Adjusters and Casualties: The Anatomy of Labor Market Displacement*

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Abstract

We analyze the full distribution of realized earnings impacts following job displacement, using a new method that combines matching and synthetic control group approaches at the individual level. The distribution of estimated earnings losses is highly skewed. Average losses, as estimated by conventional event studies, are driven by a small number of workers who suffer catastrophic losses, while most recover quickly. Observable worker characteristics explain only a small fraction of the variance in earnings losses. Instead, we find substantial heterogeneity in earnings losses even among workers displaced by the same firm who have identical observed characteristics such as education, age, and gender. Workers with minimal earnings losses adjust quickly by switching industries, occupations, and especially regions, while comparable workers with catastrophic losses adjust slowly, even though they are forced to make comparable numbers of switches in the long run.

Keywords: displacement losses, synthetic control groups, distributions of treatment effects

JEL Classification: J24, J64, O30

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1 Introduction

Decades of economic research have consistently highlighted that firm closures and job displacements cause large and enduring earnings losses that vary substantially across worker groups.[1] However, critical gaps remain in our understanding of how losses are distributed more generally because most previous studies focus on average earnings losses, and therefore fail to capture the considerable variation in earnings losses among observably similar workers.

This paper analyzes the full distribution of earnings losses following job displacement using social security data from Germany. We construct individual-level control groups using a novel blend of matching and synthetic control methods that allow us to characterize the complete distribution of earnings losses rather than focusing solely on average effects. We find that displaced workers' earnings losses are far from normally distributed, implying that average estimates from conventional event studies overstate the impact of firm closure for most workers. Moreover, against conventional wisdom, our results suggest that the heterogeneity of displacement losses is substantially greater within rather than across groups of workers with similar observable characteristics. That is, we find substantial variation in earnings losses even among workers who share the same commonly observable characteristics (such as education, occupation, and age) and who were displaced by the same firm.

Using administrative data covering all plant closures in West Germany from 2000 to 2005, we can track displaced workers' wages and earnings. This dataset offers detailed information on wages, employment status, and matched firm-worker characteristics for workers' entire careers—granularity often lacking in comparable U.S. data. The high frequency and richness of the data allow us to compare earnings trajectories of otherwise observably similar workers who differ only in their exposure to firm closures.

Our main contribution to the literature is to analyze the full distribution of estimated earnings losses following firm closures. We extend the matching approach of Schmieder et al. (2023) by incorporating synthetic control methods, allowing us to construct a tailored control for each displaced worker. We create a synthetic counterpart for each displaced worker matched on demographics, firm characteristics, and pre-closure earnings and then compare each displaced worker's actual earnings to those of their synthetic control. This approach enables us to analyze the entire distribution of estimated displacement losses, both across and within subgroups of workers with comparable observable characteristics.

Our paper produces three main results. First, our approach reveals substantial dispersion and skewness in estimated earnings losses following displacement. The distribution is highly skewed: the well-documented large and persistent earnings declines following firm closure are driven by a relatively small subset of workers who experience severe and prolonged losses. In contrast, cumulative earnings losses of the modal displaced worker during the five years post-layoff are only three months of pre-closure earnings. Additionally, nearly a quarter of displaced workers earn more than their synthetic controls following displacement, consistent with recent evidence that many workers hold incorrect beliefs about their outside options (Jäger et al.,

[1] E.g., Jacobson et al. (1993); Couch and Placzek (2010); Schmieder et al. (2010, 2023); Lachowska et al. (2020); Davis and von Wachter (2011), Chan and Stevens (1999), Chan and Huff Stevens (2001), Schwerdt et al. (2010), Hanushek et al. (2017), Illing et al. (2024).

2024) or have other reasons for not leaving their firm. For example, Farber (2017) finds that a similarly substantial minority of U.S. workers laid off during the Great Recession saw higher earnings in their new jobs.

Second, we find a substantial overlap in loss distributions across demographic groups. Our method reproduces established patterns from standard event study estimates of the *average* cost of displacement, including that less-educated workers, women, and older workers experience above-average losses. But observable characteristics explain only a small fraction of the total variation. More specifically, commonly observable pre-displacement characteristics explain less than 20 percent of the variation in estimated earnings losses. As a result, differences in losses are much larger *within* groups of observably similar individuals than *between* different demographic groups.[2]

Third, adjustment behavior differs sharply between workers with small estimated earnings losses ("adjusters") and those with large estimated losses ("casualties"). Consistent with previous research, we find that move to lower-wage firms explains a substantial portion of displaced workers' average earnings losses (Schmieder et al., 2023; Lachowska et al., 2020; Fackler et al., 2021). However, we observe casualties not only move to substantially lower-paying firms, but also earn less than the typical worker at their new firms. In contrast, adjusters tend to move to slightly higher-wage firms and, more importantly, earn substantially more than the average worker at those firms. Adjustment patterns also differ in timing and stability: adjusters make decisive moves across firms and occupations immediately after displacement, whereas casualties initially move less but then experience long-term instability, repeatedly changing employers, occupations, and locations without regaining their pre-closure earnings levels.

We assess and address potential concerns with the synthetic control method, particularly the risk that overfitting in the pre-treatment period could bias post-closure estimates, in multiple ways. We conduct several robustness exercises. First, we show that our synthetic control approach yields average earnings loss estimates that closely match those from a conventional event study across the full distribution of losses. Second, following Abadie (2021), we demonstrate that earnings fluctuations among displaced workers are significantly wider than among a matched set of non-displaced workers, suggesting that our results are not driven by noise. Third, we confirm that deviations between displaced workers and their synthetic controls are statistically indistinguishable in the pre-treatment period, even in the tails of the loss distribution. Finally, we re-estimate the distribution of earnings losses on 10 percent subsamples and find that the shape and variance of the distribution remain stable, indicating that our results are not sensitive to random outliers.

We make four main contributions to the large literature analyzing displaced workers' earnings losses. First, we develop a methodology for estimating the full distribution of earnings losses and show that it is feasible to trace this distribution for a complete set of firm closures. Second, we refine the existing research that documents average displacement losses differing by

[2]This finding is consistent with evidence that even workers who hold the same job and earn the same wage can differ substantially in productivity (Mas and Moretti, 2009; Lazear et al., 2015; Sandvik et al., 2020). Such differences may contribute to the wide heterogeneity in displacement losses, even within observably similar groups.

education,[3] gender,[4] tenure,[5] worker-firm match,[6] and firm characteristics.[7] This refinement shows that the observable characteristics explain only a modest fraction of the overall variance in losses, suggesting that programs targeted at such observable characteristics will not distinguish well between the truly needy and the rest of the displaced population. Moreover, we document differences in the post-displacement careers of workers who adjust to firm closures and otherwise similar workers who become casualties.[8]

Third, our findings contribute to interpreting recent estimates of displacement loss distributions derived from machine learning methods. Gulyas et al. (2021) and Athey et al. (2023) leverage machine learning techniques to estimate heterogeneous displacement losses based on high-dimensional interactions of observed worker characteristics. These methods are particularly useful in settings where rich administrative data can uncover potentially nonlinear relationships between earnings losses and worker and firm characteristics. However, in this case where, as we have shown, observable worker attributes explain only a modest share of the variation in outcomes, these methods tend to shrink the variance of loss estimates by grouping together workers with similar characteristics but divergent realized post-displacement earnings trajectories. The essential difference between these machine learning approaches and ours is that we estimate the heterogeneity of earnings losses for displaced workers. This, in turn, allows us to analyze contrasting earnings and career paths of adjusters and casualties with identical (observed) pre-treatment characteristics.

Fourth, our work intersects with studies of the impact of trade exposure and of the prevalence and mediating impact of lifelong learning and human capital investments. While differences in loss patterns by trade impact do exist, they are small and do not change the patterns of losses that we find. When we look for worker adjustments to firm closures, we find little such activity and what exists does not systematically interact with earnings loss patterns.

The paper proceeds as follows. Section 2 outlines the synthetic control strategy used to estimate individual-level earnings losses. Section 3 describes the data sources used for our analysis. We summarize our main results in Section 4. In Section 5, we compare our estimates to losses estimated with alternative methods used previously in the literature. In Section 6, we document margins of adjustment that explain some of the variation in earnings losses among displaced workers. In Section 7, we investigate additional sources of heterogeneity in workers' responses to displacement and explore whether alternative channels of labor market adjustment—such as trade exposure or upgrading education—further explain differences in earnings losses. Section

[3] Schwerdt et al. (2010), Hanushek et al. (2017).

[4] Illing et al. (2024).

[5] Chan and Stevens (1999), Chan and Huff Stevens (2001).

[6] Moore and Scott-Clayton (2025), Lachowska et al. (2020), Gulyas et al. (2021), Fackler et al. (2021), Graham et al. (2023).

[7] Fackler et al. (2021) show that workers who are displaced by larger firms forgo larger firm wage premia than those who are displaced by smaller firms, and Raposo et al. (2021) show that job titles account for 37 percent of the average earnings losses.

[8] Two other lines of research into firm closures that are beyond the scope of this project consider country-specific institutions and business cycles. On the first, Bertheau et al. (2022) find that displacement losses tend to be lower in countries with more generous welfare systems, and Janssen (2018) shows that displacement losses are larger under flexible as opposed to rigid wage bargaining systems. On the second, Davis and von Wachter (2011) and Schmieder et al. (2023) show that the magnitude of displaced workers' average earnings losses vary strongly with the business cycle. These business cycle effects might alter the magnitude of overall losses, but they seem unlikely to change the overall distributional conclusions of our work.

8 concludes.

2 Empirical strategy

The standard approach to estimating earnings losses from worker displacement compares the outcomes of displaced workers to those of workers employed in firms that do not close. Event study designs recover average losses—and average losses for broad subgroups—but they cannot estimate the full distribution of losses among observably similar workers.

We estimate individual-specific estimates of earnings deviations relative to a synthetic counterfactual, which we use to characterize the distribution of earnings losses across displaced workers. This approach adapts synthetic controls, traditionally applied to aggregate units, to the worker level in order to approximate each displaced worker's counterfactual earnings trajectory. Our approach is closely related to Schmieder et al. (2023), who match displaced workers to observably similar non-displaced workers to estimate average short-term earnings losses. We extend their logic in two ways: first, we construct a refined donor pool for each displaced worker using exact matching on key job characteristics and pre-closure firm attributes; and second, within this donor pool, we use synthetic control weights to match long pre-displacement earnings trajectories. The resulting individual-level estimates can then be aggregated to allow us to study average effects and the distribution of estimated losses across workers.

2.1 Conceptual framework

For a displaced worker i, let $Y_i(1)$ denote post-displacement earnings and $Y_i(0)$ denote the counterfactual earnings absent displacement. Following the standard potential-outcomes framework, the counterfactual earnings process is:

$$Y_i(0) = \mu(H_i) + \beta X_i + \varepsilon_{i0} \tag{1}$$

where $\mu(H_i)$ is the worker's pre-displacement earnings history, X_i includes other observed characteristics (e.g., gender, occupation, level of education), and ε_{i0} captures idiosyncratic shocks. Displacement generates an individual-specific loss

$$Y_i(1) = Y_i(0) - \tau_i(A_i) + \epsilon_{i1}$$

where the displacement effect τ_i conceptually may vary with unobserved adjustment ability A_i,[9] which affects post-closure earnings but does not directly affect counterfactual earnings conditional on pre-treatment earnings trajectories. Formally,

$$A_i \perp \varepsilon_{i0} | \mu(H_i), X_i \tag{2}$$

This condition is plausible in settings like ours, where we observe multi-period earnings histories that embed persistent determinants of earnings growth. If adjustment ability systematically af-

[9]This unobserved ability captures heterogeneity in workers' job search ability, flexibility in switching occupations or regions, access to liquidity that enables longer search spells, or other forms of luck or resilience that help a worker navigate the post-closure transition.

fected counterfactual earnings, it would appear in the pre-closure trajectory, which we explicitly match when constructing synthetic controls.

2.2 Constructing individual synthetic control estimates

We construct our estimator in two steps. First, for each displaced worker i, we construct an initial donor pool J_i of non-displaced workers who match exactly on gender, education, one-digit industry, and three-digit occupation.[10] To refine comparability, we compute the root-mean-squared difference (RMSD) between worker i's pre-closure earnings trajectory and the trajectories of all workers in the matched pool over the five years preceding closure. We retain the 20 donors with the smallest RMSD values.[11] This matching step limits extrapolation: an early-career manufacturing worker is never compared to a senior manager with similar earnings but entirely different career prospects.

Second, within this refined donor pool, we construct synthetic control weights w_{ij}^* by minimizing the discrepancy between the displaced worker and a convex combination of donors over the pre-closure period. Formally, the weights are chosen to minimize the squared differences in age, firm size in the fifth year before closure, and annual earnings over four years leading up to closure,[12] subject to the usual non-negativity and summation constraints.

For each displaced worker and each post-closure year t, the individual-level treatment effect estimate is

$$\hat{\tau}_i(A_i) = Y_i(1) - \sum_{j \in J_i} w_{ij}^* Y_j = \underbrace{Y_i(1) - Y_i(0)}_{\tau_i(A_i)} + \underbrace{\left[Y_i(0) - \sum_{j \in J_i} w_{ij}^* Y_j \right]}_{\text{Counterfactual error}} \tag{3}$$

2.3 Identification and aggregation

Individual-level synthetic controls rely on relatively few donors and are inherently noisy due to unrelated transitory shocks in donor earnings. Therefore, our synthetic control group ap-

[10]Section 3 describes the sample restrictions we apply to both displaced and donor workers, including requirements on the length of observed earnings histories and the headcount stability of the closing firm. Consistent with much of the displacement literature, we restrict donor workers to those who do not experience a firm closure during our observation window (2000-2005). By constructing donor pools composed only of workers who never experience a closure, we avoid complications that arise in two-way fixed-effects models with staggered treatment timing (e.g., Goodman-Bacon (2021); Roth et al. (2023)).

However, as emphasized by Krolikowski (2018), excluding workers who eventually experience displacement may introduce selection bias if the remaining donor workers are unusually stable. We eliminate the most severe version of this concern by removing the extremely small number of individuals who experience multiple closures within our window. Some residual bias may remain if our donor pools disproportionately draw on workers who happen not to experience displacement during the sample period. In practice, this source of bias appears limited: for a random sample of 500 displaced workers, we re-estimated donor pools while relaxing the restriction so that control workers may experience a closure in any year after the treated worker's displacement year. Few displaced workers' synthetic controls placed positive weight on future-displaced individuals, and the resulting differences in estimated displacement effects were approximately 1%.

[11]Results are robust to using 10 or 30 donors. In practice, the synthetic control weights place positive mass on far fewer than 20 donors.

[12]From five years pre-closure to the two years pre-closure. We omit the year before firm closure to account for any anticipation effects. The four-year pre-intervention period is chosen to balance estimation bias and sample restrictions, though our results remain consistent when using a longer period (e.g., 10 years) See F. Weights are also constrained to be positive and sum to one.

proach at the individual level does nor produce a consistent estimator $\hat{\tau}_{it}$ of the individual-level treatment effect.

Instead, we write:

$$\hat{\tau}_{it}(A_i) = \tau_{it}(A_i) + \eta_{it} \tag{4}$$

where η_{it} is a counterfactual estimation error. Following Abadie (2021), identification of average effects requires a mean-correctness condition: idiosyncratic donor-side noise averages out as we aggregate across many displaced workers. Under this assumption,

$$\frac{1}{n_1}\sum_{i=1}^{n_1} \hat{\tau}_{it}(A_i) \xrightarrow{p} \mathbb{E}[\tau_{it}] \tag{5}$$

providing a consistent estimate of the average treatment effect on the treated.

Crucially, the same logic holds when we average synthetic-control estimates within groups defined by the realized losses themselves. Let S_C denote the set of displaced workers whose estimated cumulative losses place them in percentile bin C of the loss distribution. Then the conditional average treatment effect for that percentile is

$$\hat{\tau}^C = \frac{1}{|S_C|}\sum_{i \in S_C} \hat{\tau}_i \tag{6}$$

Because workers in the same percentile bin have extremely similar pre-closure earnings trajectories—and because the mean-correctness argument applies within each sufficiently large stratum—$\hat{\tau}^C$ converges to the average treatment effect for workers whose estimated losses place them in percentile bin C. This procedure is closely related to the logic of distributional synthetic control methods (Gunsilius, 2023): once counterfactual trajectories are constructed using only pre-treatment information, heterogeneity can be analyzed across quantiles of the post-treatment outcome distribution. In other words, averaging across individuals, and across individuals within percentile strata, removes donor-specific noise under plausible mean-correctness conditions.

The empirical sections validate this logic through placebo tests, permutation exercises, and comparisons to event-study estimates. These checks demonstrate that the dispersion we uncover reflects genuine heterogeneity in earnings losses rather than artifacts of the synthetic control procedure.

3 Data

Our primary data come from the Integrated Employment Biographies (IEB) provided by the German Federal Employment Agency. The IEB includes comprehensive social security records for Germany from 1975 to 2022, covering all employees subject to social security contributions and recipients of unemployment benefits.[13] For each worker, the IEB records earnings, time spent in each job, and various demographic and job characteristics. Unique identifiers for individuals and establishments allow us to track workers and firms over time. We supplement the

[13] The data exclude students, military personnel, civil servants, self-employed workers, and individuals who entirely leave the labor market.

IEB data with the Establishment History Panel (BHP), which provides firm-level information such as size, median wages, and industry for firms with at least one socially insured worker as of June 30^{th} of each year.

Our analytical sample includes individuals who had at least one employment spell in the private sector in West Germany between 2000 and 2005.[14] During this period, Germany's economy experienced a major downturn and came to be regarded as the 'sick man of Europe.' The era was marked by high unemployment rates, and many firms went out of business. We follow these individuals throughout their entire careers, which may start before 2000 and extend beyond 2005. This longitudinal approach allows us to observe long-term trends and outcomes for workers affected by firm closures during this period.

Our treatment group consists of all workers who separated from closing firms between 2000 and 2005. Identifying firm closures is challenging due to the potential for misinterpreting simple changes in establishment identification numbers as closures.[15] To accurately identify genuine closures and exclude cases of mere administrative changes, we follow Hethey-Maier and Schmieder (2013) by considering a vanishing establishment identification number as a firm closure only if fewer than 30 percent of the workers from the closing firm transfer to the same subsequent establishment.

We restrict our analysis to closing firms that had at least 50 employees and did not experience employment fluctuations above 30 percent in the three years prior to closure. At the individual level, we include workers who were, at the time of closure, between the ages of 21 and 55, had at least two years of tenure with their firm, and had positive earnings for five years before the closure. We also include all workers who left their closing firms within two years prior to the closure to capture potential anticipation effects.[16] Some workers permanently leave the sample for reasons such as retirement, self-employment, or government employment. Following Schmieder et al. (2023) and Davis and von Wachter (2011), we retain these individuals in the sample with zero earnings.

Our primary labor market outcome is annual earnings, which is the sum of earnings from all employment spells within each year. We standardize earnings to 2010 Euros and remove the few observations with earnings below the social security thresholds, as these are likely to reflect data entry errors.

In addition to annual earnings, we can also estimate firm closure effects on wages. Daily wages are measured as of June 30^{th} each year to align the individual-level data from the IEB with the firm data from the BHP. However, daily wages are more volatile due to variations in working hours and bonuses, and we are unable to calculate hourly wages because we lack data on hours worked.

Earnings data are top-coded for approximately 10 percent of workers with earnings above the annual German social security contribution ceiling. To impute the missing upper tail of

[14] We exclude firms in agriculture and mining.

[15] While many previous studies examine displacements triggered by both firm closures and mass layoffs, our analysis focuses solely on layoffs resulting from firm closures. This restriction serves two purposes: first, it reduces the likelihood of mis-classifying internal workforce shifts within the same firm as layoffs and, second, it addresses concerns about potential adverse selection among workers laid off in partial layoffs.

[16] More than 90% of treated workers separate from the closing firm in the quarter before or quarter of the closure.

the earnings distribution, we use a two-stage stochastic imputation procedure to estimate the missing upper tail of the earnings distribution.[17]

In addition to earnings, we observe each worker's annual days of employment and unemployment, tenure with each firm, experience, gender, age, occupation (four-digit level), industry (three-digit level), and the location of work and residence (municipality level). The education variable, which is not required for administrative purposes, is sometimes missing or inconsistent. To address this issue, we follow the imputation procedure of Fitzenberger et al. (2006) to correct and impute missing values.[18]

Table 1 presents descriptive profiles of the displaced workers and the pool of non-displaced workers from which we construct the synthetic controls. The non-displaced workers are all German workers who meet the same firm size and tenure requirements as the displaced workers. Our sample includes $15,500$ displaced workers who lost their jobs due to firm closures between 2000 and 2005. The potential donor pool of non-displaced workers comprises more than $560,000$ individuals. Earnings and demographic characteristics for displaced workers are measured in the year prior to firm closure.

— Table 1 about here—

Differences between the displaced workers and the pool of comparable non-displaced workers primarily reflect differences in the types of firms that were more likely to close during this period — specifically, construction, retail, and manufacturing firms. Consequently, the displaced worker sample skews slightly more male, is more concentrated in these industries, and includes fewer workers from extremely large firms. Average earnings for displaced workers ($48,000$ Euros) are similar to those of non-displaced workers ($50,000$ Euros). However, displaced workers exhibit higher tenure at their pre-closure firms (6.6 years on average) compared to the non-displaced worker pool (3.6 years).

Mechanically, the synthetic control procedure weights workers in the non-displaced pool to match the characteristics of displaced workers. Our pre-matching on worker and firm characteristics ensures exact matches on gender, age, tenure, education, and industry; within these cells, the synthetic control weighting selects for each displaced worker the combination of non-displaced workers whose pre-closure earnings trajectories most closely resemble those of the displaced worker.

[17]Following Card et al. (2013), we first fit a series of Tobit models for each year and education group. We then calculate imputed values for each censored observation using the estimated parameters from these models and a random draw from the left-censored distribution. Control variables include gender, age, age squared, a dummy for older individuals, tenure, and tenure squared. A second round of imputations incorporates each worker's average log wage in all other periods and the average annual wage of their current co-workers (leave-out means). If a worker is observed only once, we set their mean wage to the sample mean and include a dummy variable in the subsequent estimation.

[18]We perform an imputation in the style of the IP1 procedure described in Fitzenberger et al. (2006). If an individual is observed in multiple parallel spells within the same period, we assign the highest education category observed. Since a worker's highest education cannot decline over time, we then carry forward their highest educational degree to all subsequent spells. For missing data, we backdate the degree to the typical age of attainment.

4 The distribution of displacement losses

By constructing a synthetic "twin" for each displaced worker, we can examine the distribution of estimated earnings losses for displaced workers relative to comparable non-displaced workers' earnings trajectories. We proceed in four steps. First, we illustrate the methodology by focusing on workers displaced from a single firm, providing a specific example of how we construct and interpret individual-level synthetic controls. Second, we estimate the overall distribution of earnings losses across the universe of closures in our sample and show that our method reproduces average earnings losses that closely match estimates from conventional event study approaches. Third, to provide details about the overall heterogeneity, we show the distinct earnings patterns for deciles of the distribution. Finally, we explore this heterogeneity in displacement losses by examining variation in outcomes across the different subgroups of workers that have been the focus of prior analyses.

Throughout these analyses, we normalize earnings losses relative to the average annual earnings a worker received in the three years prior to firm closure (years -3 to -1, with closure occurring at year 0). Consequently, closure effects are often expressed in "years of earnings" lost.[19]

4.1 Case study of a single HVAC firm

To illustrate and motivate our focus on the heterogeneity of outcomes for displaced workers, we present the estimated individual earnings losses of a single heating, ventilation, and air conditioning (HVAC) installation and repair firm that closed in 2000.[20] For this firm, 30 displaced workers met our criteria of having at least two years of tenure at displacement and positive earnings throughout the five years before leaving the closing firm. The firm employed 26 men and four women. 29 workers completed an apprenticeship degree and one held a university degree in the year before the firm closed. The majority (20 out of 30) held jobs in the occupation of sanitation, heating and air conditioning technology; three were office clerks; three were technical draftsmen; one was an accountant; one an electrical engineer; one a machine builder; and one a warehouse manager.

On average, these workers have estimated earnings losses of approximately $11,000$ Euros in the first year after the closure and their average earnings losses reached a maximum of approximately $18,000$ Euros in the third year after displacement (Figure 1), corresponding to approximately 38 percent of their pre-displacement earnings.

— Figure 1 about here—

However, focusing on average losses masks the substantial heterogeneity in individual outcomes. Figure 2 illustrates this heterogeneity by separately plotting the earnings losses of each of the 30 displaced workers relative to their synthetic controls. In each panel, the solid black line

[19] Notably, it is possible for a worker's annual earnings loss to exceed one year of pre-displacement earnings. For instance, if a worker experiences zero earnings in a given year while their counterfactual earnings would have grown, that single-year loss may exceed one full year of their baseline earnings.

[20] As mentioned previously, our synthetic control group approach does not produce consistent estimates on the individual level. Therefore, the exercise is primarily illustrative.

shows the displaced worker's earnings, while the dashed line shows the synthetic counterfactual earnings of their synthetic control. Although we do not claim to be able to consistently estimate individual earnings losses for each individual worker, the figures confirm a strong pre-closure fit between the synthetic control estimates and actual earnings. Post-closure, however, trends vary substantially across workers. Roughly one-third show immediate and sharp estimated losses. Earnings for some of these workers recover, while earnings losses for others grow over time. But a substantial fraction of workers recovers quickly after initial losses, and some workers exhibit earnings as high as—or even higher than—their synthetic controls.[21] These divergent patterns lead to stark differences in economic outcomes: some workers experience substantial earnings losses—up to a cumulative $50,000$ Euros in the years following closure, while many others experience no significant losses and continue to follow the trajectories of their synthetic controls.

This stark variation among observably similar workers underscores the substantial heterogeneity in earnings losses from firm closures. It also highlights a key limitation of focusing solely on average losses (Figure 1): such averages obscure the severe disruptions faced by some workers and the resilience or minimal impact experienced by others.

— Figure 2 about here—

4.2 The distribution of dynamic losses

The distribution of estimated economic losses experienced by the full sample of displaced workers is highly skewed. Figure 3 plots the loss distribution for all displaced workers during the period five years before and five years after their firm closes. The solid line in the figure represents the progression of average earnings losses of displaced workers over this period, while the red figures show the full distributions of losses in each year after closure.[22]

By construction, the synthetic controls match displaced workers closely in the pre-closure period on the moments used to construct the weights, so average pre-treatment differences are near zero. However, following displacement, average earnings losses rise to approximately 20 percent of the worker's average pre-displacement earnings. This result aligns with previous findings by Schmieder et al. (2023), who estimate short-term earnings losses ranging from 18 to 25 percent in Germany during the same period.[23]

— Figure 3 about here—

[21] Figure A.1 demonstrates that these observed earnings discontinuities stem from the firm closure rather than the synthetic control methodology. Following Abadie (2021), we run placebo tests comparing earnings losses for each control worker (the difference between solid and dashed lines in Figure A.1) to placebo losses for non-displaced individuals in each worker's donor pool. In virtually all cases, the observed post-closure earnings lie at the extreme tail of the placebo distributions, suggesting that the discontinuities are unlikely to be driven by the synthetic control procedure alone.

[22] Because individual synthetic control estimates are noisy, we interpret these figures as describing the distribution of estimated earnings impacts, and we rely on placebo and permutation exercises below (and in the Appendix) to assess the extent to which the observed dispersion reflects signal rather than counterfactual estimation error.

[23] While Schmieder et al. (2023) include both firm closures and mass layoffs in their analysis, our focus solely on firm closures likely accounts for the slightly larger estimated losses, as firm closures generally lead to larger earnings and wage reductions (e.g., Hijzen et al., 2010).

There is some distribution in the pre-treatment differences between displaced workers and their synthetic controls, as seen in the grey shaded distributions in the pre-closure periods. Achieving perfect balance in pre-trends for each displaced worker is not possible, but the distributions for all five pre-treatment periods are closely centered around zero. We cannot reject the null hypothesis of normality for any of them.

By contrast, post-closure earnings losses for displaced workers are not distributed normally. Each year's distribution is strongly left-skewed and bimodal, indicating that the modal loss of annual earnings is considerably smaller than the average loss. A substantial proportion of workers experience small earnings changes that are close to zero or even positive, while a smaller group suffers severe losses—particularly, but not only, as a result of displaced workers experiencing periods of zero earnings.[24]

In the Appendix, we conduct a battery of robustness exercises to demonstrate our results are consistent with genuine variation in outcomes rather than artifacts of measurement. These exercises include subsetting to workers with a narrower range of deviations in pre-layoff earnings between the treated worker and their synthetic control (Appendix C),[25] showing that the distributions of deviations between displaced workers and their synthetic controls for workers in the top and bottom earnings loss quartiles are statistically indistinguishable in the pre-treatment periods (Appendix E), repeating the analysis for a subset of workers whose synthetic control weights we estimate using 10 years of pre-layoff earnings data (Appendix F), and in a permutation exercise (Appendix G) using placebo estimates as suggested by Abadie et al. (2010).

Figure 4 further highlights the bimodality of the loss distribution by comparing the five-year cumulative earnings losses of displaced workers to a normal distribution. The distribution of cumulative earnings is markedly left-skewed (with skewness of -0.43). On average, displaced workers lose 1.26 years of earnings over the five years post-displacement. The modal loss (0.28 earnings years) is lower, both substantively and statistically.

— Figure 4 about here—

A non-negligible share of displaced workers earns more than their synthetic controls in the long run. Over the five years post-closure, nearly one-fifth of displaced workers ($3,631$ individuals) exhibit earnings gains relative to their non-displaced controls. While this result may seem counterintuitive, it is consistent with findings from the U.S. For example, Farber (2017) reports that 28 percent of full-time workers secured jobs with relatively higher earnings following a job displacement. Similarly, it is consistent with recent evidence from Germany that suggests workers may underestimate the returns to job mobility (Jäger et al., 2024).

One potential concern is that the shape of the loss distribution in Figure 4 may primarily reflect measurement error in our synthetic control group approach. However, permutation analysis indicates that the distributions are not simply the result of measurement error. We conduct a permutation exercise similar to a bootstrap approach where we re-estimate the earnings distribution using 200 ten-percent samples that mimic our synthetic controls. Given the nature of our synthetic control approach, the influence of random outliers in these small samples will

[24]See Appendix B for robustness excluding workers with zero earnings.

[25]The concern here being that displaced workers in the lower tail of the earnings loss distribution may have synthetic comparisons that are systematically biased in the opposite direction of those in the upper tail.

be substantially larger than in our main sample. Nevertheless, the distributions of estimates from these alternative control samples consistently produce a similar picture, with earnings loss distributions that are almost identically shaped (Appendix H).

4.3 Earnings patterns across deciles

Plots of earnings patterns by decile of cumulative losses highlight the heterogeneity of outcomes following displacement. Figure 5 divides the sample of displaced workers into deciles based on their cumulative five-year losses and plots the pattern of average annual earnings losses that underlies the overall distribution.[26] The solid line in each panel summarizes the average annual earnings for displaced workers in each decile, while the dashed line shows average earnings for the relevant synthetic controls.

Before firm closure, earnings levels and trajectories are similar across deciles of eventual earnings loss. However, following displacement, the figures show substantial and persistent divergence in earnings outcomes. Workers in the six deciles with greatest earnings losses experience large declines at the time of the firm closure and, on average, never return to their pre-closure earnings path. Conversely, the average earnings for workers in the top two deciles increase following the firm closure and remain persistently greater than their synthetic controls. For workers in the decile with the smallest earnings losses, average earnings five years post-closure are 20 percent greater than those of their synthetic controls.

—Figure 5 about here—

Section 5.1 shows that event-study estimates closely replicate the decile-specific patterns obtained using our synthetic control approach approach for virtually all deciles. The event studies also allow us to present confidence bands for classical inference.

4.4 Earnings loss heterogeneity by worker and firm characteristics

Our estimates are consistent with previous research showing that the average earnings losses from job displacement vary across workers of different age (e.g., Kletzer and Fairlie, 2003), education (e.g., Farber, 2017), gender (e.g., Illing et al., 2024), and firm size (e.g., Lachowska et al., 2020; Fackler et al., 2021). However, our estimates suggest a more nuanced interpretation. We consider the extent to which these observable worker and firm characteristics can explain not only the average losses but also the distribution of earnings losses among displaced workers. Our contribution here is to assess the extent to which observationally similar workers within the same occupation or firm exhibit similar earnings losses.

Figure 6 shows the heterogeneous losses of cumulative earnings (as a percent of pre-closure earnings) across three readily identified subgroups of displaced workers. Panel A plots the distributions by education level: high (university degree), medium (apprenticeship degree), and low (no formal education beyond a high school diploma). Panel B plots the distributions by

[26] Deciles are defined based on estimated cumulative earnings deviations relative to synthetic controls, rather than on latent treatment effects; accordingly, the figures summarize average earnings paths for workers ranked by realized estimated losses.

age, comparing younger workers (below 30) to older workers (above 45). Panel C plots the distributions for women and men.

For all subgroups, the average earnings losses, indicated by the vertical lines, align with the average losses documented in the literature. Specifically, we find that displaced workers with lower education levels experience substantially larger average earnings losses (1.9 years) than those who are medium (1.2 years) or highly educated (0.8 years). Older workers experience, on average, larger losses (1.5 years) than younger ones (1.1 years), and women experience larger losses (1.6 years) than men (1.1 years).

But conclusions about the incidence of differential losses must be tempered by the substantial overlap of losses across all subgroups that underscores the within-group heterogeneity. Even among women, low-educated workers, and older workers—who, on average, experience larger losses—a substantial proportion experiences only moderate losses or even gains following displacement.

Panel C illustrates this pattern most strikingly for women and men. Women lose, on average, approximately one-half year more of their pre-displacement earnings over the five years following firm closure compared to men. However, while the distribution of women's earnings losses is bimodal, with a second peak at approximately -4 years of loss, the distribution of men's earnings losses is also strongly left skewed. The bimodality in the distribution of women's losses is consistent with a relatively large minority of women withdrawing entirely from the labor market, earning nothing for much of the five years post-displacement. Yet, about 26 percent of men and 22 percent of women lose less than one month's worth of their pre-displacement earnings (spread across five years of post-displacement experience). In sum, the clear differences in average losses by education, age, and gender mask the heterogeneity of losses both within and across the loss distributions.

— Figure 6 about here—

The striking overlap in the distribution of earnings losses across subgroups suggests that observed pre-treatment characteristics have limited explanatory power. We assess this formally by decomposing the variance of the earnings losses. We begin by estimating a linear regression of the following form:

$$\hat{L}_i = X'_{i(-1)}\beta + \theta_{i(-1)} + \vartheta_{i(-1)} + r_{i(-1)} + u_{i(-1)} \tag{7}$$

where the dependent variable $\hat{L}_i = \sum_{t=1}^{t=5} \widehat{Loss_{it}}$ represents worker i's cumulative earnings losses over the five years following firm closure.[27] The vector $X'_{i(-1)}$ includes fixed worker characteristics such as education, a cubic function of age, gender, and citizenship. The terms $\theta_{i(-1)}$, $\vartheta_{i(-1)}$, and $r_{i(-1)}$ control for (closing) firm, three-digit occupation, and municipality, respectively. The error term is denoted by $u_{i(-1)}$. We then decompose the variance of the accumulated earnings losses as follows:

[27]Where $\widehat{Loss_{it}}$ is the difference in i's actual versus synthetic control earnings in year t as a percent of i's pre-closure earnings.

$$
\begin{aligned}
Var(\hat{L}_i) =& Var(X'_{i(-1)}\hat{\beta}) + Var(\hat{\theta}_{i(-1)}) + Var(\hat{\vartheta}_{i(-1)}) + Var(\hat{r}_{i(-1)}) + \\
& 2Cov(X'_{i(-1)}\hat{\beta}, \hat{\theta}_{i(-1)}) + ... + 2Cov(X'_{i(-1)}\hat{\beta}, \hat{r}_{i(-1)}) + Var(\hat{u}_{i(-1)})
\end{aligned} \tag{8}
$$

where the $Var(.)$ terms represent the variances of the outcomes and controls, the covariance terms capture all potential combinations, and $Var(\hat{u}_{i(-1)})$ is the variance of the error term.

Table 2 presents the decomposition results. While unsurprising given the distributional overlaps in Figure 6, observed fixed individual characteristics explain little of the variance in estimated displacement losses. The first column shows the variance decomposition for the entire sample, revealing that observed pre-displacement characteristics (e.g., education, gender, age, firm, and occupation fixed effects) explain 17 percent of the total variance in earnings losses. The remaining 83 percent of the variance cannot be explained by these features.

In this decomposition, the displaced worker's closing firm is the strongest predictor of earnings losses, followed by their pre-displacement occupation. Extending this variance decomposition to subgroups reveals substantial heterogeneity in the explanatory power of worker and firm characteristics.[28] For instance, pre-displacement firms explain a larger share of earnings losses for less-educated workers, whereas occupations play a greater role among highly educated workers. This pattern is consistent with firm-specific human capital playing a larger role for low-educated workers, while highly educated workers appear to rely more heavily on occupation-specific skills. Notably, observable worker attributes that typically influence wages—such as age, gender, education, and citizenship—explain little of the heterogeneity in displacement outcomes. Thus, this analysis underscores the importance of unobserved factors or idiosyncratic shocks shaping post-displacement earnings trajectories.

— Table 2 about here—

On methodological grounds, however, these results might be simply driven by noise in our synthetic control group estimates of counterfactual earnings. To provide evidence against this concern, the second column of Table 2 decomposes the variance in counterfactual earnings for the synthetic control workers. In other words, instead of using the estimated earnings losses as a dependent variable, we only use the earnings of the synthetic controls as the dependent variable. If the counterfactual earnings were driven by random noise, observable pre-treatment characteristics should explain little of their variance. Yet, the observable characteristics account for approximately 70 percent of the variance in counterfactual earnings—much more than in the estimated displacement losses, indicating that most of the variance in earnings losses is attributable to differences in individual post-displacement career paths rather than noise in our synthetic control group estimates.

This decomposition suggests that many factors not observable to researchers or policymakers—such as minor ability differences, family-related factors, or pure luck—influence the degree to which a worker's labor market activities are disrupted by firm closures.

[28] See Table H.1 for the decomposition by subgroup.

5 Alternative estimation approaches

The established approach to estimating losses from worker displacements has been event studies that compare average earnings of each displaced worker to those of a comparison group that suffered no displacement. We reproduce this approach with the comparison group created by our synthetic controls and extend this standard estimation to describe the loss patterns at different points of the estimated distribution—something made possible by our estimation of the entire distribution of losses.

A more recent approach applies machine learning techniques to estimate heterogeneous treatment effects based on high-dimensional interactions of observable characteristics of displaced workers. We reproduce this approach and show that it produces substantially less variance in estimated losses than our synthetic control-based approach.

5.1 Synthetic controls reproduce estimates using conventional event study approaches

Although our approach to estimating earnings losses from firm closures departs from the standard methods used in the literature, we can show that the synthetic control approach produces estimates of *average* earnings losses consistent with conventional methods. Specifically, we demonstrate that the synthetic control-based estimates align closely with those traditionally documented in the event study literature.

In order to compare our synthetic control estimates to those from a standard event study, we use Mahalanobis distance matching to pair each displaced worker in the treatment group with one non-displaced worker in the control group, drawn from the set of all never-displaced workers.[29] We match on the same baseline variables employed in the synthetic control approach (i.e., age, gender, education, three-digit occupation, one-digit industry, and firm size). As before, the matched non-displaced workers were employed at large firms with more than 50 employees and had at least two years of tenure before their displacement year.

We follow Schmieder et al. (2023) and run the following event studies for each displacement year between 2000 and 2005 separately and aggregate the coefficient estimates using the inverse of the standard errors from each separate regression as weights.

$$Y_{it} = \alpha + \lambda_t + \kappa \mathbb{I}(\text{displaced}_i) + \sum_{k=-5}^{5} \delta_k \mathbb{I}(\text{displaced}_i) + X_{it}\beta + \epsilon_{it} \tag{9}$$

In this equation, Y_{it} is annual earnings; λ_t are calendar year fixed effects; δ terms capture earnings trajectories from five years before until five years after firm closure; $\mathbb{I}(\text{displaced}_i)$ is an indicator of whether the individual is displaced or not; X_{it} includes a set of control variables; and ϵ_{it} is the error term. By considering separate event studies for each displacement year and focusing only on workers never treated as the control group, we avoid common issues found in two-way fixed effects models with multiple treatment times that rely on "not yet treated" units as controls (Goodman-Bacon, 2021; Schmieder et al., 2023).

— Figure 7 about here—

[29]See Section 3 for detail on sample restrictions used to align the control and displaced worker groups.

By using our information about where each displaced worker falls in the distribution of losses, we can expand on standard event studies by looking not only at average losses but also at losses at different deciles of the estimated loss distribution (Figure 7). The first panel compares the average effects including all displaced workers, allowing a direct comparison of average earnings losses between the event study approach and our individual-level synthetic control method. For the remaining panels, based on our prior synthetic control estimates, we restrict the sample to the deciles of workers with the largest and smallest estimated losses. Within each decile, we again run event study regressions on those workers and their matched controls, enabling a comparison of the two methods across different segments of the estimated loss distribution.[30]

The figure reveals a striking similarity between the two sets of estimates for the full sample and for a majority of estimated earnings losses across the separate deciles. For the smallest losses in the 9^{th} and 10^{th} decile (based on the synthetic control estimates), the synthetic control group approach produces slightly larger gains than the event study approach. The shaded areas represent the 95 percent confidence bands for the event study. For all deciles except the eighth and ninth, our synthetic control estimates are below (or, for the tenth decile, above) the confidence band for the event study estimates. However, it is a priori not obvious whether estimates from either of the methods are more biased at the upper tail of the distribution. For example, Arkhangelsky and Hirshberg (2023) show that the synthetic control group approach is even less biased than regular difference-in-differences estimators under many circumstances.

5.2 Heterogeneous treatment with machine learning methods

A recent literature uses machine learning techniques to estimate heterogeneous treatment effects based on observed characteristics. Gulyas et al. (2021) and Athey et al. (2023) employ Generalized Random Forest (GRF) models to study the earnings impacts of displacement in Austria and Sweden, respectively. These papers estimate heterogeneous conditional average treatment effects (CATEs)[31], capturing high-dimensional interactions among workers' pre-treatment characteristics.

The GRF approach has two notable advantages over standard approaches. First, it can estimate heterogeneous treatment effects for more finely defined subgroups than those we examine here. In principle, grouping individuals who are similar on certain fixed person- and firm-level characteristics that predict displacement losses can approximate aspects of the loss distribution when observable characteristics explain a substantial share of outcome variation. Second, by splitting the sample according to high-dimensional combinations of observable features, the method can reveal which covariates best explain heterogeneity in post-displacement earnings outcomes.

However, these advantages strongly depend on the number of observable pre-treatment

[30] Note that these deciles are based on the estimated distribution of losses from the synthetic control approach. Thus, the results should not be interpreted as quantile regression estimates, but rather as the average losses for workers falling into the lowest or highest quartile of the synthetic control-estimated loss distribution.

[31] The Generalized Random Forest algorithm partitions workers non-parametrically by recursively splitting the covariate space in a way that maximizes heterogeneity in earnings losses across sub-groups. Within each resulting partition, it estimates local treatment effects, yielding conditional average treatment effects. In practice, GRFs generate multiple treatment effect estimates by repeatedly applying the algorithm to bootstrap samples of the data, and then aggregate these estimates to produce the final CATEs.

characteristics and the extent to which these factors explain the heterogeneity in outcomes. If there are few pre-treatment characteristics with low explanatory power, GRF may produce substantially less dispersion in estimated effects and overstate the importance of observable worker and firm characteristics in determining heterogeneity in outcomes.

We can directly compare our synthetic control group approach to the GRF approach. Following (Athey et al., 2023), we first use propensity score matching to pair each displaced worker with three never-displaced workers, matching on demographic characteristics, firm attributes, and the level and trend of pre-closure earnings. Second, we estimate CATEs via a GRF, where the outcome is the ratio of actual earnings one year after closure to earnings one year before closure.[32][33]

Figure 8 evaluates whether GRF-predicted CATEs meaningfully organize realized earnings losses as measured by the synthetic control approach. We sort workers into deciles based on their predicted CATEs and plot, within each decile, the mean and interquartile range of synthetic control-estimated dynamic earnings losses. If observable characteristics were highly predictive of treatment-effect heterogeneity, we would expect sharp monotonic separation in mean losses across CATE deciles and limited dispersion within each bin—particularly in the first year post-closure, the year used to train the GRF.

—Figure 8 about here—

While the CATE bins exhibit a general increase in mean first-year losses, the between-decile differences are modest relative to the substantial dispersion that persists within each decile. For example, the quartile with smallest first-year losses (25th percentile) in the CATE decile predicted to experience the largest losses experiences smaller losses than the mean first-year loss in the 9th CATE decile.[34] In other words, many workers predicted by the GRF to experience the most severe losses in fact experience losses comparable to—or smaller than—the average worker in higher-ranked deciles. At the same time, large dispersion remains within each decile, indicating that workers with similar predicted CATEs frequently experience markedly different realized earnings losses.

This pattern is consistent with the limited explanatory power of observed worker and firm characteristics documented in Section 4.4. Because observable characteristics explain only a small fraction of the total variation in losses, grouping workers by predicted CATEs necessarily compresses between-group variation while leaving substantial within-group heterogeneity.

[32]We include gender, education, industry, nationality, years of work experience, years of tenure at closing firm, AKM firm effect, level of earnings in the year before closure, trend in earnings in the years before closure. Our feature set is more limited than in Gulyas et al. (2021) or Athey et al. (2023), but it includes the main variables driving the variation in their analyses. We implement GRF following the procedure outlined in Athey et al. (2023). For each displaced worker, we pick three never-displaced workers from the pool of all never-displaced workers using a nearest-neighbor matching in the year before the worker is displaced. We assign control workers a placebo "treatment" year based on this matching. We implement the causal forest using the grf package in R (Tibshirani et al., 2024).

[33]We use this outcome, rather than our cumulative loss measure, to align with the loss measures used in the literature.

[34]Extending the same CATE-based bins to the full post-displacement horizon yields a similar conclusion. Figure H.2 plots dynamic syntehtic control estimated losses binned by CATE. Differences in mean losses across CATE deciles remain modest over time, while substantial within-decile dispersion persists throughout the five-year post-closure period. Thus, even though the GRF is trained on first-year outcomes, the limited sorting power of observables is not confined to the initial year but characterizes longer-run adjustment dynamics as well.

Taken together, our results suggest when observed characteristics account for little of the variation in losses, GRF produces substantially less dispersion in estimated effects, compressing variation at both tails of the distribution relative to the synthetic control–based estimates. By contrast, the synthetic control approach does not rely on observable characteristics to structure inference, allowing it to capture a broader range of losses. The synthetic control method provides a more complete picture of the distribution of estimated displacement losses, but it is not suitable for making out-of-sample loss predictions.

6 Adjusters and casualties

It is informative to shift our focus from pre-closure characteristics to the post-closure dynamics related to recovery from displacement. To sharpen the focus on the heterogeneity of earnings losses, we contrast the post-displacement labor market trajectories of individuals in the top quartile of losses ("casualties") with those in the bottom quartile of losses ("adjusters"). The goal here is strictly descriptive: to document how earnings losses co-vary with post-displacement labor market trajectories, leaving further exploration of causal drivers for future work. Throughout this section, differences between adjusters and casualties should be interpreted as associations between earnings outcomes and observed post-displacement trajectories, not as evidence on causal mechanisms.

6.1 Labor market trajectories of adjusters and casualties

The previous analysis showed small differences in average losses by demographic characteristics and firm attributes, and unsurprisingly these carry through to small differences between adjusters and casualties. These differences, however, are not the driving force behind the enormous aggregate differences in displacement losses. Adjusters are more likely to be male, slightly older, and possess higher levels of education (Table 3). They are also displaced from slightly larger and higher-paying firms.

Consistent with Schwerdt (2011), adjusters tend to leave their firms slightly earlier than casualties, although differences across groups are small. Most workers in both groups exit within the quarter of the closure—70 percent of adjusters and 73 percent of casualties. However, 23 percent of adjusters leave one quarter earlier compared to 20 percent of casualties, and fewer than 6 percent in either group depart three quarters before closure.

—Table 3 about here—

Importantly, we see very different recovery patterns that hold across demographic groups. Earnings losses following a job loss can stem from a variety of factors: taking a lower-wage job, experiencing unemployment, or working fewer hours. Conversely, some workers exhibit post-displacement earnings that match or even exceed their synthetic controls. Differences in the recovery paths of adjusters and casualties become apparent by the first year post-closure. Table 4 compares the wage[35] and employment trajectories of adjusters and casualties relative to their

[35] The IEB data record worker wages as of June 30th each year. Wages are missing for workers not employed on that date. We exclude a small number of observations (less than 0.5 percent of casualties and up to 4 percent of

synthetic control. Adjusters swiftly enter jobs with wages comparable to or higher than their counterfactual wages. Within a year, over 60 percent earn higher wages, and nearly 75 percent are employed full-time. By year five, nearly all adjusters have returned to full-time work, with over 80 percent earning wages in excess of their synthetic controls. These outcomes align with Figure 5, which shows that workers in the lowest loss deciles exhibit post-displacement earnings paths that exceed their synthetic controls.[36]

—Table 4 about here—

In contrast, casualties exhibit prolonged periods of low earnings and non-employment, illustrated in the right panel of Table 4. During the first four years post-closure, 30-40 percent remain fully unemployed. By year five, a quarter are still out of gainful employment, and another quarter are not employed full-time. Among those re-employed, wages frequently fall substantially below their synthetic controls.

This result is in line with Fallick et al. (2025), who find that earnings losses after job displacement are strongly mediated by long joblessness spells. However, joblessness does not explain the entire picture in our case. Notably, casualties, defined by cumulative five-year losses, make up three-quarters of the workers in the quartile of all workers with the largest earnings losses in the fifth year post-closure. Only 7 percent of casualties achieve wages comparable to those in their pre-closure firm. This pattern indicates that low earnings among casualties are not driven solely by temporary non-employment, but also by persistently lower earnings upon re-employment.

6.2 The role of firm transitions

Previous research has highlighted that establishment effects account for a substantial portion of displaced workers' average wage losses. For example, Schmieder et al. (2023) found that establishment effects explain nearly half of the negative wage impact on reemployment wages.

Figure 9 assesses the explanatory power of establishment effects in explaining wage losses at the extremes of the earnings loss distribution. The figure plots displacement-related losses in wages and establishment fixed effects. To estimate persistent differences in employer-specific daily wages, we apply the Abowd et al. (1999) (hereafter, "AKM") model, following the implementation of Card et al. (2013) for Germany. Using synthetic control weights from our earnings analysis, we construct a counterfactual path of AKM effects for each displaced worker.

The figure compares wage and AKM losses for all displaced workers (Panel A) and separately for adjusters and casualties (Panels B and C). Across all workers, we estimate persistent wage decreases of approximately 20 percent and decreases in firm AKM effects of approximately 8 percent. Thus, moving to firms with lower AKM account for about 40 percent of the average wage losses among displaced workers.[37]

adjusters) where workers are recorded as being employed for the full year but have missing wages. We calculate a counterfactual wage by applying the synthetic control weights from our primary approach for annual labor earnings to the daily wages of workers in the donor pool.

[36]Such a result is consistent with work by Farber (2017).

[37]This result aligns qualitatively with Schmieder et al. (2023) for the period after 2001 but indicates slightly larger wage losses compared to Schmieder et al. (2010). One possible explanation is that our analysis focuses

For adjusters and casualties, however, the association between differences in firm AKM and wage changes is more nuanced. Adjusters, on average, do not move to firms with markedly different AKM compared to their previous employers. Consequently, AKM differences explain less than 10 percent of the average wage increases for adjusters. Adjusters transition into roles with higher earnings relative to their synthetic controls, often at firms with comparable AKM effects.

In contrast, casualties experience a 20 percent decrease in firm AKM effects between their closed and post-layoff firms. However, their substantially larger wage losses—approximately 60 percent—indicate that casualties transition to firms with substantially lower AKM effects and also earn lower wages relative to other workers at those firms.

—Figure 9 about here—

6.3 Differences in adjustment behavior

Beyond firm effects, we consider differences in ex-post margins of adjustment between adjusters and casualties. While necessarily descriptive, the differences between adjusters and casualties in transitions across occupations, industries, firms, and labor market regions are nevertheless informative for understanding heterogeneity in outcomes.

Adjusters and casualties generally make a similar number of transitions, but adjusters exhibit faster transitions into new employment, while casualties spend longer periods out of employment. Figure 10 illustrates the labor mobility patterns of casualties and adjusters over time. In each panel, the solid line indicates the fraction of adjusters who make a given transition (e.g., firm switch) between any consecutive years $t-1$ and t, while the dashed line shows the fraction of firm switchers among casualties. We do not count switches into unemployment as switches; however, workers who become non- or unemployed are coded as switchers upon re-entering the labor market in a different firm.

Necessarily, nearly all adjusters have an immediate firm change upon displacement, but the fraction of adjusters switching firms drops to less than 10 percent after the first year, suggesting they quickly secure stable matches. Among casualties, only about 40 percent switch firms immediately after displacement, with the rest remaining non- or unemployed. As casualties gradually reenter employment, their firm-switching rates remain elevated compared to adjusters. Interestingly, by the end of the five-year period post-closure, adjusters and casualties have made a similar cumulative number of firm switches.

—Figure 10 about here—

On average, both adjusters and casualties switch industries more than once in the long run, but their short-term dynamics differ significantly (upper-right panel of Figure 10). Over 60 percent of adjusters switch industries immediately after displacement, compared to only 30 percent of casualties. This disparity is partly due to the fact that only 40 percent of casualties manage to reenter employment in the first year. Among employed casualties, however, three-quarters switch industries. Industrial mobility remains elevated for casualties in the long run.

solely on firm closures, whereas previous literature, which includes mass layoffs, has found smaller effects for layoffs relative to firm closures (Hijzen et al., 2010).

Short-term occupational mobility is substantial for both groups, with approximately 40 percent of adjusters and 30 percent of casualties switching occupations immediately after displacement (lower-left panel). After the first year, adjusters exhibit much greater occupational stability, while casualties continue switching occupations. Over the long run, casualties switch occupations an average of 1.35 times, compared to only 0.85 times for adjusters. This higher occupational switching among casualties suggests they may lose more of the returns on their occupation-specific human capital (e.g., Gathmann and Schönberg, 2010). Conversely, adjusters demonstrate substantial flexibility immediately after displacement, coinciding with earnings paths that recover more quickly.

Geographic mobility across 50 large German local labor markets also differs (lower-right panel). Although these distinctive regions are relatively large, we observe that quite a large fraction of both adjusters and casualties relocates to another labor market region. Moreover, adjusters appear to be more flexible than casualties, as they are more likely to quickly find employment in other regions.

In sum, Figure 10 shows that adjusters demonstrate substantial flexibility in the short run, while casualties exhibit lower re-employment rates in the short run and follow unstable adjustment patterns in the long run.

6.4 Exactly matched sample

Several demographic and background differences may contribute to these divergent adjustment patterns. Casualties, for example, are less educated and slightly older than adjusters, factors that may limit their flexibility (see Table 3). We can further refine the comparisons by analyzing whether workers with identical pre-treatment characteristics exhibit different post-displacement earnings and mobility trajectories following the same firm closure. Specifically, we perform an exact match between adjusters and casualties, selecting statistical twins displaced from the same firm, working in the same 3-digit occupation, sharing the same gender, and belonging to the same age category prior to displacement. Although this exact matching leaves us with only 855 individuals in each group, it allows us to isolate differences in adjustment behavior among workers in nearly identical circumstances.

When we reproduce the prior comparisons of mobility patterns using the matched sample (Figure 11), the results are virtually unchanged. Adjusters transition more quickly into their next labor market position, whereas casualties are slower and more prone to ineffective transitions.

—Figure 11 about here—

Taken together, these comparisons highlight the substantial variation in earnings losses and labor market trajectories for displaced workers. While many workers experience large and persistent losses, nearly a quarter exhibit faster transitions into stable employment that may even place them on a higher earnings path than if they had remained at their struggling firm. Although establishment effects account for a substantial portion of wage losses—particularly for casualties—a larger portion of these losses cannot be explained by fixed worker characteristics or establishment-switching effects. Adjusters exhibit greater flexibility through earlier firm and occupation switching. While these transitions may be costly in the short term, they are associated with more stable observed employment and higher earnings in the long run.

7 Trade exposure and other margins of adjustment

Public discussions frequently point to other factors as contributing to relative losses, including differences in earnings losses for workers exposed to trade shocks and the potential for education updating to mitigate losses. In all cases, the differences we detect are quite small, suggesting that none of these channels are primary sources of heterogeneity in displaced worker earnings losses and post-closure career outcomes. Figures and tables to support these analyses are available in the Appendix.

7.1 Heterogeneity by trade exposure

Previous research has shown that workers displaced due to competition from trade experience more severe consequences compared to those displaced for other reasons (Autor et al. (2016)). The underlying logic is that when a single firm in an unaffected industry faces a shock, workers may still find opportunities in the same occupation or industry at other firms that are not affected. In contrast, a trade shock impacts an entire industry, reducing the available options for workers when their firm shuts down.

We categorize displaced workers based on the level of trade exposure faced by their closing firms. We construct the trade exposure measure at the industry-by-region level. Following Eggenberger et al. (2022), we first measure industry exposure to trade competition at the one-digit level and then scale these estimates by the share of workers employed in that industry within the region where the firm is located. Using this measure of trade exposure, we divide manufacturing workers into two groups according to their exposure to trade shocks during the period. Workers from firms with negative trade exposure were employed in industries where Germany became a net importer, while workers from firms with positive trade exposure were employed in industries where Germany became a net exporter.

Consistent with the literature, we observe that average earnings losses are smaller for workers in positively as compared to negatively trade-exposed industries (Appendix Figure H.3). However, we also find substantial overlap in the earnings loss distributions across these groups. Furthermore, in our variance decomposition, trade exposure is absorbed by the displacing firm, indicating that trade exposure is not a primary driver of earnings losses.

7.2 Education updating

One potential explanation for the relative success of adjusters compared to casualties would be a greater willingness to acquire additional human capital by returning to school or training after a layoff. Losing one's job reduces the opportunity cost of pursuing further education or training in a new field.

We estimate a generalized difference-in-differences regression model of the form:

$$E_{i,t} = \alpha + \beta \text{post}_t + \kappa(\text{adjuster}_i * \text{post}_t) + \epsilon_{i,t}$$

where $E_{i,t}$ is an indicator for whether individual i's education level is higher than the level recorded at the time the individual was laid off. The interaction term captures whether adjusters are more likely than casualties to experience an educational update after the layoff. This allows us to directly assess whether the likelihood of returning to school or acquiring new skills differs between the two groups.

A limitation of our data is that educational attainment—other than apprenticeship training—is more prone to measurement error than other variables. Unlike apprenticeship programs, which are tied to specific rules for social security contributions, social security payments and unemployment benefits are not linked to a worker's level of education. As a result, it is unlikely that we accurately observe educational updates during employment spells, making it difficult to accurately measure the timing of these updates. Therefore, for this analysis, we test only whether adjusters and casualties differ in the likelihood that the worker's level of education five years after firm closure is greater than in the year before they were displaced from the closing firm.

We record an educational update when an individual with a high school diploma or less (low-educated) obtains vocational training or a university degree, or when an individual with vocational training (medium-educated) attains a university degree. We estimate the likelihood of increasing educational attainment separately for low- and medium-educated workers, and we report these estimates for all adjusters/casualties as well as for our "matched" sample of adjusters and casualties (workers from the same firm that share the same occupation, gender, age, and education level).

Our estimates indicate that very little educational updating occurs following displacement, and that adjusters are no more likely than casualties to pursue additional education (Appendix Table H.2). If anything, the estimates suggest that adjusters may be less likely to update their education. These results hold whether we consider all displaced workers or restrict comparisons to the matched sample. The estimates are precise but extremely small in magnitude. Our findings align with those of Minaya et al. (2023), who estimate that less than 2 percent of displaced workers in the US enroll in community college after a mass layoff.

8 Conclusion

Earnings losses from firm closures are unevenly distributed across displaced workers. This paper exploits administrative data on the universe of firm closures in Germany between 2000 and 2005. To construct the full distribution of estimated earnings losses across individuals, we employ a novel approach that constructs a synthetic control worker for each individual worker displaced by a firm closure.

The distribution of estimated earnings losses implies that average earnings losses, as commonly estimated using classical event studies, are not representative of the earnings impacts experienced by the majority of displaced workers. At the same time, the averages miss the extent of loss for the minority of workers who experience extremely large and persistent earnings losses from firm closures.

From our estimates of individual-specific losses from firm closures, we can reproduce the past average loss patterns across demographic and firm characteristics. But these average differences conceal dramatically different loss patterns for individuals with identical observed characteristics. Worker and firm characteristics commonly observable to the researcher explain only a small fraction of the workers' displacement losses.

Examining post-displacement labor market trajectories for workers with small versus large estimated earnings losses reveals stark differences in observed adjustment paths. Workers with smaller losses tend to transition more rapidly across firms, occupations, industries, and regions, while workers with larger losses experience longer periods of non-employment and greater instability in subsequent job matches. These patterns highlight substantial heterogeneity in post-closure outcomes that is not well explained by observable worker or firm characteristics.

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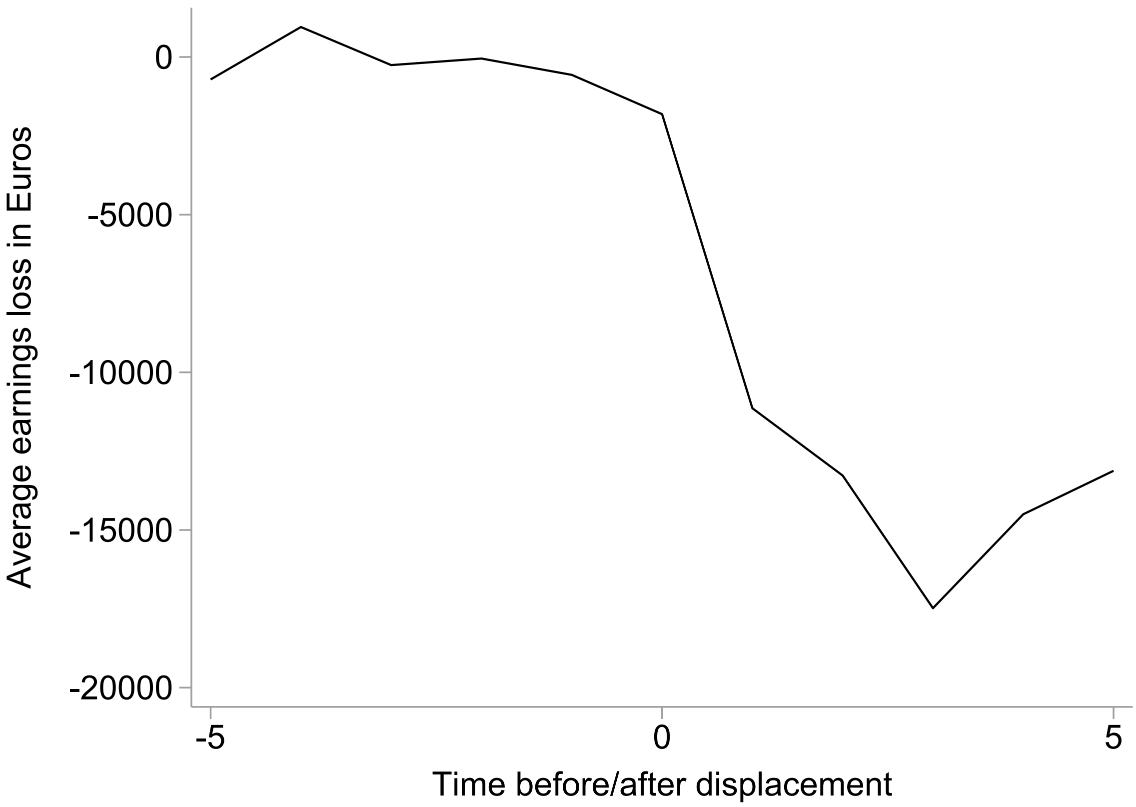

Figure 1: Average earnings loss for employees of a closing firm (case study)

Notes: The figure displays the average earnings losses of the displaced workers of one single closing firm that specialized in HVAC installation and repair. The y-axis measures the earnings losses in 2010 Euros. The x-axis displays the time before/after the firm closure in years. Source: IEB.

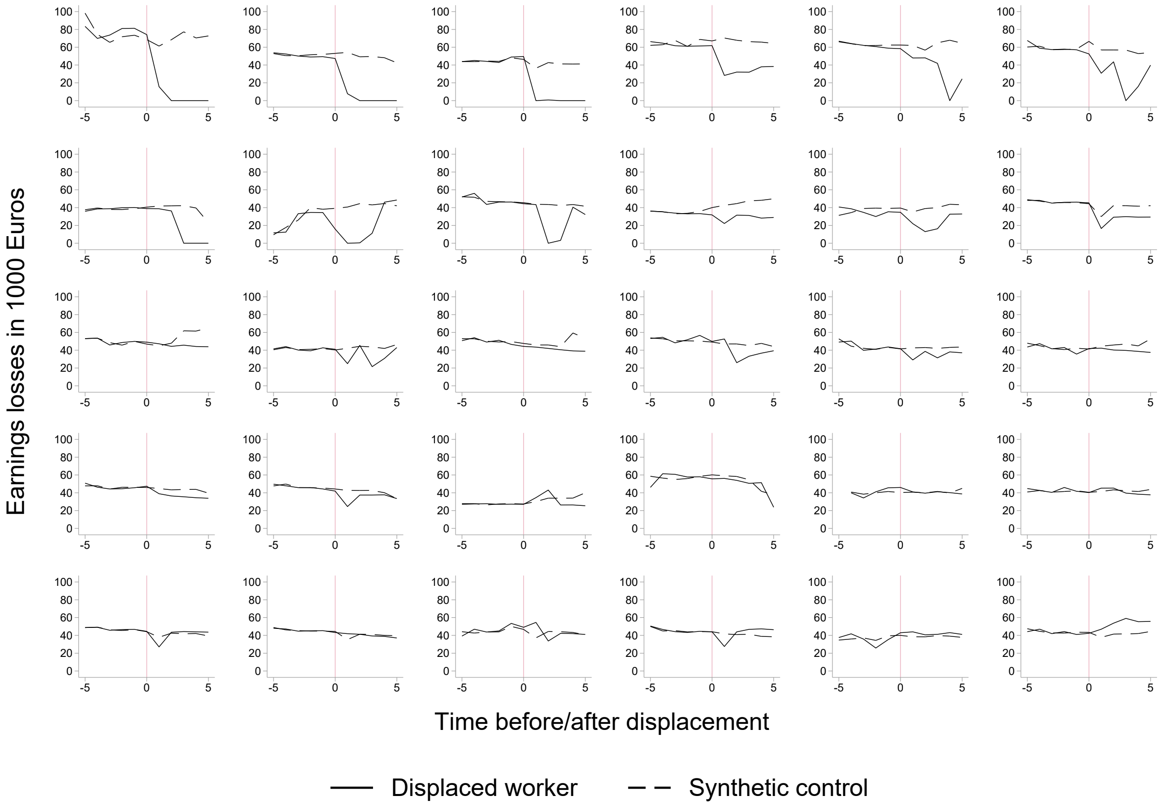

Figure 2: Individual earnings loss for employees of a closing firm (case study)

Notes: The figure plots the estimated earnings losses of 30 workers displaced from a single closing firm that specialized in HVAC installation and repair. In each panel, the solid black line plots the earnings losses (in thousands of 2010 Euros) of a single displaced worker relative to their synthetic control (dashed line). The y-axis measures the earnings in 2010 Euros. The x-axis displays the time before/after the displacement in years. Sub-figures are ordered by the magnitude of the worker's cumulative earnings loss. Source: IEB.

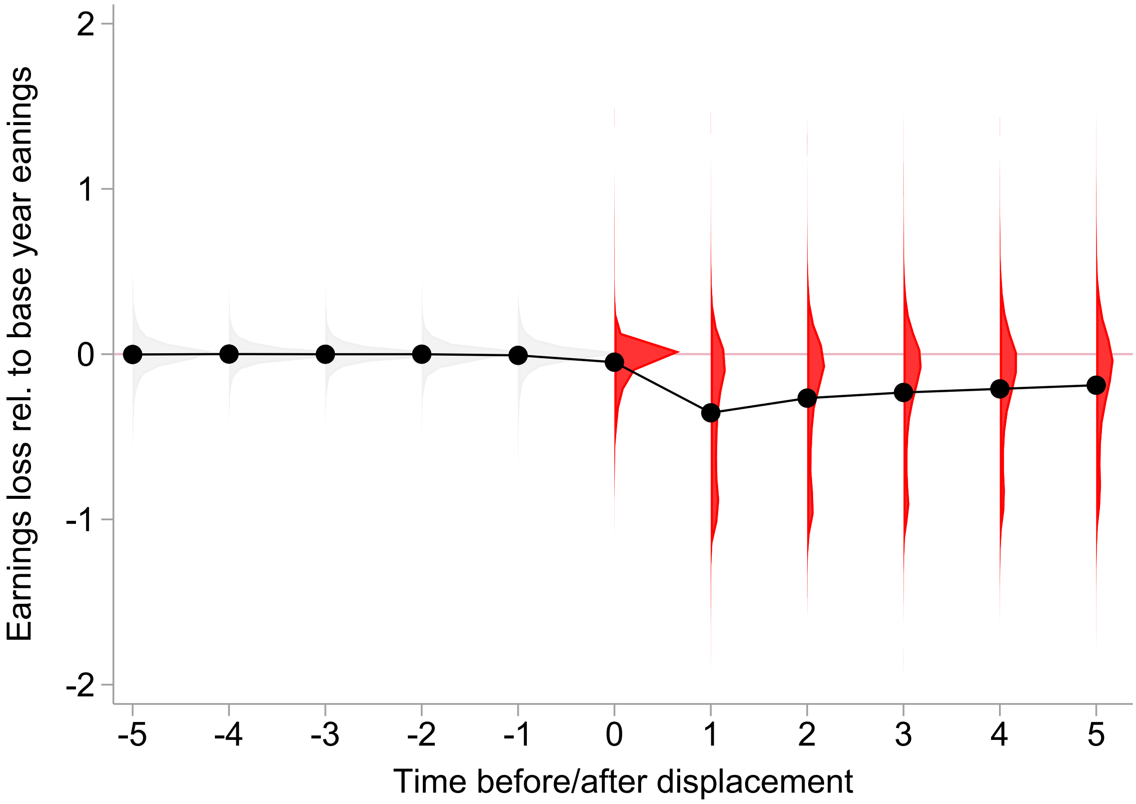

Figure 3: Distribution of relative earnings losses after firm closure (unconditional)

Notes: The figure displays the distribution of displaced workers' earnings losses throughout a period of five years before until five years after a firm closure. The earnings losses are measured relative to the individual worker's average earnings in the three years before the displacement. The dots represent the mean earnings losses for each period. The shaded areas represent the distribution of the displaced workers' earnings loss estimates. To plot the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. Second, we use an Epanechnikov kernel to plot the distribution of earnings losses from the individual earnings losses in each period. Source: IEB.

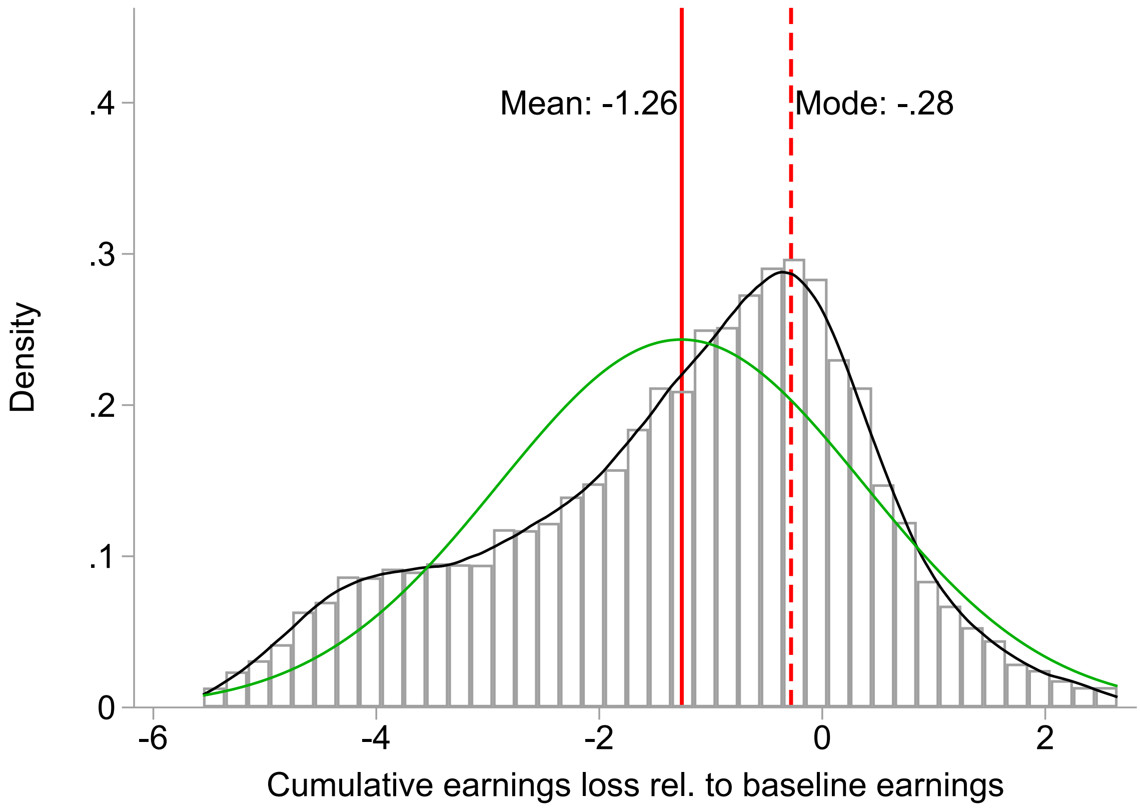

Figure 4: Distribution of five-year cumulative earnings loss relative to normal distribution

Notes: The figure plots the distribution of displaced workers' unconditional cumulative earnings losses over the five-year period after firm closure. Earnings losses are measured as the sum of the difference in actual and synthetic control earnings in the five years after firm closure, normalized by the displaced worker's baseline earnings. To plot the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. Second, we use an Epanechnikov kernel to plot the distribution of earnings losses from the individual earnings losses in each period. The solid line plots the mean of the distribution; the dashed line plots the mode. Source: IEB.

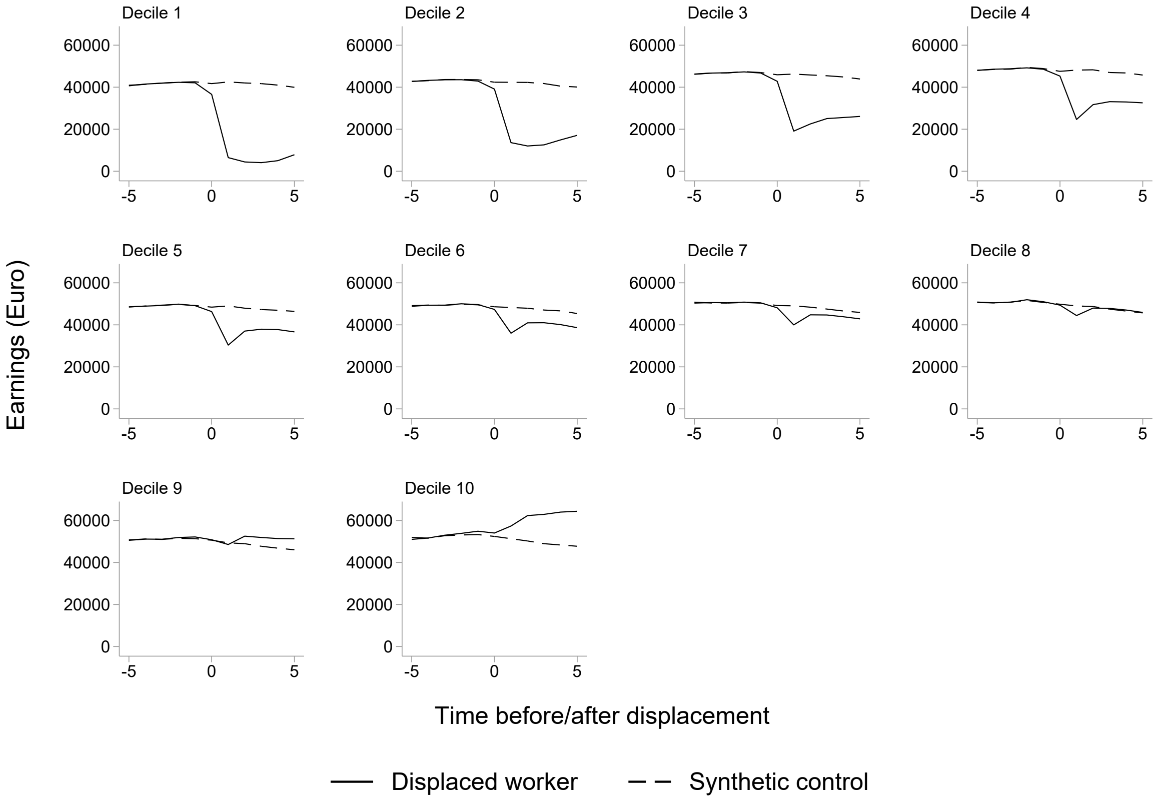

Figure 5: Average earnings loss for displaced and synthetic control group at deciles of the cumulative loss distribution

Notes: The figure plots average annual earnings for displaced workers (solid line) and their synthetic controls (dashed line), binned by decile of cumulative earnings loss across the five years post-closure. The upper-left panel plots the respective earnings trends for displaced workers and their corresponding synthetic controls in the decile with largest cumulative losses, the second panel plots earnings for the decile with second-largest cumulative losses, etc. 95 percent confidence intervals around the averages are shaded.

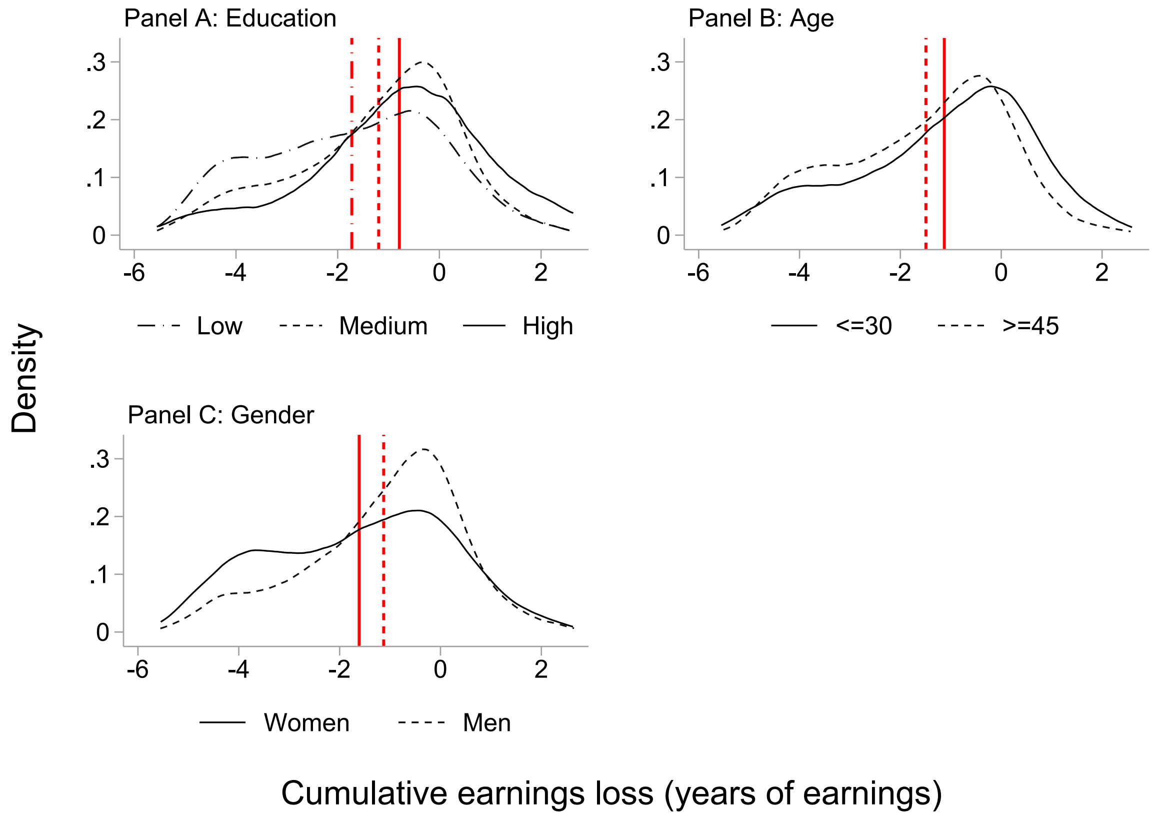

Figure 6: Distributions of earnings losses by pre-treatment characteristics

Notes: The figure displays the distributions of displaced workers' cumulative earnings losses over the five-year period after firm closure, split by different worker characteristics (measured one year before the displacement). Panel A shows separate distributions by education, Panel B by age, and Panel C by gender. To plot the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. Second, we use an Epanechnikov kernel to plot the distribution of earnings losses from the individual earnings losses in each period. The red lines represent the mean earnings in each cell. Source: IEB.

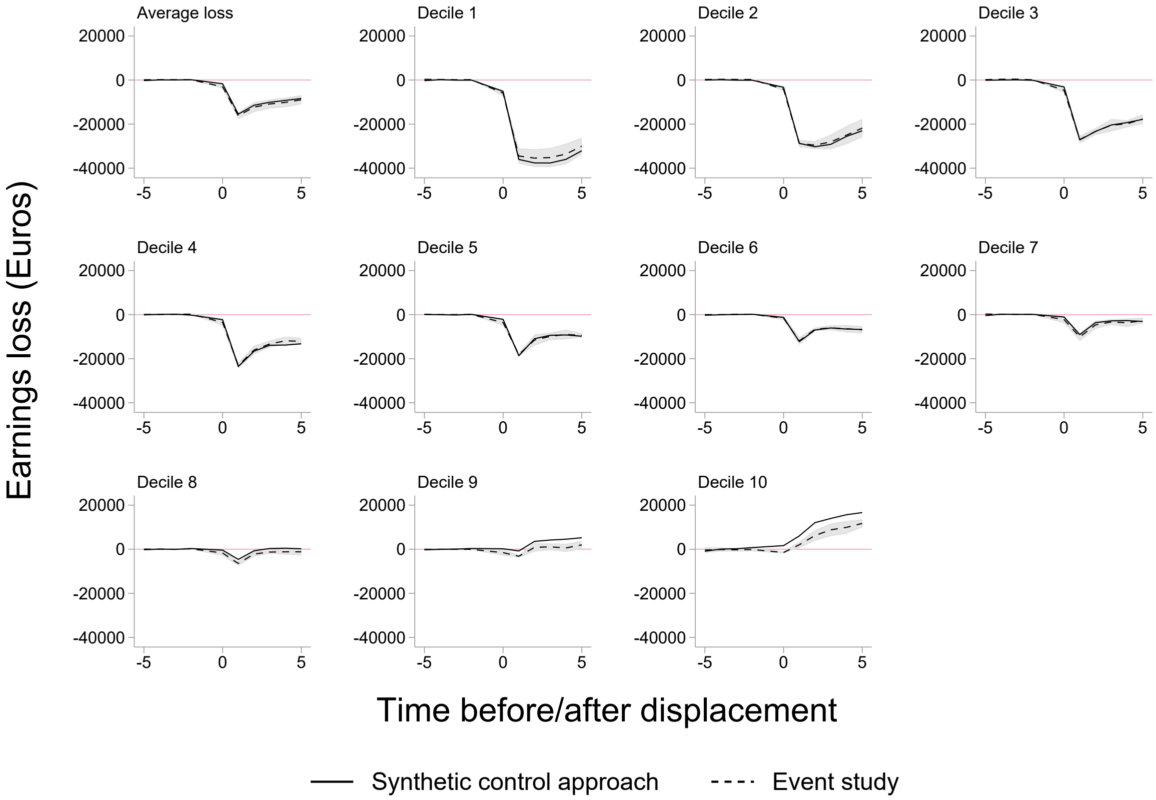

Figure 7: Regular event study vs. synthetic control group approach

Notes: This figure compares estimated earnings losses between synthetic control and event study approaches. The synthetic control series plots average earnings differences between displaced workers and their synthetic controls, averaged across all such comparisons. The event study series plots average coefficients from event study regressions centered on the time of firm closure, using Mahalanobis distance matching based on characteristics used for synthetic control matching. Separate event studies are estimated for each displacement year, and the figure plots the average of these estimates. The figure plots comparisons separately for mean earnings losses and for each decile. The shaded areas represent the 95 percent confidence bands from the event study estimates.

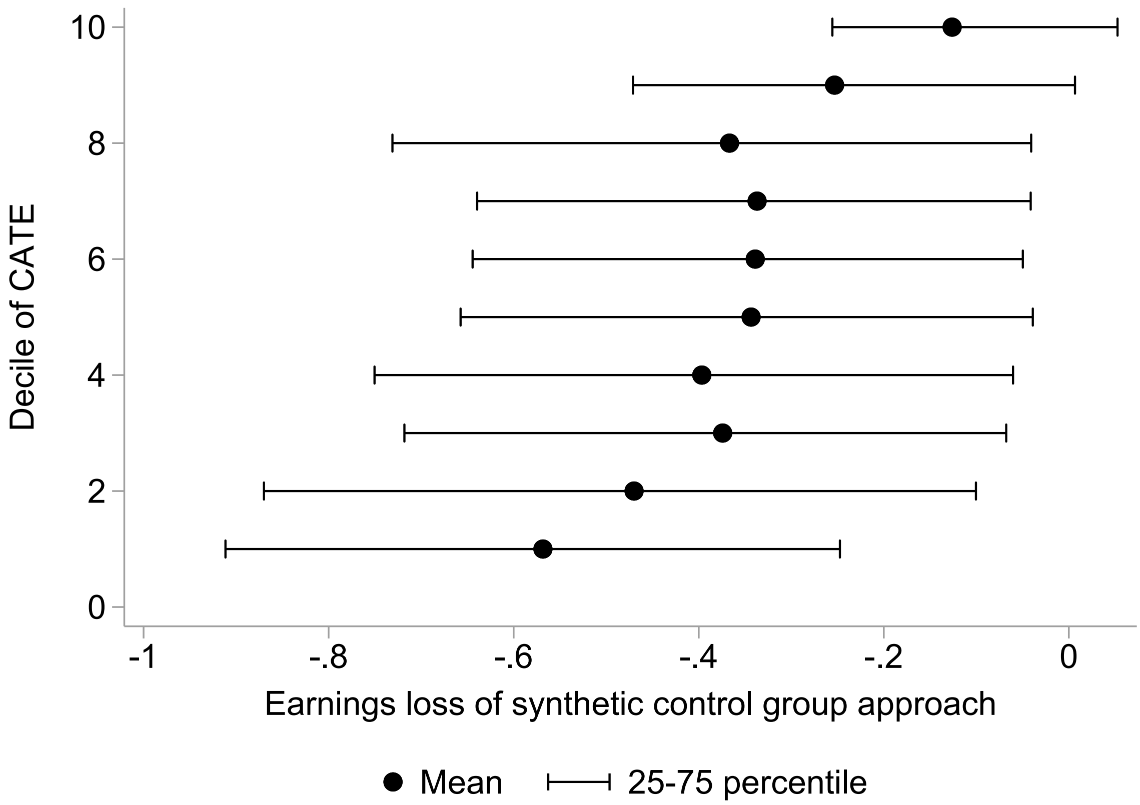

Figure 8: Synthetic control group losses vs CATE

Notes: The figure plots the mean and interquartile range of first year post-closure synthetic control losses for workers binned by CATE estimate decile from a generalized random forest. For both the CATE decile binning and the synthetic control earnings loss estimation, losses are measured as earnings one year post-layoff divided by earnings one year pre-layoff.

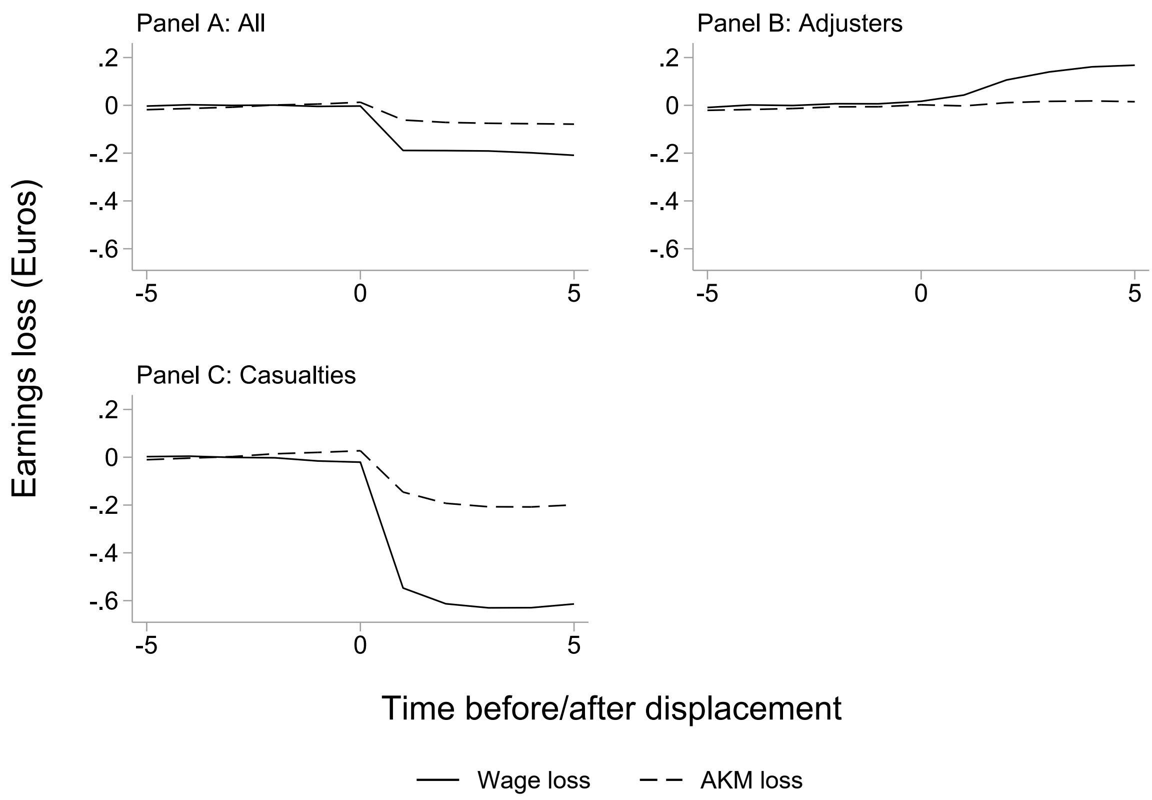

Figure 9: Contribution of closing firm fixed effect (AKM) to wage losses

Notes: The plots compare overall wage losses from firm closure to the change in the firm fixed effect (AKM) for workers who are displaced, and subsequently switch firms, following a firm closure. Each panel plots the trends separately for "adjusters" (in the lowest quartile of earnings losses) and "casualties" (in the highest quartile of earnings losses). Panel A plots the trend in average wage losses and AKM losses for all workers, Panel B for adjusters, and Panel C for casualties. The figure restricts to observations with positive wage larger than zero. We measure percentage changes as $exp^{ln(wage)} - 1$.

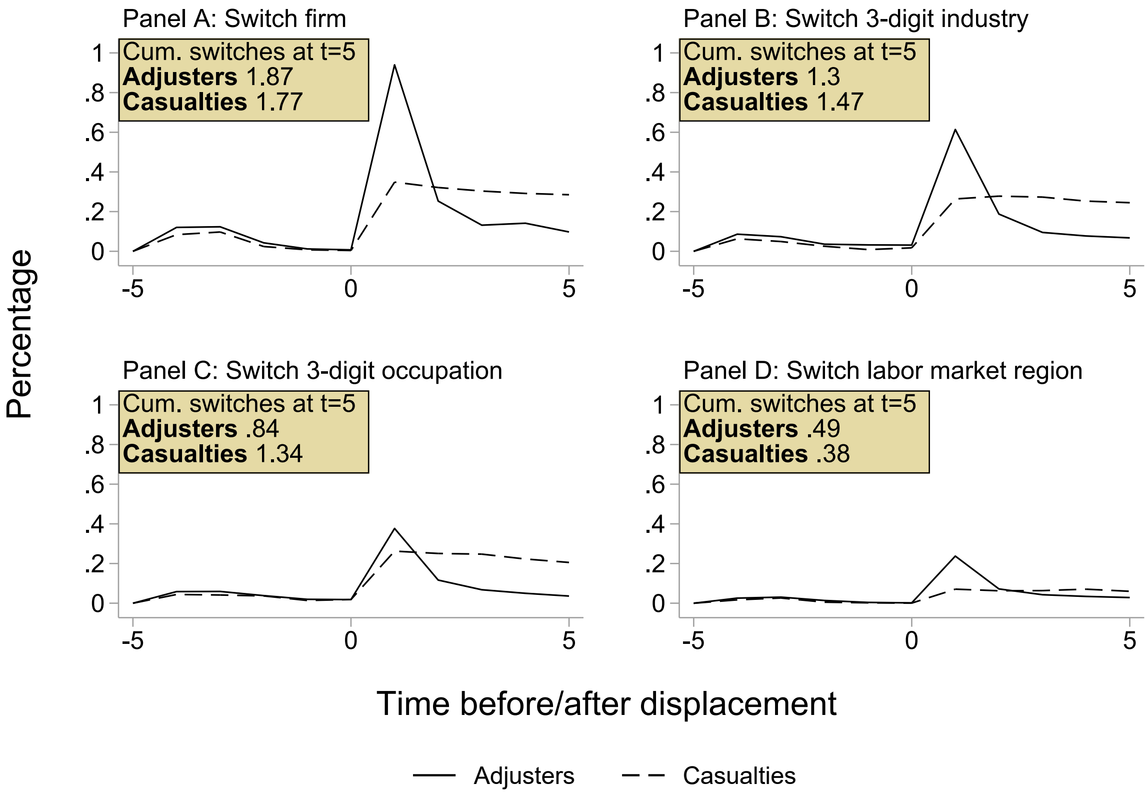

Figure 10: Labor mobility of adjusters and causalities

Notes: The figure compares the frequency of different margins of response to firm closure for "adjusters" (in the lowest quartile of earnings losses, plotted by the solid line) and "casualties" (in the highest quartile of earnings losses, plotted by the dashed line) around firm closures (time = 0). The panels plot (starting in the upper left and moving clockwise) the share of workers who change, year-over-year, their firm, industry, three-digit (KldB) occupation, and labor market region (LLM50). All changes are conditional on being employed during that year. The box in the corner of each panel summarizes the average number of switches by type, cumulative over the five years post-firm closure, separately for adjusters and casualties.

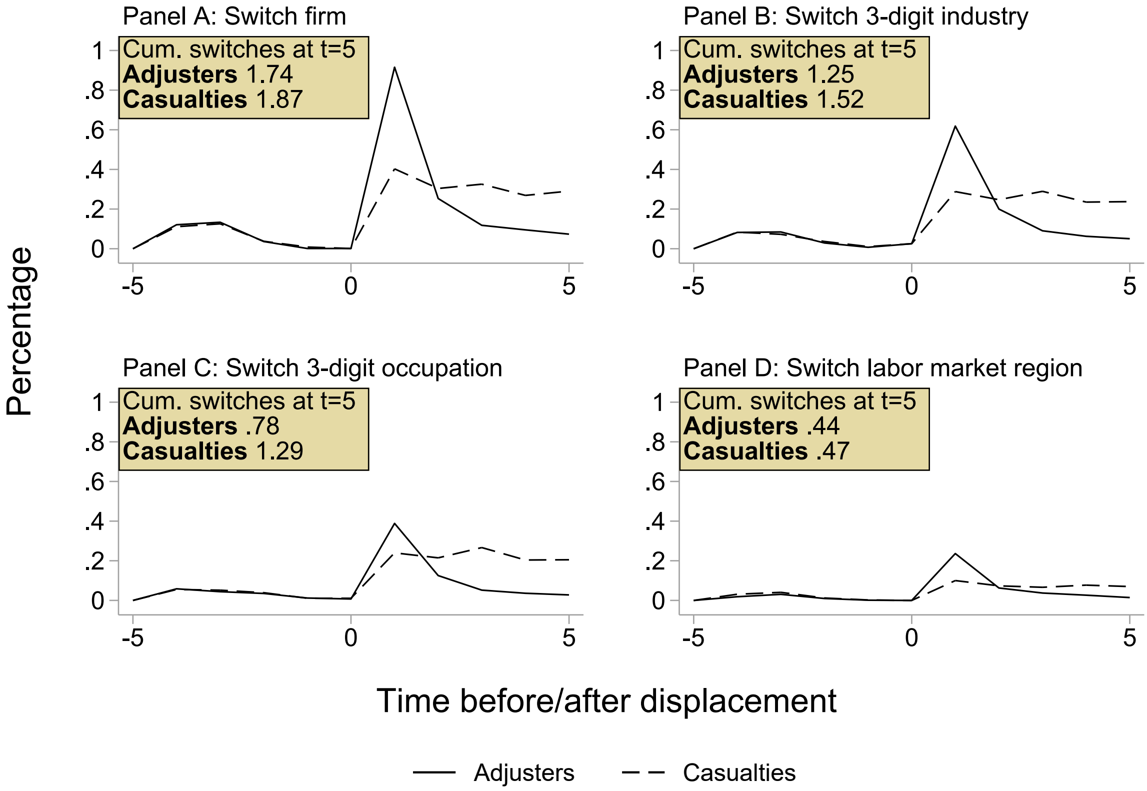

Figure 11: Labor mobility of adjusters and causalities (matched sample)

Notes: The figure compares the frequency of different margins of response to firm closure for "adjusters" (in the lowest quartile of earnings losses, plotted by the solid line) and "casualties" (in the highest quartile of earnings losses, plotted by the dashed line) around firm closures (time = 0). The figure restricts to a matched sample comprised of a sub-sample of workers who come from the same firm and have same education, gender, and occupation. The panels plot (starting in the upper left and moving clockwise) the share of workers who change, year-over-year, their firm, industry, three-digit (KldB) occupation, and labor market region (LLM50). All changes are conditional on being employed during that year. The box in the corner of each panel summarizes the average number of switches by type, cumulative over the five years post-firm closure, separately for adjusters and casualties.

Tables in text

Table 1: Raw descriptive statistics of displaced and non-displaced workers

	Non-displaced	Displaced
Worker characteristics		
Annual labor earnings	50,226	48,766
Percent female	0.318	0.283
Tenure in current job	3.618	6.631
Age (in years)	39.4	39.2
Education (% of individuals)		
Low educated (no vocational degree)	0.223	0.149
Medium educated (apprenticeship degree)	0.715	0.831
High educated (university degree)	0.062	0.020
Firm size	554	145
Industry (% of individuals)		
Manufacturing	0.457	0.461
Wholesale and retail	0.170	0.217
Construction	0.093	0.168
All other industries	0.280	0.154
Individuals	567,508	15,500

Notes: The table summarizes characteristics of displaced workers (all workers at a German firm that closed between 2000-05) and non-displaced workers who meet the same inclusion criteria for our analysis as the displaced workers (employed at firms with more than 50 employees and having at least two years of tenure at that firm). Source: IEB 1984-2010.

Table 2: Variance decomposition of displacement losses

	Share of variation	
	Treated earnings losses	Earnings of synthetic controls
Individual char.	0.017	0.107
Education	0.001	0.062
Pre-displacement firm f.e.	0.128	0.272
Pre-displacement occupation f.e.	0.037	0.208
Pre-displacement region f.e.	0.010	0.066
Citizenship	0.006	0.003
Residual	0.825	0.302
Covariances	-0.024	-0.020
Total variance of loss	1.000	1.000

Notes: The table decomposes the variance in earnings losses into portions explained by individual and displacement firm fixed characteristics. "Individual characteristics" include age and gender. Education takes three levels: low educated (less than an apprenticeship), medium educated (apprenticeship), and high educated (university degree). Firm and occupation fixed effects are recorded in the year prior to firm closure.

Table 3: Characteristics of adjusters and casualties

	Casualties	Adjusters
Worker characteristics at time of closure		
Percent female	0.399	0.249
Tenure	5.488	5.229
Age (in years)	38.8	37.2
Log daily wages	4.494	4.670
Education:		
Low	0.223	0.113
Medium	0.764	0.860
High	0.013	0.028
Log firm wage	4.408	4.510
Closing firm characteristics		
No. employees	155	174
Separation and loss		
Quarter of leaving before closure:		
Less than 1	0.697	0.647
2	0.229	0.271
3 or more	0.074	0.082
Relative loss (years of earings) (years of earnings)	-3.569	0.618
Observations	3,875	3,875

Notes: This table compares characteristics of adjusters (workers in the smallest quartile of cumulative earnings losses) and casualties (workers in the largest quartile of cumulative earnings losses).

Table 4: Wage and employment states for adjusters and casualties (percentages)

	Adjusters					Casualties				
Years after closure	1	2	3	4	5	1	2	3	4	5
No wage										
Not employed, full year	1.0	0.2	0.2	0.2	0.4	34.8	38.9	36.4	31.1	22.8
Partial year employed	5.0	0.9	0.5	0.4	0.2	30.7	14.5	8.3	6.2	4.9
Full year employed	0.1	0.0	0.0	0.0	0.0	0.1	0.3	0.3	0.3	0.1
Wage loss 0-10%										
Partial year employed	4.5	0.6	0.4	0.4	0.7	2.3	2.0	1.4	0.9	0.5
Full year employed	16.5	15.8	13.6	11.8	12.8	1.9	1.3	0.9	1.0	2.1
Wage loss 10-50%										
Partial year employed	2.6	0.4	0.2	0.3	0.5	8.0	10.7	9.5	8.7	8.8
Full year employed	8.8	6.6	4.6	5.3	5.6	6.3	9.5	15.2	19.4	23.9
Wage loss $>$ 50%										
Partial year employed	0.5	0.1	0.1	0.0	0.1	6.0	8.9	8.7	8.4	8.6
Full year employed	0.5	0.3	0.2	0.2	0.2	4.4	10.7	17.5	22.0	24.9
Wage gain										
Partial year employed	11.2	4.2	2.5	2.0	2.8	3.1	2.1	1.2	1.0	0.9
Full year employed	49.4	70.9	77.8	79.4	76.7	2.4	1.2	0.5	1.0	2.5

Notes: This table shows a set of the following post-displacement outcomes as a percentage of adjusters and of casualties by year over the first five years after displacement. The table splits workers into three employment categories: not employed for the entire year, employed for part of the year (between $0 - 300$ days), and employed for the full year ($>$ 300 days). The table compares the worker's wage in a given year to a counterfactual wage calculated by applying the synthetic control weights of our main approach for annual labor earnings to the daily wages of workers in the donor pool.

Appendix

A Inference for single firm in the craft sector

To provide inference for the earnings losses of the individual workers in our craftsmen firm example, we follow the permutation method proposed by Abadie et al. (2010). In more detail, we treat each of the 20 donors from each displaced workers' donor pool as a potentially displaced worker and perform placebo estimates for each using our synthetic control group approach. If the observed effects are truly due to the firm closure rather than unrelated unobserved differences, these estimates should be near zero or bounded away from zero. Instead, the actual treatment effect at the negative extreme of the placebo distribution.

The gray lines in Figure A.1 depict the placebo estimates, while the black lines depict the actual displacement effects. In nearly all cases where displaced workers experience meaningful earnings losses, the black lines lie at the extreme of the placebo distribution, indicating statistical significance with a p-value of 5 percent or lower. Instead, the black lines of displaced workers whose treatment effects lie close to zero lie within the placebo distribution indicating that these workers do not suffer meaningful earnings losses that are statistically significant.

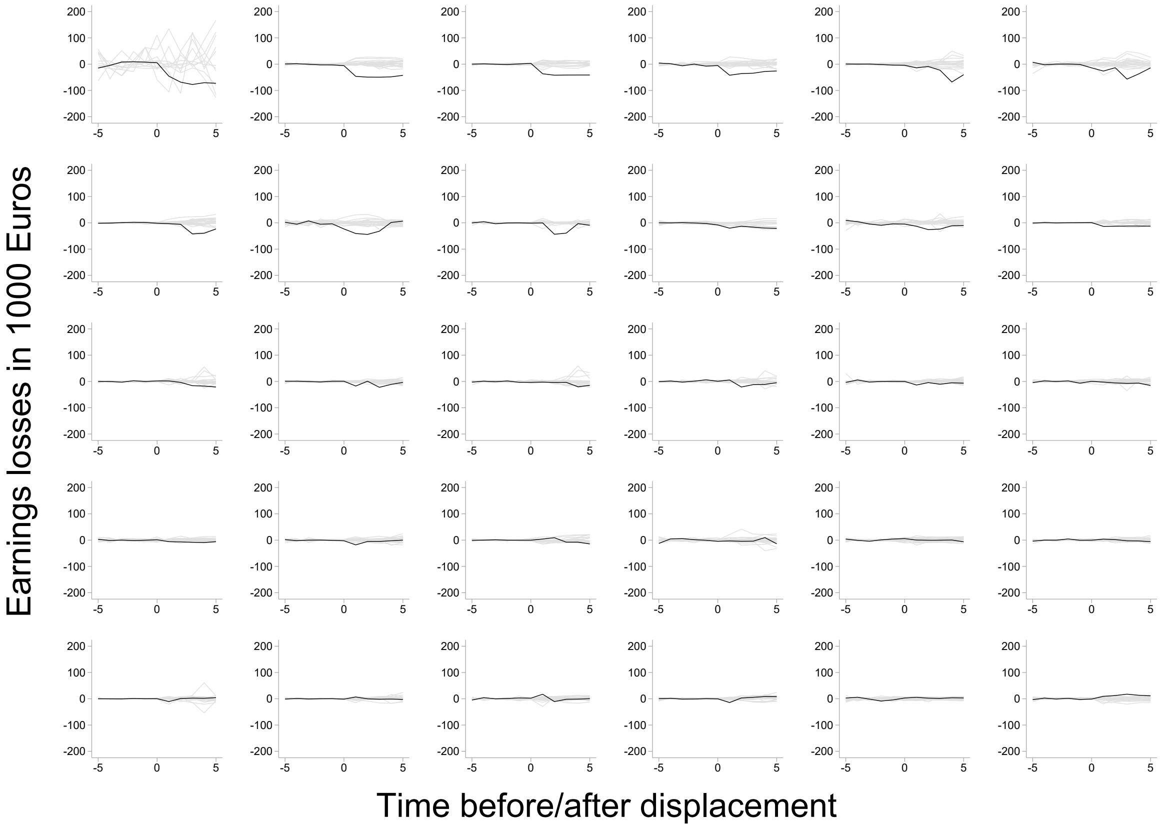

Figure A.1: Individual earnings loss of a closing firm (case study)

Notes: The figure plots the estimated earnings losses of 30 workers displaced from a single closing firm that specialized in HVAC installation and repair. In each panel, the solid black line plots the earnings losses (in 2010 Euros) of a single displaced worker relative to their synthetic control. To demonstrate that the earnings effects of the layoff arise from exposure to the firm closure, rather than due to the selection of control observations, the grey lines plot results from a permutation exercise where, for each of the treated worker's 20 control "donor" workers, we construct a synthetic control from the remaining donors and plot the difference in earnings trends for each of these 20 workers. The y-axis measures the earnings in 2010 Euros. The x-axis displays the time before/after the firm closure in years. Source: IEB.

B Main event study excluding observations with zero earnings

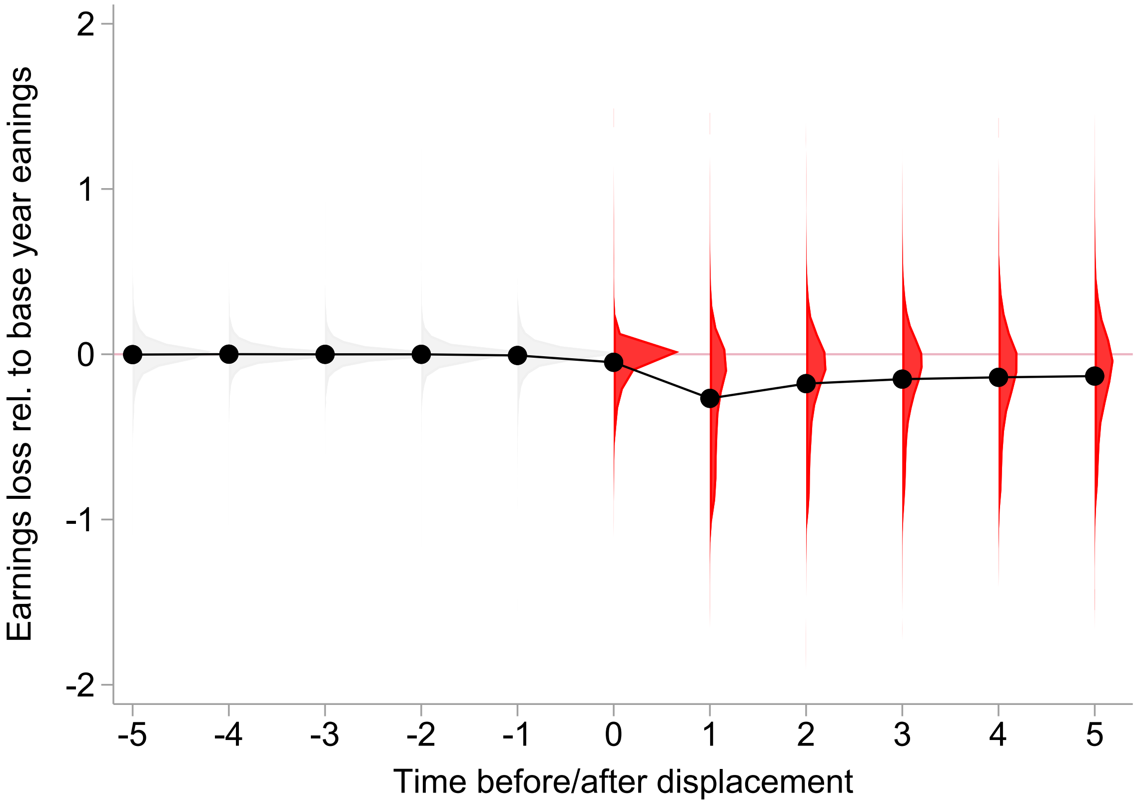

Figure B.1: Distribution of relative earnings losses excluding observations with zero annual earnings

Notes: The figure displays the distribution of displaced workers' earnings losses throughout a period of five years before until five years after a firm closure. The earnings losses are measured relative to the individual worker's average earnings in the three years before the displacement. The dots represent the mean earnings losses for each period. We exclude all observations that have zero earnings throughout the entire year. The dots represent the mean earnings losses for each period respectively.The shaded areas represent the distribution of the displaced workers' earnings loss estimates. To plot the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. Second, we use an Epanechnikov kernel to plot the distribution of earnings losses from the individual earnings losses in each period. Source: IEB.

C Narrow deviation robustness

This Appendix provides a robustness check for the estimates of the dynamic development of the loss distribution on a sample of displaced workers with perfectly matching pre-trends. More specifically, we reproduce our main result as displayed in Figure 3 on a sample that only includes displaced workers for whom we found synthetic control group weights such that the absolute gap between their pre-treatment earnings and those of their synthetic controls never exceeds five percent of their average pre-treatment earnings.

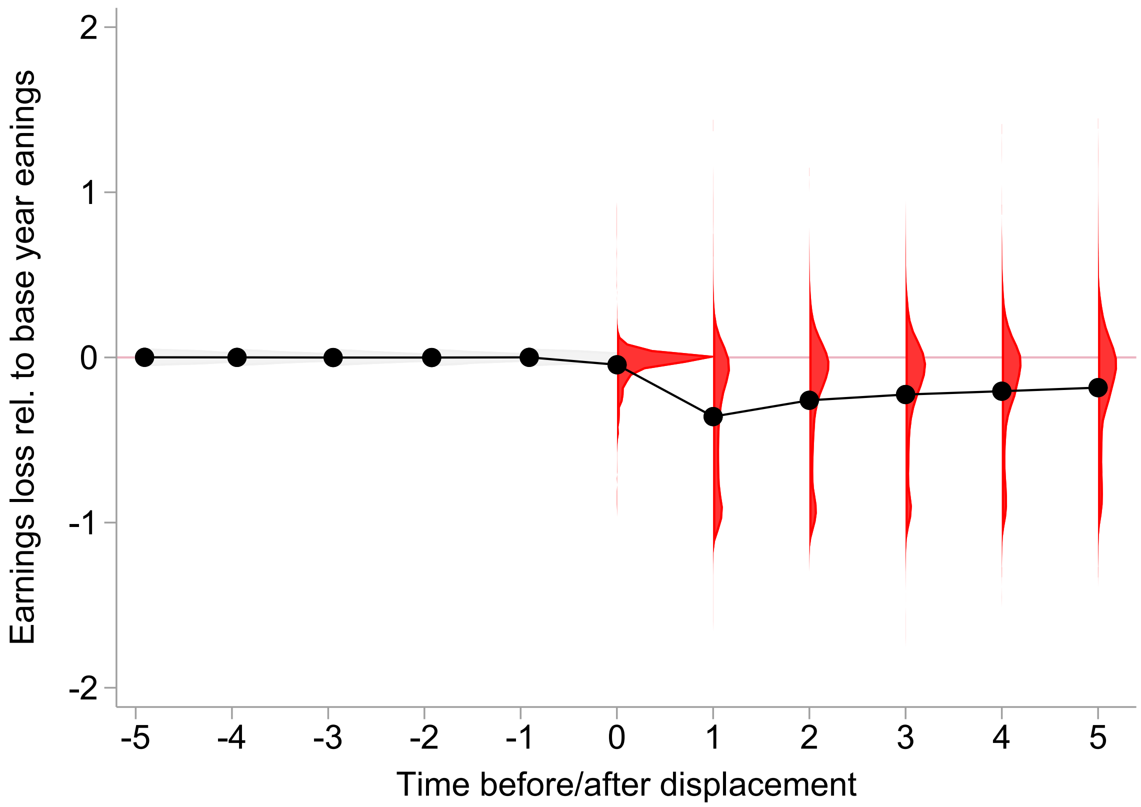

Figure C.1: Distribution of relative earnings losses with perfect pre-trends

Notes: The figure displays the distribution of displaced workers' earnings losses throughout a period of five years before until five years after a firm closure. The earnings losses are measured relative to the individual worker's baseline earnings measured as the average earnings throughout a period of three years before the displacement. The dots represent the mean earnings losses for each period respectively. The shaded areas represent the distribution of the displaced workers' earnings losses. To estimate the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. For this figure, we restrict only to displaced workers for which the pre-closure difference between treated and synthetic control never exceeds five percent of their average pre-treatment earnings. Second, we use an Epanechnikov kernel to estimate the distribution of earnings losses from the individual earnings losses in each period. Source: IEB.

Figure E.1 reveals that the pre-treatment earnings gaps between the displaced workers and their synthetic controls are very strongly centered around zero with virtually no tails in the distribution. However, the distributions of the post-displacement earnings losses are qualitatively the same as in Figure 3.

D Pre-trends

Our main identification assumption—particularly at the extreme percentiles of the loss distribution—is that workers' adjustment ability A_i would not have influenced displaced workers' earnings trajectories in the absence of the treatment. In other words, we assume that the observed pre-trend allows us to adequately extrapolate workers' counterfactual earnings had the treatment not occurred. The central argument supporting this strategy is that any unobserved differences that would have affected workers' wages in the absence of the treatment should also have affected their wages consistently in the years preceding it. Although we cannot directly test this assumption, this section provides additional evidence in its favor.

Therefore, we, first, construct synthetic control weights using yearly earnings from periods $t-2$ to $t-6$ prior to displacement.[38] Second, we apply these weights to trace the reweighted earnings trajectories of displaced workers and their synthetic controls over a ten-year pre-treatment window. While matching on a short pre-treatment window ensures alignment close to displacement, unobserved heterogeneity relevant for earnings trajectories would typically generate persistent differences in levels or trends over time. Examining earnings dynamics over a much longer pre-treatment horizon therefore provides a meaningful stress test: if the synthetic controls closely track displaced workers for up to ten years before treatment, it becomes less plausible that large, systematic deviations would have arisen in the absence of treatment after displacement. We implement this exercise separately at different percentiles of the earnings-loss distribution to assess whether the synthetic-control fit deteriorates in the upper or lower tails, where our identification assumption is most demanding. Finding a comparable long-run pre-treatment fit across percentiles supports the plausibility of our identification strategy.

The left panel of Figure D.1 presents the results in an event-study plot accompanied by kernel density distributions. These distributions capture the differences between displaced workers and their individual synthetic controls. Because we construct the synthetic-control weights using periods -6 to -2, the gray distributions in these periods are mechanically narrower than those further back in the pre-treatment window (-10 to -7). However, all pre-displacement distributions are statistically indistinguishable from a normal distribution, indicating no systematic divergence between displaced workers and their synthetic controls, either at the mean or in the tails of the distribution.

The right panel of Figure D.1 presents conditional average treatment effects for the average worker, casualties, and adjusters. Again, we detect no meaningful differences in pre-trends over the period from -10 to -7.

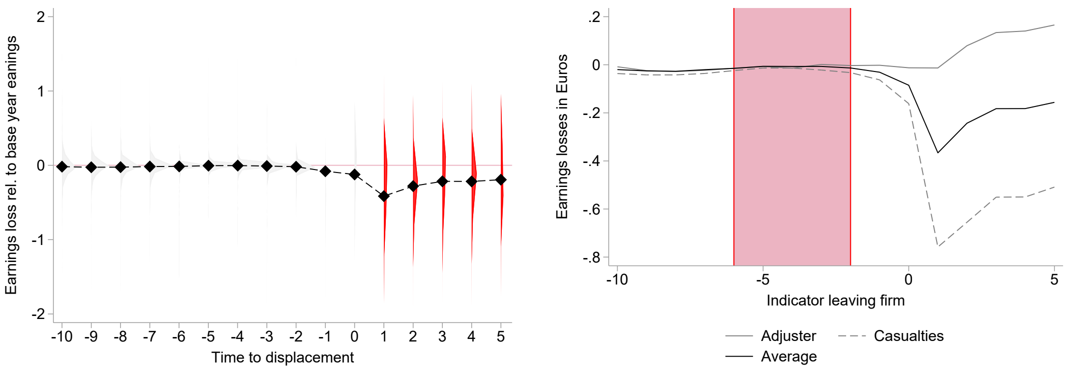

Figure D.1: Pre-displacement distributions of earings differences between displaced workers and their synthetic controls

Notes: This figure plots the distribution of the differences in earnings between displaced workers and their synthetic conntrols before the treatment. The solid line shows the results for adjusters and the dashed line for casualties.

[38]We deliberately exclude t-1 to avoid potential anticipation effects.

E Pre-treatment deviations uncorrelated with earnings loss estimates

One potential concern might be that the quality of our synthetic control group approach might systematically differ between those in the upper and lower tail of the distribution, i.e., casualties and adjusters. This problem would arise if the synthetic controls weights were of lower quality in the tails of the treatment effect distribution than at the mean or median, such that the earnings losses were systematically upward biased for casualties and systematically downward biased for adjusters. Figure E.1 presents separate distributions of earnings differences between displaced workers and their synthetic counterfactual before the treatment. The figure reveals that the distributions of pre-displacement earnings differences are strongly centered around zero and virtually identical for casualties and adjusters. Thus, Figure E.1 does not reveal any evidence that the estimates are systematically biased for adjusters or casualties.

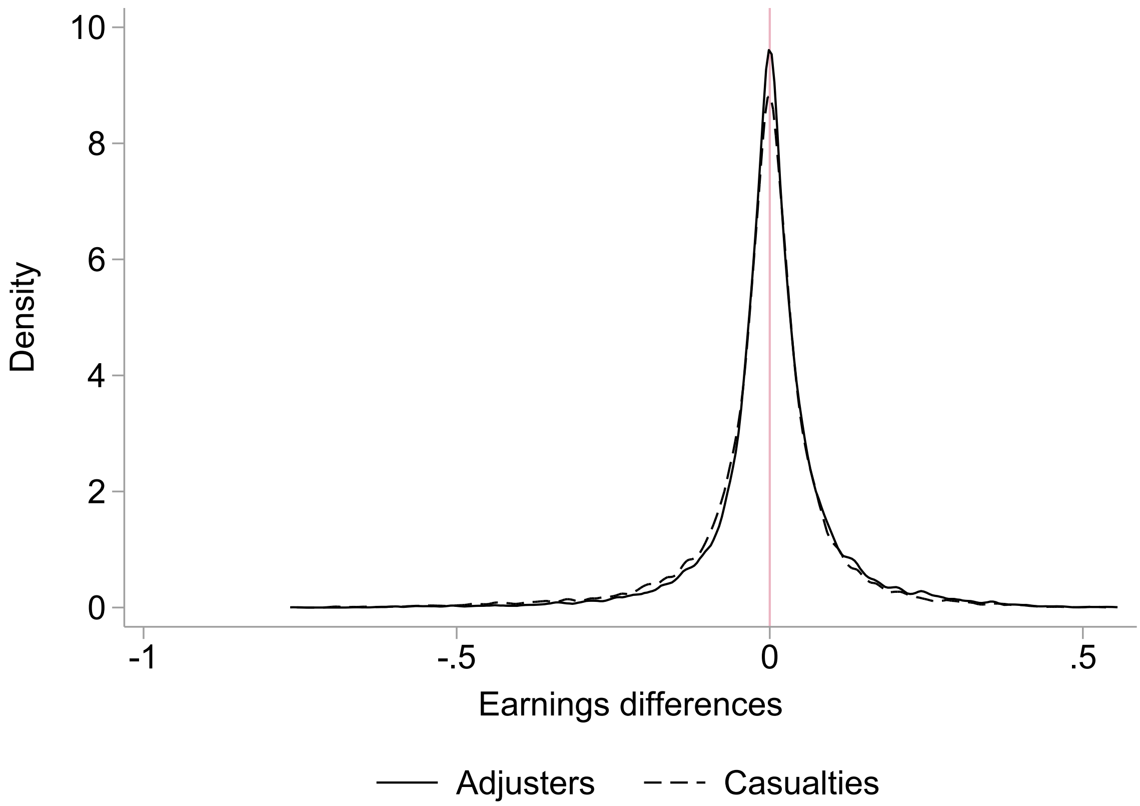

Figure E.1: Pre-displacement distributions of earnings differences between displaced workers and their synthetic controls

Notes: This figure plots the distribution of the differences in earnings between displaced workers and their synthetic conntrols before the treatment. The solid line shows the results for adjusters and the dashed line for casualties.

F Estimation using data from longer pre-trends

The quality of synthetic control group estimates crucially depends on the variation in the data and the number of available pre-treatment periods, i.e., Abadie et al. (2010) show that the bias of the synthetic control estimator is bounded by a function that decreases with the number of pre-treatment periods. Therefore, F.1 presents synthetic control group estimates of displaced workers' earnings losses, using a longer pre-treatment period of ten instead of five years to construct the synthetic weights. Naturally, we can only conduct these long-term estimates for the subset of workers whom we observe for at least ten years before job loss. On average, this subset of workers is older, more highly educated, and less likely to be female than the workers included in our main analysis.

Panel A shows the distribution of earnings losses for this subset of workers, using both approaches: ten pre-treatment years (dashed line) and five pre-treatment years (solid line). Because these workers are more highly educated, less likely to be female, and older, the distribution of their earnings losses is less skewed than in our main analysis. Most importantly, however, the results reveal no major difference between the approach using five and the one using ten pre-treatment years to estimate the synthetic control weights.

Panel B shows the correlation between displaced workers' earnings losses estimated using ten and five pre-treatment years. The results reveal a strong correlation of over 0.8 between the two sets of estimates.

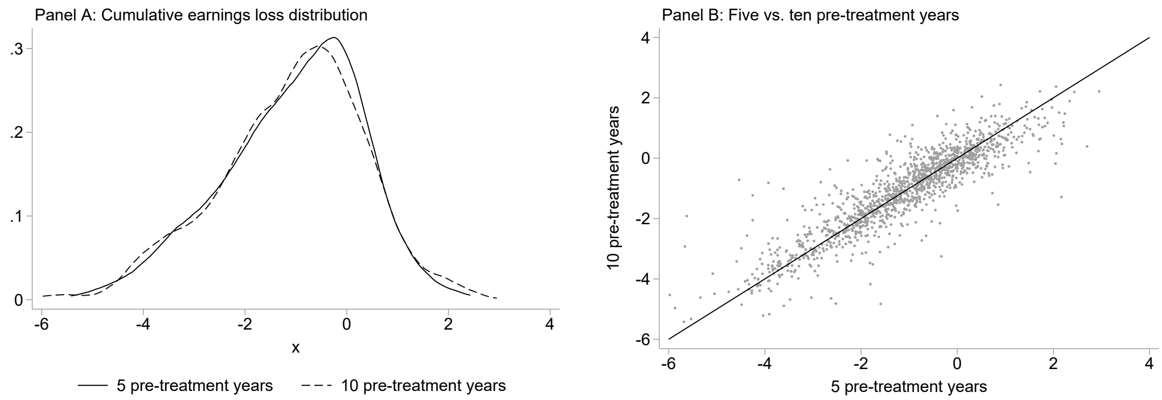

Figure F.1: Long vs. short pre-treatments

G Inference for dynamic distributions

To demonstrate that our findings are driven by earnings losses stemming from firm closure (rather than artifacts of the synthetic control estimation), we estimate the distribution of placebo earnings losses for workers in the "donor pools" of our displaced workers. Using a modified version of the permutation method proposed by Abadie et al. (2010), we randomly select one donor from each displaced worker's donor pool and estimate the placebo intervention for only that selected donor. This process yields approximately $16,000$ placebo estimates. G.1 compares the distributions of the estimated displacement losses with these placebo estimates at -5, -1, 1 and 5 years after displacement. Panel A and B plot the pre-displacement distributions at five and one year(s) before workers exit their closing firms. Panel C and D plot the post-displacement distributions at one and five year(s) after workers exit their closing firms. The black lines show the kernel density estimates for the displaced workers' loss distribution, while the dashed lines show the distribution of their placebos.

If the synthetic control approach fits the data well, the pre-displacement loss and placebo distributions should be similar: approximately normally distributed, centered at zero, and exhibiting minimal variance. In contrast, the distribution of post-displacement earnings changes should lie at the extremes relative to the placebo distributions. Figure G.1 confirms this prediction. Before the displacement, the loss and placebo distributions are normally distributed and virtually indistinguishable. However, after the displacement, the loss distributions deviate strongly from normal distributions and their left tails lie at the extremes of the placebo distributions, which continue to resemble normal distributions with low variance.

By contrast, the right tails of the loss distributions are not shifted to the extremes of their placebo distributions. This result does not necessarily indicate that the right tails are noisily estimated—especially as our register data is largely free from measurement error. Rather, it suggests that some displaced workers benefit from improved job matching, much like workers in the donor pool who switch jobs for reasons unrelated to firm closures.

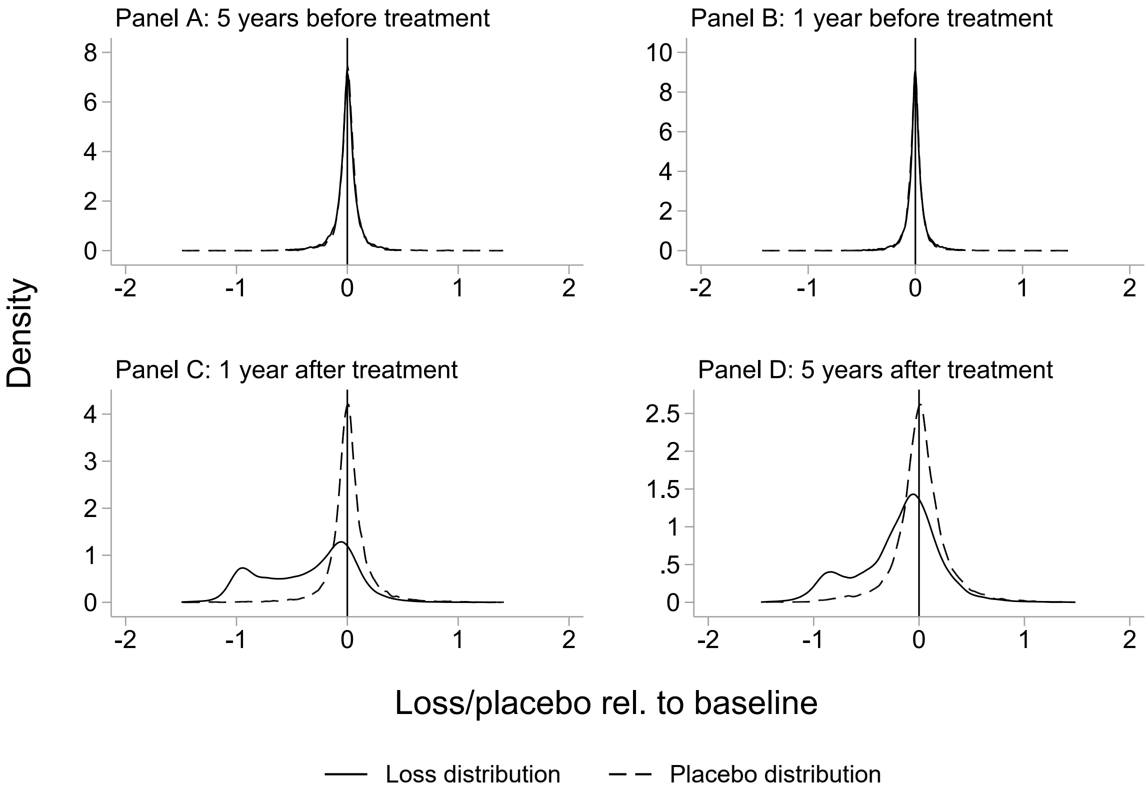

Figure G.1: Comparison between loss and placebo distribution

Notes: The black lines display the kernel density estimates of the distributions of displaced workers' earnings losses. The earnings losses are measured relative to the individual worker's baseline earnings measured as the average earnings throughout a period of three years before the displacement. The dashed lines the kernel density estimates placebo distribution that we obtained by estimating the intervention separately for a selected sample of the donor pool. Source: Own calculations with IEB.

H Subsample permutation exercise

The following figure presents the results from a permutation exercise for which we have drawn 200 ten-percent samples of our data to re-estimate the distribution of displaced workers earnings losses. Unlike in a bootstrapping exercise for which we would randomly pull samples of the same size, we purposefully only used ten percent samples to increase the likelihood of outliers to influence the results. The gray lines represent the results from the permutation exercises, the black line shows the results from the entire sample.

The figure reveals that the shape of the distribution of displacement losses is fairly robust.

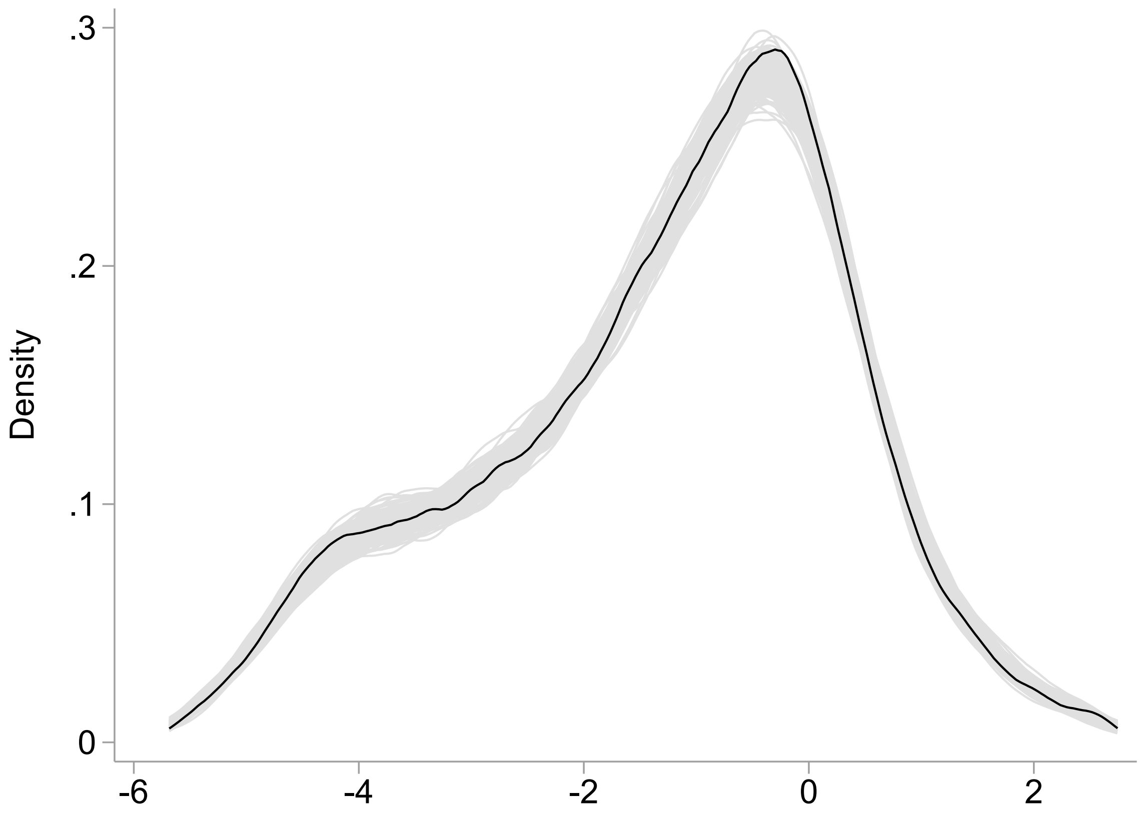

Figure H.1: Permutation of loss distribution on 200 ten percent samples

Notes: This figure plots the distribution of earnings losses derived from 200 ten-percent samples from our broader sample of displaced workers. Earnings losses represent the cumulative log difference between actual and synthetic control earnings over the five years following a worker's firm closure.

Additional figures and tables

Table H.1: Displacement loss variance decomposition by subgroup

	Low educated	Medium educated	High educated	Women	Men
Individual characteristics	0.020	0.017	0.017	0.010	0.019
Education				0.000	0.003
Firm fixed effects	0.299	0.137	0.233	0.220	0.154
Occupation fixed effects	0.152	0.037	0.232	0.109	0.047
Region fixed effects	0.085	0.010	0.094	0.062	0.016
Citizenship	0.030	0.004	0.030	0.006	0.006
Residuals	0.636	0.822	0.635	0.741	0.810
Covariances	-0.222	-0.027	-0.240	-0.147	-0.054
Total variance of loss	1.000	1.000	1.000	1.000	1.000

Notes: The table decomposes the variance in earnings losses into portions explained by individual and displacement firm fixed characteristics. "Individual characteristics" include age and gender. Education takes three levels: low educated (less than an apprenticeship), medium educated (apprenticeship), and high educated (university degree). Firm and occupation fixed effects are recorded in the year prior to firm closure.

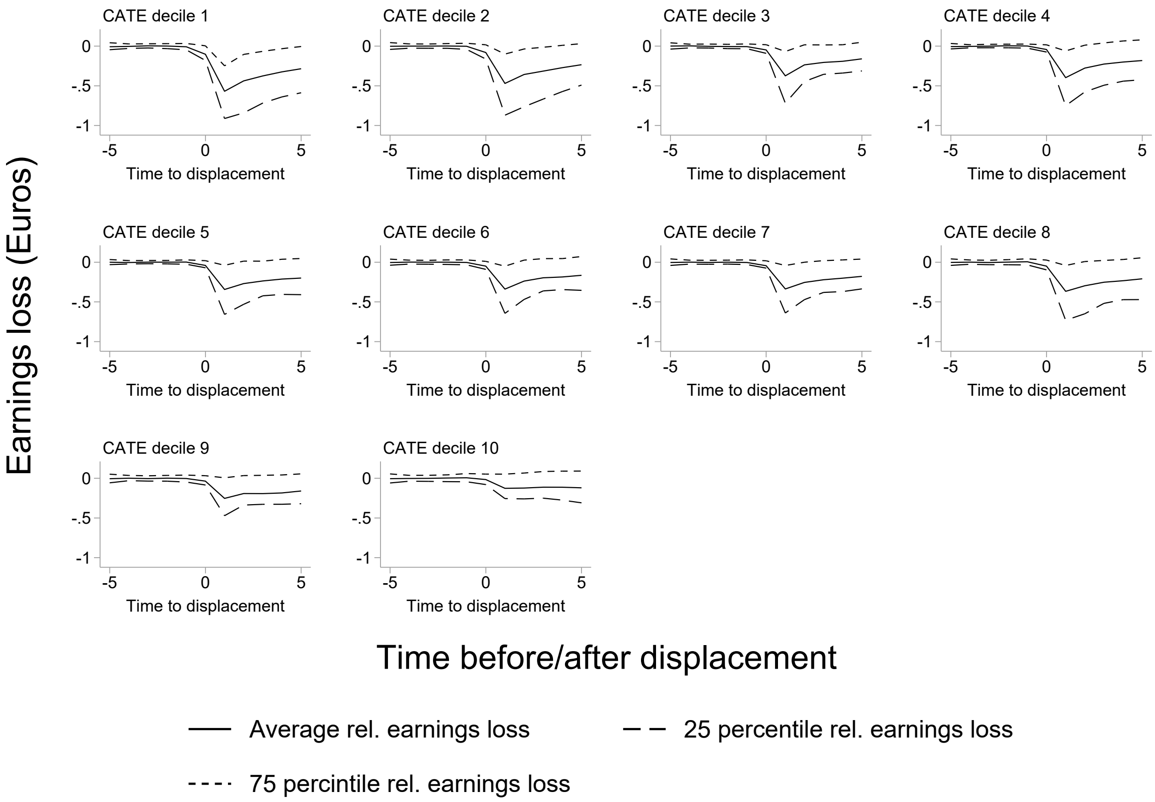

Figure H.2: Synthetic control group losses vs CATE, dynamic comparison

Notes: The figure plots the dynamic mean and interquartile range of synthetic control losses for workers binned by CATE estimate decile from a generalized random forest. Workers are assigned into bins based on the decile of CATE estimated first-year earnings losses (relative to earnings in the year before closure), estimated using a generalized random forest. The figure plots the mean and interquartile range for workers in each decile bin.

Table H.2: Differences in education updating between adjusters and casualties

	Low educated		Medium educated	
	Unmatched	Matched	Unmatched	Matched
Adjuster	-0.016**	-0.056**	-0.015***	-0.013
	(0.007)	(0.023)	(0.004)	(0.008)
Observations	2630	430	12512	2836
R^2	0.005	0.017	0.004	0.007

Notes: This table summarizes differences in worker educational updating between "adjusters" (workers in the smallest quartile of earnings losses) and "casualties" (workers in the largest quartile of earnings losses). Estimates come from a regression of an indicator that takes value 1 if a worker achieves a higher level of education in the five years following firm closure on a dummy for adjuster vs casualty. Education takes three levels: low educated (less than an apprenticeship), medium educated (apprenticeship), and high educated (university degree). The "unmatched sample" compares all adjusters and casualties; in the "matched sample," each casualty is paired with an adjuster of the same gender, age category, and pre-displacement occupation (three-digit).

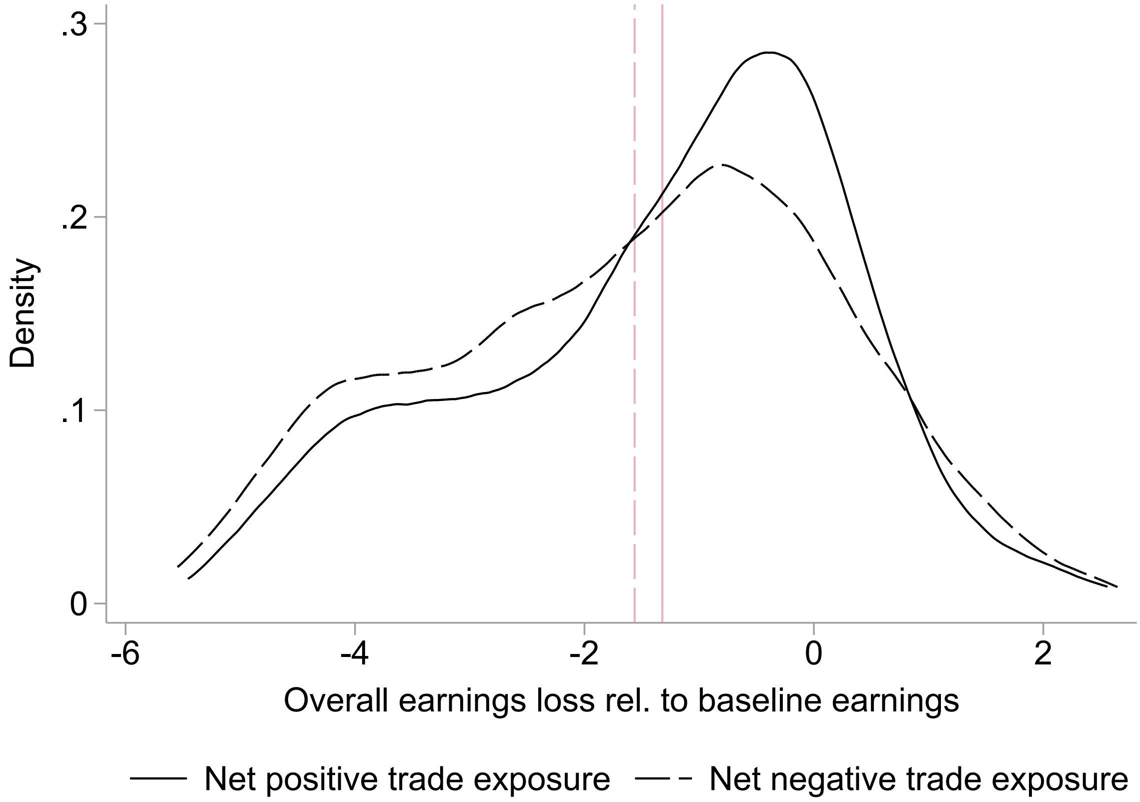

Figure H.3: Distributions of earnings losses by trade exposure

Notes: The figure displays the distributions of displaced workers' cumulative earnings losses over the five-year period after firm closure, split by trade exposure. We construct the trade exposure measure at the one-digit industry-by-region level by first measuring industry exposure to trade competition and then scaling by the share of workers employed in that industry within the region where the firm is located (Eggenberger et al., 2022). To estimate the distribution of earnings losses, we first use a synthetic control group approach to estimate the earnings losses for each individual displaced worker in the data. Second, we use an Epanechnikov kernel to estimate the distribution of earnings losses from the individual earnings losses in each period. The red lines represent the mean earnings in each cell. Source: IEB.