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Unintended Transitions: Employment Outcomes Following Firm Closures During Vocational Education and Training

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Contents

Abstract

Dual vocational education and training (VET) plays a central role in the school-to-work transition in Germany, yet a substantial share of training contracts is terminated prematurely. Estimating the causal effects of such disruptions is challenging due to endogenous selection into dropout. This study exploits training firm closures as unexpected shocks that displace apprentices during VET. Using administrative data on the universe of German apprentices between 2008 and 2022, a matched event study design estimates the impact of displacement on employment outcomes over the subsequent eight years. The results show that displacement during VET leads to small but persistent earnings losses. Relative to non-displaced apprentices, earnings from regular employment fall by up to 780€ per year (about 4.5 percent) four years after displacement and by roughly 2 percent in later years. These losses are modest compared to displacement from regular employment, consistent with strong institutional support that helps affected apprentices to continue with a new training firm and complete VET. However, apprentices without a school degree experience substantially larger and increasing earnings losses. A decomposition analysis indicates that the overall earnings losses are largely driven by substantial penalties among apprentices who drop out after displacement. In contrast, those who complete VET experience only small earnings losses, and displacement increases the risk of dropout only marginally. Overall, the findings highlight the protective role of institutional arrangements in the German VET system, while also showing that not all apprentices access or benefit from these supports equally.

Zusammenfassung

Die duale Berufsausbildung spielt eine zentrale Rolle beim Übergang von der Schule in den Arbeitsmarkt in Deutschland. Dennoch wird ein erheblicher Anteil der Ausbildungsverträge vorzeitig gelöst. Die Schätzung der kausalen Effekte solcher Ausbildungsabbrüche ist schwierig, da die Entscheidung zum Abbruch endogen ist. Diese Studie nutzt Betriebsschließungen von Ausbildungsbetrieben als unerwartete Schocks, die Auszubildende während ihrer Ausbildung verdrängen. Auf Basis administrativer Daten aller Auszubildenden in Deutschland zwischen 2008 und 2022 wird mithilfe eine Ereignisstudie mit einer gematchten Kontrollgruppe der Einfluss des Verlusts des Ausbildungsplatzes auf Beschäftigungs- und Einkommensverläufe über die folgenden acht Jahre untersucht. Die Ergebnisse zeigen, dass der Verlust des Ausbildungsplatzes zu kleinen, aber anhaltenden Einkommensverlusten führt. Im Vergleich zu nicht betroffenen Auszubildenden sinken die Einkommen aus regulärer Beschäftigung vier Jahre später um bis zu 780 Euro pro Jahr (etwa 4,5 Prozent) und liegen auch in den Folgejahren noch rund 2 Prozent niedriger. Diese

Verluste fallen im Vergleich zu Verlusten einer regulären Beschäftigung moderat aus. Dies steht im Einklang mit der starken institutionellen Unterstützung im deutschen Berufsbildungssystem, die betroffenen Auszubildenden hilft, ihre Ausbildung in einem neuen Betrieb fortzusetzen und erfolgreich abzuschließen. Allerdings verzeichnen Auszubildende ohne Schulabschluss deutlich größere und im Zeitverlauf zunehmende Einkommensverluste. Eine Dekompositionsanalyse zeigt, dass die gesamten Einkommensverluste zu einem großen Teil von denjenigen Auszubildenden verursacht werden, die ihre Ausbildung im Anschluss abbrechen. Demgegenüber erfahren Personen, die ihre Ausbildung abschließen, nur geringe Einkommenseinbußen, und der Verlust des Ausbildungsplatzes erhöht das Risiko eines Ausbildungsabbruchs nur geringfügig. Insgesamt zeigen die Ergebnisse die wichtige unterstützende Rolle verschiedener Institutionen im dualen Berufsbildungssystem, der zuständigen Kammern und der Berufsberatung der Bundesagentur für Arbeit. Zugleich wird deutlich, dass nicht alle Auszubildenden gleichermaßen Zugang zu diesen Unterstützungsmechanismen haben oder in gleichem Maße von ihnen profitieren.

JEL

J24, J63, M53

Keywords

Displacement, Firm Closure, Vocational Training

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1 Introduction

A smooth transition from school into the labor market is widely recognized as a key step in career development and long-term success (e.g., Blokker et al., 2023; De Vos/Van der Heijden/Akkermans, 2020; Ryan, 2001). Early unemployment can have persistent negative effects on labor market outcomes over the life course (Schmillen/Umkehrer, 2017; Gregg, 2008; Luijkx/Wolbers, 2009). Across OECD countries, completing upper secondary education is regarded as an important prerequisite for a successful school-to-work transition (OECD, 2023), as it lowers the risk of early unemployment and shapes later labor market trajectories.

In many European countries, including Germany, Austria, Switzerland, and Denmark (Patzina/Wydra-Somaggio, 2020), dual vocational education and training (VET) plays a central role in the transition from compulsory schooling to skilled work. In Germany, about half of all young adults enter the dual VET system (Dohmen/Bayreuther/Sandau, 2023 for the years 2002–2021), which is considered to support smooth labor market integration (Eichhorst et al., 2015; Parey, 2016). Most apprentices remain with their training firms after graduation, with retention rates reaching 79% in 2024 (Fitzenberger/Leber/Schwengler, 2025). For Switzerland, Oswald-Egg/Renold (2021) show that VET graduates have shorter job-search durations at labor market entry than graduates from other educational pathways. Dual VET therefore functions as an effective *safety net* that lowers the risk of early unemployment (Saar/Martma, 2021).

Despite rising educational attainment across OECD countries, about every fourth young adult still leaves the education system without an upper secondary qualification(OECD (2021)). In Germany, the share of individuals below age 25 without a completed vocational degree has risen to 17% (Kalinowski (2024)). Within the dual VET system, premature training contract terminations have also become more frequent, and reach 30% in 2023 (Bundesinstitut für Berufsbildung (BIBB) [Federal Institute for Vocational Education and Training] (2025)). About one third of affected apprentices exit the system without obtaining a VET degree (Wydra-Somaggio (2021)). Across educational systems, dropout processes differ in their underlying causes and can broadly be classified as voluntary or involuntary. In Germany, voluntary VET dropouts tend to be negatively selected with respect to performance, ability, and socioeconomic background (Böhn/Deutscher (2022); Krötz/Deutscher (2022)), which makes it difficult to disentangle selection from causal effects. While apprentices may leave their training relationship at any point during VET, involuntary VET dropouts caused by firms are only permitted in the event of an establishment closure. When a training firm closes, affected apprentices receive institutional support in their search for a new training placement, mainly through the

Federal Employment Agency and the responsible chambers that supervise VET and organize final examinations (see the *Vocational Education and Training Act (BBiG)*).

Against this background, the present study follows apprentices who experience displacement during VET due to firm closures and addresses the following research questions. How does displacement affect early career labor market outcomes? Which mechanisms drive the observed earnings losses, and in particular, how important is the increased risk of leaving VET without a degree? Does the timing of displacement matter, and are certain sociodemographic groups more strongly affected than others?

From a job search perspective, displaced apprentices choose between new VET positions, regular employment, and unemployment by comparing expected utilities (cf. Mortensen (1970); McCall (1970); Pissarides (2000)). Compared with apprentices who remain in their original training firm, displacement substantially lowers the value of the outside option. While non-displaced apprentices continue to receive a stable training wage and face a high likelihood of completing a recognized qualification, displaced apprentices confront unemployment risks and a more uncertain early-career trajectory. As a result, they are more likely to accept less attractive offers, such as lower-paying training positions or positions in a different occupation, reducing match quality and the probability of successful training outcomes. In addition, displaced apprentices lose firm-specific human capital as well as access to professional networks and informal support structures. These disadvantages may translate into longer training durations, particularly for apprentices who switch occupations and may need to repeat vocational school classes or acquire new occupational skills. However, also within the same occupation, adjustment costs can arise if training firms differ in tools, production processes, and organizational practices. Taken together, poorer match quality and the loss of firm- and occupation-specific capital make it plausible that displaced apprentices need more time to complete VET and face a higher risk of premature dropout or failure in final examinations (cf. Krötz/Deutscher (2022); Böhn/Deutscher (2022)). Instead of continuing VET at a new firm, displaced apprentices may also take up regular employment. While these jobs often offer higher wages than apprenticeship positions, they hinder the graduation from VET and are therefore associated with higher risks of unstable employment and future unemployment. In the most adverse case, displaced apprentices may experience prolonged unemployment, which is particularly harmful early in the career (cf. Gregg (2008)). Overall, displacement during VET increases both the risk of long term unemployment and the probability of dropping out of vocational training.

Apprentices who drop out of VET leave the system without a formal degree and complete only part of the intended training, which implies fewer years of work experience and vocational schooling. According to Mincer's earnings framework, such reductions are associated with lower future earnings (cf. Mincer (1974)). From a human capital perspective, dropouts accumulate less general and firm-specific human capital than graduates (cf.

Becker (1993)). Credentialist approaches further emphasize that the absence of a formal certificate constitutes a negative signal (cf. Collins (1979); Brown (2001)). As a result, VET dropouts face a double disadvantage: lower human capital and the absence of a credential certifying their training. The magnitude of these disadvantages depends on the timing of dropout, with early dropouts falling further behind than those who leave shortly before graduation (cf. Patzina/Wydra-Somaggio (2020)). At the same time, displacement may also create opportunities for improvement: Searching for a new training firm can allow apprentices to move into better matches rather than remaining in poorly performing firms. Compared with displaced regular workers, displaced apprentices are embedded in a dense institutional support framework that facilitates continued participation in VET. During this period of vocational reorientation, apprentices may also reconsider occupational choices and move away from low-paying occupations. These short-term decisions can shape longer-term employment trajectories, leading to heterogeneous outcomes.

To capture these pathways, this study follows displaced apprentices for eight years after the displacement event. I use rich administrative data with daily employment histories for the full population of apprentices in the German dual VET system between 2008 and 2022. Displacement is identified through establishment closures, and a matched comparison group of observationally similar non-displaced apprentices is constructed using a matching approach.

Event-study difference-in-differences estimates reveal that displacement during VET leads to small but persistent earnings losses, peaking at about 4.5% four years after displacement and declining to roughly 2% in later years. These losses arise from both shorter employment durations and lower wages, with each component small in magnitude but persistent. I examine the role of various mechanisms using a decomposition of the difference-in-differences estimate. While graduation probabilities differ only slightly across groups, displaced graduates experience modest long-run earnings losses of less than 200€ per year. The largest share of the overall effect arises from displaced apprentices who drop out of VET and experience substantially lower earnings. This reflects immediate unemployment risks and involuntary acceptance of lower quality jobs. By contrast, voluntary dropouts in the control group are more likely to have planned subsequent transitions.

In addition to earnings, the analysis examines occupational and regional mobility. Displacement increases both forms of mobility, consistent with evidence for displaced regular workers (cf. Huttunen/Møen/Salvanes (2018); Fackler/Rippe (2017)). Occupational switches are associated with lower earnings (cf. Fitzenberger/Licklederer/Zwiener (2015)), while increasing regional mobility partly mitigates losses in the short run but not in the long run.

Despite the modest negative average effect on earnings, the estimates reveal substantial heterogeneity in treatment effects. Following Heckman/Smith/Clements (1997), I show that the variation in individual treatment effects is substantial. Apprentices with a high school degree exhibit displacement effects close to zero, indicating that a strong ability signal partly offsets the negative signal associated with the interruption, whereas apprentices without a school degree experience the largest earnings penalties. Timing also matters: early displacement leads to the largest earnings losses, consistent with elevated unemployment risks, while displacement in the second year generates the largest wage losses among those who remain employed. Later displacement is associated with smaller penalties, although these differences are not statistically significant.

This study contributes to various strands of the literature, the first being the literature on estimating the returns to education. A longstanding and influential literature in labor economics estimates the returns to additional schooling or college attendance across different institutional settings. (e.g., Angrist/Krueger (1991); Ashenfelter/Harmon/Oosterbeek (1999); Card (2001); Harmon/Oosterbeek/Walker (2003); Blundell et al. (1999)). Other studies examine the returns to different educational pathways (see Brébion (2019) and Bentolila/Cabrales/Jansen (2023) for dual vs. school-based VET in France and Spain, respectively) using instrumental variable strategies. For the German VET system, previous studies consistently document positive returns to completing VET relative to having no vocational qualification (cf. Riphahn/Zibrowius (2016); Krueger/Pischke (1995); Cooke (2003); Clark (2001)). However, this literature typically does not distinguish between individuals who never enter VET and those who start but do not complete it. In contrast, my study focuses on individuals who enter VET and explicitly differentiates between graduates and dropouts. While labor market entrants who never enter VET are beyond the scope of this analysis, this distinction allows a more precise assessment of the consequences of interrupted vocational training.

Second, this study contributes to the literature on the labor market consequences of educational dropouts and interruptions. Prior research documents persistent disadvantages for dropouts across education levels and countries (cf. Oreopoulos (2007); Rumberger/Lamb (2003); Hällsten (2017); Schnepf (2017)). For the German VET system, several studies highlight the high volatility of post-dropout pathways, as many apprentices switch firms or occupations and thereby interrupt rather than permanently exit VET (cf. Wydra-Somaggio (2021); Krötz/Deutscher (2022)). Other work examines selection into voluntary dropout in detail (cf. Böhn/Deutscher (2022); Glaesser (2006); Bessey/Backes-Gellner (2008); Rohrbach-Schmidt/Uhly (2015)). Kotte (2018) provides a detailed descriptive analysis of pathways following a first interruption of VET, showing that uninterrupted completion is associated with the most favorable employment outcomes, while non-completion yields the poorest outcomes, with interrupted pathways lying in between. Patzina/Wydra-Somaggio (2020) compare wage and employment trajectories of

VET graduates and dropouts and show that dropouts exhibit flatter profiles over the first eight years, with substantial wage and employment penalties. They also demonstrate that firm quality and the timing of dropout play an important role in shaping post-VET outcomes. Ostermann/Patzina/Morris (2026) are the first to provide a causal estimate for the effect of dropping out of VET based on an instrumental variable approach. Whereas their approach identifies effects for the subgroup of compliers, my approach does not rely on an interpretation specific to compliers only, although it is based on a more restrictive sample of apprentices and displacement events. In addition, my event study approach allows me to analyze the dynamics of the effect over time. Despite these methodological differences, both studies point to the same substantive conclusion: earnings losses following dropout from the initial training position are substantial and persist over time.

Third, I contribute to the literature on estimating the consequences of a job loss by considering a specific group of young employees at labor market entry. Previous research documents substantial and persistent earnings penalties following displacement from regular jobs, with considerable heterogeneity across contexts. von Wachter/Bender (2006) find initial wage losses of around 15% among young displaced workers in Germany, declining to about 5% after five years. Other studies report even larger penalties for other countries, particularly for low-paid and low educated workers (cf. Jacobson/LaLonde/Sullivan (1993); Couch/Placzek (2010); Huttunen/Møen/Salvanes (2018); Davis/von Wachter (2011); Lachowska/Mas/Woodbury (2020); Schmieder/von Wachter/Heining (2023)). However, little has been known about the consequences of loosing a VET position. Several features may help explain the modest results for being displaced from dual VET compared to a regular job: Apprentices are at an early career stage, have accumulated little firm-specific human capital, and benefit from institutional support that facilitates fast transitions into new training positions. In this sense, displacements during VET differ from displacements from regular employment along key institutional and career-stage dimensions. The only related study is Fersterer/Pischke/Winter-Ebmer (2008) by analyzing firm closures during or shortly after VET in Austria. In contrast, this study focuses explicitly on displacement during VET, compares displaced and non-displaced apprentices, and analyzes further outcomes beyond earnings.

The remainder of the chapter is structured as follows. Section 2 describes the institutional background. Section 3 presents the data and empirical approach. Section 4 reports the main results, while Section 5 examines different mechanisms and Section 6 explores heterogeneity in treatment effects. Sections 7 and 8 assess the plausibility of the identifying assumptions by examining pre-treatment dynamics and presenting robustness exercises. Section 9 concludes.

2 Institutional Background

VET represents the main pathway after compulsory schooling in Germany. According to the German Federal Institute for Vocational Education and Training (BIBB), the training entry rate was 51% in 2021, meaning that about half of an age cohort enters VET at some point.[1] Due to its dual structure, which combines firm-based training with vocational schooling, and firms' strong commitment to skill formation, Dustmann/Schönberg (2012) show that the German VET system achieves high enrollment rates and supports efficient skill acquisition. The organization and oversight of VET, including final examinations, are carried out by the *competent bodies* (*zuständige Stellen*; see Section 71 BBiG), most commonly the *Chambers of Commerce and Industry (IHK)* or the *Chambers of Crafts (HWK)*, depending on the occupation and sector.[2] Apprentices receive a collectively bargained monthly wage. Since 2020, apprenticeship pay has been subject to a statutory minimum wage (Muehlemann/Pfeifer/Wittek, 2020). For contracts signed in 2025, minimum VET pay ranges from 682€ in the first year to 955€ in the fourth year.[3] The VET system comprises 324 recognized occupations with training durations of two, three, or three and a half years (BIBB, 2024b). Depending on school qualifications and approval by the competent body, apprentices may shorten their training period by taking final exams early.[4] Training can also be extended, for example because of weak performance, long absences, or a failed final exam.[5] After graduation, apprentices receive an occupation-specific vocational certificate that serves as a signal to future employers (Lindner, 1998).

Most apprentices enter VET with school qualifications below university entrance level (*Hauptschulabschluss* or *Mittlere Reife*). However, the share of school leavers with university entrance qualifications (*Abitur* or *Fachabitur*) entering VET has increased in recent years (see BIBB (2024a) for developments between 2007 and 2022). Fitzenberger et al. (2025) further shows that, besides recent school leavers, increasing shares of older adults and migrants have entered VET. One major advantage of dual apprenticeships is the high probability of continued employment at the training firm. Retention rates have increased substantially over time (2010: 61% vs. 2024: 79%; see Fitzenberger/Leber/Schwengler (2025)).[6] Since 2009, the number of apprenticeship positions in Germany has exceeded the

1 See BIBB (2025) for the measurement of this indicator.
2 Apprentices in the public sector often fall outside the chamber system; here, the competent body is designated by the relevant federal or state authority (see §71 BBiG). For brevity, I use the term *responsible chamber* to refer to the competent body, since chambers fulfill this role for most apprentices.
3 See BIBB (2024) for details.
4 For VET programs lasting three or three and a half years, apprentices with at least an intermediate secondary school degree (*Mittlere Reife*) may shorten training by up to six months, while those with a university entrance qualification may shorten it by up to one year. This requires approval by the competent body upon joint application by the apprentice and the training firm (Section 8 BBiG).
5 See Section 8 BBiG.
6 These figures capture only realized retentions and do not distinguish between apprentices who did not

number of applicants. Over the following years, the number of applicants declined, while vacancies increased until the onset of the Covid-19 pandemic and then fell sharply. At the same time, matching frictions in the apprenticeship market intensified. Between 2013 and 2021, the number of unfilled apprenticeship positions increased by 68%, whereas the number of unsuccessful applicants declined by only 4% (cf. Fitzenberger et al. (2025)). These trends suggest growing difficulties for firms in filling apprenticeship positions despite the persistence of unsuccessful applicants.

During the probationary period (the first one to four months), either party may terminate the VET contract without notice or justification.[7] Apprentices may also quit at any time subject to statutory notice requirements. After probation, training firms may terminate contracts only for *good cause*. One important reason is the closure of the training establishment. By contrast, economic difficulties, lack of orders, or insolvency[8] are generally not sufficient grounds. Other employer-initiated terminations include severe misconduct, such as repeated unexcused absences or persistent refusal to work. In such cases, termination usually requires prior formal warnings and is considered a last resort. Employers may also terminate contracts in cases of long-term illness (see Lakies, 2013).

Displaced apprentices generally face three options: continuing the same VET program with another firm, switching to another VET occupation, or leaving the dual VET system altogether, for example for school-based training or university studies. If apprentices continue training at another firm, previously completed examinations and training periods are usually credited, such that a firm switch does not mechanically require a longer training period. Nevertheless, completion may still be delayed because apprentices need to adapt to different technologies, production processes, or organizational routines. If apprentices switch occupations, the extent to which prior training counts toward the new program depends on the similarity of the curricula. In such cases, overall training duration may increase further. By contrast, apprentices who leave the dual VET system for alternative educational pathways generally need to start from the beginning.[9]

When establishments close, institutional actors play an important role in stabilizing apprentices' training trajectories. In particular, the responsible chambers and the vocational guidance services of the Federal Employment Agency (*Bundesagentur für Arbeit, BA*) support affected apprentices in finding new training positions. Both institutions operate through dense regional networks. The BA maintains regional directorates and local

receive an offer and those who declined one.

7 See Sections 20 and 22(1) BBiG.

8 Insolvency proceedings do not automatically terminate apprenticeships. Before proceedings formally open, only an actual establishment closure justifies dismissal for good cause (Section 22(2) BBiG). After proceedings open, termination with three months' notice is permissible only if training can no longer be provided.

9 Some programs may partially recognize prior VET experience, for example through internship credits, but this depends on the institutional setting.

employment agencies, while chambers provide local counseling and training-related services through regional districts.[10]

If only one establishment within a multi-establishment firm closes,[11] apprentices may transfer to another establishment within the same firm,[12] although long commuting distances may make such transfers unattractive. If the entire training firm closes, internal relocation is impossible. In some cases, firms inform the responsible chambers before closure becomes imminent. If this happens early enough, apprentices may even complete training ahead of schedule.[13]

When training establishments close, the responsible chambers provide counseling and help apprentices find alternative training firms (Section 76 BBiG), usually within the same occupation and through local employer networks. If no suitable position is available, or if apprentices seek advice on alternative pathways, the BA provides additional counseling and vocational guidance. Displaced apprentices are typically treated as high-priority cases when appointments are scheduled.[14] As a measure of last resort, the BA may arrange publicly funded out-of-company vocational training (*Berufsausbildung in außerbetrieblichen Einrichtungen, BaE*) for young people facing particular hardships.[15]

Besides the chambers and the BA, many vocational schools also provide youth social work services that offer counseling and support during training. The institutional design and funding of these services vary across federal states because of Germany's federal structure.[16] Where available, these services constitute an additional low-threshold support mechanism by helping apprentices identify next steps and search for new training positions when closures become imminent.[17]

Even if the search for a new training firm temporarily interrupts firm-based training, apprentices may often continue attending vocational school classes by agreement. Although attendance is usually tied to an apprenticeship contract, transitional periods of

[10] See BA and IHK for details. Chambers act as competent bodies and support apprentices when training relationships are disrupted, including through counseling and placement assistance (Sections 71 and 77 BBiG).
[11] VET contracts are concluded between the apprentice and the training establishment (Section 10 BBiG). When I refer to the training establishment as the *firm*, I mean the specific establishment providing the training, not a broader corporate group.
[12] See, for example, guidance issued by German chambers of crafts on contractual changes in apprenticeship training during operational restructuring (HWK).
[13] Based on information from Christine Gräff (IHK Nürnberg, December 2025). See also footnote 4.
[14] According to Christopher Leopold (career counselor at the Employment Agency in Fürth, January 2026).
[15] See BA (2026) and BA (2024) for details.
[16] For example, Bavaria regulates youth social work through a state-level guideline (StMAS Bayern, 2024), while Hamburg and Schleswig-Holstein provide separate frameworks for vocational schools (HIBB, 2024; BSA-SH, 2014).
[17] This option was highlighted by Christopher Leopold (career counselor at the Employment Agency in Fürth, January 2026).

about 6–8 weeks are commonly allowed.[18] In addition to regular occupation-specific classes, vocational schools often offer preparatory programs for young adults without apprenticeship contracts. While these programs mainly target school leavers who have not secured a VET position, they may also bridge interruptions for displaced apprentices, especially early in VET.

During past economic crises, nationwide financial incentives encouraged firms to take over apprentices displaced by establishment closures. From 2008 to 2010, the federal government introduced a VET premium (*Ausbildungsbonus*, Section 421r SGB III) for firms providing apprenticeship places to applicants facing difficulties in finding training positions, including displaced apprentices. The premium depended on the apprentice's monthly wage at the new firm and ranged from 4,000€ to 6,000€ (see Bonin et al., 2013). During the Covid pandemic, the federal program *Save Apprenticeships* (*Ausbildungsplätze sichern*) was introduced in 2020. Firms received a one-off payment of 3,000€ for each apprentice taken over from a closing training firm.[19] In March 2021, the program was extended and the premium doubled to 6,000€.[20]

Several federal states also introduced supplementary programs. Baden-Württemberg's *Azubi Transfer – Ausbildung fortsetzen* provides firms with 1,200€ per transferred apprentice.[21] In Hesse, firms can receive non-repayable wage subsidies covering 50% of apprenticeship wages in the first year and 25% in the second year.[22] Rhineland-Palatinate offers a premium of 2,500€ per transferred apprentice,[23] while Lower Saxony introduced regionally staggered premiums in 2022.[24] In Berlin, firms may receive wage subsidies covering up to 75 percent of apprenticeship wages, capped at 5,000€ per apprentice.[25] Overall, institutional measures such as counseling, placement support, and financial incentives aim to reduce training disruptions and help apprentices continue VET after establishment closures.

[18] This description is confirmed by conversations with Christine Gräff (IHK Nürnberg, December 2025), Christopher Leopold (Employment Agency Fürth, January 2026), and Sonja Singer (youth social worker at vocational schools in Fürth, January 2026).
[19] See BA (2021).
[20] See Press and Information Office of the Federal Government (2021).
[21] See WM-BW (2021).
[22] See HMWVW.
[23] See ISB.
[24] See NBank (2022).
[25] See SenASGIVA (2025). Schleswig-Holstein additionally introduced temporary Covid-related support in 2020 for firms taking over apprentices from insolvent companies (LTSH, 2021).

3 Data and Empirical Approach

3.1 German Administrative Data

The primary data source for this study is the *Integrated Employment Biographies* (IEB). These data provide a complete daily record of employment spells subject to social security contributions (excluding civil servants and self-employment), as well as registered unemployment spells. The IEB contain a rich set of labor market characteristics, including employment status, wages, industry, occupation, and firm identifiers, alongside sociodemographic information such as age, gender, and education. My analysis uses a sample drawn from the IEB that includes all individuals who entered a dual VET program between 2008 and 2018.[26]

Information on firm characteristics is obtained from the Establishment History Panel (BHP; Betriebs-Historik-Panel). This dataset contains half-yearly information on all establishments in Germany, measured on June 30 and December 31 each, including workplace location, industry affiliation, workforce composition, and average wages. Importantly, the BHP also includes an additional file that records firm closures between the end of June of successive years. Based on the set of firms I can identify as closing while offering VET (see Section 3.3 for details), I additionally use a dataset containing all employment spells from the IEB in these closing training firms during the two years prior to closure.

Further additional data sources are used to enrich the analysis. To capture occupational similarity, I use expert-based information on occupational tasks from the BERUFENET database, combined with established measures of occupational similarity.[27] For the matching approach, I incorporate information from the Statistics Department of the BA on VET positions, applicants, and signed training contracts at a disaggregated level. Finally, to extend the event study specification, I use data on local unemployment rates at a detailed regional level from INKAR (Indicators and Maps for Spatial and Urban Development), published by the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

[26] The IEB do not report school-based vocational training.

[27] Specifically, occupational similarity is measured based on task inputs, following Gathmann/Schönberg (2010). Further details on this measure are presented in Appendix A2.4. The corresponding data source was provided by my colleagues Florian Hack and Britta Matthes.

3.2 Data Preparation

The data preparation consists of several steps to define a relevant sample of apprentices at labor market entry and to meaningfully construct treatment status and outcome variables. I restrict the IEB data to individuals who enter VET shortly after leaving school by keeping only those aged 14–25 at the start of their first VET spell and excluding anyone who already holds a vocational or university degree. I group relevant spells[28] into a single VET episode if they meet the following criteria: (i) the spell is classified as VET in the IEB; (ii) there is no interruption longer than three months since the start of the VET episode; (iii) no switch in training occupation or training firm is observed; and (iv) time spent in the training occupation does not exceed four years. A VET episode may end with successful graduation, unsuccessful completion, or premature dropout. In addition, the *first* VET episode ends at the latest as a vocational degree is recorded.[29] Additional cleaning steps contains removals of inconsistencies and implausible records. Appendix A2.1 contains a detailed description of all cleaning steps and the percentage share of individuals dropped at each step.

In some cases, apprentices switch training firms or occupations with little or no interruption. To study VET-related outcomes such as graduation following displacement during VET, and to compare these outcomes to those without displacement, it is therefore important to account for the training period after a firm switch. This may, in some cases, also include an occupational switch. I thus define *total VET episodes* as the combination of consecutive VET episodes without long gaps in between, allowing for switches in training firms or occupations.[30]

In the main specification, I restrict attention to displacements occurring within the *first VET episode*, meaning within the first training firm and first VET occupation.[31] Only the first VET episode is used to define treatment, while later VET episodes are observable as subsequent outcomes.[32] After all cleaning steps, the resulting sample consists of 4.1 million apprentices who start VET between 2008 and 2018, i.e. finish between 2010 and 2022 at latest.

A key challenge is identifying whether an apprentice obtained a VET degree during the first

[28] The IEB splits employment histories into multiple spells, for example at the start of each calendar year or when employment characteristics change.
[29] For later training episodes after a successful VET completion, it is not possible to determine whether an additional VET degree was obtained.
[30] Accordingly, the first total VET episode starts on the same date as the first VET episode and ends in cases (i), (ii), or (iv) defined in the previous paragraph.
[31] Due to the occupational codes given in the IEB, some switches between closely related VET occupations are not observable. In particular, a single 5-digit KldB2010 occupation can comprise multiple VET occupations that are similar in training content but differ in duration (e.g., 2-, 3-, or 3.5-year programs). Such unobserved switches are therefore treated as remaining within the same occupation.
[32] As a robustness check, I allow for both occupation and firm transitions and study displacements within the *first total VET episode* This is further described in Section 8.2

VET episode. Information on educational attainment is sometimes reported inconsistently, especially for individuals who continue working at the same firm after graduation. To determine VET graduation status, I use all available employment and unemployment spells following the first VET episode.[33] If a VET degree is reported in any of these subsequent spells, the individual is classified as a *VET graduate*. If no degree is ever reported, the individual is classified as a dropout.[34]

Several variables used in the matching procedure and subgroup analyses require consistent definitions. Educational attainment is imputed following Fitzenberger/Hillerich-Sigg/Sprietsma (2020). I distinguish three school-degree categories. The first group, *No / Unknown*, includes individuals without a formal school degree as well as cases with missing information. The second group, *Below University Entrance*, comprises individuals holding a *Hauptschulabschluss* or *Mittlere Reife*.[35] The third group, *University Entrance*, includes individuals with a *(Fach-)Abitur*.[36] Foreign citizenship is assigned if it is observed in any spell. All wages are deflated to real 2015 values using the GDP deflator, with 2015 normalized to 100.[37]

Employment trajectories are transformed into a half-yearly panel observed in calendar half-years from 2006 to 2023. Some outcome variables are defined cumulatively within each half year, such as days in employment and half-yearly earnings. Other outcome variables, including measures of occupational and regional mobility, are defined at the midpoint of each half-year, that is, on 31 March or 30 September. Periods without observed employment or unemployment spells are considered as non-employment with zero days in several labor market states and zero earnings.

Since individuals enter VET and hence experience displacement in different calendar half-years, the length of observable periods before and after the displacement event varies across individuals. Throughout the analysis, I use up to eight half-years prior to displacement and up to eight years after displacement. This construction results in an unbalanced panel at the beginning and end of individual observation windows; the event-study specification includes calendar-time and relative-time fixed effects to mitigate concerns arising from this imbalance.

[33] I restrict the analysis to spells before entry into a subsequent VET program, so as to measure graduation for the current training episode.

[34] In the final matched sample, 194 apprentices lack any usable education information after the first complete VET episode. This occurs either because all subsequent spells report missing education or because no further spells are observed. These individuals are kept in the sample and their graduation status is treated as missing in most analyses.

[35] These degrees are typically obtained after 9 to 10 years of schooling.

[36] These degrees are typically obtained after 12 to 13 years of schooling and provide access to higher education

[37] Yearly GDP deflator values published by the Federal Statistical Office are used, see here.

3.3 Measuring VET Displacement

To identify displaced apprentices, I follow an approach similar to Schmieder/von Wachter/Heining (2023) and draw on information from the Establishment History Panel (BHP). To exclude events such as mergers, takeovers, or changes in establishment identifiers, I follow Hethey-Maier/Schmieder (2013) and remove these cases from the further analysis.

To define the timing of firm closures, I use additional administrative data. For each closing VET establishment identified using the BHP, I consider all employment spells in the final year and predict the closure date as the last observed end date of any employment spell at that establishment. While the firm may continue to exist formally beyond this date, it no longer employs workers. I then define the calendar half-year in which the firm closes based on this predicted closure date.[38]

The treatment status of a *VET displacement* is defined dynamically. An apprentice is classified as treated in half-year c if the individual is in the first VET episode and the training firm closes in the respective half-year.[39] Using this definition, 26,233 out of 4.1 million apprentices are classified as displaced during VET.

Table A1 in Appendix A1 summarizes key sociodemographic characteristics at the start of VET. Column (1) reports means and standard deviations for the full population of apprentices, while Column (2) reports the corresponding statistics for apprentices who later experience displacement. While 44% of all VET entrants are female, women are slightly overrepresented among displaced apprentices and account for about half of this group, which is be related to gender differences in occupational choices. The age distribution of displaced apprentices closely resembles that of the overall population, with only a small tendency toward higher ages.

Differences are more pronounced with respect to educational attainment. Apprentices with a *University Entrance* qualification are underrepresented among those affected by firm closures, with shares of 18% compared to 24% among all apprentices. By contrast, apprentices with no recorded school degree or missing information constitute a larger share among displaced apprentices (24% vs. 17%). As expected, training firms that close in

[38] At the time of data preparation, I used the BHP only in the one half-yearly version that provides records as of June 30 in each year. The availability of the complementary December 31 records was not yet known to me. Although combining both waves would have been sufficient to define treatment at the half-year level for the main analysis, the exact closure date is useful for additional analyses of short-run dynamics prior to closure.

[39] This definition also includes apprentices who leave the firm shortly before the predicted closure date within the same half-year. Such exits may reflect anticipation effects and thus introduce selectivity. As a robustness check, I therefore restrict the treatment group to cases in which the difference between the VET exit date and the predicted closure date does not exceed 15 days. This is further outlined in Section 8.2.

the near future pay lower VET wages to their apprentices. Average daily wages amount to 21.75€ in closing firms, compared to 23.76€ in firms that remain open. In addition, closing firms are more frequently located in the lower quartiles of the AKM firm fixed effect distribution.

The probability of experiencing a firm closure during VET also varies across occupational fields, as shown in Columns (1) and (2) of Table A3 in Appendix A1. VET Displacements occur disproportionately often in *occupations in tourism, hotels and restaurants*, *occupations in food production and processing*, and *occupations in safety and health protection, security, and surveillance*. At the lower end of the distribution are occupations in *construction scheduling, architecture, and surveying* as well as in *law and public administration*.[40]

Further regional differences emerge, as apprentices in some federal states experience firm closures more frequently than in others, see Columns (1) and (2) of Table A2 in Appendix A1. Displacements occur relatively rarely in Saxony, Saxony-Anhalt, and Thuringia, whereas apprentices in Berlin and Schleswig-Holstein are comparatively more often affected. A potential explanation for this somewhat surprising pattern may be differential selection into training provision across East and West Germany, as the share of firms offering VET is lower in East Germany than in West Germany (Leber/Roth/Schwengler, 2023). This could imply that, in West Germany, a broader set of firms - including those with a higher risk of closure – participates in training, which may be one reason why displacement during VET appears less common in some eastern federal states. The distribution of VET entry years among all apprentices, shown in Figure A1 in Appendix A1, follows a relatively stable pattern from 2012 onward, with slightly lower entry rates between 2008 and 2011, likely reflecting the financial crisis. The evolution of displaced apprentices, shown on the right axis, suggests a broadly similar co-movement, especially in the earlier years during the financial crisis.

3.4 Constructing a Matched Sample of Displaced and Non-Displaced Apprentices

After defining VET displacements, the next step is to construct a control group of non-displaced apprentices. Analogous to the treatment group, the pool of potential control observations is defined at the half-year level. Individuals must (i) be apprentices in their first VET episode and (ii) not yet have experienced a VET displacement. As a result, apprentices who are not yet treated but later can serve as controls. However, the number of ever-treated

[40] This pattern also holds for the two quantitatively smallest VET occupational groups in my sample: Only two apprentices are displaced in occupation 42 (*occupations in geology, geography, and environmental protection*), while no displaced apprentices are observed in occupation 91 (*occupations in philology, literature, humanities, social sciences, and economics*).

individuals is small relative to the potential control pool, with $26,233$ displaced apprentices compared to more than 4 million potential controls.[41]

Apprentices can therefore appear in the final matched sample at multiple points in time. They may serve as controls in some half-years and be treated in others. Following Rubin (1980) and Imai/King/Stuart (2008), I combine exact matching on a small set of discrete variables[42] with Mahalanobis distance matching on continuous variables.[43] Matching is performed one-to-one within cohort half-years, such that each displaced apprentice is matched to a single suitable control. If no suitable control is available, the treated apprentice is excluded from the matched sample. A control apprentice may be matched to more than one treated apprentice or be used as a control in multiple half-years.

To ensure close comparability, treated and control apprentices are required to (i) start VET in the same cohort and (ii) be observed in the same (actual or fictional) displacement half-year.[44] Table 1 reports the full set of matching variables. Exact matching is imposed on key sociodemographic characteristics, including gender, foreign citizenship, and schooling. In addition, apprentices are matched exactly on the 2-digit KldB 2010 training occupation (BA, 2010), which comprises 35 categories, on the quartile of the AKM firm fixed effect,[45] and on the federal state in which training takes place. This procedure ensures that matched apprentices train in firms with similar wage structures and in comparable regional labor markets.

Conditional on exact matching, final match pairs are selected using Mahalanobis distance based on standardized continuous covariates,[46] as listed in Table 1, Column (2). To avoid systematic differences between treated and control apprentices, Mahalanobis distances are required to fall below the 95th percentile of the χ^2-distribution with $df = 11$, i.e., potential matched paris with larger Mahalanobis distances are discarded from the sample.[47]

The continuous variables used for distance matching include age at VET start,[48] the average VET wage, the number of days spent in VET up to the treatment half-year, and earnings

[41] All apprentices who start VET at a young age, remain in the data after cleaning, and are never displaced during VET are used as potential never-treated controls.
[42] Some of them, such as occupation and region, are broadened to have 35 and 16 categories, respectively.
[43] Implemented using `kmatch` in Stata.
[44] That is, both individuals in a matched pair start VET at the same time. In a given half-year, one apprentice is displaced due to the closure of their training firm, while the other continues VET without interruption.
[45] AKM firm fixed effects are grouped into quartiles due to estimation noise, particularly for small training firms. Missing firm fixed effects are matched exactly as well.
[46] This means they are standardized by subtracting their mean and dividing by their standard deviation. This gives each variable equal weight when computing the Mahalanobis distance.
[47] This threshold follows from the fact that the Mahalanobis distance is computed using 11 approximately standard normally distributed variables. Under this assumption, the distance between two observations drawn from the same distribution converges asymptotically to a $\chi^2(df = 11)$ distribution. See Davidson/MacKinnon (2004) for a derivation.
[48] I measure age in full years only.

(1) Exact matching	(2) Mahalanobis distance matching
Year of VET start Year of VET Gender Foreign citizenship School degree (4 categories) Training occupation category (35) Federal state of residence AKM establishment FE (4 quartiles; missing)	Age at VET start VET Wage Days in VET Earnings beside VET VET tightness Share of VET positions unfilled Training firm size Employment Δ in training firm in prev. two years Share of apprentices in training firm Share of unskilled employees in training firm Share of old employees in training firm

Notes: This table gives information on all variables that are included in the matching approach. The variables in Column (1) are matched exactly, whereas those in Column (2) are included in the Mahalanobis distance matching after standardizing.
Sources: IEB V17.01.00, BHP 7523-V1, Data from the Statistics Office of the Federal Employment Agency. Own calculation.

Table 1: Matching Variables

outside VET. In addition, I include measures of VET market conditions, namely VET tightness, defined as the number of VET positions per applicant, and the share of unfilled VET positions. Both variables are measured at a disaggregated level by the two-digit KldB 2010 occupation and local labor agency district.[49] Establishment characteristics are also incorporated, including firm size, the shares of unskilled and older workers,[50] the number of apprentices, and two-year employment growth. These variables ensure that matched apprentices train in comparable firms with similar workforce structures and employment dynamics.

Out of 26,233 displaced apprentices, 22,496 can be matched successfully. Table A4 in Appendix A1 shows that unmatched displaced apprentices tend to have less favorable sociodemographic characteristics along several dimensions, as they exhibit lower educational attainment, lower wages, and are more likely to train in lower-paying firms. As a consequence, the estimated treatment effects may understate the true displacement effects, since the matched sample underrepresents some of the most disadvantaged apprentices. To the extent that these individuals experience larger earnings losses following displacement, a more representative matching would likely yield more negative estimated effects.[51] Importantly, the event-study estimates aim to identify the average treatment effect on the treated at each event half-year (relative to the omitted pre-event period) within the matched sample. Given the underrepresentation of particularly disadvantaged apprentices, these estimates should therefore be interpreted as a lower bound on the

[49] I use the regional structure from 2021, which consists of 155 labor agency districts. By merging the three districts of Berlin into one to obtain a meaningful local labor market, the final structure comprises 153 labor agency districts. See also here for an overview.
[50] More precisely, I include the share of workers without a professional degree (or for whom no such information is available) and the share of workers aged at least 50 years.
[51] See Section 6.2 for the results.

Variable	(1) Treated	(2) (SD)	(3) Control	(4) (SD)	(5) SMD	(6) δ^*
Age at VET Start	18.30	(2.098)	18.13	(1.909)	0.0848	0.034
Mean Daily Wage from VET	24.39	(7.542)	24.92	(7.334)	-0.0709	0.036
Days in VET	155.97	(45.301)	158.67	(42.803)	-0.0612	0.027
Earnings beside VET	751.03	(2411.275)	729.47	(2496.175)	0.0088	0.029
Number of Employees in Training Firm	101.16	(425.980)	143.13	(406.527)	-0.1008	0.129
Change in Employment During Last 2 Years	0.14	(1.265)	0.12	(0.660)	0.0220	0.133
Share of Apprentices	0.18	(0.172)	0.16	(0.149)	0.1480	0.075
Share of Unskilled Workers	0.24	(0.164)	0.21	(0.142)	0.1808	0.084
Share of Old Employees	0.23	(0.157)	0.25	(0.140)	-0.0985	0.078
Share VET Positions Unfilled	0.67	(0.209)	0.67	(0.206)	-0.0072	0.014
VET Tightness	1.47	(3.555)	1.53	(4.965)	-0.0133	0.013

Notes: This table shows means and standard deviations of all variables in the distance matching, for the treatment and control group, in columns 1-4. Column (5) reports standardized mean differences (SMD) between both groups. Column (6) reports a minimal equivalence margin δ^* required to conclude distributional equivalence among treated and controls.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table 2: Balance Table: Treatment vs. Control Group

average effect of displacement on future earnings.

A potential concern with this matching approach is that large differences in size, measured by the number of apprentices across federal states and 2-digit KldB 2010 occupational categories, may lead to weaker matching performance for apprentices from smaller states or occupational groups. To address this concern, I report the shares of matched displaced apprentices by federal state and occupational category. Column (4) of Table A2 in Appendix A1 reports the share of displaced apprentices matched within each federal state.[52] Even in the smallest federal states, such as Bremen, at least two thirds of displaced apprentices are matched. A qualitatively similar pattern emerges when considering occupational categories, as shown in Column (4) of Table A3 in Appendix A1. Matching rates vary more strongly across occupations, ranging from a maximum of 92% for *technical occupations in machine-building and the automotive industry* to a minimum of 37% for the relatively small group of *occupations in cleaning services*. These patterns suggest that match rates vary across states and occupations, but without a uniform underrepresentation of smaller federal states or occupational groups.[53]

Table 2 provides an overview of the balance of the continuous matching variables among the matched treated and controls. Following Rosenbaum/Rubin (1985), I report in Column (5) the standardized mean difference (SMD) between the matched treated and controls (also

[52] While a broad inspection suggests some positive correlation between matching rates and the overall number of apprentices (see Column (1)), the pattern is not monotonic. For example, North Rhine–Westphalia, the largest federal state, ranks only fourth in terms of matching share.
[53] Robustness analyses that apply alternative weighting approaches further address this concern by considering broader 1-digit occupational fixed effects on the KldB 2010 scale and a *West* versus *East Germany indicator*. These analyses are described in Section 8.4.

referred to as *mean standardized bias*) of all continuous covariates.[54] The SMD is widely recommended in the matching and causal inference literature as the primary diagnostic for assessing covariate balance, as it is independent of sample size and allows for a comparison of imbalance across covariates measured on different scales (Stuart, 2010; Austin, 2009; Caliendo/Kopeinig, 2008; Rosenbaum/Rubin, 1985).[55] In my example the maximal value of the SMDs across all continuous covariates remains below 0.2, which indicates only small residual imbalance according to commonly used benchmarks in the literature (see Stuart, 2010). Considering all continuous matching variables jointly, the mean SMD equals 0.08. Together with the seven discrete matching variables which are matched exactly and therefore exhibit a standardized mean difference of zero, the overall average standardized difference across all covariates amounts to 0.05. This value is reported as indicative of sufficiently good match quality in empirical studies in labor economics (see Caliendo/Kopeinig, 2008) and suggests that the matching procedure achieves a high degree of covariate balance between treated and control units.

Rather than considering differences in mean values only, the matching literature further emphasizes assessing balance along the entire distribution among treated and control units (Stuart, 2010; Austin, 2009; Rosenbaum/Rubin, 1985). For a graphical visualization, Figures A2 and A3 in Appendix A1 report kernel density estimates separately among matched treated and control apprentices for all variables used in the distance matching. A visual inspection suggests that in most cases these density estimates look very similar across treated and controls, even though slight differences remain visible. For a quantitative assessment of the similarity of the respective distributions between treated and controls, I report in Column (6) of Table 2 the minimal equivalence margin δ^* required to conclude distributional equivalence between treated and control apprentices.

Intuitively, δ^* can be interpreted as the smallest distributional difference that must be allowed for to conclude equivalence. For instance, a value of $\delta^* = 0.034$ for the age distribution at VET start indicates that treated and control groups can be regarded as distributionally equivalent once one allows for a maximum discrepancy of 3.4 percentage points between their empirical cumulative age distributions. Applying the same logic, for six (nine) of the eleven variables, the required equivalence margins remain below 5 (10) percentage points. Even in the least favorable case (firm size), the implied equivalence margin corresponds to a maximal deviation of 13 percentage points. Additional details on

[54] I.e. for a variable x it is given by,

$$\text{SMD(x)} = \frac{\mu_x^T - \mu_x^C}{\sqrt{\frac{(\sigma_x^T)^2+(\sigma_x^C)^2}{2}}}$$

.

[55] The matching literature explicitly discourages relying on hypothesis tests for balance assessment in matched samples and instead advocate the use of the SMD (Imai/King/Stuart, 2008; Austin, 2009). Indeed, for seven of the eleven variables considered, a t-test on mean equivalence yields p-values smaller than 0.05. Details are available upon request.

the meaning and computation of δ^* are presented in Appendix A2.1.

Notably, only 3% of all potential match pairs were discarded by applying the caliper on the Mahalanobis distance. This further indicates that given balance in the exact matching variables, bad match quality in the continuous matching variables used in the Mahalanobis distance matching due not play a very important role.[56] A further robustness exercise in Section 8.3 further shows that including these lower-quality match pairs keeps the results unchanged. An additional exercise in Section 8.3 varies the assignment of the matched control when multiple candidates have a similar matching distance and shows that the one-to-one matching results are not sensitive to random tie-breaking.

Overall, while the matching procedure does not achieve perfect alignment across all continuous matching variables simultaneously, the remaining discrepancies are economically small for most variables. Taken jointly, the resulting Mahalanobis distances are generally low, which indicates close multivariate similarity between matched treated and control observations. Moreover, as the most important matching variables coincide exactly within each matched pair (a feature that is not always ensured in alternative approaches such as propensity score matching), the matched sample can be considered fairly well balanced overall. Among the matched displaced apprentices, 36% of all displacements occurred in the first year of VET, 32% in the second year, 23% in the third year, and the remaining 9% in the fourth year of VET.

3.5 Estimation Strategy: Event Study and Matched Difference-in-Differences Design

To study the dynamic effect of being displaced from the training firm, I employ an event study analysis. Let Y_{it} denote the outcome of interest for individual i in calendar half-year t. Each individual is assigned to a cohort $c(i)$, defined as the displacement half-year of the treated apprentice to whom they are matched. For treated apprentices, $c(i)$ equals their own treatment half-year, while matched controls inherit the cohort of their treated counterpart. Let D_i be an indicator equal to one for treated apprentices and zero for matched control apprentices. I estimate the following event study:

$$Y_{it} = \alpha_i + \eta_t + \sum_{k=0}^{16} \beta_k \mathbf{1}(t - c(i) = k) D_i + \sum_{k=0}^{16} \gamma_k \mathbf{1}(t - c(i) = k) + \varepsilon_{it}. \tag{1}$$

[56] One might expect than 5% would be dropped because of exceeding the $95.$ percentile of the χ^2-distribution even if the continuous matching variables among treated and controls would follow the same distribution.

The period $k = -1$ is omitted as the reference category in order to avoid perfect collinearity. The main coefficients of interest are the β_k, $k = 0, \ldots, 16$, which measure the difference in outcomes between displaced apprentices and their matched controls k half-years after displacement, relative to the baseline period $k = -1$. I include individual fixed effects α_i[57] to account for time-invariant individual characteristics and calendar half-year fixed effects η_t to absorb common shocks.

Similar to Schmieder/von Wachter/Heining (2023), I additionally estimate relative half-year fixed effects γ_k in order to capture systematic changes over the life cycle relative to the (fictional) displacement event. Following Abadie/Spiess (2022), standard errors are clustered at the level of matched pairs, which is appropriate when estimating treatment effects in a matched sample of treated and control observations. The main outcome Y_{it} considered are *half-yearly earnings from regular employment*.[58] To study employment outcomes following VET displacement in a comprehensive way, I also analyze a broad set of additional outcomes. These include employment and wages, as well as indicators of subsequent VET success, occupational quality, and regional and occupational mobility.

3.6 Specification, Identification Assumption and Robustness Strategy

A causal interpretation of the coefficients β_k in Equation (1) requires a set of identifying assumptions on treatment assignment and potential outcomes, as well as correct specification of the empirical model. This section formalizes these assumptions and briefly outlines the robustness exercises and additional analyses to assess their plausibility.

3.6.1 Conditional Mean Independence

The baseline event study specification assumes that the conditional expectation of outcomes in Equation (1) is correctly specified, with

$$\mathbb{E}[\varepsilon_{it} \mid D_i, \alpha_i, \eta_t, \{\mathbf{1}(t - c(i) = k)\}_{k \in \mathcal{K}}] = 0, \qquad \forall i \in \mathcal{M},\ \forall t. \tag{2}$$

[57] In the stacked sample, the same underlying person can appear multiple times, for example as a control unit in different cohorts, or first as a not-yet-treated control in an earlier cohort and later as a (matched) treated individual in a subsequent cohort. Each appearance is treated as a separate observation in the outcome Y_{it} and treatment indicator D_i, where the subscript i indexes sample observations. By contrast, the individual fixed effect α_i is defined at the level of the underlying person. If a person appears multiple times in the sample, the same fixed effect is therefore applied to all of their observations. This notation is chosen to keep the subscripts simple.

[58] See also Table A9 for a detailed description.

where $\mathcal{K} = \{0, ..., 16\}$ and $\mathcal{M}$ denotes the matched sample.[59] This condition is not directly testable, as it concerns the conditional mean of the unobserved error term. Violations may arise if the fixed effects do not capture important time-varying confounders, or if relevant determinants of earnings are omitted. Section 8.1 presents robustness analyses that vary the functional form of the event study specification and the set of included controls to assess the sensitivity of the results.

3.6.2 Consistency and Stable Units Treatment Values

Consistency requires that observed outcomes correspond to potential outcomes under the realized treatment status. Treatment is defined as the closure of an apprentice's training firm during VET, which is a clearly defined and objectively measured event in the linked administrative data. Accordingly, outcomes observed for treated individuals correspond to $Y_{it}(1)$, while outcomes observed for control individuals correspond to $Y_{it}(0)$.[60] The observed outcomes are thus given by

$$Y_{it} = D_i \cdot Y_{it}(1) + (1 - D_i) \cdot Y_{it}(0), \qquad \forall i \in \mathcal{M},\ \forall t \in \{2006/1, ..., 2023/2\}. \tag{3}$$

Here, Y denotes a generic outcome variable. This definition, together with the administrative measurement of firm closures, ensures a transparent mapping from the data to treatment status and makes violations of the consistency assumption unlikely.
For a causal interpretation of the estimated event-study effects in Equation (1), the stable unit treatment value assumption (SUTVA) must hold. This assumption requires that each individual's potential outcomes depend only on their own treatment status and are not affected by the treatment status of other individuals through interference or spillover effects. Formally, SUTVA states that

$$Y_{it}(d_i, \mathbf{d}_{-i}) = Y_{it}(d_i) \qquad \forall i \in \mathcal{M},\ \forall t \in \{2006/1, ..., 2023/2\}, \tag{4}$$

where $d_i \in \{0, 1\}$ denotes a generic treatment status, and $\mathbf{d}_{-i}$ denotes the treatment assignment of all individuals other than i. In this example, SUTVA states that an apprentice's outcomes are affected only by whether their own training firm closes, and not by closures

[59] The notation $\forall t$ refers to all half-years from 2008/1 to 2023/2 that appear in the estimation of Equation (1)
[60] Individuals may enter the stacked sample more than once; thus, the index i refers to cohort specific observations rather than unique persons.

experienced by other apprentices through, for example, displacement effects or changes in local labor market conditions. Violations of this assumption are unlikely because firm closures during VET are rare events (see the small share of displaced apprentices in Table A1 in Appendix A1), which limits the scope for spillovers across apprentices. Moreover, substantial interference would typically require closures to be highly clustered within specific local labor markets or occupations to affect outcomes of untreated individuals, which is unlikely given the overall distribution of closure events (see Section 3.3).

3.6.3 No Anticipation Effects

For a causal interpretation of Equation (1), displacement is assumed to affect outcomes only from the displacement half-year $c(i)$ onward, and not in any pre-displacement period. Formally,

$$Y_{it}(1) = Y_{it}(0), \qquad \forall i \in \mathcal{M} \text{ with } D_i = 1, \; \forall t < c(i). \tag{5}$$

Equivalently, letting $k = t - c(i)$ denote event time, the assumption holds for all $k < 0$. A violation of Equation (5) would arise if apprentices adjust their behavior prior to the closure, for example by searching for alternative training firms in response to indications of an economic downturn at the training firm. Although this assumption is not testable as it includes counterfactual outcomes, Section 7 offers supportive evidence by analyzing pre-treatment dynamics. In addition, Section 8.2 presents robustness checks to check for sorting and selective mobility which may point to anticipation effects. Specifically, I (i) exclude treated apprentices who leave closing firms slightly before the actual closure date (and their matched counterparts) and (ii) exclude treated apprentices at firms that exhibit strong strong employment declines prior to closure (and their matched counterparts). Together, these exercises examine whether the results may be driven by forward-looking adjustments before displacement rather than by the closure event itself.

3.6.4 Parallel Trends

Identification relies on the assumption that, within the matched sample, displaced apprentices and their matched controls would have followed parallel trends in outcomes in the absence of displacement. As both the risk set and the treatment itself are defined in a dynamic way, denote by the variable G_i the timing of displacement for each individual in the matched sample, with $G_i = g$ if an individual is displaced in half-year g, and $G_i = \infty$ for never-treated.[61] Further denote by $\mathcal{C}(g, t)$ the set of individuals who are matched controls

[61] Thus, for not-yet-treated controls, $G_i > c(i)$.

assigned to treatment cohort g and untreated at calendar half-year t:

$$\mathcal{C}(g,t) = \{i \in \mathcal{M} : c(i) = g,\ G_i > t\}.$$

The parallel trends assumption can be expressed as

$$\mathbb{E}[Y_{it}(0) - Y_{it'}(0) \mid G_i = g, i \in \mathcal{M}] = \mathbb{E}[Y_{it}(0) - Y_{it'}(0) \mid i \in \mathcal{C}(g,t), i \in \mathcal{M}]\,, \tag{6}$$

for all treatment cohorts g and calendar half-years $t > t'$ in the estimation period. Since Equation (6) contains counterfactual outcome paths for treated apprentices, it cannot be tested directly. I therefore evaluate the plausibility of this assumption through several complementary analyses that consider possible violations of parallel trends. First, Section 7.1 examines pre-treatment outcome dynamics by estimating an event study specification for pre-treatment periods to assess whether treated and control apprentices already exhibit differential trends prior to displacement. Second, Section 8.1 re-estimates the event study including county by calendar half-year and occupation by calendar half-year fixed effects, which accounts for region-specific and occupation-specific shocks that could affect treated and control apprentices differently over time. Third, Sections 8.2 and 8.3 assess robustness to the inclusion of apprentices who switch training firms or occupations prior to closure and to including of the full pre-treatment history of time-varying matching variables. These exercises address the concern that firm closures may be preceded by gradual declines in firm performance or prior sorting of apprentices, and check whether apprentices without prior switches may still follow different trajectories.

3.6.5 No Differential Sample Selection

The identification strategy further assumes that sample selection around the event is not systematically related to potential outcomes. In this context, a concern is that apprentices may leave their training firm prior to closure, which can change the composition of observed treated and control units around the treatment date. To assess the sensitivity of the estimates to such selection, I conduct two robustness analyses (see Section 8.2). First, I relax the restriction that apprentices must experience the closure in their initial training firm or occupation by also including apprentices whose closure exposure occurs only at a subsequent training firm or after an occupational switch. This addresses the possibility that selective mobility in earlier periods affects which apprentices are classified as treated. Second, I adopt a more restrictive treatment definition by excluding apprentices who leave closing firms shortly before the recorded closure date (and their matched counterparts), which targets potentially selective mobility within the final pre-closure period.

3.6.6 No Forbidden Comparisons

In settings where individuals are treated at different points in time, standard event study estimators can be biased if individuals who have already been treated are used as controls for individuals who are not yet treated. To avoid such "forbidden comparisons", I construct the risk set such that, in each calendar half-year, the control group consists only of apprentices in their first VET who have not yet experienced a firm closure (see also Section 3.4). Consequently, identification relies exclusively on comparisons between treated apprentices and apprentices who are still untreated at that time.

3.6.7 Common Support and Alternative Balancing Strategies

The identification additionally requires common support, that is, overlap such that treated apprentices can be compared to observationally similar matched control apprentices within a relevant set of covariates $\mathcal{X}_{\mathcal{M}}$:

$$\mathbb{P}\left(D_i = 1 \mid X_i\right) < 1, \qquad \forall X_i \in \mathcal{X}_{\mathcal{M}}. \tag{7}$$

In the main analysis, I use one-to-one matching to ensure common support. This procedure restricts the sample to treated apprentices for whom suitable control matches exist and discards observations outside the region of common support (see Section 3.4). I then estimate the average treatment effect on the treated (ATT) at each event half-year k within the matched sample,

$$\text{ATT}_{k,\mathcal{M}} = \mathbb{E}[Y_{i,k}(1) - Y_{i,k}(0) \mid D_i = 1,\, i \in \mathcal{M}]\,, \tag{8}$$

where $Y_{i,k}(d)$ denotes the potential outcome of individual i under treatment status $d \in \{0,1\}$, $k = t - c(i)$ denotes the event half-year, and $\mathcal{M}$ denotes the matched sample. This one-to-one procedure yields a control group that is highly comparable to treated apprentices and achieves good covariate balance (see Section 3.4), while limiting the influence of weakly comparable controls and avoiding extreme weights and including all displaced apprentices in principle. One limitation is that not all treated apprentices an be matched. Another is that the matching approach does not make use of the full pool of untreated apprentices, which may reduce statistical precision. To assess sensitivity to the construction of the control group, Section 8.4 presents robustness analyses based on alternative weighting approaches that make use of all treated apprentices and a larger share of the untreated sample. These approaches rely on the assumption of conditional independence given the propensity score,

$$Y_{i,k}(0) \perp D_i \mid p(\tilde{X}_i), \tag{9}$$

where $p(\tilde{X}_i) = \mathbb{P}(D_i = 1 \mid \tilde{X}_i)$.[62] As alternatives to one-to-one matching, I consider an n-to-one matching approach that retains all control apprentices that coincide within a set of exact matching variables, and weights controls within each profile by cohort-specific treatment probabilities, as well as an inverse probability weighting (IPW) estimator based on propensity scores obtained from a probit model. While IPW exploits the full sample, it relies more strongly on correct propensity score specification and may be sensitive to extreme weights. I therefore additionally report estimates based on trimmed weights. The corresponding results are presented in Section 8.4.

4 Results

4.1 Pathways After Displacement

Long-term outcomes may be shaped by the initial transitions individuals experience in the short run. I therefore examine a set of indicators that capture subsequent training success and early employment outcomes within the first year after the (fictional) displacement. Table 3 shows these raw group differences, that is, without accounting for any fixed effects. To examine how these short-run outcomes are related to apprentices' sociodemographic characteristics and training firm attributes, I estimate regression models that control for a rich set of covariates.[63] A detailed notation and explanation of the estimation framework are provided in Appendix A2.3.

[62] The variables $\tilde{X}_i$ need not coincide with the matching variables X_i used to construct the baseline matched sample and may include additional or more coarsely grouped covariates to ensure stable propensity score estimation.

[63] Binary outcomes such as whether an apprentice completes VET are analyzed using probit models, which estimate how covariates affect the probability of the outcome. Outcomes that vary on a continuous scale such as the time until graduation are analyzed using linear regression models. Corresponding probit and linear regression results are shown in Tables A5 to A8 in Appendix A2.3.

	Treated	Control	Treated				Control			
	All	All	Year 1	Year 2	Year 3	Year 4	Year 1	Year 2	Year 3	Year 4
Panel a: VET-Related Outcomes										
VET Graduates *(%)*	78.7	82.0	71.2	79.2	86.2	87.7	74.0	84.4	88.1	89.6
VET Dropouts *(%)*	21.3	18.0	28.8	20.8	13.8	12.3	26.0	15.6	11.9	10.4
Time Until Graduation *(Half Years)*	4.3	4.0	6.2	4.6	2.5	0.9	6.2	4.1	2.2	0.7
Time Until Dropout *(Half Years)*	1.9	2.0	2.1	2.0	1.3	0.6	2.2	2.2	1.2	0.4
Retention by Training Firm *(First Training Firm, %)*	0.0	44.3	0.0	0.0	0.0	0.0	36.5	44.7	49.2	55.3
Outcomes in the First Year After (Fictional) Displacement										
Panel b: Continue VET (%)	84.9	87.5	90.9	92.9	81.9	40.4	93.1	95.8	85.8	40.6
Among These:										
VET Graduates *(%)*	80.4	83.3	75.5	81.6	86.6	82.7	77.9	85.9	87.9	84.6
Time Until Graduation *(Half Years)*	4.5	4.3	6.0	4.6	2.7	1.6	6.1	4.0	2.4	1.2
Time Until Dropout *(Half Years)*	2.4	2.4	2.8	2.5	1.6	1.2	2.8	2.5	1.3	0.7
Switch Of Training Occupation *(%)*	12.9	9.1	11.9	10.6	16.8	19.4	8.1	6.2	13.3	17.5
Switch VET Workplace County *(%)*	22.8	9.2	20.1	21.0	29.2	28.3	8.2	6.5	13.7	15.9
Δ in Firm FE among Training Firm Switches	0.017	0.047	0.017	0.014	0.021	0.016	0.060	0.039	0.044	0.021
Panel c: Take Up Regular Employment (%)	10.7	9.0	4.6	4.6	15.5	44.3	3.3	2.9	12.3	44.8
Among These:										
Employed Outside Training Occupation *(%)*	39.2	32.5	75.9	59.0	32.5	23.0	82.6	60.7	25.8	16.4
Employed in Specialist Occupation *(%)*	78.1	80.1	58.1	65.0	80.6	88.9	45.7	57.3	87.0	90.5
Employment Outside Training County *(%)*	41.3	27.9	51.4	48.9	39.5	35.9	50.6	42.7	26.2	19.0
Δ in Firm FE among Firm Switches	0.018	0.023	-0.002	0.037	0.015	0.020	-0.039	0.070	0.015	0.054
Panel d: Longer Gap (%)	4.4	3.5	4.5	2.5	2.7	15.3	3.6	1.3	1.9	14.6
Among These:										
Duration of the Gap *(Half Years)*	4.2	4.1	4.6	5.7	5.1	2.4	5.4	5.3	4.3	2.4
Registered Unemployment *(%)*	25.3	20.1	21.5	43.4	27.1	18.3	22.4	43.5	19.0	10.8
Unobserved in Admin Data *(%)*	74.7	79.9	78.5	56.6	72.9	81.7	77.6	56.5	81.0	89.2

Notes: This table shows raw differences in a large set of outcome variables between treated and control apprentices, and separately by the current year of VET at the time of (fictional) displacement.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table 3: Pathways After (Fictional) VET Displacement

4.1.1 VET-Related Outcomes

The most important short-run outcome is whether apprentices complete their VET program, as emphasized in studies on voluntary dropout (Wydra-Somaggio, 2021; Kotte, 2018). Panel (a) of Table 3 shows that a large majority of apprentices in the sample obtain a VET degree, with a completion rate of 82.0% among non-treated apprentices. Treated apprentices are 3.3 percentage points less likely to complete VET.[64] Columns (2) to (6) indicate that the probability of successful completion increases with the amount of time spent in VET prior to displacement.[65] A similar pattern is observed for control apprentices in Columns (7) to (10). This pattern reflects the fact that most dropouts occur early in training, often within the first probation period (Rohrbach-Schmidt/Uhly, 2015).[66] Nevertheless, graduation rates among treated apprentices remain consistently lower. The extent to which this affects the future earnings of treated and control apprentices is examined in Section 5.0.1.

Graduation probabilities vary not only with displacement and its timing, but also with individual and firm characteristics.[67] This is reflected in the estimated probit model reported in Column (1) of Table A5. Younger apprentices, natives, individuals with higher school degrees, and apprentices training at higher-quality firms, as measured by the AKM firm fixed effect, are significantly more likely to complete VET. Correspondingly, the opposite patterns emerge when considering VET dropout outcomes.[68]

Another indicator of VET success is the time required to graduate. Completing VET on schedule, or earlier, indicates high training quality or strong individual ability and can therefore be interpreted as evidence of a good match.[69] By contrast, extended training durations typically arise in cases of occupational changes or indicate lower match quality.

The average time to graduation among control apprentices is about 4.0 half-years and increases only marginally for treated apprentices, by less than half a year on average.[70] As

[64] For a very small share of treated apprentices (1.6%), the processed administrative data record VET graduation at the time they leave the closing firm. Although misreporting may play some role, this suggests that the sample does not systematically include apprentices who had already completed VET by the time their training firm closes.
[65] Among the small group of treated apprentices who are recorded as VET graduates when leaving the training firm (see Footnote 64), 73% are in years 3 or 4 of VET.
[66] The authors report that about one third of dropouts occur within the probation period (months 1-4), and about two thirds in total within the first year.
[67] Section 4.1 examines how VET graduation after displacement varies across subgroups and firm characteristics.
[68] This outcome includes both premature dropouts and exam failures.
[69] Since the exact planned duration of training is not observed, unobserved differences between treated and control apprentices within a matched pair may exist. Under the assumption that these differences do not differ systematically across groups, the result that treated apprentices require slightly more time to graduate remains valid.
[70] Note that the definitions of *VET Graduate* and *Time Until Graduation* differ slightly. For graduation status, only the first total VET episode is considered. Individuals who drop out but complete a new VET program years

expected, the duration spent in VET decreases with the amount of training completed prior to displacement, although levels remain consistently higher for treated apprentices. This pattern indicates that, regardless of timing, displacement during VET slightly prolongs the time required to complete training. Females, natives, older apprentices, and more highly educated individuals tend to complete VET more quickly, and completion is also faster among those training at better firms, as shown by the linear model estimates reported in Column (2) of Table A5 in Appendix A1.

At the same time, displacement does not substantially affect the time until VET dropout – the average duration is 1.9 half-years among treated apprentices and 2.0 among controls – and follows a similar pattern across years as VET graduation. Moreover, older apprentices and those training at higher-quality firms drop out significantly later, as shown in Column (3) of Table A5.

Due to the firm closure, none of the treated apprentices are retained by their training firm. Among control apprentices, about 44% remain with their training firm after finishing VET. The remaining controls consist of individuals who drop out of VET, switch training firms, are not offered continued employment, or decline an offer from their training firm. Related to higher dropout and switching rates early during VET, the share of control apprentices who remain with their training firm increases with training tenure. Although some of these individuals do not complete VET at their training firm, their retention rate remains substantially lower than the overall share of VET graduates who stay with their training firm, which amounts to 61% in 2010 and 79% in 2024 (Fitzenberger/Leber/Schwengler, 2025). While this analysis does not control for switches or dropouts during VET, the pattern suggests that control firms – which perform similarly to their matched closing firms by construction – offer continued employment to their apprentices less frequently than the average training firm.

The effect of displacement on later labor market outcomes likely depends on apprentices' immediate transitions after displacement. Displaced apprentices who continue VET quickly at another firm may show graduation probabilities similar to those of the control group.[71] Leaving VET for regular employment may raise short-run earnings but worsen longer-run trajectories, while unemployment spells after displacement are associated with the largest and most persistent losses.

To classify employment trajectories within the first year after displacement, I distinguish

later are therefore coded as *VET Dropout*. For *Time Until Graduation*, interruptions are not taken into account.

[71] I occasionally use the terms *displaced* and *non-displaced* for the treatment and control group, respectively, or the terms *(VET) displacement* or *firm closure* in order to better illustrate the treatment. However, the control group also includes a small number of apprentices who are displaced later during VET. Their share is only 0.5% of the control group.

three mutually exclusive outcomes: (1) continuing dual VET,[72] (2) entering regular employment,[73] (3) unemployment or being unobserved in the administrative data.[74]

4.1.2 Continuing VET

As expected, the probability of continuing VET is lower after displacement, although the difference remains modest at 2.6 percentage points. A large majority of treated apprentices (85%) nevertheless manages to continue VET at another firm within the first year, as shown in Panel (b) of Table 3.[75] In both groups, apprentices at the beginning of training are more likely not to continue VET than those in later stages. Among treated apprentices, 9% do not continue VET in the first year compared to 7% among controls, while the corresponding shares for second-year apprentices are 7% and 4%, respectively. For the control group, this pattern is consistent with evidence that most dropouts occur in the first year (Rohrbach-Schmidt/Uhly, 2015). For treated apprentices, this suggests that those affected early in training face greater difficulty in securing a new training firm than those who have already completed their first year. At later stages of training, the probability of continuing VET declines mechanically as more apprentices complete their programs. Overall, older apprentices, those with a school degree below the university entrance qualification, and those training at firms with higher AKM FE categories are more likely to continue VET in the short run, as shown by the probit estimates in Column (1) of Table A6 in Appendix A1.

Notably, even after excluding immediate dropouts, the likelihood of graduating remains lower among displaced apprentices $(80.4\%$ among treated vs. 83.3% among controls). This pattern suggests that training relationships formed following displacement tend to exhibit, on average, lower match or training quality than in uninterrupted apprenticeships. Consistent with this interpretation, the tendency toward slightly longer VET durations after displacement persists when immediate dropouts are excluded.

One plausible explanation is that treated apprentices switch training occupations more

[72] For control apprentices, this category includes remaining at the same firm and occupation as well as switching firms or occupations. For displaced apprentices, continuing VET always implies finding a new training firm. VET spells with zero or missing wages are excluded.

[73] Regular employment includes all non-VET employment relationships and excludes spells with zero or missing wages.

[74] These states are assigned hierarchically. Any VET participation during the first year takes precedence over the other states, and regular employment takes precedence over unemployment or being unobserved. The category of being unobserved captures periods without recorded dual VET or regular employment and may include activities not observed in the IEB, such as school-based VET, university studies, out-of-company vocational training (*BaE*), other educational programs, or time spent abroad. VET and employment spells with zero or missing wages are excluded and therefore fall into the unobserved category. Occasional records of participation in active labor market programs, receipt of other benefits, or job search activity outside registered unemployment are also assigned to the residual *unobserved* category.

[75] Among control apprentices, those who do not continue VET leave voluntarily for reasons discussed in Krötz/Deutscher (2022), Wydra-Somaggio (2021), and Bessey/Backes-Gellner (2008), among others.

frequently than controls, with rates of 12.9% compared to 9.1%. Strikingly, switching training occupation is even more likely when displacement occurs late in training.[76] Few sociodemographic characteristics are strongly associated with occupational switching, as shown by the estimation results in Column (2) of Table A6. Younger apprentices, those holding a university entrance qualification, and German citizens are significantly less likely to change occupations.

An even larger share of 22.8% of treated apprentices continues training in another county (compared to 9.2% among controls), with this share increasing slightly with time spent in training. This pattern is more pronounced among older apprentices, who are likewise more geographically mobile (see Column (3) of Table A6). Considering the AKM firm fixed effects as a proxy for training quality, changes relative to the initial training firm are small. Across all subgroups, these changes remain well below one standard deviation of the AKM firm fixed effect distribution.[77] If anything, both displaced apprentices and voluntary movers tend to move upward along the job ladder, with voluntary movers exhibiting slightly more favorable transitions.

4.1.3 Take-Up of Regular Employment

In contrast to the pattern for continuing VET, entry into employment during the year after the (fictional) displacement is slightly more common among treated apprentices, with rates of 10.7% compared to 9.0% among controls. These probabilities increase over time as apprentices may complete their training, as shown in Panel (c) of Table 3, and is significantly larger for males, non-natives, apprentices with no school degree and those at firms with lower AKM FE (see Column (1) of Table A7).

Upon entering employment, treated apprentices are less likely to work in adequate positions, defined as specialist occupations (78.1% vs. 80.1%) or jobs within their original training occupation (39.2% vs. 32.5% work outside). Working outside the training occupation is especially pronounced among apprentices displaced early, whereas the chances to work in specialist occupations seem to increase with the time spent in VET.[78]

A substantial share of treated apprentices (41.3%) take up employment outside the county in which they trained. The large gap relative to non-treated apprentices, amounting to 13.4

[76] Voluntary occupational switches among controls also occur mainly at late stages of training.
[77] The standard deviation of the AKM firm fixed effect estimates is $\sigma = 0.414$ (2007–2013) and $\sigma = 0.322$ (2014–2021) (Lochner/Wolter/Seth, 2024).
[78] Similar patterns are observed for control apprentices who leave VET voluntarily. Differences by sociodemographic characteristics are limited, as shown in Columns (2) and (3) of Table A7. Female apprentices are more likely to work in adequate occupations, whereas foreign apprentices switch occupations more frequently.

percentage points, may suggest that regional mobility among displaced apprentices is driven less by choice and more by the need to accept available job offers.[79] As for apprentices who continue VET, those who take up employment tend to move to firms with AKM firm fixed effects similar to those of their training firms, with a slight upward tendency for both groups. Only apprentices who switch firms in the first year of VET tend to move to slightly lower-quality firms.

4.1.4 Longer Gaps

A small share of apprentices neither continue VET nor enter employment within the first year, amounting to 3.5% among controls and 4.4% among treated apprentices, as shown in Panel (d) of Table 3. Notably, this outcome is most prevalent among fourth-year apprentices. On average, individuals in this group spend at least two years outside both VET and employment,[80] and displaced apprentices are more frequently registered as unemployed, with rates of 25.3% compared to 20.1% among non-displaced. Longer gaps are more common among natives, younger apprentices, individuals with school degrees below university entrance qualifications, and those who trained at firms with higher AKM firm fixed effects, as shown in Column (1) of Table A8 in Appendix A1.[81]

Among apprentices with longer gaps, 74.7% of treated apprentices and 79.9% of control apprentices are unobserved in the administrative data rather than being registered as unemployed.[82] The high share observed in the fourth year may suggest that some of these individuals transition into tertiary education, which is consistent with the strong association with holding a university entrance qualification reported in Column (4) of Table A8.

Taken together, the short-run pathways of displaced apprentices suggest that most continue in VET at an early stage. The share who takes up regular employment is only slightly higher among displaced than non-displaced apprentices, while extended periods outside both employment and VET appear to be relatively uncommon. At the same time, the patterns vary noticeably depending on the timing of displacement during VET and across sociodemographic groups. With these short-run outcomes in view, I now turn to the event study results to examine how they translate into longer-run employment trajectories.

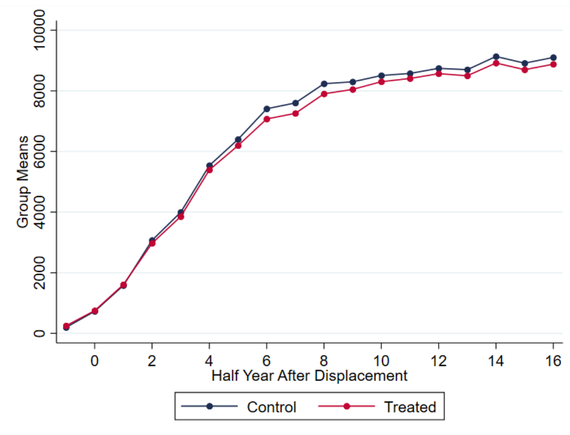
[79] Column (4) of Table A7 shows that none of the covariates significantly predict regional mobility.
[80] As the sample is right-truncated at half-year 2023/2, the average of four half-years represents a lower bound.
[81] However, as Column (2) of Table A8 shows, the duration of these gaps does not vary systematically with sociodemographic characteristics.
[82] Registered unemployment is defined as receiving unemployment benefits I or II at least once within the first year after the (fictional) displacement.

4.2 Event Study Results

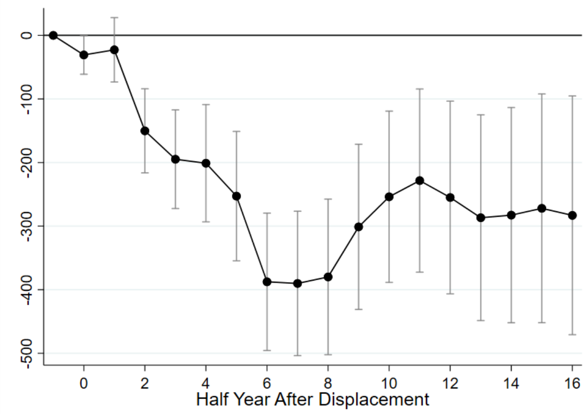
4.2.1 Earnings from Regular Employment

Figure 1: Effect of VET Displacements on Earnings

a) Earnings from regular employment (excl. VET) (levels)



b) Earnings from regular employment (excl. VET) (DiD)



Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure 1 summarizes the evolution of the main outcome variable of *half-yearly earnings from regular employment* after the (fictional) VET firm closure. Panel (a) displays raw mean earnings for treated and control apprentices over event time without controlling for any fixed effects. Half-year $k = -1$ corresponds to the reference period and is omitted as the baseline category in the event study estimation. It can be seen that earnings increase over the life cycle in both groups, but displaced apprentices exhibit a slightly flatter growth profile. Since this measure does not include VET earnings, the early event periods contain a large share of apprentices who do often not work in addition to VET therefore show zero earnings.

Panel (b) of Figure A4 in Appendix A1 shows the share of individuals in regular employment. Participation in regular employment rises sharply in earlier event periods as apprentices complete VET and enter the labor market. Nevertheless, due to substantial time spent in registered unemployment or in activities such as dual or school-based VET, as well as temporary withdrawals from the labor force, the employment share never exceeds 70%.

Panel (b) of Figure 1 reports the estimated coefficients β_k for $k = 0, \ldots, 16$ together with

95% confidence intervals.[83] Under the identifying assumptions outlined in Section 3.6, the estimated difference-in-differences event study coefficients β_k identify the average treatment effect on the treated on earnings at each event time $k = 0, ..., 16$, relative to the omitted reference period $k = -1$. The overall R^2 of 0.613 indicates that the event study specification in Equation (1) captures a substantial fraction of earnings variation, while the within R^2 of 0.420 suggests meaningful explanatory power for within-individual earnings variation over time (see Column (1) of Table A10).

Related to a declining number observations over event time due to the imbalance of the sample, the confidence intervals widen accordingly.[84] In the first few half-years after the (fictional) displacement, the estimated difference-in-differences effect on earnings becomes increasingly negative and peaks in the seventh event half-year, when displaced apprentices earn about $390€$ less than non-displaced apprentices (relative to the omitted reference period). This pattern reflects the increasing share of apprentices who complete VET and mostly enter regular employment, which generates positive earnings in the years following displacement for both the treatment and control groups. At this peak ($k = 7$), the standard deviation of earnings among the control group is about $7,000€$ so an estimated effect of $390€$ corresponds to roughly -0.06 standard deviations (relative to the omitted reference period $k = -1$). When predicting earnings for treated apprentices in the counterfactual scenario without treatment,[85] the implied average shortfall of $390€$ corresponds to a 4.5% reduction relative to the no-displacement case.

Later onward, regular employment earnings represent the main income source. In the sixth year after displacement, when individuals are on average around 25 years old, the estimated earnings shortfall is about $483€$ per year for displaced apprentices relative to non-displaced apprentices.[86] This difference corresponds to about 0.03 standard deviations of earnings in the control group (relative to the omitted reference period $k = -1$). In the counterfactual scenario without displacement, predicted earnings for displaced apprentices are roughly 2% higher. Overall, these effects are markedly smaller than those in von Wachter/Bender (2006), who report relative earnings losses of about 15% immediately and 5% in the mid run for recent VET graduates in the 1990s. Even larger losses appear in studies of displaced workers from regular employment with higher tenure and in other countries (e.g., Couch/Placzek, 2010; Jacobson/LaLonde/Sullivan, 1993; Schmieder/von Wachter/Heining, 2023).

[83] The same estimation results are reported numerically in Column (1) of Table A10 in Appendix A1 to complement the graphical presentation.

[84] Panel (a) of Figure A4 in Appendix A1 shows the number of observations per event half-year and illustrates the declining sample size over the event time. Since treated and control apprentices within each match pair are displaced in the same calendar half-year, their shares are exactly 0.5 at each point in time.

[85] Counterfactual earnings are predicted based on the estimated individual, calendar, and event half-year fixed effects.

[86] That is, 228.4€ plus 255.0€ at event times $k = 11$ and $k = 12$, respectively. See also Column (1) of Table A10.

As a comparison, Figure A5 in Appendix A1 shows the same results as Figure 1, but restricts the sample to a balanced panel. The estimated effect is slightly larger when excluding later displacement cohorts, i.e., those displaced from 2016 onward. In addition, Panels (a) and (b) of Figure A6 in Appendix A1 present the corresponding results using total earnings, which include earnings from VET. Relative to the estimates based on earnings net of VET earnings, the estimated effect is slightly smaller. This difference likely reflects that displaced apprentices remain in VET longer in later periods, which attenuates overall earnings losses. The estimated effects are smaller than 0.06 standard deviations of control-group earnings (relative to the omitted reference period $k = -1$). Although the estimates remain statistically significant for most event periods, the implied loss in total earnings due to displacement corresponds to only 3% around the peak at $k = 6$ and about 2% in the longer run, compared to the counterfactual scenario without displacement.

4.2.2 Employment and Wages

Lower earnings may arise through two channels: less time spent in employment and lower wages while employed. To disentangle these mechanisms, Figure 2 reports mean levels and event study difference-in-difference estimates for *days in employment* and *daily wages*.[87]

Panel (a) of Figure 2 shows the evolution of *days in regular employment* averaged across both groups. Days in employment increase up to event time $k = 6$, which reflects rising regular employment participation in both groups, and then remain broadly stable at around 110-120 days per half-year in later periods among both groups. Panel (c) reports average wages conditional on employment. Average wages rise steadily over event time for both treated and control apprentices, with the increase flattening somewhat in later periods. The results suggest that both time spent in employment and wages decline slightly after displacement, although the estimated magnitudes are small. For time spent in employment, the event study estimates (see Panel (b) of Figure 2) display a dynamic pattern similar to that for earnings: the estimated effect becomes more negative over the first post-displacement periods and peaks in the fourth year after displacement, when displaced apprentices work about 16 fewer days per year than non-displaced apprentices. The estimated earnings loss from displacement subsequently narrows to roughly 12 days per year in later periods. In standard deviation units, the estimated effect remains below 0.1 standard deviations of control-group employment duration throughout the estimation period (relative to the omitted reference period $k = -1$). Based on counterfactual predictions without displacement, this corresponds to about 3% fewer days spent in employment at the peak and roughly 1% at later event times. The estimated effect on daily wages (Panel (d)) is around 2€ per day throughout most event periods, although the

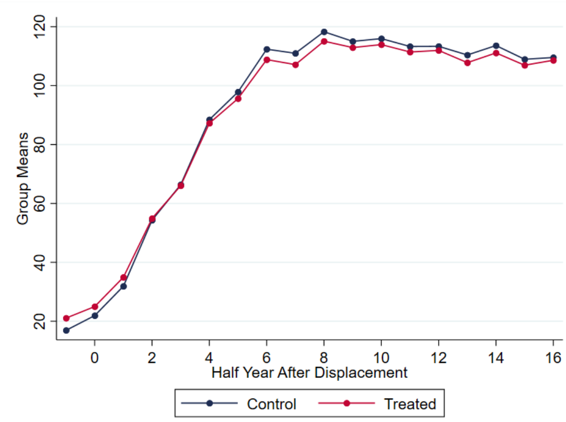
[87] Columns (2) and (3) of Table A10 in Appendix A1 provide the exact numeric results.

estimates are imprecise due to large standard errors. The point estimates correspond to implied wage losses of roughly 1% to 2% relative to the counterfactual scenario without displacement.

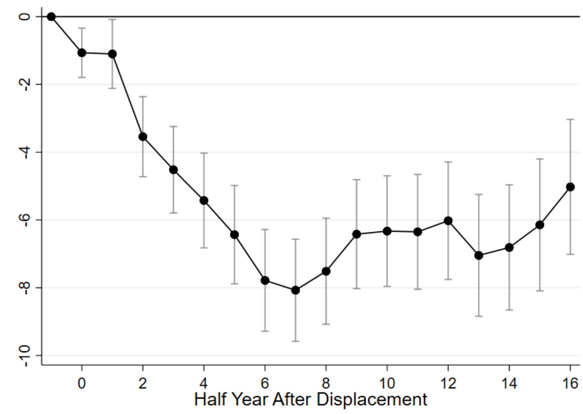
In addition, Panels (c) and (d) of Figure A6 in Appendix A1 examine registered unemployment. Panel (c) shows that displaced apprentices spend more days unemployed, averaging about 14 to 15 days per half-year from the second year onward. However, the corresponding difference in levels relative to the control group – who also entered VET in firms facing economic difficulties – remains small at roughly two days or less per half-year. The event study estimates reported in Panel (d) indicate statistically significant differences only in the first two years after displacement, when many individuals are still enrolled in VET and displaced apprentices face a slightly higher likelihood of unemployment. In the medium and longer run, the estimated effects become smaller and statistically insignificant.

Figure 2: Effect of VET Displacements on Employment and Wages

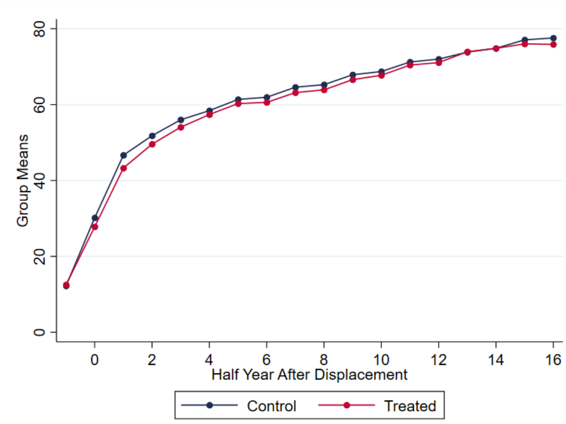
a) Days in regular employment (excl. VET) (levels)



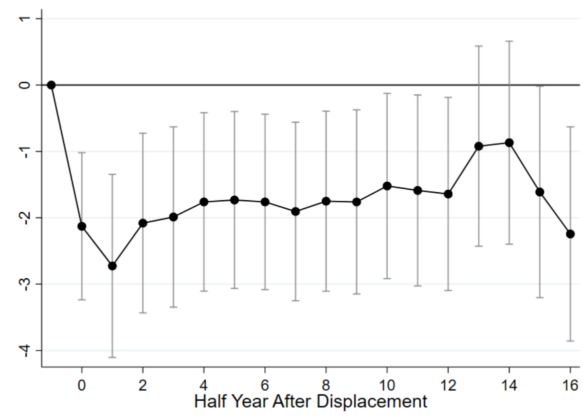
b) Days in regular employment (excl. VET) (DiD)



c) Mean daily wage from regular employment (excl. VET, levels)



d) Mean daily wage from regular employment (excl. VET, DiD)



Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

4.2.3 Job and Firm Quality

Overall, the effects of displacement on the main outcome variables are persistent but small. Figure 3 examines the evolution of additional indicators of employment quality and training success for displaced and non-displaced apprentices. Panels (a) and (b) focus on employment in specialist occupations, which correspond to the adequate requirement level for VET graduates and are measured using the fifth digit of the KldB 2010 occupational classification (BA, 2010).[88] Panel (a) shows that employment in VET-adequate occupations constitutes the most common pathway, although its prevalence declines slightly over time as an increasing share of individuals move into higher-level occupations (see Panel (c) of

[88] In addition, Table A11 in Appendix A1 shows the exact numeric results.

Figure A7 in Appendix A1). Treated apprentices are persistently less likely to work in such adequate occupations: Panel (b) of Figure 3 shows a stable estimated gap of about 3 percentage points in the event study coefficients. Turning to other requirement levels in Figure A7, displaced apprentices are more likely to work in helper occupations, i.e., jobs below the VET requirement level. The corresponding difference remains persistent at roughly 2 percentage points. By contrast, the probability of entering a complex or highly complex occupation – i.e., jobs above the VET requirement level – increases slightly in the short run after displacement but declines over time (see Panel (d)).[89] In the longer run, these effects become smaller and remain statistically insignificant for most event half-years.

Additionally, I use AKM firm fixed effects as a proxy for the quality of subsequent firms. Panel (e) of Figure 3 shows that both treated and control apprentices move upward along the job ladder on average over time and enter higher-paying firms on average. Somewhat unexpectedly, the event study estimates indicate that displaced apprentices are employed at firms with slightly higher AKM firm fixed effects than their control counterparts, and the estimated gap increases over event time (see Panel (f)). A possible explanation could be the presence of subsidies or premia paid to firms that take over apprentices from closing establishments, which apply at least in certain periods and federal states. This pattern stands in contrast to the evidence for displaced regular workers in Germany, who typically transition to firms with lower AKM firm fixed effects (Fackler/Mueller/Stegmaier, 2021; Krolikowski, 2018). The results further indicate that the modest wage penalties observed for displaced apprentices cannot be explained by sorting into lower-paying firms. If anything, transitions into slightly higher-paying firms tend to attenuate wage losses. Although the event study estimates are significantly positive in most periods, their magnitude remains very small. Differences do not exceed 0.02, even at the upper end of the confidence interval, which is small in magnitude relative to the standard deviation of AKM firm fixed effects overall.[90]

4.2.4 Training Success

Panel (e) of Figure 3 shows that the share of apprentices who complete VET rises mainly in the earlier event periods and remains largely unchanged from the sixth year onward. Panel (f) shows that displaced apprentices are less likely to have obtained a VET degree at most

[89] Two higher requirement levels, *complex* and *highly complex occupations*, typically require a university degree or advanced professional qualifications, such as a master craftsman/-woman's diploma *(in German: Meister/in)*, a business administrator's diploma *(Fachwirt/-in)*, or a certified technician's diploma *(Techniker/-in)*. These qualifications are uncommon within the short run after completing VET. See Table A9 in Appendix A1 for definitions.

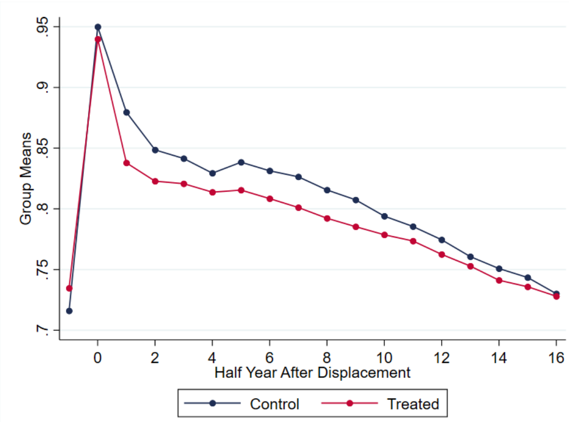
[90] Across all firms included in the estimation, the standard deviation of the firm fixed effects is $\sigma = 0.414$ for 2007–2013 and $\sigma = 0.322$ for 2014–2021 (Lochner/Wolter/Seth, 2024). In the present analysis sample, the AKM firm fixed effect distribution is somewhat more concentrated, with standard deviations ranging between 0.20 and 0.23 over event time.

event times. At the same time, they gradually catch up, and the estimated difference becomes statistically insignificant by $k = 16$. The maximum gap of about 3 percentage points occurs around the same time as the peak earnings loss. This coincidence may partly account for why the earnings gap increases until the fourth year after displacement and then narrows slightly thereafter.

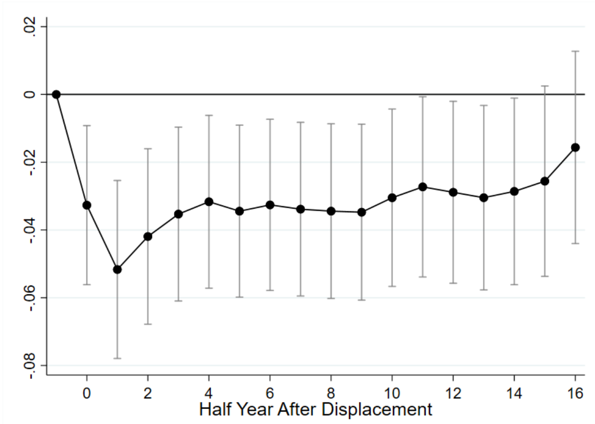
Differences also emerge when considering university graduations (see Panels (e) and (f) of Figure A7 in Appendix A1). Since university studies typically last at least three years, university graduations begin to increase only in later periods. Yet, because the sample is negatively selected with respect to educational background, university graduation is a rare outcome. Only about 6% of control apprentices complete a university degree within eight years after the displacement of their matched counterparts. Treated apprentices are, on average, about 1 percentage point less likely to do so, as indicated by the event study estimates in Panel (f). This pattern suggests that school leavers with a university entrance qualification who experience displacement do not systematically leave the VET system to pursue university studies, as discussed further in Section 6.2.

Figure 3: Effect of VET Displacements on Job and Firm Quality, and VET Graduation

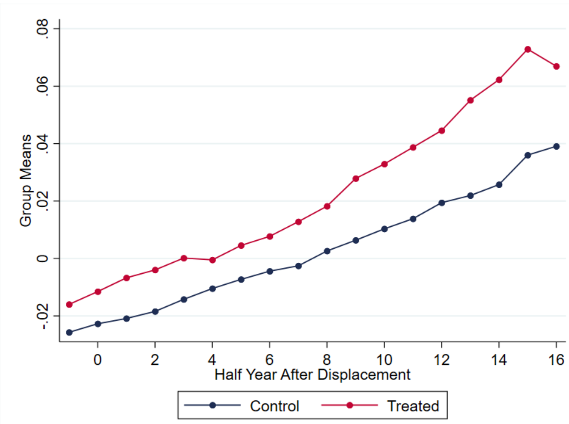
a) 1: Employed in specialist occupation (levels)



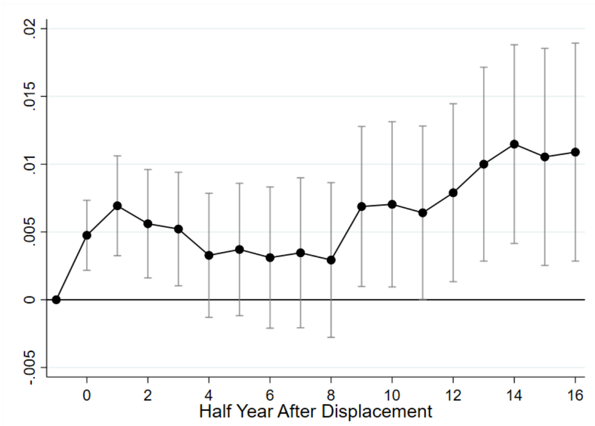
b) 1: Employed in specialist occupation (DiD)



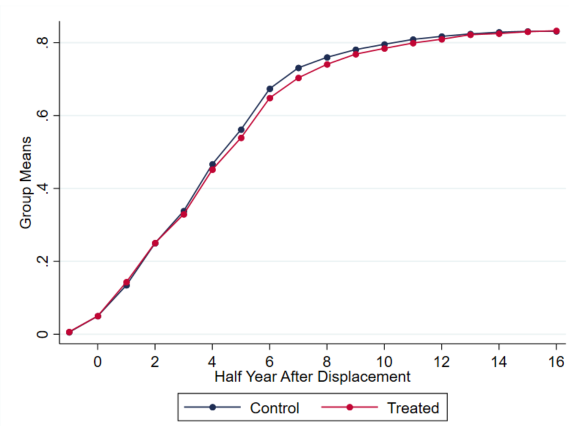
c) △ in firm FE (Reg. Employment, levels)



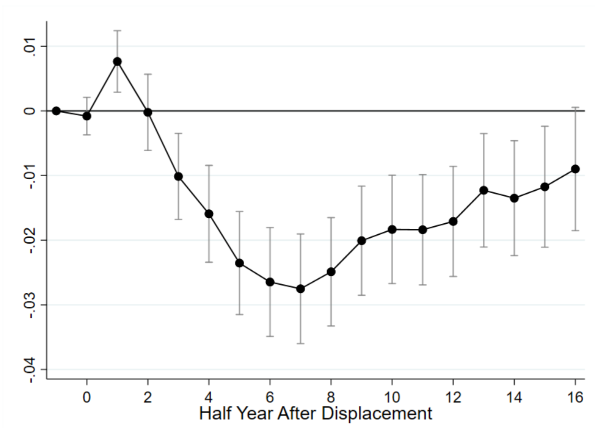
d) △ in firm FE (Reg. Employment, DiD)



e) 1: Holding vocational degree (levels)



f) 1: Holding vocational degree (DiD)



Notes: For exacts variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

4.2.5 Mobility Outcomes

Previous studies on displacement study occupational (e.g. Poletaev/Robinson, 2008) and regional mobility after job loss from regular employment (e.g., Huttunen/Møen/Salvanes, 2018; Fackler/Rippe, 2017). Against this background, I examine how occupational and regional mobility is affected after displacements from VET. Given their young age, displaced apprentices are expected to exhibit lower regional mobility than regular workers in the short run, as the German Microcensus 2022 reports that around two thirds still live with their parents (Destatis, 2023). Concerning occupational mobility, human capital theory suggests that apprentices may be more willing to switch occupations than regular employees, as they have accumulated less occupation-specific human capital. At the same time, institutional support structures may facilitate transitions by helping apprentices find a new training position more easily than regular workers. Panel (a) of Figure 4 displays the share of apprentices employed outside their initial training occupation. After the (fictional) displacement at $k = 0$, the share of individuals working outside their first VET occupation rises in both groups, with consistently higher levels among the treated. At event time $k = 16$, a slight majority among both treated and controls is no longer employed in their initial VET occupation. This suggests somewhat higher occupational mobility than reported by Eckardt (2025) among randomly drawn VET graduates,[91] which may reflect the negative selection of the analysis sample relative to the overall population of apprentices. The event study estimates indicate that displaced apprentices are about 4 percentage points more likely than controls to switch occupations in the short run.[92] Over time, the effect fades but remains significant at 2 percentage points by the end of the estimation period.

Also when measuring task similarity between the current occupation and the original training occupation – rather than using a binary indicator of occupational mobility – treated individuals exhibit larger differences than controls. Panel (c) of Figure 4 shows that over time, individuals in both groups tend to be employed in occupations that are less similar to their training occupation. Panel (d) shows the corresponding event study estimates and indicates that occupational similarity decreases on average by about 0.04 units in the short run ($k = 2$ to $k = 4$) following displacement, but then attenuates and becomes statistically insignificant in later periods.[93] Given a control-group standard deviation of about 0.4 in the short run, the estimated decline in similarity amounts to at most about 0.1 standard deviations. Since individuals may switch their training occupation not only after

[91] Using administrative data from 1975-2010, she documents that about two thirds work in their VET occupation five years after graduation.

[92] See Table A9 for precise variable definitions and Column (1) of Table A11 in Appendix A1 for the numeric estimation results.

[93] Numeric estimates are reported in Column (2) of Table A12. Task similarity is measured using the cosine similarity of task profiles at the 3-digit KldB 2010 level, following Gathmann/Schönberg (2010). Thereby, I am able to distinguish between finer occupational categories (144 in total, compared to 37 categories on the 2-digit level (BA, 2010). The measure ranges from 0 to 1, where 1 indicates identical task structures. See Appendix 3.4 for further details.

displacement but also voluntarily,[94] I additionally compare the current occupation to the individual's most recent VET occupation within the first total VET episode. Panels (a) and (b) of Figure A8 in Appendix A1 show that the results closely mirror those based on the first training occupation.

Beyond occupational mobility, apprentices may also seek employment outside the county[95] where they started their training. Panel (e) of Figure 3 shows that, in both groups, an increasing share is employed outside the county of VET start. By the end of the observation period, about half of all employed individuals work outside that county.[96] Panel (f) shows the corresponding event study estimates. Conditional on employment, displaced apprentices are more likely to work in another county than their control counterparts. In the short run, the estimated difference is about 15 percentage points. It then declines over time, falling to around five percentage points after five years and stabilizing thereafter.

Panels (c) and (d) of Figure A8 in Appendix A1 examine half-yearly employer switches as an indicator of mobility and employment instability. A pronounced spike occurs at $k = 1$, since all employed treated apprentices start working for a new employer by construction.[97] During the first two years after displacement, employer changes are slightly more frequent among treated apprentices, which suggests lower match quality or a longer adjustment period. In later years, both groups display similar mobility patterns, with the share of employer switches declining gradually from about 14% in event half-year 8 to 10% in event half-year 20. The event study estimates indicate no statistically significant differences between treated and control apprentices. This suggests that control apprentices who start training at firms in an equally poor economic situation exhibit employment stability similar to that of apprentices who experience a firm closure during VET.

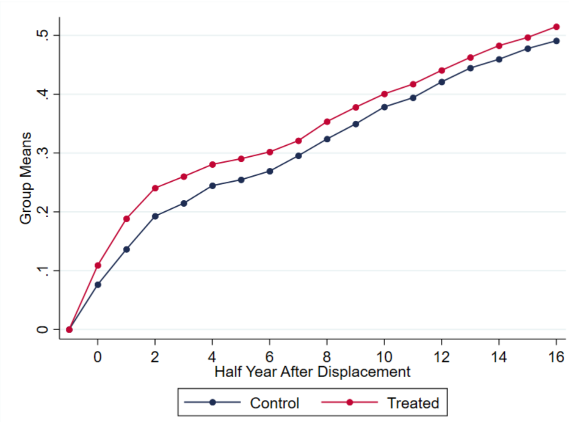
Panels (a) and (b) of Figure A9 in Appendix A1 further illustrate how retention at the training firm evolves over time. Following firm closure, displaced apprentices mechanically cannot remain employed at their original training firm, so their retention drops to zero. Among control apprentices, retention at the training firm declines steadily from 40% after one year and reaches 13% by event half-year $k = 16$. Although these results are reported at event times and therefore do not allow for precise conclusions about retention offers at graduation, the retention rate among displaced apprentices remains substantially lower than the overall share of VET graduates who stay with their training firm.[98]

[94] As Table 3 in Section 4.1 shows, 13% among treated and 9% among controls do so between event times $k = 0$ and $k = 2$.
[95] *County* refers to *(Land-)Kreise* and *kreisfreie Städte* in German, i.e. rural districts and independent cities. Currently, there are 400 such counties in Germany, see here.
[96] See Table A9 for the precise definition and Column (2) of Table A12 for the estimation results.
[97] As employment spells are aggregated to half-year intervals, about 4% of treated apprentices already exhibit the firm switch at $k = 0$, so the share at $k = 1$ is not exactly one.
[98] This share was 61% in 2010 and 79% in 2024 (Fitzenberger/Leber/Schwengler, 2025).

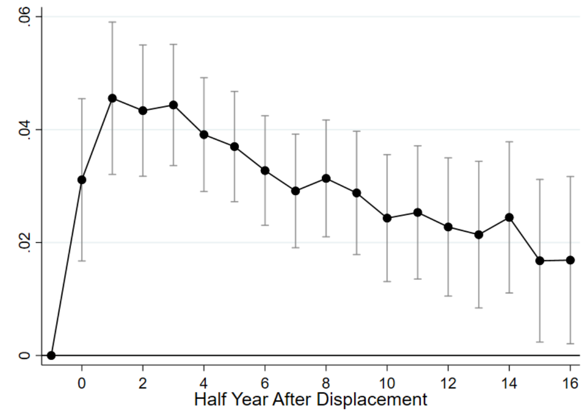
Consistent with this pattern, the event study coefficients show a large post-treatment shortfall in retention, peaking around 40 percentage points at $k = 2$ and remaining about 13 percentage points by the end of the observation period (relative to reference period $k = -1$). Allowing for firm switches during VET, a similar pattern emerges at slightly higher levels. At $k = 16$, 16% of control apprentices are employed either at their first or at their last training firm, as shown in Panel (c). Treated apprentices remain less likely to be employed at their last training firm, with only 7% doing so at $k = 16$. The estimated displacement effect is largest in the short run, at about 38 percentage points in $k = 1$, and narrows to roughly 10 percentage points in later periods. This pattern may indicate that displaced apprentices continue VET at firms that are less likely to retain them, or that they are more likely to leave their new firm again in the near future.

Figure 4: Effect of VET Displacements on Occupational and Regional Mobility

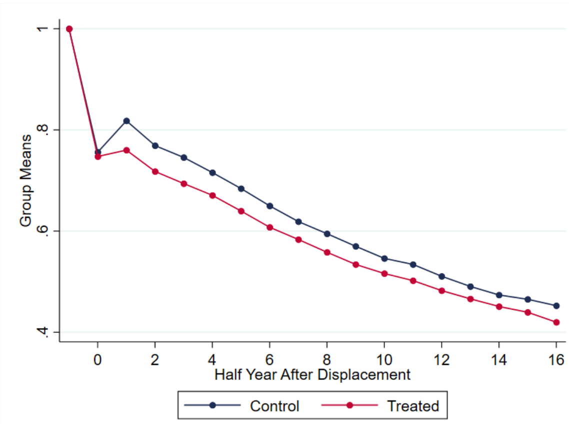
a) 1: Employment outside first VET occupation (excl. VET, levels)



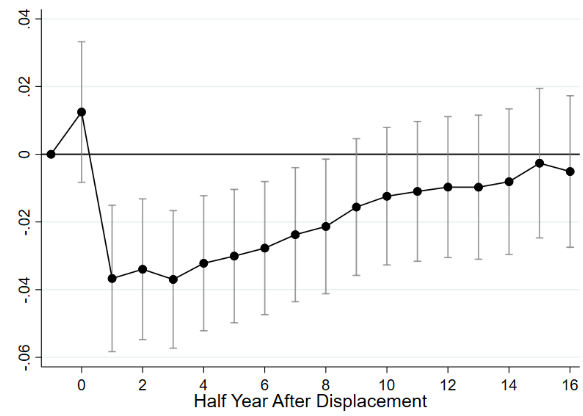
b) 1: Employment outside first VET occupation (excl. VET, DiD)



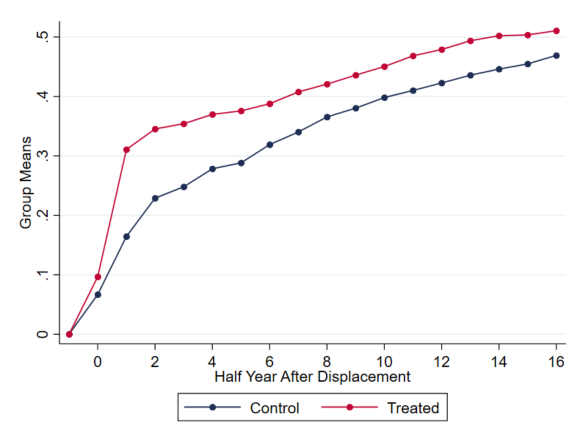
c) Occupational similarity to first VET occupation (reg. employment, levels)



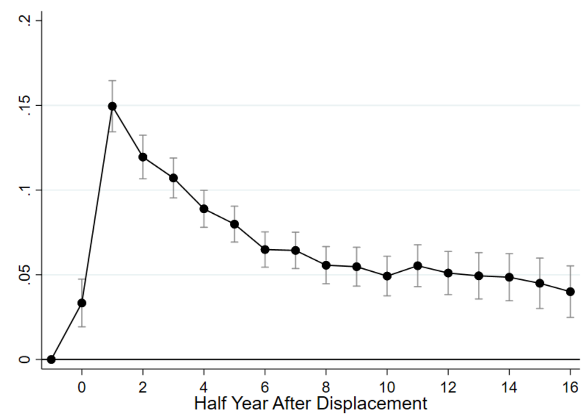
d) Occupational similarity to first VET occupation (reg. employment, DiD)



e) 1: Employed outside VET-start county (excl. VET, levels)



f) 1: Employment outside VET-start county (excl. VET, DiD)



Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

5 Mechanisms

In this section, I examine mechanisms behind the earnings effects estimated in the event study difference-in-differences framework. To this end, I decompose the displacement effect to assess the relative importance of different channels through which displacement affects subsequent employment outcomes. The decomposition distinguishes between between-group (compositional) and within-group (structural) components, in the spirit of the decomposition framework of Fortin/Lemieux/Firpo (2011).[99]

5.0.1 Graduating from VET

A key determinant of later employment outcomes is whether individuals obtain a VET degree (Patzina/Wydra-Somaggio, 2020). As indicated by the event study results for VET graduation in Panel (f) of Figure 3, displacement increases the risk of not completing VET successfully. I therefore examine how the graduation gap between displaced and non-displaced apprentices contributes to the event study estimates of earnings losses following displacement. To this end, I define $G_i = 1$ if an apprentice successfully graduates from VET during their first VET episode, and $G_i = 0$ if they drop out or ultimately fail the final exams.[100] I define G_i to be time-invariant, i.e. set $G_i = 1$ if an apprentice graduates at any point in time in the observation period. I further denote by Δ_k the unadjusted event time difference in earnings between treated and control apprentices, i.e.,

$$\Delta_k := \mathbb{E}\left[\text{Earn}_k^{k=-1}|D=1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D=0\right] , \tag{10}$$

where expectations are taken over the *observed* outcomes of treated and controls in the matched sample $\mathcal{M}$ at event time $k = t - c(i)$ after treatment, i.e., without invoking any counterfactual outcomes. The notation $\text{Earn}_k^{k=-1}$ indicates that the value in the reference category is subtracted at each point in time.[101] As the event study specification in Equation (1) contains individual fixed effects, calendar half-year fixed effects, and event half-year

[99] While the empirical setting differs, the interpretation of the resulting terms as between-group and within-group effects is conceptually similar. The specific form of the decomposition is newly proposed in this paper.

[100] In the matched data set, 194 apprentices have no information on their VET graduation status. However, this exercise requires an exact decomposition, and a very small third group of missing cases would not be informative. I therefore classify these apprentices as *dropouts* in this analysis.

[101] This means $\text{Earn}_{i,k}^{k=-1} := \text{Earn}_{i,k} - \text{Earn}_{i,-1}, k = 0, ..., 16$. Since the conditional expectation $\mathbb{E}[\cdot|\cdot]$ is taken over all individuals i in both the treatment and control group, I omit the subscript i the notation.

fixed effects and the panel is imbalanced, Δ_k is in general *not* equal to the corresponding coefficient β_k in Equation (1). However, Figure A10 in Appendix A1 show that Δ_k aligns well with the estimate β_k from the event study, i.e. the fixed effects play only a minor role.[102] Hence, I further refer to Δ_k as the event study difference-in-differences estimate of the effect of VET displacement on earnings at event time k. As shown in detail in Appendix A2.7, Δ_k can be decomposed into three different components:

$$\begin{aligned}\Delta_k = & \big(\mathbb{P}(G=1 \mid D=1) - \mathbb{P}(G=1 \mid D=0)\big) \\ & \cdot \Big(\mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=0, G=1\big] - \mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=0, G=0\big]\Big) \\ & + \mathbb{P}(G=1 \mid D=1) \cdot \Big(\mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=1, G=1\big] - \mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=0, G=1\big]\Big) \\ & + \mathbb{P}(G=0 \mid D=1) \cdot \Big(\mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=1, G=0\big] - \mathbb{E}\big[\text{Earn}_k^{k=-1} \mid D=0, G=0\big]\Big) \\ := & C_k + W_k^1 + W_k^0. \end{aligned} \tag{11}$$

The compositional term C_k captures how much of the earnings effect arises because displaced apprentices are less likely to complete VET – i.e.,

$$\mathbb{P}(G=1|D=1) < \mathbb{P}(G=1|D=0)$$

in Equation (11) – and because graduation is positively associated with earnings in the absence of displacement – i.e.,

$$\mathbb{E}\big[\text{Earn}_k^{k=-1}|D=0, G=1\big] > \mathbb{E}\big[\text{Earn}_k^{k=-1}|D=0, G=0\big].$$

The terms W_k^1 and W_k^0 measure, within each group of graduates and dropouts, the average earnings difference between treated (displaced) apprentices and non-displaced controls, respectively. The contribution of displacement penalties among graduates (W_k^1) equals the average earnings loss from displacement for graduates,

$$\mathbb{E}\left[\text{Earn}_k^{k=-1} \mid D=1, G=1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1} \mid D=0, G=1\right],$$

multiplied by the probability of graduating conditional on being displaced,

$$\mathbb{P}(G=1 \mid D=1).$$

Analogously, the dropout component enters the overall effect as the dropout-specific earnings loss from displacement, weighted by the probability of dropping out among

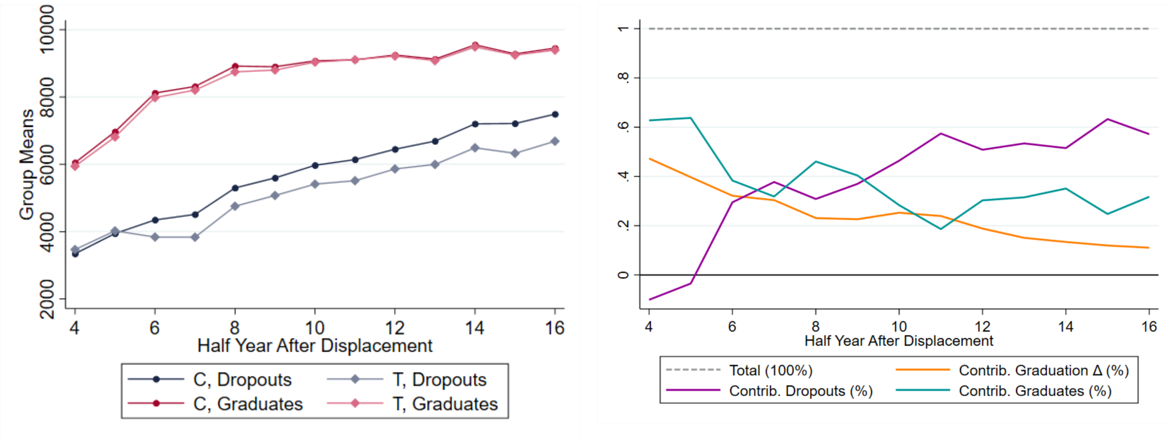
[102] See also Columns (1) and (3) of Table A23 in Appendix A1 for a numerical overview.

displaced apprentices.[103]

Figure 5: Earnings and Training Success

a) Earnings by training success

b) Contribution of compositional component and within-group components



Notes: Panel (a) shows average earnings for the respective groups. Panel (b) shows the contribution of the different components to the earnings effect Δ_k. The orange line shows the share C_k/Δ_k, i.e., the fraction of the displacement penalty explained by compositional changes. The turquoise line shows the share explained by earnings differences associated with displacement among graduates, W_k^1/Δ_k, while the purple line shows the corresponding share among dropouts, W_k^0/Δ_k.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Panel (a) of Figure 5 shows the evolution of average earnings for graduates and dropouts among both treated and controls. As expected, graduates earn substantially more than dropouts on average. Notably, the average earnings gap between displaced and non-displaced apprentices is markedly larger among dropouts. Using the decomposition in Equation (11), Panel (b) illustrates the contributions of the compositional channel C_k, i.e. the higher dropout risk among the displaced multiplied by the earnings penalty for dropouts among controls, and the within-group channels W_k^1 and W_k^0, i.e. the earnings losses from displacement within graduates and within dropouts.[104] Because graduation occurs roughly four half-years after displacement, this figure begins at $k = 4$.[105] Some time variation emerges: in the short run, most of the emerging negative earnings effect stems from the displacement penalty among graduates, likely to some extent because displaced graduates finish VET later as some had to make concessions to continue training. Over time, the contribution from within-group differences among graduates declines, while the contribution from within-group differences among dropouts increases. By five years after displacement ($k = 9$), the within-dropouts component W_k^0 accounts for the largest percentage share of the negative earnings effect. The positive signs of both within-group

[103] None of these terms are causal, since VET graduation itself is affected by displacement.
[104] A complete overview of this decomposition exercise is provided in Table A13 in Appendix A1.
[105] Panel (b) of Figure A11 in Appendix A1 presents the same decomposition starting at $k = 0$, which also captures earnings losses before graduation.

channels W_k^0 and W_k^1 indicate average earnings losses for displaced apprentices in both groups. The average earnings differences associated with VET displacement among graduates and dropouts, respectively[106] are shown in Panel (a) of Figure A11 in Appendix A1.

As the group-specific displacement penalty is much larger among dropouts than among graduates and continues to grow over time up to -900€ in $k = 15$, the contribution of W_k^0 increases steadily, despite the relatively small share of apprentices who drop out.[107]

Under treatment, the dropout probability rises by only 3 percentage points ($\mathbb{P}(G = 1|D = 1) - \mathbb{P}(G = 1|D = 0) = 0.03$ in Equation (11)). Therefore, the contribution of the compositional channel C_k remains comparatively small. This holds even though, among controls, the earnings difference between graduating and dropping out,

$$\mathbb{E}\left[\text{Earn}_k^{k=-1}|D = 0, G = 1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D = 0, G = 0\right]$$

in Equation 11 is substantially larger than the difference associated with displacement, see Panel (c) of Figure A11: it increases to about 4000€ on average earned less at its peak at event half-year $k = 7$ and later falls to about 2100€ at $k = 16$. At its peak at $k = 7$, this implies that control-group dropouts earn only 48% of what control-group graduates earn. Taken together, the large share of displaced apprentices who eventually graduate from VET contributes little to the medium-run negative earnings effect. This suggests that successful graduation at a new training firm largely offsets the displacement penalty,[108] although selection into VET graduation may differ somewhat between treated and control apprentices (see Panels (e) and (f) of Figure 3).[109] By contrast, apprentices who leave the VET system after displacement experience substantially worse outcomes, likely beyond what differences in selection into dropout alone can explain.

This pattern likely reflects the distinct circumstances faced by dropouts in the two groups: Displaced dropouts may not have anticipated their firm closure in the longer run. Thus, they face sudden unemployment risk and may be more willing to take up unattractive options compared to non-displaced. In contrast, dropouts in the control group are more likely to leave VET voluntarily. They may therefore have planned their subsequent steps in advance and can exit without the immediate risk of unemployment, as continuing VET would have

[106] These are given by

$$\mathbb{E}\left[\text{Earn}_k^{k=-1}|D = d, G = 1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D = d, G = 1\right], \qquad d \in \{0, 1\}.$$

[107] The dropout share among the treated is $\mathbb{P}(G = 0|D = 1) = 0.22$ at $k = 0$, with only slight variation over time due to the imbalance of the sample. This is shown in Panel (d) of Figure A11.

[108] As shown in Panel (a) of Figure A11 and Column (10) of Table A13, the displacement penalty among graduates is just above 200€ at $k = 5$ and remains mostly below 100€ in later periods.

[109] A formal causal mediation analysis in the sense of Huber (2019) is beyond the scope of this study.

remained a viable option. In addition, some control-group apprentices who ultimately failed the final exams, and are therefore classified as VET dropouts, may have remained at their training firm and thus experienced no disruption.

5.0.2 Delaying Graduation and Mobility

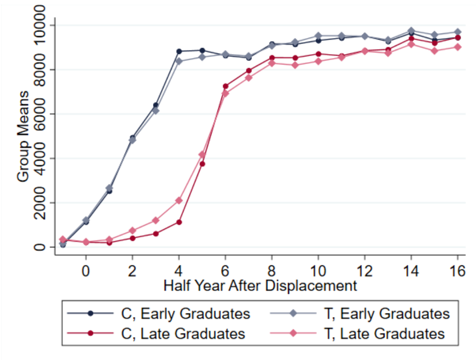
The event-study results in Panel (f) of Figure 3 reveal differences not only in whether apprentices graduate from VET, but also in the timing of graduation. Displaced apprentices are overall less likely to graduate, although they partly catch up with the control group in later event periods. This suggests delays in VET completion among the treated group. Such delays are relevant for earnings dynamics, as later graduation also postpones entry into regular employment and, consequently, subsequent earnings growth. The extent to which these delayed VET graduations drive the observed earnings effects from displacement is now determined in a following decomposition exercise. To avoid a decomposition across many graduation cohorts, I construct a binary indicator distinguishing between *early* and *late* graduates. Since both the mean and median graduation time equal four half-years in the control group, I define $T_i = 1$ for graduates taking more than four half-years and $T_i = 0$ otherwise. Panel (a) of Figure 6 shows average earnings for displaced and non-displaced apprentices by graduation timing. Late graduates follow a similar earnings trajectory that is essentially shifted to the right, with slightly steeper short-run earnings growth among control late graduates than among treated late graduates. As a result, they fall behind early graduates, especially in event periods $k = 2$ to $k = 4$, when early graduates enter the labor market while late graduates are still in VET.

Moreover, the event study results show that displacement during VET increases both occupational and regional mobility. Given their small magnitude (see Figure 4), their contribution to overall earnings losses is likely limited, though their direction remains informative. If employment outside the training occupation is associated with higher earnings, a larger share of occupational movers among displaced apprentices would generate a positive compositional effect; the same logic applies to regional mobility. Existing evidence on occupational mobility after VET is mixed: Fitzenberger/Licklederer/Zwiener (2015) find wage gains for occupation switches within firms but losses when both firm and occupation change after graduation, whereas Eckardt (2025) reports lower earnings outside the training occupation. Panel (b) of Figure 6 shows that average earnings are higher among individuals remaining in their training occupation in both groups, and that these differences exceed those by displacement status.

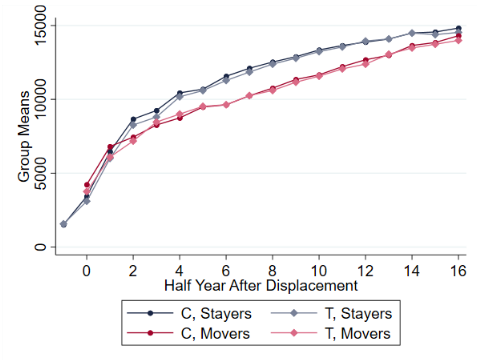
The event study results further indicate that displaced apprentices are more likely to work outside the county where they started training (see Panel (d) of Figure 4). Regional mobility may reflect unsuccessful local job search and thus lower earnings, but moves to regions

Figure 6: Earnings by Graduation Time and Mobility Outcomes

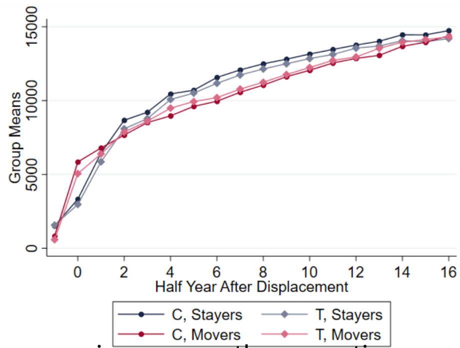
a) Earnings by graduation time (VET graduates only)



a) Earnings by occupational mobility



c) Earnings by regional mobility



Notes: Each panel shows average earnings among the respective groups over time.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

with better opportunities may also raise earnings. Fackler/Rippe (2017) find insignificantly higher earnings among regional movers after displacement from regular employment in Germany, whereas Huttunen/Møen/Salvanes (2018) document lower earnings among regional movers in Norway, with substantial heterogeneity across migration directions.

Against this backdrop, Panel (c) of Figure 6 shows event-time earnings patterns for regional stayers and movers among treated and control apprentices. In the very short run at $k = 0$, movers earn more than stayers, likely reflecting immediate transitions into regular employment outside the home region after displacement. Among apprentices employed in the same county where they started VET, controls earn more on average, whereas among those employed elsewhere, treated apprentices earn more. This may reflect selection into regional mobility: treated apprentices may move in response to favorable job offers, while control apprentices who move may do so under less favorable conditions.

I therefore derive a new descriptive formula combining all mechanisms simultaneously. The intuition is as follows: apprentices first decide whether to graduate from VET; second, conditional on graduating, how long graduation takes; third, whether to enter employment in a given event half-year; fourth, whether employment occurs in the original training occupation or another occupation; and fifth, whether employment takes place inside or outside the county where training started. One can show that the difference-in-difference estimate for earnings losses following displacement (excluding fixed effects) in event half-year k can be decomposed as follows:

$$\begin{aligned}
\Delta_k = C_k^{\text{Grad}} + C_k^{\text{Delay | Grad}} &+ \sum_{\substack{s\in\{\text{Drop.}\\ \text{Early Grad.,}\\ \text{Late Grad.}\}}} C_k^{\text{Unempl. | s}} + \sum_{\substack{s\in\{\text{Drop.}\\ \text{Early Grad.,}\\ \text{Late Grad.}\}}} C_k^{\text{Occ Move | s}} \\
&+ \sum_{\substack{s\in\{\text{Drop.}\\ \text{Early Grad.,}\\ \text{Late Grad.}\}}} C_k^{\text{Reg. Move | s, Occ Stay}} + \sum_{\substack{s\in\{\text{Drop.}\\ \text{Early Grad.,}\\ \text{Late Grad.}\}}} C_k^{\text{Reg. Move | s, Occ Move}} \\
&+ \sum_{\substack{s\in\{\text{Drop.}\\ \text{Early Grad.,}\\ \text{Late Grad.}\}}} \sum_{\substack{m\in\{\text{Occ Stay, Reg Stay}\\ \text{Occ Stay, Reg Move}\\ \text{Occ Move, Reg Stay}\\ \text{Unemployed}\}}} W_k^{\text{m, s}} \qquad (12)
\end{aligned}$$

Each C-term in Equation (12) is given by the product of two components:

1. How displacement alters the probability for a given outcome/category, relative to the relevant reference category.
2. How much that category is associated with higher or lower expected earnings relative to the relevant reference category, measured among controls only.

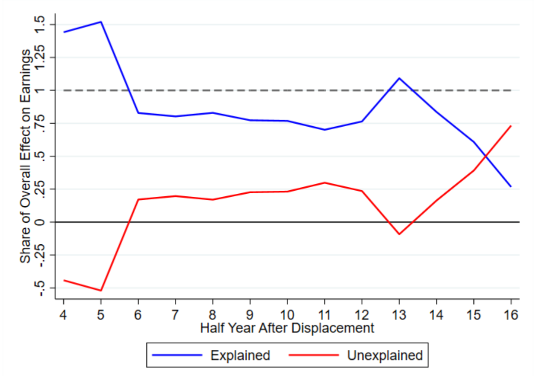
Each W-term $W_k^{m,s}$ is given by the product of two components:

1. The probability that treated apprentices fall into outcome category (m, s).
2. The earnings differences of treated against control apprentices, considering the subgroup with outcome categories (m, s) only.

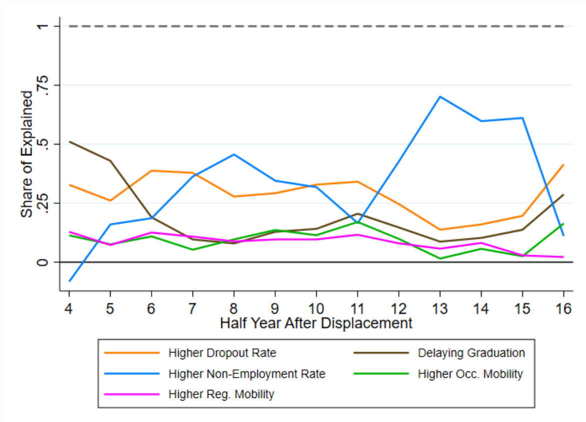
Together, the C-terms describe how earnings are affected by displacement through different mechanisms considered. In total, they give the part of the displacement effect on earnings that is *explained* by the different mechanisms. The W-terms capture the earnings penalties from displacement that remain *unexplained* by the mechanisms. The detailed formula is provided in Appendix A2.7.

Figure 7: Combining All Mechanisms: Explained Components and Contributions

a) Explained (compositional terms) vs. unexplained (within-group terms) parts of the overall effect on earnings (DiD estimate)



b) Explanatory mechanisms separately



Notes: Panel (a) shows the share total contribution of all C-terms and W-terms in Equation (12), respectively, to the effect on earnings, i.e. their sum divided by Δ_k each. Panel (b) shows each single C-term in Equation (12) divided by their sum.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Panel (a) of Figure 7 decomposes the dynamic displacement effect on earnings into explained and unexplained components. Within the first periods after (fictional) displacement (until $k = 6$, i.e., the first three years), the mechanisms even overexplain the effect, suggesting that overall – or at least within some subgroups – the displaced have higher earnings on average than the controls. During the following six years, the explanatory power of the mechanisms remains high at more than 75%. After a peak at $k = 13$, it declines in the final periods of the estimation window. At the same time, the unexplained share is positive and almost stable between $k = 6$ and $k = 12$, reaches a negative peak at $k = 13$ – again indicating slightly higher earnings among the treated than among the controls – but then reverses and increases in magnitude in the final periods. This points to different earnings trajectories across subgroups. For example, apprentices who drop out enter regular employment earlier and therefore initially earn more than those who remain in VET, but this reverses in the longer run.
Considering the different mechanisms in Panel (b) separately, it becomes clear that their explanatory power evolves heterogeneously. Delayed graduation (in brown) contributes around 50% at event time $k = 4$, but its contribution then declines rapidly. The higher dropout risk following displacement explains a relatively stable share of the earnings penalty within the first five years after displacement. In the longer run, the higher unemployment rate among the displaced explains the largest part of the effect. This shows that, when considering different mechanisms jointly, unemployment does not directly drive earnings losses immediately after displacement, but becomes even more important later on. Both the occupational and regional mobility surplus contribute positively to the

displacement effect, suggesting that switching occupations or taking up employment in other regions is (at least descriptively) associated with lower earnings compared to staying in the same occupation and region, conditional on being employed.

The detailed formula, containing 14 compositional terms and 15 within-group terms, allows the detection of differential patterns within smaller subgroups. However, these estimates may be based on very small subgroups and therefore exhibit substantial volatility. Figures A12 and A13 in Appendix A1 provide these results. Figure A12 shows how much displaced individuals in each subgroup earn less (descriptively) than the controls. Overall, among those working in the same occupation but in different regions, the displaced earn more than the controls. Displacement penalties are most negative among dropout groups and largest positive (or only modestly negative) among early graduates. Figure A13 shows how selected categories relate to earnings relative to the relevant reference categories among the controls. Switching occupations is also negatively associated with earning, especially among dropouts and when staying in the same region. For regional mobility, this association is weakest, and among both early and late graduates, those who move across regions but work in their training occupation after displacement earn even more on average than their matched counterparts in the same subgroup (cf. Figure A12). Mechanically, non-employment is associated with lower earnings compared to working in the training occupation among both dropouts and graduates, irrespective of graduation timing. More generally, the role of regional mobility already points at substantial heterogeneity in adjustment patterns after displacement, which I examine in the next section.

6 Heterogeneity Analyses

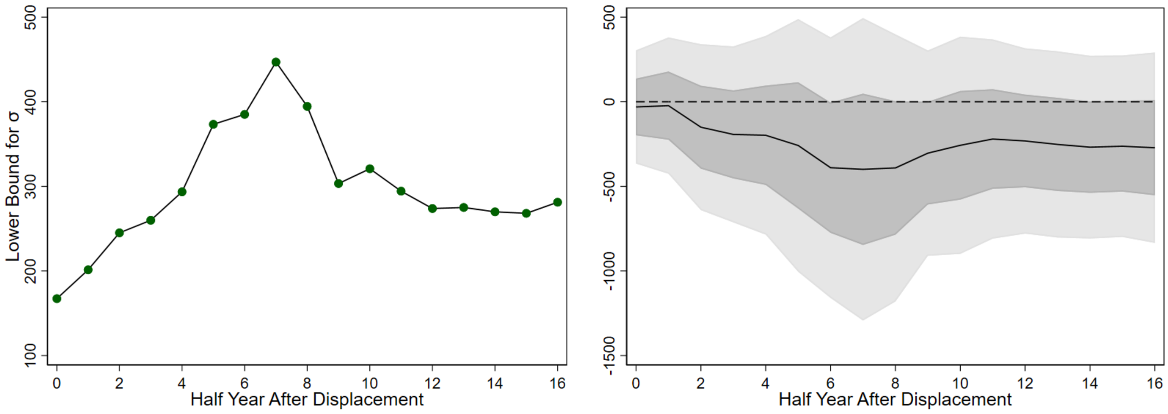
The event study results reveal a small negative effect of displacement on earnings that persists over time. However, this estimated effect may mask meaningful heterogeneity across individuals. Some apprentices may experience considerably larger earnings losses after displacement during VET, e.g. when having difficulties in finding a new VET position, while others may even benefit, e.g. when moving into higher-paying firms. To obtain a first assessment of the extent of this variation, I follow Heckman/Smith/Clements (1997) and compute a lower bound for the standard deviation of the distribution of the individual treatment effect.[110]

[110] As in the previous section, this exercise uses the event study difference-in-differences estimates without fixed effects, i.e. Δ_k as defined in Equation (10). This allows me to apply the approach as proposed by Heckman/Smith/Clements (1997) exactly. As Figure A10 in Appendix A1 shows, Δ_k does not differ substantially from β_k. Further details on this approach are presented in Appendix A2.5.

Figure 8: Variation in Individual Effects of VET Displacement on Earnings

a) Lower bound for the standard deviation of the distribution of the individual displacement effect on earnings

b) Event study estimate and bands of standard deviation



Notes: Panel (a) shows the estimate for the lower bound of the standard deviation of the treatment effect on earnings. Panel (b) combines the event study estimates with the lower bound on the standard deviation by displaying bands of one standard deviation (dark grey) and two standard deviations (light grey) around the event study estimates. Note that this neither implies that the distribution of the treatment effect is symmetric nor makes concrete statements about the coverage of these bands. Δ_k is defined as in Equation (10).
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

The results shown in Figure 8 indicate substantial variation in the distribution of individual treatment effects. As illustrated in Panel (a), the standard deviation exceeds the magnitude of the event study estimate[111] itself. Since the true distribution of treatment effects is unknown and needs not be symmetric, the bands of one (dark grey) and two (light grey) standard deviations in Panel (b) should not be interpreted as the underlying distribution of treatment effects, but merely as variation around the event study estimate. In most periods, the value 0 lies within one standard deviation of the average. This finding suggests that some individuals even experience higher earnings following displacement during VET. While these results do allow for definitive conclusions about the exact shape of the treatment effect distribution, they clearly point to pronounced heterogeneity in the individual earnings effects of VET displacement.

Section 4.1 already highlighted differences in short-run outcomes following displacement by timing and subgroup. Given the substantial heterogeneity observed also in the longer run, an important question is how baseline event study estimates vary across individuals. I examine this by interacting the treatment indicator with subgroup indicators and estimating subgroup-specific event study effects.

[111] I.e., the unadjusted event time difference Δ_k, which equals to an event study estimate when excluding the set of fixed effects.

6.1 Time of Displacement During VET

To examine how the long-run responses to VET displacement depend on when the displacement occurs during VET, I estimate the following event study specification in which the treatment indicator is interacted with the year of VET in which displacement takes place:

$$
\begin{aligned}
Y_{it} = \alpha_i + \eta_t + \sum_{k=0}^{16} \gamma_k \, \mathbf{1}(t - c(i) = k) + \sum_{k=0}^{16} \beta_k \Big(\mathbf{1}(t - c(i) = k) \cdot D_i \Big) \\
+ \sum_{k=0}^{16} \sum_{v=2}^{4} \lambda_{k,v} \Big(\mathbf{1}(t - c(i) = k) \cdot D_i \cdot \mathbf{1}(V_i = v) \Big) + \varepsilon_{it}.
\end{aligned} \tag{13}
$$

The variable V_i denotes the apprentice's current VET year at the time of the (fictional) displacement, taking values $y \in \{1, 2, 3, 4\}$. As the matching procedure pairs treated and control apprentices in the same VET year (see Section 3.4), both individuals in a matched pair share the same value of V_i. To avoid multicollinearity, the value $v = 1$, i.e. the first year, is excluded as reference category. Thus, β_k captures the average displacement effect at event time k relative to $k = -1$ for individuals displaced in VET year 1. For individuals displaced in VET year $v \in \{2, 3, 4\}$, the corresponding effect at event time k is $\beta_k + \lambda_{k,v}$ (also relative to $k = -1$). As in the main specification in (1), I cluster standard errors at the match pair level.[112]

Figure 9 illustrates heterogeneity in the event study estimates for the main outcome variables by the timing of displacement during VET.[113] Figure A15 in Appendix A1 presents the corresponding mean outcomes by timing of displacement across event time and thereby provides a benchmark for the magnitude of the estimated effects.

Partly related to smaller sample sizes, the estimates often do not differ significantly across groups. Nevertheless, several distinct patterns emerge: Panel (a) of Figure 9 shows that earnings decline in the short run only for displacements in year 4, consistent with most apprentices displaced late in VET transitioning to the labor market shortly after displacement. At the same time, most apprentices displaced earlier in VET continue training

[112] For creating the corresponding graphs, both the important coefficient estimates and their standard error are extracted from the regression output manually in Stata. The standard errors are manually computed by se$(\beta_k + \lambda_{k,v}) = \sqrt{(\hat{V}(\beta_k) + \hat{V}(\lambda_{k,v}) + \widehat{Cov}(\beta_k, \lambda_{k,v})}$.

[113] For a complete overview, Table A14 in Appendix A1 reports the numeric estimates.

at another firm.[114] Hence, this pattern likely reflects short-run retention offers for many matched controls, whereas displaced apprentices must search for a job on their own and therefore earn less initially. These short-run earnings losses decline over time and become insignificant at later event times.

As Panels (b) and (c) indicate, this short-run earnings penalty for the latest displacement reflects both reduced employment probabilities and lower daily wages among those employed. This finding implies that apprentices displaced late in VET face difficulties in finding regular employment and tend to accept lower wage offers in the short run. Over the longer run, they partially catch up to non-displaced controls, primarily due to a diminishing employment effect. Wage losses, however, remain relatively stable over time.

The earlier displacement occurs during VET, the more time apprentices typically spend in training after displacement in order to complete VET, and the later they enter regular employment. Consequently, short-run earnings losses in Panel (a) are smallest for early displacements and tend to emerge only later, when these apprentices complete VET and enter the labor market. In the longer run, earnings losses are largest for early displacements in years 1 and 2. This pattern suggests that apprentices displaced later in VET, who have accumulated more training and work experience and completed a larger share of their training program, face smaller long-term penalties. This pattern may reflect either a greater willingness among training firms to hire displaced apprentices who are closer to completing their VET, or improved prospects in regular employment due to the greater training experience they have accumulated.

Examining employment and wages separately reveals additional differences (see Panels (b) and (c) of Figure 9). For displacements in year 1, the earnings penalty appears to be driven somewhat more by lower employment probabilities, which is consistent with higher dropout rates and related difficulties in finding employment (see Table 3 in Section 4.1). For displacements in year 2, the wage penalty is more pronounced, which may suggest that they end up in lower paying jobs more often.

As the scheduled duration of VET programs ranges between 2 and 3.5 years, all displacements in the first year of VET (36% in the matched sample) clearly occur before the planned end of training. For a large majority of displacements in the second year (32%), this is also likely to be the case, although this cannot be verified directly in the data.[115] Some apprentices displaced in the third year of VET (23% in the matched sample) likely experience displacement only after their scheduled graduation,[116] although the

[114] As shown in Table 3, only 40% of apprentices displaced in the fourth year continue VET, compared to 85% among displaced apprentices overall.
[115] VET programs with a scheduled duration of two years can be completed within 1.5 years. See Footnote 4 for further details.
[116] Across the German VET system as a whole, only a small share of apprentices enter programs with a

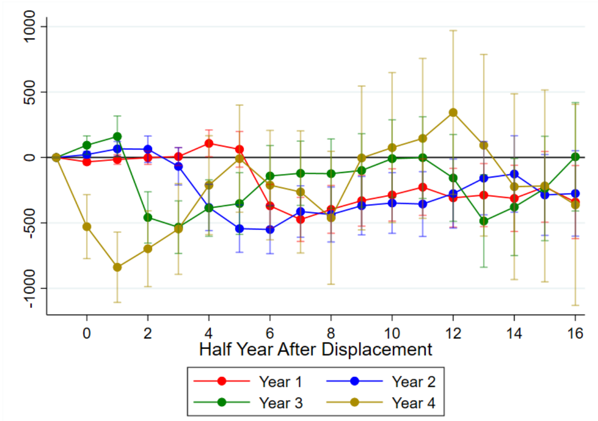
occupational codes do not allow a precise distinction in all cases.[117] This issue is likely even more pronounced in the fourth year (9%). As an additional check, I re-estimate Equation (1) separately for displacements that occur up to the second year of VET and up to the third year of VET. This approach excludes later displacements, which may be linked to prolonged training and potential negative selection (see Section 2). Figure A16 in Appendix A1 reports the corresponding event study estimates in Panel (b) and the full-sample estimates in Panel (a). When restricting the sample to displacements within the first three years of VET, the onset of the negative earnings effect shifts slightly to later event times, a pattern that is also visible in Panel (a) of Figure 9. Consistent with these findings, excluding displacements in the third year further delays the emergence of the estimated effect and leads to slightly larger average estimated earnings losses in the longer run.

scheduled duration of two years, at a stable rate of about 8–9% across cohorts over the observation period (see BIBB statistics on newly signed VET contracts, https://www.bibb.de/de/167595.php). Their share is probably somewhat higher in the analysis sample as it is negatively selected in terms of educational attainment (see also Uhly/Kroll/Krekel (2011) for a report on 2-yeat VET programs).

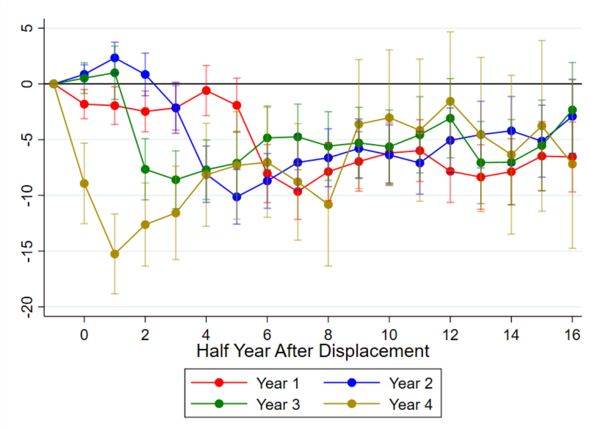
[117] For example, the 5-digit KldB 2010 codes for two frequently chosen VET programs of *salesperson* (*Verkäufer/-in*) and *retail merchant* (*Einzelhandelskaufmann/-frau*), with scheduled durations of two and three years, respectively, are identical, see BIBB (2024b)).

Figure 9: Effect of VET Displacements on Earnings, Employment, and Wages, by Displacement Year

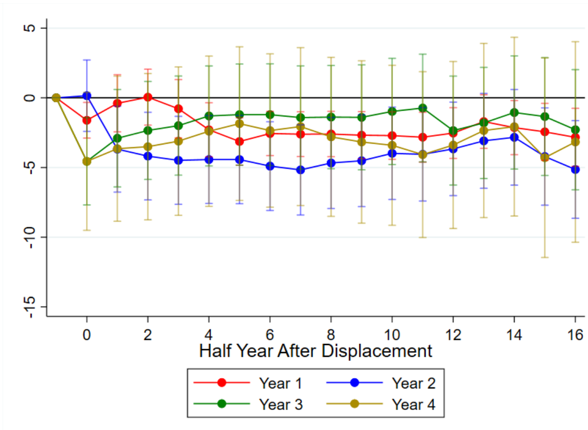
a) Earnings from regular employment (excl. VET, DiD)



b) Days in regular employment (excl. VET, DiD)



c) Mean daily wage from regular employment (excl. VET, DiD)



Notes: For exacts variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A14 in Appendix A1 provides additional insight into the mechanisms behind these patterns. Displacement in the fourth year of VET substantially increases the likelihood that apprentices work outside their training occupation, relative to matched individuals who remain in training and are close to completion (Panel d)). At the same time, displacement in the fourth VET year leads to post-displacement occupations that are, relative to the matched comparison group, the most similar in terms of task structure (Panel (e)). Panel (b) further indicates that displacement in the fourth VET year increases the likelihood of transitioning into lower-quality firms. This contrasts with earlier displacement years, where displaced apprentices tend to enter better firms than their matched control counterparts. Compared to their matched counterparts, apprentices displaced in year 4 are even more likely to find employment in adequate occupations (Panel (a)).

Displacements in year 2 appear most detrimental to training success (see Panel (c)). This may partly reflect that voluntary dropout rates among the control group decline with time spent in VET, which narrows the estimated gap between treated and controls for displacements in year 1 (see also Table 3). In line with this, apprentices displaced in year 2 work in occupations that are, on average, least similar to their initial VET occupation in the long run (Panel (e)).

6.2 Subgroup Analysis

To examine whether the treatment effect varies across sociodemographic characteristics, I generalize Equation (13) by allowing the variable V to represent different subgroup definitions. Specifically, whereas V previously captured the current year of VET, it now indexes sociodemographic groups and takes a discrete set of values:

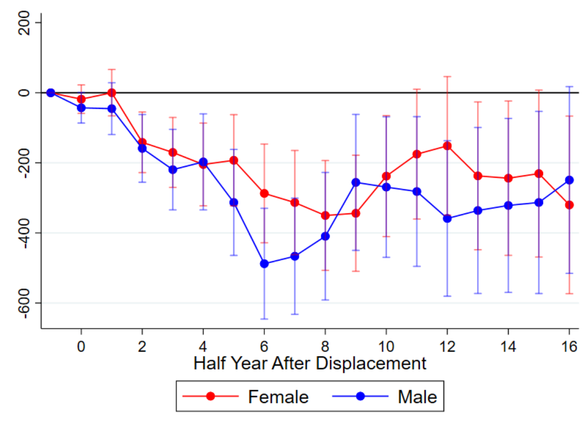
$$\begin{aligned} Y_{it} = \alpha_i + \eta_t + \sum_{k=0}^{16} \gamma_k \, \mathbf{1}(t - c(i) = k) + \sum_{k=0}^{16} \beta_k \Big(\mathbf{1}(t - c(i) = k) \cdot D_i \Big) \\ + \sum_{k=0}^{16} \sum_{\substack{v \in \mathcal{V} \\ v \neq v^0}} \lambda_{k,v} \Big(\mathbf{1}(t - c(i) = k) \cdot D_i \cdot \mathbf{1}(V_i = v) \Big) + \varepsilon_{it}. \end{aligned} \tag{14}$$

Here, V stands for a discrete variable taking a (small) set of values, $v \in \mathcal{V}$, with one base category v^0 excluded to avoid perfect collinearity. Specifically, I consider the following variables for V: gender (male or female), citizenship (German or foreign), region (East or West Germany), school degree (three categories), and firm size (four categories). The firm-size categories are chosen to match those used in, for example, von Wachter/Bender (2006): 1-9, 10-49, 50-249, and ≥ 250 employees.[118]

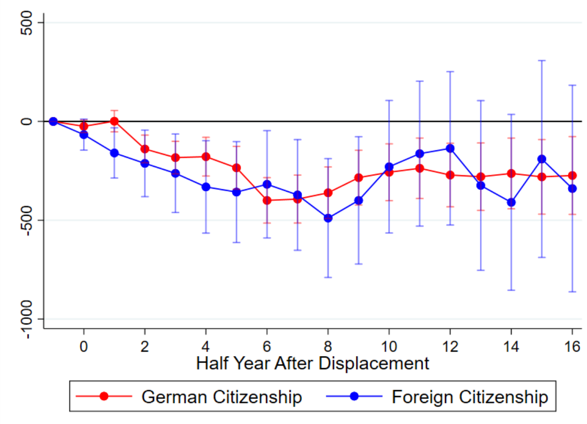
[118] For the distribution of these variables, see also Column (1) of Table A4 in Appendix A1. 22% of treated apprentices are at establishments with at most nine employees, 49% from those with $10–49$ employees, 23% from those with $50–249$ employees, and 6% from establishments with at least 250 employees, measured in the half-year of the closure each.

Figure 10: Effect of VET Displacement on Earnings, by Subgroups

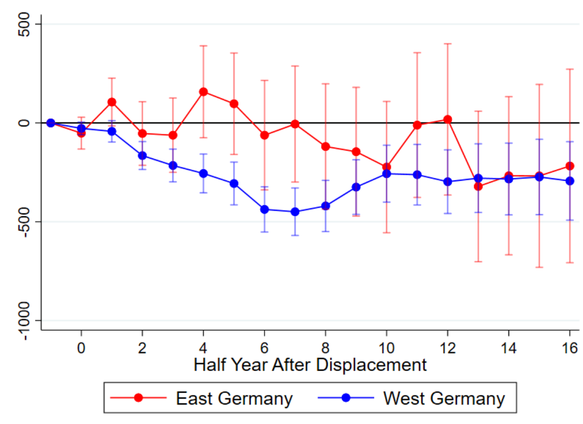
a) By gender



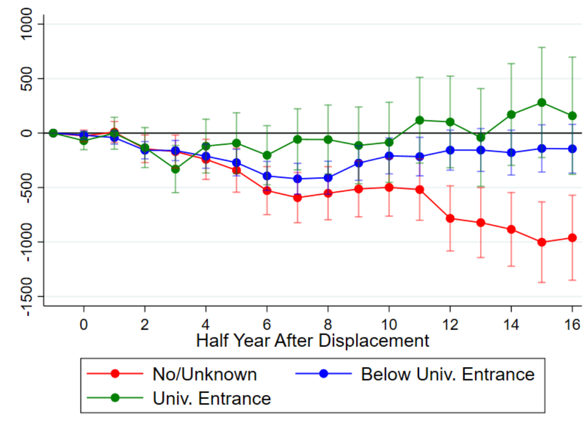
b) By citizenship (German vs. any foreign)



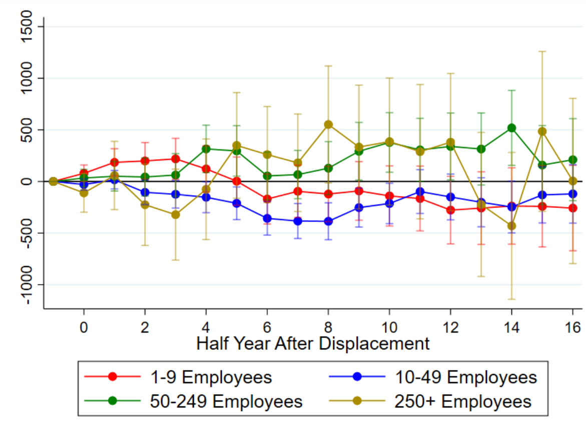
c) By region (East vs. West Germany)



d) By school degree (none/unknown, below univ. entrance, univ. entrance)



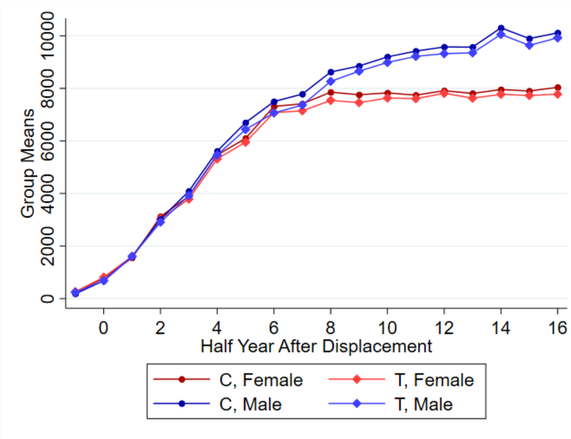
e) By firm size



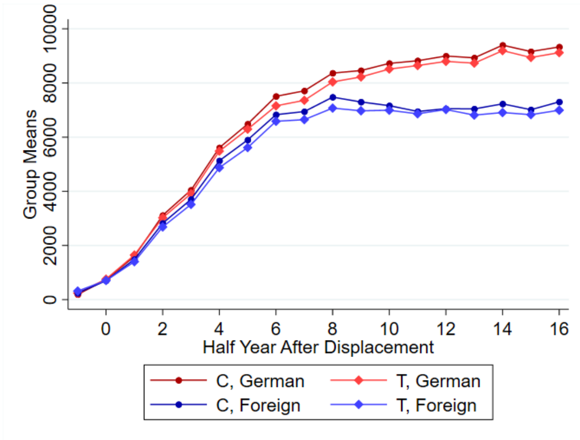
Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure 11: Evolution of Earnings, by Subgroups

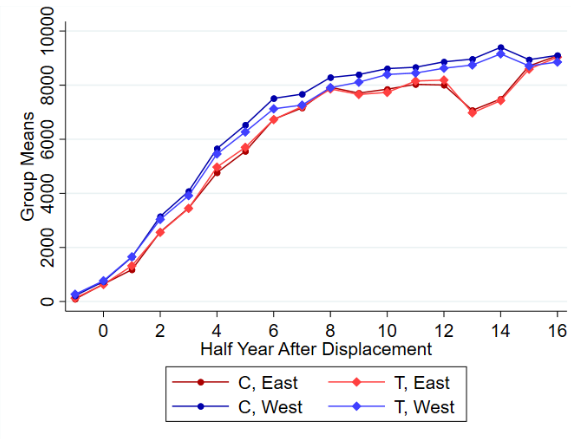
a) By gender



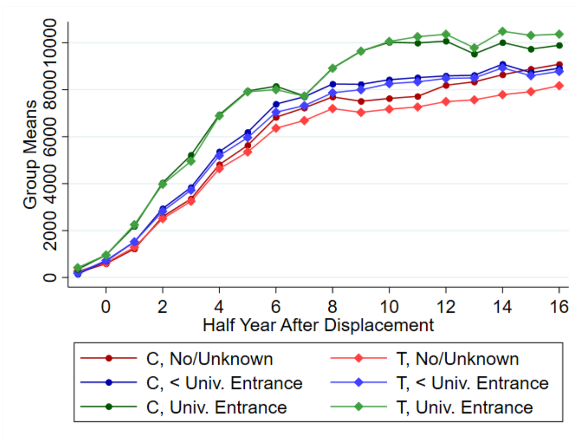
b) By citizenship (German vs. any foreign)



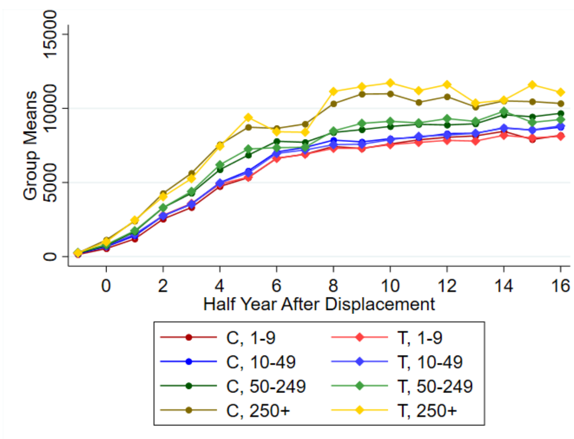
c) By region (East vs. West Germany)



d) By school degree (none/unknown, below univ. entrance, univ. entrance)



e) By firm size



Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Gender

I first examine displacement effects by gender.[119] As shown in Panel (a) of Figure 11, average earnings growth is considerably steeper for males than for females in both groups. This difference is entirely driven by wages growing more steeply among males, as illustrated in Panel (b) of Figure A23 in Appendix A1. Importantly, gender appears to be a stronger predictor of earnings than whether an apprentice experienced displacement during VET. The divergence in earnings trajectories becomes visible only around four years after the (fictional) displacement, which suggests that earnings from mini-jobs during VET and at the initial transition into regular employment are broadly similar across genders. Panel (a) of Figure 10 displays the event study estimates for earnings by gender, showing β_k for the reference group of males and $\beta_k + \lambda_{k,v_1}$ for females.[120] Although the estimated effects do not differ significantly by gender, the point estimates indicate that males experience a somewhat larger absolute earnings penalty after displacement during VET. In relative terms, however, the penalty remains larger for females due to their lower earnings levels in the control group. One possible explanation is that males more often choose smaller, i.e., less common VET occupations, for example in manufacturing, whereas females concentrate more strongly in a smaller set of common occupations (Pfahl/Unrau/Lindhorn, 2025). Heterogeneity also emerges with respect to mobility outcomes: Female apprentices are more frequently employed in VET-adequate occupations even within the control group (see Panel (c) of Figure A23). Moreover, the estimated displacement penalty is smaller for women than for men (Panel (c) of Figure A17). Among the control group, female apprentices are more likely to work outside their initial training occupation (Panel (e) of Figure A23). Moreover, the estimated displacement effect on occupational mobility is larger for women than for men (Panel (e) of Figure A17), yet they are also more likely to end up in VET-adequate positions. This pattern may suggest greater success in transferring skills across occupations.[121] By contrast, males more often respond to VET displacement by relocating to another county (Panel (f) of Figure A17). This finding is in line with evidence reported by Duan/Jost/Jost (2022), who document higher regional mobility among males displaced from regular employment in Germany.

Citizenship

Section 4.2 revealed that short-run transitions differ between natives and non-natives, with non-natives tending to be more mobile across regions and occupations (see Tables A6 and A7 in Appendix A1). Pronounced differences also emerge in the longer run. Panel (b) of

[119] The data contain information only on the two genders *male* and *female*, with no changes observed over time.
[120] The estimation results are shown numerically in Table A15 in Appendix A1.
[121] This pattern may be driven by some female-dominated occupations, such as many clerical occupations, which frequently rely on transferable skills (including digital and communication skills) that are applicable across a wide range of occupations (European Commission, 2011).

Figure 11 reports average earnings for displaced and non-displaced apprentices, separated by citizenship status (German versus non-German).[122]

As for gender, the gap in average earnings between natives and non-natives is substantially larger than the difference between the treatment and the control group. When interacting the treatment effect with citizenship status (native vs. non-native), the earnings effects of VET displacement do not differ significantly between the two groups across event time.[123] If anything, the event study point estimates suggest that apprentices with foreign citizenship experience slightly larger earnings penalties after displacement. This pattern is consistent with findings by Balgova/Illing (2024), who document larger earnings losses after mass layoffs among migrants than among Germans, although the magnitude of the effects in my study is smaller. However, non-native apprentices face larger relative losses due to their lower earnings levels in the control group. Pronounced difference also emerge for mobility and training outcomes. Among the control group, non-native apprentices are generally less likely than natives to work in a VET-adequate occupation (Panel (c) of Figure A24 in Appendix A1). Moreover, even in the absence of displacement, non-native apprentices exhibit lower rates of VET success (Panel (d)). Panel (d) of Figure 10 in Appendix A1 further indicates that displacement lowers the likelihood of VET completion for non-native apprentices. One possible explanation is that native apprentices may have a higher chance of finding a new training firm or employer, potentially due to discrimination in the VET market (see Köhler/Wiemann (2025) for experimental evidence). At the same time, I find higher regional mobility among non-native apprentices even in the longer run. This pattern is evident both in control-group levels and in the estimated response to displacement (Panel (f) of Figure A24 and Panel (f) of Figure A18). Switching occupations is generally associated with lower earnings, while working in other regions after displacement is associated with higher earnings (see Section 5.0.2). Since both mobility patterns are more common among non-natives, their effects may partly offset each other.

East vs. West Germany

Local outside options also differ markedly across regions. Although a detailed regional analysis lies beyond the scope of this study, I distinguish between East and West Germany.[124] Earnings trajectories for former apprentices in West Germany increase steadily over time for both treated and control groups on average (see Panel (c) of Figure 11). By contrast, earnings trajectories in East Germany exhibit greater volatility. Panel (c) of Figure

[122] Notably, the data do not contain information on individual's race or migration background. Citizenship is defined as time-invariant over the entire observation period; see also Section 3.2.
[123] The estimation results are shown numerically in Table A16 in Appendix A1.
[124] The assignment is based on the workplace of the first VET training firm, which may in rare cases differ from the apprentice's place of residence. Berlin is fully assigned to East Germany.

10 reports the event study estimates of the earnings effects by region over event time.[125] Perhaps surprisingly, apprentices displaced in West Germany experience substantially larger earnings losses than those displaced in East Germany. Figures A25 and A19 in Appendix A1 provide further evidence on potential reasons behind this pattern: First, Panel (d) of Figure A19 indicates that displacement reduces the likelihood of obtaining a VET degree in West Germany only. This finding is especially relevant given that apprentices in East Germany may face fewer options to continue VET after displacement (see Leber/Roth/Schwengler, 2023).[126] Moreover, among the control group, apprentices in West Germany exhibit higher overall mobility across occupations and regions (Panels (e) and (f) of Figure A25). By contrast, East-German apprentices are more likely to respond to displacement by relocating to another region (Panel (f) of Figure A19), which has been associated with higher earnings after displacement (see Section 5.0.2). Overall, these patterns suggest that the role of vocational qualifications and mobility responses differs across regions, and that displaced apprentices in West and East Germany adjust to firm closures in systematically different ways.

Schooling

Outside options vary systematically by apprentices' school degree. Educational attainment shapes both the set of alternatives to continuing VET after displacement and the signal of ability beyond formal vocational certification. Apprentices in the *No / Unknown* group, i.e. those without a formal school degree or missing information, are therefore likely to face greater difficulties in securing a new training position following displacement. By contrast, apprentices with *University Entrance* qualifications[127] generally have broader outside options, since they also have access to university studies. In addition, their higher school degree may signal higher ability to potential employers and facilitate the search for a new training firm after displacement. Following this rationale, the group with *Below University Entrance* qualifications[128] falls in between.

Panel (d) of Figure 11 illustrates average earnings trajectories by treatment status across school degree groups. In line with predictions, earnings growth is stronger for apprentices with higher school degrees in both groups, a pattern driven entirely by higher wages (see Panel (b) of Figure A26 in Appendix A1). The event study estimates in Panel (d) of Figure 10 indicate that the earnings penalty from displacement differs markedly by educational group.[129] Apprentices displaced in the *No / Unknown* group experience the largest negative

[125] The estimation results are shown numerically in Table A17 in Appendix A1.
[126] This pattern may indicate that apprentices displaced in East Germany continue VET at *better* training firms than those in West Germany, possibly due to a more favorable selection of training firms in East Germany.
[127] This group includes apprentices holding a *(Fach-)Abitur* in Germany.
[128] This group includes apprentices holding a *Hauptschulabschluss* or *Mittlere Reife* in Germany.
[129] Numerical estimates are reported in Table A20 in Appendix A1.

earnings effects. In contrast, apprentices with *University Entrance* qualifications are not significantly affected, and the point estimates even suggest slightly higher earnings in the longer run. Hence, for apprentices with the broadest outside options and the strongest signal of high ability, VET displacement does not lead to persistent earnings losses and even yields more favorable trajectories than those of the non-displaced controls, who may remain in lower-performing training firms. By contrast, apprentices without a school degree face the largest earnings penalties from displacement, which even widen over time.

Consistent with these findings, displaced apprentices with *University Entrance* qualifications are more frequently employed in (highly) complex occupations compared to their control counterparts (see Panel (b) of Figure A21 in Appendix A1). However, this pattern does not appear to be driven by higher rates of university graduation, as displaced apprentices remain less likely to obtain a university degree than their control counterparts (see Panel (d) of Figure A21). Interestingly, displacement increases the likelihood of successfully completing VET only among apprentices in the *No / Unknown* group (see Panel (d) of Figure A20). One possible explanation is that displaced apprentices receive targeted support from the responsible chambers and the BA when they search for a new training firm. By contrast, non-displaced apprentices without a school degree in the control group may remain in lower-performing firms with weaker training environments and thus face comparatively higher dropout risks or more frequent failure in the final examinations.[130]

These results may also help to interpret the heterogeneity by timing of displacement, if timing is correlated with apprentices' school degrees. Ex ante, the direction of this relationship is not obvious. Apprentices with higher school degrees tend to enter VET programs with a longer scheduled duration more often,[131] but may also complete VET earlier than scheduled more often (see Section 2). Table A19 in Appendix A1 reports the distribution of school degrees by the timing of the (fictional) training firm closure. From year 1 to year 3, the share of apprentices without a school degree declines, while the shares in the *Below University Entrance* and *University Entrance* groups increase slightly. In year 4, apprentices in the *Below University Entrance* group dominate most strongly, whereas the shares with either a university entrance qualification or without a degree or missing information no degree are substantially smaller. This pattern is likely driven both by heterogeneous selection into earlier voluntary dropouts across educational groups (see also Column (1) of Table A6) and by selection into different VET program lengths and earlier graduations, which cannot be observed precisely.

Figure A28 in Appendix A1 reports the event study estimates for the displacement effect on earnings by school degree and year of VET.[132] Differences across educational groups are

[130]As Panel (d) of Figure A26 shows, apprentices with no school degree still have the lowest VET graduation rates in both treatment groups.
[131]This holds especially for two-year programs (cf. Uhly/Kroll/Krekel, 2011).
[132]To avoid triple interactions, I estimate Equation (14) separately by (fictional) displacement timing and

largest in the first year of VET and become smaller in the second year. In later years, the estimates no longer differ significantly. In particular, in year 3 – when most apprentices are likely still in VET within the scheduled duration – displacement penalties are very similar across groups. A slightly different pattern emerges in year 4, where apprentices with a university entrance qualification fare worse than those with lower school degrees. Cautiously, this may reflect differential selection, as apprentices with a university entrance qualification may have better chances to graduate earlier,[133] which would leave a more selected group behind. These patterns suggest that, especially in cases of early displacement—when apprentices have accumulated little work experience at the training firm—the school degree becomes an especially important signal for potential training firms and can shape subsequent career outcomes.

Firm Size

The firm size of the initial firm is another dimension that has been widely studied in the job displacement literature. For regular workers, previous studies document larger and more persistent earnings losses following displacement from large firms. For Germany, von Wachter/Bender (2006) show that recent VET graduates displaced from firms with more than 250 employees experience the strongest and most persistent earnings penalties, a pattern that is consistent with evidence for regular workers in Germany (Fackler/Mueller/Stegmaier, 2021). A common interpretation is that large firms pay higher wages and offer more structured career ladders, so displacement implies the loss of firm wage premia and promotion prospects, which leads to sizeable earnings gaps after moves to smaller or lower-paying firms (Fackler/Mueller/Stegmaier, 2021).

For apprentices displaced during VET, however, a different pattern emerges. Panel (e) of Figure 10 shows that apprentices displaced from larger training firms do not suffer larger earnings losses.[134] Using the firm-size categories as von Wachter/Bender (2006) and applying them to the training firm from which the apprentice is displaced,[135] the event study estimates indicate that apprentices displaced from firms with more than 250 employees earn, on average, slightly more than their matched counterparts. The modest earnings loss from displacement overall masks substantial negative effects for apprentices displaced from small firms (fewer than 50 employees), while effects are slightly positive for those displaced from larger firms.

interact the treatment indicator with schooling within each specification.
[133] They may do so even one year in advance, see Footnote 4.
[134] The estimation results are shown numerically in Table A21 in Appendix A1.
[135] A limitation of this approach is that firm size is not matched exactly but enters only as a continuous variable in the matching procedure (see Table 1). As a result, treated and control apprentices within a matched pair may fall into different firm-size categories. The results therefore have a descriptive interpretation and do not support a causal comparison across firm-size groups.

Several mechanisms may contribute to this pattern. Larger training firms may have broader employer networks and more organizational capacity, which can facilitate placement at another training firm after displacement. In addition, training at a large firm may serve as a more favorable signal to future training firms than training at a small establishment.

The observed differences across firm size groups may contribute to understanding several of the other heterogeneity patterns: Within the sample, displaced apprentices in East Germany more often start VET at larger firms than displaced apprentices in West Germany,[136] with average firm sizes of 338 employees in East Germany compared to 64 in West Germany among the displaced. Firm size is also correlated with apprentices' educational attainment: treated apprentices without a school degree train at firms with 47 workers on average, those below university entrance qualification at firms with 84, and those with university entrance qualifications at firms with 218 employees. Gender differences in firm size are modest, with averages of 108 employees for male and 94 for female treated apprentices. Non-natives in the treatment group train at somewhat smaller firms on average (77 employees, compared to 105 among natives), but they face larger displacement penalties overall. Finally, firm size is also related to the timing of displacement: apprentices displaced in their first year of VET train in firms with about 117 employees on average, compared to 141 employees among those displaced in their fourth year.[137] Potential reasons include differences in the programs offered across firms and differences in how often apprentices shorten or extend their training programs (see Section 2).

Additional evidence in Figures A29 and A27 in Appendix A1 complements these findings by showing results for additional outcome variables. Employment is not significantly affected for apprentices displaced from large training firms (Panel (a) of Figure A29), which points to relatively successful placement after displacement from a large closing training firm. While average wages are higher in larger training firms already among the control group (Panel (b) of Figure A27), the event study estimates indicate that wages tend to increase after displacement, especially for apprentices in the second largest firm-size group (Panel (b) of Figure A29).

VET graduation outcomes show a more nuanced pattern. Apprentices displaced from the smallest firms are more likely to have graduated in the short run than their matched controls, but they are less likely to hold a VET degree in the longer run than displaced apprentices from larger firms (Panel (d) of Figure A22). This pattern may reflect faster VET completion among some of these apprentices, but higher dropout rates among others in the longer run. The event study estimates indicate that apprentices displaced from very

[136] This contrasts with the overall firm-size distribution across the both regions (see Frei/Kriwoluzky/Putzing, 2019) and may reflect differences in selection into providing training in East and West Germany (see also Leber/Roth/Schwengler, 2023).
[137] For apprentices displaced in years 2 and 3 of VET, their average training firm sizes are 113 and 132 employees, respectively.

large firms with more than 250 employees experience the smallest increases in regional and occupational mobility (Panels (e) and (f) of Figure A29) and are most likely to work in VET-adequate occupations after displacement (Panel (c)). By contrast, patterns across the medium firm-size categories, and differences in levels in the absence of treatment, are not strictly monotonic.

Taken together, these subgroup analyses show that the effects of displacement during VET differ markedly across apprentices and training contexts. Firm closures during VET are not a uniform shock, but instead interact with individual and firm characteristics.

6.3 Descriptive Evidence by Predicted Displacement Type

Beyond differences by individual educational background, outcomes may also vary with the structure of the training firm in which the displacement occurs. Apprentices trained at establishments belonging to multi-establishment firms may be offered the possibility to continue training at another site, whereas apprentices in single-establishment firms, or firms where all establishments close, do not have such an option. I refer to these two cases as *mild* and *strong* displacements, respectively. Differences in outcomes across the two groups may arise due to differences in disruption severity.

Direct information on firm structure is limited.[138] I therefore infer establishment structure from observed post-displacement training transitions. If a large share of apprentices displaced from a given establishment continue VET at the same new establishment, this likely reflects internal reallocation within a multi-establishment firm.[139] Building on this idea, I apply the following heuristic descriptive classification: If at least 5 apprentices are displaced from a closing establishment and at least 80% of them continue VET at the same new establishment, I classify this as a multi-establishment (mild) displacement. If fewer than 50% do so, I classify it as a single-establishment (strong) displacement. All remaining cases, which mainly involve small establishments or medium transition shares, are classified as unclear. Using this heuristic classification, 17% of cases are classified as mild displacements, 4% as strong displacements, and 78% remain unclear. However, because this classification is based on realized post-displacement behavior, the resulting estimates should be interpreted descriptively rather than causally.

Figure A29 in Appendix A1 compares the association between VET displacement and future

[138] In principle, firm-level information can be obtained by linking BHP establishments to firms in the Mannheim Enterprise Panel, although matching rates remain limited. In my case, this linkage works for fewer than 1% of observations, so the Mannheim Enterprise Panel does not provide a suitable data source.

[139] However, it cannot be ruled out that a firm hires a large number of external apprentices, or that displaced apprentices decline internal offers.

earnings in the main analysis sample (Panel (a)) with estimates from a sample excluding likely *mild displacements* (Panel (b)), who may receive job offers at another site of their training firm.[140] Notably, the firm type of control apprentices is unknown and not matched on, so comparisons may mix different firm structures. With this caveat in mind, excluding *mild displacements* gives slightly larger earnings losses than for the overall sample, although differences remain small, partly due to the limited number of strong displacements. This pattern is consistent with the notion that closures without an internal alternative lead to earnings losses that are at least as large as those in the main specification.

Panel (c) shows the event study estimates by predicted displacement type.[141] Apprentices classified as mildly displaced who may retain an internal option show slightly better earnings outcomes than their non-displaced controls from low-performing establishments in the longer run. Those affected by closures predicted to be strong exhibit even higher earnings in the short run relative to their controls, which may reflect faster entry into regular employment, while their earnings losses in the longer run are similar in magnitude to those in cases classified as unclear.

7 Pre-Treatment Evidence on Identification Assumptions

To interpret the estimation results shown in the previous sections causally, it must hold that the firm closure has not been anticipated, and that in the absence of firm closure, treated apprentices would have followed the same counterfactual outcome path as comparable control apprentices. This is closely related to the two main identifying assumptions: First, apprentices must not adjust their behavior prior to firm closure (Equation (5)), and second, treated apprentices would follow parallel trends to control apprentices in the absence of displacement (Equation (6)). Since such counterfactual outcomes for treated individuals are unobservable, I assess the plausibility of these assumptions by examining observed pre-treatment dynamics.

[140] The estimation results are shown numerically in Table A22 in Appendix A1.
[141] Precisely, I estimate Equation (14) by setting $V_i \in \{\text{mild}, \text{strong}, \text{unclear}\}$.

7.1 Event Study Evidence in the Pre-Treatment Period

In a first exercise, I estimate a difference-in-differences event study specification for the pre-treatment period to assess whether treated and control apprentices exhibit similar outcome trajectories prior to closure. As the main outcome variables are defined only after treatment – namely once individuals enter regular employment – pre-treatment dynamics cannot be assessed directly including the estimation of leads in the main event study in Equation (1). Instead, this section evaluates pre-treatment dynamics by analyzing outcome variables that are observed before treatment. Specifically, I study the evolution of VET earnings at the initial training firms for both treated and control apprentices. The rationale for considering this variable is that apprentices may take their own earnings evolution into account when assessing their training firm's economic performance, which may, in the worst case, result in a closure. Thus, even if apprentices in the treatment and control group display similar patterns, this does not rule out that apprentices are concerned about their training firm's performance – but, most importantly, that both groups would evaluate their training firm's economic situation similarly.

From the BHP, I use additional variables on the number of coworkers. First, I include the number of coworkers and the number of other apprentices.[142] I include these variables because inflows and outflows could provide apprentices with further information about their training firm's performance. While inflows of new workers suggest good economic performance, outflows may indicate weaker performance. Inflows of other apprentices are especially informative, as new apprentice hires suggest good firm performance: If firms face poor economic prospects, they may be less likely to hire new apprentices (Wolter/Ryan, 2011). By contrast, apprentices' outflows could reflect anticipation effects if some apprentices leave because they expect poor prospects, which may in turn affect the prospects of those who remain. Besides the number of coworkers, compositional changes in the workforce might also affect an apprentice's assessment of the training firm's performance. In particular, changes in the wage structure and the occupational composition of coworkers may signal changing firm conditions and thereby shape apprentices' expectations about future prospects at the firm. Hence, I also include the median coworker wage, as well as the shares of coworkers in two different requirement-level categories, i.e., *helper* and *specialist occupations*, respectively. Accordingly, differential pre-treatment dynamics between treated and control apprentices would speak against the identifying assumptions. For each of these variables, I estimate its association with subsequent treatment using the following difference-in-differences event study specification:

[142] To define these variables, I subtract one from the number of all employees and apprentices in order to obtain each apprentice's number of coworkers and apprentice colleagues.

$$Y_{it} = \alpha_i + \eta_t + \sum_{k=-7}^{k=-2} \beta_k \mathbf{1}(t - c(i) = k) D_i + \sum_{k=-7}^{k=-2} \gamma_k \mathbf{1}(t - c(i) = k) + \varepsilon_{it}, \tag{15}$$

where all variables are defined as in Equation (1). Since VET lasts at most four years, the estimation window is restricted to seven half-years prior to treatment.[143] Event time $k = -1$ is omitted as the reference category. Under the identifying assumptions of the main event study design, the estimated coefficients $\widehat{\beta_k}$ should not be significantly different from zero.

The estimation results of Equation (15) are shown in Figure 12. Across all outcome variables considered, there are no pronounced differences in overall trends between the treatment and control group. Notably, the sample size increases over event time as an increasing number of apprentices starts their apprenticeship, and therefore the standard errors become smaller over the course of event time. If anything, the number of coworkers and other apprentices evolves even slightly more favorably in the treatment group. Overall, I do not find evidence of differential pre-treatment trends between treated and control apprentices during the training period, and the estimated coefficients $\widehat{\beta_k}$ are small and mostly statistically insignificant.

Figure A30 in Appendix A1 provides additional information on the raw average levels of these variables among treated and control units by considering raw means in both groups. Slight level differences are observable: control apprentices have, on average, somewhat more coworkers with slightly higher wages. These differences do not undermine the validity of the parallel trends assumption. Moreover, examining the evolution of outcomes for the treatment group, no clear change emerges in the periods immediately preceding firm closure, consistent with the assumption that the closure could not have been anticipated.

Overall, since most differences in pre-treatment dynamics are statistically insignificant, the parallel trends assumption in Equation (6) appears plausible, namely that, in the absence of firm closure, treated apprentices would have evolved similarly to the matched controls in the post-treatment periods.

Notably, slight kinks in apprentices' log earnings and in coworkers' wages emerge at event time $k = -2$, where the coefficient estimates from Equation (15) are weakly significant.[144] This could make treated apprentices who later experience a closure more likely to suspect an impending firm closure than control apprentices, as these patterns indicate less

[143] For instance, individuals displaced in the eighth half-year of VET have non-zero VET earnings in event half-year $k = -7$.

[144] For both log VET earnings in Panel (a) and median coworker wages in Panel (d), $\widehat{\beta}_{-2}$ is positive and marginally statistically significant at the 5% level.

favorable wage growth for the treatment group in the subsequent period $k = -1$, i.e., shortly before treatment.

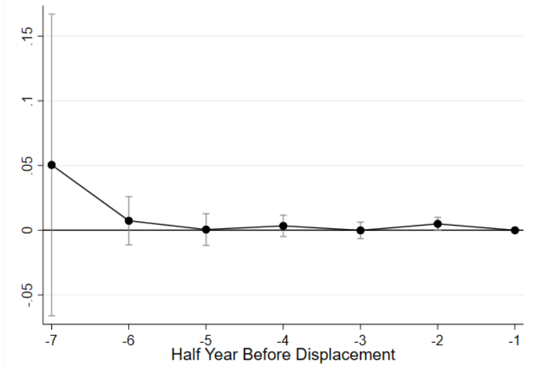
I further examine the economic relevance of these differences by calculating the smallest equivalence margin δ^* that would be needed to declare $\widehat{\beta}_{-2}$ equivalent to zero between treated and control apprentices. In other words, δ^* serves as an upper bound on the magnitude of growth differences between $k = -2$ and $k = -1$ that remain consistent with the data, and thus indicates how closely treated and control apprentices must track each other for equivalence to hold.[145] Further details on this procedure are provided in Appendix A2.6.

For log VET earnings, this marginal δ^* equals 0.005, which implies that, up to economically negligible differences of 0.5 log points, VET earnings among treated and control apprentices evolve equivalently between event times $k = -2$ and $k = -1$. For median coworker wages, δ^* equals 0.332. This implies that, even when allowing for economically small differences of up to 33 cents in daily wages, the wage trajectories over the last two half-years prior to treatment do not exhibit clear evidence of differential anticipatory dynamics. As the average level differences among the treatment group (see Figure A30 between $k = -2$ and $k = -2$ are also small, the findings are consistent with the no-anticipation assumption in Equation (5), although some anticipatory behavior cannot be fully ruled out.

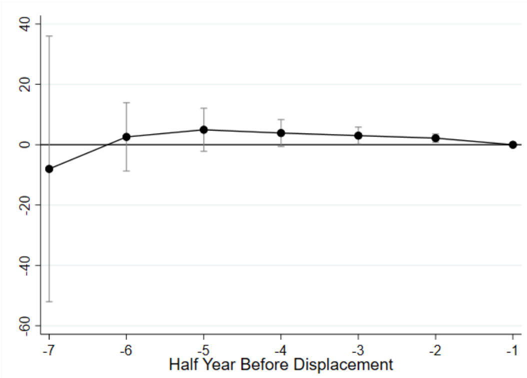
[145]This follows the same logic as the equivalence test for distributions presented in Section 3.4.

Figure 12: Pretrends Analysis

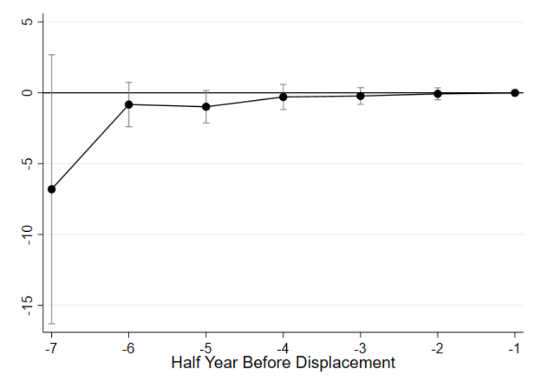
a) Log earnings from VET (DiD)



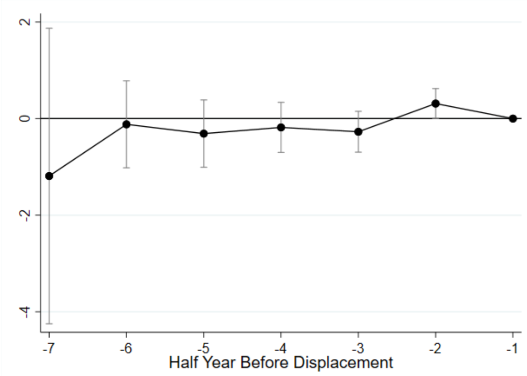
b) Number of coworkers (DiD)



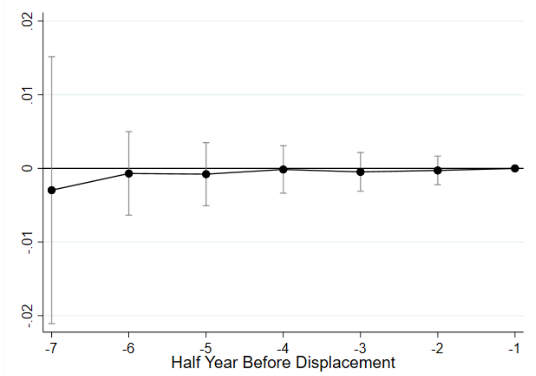
c) Number of co-apprentices (DiD)



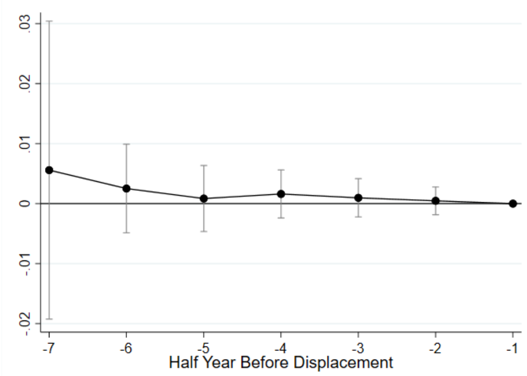
d) Median coworker's wage (DiD)



e) Share of coworkers in helper occupations (DiD)



f) Share of coworkers in specialist occupations (DiD)



Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

7.2 Monthly Pre-Treatment Dynamics Within Closing Firms

While the previous analysis shows that treated and control apprentices exhibit largely parallel VET trajectories in the years prior to treatment, firm closures may still be preceded by short-run adjustments. More specifically, employment may already decline in the months leading up to closure. However, the half-yearly event study design allows only limited conclusions about more granular employment dynamics. Since the firm-level outcomes in the event study in Equation (15) are drawn from the BHP and reported only at a half-yearly frequency, they may miss short-run adjustments in the months leading up to closure. To investigate such short-run dynamics, I make use of the employment spell data from the closing firms and examine how employment evolves during the final year prior to closure. This allows me to detect adjustments that may remain hidden in the half-yearly event study specification used in the previous analysis. Under the identifying assumptions of the half-yearly framework in Equation (1), modest employment declines in the last six months before closure are not necessarily problematic. By contrast, pronounced reductions occurring earlier would be consistent with anticipation effects. In that case, the no anticipation condition in Equation (5) would be violated, as it would imply that treatment effects emerge before the before the designated treatment half-year.[146]

Figure A31 in Appendix A1 illustrates the evolution of employment in closing firms over the twelve months prior to closure. Panel (a) shows employment levels using weights based on how many apprentices each firm displaces in the matched sample. In particular, each firm is weighted by the number of displaced apprentices it contributes to the main analysis. As a result, the figure reflects the exposure most relevant for the baseline estimates and places more weight on firms that displace more apprentices. Panel (b) presents unweighted employment levels by treating each firm equally and thus reflects employment dynamics in a typical closing firm.

Both panels show a clear and consistent pattern: employment remains relatively stable until approximately six months before closure, after which a sharp decline sets in. Part of this pattern may be driven by the strong seasonality of firm closures in my sample, as more than 90% of closures occur by the end of December. As a result, these patterns may also resemble employment dynamics within calendar years. However, there is no evidence of a gradual employment run-down in earlier months. This finding supports the validity of the half-yearly event study design in Equation (1), as it suggests that the main employment adjustment occurs primarily in the immediate pre-closure period.

In addition, Figure A32 in Appendix A1 confirms that these patterns are not mainly driven by

[146] Precisely, this would indicate that the no anticipation assumption in Equation (5) is violated for at least $k = -1$.

seasonal employment trends, as no remarkable downturns in employment are observed in the last but first year before firm closure. Figure A33 in Appendix A1 complements this evidence by displaying binned regression fits separately for the first and second half of the pre-closure year, which shows that employment does not decline during the last but first half-year prior to closure and even shows a slight increase on average. Additional descriptive evidence is provided in Figure A34 in Appendix A1, which examines the evolution of the number of apprentices as the main target group of this analysis, and the average daily wage of full-time workers[147] at the training firms during the last year before closure. These results indicate neither differential apprentice drop-out patterns nor early selective exits of higher-wage workers. Although the remaining workforce appears negatively selected on average, this pattern arises only shortly before closure and does not reflect a gradual change over a longer period.

Overall, the absence of pronounced pre-closure dynamics more than one half-year before treatment suggests that treatment timing at the half-year level is difficult to predict in the longer run and that anticipation effects at horizons longer than six months are unlikely to be a major concern. This makes the half-yearly event study design in Equation (1) a reasonable approximation in this context. Overall, this pattern is consistent with the identifying assumptions discussed in Section 3.6 and lends support to interpreting the coefficients β_k in Equation (1) as displacement effects.

8 Robustness Exercises

I conclude the empirical analysis by presenting a set of robustness exercises for the main empirical approach, i.e., estimating the effect of VET displacement on earnings using a matched event study design. Each robustness exercise addresses a specific aspect of the empirical strategy. Some analyses probe key identifying assumptions from Section 3.6, including the absence of anticipation effects (Equation (5)) and the plausibility of parallel trends in counterfactual outcomes (Equation (6)), as well as the sensitivity of the results to differential sample selection. Other exercises assess the sensitivity of the results to modeling choices and to the construction of the matched control group, including alternative matching specifications and weighting approaches such as IPW. Additional exercises examine the quality and robustness of the baseline matching procedure and

[147] For the analysis of overall employment and the number of apprentices, I use monthly aggregates that count workers irrespective of their exact employment duration within the month. In contrast, when examining wage dynamics, I weight wages by the time employed during the respective month in order to account for differences in exposure and avoid mechanically overstating wage changes driven by short employment spells.

confirm sufficient comparability of the resulting matches (see Section 3.4 for the main specification).[148]

8.1 Robustness to Alternative Event Study Specifications

This section presents robustness checks based on alternative event study specifications. These exercises assess the sensitivity of the results to alternative functional-form choices in Equation (1) and address potential concerns regarding potential misspecification or omitted variables.

First, as discussed in Section 3.6, individual fixed effects may be less informative over the full estimation window. In early event periods, most individuals are still in VET, so level differences in outcomes such as earnings outside VET are often close to zero and may not capture systematic earnings differences that emerge later in the career. To address this concern, I estimate the following event study specification without individual fixed effects:

$$Y_{it} = \eta_t + \sum_{k=0}^{16} \beta_k \mathbf{1}(t - c(i) = k) D_i + \sum_{k=0}^{16} \gamma_k \mathbf{1}(t - c(i) = k) + \varepsilon_{it}. \tag{16}$$

The resulting estimates for the main outcome *earnings from regular employment*, presented in Panel (b) of Figure A35 in Appendix A1, closely mirror the main event study results in Panel (a), which rely only on treatment indicators and calendar half-year fixed effects.[149]

Second, order to account for region-specific and occupation-specific shocks that could affect treated and control apprentices differently, I estimate the following alternative specification:

$$Y_{it} = \alpha_i + \sum_{k=0}^{16} \beta_k \mathbf{1}(t - c(i) = k) D_i + \sum_{k=0}^{16} \gamma_k \mathbf{1}(t - c(i) = k) + \rho_{r(i)ct} + \kappa_{o(i)ct} + \varepsilon_{it}, \tag{17}$$

[148] All additional analyses from the previous sections are also available for the robustness exercises upon request. This includes balance tests, short-run outcomes, event study results, as well as the analyses of mechanisms, effect heterogeneity, and the analysis of pre-treatment dynamics.

[149] See also Columns (1) and (2) of Table A23 in Appendix A1 for a numerical overview. Importantly, the R^2 shrinks down from 0.613 in the main specification to 0.178 when excluding individual fixed effects.

where $\rho_{r(i)t}$ captures workplace county by calendar half-year fixed effects, with $r(i)$ denoting the workplace county of individual i at event time $k = 0$, and $\kappa_{o(i)t}$ captures occupation by calendar half-year fixed effects, with $o(i)$ denoting individual i's VET occupation measured at the two-digit level of the KldB 2010 classification. The results for the earnings outcome, shown in Panel (c) of Figure A35, closely resemble the main event study estimates that include only overall calendar half-year fixed effects. This suggests that time-varying county-specific or occupation-specific shocks are unlikely to explain the estimated effects, which supports the plausibility of the parallel trends assumption in Equation (6) within the matched sample.[150]

Third, in order to address the possibility of omitted variables bias in the main specification (Equation (1)), I further estimate an enriched event study specification:

$$Y_{it} = \alpha_i + \eta_t + \sum_{k=0}^{16} \beta_k \mathbf{1}(t - c(i) = k) D_i + \sum_{k=0}^{16} \gamma_k \mathbf{1}(t - c(i) = k) + \zeta_i x_i + \varepsilon_{it}. \qquad (18)$$

where x_i includes half-year age polynomials, local unemployment rates (by county and year), and occupation fixed effects (37 KldB 2010 2-digit occupations plus one category for missing occupation, i.e., non-employment). The results for earnings, shown in Panel (d) of Figure A35, closely resemble the main event study estimates that rely only on the treatment and the set of fixed effects, which indicates that adding further variables does not change the findings.[151] The stability of the estimates across these alternative specifications provides additional reassurance that the baseline results are not driven by particular modeling choices, and is broadly consistent with the conditional mean independence assumption in Equation (2) underlying the event study design in Equation (1).

8.2 Robustness to Treatment Definition and Pre-Closure Dynamics

Next, I empirically assess the identifying assumptions of the difference-in-differences event study design by considering alternative definitions of VET displacement and alternative specifications of the pre-closure dynamics. Together, these exercises examine key identifying assumptions as well as the sensitivity of the results to selective sample

[150] See also Columns (1) and (5) of Table A23 for a numerical overview. The Within R^2 decreases substantially, consistent with these additional fixed effects absorbing a large share of within-individual variation.

[151] See also Columns (1) and (5) of Table A23 for a numerical overview. Including these variables increases the R^2 from 0.613 to 0.684, suggesting that they contribute meaningfully to the model's explanatory power. However, as occupational choice, and possibly also the place of residence (which determines the local unemployment rate) are themselves influenced by the treatment, this specification is included only as an additional exercise.

selection.[152]

The first robustness check relaxes the definition of the first VET episode in which firm closures are considered. In the main specification, apprentices are classified as displaced only if the closure occurs at the firm where they started VET and within their initial training occupation. This restriction ensures that treated and control apprentices follow comparable VET trajectories without prior firm or occupation switches. However, firm closures that occur at a subsequent training firm or after an occupational switch may have similar consequences and therefore should not necessarily be excluded when studying exposure to firm closures during VET. To account for these cases, I broaden the definition of the first VET episode to allow both firm and occupation switches.[153]

The resulting sample is comparable to the main sample in terms of sociodemographic characteristics, for example showing a similar distribution of school degrees[154] and being employed at closing training firms of similar sizes.[155] While switching firms may in general increase total time spent in VET, the timing of displacement is not substantially delayed.[156]

Panel (b) of Figure A36 in Appendix A1 presents the resulting event study difference-in-differences estimates of the effect on earnings. Compared to the main specification shown in Panel (a), differences are minimal, and none of the estimated β_k coefficients differ significantly. Confidence bands become slightly narrower, which reflects the larger matched sample of $25,149$ displaced apprentices when allowing for firm and occupation switches. The stability of the estimation results supports the plausibility of the parallel trends condition in counterfactual outcomes (Equation (6)), as treated and control apprentices remain comparable even under a broader definition of VET episodes that is only imperfectly captured by the matching approach.[157] Importantly, this robustness check further alleviates concerns about selective sample composition prior to treatment, as the sample composition remains broadly similar when expanding the pool of displaced apprentices and the estimated effects change only modestly.

[152]Columns (2) to (4) of Table A24 in Appendix A1 give a numerical overview of the estimation results for each exercise.

[153]When matching displaced apprentices to controls, I impose exact matching on these switches to maintain comparability in VET paths.

[154]22% have no school degree, 59% have a school degree below university entrance qualification and 18% have a university entrance qualification.

[155]22% in firms with nine employees or less, 49% in firms with $20-49$ employees, 23% in firms with $50-249$ employees, and 6% in firms with at least 250 employees.

[156]32% of all closures still happen in the first year of VET, 31% in year two, 26% in year 3, a slightly higher share of 11% in year 4, plus a share smaller than 1% even later, counted from the start in the first program.

[157]While I control for firm and occupational switches using a dummy variable, I do not account for their timing, whether they occur jointly, the similarity between the respective occupations, or the regional distance between or industries of the respective firms.

Another concern is that the closure may in some cases be anticipated some time in advance. If anticipation occurs well before the actual closure, apprentices who remain at the training firm until the calendar half-year of closure may form a selected subgroup of those whose firms will close in the near future.[158] Apprentices who are especially likely to anticipate closure may be those training at firms experiencing strong and persistent economic decline. Therefore, I exclude all matched pairs in which the treated apprentice's firm falls within the bottom 10th percentile of relative employment growth between $k = -2$ and $k = 0$ within the sample.[159] This cutoff corresponds to relative employment declines of ˘22% or more. After removing these cases, $20,222$ matched treated apprentices remain.

While apprentices in firms with strong employment declines do not appear negatively selected overall,[160] their post-displacement trajectories could differ if closure becomes predictable. Excluding these cases therefore provides an indirect check on the role of anticipation. If the estimates remain similar, this suggests limited scope for systematic pre-closure adjustments, such as an earlier search for a new training firm. Panel (c) of Figure A36 in Appendix A1 shows the resulting event study difference-in-differences estimates of the effect on earnings. Despite potential offsetting forces—for example, earlier search time on the one hand and remaining unobserved differences on the other—the estimates remain close to the main specification. This pattern suggests that gradual economic declines prior to firm closure do not materially confound the estimated displacement effects. This reduces concerns about anticipation-driven adjustments under predictable closures (Equation (5)), as even apprentices who may have observed declines at firms that close soon do not appear to anticipate the upcoming closure.

A further concern is that apprentices who leave the closing firm before the estimated closure date, and are therefore classified as treated, may experience the treatment only ex ante. These early leavers may have anticipated the closure and adjusted their behavior in advance, which would enable them to transition into better opportunities compared to the

[158] I do not extend the analysis to firms that close later for several reasons: as VET contracts last only a few years, many anticipated closures would occur after individuals have already completed VET. In such cases, some apprentices would not receive a job offer, others might switch firms directly after graduation, and some would remain and be displaced as regular employees. These scenarios cannot be cleanly distinguished in administrative data. At the same time, restricting the analysis to early closures during VET would capture only a subset of relevant cases. The half-yearly treatment definition provides a reasonable compromise by identifying displacements that occur during VET while still allowing for limited anticipation.

[159] Precisely, I compute for each treated apprentice i the relative employment growth at their training firm $f(i)$ by:

$$\text{RelChange}_i := \frac{N^{\text{empl}}_{f(i),k=0} - N^{\text{empl}}_{f(i),k=-2}}{N^{\text{empl}}_{f(i),k=-2}},$$

with $N^{\text{empl}}_{f(i),k}$ being the number of employees at training firm $f(i)$ of individual i at event time k. Doing so, I always compare employment numbers at two subsequent calendar years, which reduces concerns about seasonal changes in employment.

[160] The distribution of school degrees in this subsample remains close to the main sample, with 21% without a degree or without information, 59% with a degree below university entrance qualification, and 19% with a university entrance qualification.

overall sample. To address this issue, I exclude all treated apprentices who do not leave the closing establishment within a 15-day window around the estimated closure date together with their matched counterparts.[161] This leaves $17{,}576$ displaced apprentices, of whom $17{,}179$ exit exactly on the predicted closure date. A brief comparison with the previous exercise suggests that early exits are not systematically more common at firms with strong declines, which alleviates potential concerns that economic performance is the main driver of leaving in advance.[162] Furthermore, the excluded group does not appear to be selective per se.[163]

Panel (d) of Figure A36 reports the resulting event study estimates. In the short run, the pattern changes noticeably. Unlike in the main specification, the causal effect on earnings is significantly stronger from the first post-event period onward once apprentices who leave the closing firm shortly before the closure date are excluded. At later event times, the pattern partially reverses, with earnings losses becoming slightly smaller. However, due to large standard errors, these differences are not statistically significant. Overall, this pattern is consistent with early leavers being positively selected relative to those who remain until closure, at least in the short run. However, VET graduations appear to play only a minor role for these exits.[164] Likewise, there is no evidence that the results are driven by voluntary dropouts irrespective of the upcoming closure: a similar share of 37% (compared to the main sample) is displaced in the first year.[165] However, the timing of displacement points to one potential mechanism: only 4% of those displaced in the fourth year of VET are in the remaining sample, compared to 9% in the overall sample.[166] This may cautiously indicate different short-run dynamics among apprentices displaced very late in VET, i.e., shortly before or around their final examinations. In particular, leaving slightly earlier rather than in the final weeks before the exams appears beneficial, as these apprentices are able to find an alternative position for the last months of training.

Overall, these results suggest that apprentices who exit before the closure date tend to experience better short-run earnings outcomes, consistent with early exits reflecting quicker reallocation into alternative training positions. At the same time, this evidence does not directly contradict the no-anticipation restriction in Equation (5) at the half-yearly level, since these apprentices are still observed in their training firms at the beginning of the last

[161] In the main sample, 78% of treated apprentices leave within 15 days of the predicted closure. As discussed in Section 3.2, closure dates are inferred from employment spells and may not coincide with the official firm closure.
[162] Using the sample from the previous exercise as the base group, a very similar share of 79% would be excluded for leaving the firm earlier (see Footnote 161).
[163] For instance, school degrees are distributed very similarly in the remaining group as in the overall sample: 22% have no degree or missing information, 59% have a school degree below university entrance qualification, and 20% have a university entrance qualification.
[164] The share of those reported as VET graduates is 1.5% and thus closely resembles the share in the overall sample. Still, these shares should be treated with caution due to potential misreporting (see Footnote 64).
[165] Dropouts occur primarily at the beginning of VET (Rohrbach-Schmidt/Uhly, 2015).
[166] 34% are in the second year and 24% in the third year in the remaining sample.

half-year prior to closure. While the evidence in Section 7 does not point to strong anticipation effects at earlier event times, this exercise indicates that selective exits prior to closure can still matter in the short run. However, if anticipation leads some apprentices to leave even earlier, this likely reflects transitions into better alternatives, and thus comparatively more favorable short-run outcomes than for apprentices who remain until shortly before closure. In this sense, the baseline estimates should be interpreted as capturing effects for apprentices who remain in the closing firm until the final pre-closure period ("late stayers"), rather than for the full pool of apprentices ever exposed to firm closures.

At the same time, including all apprentices who start training at firms that close within the next four years (according to Fersterer/Pischke/Winter-Ebmer (2008)) would make the interpretation less clear.[167] For some early leavers, closure would occur only after VET completion. The estimated effects would then capture not only disruptions to VET but also the loss of retentions at graduation and job loss shortly thereafter.[168] Given that VET examinations take place every six months, the half-yearly treatment definition represents a transparent trade-off between capturing apprentices directly affected by firm closures and maintaining a consistent interpretation of treatment across individuals. Nevertheless, the resulting estimates should be interpreted with caution, since selective exits shortly before closure may still affect the short-run dynamics.

8.3 Robustness to Alternative Matching Designs

In this section, I assess the robustness of the results to alternative implementations of the matching procedure. The exercises examine whether the estimated displacement effects are sensitive to restrictions on match quality, randomness in control selection, or to moderate changes in how continuous matching variables and their pre-treatment histories are incorporated into the matching design.[169]

First, I relax the restriction that the Mahalanobis distance for the continuous matching variable (see Column (2) of Table 1) must fall below the predefined significance threshold (see Section 3.4 for details). Under this more flexible matching assignment, $23,364$ treated

[167] However, restricting the sample to firm closures occurring shortly after VET entry would exclude some training-firm closures that happen during VET.
[168] Additionally, some apprentices may drop out voluntarily – especially early in training – independently of the firm closure occurring only years later. Moreover, without detailed information, apprentices who decline retention offers at graduation would still be classified as treated and cannot be distinguished from those who did not receive an offer.
[169] Columns (5) to (8) of Table A24 in Appendix A1 give a numerical overview for the estimation results for each exercise.

apprentices can be matched.[170] As the sample in the main approach decreases by only 3% due to this restriction, this further supports the balance between treated and control units using the given combination of exact and distance matching, as bad-quality matches do not seem to play an important role. Panel (b) of Figure A37 in Appendix A1 shows that the resulting event study estimates of the effect on earnings closely resemble those in Panel (a), with only minor deviations attributable to the modified matching procedure. Thus, the results remain very similar after inclusion of a small number of less comparable matched pairs, which provides additional support for the baseline matching specification (see Section 3.4).

Second, I examine the role of randomness in the one-to-one matching of displaced and non-displaced apprentices. This issue arises in roughly 5,000 cases in which a displaced apprentice has multiple potential controls with identical Mahalanobis distances across all covariates.[171] To assess whether this random control selection affects the results, I construct an alternative matching scheme. When exactly two equally distant controls exist, I assign the control not chosen in the main specification; when three or more exist, I randomly select a different control than before. This approach generates a match assignment that differs substantially from the baseline rather than representing another random draw. Panel (c) of Figure A37 shows that the resulting event study estimates remain very close to those in the main specification. This indicates that the one-to-one matching results are not sensitive to the random selection of the matched control when multiple candidates have the same distance.

Beyond randomness in the selection of control apprentices, small modifications to the matching procedure itself may also affect the estimated effects. To assess this, I implement two additional robustness checks that slightly alter the matching design. In the first exercise, I keep the set of exact matching variables unchanged, and discretize the continuous matching variables into quintiles of their respective distributions and perform the matching within these bins.[172] Panel (d) of Figure A37 shows that the resulting event study estimates closely resemble those of the main specification. This indicates that the findings are robust to moderate changes in how continuous matching variables are aligned. Moreover, this pattern indicates that common support (Equation (7)) does not rely on quintile-by-quintile restrictions, and that treated and control apprentices remain well balanced even without requiring matched pairs to fall within the same quintiles.

[170] I.e., in the main specification, 868 potential match pairs are discarded because of relatively large Mahalanobis distance.

[171] This number appears large, but for apprentices in the same training firm and occupation, most continuous relate to firm characteristics and are then identical by construction.

[172] See Column (2) of Table 1 for the list of continuous matching variables. Within each quintile, Mahalanobis distance matching is applied to the same set of standardized variables. No additional distance caliper is imposed, as treated and control apprentices cannot differ substantially once they fall into the same quintile on every matching variable. Using this approach, $21,716$ treated apprentices can be matched.

Second, a further concern may be whether differences in the evolution of matching variables over time could influence the results. As a complementary exercise to the pre-treatment event study analysis in Section 7.1, I implement an alternative matching strategy that conditions on a richer pre-treatment history of the continuous matching variables listed in Column (2) of Table 1.[173] Panel (e) of Figure A37 reports the resulting event study estimates. When accounting for the extended variable history, the estimated effects on earnings become only slightly larger, and the differences relative to the baseline specification remain statistically insignificant. Overall, these findings suggest that the results are not highly sensitive to conditioning on a richer set of pre-treatment trajectories, which is consistent with the adequacy of the baseline matching specification. The similarity of the estimates is also consistent with the plausibility of the parallel trends condition in Equation (6), as this suggests that the results do not change substantially when allowing importing firm-level characteristics to evolve differently across treated and control apprentices in the matching procedure.

8.4 Robustness to Alternative Weighting Strategies

The one-to-one matching design in the main specification ensures close comparability between treated and control apprentices and avoids reliance on strong functional form assumptions. However, it discards many potentially informative control observations and may therefore reduce statistical precision. I therefore consider weighting approaches that allow each treated apprentice to be compared to multiple controls. These methods assume that, conditional on the observed variables used for weighting, treatment assignment is independent of the potential outcomes (Equation (9)). They allow each treated apprentice to be compared to multiple controls and assign higher weight to more similar observations. This can increase precision by using a larger share of the available control sample, but it relies on an appropriate choice of weights to ensure that less comparable controls receive sufficiently low influence.

If both the weighting and the matching approach are well specified, the point estimates should remain unchanged, while precision is expected to improve when choosing the weighting approach. By comparing results across these strategies, I assess whether the main findings depend on the trade-off between match quality and sample size, and on the choice of weights that determines the influence of less comparable control observations.[174]

[173] This approach adds multiple lags of the covariates used in the main specification to the Mahalanobis distance matching, resulting in 49 continuous variables. As in the baseline approach, match pairs with distances exceeding the 95th percentile of the χ^2-distribution with 49 degrees of freedom are discarded. An overview of these variables is given in Footnote 176.

[174] Table A25 in Appendix A1 presents numerical results for these exercises.

First, I rely solely on the discrete matching variables listed in Column (1) of Table 1 and exclude all treated and control apprentices for whom no suitable counterpart exists with the same profile of exact matching variables. For each remaining profile E of the exact matching variables within each treatment cohort c, I estimate a cohort- and profile-specific propensity score defined as

$$\hat{p}_{Ec} = \hat{P}(D = 1|E, c) = \frac{N^T_{E,c}}{N_{E,c}}, \tag{19}$$

with $N_{E,c}$ being the number of individuals with profile E in the risk set of displacement at cohort c, and $N^T_{E,c}$ the number of displaced apprentices with profile E at cohort c. Doing so, I apply a n-to-one matching approach on the previous set of exact matching variables only. Under this approach, all treated and control apprentices that share the same cohort and exact variable profile receive an identical weight which reflects the empirical treatment probability for the respective profile. Given the estimated propensity scores from Equation (19), treated and control individuals are weighted by using normalized ATT weights given by

$$w_{iEc} = \begin{cases} 1, & \text{if } D_i = 1, \\ \frac{N^T_{E,c}}{N_{E,c}} \cdot \frac{\hat{p}_{Ec}}{1-\hat{p}_{Ec}}, & \text{if } D_i = 0, \end{cases} \tag{20}$$

for an individual i with exact-variable profile E at risk set in cohort c. The resulting event study difference-in-differences estimates of the effect on earnings are shown in Panel (b) of Figure A38 in Appendix A1. As expected, precision improves due to the substantially larger pool of control observations. In this specification, $25,091$ treated apprentices contribute to the estimation, alongside $2,201,128$ matched controls.[175] The estimated VET displacement effects differ only modestly from the main specification shown in Panel (a). The point estimates are slightly more negative, which is consistent with the inclusion of some less well-matched control apprentices from somewhat better performing firms, all of whom receive the same weight as better-matched controls. Importantly, standard errors are noticeably smaller, which reflects the gains in statistical precision from using a larger share of the available sample.

Second, instead of assigning equal weights to all control apprentices within a given exact-variable profile and cohort, one may allow for a more flexible adjustment with respect to the continuous variables used in the main matching design. Inverse probability reweighting (IPW) offers a natural framework for this purpose. By reweighting control observations according to their estimated probability of displacement conditional on observed covariates, IPW constructs a synthetic comparison group that matches the treated group in expectation. This approach uses the full relevant sample and also addresses

[175] As in the main approach, control apprentices can be used multiple times, and treated individuals can serve as controls in earlier cohorts.

remaining covariate imbalances, given correct propensity score specification. To implement this strategy, I estimate a probit model for displacement for each cohort c at risk of displacement that includes a rich set of covariates. To ensure sufficiently large estimation samples for discrete variables, I use broader groupings: gender, citizenship (German/foreign), a coarse regional indicator (East vs. West Germany), the 1-digit training occupation (9 categories), the current VET year, and the VET start year. In addition, I include 49 continuous variables that capture both current values and the histories of important time-varying matching variables, as listed in Column (2) of Table 1.[176] From the resulting propensity score estimates $\hat{p}_i := \hat{P}(D_i = 1|x)$ I construct normalized ATT weights given by

$$w_{iEc} = \begin{cases} 1, & \text{if } D_i = 1, \\ \frac{N_{E,c}^T}{N_{E,c}} \cdot \frac{\hat{p}_i}{1-\hat{p}_i}, & \text{if } D_i = 0. \end{cases} \quad (21)$$

Using this approach, an even larger group of treated apprentices can be included in the estimation, amounting to $26,198$ individuals. As many apprentices are at risk of VET displacement in multiple cohorts, the pool of control observations expands substantially as well, yielding $18,779,403$ control observations. The resulting increase in sample size further improves estimation precision. Panel (c) of Figure A38 presents the corresponding event study estimates for earnings. In the medium-run period, where the main specification exhibits the strongest effects, the point estimates become slightly more negative.

At the same time, the results from IPW are sensitive to extreme weights. Excluding the top 0.5% of weights[177] substantially attenuates the estimation results and brings them close to those of the main specification, as shown in Panel (d). This sensitivity suggests that some control observations receive disproportionately large weights, which indicates that their estimated treatment probabilities $\hat{p}_i$ may be overstated. Given the large standard errors in the baseline IPW results, the differences across approaches remain statistically insignificant. This sensitivity highlights the importance of weight stability in reweighting-based estimators and motivates the use of one-to-one matching as the baseline design.

Overall, the main findings are broadly robust across specifications. Relaxing the matching restrictions, incorporating richer covariate histories, and applying alternative n-to-one matching or inverse probability weighting approaches yields estimates that are generally similar to the baseline, although some exercises lead to modest differences in magnitude

[176] These are: (i) all continuous matching variables in Column (2) of Table 1 as well as VET tightness and the ratio of unfilled vacancies in the given labor agency district; (ii) their first lags except for *change in employment*, (iii) the mean of lags 2-3 for each, except for *change in employment*, (iv) the mean of lags 4-7 for each, except for *change in employment*.

[177] Note that these stem completely from the group of controls due to the definition of the weights, see Equation (21).

and precision. While these checks cannot rule out all remaining sources of bias, they provide reassurance that the main conclusions are not driven by a specific matching or weighting strategy.

9 Conclusion

This paper studies the labor market consequences of involuntary VET disruptions by examining apprentices displaced due to training firm closures. Firm closures during VET constitute a distinct setting relative to both other involuntary educational dropouts and job loss in regular employment. Unlike educational dropouts following severe misconduct or poor academic performance – such as exam failure among students – firm closures occur independently of individual performance and behavior. They also have direct labor market implications, since many apprentices would likely otherwise remain at their training firm after graduation, and this option no longer exists. Compared to job loss in regular employment, displaced apprentices lose not only their job but also experience an interruption of their educational pathway. At the same time, institutional support from chambers and the Federal Employment Agency often facilitates continuation of training at another firm.

Using rich administrative data and a matched event study design, the analysis provides causal evidence on the short- and medium-run effects of displacement during a critical phase of skill formation. The difference-in-differences event study results show that displacement during VET leads to small but persistent earnings losses over the subsequent eight years. While these effects are modest relative to displacement from regular employment among recent VET graduates in the 1990s (von Wachter/Bender, 2006), they are statistically significant and reflect persistent differences in early-career trajectories.

A key finding is that the average difference-in-differences estimate masks substantial heterogeneity. Many displaced apprentices continue training and complete VET at a new firm, and their longer-run earnings penalties remain limited. By contrast, apprentices who drop out of VET after displacement face pronounced and persistent earnings losses. This pattern indicates that the main risk of displacement during VET is not universal scarring, but a higher likelihood of leaving without a formal qualification. Such exits also occur under more constrained circumstances than in the control group, since displaced apprentices face an immediate risk of unemployment and of leaving the VET system altogether, whereas control-group dropouts can plan their exit in advance.

A further important insight is that VET disruptions have stratifying effects and disproportionately affect already disadvantaged groups, especially apprentices with lower prior educational attainment. Timing also matters: disruptions early in VET lead to larger earnings losses than those later in training, consistent with higher non-employment risks and incomplete skill accumulation. At the same time, the institutional context of the German dual system likely mitigates the long-run effects of training disruptions. Apprentices are embedded in a dense support structure involving vocational schools, chambers, and the Federal Employment Agency, and targeted instruments can provide additional support during economic crises. This institutionalized support seems to facilitate a quick return to VET and to limit time spent outside training, thereby reducing the loss of firm-specific skills and supporting VET completion. Yet not all apprentices access or benefit equally from these supports, and the costs of firm closure are not fully offset: even those who complete VET still fare slightly worse on average in the longer run.

These findings may differ in settings with weaker vocational institutions or more school-based training systems. Future research could examine comparable displacement events in other dual systems, such as Austria or Switzerland, and extend the horizon beyond early careers. Overall, the German dual system provides substantial support to apprentices in the event of training firm closures, although it does not eliminate the consequences entirely. Another important factor facilitating the continuation of VET at a new firm is that, throughout the observation period, many firms reported difficulties in filling their apprenticeship positions (Fitzenberger et al. (2025); Bundesinstitut für Berufsbildung (BIBB) [Federal Institute for Vocational Education and Training] (2025)). As a result, apprentices affected by firm closures were likely to face relatively favorable conditions when searching for a new training firm. In periods with greater competition for apprenticeship positions, however, continuation rates may be considerably lower.

The consequences of displacement during VET are smaller when apprentices continue and complete training, but substantial when displacement triggers an unintended exit from VET. The long-run effects therefore depend less on displacement itself than on whether apprentices complete VET.

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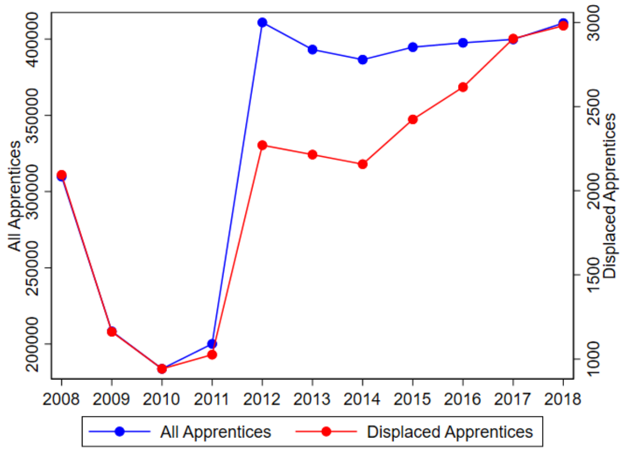
Appendix

A1 Appendix A.1 - Additional Tables and Figures

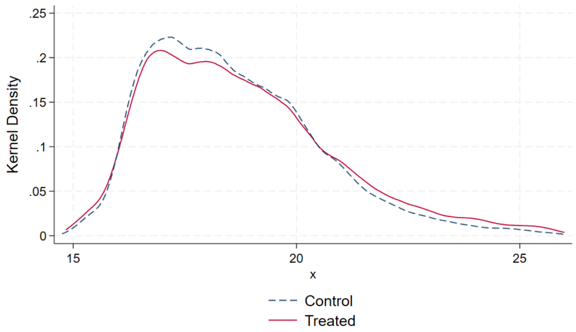


Notes: Measured on the left y axis, the blue line shows the numbers of all new apprentices entering VET. Note that e.g. those doing a second apprenticeship, having obtained professional degrees before or in older age groups are not included. One the right y axis, the red line shows the yearly number of new apprentices who later experience a firm closure during VET.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

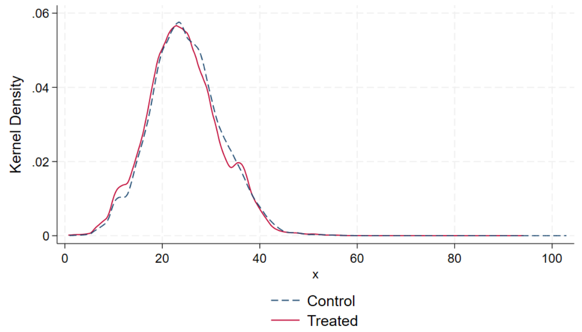
Figure A1: VET Entries and Later Firm Closures During VET over Time

Figure A2: Balance in Continuous Matching Variables

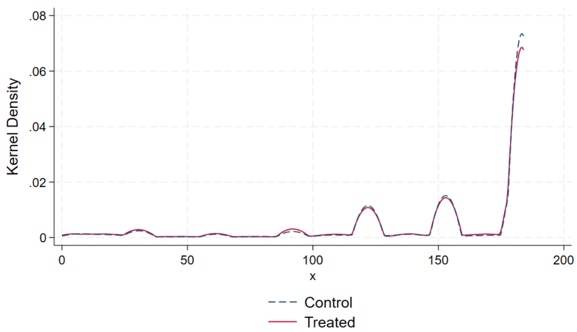
a) Age at VET start



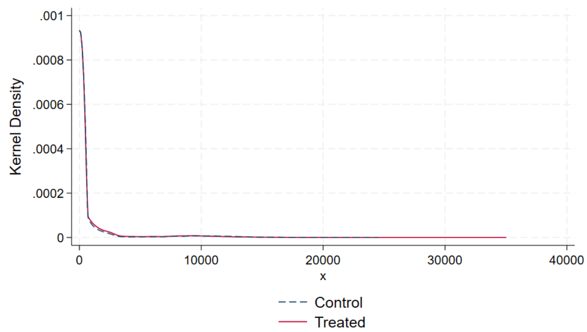
b) Mean daily wage from VET)



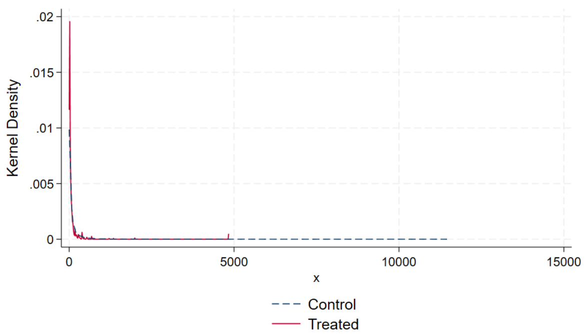
c) Days in VET



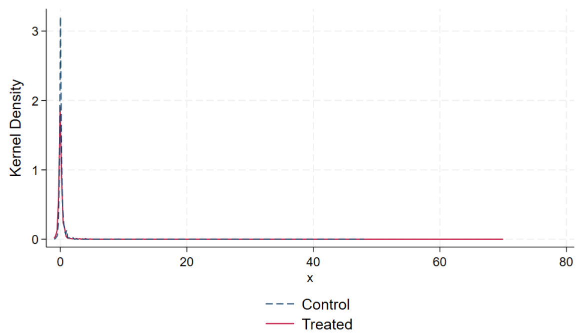
d) Earnings beside VET



e) Number of employees in the training firm



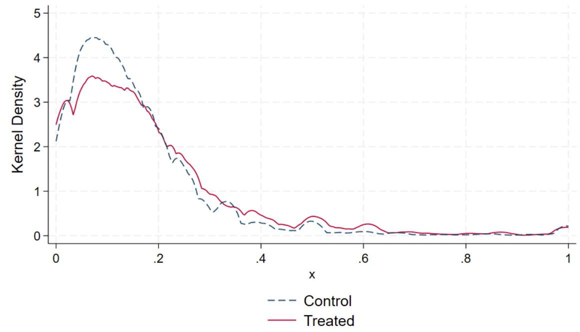
f) Change in employment during last 2 years



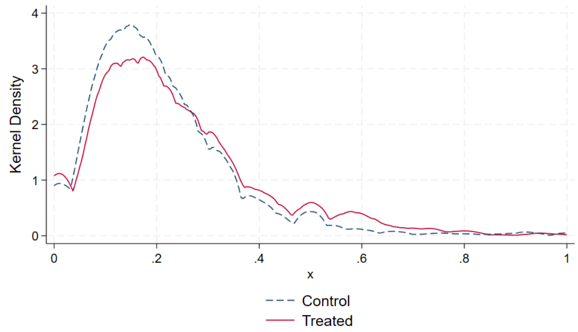
Notes: This figure shows kernel density estimates for different covariates. Epanechnikov kernel and the Silverman rule of thumb for the bandwidth are chosen. The variable *Age at VET start* was rounded to full years for the matching approach. For visualization purposes, this figure displays age in days.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A3: Balance in Continuous Matching Variables (Continued)

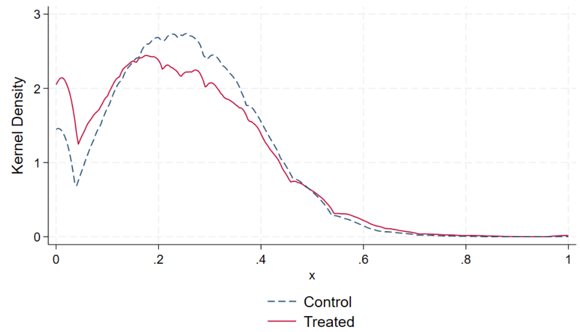
a) Share of apprentices



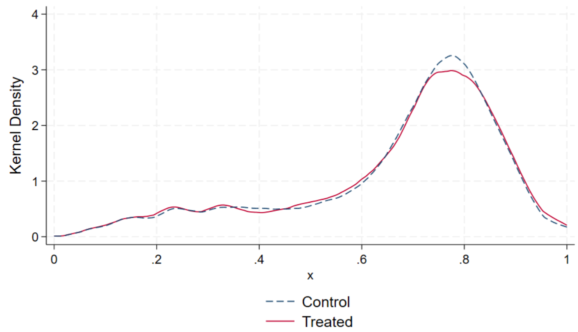
b) Share of unskilled workers



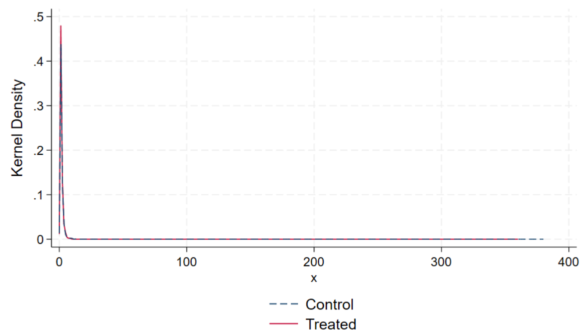
c) Share of old employees



d) Share of VET positions unfilled by occ. and labor agency district



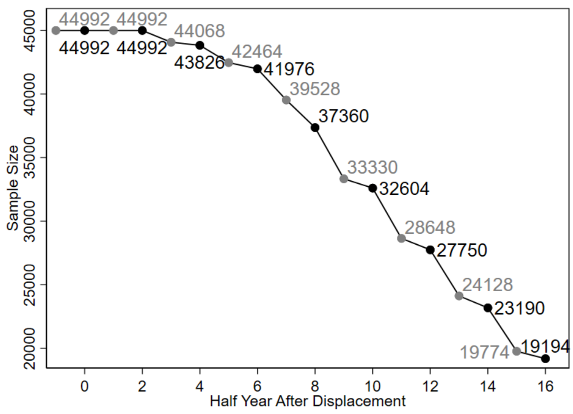
e) VET tightness by occ. and labor agency district



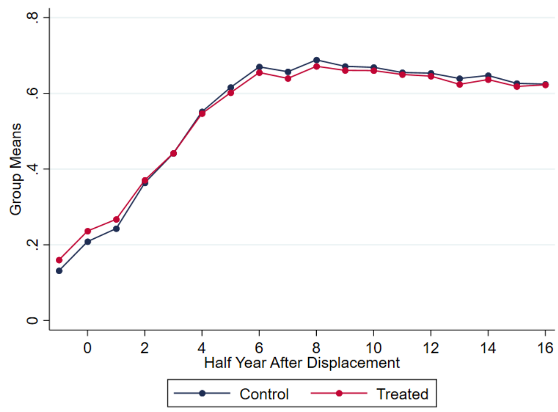
Notes: This figure shows kernel density estimates for different covariates. Epanechnikov kernel and the Silverman of thumb for the bandwidth are chosen.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A4: Sample Size and Employment Participation

a) Sample size over time



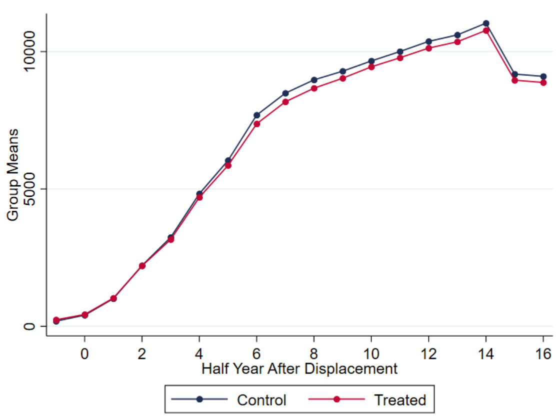
b) Employment participation (excl. VET)



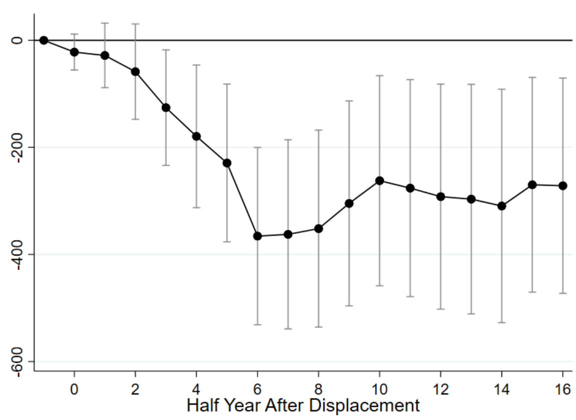
Notes: Panel (a) shows the number of individuals in the sample over the event time k, $k = -1, ..., 16$. Panel (b) shows the share of individuals in regular employment (excluding VET).
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A5: Effect of VET Displacements on Earnings Using a Balanced Panel

a) Earnings from regular employment (excl. VET, levels)



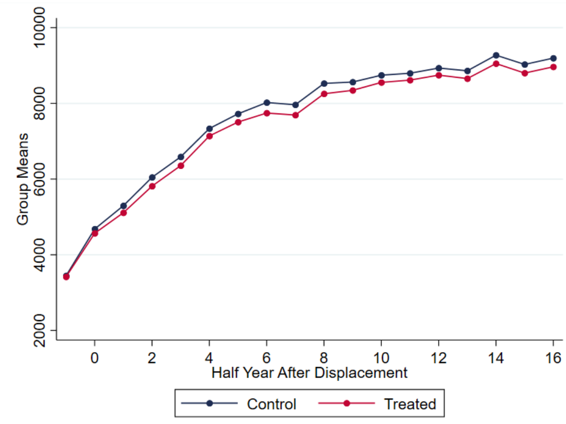
b) Earnings from regular employment (excl. VET, DiD)



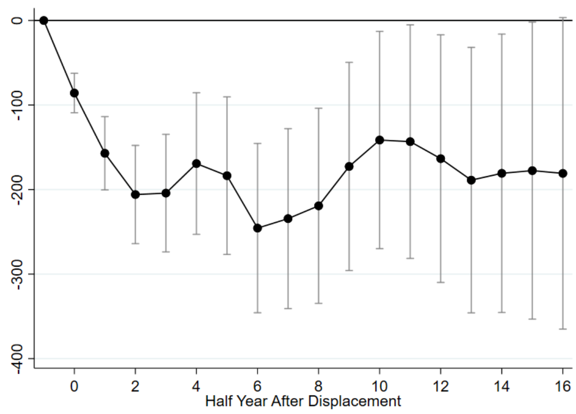
Notes: Only displacements up to half-year 2015/2 are considered, yielding a balanced panel for the subsequent eight years through 2023/2.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A6: Effect of VET Displacements on Further Employment Outcomes

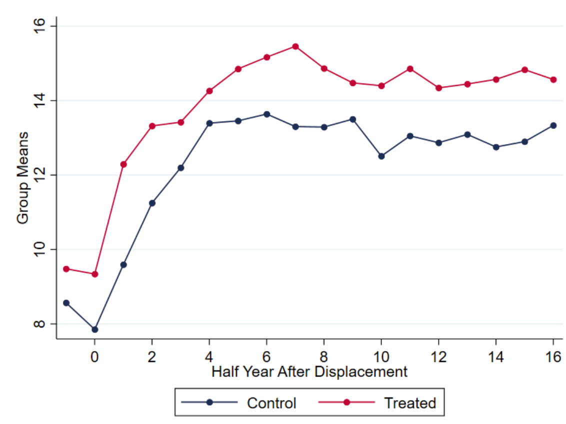
a) Total earnings (incl. VET, levels)



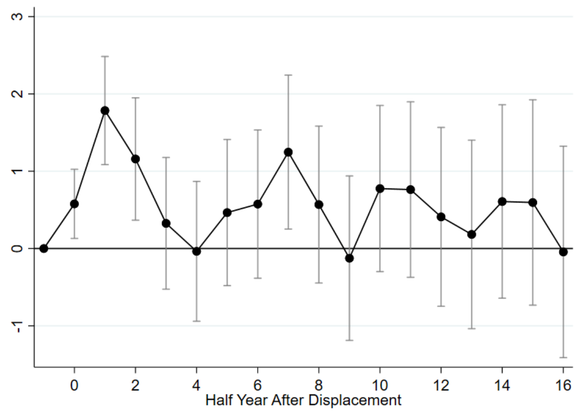
b) Total earnings (incl. VET, DiD)



c) Days in registered unemployment (levels)



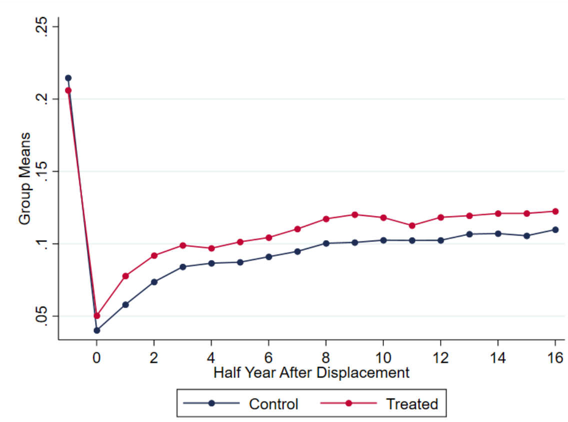
d) Days in registered unemployment (DiD)



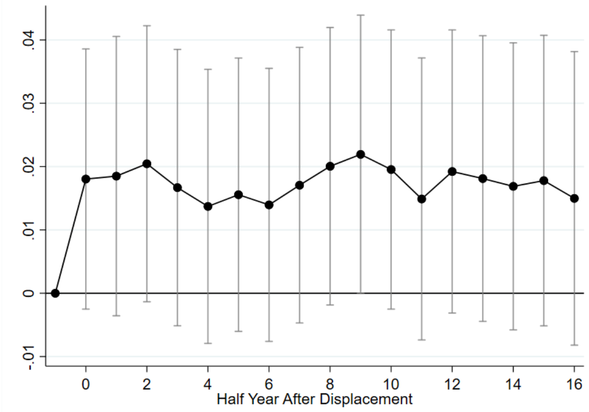
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A7: Effect of VET Displacements on Further Measures of Job Quality and Additional Educational Outcomes

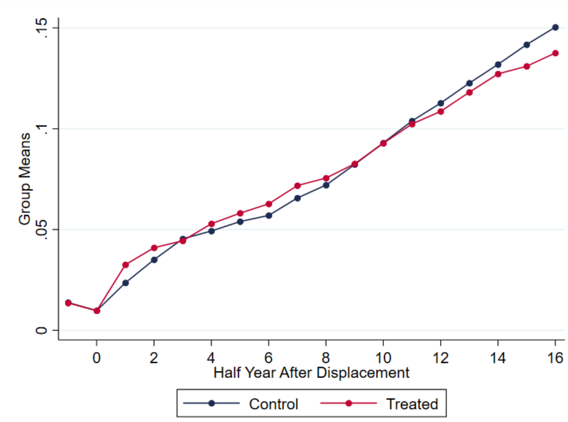
a) 1: Employed in helper occupation (levels)



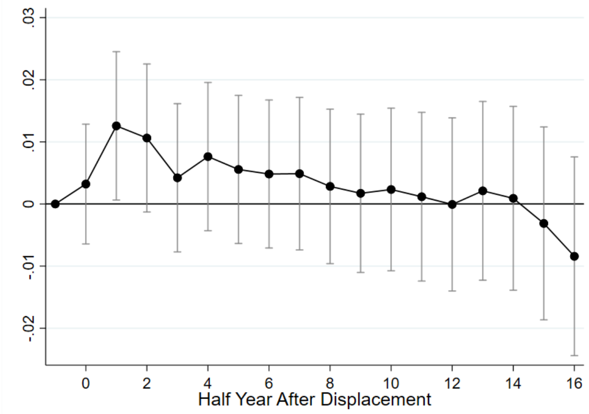
b) 1: Employed in helper occupation (DiD)



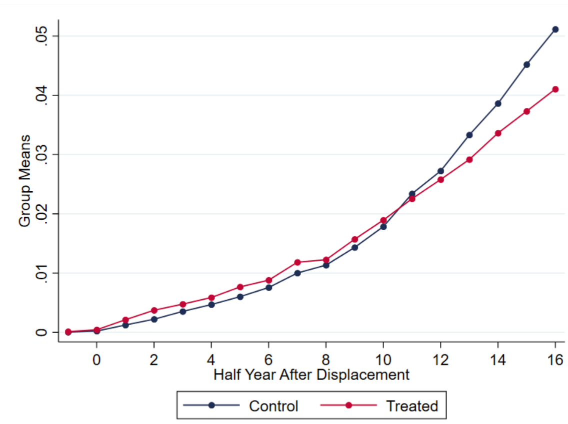
c) 1: Employed in (highly) complex occupation (levels)



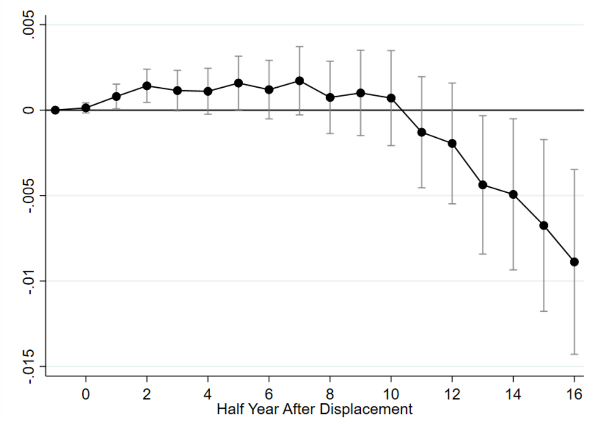
d) 1: Employed in (highly) complex occupation (DiD)



e) 1: Holding university degree (levels)



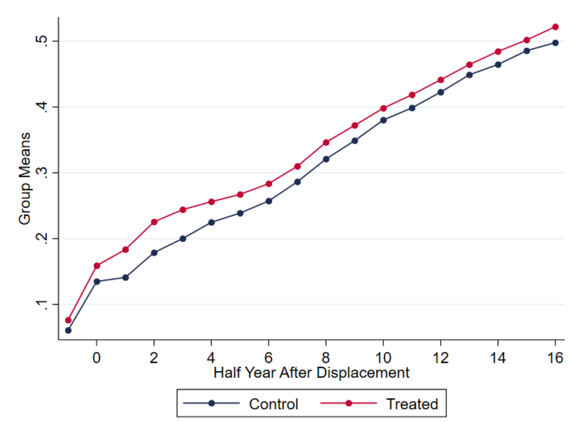
f) 1: Holding university degree (DiD)



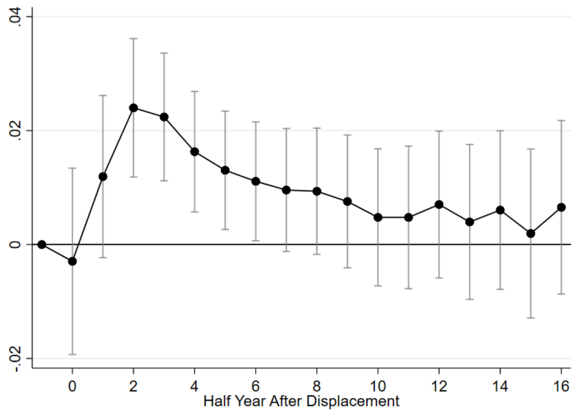
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A8: Effect of VET Displacements on Further Mobility Outcomes

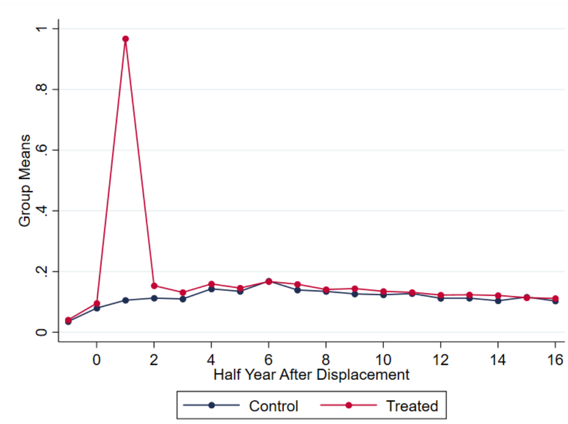
a) 1: Employed outside last VET occupation (levels)



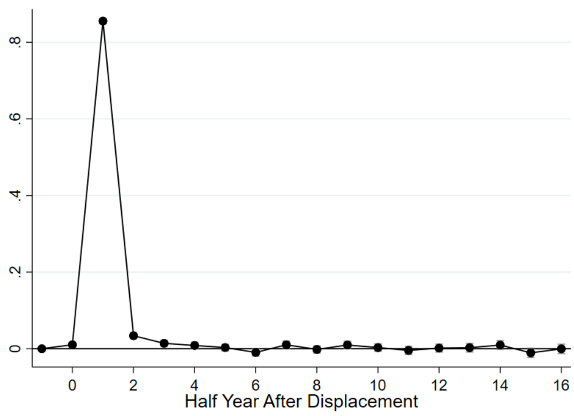
b) 1: Employed outside last VET occupation (DiD)



c) 1: Switched employer from $t-1$ to t (incl. VET, levels)



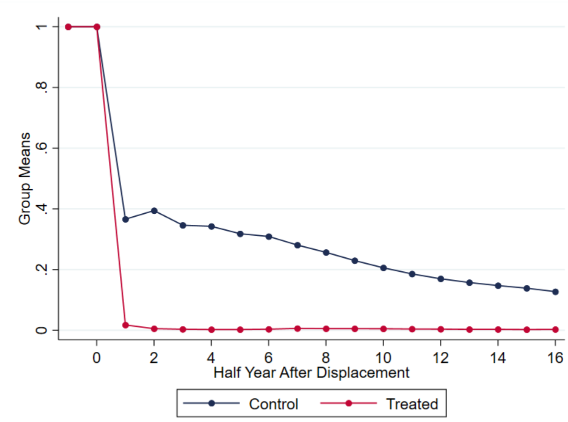
d) 1: Switched employer from $t-1$ to t (incl. VET, DiD)



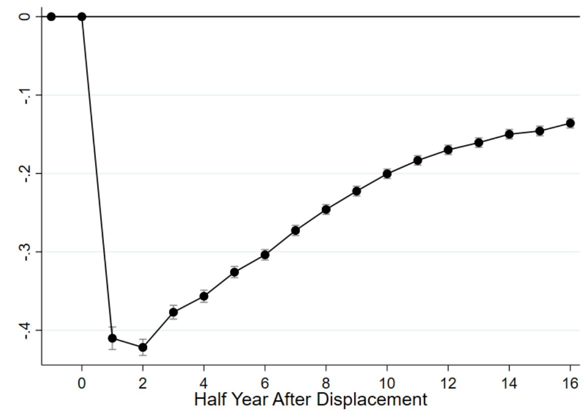
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A9: Effect of VET Displacements on Staying at the Training Firm

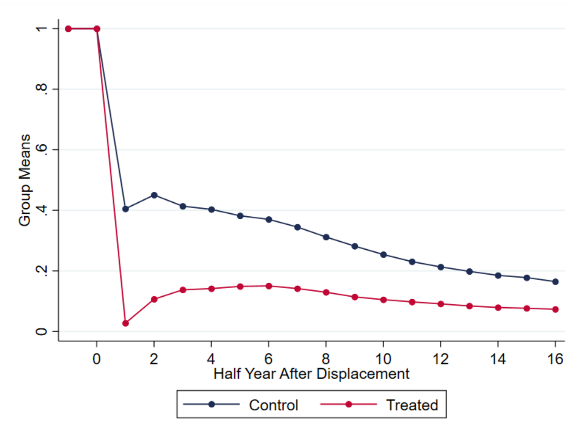
a) 1: Employed at the first training firm (excl. VET, levels)



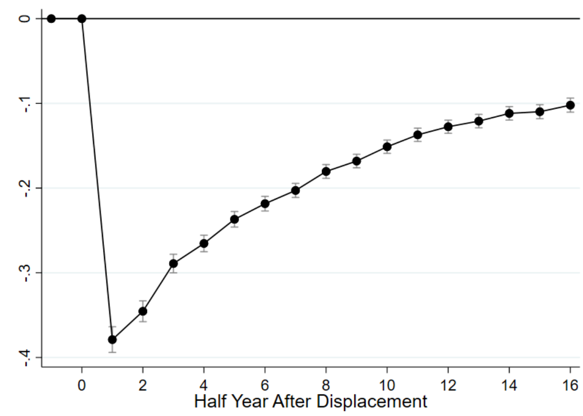
b) 1: Employed at the first training firm (excl. VET, DiD)



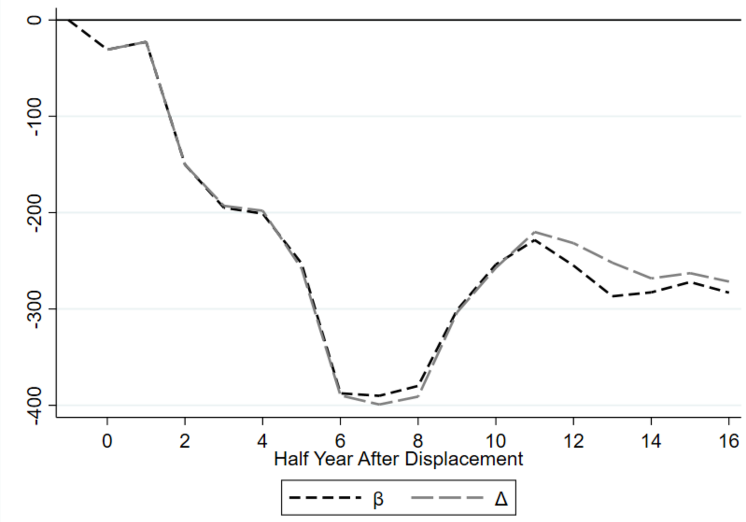
c) 1: Employed at the first/last training firm (excl. VET, levels)



d) 1: Employed at the first/last training firm (excl. VET, DiD)



Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

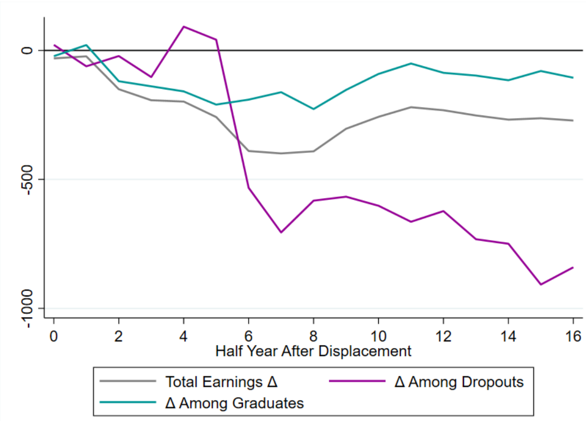


Notes: This graph shows the event study difference-in-differences estimates for the effect of VET displacements on earnings, when either omitting (Δ_k) or including fixed effects α_i, η_t, and γ_k (see Equation (1), β_k).
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

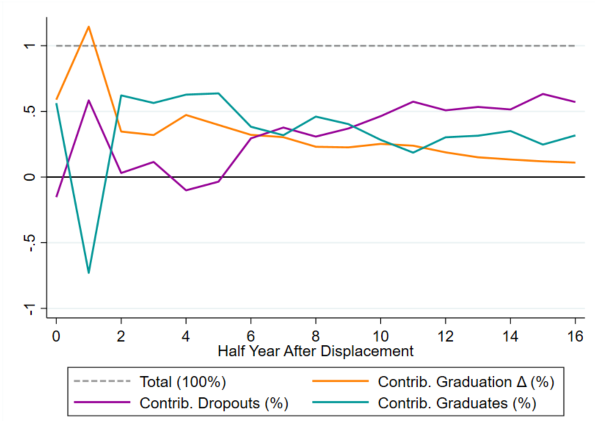
Figure A10: Effect of VET Displacement on Earnings (With/Without Fixed Effects)

Figure A11: Additional Material for the Analysis of Earnings and Training Success

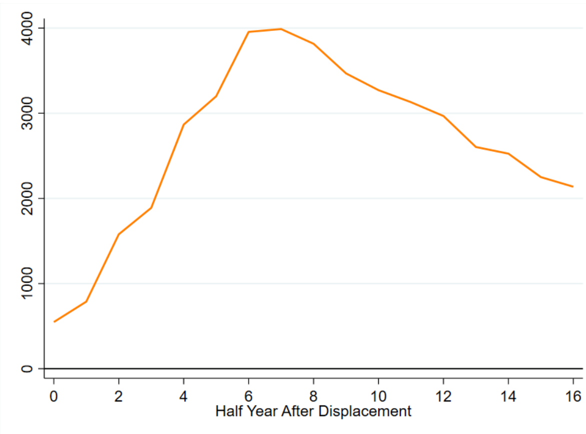
a) Displacement penalties among VET graduates and dropouts



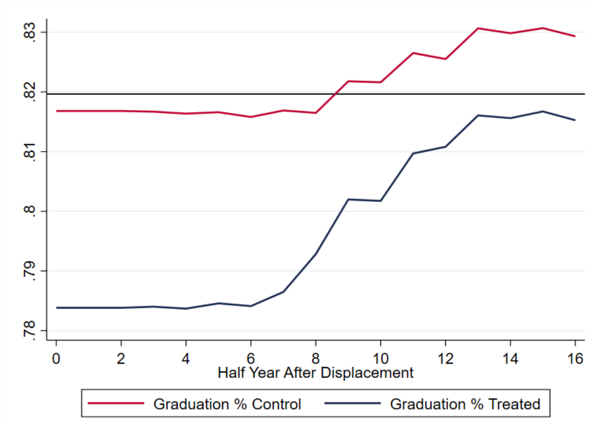
b) Contributions of compositional term and within-group terms



c) Earnings surplus for VET graduates (among controls)



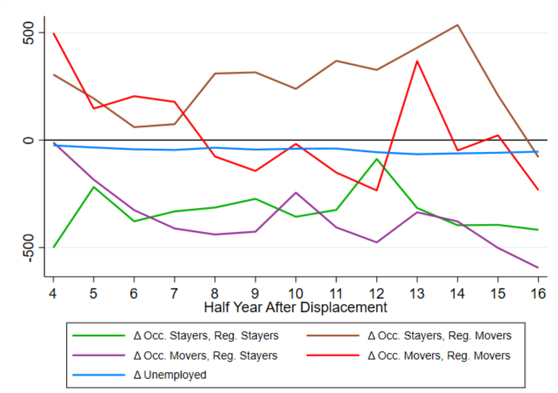
d) VET graduation shares among treated and controls



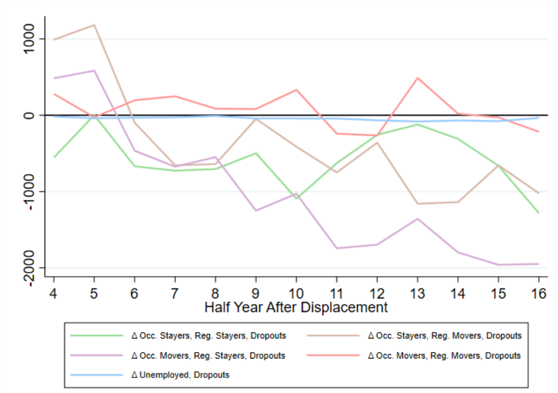
Notes: Panel (a) shows the earnings differentials associated with displacement within the group of (future) VET graduates (turquoise line) and (future) VET dropouts (purple line). These terms correspond to $\mathbb{E}\left[\text{Earn}_k^{k=-1}|D=1,G=1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D=0,G=1\right]$ and $\mathbb{E}\left[\text{Earn}_k^{k=-1}|D=1,G=1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D=0,G=0\right]$ in Equation (11), respectively.
Panel (b) shows the contribution of the different components to the estimated earnings effect (Δ_k), similar to Panel (b) in Figure 5, but starting at $k=0$ when most (future) graduates have not graduated yet.
Panel (c) shows the earnings surplus of VET graduates relative to dropouts among the controls, i.e. the term $\mathbb{E}\left[\text{Earn}_k^{k=-1}|D=0,G=1\right] - \mathbb{E}\left[\text{Earn}_k^{k=-1}|D=0,G=0\right]$ in Equation (11).
Panel (d) shows the share of graduates among both treated and controls over time, i.e. the terms $\mathbb{P}(G=1|D=0)$ and $\mathbb{P}(G=1|D=0)$ in Equation (11), respectively. The differences across event time are driven by sample imbalance.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A12: Additional Material for the Analysis of the Mechanisms Jointly 1/2

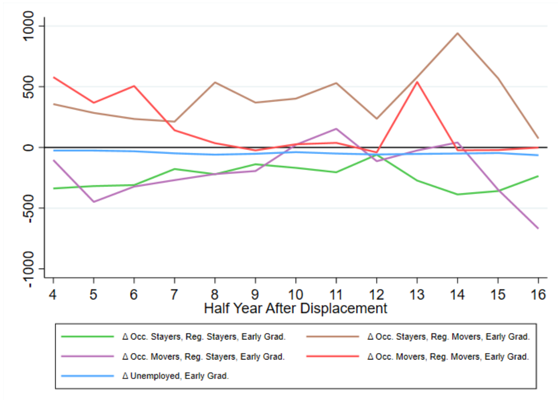
a) Displacement penalties within groups all graduates/dropouts jointly



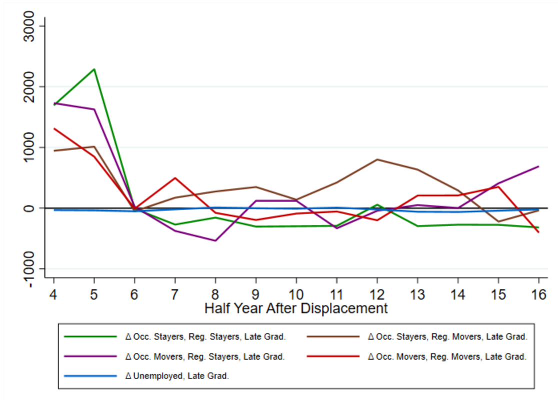
b) Displacement penalties Dropouts



c) Displacement penalties Early graduates



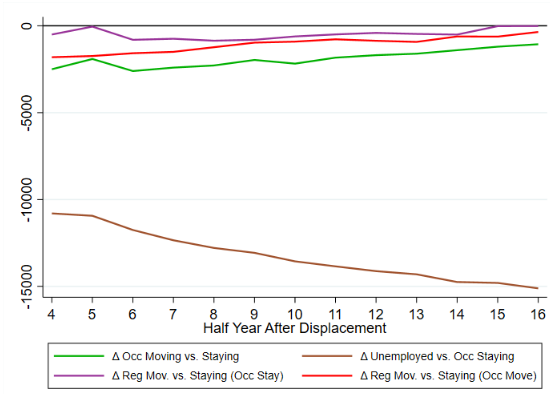
d) Displacement penalties Late graduates



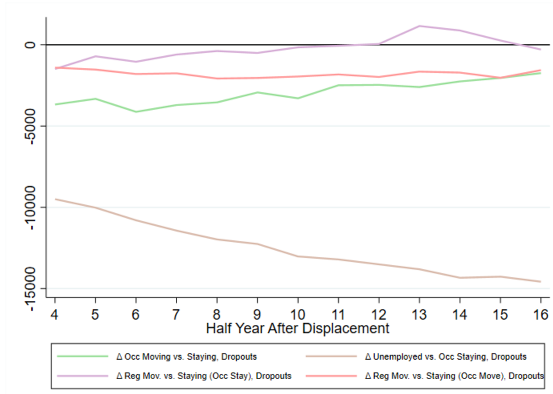
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A13: Additional Material for the Analysis of the Mechanisms Jointly 2/2

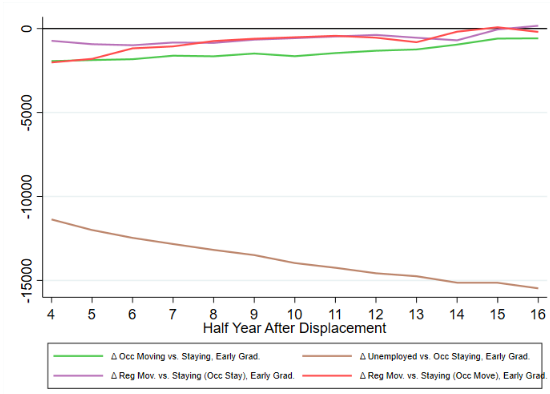
a) All graduates/dropouts jointy



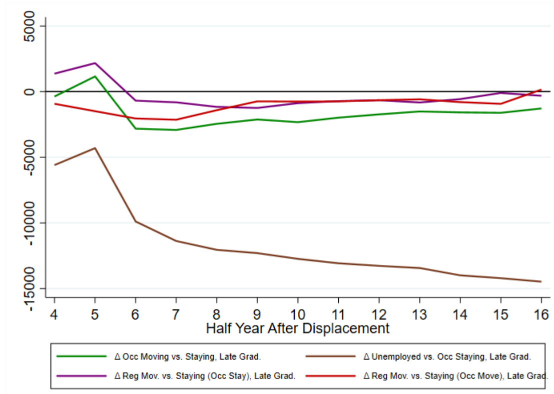
b) Dropouts



c) Early Graduates



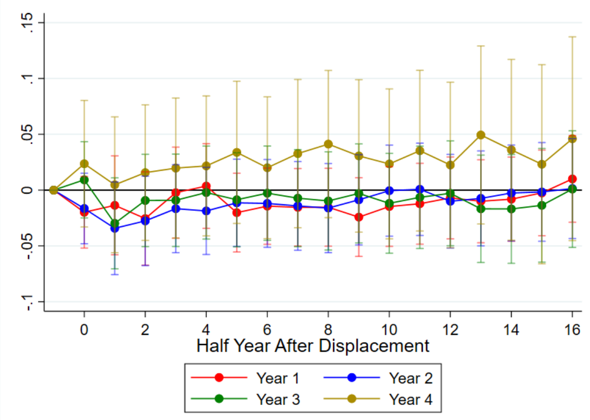
d) Late Graduates



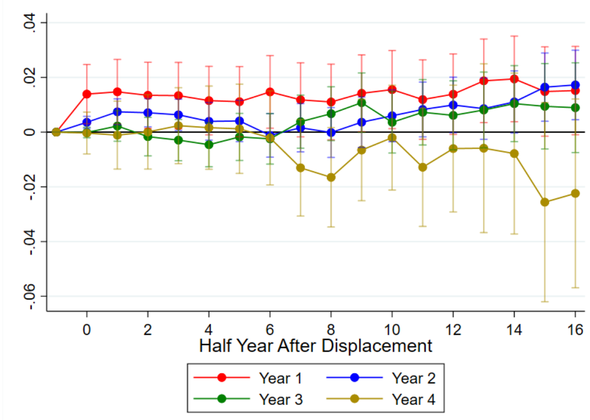
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A14: Effect of VET Displacement on Further Outcomes, By Displacement Year

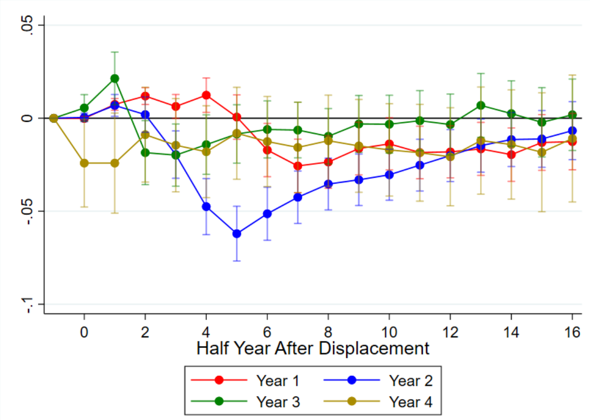
a) 1: Employed in specialist occupation



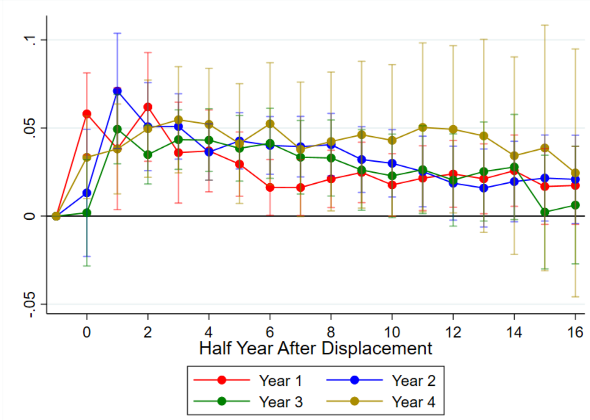
b) △ in firm FE



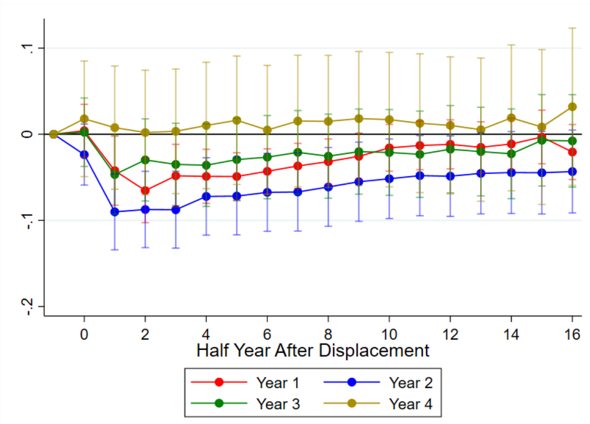
c) 1: Holding VET degree



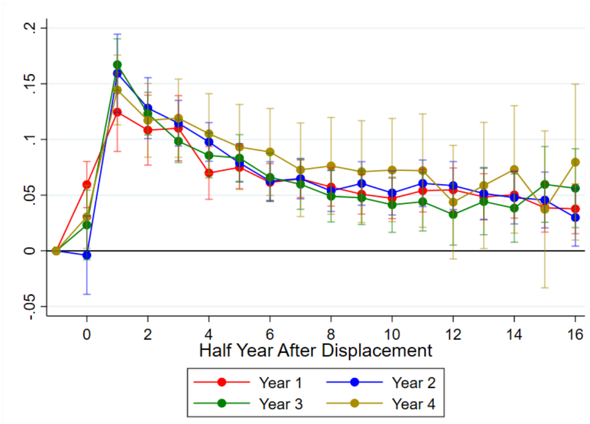
d) 1: Employed outside first VET occupation



e) Occupational similarity to first VET occupation



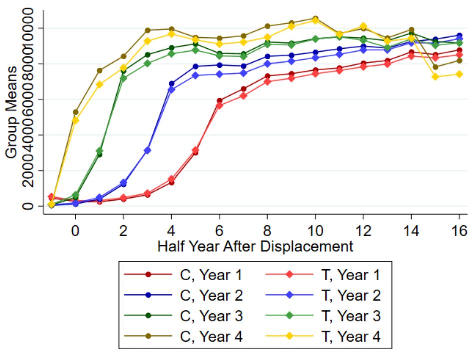
f) 1: Employed outside VET-start county



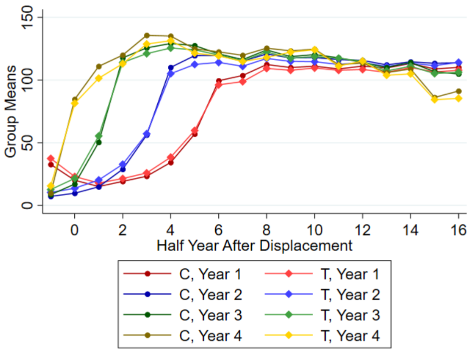
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A15: Evolution of Earnings, Employment, and Wages, by Displacement Year

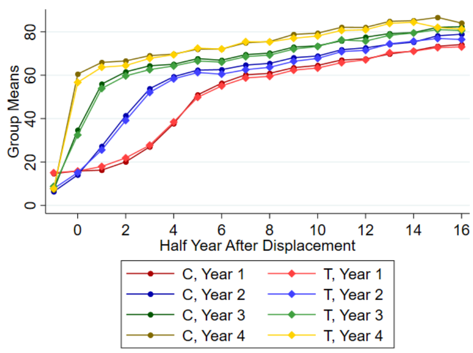
a) Earnings from regular employment (excl. VET, levels)



b) Days in regular employment (excl. VET, levels)



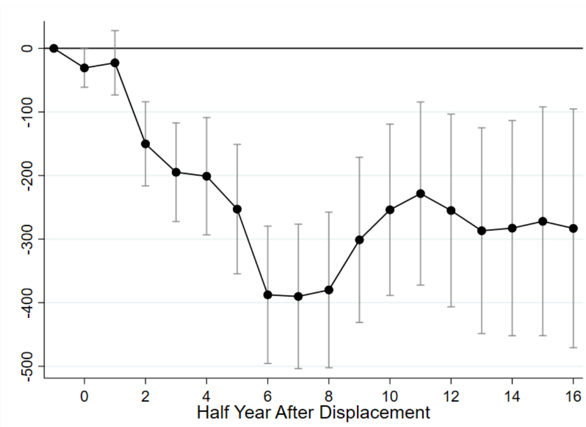
c) Mean daily wage from regular employment (excl. VET, levels)



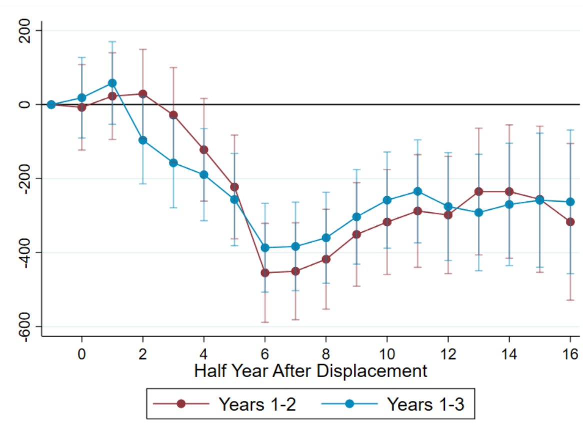
Notes: For exact variable definitions see Table A9 in Appendix A1.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A16: Effect of VET Displacement on Earnings: Full Sample vs. Early Displacement Only

a) All apprentices



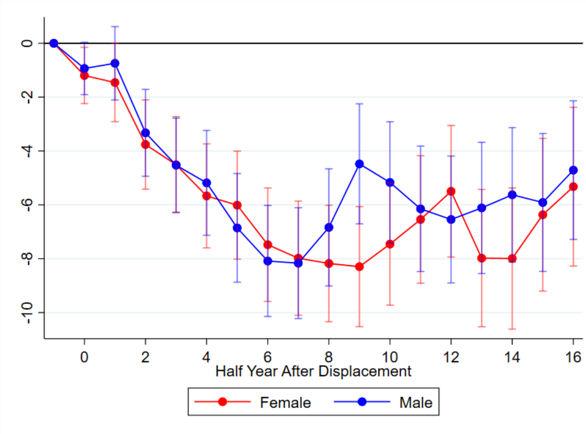
b) Excluding potential VET prolongation cases



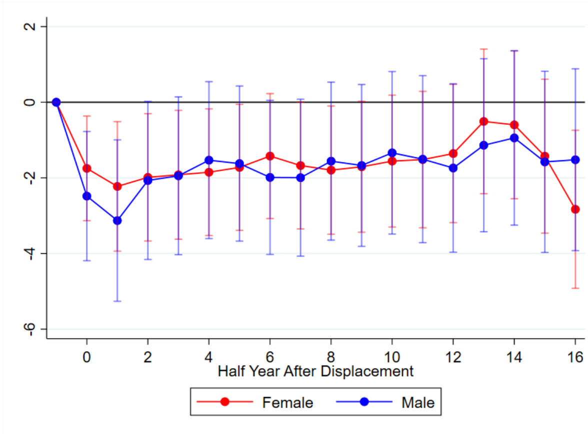
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A17: Effect of VET Displacement on Further Outcomes, by Gender

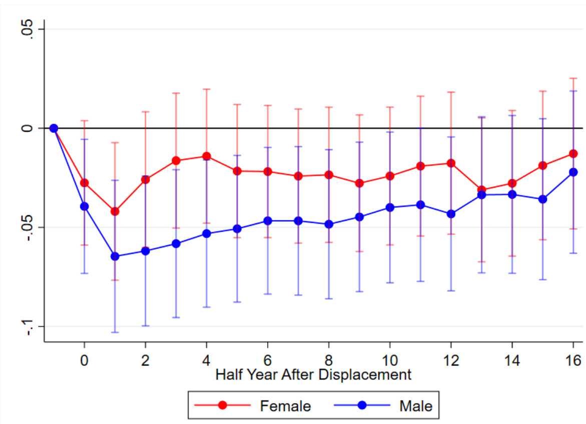
a) Days in regular employment (excl. VET)



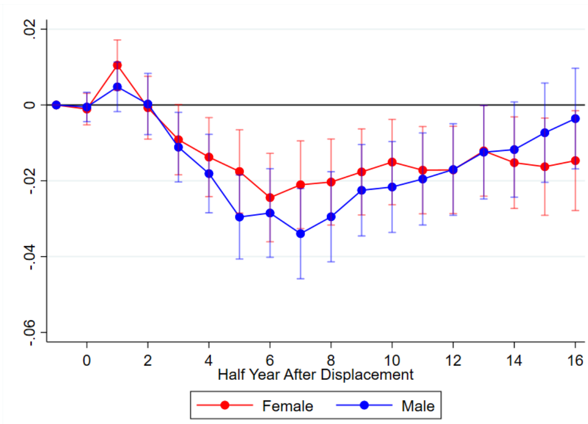
b) Mean daily wage from regular employment (excl. VET)



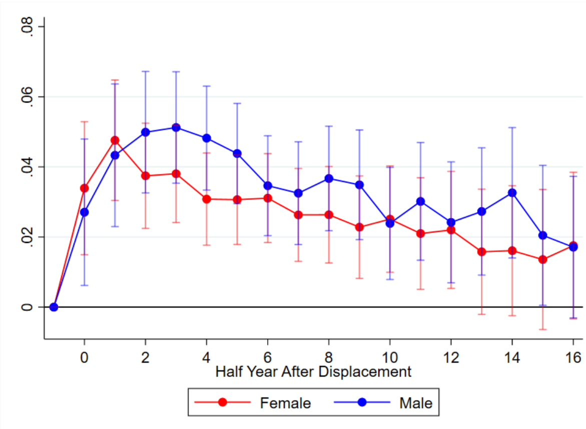
c) 1: Employed in specialist occupation



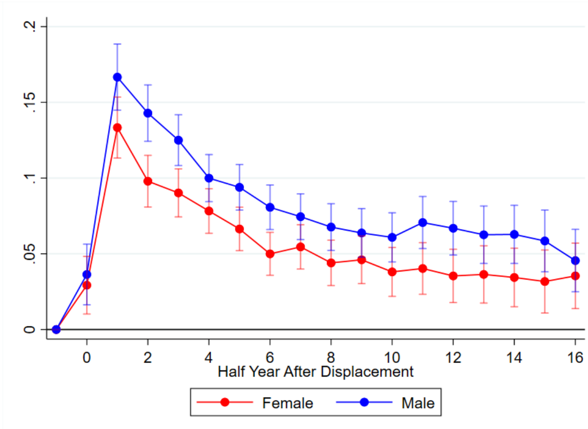
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



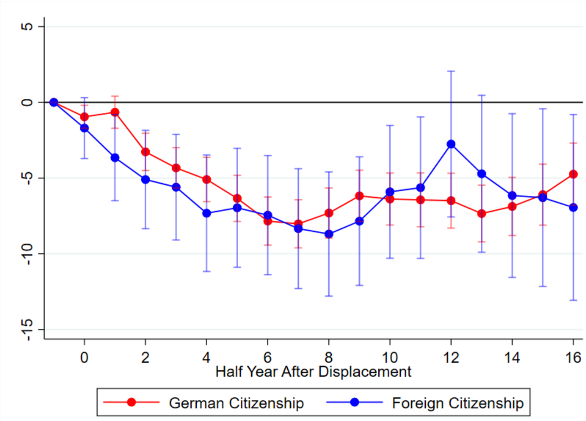
f) 1: Employed outside VET-start county



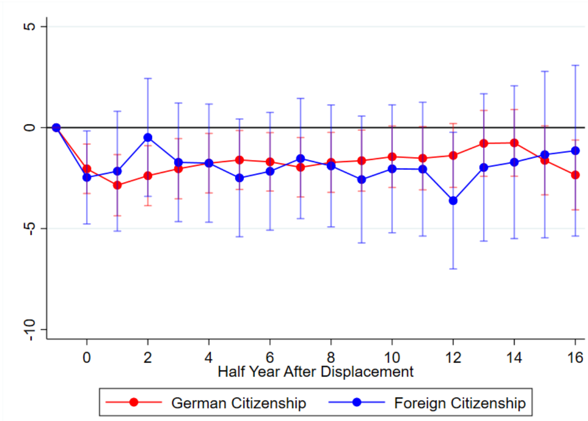
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A18: Effect of VET Displacement on Further Outcomes, by Citizenship

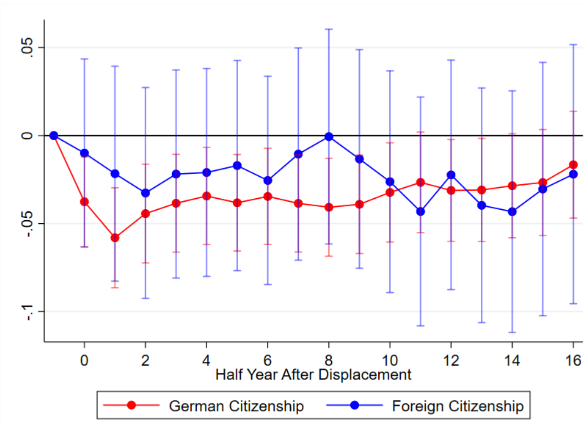
a) Days in regular employment (excl. VET)



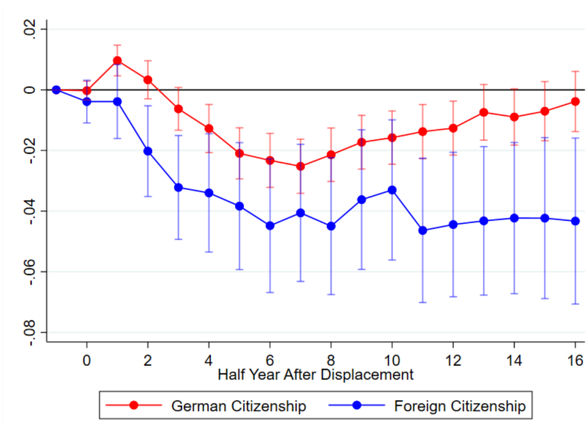
b) Mean daily wage from regular employment (excl. VET)



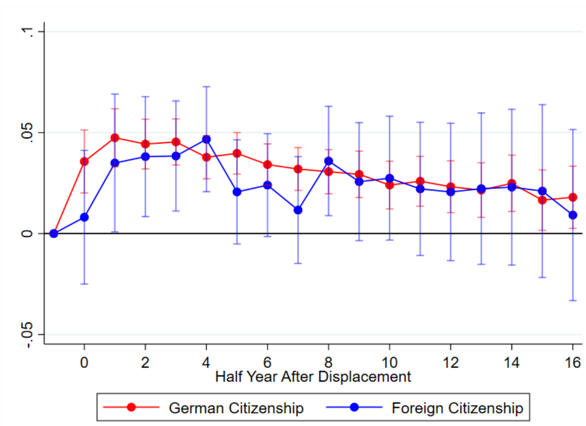
c) 1: Employed in specialist occupation



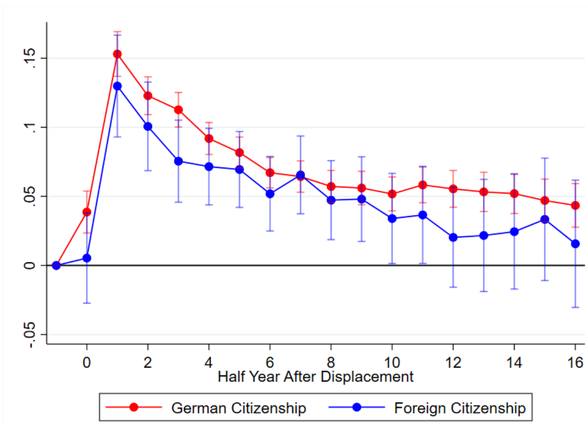
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



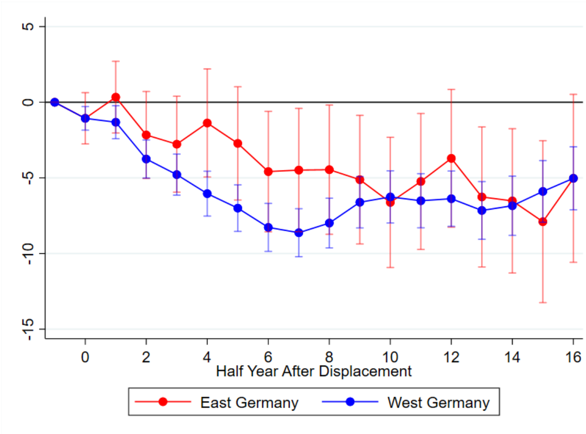
f) 1: Employed outside VET-start county



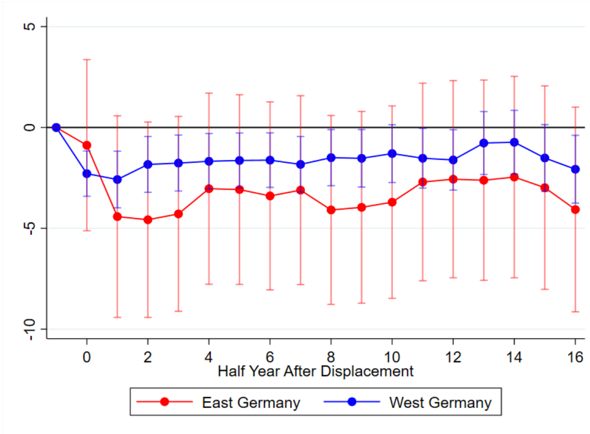
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A19: Effect of VET Displacement on Further Outcomes, by Region

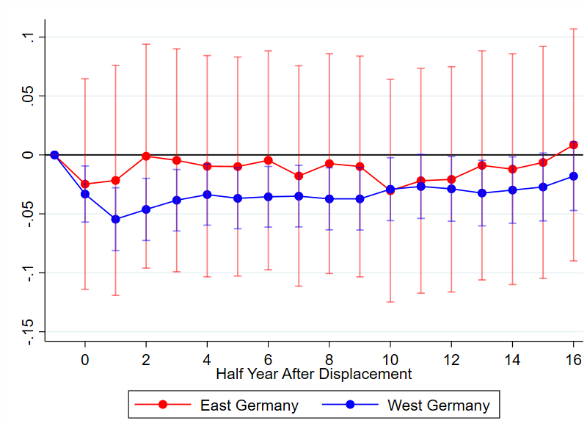
a) Days in regular employment (excl. VET)



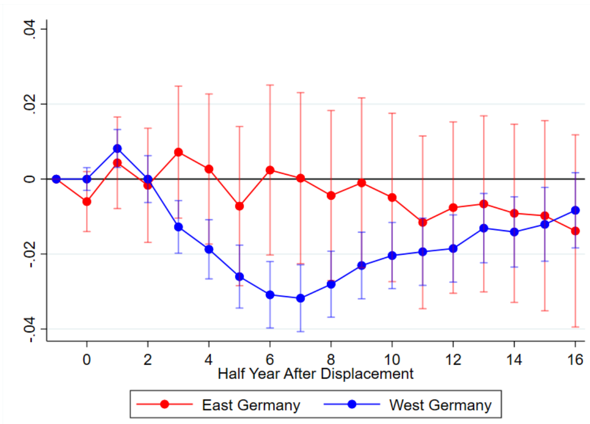
b) Mean daily wage from regular employment (excl. VET)



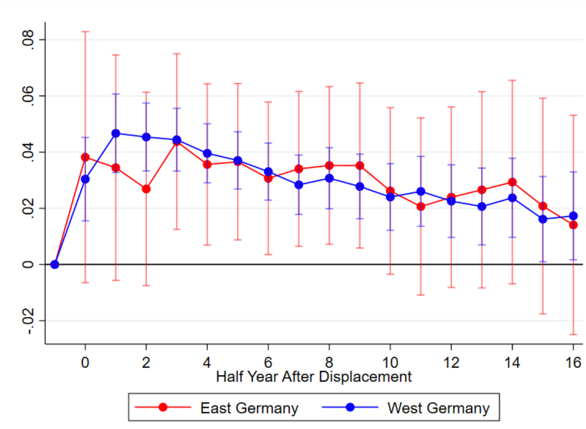
c) 1: Employed in specialist occupation



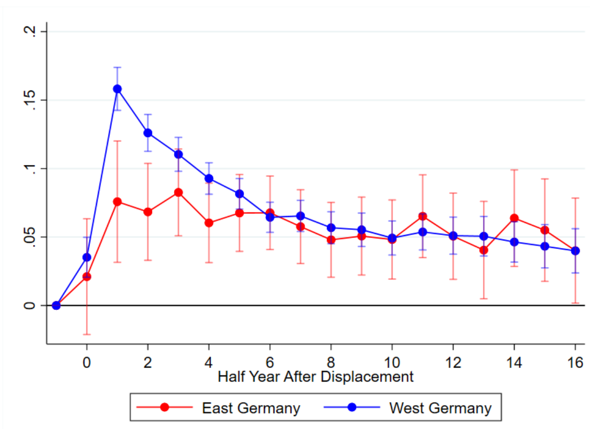
d) e: Holding vocational degree



e) 1: Employed outside first VET occupation



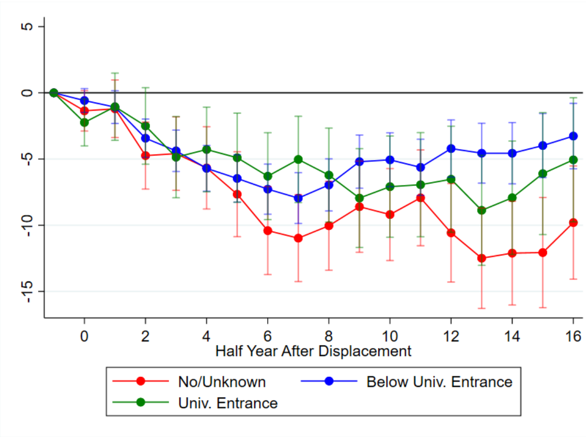
f) 1: Employed outside VET-start county



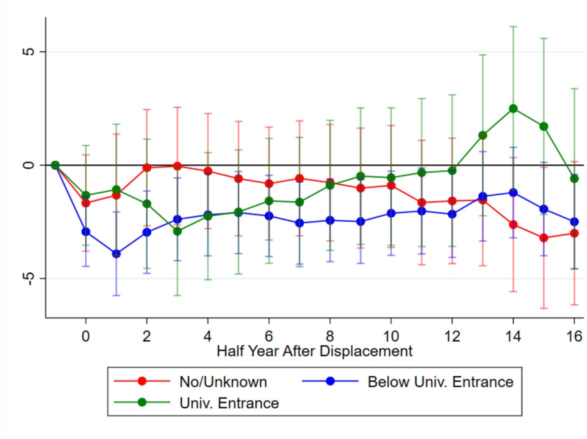
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A20: Effect of VET Displacement on Further Outcomes, by School Degree

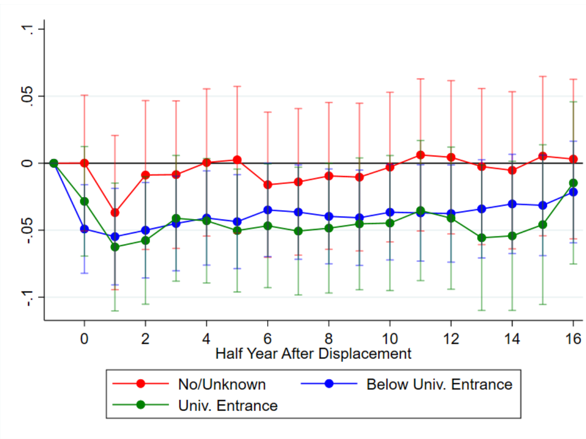
a) Days in regular employment (excl. VET)



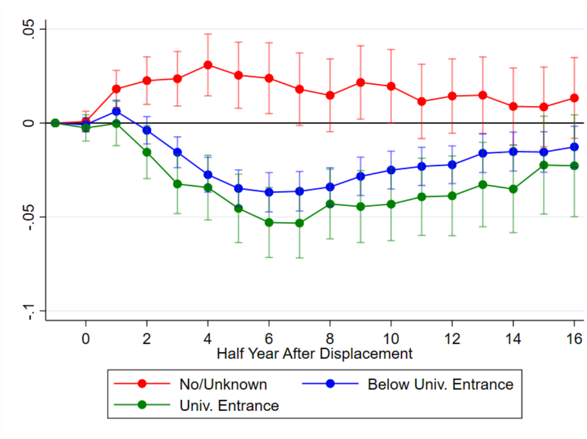
b) Mean daily wage from regular employment (excl. VET)



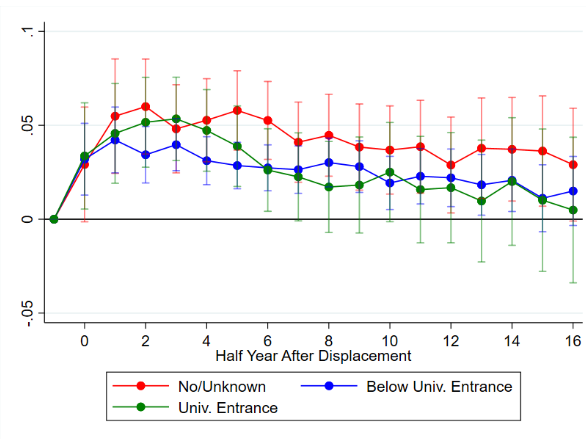
c) 1: Employed in specialist occupation



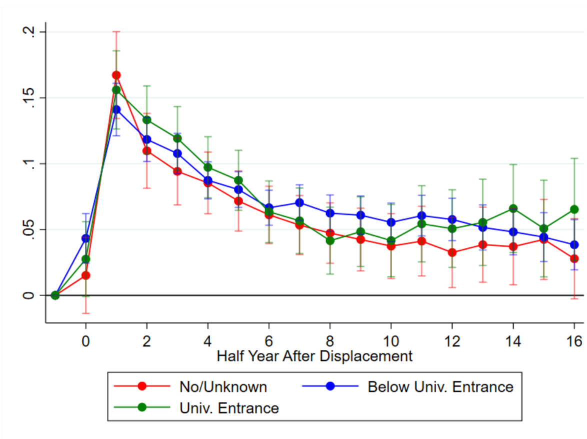
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



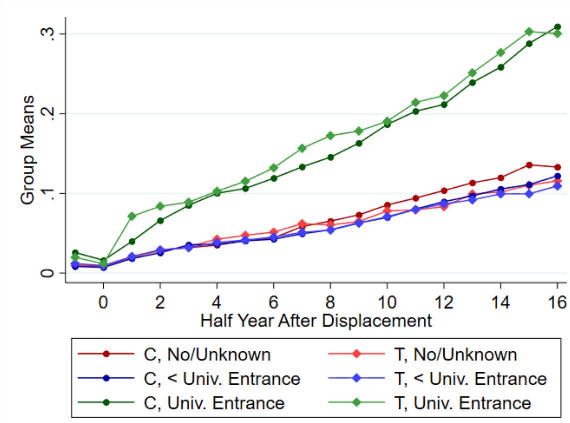
f) 1: Employed outside VET-start county



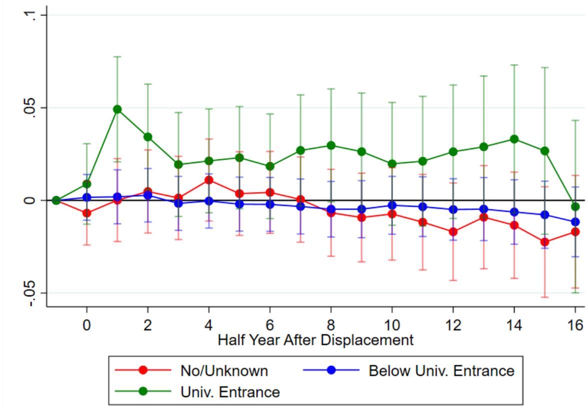
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A21: Effect of VET Displacement on High Job Quality and Tertiary Education, by School Degree

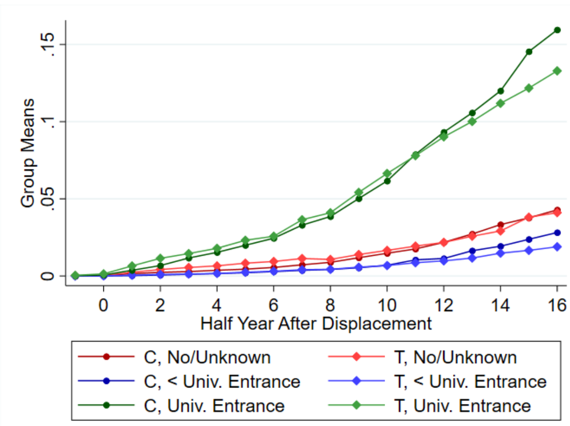
a) 1: Employed in (highly) complex occupation (levels)



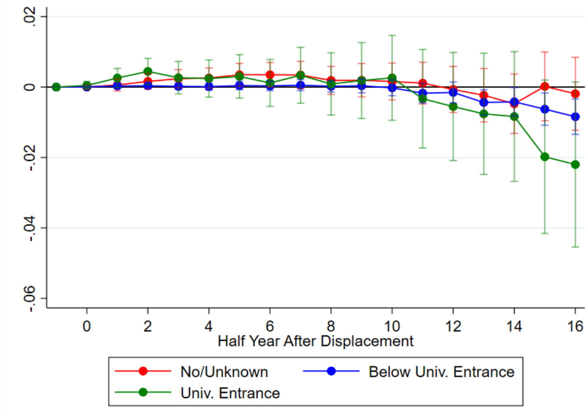
b) 1: Employed in (highly) complex occupation (DiD)



c) 1: Holding university degree (levels)



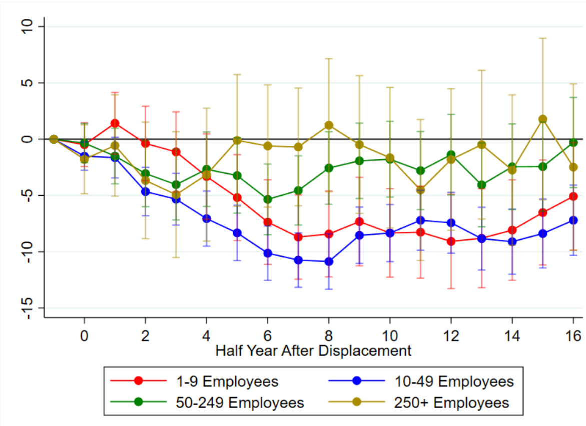
d) 1: Holding university degree (DiD)



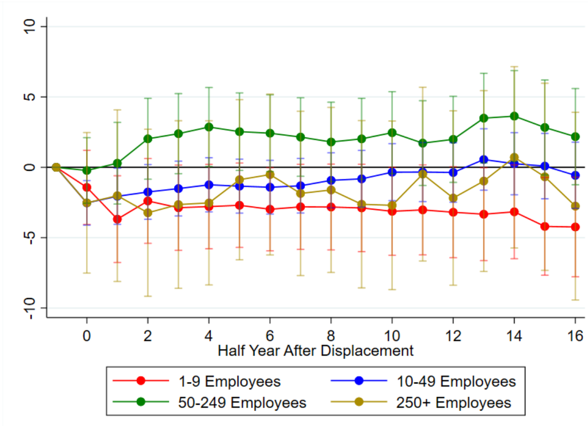
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A22: Effect of VET Displacement on Further Outcomes, by Firm Size

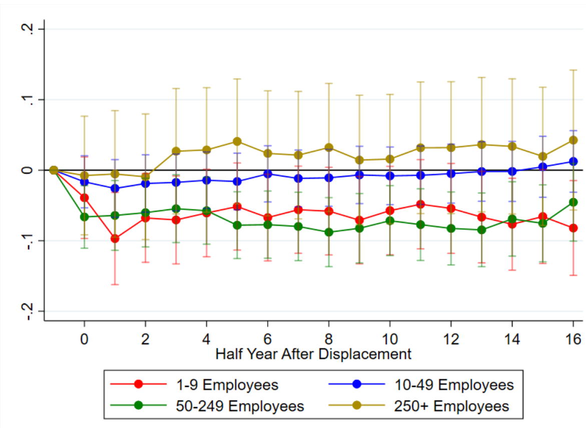
a) Days in regular employment (excl. VET)



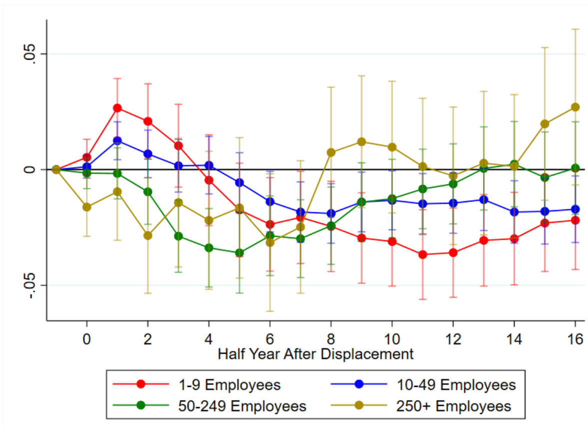
b) Mean daily wage from regular employment (excl. VET)



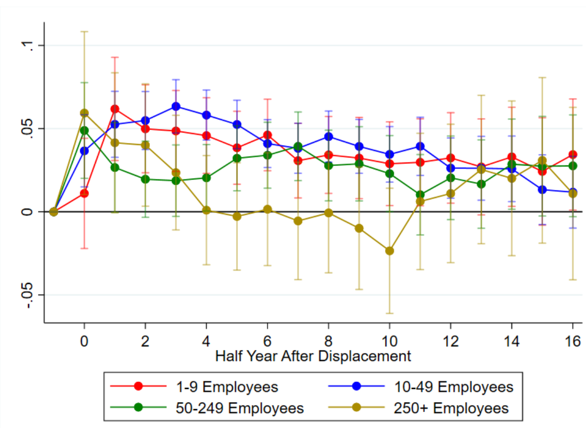
c) 1: Employed in specialist occupation



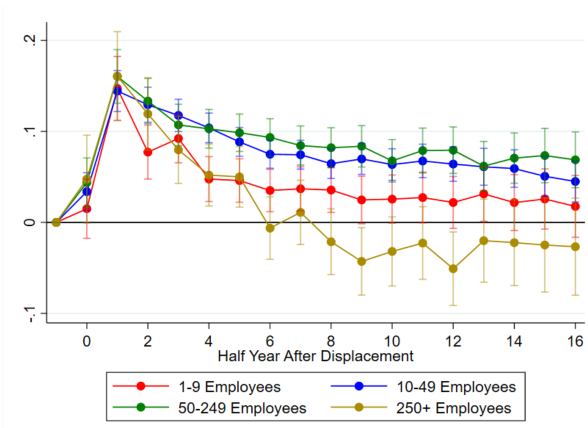
d) e: Holding vocational degree



e) 1: Employed outside first VET occupation



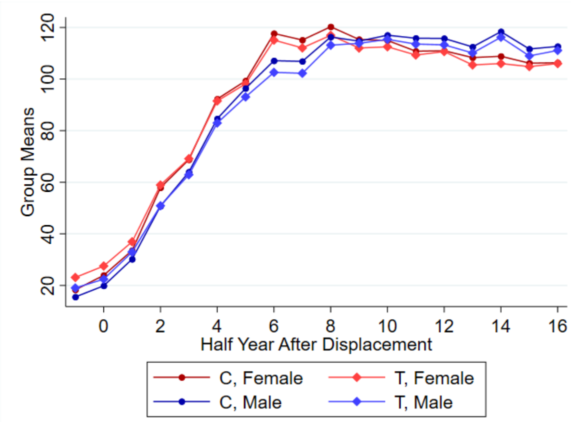
f) 1: Employed outside VET-start county



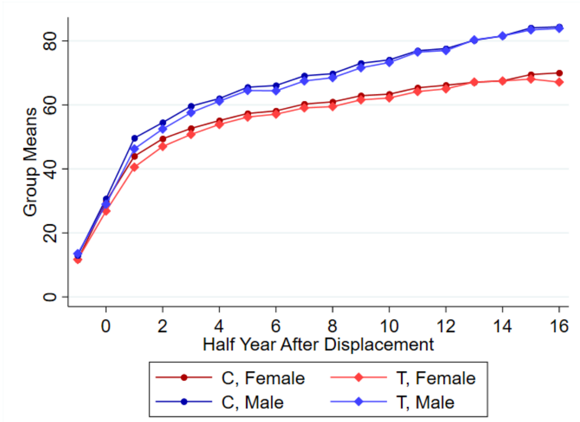
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A23: Evolution of Further Outcomes, by Gender

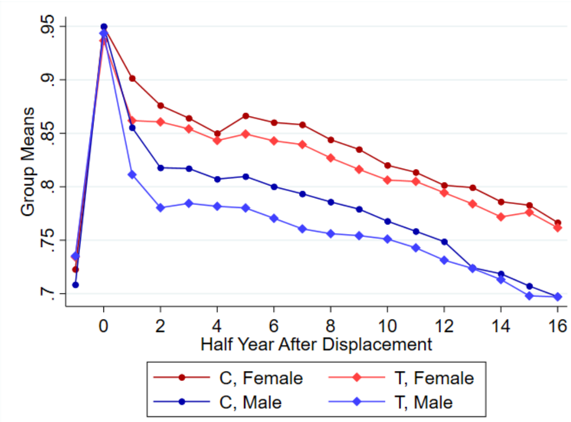
a) Days in regular employment (excl. VET)



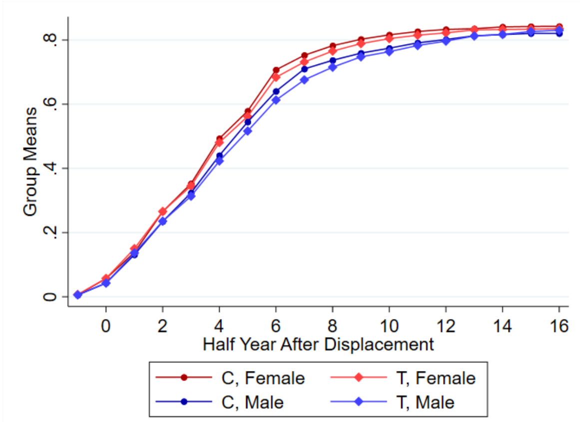
b) Mean daily wage from regular employment (excl. VET)



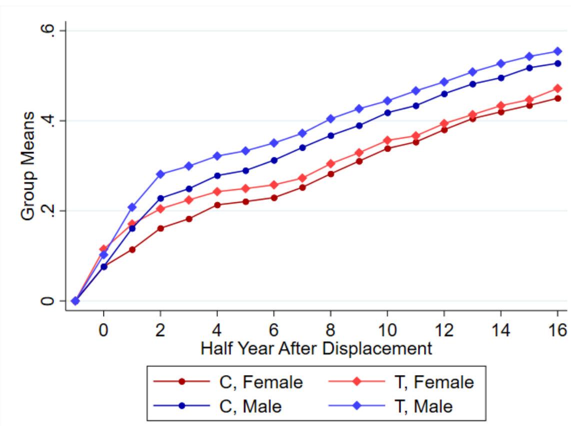
c) 1: Employed in specialist occupation



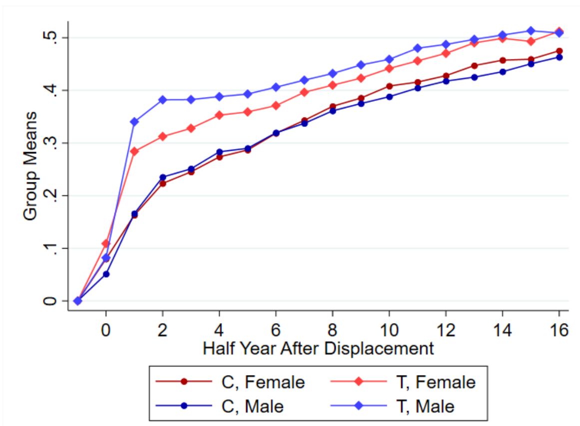
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



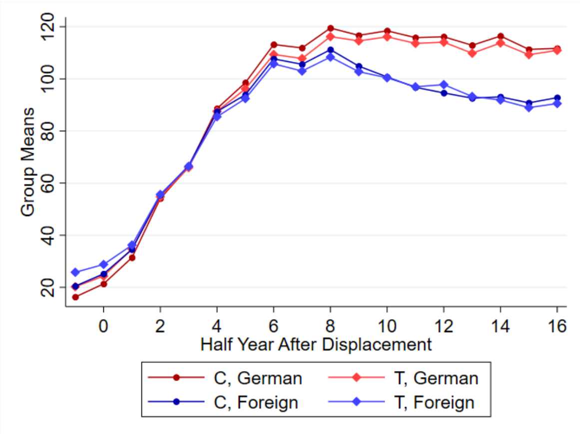
f) 1: Employed outside VET-start county



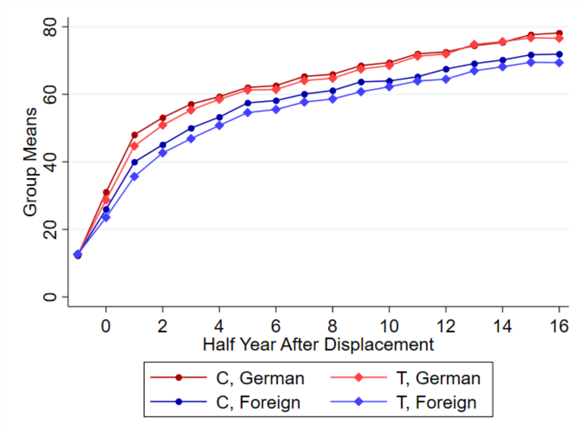
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A24: Evolution of Further Outcomes, by Citizenship

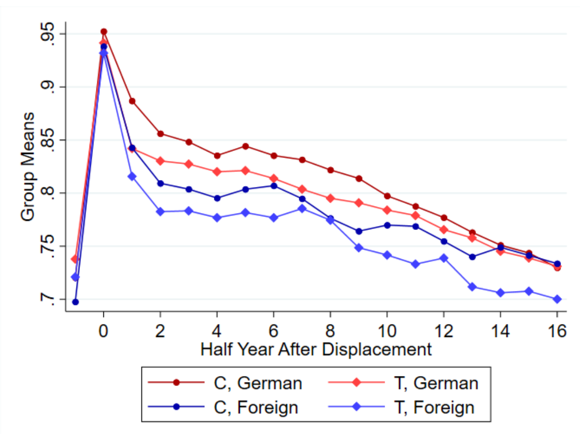
a) Days in regular employment (excl. VET)



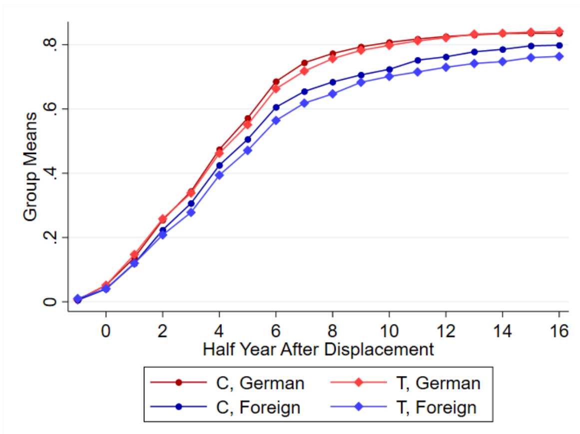
b) Mean daily wage from regular employment (excl. VET)



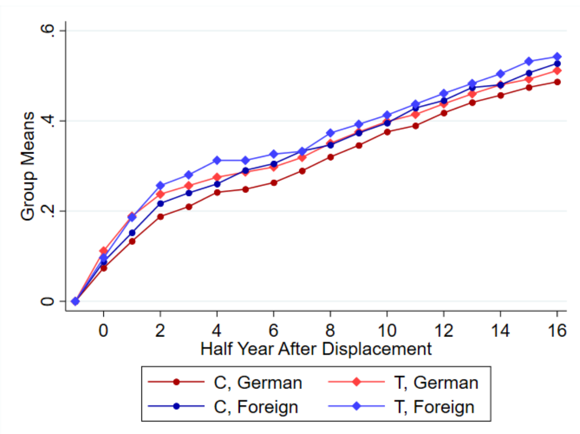
c) 1: Employed in specialist occupation



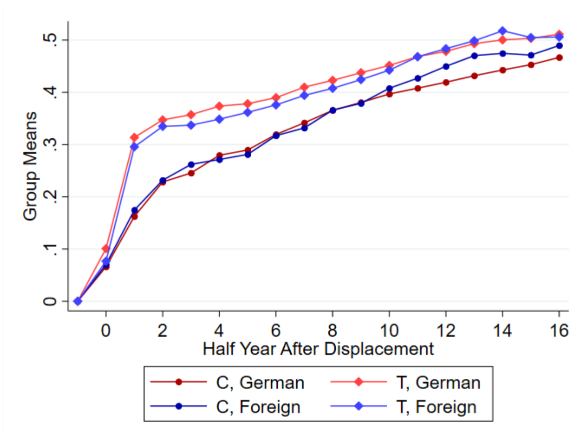
d) 1: Holding VET Degree



e) 1: Employed outside first VET occupation



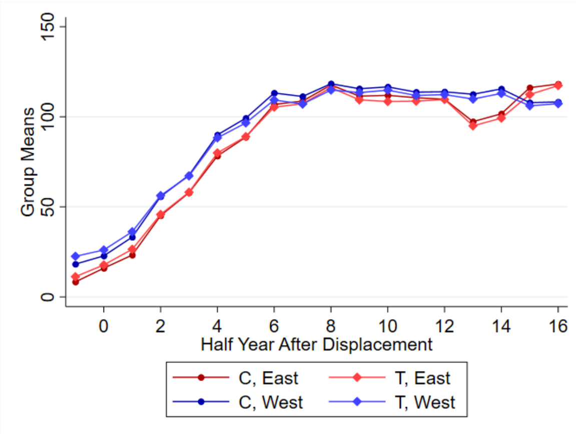
f) 1: Employed outside VET-start county



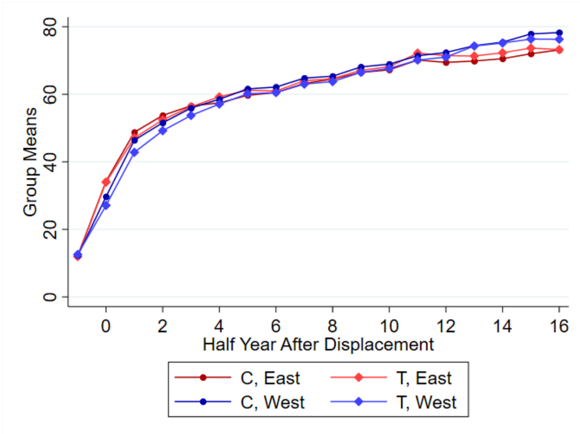
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A25: Evolution of Further Outcomes, by Region

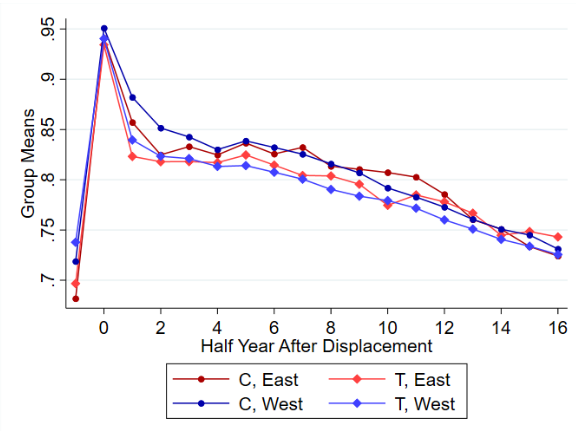
a) Days in regular employment (excl. VET)



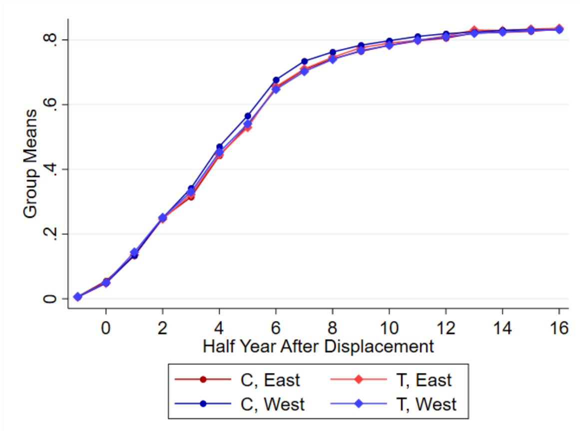
b) Mean daily wage from regular employment (excl. VET)



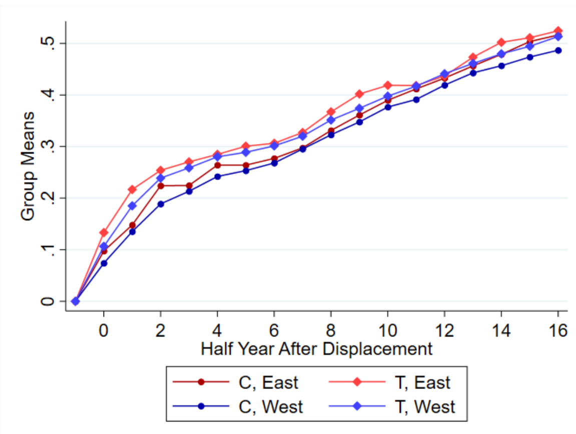
c) 1: Employed in specialist occupation



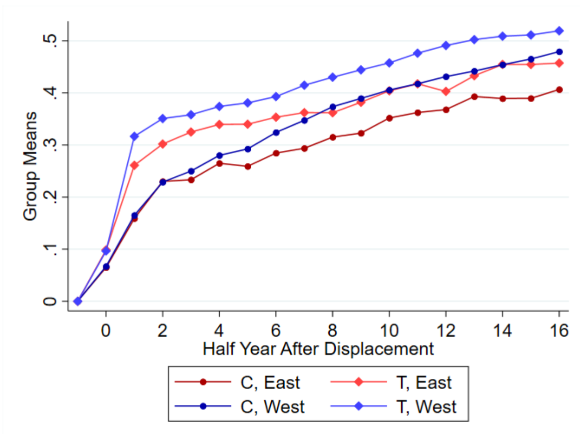
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



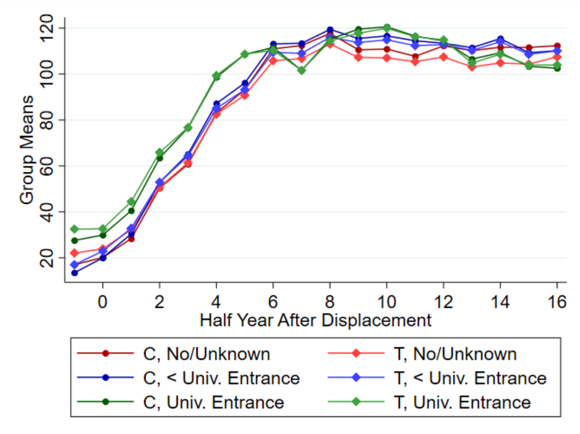
f) 1: Employed outside VET-start county



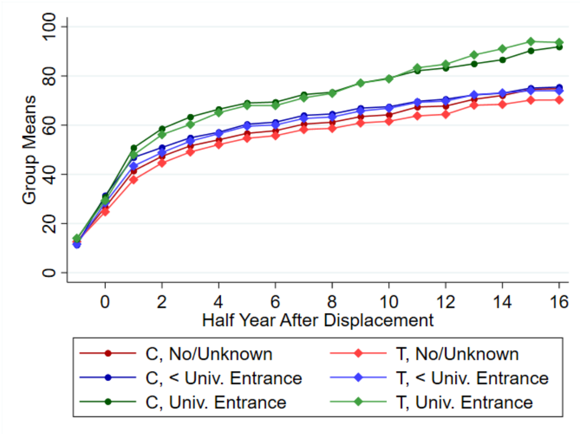
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A26: Evolution of Further Outcomes, by School Degree

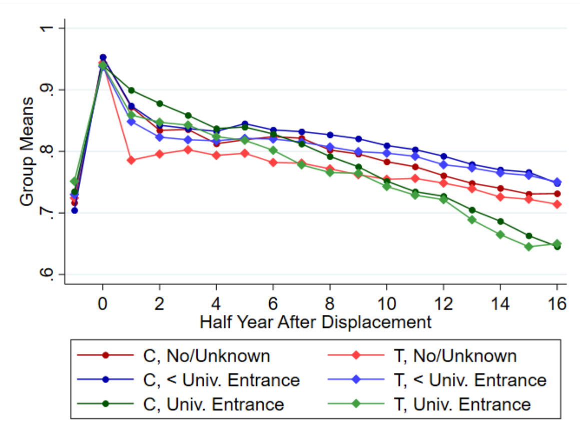
a) Days in regular employment (excl. VET)



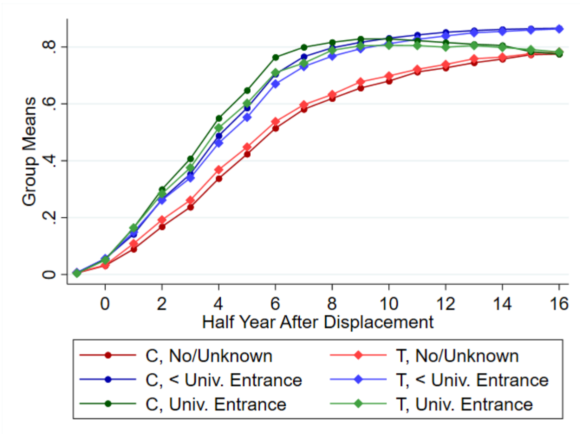
b) Mean daily wage from regular employment (excl. VET)



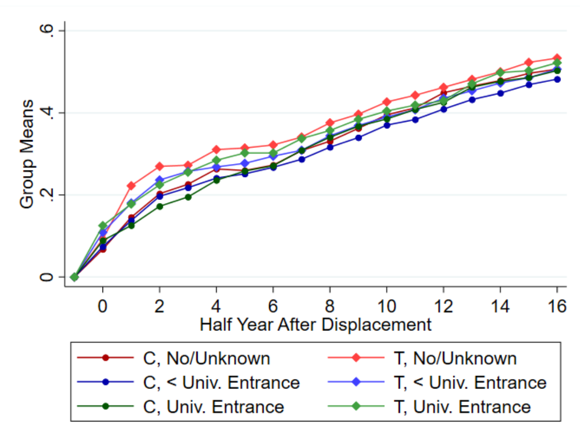
c) 1: Employed in specialist occupation



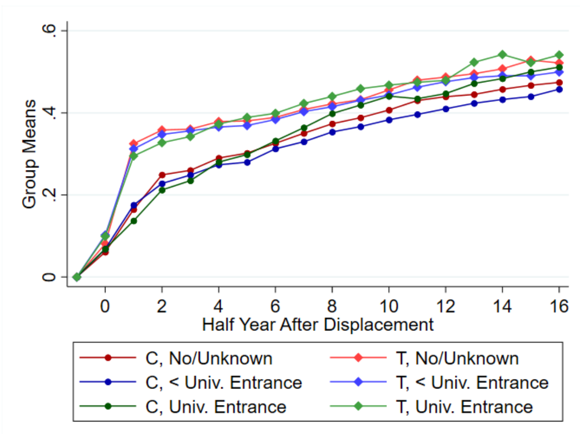
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



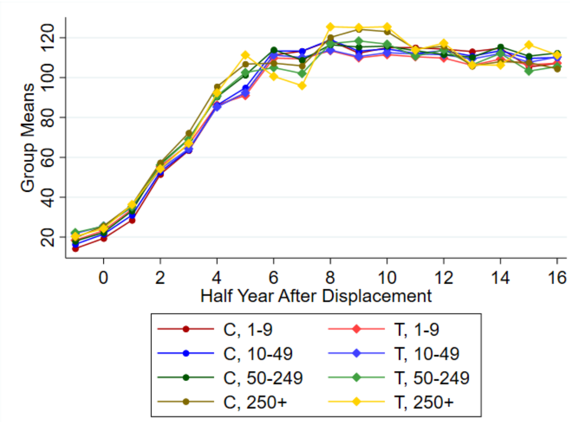
f) 1: Employed outside VET-start county



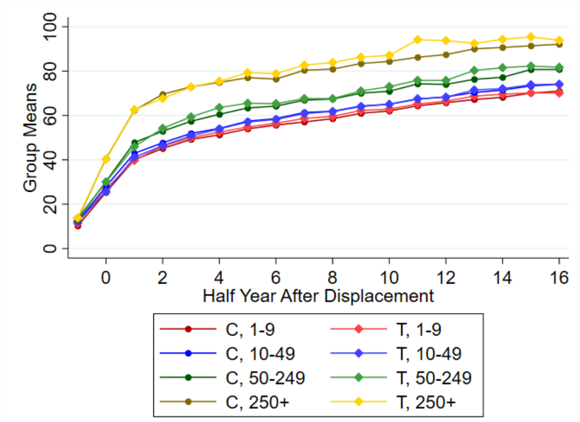
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A27: Evolution of Further Outcomes, by Firm Size

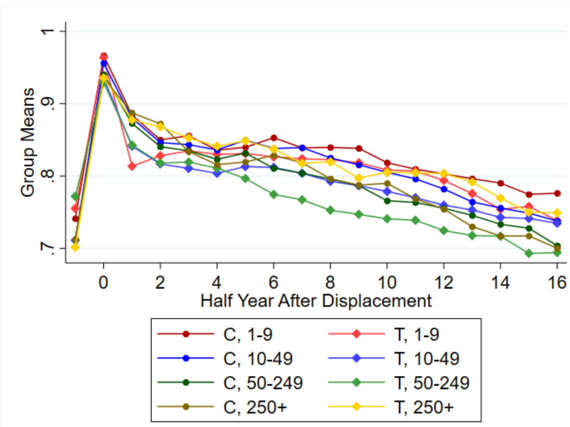
a) Days in regular employment (excl. VET)



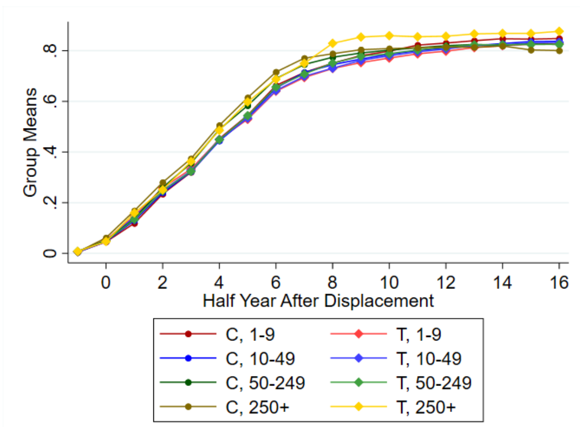
b) Mean daily wage from regular employment (excl. VET)



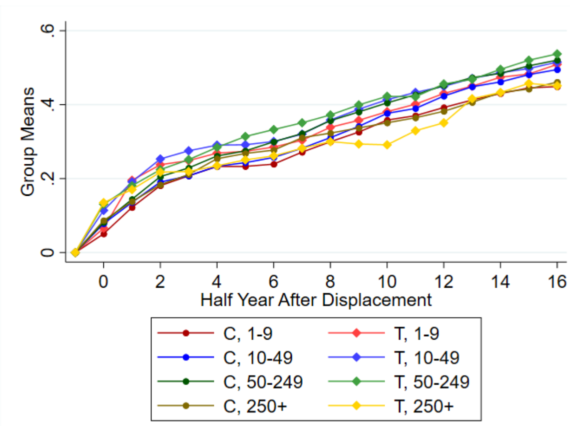
c) 1: Employed in specialist occupation



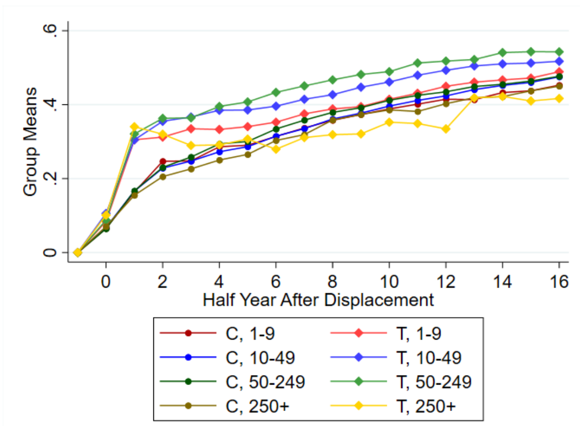
d) 1: Holding vocational degree



e) 1: Employed outside first VET occupation



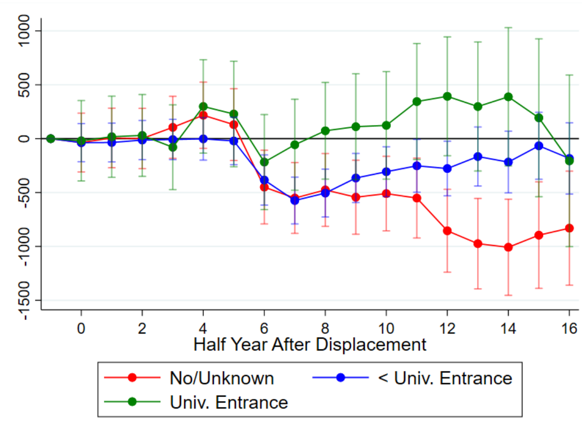
f) 1: Employed outside VET-start county



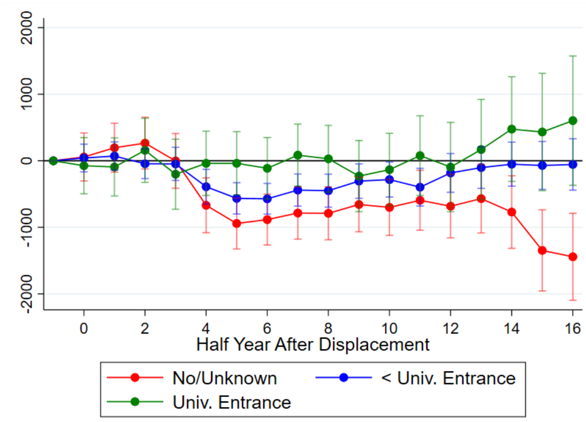
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A28: Effect of VET Displacement on Earnings, by School Degree and Displacement Year

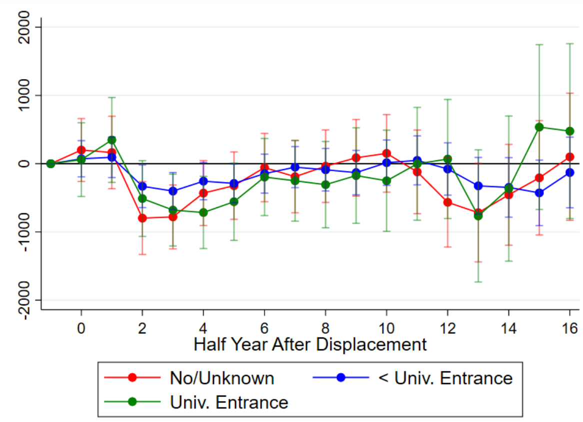
a) Year 1



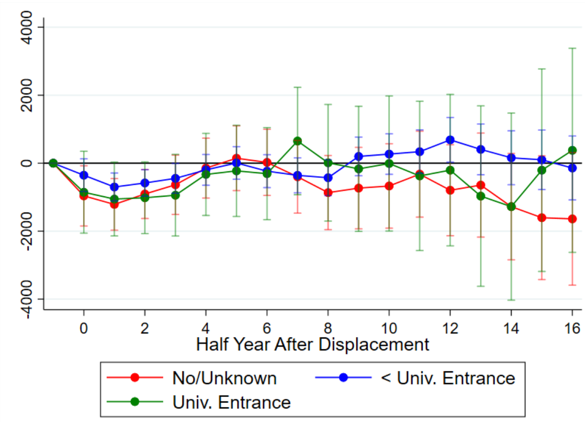
b) Year 2



c) Year 3



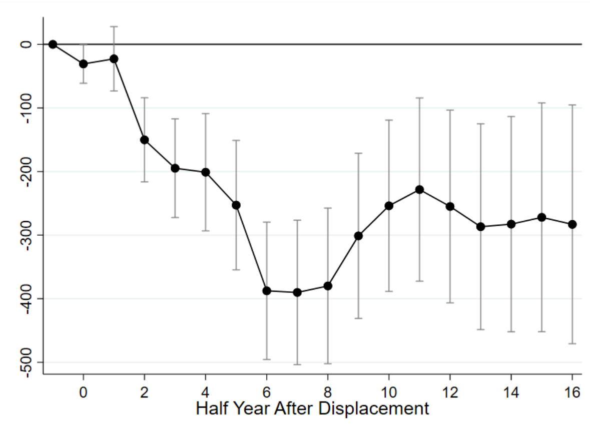
d) Year 4



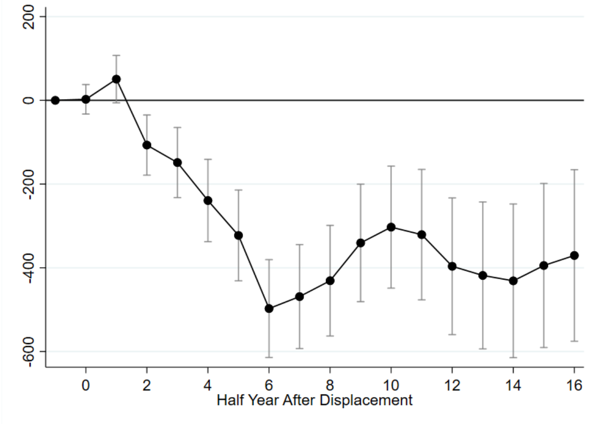
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A29: VET Displacement and Future Earnings by Displacement Type

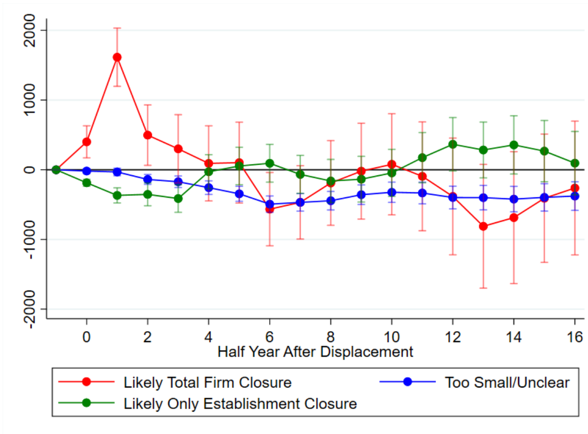
a) Full sample



b) Excluding cases of predicted mild closures



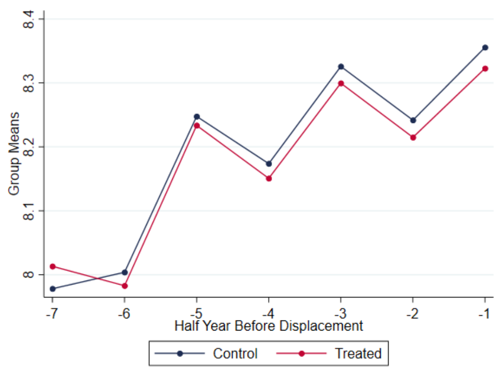
c) By predicted closure type



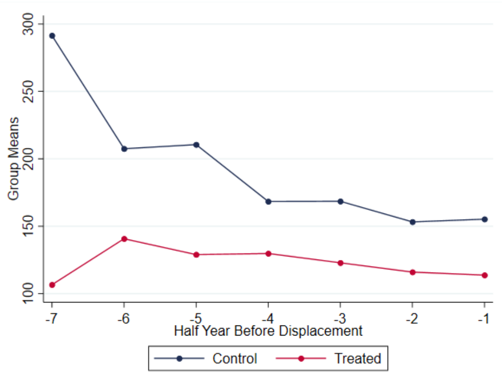
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A30: Pretrends Analysis: Mean Outcome Levels

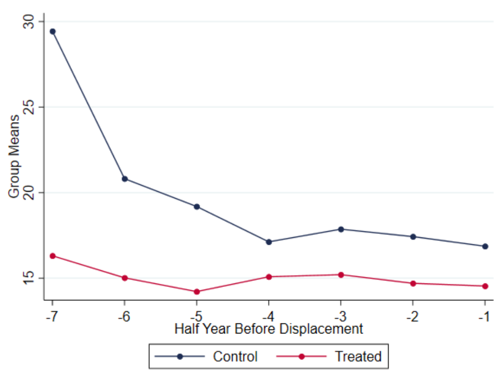
a) Log earnings from VET (levels)



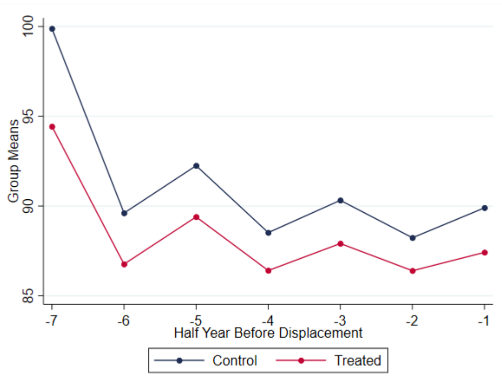
b) Number of coworkers (levels)



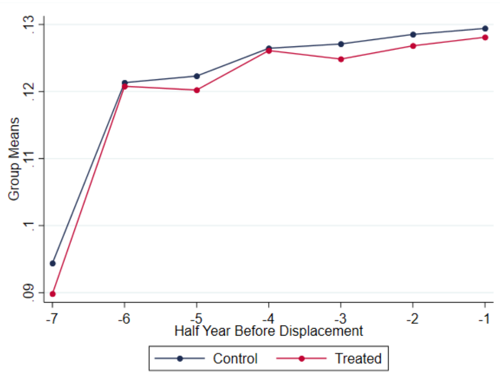
c) Number of co-apprentices (levels)



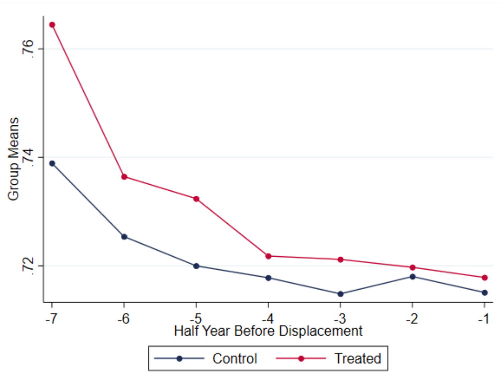
d) Median coworker's wage (levels)



e) Share of coworkers in helper occupations (levels)



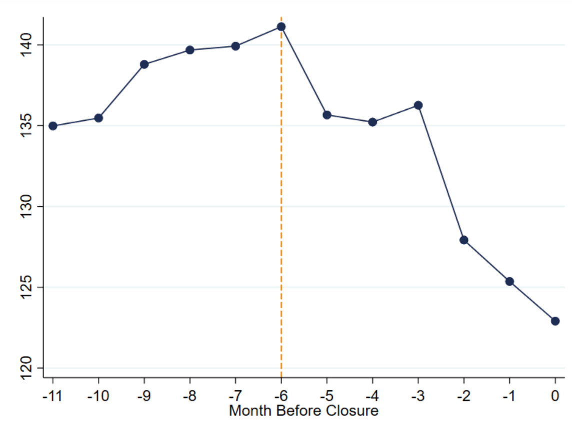
f) Share of coworkers in specialist occupations (levels)



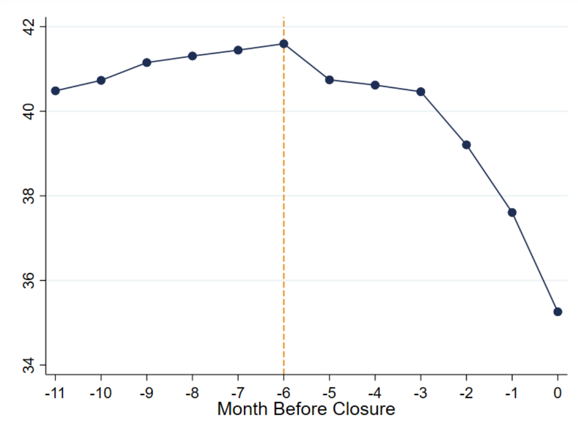
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A31: Employment in Closing Firms During the Last Year

a) Number of employees (levels, weighted)



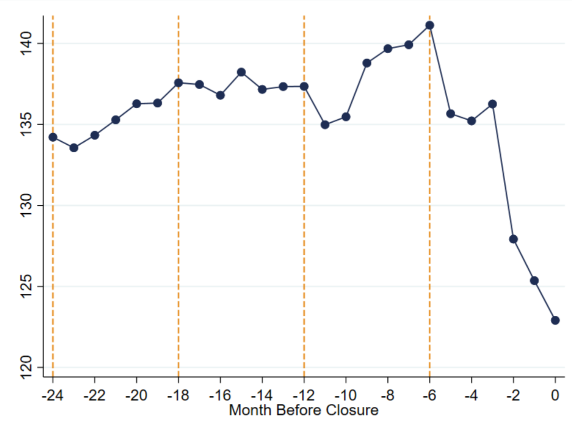
b) Number of employees (levels, unweighted)



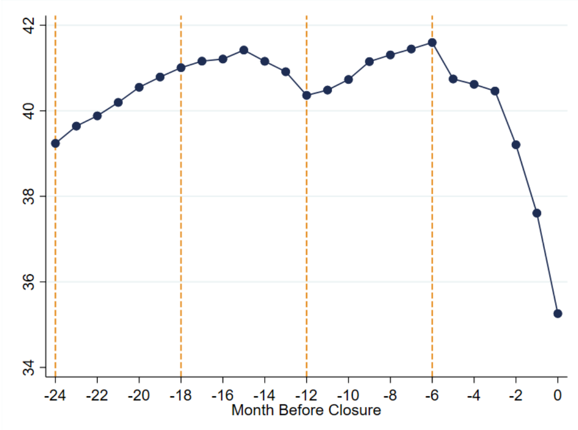
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A32: Employment in Closing Firms During the Last Two Years

a) Number of employees (levels, weighted)



b) Number of employees (levels, unweighted)

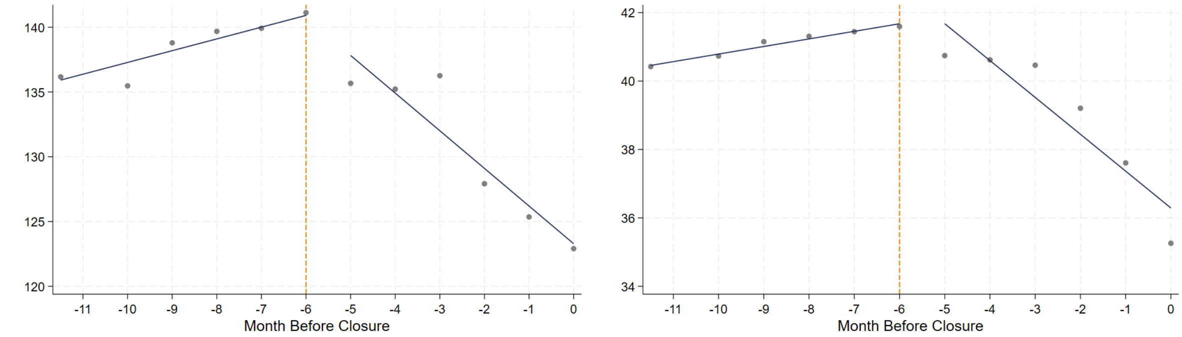


Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A33: Employment in the Closing Firms During the Last Year, Regression Fit By Half-Year

a) Number of employees (levels, weighted)

b) Number of employees (levels, unweighted)

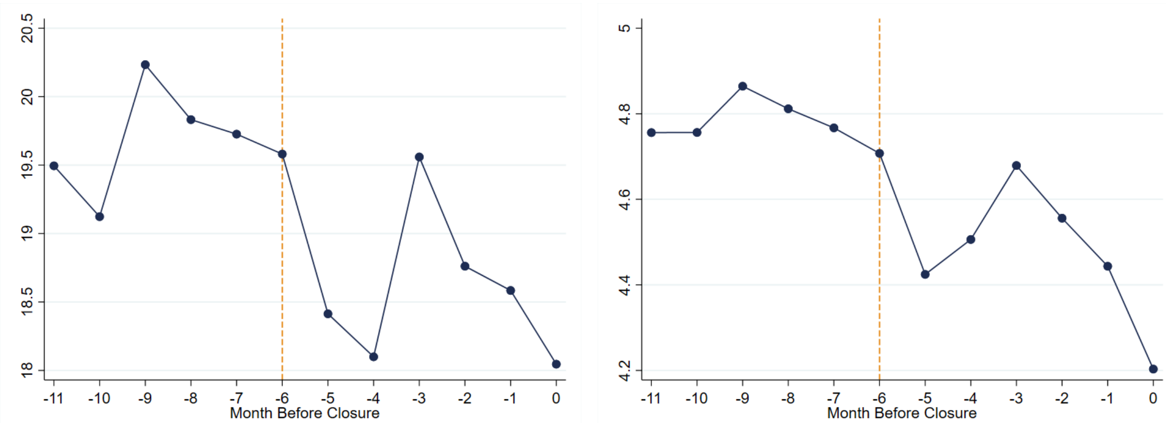


Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A34: Apprentices and Wages in Closing Firms During the Last Year

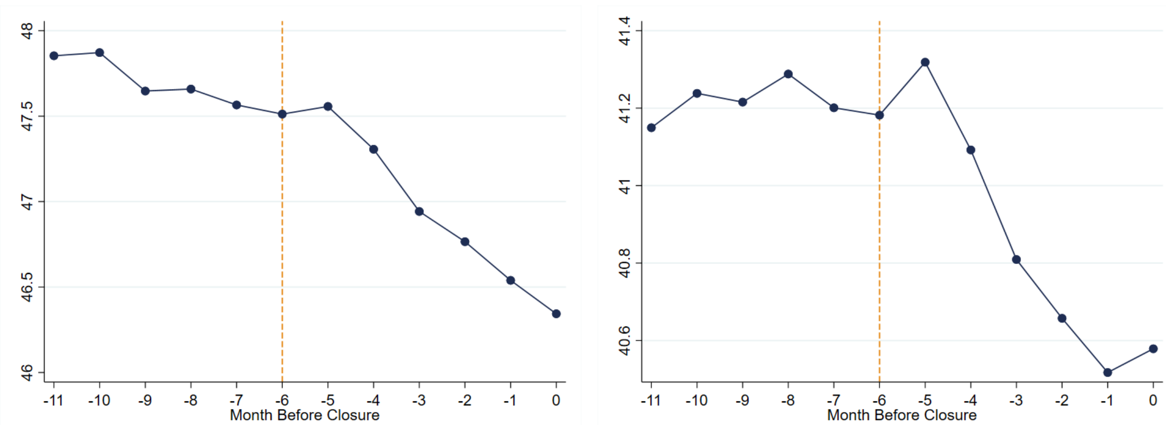
a) Number of apprentices (levels, weighted)

b) Number of apprentices (levels, unweighted)



c) Mean full-time daily wage (levels, weighted)

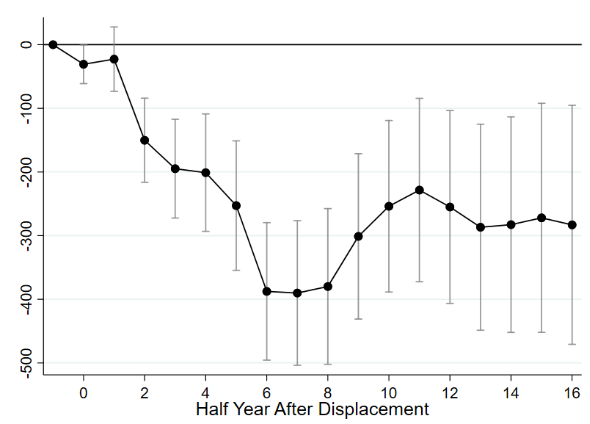
d) Mean full-time daily wage (levels, unweighted)



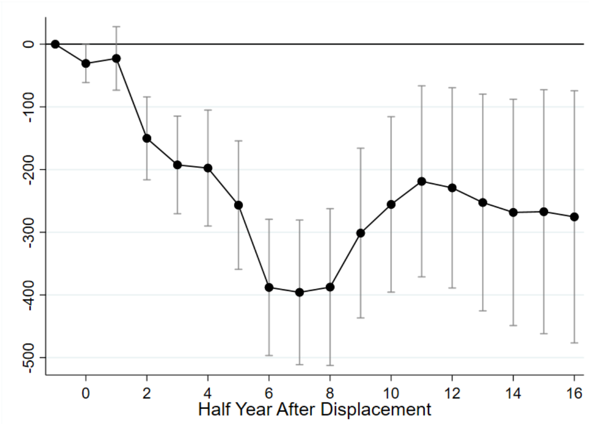
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A35: Robustness Analysis: Effect of VET Displacement on Earnings (Alternative Event Study Specifications)

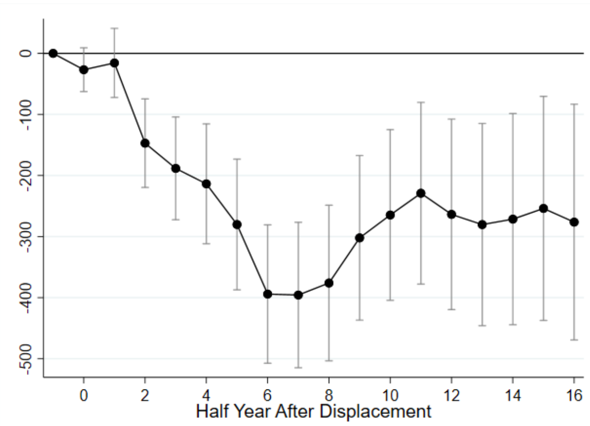
a) Main specification



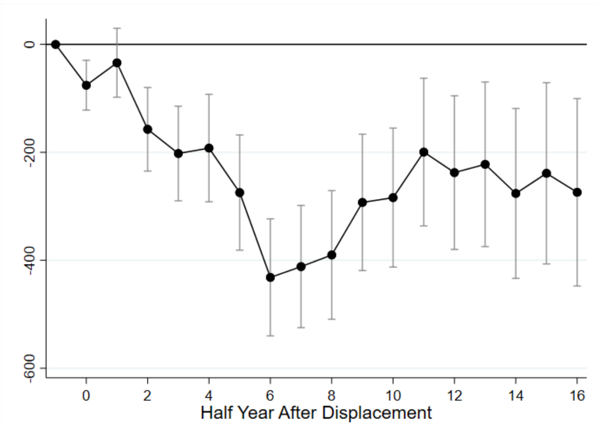
b) Robustness check 8.1.a): excluding individual FE



c) Robustness check 8.1.c): region- and occupation-specific time FE



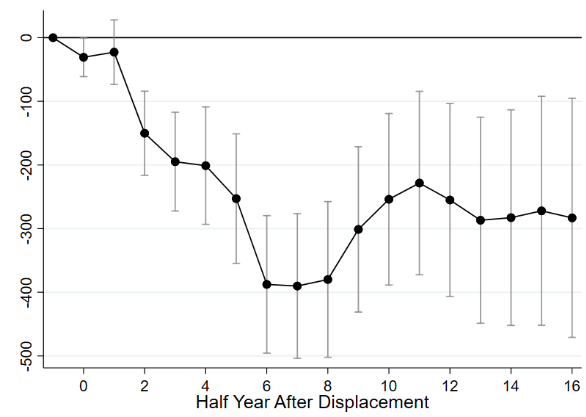
d) Robustness check 8.1.c): extended event study specification



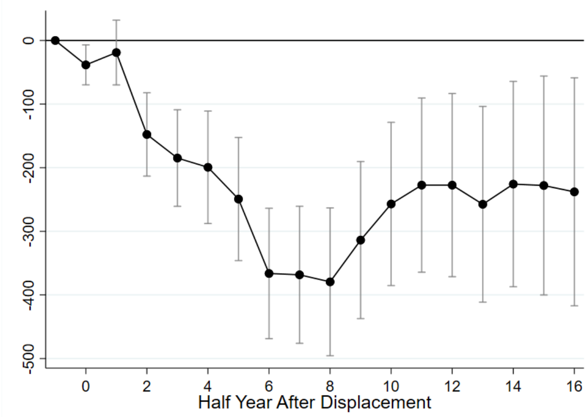
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A36: Robustness Analysis: Effect of VET Displacement on Earnings (Alternative Treatment Definitions)

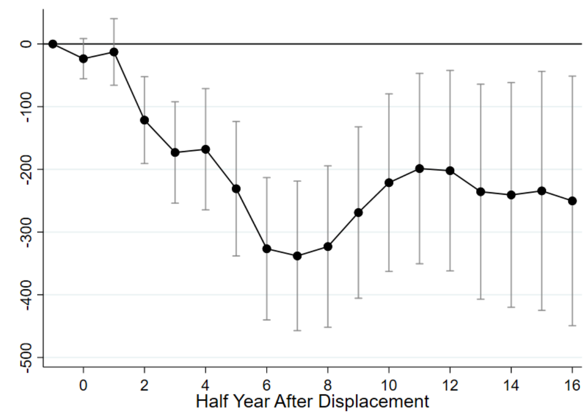
a) Main specification



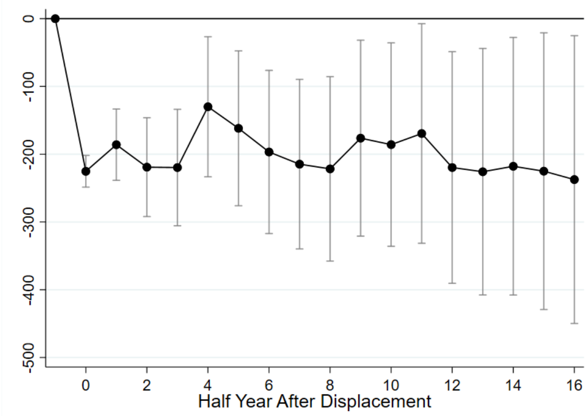
b) Robustness check 8.2.b): allowing for previous switches during VET



c) Robustness check 8.2.b): excluding closing firms with strong declines



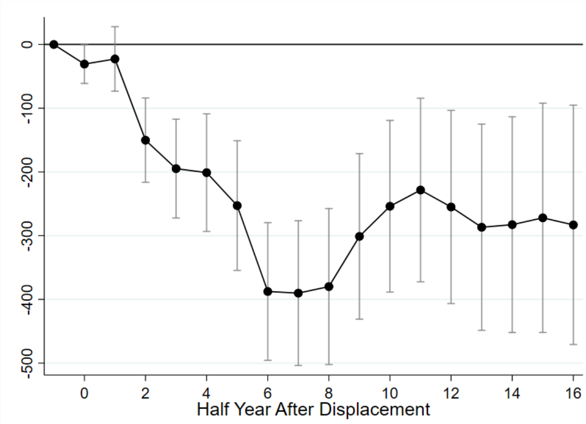
d) Robustness check 8.2.c): excluding early leavers



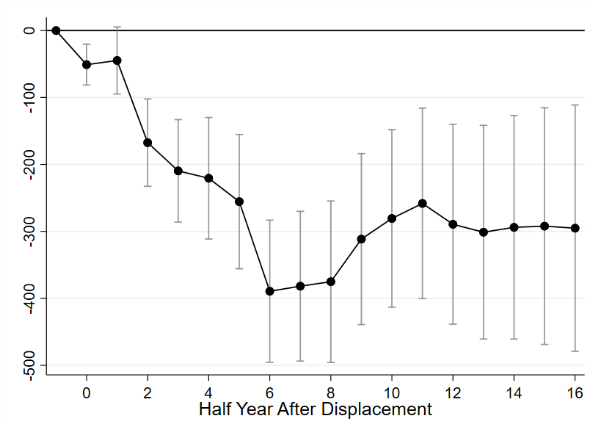
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A37: Robustness Analysis: Effect of VET Displacement on Earnings (Alternative Matching Designs)

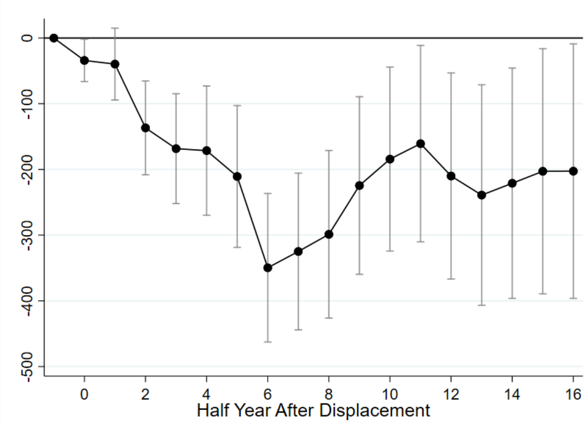
a) Main specification



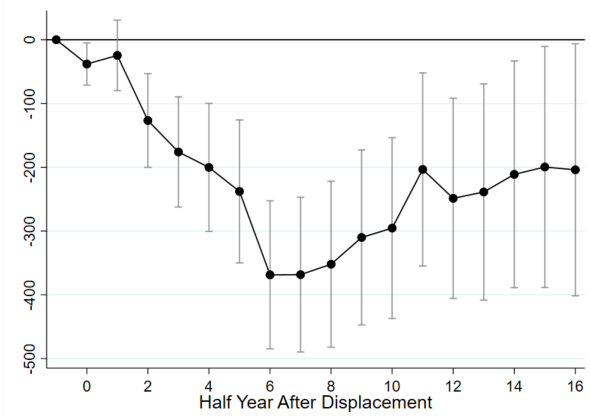
b) Robustness check 8.3.a):
omitting the caliper in the Mahalanobis distance matching



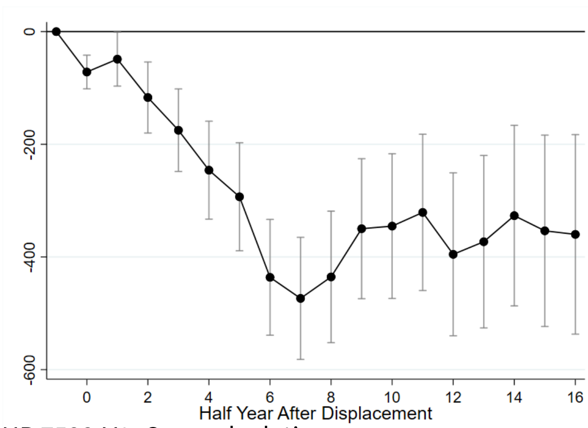
c) Robustness check 8.3.b):
accounting for randomness in matching



d) Robustness check 8.3.c):
matching within quintiles



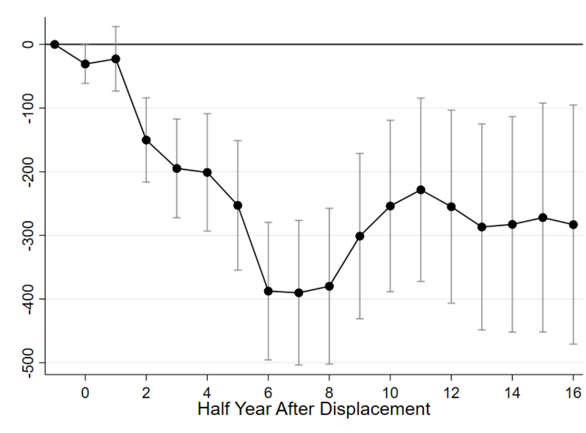
e) Robustness check 8.3.d):
matching on extended pre-treatment history



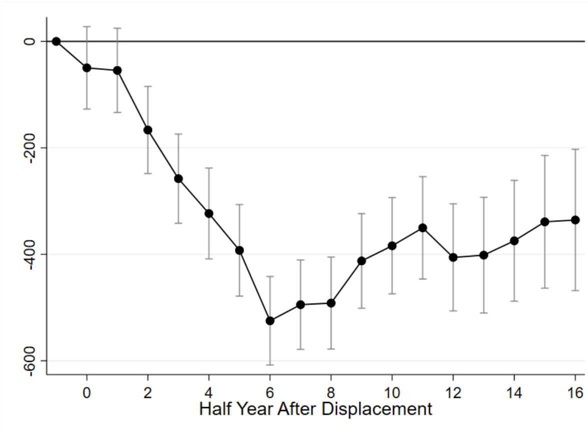
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Figure A38: Robustness Analysis: Effect of VET Displacement on Earnings (Alternative Weighting Strategies)

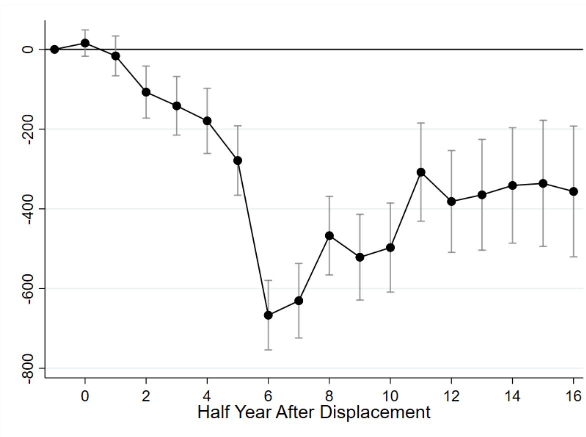
a) Main specification



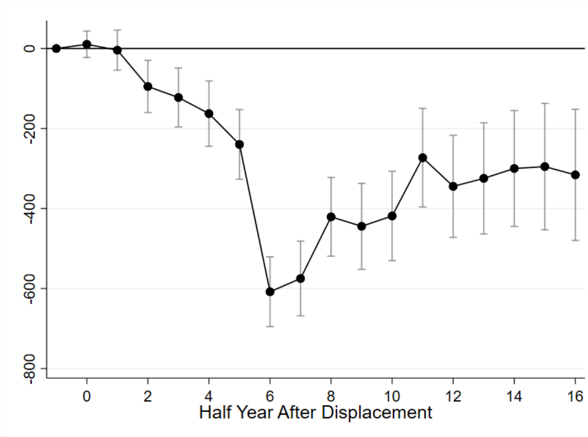
b) Robustness check 8.4.a): 1:n matching



c) Robustness check 8.4.b): inverse probability reweighting



d) Robustness check 8.4.c): inverse probability reweighting, excluding the top 0.5% of weights



Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

	(1) All Apprentices	(2) Displaced Apprentices
Female	0.438	0.492
	(0.496)	(0.500)
Foreign Citizenship	0.115	0.167
	(0.319)	(0.373)
Age	18.656	18.732
	(2.117)	(2.161)
East Germany	0.156	0.161
	(0.363)	(0.367)
No School Degree or Unknown	0.169	0.243
	(0.375)	(0.429)
Low or Medium School Degree	0.593	0.573
	(0.491)	(0.495)
High School Degree	0.238	0.184
	(0.426)	(0.387)
VET Wage	23.764	21.850
	(7.929)	(6.972)
Employees at Training Firm	110.613	99.108
	(393.101)	(449.848)
Apprentices at Training Firm	10.994	10.009
	(33.332)	(30.437)
AKM-Firm FE of Training Firm		
1st Quartile	0.063	0.118
	(0.242)	(0.323)
2nd Quartile	0.170	0.266
	(0.376)	(0.442)
3rd Quartile	0.261	0.289
	(0.439)	(0.453)
4th Quartile	0.382	0.248
	(0.486)	(0.432)
Missing	0.124	0.079
	(0.329)	(0.270)
Observations	4104041	26233

Notes: The table provides sample characteristics of all apprentices (column 1) and of the subset who are later displaced during VET(column 2). Standard deviations in parantheses.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A1: Sample Characteristics: All vs. Displaced Apprentices

	(1) Overall	(2) Displaced	(3) Matched Sample	(4) % Matched
Schleswig-Holstein	3.92	5.15	4.80	75.9
Hamburg	1.76	2.08	1.88	75.0
Lower Saxony	12.61	12.09	12.36	88.8
Bremen	0.76	0.94	0.74	66.5
North Rhine-Westphalia	22.91	23.79	24.40	88.4
Hesse	7.05	7.00	6.70	82.9
Rhineland-Palatinate	5.12	5.24	5.06	83.6
Baden-Württemberg	13.76	12.89	13.86	91.1
Bavaria	17.04	15.12	15.80	90.1
Saarland	1.22	1.31	1.08	71.4
Berlin	2.59	4.64	4.35	77.6
Brandenburg	2.25	2.73	2.38	77.0
Mecklenburg-Western Pomerania	1.62	1.53	1.36	77.9
Saxony	3.36	2.55	2.40	82.6
Saxony-Anhalt	2.06	1.52	1.37	82.4
Thuringia	1.95	1.42	1.43	86.3

Notes: This table shows the shares in percentage points of the 16 federal states, in column (1) among all apprentices at VET start, in column (2) among all displaced apprentices, and in column (3) among the matched sample. Note that by construction, the shares among treated and controls are equal. Column (4) shows the share of displaced apprentices matched by federal state.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A2: Federal State Composition Across Analysis Samples

	(1) Overall	(2) Displaced	(3) Matched Sample	(4) % Matched
Occ. in agriculture, forestry, and farming (11)	1.22	0.56	0.47	73.0
Occ. in gardening and floristry (12)	1.20	1.04	1.03	85.0
Occ. in production/processing of raw materials, glass-/ceramic-making, -processing (21)	0.24	0.10	0.06	53.8
Occ. in plastic-making and -processing, and wood-working and -processing (22)	2.60	2.15	2.10	83.2
Occ. in paper-making and -processing, printing, and in technical media design (23)	0.80	0.72	0.64	75.0
Occ. in metal-making and -working, and in metal construction (24)	4.48	3.10	3.19	87.9
Technical Occ. in machine-building and automotive industry (25)	8.55	7.82	8.39	92.2
Occ. in mechatronics, energy electronics and electrical engineering (26)	6.07	4.53	4.73	89.4
Occ. in technical R&D, construction, and production planning and scheduling (27)	1.06	0.72	0.77	91.1
Occ. in textile- and leather-making and -processing (28)	0.33	0.19	0.10	50.0
Occ. in food-production and -processing (29)	3.33	6.18	5.99	82.4
Occ. in construction scheduling, architecture and surveying (31)	0.17	0.07	0.03	42.1
Occ. in building construction above and below ground (32)	2.10	1.17	1.18	87.0
Occ. in interior construction (33)	2.68	2.14	2.10	84.9
Occ. in building services engineering and technical building services (34)	2.64	2.38	2.43	88.3
Occ. in mathematics, biology, chemistry and physics (41)	1.06	0.74	0.75	87.6
Occ. in geology, geography and environmental protection (42)	0.12	0.01	0.00	50.0
Occ. in computer science, information and communication technology (43)	1.56	1.05	1.10	88.8
Occ. in traffic and logistics (without vehicle driving) (51)	3.62	3.26	3.20	83.7
Drivers and operators of vehicles and transport equipment (52)	0.56	0.34	0.22	56.7
Occ. in safety and health protection, security and surveillance (53)	0.27	0.46	0.24	43.0
Occ. in cleaning services (54)	0.17	0.16	0.08	37.2
Occ. in purchasing, sales and trading (61)	3.64	2.53	2.42	82.1
Sales Occ. in retail trade (62)	10.46	13.04	13.60	88.9
Occ. in tourism, hotels and restaurants (63)	4.41	9.63	9.44	83.5
Occ. in business management and organisation (71)	10.67	8.18	8.05	84.2
Occ. in financial services, accounting and tax consultancy (72)	4.89	2.62	2.58	85.3
Occ. in law and public administration (73)	2.83	1.29	1.26	86.7
Medical and health care Occ. (81)	11.13	16.01	16.43	88.9
Occ. in non-medical healthcare, body care, wellness and medical technicians (82)	4.48	5.70	5.81	87.8
Occ. in education and social work, housekeeping, and theology (83)	1.41	0.72	0.63	75.3
Occ. in philology, literature, humanities, social sciences, and economics (91)	0.01	0.00	.	.
Occ. in advertising and marketing, in commercial and editorial media design (92)	0.66	0.76	0.55	61.8
Occ. in product design, artisan craftwork, fine arts, making of musical instruments (93)	0.41	0.34	0.29	71.1
Occ. in the performing arts and entertainment (94)	0.19	0.28	0.55	39.7

Notes: This table shows the shares in percentage values of the 2-digit Occ., in column (1) among all apprentices at VET start, in column (2) among all displaced apprentices, and in column (3) among the matched sample. Note that by construction, the shares among treated and controls are equal. Column (4) shows the share of displaced apprentices matched by 2-digit occupation. In occupation 91 *(Occupations in in philology, literature, humanities, social sciences, and economics)*, no single apprentices was displaced. Only two apprentices were displaced in occupation 42 *(Occupations in geology, geography and environmental protection)*, with one of them being matched.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

	(1) Matched	(2) Unmatched
Female	0.499	0.453
	(0.500)	(0.498)
Foreign Citizenship	0.150	0.271
	(0.357)	(0.444)
Age	19.361	19.932
	(2.259)	(2.480)
East Germany	0.133	0.204
	(0.340)	(0.403)
School Degree: No/Unknown	0.220	0.382
	(0.414)	(0.486)
School Degree: Below Univ. Entrance	0.591	0.468
	(0.492)	(0.499)
School Degree: Univ. Entrance	0.189	0.150
	(0.392)	(0.357)
Displaced in 1st Year of VET	0.359	0.456
	(0.480)	(0.498)
Displaced in 2nd Year of VET	0.318	0.282
	(0.466)	(0.450)
Displaced in 3rd Year of VET	0.233	0.139
	(0.423)	(0.346)
Displaced in 4th Year of VET	0.090	0.123
	(0.287)	(0.328)
VET Wage	24.825	23.630
	(7.398)	(11.709)
Earnings Beside VET	251.454	675.502
	(876.159)	(1913.968)
Employees at Training Firm	101.156	86.052
	(425.980)	(360.078)
Apprentices at Training Firm	12.009	22.797
	(31.063)	(52.381)
Δ Employees in Last 2 Years	0.144	2.169
	(1.265)	(18.866)
Share of Apprentices	0.179	0.277
	(0.172)	(0.321)
Share of Old Employees	0.231	0.167
	(0.157)	(0.168)
Share of Unskilled Employees	0.236	0.323
	(0.164)	(0.237)
AKM-Firm FE of Training Firm		
1st Quartile	0.110	0.215
	(0.313)	(0.411)
2nd Quartile	0.277	0.260
	(0.447)	(0.438)
3rd Quartile	0.312	0.217
	(0.463)	(0.412)
4th Quartile	0.270	0.186
	(0.444)	(0.389)
Missing	0.031	0.123
	(0.173)	(0.328)
VET Tightness	1.471	2.850
	(3.555)	(17.737)
Share of Vacancies Unfilled	0.668	0.668
	(0.209)	(0.238)
Observations	22496	3729

Notes: Sample characteristics of matched (column 1) and unmatched displaced apprentices (column 2). Standard deviations in parentheses.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A4: Sample Characteristics: Matched vs. Unmatched Displaced Apprentices

	(1) 1: VET Graduate	(2) Time Until Graduation	(3) Time Until Dropout
Treatment	-0.085***	-0.034	-0.135**
	(0.021)	(0.056)	(0.067)
VET Year 2	0.430***	-2.007***	-0.187***
	(0.025)	(0.049)	(0.071)
VET Year 3	0.673***	-3.690***	-1.269***
	(0.030)	(0.051)	(0.073)
VET Year 4	0.935***	-5.280***	-2.112***
	(0.049)	(0.061)	(0.091)
VET Year 2 $\times$ Treatment	-0.114***	0.572***	0.035
	(0.033)	(0.073)	(0.094)
VET Year 3 $\times$ Treatment	0.003	0.282***	0.252***
	(0.038)	(0.070)	(0.092)
VET Year 4 $\times$ Treatment	-0.012	0.188**	0.355***
	(0.056)	(0.077)	(0.109)
Female	0.085***	-0.129***	-0.006
	(0.019)	(0.038)	(0.051)
Age (at Begin of Half-Year)	-0.012***	-0.063***	-0.048***
	(0.004)	(0.008)	(0.011)
Foreign Citizenship	-0.203***	0.160***	0.028
	(0.020)	(0.044)	(0.056)
School Degree: Below Univ. Entrance	0.223***	-0.317***	-0.130***
	(0.018)	(0.042)	(0.049)
School Degree: Univ. Entrance	0.291***	-0.614***	0.114*
	(0.024)	(0.049)	(0.069)
Training Firm AKM FE in Q2	0.066**	-0.009	0.173**
	(0.028)	(0.065)	(0.079)
Training Firm AKM FE in Q3	0.147***	-0.136**	0.281***
	(0.029)	(0.063)	(0.080)
Training Firm AKM FE in Q4	0.177***	-0.204***	0.405***
	(0.030)	(0.065)	(0.085)
Training Firm AKM FE Missing	0.076*	-0.049	0.319***
	(0.040)	(0.086)	(0.113)
Constant	0.769***	8.404***	2.400***
	(0.096)	(0.239)	(0.278)
Pseudo R Squared	0.065		
R Squared		0.358	0.123
Observations	44798	37565	8654

Notes: This table shows the estimation results for a probit model for VET graduation (Column (1)) and linear models for the time until graduation (Column (2)) and for dropping out (Column (3)). Estimated fixed effects are available upon request.
$^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A5: Estimation Results: Short-Run Outcomes on Training Success

	(1) 1: Continue VET	(2) 1: Switch VET Occ	(3) 1: Switch VET County	(4) Δ Firm FE
Treatment	-0.144***	0.223***	0.558***	-0.041***
	(0.030)	(0.028)	(0.027)	(0.006)
VET Year 2	0.313***	-0.184***	-0.171***	-0.020***
	(0.037)	(0.033)	(0.033)	(0.008)
VET Year 3	-0.348***	0.248***	0.247***	-0.007
	(0.034)	(0.035)	(0.034)	(0.007)
VET Year 4	-1.986***	0.700***	0.557***	-0.037***
	(0.044)	(0.064)	(0.064)	(0.013)
VET Year 2 $\times$ Treatment	-0.140***	0.077*	0.153***	0.017**
	(0.047)	(0.042)	(0.040)	(0.008)
VET Year 3 $\times$ Treatment	-0.036	-0.071	-0.008	0.015*
	(0.040)	(0.043)	(0.041)	(0.007)
VET Year 4 $\times$ Treatment	0.150***	-0.154*	-0.075	0.033**
	(0.045)	(0.082)	(0.079)	(0.015)
Female	0.040*	-0.036	-0.004	-0.006*
	(0.023)	(0.024)	(0.021)	(0.003)
Age (at Begin of Half-Year)	-0.045***	0.023***	0.038***	0.002***
	(0.005)	(0.005)	(0.004)	(0.001)
Foreign Citizenship	-0.093***	0.087***	0.054**	-0.003
	(0.025)	(0.026)	(0.024)	(0.004)
School Degree: Below Univ. Entrance	0.106***	-0.031	0.006	-0.003
	(0.022)	(0.023)	(0.021)	(0.003)
School Degree: Univ. Entrance	0.076**	-0.157***	-0.070**	0.006
	(0.029)	(0.031)	(0.028)	(0.004)
Training Firm AKM FE in Q2	0.169***	-0.067*	-0.071**	-0.126***
	(0.035)	(0.037)	(0.035)	(0.007)
Training Firm AKM FE in Q3	0.208***	-0.072*	-0.016	-0.176***
	(0.036)	(0.037)	(0.034)	(0.008)
Training Firm AKM FE in Q4	0.218***	-0.058	0.002	-0.203***
	(0.037)	(0.039)	(0.036)	(0.008)
Training Firm AKM FE Missing	0.130***	0.123**	0.027	-0.131***
	(0.048)	(0.051)	(0.046)	(0.013)
Constant	2.033***	-1.807***	-2.102***	0.178***
	(0.124)	(0.120)	(0.111)	(0.017)
Pseudo R Squared	0.235	0.059	0.075	
R Squared				0.101
Observations	44990	38377	38377	21559

Notes: This table shows the estimation results for probit models for continuing VET (Column (1)), for switching training occupation (Column (2)) and workplace county (Column (3)), a and linear model for the change in the new training firm's AKM Firm FE relative to the first one, among those switching firms only (Column (4)). Estimated fixed effects are available upon request.
$^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A6: Estimation Results: Short-Run Outcomes For Continuing VET

	(1) 1: Reg. Employment	(2) 1: Switch Occ	(3) 1: Specialist Occ.	(4) 1: Switch County	(5) $\triangle$ Firm FE
Treatment	0.104***	-0.248**	0.298***	0.005	0.028
	(0.036)	(0.124)	(0.108)	(0.106)	(0.026)
VET Year 2	-0.429***	-0.558***	0.193	-0.115	0.092**
	(0.050)	(0.134)	(0.125)	(0.124)	(0.037)
VET Year 3	-0.263***	-1.456***	1.152***	-0.524***	0.072***
	(0.053)	(0.118)	(0.112)	(0.105)	(0.027)
VET Year 4	-0.023	-2.001***	1.437***	-0.878***	0.121***
	(0.067)	(0.117)	(0.109)	(0.102)	(0.025)
VET Year 2 $\times$ Treatment	0.178***	0.130	-0.057	0.105	-0.052
	(0.063)	(0.169)	(0.159)	(0.155)	(0.042)
VET Year 3 $\times$ Treatment	0.037	0.384***	-0.543***	0.330***	-0.024
	(0.065)	(0.144)	(0.136)	(0.127)	(0.030)
VET Year 4 $\times$ Treatment	-0.055	0.508***	-0.404***	0.516***	-0.055*
	(0.068)	(0.142)	(0.136)	(0.125)	(0.029)
Female	-0.104***	-0.095	0.258***	0.049	0.003
	(0.031)	(0.059)	(0.061)	(0.054)	(0.012)
Age (at Begin of Half-Year)	0.014**	-0.003	0.028**	0.022*	0.002
	(0.006)	(0.012)	(0.014)	(0.011)	(0.002)
Foreign Citizenship	0.081**	0.168***	-0.079	0.021	-0.010
	(0.033)	(0.062)	(0.064)	(0.057)	(0.014)
School Degree: Below Univ. Entrance	-0.122***	0.083	-0.031	0.025	-0.033***
	(0.029)	(0.056)	(0.060)	(0.052)	(0.012)
School Degree: Univ. Entrance	-0.144***	-0.097	-0.048	-0.059	-0.001
	(0.040)	(0.077)	(0.082)	(0.071)	(0.016)
Training Firm AKM FE in Q2	-0.095**	0.029	0.027	-0.000	-0.201***
	(0.045)	(0.089)	(0.093)	(0.083)	(0.025)
Training Firm AKM FE in Q3	-0.154***	0.066	0.104	0.074	-0.272***
	(0.045)	(0.089)	(0.093)	(0.082)	(0.026)
Training Firm AKM FE in Q4	-0.209***	0.085	0.118	0.058	-0.350***
	(0.048)	(0.094)	(0.098)	(0.086)	(0.027)
Training Firm AKM FE Missing	-0.175***	0.257**	-0.021	0.002	-0.213***
	(0.066)	(0.117)	(0.120)	(0.105)	(0.041)
Constant	-1.734***	1.187***	-0.481	-0.034	0.161**
	(0.156)	(0.445)	(0.414)	(0.381)	(0.069)
Pseudo R Squared	0.191	0.219	0.165	0.086	
R Squared					0.189
Observations	44990	4434	4430	4425	2729

Notes: This table shows the estimation results for probit models for taking up regular employment (Column (1)), for switching occupation (Column (2)), working in a specialist occupation (Column (3)) and workplace county (Column (4)), a and linear model for the change in the new firm's AKM Firm FE relative to the firm where VET was started, among those switching firms only (Column (5)). Estimated fixed effects are available upon request.

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A7: Estimation Results: Short-Run Outcomes for Taking Up Regular Employment

	(1) 1: Longer Gap	(2) Gap Duration	(3) 1: Unemployment	(4) 1: Unobserved
Treatment	-0.144***	-0.795**	-0.000	0.000
	(0.030)	(0.369)	(0.117)	(0.117)
VET Year 2	0.313***	-0.059	0.564***	-0.564***
	(0.037)	(0.570)	(0.171)	(0.171)
VET Year 3	-0.348***	-0.893*	0.175	-0.175
	(0.034)	(0.462)	(0.192)	(0.192)
VET Year 4	-1.986***	0.051	0.271	-0.271
	(0.044)	(0.550)	(0.220)	(0.220)
VET Year 2 × Treatment	-0.140***	1.047	-0.049	0.049
	(0.047)	(0.710)	(0.209)	(0.209)
VET Year 3 × Treatment	-0.036	1.727***	0.197	-0.197
	(0.040)	(0.572)	(0.233)	(0.233)
VET Year 4 × Treatment	0.150***	0.680*	0.319*	-0.319*
	(0.045)	(0.388)	(0.182)	(0.182)
Female	0.040*	-0.346	0.147	-0.147
	(0.023)	(0.247)	(0.098)	(0.098)
Age (at Begin of Half-Year)	-0.045***	0.100*	0.075***	-0.075***
	(0.005)	(0.055)	(0.020)	(0.020)
Foreign Citizenship	-0.093***	0.101	0.268***	-0.268***
	(0.025)	(0.232)	(0.096)	(0.096)
School Degree: Below Univ. Entrance	0.106***	-0.448**	-0.098	0.098
	(0.022)	(0.217)	(0.086)	(0.086)
School Degree: Univ. Entrance	0.076**	0.026	-0.918***	0.918***
	(0.029)	(0.306)	(0.149)	(0.149)
Training Firm AKM FE in Q2	0.169***	0.324	0.088	-0.088
	(0.035)	(0.370)	(0.133)	(0.133)
Training Firm AKM FE in Q3	0.208***	0.343	0.031	-0.031
	(0.036)	(0.361)	(0.135)	(0.135)
Training Firm AKM FE in Q4	0.218***	0.139	-0.312**	0.312**
	(0.037)	(0.375)	(0.150)	(0.150)
Training Firm AKM FE Missing	0.130***	0.366	-0.246	0.246
	(0.048)	(0.489)	(0.193)	(0.193)
Constant	2.033***	3.338**	-1.879***	1.879***
	(0.124)	(1.351)	(0.483)	(0.483)
Pseudo R Squared	0.235		0.170	0.170
R Squared		0.207		
Observations	44990	1773	1736	1736

Notes: This table shows the estimation results for probit models for having a longer gap after (fictional) displacement (Column (1)), a linear model for the duration of this gap, measured in half-years (Column (2)), as well as probit model for being registered unemployed (Column (3)) and being unobserved in administrative data (Column (4)). Estimated fixed effects are available upon request.
$^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A8: Estimation Results: Short-Run Outcomes for Longer Gaps

Variable	Definition and Further Details
Variables cumulatively defined over the whole half year	*Variable definition considers all spells within a half year.*
Earnings from Regular Employment (excl. VET)	Half-Yearly Earnings from Any Employment (incl. Mini-Job) but not from VET; Zero in Case of Non-Employment.
Days in Regular Employment (excl. VET)	Days per Half Year in Any Employment (incl. Mini-Job) but not in VET. Zero in Case of Non-Employment. Value range from 0 up to 184 days depending on first/second half year per year and leap years. Taking integer values only.
Mean Daily Wage from Regular Employment (excl. VET)	*Earnings From Regular Employment (excl. VET)* divided by *Days in Regular Employment (excl. VET)*. Missing in case of zero days in regular employment (excl. VET) in the respective half year.
Earnings From Full-Time Employment	Half-Yearly Earnings from Full-Time Employment Only (VET is excluded). Zero in case of no full-time employment in the respective half year.
Total Earnings	Half-Yearly Earnings from all kinds of Employment, incl. VET.
Days in Registered Unemployment	Days per half year when either unemployment benefits I or II are received. This includes time periods when unemployment benefits are received in addition to employment. Value range from 0 up to 184 days depending on first/second half year per year and leap years. Taking integer values only.
Variables defined once per half year	*In case of multiple employment spells per half year, (1) the spell at March 31 or September 30 is chosen, respectively, and (2) the spell with the highest daily wage. Until $k = -1$, VET information always rules out information from other employment.*
Employed in Helper Occupation	$= 1$ in case of any employment in an occupation with requirement level 1, $= 0$ in case of employment in an occupation with a different requirement level. Missing in case on no employment.
Employed in Specialist Occupation	$= 1$ in case of any employment in an occupation with requirement level 2, $= 0$ in case of employment in an occupation with a different requirement level. Missing in case on no employment.
Employed in (Highly) Complex Occupation	$= 1$ in case of any employment in an occupation with requirement level 3 or 4, $= 0$ in case of employment in an occupation with a different requirement level. Missing in case on no employment.
Employed Outside First VET Occupation	$= 1$ in case of employment in the same KldB (2010) 2-digit occupation as the first VET occupation, $= 0$ in case of employment in a different occupation. Missing in case on no employment.
Employed Outside Last VET Occupation	$= 1$ in case of employment in the same KldB (2010) 2-digit occupation as the last VET occupation, $= 0$ in case of employment in a different occupation. Missing in case on no employment.
Occupational Similarity to First VET Occupation	I follow Gathmann/Schönberg (2010) and the cosine similarity between the current occupation to the first VET occupation given by their task profiles. See Appendix A2.4 for further information.
Holding Vocational Degree	$= 1$ if a vocational degree has been obtained, $= 0$ if no vocational degree has been obtained (yet). This variable is set after imputing information on education according to Fitzenberger/Hillerich-Sigg/Sprietsma (2020). It is therefore non-decreasing over time, but can be missing if no information (so far) on VET graduation is given.
Holding University Degree	$= 1$ if any degree from a university or a university of applied sciences has been obtained, $= 0$ if no university degree has been obtained (yet). This variable is set after imputing information on education according to Fitzenberger/Hillerich-Sigg/Sprietsma (2020). It is therefore non-decreasing over time, but can be missing if no information (so far) on VET graduation is given.
Δ in Firm FE	Uses the estimated AKM Firm FE by Lochner/Wolter/Seth (2024). For the years until 2013, the estimates from 2007-2013 are used. For all later years, the estimates from 2014-2021 are used. Also defined in case of (dual) VET. Can be missing if no AKM FE could be estimated or in case of no employment.
Employed Outside VET-Start County	$= 1$ if the current workplace county is different to the workplace county where the first VET started, $= 0$ if the current workplace county is the same. Missing in case of no employment, or in rare cases no information on workplace location. *County* refers to German *Landkreise* and *kreisfreie Städte*, i.e. rural districts and independent cities, for a total of about 400 administrative units.
Switched Employer	$= 1$ in case of employment at the same employer at time t as in $t - 1$, $= 0$ in case of a different employer. Missing in case of no employment. Unlike most other outcomes, this measure also includes VET.
Employed at First Training Firm	$= 1$ in case of regular employment at the same firm where VET was started, $= 0$ in case of non-employment, employment (or further VET episodes) at another firm. Missing during the first VET episode.
Employed at First/Last Training Firm	$= 1$ in case of regular employment at the same firm where VET was started, *or* the last firm during the first total VET episode, $= 0$ in case of non-employment, employment (or further VET episodes) at another firm. Missing during the first total VET episode.

Table A9: Variable Table

	(1) Earnings	(2) Employment	(3) Wages
Effect at $k = 0$	-30.720**	-1.065***	-2.128***
	(15.223)	(0.364)	(0.554)
Effect at $k = 1$	-22.703	-1.099**	-2.726***
	(25.307)	(0.508)	(0.690)
Effect at $k = 2$	-150.159***	-3.541***	-2.080***
	(33.054)	(0.590)	(0.676)
Effect at $k = 3$	-194.828***	-4.517***	-1.988***
	(38.799)	(0.638)	(0.679)
Effect at $k = 4$	-201.036***	-5.425***	-1.761***
	(46.120)	(0.699)	(0.672)
Effect at $k = 5$	-252.808***	-6.433***	-1.732***
	(50.871)	(0.726)	(0.666)
Effect at $k = 6$	-387.595***	-7.783***	-1.761***
	(54.015)	(0.752)	(0.661)
Effect at $k = 7$	-390.149***	-8.073***	-1.905***
	(56.795)	(0.754)	(0.672)
Effect at $k = 8$	-379.877***	-7.514***	-1.750***
	(61.183)	(0.782)	(0.678)
Effect at $k = 9$	-301.148***	-6.419***	-1.761**
	(64.963)	(0.804)	(0.694)
Effect at $k = 10$	-253.852***	-6.328***	-1.520**
	(67.371)	(0.816)	(0.698)
Effect at $k = 11$	-228.393***	-6.350***	-1.588**
	(72.035)	(0.848)	(0.720)
Effect at $k = 12$	-254.990***	-6.022***	-1.642**
	(75.771)	(0.866)	(0.727)
Effect at $k = 13$	-286.819***	-7.046***	-0.921
	(80.903)	(0.899)	(0.754)
Effect at $k = 14$	-282.736***	-6.813***	-0.868
	(84.631)	(0.924)	(0.764)
Effect at $k = 15$	-272.055***	-6.145***	-1.612**
	(89.955)	(0.973)	(0.796)
Effect at $k = 16$	-283.076***	-5.023***	-2.244***
	(93.878)	(0.995)	(0.807)
Observations	637,808	637,808	325,033
R^2	0.613	0.551	0.743
R^2 Adjusted	0.584	0.517	0.708
Within R^2	0.420	0.396	0.288

Notes: Estimated fixed effects are available upon request. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A10: Estimation Results: Earnings, Employment, and Wages

	(1) 1: Employed in Specialist Occ.	(2) Δ in Firm FE	(3) 1: Holding VET Degree
Effect at $k = 0$	-0.033***	0.005***	-0.001
	(0.012)	(0.001)	(0.001)
Effect at $k = 1$	-0.052***	0.007***	0.008***
	(0.013)	(0.002)	(0.002)
Effect at $k = 2$	-0.042***	0.006***	-0.000
	(0.013)	(0.002)	(0.003)
Effect at $k = 3$	-0.035***	0.005**	-0.010***
	(0.013)	(0.002)	(0.003)
Effect at $k = 4$	-0.032**	0.003	-0.016***
	(0.013)	(0.002)	(0.004)
Effect at $k = 5$	-0.034***	0.004	-0.024***
	(0.013)	(0.002)	(0.004)
Effect at $k = 6$	-0.033***	0.003	-0.026***
	(0.013)	(0.003)	(0.004)
Effect at $k = 7$	-0.034***	0.003	-0.028***
	(0.013)	(0.003)	(0.004)
Effect at $k = 8$	-0.034***	0.003	-0.025***
	(0.013)	(0.003)	(0.004)
Effect at $k = 9$	-0.035***	0.007**	-0.020***
	(0.013)	(0.003)	(0.004)
Effect at $k = 10$	-0.030**	0.007**	-0.018***
	(0.013)	(0.003)	(0.004)
Effect at $k = 11$	-0.027**	0.006**	-0.018***
	(0.013)	(0.003)	(0.004)
Effect at $k = 12$	-0.029**	0.008**	-0.017***
	(0.013)	(0.003)	(0.004)
Effect at $k = 13$	-0.030**	0.010***	-0.012***
	(0.014)	(0.004)	(0.004)
Effect at $k = 14$	-0.029**	0.011***	-0.013***
	(0.014)	(0.004)	(0.004)
Effect at $k = 15$	-0.026*	0.011***	-0.012**
	(0.014)	(0.004)	(0.005)
Effect at $k = 16$	-0.016	0.011***	-0.009*
	(0.014)	(0.004)	(0.005)
Observations	325,033	411,005	637,808
R^2	0.548	0.622	0.688
R^2 Adjusted	0.485	0.579	0.665
Within R^2	0.025	0.022	0.518

Notes: Estimated fixed effects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A11: Estimation Results: Job Quality, Firm Quality, and Training Success

	(1) 1: Employed Outside First VET Occ	(2) Similarity to First VET Occ	(3) 1: Employed Outside VET-Start County
Effect at $k = 0$	0.031***	0.012	0.033***
	(0.007)	(0.010)	(0.007)
Effect at $k = 1$	0.046***	-0.037***	0.150***
	(0.007)	(0.011)	(0.008)
Effect at $k = 2$	0.043***	-0.034***	0.120***
	(0.006)	(0.010)	(0.006)
Effect at $k = 3$	0.044***	-0.037***	0.107***
	(0.005)	(0.010)	(0.006)
Effect at $k = 4$	0.039***	-0.032***	0.089***
	(0.005)	(0.010)	(0.005)
Effect at $k = 5$	0.037***	-0.030***	0.080***
	(0.005)	(0.010)	(0.005)
Effect at $k = 6$	0.033***	-0.028***	0.065***
	(0.005)	(0.010)	(0.005)
Effect at $k = 7$	0.029***	-0.024**	0.064***
	(0.005)	(0.010)	(0.005)
Effect at $k = 8$	0.031***	-0.021**	0.056***
	(0.005)	(0.010)	(0.005)
Effect at $k = 9$	0.029***	-0.016	0.055***
	(0.005)	(0.010)	(0.006)
Effect at $k = 10$	0.024***	-0.012	0.049***
	(0.006)	(0.010)	(0.006)
Effect at $k = 11$	0.025***	-0.011	0.055***
	(0.006)	(0.010)	(0.006)
Effect at $k = 12$	0.023***	-0.010	0.051***
	(0.006)	(0.010)	(0.006)
Effect at $k = 13$	0.021***	-0.010	0.049***
	(0.006)	(0.011)	(0.007)
Effect at $k = 14$	0.024***	-0.008	0.049***
	(0.007)	(0.011)	(0.007)
Effect at $k = 15$	0.017**	-0.003	0.045***
	(0.007)	(0.011)	(0.007)
Effect at $k = 16$	0.017**	-0.005	0.040***
	(0.007)	(0.011)	(0.008)
Observations	357,123	259,427	363,404
R^2	0.664	0.738	0.630
R^2 Adjusted	0.620	0.702	0.582
Within R^2	0.193	0.177	0.180

Notes: Estimated fixed effects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A12: Estimation Results: Occupational and Regional Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
k	Δ_k	C_k	W_k^0	W_k^1	C_k/Δ_k	W_k^0/Δ_k	W_k^1/Δ_k	d_{C_k}	$d_{W_k^0}$	$d_{W_k^1}$	s_{C_k}	$s_{W_k^0}$	$s_{W_k^1}$
0	-30.720	-18.099	4.686	-17.307	0.589	-0.153	0.563	548.735	21.678	-22.080	-0.033	0.216	0.784
1	-22.703	-26.006	-13.259	16.563	1.146	0.584	-0.730	788.461	-61.336	21.131	-0.033	0.216	0.784
2	-150.159	-52.074	-4.654	-93.430	0.347	0.031	0.622	1578.775	-21.531	-119.198	-0.033	0.216	0.784
3	-192.911	-61.750	-22.276	-108.885	0.320	0.115	0.564	1889.708	-103.137	-138.882	-0.033	0.216	0.784
4	-198.023	-93.686	19.968	-124.304	0.473	-0.101	0.628	2867.241	92.311	-158.614	-0.033	0.216	0.784
5	-258.097	-102.483	8.988	-164.602	0.397	-0.035	0.638	3199.877	41.722	-209.799	-0.032	0.215	0.785
6	-389.725	-125.341	-114.975	-149.409	0.322	0.295	0.383	3955.878	-532.574	-190.544	-0.032	0.216	0.784
7	-399.186	-121.304	-150.641	-127.241	0.304	0.377	0.319	3989.116	-705.512	-161.785	-0.030	0.214	0.786
8	-390.990	-90.297	-120.613	-180.079	0.231	0.308	0.461	3816.167	-582.185	-227.136	-0.024	0.207	0.793
9	-303.555	-68.672	-112.270	-122.612	0.226	0.370	0.404	3467.952	-566.963	-152.887	-0.020	0.198	0.802
10	-257.267	-65.009	-119.359	-72.899	0.253	0.464	0.283	3270.936	-602.037	-90.925	-0.020	0.198	0.802
11	-220.049	-52.663	-126.391	-40.994	0.239	0.574	0.186	3130.084	-664.133	-50.629	-0.017	0.190	0.810
12	-231.661	-43.633	-117.816	-70.211	0.188	0.509	0.303	2967.705	-622.744	-86.593	-0.015	0.189	0.811
13	-252.011	-37.985	-134.593	-79.434	0.151	0.534	0.315	2603.669	-731.738	-97.338	-0.015	0.184	0.816
14	-268.222	-35.946	-138.161	-94.115	0.134	0.515	0.351	2525.999	-749.289	-115.392	-0.014	0.184	0.816
15	-262.800	-31.411	-166.325	-65.064	0.120	0.633	0.248	2250.410	-907.533	-79.665	-0.014	0.183	0.817
16	-271.585	-30.068	-155.312	-86.205	0.111	0.572	0.317	2137.478	-840.681	-105.740	-0.014	0.185	0.815

Notes: Column (1) contains the event study difference-in-differences estimate for the effect VET displacement on earnings when excluding the set of fixed effects. Columns (2) to (4) capture the absolute contribution of the three components C_k, W_k^0 and W_k^1 to the estimated earnings effect Δ_k, respectively. In Columns (5) to (7), these are divided by Δ_k in order to assess their relative importance over the event time according to Panel (b) of Figure 5. Columns (8) to (13) contain the single factors of each component: Columns (8) to (10) show the earnings differences d_{C_k}, $d_{W_k^0}$ and $d_{W_k^1}$, i.e. between control graduates and dropouts, non-graduated treated and controls, and graduated treated and controls, respectively. These correspond to Panels (c) and (a) of Figure A11, respectively. Columns (11) to (13) show the weights of each channels s_{C_k}, $s_{W_k^0}$ and $s_{W_k^1}$, i.e. the graduation surplus among treated relative to controls, the share of dropouts among treated, and the share graduates among treated, which is also shown graphically in Panel (d) of Figure A11. The differences across event time are due to sample imbalance. See also Appendix A2.7 for a detailed description.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A13: Decomposition Results for Earnings and Training Success

	(1) Earnings							
	(a) Year 1		(b) Year 2		(c) Year 3		(d) Year 4	
Effect at $k = 0$	-33.340**	(10.693)	22.021	(12.162)	94.208**	(36.177)	-528.530***	(124.779)
Effect at $k = 1$	-15.027	(18.755)	65.975*	(28.697)	160.653*	(79.870)	-838.799***	(137.042)
Effect at $k = 2$	-1.481	(25.911)	63.264	(51.509)	-458.375***	(100.021)	-697.095***	(147.922)
Effect at $k = 3$	7.290	(34.966)	-67.800	(72.861)	-532.429***	(102.463)	-546.114**	(176.849)
Effect at $k = 4$	108.122*	(52.309)	-381.590***	(90.574)	-386.295***	(110.024)	-211.439	(192.991)
Effect at $k = 5$	62.432	(69.475)	-543.553***	(92.407)	-351.967**	(120.346)	-8.737	(208.723)
Effect at $k = 6$	-369.538*	(83.732)	-550.390***	(94.255)	-140.913	(118.528)	-211.143	(213.306)
Effect at $k = 7$	-473.254*	(85.841)	-412.886***	(100.383)	-120.693	(125.260)	-263.240	(238.053)
Effect at $k = 8$	-396.583*	(93.056)	-435.522***	(107.206)	-123.374	(135.233)	-460.732	(259.349)
Effect at $k = 9$	-329.956*	(99.189)	-368.229**	(113.773)	-99.055	(143.233)	-3.499	(280.402)
Effect at $k = 10$	-286.102*	(102.071)	-347.842**	(118.148)	-8.535	(151.272)	75.624	(292.550)
Effect at $k = 11$	-226.672**	(110.197)	-355.596**	(126.256)	-1.076	(159.492)	145.994	(311.867)
Effect at $k = 12$	-308.289*	(114.722)	-275.568*	(134.651)	-156.426	(169.280)	344.070	(319.385)
Effect at $k = 13$	-287.145*	(123.045)	-158.012	(143.097)	-485.318**	(180.115)	93.559	(353.992)
Effect at $k = 14$	-312.376*	(129.191)	-125.909	(149.095)	-378.510*	(189.277)	-222.949	(362.100)
Effect at $k = 15$	-224.560	(137.792)	-286.204	(157.703)	-236.354	(203.526)	-217.131	(374.037)
Effect at $k = 16$	-340.739*	(142.504)	-274.961	(166.540)	5.427	(211.484)	-361.897	(391.902)
Observations	637808							
R^2	0.694							
R^2 Adjusted	0.662							
Within R^2	0.474							

Notes: Estimated fixed effects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A14: Estimation Results: Earnings by Displacement Year

	(1) Earnings			
	(a) Male		(b) Female	
Effect at $k = 0$	-42.980	(22.257)	-18.409	(20.765)
Effect at $k = 1$	-45.411	(37.687)	0.102	(33.769)
Effect at $k = 2$	-158.791**	(49.255)	-141.490**	(44.074)
Effect at $k = 3$	-219.411***	(58.586)	-170.272***	(50.894)
Effect at $k = 4$	-197.318**	(69.888)	-204.699***	(60.228)
Effect at $k = 5$	-313.04***	(77.107)	-192.697**	(66.402)
Effect at $k = 6$	-487.807***	(80.646)	-287.228***	(71.860)
Effect at $k = 7$	-466.481***	(84.503)	-313.367***	(75.836)
Effect at $k = 8$	-409.441***	(92.863)	-350.221***	(79.905)
Effect at $k = 9$	-255.768**	(98.969)	-343.887***	(84.538)
Effect at $k = 10$	-269.200**	(102.300)	-237.946**	(88.015)
Effect at $k = 11$	-281.830**	(108.947)	-175.175	(94.484)
Effect at $k = 12$	-358.721**	(113.164)	-151.544	(100.879)
Effect at $k = 13$	-336.043**	(120.821)	-237.221*	(107.557)
Effect at $k = 14$	-321.508*	(126.638)	-243.803*	(112.322)
Effect at $k = 15$	-313.104*	(132.596)	-230.673	(121.450)
Effect at $k = 16$	-249.077	(135.995)	-320.058*	(129.288)
Observations	637808			
R^2	0.615			
R^2 Adjusted	0.586			
Within R^2	0.423			

Notes: Estimated fixed effects are available upon request. $^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A15: Estimation Results: Earnings by Gender

	(1) Earnings			
	(a) German Citizenship		(b) Foreign Citizenship	
Effect at $k = 0$	-24.417	(16.458)	-66.567	(40.036)
Effect at $k = 1$	1.321	(27.484)	-159.332*	(64.857)
Effect at $k = 2$	-139.271***	(35.811)	-212.078*	(85.935)
Effect at $k = 3$	-182.947***	(42.008)	-262.291**	(101.214)
Effect at $k = 4$	-178.191***	(50.013)	-331.205**	(119.263)
Effect at $k = 5$	-234.486***	(55.258)	-357.288**	(130.224)
Effect at $k = 6$	-399.349***	(58.663)	-317.880*	(138.438)
Effect at $k = 7$	-392.696***	(61.862)	-371.718**	(142.841)
Effect at $k = 8$	-360.886***	(66.674)	-488.737**	(153.392)
Effect at $k = 9$	-284.210***	(70.686)	-399.414*	(164.329)
Effect at $k = 10$	-257.040***	(73.261)	-229.147	(170.998)
Effect at $k = 11$	-236.919**	(78.042)	-162.902	(187.054)
Effect at $k = 12$	-271.111***	(82.009)	-136.239	(197.888)
Effect at $k = 13$	-279.527**	(87.097)	-323.874	(219.016)
Effect at $k = 14$	-263.042**	(91.217)	-409.212	(226.972)
Effect at $k = 15$	-280.347**	(96.270)	-190.167	(254.137)
Effect at $k = 16$	-273.315**	(100.367)	-339.380	(266.633)
Observations	637808			
R^2	0.613			
R^2 Adjusted	0.584			
Within R^2	0.420			

Notes: Estimated fixed effects are available upon request. $^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A16: Estimation Results: Earnings by Citizenship

	(1) Earnings			
	(a) West Germany		(b) East Germany	
Effect at $k = 0$	-27.528	(16.392)	-51.521	(41.019)
Effect at $k = 1$	-42.436	(27.611)	105.881	(61.698)
Effect at $k = 2$	-165.026	(35.983)	-53.278	(82.121)
Effect at $k = 3$	-215.089***	(42.252)	-61.981	(95.868)
Effect at $k = 4$	-255.503***	(49.965)	157.737	(118.647)
Effect at $k = 5$	-306.282***	(55.118)	97.363	(130.906)
Effect at $k = 6$	-437.337***	(58.397)	-61.757	(141.310)
Effect at $k = 7$	-449.275***	(61.344)	-5.506	(149.689)
Effect at $k = 8$	-419.934***	(66.068)	-119.159	(161.782)
Effect at $k = 9$	-324.532***	(70.523)	-145.414	(166.086)
Effect at $k = 10$	-256.723***	(73.330)	-223.183	(169.424)
Effect at $k = 11$	-261.713***	(78.049)	-10.749	(186.765)
Effect at $k = 12$	-297.221***	(82.161)	17.930	(195.361)
Effect at $k = 13$	-279.293**	(88.582)	-321.190	(194.278)
Effect at $k = 14$	-283.585**	(92.594)	-267.191	(204.045)
Effect at $k = 15$	-273.374**	(97.184)	-267.751	(236.056)
Effect at $k = 16$	-293.307**	(101.228)	-217.593	(249.828)
Observations	637808			
R^2	0.613			
R^2 Adjusted	0.586			
Within R^2	0.420			

Notes: Estimated fixed effects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A17: Estimation Results: Earnings by Region

	(1) Earnings					
	(a) No / Unknown		(b) Below Univ. Entrance		(c) Univ. Entrance	
Effect at $k = 0$	-26.299	(27.541)	-19.781	(19.387)	-70.006	(42.230)
Effect at $k = 1$	9.141	(49.060)	-41.317	(30.479)	-1.647	(74.669)
Effect at $k = 2$	-144.596*	(64.789)	-157.630***	(40.622)	-133.311	(93.541)
Effect at $k = 3$	-169.768*	(77.179)	-160.446***	(47.249)	-330.416**	(110.390)
Effect at $k = 4$	-241.170*	(94.003)	-212.148***	(56.834)	-119.797	(126.158)
Effect at $k = 5$	-341.202***	(103.131)	-271.120***	(62.252)	-92.074	(141.822)
Effect at $k = 6$	-528.027***	(112.503)	-393.619***	(68.117)	-203.111	(138.404)
Effect at $k = 7$	-592.655***	(117.640)	-419.857***	(72.286)	-57.700	(142.972)
Effect at $k = 8$	-552.218***	(124.361)	-409.701***	(77.268)	-59.314	(161.636)
Effect at $k = 9$	-512.609***	(131.001)	-275.069***	(81.159)	-113.175	(179.737)
Effect at $k = 10$	-498.551***	(134.691)	-209.537**	(84.303)	-85.015	(187.557)
Effect at $k = 11$	-518.399***	(144.024)	-215.914**	(90.160)	118.029	(201.062)
Effect at $k = 12$	-783.361***	(152.610)	-156.758	(94.048)	102.018	(214.440)
Effect at $k = 13$	-821.801***	(163.756)	-155.912	(100.545)	-40.653	(228.814)
Effect at $k = 14$	-883.660***	(172.720)	-179.363	(105.166)	170.118	(238.117)
Effect at $k = 15$	-1001.663***	(188.770)	-141.001	(110.415)	280.339	(258.228)
Effect at $k = 16$	-960.796***	(199.070)	-144.372	(114.143)	158.710	(274.733)
Observations	637808					
R^2	0.614					
R^2 Adjusted	0.581					
Within R^2	0.421					

Notes: Estimated fixed effects are available upon request.
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A18: Estimation Results: Earnings by School Degree

	Share Among Sample	No/Unknown	Below Univ. Entrance	Univ. Entrance
Year 1	35.9	24.9	56.8	18.3
Year 2	31.8	21.9	58.3	19.7
Year 3	23.3	19.3	59.8	20.9
Year 4	9.0	17.9	68.8	13.2

Notes: Descriptive results on a cross-sectional level (all individuals at the time of displacement). All values are shown as percentages.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A19: Educational Composition by Displacement Year

	(1) Earnings					
	(a) No / Unknown		(b) Below Univ. Entrance		(c) Univ. Entrance	
Effect at $k = 0$	-26.299	(27.541)	-19.781	(19.387)	-70.006	(42.230)
Effect at $k = 1$	9.141	(49.060)	-41.317	(30.479)	-1.647	(74.669)
Effect at $k = 2$	-144.596*	(64.789)	-157.630***	(40.622)	-133.311	(93.541)
Effect at $k = 3$	-169.768*	(77.179)	-160.446***	(47.249)	-330.416**	(110.390)
Effect at $k = 4$	-241.170*	(94.003)	-212.148***	(56.834)	-119.797	(126.158)
Effect at $k = 5$	-341.202***	(103.131)	-271.120***	(62.252)	-92.074	(141.822)
Effect at $k = 6$	-528.027***	(112.503)	-393.619***	(68.117)	-203.111	(138.404)
Effect at $k = 7$	-592.655***	(117.640)	-419.857***	(72.286)	-57.700	(142.972)
Effect at $k = 8$	-552.218***	(124.361)	-409.701***	(77.268)	-59.314	(161.636)
Effect at $k = 9$	-512.609***	(131.001)	-275.069***	(81.159)	-113.175	(179.737)
Effect at $k = 10$	-498.551***	(134.691)	-209.537**	(84.303)	-85.015	(187.557)
Effect at $k = 11$	-518.399***	(144.024)	-215.914**	(90.160)	118.029	(201.062)
Effect at $k = 12$	-783.361***	(152.610)	-156.758	(94.048)	102.018	(214.440)
Effect at $k = 13$	-821.801***	(163.756)	-155.912	(100.545)	-40.653	(228.814)
Effect at $k = 14$	-883.660***	(172.720)	-179.363	(105.166)	170.118	(238.117)
Effect at $k = 15$	-1001.663***	(188.770)	-141.001	(110.415)	280.339	(258.228)
Effect at $k = 16$	-960.796***	(199.070)	-144.372	(114.143)	158.710	(274.733)
Observations	637808					
R^2	0.614					
R^2 Adjusted	0.581					
Within R^2	0.421					

Notes: Estimated fixed effects are available upon request.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A20: Estimation Results: Earnings by School Degree

	(1) Earnings							
	(a) $1-9$ Employees		(b) $10-49$ Employees		(c) $50-249$ Employees		(d) ≥ 250 Employees	
Effect at $k=0$	79.007	(41.198)	-29.097	(27.715)	33.489	(42.869)	-110.549	(95.538)
Effect at $k=1$	185.386**	(67.275)	16.735	(45.843)	50.824	(73.283)	57.799	(168.654)
Effect at $k=2$	198.816*	(90.669)	-105.332	(60.740)	43.365	(94.224)	-225.174	(201.286)
Effect at $k=3$	219.043*	(101.398)	-124.053	(68.144)	62.449	(105.740)	-321.695	(224.819)
Effect at $k=4$	121.327	(114.320)	-152.983*	(76.419)	315.275**	(117.263)	-75.922	(248.569)
Effect at $k=5$	2.851	(119.455)	-211.624**	(80.763)	295.980*	(124.521)	349.175	(261.043)
Effect at $k=6$	-169.794	(123.646)	-357.433***	(83.017)	53.579	(118.069)	260.538	(237.584)
Effect at $k=7$	-95.351	(129.644)	-383.391***	(86.305)	66.772	(119.477)	180.597	(240.985)
Effect at $k=8$	-121.717	(136.825)	-385.972***	(91.081)	128.772	(131.446)	552.069	(289.219)
Effect at $k=9$	-91.667	(144.616)	-253.251**	(96.246)	292.759*	(142.697)	333.402	(306.033)
Effect at $k=10$	-139.953	(148.457)	-213.261*	(99.670)	377.943*	(147.155)	386.379	(314.097)
Effect at $k=11$	-165.366	(160.324)	-97.994	(108.256)	306.073*	(155.750)	289.015	(331.907)
Effect at $k=12$	-277.470	(166.998)	-150.506	(113.133)	339.476*	(165.075)	381.141	(339.517)
Effect at $k=13$	-257.887	(179.554)	-201.352	(121.137)	314.478	(178.042)	-222.951	(356.090)
Effect at $k=14$	-237.703	(188.587)	-247.339	(128.241)	518.872**	(185.460)	-430.304	(363.143)
Effect at $k=15$	-241.058	(200.769)	-130.596	(138.645)	158.854	(195.122)	485.168	(395.355)
Effect at $k=16$	-257.855	(211.227)	-120.281	(144.589)	210.985	(203.009)	4.674	(408.574)
Observations	637808							
R^2	0.615							
R^2 Adjusted	0.586							
Within R^2	0.422							

Notes: Estimated fixed effects are available upon request. $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A21: Estimation Results: Earnings by Firm Size

	Earnings
Coefficient at $k=0$	2.520
	(17.641)
Coefficient at $k=1$	50.675*
	(28.301)
Coefficient at $k=2$	-106.863***
	(35.953)
Coefficient at $k=3$	-148.590***
	(41.847)
Coefficient at $k=4$	-239.324***
	(49.179)
Coefficient at $k=5$	-322.679***
	(54.238)
Coefficient at $k=6$	-497.381***
	(58.421)
Coefficient at $k=7$	-468.811***
	(62.128)
Coefficient at $k=8$	-430.863***
	(66.074)
Coefficient at $k=9$	-340.547***
	(70.195)
Coefficient at $k=10$	-302.900***
	(72.903)
Coefficient at $k=11$	-320.808***
	(77.925)
Coefficient at $k=12$	-396.416***
	(81.684)
Coefficient at $k=13$	-418.286***
	(87.758)
Coefficient at $k=14$	-431.157***
	(91.792)
Coefficient at $k=15$	-394.520***
	(98.003)
Coefficient at $k=16$	-370.558***
	(102.454)
Observations	523,218
R^2	0.610
Adjusted R^2	0.580
Within R^2	0.415

Notes: Estimated fixed effects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A22: Estimation Results: Earnings When Excluding Mild Closures

	(1) Main	(2) No Person FE	(3) No FE	(4) Occ x t + Reg x t FE	(5) Extended
Effect at $k = 0$	-30.720**	-30.720**	-30.720**	-26.764	-75.701***
	(15.223)	(15.223)	(15.235)	(17.911)	(23.069)
Effect at $k = 1$	-22.703	-22.703	-22.703	-15.631	-34.067
	(25.307)	(25.309)	(25.253)	(28.301)	(31.916)
Effect at $k = 2$	-150.159***	-150.159***	-150.159***	-147.054***	-157.292***
	(33.054)	(33.058)	(33.042)	(36.248)	(38.700)
Effect at $k = 3$	-194.828***	-192.517***	-192.517***	-188.358***	-202.178***
	(38.799)	(38.926)	(38.941)	(42.088)	(43.839)
Effect at $k = 4$	-201.036***	-197.562***	-197.562***	-213.604***	-192.159***
	(46.120)	(46.225)	(46.243)	(49.059)	(49.725)
Effect at $k = 5$	-252.808***	-256.674***	-256.674***	-280.225***	-274.608***
	(50.871)	(51.217)	(51.274)	(53.508)	(53.372)
Effect at $k = 6$	-387.595***	-387.925***	-387.925***	-394.165***	-431.766***
	(54.015)	(54.315)	(54.394)	(56.677)	(54.246)
Effect at $k = 7$	-390.149***	-395.777***	-395.777***	-395.680***	-411.723***
	(56.795)	(57.655)	(57.787)	(59.560)	(56.563)
Effect at $k = 8$	-379.877***	-387.433***	-387.433***	-376.069***	-390.145***
	(61.183)	(62.520)	(62.626)	(63.668)	(59.632)
Effect at $k = 9$	-301.148***	-301.301***	-301.301***	-302.099***	-292.759***
	(64.963)	(67.682)	(67.806)	(67.392)	(63.159)
Effect at $k = 10$	-253.852***	-255.559***	-255.559***	-264.624***	-283.954***
	(67.371)	(69.982)	(70.113)	(69.865)	(64.427)
Effect at $k = 11$	-228.393***	-218.751***	-218.751***	-229.036***	-199.462***
	(72.035)	(76.195)	(76.344)	(74.390)	(68.401)
Effect at $k = 12$	-254.990***	-229.134***	-229.134***	-263.657***	-237.562***
	(75.771)	(79.853)	(79.960)	(78.014)	(71.137)
Effect at $k = 13$	-286.819***	-252.545***	-252.545***	-280.358***	-222.185***
	(80.903)	(86.432)	(86.534)	(82.777)	(76.235)
Effect at $k = 14$	-282.736***	-268.296***	-268.296***	-271.432***	-276.192***
	(84.631)	(90.223)	(90.320)	(86.480)	(78.706)
Effect at $k = 15$	-272.055***	-267.161***	-267.161***	-253.900***	-238.864***
	(89.955)	(97.273)	(97.474)	(91.731)	(84.000)
Effect at $k = 16$	-283.076***	-275.396***	-275.396***	-276.319***	-274.076***
	(93.878)	(100.546)	(100.768)	(96.535)	(86.835)
Observations	637,808	637,808	637,808	637,493	571,770
R^2	0.613	0.314	0.178	0.634	0.684
Adjusted R^2	0.584	0.314	0.178	0.598	0.657
Within R^2	0.420			0.069	0.531

Notes: Estimated fixed effects are available upon request. $^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A23: Estimation Results: Earnings for Alternative Event Study Specifications

	(1) Main	(2) 8.2.a)	(3) 8.2.b)	(4) 8.2.c)	(5) 8.3.a)	(6) 8.3.b)	(7) 8.3.c)	(8) 8.3.d)
Effect at $k = 0$	-30.720**	-38.355**	-23.455	-225.244***	-50.931***	-34.094**	-38.069**	-71.694***
	(15.223)	(15.731)	(16.017)	(11.681)	(15.250)	(16.098)	(16.491)	(14.948)
Effect at $k = 1$	-22.703	-18.958	-12.728	-185.944***	-44.692*	-39.641	-24.601	-48.828**
	(25.307)	(25.446)	(26.528)	(26.321)	(25.059)	(27.338)	(27.638)	(23.911)
Effect at $k = 2$	-150.159***	-147.712***	-121.443***	-219.114***	-167.398***	-136.688***	-126.633***	-116.949***
	(33.054)	(32.724)	(34.627)	(36.466)	(32.589)	(35.667)	(36.701)	(31.515)
Effect at $k = 3$	-194.828***	-184.816***	-172.998***	-219.652***	-209.584***	-168.325***	-175.952***	-175.016***
	(38.799)	(37.921)	(40.426)	(42.968)	(38.254)	(41.805)	(43.212)	(36.626)
Effect at $k = 4$	-201.036***	-199.328***	-167.896***	-129.941**	-220.592***	-171.304***	-200.168***	-245.939***
	(46.120)	(44.186)	(48.356)	(51.689)	(45.389)	(49.191)	(50.199)	(43.450)
Effect at $k = 5$	-252.808***	-249.243***	-230.862***	-161.778***	-255.501***	-210.690***	-237.896***	-293.188***
	(50.871)	(48.368)	(53.611)	(57.141)	(50.147)	(53.952)	(56.078)	(47.920)
Effect at $k = 6$	-387.595***	-366.230***	-326.610***	-196.699***	-389.358***	-349.620***	-368.689***	-436.165***
	(54.015)	(51.241)	(56.743)	(60.198)	(53.098)	(56.538)	(58.036)	(51.385)
Effect at $k = 7$	-390.149***	-368.327***	-337.966***	-214.686***	-381.791***	-324.835***	-368.365***	-473.509***
	(56.795)	(53.871)	(59.620)	(62.562)	(55.920)	(59.653)	(60.695)	(54.198)
Effect at $k = 8$	-379.877***	-379.389***	-323.086***	-221.613***	-375.076***	-298.726***	-352.022***	-435.389***
	(61.183)	(58.112)	(64.345)	(68.028)	(60.287)	(63.771)	(65.124)	(58.380)
Effect at $k = 9$	-301.148***	-313.721***	-268.817***	-176.270**	-311.461***	-224.449***	-310.053***	-349.832***
	(64.963)	(61.716)	(68.334)	(72.285)	(63.866)	(67.585)	(68.666)	(62.119)
Effect at $k = 10$	-253.852***	-256.991***	-221.178***	-185.829**	-280.640***	-184.216***	-295.410***	-345.217***
	(67.371)	(64.134)	(70.784)	(75.043)	(66.286)	(69.982)	(70.873)	(64.191)
Effect at $k = 11$	-228.393***	-227.330***	-198.708***	-169.464**	-258.184***	-160.733**	-203.374***	-320.898***
	(72.035)	(68.468)	(75.914)	(80.989)	(71.118)	(74.732)	(75.710)	(69.358)
Effect at $k = 12$	-254.990***	-227.361***	-202.057**	-219.629**	-289.349***	-209.989***	-248.734***	-395.280***
	(75.771)	(71.985)	(79.934)	(85.451)	(74.563)	(78.431)	(78.523)	(72.289)
Effect at $k = 13$	-286.819***	-257.502***	-235.587***	-225.861**	-301.171***	-238.984***	-238.798***	-372.947***
	(80.903)	(76.927)	(85.736)	(90.939)	(79.792)	(83.903)	(84.768)	(76.528)
Effect at $k = 14$	-282.736***	-225.758***	-240.724***	-217.797**	-293.912***	-221.008**	-211.097**	-326.669***
	(84.631)	(80.707)	(89.586)	(95.036)	(83.434)	(87.665)	(88.865)	(80.135)
Effect at $k = 15$	-272.055***	-227.964***	-234.256**	-225.018**	-292.134***	-202.792**	-199.598**	-353.576***
	(89.955)	(86.031)	(95.290)	(102.071)	(88.345)	(93.272)	(94.483)	(84.916)
Effect at $k = 16$	-283.076***	-237.923***	-250.269**	-237.401**	-295.138***	-202.579**	-204.005**	-359.889***
	(93.878)	(89.620)	(99.498)	(106.181)	(92.018)	(96.868)	(98.818)	(88.539)
Observations	637,808	706,616	572,074	501,438	661,818	637,808	596,408	710,904
R^2	0.613	0.611	0.613	0.620	0.611	0.613	0.614	0.611
Adjusted R^2	0.584	0.582	0.583	0.591	0.582	0.584	0.584	0.582
Within R^2	0.420	0.416	0.421	0.436	0.417	0.420	0.423	0.417

Notes: Estimated fixed effects are available upon request.$^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A24: Estimation Results: Earnings for Alternative Treatment Definitions and Matching Designs

	(1) Main	(2) $n:1$-Matching	(3) IPW	(4) IPW, exl. top 0.5%
Effect at $k=0$	-30.720**	-49.668	15.755	10.633
	(15.223)	(38.696)	(16.516)	(16.518)
Effect at $k=1$	-22.703	-54.423	-16.449	-4.073
	(25.307)	(39.553)	(24.991)	(24.992)
Effect at $k=2$	-150.159***	-166.408***	-107.223***	-94.871***
	(33.054)	(40.929)	(32.618)	(32.620)
Effect at $k=3$	-194.828***	-257.768***	-141.630***	-122.374***
	(38.799)	(41.902)	(36.765)	(36.767)
Effect at $k=4$	-201.036***	-323.240***	-179.383***	-162.624***
	(46.120)	(42.660)	(40.842)	(40.843)
Effect at $k=5$	-252.808***	-392.475***	-278.829***	-239.755***
	(50.871)	(43.002)	(43.539)	(43.541)
Effect at $k=6$	-387.595***	-524.756***	-666.741***	-607.967***
	(54.015)	(41.591)	(43.594)	(43.596)
Effect at $k=7$	-390.149***	-494.494***	-630.457***	-574.898***
	(56.795)	(41.921)	(46.778)	(46.780)
Effect at $k=8$	-379.877***	-491.466***	-467.209***	-420.761***
	(61.183)	(43.206)	(49.276)	(49.278)
Effect at $k=9$	-301.148***	-412.313***	-521.339***	-444.392***
	(64.963)	(44.432)	(53.728)	(53.731)
Effect at $k=10$	-253.852***	-383.842***	-497.112***	-418.525***
	(67.371)	(45.229)	(55.850)	(55.852)
Effect at $k=11$	-228.393***	-350.202***	-307.942***	-272.912***
	(72.035)	(48.095)	(61.613)	(61.615)
Effect at $k=12$	-254.990***	-405.719***	-381.496***	-344.559***
	(75.771)	(50.308)	(63.859)	(63.861)
Effect at $k=13$	-286.819***	-401.466***	-364.800***	-324.573***
	(80.903)	(54.325)	(69.470)	(69.472)
Effect at $k=14$	-282.736***	-374.524***	-341.305***	-299.812***
	(84.631)	(56.745)	(72.410)	(72.413)
Effect at $k=15$	-272.055***	-338.932***	-336.201***	-295.140***
	(89.955)	(62.338)	(79.066)	(79.069)
Effect at $k=16$	-283.076***	-335.390***	-356.451***	-315.841***
	(93.878)	(66.319)	(81.941)	(81.944)
Observations	637,808	32569443	265355111	266282316
R^2	0.613	0.610	0.597	0.593
Adjusted R^2	0.584	0.592	0.569	0.5565
Within R^2	0.420	0.414	0.404	0.403

Notes: Estimated fixed affects are available upon request. $^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$.
Sources: IEB V17.01.00, BHP 7523-V1. Own calculation.

Table A25: Estimation Results: Earnings for Alternative Weighting Strategies

A2 Appendix A.2 - Additional Material

A2.1 Data Preparation

This section provides a detailed description of the data preparation process. The initial data set includes all individuals with at least one IEB spell classified as dual VET[178] between 2008 and 2018. The sample is then restricted in several steps.

Before dropping any individuals, all VET spells that are likely to correspond to internships are relabeled. VET spells are classified as internships if they do not start between August and December or, if they do start within this period, if their duration is shorter than fifteen days.[179] These spells account for 1.0% of all VET spells.

First, an age restriction is applied to retain only plausible labor market entrants. I define the relevant age range as 14 to 25 years. Individuals who are older at the beginning of their first VET spell, or who are younger in very rare cases likely due to misreporting, are excluded. This restriction affects 6.3% of individuals at this stage of the sample construction.

Second, individuals with missing VET occupation codes or with 2-digit KldB 2010 occupational codes that are either not officially defined or do not correspond to occupations offering VET programs are excluded.[180] These cases likely result from misreporting and cannot be used meaningfully in the subsequent analysis. This restriction removes 16.3% of individuals.

Third, the occupational code includes information on the requirement level. For dual VET, the requirement level is 2. Requirement level 1 may correspond to training programs designed for individuals with disabilities, which are not the focus of this study. Requirement levels 3 and 4 may capture internal firm training programs for VET or university graduates rather than dual VET itself. I therefore exclude all individuals with requirement levels 1, 3, or 4. This restriction removes 10.5% of individuals.

Fourth, as the data were ordered starting on 1 January 2006, the sample includes some individuals with VET spells prior to the official start of the 2008/09 apprenticeship cohort. These individuals are therefore excluded, which removes 24.0% of all individuals with any VET spells. While it cannot be completely ruled out that some remaining individuals have VET experience prior to 2006, the age restriction makes this unlikely.

[178]These are employment spells with employment states 120, 121, 122, 141, and 144.

[179]More precisely, this applies if the resulting VET episode lasts no longer than 15 days.

[180]The occupational categories 84 and 1 do not include any VET programs (see BIBB, 2024b). In addition, any code other than 11, 12, 21, 22, 23, 24, 25, 26, 27, 28, 29, 31, 32, 33, 34, 41, 42, 43, 51, 52, 53, 54, 61, 62, 63, 71, 72, 73, 81, 82, 83, 91, 92, 93, and 94 is not a valid 2-digit occupational code (see BA, 2010).

Fifth, the sample of apprentices is restricted to individuals without a professional degree. This includes degrees from dual VET, school-based VET, as well as diplomas from universities and universities of applied sciences. Individuals holding such a degree according to the IEB are therefore excluded. This restriction removes 3.3% of individuals at this stage of the data preparation.

Sixth, missing information on VET wages cannot be handled meaningfully in either the matching procedure or the inverse probability weighting approach. I therefore exclude a small group of individuals with missing VET wage information throughout their first VET episode. This group amounts to less than 0.1% of all individuals.

Seventh, a VET program within a given training occupation and firm should plausibly not exceed four years. In some cases, however, training firms may retain former apprentices as regular employees without properly updating the employment status in the IEB, such that regular employment spells are still recorded as apprenticeship spells. Following Möller/Umkehrer (2015), I infer likely VET graduations by identifying cases in which wage increases during VET exceed the 95th percentile of the wage change distribution between two VET spells. After applying this correction, a small share of apprentices still exhibit VET durations longer than four years (1.4%). These individuals are excluded from the subsequent analysis.

Eighth, after excluding likely internship spells at the beginning of the data preparation, some individuals remain who do not have any subsequent VET spells, or whose further VET spells do not survive the previous cleaning steps. These individuals may stem from internships within preparatory training programs, school-based VET, or university studies. As this small group, accounting for 4% of individuals, does not substantively belong to the population of apprentices studied here, it is excluded from the sample.

Ninth, in order to classify individuals as treated and to use them as potential controls, information on the training establishment from the BHP is required. For a small share of 0.1% of the remaining apprentices, this information is missing. These individuals are therefore excluded in the final step of the data preparation.

After applying all sample restrictions, the final analysis sample consists of apprentices with valid and plausibly defined VET episodes, complete occupational information, and observed training establishments.

A2.2 Test on Equivalence of Two Distributions

Let Y denote a continuous outcome variable and let $F_0(Y;\cdot)$ and $F_1(Y;\cdot)$ be the cumulative distribution functions in the control and the treatment group, respectively. The classical two-sample Kolmogorov–Smirnov (KS) statistic is defined as the supremum of the absolute difference between $F_0(Y;\cdot)$ and $F_1(Y;\cdot)$, estimated by their empirical counterparts:

$$D_n := \sup_y \left| \widehat{F}_0(y) - \widehat{F}_1(y) \right|, \tag{A1}$$

where n represents the number of observations in the treatment and the control group (they have same size this example). In an equivalence-testing framework, the researcher specifies an equivalence margin $\delta > 0$ that represents the largest distributional deviation which is still considered practically negligible.

In the traditional Kolmogorov Smirnov test, the null hypothesis is equality of both cumulative distribution functions. However, if the concern is about whether they are *not equal*, such tests could not allow for conclusions with high statistical significance whether both cumulative distribution functions are indeed equal, if this null cannot be rejected. Instead, following the general approach to equivalence testing (Wellek, 2010), the hypotheses are formulated in terms of a distance measure and reverse the roles of null and alternative relative to the classical Kolmogorov-Smirnov test:

$$H_0 : D \geq \delta \qquad \text{(non-equivalence; distributions differ by at least } \delta\text{)}, \tag{A2}$$

$$H_1 : D < \delta \qquad \text{(equivalence; distributions differ by less than } \delta\text{)}. \tag{A3}$$

Inference can be conducted by constructing a one-sided upper $(1-\alpha)$ confidence interval for D via bootstrap resampling. A standard implementation proceeds as follows:

- Resample within groups: draw bootstrap samples of sizes n from the control and the treatment group, each with replacement.
- For each bootstrap replication $b = 1, ..., B$, compute the KS statistic $D_n^{(b)}$. I draw $B = 1000$ bootstrap samples.
- Define the one-sided upper confidence bound $U_{1-\alpha}$ as the empirical $(1-\alpha)$-quantile of

the bootstrap distribution

$$\{D_n^{(b)}\}_{b=1,\ldots,B}.$$

I set $\alpha = 0.05$.

Equivalence is concluded if this upper bound lies below the equivalence margin:

$$\text{Reject } H_0 \text{ and conclude equivalence if } U_{1-\alpha} < \delta. \tag{A4}$$

Equivalently, equivalence is established if the upper end of a one-sided confidence interval for the KS test statistic D_n falls below the pre-specified margin δ.

In addition to testing equivalence for a pre-specified margin δ, it is often informative to report the marginal (or minimal) equivalence margin, i.e. the smallest value of δ for which equivalence would be concluded at level α. Formally, I define:

$$\delta^* := \inf\{\delta > 0 : \text{ equivalence is concluded at level } \alpha\}. \tag{A5}$$

Using the bootstrap approach for obtaining a confidence interval described above, the equivalence decision depends on whether $U_{1-\alpha} < \delta$. Hence, the marginal equivalence margin δ^* is obtained directly as the upper $(1-\alpha)$ confidence bound:

$$\delta^* = U_{1-\alpha}. \tag{A6}$$

When interpreting δ^*, equivalence can be established for any $\delta > \delta^*$, whereas equivalence cannot be established for any $\delta < \delta^*$ at the chosen significance level.

A2.3 Pathways after Displacement

Both the probit and the linear model outlined in Section 4.1 are estimated on a cross-sectional level, i.e. including all apprentices of the matched sample once at the timing of their (fictional) displacement. These probit models can be denoted in the following equation:

$$
\begin{aligned}
Z_i &= \text{const} + \beta \cdot D_i + \sum_{y \neq 1} \lambda_y \mathbf{1}(Y_i = y) D_i + \rho_{y(i)} + \eta_{c(i)} + \theta_{s(i)} + \kappa_{o(i)} + \zeta_i x_i + \varepsilon_i, \\
Y_i &= \begin{cases} 1, \text{ if } Z_i \geq 0 \\ 0, \text{ if } Z_i < 0. \end{cases}
\end{aligned} \tag{A7}
$$

Y_i represents the observed binary outcome variable of interest.[181] I include the fixed effects $\rho_{y(i)}$ to account for base differences between the different years of VET $y(i)$ of apprentice i.[182] To allow treatment effects to differ across VET years, I include interaction terms λ_y. For both $\rho_{y(i)}$ and λ_y, year 1 is excluded as base category to avoid perfect collinearity. The $\eta_{c(i)}$ are half-year fixed effects for accounting for differences between the different displacement cohorts c. Regional differences are absorbed by estimating federal state fixed effects $\theta_{s(i)}$, with $s(i)$ being the federal state s of the workplace of apprentice i. By including VET occupation fixed effects $\kappa_{o(i)}$, I account for systematic differences between apprentice i's training occupations $o(i)$.[183] The vector x_i contains the same sociodemographic characteristics and properties about the training firm as Equation (18).[184] Following Abadie/Spiess (2022), I cluster standard errors on the level of matched pairs.

Accordingly, the estimated linear models are given by

$$
Y_i = \text{const} + \beta \cdot D_i + \sum_{y \neq 1} \lambda_y \mathbf{1}(Y_i = y) D_i + \rho_{y(i)} + \eta_{c(i)} + \theta_{s(i)} + \kappa_{o(i)} + \varepsilon_i, \tag{A8}
$$

[181] For computing these outcome variables, the whole data set was used, i.e. all calendar half-years from 2006/1 to 2023/2. However, by definition they are time-invariant for each individual. E.g. an individual is assigned to be a *VET Graduate* if they graduate from VET at any point in time.

[182] Similar to Equation (1), the same underlying person can appear multiple times. Each appearance is treated as a separate observation in the outcome Y_i and treatment indicator D_i, where the subscript i indexes sample observations. By contrast, the individual fixed effect α_i is defined at the level of the underlying person. If a person appears multiple times in the sample, the same fixed effect is therefore applied to all of their observations. This notation is chosen to keep the subscripts simple.

[183] The 2-digit occupation structure from the KldB 2010 classification is used.

[184] These involve: gender, age (measured at the begin of the respective half-year), foreign citizenship, school degree (three categories), AKM-firm FE of the training firm (four quartiles or missing), workplace federal state FE (16) and VET Occupation FE (35 categories).

with Y_i being the observed continuous (or multinomial) outcome.[185] The same set of fixed effects and the same covariates as in Equation (A7) are included. I follow Abadie/Spiess (2022) and cluster standard errors on the match pairs level.

[185] Similarly to the probit model, the whole data set is used for computing these variables, but they are time invariant by construction.

A2.4 Occupational Similarity

For measuring occupational similarity, I follow Gathmann/Schönberg (2010) who use survey data on task profiles. I rely on the 3-digit KldB 2010 occupational structure and an expert data base with task profiles for each occupations from BERUFENET. Thereby, I am able to distinguish between finer occupational categories (144 in total, compared to 37 categories on the 2-digit level (BA, 2010). Finally, I use a data set with a measure on the occupational similarity of each occupational pair.[186]

This measure for occupational similarity between occupations k and l is defined as follows: Let $q_k = (q_{k1}, ..., q_{kS})$ denote the share of workers in training occupation k who perform tasks s, $s = 1, ..., S$, and analogously $q_l = (q_{l1}, ..., q_{lS})$ for the current occupation. The cosine similarity (also called *angular separation*) between the task profiles of occupations k and l is defined as:

$$\text{cos\_sim}_{k,l} = \frac{\sum_{s=1}^{N} q_{ks} \cdot q_{ls}}{\left[\left(\sum_{s=1}^{N} q_{ks}^2\right) \cdot \left(\sum_{s=1}^{N} q_{ls}^2\right)\right]}. \tag{A9}$$

This measure ranges from 0 to 1, with 1 indicating identical task structures.

[186] This has been provided by my colleagues Florian Hack and Britta Matthes.

A2.5 Variation of Treatment Effects

I follow Heckman/Smith/Clements (1997) to compute a lower bound for the standard deviation of the treatment effect. Let F_Y^1 denote the cumulative distribution function of outcome variable Y among the treatment group, and F_Y^0 among the control group, respectively. They propose to evaluate the difference between the quantile functions $Q_1^-(p)$ and $Q_0^-(p)$ of treated and controls, respectively, i.e. computing

$$\tau(p) = Q_1^-(p) - Q_0^-(p) \qquad \forall p \in (0, 1). \tag{A10}$$

Hereby $Q^-(p) = \inf\{x : F(x) \geq p\}$ corresponds to the *smallest* p-quantile given a cumulative distribution function F. In case of a constant treatment effect, the quantile difference $\tau(p)$ equals the average treatment effect for all $p \in (0, 1)$. Its standard deviation $SD(\tau)$ then gives a lower bound for the standard deviation of the treatment effect. It is a lower bound as the procedure relies on the assumption of rank preservation across treated and controls. For further details, see Heckman/Smith/Clements (1997).

I consider the effect at each event time $k \geq 0$ separately and implement their approach by considering $\tau^{(k)}(p)$ at each percentile, i.e., $p \in \{0.01, ..., 0.99\}$, and report

$$SD^{(k)}(\tau) = \frac{1}{98} \sum_{p=0.01}^{0.99} (\tau^{(k)}(p) - \overline{\tau^{(k)}(p)}), \qquad k = 0, ..., 16$$

as a lower bound for the standard deviation for the estimated effects.

A2.6 Equivalence Test for Coefficient Estimates

To assess whether an estimated coefficient estimate from the event study in Equation (15) is practically indistinguishable from zero, I implement an equivalence test following Wellek (2010). Let β_{-2} denote the true coefficient at event time $k = -2$, and let $\delta > 0$ be given. An equivalence test reverses the usual logic of hypothesis testing by taking non-equivalence as the null hypothesis:

$$H_0 : \beta_{-2} \leq -\delta \text{ or } \beta_{-2} \geq \delta, \qquad H_1 : -\delta < \beta < \delta. \tag{A11}$$

Unlike Wald tests, the equivalence test can therefore formally establish that a coefficient estimate is statistically indistinguishable from zero within a pre-specified tolerance, at significance level α. It can be shown that the composite null hypothesis in (A11) can be tested given the following decision rule (Wellek, 2010): Let $\hat{\beta}_{-2}$ be the point estimate of β_{-2} and SE$_{\beta_{-2}}$ its standard error. At significance level α, equivalence is concluded if both one-sided null hypotheses are rejected:

$$H_0^- : \beta_{-2} \leq -\delta, \qquad H_0^+ : \beta_{-2} \geq \delta. \tag{A12}$$

Accordingly, equivalence holds if the $(1 - 2\alpha)-$ confidence interval for β_{-2} lies entirely inside the equivalence region $[-\delta, +\delta]$:

$$\hat{\beta}_{-2} - z_{1-\alpha} \cdot \mathrm{SE}(\hat{\beta}_{-2}) > -\delta, \tag{A13}$$

$$\hat{\beta}_{-2} + z_{1-\alpha} \cdot \mathrm{SE}(\hat{\beta}_{-2}) < \delta, \tag{A14}$$

where $z_{1-\alpha}$ denotes the $(1 - \alpha)$-quantile of the standard normal distribution. As the choice of δ is inherently subjective, Seaman/Serlin (1998) propose to report the minimal equivalence bound δ^* for which equivalence can still be established given the data. This corresponds to the smallest δ such that the entire $(1 - 2\alpha)$ confidence interval is contained in $[-\delta, +\delta]$. Therefore, the marginal equivalence bound is given by

$$\delta^* = \mathsf{max}\Big\{\big|\hat{\beta}_{-2} - z_{1-\alpha} \cdot \mathsf{SE}(\hat{\beta}_{-2})\big|,\ \big|\hat{\beta}_{-2} + z_{1-\alpha} \cdot \mathsf{SE}(\hat{\beta}_{-2})\big|\Big\}. \tag{A15}$$

The resulting δ^* is the smallest tolerance level at which the data remain consistent with an effect sufficiently close to zero to be considered economically negligible. These are reported in Section 7.1 when estimating the event study for the pre-treatment periods in order to quantify the economic importance of the coefficients β_{-2} for selected outcome variables.

A2.7 Mechanisms Analysis

In this section, I derive the decomposition of the event study difference-in-difference estimate Δ_k when omitting the fixed effects, as shown in Equation (11) for the case of VET success. Importantly, this analysis relies on observed outcomes for treated and control apprentices in the matched sample and does not invoke counterfactual outcomes. All terms in the decomposition are defined as conditional expectations and probabilities for the treated and control groups, as well as for subgroups within each of these groups. All expectations and probabilities are computed using the observed outcomes of the respective group within the matched sample at event time k, i.e., without invoking any counterfactual outcomes. In the empirical implementation, these conditional expectations and probabilities are estimated by the corresponding sample means and shares, respectively.
To this end, let $Y_{i,k}^{-1}$ be an outcome of interest for individual i at event time $k = t - c(i)$ relative to base period $k = -1$, i.e., at each time, the value in base period $k = -1$ is subtracted ($Y_{i,k}^{-1} := Y_{i,k} - Y_{i,-1}$). Throughout Section 5, earnings compared to base period $k = -1$ are considered as outcome variable. Further denote by D_i the treatment indicator.
The contribution of changes between and within a binary variable G taking values a small set of discrete values is to be decomposed. In the first analysis in Section 5.0.1, $G_i = 0$ stands for VET dropouts and $G_i = 1$ for graduates, which are by definition time-invariant. As both the treatment indicator D and the mechanism variable G are constant for all event times, the subscript k is omitted for these variables. To keep the notation as easy as possible, the subscript i is also omitted.

The derivation of Equation (11) is as follows:

$$
\begin{aligned}
\Delta_k &= \mathbb{E}\big[Y_k^{-1}|D=1\big] - \mathbb{E}\big[Y_k^{-1}|D=0\big] \qquad \text{(A16)}\\
&= \sum_{j\in\{0,1\}} \Big(\mathbb{P}(G=j|D=1)\cdot\mathbb{E}\big[Y_k^{-1}|D=1,G=j\big]\\
&\qquad -\mathbb{P}(G=j|D=0)\cdot\mathbb{E}\big[Y_k^{-1}|D=0,G=j\big]\Big)\\
&\underset{187}{=} \mathbb{P}(G=0|D=1)\cdot\mathbb{E}\big[Y_k^{-1}|D=1,G=0\big] + \mathbb{P}(G=1|D=1)\cdot\mathbb{E}\big[Y_k^{-1}|D=1,G=1\big]\\
&\quad + \Big(\mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=0)\Big)\cdot\mathbb{E}\big[Y_k^{-1}|D=0,G=1\big]\\
&\quad + \Big(\mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=1) - \mathbb{P}(G=0|D=0)\Big)\cdot\mathbb{E}\big[Y_k^{-1}|D=0,G=0\big]\\
&= \mathbb{P}(G=0|D=1)\cdot\mathbb{E}\big[Y_k^{-1}|D=1,G=0\big] + \mathbb{P}(G=1|D=1)\cdot\mathbb{E}\big[Y_k^{-1}|D=1,G=1\big]\\
&\quad + \Big(\mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=0)\Big)\cdot\mathbb{E}\big[Y_k^{-1}|D=0,G=1\big]
\end{aligned}
$$

$$
\begin{aligned}
&\quad - \mathbb{P}(G=1|D=1) \cdot \mathbb{E}\big[Y_k^{-1}|D=0, G=1\big] \\
&\quad \underset{188}{+} \Big(1 - \mathbb{P}(G=0|D=1) - 1 + \mathbb{P}(G=1|D=0)\Big) \cdot \mathbb{E}\big[Y_k^{-1}|D=0, G=0\big] \\
&\quad - \mathbb{P}(G=1|D=1) \cdot \mathbb{E}\big[Y_k^{-1}|D=0, G=0\big] \\
&= \Big(\mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=0)\Big) \\
&\qquad\qquad \cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&\quad + \mathbb{P}(G=1 \mid D=1) \cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big]\Big) \\
&\quad + \mathbb{P}(G=0 \mid D=1) \cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=0\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&:= C_k + W_k^1 + W_k^0.
\end{aligned}
$$

In the example of VET graduation vs. dropout, the decomposition can be expressed as

$$
\Delta_k = \underbrace{[\mathbb{P}(G=1|D=1) - \mathbb{P}(G=1|D=0)]}_{\substack{\text{Treatment-Induced Change} \\ \text{in Graduation Share}}} \cdot \underbrace{\Delta Y^{-1}_{G=1,G=0|D=0}}_{\substack{\text{Graduation Surplus} \\ \text{among Controls}}} \tag{A17}
$$

$$
+ \underbrace{\mathbb{P}(G=1|D=1)}_{\substack{\text{Graduation Share} \\ \text{among Treated}}} \cdot \underbrace{\Delta Y^{-1}_{D=1,D=0|G=1}}_{\substack{\text{Treatment Differential} \\ \text{among Graduates}}} + \underbrace{\mathbb{P}(G=0|D=1)}_{\substack{\text{Dropout Share} \\ \text{among Treated}}} \cdot \underbrace{\Delta Y^{-1}_{D=1,D=0|G=0}}_{\substack{\text{Treatment Differential} \\ \text{among Dropouts}}} \tag{A18}
$$

$$
= C_k + W_k^1 + W_k^0. \tag{A19}
$$

The decomposition analyses for the role of delaying graduations from VET following displacement in Section 5.0.2 is done in a similar way by considering only graduates and setting $G_i = 0$ for early and $G_i = 1$ for late graduates, respectively.
When considering the role of increasing mobility over the event time as in Section 5.0.2, I use a grouping variable $G_{i,k}$ that takes three different values, say $j = 0, 1, 2$. This is because both occupational and regional mobility and measured only in case of regular employment, but non-employed individuals contribute to the event study estimate of the displacement effect on earnings as well. Importantly and unlike for the previous cases, both occupational and regional mobility are varying over time. Setting $G_{i,k} = 0$ for staying (in the same occupation or region, respectively), $G_{i,k} = 1$ for moving (to another occupation or region, respectively), and $G_{i,k} = 2$ in case of no employment in the respective event half year k, and using similar transformation steps as in Equation (A16), one can decompose:

[187] Two terms are both added and subtracted: $\pm \mathbb{P}(G=1|D=1) \cdot \mathbb{E}\big[Y_k^{-1}|D=0, G=0\big]$ and $\pm \mathbb{P}(G=1|D=1) \cdot \mathbb{E}\big[Y_k^{-1}|D=0, G=1\big]$.
[188] The equalities $\mathbb{P}(G=1|D=1) = 1 - \mathbb{P}(G=0|D=1)$ and $\mathbb{P}(G=0|D=0) = 1 - \mathbb{P}(G=1|D=0)$ are used.

$$\Delta_k = C_k^1 + C_k^2 + W_k^0 + W_k^1 + W_k^2. \tag{A20}$$

C_k^1 hereby stands for the contribution of occupational (regional) mobility as opposed to employment in the previous VET occupation (region), whereas C_k^2 capture the contribution of non-employment against employment in the previous VET occupation (region).

W_k^0 denotes the contribution of differences within the group of occupational (regional) stayers, W_k^1 the contribution of differences within the group of occupational (regional) movers, and finally W_k^2 the contribution of differences within the group of non-employed. In formulae, these terms can be written as

$$\begin{aligned} C_k^j =& \Big(\mathbb{P}(G_k = j|D=1) - \mathbb{P}(G_k = j|D=0)\Big) \\ &\cdot \Big(\mathbb{E}\big[Y_k^{-1}|D=0, G_k = j\big] - \mathbb{E}\big[Y_k^{-1}|D=0, G_k=0\big]\Big), \quad j=1,2, \end{aligned}$$

and

$$\begin{aligned} W_k^j = \mathbb{P}(G_k = j|D=1) \cdot \Big((\mathbb{E}\big[Y_k^{-1}|D=1, G_k=j\big] - \mathbb{E}\big[Y_k^{-1}|D=0, G_k=j\big]\Big), \\ j = 0,1,2. \end{aligned}$$

The exact derivation is as follows:

$$\begin{aligned} \Delta_k &= \mathbb{E}\big[Y_k^{-1} \mid D=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0\big] \\ &= \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=1) \cdot \mathbb{E}\big[Y_k^{-1} \mid D=1, G=j\big] \\ &\quad - \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=0) \cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big] \\ &= \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=1) \cdot \mathbb{E}\big[Y_k^{-1} \mid D=1, G=j\big] \\ &\quad - \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=1) \cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big] \\ &\quad + \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=1) \cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big] \end{aligned}$$

$$
\begin{aligned}
&\quad - \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=0)\cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big] \\
&= \sum_{j\in\{0,1,2\}} \mathbb{P}(G=j \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=j\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big]\Big) \\
&\quad + \sum_{j\in\{0,1,2\}} \Big(\mathbb{P}(G=j \mid D=1) - \mathbb{P}(G=j \mid D=0)\Big)\cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=j\big] \\
&= \mathbb{P}(G=0 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=0\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&\quad + \mathbb{P}(G=1 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big]\Big) \\
&\quad + \mathbb{P}(G=2 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=2\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=2\big]\Big) \\
&\quad + \Big(\mathbb{P}(G=0 \mid D=1) - \mathbb{P}(G=0 \mid D=0)\Big)\cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big] \\
&\quad + \Big(\mathbb{P}(G=1 \mid D=1) - \mathbb{P}(G=1 \mid D=0)\Big)\cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big] \\
&\quad + \Big(\mathbb{P}(G=2 \mid D=1) - \mathbb{P}(G=2 \mid D=0)\Big)\cdot \mathbb{E}\big[Y_k^{-1} \mid D=0, G=2\big] \\
&= \mathbb{P}(G=0 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=0\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&\quad + \mathbb{P}(G=1 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big]\Big) \\
&\quad + \mathbb{P}(G=2 \mid D=1)\cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=1, G=2\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=2\big]\Big) \\
&\quad + \Big(\mathbb{P}(G=1 \mid D=1) - \mathbb{P}(G=1 \mid D=0)\Big) \\
&\qquad\quad \cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=0, G=1\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&\quad + \Big(\mathbb{P}(G=2 \mid D=1) - \mathbb{P}(G=2 \mid D=0)\Big) \\
&\qquad\quad \cdot \Big(\mathbb{E}\big[Y_k^{-1} \mid D=0, G=2\big] - \mathbb{E}\big[Y_k^{-1} \mid D=0, G=0\big]\Big) \\
&=: C_k^1 + C_k^2 + W_k^0 + W_k^1 + W_k^2, \qquad \text{(A21)}
\end{aligned}
$$

where in the last step, the equalities

$$\mathbb{P}(G=0|D=1) = 1 - \mathbb{P}(G=1|D=1) - \mathbb{P}(G=2|D=1)$$

and

$$\mathbb{P}(G=0|D=0) = 1 - \mathbb{P}(G=1|D=0) - \mathbb{P}(G=2|D=0)$$

are used.

In the remainder of this section, I briefly describe how all mechanisms are combined in Section 5.0.2. I split this tasks into three subtasks. First, I combine the decompositions of Sections 5.0.1 and 5.0.2. Second, I combine the two mobility mechanisms considered in

Section 5.0.2. Third, I combine the two previous distinct decompositions such that finally all mechanisms are considered jointly.

For the first part, I set $G_i = 1$ for VET graduates and $G_i = 0$ for VET dropouts. For early graduates, I set $T_i = 0$ and for late graduates, I set $T_i = 1$.[189]

With the same notation as for the previous decomposition and applying the law of total probability and smartly adding and subtracting new terms and re-arranging, one can show that:

$$
\begin{aligned}
\Delta_k &= \mathbb{E}[Y_k^{-1}|D=1] - \mathbb{E}[Y_k^{-1}|D=0] \\
&= \mathbb{P}(G=0|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,G=0] \\
&\quad + \mathbb{P}(G=1|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,G=1] \\
&\quad - \mathbb{P}(G=0|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,G=0] \\
&\quad - \mathbb{P}(G=1|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,G=1] \\
&= \mathbb{P}(G=0|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,G=0] \\
&\quad + \mathbb{P}(G=1|D=1)\cdot\mathbb{P}(T=0|D=1,G=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,G=1,T=0] \\
&\quad + \mathbb{P}(G=1|D=1)\cdot\mathbb{P}(T=1|D=1,G=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,G=1,T=1] \\
&\quad - \mathbb{P}(G=0|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,G=0] \\
&= \underbrace{[\mathbb{P}(G=1|D=1)-\mathbb{P}(G=1|D=0)]\cdot\big[\mathbb{E}[Y_k^{-1}|D=0,G=1]-\mathbb{E}[Y_k^{-1}|D=0,G=0]\big]}_{C_k^{Grad}} \\
&\quad + \underbrace{\begin{aligned}&\mathbb{P}(G=1|D=1)\cdot[\mathbb{P}(T=1|G=1,D=1)-\mathbb{P}(T=0|G=1,D=1)] \\ &\quad\cdot\big[\mathbb{E}[Y_k^{-1}|D=0,G=1,T=1]-\mathbb{E}[Y_k^{-1}|D=0,G=1,T=0]\big]\end{aligned}}_{C_k^{\text{Delay | Grad}}} \\
&\quad + \underbrace{\mathbb{P}(G=0|D=1)\cdot\big[\mathbb{E}[Y_k^{-1}|D=1,G=0]-\mathbb{E}[Y_k^{-1}|D=0,G=0]\big]}_{W_k^{\text{Drop}}} \\
&\quad + \underbrace{\begin{aligned}&\mathbb{P}(T=0|G=1,D=1)\cdot\mathbb{P}(G=1|D=1) \\ &\quad\cdot\big[\mathbb{E}[Y_k^{-1}|D=1,G=1,T=0]-\mathbb{E}[Y_k^{-1}|D=0,G=1,T=0]\big]\end{aligned}}_{W_k^{\text{Early Grad}}} \\
&\quad + \underbrace{\begin{aligned}&\mathbb{P}(T=1|G=1,D=1)\cdot\mathbb{P}(G=1|D=1) \\ &\quad\cdot\big[\mathbb{E}[Y_k^{-1}|D=1,G=1,T=1]-\mathbb{E}[Y_k^{-1}|D=0,G=1,T=1]\big]\end{aligned}}_{W_k^{\text{Late Grad}}}.
\end{aligned} \tag{A22}
$$

A more detailed explanations of the steps in between is available upon request.

[189] For dropouts, T_i is not defined.

Second, one can decompose Δ using only the two mobility mechanisms jointly. To do so, I now define

$$O_{it} = \begin{cases} 0, & \text{if } i \text{ is employed in their first VET occupation in calendar half-year } t, \\ 1, & \text{if } i \text{ is employed outside their first VET occupation in calendar half-year } t, \\ 2, & \text{if } i \text{ is not employed in calendar half-year } t, \end{cases} \tag{A23}$$

and analogeously for regional mobility

$$R_{it} = \begin{cases} 0, & \text{if } i \text{ is employed in the county of their first VET in calendar half-year } t, \\ 1, & \text{if } i \text{ is employed outside the county of their first VET in calendar half-year } t, \\ 2, & \text{if } i \text{ is not employed in calendar half-year } t, \end{cases} . \tag{A24}$$

By definition, we have $O_{it} = 2 \Leftrightarrow R_{it} = 2$. By applying the law of total probability and smartly adding and subtracting further terms and rearranging, one can show:

$$\begin{aligned}
\Delta_k &= \mathbb{E}[Y_k^{-1}|D=1) - \mathbb{E}[Y_k^{-1}|D=0] \\
&= \mathbb{P}(O_k=0|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,O_k=0] + \mathbb{P}(O_k=1|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,O_k=1] \\
&\quad + \mathbb{P}(O_k=2|D=1)\cdot\mathbb{E}[Y_k^{-1}|D=1,O_k=2] \\
&\quad - \mathbb{P}(O_k=0|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,O_k=0] - \mathbb{P}(O_k=1|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,O_k=1] \\
&\quad - \mathbb{P}(O_k=2|D=0)\cdot\mathbb{E}[Y_k^{-1}|D=0,O_k=2] \\
&= \underbrace{\Big(\mathbb{P}(O_k=2|D=1)-\mathbb{P}(O_k=2|D=0)\Big)\cdot\Big(\mathbb{E}[Y_k^{-1}|D=0,O_k=2)-\mathbb{E}[Y_k^{-1}|D=0,O_k=0]\Big)}_{C_k^{\text{Unempl}}} \\
&\quad + \underbrace{\Big(\mathbb{P}(O_k=1|D=1)-\mathbb{P}(O_k=1|D=0)\Big)\cdot\Big(\mathbb{E}[Y_k^{-1}|D=0,O_k=1]-\mathbb{E}[Y_k^{-1}|D=0,O_k=0]\Big)}_{C_k^{\text{Occ Move}}} \\
&\quad + \underbrace{\begin{array}{l}\mathbb{P}(O_k=0|D=1)\cdot\Big(\mathbb{P}(R_k=1|D=1,O_k=0)-\mathbb{P}(R_k=1|D=0,O_k=0)\Big) \\ \quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=0,O_k=0,R_k=1]-\mathbb{E}[Y_k^{-1}|D=0,O_k=0,R_k=0]\Big)\end{array}}_{C_k^{\text{Reg Move | Occ Stay}}} \\
&\quad + \underbrace{\begin{array}{l}\mathbb{P}(O_k=1|D=1)\cdot\Big(\mathbb{P}(R_k=1|D=1,O_k=1)-\mathbb{P}(R_k=1|D=0,O_k=1)\Big) \\ \quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=0,O_k=1,R_k=1]-\mathbb{E}[Y_k^{-1}|D=0,O_k=1,R_k=0]\Big)\end{array}}_{C_k^{\text{Reg Move | Occ Move}}}
\end{aligned} \tag{A25}$$

$$
\begin{aligned}
&+\underbrace{\mathbb{P}(O_k=2|D=1)\cdot\Big(\mathbb{E}[Y_k^{-1}|D=1,O_k=2,R_k=2]-\mathbb{E}[Y_k^{-1}|D=0,O_k=2,R_k=2]\Big)}_{W_k^{\text{Unempl}}}\\
&+\underbrace{\begin{aligned}&\mathbb{P}(O_k=0|D=1)\cdot\mathbb{P}(R_k=0|D=1,O_k=0)\\&\quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=1,O_k=0,R_k=0]-\mathbb{E}[Y_k^{-1}|D=0,O_k=0,R_k=0]\Big)\end{aligned}}_{W_k^{\text{Occ Stay, Reg Stay}}}\\
&+\underbrace{\begin{aligned}&\mathbb{P}(O_k=0|D=1)\cdot\mathbb{P}(R_k=1|D=1,O_k=0)\\&\quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=1,O_k=0,R_k=1]-\mathbb{E}[Y_k^{-1}|D=0,O_k=0,R_k=1]\Big)\end{aligned}}_{W_k^{\text{Occ Stay, Reg Move}}}\\
&+\underbrace{\begin{aligned}&\mathbb{P}(O_k=1|D=1)\cdot\mathbb{P}(R_k=0|D=1,O_k=1)\\&\quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=1,O_k=1,R_k=0]-\mathbb{E}[Y_k^{-1}|D=0,O_k=1,R_k=0]\Big)\end{aligned}}_{W_k^{\text{Occ Move, Reg Stay}}}\\
&+\underbrace{\begin{aligned}&\mathbb{P}(O_k=1|D=1)\cdot\mathbb{P}(R_k=1|D=1,O_k=1)\\&\quad\cdot\Big(\mathbb{E}[Y_k^{-1}|D=1,O_k=1,R_k=1]-\mathbb{E}[Y_k^{-1}|D=0,O_k=1,R_k=1]\Big)\end{aligned}}_{W_k^{\text{Occ Move, Reg Move}}}.
\end{aligned}
\tag{A26}
$$

A more detailed explanations of the steps in between is available upon request.

Third, for finally combining all mechanisms, I do not show the exact formula each component step by step. The intuition is the following: consider the first decomposition in Equation (A22) as a starting point. The compositional components,

$$
C_k^{\text{Grad}} + C_k^{\text{Delay | Grad}},
$$

remain unchanged in the final decomposition. The three within-group components are further decomposed in a way such that the decomposition in Equation (A25) is applied to each of them separately. This gives the following final decomposition:

$$
\begin{aligned}
\Delta_k = {} & C_k^{\text{Grad}} + C_k^{\text{Delay | Grad}}\\
&+ C_k^{\text{Occ Move | Drop}} + C_k^{\text{Unempl | Drop}}\\
&+ C_k^{\text{Occ Move | Early Grad}} + C_k^{\text{Unempl | Early Grad}}\\
&+ C_k^{\text{Occ Move | Late Grad}} + C_k^{\text{Unempl | Late Grad}}\\
&+ C_k^{\text{Reg Move | Drop, Occ Stay}} + C_k^{\text{Reg Move | Drop, Occ Move}}\\
&+ C_k^{\text{Reg Move | Early Grad, Occ Stay}} + C_k^{\text{Reg Move | Early Grad, Occ Move}}\\
&+ C_k^{\text{Reg Move | Late Grad, Occ Stay}} + C_k^{\text{Reg Move | Late Grad, Occ Move}}\\
&+ W_k^{\text{Drop, Occ Stay, Reg Stay}} + W_k^{\text{Drop, Occ Stay, Reg Move}}\\
&+ W_k^{\text{Drop, Occ Move, Reg Stay}} + W_k^{\text{Drop, Occ Move, Reg Move}}
\end{aligned}
$$

$$
\begin{aligned}
&+ W_k^{\text{Drop, Unempl}} \\
&+ W_k^{\text{Early Grad, Occ Stay, Reg Stay}} + W_k^{\text{Early Grad, Occ Stay, Reg Move}} + W_k^{\text{Early Grad, Occ Move, Reg Stay}} \\
&+ W_k^{\text{Early Grad, Occ Move, Reg Move}} + W_k^{\text{Early Grad, Unempl}} \\
&+ W_k^{\text{Late Grad, Occ Stay, Reg Stay}} + W_k^{\text{Late Grad, Occ Stay, Reg Move}} \\
&+ W_k^{\text{Late Grad, Occ Move, Reg Stay}} + W_k^{\text{Late Grad, Occ Move, Reg Move}} + W_k^{\text{Late Grad, Unempl}}
\end{aligned}
$$

where the terms in lines 2, 5 and 8-9 stem from W^{Drop}, those in lines 3, 6 and 10-11 from $W^{\text{Early Grad}}$, and those in lines 4, 7 and 12-13 stem from $W^{\text{Late Grad}}$ in Equation (A22).

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