Adjusters and Casualties: The Anatomy of Labor Market Displacement*

Eric A. Hanushek† Simon Janssen‡ Jacob D. Light §
Lisa Simon ¶

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Abstract

Earnings losses from job displacement are highly skewed: a small number of workers suffer catastrophic losses while the majority recover quickly. Conventional event study methods, which focus on average effects, obscure this critical heterogeneity and overstate the losses experienced by most displaced workers. Using comprehensive administrative data from firm closures in West Germany (2000–2005), we employ synthetic controls to estimate the full distribution of earnings losses at the individual level. Consistent with existing work, we find that older, less educated, and female workers face higher average losses; however, these fixed characteristics explain only a small fraction of the variance. Instead, differences in post-layoff adaptability – such as switching occupation or relocating geographically – distinguish adjusters from those most impacted.

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

JEL Classification: J24, J64, O30

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†Hoover Institution

‡Institute for Employment Research (IAB)

§Hoover Institution

¶Revelio Labs

1 Introduction

Decades of economic research have consistently highlighted the large and enduring impacts of firm closures and job displacements on workers' earnings, revealing substantial and often persistent losses.[1] Recent research further highlights the uneven impact across different groups of workers. However, critical gaps remain in our understanding of how losses are distributed more generally among workers and what drives any heterogeneity in outcomes. Existing studies, which focus on average earnings losses, fail to capture the considerable variation in individual experiences within identified groups following displacement. This omission has significant implications for the design of effective policies to support displaced workers.

This paper analyzes the full distribution of earnings losses following job displacement. We estimate losses by comparing earnings of displaced workers to those of an individual-level control group identified by a combination of matching and synthetic-control methods. Conclusions based on the distributions of earnings and employment trajectories of displaced workers differ substantially from conventional wisdom on the nature and scope of such displacements. The distribution of individual earnings losses is far from normal, implying that the average loss typically estimated from event study approaches significantly overstates the effect of firm closure for the majority of workers.

Using administrative data covering all German plant closures from 2000-2005, we trace the wage and earnings patterns of displaced workers for five years after a closure. We extend the matching approach of Schmieder et al. (2023) to synthetic-control methods that allow us to develop a control for each displaced worker and then to estimate the entire distribution of economic losses. This highly skewed distribution shows that the modal displaced worker loses little, some workers actually gain, and a small group of workers endures catastrophic and persistent losses.

[1] Jacobson et al. (1993), Couch and Placzek (2010), Schmieder et al. (2010), Davis and von Wachter (2011).

From our distribution of losses, we can reproduce standard event study estimates of the *average* cost of displacement, along with previously identified average differences across demographic and worker groups: lower educated workers, women, and older workers suffer above average losses. But the larger conclusion from comparing gains and losses across demographic groups is the near complete overlap of the underlying loss distributions. Observable characteristics explain only a small fraction of the overall variance in losses; 83 percent of the variation in earnings losses cannot be explained by worker and closing firm characteristics conventionally studied in the literature.

There are dramatic differences in adjustment behavior between those who fare best after closure ("adjusters") and those who fare worst after closure ("casualties"). When adjusters move to change occupation or geographic location, they do so quickly and decisively. Casualties, on the other hand, keep changing both occupation and location without recovering to their pre-closure earnings levels.

We make three 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 education,[2] gender,[3] tenure,[4], worker-firm match,[5] and firm characteristics.[6] This refinement shows that the observable characteristics explain little 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.[7] Third, by track-

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

[3]Illing et al. (2024).

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

[5]Moore and Scott-Clayton (2019), Lachowska et al. (2020), Gulyas et al. (2021), Fackler et al. (2021), Graham et al. (2023).

[6]Fackler et al. (2021) show that workers who are displaced by larger firms forgo larger firm wage premiums 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.

[7]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) finds that

ing outcomes over time, it is possible to describe the behavioral differences between those emerging from a firm closure in a good economic position and those who end up significantly harmed.

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 using 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 8 concludes.

2 Empirical strategy

The canonical approach to estimating the effects of worker displacement relies on event study estimates that contrast the outcomes of displaced workers with a comparison group of workers in firms not closing. While providing estimates of mean losses, these methods cannot recover the full distribution of earnings losses among displaced workers and fail to identify the prevalence of those with the same pre-treatment characteristics who experience heterogeneous outcomes. Thus, standard event studies cannot, for example, identify workers who adjust to firm closures without incurring significant earnings losses.

We estimate individual-level earnings losses using a hybrid synthetic control

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' earnings losses strongly varies with the business cycle. These business cycle effects might alter the magnitude of overall losses, but they seem unlikely to change the overall conclusions of our work.

group strategy for each displaced worker. This approach builds on the work of Schmieder et al. (2023), who use a classical matching procedure to pair each job loser with a statistical twin in order to calculate average short-term wage losses across individuals. We extend this methodology by overlaying synthetic control techniques onto an initial matching of displaced workers, enabling us to estimate dynamic displacement losses for each individual.

Synthetic control methods have traditionally been applied to estimate the effects of aggregate interventions on large units, such as cities or regions (Abadie, 2021). However, recent studies have adapted these methods for disaggregated data, and Arkhangelsky and Hirshberg (2023) have shown that synthetic control approaches serve as a natural alternative to event study difference-in-difference estimators in settings with numerous treated and control units. Our application of synthetic controls at the individual level allows us to capture and analyze the distributional effects of displacement.

The potential donor pool of non-displaced workers is extensive, but the relevant donor pool — those who serve as suitable comparisons for displaced workers — is much smaller. For example, comparing a late-career professor to a young manufacturing worker, even if they have similar earnings trajectories before the closure, would not be appropriate. Instead, we seek to compare displaced workers with non-displaced workers who, a priori, share similar job characteristics and career stages.[8]

For each displaced worker, we consider for the donor pool all nondisplaced workers with the same gender, education, one-digit industry, and three-digit occupation.[9] From this broad pool, we calculate the root mean squared difference (RMSD) be-

[8]Implicitly, any synthetic control strategy defines a relevant donor pool from the set of all possible comparison units. For example, in the classic Abadie et al. (2010), all US states besides California are in the donor pool to estimate the effect of California's Proposition 99 on tobacco consumption. It is certainly possible that including Canadian provinces or Mexican states in the donor pool may increase pre-treatment fit, but they would be unsuitable controls for reasons unrelated to minimizing pre-treatment fit.

[9]In developing these broad comparison groups of nondisplaced workers, we apply the same inclusion criteria (described below) that apply to the displaced worker sample. These involve restrictions on firm size and worker tenure within the firm.

tween the earnings trajectory of each displaced worker and potential donors within the matched sample over the five years prior to firm closure. We then select the 20 donors with the lowest RMSDs relative to each displaced worker.[10] This pre-matching approach both minimizes interpolation bias by ensuring that the donor pool consists of non-displaced workers with very similar pre-displacement careers and makes the subsequent synthetic control calculations more tractable.[11]

After identifying the relevant donor pool J_i for each displaced worker i, we construct synthetic control weights w^*_{ij} for each $j \in J_i$ that minimize the difference in pre-treatment outcomes between worker i and their synthetic control during the four years leading up to the year of displacement (backdated by one year to account for potential anticipation effects). These weights are based on continuous measures of age, firm size in the fifth year before closure, and annual earnings during the pre-intervention period.[12]

Using these synthetic controls, we estimate the effect of firm closure on worker earnings ($\hat{\tau}_{it}$) as follows:

$$\hat{\tau}_{it} = \left(Y_{it} - \sum_{j \in J_i} w^*_{ij} Y_{jt} \right) \tag{1}$$

where Y_{it} represents the annual earnings of worker i in year t, and Y_{jt} are the donor outcomes in year t.[13] By constructing an explicit synthetic control group

[10]We arbitrarily selected 20 donors to reduce the computational burden, though our results are robust to using 10 or 30 donors. Note, however, that the set of individuals with nonzero weights in constructing the synthetic control for each worker is invariably less than the 20 possible donors.

[11]To further reduce the influence of unrelated transitory shocks, we impose additional restrictions that match our restrictions on displaced workers: all treated workers and donors must have had at least two years of tenure, five years of positive wage observations, and worked in firms with at least 50 employees before the firm closure. Moreover, we exclude firms that exhibited size changes greater than 30 percent prior to the closure.

[12]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). Weights are also constrained to be positive and sum to one.

[13]For some displaced workers, it is not possible to find weights such that their pre-trends perfectly balance. We exclude the one percent of displaced workers with the most extreme negative and positive earnings deviations. Appendix F provides a robustness check that only includes displaced workers whose pre-trends never deviate by more than one percent from their average pre-treatment earnings.

that remains unaffected by the treatment throughout the observation window,[14] we avoid issues commonly encountered in two-way fixed effects models with multiple treatment times (e.g., Goodman-Bacon (2021a) and Roth et al. (2023)).

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 employees subject to social security contributions and recipients of unemployment benefits.[15] 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 IEB data with the Establishment History Panel (BHP), which provides firm-level information such as size, median wages, and industry for establishments with at least one socially insured worker as of June 30th each year.

We focus on individuals who had at least one employment spell in the private sector in West Germany between 2000 and 2005.[16] 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.[17] Identifying firm closures is challenging due to the po-

[14]We drop the small number of workers who suffer more than one firm closure during our observation period of 2000-2005

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

[16]We exclude firms in agriculture and mining.

[17]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.

tential for misinterpreting simple changes in establishment identification numbers as closures. To accurately identify genuine closures and exclude cases of mere administrative changes, we follow Hethey-Maier and Schmieder (2013) and consider 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 large employment fluctuations in the three years prior to closure.[18] At the individual level, we include workers who were between the ages of 21 and 55, had at least two years of tenure with their firm, and remained in the sample with 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. 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 includes 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 30th 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

[18] Specifically, we exclude firms where employment fluctuated by more than 30 percent in any of the three pre-closure years.

missing upper tail of the earnings distribution, we use a two-stage stochastic imputation procedure to estimate the missing upper tail of the earnings distribution.[19]

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, we follow the imputation procedure of Fitzenberger et al. (2006) to correct and impute missing values.[20]

Table 1 presents descriptive statistics for both displaced workers and the aggregate pool of nondisplaced workers. The first column presents statistics for the potential donor pool from which we select the most comparable workers for each displaced worker. The pool of nondisplaced workers meets the analytical restrictions for displaced workers including employment in large firms with a stable workforce, at least two years of tenure, and positive earnings for at least five years. The second column provides statistics for displaced workers one year before they leave the closing firm. Although displaced workers may differ from this broader pool of non-displaced workers, the subset of non-displaced workers assigned positive weight in the synthetic control analysis has, by construction, identical characteristics to the displaced worker sample for all of the worker and firm attributes listed in Table 1.

Our sample contains $16,135$ displaced workers who lost their jobs due to firm

[19]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.

[20]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.

closures between 2000 and 2005. The entire potential donor pool of non-displaced workers contains more than $560,000$ workers.

— Table 1 about here—

Displaced workers are found across the German economy. On average, non-displaced workers earn approximately $48,000$ Euros per year while displaced workers only earn approximately $45,000$ Euros. Non-displaced workers in the potential donor pool are also slightly more likely to be female, have less tenure, and are somewhat older. Moreover, displaced workers are slightly less likely to have a university degree and more likely to have completed an apprenticeship. Most displaced workers were employed in the manufacturing sector (45 percent), the wholesale and retail sector (22 percent), or the construction sector (16 percent). Although there are statistically significant differences in the distribution of workers across industries, these differences are qualitatively modest.

4 The distribution of displacement losses

By constructing a synthetic twin for each displaced worker, we can examine the full distribution of economic losses experienced by displaced workers. We proceed in four steps. First, we illustrate the methodology by focusing on workers displaced from a single manufacturing firm, providing a concrete example of how we construct and interpret individual-level synthetic controls. Second, we show that our method reproduces average earnings losses that closely match estimates from conventional event-study approaches. Third, we estimate the overall distribution of earnings losses across the universe of closures in our sample. Finally, we explore heterogeneity in displacement losses by examining how outcomes vary across different worker subgroups.

Throughout these analyses, we measure 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.[21]

4.1 Case study of a single manufacturing firm

To illustrate and motivate our focus on the heterogeneity of outcomes for displaced workers, we highlight the economic losses to workers of a single manufacturing firm that closed between 2000 and 2005. For this firm, 30 displaced workers met our criteria of having at least two years of tenure at displacement and positive wages throughout the five years before leaving the closing firm. All of these workers were men. Twenty of them held an apprenticeship degree, and ten of them had no degree in the year before the firm closed. The majority (24 out of 30) held jobs in the occupation of industrial process and plant engineering for ceramic materials, while five were machine builders, and one was an accountant.

On average, these workers experienced earnings losses of approximately [NTD] Euros in the first year after the closure (figure 1) – approximately [NTD] percent of pre-displacement earnings. Five years out, average earnings for these workers had not recovered to pre-closure levels.

— Figure 1 about here—

However, focusing on average losses masks 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 shows the displaced worker's earnings, while the dashed line represents the counterfactual earnings of their synthetic control. The figures confirm a good pre-closure fit between the synthetic control estimates and actual earnings. Post-closure, however, trends diverge significantly across workers.

[21] 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.

Roughly one-third experience immediate and sharp earnings losses. Earnings for some of these workers recover, while earnings losses for others grow over time. But a significant fraction of workers recover quickly after initial losses, and some exhibit earnings as high as—or even higher than—their synthetic controls.[22] These divergent patterns lead to stark differences in economic outcomes: some workers suffer 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 their synthetic controls' trajectories.

This stark variation among observably similar workers underscores the substantial heterogeneity in earnings responses to 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 full sample of firm closures shows the highly skewed distribution of economic losses experienced by displaced workers. 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.

By construction, there are no average pre-treatment differences between the earnings of displaced workers and their synthetic controls, as our methodology imposes balanced pre-trends. However, following displacement, average earnings losses rise

[22] Figure F.4 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 2) to placebo losses for non-displaced individuals in each worker's donor pool. As expected, the placebo estimates show parallel pre-trends and no treatment effects post-closure.

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—

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. Importantly, these pre-treatment distributions do not systematically correlate with the post-treatment distributions of displaced workers' earnings losses (Appendix E).

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.

Figure 4 further highlights the bimodality of the loss distribution by comparing the five-year accumulated earnings losses of displaced workers to a normal distribution. The distribution of accumulated earnings is markedly left-skewed (with skewness of -0.43). On average, displaced workers experience a loss equivalent to 1.26 years of earnings over the five years post-displacement, but the modal loss is significantly lower, at just -0.28 earnings years.

— Figure 4 about here—

[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 more significant earnings and wage reductions (e.g., Hijzen et al., 2010).

Interestingly, a non-negligible share of displaced workers actually profits from displacement, earning more than their synthetic controls in the long run. Over the five years post-closure, nearly one-fifth ($3,631$ individuals) of displaced workers exhibit positive earnings gains relative to their non-displaced controls. While this result may seem counter-intuitive, 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. Noneconomic factors could enter: workers may switch to higher-paying jobs when forced to change jobs – overcoming inertia from friends and family, locational preferences, and the like. Or, they might have previously underestimated the benefits of job mobility, as suggested by recent evidence from Germany (Jäger et al., 2024).

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 small control samples that mimic our synthetic controls. Given the nature of our synthetic control approach, the influence of random outliers in these samples will be larger than in our main sample. Nevertheless, the distributions of estimates from these alternative control samples consistently present a similar picture, with similarly shaped distributions of earnings losses (Appendix C).

4.3 Earnings patterns across deciles

By plotting earnings patterns by decile of accumulated losses, we show the significant heterogeneity of outcomes following displacement. Figure 7 divides the sample of displaced workers into deciles based on their accumulated five-year losses and plots the pattern of average annual earnings losses that underlie the overall distribution. The dark line in each panel summarizes the average annual earnings loss for displaced workers in each decile, while the dashed lines show earnings patterns of the relevant synthetic controls.

Workers in the six deciles with greatest earnings losses suffer significant setbacks at the time of the firm closure and, on average, do not recover to their pre-closure earnings path. Conversely, the average earnings for workers in the top two deciles actually 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.

To assess whether these losses (or gains) are meaningfully different from what might have been expected in the absence of firm closure, we perform the placebo method for inference proposed Abadie et al. (2010). For each displaced worker, we select at random a control worker from the donor pool that contributes non-zero weight to the displaced worker's synthetic control. We call these workers our placebo workers. For each placebo worker, we estimate placebo earnings losses using the same synthetic control approach described in Section 2. This procedure yields more than $16,000$ placebo estimates.

[NTD - check that this is being described correctly]

We plot the placebo synthetic control estimates as the thin, light series in Figure 7. We split the placebo workers into deciles based on cumulative placebo earnings losses. Figure 7 plots fifty randomly selected placebos within each decile.[24] Darker gray shades indicate a higher concentration of overlapping placebo estimates, while lighter shades represent fewer overlaps.[25]

[24]Even if a synthetic control closely aligns with the pre-intervention trajectory of the treated unit, pre-intervention trajectories might not closely align for all donors. To address this challenge, we follow Abadie et al. (2010) and exclude any potential donor within each decile whose pre-intervention mean squared prediction error (MSPE) exceeds five times the average MSPE of the displaced workers. This adjustment reduces the placebo estimates by approximately one percent within each decile.

[25]We plot individual earnings losses rather than averages to illustrate how extreme some of the displaced worker earnings loss estimates are relative to the distribution of placebo workers. We plot an alternative summary of the placebo test in figure F.3, where we split the placebo workers into deciles based on cumulative earnings loss, then draw ten ten-percent samples within each decile and plot their average earnings loss. The placebo lines draw starker contrast to the differences between displaced and placebo worker earnings losses, especially for workers with the largest losses and those with higher earnings post-closure.

At the top and bottom of the distribution, the earnings of displaced workers differ sharply from those in the permutation distribution. For displaced workers in the five deciles of greatest earnings loss, the average losses fall far outside the range of placebo workers' earnings losses. For deciles six to nine, the average effects move closer to the center of the placebo distribution, suggesting that the displaced workers' earnings losses are not statistically different from zero in the medium run. In contrast, for the tenth decile, we observe that the average effect once again stands out as extreme relative to the placebo distribution, suggesting that a portion of workers achieves even higher earnings post-closure than would have been expected had their firm not closed.

—Figure 7 about here—

4.4 Earnings loss heterogeneity by worker and firm characteristics

Our estimates are consistent with previous research that has shown that the average earnings losses from job displacement vary significantly 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, they 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. The new element is an investigation of the extent that observationally similar workers within the same occupation or firm experience similar earnings losses.

Figure 5 shows the heterogeneous losses of cumulative earnings (scaled to 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 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 red vertical lines, align with the average losses documented in the literature. Specifically, we find that displaced workers with lower education levels experience substantially larger 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 (1.14 years), and women experience larger losses (1.6 years) than men (1.15 years).

But conclusions about the incidence of large losses must be tempered by the substantial overlap of losses across all subgroups that reveals significant within-group heterogeneity. Even among women, low-educated workers, and older workers—who, on average, experience larger losses – a significant 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 indicates that a relatively large minority of women may withdraw entirely from the labor market, earning nothing in 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 results both within and across the loss distributions. As summarized in Table 2, nobody is immune from possible losses but many do escape relatively unharmed.

— Figure 5 about here—

— Table 2 about here—

The striking overlap in the distribution of earnings losses across subgroups suggests that observable 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:

$$L_i = X'_{i(-1)}\beta + \theta_{i(-1)} + \vartheta_{i(-1)} + r_{i(-1)} + u_{i(-1)} \tag{2}$$

where the dependent variable $L_i = \sum_{t=1}^{t=5} Loss_{it}$ represents worker i's cumulative earnings losses over the five years following firm closure. 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 firm, three-digit occupation, and municipality fixed effects, respectively. The error term is denoted by $u_{i(-1)}$. We then decompose the variance of the accumulated earnings losses as follows:

$$\begin{aligned} Var(L_i) = & Var(X'_{i(-1)}\hat{\beta}) + Var(\hat{\theta}_{i(-1)}) + Var(\hat{\vartheta}_{i(-1)}) + Var(\hat{r}_{i(-1)}) + \\ & 2Cov(X'_{i(-1)}\hat{\beta}, \hat{\theta}_{i(-1)}) + ... + 2Cov(X'_{i(-1)}\hat{\beta}, \hat{r}_{i(-1)}) + Var(\hat{u}_{i(-1)}) \end{aligned} \tag{3}$$

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.

While unsurprising given the distributional overlaps in Figure 5, fixed individual characteristics explain little of the variance in the economic impact of displacement. Table 3 presents the decomposition results. The first column shows the variance decomposition for the entire sample, revealing that observable pre-displacement characteristics (e.g., education, gender, age, firm, and occupation fixed effects) explain only 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 closing firm of the displaced worker is the strongest predictor of earnings losses, followed by pre-displacement occupation. This formulation addresses whether workers who are displaced from *the same firm* experience similar earnings losses because it includes just the closing firms and not any subsequent employing firms, but it does not fully consider the role of firms in displacement losses. In Section 6, we directly address how switching to a firm with a lower AKM is a driver of wage losses.

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

— Table 3 about here—

On methodological grounds, however, these results could 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 3 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 stems from differences in individual post-displacement career paths rather than noise in our synthetic control group estimates.

When we extend the variance decomposition to different subgroups, we find substantial variation in the relative explanatory power of pre-treatment worker and firm characteristics.[26] For example, pre-displacement firms explain a larger fraction of earnings losses for low-educated workers than for those with medium or high

[26]See Appendix Table F.1.

education, while occupations play a more significant role in explaining variance among high-educated workers. Nonetheless, the dominant conclusion that these observable characteristics explain little of the variances in losses remains.

5 Alternative estimation approaches

The primary 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 in the distribution – something made possible by our estimation of the entire distribution of losses.

A more recent approach has been to apply machine learning models to map heterogeneous treatment effects based on high-dimension interactions of observable characteristics of displaced workers. We reproduce this approach and show that it underestimates the variance and pattern of losses that we previously identified.

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 literature.

In order to compare our synthetic control estimates to those from a standard event study, we use propensity score matching to pair displaced workers in the treatment group with non-displaced workers in the control group. 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, we restrict the sample to workers who were employed at large firms with more than 50 employees and had at least two years of tenure before their displacement year.

We run the following event studies for each displacement year between 2000 and 2005 separately and aggregate the coefficient estimates using the observations from each separate regression as weights.

$$Y_{it} = \alpha + \lambda_t + \sum_{k=-5}^{5} \delta_k + X_{it}\beta + \epsilon_{it} \tag{4}$$

In this equation, Y_{it} is annual earnings; λ_t are year fixed effects; $\sum_{k=-5}^{5} \delta_k$ are event-time dummies capturing earnings trajectories from five years before until five years after firm closure; 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, 2021b).

— Figure 6 about here—

By using our information about where each displaced worker falls in the distribution of losses, we can actually expand on standard event studies by looking not only at average losses but also at losses at the top and bottom of the distribution (Figure 6). The first plot includes 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 second and third event studies, we use the synthetic control estimates to restrict the sample to the quartiles of workers with the largest and smallest losses, respectively. Within each quartile, 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 loss distribution.[27]

[27]Note that these quartiles are based on the estimated distribution of losses from the synthetic

The figure reveals a striking similarity between the two sets of estimates for the full sample of displaced workers and the subgroup experiencing the largest losses. For the smallest losses (based on the synthetic control estimates), the synthetic control group approach produces slightly larger gains than the event study approach. It is not obvious whether estimates from either of the methods is more biased at the upper tail of the distribution. However, Arkhangelsky and Hirshberg (2023) show that the synthetic control group approach is even less biased than regular diff-in-diff estimators under many circumstances.

5.2 Heterogeneous treatment with machine learning methods

A growing literature has begun using machine learning techniques to estimate heterogeneous treatment effects based on observed characteristics. Most recently, 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 focus on heterogeneous conditional average treatment effects (CATEs), capturing high-dimensional interactions among workers' pre-treatment characteristics.

The GRF approach has two notable advantages. First, it can estimate heterogeneous treatment effects within 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 the distribution of these losses. 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, one limitation of GRF is that the resulting CATE estimates are not designed to map directly into subgroup-level levels of earnings losses.[28] In light of

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.

[28]For example, Athey et al. (2023) show that while the CATE estimates successfully rank workers

that, we compare rankings generated by our synthetic control approach to those from a GRF model, to see whether the two methods yield similar impressions of how much earnings losses vary across workers.

To implement this comparison, we train a causal forest on our German displacement data. First, we 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. We then estimate CATEs via a GRF, where the outcome is the ratio of actual earnings one year after closure to earnings one year before closure, and the covariates include [NTD: specify covariates].[29]

These GRF-based CATEs yield an ordinal ranking of workers by predicted displacement losses. We compare that ranking to our synthetic control-based ranking, where losses are defined as the difference between actual and synthetic control estimated earnings in the year after firm closure, expressed as a percent of earnings in the year before firm closure.

In Appendix Table F.2, we split workers into deciles by each ranking and, for each decile, report the average raw (unadjusted) earnings losses from year -1 to year $+1$. The GRF ranking produces a narrower range of losses (from -0.47 to -0.11) than the synthetic control ranking (from -0.83 to $+0.18$). The compression in the distribution of GRF-ranked earnings losses reflects the fact that the GRF must rely on observables that, as shown in Section 4.4, explain only a modest share of the total loss variation. Accordingly, many workers with actually disparate losses end up averaged together in the same subgroup, resulting in more muted high and low tails. Indeed, although the raw earnings losses in these deciles are not themselves causal estimates, it would require stark assumptions about counterfactual earnings growth to reconcile the much flatter distribution from the GRF with the more dispersed

by predicted earnings losses, the absolute levels can underestimate the full spread in losses (p. 20).

[29] 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 [NTD: details on training, tuning, software packages, etc.].

distribution from the synthetic control.

In sum, GRF methods are highly valuable for detecting complex, nonlinear interactions among observed covariates and for highlighting which variables have the most explanatory power. But they necessarily group workers who are similar on those observables, even if the heterogeneity in their outcomes is not explained by available observable characteristics. Consequently, GRF-based estimates of displacement losses may understate the true dispersion when, as in our setting, large portions of the variation arise from unobserved factors.

6 Adjusters and casualties

It is informative to shift 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 observed behavior of individuals in the top quartile of losses ("casualties") with those in the bottom quartile of losses ("adjusters"). The goal here is not to identify causal mechanisms behind the different outcomes but to characterize key observed behavioral choices made during the adjustment process as a benchmark for further research into these dynamics.

6.1 Labor market trajectories of adjusters and casualties

As expected from the prior analysis of differences in losses by individual characteristics, adjusters and casualties differ on average in baseline characteristics. Adjusters are more likely to be male and posses higher levels of education (Table 4). They are also found in slightly larger and higher paying firms. Finally, consistent with Schwerdt (2011), we find that 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 4 about here—

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 adjust effectively or even benefit from a layoff by finding better-paying jobs or jobs with work more hours. Differences in the recovery paths of adjusters and casualties become apparent by the first year post-closure. Table 5 compares the wage[30] and employment trajectories of adjusters and casualties relative to their synthetic control. Adjusters swiftly secure jobs with wages comparable to or higher than their counterfactuals 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 exceeding expectations. These outcomes align with Figure 7, which shows that workers in the lowest loss deciles capitalize on opportunities created by the closure (Farber, 2017).

—Table 5 about here—

In contrast, casualties face prolonged and often incomplete recovery, as illustrated in the right panel of Table 5. 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 significantly below their synthetic controls.

Notably, in the fifth year post-closure, casualties make up three-quarters of the workers in the quartile with largest earnings losses. Only 7 percent of casualties

[30]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 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.

achieve wages comparable to those in their pre-closure firm. This suggests that casualties are not merely those who temporarily exit the labor force, and therefore suffer earnings losses when they are not employed, but are workers who face persistent earnings penalties upon re-entering employment.

6.2 *The role of firm transitions*

Previous research has highlighted that establishment effects account for a significant 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 8 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 regressions, 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 explains about 40 percent of the average wage losses among displaced workers.[31]

For adjusters and casualties, however, differences in firm AKM account for a smaller portion of wage changes. 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.

[31] 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 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).

This suggests that adjusters improve their outcomes by securing better roles 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 not only transition to inferior firms but also accept lower-quality positions at those firms.

—Figure 8 about here—

6.3 Differences in adjustment behavior

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

Adjusters and casualties generally make a similar number of transitions, but adjusters move quickly and decisively than casualties, who struggle to find new positions. Figure 9 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 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 reentering 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 majority remaining non- or unemployed. As casualties gradually reenter the labor force, 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 9 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 9). 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.

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. Conversely, adjusters demonstrate substantial flexibility immediately after displacement, suggesting an ability to transfer human capital effectively across occupations.

Geographic mobility across 50 large German local labor markets also differs (lower-right panel). While geographic moves are relatively rare, adjusters are more likely to relocate immediately after displacement. Over time, both groups change regions to a similar extent, but adjusters consistently exhibit greater initial mobility.

Overall, Figure 9 shows that adjusters demonstrate significant flexibility in the short run, while casualties struggle to secure employment 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 more likely to be less educated and slightly older than adjusters, factors that may limit their flexibility (see Table 4). We can further refine the comparisons by analyzing whether workers with identical pre-treatment characteristics respond differently to the same displacement shock. 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 10), the results are virtually unchanged. Adjusters move decisively into their next labor market position, whereas casualties are slower and more prone to ineffective transitions.

—Figure 10 about here—

Taken together, this section highlights the substantial variation in earnings losses and labor market trajectories for displaced workers. While many workers experience large and persistent losses, nearly a quarter adjust quickly, landing stable jobs 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 significant 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 demonstrate greater flexibility through quicker firm and occupation switching. While these transitions may be costly in the short term, they often lead to more stable employment and better long-term outcomes.

7 Heterogeneity and other margins of adjustment

Public discussions frequently point to other elements as being important factors to explain 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 F.2). 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 is their greater willingness to acquire additional human capital by returning to school after a layoff. Losing one's job reduces the opportunity cost of pursuing further education or training in a new field. The pursuit of education or training after displacement could explain part of the divergence in earnings recovery between the two groups.

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 non-adjusters 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 is not tracked through administrative records; instead, it is updated when a worker is hired by a new firm, and these updates are infrequent. As a result, it is unlikely that we observe educational updates during a given employment spell, 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 consisting of 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 mass layoffs, and that adjusters are no more likely than non-adjusters to pursue additional education (Appendix Table F.3). In fact, 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. (2020), 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 very 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 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 distributions of earnings losses imply that average earnings losses, as commonly estimated using classical event studies, significantly overstate the losses for the large fraction of workers who readily adjust to the closures. At the same time, the averages miss the extent of loss for the minority of workers who are catastrophically impacted by firm closures.

Worker and firm characteristics commonly observable to the researcher explain only a small fraction of the workers' displacement losses. Looking at the behavioral differences between the economic winners (adjusters) and economic losers (casualties) indicates that adjusters quickly find stable new circumstances – changing occupation, industry, and geographic region immediately if necessary. Casualties are slower to adjust and frequently do not move into stable situations.

Because those who are truly harmed by firm displacements are difficult to identify ex ante, policies to deal with firm closures must necessarily be more refined to deal with displaced workers after market opportunities unfold.

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Figures in the Text

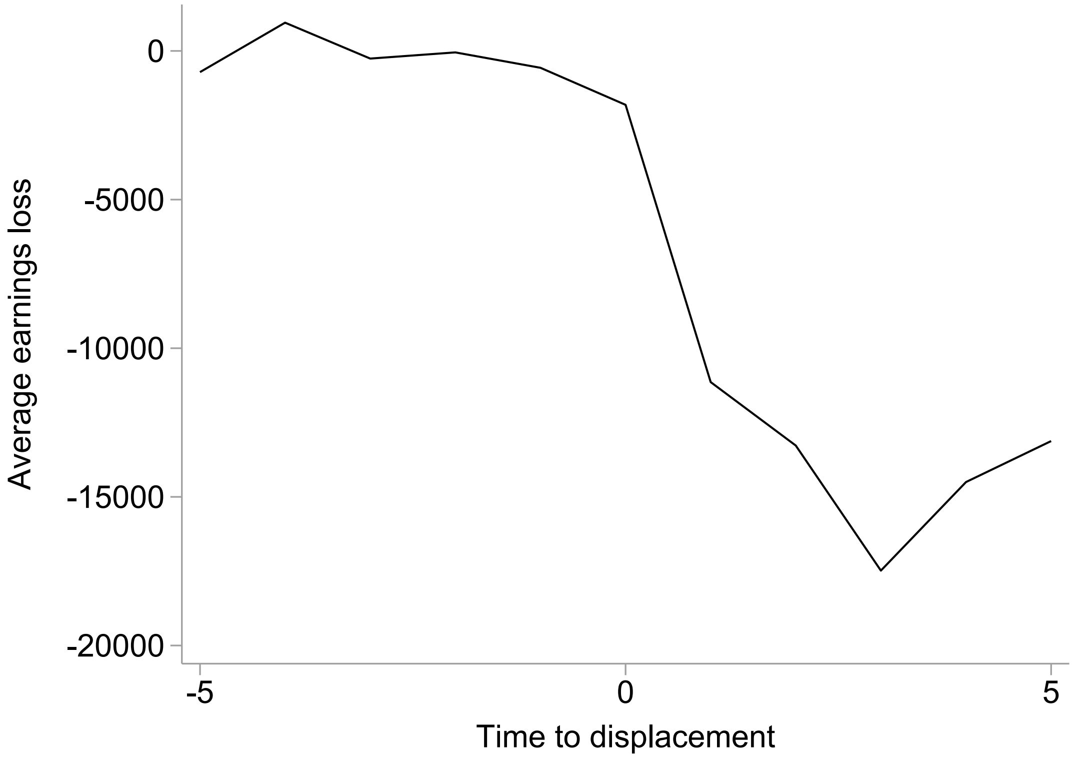

Figure 1: Average earnings loss of a closing firm in the manufacturing sector (case study)

Notes: The figure displays the average earnings losses of the displaced workers of one single closing firm in the manufacturing sector. 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.

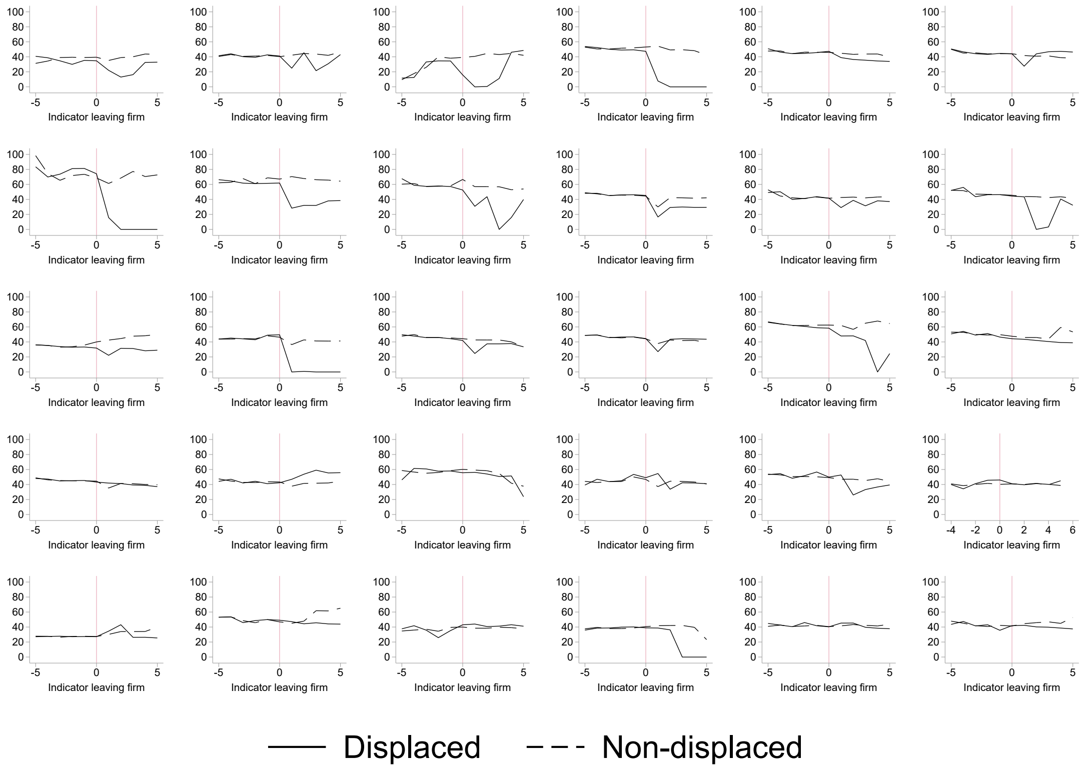

Figure 2: Individual earnings loss of a closing firm in the manufacturing sector (case study)

Notes: The figure plots the estimated earnings losses of 30 workers displaced from a single closing firm in the manufacturing sector. In each panel, the solid black line plots the earnings losses (in 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 firm closure in years. Source: IEB.

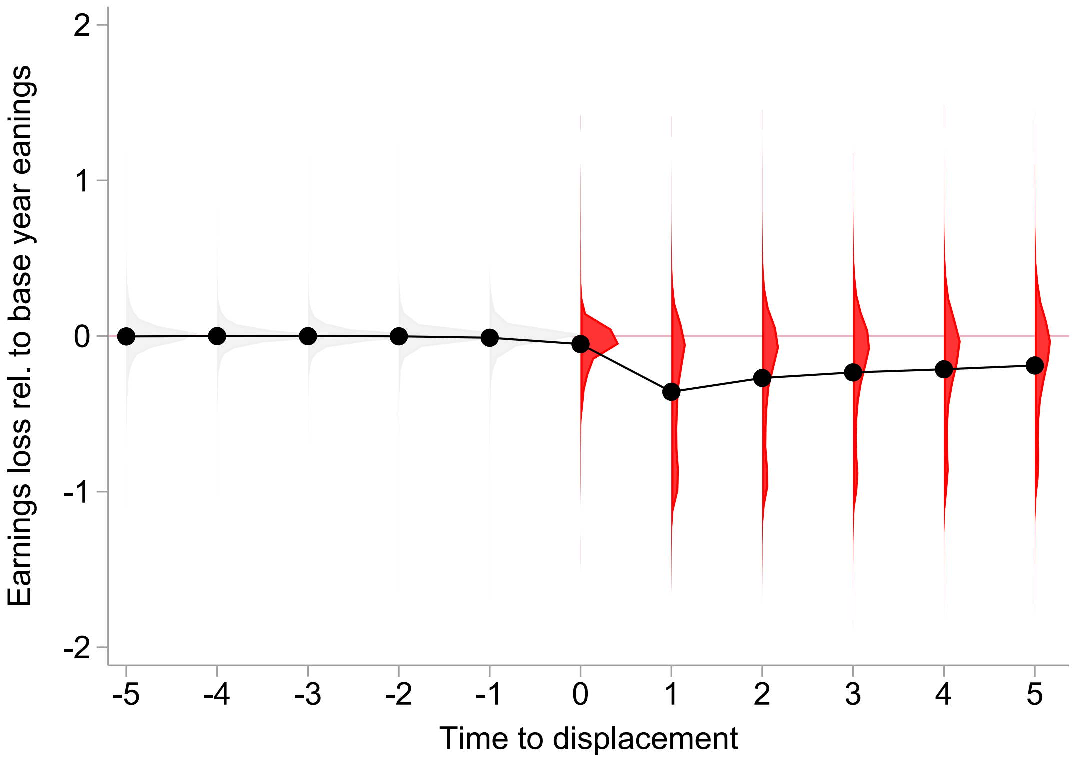

Figure 3: Distribution of relative earnings losses after firm closure (unconditional)

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 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 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.

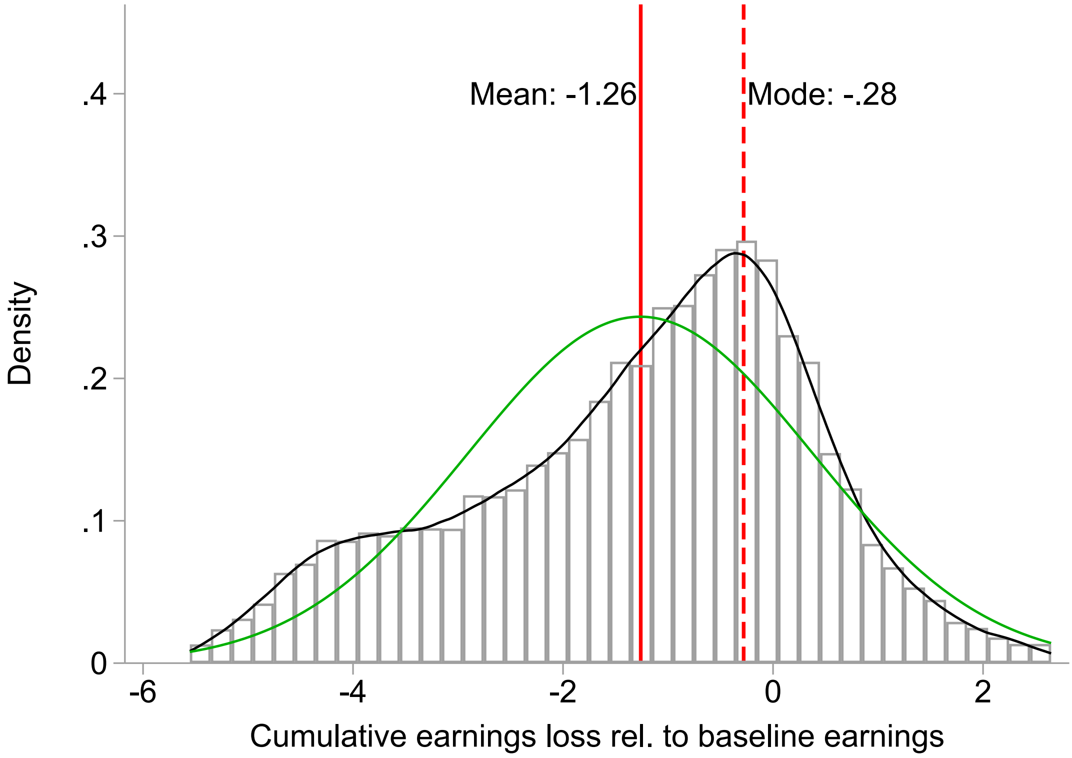

Figure 4: Distribution of five-year cumulative earnings loss relative to normal distribution

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.

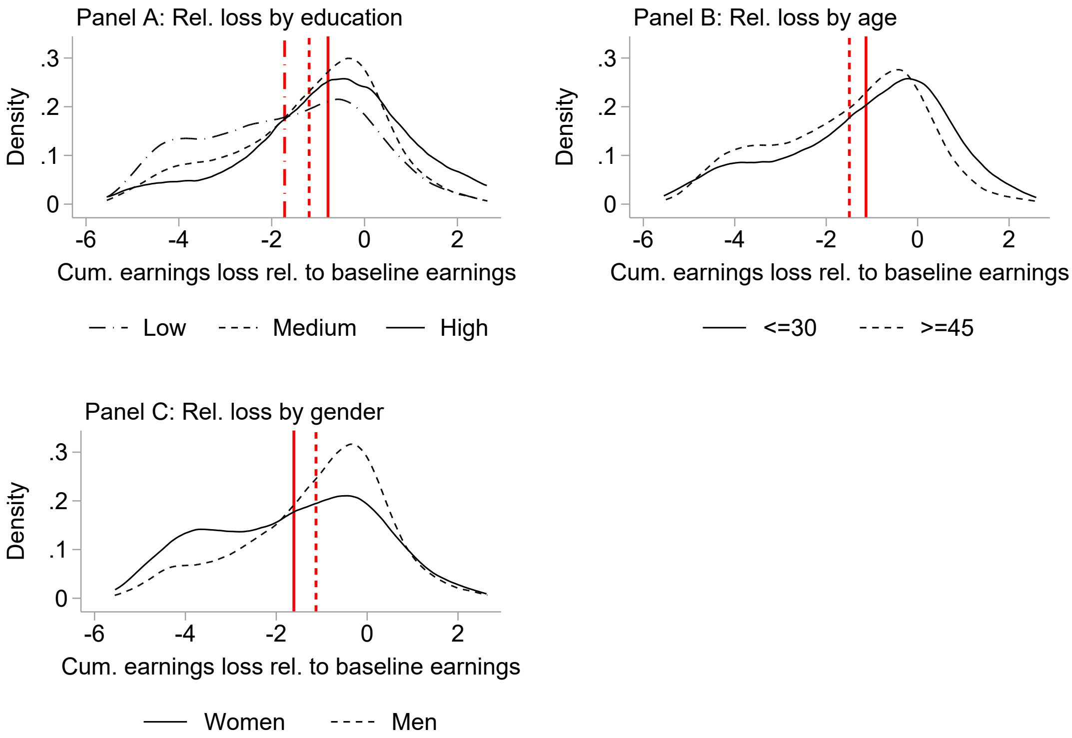

Figure 5: Distributions of earnings losses by pre-treatment characteristics

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.

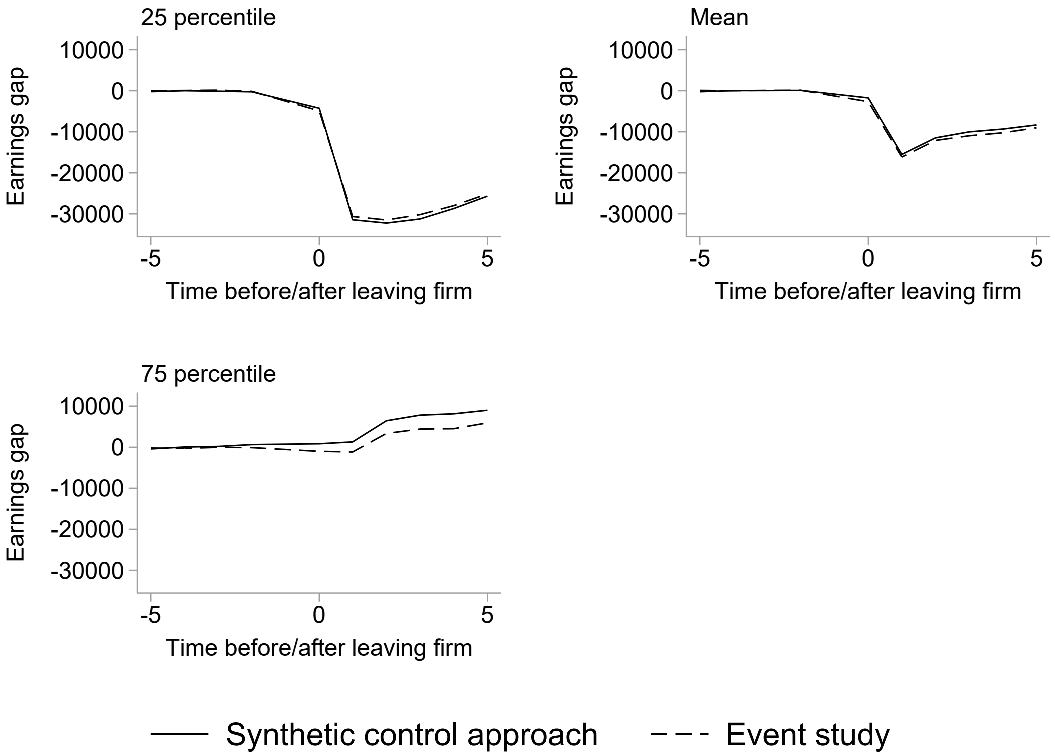

Figure 6: Regular event study vs. synthetic control group approach

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 propensity score weighting 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, earnings for workers in the quartile with largest cumulative earnings losses (25 percentile), and earnings for workers in the quartile with smallest cumulative earnings losses (75 percentile).

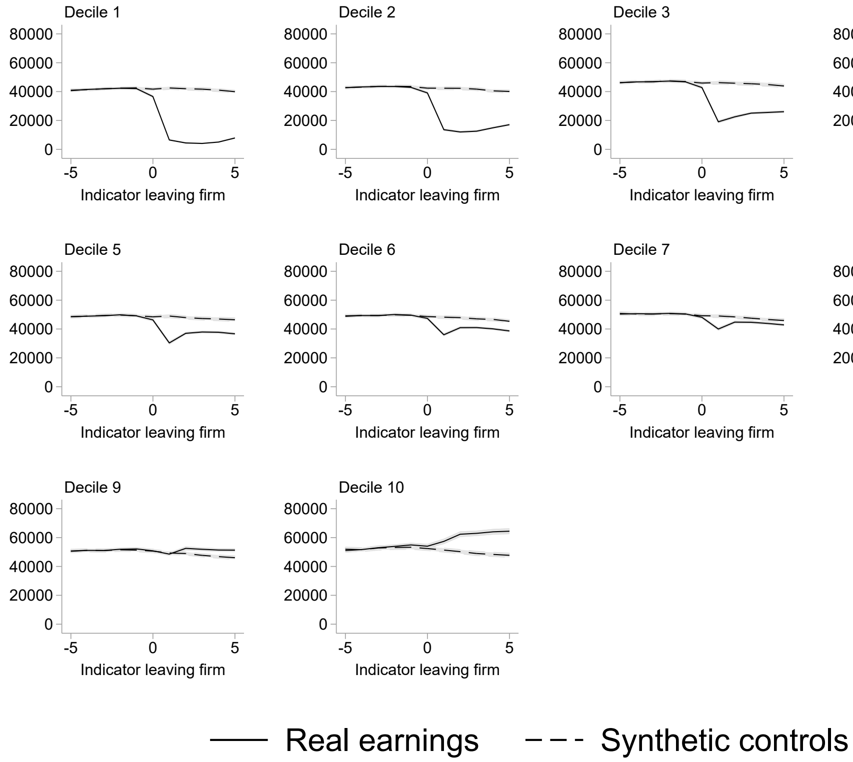

Figure 7: Average earnings loss for treatment and synthetic control group at deciles of the cumulative loss distribution

Notes: The figure displays the average earnings losses for the treatment and synthetic controls within the deciles of their loss distribution. We bin treated workers into deciles according to estimated cumulative earnings losses in the five years post-firm closure as a percentage of pre-closure earnings. The first sub-figure displays the average earnings of displaced workers and their synthetic controls for those workers whose earnings losses lay below the ten percent decile of the total loss distribution. The second figure presents the same results for the second decile and so forth. Each panel compares the average earnings losses for displaced workers to earnings losses for placebo workers selected from the donor pools of the displaced workers. We estimate placebo earnings losses using the synthetic control method, split placebo workers into deciles based on cumulative earnings losses, then select 100 workers for each decile of earnings loss and overlay their loss estimates as the light lines in each panel. The lines are darker where the placebo losses overlap.

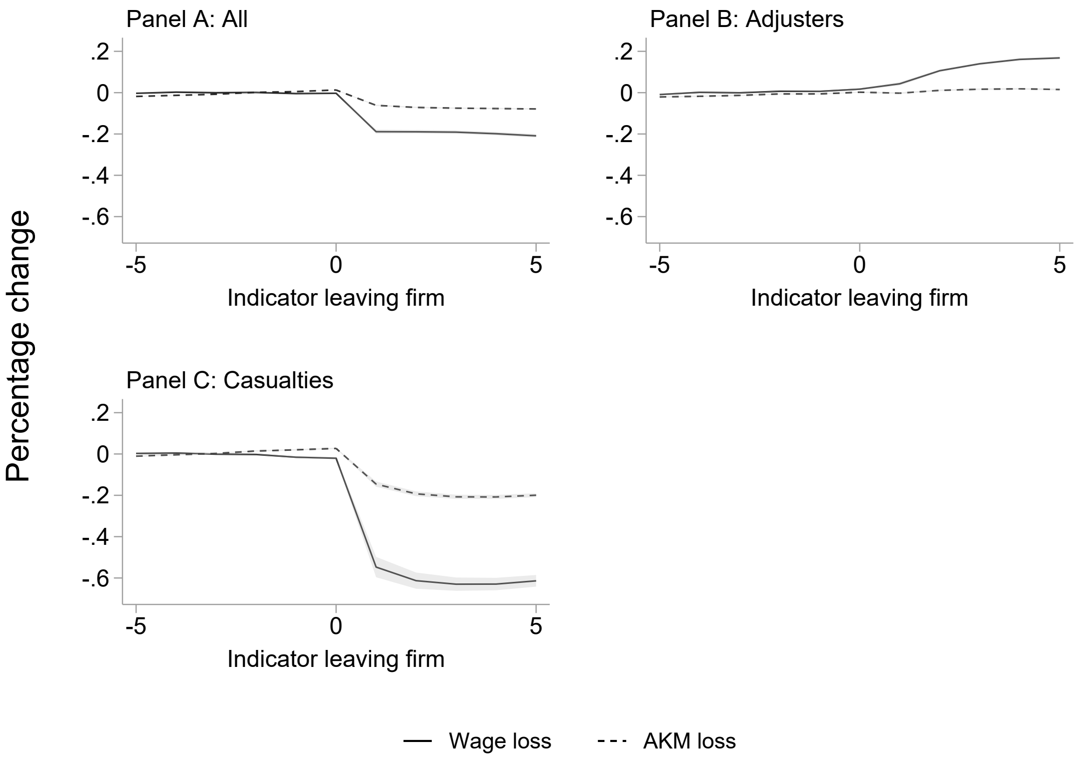

Figure 8: Contribution of closing firm fixed effect (AKM) to wage losses

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 $exp^{ln(wage)} - 1$. Bootstrap standard errors are plotted as the shaded region.

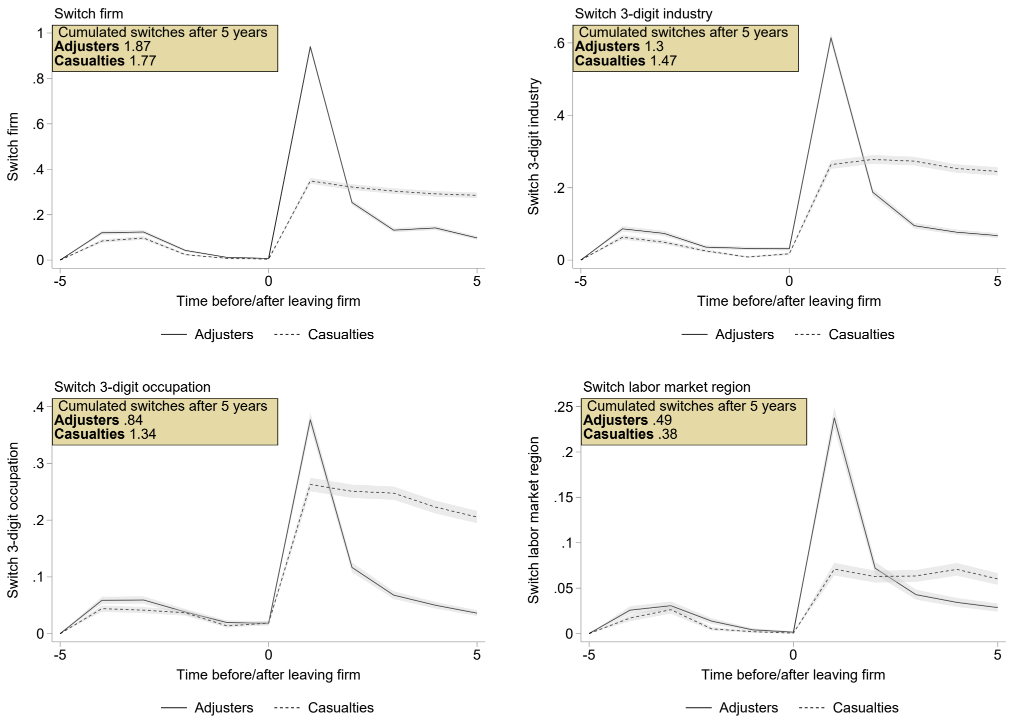

Figure 9: Fraction of workers who switch firm, industry, occupation, and region after firm closure

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. Bootstrap standard errors are plotted as the shaded region surrounding each line.

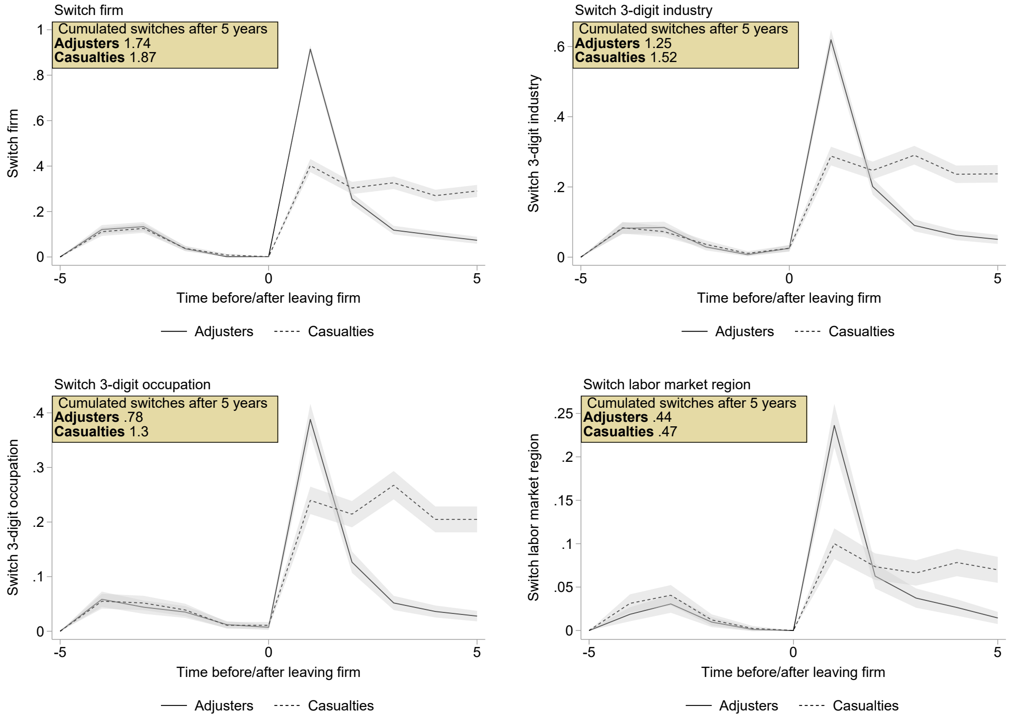

Figure 10: Fraction of workers who switch firm, industry, occupation, and region after firm closure (matched sample)

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. Bootstrap standard errors are plotted as the shaded region surrounding each line.

Tables in text

	Non-displaced [1]	Displaced [2]
Worker characteristics		
Annual labor earnings	48,381	45,737
Percent Female	0.318	0.290
Tenure in current job	3.6	6.6
Age (in years)	39.4	39.2
Education (% of individuals)		
Low educated (no vocational degree)	0.190	0.137
Medium educated (apprenticeship degree)	0.746	0.838
High educated (university degree)	0.064	0.025
Firm characteristics		
No. employees total	554	152
Industry (% of individuals)		
Manufacturing	0.457	0.449
Wholesale and retail	0.170	0.217
Construction	0.093	0.165
Individuals	567,508	16,135

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

Notes: The table summarizes characteristics of displaced workers (all workers at a German firm that closed between 2000-05) and non-displaced workers. Source: IEB 1984-2010.

	All	Education			Gender	
		Low	*Medium*	*High*	*Women*	*Men*
Mean	-1.277	-1.842	-1.198	-0.798	-1.606	-1.143
Mode	-0.405	-0.284	-0.284	-0.466	-0.494	-0.334
Skewness	-0.419	-0.057	-0.476	-0.466	-0.105	-0.537
25th Percentile loss	-2.371	-3.273	-2.209	-1.781	-3.106	-2.070
75th Percentile loss	-0.087	-0.466	-0.052	0.346	-0.209	-0.053
% Lose $<$ 1 month	0.249	0.166	0.260	0.354	0.222	0.260
Individuals	16,135	2,238	13,499	398	4,678	11,457

Table 2: Characteristics of relative loss distribution

Notes: The table summarizes moments from the distribution of cumulative earnings losses for displaced workers. Losses are log differences for displaced workers relative to their synthetic control over the five years following firm closure. Education takes three levels: low educated (less than an apprenticeship), medium educated (apprenticeship), and high educated (university degree). The first column gives the proportion of the variances in losses associated with each listed factor. The second column gives the proportion of variance in annual earnings associated with each factor of the synthetic controls.

	Share of variation	
	Treated earnings losses	Earnings of synthetic controls
Individual char.	0.016	0.101
Education	0.001	0.081
Pre-displacement firm f.e.	0.125	0.265
Pre-displacement occupation f.e.	0.030	0.172
Pre-displacement region f.e.	0.006	0.052
Citizenship	0.006	0.002
Residual	0.830	0.291
Covariances	-0.014	0.037
Total variance of loss	1.000	1.000

Table 3: Variance decomposition of displacement losses

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.

	Casualties	Adjusters	Difference	p-value
Worker characteristics at time of closure				
Percent female	0.332	0.224	0.108	0.000
Tenure	5.6	5.5	0.148	0.001
Age (in years)	38.8	37.9	0.911	0.000
Log daily wages	4.6	4.7	-0.101	0.000
Education:				
Low	0.178	0.095	0.082	0.000
Medium	0.803	0.880	-0.076	0.000
High	0.019	0.025	-0.006	0.038
Closing firm characteristics				
No. employees	161	183	-21.9	0.021
Log firm wage	4.4	4.5	-0.065	0.000
Separation and loss				
Quarter of leaving before closure:				
Less than 1	0.729	0.698	0.031	0.001
2	0.208	0.235	-0.027	0.002
3 or more	0.063	0.067	-0.004	0.390
Relative loss (years of earings)	-2.5	-0.064	-2.4	0.000
Observations	4,839	4,841		

Table 4: Characteristics of adjusters and casualties

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).

	Adjusters					Casualties				
Years after closure	1	2	3	4	5	1	2	3	4	5
No wage										
Unemployed full year	1.1	0.2	0.2	0.2	0.4	35.3	39.9	37.4	31.9	23.4
Partial year employed	5.9	1.4	0.6	0.6	0.4	31.6	15.4	9.2	7.1	5.6
Wage loss > 50%										
Partial year employed	0.5	0.1	0.1	0.0	0.1	5.8	8.6	8.4	8.1	8.2
Full year employed	0.5	0.3	0.1	0.2	0.2	4.4	10.3	17.0	21.1	23.7
Wage loss 10-50%										
Partial year employed	2.4	0.4	0.2	0.3	0.4	7.2	10.3	9.2	8.2	8.6
Full year employed	8.2	5.7	3.8	4.5	4.7	5.9	9.1	14.9	19.3	23.5
Wage loss 0-10%										
Partial year employed	4.5	0.6	0.3	0.3	0.6	2.3	1.9	1.4	0.9	0.6
Full year employed	15.6	15.1	12.5	10.7	11.7	1.9	1.1	0.9	1.0	2.1
Wage gain										
Partial year employed	11.2	4.3	2.6	2.2	3.0	3.1	2.0	1.2	1.0	1.1
Full year employed	50.2	71.8	79.6	81.1	78.7	2.5	1.4	0.5	1.3	3.2

Table 5: Wage and employment states for adjusters and casualties (percentages)

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: non- or unemployed 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 manufacturing sector

In Figure A.1, we compare the earnings losses of a displaced worker (black line) to the placebo earnings losses of workers in the donor pool (light grey lines). For each worker (displaced and placebo), we plot the difference between annual earnings in a given year and baseline earnings, calculated as the average earnings over the three years preceding the firm closure (or placebo firm closure). The light grey lines generally cluster around no earnings change, while displaced workers who experience significant earnings losses stand out as clear deviations from the placebo trends.

B 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. The figures below compare the distributions of the estimated displacement losses with these placebo estimates at -5, -1, 1 and 5 years after displacement.

We first demonstrate that, prior to firm closure (or simulated firm closure for the placebos), the distributions of earnings differences between treated and synthetic control workers are distributed comparably for the displaced and placebo workers. Figure B.1 plots the pre-displacement distributions at five and one year(s) before

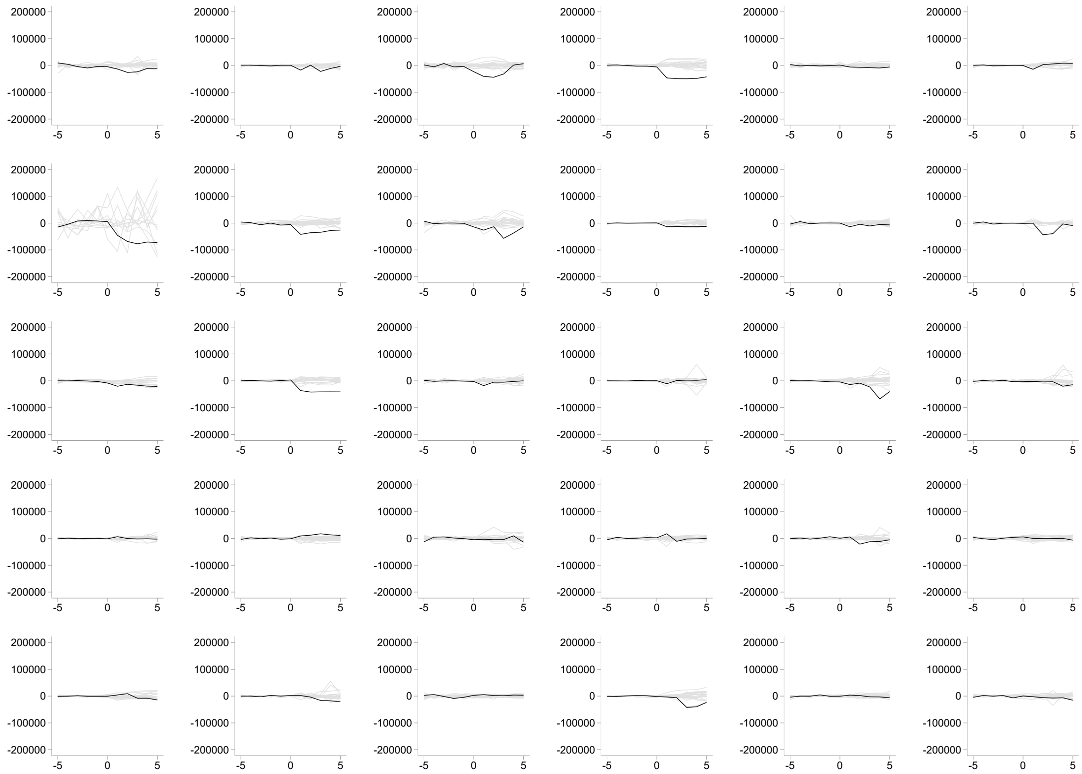

Figure A.1: Individual earnings loss of a closing firm in the manufacturing sector (case study)

Notes: The figure plots the estimated earnings losses of 30 workers displaced from a single closing firm in the manufacturing sector. 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.

workers exit their closing firms. The black line shows the kernel density estimate for the displaced worker loss distribution, while the gray shading shows the placebo distribution. If the synthetic control approach accurately fits the data, the two distributions should look alike, approximately centered at zero, and exhibit minimal variance. Figure B.1 confirms this prediction.

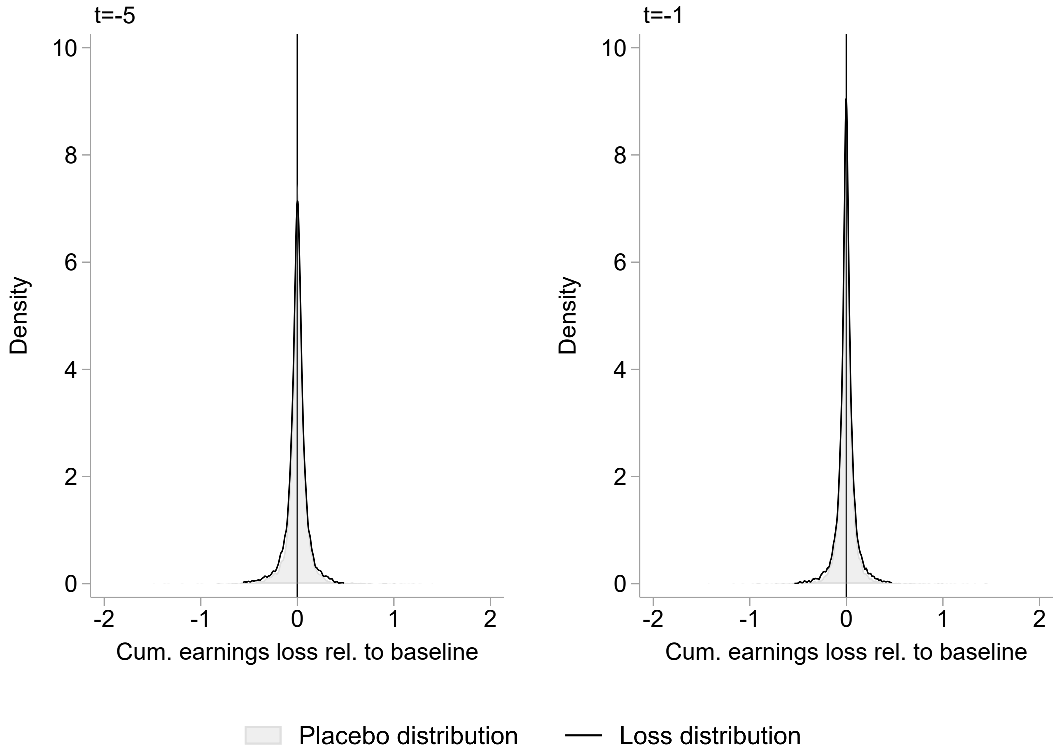

Figure B.1: Comparison between loss and placebo distribution pre-closure

Notes: The black lines display kernel density estimations for the distributions of displaced workers' earnings losses year before 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 gray shaded areas display the distribution of placebo estimates that we obtained by estimating the intervention separately for a selected sample of the donor pool. The left panel shows the results for the period of five years before a worker leaves the closing firm, and the right panel for the period of one year before. Source: IEB.

If the distribution of post-displacement earnings changes contains large negative losses or positive gains, the loss distribution should appear at the extremes relative to the permutation distributions. For workers with earnings losses, we would expect the permutation distribution to be centered close to zero (or bounded away from zero in the positive region, since treated workers can serve as donors in the placebo estimates). For workers with earnings gains, the permutation distribution should again be centered near zero (or bounded away from zero in the negative region).

Figure B.2 and Figure B.3 confirm this pattern. The distribution for workers who experience earnings losses shifts further from the placebo distribution's center than the distribution for workers who experience gains. Nevertheless, in both cases, the figures support that the observed post-displacement distributions reflect actual losses or gains rather than mis-specifications in the synthetic control procedure.

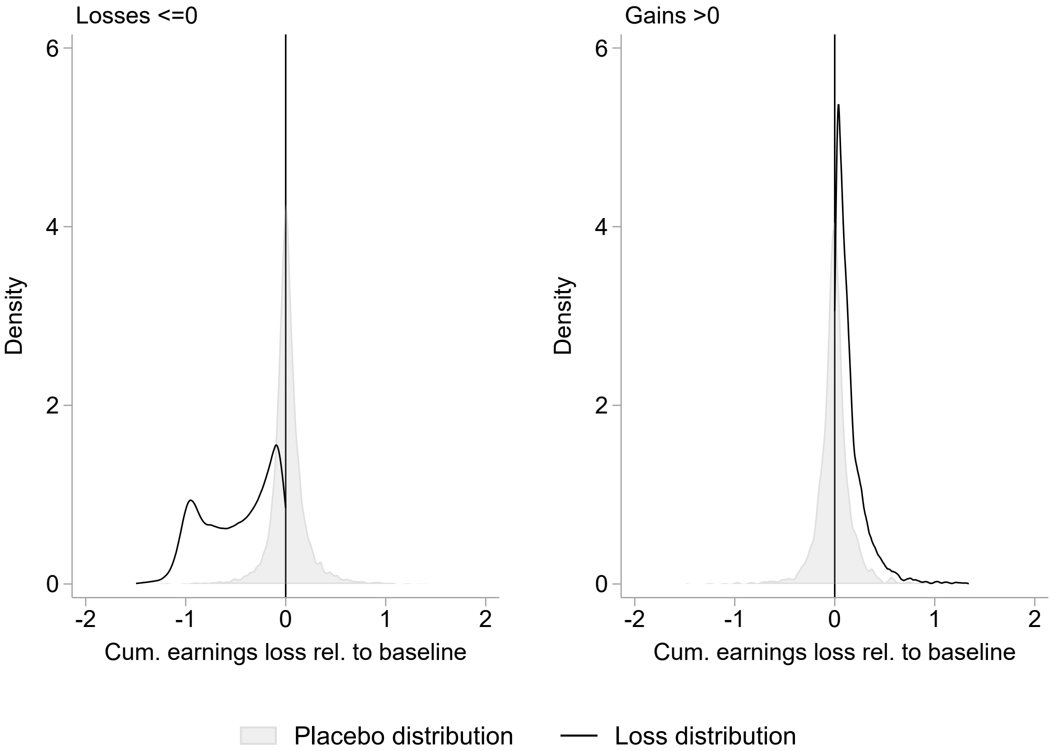

Figure B.2: Comparison between loss and placebo distribution one year after closure

Notes: The black lines display histograms of the distributions of displaced workers' earnings losses year before 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 gray shaded areas display the distribution of placebo estimates that we obtained by estimating the intervention separately for a selected sample of the donor pool. The left panel shows the results for displaced workers who experience real earnings losses. The right panel shows the results for workers who experience earnings gains above zero. Source: IEB.

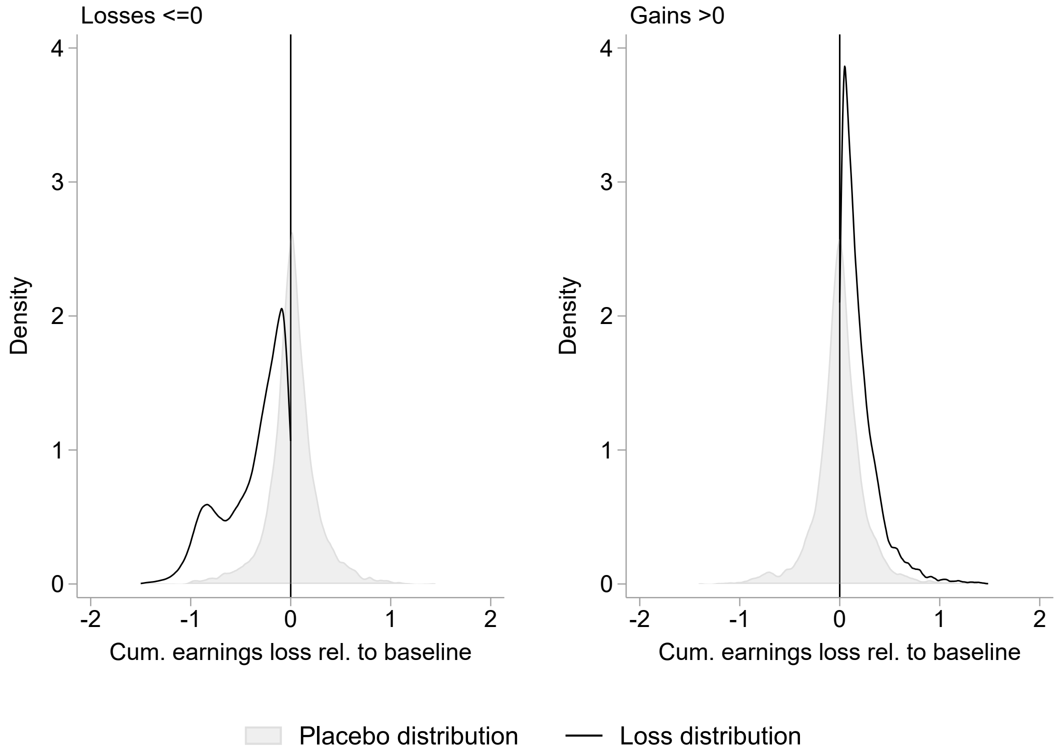

Figure B.3: Comparison between loss and placebo distribution five years after closure

Notes: The black lines display histograms of the distributions of displaced workers' earnings losses year before 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 gray shaded areas display the distribution of placebo estimates that we obtained by estimating the intervention separately for a selected sample of the donor pool. The left panel shows the results for displaced workers who experience real earnings losses. The right panel shows the results for workers who experience earnings gains above zero. Source: IEB.

C 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 sample to increase the likelihood of outliers to influence the results. The gray lines represent the results from the permutation exercise, 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.

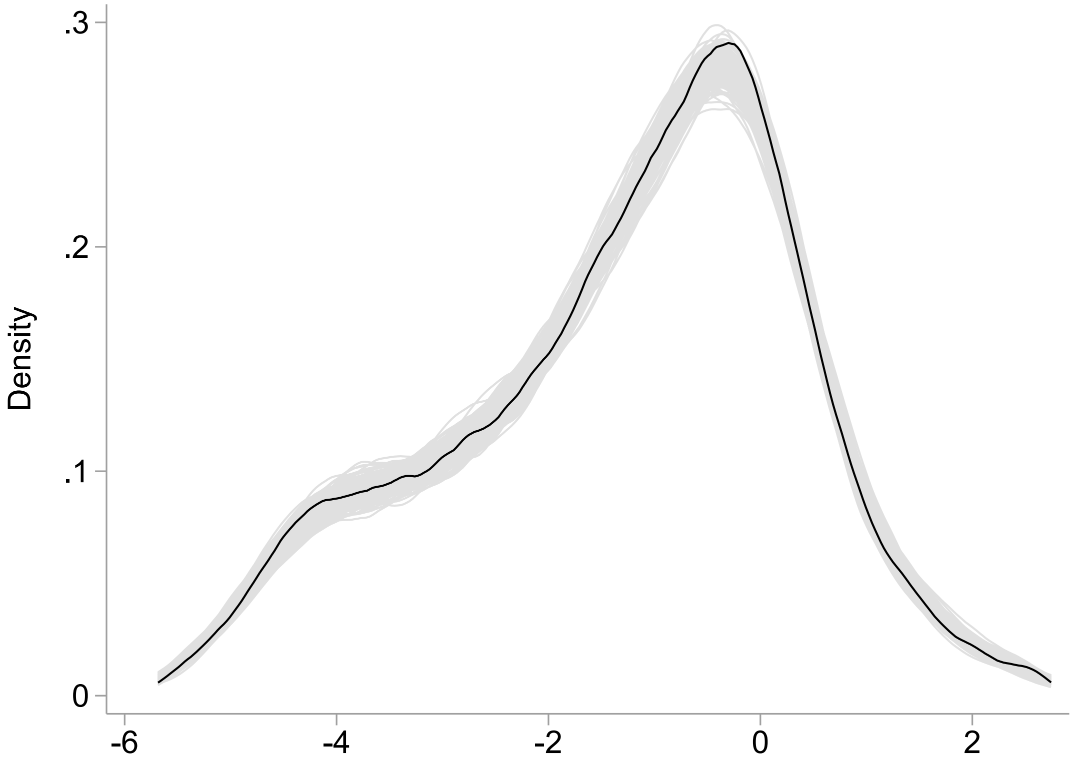

Figure C.1: Permutation of loss distribution on 200 ten percent samples

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.

D 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.

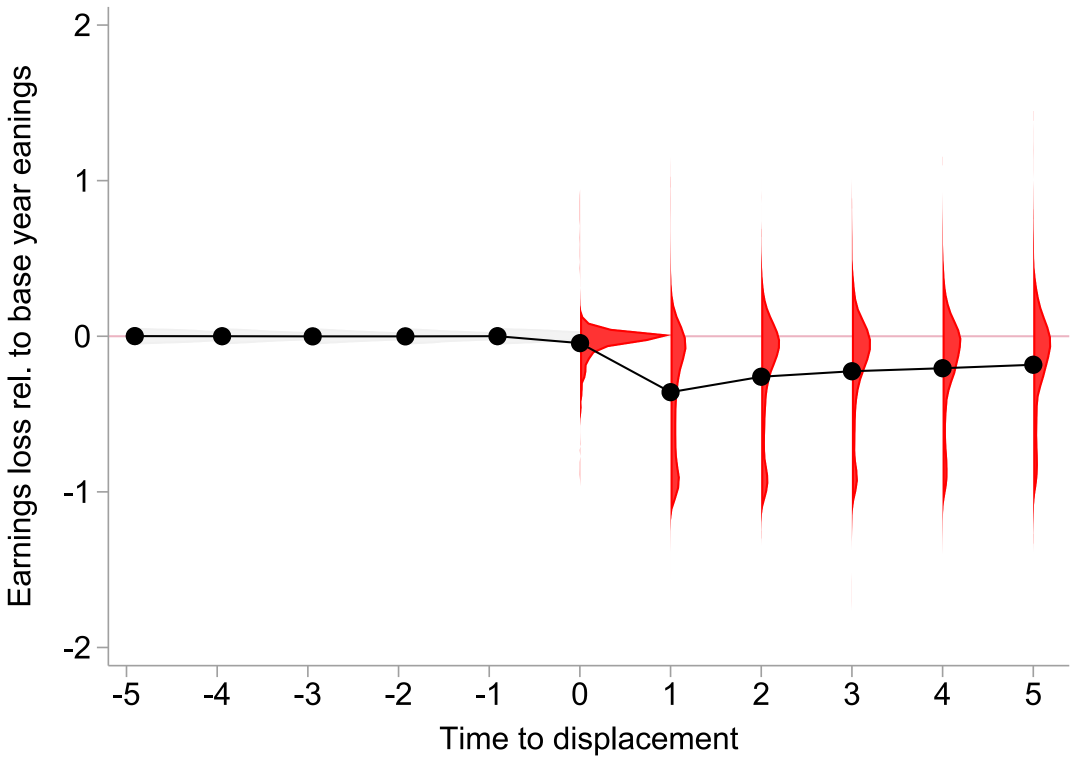

Figure D.1: Distribution of relative earnings losses with perfect pre-trends

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 F.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.

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 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 F.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 F.1 does not reveal any evidence that the estimates are systematically biased for adjusters or casualties.

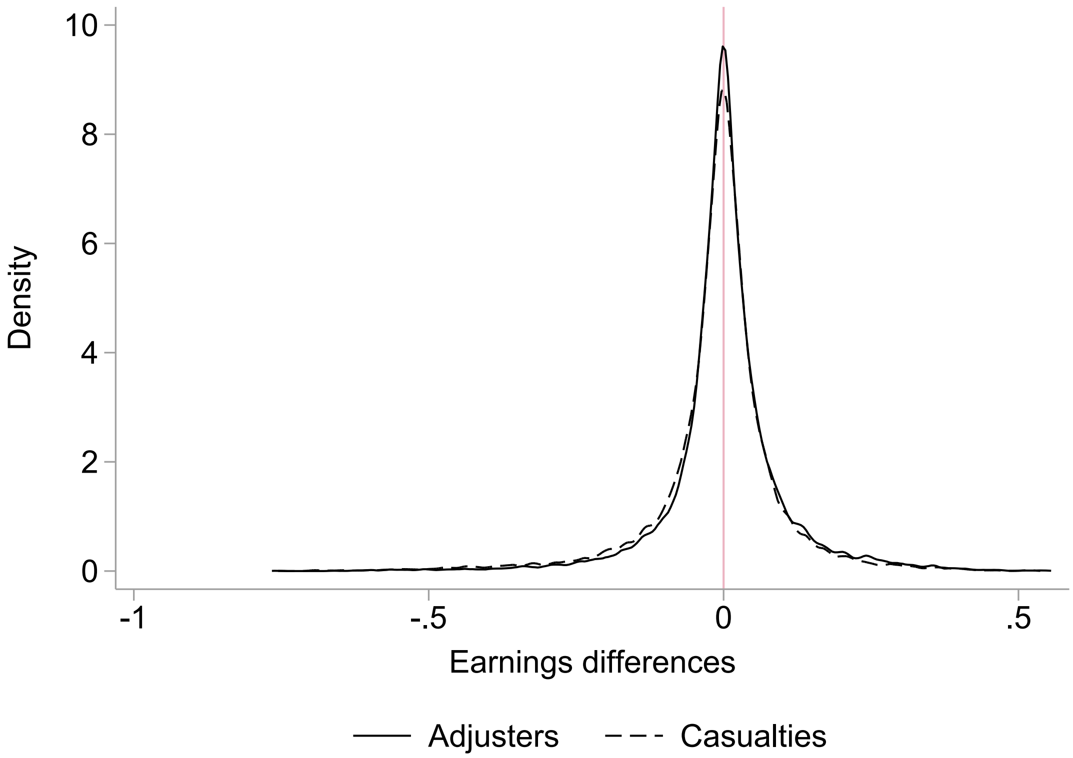

Figure E.1: Pre-displacement distributions of earings differences between displaced workers and their synthetic controls

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.

F Main event study excluding observations with zero earnings

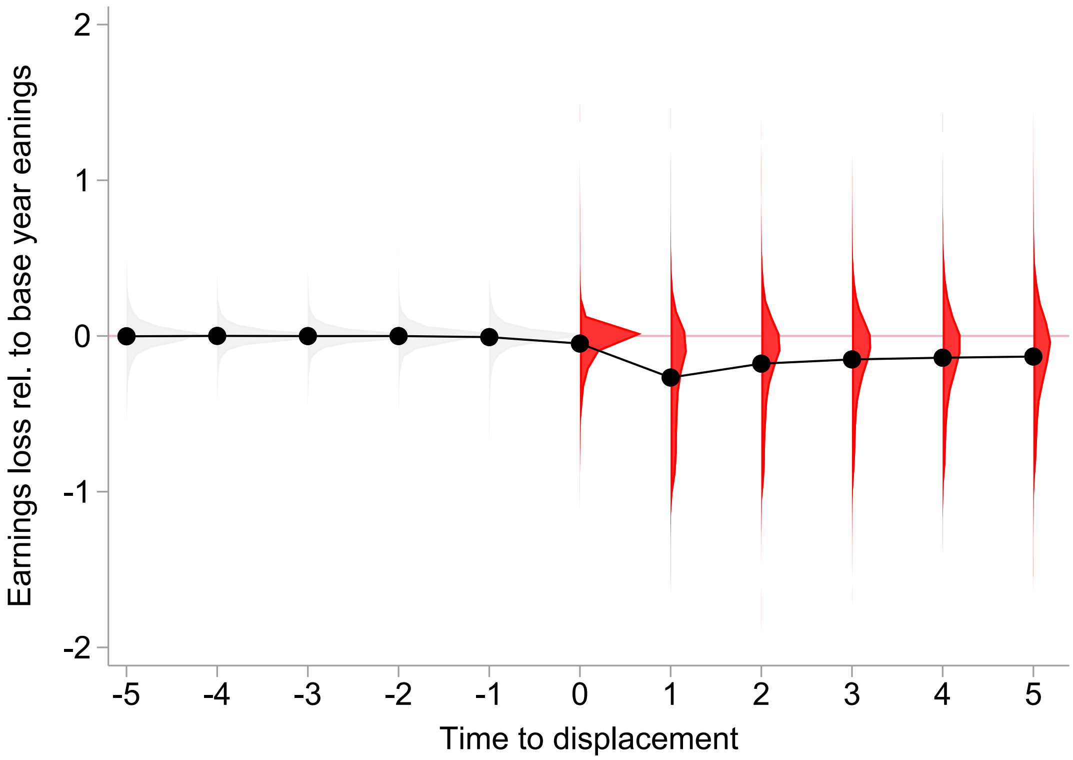

Figure F.1: Distribution of relative earnings losses excluding observations with zero annual earnings

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 baseline earnings measured as the average earnings throughout a period of three years before the displacement. 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 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.

Additional figures and tables

	All		Low educated		High educated	
	Variance comp.	Share of total	Variance comp.	Share of total	Variance comp.	Share of total
Total variance of loss	2.703	1.000	3.014	1.000	2.929	1.000
Individual char.	0.045	0.017	0.070	0.023	0.031	0.011
Education	0.004	0.001	0.000	0.000	0.000	0.000
Pre-closure res. wage	0.040	0.015	0.048	0.016	0.243	0.083
Firm f.e.	0.356	0.132	0.994	0.330	0.564	0.192
Occupation f.e.	0.062	0.023	0.507	0.168	0.993	0.339
Region f.e.	0.022	0.008	0.345	0.115	0.545	0.186
Citizenship	0.026	0.009	0.166	0.055	0.102	0.035
Residuals	2.141	0.792	1.968	0.653	1.597	0.545
Covariances	0.006	0.002	-1.085	-0.360	-1.146	-0.391
Individuals	15,960		2,213		383	

Table F.1: Displacement loss variance decomposition by subgroup

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.

	Earnings loss	
Decile	Causal forest	Synthetic control
1	-0.473	-0.834
2	-0.400	-0.809
3	-0.343	-0.635
4	-0.367	-0.453
5	-0.305	-0.278
6	-0.288	-0.167
7	-0.300	-0.093
8	-0.315	-0.040
9	-0.217	0.013
10	-0.108	0.179

Table F.2: Compare average earnings losses by decile rank, causal forest vs synthetic control

Notes: This table compares earnings losses for workers binned according to causal forest and synthetic control earnings loss estimates. Earnings losses are measured as earnings in the year after firm closure divided by earnings in the year before firm closure. We estimate earnings losses using the causal forest and synthetic controls to arrange individuals into deciles of earnings loss. Within each decile, we calculate the average raw earnings loss (i.e. just earnings in the year after closure divided by earnings in the year before closure).

	Low educated Unmatched	Matched	Medium educated Unmatched	Matched
Adjuster	-0.016**	-0.056**	-0.015***	-0.013
	(0.007)	(0.023)	(0.004)	(0.008)
Observations	2630	430	12514	2840
R^2	0.005	0.017	0.004	0.007

Table F.3: Differences in education updating between adjusters and casualties

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).

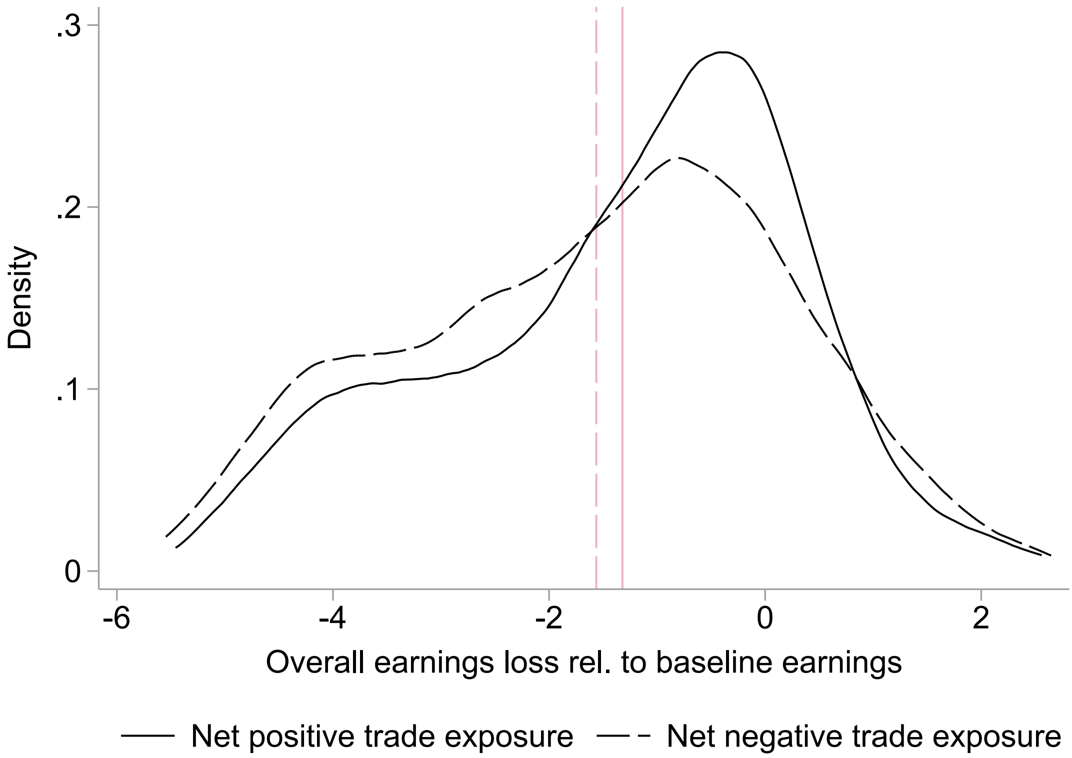

Figure F.2: Distributions of earnings losses by trade exposure

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.

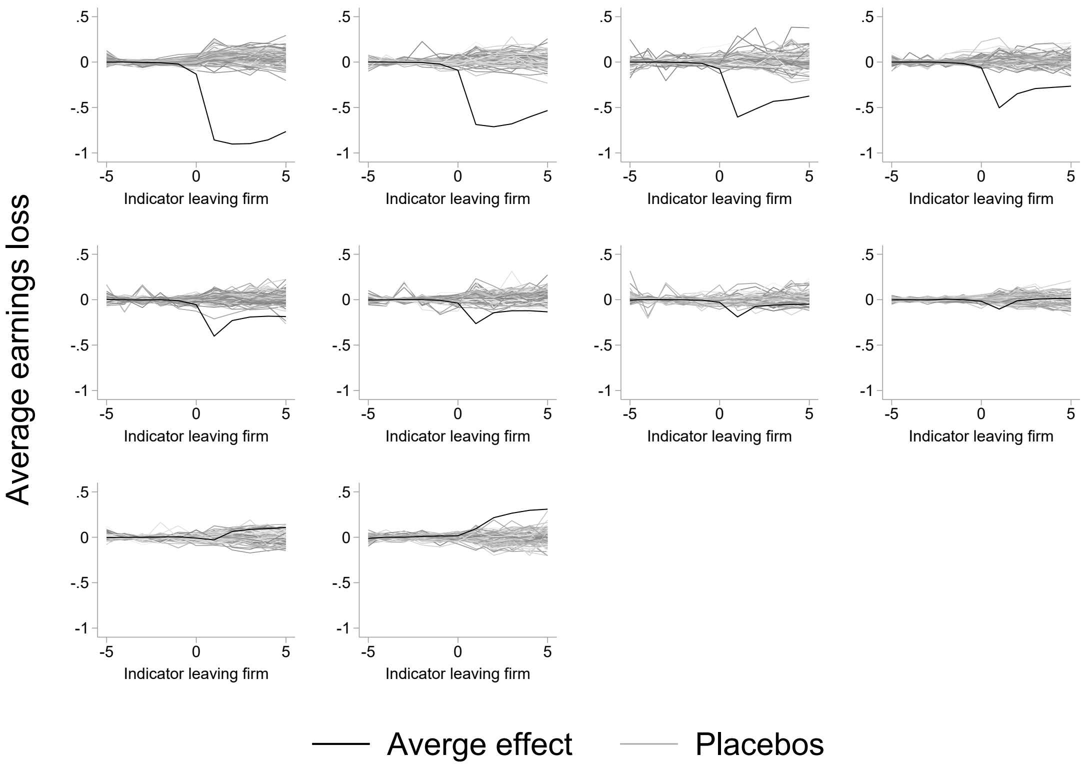

Figure F.3: Average earnings for treatment and synthetic control group at deciles of the cumulative loss distribution with alternative placebo losses

Notes: The figure displays the average earnings for the treatment and synthetic controls within the deciles of their loss distribution. For this purpose we have binned the treated workers into deciles according to the magnitude of their estimated cumulative earnings losses. The first subfigure displays the average earnings of displaced workers and their synthetic controls for those workers whose earnings losses lay below the ten percent decile of the total loss distribution. The second figure presents the same results for the second decile and so forth. Each panel compares the average earnings losses for displaced workers to earnings losses for placebo workers selected from the donor pools of the displaced workers. We estimate placebo earnings losses using the synthetic control method, split placebo workers into deciles based on cumulative earnings losses, then draw 100 one-percent samples of placebo workers (approximately 15 placebo treatments) and plot the average losses for each sample as the lighter lines.

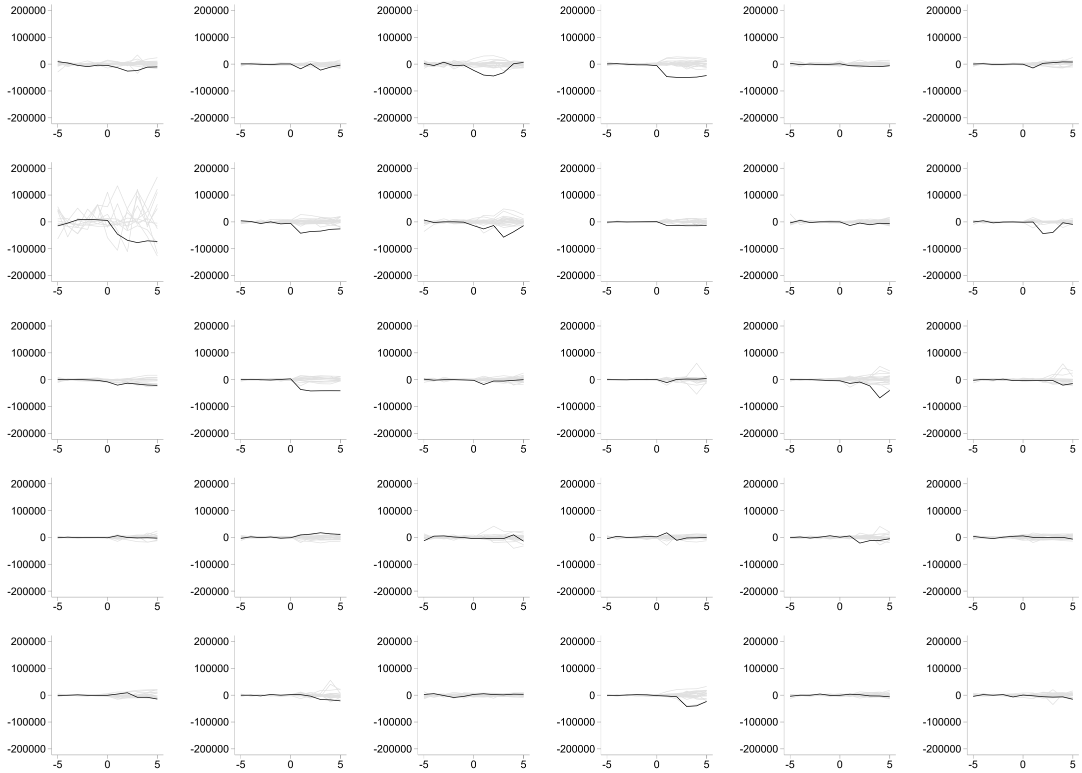

Figure F.4: Individual earnings loss of a closing firm in the manufacturing sector (case study)

Notes: The figure plots the estimated earnings losses of 30 workers displaced from a single closing firm in the manufacturing sector. 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.