Labour Market Policies and their Effect on the School-to-Work Transition

Inauguraldissertation

zur

Erlangung des Doktorgrades

der

Wirtschafts- und Sozialwissenschaftlichen Fakultät

der

Universität zu Köln

2026

vorgelegt von

Lina Tobler, M.Sc.

Referentin: Prof. Dr. Marita Jacob, Universität zu Köln
Korreferent: Prof. Dr. Karsten Hank, Universität zu Köln
Tag der Promotion: 13.10.2026

Acknowledgements

Before starting with the substantial part of my dissertation, I want to express my gratitude to the people who have supported me throughout this time.

First and foremost, I would like to thank my first supervisor Marita Jacob for her guidance and valuable feedback whenever I needed it as well as her support of my professional and personal development. Her advice and experience have not only helped me in sharpening my arguments but also more generally in navigating the world of academia. I also want to thank my second supervisor Karsten Hank who believed in my work while giving me great freedom in pursuing my research.

I am indebted to my co-author Lukas Fervers who led the project and who came up with the initial ideas that made this project (and therefore my dissertation in this project) possible. Thank you for your constant feedback and advice, especially on how to improve the work methodologically – I really learned a lot. You instilled in me the appreciation for a sound robustness check. I also want to thank my co-author Annika Reiff with whom I spent quite some hours in the GESIS Safe Room (thanks to the GESIS team) to discuss data preparation and analyses. Thanks to both Lukas and Annika for the improvised lunch breaks throughout Cologne's city centre to clear our minds and get new energy for the sometimes-tricky data questions.

Moreover, I would like to thank everyone at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) in Nuremberg for providing me with the data access. At the start of this project, I went on a trip to the IAB to get acquainted with the data. I want to thank Clara Wolf as well as Silke Anger and her team for the hospitality. A special thanks goes out to Hans Dietrich who took the time to chat with me about his experiences with the datasets and potential challenges I would need to keep in mind when using them.

Thank you, Veronika Knize and Bernhard Christoph, with whom I worked together in a previous project, who introduced me to the IAB and with whom I wrote my first research articles. This really helped preparing me for the PhD adventure.

Thank you, Julia Leesch, my co-author with whom I developed and published a paper while both working on our respective dissertations. Pursuing a side project with a friend was a fun distraction from my PhD work, which ultimately helped me to better understand the broader subject area and research related to my dissertation.

Further, I would like to thank my colleagues Amelie Reiner, Paula Steinhoff, Lisa Jessee, Bettina Hünteler, Shannon Taflinger, Ansgar Hudde, Emilia Kmiotek-Meier and all the other friendly faces on the third floor as well as the ME office crew and everyone else I met during this time. Thank you for providing me with just the right amount of fun at the office as well as after work. There is no better sign of a lovely work environment than when colleagues become friends.

I would also like to thank André Ernst (as well as his predecessor Joël Binckli) and Petra Altendorf for assisting me in manoeuvring through the administrative and bureaucratic world of academia and the University.

Thanks to my family and non-work friends for cheering me on and believing that I could do it.

And finally, but most importantly, thank you Lena – for always being there, for standing me when I am stressed out, for enjoying life with me when I am not. I would never have done this without you. Thank you for everything.

Table of contents

Chapter 1. Introduction 1

1.1 Background and aim of the dissertation 1

1.2 Theoretical background 5

1.3 Method 8

1.3.1 Analytical strategy: the difference-in-differences design 8

1.3.2 Data 11

1.4 Overview and summary of the separate studies 13

1.4.1 Summary of Study 1: Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups 15

1.4.2 Summary of Study 2: The long-term effects of monetary (dis-)incentives on apprenticeship training: Does an apprenticeship minimum wage lead to more or less successful training completions? 17

1.4.3 Summary of Study 3: Efficient stabilizer or deadweight effect? The impact of subsidies for apprenticeship training during an economic downturn 19

1.5 Discussion and Conclusion 22

1.6 Outlook 26

1.7 Development of the studies and contribution of co-authors 28

1.8 References 29

Chapter 2. Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups 38

2.1 Introduction 38

2.2 Theoretical background and related literature 41

2.2.1 Theoretical background 41

2.2.2 Related literature 44

2.3 Methodology 47

2.3.1 Research design 47

2.3.2 Data, variables and sample 49

2.4 Results 53

2.4.1 Main results 53

2.4.2 Robustness checks 58

2.5 Conclusion and discussion 62

2.6 References 65

2.7 Appendix 72

Chapter 3. The long-term effects of monetary (dis-)incentives on apprenticeship training: Does an apprenticeship minimum wage lead to more or less successful training completions? 75

3.1 Introduction 76

3.2 Previous research and theoretical background 79
3.2.1 Perspective of firms 80
3.2.2 Perspective of apprentices 82
3.3 Methods 83
3.3.1 Analytical approach 83
3.3.2 Data, variables and sample 87
3.4 Results 90
3.4.1 Main results 90
3.4.2 Sensitivity analyses 94
3.5 Discussion and conclusion 96
3.6 References 99
3.7 Appendix 104
Chapter 4. Efficient stabilizer or deadweight effect? The impact of subsidies for apprenticeship training during an economic downturn 109
4.1 Introduction 110
4.2 Institutional background, theory and related work 112
4.3 Empirical analysis 116
4.3.1 Research design and estimation 116
4.3.2 Data and variables 120
4.4 Results 123
4.4.1 Main results 123
4.4.2 Robustness and sensitivity analyses 128
4.5 Discussion and conclusion 133
4.6 References 136
4.7 Appendix 141
Data availability statement 149

CHAPTER 1. INTRODUCTION

1.1 Background and aim of the dissertation

The transition from school to work is not only a critical passage in the life of an individual with regard to their occupation and career but also for societies and economies. Facilitating that people are smoothly integrated into the labour market ensures societal welfare and productivity, for example by preventing unemployment (e.g., Ryan, 2001; Shavit and Müller, 1998). A close link between education and occupation provides favourable conditions for an effective labour market integration (e.g., Allmendinger, 1989; Bol et al., 2019; DiPrete et al., 2017; Iannelli and Raffe, 2007).

A way to ensure a smooth transition is via vocational education and training (VET) embedded in a country's institutional setup (Barbieri et al., 2018; Busemeyer et al., 2022; Carstensen and Ibsen, 2021; Ibsen and Thelen, 2025). Especially firm-based training has been associated with good employment probabilities at the beginning of people's working life (e.g., Forster and Bol, 2018; Forster et al., 2016; Rözer and Bol, 2019). Former trainees may later be hired as regular employees by their training firm (see e.g., Acemoglu and Pischke, 1998; Acemoglu and Pischke, 1999; Muehlemann and Wolter, 2014). Alternatively, other firms may become an employer because in a standardised training system, educational certificates send clear signals to employers (Allmendinger, 1989; cf. Spence, 1973).

At the heart of an efficient training system is the demand for trainees by firms and the supply of people willing to be trained.[1] Applying classic labour market theory (Borjas, 2013) to the case of the training market, supply and demand depend on the costs and benefits for employers (firms) and workers (trainees) alike, and balances out at the market equilibrium, where supply equals demand. However, this equilibrium may not be reached: low wages may

[1] Note that the terms 'supply' and 'demand' are used according to the general labour market literature (literature on training sometimes uses 'supply' to describe the employers' side and 'demand' to describe people interested in apprenticeships).

lead to shortages in supply despite the demand for trainees by firms. Similarly, following the theoretical argument, high costs of training should reduce firms' demand for trainees (for the classic economic argument of labour supply and demand and the role of wages, see Borjas, 2013). This may occur, for example, in times of economic crises and uncertainties with regard to the financial future of firms. Both scenarios – supply and demand shortages – threaten the functioning of the training system.

In such cases, policies can help to strengthen supply or demand by altering incentive structures for the actors involved (Brunello, 2009), for example by means of financial incentives that alter costs and benefits (e.g., Boeri and van Ours, 2013; Borjas, 2013). Whether such policies actually provide viable solutions to supply or demand challenges depends on their design (Boeri and van Ours, 2013; see also Brunello, 2009: 159), for example with regard to the incentive structure or the size of the incentive. This necessitates careful policy evaluation to inform policy makers, future policy making and other research concerned with labour and training markets.

This dissertation considers the case of the German dual education (apprenticeship) system, which has been discussed as a success story with regard to the integration of young people into the labour market: Dual education has been associated with low youth unemployment and stable careers for apprentices (e.g., Breen, 2005; Dieckhoff, 2008; Eichhorst et al., 2015; Riphahn and Zibrowius, 2016; Ryan, 2001). However, in recent years, the dual education system has witnessed a decline in apprenticeship contracts from 564,307 in 2009 to 525,081 in 2019. At the same time, the number of unfilled apprenticeship places has been rising from 17,766 to 53,137 over the same time span (BMBF, 2020: 33; BIBB, 2020; see also Fitzenberger et al., 2025). This presents a threat to the economy because unfilled training places imply potential skill shortages in the future (Brunello and Wruuck, 2021; Bundesagentur für Arbeit, 2019; Schönfeld et al., 2020; see also BIBB, 2020). Additionally, the recent COVID-19 pandemic took its toll on the labour market in the early 2020s with shutdown measures, declines

in demand and insecurities about future productivity and financial stability (see e.g., Adams-Prassl et al., 2020; Bauer and Weber, 2021; Eurostat, 2020), which also brought up uncertainties with regard to investments in training (see e.g., Dummert and Umkehrer, 2025; Fassbender, 2022; Muehlemann et al., 2020; Smith, 2024).

In an effort to meet these labour market challenges, the German government introduced two policies to target the supply of and demand for apprentices. First, a minimum apprenticeship wage (MAW) was introduced for all new apprenticeship contracts starting their training in 2020. In their first training year, new apprentices had to be paid at least €515 per month (Deutscher Bundestag, 2019b). The declared aim of the government was to increase the attractiveness of the dual education system (Deutscher Bundestag, 2019a; see also BMBF, 2019). Second, an apprenticeship subsidy (AS) was introduced to buffer potential negative effects of the COVID-19 crisis on training by firms. Firms that managed to maintain their training levels as compared to the past three years could receive €2000 per apprenticeship contract and €3000 for each additional apprenticeship (BMAS/BMBF, 2020; Statistik der Bundesagentur für Arbeit, 2022). Overall, the two policies were designed to revitalize or stabilize the dual education system and apprenticeship market.

Empirically, however, it is unclear whether the two policies actually reached the goals desired by policy makers with regard to *training activity*.[2] The aim of this dissertation is to evaluate both policies in order to understand their effect on training activity empirically as well as in terms of their intended political aims. Therefore, this dissertation poses three research questions: First, did the MAW lead to more entrants into the training system? Second, and related to the first question, did the MAW affect training completions? And third, was the AS able to stabilize training activity in crisis times?

[2] I use this term as an umbrella term to describe the different outcomes evaluated in the dissertation, which each represent aspects of training activity.

Study 1 considers the new MAW with regard to training offers and take-ups at the start of the training period, arguing that higher wages may indeed attract applicants, though it may simultaneously reduce training offers due to the increased costs for firms. Study 2 takes a longer-term perspective and evaluates the effect of the MAW on successful training completions further down the timeline, arguing that effects at the start of the training period may not automatically translate to training completions due to the altered incentive structures. Study 3 analyses the effect of the AS on training activity in terms of new apprenticeships as well as stability in training levels.

Beyond providing a mere policy evaluation, this dissertation contributes to the existing labour market and policy literature in various ways. Overall, it contributes to the literature on the costs and benefits of training (e.g., Muehlemann et al., 2010; Muehlemann and Wolter, 2014). With regard to the different studies, it adds to the more general minimum wage literature that considers the regular labour market (e.g., Bossler and Schank, 2023; Card and Krueger, 1994; de Linde Leonard et al., 2014; Meer and West, 2016; Neumark and Wascher, 2008) and – more specifically – contributes to the so far rather scarce literature on the employment effects of minimum apprenticeship wages on the training market (Langen and Dörsam, 2025; Higton et al., 2012; Linckh et al., 2023; Papps, 2020). Moreover, it offers insights on the influence of wages when it comes to apprenticeship completion, thereby adding to the literature on training completion versus dropout (Bessey and Backes-Gellner, 2015; Böhn and Deutscher, 2022; Neuber-Pohl, 2021; Snell and Hart, 2008; Smyth and Zimba, 2019). Further, it fills a research gap on the role of training subsidies in crisis times by providing crisis-specific insights to the existing literature on the effectiveness of training subsidies in various forms (Gambin and Hogarth, 2021; van den Berg et al., 2006; Westergaard-Nielsen and Rasmussen, 1999; Ramirez and Weber, 2025).

The dissertation is structured as follows: in the introductory chapter, I describe the theoretical background on the decision to train from the perspective of firms as well as the

decision to take-up an apprenticeship from the perspective of prospective apprentices. This theoretical foundation informs expectations for the different policies. Further, I explain the analytical strategy and datasets that were used in the analyses and provide an overview and extended summaries of the separate studies that are part of the dissertation. Following this, I summarise and discuss all studies' findings with regard to the policies' political aims and their respective designs, asking what we have learned about supply and demand on the training market. Finally, I discuss avenues for future research. Chapters 2, 3 and 4 comprise of said studies and present the respective papers. In chapter 2, the MAW policy is analysed with respect to the short-term effects of its introduction, namely training offers and take-ups at the start of the training period. Chapter 3 focuses on longer-term effects of the MAW by analysing apprenticeship completions. Chapter 4 evaluates the AS for firms that was introduced to buffer COVID-19 related reductions in apprenticeship training.

1.2 Theoretical background

The classic economic theory: In its theoretical argumentation, this dissertation starts out at the labour market equilibrium, i.e., where supply and demand meet and the market is cleared, according to the classic economic literature (Borjas, 2013). The equilibrium is reached when the costs of labour that make firms want to hire and wages that make people want to work, align (see Borjas, 2013). With respect to the training market, the argument could be translated to costs of training and training wages.

The difference between the general labour market and the training market: Before simply applying the theoretical ideas of the general labour market to the training market, it should be noted that the they differ in two regards. First, contrary to the general labour market, where non-participation is the only alternative (i.e., the choice between work and leisure (see Borjas, 2013: chapter 2)), other training options apart from firm-based training exist and present

alternatives in the transition from school to work (see Boeri and van Ours, 2013: chapter 8). Examples include school-based training or tertiary education – at least for those who fulfil university entry criteria. Therefore, supply on the training market also depends on alternative markets and the attractiveness of options they present (e.g., Busemeyer and Garritzmann, 2017; Di Stasio, 2017; Smith, 2023). Second, the costs of labour, i.e., the costs for a worker to perform a job and produce some kind of output, are not readily comparable to the costs of training. Firms first have to make an investment in a trainee's skills first before this trainee can be productive (Becker, 1962). Previous literature has considered the question of why firms invest in training and argued that it depends on the costs and benefits of training (Muehlemann and Wolter, 2014; Muehlemann et al., 2007; Wolter et al., 2006). Firms are willing to train if they can use trainees as cheap labour or if they can expect a return on their investment from a trainee who continues to work at the firm as a skilled worker (Mohrenweiser and Backes-Gellner, 2010; Mohrenweiser and Zwick, 2009). The argument is based on information asymmetries (for an overview, see Acemoglu and Pischke, 1999): for example, as Acemoglu and Pischke (1998) argue, non-training firms know less about the abilities of the workers they hire than the firm that trained them and offer wages that are influenced by said uncertainties. Knowing more about their former trainees' abilities, training firms can offer wages that are higher than wage offers by non-training firms but lower than the marginal productivity of the workers. This implies that workers will stay with their training firms which, in turn, benefit from their investment in training (Acemoglu and Pischke, 1998). Therefore, contrary to the general labour market, cost and benefit considerations with regard to the demand by firms on the training market often depend on future profitability rather than immediate output (Becker, 1962).[3]

Relevance of the classic theoretical assumptions for this dissertation: Despite the differences between the general labour market and the training market, the basic assumption of

[3] Note that this argument does not apply to firms which employ apprentices as substitutes for regular workers (see e.g., Mohrenweiser and Backes-Gellner, 2010).

an interplay of supply and demand that depends on costs and benefits is useful when considering the potential effects of the two labour market policies in question, which target supply and/or demand. The MAW targets the supply side and can present an incentive for potential applicants to take up and complete training (Borjas, 2013; Dummert et al., 2019; Neuber-Pohl, 2021). Higher apprenticeship wages should theoretically increase supply. At the same time, this implies an increase in costs for firms on the demand side, which should reduce their willingness to offer training (Muehlemann and Wolter, 2014; Muehlemann et al., 2007), thereby potentially reducing demand simultaneously. However, if firms decide to train despite the higher costs, their incentive to achieve successful training completions in order to reap the benefits of their now larger investment should be higher (see also Rohrbach-Schmidt and Uhly, 2015).

The AS targets the demand side by reducing the costs of training, thereby incentivising firms to offer it (see e.g., Borjas, 2013; Muehlemann and Wolter, 2014). A subsidy may therefore increase demand or at least stabilise it in crisis times (Brunello, 2009; Muehlemann et al., 2020).

From theoretical assumptions to empirical impact: While the theoretical effect of such policies on supply and demand can clearly be described, expectations on their actual empirical impact cannot be formulated as easily because policy effects depend on the design of the policy and the existing institutional setup (Boeri and van Ours, 2013; see also Brunello, 2009: 159). For example, the effect of a minimum wage on supply and demand depends on its level: Following the classic theory (Borjas, 2013), a relatively low minimum wage may not present enough of an incentive to increase supply, while a relatively high minimum wage may present a strong disincentive for firms, which might then reduce their demand. Moreover, depending on the number of people starting an apprenticeship (i.e., the result of supply versus demand configurations), training completions may also be affected, for example via demand reductions at the start of the training period and a smaller starting cohort. However, as argued above, completions may alternatively be compensated by an increased incentive to complete the

training for both apprentices and firms (see Muehlemann and Wolter, 2014; Neuber-Pohl, 2021; Rohrbach-Schmidt and Uhly, 2015). Similarly, the effect of a training subsidy is expected to depend on factors such as the type, level and eligibility criteria of the subsidy (see e.g., Brunello, 2009: 157ff.; Ramirez and Weber, 2025). For example, if the subsidy is too low for firms to be willing to commit, it might fail to accomplish the intended policy aim. Alternatively, if the subsidy is high but eligibility criteria are too strict, it may only benefit a very selected group of firms. Therefore, an analysis of the two polices provides insights into their actual empirical effects on training activity while also testing theoretical expectations on supply and demand more generally.

1.3 Method

1.3.1 Analytical strategy: the difference-in-differences design

To evaluate the respective effects of the MAW and the AS, a difference-in-differences (DiD) design is applied. The design provides a suitable strategy for the evaluation of policies in quasi-experimental settings. With regard to labour market research, the most prominent example of its application is probably the quasi-experimental study by David Card and Alan Krueger on the employment effect of a minimum wage increase in New Jersey (Card and Krueger, 1994).

While a basic description of the DiD idea is necessary to later introduce the methodological approaches of this dissertation's studies, the following elaboration is only a condensed repetition of various methodological contributions of previous research and methodological textbooks. Therefore, see e.g., Angrist and Pischke (2008) or Huntington-Klein (2022) for an in-depth discussion of the DiD design and its assumptions.

In policy evaluation, the aim is to find out whether the policy led to any changes among those affected by it. This dissertation focuses on firms as the unit of interest. To do so, one can

compare the outcome of interest over time, from before the policy introduction to the time thereafter. However, this comparison alone is error prone because it remains unclear whether the changes in the affected group are actually due to the policy or attributable to other factors that changed over time. Therefore, the basic idea of the DiD approach is to compare the development of the affected group (the so-called 'treatment group') to the development of a group that is not affected but otherwise comparable to the treatment group (the so-called 'control group') over the same time span, namely from before to after the policy introduction (i.e., the treatment). The control group, therefore, serves as the non-observable counterfactual of the treatment group (for an overview of the *potential outcomes framework*, see Imbens and Rubin, 2015). The difference between the treatment and control group over time is attributed to the treatment, the so-called 'average treatment effect on the treated' (ATT). Formally, the estimated ATT (δ_{ATT}) is calculated as the difference in outcomes (y) of the treatment group (T) between the time *after* the treatment (1) and the time *before* the treatment (0) minus the difference in outcomes of the control group (C) over the same period:

$$(1.1)\ \widehat{\delta_{ATT}} = (\overline{y_{T,1}} - \overline{y_{T,0}}) - (\overline{y_{C,1}} - \overline{y_{C,0}})$$

In this dissertation, the ATT concerns the difference between treated firms affected by the respective policy and control firms that are not affected by it. This group comparison over time rests on the assumption that both groups would have developed similarly in the absence of treatment. Formally, the parallel (or: common) trends assumption states that

$$(1.2)\ E(Y_1^0|d=1) - E(Y_0^0|d=1) = E(Y_1^0|d=0) - E(Y_0^0|d=0)$$

Here, the superscript 0 represents the outcome in the absence of treatment, i.e., the hypothetical outcome. The subscripts 0 and 1 denote the time periods before versus after the treatment. d indicates the actual treatment status. Therefore, as stated above, it is necessary that treatment

and control group are similar in order to be able to plausibly argue for parallel trends in the absence of treatment.

The ATT can be estimated by using a two-way fixed effects (TWFE) model (Wooldridge, 2021):

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

Here, y_{it} is the outcome of interest, μ_i denotes the individual (firm) fixed effect, θ_t is the time fixed effect, δ is the estimated treatment effect and $treat_{it}$ indicates the treatment status of individual (firm) i at time t. u_{it} is the idiosyncratic error term.

In a TWFE, time-invariant covariates are controlled for by design. In DiD approaches, covariates are usually constructed to be time-invariant, either because they are in fact constant or because they are measured pre-treatment, for example at the last time point before the treatment took place. Measuring covariates pre-treatment is necessary to make sure that the covariates are not influenced by the treatment in any way. Therefore, in a TWFE model, pre-treatment (constant) characteristics are fixed. With regard to firms, pre-treatment characteristics may, for example, include firm size or the shares of certain employee groups.

However, if said characteristics differ between the two groups and can be assumed to affect the outcome of interest, the parallel trends assumption is violated as the groups are not comparable in terms of covariates. For example, the effect of the MAW on training activity may differ depending on how much a firm was affected by COVID-19. It is reasonable to assume that treated firms may be affected differently by the pandemic than control group firms, e.g., depending on their location or industrial sector. Similarly, the effect of the AS on training activity may depend on firm and regional characteristics which may differ between treatment and control group. In such cases, it is then unclear if any differences in the outcome (here: training activity) can be attributed to the specific policy or covariate-related influences that simply differ between treatment and control group. Therefore, in this dissertation, several

methodological advancements suggested by researchers over the past years were applied to address potential 'comparability' challenges with regard to covariates (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020; Wooldridge, 2023). The exact nature of these challenges in each study, and how they were addressed, are described in the respective chapters. By including covariates that are assumed to systematically differ between treatment and control group, parallel trends are only assumed conditional on these covariates, which relaxes the initial parallel trends assumption. In the analyses of both the MAW and the AS, using DiD approaches that account for covariates make it more plausible to argue that treatment and control group firms would have developed similarly in the absence of treatment (i.e., had the policies not been introduced).

1.3.2 Data

For this dissertation, different data sources from the Institute for Employment Research (IAB) and external sources were combined.[4] First, the IAB Establishment Panel (BP) comprises a rich set of firm-level survey information on more general firm characteristics as well as various year specific modules. These additional modules provide additional information, such as firms' affectedness by COVID-19 in the 2020 survey wave. This is particularly relevant for the evaluation of both policies: The MAW was introduced during the pandemic; the AS was designed specifically for those firms that were negatively economically affected by COVID-19. Moreover, the 2020 survey wave includes a question on the affectedness by the new MAW, which was used to construct the treatment variable. Study 1 and 3 use the BP survey covering the time up to 2022 (Bächmann et al., 2023a, Bächmann et al., 2023b). For study 2, the BP version that additionally includes the 2023 survey wave was used (Bellmann et al., 2024a;

[4] Data preparation and analyses were conducted using Stata (StataCorp, 2021, 2023)

Bellmann et al., 2024b). Using syntax provided by the Research Data Centre (FDZ) of the IAB, the yearly data was transformed into a panel dataset (Umkehrer, 2017).

To further enrich the data, the BP data can be merged with register information from the IAB Establishment History Panel (BHP; Ganzer et al., 2023a; Ganzer et al., 2023b; Ganzer et al., 2024a; Ganzer et al., 2024b). The BHP includes variables on additional basic (non-survey) firm characteristics, such as employee numbers for different groups of employees (e.g., by different contract type, skill level, sex), information on inflow (new employees) and outflow (employees who leave) and mean or median employee wages. A rich set of covariates is particularly useful, since it ensures the comparability between treatment and control group, which is the core condition of DiD designs. Moreover, the BHP is useful, as register data does not to suffer from any survey bias and is available for all years in which the firm existed, independent on whether it took part in the BP survey or not. This limits any problems related to missing data, thereby providing a valuable source for pre-treatment covariates. Moreover, the BHP also provides information on firm closures, which can inform panel attrition analyses and variables that can be used as alternative outcomes in robustness and placebo analyses (which are discussed in the respective chapters). Therefore, the BHP information were merged to the BP survey data. In an additional step, plausibility checks were performed to detect inconsistencies between the two sources. This may be due to the BP survey not targeting the firm or subunit meant to be targeted, for example the corporation instead of a regional office. To do so, employee, trainee and trainee inflow numbers were compared across datasets. In later analyses, cases with substantial deviations were excluded. More details on this exclusion can be found in the sample descriptions of the respective studies (Chapters 2-4).

The dataset was further combined with external regional information to be used as additional control variables, as treatment and control group are likely not randomly distributed across regions. Hence, in addition to firm characteristics, regional characteristics may also differ between treatment and control group and, in turn, influence the outcome variables of

interest. In terms of regional labour market characteristics, regions vary with regard to their economic strength or unemployment. In addition, the COVID-19 pandemic affected regions differently. These region-specific factors could lead to differences in training activity between treatment and control groups that are not related to the treatment (i.e., the respective policies), thereby violating the assumption of parallel trends and biasing estimations of the actual treatment effect. Therefore, the dataset was enriched with information on regional GDP (Statistische Ämter des Bundes und der Länder, 2023), unemployment (Bundesagentur für Arbeit, 2024) and COVID-19 incidence rates (Robert Koch-Institut, 2024) that was merged via district level codes (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2024) that are also available in the BP survey data. For the analysis of the apprenticeship subsidy, the data was further combined with external industry-specific information on short time work provided by the statistics service of the Federal Employment Agency (BA; Statistik der Bundesagentur für Arbeit, 2025).

Overall, the final dataset combines information of multiple data sources to answer the research questions of the different studies. It comprises data on the different outcomes of training activity, variables to construct the treatment status, a vast set of relevant control variables to ensure a better comparability with respect to the parallel trends assumption as well as variables to perform robustness checks and sensitivity analyses with.

1.4 Overview and summary of the separate studies

In the following I provide extended summaries of the three studies. An overview can be found in Table 1.1.

Table 1.1: Overview of the different studies

Chapter	Title	Co-Author(s)	Publication Status	Data	Method	Outcome Variable	Main Independent / Treatment Variable
Chapter 2 (Study 1)	Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups	Lukas Fervers, Annika Reiff	Under review (Preprint available on SocArXiv: https://doi.org/10.31235/osf.io/hjb74_v2)	IAB Establishment Panel (IAB-BP 2022), Establishment History Panel (BHP), regional data	Difference-in-differences (DiD), various implementations	Offered apprenticeship training places, filled apprenticeship training places	Being affected by the introduction of the apprenticeship minimum wage policy (=having paid below the new wage threshold before the introduction)
Chapter 3 (Study 2)	The long-term effects of monetary (dis-)incentives on apprenticeship training: Does an apprenticeship minimum wage lead to more or less successful training completions?	-/-	Submitted	IAB Establishment Panel (IAB-BP 2023), Establishment History Panel (BHP), regional data	Difference-in-differences (DiD), various implementations	Completed apprenticeship trainings	Being affected by the introduction of the apprenticeship minimum wage policy (=having paid below the new wage threshold before the introduction)
Chapter 4 (Study 3)	Efficient stabilizer or deadweight effect? The impact of subsidies for apprenticeship training during an economic downturn	Lukas Fervers	Revised version published in *Economics of Education Review* (after the submission of the dissertation)	IAB Establishment Panel (IAB-BP 2022), Establishment History Panel (BHP), regional data	(Intent-to-treat) difference-in-differences (DiD), various implementations	Trainee inflow (number of new trainees), dummy variable of having maintained the average training level of the past three years	Eligibility to receive the apprenticeship bonus (firm size)

1.4.1 Summary of Study 1: Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups

In the first study, we evaluate the effect of the MAW introduction on apprenticeship offers and take-ups, thereby focusing on the start of the training period. The MAW was introduced by the German government to make dual education more attractive to potential apprentices (BMBF, 2019; Deutscher Bundestag, 2019a). According to this new policy, firms had to pay new apprentices who started their training in 2020 at least €515 per month (Deutscher Bundestag, 2019b). While this policy was aimed at the supply side by increasing the attractiveness of the dual education system, we argue that its impact is theoretically ambiguous. Along the lines of classic economic theory (Borjas, 2013), a wage increase could make apprenticeships more attractive to young people which may lead to an increase in applications, on the one hand. On the other hand, however, firms on the demand side may reduce the number of positions they offer because training becomes more expensive for them. Which of these effects dominates empirically, is an open question that we seek to answer in study 1.

In addition to empirical evidence on the effect of this particular policy, an analysis of its impact contributes to the vast body of literature on the effect of minimum wages in more general terms (e.g., Bossler and Schank, 2023; Card and Krueger, 1994; de Linde Leonard et al., 2014; Meer and West, 2016; Neumark and Wascher, 2008) as well as to the so far limited literature on the effect of minimum wages for apprentices on training (Langen and Dörsam, 2025; Higton et al., 2012; Linckh et al., 2023; Papps, 2020).

To provide an answer to the research question, we use BP data combined with BHP register data and additional regional data on unemployment, GDP and COVID-19 incidences (see Section 1.3.2). Using a DiD approach, we define a treatment group and a control group of firms. Treated firms are defined as firms which – prior to the policy introduction in 2020 – paid at least one of their new apprentices below the new MAW threshold of €515 per month. The

control group of firms comprises of those firms which already paid new apprentices more than €515 per month prior to the MAW introduction.

Two outcome variables were considered: the number of offered and the number of filled training places, i.e., apprenticeship take-ups. We compare the treatment and control group with regard to both outcomes between the pre-treatment period and the post-treatment period using different model specifications, starting with a two-way fixed effects (TWFE) estimator without using any control variables. Moreover, we use a difference-in-differences approach with pre-matching (CS-DiD) to make treatment and control group firms more similar with regard to covariates (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020).

Depending on the model specification, the estimated treatment effects for both offered and filled training places are either insignificant (negative point estimates) or significant and negative. Ranging between about -.3 (offered trainings)/-.2 (filled trainings) in the TWFE and -.7 (offered trainings)/-.6 (filled trainings) in the CS-DiD models, the empirical evidence points to the MAW policy having led to reductions in offers and take-ups. Moreover, with the negative effect on filled training places being only slightly smaller than the effect on offered trainings, the results show that reductions in offers by firms on the demand side dominated theoretically possible supply increases from take-ups.

We conducted several robustness checks to ensure the validity of our results. The results are robust with regard to different model and sample specifications, post-treatment selection, dropout or placebo tests in time to check for diverging pre-treatment trends. Moreover, we conducted placebo tests in space using alternative outcomes that should not be associated with the introduction of the MAW policy, namely the inflow of new employees and general vacancies. Since the estimates for these placebo tests are insignificant, this supports our assumption that the actual treatment effects we find are due to the MAW introduction and not caused by some other influence. We further checked whether firms adjusted their hiring strategy

due to increased costs for apprentices by hiring more mini jobbers as cheap labour. We do not find any indication for this.

We conclude that the MAW policy did not lead to more entrants into the training system, which was the intended policy goal. This is mostly due to the *reductions* in demand (with the negative effect being significant only in some model specifications). However, the effect of the policy on short-term outcomes at the beginning of the training phase is only part of the story. Another question concerns longer-term effects on apprenticeship completions at the end of the training phase. Understanding such longer-terms effects are equally important, though, as only people who completed their training provide labour potential for a future skilled workforce (see also Smyth and Zimba, 2019: 90).

1.4.2 Summary of Study 2: The long-term effects of monetary (dis-)incentives on apprenticeship training: Does an apprenticeship minimum wage lead to more or less successful training completions?

The second study analyses the effect of the MAW on successful apprenticeship completions, thereby changing its focus to the longer-term impact of the policy. The previous analyses in study 1 show that the MAW did not lead to more entrants into the training system. If anything, the policy effect tends to be negative. I argue that on the one hand, it is theoretically possible that the MAW's negative effect on apprenticeship offers and take-ups at the start of the training period could translate into negative effects in the longer-term, i.e., at the completion stage (merely via less entrants into the system). On the other hand, however, this may not necessarily be the case. An increase in apprenticeship wages may change the employers' as well as the apprentices' motivation to achieve a successful completion: for firms, apprenticeships become more expensive, hence, they should have an increased motivation to achieve a return on their investment (Muehlemann and Wolter, 2014). Reaping these benefits is only possible if an apprentice completes the training and can continue to work as a skilled employee (see

Rohrbach-Schmidt and Uhly, 2015). For apprentices, higher wages should make continued training more attractive (Neuber-Pohl, 2021).

Similar to study 1, I employ a DiD design to analyse this open empirical question. I compare treated firms, i.e., those for which the MAW implied wage increases for at least one new apprentice in 2020, against a control group of firms who already paid above the threshold before the policy introduction. To do so, I analyse the development of successful completions of treatment and control group firms between the pre-treatment period 2019-2021 and the post treatment time point of 2023 (i.e., 2020 + 3 year of regular training time). For the analysis, I use survey data from the BP that includes the 2023 survey wave, combined with register and external regional data (see Section 1.3.2).

I apply different model specifications, starting with a TWFE model without controls. In addition to this classic approach, I employ CS-DiD (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020) to control for covariates via pre-matching. Moreover, as recently proposed by Wooldridge (2023), I present another model in which treatment and time effects are interacted with the control variables. While all these approaches assume linear effects, I additionally calculate a non-linear Poisson regression model that also interacts the treatment and time variables with the control variables, thereby also following Wooldridge (2023). This is done to account for the fact that training completions are a count variable that cannot be negative, thereby preventing a potential corner solution problem.

The study's findings indicate that completions were not affected by the MAW policy. If at all, the estimates point to a positive – but non-significant – effect of about 0.3 completions in all model specifications, meaning that treated firms achieve 0.3 completions more between pre-treatment and post-treatment period, compared to the control group. However, due to the lack of significance, this interpretation should be treated with caution. The results are robust across the different model specifications and to various sample definitions. However, while a placebo time test reveals non-significant pre-treatment effects (i.e., what is desired in such

placebo tests), they still show an indication for non-parallel trends before the treatment. This prompts the need for future research to further investigate the effect of the policy on completions, potentially with a larger sample in a different dataset. Moreover, as the MAW further increased over the recent years, analysing this staggered treatment effect over multiple post-treatment years could be an avenue for future research.

So far, study 2 complements the evaluation of the new MAW policy with regard to long-term effects, showing that even though reductions in offers and take-ups seem to have occurred, this did not translate into reductions in completions further down the line. This finding has implications for the design of future minimum wage policies on the apprenticeship market: It shows that – at least at the lower end of the wage distribution – a welfare increase for apprentices via increased wages does not seem to have negative labour market implications with respect to apprenticeship completions and the provision of a future skilled workforce. In addition to informing the longer-term implications of this particular policy, the study contributes to the literature on the role of apprenticeship wages for training completion (Bessey and Backes-Gellner, 2015; Böhn and Deutscher, 2022; Neuber-Pohl, 2021; Snell and Hart, 2008; Smyth and Zimba, 2019).

1.4.3 Summary of Study 3: Efficient stabilizer or deadweight effect? The impact of subsidies for apprenticeship training during an economic downturn

Study 3 evaluates the AS, which targeted the demand side (i.e., firms) and was aimed at alleviating potential negative COVID-19 related effects on training. As economic downturns may come along with reductions in training by firms (Brunello, 2009; Bellmann et al., 2014), the German government implemented the AS in 2020 to support small and medium sized firms in their training activity. To qualify for the subsidy, firms had to be negatively economically affected by the COVID-19 pandemic and had to have less than 250 employees (i.e., full-time equivalents). Given the eligibility criteria, firms could receive €2000 for each apprenticeship

that started in the fall of 2020 if they maintained the number of apprenticeships at the average level of the past three years. For each additional apprenticeship, firms could get €3000 (BMAS/BMBF, 2020; Statistik der Bundesagentur für Arbeit, 2022).

To analyse the effect of the AS, we use data from the BP which was combined with BHP register data, external regional data and external data on short-time work per industrial sector (see Section 1.3.2). We consider firms' (non-)eligibility for the subsidy in an intent-to-treat difference-in-differences framework. Eligibility is determined via firm size and COVID-19 affectedness. We focus on eligibility rather than on the actual receipt of the subsidy because the treatment status depends on the desired outcome. Hence, the treatment assignment is not exogeneous (for similar approaches with respect to wage subsidies, see Boockmann et al., 2012; Schünemann et al., 2015).

We analyse two related outcome variables: the number of new apprentices as a continuous outcome and a dummy variable measuring whether the firm has maintained their training levels with respect to the past three years. While the first outcome informs about the inflow of new apprentices more generally, the second one relates more closely to the government's definition of the AS (see BMAS/BMBF, 2020; Statistik der Bundesagentur für Arbeit, 2022).

Each outcome is analysed using four different model specifications: first, we employ a linear TWFE estimator without covariates, second a linear TWFE approach with covariates, in which the treatment and time variable are interacted with the control variables, as suggested by Wooldridge (2023). For the continuous outcome, this is estimated by means of OLS, for the binary outcome we use a linear probability model. Third, we use a non-linear approach without, and forth, with control variables, also by interacting the treatment and time variable with the control variables (Wooldridge, 2023). Using a non-linear approach in addition to linear estimations is warranted since the number of apprentices is a non-negative count variable. Since treatment eligibility directly depends on firm size, it is possible that any drops in apprenticeship numbers may differ between non-eligible control group firms and eligible treatment firms,

simply because apprenticeship numbers cannot decrease below zero. Therefore, an assumption of parallel pre-treatment trends, which is necessary in a DiD design, may be implausible – at least in absolute terms. A non-linear approach accounts for this problem by assuming parallel trends in growth rates (Wooldridge, 2023). For the continuous outcome, we employ Poisson regressions, for the binary outcome, we use logistic regression models.

Comparing eligible treatment firms with non-eligible control firms, we find no effect of the AS on the number of new apprentices (continuous outcome). However, we do find a positive effect on the probability to maintain the level of training. Depending on the model specification, whether a linear versus non-linear estimation was used and whether control variables were included or not, eligible firms had a higher probability of 11.5 to 14.5%. to maintain their training levels compared to non-eligible firms. Further, the results are robust to various sensitivity analyses, including tests for non-parallel trends, placebo outcomes and panel attrition.

While the study's findings may sound counterintuitive at first, since a higher probability to maintain training levels should also become visible in the number of apprentices, we suggest that these results are attributable to behavioural changes of firms at the margin of maintaining their training levels. Potentially, those firms (i.e., firms that would have just about kept their level even without the subsidy) decided to hire a few additional apprentices in order to qualify for the receipt of the subsidy payment. Given that we did not find effects on apprenticeship numbers, this would imply deadweight effects rather than the policy being an efficient tool to stabilise apprenticeship training during the crisis for all eligible firms and especially those that were severely affected by it. We discuss that this finding is not surprising given the policy's incentive structure. The AS is designed in a way so that firms receive a considerable amount of money for hiring only a few additional apprentices. Thereby, the policy incentivises firms at the margin to adjust their hiring strategy just slightly.

Our evaluation of the AS empirically highlights the importance of understanding the incentive structure of a policy and informs the design of future policy incentives on the labour market. Moreover, the study contributes to the literature on the effects of training subsidies in general (Gambin and Hogarth, 2021; van den Berg et al., 2006; Westergaard-Nielsen and Rasmussen, 1999; Ramirez and Weber, 2025) and – more specifically – adds to this research by focusing on the impact of subsidies in the face of economic downturns.

1.5 Discussion and Conclusion

The school-to-work transition is a decisive factor in the life of young adults, with the VET system playing a central role for the labour market integration of young adults (e.g., Barbieri et al., 2018; Ryan, 2001; Shavit and Müller, 1998). Given that supply and demand on the training market depends on the institutional setup (Boeri and van Ours, 2013; Muehlemann and Wolter, 2014), this dissertation focused on analysing the role of labour market policies on the supply of and demand for apprenticeship places, namely minimum wages for apprentices as an incentive for the supply side and training subsidies as an incentive for the demand side.

To do so, two recent policies that were implemented in Germany were evaluated. First, the MAW introduced a wage floor of €515 per month paid to new apprentices in 2020 aiming to revitalise the dual education system (Deutscher Bundestag, 2019a; Deutscher Bundestag, 2019b). The results indicate that the MAW did not lead to new entrants into the system as effects on both offered and filled training places tended to be negative. However, when considering training completion, the negative effects do not seem to have translated into completions at the end of the training period. If anything, results point to positive (albeit non-significant) effects of the MAW on completions.

Second, the AS was targeted at firms to incentivise training during the COVID-19 crisis by offering subsidy payments of €2000 per apprenticeship contract if they maintained their

training levels as well as €3000 for each additional apprenticeship contract (BMAS/BMBF, 2020; Statistik der Bundesagentur für Arbeit, 2022).

Results on the effect of the AS show that while the policy did not have an effect on the number of new apprentices during the COVID-19 crisis, it did affect firms with regard to maintaining their pre-crisis training levels. This indicates that firms just at the margin of maintaining their level (even without the AS) now had a substantial financial incentive to hire only a few additional apprenticeships than initially planned.

Given the findings presented, what did we learn about supply and demand on the training market? The answer to this question is threefold. *First*, while theoretically, supply and demand are equally important factors to facilitate a functioning training market, the analyses of the two policies made clear that demand is a necessary and preceding condition to accommodate any supply. With respect to theoretical expectations on the effect of an apprenticeship minimum wage, one could assume a revitalisation of the training system in a market where demand exceeds supply – which is the case in Germany: By financially incentivising supply, it may be pushed upward (see theoretical argument in Chapter 2 and Borjas, 2013). Therefore, in theory, a minimum wage may work to get closer to a 'training market equilibrium' despite potential simultaneous reductions in demand. However, the empirical results presented in this dissertation point to negative effects on demand even at this rather low wage floor in comparison with average training wages (see Schönfeld and Wenzelmann, 2020: 14f.). This indicates that a minimum wage at the lower end of the wage distribution targets firms which are simply not able to pay their apprentices a higher wage. Requiring firms to pay higher wages by means of a minimum wage policy leads them to reduce their demand for apprentices, which in turn leads to less places that can be filled, even if supply increases. Therefore, when designing a policy to incentivise supply, it is crucial to bear in mind the potential effects on demand. The analysis of the MAW showed that even though the policy was targeted at the supply side in an

effort to revitalise the training system, an apprenticeship minimum wage does not seem to present a viable option as it simultaneously affects demand negatively.

Moreover, although an evaluation based on firm level data does not directly inform about apprentice behaviour, a minimum wage far below average training wages questions the effectiveness in terms of supply changes, given that alternative training paths, such as university studies or school-based training, exist. If anything, a minimum wage design such as this, may lead to a redistribution of apprentices, meaning that apprentices who would not have pursued a particular apprenticeship because of low wages and rather taken up a different apprenticeship in a different firm may now be inclined to do so. However, such a re-allocation of people to training places still does not lead to more entrants into the training system. People deciding between an apprenticeship and an alternative training path, such as university studies, may hardly be convinced to choose the apprenticeship by a minimum wage at the given level. However, even hypothetical increases in supply due to the financial incentive do require the demand for it. And the empirical results indicated reductions in demand by firms. Therefore, with regard to generating more entrants into the training system, a minimum wage for apprentices does not seem to provide a viable policy tool.

Second, the story is more nuanced when it comes to the supply of skilled workers (i.e., former apprentices) with a completed training who are potentially available for the general labour market. The results indicate that the reduced demand (as seen in reductions in offered and filled training places) did not translate into reductions in completions. This may be interpreted as a sign of the labour market having become more efficient in terms of training activity, as completions were not affected despite less people having started apprenticeships. On the one hand, this implies welfare increases for young adults as the wage floor presents more financial security at the lower end of the wage distribution. On the other hand, as argued in Chapter 3, the non-negative effect on completions likely is (at least partly) due to increased selection. This has societal implications with respect to the training systems' inclusivity and the

skill formation of the less able (see Haasler, 2020; Jacob and Solga, 2015). Hence, any welfare increases induced by a minimum wage for apprentices do not necessarily imply welfare increases for all.

Third, with regard to a training subsidy for firms – which targeted the demand side – the evaluation of the AS showed that a subsidy in crisis times may be able to stabilise training activity. However, it also illustrated the importance of understanding the policy design (cf. Boeri and van Ours, 2013). The finding that the policy did not affect apprentice numbers but did have an effect on maintaining pre-crisis training levels, implies that it was especially firms at the margin of maintaining their training levels anyway that were incentivised by the policy (see Chapter 4). Hence, when offering large benefits in exchange for a relatively small behavioural adjustment – as did the AS – the policy will be most attractive to those firms which only have to make small adjustments in their hiring behaviour. A subsidy to cushion an economic downturn, such as the COVID-19 pandemic, designed like the AS therefore does seem to target mostly those firms which do not actually struggle that much. Firms that have to invest more in order to receive any subsidy payments, will be less incentivised in comparison. Therefore, future policies that aim to cushion crisis effects, may, for example, include tiered benefits that depend on a firm's effort and the extent of their hiring adjustment, so that firms which adjust and thereby invest the most also benefit the most.

Overall, this dissertation showed that – similar to the general labour market – training market policies need to consider both potential effects on supply and demand. Moreover, it illustrated how such policies may or may not achieve the intended political aim, which implies that policy makers should carefully consider the design of each policy and the (dis-)incentive structures it creates.

1.6 Outlook

Towards the end of this dissertation, the remaining question is ‘Where do we go from here?’. The studies presented herein could provide answers to the research questions on the effects of a minimum apprenticeship wage on training activity at the start of the apprenticeship as well as on completions at the end of the training period, and whether a training subsidy can stabilize training activity during times of crisis. However, the findings and implications also revealed opportunities for future research.

As the sample sizes in our analyses did not allow for a differentiation of the analyses by industry, further research may look at industry-specific effects of minimum wages or training subsidies on the apprenticeship market. Whereas the studies presented in this dissertation controlled for firm and regional characteristics, analyses showing industry- or region-specific estimations of the policy effects of financial incentives could inform future policy design about more industry or regionally tailored approaches to revitalising and/or stabilising the training market.

Another avenue for future research is the focus on effects of training market policies on (future) apprentices. Using firm-level data, this dissertation focused on the firm side. However, the field could benefit from additional research on (prospective) apprentice behaviour with regard to policy incentives. This relates to various strands of research. First, it can be linked to research on public opinion and the attractiveness of vocational education (e.g., Busemeyer and Garritzmann, 2017; Di Stasio, 2017; Russo et al., 2019; Smith, 2023). Future research may build on this and look into the effect of policies on the perception of the attractiveness of VET.

Second, it relates to research on the role of institutions when it comes to the decision on an educational pathway (Busemeyer and Jensen, 2012). More research is needed on the long-term effect of policies on the broader institutional setting (for recent research on this, see Ibsen and Thelen, 2025). Third, it can be linked to research on returns to education of VET and alternative educational pathways, especially tertiary education (Allmendinger, 1989; Forster

and Bol, 2018; Forster et al., 2016; Rözer and Bol, 2019). In times of educational expansion, more and more people receive school-leaving certificates that qualify them for university studies (Bol, 2015). Therefore, for an increasing share of people, there are alternative pathways in their transition from school to work beside VET. Considering current and future costs and benefits of tertiary education versus VET, life course research showed that while vocational education offers more stable transitions into the labour market, generally educated people have an advantage later in their careers (Forster and Bol, 2018; Forster et al., 2016; Hanushek et al., 2017; Rözer and Bol, 2019). These findings may present relevant arguments for young people contemplating their future careers and deciding on their most beneficial option (Becker and Hecken, 2009; Breen and Goldthorpe, 1997). Future research could evaluate how policies alter the perception of VET vis-à-vis alternative options and the decision to pursue a vocational training path.

Finally, it relates to research on matching problems on the training market (e.g., Eberhard et al., 2019; Fitzenberger et al., 2025). While the German training market as a whole has been characterised by an increase in unfilled apprenticeship places in past years (BMBF, 2020: 33; BIBB, 2020), the lack of apprentice supply is not present in all industries and occupations. As previous research could show, the supply of apprentices to fill training positions depends on firm-specific characteristics as well as regional factors (Dummert et al., 2019). Hence, the lack of apprentices is not distributed equally across firms. While the analyses presented in this dissertation take account of this via the methodological design, future research could add to the results presented here by focusing on supply side effects of training market policies, i.e., to what extent such policies are able to incentivise young people to become apprentices in general, but also with regard to less attractive firms and/or occupations.

On a concluding note, when it comes to the future of the (German) apprenticeship system – a question that has been concerning research for some time (see e.g., Culpepper and Finegold, 1999) – this dissertation has shown that it is important to consider policy options to increase

supply without simultaneously affecting demand in order to achieve a revitalisation or stabilisation. Potentially, rather than offering benefits to firms in exchange for training, such as the AS (see also Gambin and Hogarth, 2021; van den Berg et al., 2006; Westergaard-Nielsen and Rasmussen, 1999), a subsidy to directly finance (higher) wages for apprentices that do not imply increased costs for firms may present such an option (cf. Ramirez and Weber, 2025). However, this dissertation has also shown that the policy design is decisive for its empirical impact. Therefore, whether a subsidy such as this could be effective in reaching intended policy goals in terms of supply and demand, and what this implies for government expenditure, is subject to future research – potentially someone else's dissertation.

1.7 Development of the studies and contribution of co-authors

All three papers are part of the project 'Politische Reformen zur Förderung betrieblicher Ausbildung' funded by the Hans Böckler Foundation (grant number 2023-22-5). The project was led by Prof Marita Jacob and Dr Lukas Fervers who developed the initial theoretical ideas and methodological approaches for the analyses of the MAW (see chapter 2, study 1) and AS (see chapter 4, study 3). For my single-authored paper (chapter 3, study 2) which analyses the effect of the MAW on apprenticeship completions, I developed the initial idea of the first study further in order to approach the remaining research gap of longer-term policy effects. In the following, I describe my own and my co-authors contributions to the three studies.

I am the first author of study 1, which is currently under review. I prepared the main data and, together with my co-authors Lukas Fervers and Annika Reiff, analysed it and interpreted the results. With regard to the manuscript, I took the lead and wrote the first draft of the data, sample and variable section as well as the section on results plus robustness checks and the conclusion. Lukas Fervers developed the methodological approach and wrote the section describing the research design. Annika Reiff prepared the additional regional data to be merged

with the main data and, together with Lukas Fervers, wrote the first draft of the theoretical background section. In the further development of the paper, we contributed equally to reviewing and further editing the manuscript.

Study 2 is single-authored by me – therefore, I am responsible for all parts. I designed the theoretical framework, prepared and analysed the data and wrote the manuscript. As this paper builds on the first study on apprenticeship minimum wages using a different data version, I was able to use and adjust the existing syntax files for my analyses and use the external regional data. The paper has been submitted.

Study 3 is co-authored by Lukas Fervers who is the first author of this paper. He was responsible for designing the methodological approach and the data analysis. He also prepared the industry-specific external data to be merged with the main dataset. I was responsible for the review of the previous literature. We both contributed to the data preparation of the main dataset and to the process of writing, reviewing and editing of the first draft of the manuscript. For the external regional data, we used the regional dataset that was initially prepared for study 1. In further revision steps of the paper, Lukas Fervers took the lead. This paper has been revised and resubmitted (the dissertation includes the revised version).

1.8 References

Acemoglu D, Pischke J-S (1998) Why Do Firms Train? Theory and Evidence. *The Quarterly Journal of Economics* 113(1): 79–119.

Acemoglu D, Pischke J-S (1999) Beyond Becker: Training in Imperfect Labour Markets. *The Economic Journal* 109(453): 112–142.

Adams-Prassl A, Boneva T, Golin M, and Rauh C (2020) Inequality in the impact of the coronavirus shock: Evidence from real time surveys. *Journal of Public Economics* 189, 104245: 1–33.

Allmendinger J (1989) Educational systems and labor market outcomes. *European Sociological Review* 5(3): 231–250.

Angrist JD, Pischke J-S (2008) *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Bächmann A-C, Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, et al. (2023a) *IAB-Betriebspanel (IAB BP) – Version 9322 v1*. 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.

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.

Bauer A, Weber E (2021) COVID-19: how much unemployment was caused by the shutdown in Germany? *Applied Economics Letters* 28(12): 1053–1058.

Becker GS (1962) Investment in Human Capital: A Theoretical Analysis. *Journal of Political Economy* 70(5): 9–49.

Becker R, Hecken AE (2009) Higher Education or Vocational Training? An Empirical Test of the Rational Action Model of Educational Choices Suggested by Breen and Goldthorpe and Esser. *Acta Sociologica* 52(1): 25–45.

Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, Tschersich N, et al. (2024a) *IAB-Betriebspanel (IAB-BP) – Version 9323 v1*. Nürnberg: Forschungsdatenzentrum (FDZ) der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.IABBP9323.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.

Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, Tschersich N, et al. (2024b) *IAB-Betriebspanel (IAB-BP) 1993-2023*. FDZ-Datenreport, 10/2024 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2410.de.v1.

Bellmann L, Gerner H-D, and Leber U (2014) Firm-Provided Training During the Great Recession. *Jahrbücher für Nationalökonomie und Statistik* 234(1): 5–22.

Bessey D, Backes-Gellner U (2015) Staying Within or Leaving the Apprenticeship System? Revisions of Educational Choices in Apprenticeship Training. *Jahrbücher für Nationalökonomie und Statistik* 235(6): 539–552.

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/dienst/publikationen/de/16673 (Accessed 4 August 2026).

BMAS/BMBF (2020) *Erste Förderrichtlinie für das Bundesprogramm „Ausbildungsplätze sichern“: Bekanntmachung*. BAnz AT 31.07.2020 B1.

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).

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

Böhn S, Deutscher V (2022) Dropout from initial vocational training – A meta-synthesis of reasons from the apprentice's point of view. *Educational Research Review* 35, 100414: 1–14.

Bol T (2015) Has education become more positional? Educational expansion and labour market outcomes, 1985–2007. *Acta Sociologica* 58(2): 105–120.

Bol T, Ciocca Eller C, van de Werfhorst HG, and DiPrete TA (2019) School-to-Work Linkages, Educational Mismatches, and Labor Market Outcomes. *American Sociological Review* 84(2): 275–307.

Boockmann B, Zwick T, Ammermüller A, and Maier M (2012) Do Hiring Subsidies Reduce Unemployment Among Older Workers? Evidence from Natural Experiments. *Journal of the European Economic Association* 10(4): 735–764.

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

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

Breen R (2005) Explaining Cross-national Variation in Youth Unemployment: Market and Institutional Factors. *European Sociological Review* 21(2): 125–134.

Breen R, Goldthorpe JH (1997) Explaining Educational Differentials: Towards a Formal Rational Action Theory. *Rationality and Society* 9(3): 275–305.

Brunello G (2009) The effect of economic downturns on apprenticeships and initial workplace training: a review of the evidence. *Empirical Research in Vocational Education and Training* 1(2): 145–171.

Brunello G, Wruuck P (2021) Skill shortages and skill mismatch: A review of the literature. *Journal of Economic Surveys* 35(4): 1145–1167.

Bundesagentur für Arbeit (2019) *Statistik/Arbeitsmarktberichterstattung: Fachkräfteengpassanalyse.* Berichte: Blickpunkt Arbeitsmarkt – Fachkräfteengpassanalyse. Nürnberg.

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.

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. *American Economic Review* 84(4): 772–793.

Carstensen MB, Ibsen CL (2021) Three dimensions of institutional contention: efficiency, equality and governance in Danish vocational education and training reform. *Socio-Economic Review* 19(3): 1037–1063.

Culpepper PD, Finegold D (eds) (1999) *The German Skills Machine: Sustaining Comparative Advantage in a Global Economy*. New York, Oxford: Berghahn Books.

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.

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.

Dieckhoff M (2008) Skills and occupational attainment: a comparative study of Germany, Denmark and the UK. *Work, Employment and Society* 22(1): 89–108.

DiPrete TA, Bol T, Ciocca Eller C, and van de Werfhorst HG (2017) School-to-Work Linkages in the United States, Germany, and France. *American Journal of Sociology* 122(6): 1869–1938.

Dummert S, Leber U, and Schwengler B (2019) Unfilled Training Positions in Germany – Regional and Establishment-Specific Determinants. *Journal of Economics and Statistics* 239(4): 661–701.

Dummert S, Umkehrer M (2025) The effects of the COVID-19 pandemic on vocational education and training in Germany. *Journal of Vocational Education & Training* 77(3): 631–654.

Eberhard V, Granato M, Herzer P, Matthes S, Schnitzler A, Schratz R, et al. (2019) *Ausbildungsmarkt in der Krise? Betriebe ohne Azubis, Jugendliche ohne Ausbildungsstellen*. Ergebnisse des BIBB-Expertenmonitors 2018 zu Berufsorientierung und Passungsproblemen auf dem Ausbildungsmarkt. Bonn: BIBB.

Eichhorst W, Rodríguez-Planas N, Schmidl R, and Zimmermann KF (2015) A Road Map to Vocational Education and Training in Industrialized Countries. *ILR Review* 68(2): 314–337.

Eurostat (2020) *Impact of COVID-19 on main GDP aggregates including employment*. Available from: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Impact_of_COVID-19_on_main_GDP_aggregates_including_employment#GDP_estimates_published_for_2020-Q1 (Accessed 12 May 2026).

Fassbender U (2022) Collective Skill Formation Regimes in Times of Covid-19: A Governance-Focused Analysis of the German Dual Training System. *International Journal for Research in Vocational Education and Training* 9(2): 167–194.

Fitzenberger B, Heusler A, Houštecká A, and Wicht L (2025) The composition of applicants, mismatch, and matching efficiency in the German VET market. *Labour Economics* 95, 102755: 1–28.

Forster AG, Bol T (2018) Vocational education and employment over the life course using a new measure of occupational specificity. *Social Science Research* 70: 176–197.

Forster AG, Bol T, and van de Werfhorst HG (2016) Vocational Education and Employment over the Life Cycle. *Sociological Science* 3: 473–494.

Gambin L, Hogarth T (2021) Employers' behavioural responses to the introduction of an apprenticeship levy in England: an ex ante assessment. *Journal of Vocational Education & Training* 73(3): 476–501.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023a) *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.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023b) *Betriebs-Historik-Panel 1975-2022 (BHP 7522 v1)*. Forschungsdatenzentrum 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, and Wolter S (2024a) *Betriebs-Historik-Panel 1975–2023*. FDZ-Datenreport, 09/2024 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2409.de.v1.

Ganzer A, Schmucker A, and Wolter S (2024b) *Betriebs-Historik-Panel 1975-2023 (BHP 7523 v1)*. Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.BHP7523.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.

Haasler SR (2020) The German system of vocational education and training: challenges of gender, academisation and the integration of low-achieving youth. *Transfer: European Review of Labour and Research* 26(1): 57–71.

Hanushek EA, Schwerdt G, Woessmann L, and Zhang L (2017) General Education, Vocational Education, and Labor-Market Outcomes over the Lifecycle. *Journal of Human Resources* 52(1): 48–87.

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.

Huntington-Klein N (2022) *The Effect: An Introduction to Research Design and Causality*. Chapman and Hall/CRC Press.

Iannelli C, Raffe D (2007) Vocational Upper-Secondary Education and the Transition from School. *European Sociological Review* 23(1): 49–63.

Ibsen CL, Thelen K (2025) Arrangers and orchestrators: the diverging role of the state in Danish and German vocational education and training. *Socio-Economic Review* 23(4): 1947–1973.

Imbens GW, Rubin DB (2015) *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge University Press.

Jacob M, Solga H (2015) Germany's Vocational Education and Training System in Transformation: Changes in the Participation of Low- and High-Achieving Youth Over Time. *European Sociological Review* 31(2): 161–171.

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.

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

Mohrenweiser J, Backes-Gellner U (2010) Apprenticeship training: for investment or substitution? *International Journal of Manpower* 31(5): 545–562.

Mohrenweiser J, Zwick T (2009) Why do firms train apprentices? The net cost puzzle reconsidered. *Labour Economics* 16(6): 631–637.

Muehlemann S, Pfeifer H, Walden G, Wenzelmann F, and Wolter SC (2010) The financing of apprenticeship training in the light of labor market regulations. *Labour Economics* 17(5): 799–809.

Muehlemann S, Pfeifer H, and Wittek BH (2020) The effect of business cycle expectations on the German apprenticeship market: estimating the impact of Covid-19. *Empirical Research in Vocational Education and Training* 12, 8: 1–30.

Muehlemann S, Schweri J, Winkelmann R, and Wolter SC (2007) An Empirical Analysis of the Decision to Train Apprentices. *Labour* 21(3): 419–441.

Muehlemann S, Wolter SC (2014) Return on investment of apprenticeship systems for enterprises: Evidence from cost-benefit analyses. *IZA Journal of Labor Policy* 3, 25: 1–22.

Neuber-Pohl C (2021) Apprenticeship non-completion in Germany: a money matter? *Empirical Research in Vocational Education and Training* 13, 12: 1–32.

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

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

Ramirez JV, Weber S (2025) What kind of support do firms prefer to hire apprentices in a recession? *Empirical Research in Vocational Education and Training* 17, 5: 1–24.

Riphahn RT, Zibrowius M (2016) Apprenticeship, vocational training, and early labor market outcomes – evidence from East and West Germany. *Education Economics* 24(1): 33–57.

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

Rohrbach-Schmidt D, Uhly A (2015) Determinanten vorzeitiger Lösungen von Ausbildungsverträgen und berufliche Segmentierung im dualen System. Eine Mehrebenenanalyse auf Basis der Berufsbildungsstatistik. *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 67(1): 105–135.

Rözer JJ, Bol T (2019) Labour Market Effects of General and Vocational Education over the Life-Cycle and across Time: Accounting for Age, Period, and Cohort Effects. *European Sociological Review* 35(5): 701–717.

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 P (2001) The School-to-Work Transition: A Cross-National Perspective. *Journal of Economic Literature* 39(1): 34–92.

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

Schönfeld G, Wenzelmann F (2020) *Tarifliche Ausbildungsvergütungen: Ergebnisse für 2019*. Bonn: Bundesinstitut für Berufsbildung (BiBB). Available from: https://www.bibb.de/dienst/publikationen/de/10818 (Accessed 4 August 2026).

Schönfeld G, Wenzelmann F, Pfeifer H, and Risius, Paula: Wehner, Caroline (2020) *Ausbildung in Deutschland – eine Investition gegen den Fachkräftemangel: Ergebnisse der BIBB-Kosten-Nutzen-Erhebung 2017/18.* Forschungs- und Arbeitsergebnisse aus dem Bundesinstitut für Berufsbildung: Report, 1/2020. Bonn: BIBB.

Schünemann B, Lechner M, and Wunsch C (2015) Do Long-Term Unemployed Workers Benefit from Targeted Wage Subsidies? *German Economic Review* 16(1): 43–64.

Shavit Y, Müller W (eds) (1998) *From School to Work: A Comparative Study of Educational Qualifications and Occupational Destinations*. Clarendon Press.

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

Smith E (2024) Rescue and recovery: The COVID and post-COVID responses of apprenticeship systems. *International Review of Applied Economics* 38(3): 337–356.

Smyth I, Zimba C (2019) An investigation into apprenticeship completion and retention in Northern Ireland: a social exchange perspective. *International Journal of Training and Development* 23(1): 89–115.

Snell D, Hart A (2008) Reasons For Non-completion And Dissatisfaction Among Apprentices And Trainees: A Regional Case Study. *International Journal of Training Research* 6(1): 44–73.

Spence M (1973) Job Market Signaling. *The Quarterly Journal of Economics* 87(3): 355–374.

StataCorp (2021) *Stata Statistical Software: Release 17*. College Station, TX: StataCorp LLC.

StataCorp (2023) *Stata Statistical Software: Release 18*. College Station, TX: StataCorp LLC.

Statistik der Bundesagentur für Arbeit (2022) *Bundesprogramm „Ausbildungsplätze sichern"*. Berichte: Arbeitsmarkt kompakt, November 2022. Hannover.

Statistik der Bundesagentur für Arbeit (2025) *Kurzarbeiterquote: Ausgewählte Berichtsmonate, Datenstand: März 2025*. Auftragsnummer 388869. Nürnberg. Available from: https://statistik.arbeitsagentur.de.

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).

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.

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

Wolter SC, Schweri J, and Mühlemann S (2006) Why Some Firms Train Apprentices and Many Others Do Not. *German Economic Review* 7(3): 249–264.

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

Wooldridge JM (2023) Simple approaches to nonlinear difference-in-differences with panel data. *The Econometrics Journal* 26(3): C31-C66.

CHAPTER 2. DOES MORE MONEY HELP? THE IMPACT OF THE APPRENTICESHIP MINIMUM WAGE ON TRAINING OFFERS AND TAKE-UPS

The paper of chapter 2 is currently under review and has been published as a preprint (SocArXiv): https://doi.org/10.31235/osf.io/hjb74_v2 (co-authors: Lukas Fervers and Annika Reiff).
The layout and/or numbering was adjusted to fit the structure of the dissertation. Minor corrections and updates have been incorporated in the version of the dissertation.

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.

2.1 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 in 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 2.2, 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 2.3 describes the dataset and variables used and

outlines our research design and estimation technique. Section 2.4 presents our main results and robustness checks before section 2.5 concludes with a short discussion of policy implications and directions for future research.

2.2 Theoretical background and related literature

2.2.1 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 2.1). 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 in 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

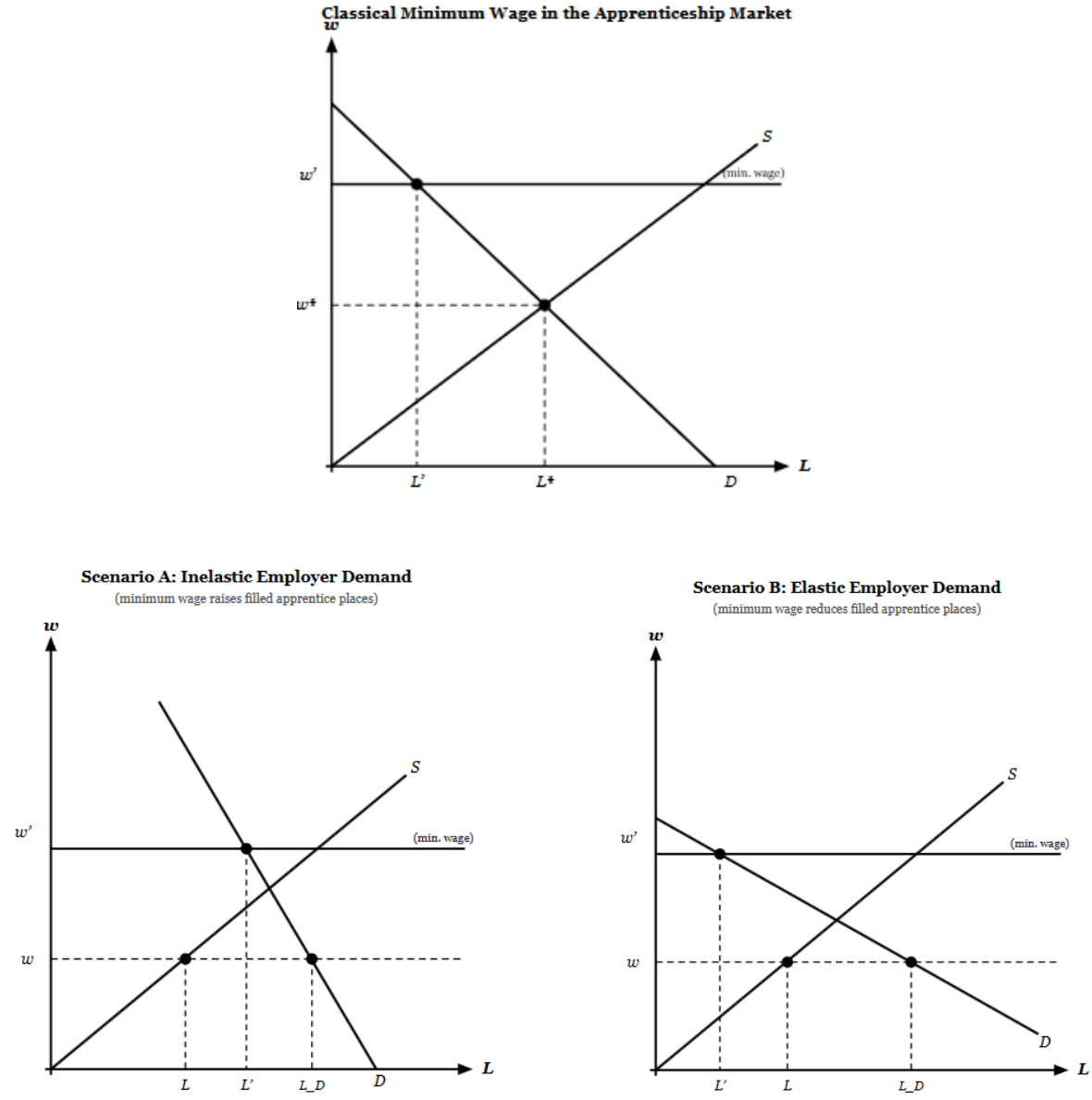
situation, young adults may choose not to enter the apprenticeship market but pursue a university education instead. In some regions and sectors, this has resulted in a shortage of apprentice supply at the given wages (e.g., BIBB, 2020; BMBF, 2020). This undermines the argument of the neo-classical model 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 2.1, 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 2.1), 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 2.1), 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 offers 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 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 2.1: Theoretical expectations regarding the effects of the MAW on offered and filled places



Source: Illustration by the authors. Note: This figure was generated with the use of Anthropic Claude AI (Sonnet 5, 2026). The authors reviewed the content and take full responsibility for the figure.

2.2.2 Related literature

Most of the literature on minimum wages focuses 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 finds 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) analysed its effect on apprenticeship postings in low-wage occupations and find 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 report 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.

2.3 Methodology

2.3.1 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.,

$$(2.1)\ \widehat{\delta_{ATT}} = (\overline{y_{T,1}} - \overline{y_{T,0}}) - (\overline{y_{C,1}} - \overline{y_{C,0}}),$$

with subscript T and C denoting the treatment and control group, and 0 and 1 the period before and after the 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.,

$$(2.2)\ y_{it} = \mu_i + \theta_t + \boldsymbol{\delta_1} * \boldsymbol{treat_{it}} + u_{it}$$

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 the autumn of 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 (see Imbens and Rubin, 2015) this can be stated as follows:

$$(2.3)\ E(Y_1^0|d = 1) - E(Y_0^0|d = 1) = E(Y_1^0|d = 0) - E(Y_0^0|d = 0)$$

(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 hires 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.

2.3.2 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., 2023a; Ganzer et al., 2023b). 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 2.2 provides a timeline of survey years, training cycles and the timing of the new policy.

Figure 2.2: Timeline of survey years, training cycles and the policy introduction

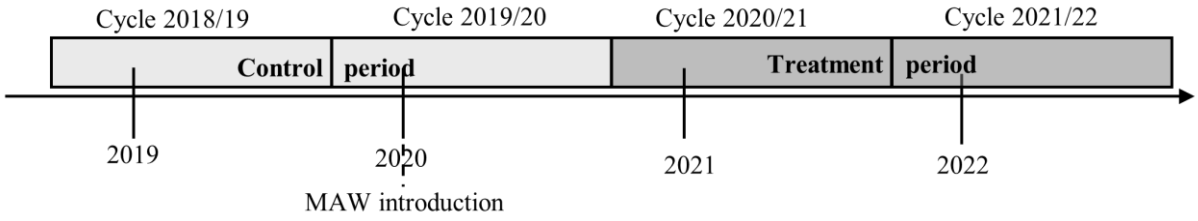


Illustration by the authors.

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.[5] 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 were 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 serve 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

[5] 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).

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 the 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 includes 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 A2.1 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 in offered and filled training places over the years. 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 A2.1 in the appendix. Since only about 4% of the total sample of firms are treated (see Table A2.1), this does not allow for additional analyses of heterogeneous effects, e.g., with regard to sectors, firm size or regions

2.4 Results

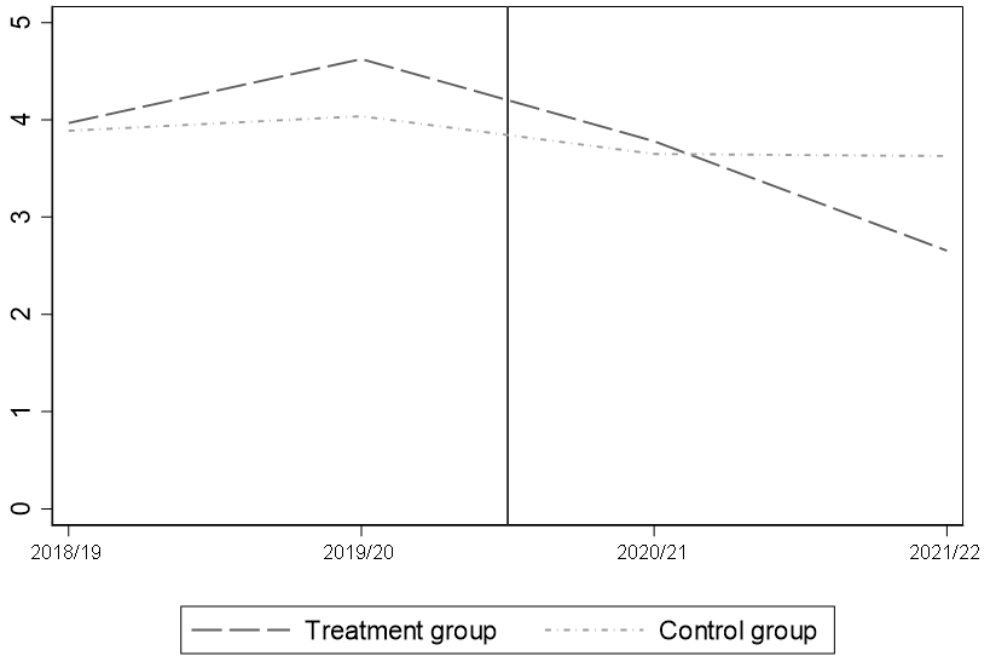
2.4.1 Main results

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 2.3 depicts descriptive trends of the number of offered trainings over time for treated and control group firms. With training 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 2.4), a very slight downward trend in the post-treatment period is observable in the control group, though the post-treatment downward trend

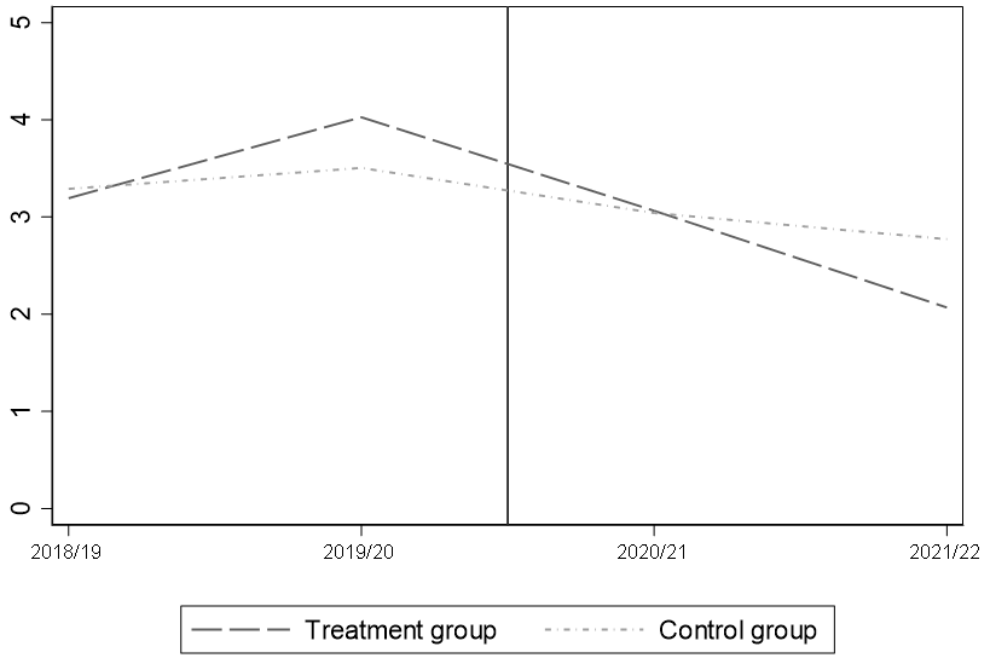
is more pronounced in the treatment group. These descriptive results already point to slightly negative effects on both outcomes. Generally speaking, the small spikes in 2020 (covering the 2019/2020 training cycle) are due to our sample construction since firms had to have at least one newly filled apprenticeship 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 2.4.2). 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, 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 in 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 2.3: 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 2.4: 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 2.1 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 (with an average of 4.06 offered and 3.53 filled places; see Table A2.1 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 the treatment as the pre-treatment period. Since 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 A2.3, Robustness: data II), where the results are similar to the TWFE estimates (and vice versa: TWFE estimates without 2019 are similar to CS-DiD effect sizes).

Table 2.1: 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 2.4.2). 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

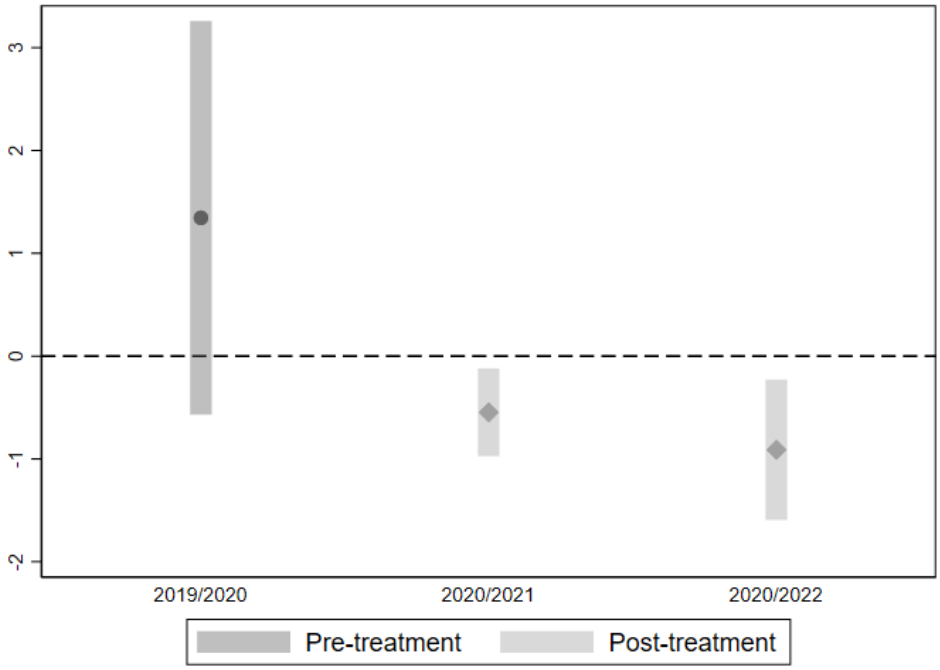
to be acknowledged as equally possible approaches. The general conclusion with regard to the effect of the reform should therefore be that it is either negative or indistinguishable from zero. Still, the results clearly show that the MAW did not lead to a substantial increase of new apprenticeships as intended.

2.4.2 Robustness checks

We conducted 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.

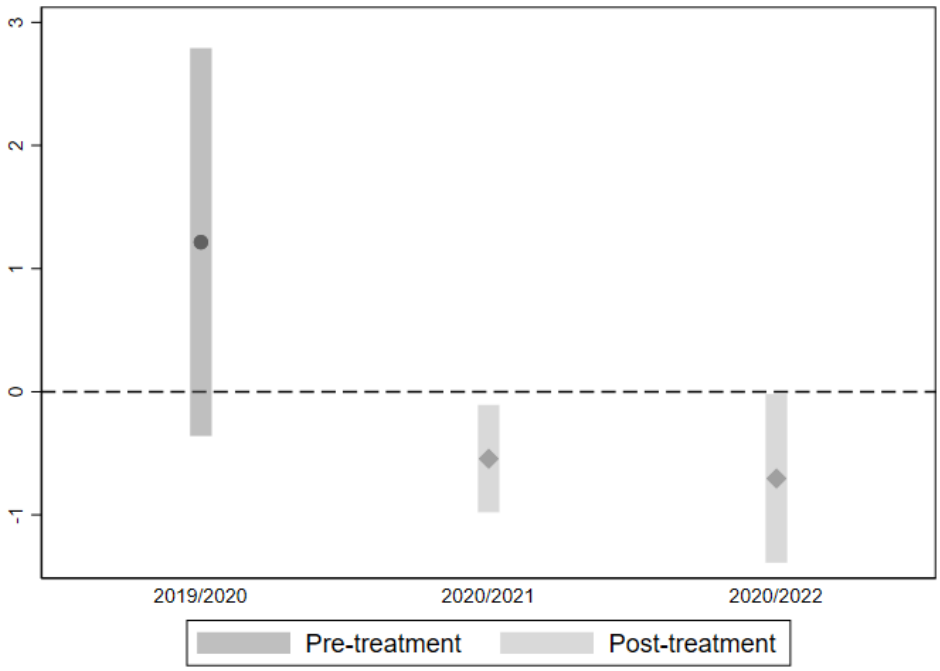
First, we conducted the typical placebo tests in time to test for diverging trends before the treatment. Figures 2.5 and 2.6 show the results for all pre- and post-treatment periods for the two outcomes. The coefficient and confidence interval labelled 2019/2020 display the test for diverging trends before the treatment (see also Appendix Table A2.2, 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. In contrast, the true effect of the reform could be even slightly more negative (as long as one does not assume anticipation effects, which are unlikely because of the institutional setting, see Section 2.4.1).

Figure 2.5: CS-DiD for offered trainings



Source: IAB-BP + BHP + regional data. CS-DiD II specification (see Table 2.1). 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 2.6: CS-DiD for filled trainings



Source: IAB-BP + BHP + regional data. CS-DiD II specification (see Table 2.1). 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 conducted 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 A2.2, Part I, columns 3/4). These groups should not have been affected by the MAW introduction. However, if treated firms diverge from control group firms, for example with regard to their COVID-19 affectedness, this placebo test should detect such differences. We did 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 A2.2, Part I, column 5). This might be expected if apprentices become more expensive due to the reform. However, we did 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 found a slightly higher probability of affected firms to be treated. This result is significant on the 10% level (Appendix Table A2.2, 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 2.1, 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 the treatment status. As our results (Appendix Table A2.2, Part II, columns 3/4) indicate, closure during COVID-19 does not affect offered or filled training places in the autumn of 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 the spring of 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-term effects such as disrupted supply chains. 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 A2.2, Part II, columns 5/6). The 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 group firms (Appendix Table A2.2, Part II, columns, 1/2).

Fifth, we assessed the robustness of the results with regard to the 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 A2.3, 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 A2.3, Robustness: estimation).

Overall, our results seem to be 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.

2.5 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 provision 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 number 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 dropout 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 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-term 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 for 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.

2.6 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*. 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/dienst/publikationen/de/16673 (Accessed 4 August 2026).

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. *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 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.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023b) *Betriebs-Historik-Panel 1975-2022 (BHP 7522 v1)*. Forschungsdatenzentrum 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.

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.

Imbens GW, Rubin DB (2015) *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge University Press.

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/dienst/publikationen/de/10818 (Accessed 4 August 2026).

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.

Declaration on the use of AI:

Anthropic Claude AI (Sonnet 5, 2026) was used to generate the graphs in Figure 2.1. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the publication.

2.7 Appendix

Table A2.1: 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 A2.2: 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 A2.3: 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

CHAPTER 3. THE LONG-TERM EFFECTS OF MONETARY (DIS-)INCENTIVES ON APPRENTICESHIP TRAINING: DOES AN APPRENTICESHIP MINIMUM WAGE LEAD TO MORE OR LESS SUCCESSFUL TRAINING COMPLETIONS?

The paper of chapter 3 has been submitted (no co-authors). The layout and/or numbering was adjusted to fit the structure of the dissertation. Minor corrections and updates have been incorporated in the version of the dissertation.

Abstract

This study analyses the effect of a new German minimum apprenticeship wage (MAW) on training completions using firm-level panel data combined with register and regional data. The study builds on research reporting negative short-term effects on training offers and take-ups. It is argued that negative effects at the apprenticeship start may not necessarily translate into negative effects on completions because a wage increase potentially changes the firms' and apprentices' motivation to achieve a completion. To answer this empirical question, a difference-in-differences design is employed to compare treated firms for which the MAW implied wage increases to a control group that already paid above the new threshold. Results indicate that the MAW did not have a negative effect on completions, thereby complementing evaluations of the policy with regard to long-term effects. This study informs the design of future apprenticeship wage policies and adds to the literature on apprenticeship wages and completion.

3.1 Introduction

Vocational education and training (VET) have been recognized as an important building block for economies and societies (e.g., Barbieri et al., 2018; Carstensen and Ibsen, 2021). It ensures the integration of youth into the labour market and the provision of skilled workers once apprentices have completed their training (e.g., Lüthi, 2025; Picchio and Staffolani, 2019). Especially the dual education system of Germany has been recognized as a VET system that provides people with skills, good job opportunities and career prospects (e.g., Dieckhoff, 2008; Eichhorst et al., 2015; Riphahn and Zibrowius, 2016), leading to comparatively low youth unemployment (e.g., Ryan, 2001; see also Breen, 2005). However, if young people lose interest in apprenticeship training and therefore do not complete or even start the apprenticeship, this could impact future skill supply. In sectors and jobs that rely on apprenticeship training, skill shortages may arise, which in turn can have implications for a country's economy as a whole. This has been the case in Germany in recent years, as the country had to witness a decreasing number of training contracts and an increasing number of training places that remained unfilled (BMBF, 2020: 33; BIBB, 2020).

Given this development, research (e.g., Billett, 2020; Busemeyer and Garritzmann, 2017; Busemeyer and Jensen, 2012; Di Stasio, 2017; Russo et al., 2019) as well as policy makers are concerned with the attractiveness of the (German) VET system (BMBF, 2019; Deutscher Bundestag, 2019a). Following political debates on how dual education could be revitalized, the German government introduced a statutory minimum apprenticeship wage (MAW) in 2020. According to this new policy, every new apprentice who started in the training year of 2020 had to be paid at least €515 per month (Deutscher Bundestag, 2019b).

Tobler et al. (2026) have analysed this policy with regard to training offers and take-ups, arguing that a revitalization of the training system depends on how severe the increasing costs affect employers and how much of an incentive the wage increase presents to potential future apprentices (see also Muehlemann and Wolter, 2014). The authors find indication for reductions

in offered training places as well as filled training places and conclude that – at first glance – there is no evidence for a revitalization of the training system via the MAW policy (Tobler et al., 2026).

However, in addition to effects on apprenticeship take-ups, the introduction of a minimum wage might additionally have an impact on training completions further down the timeline. With regard to the aim of bolstering the dual education system, the empirical question should not only be about apprenticeship take-ups, as it is people with *completed* apprenticeships who are available as skilled workers in the future.

Taking a closer look at the policy's effect on completions is especially important, given that in recent years, a rise in training non-completions could be observed (Ostermann, 2025). While this could be due to an actual dropout or a change of vocational training (Bessey and Backes-Gellner, 2015; Wydra-Somaggio, 2021), non-completion still implies stranded costs for firms (Neuber-Pohl, 2021: 4) and various potential disadvantages for apprentices: a change of training implies a longer training time with a low training wage (cf. Bessey and Backes-Gellner, 2015: 544). In case of an actual dropout, employment without a graduate certificate may lead to future wage disadvantages and comparatively unstable employment (Patzina and Wydra-Somaggio, 2020). Therefore, it is crucial to understand the policy's effect on successful completions in the long-run.

This longer-term effect on training completions is theoretically ambiguous: It is possible that the MAW led to less completions because of the reduced number of entrants into the dual education system. At the same time, less entrants do not necessarily imply less completions, as increased apprenticeship wages might lead to changes in employers' (Muehlemann and Wolter, 2014) and apprentices' (e.g., Neuber-Pohl, 2021) financial motivation to achieve successful completions. The question of which of these potential effects dominates is an empirical one that this study seeks to answer.

To do so, data from the IAB Establishment Panel (IAB-BP, Bellmann et al., 2024a; Bellmann et al., 2024b) is used to analyse the effect of the MAW introduction for apprentices in 2020 on training completions three years later. Methodologically, the study takes advantage of the fact that not all firms were affected by the apprenticeship minimum wage introduction. Only firms which paid below the new wage threshold had to increase their apprenticeship wages. Firms that already paid above the new threshold were not affected. Hence, the affected treatment group can be compared against a non-affected control group in a difference-in-differences (DiD) design.

This article contributes to the literature in two ways. First and more generally, it adds to the literature on the role of apprenticeship wages with regard to training completion (Bessey and Backes-Gellner, 2015; Böhn and Deutscher, 2022; Neuber-Pohl, 2021; Snell and Hart, 2008; Smyth and Zimba, 2019). As the apprenticeship minimum wage introduction presents an exogeneous change, the analysis not only sheds light on the motivation of apprentices in the face of higher wages but also on the firms' incentive to train and achieve successful apprenticeship completions (e.g., Acemoglu and Pischke, 1998; Acemoglu and Pischke, 1999a; Muehlemann and Wolter, 2014; Muehlemann et al., 2010). Using a quasi-experimental design is a particular strength here as it allows for a causal interpretation of effects which was not possible in previous research concerned with apprenticeship wages and training completions (Neuber-Pohl, 2021). Therefore, even though the study examines a national apprenticeship wage policy, its findings are relevant for the research field beyond the immediate national context.

Second and more specifically, this study complements previous research findings that analysed the employment effects of the apprenticeship minimum wage introduction at the beginning of the training period (Langen and Dörsam, 2025; Tobler et al., 2026). To fully evaluate the impact of the MAW, it is important to understand not only short-term but also

longer-term effects. Therefore, an analysis of the policy's effect on training completions can inform policy makers and future policy designs.

3.2 Previous research and theoretical background

Research on minimum wages for apprentices is rather scarce and mainly concerned with apprenticeship employment and training activity. Papps (2020) studied age-specific minimum wage increases in the UK that do or do not apply depending on the age of the apprentices. The author reported negative effects on training activity when wages are raised (Papps, 2020). Linckh et al. (2023) analysed a previous apprenticeship wage reform in Germany, in which wages for underage apprentices had to be raised to the level of wages for older apprentices, thereby abolishing age-based wage discrimination. The authors found that these wage increases had negative effects on apprentice employment (Linckh et al., 2023). Langen and Dörsam (2025) analysed the newly introduced MAW in Germany in low-wage occupations with regard to apprenticeship postings and were unable to find overall effects (which is mainly due to violations of the parallel trends assumption in the DiD design). However, the authors reported negative effects on apprenticeship postings in some particular production sectors (e.g., plastic-making/-processing and wood-working/-processing) (Langen and Dörsam, 2025). Tobler et al. (2026) also analysed the new German MAW and its effect on training offers as well as take-ups on the overall apprenticeship market. The authors found indication for either non-significant or negative effects for both outcomes: if anything, the policy seems to have led to decreases in offered training places that translated into a similar decrease in filled training places. This implies that the effect on employers' training offers dominated, thereby impeding possible gains with regard to new entrants into the dual education system (Tobler et al., 2026).

All these studies have in common that they look at apprenticeship employment and training activity, i.e., the number of young people who enter the training system as they

constitute a potential future workforce. This focus is comparable to the analysis of employment effects in the general labour market (for the US, see e.g. Card and Krueger, 1994; Meer and West, 2016; for the UK, see e.g. de Linde Leonard et al., 2014; Metcalf, 2008; for Germany, see e.g. Bossler and Gerner, 2020; Dütsch et al., 2025).

However, with regard to the apprenticeship market, employment effects are only one of two aspects. The second and equally important aspect concerns the number of successful training completions (cf. Smyth and Zimba, 2019: 90). Only workers with a completed apprenticeship and a certificate will be able to work in skilled jobs in their professional area, thereby counteracting potential skill shortages.

Successful apprenticeship completions depend on both firms and apprentices (Rohrbach-Schmidt and Uhly, 2015). Therefore, one has to consider the firms' as well as the apprentices' perspective in the development of a theoretical argument on the overall effect of the MAW on training completions.

3.2.1 Perspective of firms

Starting with the perspective of firms, it is important to consider the reason why firms train apprentices. Previous research has established two different firm strategies: substitution versus investment (e.g., Mohrenweiser and Backes-Gellner, 2010). Generally, the decision of firms to train apprentices depends on the costs and benefits (Muehlemann and Wolter, 2014). Firms are willing to train if the benefits of training are higher than the costs (Muehlemann et al., 2007; Wolter et al., 2006). Moreover, in following a substitution strategy, firms are willing to train as long as apprentices are not more expensive – if not more profitable – than other forms of cheap and unskilled labour (Mohrenweiser and Zwick, 2009). This implies that firms can immediately reap the benefits from their apprentices' productivity.

However, some firms are willing to offer training even though initial costs may be high. Previous research argued that this is the case if apprentices can be retained after training

completion, if finding skilled employees is hard or the skill-specificity is high (Mohrenweiser and Zwick, 2009). Earlier and more theoretical accounts, however, provide a rationale as to why firms may be willing to invest not only in specific but also general skills: Due to information asymmetries between training firms and other employers, for example regarding their former trainees' abilities, training firms can offer wages below the marginal productivity of their workers (Acemoglu and Pischke, 1998; for a review article, see Acemoglu and Pischke, 1999a). According to Acemoglu and Pischke (1998), this asymmetry relates to workers' wage determination and the workers' quitting behaviour: As the information on ability is unknown to other firms, their wage offers to workers who left their employer or who were laid off reflect expectations on productivity influenced by both high and low ability workers. Therefore, firms have an interest to increase the productivity of their workers through training. Doing so, they increase the profits made by higher ability workers whom they offer wages high enough for them to not quit (Acemoglu and Pischke, 1998). Thereby, some firms may follow an investment strategy and accept training costs during the training period (and especially in the early phase when the apprentices' skills are still low) in order to later profit from the retention of apprentices as skilled workers.

Moreover, delayed benefits also arise because firms can save on screening, hiring and firing costs (Muehlemann et al., 2010; see also Muehlemann and Wolter, 2014). Related to this, investment in training and cost-benefit calculations depend on the labour market's institutional setting (Muehlemann and Wolter, 2014). For example, a strong employment protection legislation increases the costs of layoffs in case of skill mismatch, which makes it attractive for firms to train apprentices to become skilled workers (Muehlemann et al., 2010).

Increased training costs due to the MAW introduction have several theoretical implications with regard to training completions. First, candidate selection may increase. In case of the substitution strategy, apprentices may not serve as good substitutes for cheap labour anymore which may lead firms to discontinue their training efforts or to hire only the very best

applicants. As Muehlemann and Wolter (2014) note, especially small firms depend on having their training costs covered already during the training period, as they may not be able to employ the former apprentice as a skilled worker later. This endeavour becomes more difficult for them with a minimum wage for apprentices. Therefore, especially among the smallest training firms, selection may increase – given that they continue to train when faced with higher apprenticeship wages (Muehlemann et al., 2007). In case of the investment strategy, investing in apprentices to profit from their future productivity becomes riskier for firms with regard to potential dropouts. Therefore, firms following an investment strategy may also become more selective in their hiring process to prevent early dropouts of less well-suited or less able candidates.

Second, the MAW leads to compressed wages within firms (Acemoglu and Pischke, 1999a; Acemoglu and Pischke, 1999b) as new apprentices have to be paid more even though they do not possess any skills necessary to work productively, yet. Therefore, in line with Acemoglu and Pischke (1999a; 1999b) who made this argument with regard to the general labour market, firms – independent of their training strategy – should be interested in training their apprentices to align productivity with costs as fast as possible, given that they continue their training efforts (see Muehlemann et al., 2007).

Third, especially investment strategy-firms should be more motivated to train their apprentices well and to guide them to a successful completion. This is because only upon completion, firms are able to benefit from the return on their (now larger) investment (Rohrbach-Schmidt and Uhly, 2015). The increased employer motivation due to wage compression and the generally increased investment costs could result in a better training experience for apprentices who might then in turn be more motivated to complete the training.

3.2.2 Perspective of apprentices

From the perspective of apprentices, not only a better training experience but also the increased wages as such could motivate them to stay with the training firm and complete the

apprenticeship. Previous research indicates that low apprenticeship wages play a role in early dropout decisions of apprentices (Bessey and Backes-Gellner, 2015; Böhn and Deutscher, 2022; Neuber-Pohl, 2021; Snell and Hart, 2008; Smyth and Zimba, 2019). Literature with a focus on the effect of monetary incentives on apprenticeship completion, however, is rather scarce. An exception is the study by Neuber-Pohl (2021) who used data from Germany and found that higher apprenticeship wages are positively associated with training completion.

In sum, while previous research reports that there is indication of the MAW having led to reductions in training offers and take-ups, this can but does not necessarily translate to less training completions further down the line. A potentially higher candidate motivation to graduate, a higher firm incentive to train and positive candidate selection of firms may contribute to a stable completion rate or even more completions. This article seeks to answer this open question.

3.3 Methods

3.3.1 Analytical approach

Empirical tests of the effect of higher apprenticeship wages on training completions may suffer from endogeneity bias, as higher wages are likely confounded with other favourable firm characteristics that also have an impact on the (non-)completion of peoples' apprenticeship training (Neuber-Pohl, 2021). The analysis of the MAW introduction, however, presents a quasi-experiment which can be approached by methods that aim at taking care of this endogeneity problem.

The MAW was introduced exogenously and implies a wage increase for new apprentices in firms that previously paid below the new threshold of €515 per month. The effect of this policy on successful completions is the difference between a scenario in which the MAW was introduced with a counterfactual scenario in which the MAW was not introduced. The

underlying question, therefore, is: How would the firms that were affected by this new policy have developed had the treatment not taken place? As one can only observe the actual outcome in case of treatment but not the counterfactual outcome in case of no treatment, the next best solution is a comparison of firms that were affected by the policy (i.e., the treatment group) with a control group of firms that were not affected by the policy because they already paid their apprentices more than €515 per month. This control group approximates the unobserved counterfactual development. A comparison between a treatment and control group can be realized in a DiD design. Using this research design and data, this study closely follows the analytical approach as well as the variable use and sample construction (see below) of Tobler et al. (2026). Hence, the descriptions in the methods section are similar. By extending the results to longer-term effects on successful completions, this ensures methodological comparability with regard to the short-term effects on training offers and take-ups reported there.

The parameter of interest in the DiD design is the average treatment effect, δ_{ATT}, which in this case is the effect of the MAW. This effect is estimated by comparing the difference in average outcomes – in this case the number of successful training completions – between the treatment and control group over time:

$$(3.1)\ \widehat{\delta_{ATT}} = (\overline{y_{T,1}} - \overline{y_{T,0}}) - (\overline{y_{C,1}} - \overline{y_{C,0}})$$

The subscripts T and C denote the treatment versus control group, 0 and 1 denote the period before versus after the treatment.

As the MAW introduction did not vary in treatment timing, δ_{ATT} can be estimated in a two-way fixed effects model (TWFE, Wooldridge, 2021):

$$(3.2)\ y_{it} = \mu_i + \theta_t + \boldsymbol{\delta_1} * \boldsymbol{treat_{it}} + u_{it}$$

where y_{it} denotes the number of successfully completed trainings, μ_i the individual firm fixed effect and θ_t the time fixed effect. Marked in bold, $\boldsymbol{\delta_1}$ denotes the estimated treatment effect and $\boldsymbol{treat_{it}}$ indicates the treatment status of firm i at time t. u_{it} denotes the error term.

As the MAW was introduced in 2020, the first cohort of apprentices in treated firms who did complete their training without delay finished in 2023. This is, because most dual education apprenticeships in Germany have a duration of around 3 years. Therefore, with regard to the outcome variable, the time before 2023 can be regarded as pre-treatment time, while 2023 is the post-treatment period. The year 2022 has to be left out in the analysis, as it is impossible to determine if completions in this year are completions in one of the fewer 2-year apprenticeships or regular 3-year apprenticeships that started before the MAW introduction. This makes results for 2022 hard to interpret.

As the DiD approach compares average outcomes between groups and over time, it accounts for any potential group differences as well as general time trends that affect both groups. However, it also assumes parallel trends, i.e., a parallel development of the outcome between treatment and control group in the (hypothetical) absence of treatment (for more insights into the potential outcome framework, see Imbens and Rubin, 2015):

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

(parallel trends assumption)

The superscript 0 denotes the (hypothetical) outcome in the absence of treatment, the subscripts 0 and 1 denote the time periods before and after the treatment and d denotes the actual treatment status.

However, this assumption is potentially unrealistic in the study's scenario because the treatment status may be correlated with various firm characteristics, such as firm size, employee structure, region or industry. For example, previous research has found that completion depends on regional unemployment (Gambin and Hogarth, 2016; Greig, 2019). This could potentially

lead to differences in trends of successful completions between treatment and control group before and after the MAW introduction, thereby violating the parallel trends assumption. Moreover, the policy came into effect shortly before the start of the COVID-19 pandemic. Treatment group firms, which may be located in different regions or industries than control group firms, may be affected differently by the pandemic. For example, businesses requiring close social contact, such as hairdressers or restaurants and hotels were closed during several shutdown periods over the course of the pandemic. These sectors are also known for rather low wages and are therefore theoretically overrepresented in the treatment group. Accordingly, a parallel development of trends in the absence of treatment may be a controversial assumption.

To account for this potential violation of the parallel trends assumption, this study extends the baseline TWFE specification to a DiD design with pre-matching as suggested by Callaway and Sant'Anna (2021) (CS-DiD), which is comparable to the idea of semi-parametric DiD by Abadie (2005). However, it uses a more robust estimation technique (Sant'Anna and Zhao, 2020) and is applicable to unbalanced panels, such as the one in this study. The pre-matching of treatment and control group firms takes care of potential covariate imbalances in a weighted sample. Therefore, this specification compares the development of treatment and control group firms that are more similar to each other both in terms of firm and regional characteristics as well as affectedness by COVID-19, thereby making sure that results are not biased by such differences between groups. The use of covariates implies that the parallel trends assumption can be relaxed to the assumption of parallel trends conditional on covariates, meaning that it only needs to hold once covariates are taken into account (Abadie, 2005; Callaway and Sant'Anna, 2021).

Moreover, the study employs an alternative linear approach to the use of covariates as well as a non-linear alternative that were recently suggested by Wooldridge (2023). Here, covariates are interacted with the time effect, with the treatment status as well as with a dynamic treatment effect; that is, a triple interaction of treatment status, time and covariates. The

covariates are de-meaned before entering the regression. The linear approach uses OLS regression for a manual computation of a TWFE estimator, thereby providing another specification to be compared with the baseline TWFE and the CS-DiD estimator. The non-linear approach uses a Poisson regression (as completions are a count variable) and accounts for the possibility of a corner-solution problem. For example, it is possible that the MAW led to a reduction in successful completions, e.g., because the reduction in training activity (see Tobler et al., 2026) translates into a reduction in completions further down the line. If treatment and control group firms have different training capacities (e.g., due to a potentially different firm size), one group may simply be unable to reduce completions as much as the respective other group because they can simply not reduce below 0. In this case, a linear approach would lead to wrong conclusions. The Poisson regression circumvents this described problem (Wooldridge, 2001). The plausibility of the parallel trends assumption is further evaluated by means of placebo tests in time that assess diverging pre-treatment trends.

3.3.2 Data, variables and sample

To analyse the effect of the apprenticeship minimum wage introduction in 2020 on training completions, data from the IAB Establishment panel (IAB-BP) was used (Bellmann et al., 2024a; Bellmann et al., 2024b). This yearly survey data is collected on the firm-level and includes information on basic as well as more specific firm characteristics, such as the firms' training activity. A syntax file provided by the Research Data Centre (FDZ) of the Federal Employment agency (BA) was used to combine the yearly data files into a panel dataset (Umkehrer, 2017). Moreover, the survey data was merged with additional modules of the Establishment History Panel (BHP; Ganzer et al., 2024a; Ganzer et al., 2024b). The BHP is retrieved from register data and provides additional yearly firm-specific information. To further enrich the data, external regional information on the district level GDP (Statistische Ämter des Bundes und der Länder, 2023), unemployment (Bundesagentur für Arbeit, 2024) and COVID-

19 incidences (Robert Koch-Institut, 2024) was merged via district codes (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2024) that are available in the IAB-BP (variable *Kreiskennziffer*).

The number of successfully completed apprenticeships is directly available in the survey data and was used as the outcome variable. The treatment variable was dummy coded to distinguish treated firms and control group firms. Treated firms were defined as firms in which at least one apprentice was paid below €515 in the training year of 2019/2020, which is the last training cycle prior to the MAW introduction. A question on this is available in the survey of 2020. Firms without apprentices paid below €515 constituted the control group. As the treatment definition is independent of the total number of apprentices, this implies a rather conservative measure. Hence, the estimated effects can be understood as lower bound estimates, as alternative treatment definitions, i.e., with more apprentices having to be paid below the new threshold, would most likely lead to stronger effects.

To make firms more comparable in model specifications using covariates, various control variables were used. Basic firm characteristics were included: the total number of employees, the share of female employees, the share of full-time employees and the share of employees with different skill levels. These variables were taken from the BHP and measured prior to the policy introduction in 2019. Using BHP variables as covariates offers the advantage that the information in provided independent on whether the firm has participated in the IAB-BP survey in a particular year, thereby circumventing the problem of smaller analysis samples due to missing values on covariates. Additionally, external regional variables were also included as control variables, namely the regional unemployment rate, GDP and COVID-19 incidence rates, all measured at the district level. Finally, a survey variable on whether a firm was closed (voluntarily or not) due to COVID-19 was used to control for COVID-19 affectedness.

The final analytical sample was restricted to firms with at least one employee and which were classified as a training firm in 2020. This is necessary to receive the question on

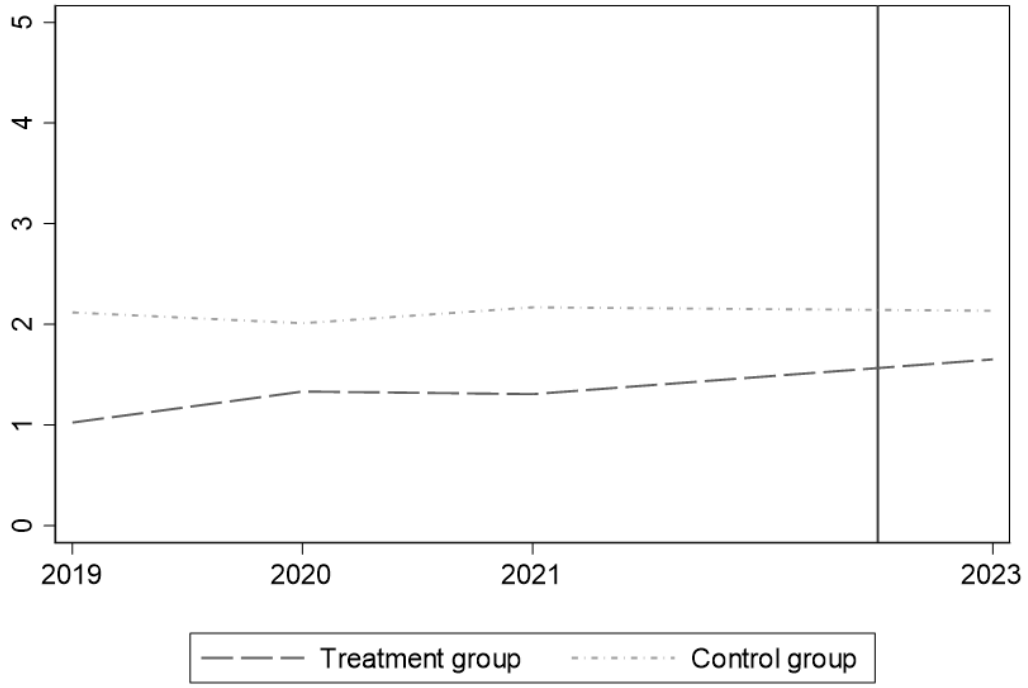
apprenticeship pay. Firms without an interview in 2020 were excluded because in this case, the treatment information would also be missing. Firms that participated in 2020 but did not provide treatment information for other reasons had to be excluded. To prevent inconsistencies in firm information over the years or between datasets (IAB-BP vs. BHP), various exclusion criteria were defined. Inconsistencies may arise, for example, because the interviewee provided information on different firm units over different waves. A similar argument can be made for inconsistencies across the two main datasets if the interviewee provided information on a firm unit in the IAB-BP survey that was not intended to be targeted. In this case, the register data would not match, e.g., a regional subunit versus the entire company. Implausible jumps over time were defined as a jump in absolute numbers in offered, filled or completed trainings of more than +/-20 and a relative change of more than >0.99/<-0.50. Inconsistencies between datasets were defined as absolute deviations in the number of employees or trainees of more than 50 and a relative deviation of more than 0.95. Moreover, deviations in the inflow of new apprentices (survey information on filled training places versus register information on trainee inflow) of more than 20 (relative: more than 0.95) were also taken into account. Cases for which the exclusion criteria applied were excluded from the analyses. Finally, for the main analyses, the sample was restricted to non-missing cases with regard to the outcome variable and the covariates as well as to cases with at least one pre-treatment and one post-treatment observation, which implies that firms had to be surveyed in the post treatment year of 2023. For robustness checks on panel attrition and re-calculations using a non-restricted sample, these final restrictions were relaxed. The main analysis sample consists of 5937 firm-year observations. A more detailed descriptive summary can be found in Table A3.1 in the appendix.

3.4 Results

3.4.1 Main results

Figure 3.1 shows the development of successful training completions between 2019 and 2023. The year 2022 is excluded as the results cannot be interpreted unambiguously (see Section 3.3.1). The vertical line separates the pre-treatment from the post-treatment period. In the control group, completions were relatively stable, with completions ranging between 2.0 and 2.3. completions over the observation period. Average completions in the treatment group increased slightly from 1.0 to about 1.3 completions between 2019 and 2020, remained stable in the year after and increased to 1.7 completions in the pre-treatment period in 2023. Therefore, the increase between 2021 and 2023 in the treatment group versus the stability in the control group might suggest a positive treatment effect. However, the increase between 2019 and 2020 that can be observed for the treatment group vis-à-vis the stability (or even small decrease) for the control group shows a difference in trends also before the actual treatment (this difference is insignificant as shown in the sensitivity analyses, see Section 3.4.2). However, this graphical examination only provides a first impression of the trends in training completions and requires further inspection.

Figure 3.1: Development of training completions



Source: IAB-BP + BHP + additional regional data. Own calculations.

Table 3.1 shows the treatment effect estimations for the different model specifications. The baseline TWFE model shows a positive treatment effect which suggests that compared to control group firms, treatment group firms experienced an increase by 0.340 completions between the pre-treatment and post-treatment period. However, the effect is not significant in terms of the commonly applied significance levels. In the CS-DiD approach that makes use of a pre-matching procedure, thereby accounting for covariates, the treatment effect becomes slightly larger (0.397), with a z-value of 1.51 indicating that the coefficient misses marginal significance just slightly.

Table 3.1: Treatment effect estimations using different model specifications

	TWFE (linear)	CS-DiD (linear)	TWFE (OLS with covariate interaction (linear))	Poisson with covariate interaction (non-linear)
Treatment effect	0.340	0.397	0.314	0.401
	(1.32)	(1.51)	(1.10)	(1.46)
Observations	5937	5798	5937	5937

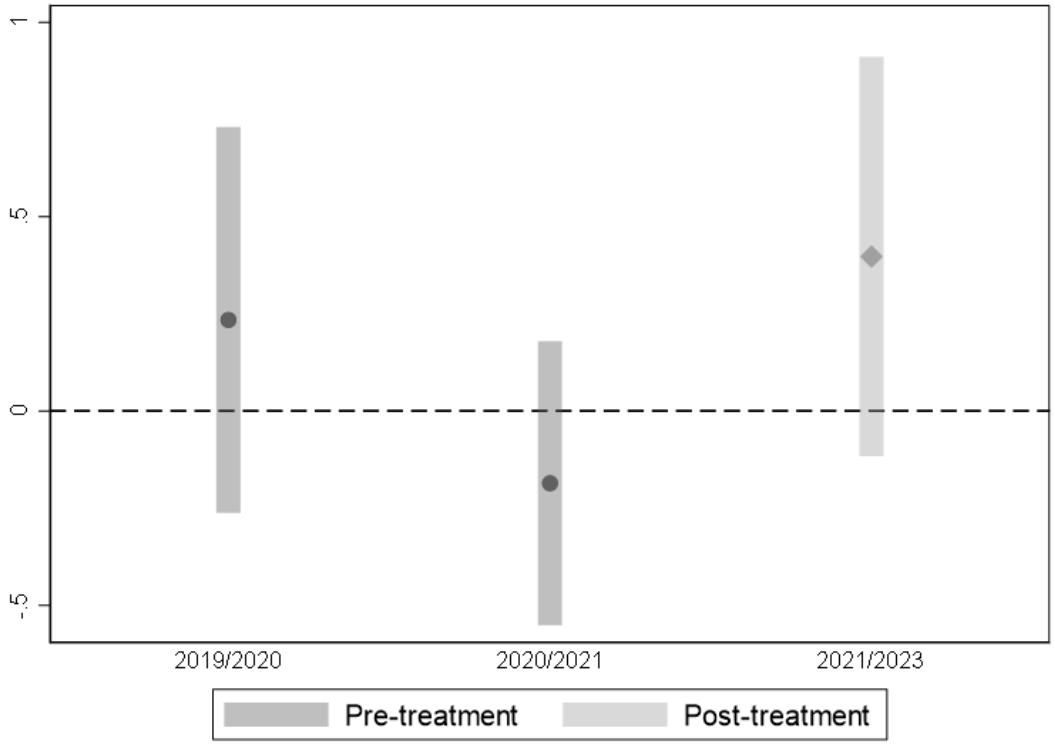
TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses. Poisson models show z-statistics in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001
Source: IAB-BP + BHP + additional regional data. Own calculations.

The similarity of the baseline TWFE and CS-DiD results already implies that the inclusion of covariates does not lead to changes in the substantive results. It should be noted that the larger effect sizes could be due to the definition of the pre-treatment period in the CS-DiD approach. CS-DiD only uses the last pre-treatment period in the estimation of the treatment effect, i.e., 2021. This means that this method compares developments between treatment and control group between the last pre-treatment time point of 2021 and the post-treatment time point of 2023. The TWFE approach uses all pre-treatment periods, thereby also taking the developments in 2019 and 2020 into account, comparing all pre-treatment time points (2019-2021) with 2023. This could lead to slightly different estimates of the treatment effect.

The results of the linear TWFE model with (de-meaned) covariate interaction – albeit showing a slightly smaller t-value – are comparable with previous specifications and lead to a similar conclusion, as does the non-linear model which also shows a similar treatment effect size. Therefore, it can be concluded that all models yield similar results: a positive effect of the MAW policy on successful completions that misses statistical significance.

Given an average in successful completions of about 2.0, an effect size of more than 0.3 is considerable. Moreover, the confidence interval of the CS-DiD estimate, which is shown in Figure 3.2 (‘Post-treatment’ coefficient on the right) illustrates that in a ‘worst-case scenario’, the treatment effect is only slightly negative at around -0.1. Hence, even in this scenario, the negative effect on completions at the end of the training period is smaller than the negative effect found for apprenticeship offers and take-ups at the beginning of the training period (which Tobler et al. (2026) report as around -0.7 and -0.6 respectively). The other coefficients in Figure 3.2 show treatment effect estimates for the pre-treatment period, i.e., tests for placebo treatment in time which are discussed in Section 3.4.2.

Figure 3.2: CS-DiD estimation showing the actual treatment effect and placebo tests in time



Source: IAB-BP + BHP + additional regional data. Own calculations. Point estimates and 95% confidence intervals shown. Note: CS-DiD calculates the treatment effect by using the post-treatment period (2023) in relation to the last pre-treatment year (2021) (see 'Post-treatment' coefficient on the right). For the placebo tests in time (see 'Pre-treatment' coefficients on the left and in the middle), the placebo post-treatment period (2021 and 2020) is compared to the previous period (2020 and 2019, respectively).

Although the models show non-significant effects, they still indicate that the MAW introduction at least did not lead to negative effects on completions. With regard to previous findings on the policy's short-term effects at the beginning of the apprenticeship, this presents an important insight into longer-term effects: Overall, the results suggest that while the introduction of the MAW seems to have led to a reduction in training offers and take-ups, as shown by Tobler et al. (2026), this did not translate into a negative effect on training completions.[6] If at all, the results (although missing statistical significance) suggest a positive treatment effect indicating that the MAW led to more completions in treatment firms. Following the theoretical argument that was put forward in this article, the reasons behind this finding

[6] This study was able to replicate the substantive results of Tobler et al. (2026), i.e., reductions in both offered and filled training positions (see Appendix Table A3.2).

likely are a compound effect of a higher apprentice and/or firm motivation to complete the training and positive apprentice selection in the face of increased costs. The societal implications of this are discussed below.

3.4.2 Sensitivity analyses

To ensure the robustness of the results, several sensitivity analyses were performed. First, to evaluate possible diverging trends during the pre-treatment period, placebo tests in time were performed using CS-DiD and the other linear as well as non-linear estimation procedures from the baseline specification (see Appendix Table A3.3). Figure 3.2 illustrates the placebo point estimates and confidence intervals of the CS-DiD model (see second column in Table A3.3), which calculates two treatment effects for the placebo periods 2019-2020 and 2020-2021 ('Pre-treatment', coefficients on the left and in the middle). The placebo estimates show a slightly positive effect for the time 2019-2020 and a negative effect for the time 2020-2021. Similar to what has been observed in the descriptive graphs in Figure 3.1, a slight divergence of trends before the treatment seems to be present. However, for both placebo periods, the effects are insignificant, as is the actual treatment effect ('Post-treatment' coefficient on the right; see also Table 3.1). Moreover, all other estimation procedures (i.e., TWFE, OLS regression with covariate interactions, Poisson regression with covariate interactions), which calculate one overall placebo treatment effect, also result in non-significant placebo tests in time.

Second, as the observation period may be influenced by COVID-19, it is important to investigate pandemic-related influences. To do so, the influence of COVID-19 related closures was used as a treatment variable to investigate whether said closures had an effect on successful completions. However, the result of this analysis suggests that, apart from the estimate for the year 2023, there is no difference in completions between firms who did and did not close their business (see Appendix Table A3.4). For 2023, there seems to be a negative effect of COVID-19 related closures on completions. While this shows that COVID-19 had a negative influence

on completions, this does not bias the main results, as these pandemic-related closures were controlled for.

Moreover, a sample selection check (logit) analysing whether firms which closed due to COVID-19 had a higher probability to belong to the treatment group confirms that results are not driven by selection into treatment (see Appendix Table A3.4). To further investigate potential COVID-19 influences, additional control variables were included in the baseline CS-DiD specification, namely problems related to a firm's demand, personnel, supply chain and liquidity (see Appendix Table A3.4). These additional covariates lead to slightly larger coefficients than the baseline CS-DiD specification and a z-value that just misses statistical significance. This alternative specification, therefore, supports the baseline results. Overall, the COVID-19 related robustness checks show that the analyses are not biased by pandemic influences that varied between treatment and control group.

Third, a dropout analysis (logit) was performed to make sure that results are not driven by attrition over the observation period. However, the analysis shows that there is no significant difference between treatment and control group to not have an interview sometime after the policy introduction or specifically in 2023 (see Appendix Table A3.5).

Forth, a robustness check to address potential concerns of selection into the survey sample was conducted. This selection could arise from the fact that the sample construction required that the firms of the analysis sample had at least one apprentice in the 2020 survey wave, with the treatment question depending on this filter. Therefore, the firm and regional characteristics of the analysis sample were compared to the sample of all firms which had at least one apprentice sometime between 2018 and 2020, i.e., all firms with relatively recent training activity (see Table A3.6 in comparison with Table A3.1 in the appendix). However, both samples are very similar in terms of their employment structure, COVID-19 affectedness, regional unemployment and regional GDP, which indicates that the analysis sample does not suffer from selectivity.

Fifth, several sensitivity analyses regarding the data and estimation were performed to evaluate the main results' robustness (see Appendix Table A3.7). A trimmed outcome variable (99%, excluding the top 1%) was used in the CS-DiD model to account for potential outliers, which did not change the results. Moreover, the baseline TWFE model was re-calculated using a sample that was not restricted to non-missing cases in terms of the outcome and covariates and the restriction of having at least two observations (one of them being post-treatment). This also did not change the results. Further, the TWFE model was re-calculated making use only of the last pre-treatment period (2021), to make it more comparable to the CS-DiD model that only uses the last pre-treatment period by design. This indeed makes the TWFE and CS-DiD estimations even more similar. Lastly, two alternative covariate weighting procedures were applied to the CS-DiD, which also had no influence on the results (see Appendix Table A3.7). In sum, all sensitivity analyses show a high robustness of the results.

3.5 Discussion and conclusion

This study investigated the effect of a new minimum wage for apprentices on successful training completions. This was prompted by the recent finding that this policy – which was introduced to increase the attractiveness of dual education in Germany – seems to have led to reductions in the numbers of offered and filled training places (Tobler et al., 2026). It was argued that these reductions at the start of the training period do not necessarily translate into negative long-term effects on completions because the increased wages might influence the incentive of employers and motivation of apprentices to complete the training.

The results of this study show that, indeed, the MAW did not lead to reductions in completions three years later. Although just slightly missing statistical significance, the effects of various model specifications and implementations with and without covariates suggest that if anything, the MAW contributed to more completions, with a treatment effect of more than

0.3 completions from the pre-treatment to the post-treatment period. However, due to the lack of significance, this positive effect should be interpreted with caution. Given the lower bound of the confidence interval which covers the area just slightly below zero, a more cautious interpretation of the results would be that the policy at least did not have a negative effect on completions that would be comparable to the size of the negative effect it had on apprenticeship offers and take-ups.

With regard to the theoretical argumentation presented in this article, this result probably arises from a combination of an increased financial motivation of apprentices to complete their training, an increased incentive for firms due to increased training costs as well as a more rigorous positive candidate selection in the hiring process. While increases in completions are most likely regarded as positive news for the society and the economy alike, increases in selection at the same time have welfare implications for school leavers with lower abilities who may not be able to find apprenticeships because of the greater firm selectivity (see Haasler, 2020; Jacob and Solga, 2015). Therefore, future research may look further into the different societal aspects of increased training completions in the face of higher apprentice wages.

This study is not without limitations. First, while the results were robust to selection, dropout, different sample definitions and additional COVID-19 controls, an indication for non-parallel trends that were observed before the treatment remain a weak spot of the analysis. Second, the reported effect may suffer from distortion. This is, the effect may be a compound effect of regular completions after 3-years (that were affected by the MAW introduction) and training completions by apprentices who started their training before 2020 and delayed their completion, e.g., because of COVID-19. However, due to the DiD design employed, this would only be an issue if delays differed between treatment and control group firms, which is unlikely. Third, while this study was able to analyse the long-term effect of the policy introduction on completions, it is not possible to evaluate further increases of the MAW in subsequent years because the data only provides information on the MAW introduction. Fourth, as the treatment

group is rather small, further subgroup analyses, for example with regard to industry, are not possible and remain a subject for future research. Finally, as the IAB-BP is a firm-level dataset, there is no explicit information on applicant behaviour. Therefore, while the theoretical argument concerns both the firm and the apprentice perspective, it is not possible to disentangle to what extent the results are driven by a changed incentive of employers and changes in the motivation of apprentices. Nevertheless, using the same data as previous research on the effect of the MAW offers a high comparability. Specifically, with regard to the policy effect, the results presented in this article alter the view on this policy put forward by Tobler et al. (2026), thereby providing an additional perspective on this policy. Moreover, this study adds to research concerned with factors that influence training completions or dropouts and the role of apprenticeship wages (Bessey and Backes-Gellner, 2015; Böhn and Deutscher, 2022; Neuber-Pohl, 2021; Snell and Hart, 2008; Smyth and Zimba, 2019).

Given that the MAW rate was rather low – with average tariff wages for apprentices in the first training year already at €848 in 2019 (Schönfeld and Wenzelmann, 2020: 14f.) – it is noteworthy that even at this low threshold, a tendency for increases in completions was found. One can only speculate about the effect of higher apprenticeship wages on completions, which also necessitates speculations about offers and take-ups: Most probably, higher wages would further decrease training offers, but may lead to more filled places as well as a higher candidate selection and an increased motivation for firms and apprentices to complete the vocational training. However, disentangling the interplay of effects on offers *prior to the start*, take-ups *at the start* and resulting completions *at the end* of the apprenticeships in light of higher apprentice minimum wages remains a task for future research.

3.6 References

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

Acemoglu D, Pischke J-S (1998) Why Do Firms Train? Theory and Evidence. *The Quarterly Journal of Economics* 113(1): 79–119.

Acemoglu D, Pischke J-S (1999a) Beyond Becker: Training in Imperfect Labour Markets. *The Economic Journal* 109(453): 112–142.

Acemoglu D, Pischke J-S (1999b) The Structure of Wages and Investment in General Training. *Journal of Political Economy* 107(3): 539–572.

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.

Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, Tschersich N, et al. (2024a) *IAB-Betriebspanel (IAB-BP) – Version 9323 v1*. Nürnberg: Forschungsdatenzentrum (FDZ) der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.IABBP9323.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.

Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, Tschersich N, et al. (2024b) *IAB-Betriebspanel (IAB-BP) 1993-2023*. FDZ-Datenreport, 10/2024 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2410.de.v1.

Bessey D, Backes-Gellner U (2015) Staying Within or Leaving the Apprenticeship System? Revisions of Educational Choices in Apprenticeship Training. *Jahrbücher für Nationalökonomie und Statistik* 235(6): 539–552.

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/dienst/publikationen/de/16673 (Accessed 4 August 2026).

Billett S (2020) Perspectives on enhancing the standing of vocational education and the occupations it serves. *Journal of Vocational Education & Training* 72(2): 161–169.

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).

Böhn S, Deutscher V (2022) Dropout from initial vocational training – A meta-synthesis of reasons from the apprentice's point of view. *Educational Research Review* 35, 100414: 1–14.

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.

Breen R (2005) Explaining Cross-national Variation in Youth Unemployment: Market and Institutional Factors. *European Sociological Review* 21(2): 125–134.

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, 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.

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. *American Economic Review* 84(4): 772–793.

Carstensen MB, Ibsen CL (2021) Three dimensions of institutional contention: efficiency, equality and governance in Danish vocational education and training reform. *Socio-Economic Review* 19(3): 1037–1063.

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.

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.

Dieckhoff M (2008) Skills and occupational attainment: a comparative study of Germany, Denmark and the UK. *Work, Employment and Society* 22(1): 89–108.

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.

Eichhorst W, Rodríguez-Planas N, Schmidl R, and Zimmermann KF (2015) A Road Map to Vocational Education and Training in Industrialized Countries. *ILR Review* 68(2): 314–337.

Gambin L, Hogarth T (2016) Factors affecting completion of apprenticeship training in England. *Journal of Education and Work* 29(4): 470–493.

Ganzer A, Schmucker A, and Wolter S (2024a) *Betriebs-Historik-Panel 1975–2023*. FDZ-Datenreport, 09/2024 (de). Nürnberg: Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.FDZD.2409.de.v1.

Ganzer A, Schmucker A, and Wolter S (2024b) *Betriebs-Historik-Panel 1975-2023 (BHP 7523 v1)*. Forschungsdatenzentrum der Bundesagentur für Arbeit (BA) im Institut für Arbeitsmarkt- und Berufsforschung (IAB). DOI: 10.5164/IAB.BHP7523.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.

Greig M (2019) Factors affecting Modern Apprenticeship completion in Scotland. *International Journal of Training and Development* 23(1): 27–50.

Haasler SR (2020) The German system of vocational education and training: challenges of gender, academisation and the integration of low-achieving youth. *Transfer: European Review of Labour and Research* 26(1): 57–71.

Imbens GW, Rubin DB (2015) *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge University Press.

Jacob M, Solga H (2015) Germany's Vocational Education and Training System in Transformation: Changes in the Participation of Low- and High-Achieving Youth Over Time. *European Sociological Review* 31(2): 161–171.

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.

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.

Mohrenweiser J, Backes-Gellner U (2010) Apprenticeship training: for investment or substitution? *International Journal of Manpower* 31(5): 545–562.

Mohrenweiser J, Zwick T (2009) Why do firms train apprentices? The net cost puzzle reconsidered. *Labour Economics* 16(6): 631–637.

Muehlemann S, Pfeifer H, Walden G, Wenzelmann F, and Wolter SC (2010) The financing of apprenticeship training in the light of labor market regulations. *Labour Economics* 17(5): 799–809.

Muehlemann S, Schweri J, Winkelmann R, and Wolter SC (2007) An Empirical Analysis of the Decision to Train Apprentices. *Labour* 21(3): 419–441.

Muehlemann S, Wolter SC (2014) Return on investment of apprenticeship systems for enterprises: Evidence from cost-benefit analyses. *IZA Journal of Labor Policy* 3, 25: 1–22.

Neuber-Pohl C (2021) Apprenticeship non-completion in Germany: a money matter? *Empirical Research in Vocational Education and Training* 13, 12: 1–32.

Ostermann K (2025) Ausbildungsabbrüche im regionalen Vergleich: Die Schere geht immer weiter auseinander. IAB-Forum, Serie "Arbeitskräftesicherung". Available from: https://www.iab-forum.de/ausbildungsabbrueche-im-regionalen-vergleich-die-schere-geht-immer-weiter-auseinander/ (Accessed 7 August 2025).

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

Patzina A, Wydra-Somaggio G (2020) Early Careers of Dropouts from Vocational Training: Signals, Human Capital Formation, and Training Firms. *European Sociological Review* 36(5): 741–759.

Picchio M, Staffolani S (2019) Does apprenticeship improve job opportunities? A regression discontinuity approach. *Empirical Economics* 56(1): 23–60.

Riphahn RT, Zibrowius M (2016) Apprenticeship, vocational training, and early labor market outcomes – evidence from East and West Germany. *Education Economics* 24(1): 33–57.

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

Rohrbach-Schmidt D, Uhly A (2015) Determinanten vorzeitiger Lösungen von Ausbildungsverträgen und berufliche Segmentierung im dualen System. Eine Mehrebenenanalyse auf Basis der Berufsbildungsstatistik. *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 67(1): 105–135.

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 P (2001) The School-to-Work Transition: A Cross-National Perspective. *Journal of Economic Literature* 39(1): 34–92.

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

Schönfeld G, Wenzelmann F (2020) *Tarifliche Ausbildungsvergütungen: Ergebnisse für 2019*. Bonn: Bundesinstitut für Berufsbildung (BiBB). Available from: https://www.bibb.de/dienst/publikationen/de/10818 (Accessed 4 August 2026).

Smyth I, Zimba C (2019) An investigation into apprenticeship completion and retention in Northern Ireland: a social exchange perspective. *International Journal of Training and Development* 23(1): 89–115.

Snell D, Hart A (2008) Reasons For Non-completion And Dissatisfaction Among Apprentices And Trainees: A Regional Case Study. *International Journal of Training Research* 6(1): 44–73.

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).

Tobler L, Fervers L, and Reiff A (2026) *Does more money help? The impact of the apprenticeship minimum wage on training offers and take-ups*. Available from: https://doi.org/10.31235/osf.io/hjb74_v2.

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).

Wolter SC, Schweri J, and Mühlemann S (2006) Why Some Firms Train Apprentices and Many Others Do Not. *German Economic Review* 7(3): 249–264.

Wooldridge JM (2001) *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

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

Wooldridge JM (2023) Simple approaches to nonlinear difference-in-differences with panel data. *The Econometrics Journal* 26(3): C31-C66.

Wydra-Somaggio G (2021) Early termination of vocational training: dropout or stopout? *Empirical Research in Vocational Education and Training* 13, 5: 1–23.

3.7 Appendix

Table A3.1: Summary descriptives of the analysis sample for the year 2020

Variable	N	Mean	St.dev.	Min.	Max.
Treated: below minimum wage (dummy)	1632	0.044	0.205	0.000	1.000
Number of successful training completions	1632	1.982	4.548	0.000	132.000
Number of trainings offered	1626	3.606	4.993	1.000	105.000
Number of filled training vacancies	1620	3.145	4.775	1.000	105.000
Number of employees 2019	1632	128.940	284.636	0.000	7869.000
Share of full-time employees 2019 (in %)	1632	62.890	25.886	0.000	100.000
Share of female employees 2019 (in %)	1632	41.817	29.370	0.000	100.000
Share of employees in skill-level 1 jobs 2019 (in %)	1632	15.358	17.596	0.000	100.000
Share of employees in skill-level 2 jobs 2019 (in %)	1632	64.928	21.999	0.000	100.000
Share of employees in skill-level 3 jobs 2019 (in %)	1632	11.228	12.751	0.000	100.000
Share of employees in skill-level 4 jobs 2019 (in %)	1632	8.057	12.170	0.000	100.000
Closure due to Corona (forced or non-forced)	1632	0.233	0.423	0.000	1.000
Unemployment rate 2019	1632	5.435	2.198	1.300	12.800
GDP 2019	1632	38537.145	15509.757	16605.000	195809.000
COVID-19 7-day incidence (aver. 15 March - 31 July 2020)	1632	97.907	67.090	10.806	701.137
Number of apprentices	1621	6.891	13.766	0.000	332.000

Source: IAB-BP + BHP + additional regional data. Own calculations.

Table A3.2: Replication of Tobler et al. 2026 (years: 2019-2022)

	TWFE: offered	TWFE: filled	CS-DiD: offered	CS-DiD: filled
Treatment effect	-0.299	-0.219	-0.744**	-0.645**
	(-1.14)	(-0.97)	(-3.04)	(-2.63)
Observations	9167	9167	9111	9111

TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses.
Source: IAB-BP + BHP + additional regional data. Own calculations. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A3.3: Placebo in time (using all baseline model specifications of Table 1)

	TWFE	CS-DiD	TWFE (OLS with covariate interaction (linear))	Poisson with covariate interaction (non-linear)
Placebo treatment effect (overall)	-0.155		-0.139	-0.108
	(-0.97)		(-0.57)	(-0.44)
Placebo treatment effect (2019/2020)		0.234		
		(0.92)		
Placebo treatment effect (2020/2021)		-0.186		
		(-1.00)		
Observations	4298	4237	4298	4298

TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses. OLS models show t-statistics in parentheses. Poisson models show z-statistics in parentheses.
Source: IAB-BP + BHP + additional regional data. Own calculations. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A3.4: Sensitivity – COVID-19

	CS-DiD: COVID-19 effect on completions	Logit: COVID-19 selection into treatment	CS-DiD: Additional COVID-19 controls
Treatment effect (overall)			0.408
			(1.54)
Treatment effect (2019/2020)	-0.174		
	(-1.01)		
Treatment effect (2020/2021)	-0.133		
	(-0.94)		
Treatment effect (2020/2022)	0.014		
	(0.08)		
Treatment effect (2020/2023)	-0.428*		
	(-2.34)		
Closure due to COVID-19		0.317	
		(1.19)	
Constant		-3.158***	
		(-22.08)	
Observations	7449	1632	5693

CS-DiD models show z-statistics in parentheses. Logit models show z-statistics in parentheses.
Source: IAB-BP + BHP + additional regional data. Own calculations. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A3.5: Attrition analysis

	Dropout: No post-treatment interview (no controls)	Dropout: No post-treatment interview (with controls)	Dropout: No interview in 2023 (no controls)	Dropout: No interview in 2023 (with controls)
Treatment group	-0.051	-0.020	-0.195	-0.100
	(-0.32)	(-0.11)	(-1.21)	(-0.56)
Observations	14482	12392	14482	12392

Logit models show z-statistics in parentheses. Source: IAB-BP + BHP + additional regional data. Own calculations.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A3.6: Firm and regional characteristics of all firms that had at least one apprentice sometime between 2018 and 2020 (for the year 2020)

Variable	N	Mean	St.dev.	Min.	Max.
Number of employees 2019	6433	143.976	662.173	0.000	44688.000
Share of full-time employees 2019 (in %)	6433	59.658	27.201	0.000	100.000
Share of female employees 2019 (in %)	6433	43.920	29.290	0.000	100.000
Share of employees in skill-level 1 jobs 2019 (in %)	6433	15.530	18.514	0.000	100.000
Share of employees in skill-level 2 jobs 2019 (in %)	6433	63.801	23.500	0.000	100.000
Share of employees in skill-level 3 jobs 2019 (in %)	6433	11.256	13.721	0.000	100.000
Share of employees in skill-level 4 jobs 2019 (in %)	6433	8.697	12.923	0.000	100.000
Closure due to Corona (forced or non-forced)	6115	0.243	0.429	0.000	1.000
Unemployment rate 2019	6442	5.441	2.220	1.300	12.800
GDP 2019	6442	39225.059	15520.076	16605.000	195809.000
COVID-19 7-day incidence (aver. 15 March - 31 July 2020)	6442	102.207	67.737	10.806	701.137
Number of apprentices	6323	7.498	30.220	0.000	1453.000

Source: IAB-BP + BHP + additional regional data. Own calculations.

Table A3.7: Sensitivity – data and estimation

	CS-DiD: 99% trimmed sample	TWFE: Non-restricted sample	TWFE: Without 2019/2020	CS-DiD: Alternative weighting (stdipw)	CS-DiD: Alternative weighting (drimp)
Treatment effect	0.390	0.367	0.371	0.396	0.397
	(1.48)	(1.38)	(1.44)	(1.51)	(1.51)
Observations	5693	11895	3193	5798	5798

TWFE models show t-statistics in parentheses. CS-DiD models show z-statistics in parentheses. Source: IAB-BP + BHP + additional regional data. Own calculations.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

CHAPTER 4. EFFICIENT STABILIZER OR DEADWEIGHT EFFECT? THE IMPACT OF SUBSIDIES FOR APPRENTICESHIP TRAINING DURING AN ECONOMIC DOWNTURN

A revised version of the paper of chapter 4 (co-author: Lukas Fervers) has been published in *Economics of Education Review* after the submission of the dissertation. The layout and/or numbering was adjusted to fit the structure of the dissertation. Link to publication: https://doi.org/10.1016/j.econedurev.2026.102851

Abstract

Economic downturns often come along with a reduction of apprenticeship training by firms, hereby threatening economies with a lack of skilled labour in the mid- and long-run. Against this background, the German government has implemented an apprenticeship subsidy for small and medium sized companies in the summer of 2020 during the COVID-19 pandemic. In brief, companies that maintained their number of new apprenticeships at the average level of the past three years could receive €2,000 for each apprenticeship starting in the autumn of 2020. We exploit eligibility for the subsidy based on firm size and compare eligible and non-eligible firms in an intent-to-treat difference-in-differences (DiD) design. The estimates show no impact on the number of new apprenticeships but a certain effect on the probability to maintain the training level at the level of the past three years. This suggests that firms that were at the margin of maintaining the level without the subsidy have hired a small number of additional apprentices to receive the subsidy. This mirrors the incentive structure of the subsidy, as firms at the margin have received a large amount of money for a small number of additional apprenticeships. These results are robust to a wide range of sensitivity and robustness checks, including linear and non-linear DiD estimation, tests for diverging trends, placebo tests with placebo outcomes as well as checks for panel attrition. The results add to the scarce literature on the impact of training subsidies in general and their impact during economic crises in particular.

4.1 Introduction

In times of economic crises, the primary economic concern – especially in the short term – is the reduction of employment levels and the increase of unemployment. However, previous research shows that crises also come along with a reduction of vocational training activities by firms, with the reduction in apprentices often being even stronger than the reduction of regular employees (Brunello, 2009; Bellmann et al., 2014). This may contribute to a lack of skilled labour in post-crisis years, and is therefore likely to have detrimental impacts on the economy in the mid- and long-run (Caves and McDonald, 2023; Hérault and Zakirova, 2015; Lüthi, 2025; Neyt et al., 2020; Riphahn and Zibrowius, 2016). In the spring of 2020, economies around the globe experienced an unprecedented downturn due to the spread of COVID-19 and the related counter measures. The negative impact on vocational training activities could be expected to be rather strong in the COVID-19 crises, as the latter raised strong insecurities about future prospects of certain industries, thus calling long-term training investments into question (Dummert and Umkehrer, 2025; Fassbender, 2022; Smith, 2024).

Against this background, the German government introduced a training subsidy for small and medium enterprises that were severely affected by the crisis (the 'apprenticeship bonus'). Eligible firms could get a subsidy of €2,000 for each apprenticeship if they upheld the level of apprenticeship training at the average level of the past three years, and €3,000 for each apprenticeship exceeding this threshold (BMAS/BMBF, 2020; Statistik der Bundesagentur für Arbeit, 2022). This follows the straightforward idea that decreasing the costs of apprenticeship training might (partly) offset the reduced or more insecure expected returns, thus mitigating the impact of the crisis on the dual education system and the long-term impact on the economy (Fassbender, 2022).

Among eligible enterprises, a substantial share of around 30% applied for and received the subsidy (Bellmann et al., 2021; see also Statistik der Bundesagentur für Arbeit, 2022). While this was discussed as a sign for its positive effect in the political discourse (Deutschlandfunk

(Broadcast Germany), 2020), it does not necessarily constitute an indication for a positive effect on the number of new apprenticeships. Considering that a certain share of companies would have maintained their level of apprenticeship training even without the subsidy, it is possible that the subsidy is subject to a deadweight effect, i.e., these firms may have pocketed-in the subsidy. In that case, it is even possible that the subsidy had no effect on the number of new apprenticeships at all. Such deadweight would imply costs for those who (indirectly) pay for the subsidy (e.g., employers via taxes or levies, employees via lower wages or the fiscus (see Kuczera, 2017)) without actually furthering the creation of training opportunities. Additionally, it would entail a redistribution of resources in favour of firms that were doing relatively well during the crisis. Assessing whether the subsidy actually fostered training activity in terms of apprenticeship places is therefore crucial from a policy-making point of view.

In this paper, we provide the first microeconometric evaluation of the apprenticeship bonus that accounts for possible deadweight effects. We exploit variation in eligibility for the subsidy based on size among companies affected by the COVID-19 crisis, and compare eligible and non-eligible firms in an intent-to-treat difference-in-differences (DiD) setting. To account for the limited nature of the dependent variable, we employ both linear and non-linear DiD estimation (Wooldridge, 2023).

The results show a nuanced picture. On the one hand, there is no effect on the total number of apprentices as measured by the absolute number, suggesting that the subsidy suffers from deadweight effects. On the other hand, analyses show that the probability that firms upheld the average level of the past three years increased. Most likely, this suggests that firms which were at the margin of upholding the past level (even without the subsidy) were pushed to upholding it in order to receive the subsidy. As this might have been achieved by hiring only a small number of additional apprentices, this effect becomes indistinguishable from zero when the impact on the total number of apprentices is considered. These results are robust to a wide range of different model specification as well as robustness and placebo tests.

Our findings contribute to different strands of research. First and most generally, they add to the discussion about costs and incentives for vocational training (e.g. Muehlemann et al., 2010; Muehlemann and Wolter, 2014). Second, they contribute to the still scarce evidence on the impact of training subsidies as a policy-instrument to foster dual education (e.g. Gambin and Hogarth, 2021; van den Berg et al., 2006; Westergaard-Nielsen and Rasmussen, 1999; Ramirez and Weber, 2025). Third and most specifically, our results extend previous research by looking at the impact of training subsidies as an automatic stabilizer in times of economic crises. Previous subsidies and their respective evaluations have rarely been implemented as a temporary reaction to economic crises. However, it is possible that the impact of subsidies differs with respect to the macroeconomic environment. Moreover, this might entail different policy-implications as the focus is not only on the impact of subsidies in general but more specifically on their potential role as a temporary stabilizer.

The remainder of this paper is organized as follows. Section 4.2 explains the institutional background and the expected effect of the subsidy in more detail and provides a brief review of related work. Section 4.3 outlines the research design and estimation strategy. Section 4.4 presents the results, before section 4.5 concludes with a discussion of the results and their possible implications for policy-making and directions for future research.

4.2 Institutional background, theory and related work

Most apprenticeships in Germany start in September. Faced with the COVID-19 outbreak in the spring of 2020, the German government implemented the training subsidy and released the directive in July 2020 (BMAS/BMBF, 2020) for apprentices starting in September, i.e., in the training year 2020/2021. Following this directive, certain firms could receive a training subsidy of €2,000 for each apprenticeship if they upheld the level of training at the average of the past three years, and €3,000 for each additional apprenticeship. Eligibility was restricted to

some additional criteria: first, firms had to be severely affected by the COVID-19 pandemic. This could be documented by either having received short-time work for at least one month, or by a reduction in revenues in April and May 2020 of at least 60% compared to the same period of the previous year. While this seems like a rather high threshold, it has to be considered that a substantial share of companies was permanently closed during April and May 2020. In addition, firms were only eligible if they had less than 250 employees, measured as full-time equivalents. To calculate full-time equivalents, employees with more than 30 hours were counted as full-time employees, employees with between 20 and 30 hours as 0.75, between 10 and 20 as 0.50 and less than 10 as 0.25 full-time equivalents. Additionally, firms that are owned or mainly owned by the state, schools and institutions of higher education were not eligible. According to a representative survey of establishments during the pandemic, about 25% of firms were eligible for the subsidy (Bellmann et al., 2020).

The idea of a subsidy is straightforward and has been argued to be an effective tool to incentivize training (Albæk, 2009). When making training decisions, firms trade off the costs against future returns, one of them being future access to skilled labour (Becker, 1962; Muehlemann et al., 2007; Muehlemann et al., 2010; Muehlemann and Wolter, 2014; Brunello, 2009; Ramirez and Weber, 2025). The optimal solution to this trade-off may change in times of economic crises. The COVID-19 crisis in particular led to an immediate and severe drop in revenues but also to a strong insecurity about future prospects of a firm or certain industries as a whole. As future returns become less secure, the incentive to train decreases during crises (Muehlemann et al., 2020). As these possible returns are difficult to increase by political interventions, reducing the costs of training by means of training subsidies is the most feasible way to re-balance costs and returns and to increase the incentive for firms to maintain the level of vocational training.

The idea of implementing training subsidies to foster vocational education has already been employed in the past. Previous evaluations of training subsidies reveal a mixed picture.

Depending on the country, incentives can take the form of direct subsidies, tax reduction schemes or levy schemes in which employers pay for training independent on their own training activity but can amortize the costs by providing training (Kuczera, 2017). Moreover, as Kuczera (2017: 22-23) describes, training subsidies may be aimed at different goals, such as the creation of new places or the increase in apprentice numbers. They can also target different sectors or apprentice subgroups based on personal attributes (Kuczera, 2017: 22-23). Therefore, a direct comparison between studies is impossible because they do not only concern different countries but also different incentives. The institutional settings and the ways in which incentivizing policies are implemented are manifold (see Kuczera, 2017).

Despite this, however, it appears that most studies do not find evidence of the positive effect on training provision and participation intended by policy makers. An exception is Westergaard-Nielsen and Rasmussen (1999) who report a positive association between subsidies and the supply of apprenticeship places by employers in Denmark during the 1980s. This relationship is only significant in some industries such as manufacturing, though (Westergaard-Nielsen and Rasmussen, 1999). Similarly, Schuß (2020) reports that the introduction of an apprenticeship fund in the care sector, which redistributes (and therefore reduces) costs for education between employers, leads to a larger number of apprenticeships in the care sector.

Various other studies do not find any evidence of a positive effect. This is the case for evaluations of the start as well as the completion phase of apprenticeships. For example, van den Berg et al. (2006) analysed a sector-specific levied training fund in the Netherlands during the early 2000s and found no indication for an effect of the subsidy on the number of new apprentices. The same is true for Morlet (2025) who evaluated the effect of the US American Apprenticeship Initiative on the number of apprenticeship contracts. Gambin and Hogarth (2021) described apprenticeship starts in England and their development before and after the introduction of a training levy in 2017. Although purely descriptive, the authors even report a

decrease in apprenticeship starts after the levy introduction. Bonin et al. (2013) analysed a previous apprenticeship bonus in Germany. This bonus was set up for the creation of additional training places for disadvantaged youth, people with learning disabilities and those who did not find an apprenticeship placement in the year before. This bonus was paid for apprenticeships between 2008 and 2010. The authors find no evidence of the desired effect. Instead, their results point to large deadweight effects, meaning that the training places that were created would have also been created without the bonus payment. In addition, with regard to the completion of apprenticeships and the further transition period, they found no effects on premature terminations (see also Fries et al., 2014), completions and integration into the regular labour market (Bonin et al., 2013). It should be noted, however, that this previous German apprenticeship bonus was introduced at a very different time that is not directly comparable to the current situation. At the time, the German apprenticeship market registered more applicants than available training places (see Bonin et al., 2013: 9). Nowadays, overall applicant numbers are lower than overall available training places (BMBF, 2020; see also BIBB, 2020). In sum, the effect of financial incentives aimed at motivating employers to engage in training seems to be rather limited in most cases.

In times of economic crises, the aim of subsidies is a slightly different one: rather than increasing training levels in general, they serve to uphold training levels for a limited amount of time in order to prevent economic difficulties at a later point in time, for example due to a lack of trained and skilled workers in the aftermath of an economic downturn (with regard to COVID-19, see Fassbender, 2022; Smith, 2024). However, research on the effect of subsidies during economic downturns is scarce. Literature that is only indirectly related to the question on apprenticeship training reports positive effects of government subsidies on firms' resilience during the COVID-19 crisis in China (Feng and Xue, 2023) and limited evidence for an effect of wage subsidies on employment during an economic downturn in Mexico (Bruhn, 2020). In a choice experiment, Ramirez and Weber (2025) analysed employer preferences for subsidies

during the COVID-19 pandemic in Switzerland. While all forms of subsidies had a positive effect on newly created (hypothetical) apprenticeship contracts, subsidies targeted at apprentices' wages were the preferred policy measure.

In sum, the literature on the impact of training subsidies is still scarce. More importantly, the possible role of subsidies in times of crisis has hardly ever been studied before. Considering the importance to avoid a strong drop in training activities during crises, as well as the high costs associated with training subsidies if they are subject to deadweight effects, this constitutes an important research gap with high policy relevance.

4.3 Empirical analysis

4.3.1 Research design and estimation

Generally speaking, our research question calls for a DiD design. In brief, we compare treated and non-treated firms from before to after the introduction of the training subsidy. This is intended to estimate the treatment effect on the treated. Following the potential outcome framework (see Imbens and Rubin, 2015), the latter can be defined as the difference between the realized outcome under treatment in the post-treatment period, $Y^1(1)$, and the potential outcome in case of no treatment, $Y^1(0)$ (superscripts denote the period before (0) and after (1) treatment; hypothetical outcomes are defined as $Y(0)$ in case of no treatment and $Y(1)$ in case of treatment). In a DiD design, the treatment effect can be estimated by means of the following model (Wooldridge, 2021):

$$(4.1)\ y_{it} = \beta_0 + \gamma D_i + \lambda post + \delta_1 (D_i * post) + u_{it}$$

where D denotes whether a unit i is eventually treated at some point in time. $post$ denotes the post treatment period, β_0 the outcome level for the non-treated in the pre-treatment period, and u_{it} the idiosyncratic error term. The treatment effect is denoted by δ_1. y_{it} is the outcome

variable of unit i at time t, which is either the total number of apprentices or a binary indicator that equals 1 if the number of new apprentices is equal or higher than the average of the past three years (and 0 otherwise).

However, in our case, there are two complications beyond the usual challenges of DiD designs. First, actually receiving the subsidy (i.e., being treated) is conditional on the intended outcome. Comparing firms actually receiving the subsidy does therefore not make sense (similar problems occur in other settings in economics, e.g., wage subsidies (Boockmann et al., 2012; Schünemann et al., 2015)). We therefore compare firms that are *eligible* for the subsidy to those that are not eligible. Following the discussion of wage subsidies by Schünemann et al. (2015), we argue that this is not a limitation but the actual parameter of interest. First, it is the more interesting question from a policy-making point of view as making certain firms or persons eligible for subsidies depending on a certain characteristic or behaviour will usually be the approach for policy measures like that. Second, looking at eligibility gives a better estimate of the elasticity of supply by employers with respect to the costs of (potential) apprenticeships. We therefore define the treatment status as being eligible for the subsidy. To ensure that firms are as comparable as possible in terms of their affectedness by COVID-19, we only include firms that received short-term allowance between March and July 2020. Variation in eligibility is therefore exclusively based on size (above or below 250 employees measured as full-time equivalents).

The second issue concerns the underlying identifying assumption. The linear TWFE estimator allows for a causal interpretation of the treatment effect if – and only if – treatment and control group had developed identically over time in the absence of treatment, i.e.,

$$(4.2)\ E(Y^1(0) - Y^0(0) \mid D = 1) = E(Y^1(0) - Y^0(0) \mid D = 0)$$

In the given context, this assumption of common trends can be questioned for two reasons related to the different size of treatment and control group firms (which is the basis for the

definition of the treatment status). First, it is simply possible that smaller and larger firms are affected differently by such unprecedented downturns, either because size as such comes along with different resources to cope with such crises, or because firms of different size differ by other characteristics which entail divergent trends over time. Second, the first dependent variable (number of apprentices) is de facto a count variable that cannot be negative. If the control group (larger firms) has considerably larger values than the treatment group (smaller firms) previous to the treatment, a drop of similar size in absolute values due to any other event might be implausible. For example, assume that the control group had a pre-treatment average of 8 apprentices and the treatment group an average of 3 apprentices. Now, if the control group average drops from 8 to 4, the linear common trend assumption would assume a development from 3 to -1 on average in the treatment group in absence of the treatment, which is not possible.

We approach these problems in four ways: First, we limit the sample with regard to firm size. We use only treated firms with at least 50 employees, and only control firms with less than 600 employees. While this leaves us with differences in firm size, this at least substantially mitigates the problem. Second, we include a vector of control variables $\boldsymbol{x}$ that might cause diverging trends (for a complete list of control variables included, see subsequent section). To avoid including control variables that are affected by the treatment, we measure all control variables pre-treatment. To allow for diverging trends with respect to the treatment group, we interact all variables with the $post$ dummy. To enable an intuitive interpretation of the coefficients (especially the $post$ coefficient), we de-mean all variables with the sample mean of the treatment group's covariates, $\overline{\boldsymbol{x}}_1$. Formally, we augment equation (4.1) in the following way:

$$(4.3)\ y_{it} = \beta_0 + \gamma D_i + \lambda post + \overline{\boldsymbol{x}}'\boldsymbol{\beta} + (x_i - \overline{\boldsymbol{x}}')\boldsymbol{\beta_{post}} * post + \delta_1(D_i * post) + u_{it}$$

While these two approaches cope with diverging trends in the linear case, we follow the recommendation by Wooldridge (2023) and additionally employ a non-linear DiD approach with an exponential mean for the continuous outcome. This estimator is based on an imputation procedure which assumes that treatment and control group would have grown at the same rate rather than in absolute values (as in the linear DiD approach). The imputation procedure works as follows: 1) Use all data of the control group (pre- and post-treatment period) plus pre-treatment data of the treatment group and run a Poisson regression of the outcome on the treatment dummy and post-period dummy. This estimates both pre-treatment differences as well as the development over time in the control group. 2) Use this model to predict outcomes of the treatment group in the post-treatment period. 3) The treatment effect is then estimated by comparing the true values of the treatment group to the predicted values. Formally, if the averages of the treatment and control group in the post and pre-treatment period are denoted by $\bar{Y}_{11}, \bar{Y}_{10}, \bar{Y}_{01}, \bar{Y}_{00}$ (the first subscript denotes treatment and control group, the second subscript denotes post and pre-treatment period), this procedure can be defined as follows (see Wooldridge, 2023: C38, equation 2.19):

$$(4.4a)\ \hat{\tau}_{ATT} = \bar{Y}_{11} - \exp[log(\bar{Y}_{10}) + (log(\bar{Y}_{01}) - log(\bar{Y}_{00}))]$$

$$(4.4b)\ \hat{\tau}_{ATT} = \bar{Y}_{11} - \bar{Y}_{10} * (\frac{\bar{Y}_{01}}{\bar{Y}_{00}})$$

The intuition behind this estimator becomes clearer when looking at equation 4.4b. The first term is the observed (average) outcome of the treatment group in the post-treatment period. The second term is the product between the pre-treatment average of the treatment group ($\bar{Y}_{10}$) and the growth rate from before to after of the control group ($\frac{\bar{Y}_{01}}{\bar{Y}_{00}}$). The second term therefore directly estimates the average for the treatment group if the latter had grown at the same rate as the control group. Hence, the treatment effect is the difference between the observed average and the estimated average under identical growth rates. This procedure can be augmented by

including covariates in the first step of the estimation (4.4a). For estimation and inference, we follow Wooldridge's (2023: C42) advice and estimate it directly with pooled quasi-maximum likelihood (QMLE). We employ both linear and non-linear DiD estimation to assess whether the results are sensitive to the nature of the dependent variable (i.e., being a count variable) and underlying identifying assumption, respectively. Regarding statistical inference, all models employ clustered standard errors to account for serial correlation as suggested by Roth et al. (2023).

In addition to the continuous outcome, we also use a binary outcome of having maintained stable training activity compared to the average of the past three years (which is the condition for receiving the subsidy). As for the continuous outcome, we use both linear and non-linear DiD, namely linear probability models (linear DiD) and a logistic mean function (non-linear DiD).

The fourth approach is mostly diagnostic. In addition to assessing diverging pre-treatment trends by means of placebo tests, we run placebo tests with a placebo outcome that is likely to be related to the outcome, though not affected by the reform, namely the inflow of workers with marginal part-time employment (mini jobbers). If these placebo effects are (not) significant, this implies that the development of firms over time is more likely to (not) differ because of other reasons and not due to the treatment. In addition, we check the robustness with regard to the inclusion of different sets of covariates and for a narrower sample definition in which firms are closer to the cut-off point and therefore more similar in size (for a detailed description see section 4.4.2).

4.3.2 Data and variables

To analyse the effect of the apprenticeship bonus, we used data from the IAB Establishment Panel (BP; Bächmann et al., 2023a; Bächmann et al., 2023b) for the survey years 2016 to 2021, which equals the training years 2015/2016 to 2020/2021 as apprenticeships start

in autumn. The final year of the observation period is the treatment period. The BP contains firm-level data that is surveyed yearly and complemented with additional survey questions focusing on a particular topic. For the survey year 2020 (i.e., the training year of 2019/2020), the BP includes detailed information on firms' affectedness by the COVID-19 pandemic.

We used syntax provided by the Data Research Centre (FDZ) of the German Federal Employment Agency (BA) to create a panel dataset from the yearly data files (Umkehrer, 2017). The BP survey data was then combined with information from the IAB Establishment History Panel (BHP; Ganzer et al., 2023a; Ganzer et al., 2023b), which is register data that includes information on firms' employee composition, their wage structure as well as the inflow of different employee groups. Moreover, using district codes provided in the BP (Kreiskennziffer), we merged the dataset with additional regional data on unemployment (Bundesagentur für Arbeit, 2024) and regional GDP (Statistische Ämter des Bundes und der Länder, 2023) on the district level (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2024). Finally, based on 3-digit industry codes (WZ08), we merged the data with information on the share of short-time work (STW), which was provided by the statistics service of the BA (Statistik der Bundesagentur für Arbeit, 2025).

We analysed two main outcomes: first, we considered the inflow of new apprentices as a continuous outcome, which is available in the mergeable BHP module. This variable is a count variable and measures the number of new apprentices employed by the firm in the respective survey year. Second, we used the apprentice inflow to create a related variable measuring whether a firm had maintained a stable level of training compared to the previous three years or even increased their number of apprentices. This measure relates more closely to how the policy was phrased. The variable was generated by averaging the number of new apprentices of the past three years and creating a dummy which becomes 1 if the current inflow is equal to or bigger than this average inflow and 0 otherwise. Moreover, if a firm had no inflow in the past three years as well as in the current year (which would imply stability in mathematical

terms), we also set the dummy to zero. In addition to the two main outcomes, we analysed a third alternative outcome variable, which is the inflow of mini jobbers. This outcome was used in a placebo test in space (see Section 4.4.2).

In models that include covariates, we considered different firm characteristics, which were measured in 2019 (from the register data). Using 2019 as a reference period for our controls provides a pre-treatment as well as a pre-COVID-19 measure. We controlled for the share of female employees and full-time employees, as well as for the median gross daily wage of the full-time employed. Moreover, we controlled for the district-level unemployment rate and GDP in 2019. Additionally, we included the maximum share of short-time workers between April and August 2020 as a measure of industry-level COVID-19 affectedness.

For our final analysis sample, we applied different sample restrictions. First, we excluded firms that had no apprentices in the pre-treatment period between 2015 and 2020. Second, we excluded those with missing values in the treatment variable as those cases cannot be used in the analyses. We only included private firms as those are the ones belonging to the policy's target group. For the same reason, we excluded those whose legal status did not fit the policy's target or whose status did not allow for a clear treatment definition. For example, we excluded public agencies, foundations and stock corporations, as the number of apprenticeships in the data refers to the single firm, whereas the restriction applies to the corporate group. Moreover, we only included independent firms, because the policy was created for small firms and we do not know the total number of employees in non-independent firms that are part of larger companies. This does not allow for a clear treatment definition.

Regarding the size of the firms, we excluded firms with pre-treatment (2019) employee numbers below 50 and above 600 to reasonably ensure comparability between treatment and control group firms. We exclude firms directly around the cut-off (between 230 and 270 full time equivalents) as the measurement of full-time equivalents might be prone to small errors and the forcing variable may enable conscious sorting to either side of the cut-off point. In

addition, as we use information from the BP survey and the BHP register data, we applied a plausibility check that the two data sources consider one and the same firm. To do so, we excluded firms with inconsistent numbers of apprentices or employees as well as inconsistencies between the BHP info on the inflow of apprentices and the BP survey information on filled apprenticeship places. In these cases, the BP survey may not have targeted the firm or subunit that was intended to be targeted (e.g., the overall corporation versus a regional office). We defined inconsistencies as an absolute deviation of more than 50 (more than 20 in the case of apprentice inflow/filled apprenticeship places) plus a relative deviation of more than 0.95. Finally, we restricted our sample to firms with at least one pre-treatment and one post-treatment observation. Applying these restrictions and additionally dropping cases with missing values on covariates or our outcome variables, we ended up with a sample of 3968 firm-year observations. We display more detailed summary descriptives in Table A4.1 in the appendix.

4.4 Results

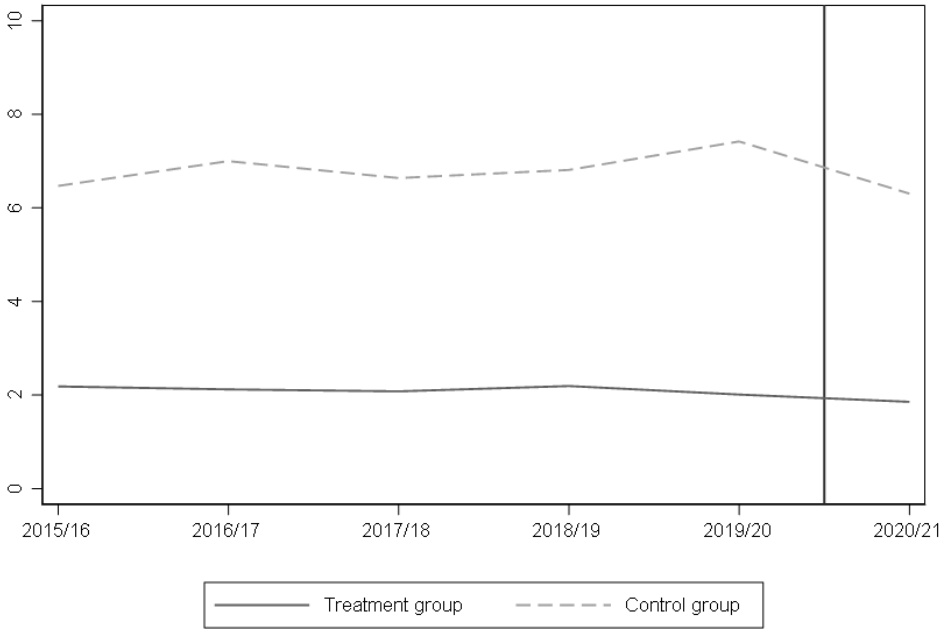
4.4.1 Main results

We start by plotting the continuous (upper panel) and binary (lower panel) outcome in Figure 4.1. For the continuous outcome, there is a rather steady development with little change from 2015/2016 to 2019/2020 in both groups (note that 2015/2016 refers to the respective training year, i.e., apprenticeships that have started in autumn 2015). After the implementation of the subsidy, which coincides with the COVID-19 crisis, there is a slight reduction that is more visible in the control group. Following the reasoning from the previous section, the drop is stronger in the control group in absolute terms but it is less clear in relative terms as the average is about three times higher in the control group (indicating that results might be sensitive to model specification). For the binary outcome, the development is more volatile in

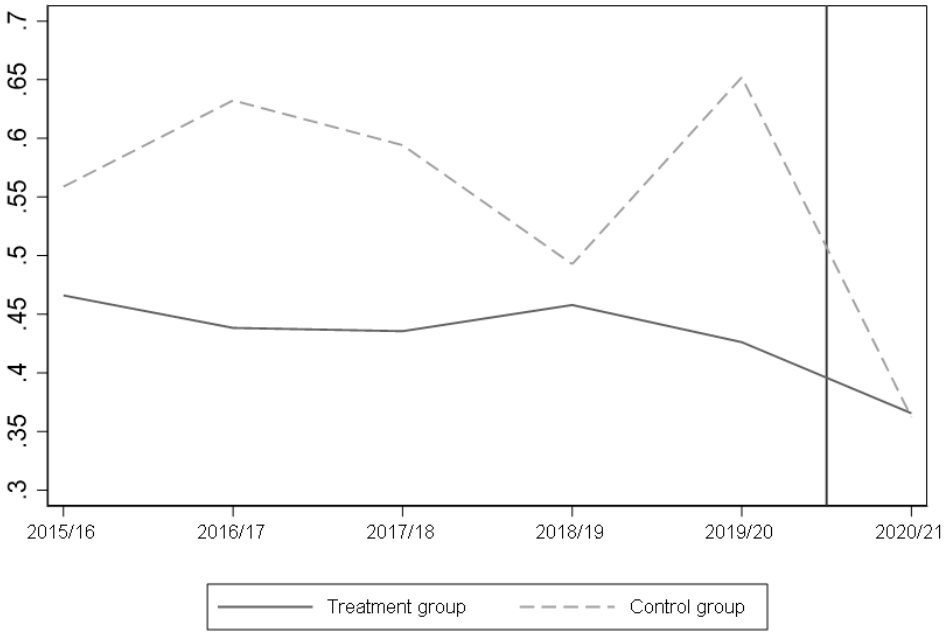
both treatment and control group and both groups are closer to each other in absolute terms. While it has to be left to statistical tests to assess whether the somewhat unequal development before treatment is significant, it is apparent that the control group drops substantially stronger after the implementation of the policy. This gives a first hint that the treatment might have induced behavioural changes in the treatment group in order to receive the subsidy.

Figure 4.1

Continuous outcome: number of new apprentices in treatment and control group per training year



Binary outcome: maintained training level compared to past training years



Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.

We continue with the results from the econometric models. Table 4.1 shows the results for the continuous outcome, with linear and non-linear estimation as well as with and without covariates. Note that the covariates for the non-linear estimation do not appear in the table as they are included in the first step of the estimation procedure but not in the final estimation of the treatment effects (see section 4.3.1). While the treatment effect estimates from the linear model are positive (see estimates in bold), the estimates from the non-linear model are negative, which mirrors the expectation from the development of group averages over time. At the same time, all models fail to reach significance at the 5% level. This suggests that there is no effect of the subsidy on the average number of apprentices.

Table 4.1: Continuous outcome (number of new apprentices)

	Lin. DiD (no cov.)	Lin. DiD (cov.)	Non-lin. DiD (no cov.)	Non-lin. DiD (cov.)
Treatment*After	**0.303**	**0.121**	**-0.088**	**-0.221+**
	(0.336)	**(0.318)**	**(0.131)**	**(0.120)**
Treatment	-4.751***	-4.504***		
	(0.460)	(0.446)		
After	-0.564+	-0.387		
	(0.325)	(0.320)		
Share: FT emp. 2019 (%)		-0.021***		
		(0.006)		
Share: female emp. 2019 (%)		-0.013*		
		(0.005)		
Median wages FT emp. 2019		0.013***		
		(0.004)		
Unemployment rate 2019		-0.001		
		(0.046)		
GDP 2019		0.000		
		(0.000)		
Share: STW		2.011**		
		(0.653)		
After # Share: FT emp. 2019 (%)		0.001		
		(0.007)		
After # Share: female emp. 2019 (%)		0.009*		
		(0.004)		
After # Median wages FT emp. 2019		0.003		
		(0.002)		
After # Unemployment rate 2019		0.057		
		(0.040)		
After # GDP 2019		-0.000		
		(0.000)		
After # Share: STW		-1.631**		
		(0.529)		
Constant	6.869***	6.623***		
	(0.449)	(0.435)		
Observations	3968	3968	3968	3968

Estimations show cluster-robust standard errors in parentheses. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

The results for the binary outcome (Table 4.2) reveal a slightly different picture. All models yield significantly positive treatment effects (see estimates in bold). The coefficients suggest that the treatment has increased the probability to maintain the average level of new apprentices from the past three years by between 11.5 and 14.5%. The point estimates slightly differ between models with the non-linear estimates indicating slightly weaker effects. Once again, this results from the control group starting from higher average values. At the same time, z-statistics are even slightly larger for the non-linear models, likely resulting from a better model fit and therefore lower unexplained outcome variance. Considering that about 40% of eligible firms (see Figure 4.1) upheld their level of apprentices in the post-treatment period, a treatment effect of 11.5 to 14.5% can be classified as at least moderately strong. At the same time, these numbers suggest that a substantial share of firms would have upheld the level of apprentices even without the treatment, pointing to non-negligible deadweight effects. It is worth noticing that results for the binary outcome barely change when covariates are included (for an in-depth discussion and an additional robustness check regarding the inclusion of covariates, see section 4.4.2).

Table 4.2: Binary outcome (maintained training level)

	Lin. DiD (no cov.)	Lin. DiD (cov.)	Non-lin. DiD (no cov.)	Non-lin.DiD (cov.)
Treatment*After	**0.145***	**0.136+**	**0.122***	**0.115***
	(0.070)	**(0.071)**	**(0.057)**	**(0.058)**
Treatment	-0.141***	-0.131***		
	(0.026)	(0.027)		
After	-0.224***	-0.215**		
	(0.067)	(0.068)		
Share: FT emp. 2019 (%)		0.000		
		(0.001)		
Share: female emp. 2019 (%)		-0.000		
		(0.001)		
Median wages FT emp. 2019		0.001+		
		(0.000)		
Unemployment rate 2019		-0.001		
		(0.004)		
GDP 2019		-0.000*		
		(0.000)		
Share: STW		0.009		
		(0.053)		
After # Share: FT emp. 2019 (%)		0.001		
		(0.001)		
After # Share: female emp. 2019 (%)		0.002		
		(0.001)		
After # Median wages FT emp. 2019		0.000		
		(0.001)		
After # Unemployment rate 2019		0.007		
		(0.009)		
After # GDP 2019		0.000		
		(0.000)		
After # Share: STW		-0.255*		
		(0.124)		
Constant	0.586***	0.575***		
	(0.024)	(0.026)		
Observations	3968	3968	3968	3968

Estimations show cluster-robust standard errors in parentheses. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

At first glance, these results seem a bit confusing, as a higher probability to maintain the level of new apprentices should translate into higher average values in terms of apprentice numbers. One possible explanation is that the subsidy initiated behavioural changes among firms that were at the margin of maintaining the average level of the past years. For example, assume three firms A, B and C had an average of 10 apprentices in the three years before the COVID-19 pandemic. Further assume that in the hypothetical case without treatment, firm A would have stopped vocational education completely, whereas firm B would have reduced to 5 apprentices and firm C would have reduced to 9 apprentices. In that case, all three firms would

have received €2,000 per apprenticeship if they had instead maintained the level at 10 apprenticeships, i.e., a total benefit of €20,000. This implies that the subsidy set a very uneven incentive structure depending on the situation of the firm (i.e., the hypothetical number of apprentices without treatment). While all three firms would have received the absolute amount of money, firm C would have received this amount for hiring 1 additional apprentice compared to the scenario without treatment, whereas firm A would have had to hire 10 additional apprentices. The marginal benefit per additionally hired apprentice therefore equals €20,000 for firm C, €4,000 for firm B but only €2,000 for firm A. Considering the costs of apprenticeships, it seems likely that the subsidy therefore only created strong incentives for firms that were very close to hiring the same number of apprentices that they hired in the past three years anyway, even without the subsidy. This may explain why there is no effect on the total number of apprentices but on the binary indicator of maintaining the level of the past three years. In the outlined example, if only firm C reacted to the subsidy and hired one additional apprentice, one out of three firms would have switched from 0 to 1, which seems like a non-negligible share. At the same time, the average number of apprentices for all three firms would have increased only marginally. This might explain the results but is also important to consider when it comes to possible policy implications.

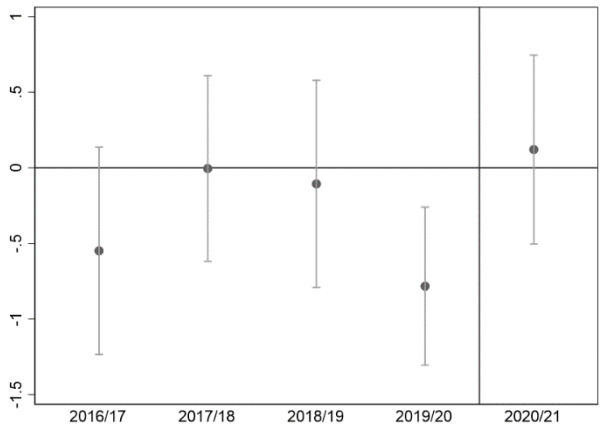
4.4.2 Robustness and sensitivity analyses

We now outline a series of robustness checks and sensitivity analyses to assess the validity of our results. Our main focus is the credibility of the identification strategy and its underlying assumptions. Specifically, one might question the common trends assumption as treatment status is determined by size, which might in turn be correlated with other factors that may cause diverging trends even without treatment. In addition to running both linear and non-linear models (see section 4.4.1), we assess the validity of our empirical approach in four ways.

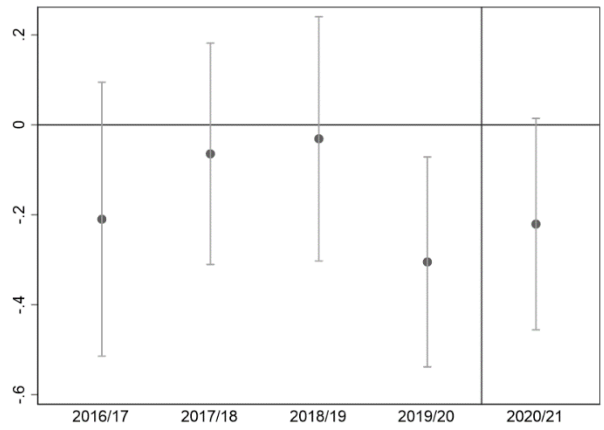
First, we conduct a standard placebo test in time to assess diverging trends before treatment. We start with the training year 2016/2017 as placebo treatment year and then move the treatment to the subsequent year from specification to specification (see Figure 4.2, regression outputs are shown in Tables A4.2-A4.5). We conduct the placebo test for both the continuous (Panel A and B) and binary outcome (Panel C and D), with linear and non-linear estimations (always including covariates). The placebo tests are mostly insignificant and do not show a clear pattern (they are sometimes positive, sometimes negative). The tests for the binary outcome are always insignificant (see Panel C and D), while the tests for the continuous outcome are mainly insignificant except for the year 2019/2020 (significantly negative). This suggests that there was a slight trend divergence to the advantage of the control group. Consequently, the true treatment effect might be slightly more positive than the ones estimated. The quite surprisingly negative effect in the non-linear DiD for the continuous outcome (see Panel B) therefore likely results from the slight trend divergence before treatment rather than an actual negative treatment effect. Relatedly, one might be concerned that the slight divergence of the outcome to the advantage of the control group might be a pull-forward effect that makes it harder to maintain the level of apprentices in subsequent years. That said, this argument would imply that the binary variable is negatively serially correlated by nature as maintaining or increasing the level of apprentices in one year makes it less likely to do so in subsequent years. However, such a negative serial correlation is not visible in the years before the treatment and placebo effects for the binary variable are insignificant for the linear and non-linear DiD model throughout the whole pre-treatment period (see Panel C and D). Finally, the binary indicator is not defined with regard to the past year but with respect to the average of the past three years, which is why the slight increase in the year before treatment start is less problematic. This refutes the outlined concern, though it has to be admitted that the slight increase in the control group might bias the effect for the binary variable upwards.

Figure 4.2: Placebo test in time, different specifications

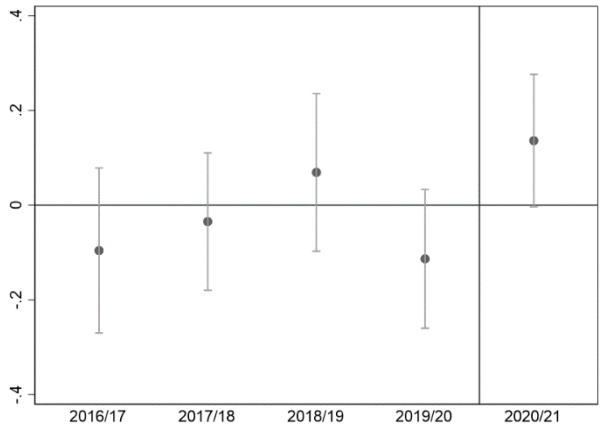
Panel A: Continuous outcome, linear estimation



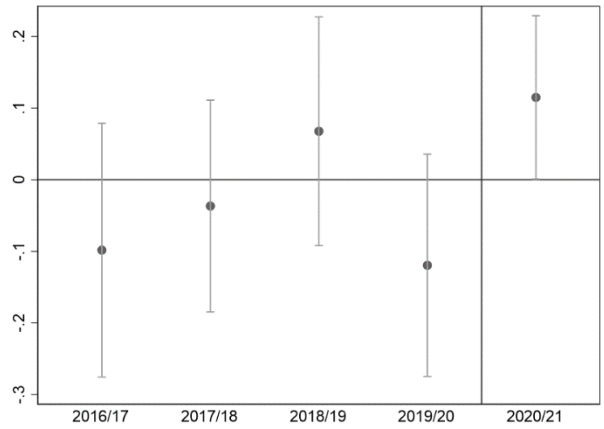
Panel B: Continuous outcome, non-linear estimation



Panel C: Binary outcome, linear estimation



Panel D: Binary outcome, non-linear estimation



Note: All shown estimations are based on specifications using covariates. The rightmost estimate shows the actual treatment effect.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.

Second, we conduct placebo tests based on a placebo outcome. To do so, we rely on the inflow from marginal part-time employees (mini jobbers). While this outcome may differ between treatment and control groups over time, it should be insensitive to the policy and thereby able to reveal any impacts that are not caused by the subsidy. Again, we conduct this test with linear and non-linear estimations and for the continuous and binary outcome. Results are outlined in Table A4.6. The placebo tests are all clearly insignificant.

Third, we employ a narrower sample definition. We re-define the threshold for inclusion in the sample to obtain treatment and control groups that are more similar in size (analogous to a tighter bandwidth in RDD analyses). While an actual RDD with firms very near the cut-off

point is not possible given the number of observations, employing an increasingly narrow sample definition can provide insights into whether and to what extent the results are sensitive to and driven by differences in firm size. To this end, we re-run the analyses and increase (decrease) the minimum (maximum) firm size to 100/500, 125/450 and 150/400. The results are displayed in Tables A4.7 to A4.10 in the appendix. For both outcomes, it becomes visible that the results barely change and, more importantly, the changes do not show a consistent pattern when the sample gets narrower (i.e., treatment effect estimates do not get consistently stronger or weaker for narrower sample definitions). For the continuous outcome (linear DiD, Table A4.7), the treatment effect changes from slightly positive to slightly negative and back again and remains insignificant throughout. For the non-linear DiD estimation (Table A4.8), the effect estimates get more negative at first but then drop below the point estimate of the baseline estimation. The treatment effect estimates for the binary outcome are displayed in Tables A4.9 (linear DiD) and A4.10 (non-linear DiD). For both estimation techniques, the estimated effect gets slightly weaker when narrowing the sample definition to 100/500 and 125/450, but gets even slightly stronger than the effect in the baseline estimation (0.158 compared to 0.136 in the linear DiD estimation) for the narrowest sample definition (150/400). Overall, these results show that changing the sample definition does not change the results systematically. This refutes concerns about differences in firm size driving the results.

Fourth, even though the placebo tests have not supported concerns regarding violations of the common trend assumption, one might still argue that results are driven by unobserved confounders, especially since firms with different characteristics might react differently to the COVID-19 shock. As always, this cannot be ruled out completely in a non-experimental context. To address this concern indirectly, we follow an approach suggested in the matching literature (Biewen et al., 2014) and assess how sensitive the results are to the inclusion of different sets of control variables. We therefore begin by just including firm characteristics (share of full-time employed workers, share of female employees and median wages). In the

second specification, we additionally include the share of short-term workers during COVID-19 in the respective sector to account for sector-specific differences in the affectedness by the pandemic. Finally, we re-run the analysis with firm-level and regional-level characteristics (regional unemployment rate and regional GDP). The results are displayed in Table A4.11. The treatment effect estimates appear to be very robust to different sets of control variables, the results barely change when different sets of control variables are included. This adds to the finding outlined in the previous section that has shown little differences between including no covariates at all or the complete set of covariates. Given that adding different sets of covariates makes little to no difference for the treatment effect estimates implies that it is unlikely that the results are driven by unobserved confounders.

In addition, we check for asymmetric attrition between treatment and control group. To do so, we regress the probability to be observed in the post-treatment period on the treatment status, once with and without covariates. The reasoning behind this is that while it is theoretically unlikely, it seems possible that the subsidy has prevented treated (but not control) firms from bankruptcy. As these would likely be less financially stable firms, this would entail a positive post-treatment selection to the advantage of the control group. The results (Table A4.12) show that there is no relation between treatment status and attrition. Moreover, we re-run the analysis and only include firms that have consistently been part of the sample from 2018 onwards. The results, which compare this balanced sample to the unbalanced sample of the baseline specification, are almost identical (see Table A4.13).

Overall, the results are very robust to different estimation methods (linear vs. non-linear DiD) and sample definitions and do not appear to be driven by panel attrition. Most importantly, a series of robustness checks refutes concerns about violations of the common trends assumption. Treatment and control group show neither (strongly) diverging trends with regard to the time before the treatment nor with respect to placebo outcomes in the post-treatment period. The results barely change when different sets of control variables are included or when

the sample is restricted to treatment and control firms with a more similar firm size. While violations of the common trends assumption can never be ruled out completely, the validity of our empirical approach is strongly supported by the outlined robustness and sensitivity analyses.

4.5 Discussion and conclusion

In sum, the results show an interesting yet somewhat unclear pattern. While there is no effect on the continuous outcome, treated firms were more likely to maintain their level of apprenticeships, as intended by the subsidy. While it is not entirely clear how this can be explained, one possible explanation is that firms at the margin of maintaining the level have been pushed towards hiring one or a small number of additional apprentices. This explanation is in line with the incentive structure of the subsidy, as the subsidy is paid for each apprenticeship. Therefore, hiring just slightly more apprentices than initially planned might seem attractive for some firms but a non-feasible endeavour for firms that would need to hire considerably more apprentices than originally intended. In addition, a non-negligible share of firms might have upheld the level of apprentices even without the treatment. These firms have captured gains from the subsidy, creating deadweight effects.

The policy-conclusions for these results are not clear cut. Nevertheless, at least two conclusions can be drawn. First, the results point to non-negligible deadweight effects. This implies that a certain amount of resources was spent to subsidize firms that were not in urgent need. Second, and more subtle, the uneven incentive structure of the subsidy can be interpreted as an implicit redistribution of resources towards firms that were not hit too hard by the COVID-19 crisis – namely, firms that did not have to reduce their training activities substantially – to the disadvantage of firms that were affected the most and had to reduce their training activities more strongly. At first glance, this does not seem to be a very optimistic conclusion regarding

the overall economic effect of the subsidy. At the same time, it has to be acknowledged that a substantive share of firms did in fact react to the subsidy (Bellmann et al., 2021; see also Statistik der Bundesagentur für Arbeit, 2022). Moreover, it should be noted that it is not easy to improve the incentive structure and avoid deadweight effects of such subsidies. This holds true especially with regard to the situation in the spring of 2020, when policy makers had to make fast decisions and the subsidy should not create overwhelming bureaucratic hurdles for firms or local authorities. Against this backdrop, the results do not suggest dismissing the idea of such subsidies completely. However, they highlight the incentive structure and distributive consequences embedded in the subsidy scheme, thereby enabling policy makers to make more informed judgments about whether such interventions are economically and socially desirable or not.

From a theoretical point of view, the results imply that the training behaviour of firms is somewhat sensitive to the costs of training, though to a limited extent, considering that the level of the subsidy is non-negligible. As past subsidies (often) did not set such a threshold, this result is difficult to compare to the results of previous studies. Therefore, the question whether it is the institutional setting or the macroeconomic environment that leads to the somewhat more optimistic result has to be left to future research.

Beyond that, this study is not without limitations and also points to new, unanswered research questions. One of the major limitations of this study is that we have no reliable information in the data on whether a certain firm has received the subsidy or not. This limits the precision with which deadweight effects and treatment effects can be disentangled. At the same time, it has to be considered that the share of subsidy-receiving firms in the population was non-negligible and we are using a representative sample of firms. Therefore, the absence of a treatment effect on the continuous outcome cannot result from a scenario in which the subsidy was simply not received by any firm.

Another aspect concerns external validity. Even though COVID-19 was a severe shock for the economy, the dual education system in Germany has increasingly struggled with unfilled places and a lack of applicants. If apprentice supply constraints are a major bottle neck in the dual education system, subsidies for creating additional places may be less effective compared to contexts that are characterized by a lack of apprenticeship places and an over-supply of potential apprentices. At the same time, the supply and demand situation strongly varies between regions and professions, with over-supply in some and under-supply in other areas (see e.g. the article on matching problems by Dummert et al. (2019)); so this would not make us expect that there is no effect at all. It remains subject to future research whether effects differ in other macroeconomic contexts. Relatedly, we have focused on a subset of firms (treated firms between 50 and 250 employees) that does not constitute a representative sample of all firms. It is possible that effects are different for smaller firms as they may differ in further characteristics compared to medium-sized firms. However, a comparison of our treatment group to a representative sample of all training firms (from the BP) shows surprisingly small differences regarding median wages or regional macroeconomic conditions and moderate differences in industrial composition (see Table A4.14). Nevertheless, it cannot be ruled out that the effect may be different for smaller firms.

With regard to future research, the long-term impact of the subsidy, e.g., on the number of completed apprenticeships or on employee turnover remains to be analysed. Even if firms have maintained their level of training more often to become eligible to the subsidy, it is also possible that firms have hired more apprentices as cheap labour or have become less selective in the hiring process (for a similar argument see Fries et al., 2014; Bonin et al., 2013 (section 5.3, p.86ff.)). Whether this is the case has to be determined by long-term analyses on whether the number of completed apprenticeships in the subsequent years was maintained.

Overall, this article has taken an initial step toward exploring the potential role of training subsidies as a stabilizer during times of crisis. While the results are not fully conclusive, the

significance of the research question warrants further investigation. Future studies should continue to examine the potential of training subsidies to function as an automatic stabilizer in periods of economic downturn, while carefully considering the incentive structure embedded in the policy design.

4.6 References

Albæk K (2009) The Danish Apprenticeship System, 1931 – 2002: The Role of Subsidies and Institutions. *Applied Economics Quarterly* 55(1): 39–60.

Bächmann A-C, Bellmann L, Gensicke M, Kohaut S, Möller I, Schwengler B, et al. (2023a) *IAB-Betriebspanel (IAB BP) – Version 9322 v1*. 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.

Becker GS (1962) Investment in Human Capital: A Theoretical Analysis. *Journal of Political Economy* 70(5): 9–49.

Bellmann L, Fitzenberger B, Gleiser P, Kagerl C, Kleifgen E, Koch T, et al. (2021) *Die Mehrheit der förderberechtigten Betriebe wird das Bundesprogramm „Ausbildungsplätze sichern" nutzen.* IAB-Forum. Nürnberg: Institut für Arbeitsmarkt- und Berufsforschung (IAB). Available from: https://iab-forum.de/die-mehrheit-der-foerderberechtigten-betriebe-wird-das-bundesprogramm-ausbildungsplaetze-sichern-nutzen/ (Accessed 8 October 2025).

Bellmann L, Fitzenberger B, Gleiser P, Kagerl C, Koch T, König C, et al. (2020) *Betriebliche Berufsausbildung in der Covid-19-Krise: Viele Betriebe kennen das Bundesprogramm „Ausbildungsplätze sichern" noch nicht: Serie „Corona-Krise: Folgen für den Arbeitsmarkt".* IAB-Forum. Nürnberg: Institut für Arbeitsmarkt- und Berufsforschung (IAB). Available from: https://iab-forum.de/ausbildungsfoerderung-in-der-covid-19-krise-viele-betriebe-kennen-das-bundesprogramm-ausbildungsplaetze-sichern-nicht/ (Accessed 7 October 2025).

Bellmann L, Gerner H-D, and Leber U (2014) Firm-Provided Training During the Great Recession. *Jahrbücher für Nationalökonomie und Statistik* 234(1): 5–22.

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/dienst/publikationen/de/16673 (Accessed 4 August 2026).

Biewen M, Fitzenberger B, Osikominu A, and Paul M (2014) The Effectiveness of Public-Sponsored Training Revisited: The Importance of Data and Methodological Choices. *Journal of Labor Economics* 32(4): 837–897.

BMAS/BMBF (2020) *Erste Förderrichtlinie für das Bundesprogramm „Ausbildungsplätze sichern“: Bekanntmachung*. BAnz AT 31.07.2020 B1.

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).

Bonin H, Fries J, Hillerich A, Maier MF, Walter T, Neureiter M, et al. (2013) *Begleitforschung „Auswirkungen des Ausbildungsbonus auf den Ausbildungsmarkt und die öffentlichen Haushalte“*. Abschlussbericht. Forschungsbericht Arbeitsmarkt, No. 438. Mannheim.

Boockmann B, Zwick T, Ammermüller A, and Maier M (2012) Do Hiring Subsidies Reduce Unemployment Among Older Workers? Evidence from Natural Experiments. *Journal of the European Economic Association* 10(4): 735–764.

Bruhn M (2020) Can Wage Subsidies Boost Employment in the Wake of an Economic Crisis? Evidence from Mexico. *The Journal of Development Studies* 56(8): 1558–1577.

Brunello G (2009) The effect of economic downturns on apprenticeships and initial workplace training: a review of the evidence. *Empirical Research in Vocational Education and Training* 1(2): 145–171.

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).

Caves KM, McDonald P (2023) Determinants and consequences of employer-provided training program resilience post-Covid-19. *Empirical Research in Vocational Education and Training* 15, 7: 1–21.

Deutschlandfunk (2020) Coronakrise: 3.000-Euro-Ausbildungsprämie erntet Lob und Kritik. 24 June. Author(s): Dierks, B., Brinkmann, R., Haas, A. Available from: https://www.deutschlandfunk.de/coronakrise-3-000-euro-ausbildungspraemie-erntet-lob-und-100.html (Accessed 8 October 2025).

Dummert S, Leber U, and Schwengler B (2019) Unfilled Training Positions in Germany – Regional and Establishment-Specific Determinants. *Journal of Economics and Statistics* 239(4): 661–701.

Dummert S, Umkehrer M (2025) The effects of the COVID-19 pandemic on vocational education and training in Germany. *Journal of Vocational Education & Training* 77(3): 631–654.

Fassbender U (2022) Collective Skill Formation Regimes in Times of Covid-19: A Governance-Focused Analysis of the German Dual Training System. *International Journal for Research in Vocational Education and Training* 9(2): 167–194.

Feng T, Xue Z (2023) The impact of government subsidies on corporate resilience: evidence from the COVID-19 shock. *Economic Change and Restructuring* 56: 4199–4221.

Fries J, Göbel C, and Maier MF (2014) Do employment subsidies reduce early apprenticeship dropout? *Journal of Vocational Education & Training* 66(4): 433–461.

Gambin L, Hogarth T (2021) Employers' behavioural responses to the introduction of an apprenticeship levy in England: an ex ante assessment. *Journal of Vocational Education & Training* 73(3): 476–501.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023a) *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.

Ganzer A, Schmucker A, Stegmaier J, and Wolter S (2023b) *Betriebs-Historik-Panel 1975-2022 (BHP 7522 v1)*. Forschungsdatenzentrum 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.

Hérault N, Zakirova R (2015) Returns to education: accounting for enrolment and completion effects. *Education Economics* 23(1): 84–100.

Imbens GW, Rubin DB (2015) *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. Cambridge University Press.

Kuczera M (2017) *Incentives for apprenticeship.* OECD Education Working Papers, No. 152: OECD. Available from: https://dx.doi.org/10.1787/55bb556d-en (Accessed 7 October 2025).

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.

Morlet GMA (2025) *The Effect of Subsidies on Training: Evidence from the United States.* SSRN Preprint. Available from: https://dx.doi.org/10.2139/ssrn.5152062 (Accessed 7 October 2025).

Muehlemann S, Pfeifer H, Walden G, Wenzelmann F, and Wolter SC (2010) The financing of apprenticeship training in the light of labor market regulations. *Labour Economics* 17(5): 799–809.

Muehlemann S, Pfeifer H, and Wittek BH (2020) The effect of business cycle expectations on the German apprenticeship market: estimating the impact of Covid-19. *Empirical Research in Vocational Education and Training* 12, 8: 1–30.

Muehlemann S, Schweri J, Winkelmann R, and Wolter SC (2007) An Empirical Analysis of the Decision to Train Apprentices. *Labour* 21(3): 419–441.

Muehlemann S, Wolter SC (2014) Return on investment of apprenticeship systems for enterprises: Evidence from cost-benefit analyses. *IZA Journal of Labor Policy* 3, 25: 1–22.

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.

Ramirez JV, Weber S (2025) What kind of support do firms prefer to hire apprentices in a recession? *Empirical Research in Vocational Education and Training* 17, 5: 1–24.

Riphahn RT, Zibrowius M (2016) Apprenticeship, vocational training, and early labor market outcomes – evidence from East and West Germany. *Education Economics* 24(1): 33–57.

Roth J, Sant'Anna PH, Bilinski A, and Poe J (2023) What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics* 235(2): 2218–2244.

Schünemann B, Lechner M, and Wunsch C (2015) Do Long-Term Unemployed Workers Benefit from Targeted Wage Subsidies? *German Economic Review* 16(1): 43–64.

Schuß E (2020) *Finanzielle Anreize zur Ausbildung in der Altenpflege*. Der Effekt der Ausbildungsumlage auf das Ausbildungsverhalten von Pflegeeinrichtungen. BIBB-Preprint. Bonn: BIBB. Available from: https://datapool-bibb.bibb.de/htmlpages/185620.html (Accessed 8 October 2025).

Smith E (2024) Rescue and recovery: The COVID and post-COVID responses of apprenticeship systems. *International Review of Applied Economics* 38(3): 337–356.

Statistik der Bundesagentur für Arbeit (2022) *Bundesprogramm „Ausbildungsplätze sichern"*. Berichte: Arbeitsmarkt kompakt, November 2022. Hannover.

Statistik der Bundesagentur für Arbeit (2025) *Kurzarbeiterquote: Ausgewählte Berichtsmonate, Datenstand: März 2025*. Auftragsnummer 388869. Nürnberg. Available from: https://statistik.arbeitsagentur.de.

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).

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.

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.

Wooldridge JM (2023) Simple approaches to nonlinear difference-in-differences with panel data. *The Econometrics Journal* 26(3): C31-C66.

4.7 Appendix

Table A4.1: Summary descriptives by treatment status (survey year 2020)

Variable	Treated	N	Mean	SD	Min.	Max.
Inflow - apprentices	0	69	7.420	4.199	0.000	20.000
Inflow - apprentices	1	603	2.012	2.578	0.000	20.000
Inflow - mini jobbers	0	69	1.710	4.095	0.000	25.000
Inflow - mini jobbers	1	603	1.789	4.456	0.000	41.000
Share: FT emp. 2019 (%)	0	69	82.573	17.584	14.953	98.006
Share: FT emp. 2019 (%)	1	603	71.387	23.639	0.803	100.000
Share: female emp. 2019 (%)	0	69	23.933	20.923	3.704	89.218
Share: female emp. 2019 (%)	1	603	34.369	23.982	1.786	100.000
Median wages FT emp. 2019	0	69	115.964	27.746	61.450	188.070
Median wages FT emp. 2019	1	603	97.021	27.927	38.530	238.360
Unemployment rate 2019	0	69	4.128	2.199	1.600	12.200
Unemployment rate 2019	1	603	5.245	2.233	1.600	12.200
GDP 2019	0	69	41670.957	18195.436	22794.000	116466.000
GDP 2019	1	603	38537.570	15237.597	21444.000	195809.000
Share: STW	0	69	0.323	0.166	0.022	0.664
Share: STW	1	603	0.281	0.166	0.010	0.731

Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.

Table A4.2: Placebo in time (continuous outcome, linear estimation)

	2017	2018	2019	2020	2021
Treatment*After(Placebo)	-0.549	-0.005	-0.106	-0.783**	0.121
	(0.349)	(0.313)	(0.349)	(0.266)	(0.318)
Treatment	-4.043***	-4.317***	-4.320***	-4.346***	-4.504***
	(0.495)	(0.471)	(0.447)	(0.447)	(0.446)
After(Placebo)	0.484	-0.072	0.169	0.648*	-0.387
	(0.341)	(0.315)	(0.347)	(0.259)	(0.320)
Share: FT emp. 2019 (%)	-0.017*	-0.018*	-0.021**	-0.023***	-0.021***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Share: female emp. 2019 (%)	-0.011+	-0.010+	-0.014*	-0.014**	-0.013*
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Median wages FT emp. 2019	0.011*	0.014**	0.012**	0.013**	0.013***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Unemployment rate 2019	-0.017	0.004	-0.007	-0.005	-0.001
	(0.055)	(0.051)	(0.046)	(0.047)	(0.046)
GDP 2019	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Share: STW	1.551*	1.846**	2.021**	1.993**	2.011**
	(0.737)	(0.705)	(0.658)	(0.661)	(0.653)
After(Placebo) # Share: FT emp. 2019 (%)	-0.003	-0.008	-0.008	0.010+	0.001
	(0.004)	(0.008)	(0.006)	(0.005)	(0.007)
After(Placebo) # Share: female emp. 2019 (%)	0.000	-0.009+	-0.003	0.007+	0.009*
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
After(Placebo) # Median wages FT emp. 2019	0.005*	-0.004	0.001	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
After(Placebo) # Unemployment rate 2019	0.042	-0.033	0.008	0.023	0.057
	(0.035)	(0.042)	(0.037)	(0.036)	(0.040)
After(Placebo) # GDP 2019	0.000	-0.000	-0.000	-0.000+	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
After(Placebo) # Share: STW	0.588	0.506	-0.106	0.083	-1.631**
	(0.501)	(0.531)	(0.500)	(0.463)	(0.529)
Constant	6.232***	6.473***	6.451***	6.493***	6.623***
	(0.481)	(0.459)	(0.436)	(0.436)	(0.435)
Observations	1295	1954	2617	3289	3968

Estimations show cluster-robust standard errors in parentheses. Note: All shown estimations are based on specifications using covariates. The rightmost estimate shows the actual treatment effect. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.3: Placebo in time (continuous outcome, non-linear estimation)

	2017	2018	2019	2020	2021
Treatment*After(Placebo)	-0.210	-0.064	-0.031	-0.305*	-0.221+
	(0.155)	(0.126)	(0.139)	(0.119)	(0.120)
Observations	1295	1954	2617	3289	3968

Estimations show cluster-robust standard errors in parentheses. Note: All shown estimations are based on specifications using covariates. The rightmost estimate shows the actual treatment effect.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.4: Placebo in time (binary outcome, linear estimation)

	2017	2018	2019	2020	2021
Treatment*After(Placebo)	-0.096	-0.035	0.069	-0.114	0.136+
	(0.089)	(0.074)	(0.085)	(0.075)	(0.071)
Treatment	-0.065	-0.113*	-0.125**	-0.108**	-0.131***
	(0.067)	(0.049)	(0.042)	(0.033)	(0.027)
After(Placebo)	0.068	0.018	-0.058	0.091	-0.215**
	(0.084)	(0.069)	(0.082)	(0.071)	(0.068)
Share: FT emp. 2019 (%)	0.003*	0.001+	0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Share: female emp. 2019 (%)	0.001	0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Median wages FT emp. 2019	0.000	0.001+	0.001	0.001	0.001+
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Unemployment rate 2019	-0.009	-0.003	-0.005	-0.003	-0.001
	(0.009)	(0.007)	(0.006)	(0.005)	(0.004)
GDP 2019	0.000	0.000	-0.000	-0.000+	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Share: STW	-0.147	-0.105	0.000	0.022	0.009
	(0.122)	(0.090)	(0.073)	(0.060)	(0.053)
After(Placebo) # Share: FT emp. 2019 (%)	-0.002	-0.003*	-0.002	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
After(Placebo) # Share: female emp. 2019 (%)	-0.001	-0.002+	0.001	0.000	0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
After(Placebo) # Median wages FT emp. 2019	0.002+	-0.001	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
After(Placebo) # Unemployment rate 2019	0.012	-0.004	0.006	0.008	0.007
	(0.012)	(0.011)	(0.011)	(0.010)	(0.009)
After(Placebo) # GDP 2019	-0.000	-0.000+	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
After(Placebo) # Share: STW	0.085	0.304*	0.085	-0.067	-0.255*
	(0.164)	(0.148)	(0.146)	(0.133)	(0.124)
Constant	0.531***	0.565***	0.572***	0.557***	0.575***
	(0.064)	(0.046)	(0.040)	(0.031)	(0.026)
Observations	1295	1954	2617	3289	3968

Estimations show cluster-robust standard errors in parentheses. Note: All shown estimations are based on specifications using covariates. The rightmost estimate shows the actual treatment effect. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.5: Placebo in time (binary outcome, non-linear estimation)

	2017	2018	2019	2020	2021
Treatment*After(Placebo)	-0.098	-0.037	0.068	-0.120	0.115*
	(0.090)	(0.075)	(0.081)	(0.079)	(0.058)
Observations	1295	1954	2617	3289	3968

Estimations show cluster-robust standard errors in parentheses. Note: All shown estimations are based on specifications using covariates. The rightmost estimate shows the actual treatment effect.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.6: Placebo outcome (mini jobbers)

	Lin. DiD (continuous Y)	Lin. DiD (binary Y)	Non-lin. DiD (continuous Y)	Non-lin. DiD (binary Y)
Treatment*After	0.451	-0.049	0.250	-0.052
	(0.596)	(0.061)	(0.459)	(0.064)
Treatment	-2.184**	-0.000		
	(0.703)	(0.026)		
After	-0.936	0.037		
	(0.615)	(0.059)		
Share: FT emp. 2019 (%)	-0.169***	-0.001**		
	(0.027)	(0.001)		
Share: female emp. 2019 (%)	-0.035*	0.001		
	(0.015)	(0.001)		
Median wages FT emp. 2019	-0.028***	-0.001*		
	(0.007)	(0.000)		
Unemployment rate 2019	0.081	-0.010*		
	(0.132)	(0.004)		
GDP 2019	0.000	0.000		
	(0.000)	(0.000)		
Share: STW	1.346	-0.083+		
	-1.562	(0.050)		
After # Share: FT emp. 2019 (%)	0.043*	0.001		
	(0.019)	(0.001)		
After # Share: female emp. 2019 (%)	0.009	-0.000		
	(0.012)	(0.001)		
After # Median wages FT emp. 2019	0.004	-0.002**		
	(0.006)	(0.001)		
After # Unemployment rate 2019	-0.073	-0.012		
	(0.094)	(0.008)		
After # GDP 2019	-0.000+	0.000		
	(0.000)	(0.000)		
After # Share: STW	-2.651*	-0.079		
	-1.237	(0.112)		
Constant	5.175***	0.363***		
	(0.751)	(0.025)		
Observations	3968	3968	3968	3968

Linear estimations show t-statistics in parentheses. Non-linear estimations show z-statistics in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.7: Linear DiD for continuous outcome, applying different firm size thresholds

	50/600	100/500	125/450	150/400
Treatment*After	0.121	-0.291	-0.288	0.281
	(0.318)	(0.357)	(0.386)	(0.378)
Observations	3968	2152	1655	1127

Estimations show cluster-robust standard errors in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.8: Non-linear DiD for continuous outcome, applying different firm size thresholds

	50/600	100/500	125/450	150/400
Treatment*After	-0.221+	-0.450**	-0.453*	-0.140
	(0.120)	(0.174)	(0.205)	(0.226)
Observations	3968	2152	1655	1127

Estimations show cluster-robust standard errors in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.9: Linear DiD for binary outcome, applying different firm size thresholds

	50/600	100/500	125/450	150/400
Treatment*After	0.136+	0.097	0.080	0.158
	(0.071)	(0.080)	(0.087)	(0.099)
Observations	3968	2152	1655	1127

Estimations show cluster-robust standard errors in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.10: Non-linear DiD for binary outcome, applying different firm size thresholds

	50/600	100/500	125/450	150/400
Treatment*After	0.115*	0.077	0.059	0.121
	(0.058)	(0.067)	(0.072)	(0.077)
Observations	3968	2152	1655	1127

Estimations show cluster-robust standard errors in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.11: Inclusion of different sets of covariates (using linear DiD)

	Y: continuous; firm covariates	Y: continuous; firm + sector covariates	Y: continuous; firm + regional covariates	Y: binary; firm covariates	Y: binary; firm + sector covariates	Y: binary; firm + regional covariates
Treatment*After	0.235	0.169	0.168	0.147*	0.137+	0.143*
	(0.327)	(0.318)	(0.327)	(0.072)	(0.071)	(0.072)
Treatment	-4.587***	-4.510***	-4.561***	-0.129***	-0.128***	-0.131***
	(0.458)	(0.451)	(0.451)	(0.027)	(0.027)	(0.027)
After	-0.501	-0.435	-0.432	-0.226***	-0.215**	-0.222**
	(0.325)	(0.315)	(0.329)	(0.068)	(0.068)	(0.069)
Share: FT emp. 2019 (%)	-0.019**	-0.021***	-0.018**	0.000	0.000	0.000
	(0.006)	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)
Share: female emp. 2019 (%)	-0.012*	-0.013*	-0.012*	-0.000	-0.000	-0.000
	(0.005)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
Median wages FT emp. 2019	0.013***	0.014***	0.012**	0.000	0.000	0.001+
	(0.004)	(0.004)	(0.004)	(0.000)	(0.000)	(0.000)
After # Share: FT emp. 2019 (%)	-0.000	0.002	-0.001	0.001	0.001	0.001
	(0.006)	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)
After # Share: female emp. 2019 (%)	0.009*	0.010**	0.009*	0.002	0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)
After # Median wages FT emp. 2019	0.002	0.001	0.004	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Share: STW		2.009**			0.014	
		(0.633)			(0.053)	
After # Share: STW		-1.753***			-0.273*	
		(0.506)			(0.122)	
Unemployment rate 2019			-0.025			-0.001
			(0.045)			(0.004)
GDP 2019			0.000			-0.000*
			(0.000)			(0.000)
After # Unemployment rate 2019			0.077*			0.010
			(0.038)			(0.009)
After # GDP 2019			-0.000			0.000+
			(0.000)			(0.000)
Constant	6.705***	6.629***	6.680***	0.574***	0.573***	0.576***
	(0.448)	(0.440)	(0.441)	(0.025)	(0.026)	(0.026)
Observations	3968	3968	3968	3968	3968	3968

Estimations show cluster-robust standard errors in parentheses. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.12: Selection/Attrition

	Firm exit (no cov.)	Firm exit (cov.)
Treatment	-0.006	-0.006
	(0.015)	(0.015)
Share: FT emp. 2019 (%)		-0.000
		(0.000)
Share: female emp. 2019 (%)		0.000
		(0.000)
Median wages FT emp. 2019		-0.000
		(0.000)
Unemployment rate 2019		-0.003*
		(0.002)
GDP 2019		-0.000
		(0.000)
Share: STW		0.029
		(0.034)
Constant	0.014	0.043
	(0.014)	(0.029)
Observations	682	682

Estimations show cluster-robust standard errors in parentheses.
Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.13: Using a balanced versus unbalanced panel

	Y: continuous; balanced	Y: continuous; unbalanced	Y: binary; balanced	Y: binary; unbalanced
Treatment*After	0.066	0.121	0.127+	0.136+
	(0.316)	(0.318)	(0.072)	(0.071)
Observations	3858	3968	3858	3968

Estimations show cluster-robust standard errors in parentheses. Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A4.14: Firm characteristics – subsample versus all firms (survey year 2020)

Variable	Treatment group (TG)	Mean (TG)	Min. (TG)	Max. (TG)	All firms (AF)	Mean (AF)	Min. (AF)	Max. (AF)
Share: FT emp. 2019 (%)	603	71.387	0.803	100.000	15052	52.790	0.000	100.000
Share: female emp. 2019 (%)	603	34.369	1.786	100.000	15052	48.093	0.000	100.000
Median wages FT emp. 2019	603	97.021	38.530	238.360	12721	98.004	3.550	351.940
Unemployment rate 2019	603	5.245	1.600	12.200	15159	5.649	1.300	12.800
GDP 2019	603	38537.570	21444.000	195809.000	15159	38935.891	16605.000	195809.000
Share: STW	603	0.281	0.010	0.731	14200	0.206	0.006	0.756
Number of emp. 2019	603	115.620	50.000	323.000	15107	74.326	0.000	44688.000
Industry								
Agriculture + mining, quarrying, electricity, water + other manufacturing	603	0.169	0.000	1.000	14307	0.121	0.000	1.000
Manufacture of capital and consumer goods	603	0.342	0.000	1.000	14307	0.132	0.000	1.000
Construction	603	0.033	0.000	1.000	14307	0.068	0.000	1.000
Wholesale, maintenance and repair of motor vehicles, motor	603	0.073	0.000	1.000	14307	0.058	0.000	1.000
Retail	603	0.035	0.000	1.000	14307	0.068	0.000	1.000
Transport, storage	603	0.061	0.000	1.000	14307	0.044	0.000	1.000
Information, communication + education	603	0.056	0.000	1.000	14307	0.063	0.000	1.000
Hotels and restaurants	603	0.041	0.000	1.000	14307	0.042	0.000	1.000
Liberal professions	603	0.086	0.000	1.000	14307	0.147	0.000	1.000
Health and social work	603	0.060	0.000	1.000	14307	0.129	0.000	1.000
Other industries and services	603	0.043	0.000	1.000	14307	0.128	0.000	1.000

Source: IAB-BP + BHP (incl. additional regional data & BA data). Calculations by authors.

DATA AVAILABILITY STATEMENT

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.