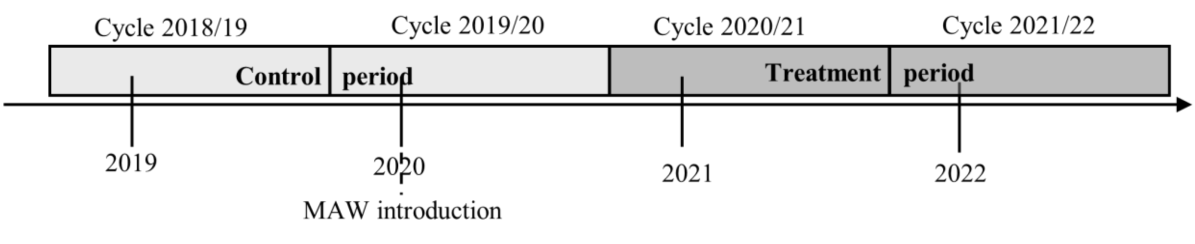


Illustration by authors.

The BP data provides variables on the number of offered as well as filled training places in each year, which we used as our outcome variables. As we are interested in the effect of the MAW introduction, we needed to identify firms affected by the introduction (treatment group) and firms that are not affected by it (control group). The BP survey wave of 2020 includes a question on whether firms have paid their new apprentices less than 515€ (i.e., the newly introduced MAW) in the training year of 2019/2020, which is the last year before the policy came into effect.[1] We used this information to determine treatment and control status. If at least one new apprentice was paid below the new minimum threshold, a firm was defined as 'treated'. As one new apprentice having been paid below the minimum is enough to be considered as treated – independent of the total number of new apprentices – this implies a rather conservative estimation of effects. Any other treatment definition, such as a certain share of apprentices below the minimum threshold would therefore likely lead to stronger effects than the ones we report. This is, because if firms would be considered to be treated only if a certain share of apprentices (or more than one apprentice) received wages below the new minimum wage level, more firms would be categorised as belonging to the control group. Due to sample size constraints, however, an evaluation of the treatment intensity is not possible. Firms without apprenticeship wages below the threshold are not treated and served as the control group. While the MAW was further increased in the years following 2020, we cannot analyse those successive increases because the BP does not include information on this. However, this should not be a problem for three reasons. First of all, the increases were small and our observation period only covers the first successive minimum wage increase (the survey year 2022 covers the training cycle 2021/2022), when the MAW was increased to €550 (BMAS, 2020). Second, as the share of treated firms is rather small even at the first introduction of the MAW, the share of firms covered by the additional increase is probably very small. Third, if anything, a stepwise increase which might lead to initial control group firms becoming treated in the training cycle 2021/2022 (survey year 2022) would bias our results towards zero. Therefore, in a worst-case scenario, we would underestimate the treatment effect for the last observation period.

We used BHP registry information for our firm-specific covariates because it has the advantage that information on establishments exist even if firms did not participate in the BP survey in a particular survey year. In order to be able to control for differences between treatment and control group that already existed prior to the policy introduction, we used covariate information from our pre-treatment period in the survey year 2019. We controlled for the total number of employees and the shares of full-time workers, women and workers with different skill levels. Moreover, we controlled for regional covariates: the district level unemployment rate, GDP and COVID-19 related 7-day incidence rate, for which we calculated the average for the pre-treatment period between 15 March and 31 July 2020. In addition, we adjusted for an additional measure of COVID-19 affectedness from the BP survey, namely a dummy variable on whether firms had closed their business (voluntarily or due to administrative order) due to the pandemic. Our final analysis sample included firms with at least one employee and in which apprentices received training in the training cycle of 2019/2020 (i.e., the 2020 survey wave). Firms had to have at least one newly filled training position in the 2019/2020 training cycle because the treatment variable depends on this filter. Related to this, we excluded firms that were not interviewed in 2020 because the treatment question was asked in this year. To address concerns about potential selectivity due to this, we compared the basic firm and regional characteristics of our final analysis sample to a sample of all firms that trained at least one apprentice at least at some point between 2018 and 2020 (see Table A1 in the appendix). This comparison shows that our sample is very similar in terms of basic characteristic, thereby assuring that firms which trained in 2020 are not substantially different from other training firms.

Moreover, we excluded firms with inconsistent numbers of trainees, new trainees (inflow) or employees between our two main data sources (BP and BHP). In these cases, it is likely that the BP survey did not target the firm or subunit that was meant to be targeted (e.g., the overall corporation versus a regional office). Cross-dataset inconsistencies were defined as an absolute deviation of more than 50 (more than 20 in the case of new trainees) plus a relative deviation of more than 0.95. Further, we excluded firms with implausible jumps over the years in offered and filled training places. Again, discrepancies over the years might indicate that the person answering the survey in subsequent years may have given information about different subunits of the firm. Implausible jumps in offered or filled training places across years were defined as a deviation of an absolute change of more than +/-20 plus a relative change of more than 0.99/-0.50. Additionally, we excluded firms with missing values on the treatment variable, outcome variables or covariates and firms which had less than two observations (with at least one post-treatment observation). Our main models are therefore based on 9205 (TWFE) / 9143 (CS-DiD) firm-year observations. We present a descriptive overview of our sample and variables for the year 2020 in Table A1 in the appendix. Since only about 4% of the total sample of firms are treated (see Table A1), this does not allow for additional analyses of heterogeneous effects, e.g., with regard to sectors, firm size or regions

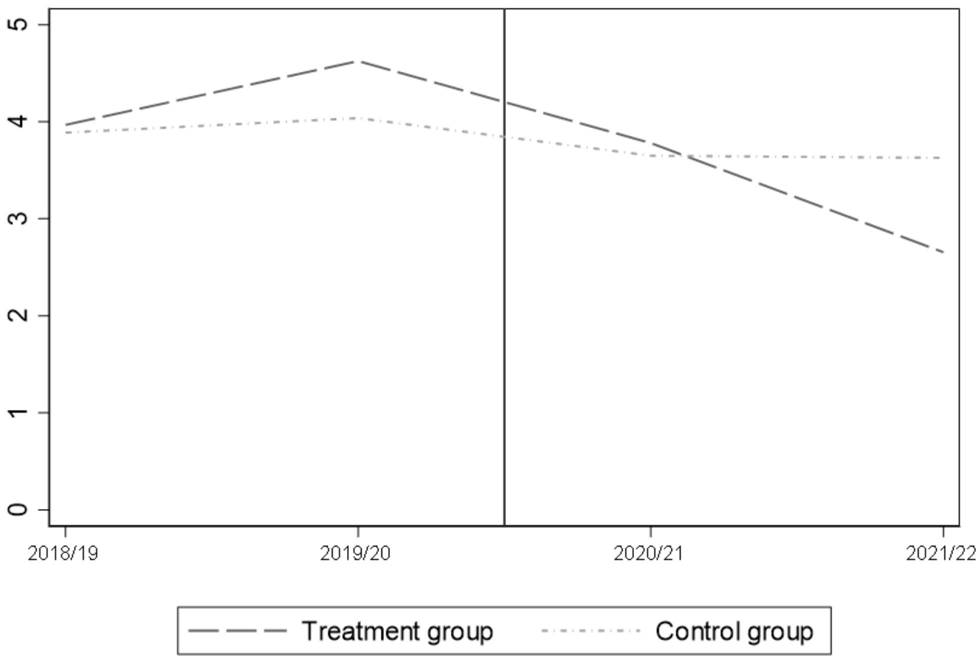
4. Results

4.1 Main results

In the following, we show unweighted descriptives and report unweighted results of our model estimations. We do so because weighting would only drive up the standard errors in our models (see Solon et al., 2015).

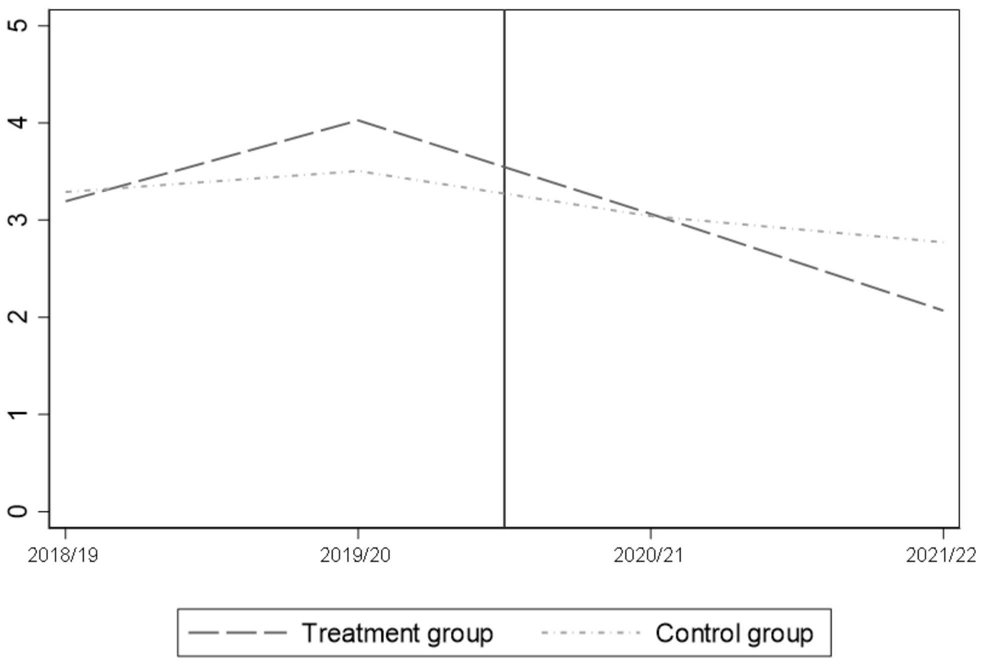
Figure 3 depicts descriptive trends of the number of offered trainings over time for treated and control firms. With apprenticeship cycles starting in September, training cycle 2020/2021 marks the first post-treatment period. We observe almost no trend in the control group and a slight upward trend in the pre-treatment period for the treatment group, followed by a trend break and downward trend in the post-treatment period. Regarding trends in the number of filled trainings over time (Figure 4), a very slight downward trend in the post-treatment period is observable in the control group, though the post-treatment downward trend is more pronounced in the treatment group. These descriptive results already point to slightly negative effects on both outcomes. Generally speaking, the small spikes in 2020 (covering the 2019/2020 training cycle) are due to our sample construction since firms had to have at least one newly filled apprenticeship in 2020 in order to receive the treatment question. The visually slightly stronger spike in the treatment group is most likely due to the smaller size of the treatment group compared to the control group, which leads to greater volatility in the graphs. It should be noted, though, that while pre-treatment trends may look as though they were non-parallel, robustness checks reveal no statistically significant difference in the pre-treatment trends between treatment and control group (see Section 4.2). While it cannot be ruled out completely that this spike may be partly due to anticipation effects, i.e., firms have hired more apprentices than necessary in the year before the reform. However, this is unlikely for three reasons. First, the exact implementation of the MAW was not yet clear when the training decisions for the previous year (training year 2019/2020) had to be made. Discussions about the reform started mid-2019 (see Deutscher Bundestag, 2019a; Deutscher Bundestag, 2019c), a draft of the new law came out in June 2019 (Deutscher Bundestag, 2019b). With the training year starting in September, firms should therefore not have had enough time to adjust their hiring strategy because of an anticipation of the MAW. Moreover, it was unclear whether the draft would become the actual law in that form. Second, the treatment group consists of rather small firms which presumably have limited capacity to simply hire more apprentices than actually necessary. Related to that, it has to be considered that the MAW was set at a rather low level. Consequently, the additional costs of hiring one additional apprentice one year earlier than actually necessary should have been much larger for most firms than simply accepting the higher wages after the introduction. Finally, while the decline in the year 2020/2021 might be the result of anticipation in 2019/2020, it is hard to explain why the reductions in offered and filled training places become even more pronounced in the second post-treatment period. We therefore do not see an indication for pre-treatment trends due to anticipation or other reasons.

Figure 3: Trends in the number of offered trainings



2019: N(control)=1722, N(treatment)=62; 2020: N(control)=2619, N(treatment)=112; 2021: N(control)=2493, N(treatment)=109; 2022: N(control)=2001, N(treatment)=87
Source: IAB-BP + BHP + regional data. Calculations by authors. Unweighted data.

Figure 4: Trends in the number of filled trainings



2019: N(control)=1722, N(treatment)=62; 2020: N(control)=2619, N(treatment)=112; 2021: N(control)=2493, N(treatment)=109; 2022: N(control)=2001, N(treatment)=87
Source: IAB-BP + BHP + regional data. Calculations by authors. Unweighted data.

Table 1 shows the results from our baseline specifications for both the TWFE and the combination of matching with DiD (CS-DiD) approach. The treatment effect estimates in our TWFE models show a negative treatment effect for both outcome variables (columns 1 and 2). Compared to the control group, the treatment group experienced a reduction in offered trainings by -0.312 places and a reduction in filled trainings by -0.230 places. However, these reductions are not significant. In contrast, in the CS-DiD models, we find significant differences between the treatment and control group. According to the models using pre-matching on firm- specific and regional covariates (columns 3 and 4), the treatment effect on offered (filled) trainings is -0.703 (-0.612). When adding closures due to COVID-19 as a further control variable in the pre-matching process (columns 5 and 6), the treatment effect coefficient remains almost unchanged. The results are moderate in size, considering pre-treatment levels of the outcome variables (average of 4.06 offered and 3.53 filled places; see Table A1 in the appendix). Relative to the pre-treatment standard deviations of 6.16 (offered) and 5.75 (filled), the effects estimated by the CS-DiD approach translate into effects of -0.11 standard deviations for both outcomes.

It should be noted though that the differences between the TWFE and CS-DiD estimates are not due to the inclusion of covariates but due to the definition of the pre-treatment period. Unlike TWFE, the CS-DiD approach relies only on the final year before treatment as pre-treatment period. As the treatment group has higher values in 2019/2020, the post-treatment drop is more pronounced compared to the TWFE estimates. This notion is confirmed by models that employ the CS-DiD approach but define 2018/2019 as pre-treatment period (see Appendix Table A3, Robustness: data II), where the results are similar to the TWFE estimates (and vice versa: TWFE without 2019 similar to CS-DiD effect sizes).

Table 1: Treatment effect estimations using TWFE and CS-DiD models

	Offered (TWFE[a])	**Filled (TWFE[a])**	**Offered (CS-DiD I[a, b])**	**Filled (CS-DiD I[a, b])**	**Offered (CS-DiD II[a, c])**	**Filled (CS-DiD II[a, c])**
Treatment effect	-0.312 (-1.22)	-0.230 (-1.03)	-0.703** (-2.87)	-0.612* (-2.47)	-0.709** (-2.88)	-0.615* (-2.49)
N	9205	9205	9143	9143	9143	9143

Source: IAB-BP + BHP + regional data. Unweighted data. Calculations by authors. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a] TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses.

[b] CS-DiD I = without controlling for COVID-19 closures.

[c] CS-DiD II = controlling for COVID-19 closures.

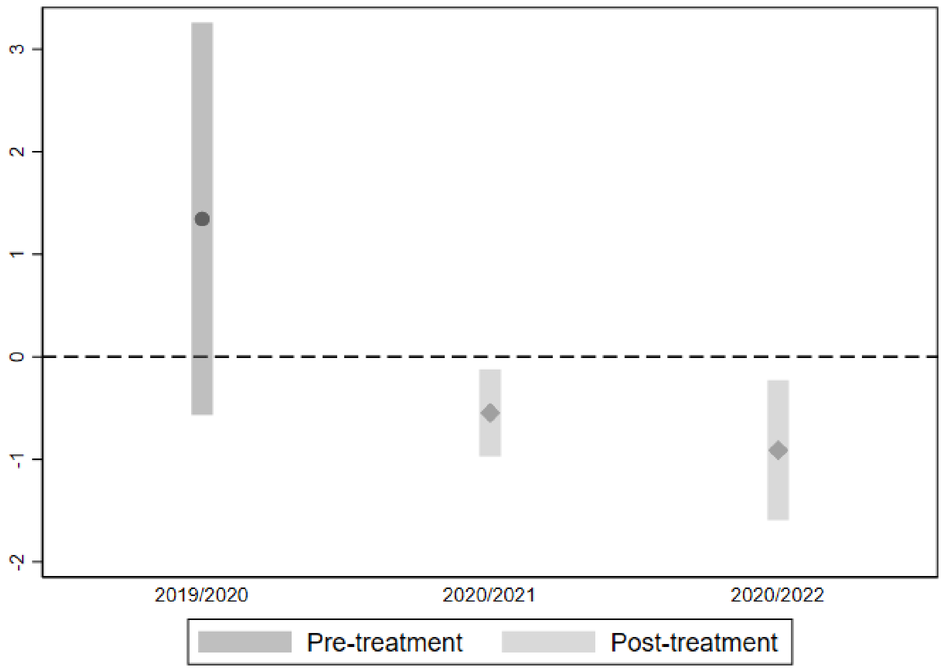
These results have several implications both at the substantive as well as at the methodological level. Most generally, the effects seem to be moderately negative on both outcomes, though – to a certain extent – size and significance depend on the chosen model. Second, the negative effect on offered places translates into a reduction in filled places of similar (albeit slightly smaller) size. This implies that the negative effect on offered trainings could not be (partly) offset by an increasing number of qualified applicants and a reduction in unfilled places. The theoretical conclusions for this finding are not entirely clear. As outlined in the theory section, in case of labour shortage before the reform, a positive effect on apprentice supply would lead to at least weaker effects on filled compared to offered places. One possible explanation is therefore that apprentice supply has not reacted to the introduction of the MAW. While the rather low level of the MAW adds to the plausibility of this explanation, this conclusion has to be drawn with some caution as it is possible that firms which had no unfilled places before the reform experienced an increase in applications (which is not visible in the data as the number of filled places for these firms depends on their apprentice demand, not supply). At the methodological level, the fact that the results do not change when COVID-19 related variables are included is a first hint that the treatment effect estimates are not driven by different affectedness by COVID-19 (for more details on this matter, see Section 4.2). Finally, it remains a challenge for interpretation that the results depend on the definition of the pre-treatment period to some extent. If one assumes the absence of anticipation effects, it is unclear what the best definition of the pre-treatment period is. Hence, the two model specifications have to be acknowledged as equally possible approaches. The general conclusion with regard to the effect of the reform should therefore be that it is either negative or indistinguishable from zero. Still, the results clearly show that the MAW did not lead to a substantial increase of new apprenticeships as intended.

4.2 Robustness checks

We conduct several further robustness checks to guarantee the robustness of the results both in terms of the credibility of our research design in general as well as concerning different estimation techniques and sample definitions.

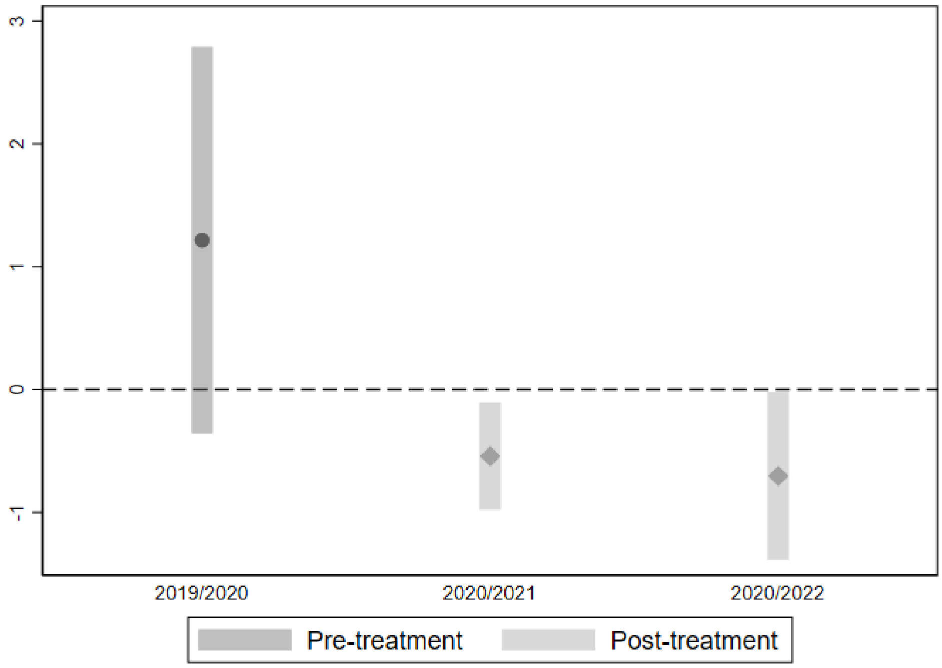
First, we conduct the typical placebo tests in time to test for diverging trends before treatment. Figures 5 and 6 show the results for all pre- and post-treatment periods for the two outcomes. The coefficient and confidence interval labelled 2019/2020 display the test for diverging trends before the treatment (see also Appendix Table A2, Part I, columns 1/2). The placebo test is positive but insignificant for both outcomes, thereby supporting the assumption of parallel pre-treatment trends. As a rather high number of firms has entered the panel only in 2019/2020, the sample size (and the power) for this test is rather small, as shown by the comparatively large confidence intervals. Nevertheless, the results show (if at all) diverging trends to the advantage of the treatment group. This implies that the negative treatment effects are not driven by diverging trends to the disadvantage of the treatment group before treatment, but that the true effect of the reform could in contrast be even slightly more negative (as long as one does not assume anticipation effects, which are unlikely due to the institutional setting, see Section 4.1).

Figure 5: CS-DiD for offered trainings



Source: IAB-BP + BHP + regional data. CS-DiD II specification (see Table 1). Calculations by authors. Note: CS-DiD calculates the ATE using the respective post-treatment period (2021 and 2022) in relation to the last pre-treatment year (2020). For the placebo test in time, the placebo post-treatment period (2020) is compared to the previous period (2019). 95% confidence intervals shown. Confidence intervals for the 2019/2020 period are larger due to the smaller sample size in the 2019 survey year (i.e., training cycle 2018/19).

Figure 6: CS-DiD for filled trainings



Source: IAB-BP + BHP + regional data. CS-DiD II specification (see Table 1). Calculations by authors. Note: CS-DiD calculates the ATE using the respective post-treatment period (2021 and 2022) in relation to the last pre-treatment year (2020). For the placebo test in time, the placebo post-treatment period (2020) is compared to the previous period (2019). 95% confidence intervals shown. Confidence intervals for the 2019/2020 period are larger due to the smaller sample size in the 2019 survey year (i.e., training cycle 2018/19).

Second, we conduct placebo tests on related outcomes that should not be affected by the reform. Specifically, we repeat our baseline specification and define general vacancies and the inflow of new employees as alternative outcomes (Appendix Table A2, Part I, columns 3/4). These groups should not have been affected by the MAW introduction. However, if treated firms diverge from control firms, for example with regard to their COVID-19 affectedness, this placebo test should detect such differences. We do not find significant placebo effects, which substantiates the credibility of our estimates. Additionally, we checked whether firms adjusted their hiring strategy by hiring more mini jobbers instead of apprentices as a form of cheap labour (Appendix Table A2, Part I, column 5). This might be expected if apprentices become more expensive due to the reform. However, we do not find a significant treatment effect on the hiring of mini jobbers, either.

Third, we had a closer look at possible confounding of treatment status with affectedness of COVID-19. To do so, we analysed differential selection into the treatment group depending on firm closures due to COVID-19 and find a slightly higher probability of affected firms to be treated. This result is significant on the 10% level (Appendix Table A2, Part I, column 6). However, since our CS-DiD results using the pre-matching approach did not show any changes in estimates between models controlling and not controlling for COVID-19 related closures (see Table 1, CS-DiD I vs. CS-DiD II), the differential selection into treatment does not seem to affect our main results. This seemingly contradictory finding can be explained by a supplementary DiD estimation where closure during COVID-19 is defined as treatment status. As our results (Appendix Table A2, Part II, columns 3/4) indicate, closure during COVID-19 does not affect offered or filled training places in autumn 2020. While this might be surprising at first glance, it should be considered that firms such as hairdressers, hotels or restaurants which were closed in spring 2020 had re-opened already in May or June 2020. At the same time, larger industrial firms might not have been closed but could have suffered from long-run effects such as disrupted supply chains. Nevertheless, we further assessed this surprising result by repeating our main CS-DiD analyses including further COVID-19 control variables on whether firms experienced difficulties with regard to demand, supply-chain related factors, personnel or liquidity (Appendix Table A2, Part II, columns 5/6), though results do not change here either. In brief, the results do not change due to COVID-19 affectedness as the latter had no impact on offered and filled places.

Fourth, we conducted a dropout analysis on firm exits (due to permanent firm closures) or missing interviews in the post-treatment period to check for systematic panel attrition. Our results could be biased if treated firms had a higher probability to leave the panel (due to bankruptcy or other reasons), as the remaining firms would most likely be a non-representative sample of (presumably) more successful firms. However, using logistic regression, we found no significant treatment effect on drop-out, meaning that treated firms were not more likely to drop-out in the post-treatment period than control firms (Appendix Table A2, Part II, columns, 1/2).

Fifth, we assessed the robustness of the results with regard to sample definition in two ways. As our dependent variables are skewed to the left, we repeated our main analyses using a 99%-trimmed sample where the top 1% of the respective outcome variable are excluded to account for sensitivity to outliers. Moreover, we dealt with missing values differently and repeated our analyses with a non-restricted sample. Here, we did not exclude firms with missing values on either one of the outcomes and covariates but used all firms available for the respective estimation. This aims at assessing whether results are driven by non-random missingness in the variables. However, these recalculations produced results that are very similar to our main specification (Appendix Table A3, Robustness: data I). The results therefore do not seem to be driven by outliers or sample selectivity.

Finally, we recalculated our CS-DiD models using alternative weighting approaches and found that our results are insensitive to this change (Appendix Table A3, Robustness: estimation).

Overall, our results seem robust with regard to placebo tests, selection, dropout, sample definition and weighting. Our main results of a negative treatment effect – i.e., a reduction in offered as well as filled training places seem to hold. However, depending on the model specification, in particular the definition of the pre-treatment period, effect sizes and significance levels vary.

5. Conclusion and Discussion

Against the background of decreasing numbers of new apprenticeships in Germany and a political reform aiming to bolster the dual education system, we analysed the effect of a nationwide minimum wage for apprentices that was introduced in 2020. To do so, we considered the policy effect on offered and filled training places and compared firms affected by the new MAW of

515€ with a control group of firms that already paid above this new threshold prior to the reform. We did not find evidence of a positive effect of the policy, which was intended by the policy makers. Rather, the results point to negative effects of similar size for both outcomes. This supports the more pessimistic view on the reform and contradicts the notion that higher apprenticeship wages at the bottom of the wage distribution would lead to a higher number of entrants into the system. The negative effect on offered places translates into a negative (and slightly smaller) effect on filled places. This suggests that there is little evidence for a positive effect on labour supply. The results are robust to a wide range of robustness checks and placebo tests. Specifically, the results do not seem to be driven by confounding of the treatment status due to COVID-19. At the same time, there is a certain sensitivity of the results concerning the definition of the pre-treatment period. Although we cannot state unequivocally that the negative treatment effect is significant in all models, we can conclude that the MAW introduction did not lead to increases in offered and filled training places but is either negative or insignificant. In sum, it seems that the reform – at least in its introductory phase – did not lead to the results intended by the policy makers.

Concerning the demand side of the dual education market, these results suggest that firms have a certain elasticity with respect to apprenticeship costs. This corroborates previous findings showing that firms adapt their supply of training in case of exogenous changes in apprenticeship costs (Langen and Dörsam, 2025; Linckh et al., 2023; Papps, 2020). While this is generally in line with standard economic theory, the reaction to the still comparatively low minimum wage supports previous research that estimates an already small return to dual education for firms (Linckh et al., 2023), which limits the scope for firms to pay higher apprenticeship wages without adjusting the number of training places. In this context, policy-makers and future research may consider incentives for firms, such as training cost subsidies and their effect on the supply of apprenticeship places and apprenticeship wages (e.g., van den Berg et al., 2006; Westergaard-

Nielsen and Rasmussen, 1999). With regard to the supply side, the results raise the question why the negative effect on offered places could not (at least partly) be offset by a higher number of qualified applicants. One possible explanation could be that there are few (groups of) persons that could have created the additional supply of potential apprentices. After completing school, young adults in Germany typically transfer to either the tertiary education system or some kind of vocational education. However, young adults that have previously decided to enter university are hardly convinced to change towards the dual education system because of slightly higher wages. That said, it should be considered that the new minimum wage was still rather low compared to other apprenticeship wages, where (for those apprentices covered by collective bargaining agreements) only 2% earned below 600€ in 2019 (Schönfeld and Wenzelmann, 2020: 10f.), with an average wage of 848€ in the first year of education (Schönfeld and Wenzelmann, 2020: 14f.). Whether our conclusions hold in subsequent stages of MAW increases from 2021 onwards has to be left to future research using alternative data sources that include information on the stepwise increases over time.

The results have implications for labour market and education research as well as policy-making but also point to unanswered questions. Most generally, the results imply that slight increases at the bottom of the apprenticeship wage distribution have little capacity to increase the number of entrants into the dual education system. At the same time, this conclusion should be interpreted with a certain degree of caution. On the one hand, this study has been limited to the effect of the MAW on training offers and the number of filled training places. A positive effect of apprenticeship minimum wages could also become visible in terms of decreased drop-out from dual education. In this context, it should be considered that completed rather than started apprenticeships are probably the more important outcome for the economy as well as for the income prospects of young adults. Relatedly, there could be further arguments in favour of the MAW as it could reduce dependence on social benefits or parental support. On the other hand, it should be considered that the apprenticeship wage increase was rather low for many affected firms. Consequently, our results are limited to the very lower end of the income distribution of apprentices. Future research could investigate the effect of the MAW on apprenticeship wages. As the dataset we used only provides information on firms we could not conduct analyses concerning the perspective of apprentices in this study. Relatedly, it has to be left to future research to evaluate whether larger increases in apprenticeship wages can attract more young adults, or whether long-run factors such as labour market prospects or non-monetary aspects such as public opinion and perceptions of prestige are more important (e.g., Busemeyer and Garritzmann, 2017; Di Stasio, 2017; Russo et al., 2019; Ryan and Lőrinc, 2018). Finally, the small sample size did not allow for an analysis of effect heterogeneity (e.g., in terms of profession or industrial sector) or analyses with varying treatment assignment rules. However, as elaborated, our treatment definition is conservative, which – if anything – would bias effects downwards, thereby also making our estimations conservative.

In any case, we conclude that the question of how to revitalise the dual education system remains a (partly) unanswered question. Given the importance of the dual education system for young adults but also the society and economy as a whole, this question should remain at the top of current labour market and education research as well as the policy agenda.

Notes

[1] Original wording in the German questionnaire: ‘Und bei wie vielen dieser besetzten Ausbildungsplätze liegt die Ausbildungsvergütung unter der neuen Mindestausbildungsvergütung in Höhe von 515 € monatlich?‘ (Authors’ translation: ‘And how many of these places are paid with an apprenticeship wage below 515€?‘) (see questionnaire of Bächmann et al., 2023a).

References

Abadie A (2005) Semiparametric Difference-in-Differences Estimators. *The Review of Economic Studies* 72(1): 1–19.

Addison JT, Blackburn ML, and Cotti CD (2012) The Effect of Minimum Wages on Labour Market Outcomes: County-Level Estimates from the Restaurant-and-Bar Sector. *British Journal of Industrial Relations* 50(3): 412–435.

Adema J, Giesing Y, Schönauer A, and Stitteneder T (2018) Minimum wages across countries. *ifo DICE Report* 16(4): 55–63.

Allegretto SA, Dube A, and Reich M (2011) Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data. *Industrial Relations: A Journal of Economy and Society* 50(2): 205–240.

Aretz B, Gregory T, and Arntz M (2013) The Minimum Wage Affects Them All: Evidence on Employment Spillovers in the Roofing Sector. *German Economic Review* 14(3): 282–315.

Bächmann A-C, Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, et al. (2023a) *IAB-Betriebspanel (IAB BP) – Version 9322 v1*. Nürnberg: Forschungsdatenzentrum (FDZ) der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.IABBP9322.de.en.v1. The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

Bächmann A-C, Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, et al. (2023b) *IAB-Betriebspanel (IAB-BP) 1993-2022*. FDZ-Datenreport, 16/2023 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2316.de.v1.

Backhaus T, Müller K-U (2023) Can a federal minimum wage alleviate poverty and income inequality? Ex-post and simulation evidence from Germany. *Journal of European Social Policy* 33(2): 216–232.

Barbieri P, Cutuli G, and Passaretta G (2018) Institutions and the school-to-work transition: disentangling the role of the macro-institutional context. *Socio-Economic Review* 16(1): 161-183.

Benassi C, Durazzi N, and Fortwengel J (2022) Comparative institutional disadvantage: Small firms and vocational training in the British manufacturing sector in comparative perspective. *British Journal of Industrial Relations* 60(2): 371–390.

BIBB (2020) *Datenreport zum Berufsbildungsbericht 2020. Informationen und Analysen zur Entwicklung der beruflichen Bildung*. Bonn: Bundesinstitut für Berufsbildung (BIBB). Available from: https://www.bibb.de/datenreport/de/datenreport_2020.php (Accessed 16 June 2025).

BMAS (2020) Mindestvergütung für Auszubildende tritt in Kraft. Available from: https://www.bmas.de/DE/Service/Presse/Meldungen/2020/neue-gesetze-mindestausbildungsverguetung.html (Accessed 2 July 2026).

BMBF (2019) *Berufsbildungsbericht 2019*. Bonn: Bundesministerium für Bildung und Forschung (BMBF). Available from: https://www.bmbfsfj.bund.de/bmbfsfj/service/publikationen/berufsbildungsbericht-2019-277654 (Accessed 24 July 2026).

BMBF (2020) *Berufsbildungsbericht 2020*. Bonn: Bundesministerium für Bildung und Forschung (BMBF). Available from: https://www.bmbfsfj.bund.de/bmbfsfj/service/publikationen/berufsbildungsbericht-2020-277918 (Accessed 24 July 2026).

Böckerman P, Uusitalo R (2009) Minimum Wages and Youth Employment: Evidence from the Finnish Retail Trade Sector. *British Journal of Industrial Relations* 47(2): 388–405.

Boeri T, van Ours J (2013) *The Economics of Imperfect Labor Markets: Second Edition*. Princeton University Press.

Boockmann B, Krumm R, Rattenhuber P, and Neumann M (2013) Turning the Switch: An Evaluation of the Minimum Wage in the German Electrical Trade Using Repeated Natural Experiments. *German Economic Review* 14(3): 316–348.

Borjas GJ (2013) *Labor Economics*. Sixth Edition. New York, NY: McGraw-Hill.

Bossler M, Gerner H-D (2020) Employment Effects of the New German Minimum Wage: Evidence from Establishment-Level Microdata. *ILR Review* 73(5): 1070–1094.

Bossler M, Schank T (2023) Wage Inequality in Germany after the Minimum Wage Introduction. *Journal of Labor Economics* 41(3): 813–857.

Bruttel O, Baumann A, and Dütsch M (2018) The new German statutory minimum wage in comparative perspective: Employment effects and other adjustment channels. *European Journal of Industrial Relations* 24(2): 145–162.

Bundesagentur für Arbeit (2024) *Arbeitslosenquote 2018/2019 auf Kreisebene*. Available from: https://statistik.arbeitsagentur.de/SiteGlobals/Forms/Suche/Einzelheftsuche_Formular.html?nn=1610104&topic_f=gemeinde-arbeitslose-quoten (Accessed 17 May 2024).

Bundesinstitut für Bau-, Stadt- und Raumforschung (2024) *Raumordnungsregionen 2019*. Available from: https://www.bbsr.bund.de/BBSR/DE/forschung/raumbeobachtung/downloads/archiv/download-referenzen.html (Accessed 17 May 2024).

Busemeyer MR, Carstensen MB, and Emmenegger P (2022) Orchestrators of coordination: Towards a new role of the state in coordinated capitalism? *European Journal of Industrial Relations* 28(2): 231–250.

Busemeyer MR, Garritzmann JL (2017) Academic, vocational or general? An analysis of public opinion towards education policies with evidence from a new comparative survey. *Journal of European Social Policy* 27(4): 373–386.

Busemeyer MR, Jensen C (2012) The impact of economic coordination and educational institutions on individual-level preferences for academic and vocational education. *Socio-Economic Review* 10(3): 525–547.

Caliendo M, Fedorets A, Preuss M, Schröder C, and Wittbrodt L (2018) The short-run employment effects of the German minimum wage reform. *Labour Economics* 53: 46–62.

Callaway B, Sant'Anna PHC (2021) Difference-in-Differences with multiple time periods. *Journal of Econometrics* 225(2): 200–230.

Card D, Krueger AB (1994) Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. *The American Economic Review* 84(4): 772–793.

Casey B (1991) Recent Developments in the German Apprenticeship System. *British Journal of Industrial Relations* 29(2): 205–222.

Cedefop (2009) *Modernising vocational education and training*. Fourth report on vocational education and training research in Europe: synthesis report. Cedefop Reference series. Luxembourg. Available from: https://www.cedefop.europa.eu/en/publications/3043#group-downloads (Accessed 9 January 2025).

de Linde Leonard M, Stanley TD, and Doucouliagos H (2014) Does the UK Minimum Wage Reduce Employment? A Meta-Regression Analysis. *British Journal of Industrial Relations* 52(3): 499–520.

Deutscher Bundestag (2019a) Bildung: Reform der beruflichen Bildung: Opposition will mehr. Available from: https://www.bundestag.de/dokumente/textarchiv/2019/kw43-de-berufliche-bildung-663262 (Accessed 7 January 2025).

Deutscher Bundestag (2019b) *Gesetzentwurf der Bundesregierung: Entwurf eines Gesetzes zur Modernisierung und Stärkung der beruflichen Bildung*. Drucksache 19/10815; 11.06.2019.

Deutscher Bundestag (2019c) *Unterrichtung durch die Bundesregierung: Berufsbildungsbericht 2019*. Drucksache 19/9515; 12.04.2019.

Di Stasio V (2017) 'Diversion or safety net?' Institutions and public opinion on vocational education and training. *Journal of European Social Policy* 27(4): 360–372.

Dolton P, Bondibene CR, and Stops M (2015) Identifying the employment effect of invoking and changing the minimum wage: A spatial analysis of the UK. *Labour Economics* 37: 54–76.

Dolton P, Bondibene CR, and Wadsworth J (2012) Employment, Inequality and the UK National Minimum Wage over the Medium-Term. *Oxford Bulletin of Economics and Statistics* 74(1): 78–106.

Dütsch M, Ohlert C, and Baumann A (2025) The Minimum Wage in Germany: Institutional Setting and a Systematic Review of Key Findings. *Jahrbücher für Nationalökonomie und Statistik* 245(1-2): 113–151.

Frings H (2013) The Employment Effect of Industry-Specific, Collectively Bargained Minimum Wages. *German Economic Review* 14(3): 258–281.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023a) *Betriebs-Historik-Panel (BHP) – Version 7522 v1*. Nürnberg: Forschungsdatenzentrum (FDZ) der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.BHP7522.de.en.v1. The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and/or remote data access.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023b) *Betriebs-Historik-Panel 1975-2022*. FDZ-Datenreport, 15/2023 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2315.de.v1.

Garloff A (2019) Did the German Minimum Wage Reform Influence (Un)employment Growth in 2015? Evidence from Regional Data. *German Economic Review* 20(3): 356–381.

Garnero A, Kampelmann S, and Rycx F (2015) Minimum wage systems and earnings inequalities: Does institutional diversity matter? *European Journal of Industrial Relations* 21(2): 115–130.

Goergen M, Brewster C, Wood G, and Wilkinson A (2012) Varieties of Capitalism and Investments in Human Capital. *Industrial Relations: A Journal of Economy and Society* 51(s1): 501–527.

Gospel H (1998) The Revival of Apprenticeship Training in Britain? *British Journal of Industrial Relations* 36(3): 435–457.

Higton J, Hirst A, Klahr R, Lefaucheux M, and Salmon C (2012) *An Assessment of the Introduction of the Apprentice Rate: Report for the Low Pay Commission:* Ipsos MORI Social Research Institute; Cambridge Policy Consultants.

Holtemöller O, Pohle F (2017) *Employment effects of introducing a minimum wage: The case of Germany.* IWH Discussion Papers, 28/2017. Halle (Saale): Leibniz-Institut für Wirtschaftsforschung Halle (IWH). Available from: https://nbn-resolving.de/urn:nbn:de:gbv:3:2-82434%0A.

König M, Möller J (2009) Impacts of minimum wages: a microdata analysis for the German construction sector. *International Journal of Manpower* 30(7): 716–741.

Langen H, Dörsam M (2025) *Minimum Wages in the Apprenticeship Market: Adverse Effects on Labor Demand?* Swiss Leading House "Economics of Education" Working Paper, No. 234.

Linckh C, Neuber-Pohl C, and Pfeifer H (2023) *The employment effects of raising negotiated minimum wages for apprentices*. Swiss Leading House "Economics of Education" Working Paper No. 202.

Lüthi S (2025) Classroom versus workbench: The impact of firm-based learning on labour market and educational outcomes. *Economics of Education Review* 109, 102719: 1–19.

Manning A (2021) The Elusive Employment Effect of the Minimum Wage. *Journal of Economic Perspectives* 35(1): 3–26.

Meer J, West J (2016) Effects of the Minimum Wage on Employment Dynamics. *Journal of Human Resources* 51(2): 500–522.

Metcalf D (2008) Why has the British National Minimum Wage had Little or No Impact on Employment? *Journal of Industrial Relations* 50(3): 489–512.

Neumark D, Salas JMI, and Wascher W (2014) Revisiting the Minimum Wage—Employment Debate: Throwing Out the Baby with the Bathwater? *ILR Review* 67(3_suppl): 608–648.

Neumark D, Wascher W (2000) Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Comment. *American Economic Review* 90(5): 1362–1396.

Neumark D, Wascher W (2008) *Minimum Wages*. Cambridge, MA: MIT Press.

Neyt B, Verhaest D, and Baert S (2020) The impact of dual apprenticeship programmes on early labour market outcomes: A dynamic approach. *Economics of Education Review* 78, 102022: 1–21.

Papps KL (2020) *How the Minimum Wage Affects Training among Apprentices.* IZA Descussion Paper, No. 13499. Bonn: Institute of Labor Economics (IZA).

Robert Koch-Institut (2024) *7-Tage-Inzidenz der COVID-19-Fälle in Deutschland*. Zenodo (Accessed 17 May 2024). DOI: 10.5281/ZENODO.7129007.

Russo G, Serafini M, and Ranieri A (2019) Attractiveness is in the eye of the beholder. *Empirical Research in Vocational Education and Training* 11(1), 7: 1–22.

Ryan L, Lőrinc M (2018) Perceptions, prejudices and possibilities: young people narrating apprenticeship experiences. *British Journal of Sociology of Education* 39(6): 762–777.

Ryan P, Backes-Gellner U, Teuber S, and Wagner K (2013) Apprentice pay in Britain, Germany and Switzerland: Institutions, market forces and market power. *European Journal of Industrial Relations* 19(3): 201–220.

Sabia JJ (2008) Minimum wages and the economic well-being of single mothers. *Journal of Policy Analysis and Management* 27(4): 848–866.

Sabia JJ (2009) Identifying Minimum Wage Effects: New Evidence from Monthly CPS Data. *Industrial Relations: A Journal of Economy and Society* 48(2): 311–328.

Sant'Anna PHC, Zhao J (2020) Doubly robust difference-in-differences estimators. *Journal of Econometrics* 219(1): 101–122.

Schmitz S (2017) *The effects of Germany's new minimum wage on employment and welfare dependency.* Diskussionsbeiträge, 2017/21. Berlin: Freie Universität Berlin, Fachbereich Wirtschaftswissenschaft. Available from: https://hdl.handle.net/10419/167586 (Accessed 6 January 2025).

Schönfeld G, Wenzelmann F (2020) *Tarifliche Ausbildungsvergütungen: Ergebnisse für 2019*. Bonn: Bundesinstitut für Berufsbildung (BIBB). Available from: https://www.bibb.de/de/118825.php (Accessed 7 January 2025).

Seeber S, Seifried J (2019) Herausforderungen und Entwicklungsperspektiven der beruflichen Bildung unter veränderten Rahmenbedingungen. *Zeitschrift für Erziehungswissenschaft* 22(3): 485–508.

Smith E (2023) Apprenticeships: The problem of attractiveness and the hindrance of heterogeneity. *International Journal of Training and Development* 27(1): 18–38.

Solon G, Haider SJ, and Wooldridge JM (2015) What Are We Weighting For? *The Journal of Human Resources* 50(2): 301–316.

Statistische Ämter des Bundes und der Länder (2023) *Gemeinsames Statistikportal: Bruttoinlandsprodukt, Bruttowertschöpfung in den kreisfreien Städten und Landkreisen der Bundesrepublik Deutschland 1992 und 1994 bis 2022*. Reihe 2, Kreisergebnisse Band 1, Tabellenblatt 6. Available from: https://www.statistikportal.de/de/vgrdl/ergebnisse-kreisebene/bruttoinlandsprodukt-bruttowertschoepfung-kreise (Accessed 17 May 2024).

Stewart MB (2002) Estimating the Impact of the Minimum Wage Using Geographical Wage Variation. *Oxford Bulletin of Economics and Statistics* 64(supplement): 583–605.

Stewart MB, Swaffield JK (2008) The Other Margin: Do Minimum Wages Cause Working Hours Adjustments for Low-Wage Workers? *Economica* 75(297): 148–167.

Umkehrer M (2017) *Combining the Waves of the IAB Establishment Panel: a Do-file for the Basic Data Preparation of a Panel Data Set in Stata*. FDZ-Methodenreport 12/2017 (en). Nürnberg: Forschungsdatenzentrum (FDZ) der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB).

van den Berg N, Meijers F, and Sprengers M (2006) More Vocational Education and Supplementary Training through Equalization of Costs? An Analysis of a Training and Development Fund in the Netherlands. *Human Resource Development International* 9(1): 5–24.

vom Berge P, Frings H (2020) High-impact minimum wages and heterogeneous regions. *Empirical Economics* 59(2): 701–729.

Westergaard-Nielsen N, Rasmussen AR (1999) The impact of subsidies on the number of new apprentices. *Research in Labor Economics* 18: 359–375.

Wooldridge JM (2021) Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators. *SSRN*. DOI: 10.2139/ssrn.3906345.

Appendix

Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups

Table A1: Summary statistics (2020)

Analysis sample						**Sample of all firms that trained sometime between 2018 and 2020**			
Variable	**N**	**Mean**	**SD**	**Min.**	**Max.**	**N**	**Mean**	**Min.**	**Max.**
Treated	2731	0.041	0.198	0	1				
Offered trainings	2731	4.061	6.164	1	124				
Filled trainings	2731	3.527	5.752	1	109				
General vacancies	1764	3.077	7.258	0	141				
New regular hires (inflow)	2731	14.791	33.318	0	694				
New mini job hires (inflow)	2731	2.003	17.359	0	821				
Number of employees (2019)	2731	148.603	337.814	1	7869	3700	174.668	0	44688
Share of full-time employees (2019)	2731	62.231	25.994	0	100	3691	63.115	0	100
Share of female employees (2019)	2731	43.190	29.703	0	100	3691	42.466	0	100
Share of skill level 1 employees (2019)	2731	15.408	17.449	0	100	3691	15.599	0	100
Share of skill level 2 employees (2019)	2731	64.991	21.570	0	100	3691	64.509	0	100
Share of skill level 3 employees (2019)	2731	11.294	12.855	0	100	3691	11.394	0	100
Share of skill level 4 employees (2019)	2731	8.306	12.238	0	100	3691	8.498	0	100
Affected by COVID-19 closures	2731	0.241	0.428	0	1	3578	0.247	0	1
Unemployment rate (2019)	2731	5.347	2.205	1.3	12.2	3704	5.323	1.3	12.8
GDP (2019)	2731	38774.645	15399.404	16605.000	195809.000	3704	39349.746	16605.000	195809.000
COVID-19 7-day incidence rate (15 March – 31 July 2020)	2731	99.987	67.079	10.806	701.137	3704	102.845	10.806	701.137
Number of trainees	2722	7.712	15.750	0	332	3686	8.952	0	1453

Source: IAB-BP + BHP + regional data. Calculations by authors.

Table A2: Robustness checks – placebo tests, selection, dropout, COVID-19

Part I						
Robustness: placebo tests	Offered (placebo: time) (CS-DiD II)	Filled (placebo: time) (CS-DiD II)	Vacancies (placebo: outcome) (CS-DiD II)	New hires (reg. emp.) (placebo: outcome) (CS-DiD II)	New hires (mini job) (placebo: outcome) (CS-DiD II)	**Robustness: selection into treatment** (COVID-19 closure) (Logit)
Treatment effect	1.345 (1.38)	1.215 (1.51)	0.631 (1.24)	2.897 (0.96)	3.692 (0.90)	0.378+ (1.81)
N	3532	3532	6224	9122	9122	2731
Part II						
Robustness: dropout analysis + COVID-19 checks	Exit (post-treatment) (Logit)	No interview (post-treatment) (Logit)	Offered (treatment: COVID-19 closure) (CS-DiD I)	Filled (treatment: COVID-19 closure) (CS-DiD I)	Offered (CS-DiD II + add. COVID-19 controls)	Filled (CS-DiD II + add. COVID-19 controls)
Treatment effect	-0.970 (-0.95)	-0.175 (-0.77)	-0.001 (-0.01)	0.045 (0.29)	-0.712** (-2.84)	-0.607* (-2.45)
N	4792	4808	9134	9134	8963	8963

Source: IAB-BP + BHP + regional data.
CS-DiD I = without controlling for COVID-19 closures, CS-DiD II = controlling for COVID-19 closures.
Logit models show t-statistics in parentheses. TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses. Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A3: Robustness checks – data, estimation

Robustness: data I	Offered (non-restricted sample) (TWFE)	Filled (non-restricted sample) (TWFE)	Offered (trimmed sample: 99%) (CS-DiD II)	Filled (trimmed sample: 99%) (CS-DiD II)
Treatment effect	-0.372	-0.254	-0.632**	-0.559*
	(-1.50)	(-1.19)	(-2.60)	(-2.32)
N	12512	12443	9008	9014

Robustness: data II	Offered (without 2019) (TWFE)	Filled (without 2019) (TWFE)	Offered (without 2020) (CS-DiD I)	Filled (without 2020) (CS-DiD I)
Treatment effect	-0.689**	-0.608*	0.433	0.486
	(-2.86)	(-2.49)	(0.81)	(1.16)
N	7421	7421	4893	4893

Robustness: estimation	Offered (CS-DiD II, stdipw)	Filled (CS-DiD II, stdipw)	Offered (CS-DiD II, drimp)	Filled (CS-DiD II, drimp)
Treatment effect	-0.710**	-0.615*	-0.710**	-0.615*
	(-2.88)	(-2.48)	(-2.88)	(-2.48)
N	9143	9143	9143	9143

Source: IAB-BP + BHP + regional data.
CS-DiD I = without controlling for COVID-19 closures, CS-DiD II = controlling for COVID-19 closures.
TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses. Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001