Adjusters and Casualties: The Anatomy of Labor Market Displacement

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Abstract

Earnings losses after job displacement are highly skewed: a small number of workers experience catastrophic losses, while most workers recover quickly. This paper documents the heterogeneity in earnings losses after job displacements and the adjustments driving these differences. We study workers from firms in West Germany that closed between 2000-2005. For each laid-off worker, we create a synthetic control from similar workers with matching earnings trajectories who weren't laid off during that period. Which workers experience the greatest losses is not ex ante predictable based on fixed characteristics, but is associated with post-layoff adaptability, like switching professions or relocating. Consequently, pre-layoff targeted policies to assist these workers might not be as effective as post-layoff interventions.

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

JEL Classification: J24, J64, O30

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To-do list

Exposition

- Does this have any policy implications?
  - Most policies currently target workers based on ex post outcomes (e.g., UI and training only for workers who remain unemployed) – our paper would seem to agree with this approach
  - What kinds of policies do governments deploy during firm closures
  - Optimal policy is actually very personal - outcomes may differ on average but the policies need to target individual ex post outcomes
  - Do you have more casualties when UI benefits are higher?

New analysis

- Random forest analysis - move discussion to accompany weighted event study analysis
- Either drop heterogeneity on firm size in figure or expand analysis/explain why our results deviate from the literature
- As a substitute for the current Figures 7 and 8: table to capture the insight from these figures. The table would look like the following: let's say a workers suffers large wage losses if the wage is 10% less than their pre-displacement wage and they are underemployed if they are out of the labor market for more than 50 days (both of these numbers are arbitrary). Then, for adjusters and casualties in each year 1-5 post-layoff, a worker can be in one of four states: large wage loss + underemployed, large wage loss + not underemployed, not large wage loss + underemployed, and not large wage loss + not underemployed. We want to calculate the % of workers in each "bucket" in each year 1-5 post-layoff, separately for the casualties and adjusters. A table like this would help to disentangle a bit how much of the casualty losses are driven by transitioning out of the labor force vs taking a lower wage, and the extent of each effect over time.
- Question: How many bootstrap samples do we run for the permutation exercise in Appendix B?
- Fill in section on education and trade - new trade figure/table

Figures and Tables

- Figure 1: for the notes and the main body, are these losses relative to the synthetic control (as in Figure 2) or absolute losses (which is how they are described currently in the paper)?

- Figure 2: do we want to simplify the figure at this point in the paper and include the placebo lines in an appendix?
  - Check why some of the year -5 earnings are so jumpy for a few of the treated workers
- Figure 3: If we are measuring earnings relative to base year (or the average of base years), how do we have earnings losses greater than 100%?

Figure 4:

- Many people keep getting held up on interpretation of % values that are less than -1. Is there another way to normalize? For example, maybe [Sum earnings Treat] / [Sum earnings Control] $\in$ [0, +].

Figure 5:

- Dotted series is not showing up clearly
- For space: remove "overall" from x-axis label
- Do we want to include Panel C? If so, need to add a discussion of our results on firm size and benchmark to literature
- For paper: calculate marginal difference in means, save as a table for us to reference and pull numbers for the text

Figure 6: the text refers to grey confidence bands but the series in the figure do not have grey shaded areas

- Change x-axis label, add y-axis label, dashed series "synthetic control earnings"

Figures 12-14: consolidate into a single figure or drop altogether. Use the same scale for all of the figures for same reason as above.

- Typo in label (casualties)

Table 1:

- Clean up of digits
- Add column for SCs with nonzero weight
- Typo: "university degree"
- Remove automatically-generated table note
- Swap "Gender" label with "% Female"

Table 2:

- Add columns for worker age and firm size

Table 3:

- Drop columns 1 and 3, move "total" row to bottom of the table
- New column headers: 1) Share of variation of earnings loss; 2) Share of variation of synthetic control earnings (or have "Share of variation" as a header over both columns, and "earnings losses" and "SC" as sub-headers)
- Add column numbers (we reference Cols 1 and 2 in the text)
- What is included in "individual characteristics" besides age and gender? Update in FN
- Can we remove the abbreviations

Remove table 4, move table 5 to appendix:

- Clean up of digits
- Change "Disrupted" to "Casualties"
- Change column title to "Adjusters" (rather than adjuster)
- Swap "Gender" label with "% Female"

Appendices

- Missing: Robustness on main result using a longer pre-period (10 years)
- Missing: Robustness check on main result restricting to treated workers whose earnings never deviate from SC by more than 1%
- Missing: Back up claim that pre-displacement deviations are uncorrelated with post-displacement losses
- Missing: Formal test that distributions of pre-treatement period earnings deviations are normal
- Present - need to fix link: Compare SC estimates to event study (check: is 25th and 75th %ile the same individuals across years or is it plotting the 25th and 75th percentiles of the distributions across years?)

    - We use our method to select the top/bottom 25th percentile, then run an event study regression only on those individuals to confirm that the event study on the same individuals yields similar estimates/patterns

- Present - need to fix link: permutation exercise

    - Question: How many permutations do we run?

- Missing: Table 3 (variance decomposition) by subgroup

1 Introduction

Decades of economic research have consistently highlighted the large and enduring impact of firm closures and job displacements on workers' earnings, revealing substantial and often persistent losses.[1] More recent research highlights the uneven impact across different groups of workers. Important for policy, but not documented in the existing research, is how these losses are distributed across displaced workers. Whether losses are shared broadly or concentrated among a few workers is relevant for designing policies to assist displaced workers, yet existing work cannot yet speak to which of these two stories best characterizes worker displacement.

We extend this prior work through construction of the entire distribution of displaced workers' earnings losses. 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. Analyzing the distributions of earnings and employment trajectories of displaced workers leads to insights that differ substantially from the conventional wisdom on the nature and scope of such displacements.

We assess these losses by comparing the earnings of workers displaced by firm closures to those of an individual-level control group, identified using a combination of matching and synthetic-control methods. This approach reveals that the distribution of individual earnings losses is far from normal, implying that the average loss typically estimated from event-study approaches mischaracterizes the effect of firm closure for the majority of workers. Using administrative data for German plant closures from 2000-2005, we can trace the wage and earnings patterns of displaced workers for five years after a closure. By extending the matching approach of Schmieder et al. (2023) with synthetic-control methods, we develop a control for each displaced worker, allowing us to look more deeply into the underlying distribution

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

of economic losses. The highly skewed distribution shows that the modal displaced worker loses little, a portion actually gains, and a final group loses dramatically.

The implications of our findings are significant. We reproduce estimates of average effects documented in standard event study analysis, along with the previously-identified differences across demographic and worker groups: lower educated workers, women, and older workers suffer above average losses. But the most obvious conclusion from the picture of gains and losses across the demographic groups is the near complete overlap of the underlying distributions. Observable characteristics explain only a small fraction of the overall variance in losses,

There are dramatic differences in adjustment choices 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. (2021).

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

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

[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.

Our project also contributes to a growing literature that uses machine learning techniques to estimate heterogeneous treatment effects. Most recently, Gulyas et al. (2021) and Athey et al. (2023) have used generalized random forest models to estimate displaced workers' earnings losses in Sweden and Austria. The papers rely on generalized random forests to estimate heterogeneous conditional average treatment effects that are based on high-dimensional interactions of the workers' pre-treatment characteristics. Instead, we use a synthetic control group approach to estimate individual displacement losses for each single worker. These estimates are unconditional on the workers' pre-treatment characteristics and allow us to analyze the variance of displacement losses not only across different combinations of observable worker and firm characteristics but also within them.

The paper proceeds as follows. Section 3 describes the data sources used for our analysis. Section 2 outlines the synthetic control strategy used to estimated individual-level earnings losses. Section **??** provides descriptive statistics. We summarize our main results in Section 5. In Section 6, we document margins of adjustment that explain some of the variation in earnings losses among displaced workers. Section 7 concludes.

2 Empirical strategy

The standard framework for estimating effects of a firm closure seeks an estimate of labor market outcomes for displaced workers had their firms not closed. Typ-

displacement losses tend to be lower in countries with more generous welfare systems and Janssen (2018) shows that displacement losses are larger under flexible than under 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 do not seem likely to change the overall conclusions of our work.

ically, we estimate these counterfactual using workers at firms that do not close. The canonical approach to estimating the effects of worker displacement often relies on standard event study methods, which contrast the outcomes of displaced and non-displaced workers. While these methods allow for the analysis of mean effects, they cannot recover the full distribution of earnings losses among displaced workers, especially those with the same pre-treatment characteristics who experience heterogeneous outcomes. Thus, standard event studies cannot identify workers who adjust to firm closures without incurring significant earnings losses.

To overcome these challenges, we estimate individual-level earnings losses using a synthetic control 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 to calculate average short-term wage losses at the individual level. 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 represents a significant innovation, allowing us to capture 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 share similar job characteristics and career stages.[8]

To achieve this, we implement a two-step procedure. First, we partition workers into cells based on gender, education, one-digit industry, and three-digit occupation. Next, we construct synthetic control weights for each displaced worker based on the pre-closure outcomes of workers within these cells. Specifically, we calculate the root mean squared difference (RMSD) between 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 the treated worker.[9] This pre-matching approach minimizes interpolation bias by ensuring that the donor pool consists of non-displaced workers with very similar pre-displacement careers.[10] 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.

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.[11]

[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] We arbitrarily selected 20 donors to reduce the computational burden, though our results are robust to using 10 or 30 donors.

[10] To further reduce the influence of unrelated transitory shocks, we impose additional restrictions: 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.

[11] The four-year pre-intervention period is chosen to balance estimation bias and sample restrictions, though our results remain consistent when using a longer period (e.g., 10 years, see Appendix [NTD]).

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.[12] By constructing an explicit synthetic control group that remains unaffected by the treatment throughout the observation window,[13] we avoid issues commonly encountered in two-way fixed effects models with multiple treatment times (e.g., Goodman-Bacon (2021) and Roth et al. (2023)).

3 Data

Our analysis is based on 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.[14] 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.

For this study, we focus on individuals who had at least one employment spell

[12] 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 [NTD] 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.

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

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

in the private sector in West Germany between 2000 and 2005.[15] 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.[16] Identifying firm closures is challenging due to the potential for misinterpreting simple changes in establishment identification numbers as closures. To address this, we follow the methodology of Hethey-Maier and Schmieder (2013), considering a vanishing establishment identification number as a firm closure only if fewer than 30% of the workers from the closing firm transfer to the same subsequent establishment. This approach helps to accurately identify genuine closures and exclude cases of mere administrative changes.

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.[17] 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 up to 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 of interest is annual earnings, which includes the sum of earnings from all employment spells within a given year. We standardize

[15] We exclude firms in agriculture and mining.

[16] 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.

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

earnings to 2010 Euros and remove only a 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 earnings. 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 by virtue of lacking data on hours worked.

Our earnings data are top-coded for approximately 10 percent of workers with earnings above the annual German social security contribution ceiling. To impute the missing upper tail of the earnings distribution, we use a two-stage stochastic imputation procedure to estimate the missing upper tail of the earnings distribution.[18]

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.[19]

Table 1 presents descriptive statistics for our main analysis variables. The first column provides statistics for displaced workers in the base period, i.e., one year before they leave the closing firm. The second column presents statistics for non-displaced workers in the potential donor pool, from which we select the most compa-

[18]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 model.

[19]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.

rable workers for each displaced worker. Both displaced and non-displaced workers meet the analytical restrictions described in the previous sections, including employment in large firms with stable workforces, at least two years of tenure, and positive earnings for at least five years.

Our sample contains $161,213$ displaced workers who lost their jobs due to firm closures between 2000 and 2005. The entire potential donor pool of non-displaced workers contains more than $560,000$ workers.

— Table 1 about here—

On average, displaced and non-displaced workers have similar annual earnings, approximately $48,000$ Euros per year. However, non-displaced workers in the potential donor pool are slightly more likely to be female, have less tenure, and are somewhat older. They are also 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 relatively modest.

4 Case study of a single manufacturing firm

To illustrate and motivate our focus on the heterogeneity of outcomes for displaced workers, we begin with the example 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.

Figure 1 plots the trend in average earnings for these workers around the firm closure. The figure shows that, on average, these workers experienced earnings losses of approximately $1,500$ Euros in the first year after the closure. Relative to their average pre-closure earnings of around $46,000$ Euros, this translates to a small average displacement effect of approximately 3 percent per year.

— Figure 1 about here—

However, focusing on average losses may obscure substantial heterogeneity in individual outcomes. Figure 2 examines the extent of this heterogeneity by separately plotting the individual loss estimates relative to the synthetic control for each of the 30 displaced workers. The solid black line represents the earnings gap of each displaced worker relative to their synthetic control. In contrast, the light grey lines show the results of a permutation exercise, where we re-estimated the earnings gaps for each non-displaced worker in the donor pool relative to a synthetic control group composed of other donors and/or the treated worker.[20]

The permutation exercises confirm a good fit before the treatment and significant displacement effects for the workers who experienced the firm closure. Importantly, Figure 2 reveals that while some workers suffered substantial earnings losses — up to 50,000 Euros in the years following the closure — many others did not experience any significant losses and continued to follow the earnings trajectories of their synthetic controls even after the firm closed. This stark variation in outcomes among observably similar workers highlights the substantial heterogeneity in earnings losses due to firm closures. Importantly, the average earnings losses depicted in Figure 1

[20]This permutation exercise serves as a placebo test, demonstrating that the observed earnings effects are due to the firm closure rather than the selection of control observations (Abadie, 2021). As expected, the placebo estimates show parallel pre-trends before the closure and no treatment effects post-closure, unless the treated worker is part of the synthetic control group for a donor.

do not adequately represent the experiences of those workers who faced severe disruptions nor those who adjusted with minimal impact.

— Figure 2 about here—

5 The distribution of displacement losses

In this section, we analyze the distribution of economic losses for displaced workers by comparing their actual earnings post-closure to the earnings of their synthetic controls. To benchmark the losses, we report losses relative to the worker's earnings in the year before firm closure.

We begin by examining the overall distribution of losses across the population of closures, comparing our results to conventional estimates from event studies. We then investigate heterogeneity among displaced workers.

5.1 Dynamic Losses

Figure 3 plots the dynamic development of the loss distributions. The figure tracks each displaced workers over 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.

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 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.[21]

[21] 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).

— Figure 3 about here—

Unlike classical event study figures, the shaded areas in Figure 3 summarizes the distributions of individual deviations between the earnings of displaced workers and their synthetic controls. We estimate these distributions using standard kernel density estimators. The grey shaded region shows the distribution of relative differences between treated and synthetic control earnings in the pre-closure period, while the red shaded region shows the post-closure distributions of displaced workers' earnings losses.

It is important to note that achieving perfect balance in pre-trends for all displaced workers is not possible. Consequently, there is some distribution in the pre-treatment differences between displaced workers and their synthetic controls. Nevertheless, the distributions for all five pre-treatment periods are closely centered around zero, and we cannot reject the null hypothesis of normality for any of them. Furthermore, these pre-treatment distributions do not systematically correlate with the post-treatment distributions of displaced workers' earnings losses (Appendix [NTD]).

Post-closure, earnings losses for displaced workers are clearly not distributed normally. Instead, 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 losses 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 1.28 years of earnings over the five years post-displacement, but the modal loss is significantly lower, at just -0.41 earnings years.

— Figure 4 about here—

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, [NTD] percent 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. Several factors could explain these gains: workers may switch to higher-paying jobs when forced to do so, or they might have previously underestimated the benefits of job mobility, as suggested by recent evidence from Germany (Jäger et al., 2024).

While our estimates of the earnings loss distribution could be influenced by measurement error, we take two additional steps to reinforce our findings. First, we compare our synthetic control group estimates with results from a conventional event study (see Appendix [NTD] for details). We align the sample of displaced workers with a control group selected through propensity score matching, based on the same variables used in our analysis (age, gender, education, three-digit occupation, and two-digit industry). The event study results, which include plots of the mean, 25th percentile, and 75th percentile of losses over the period from five years before to five years after closure, are qualitatively similar, with the event study showing slightly larger earnings losses.

Second, we conduct a permutation exercise similar to a bootstrap approach, where we re-estimate the earnings distribution using a large number of 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 [NTD]).

5.2 *Earnings loss heterogeneity by worker and firm characteristics*

Previous research 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., 2021), and firm size (e.g., Lachowska et al., 2020; Fackler et al., 2021). In this section, we explore 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. Specifically, we investigate whether observationally similar workers within the same occupation or firm experience similar earnings losses.

Figure 5 demonstrates the heterogeneous earnings losses (scaled to pre-closure earnings) across four sets of 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 workers from closing firms of different sizes, while Panel D 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 ([NTD] years) or highly educated ([NTD] years). Older workers experience on average larger losses ([NTD] years) than younger ones ([NTD] years), and women experience larger losses (1.6 years) than men (1.15 years).

However, the distributions of earnings losses overlap considerably across all subgroups, revealing significant within-group heterogeneity. Even among women, low-educated workers, and older workers—who, on average, experience larger losses—many individuals suffer only moderate losses or even gain following displacement.

Panel D illustrates this pattern most strikingly for women and men. Women lose, on average, approximately 1.6 years of their pre-displacement earnings over the five years following firm closure, compared to 1.15 years for men. However, while the distribution of women's earnings losses is bimodal, with a second peak at approximately -4 years, the distribution of men's earnings losses is strongly left-skewed. This suggests that a large fraction of both women and men experience only moderate losses. Indeed, about 26 percent of men and 22 percent of women lose less than one month's worth of their pre-displacement earnings. 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.

— Figure 5 about here—

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

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

where the dependent variable $Y_i = \sum_{t=1}^{t=5} Loss_{it}$ 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, 5-digit occupation, and municipality fixed effects, respectively. The error term is denoted by $u_{i(-1)}$.

Next, we decompose the variance of the accumulated earnings losses as follows:

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

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.

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 occurs within these subgroups. Among the characteristics considered, the closing firm of the displaced worker is the strongest predictor of earnings losses, followed by pre-displacement occupation. Other factors, such as gender, education, or citizenship, explain relatively small portions of the variance.

These findings suggest 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.

— Table 3 about here—

A potential concern is that this finding is driven by noise in our synthetic control group estimates of counterfactual earnings. We provided some suggestive aggregate evidence against this in the last section, but we can provide more micro evidence here. The second column of Table 3 decomposes the variance of the estimated counterfactual earnings, using them as the dependent variable. In other words, instead of using the estimated earnings losses as a dependent variable, we only use the control group earnings as the dependent variable. If the counterfactual earnings were driven by random noise, observable pre-treatment characteristics should explain little of their variance. However, the results show that observable characteristics account for approximately 70 percent of the counterfactual variance—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.

Appendix [NTD] extends this analysis to different subgroups, revealing substantial variation in the explanatory power of pre-treatment worker and firm characteristics. For example, pre-displacement firms explain a larger fraction of earnings losses for low-educated workers than for those with medium or high education, while occupations play a more significant role in explaining variance among high-educated workers.

6 Adjusters and casualties

We now shift our focus from pre-closure characteristics to the post-closure dynamics that influence workers' recovery from displacement. The previous section highlighted that even among observably similar individuals, recovery patterns can differ significantly. In this section, we begin by providing an overview of the earnings patterns for individuals in different deciles of the loss distribution. We then examine how the observed behaviors of individuals in the top quartile of losses ("casualties") differ from those in the bottom quartile of losses ("adjusters"). It is important to note that these groups are defined based on their outcomes; the goal here is not to identify causal mechanisms but to characterize key behavioral choices made during the adjustment process, with the aim of encouraging further research into these dynamics.

To set the stage, Figure 6 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.[22] Workers in the lowest six deciles (those with the largest accumulated losses) never fully recover from their job loss, with those in the lowest decile experiencing particularly severe earnings losses. In contrast, workers in the top two deciles (those with the smallest accumulated losses) experience little to no losses and, in some cases, even higher earnings post-closure than would have been expected had their firm not closed.

—Figure 6 about here—

[NTD - new state transition table and discussion]

Previous research has shown that establishment effects account for a significant portion of displaced workers' average wage losses. For instance, Schmieder et al. (2023) found that establishment effects could explain nearly half of the negative wage impact on reemployment wages. Figure 9 examines whether this finding holds true for adjusters and casualties, who are located at the extreme ends of the loss distribution. The left panel of Figure 9 presents the average log wage losses for both groups. As shown in the previous figure, casualties experience substantial wage losses that persist over time, while adjusters experience increasing wage gains.

—Figure 9 about here—

The right panel shows the losses in terms of establishment fixed effects. To estimate the persistent differences in employer daily wages, we use an Abowd et al. (1999) (hereafter, "AKM") model, following the AKM implementation by Card et al. (2013) for Germany. We then apply the synthetic control weights estimated from our earnings regressions to create a synthetic counterfactual path of AKM effects for each displaced worker.

The results reveal that, on average, casualties switch to lower-paying firms, while adjusters move to slightly better-paying firms. However, the gap between the log

[22][NTD: the current version of the figure does not have grey confidence bands] The grey shades represent confidence bands at a five percent level, obtained from bootstrapped standard errors with [NTD] repetitions.

wage losses of adjusters and casualties is significantly larger than the gap between their losses in AKM establishment fixed effects. This indicates that while casualties tend to move to lower-paying firms, their extreme wage losses primarily result from earning lower wages than other workers within their post-displacement firms. Similarly, adjusters benefit primarily by earning higher wages than others within their post-displacement firms, rather than from switching to higher-paying firms. These findings contrast with the average wage effect after displacement, which is largely driven by establishment effects.

Examining the behavioral differences between these groups is informative, as adjusters appear to move quickly and decisively into new labor market positions. Figure 10 illustrates the labor mobility patterns of casualties and adjusters over time. The upper left panel begins with firm mobility. The solid line represents the fraction of adjusters who switch firms between any consecutive years $t-1$ and t, while the dashed line shows the fraction of occupation 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.

Nearly all adjusters switch firms immediately upon displacement, but the fraction of adjusters who switch firms after their first year post-displacement decreases to less than ten percent, suggesting that adjusters quickly find stable matches. Among casualties, only about 40 percent switch firms after displacement, as the rest remain non- or unemployed. However, their firm mobility remains high in the long run. In contrast, more casualties reenter the labor market in subsequent years after displacement. In subsequent years, casualties continue to switch jobs frequently, suggesting poorer worker-firm matches in the years following their displacement.

—Figure 10 about here—

The upper right panel shows mobility across 5-digit industries. On average, both adjusters and casualties switch industries more than once in the long run.

However, these long-run results mask significant differences in short-term dynamics. More than 60 percent of adjusters switch their 5-digit industry immediately after displacement. In contrast, only about 30 percent of casualties leave their industry, but this comparison is affected by the fact that only 40 percent of casualties manage to reenter employment in the first year post-displacement. Thus, the relative share of industry switchers is very high—three-quarters of employed casualties switch industries. Industrial mobility remains high for casualties in the long run.

The lower left panel shows the occupational mobility of adjusters and casualties. Approximately 40 percent of adjusters and 30 percent of casualties switch occupations immediately after displacement. Short-term occupational mobility is high for both groups. However, after the first year, adjusters are substantially more stable in their occupations, while casualties continue to switch occupations. In the long run, casualties switch occupations approximately 1.35 times, while adjusters switch on average only 0.85 times. Thus, overall, occupational mobility is higher for casualties than for adjusters, supporting the argument that casualties may forgo more of the returns on their occupation-specific human capital. However, adjusters also exhibit substantial immediate post-displacement flexibility, suggesting they may be better at transferring their human capital across occupations.

The lower right panel of Figure 10 examines geographic mobility across 50 large German local labor markets. Both groups eventually change regions to a similar extent, but adjusters are much more likely to make their moves immediately.

Overall, Figure 10 suggests 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.

6.1 Other margins of adjustment

One other possible difference in adjustments involves anticipatory departure from the closing firms. The adjusters may be more attuned to the fortunes of the country and may leave earlier (see, e.g., Schwerdt et al. (2010)). Indeed, if we compare departure times, adjusters are slight more likely to leave more than one-quarter before closure ([NTD] percent) compared to casualties ([NTD] percent).

[NTD: Trade]

[NTD: education]

7 Conclusion

The earnings losses from firm closures are very unevenly distributed across the displaced workers. This paper exploits administrative data on the universe of firm closures in Germany between 2000 and 2005. These data allow us to construct the full distribution of earnings losses across individuals using a novel approach that estimates a synthetic control group for each individual displaced worker who lost his or her job in response to firm closures. The results reveal large and skewed distributions of earnings losses suggesting that average earnings losses, as commonly estimated using classical event studies, might neither be representative for the minority of workers who are catastrophically impacted by firm closures nor for the large fraction of workers who easily adjusts to the closures.

Worker and firm characteristics that are 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 occupations, industry, and geographic regions 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 and to deal with displaced workers after market opportunities unfold.

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Figures in the Text

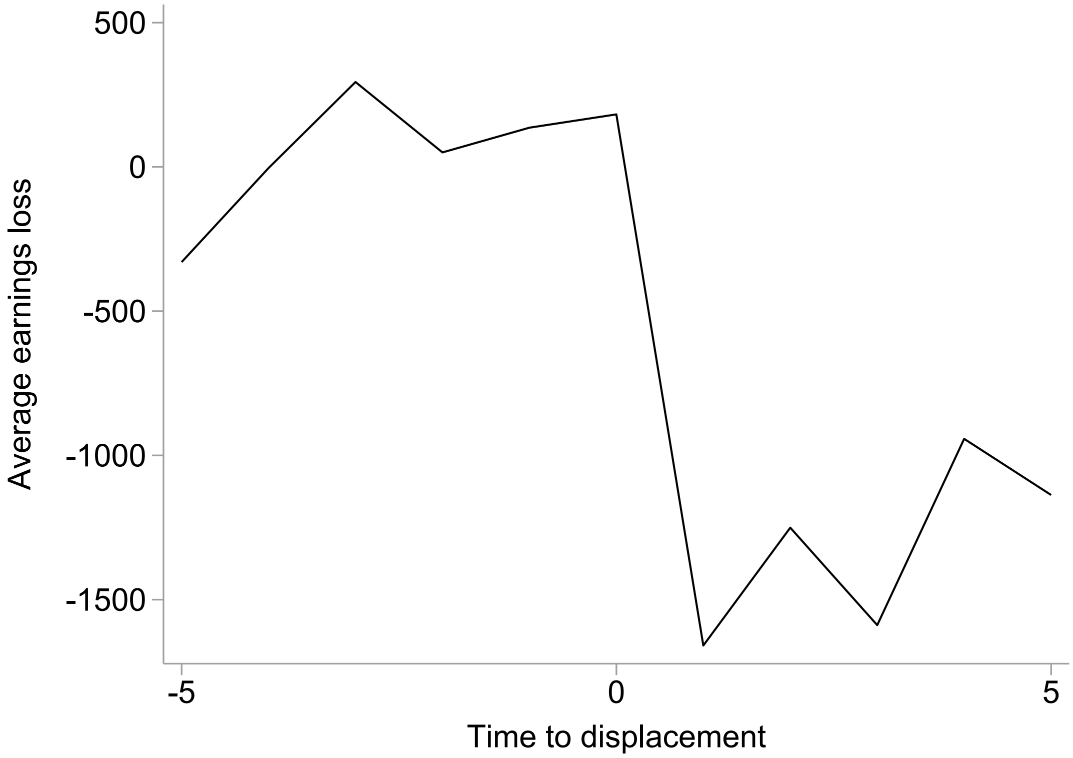

Figure 1: Average earnings loss of a closing firm in the manufacturing sector (case study)

Notes: The figure displays the absolute 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.

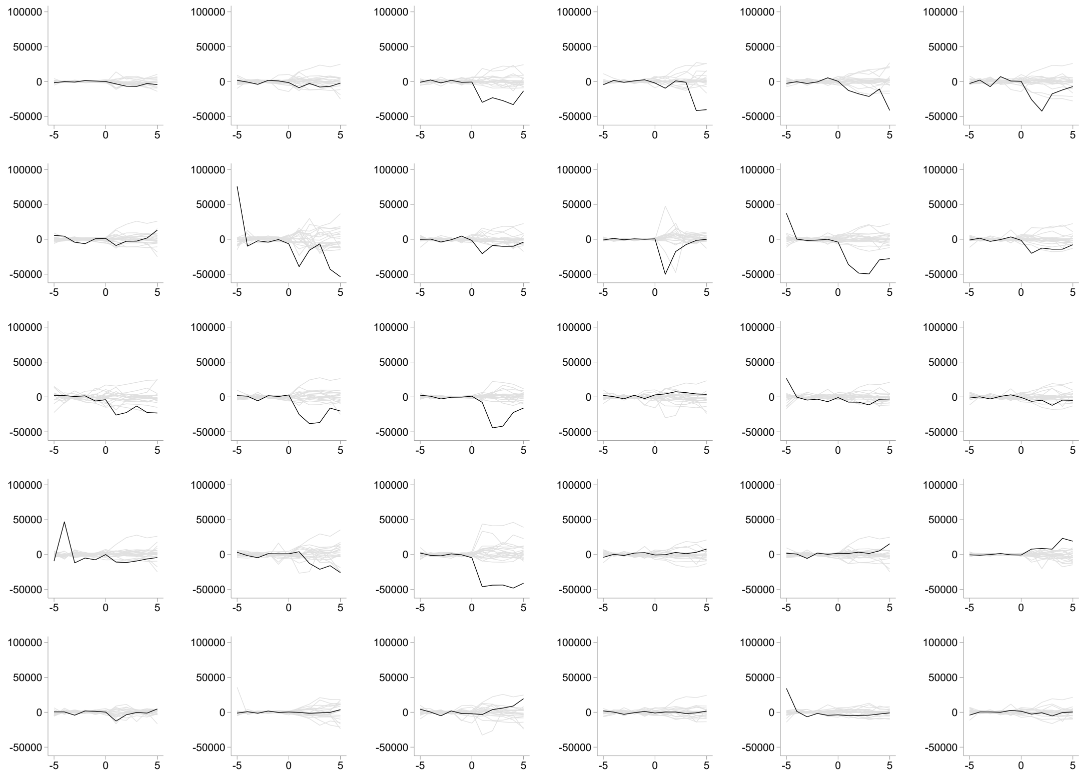

Figure 2: Individual earnings loss of a closing firm in the manufacturing sector (case study)

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. 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 losses in 2010 Euros. The x-axis displays the time before/after the firm closure in years. Source: IEB.

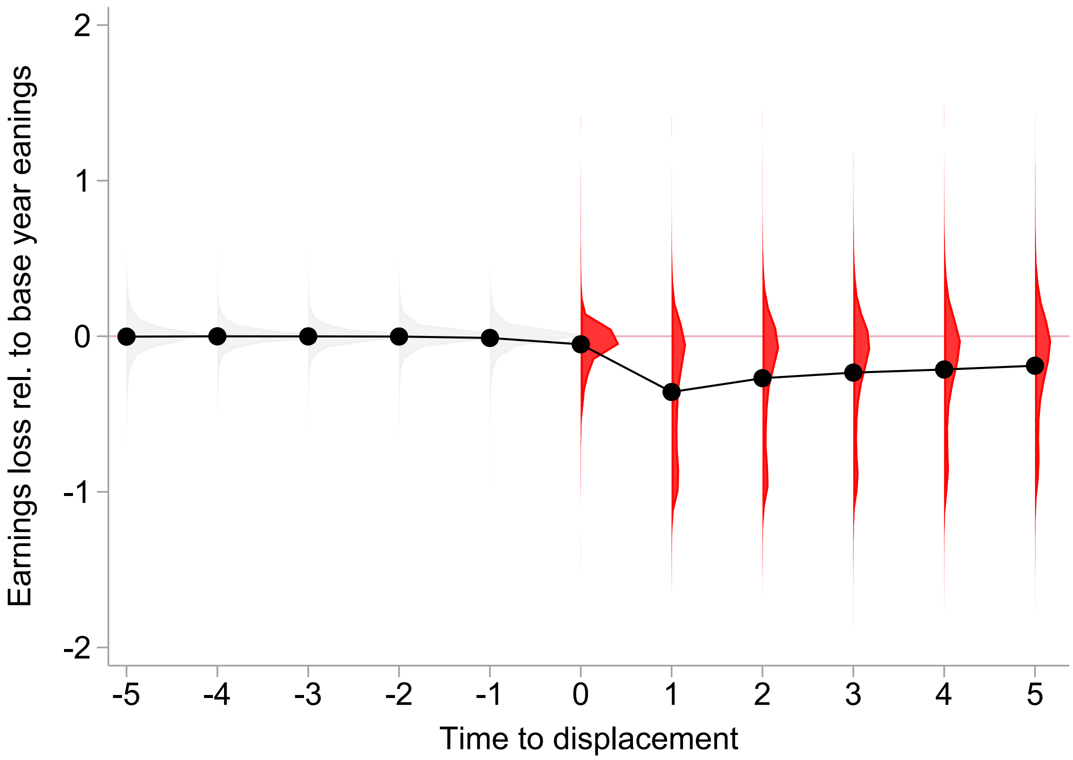

Figure 3: Distribution of relative earnings losses after firm closure (unconditional)

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 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. Source: IEB.

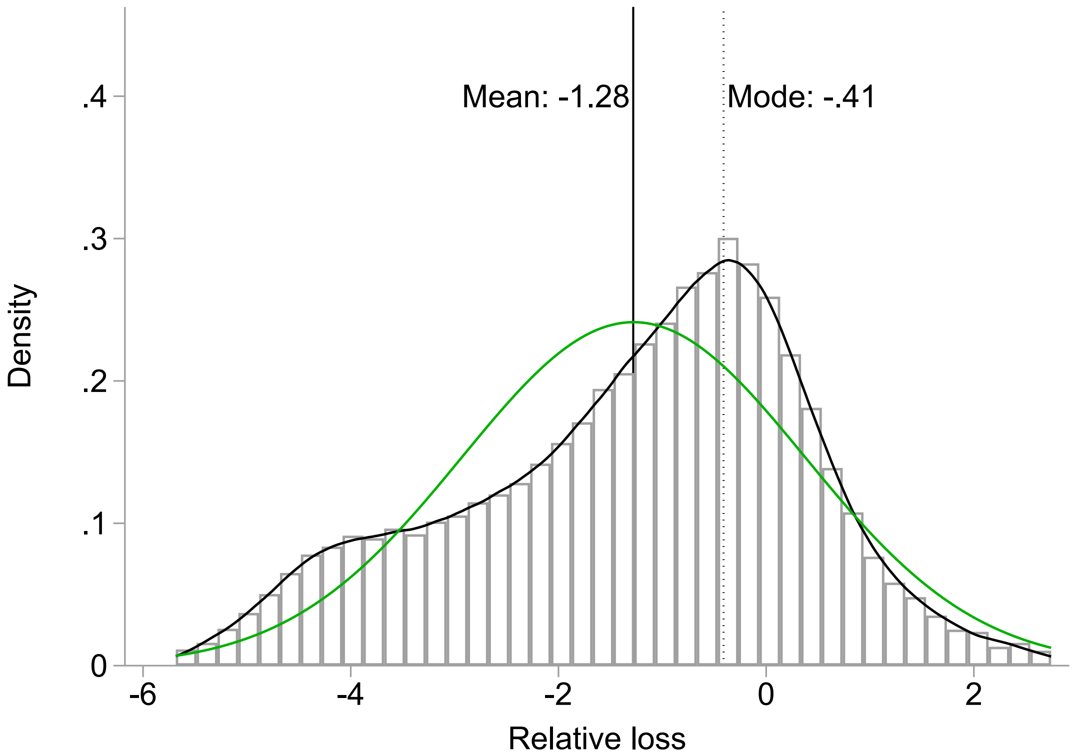

Figure 4: Distribution of five-year cumulative earnings loss relative to normal distribution

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 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 solid line plots the mean of the distribution; the dashed line plots the mode. Source: IEB.

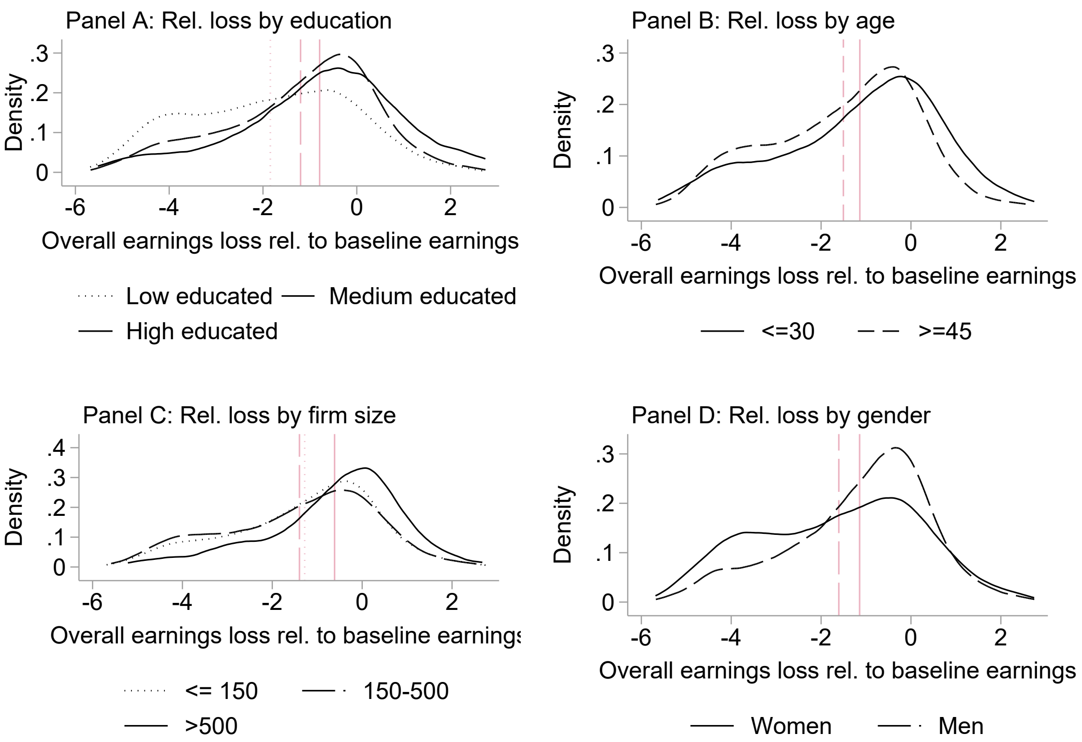

Figure 5: Distributions of earnings losses by pre-treatment characteristics

Notes: The figure displays the distribution of displaced workers' overall earnings losses throughout a period of five years after a firm closure by different worker and labor market characteristics in the base year (i.e., one year before the displacement). Panel A shows separate distributions by education, Panel B by age, Panel C by firm size, and Panel D by gender. 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.

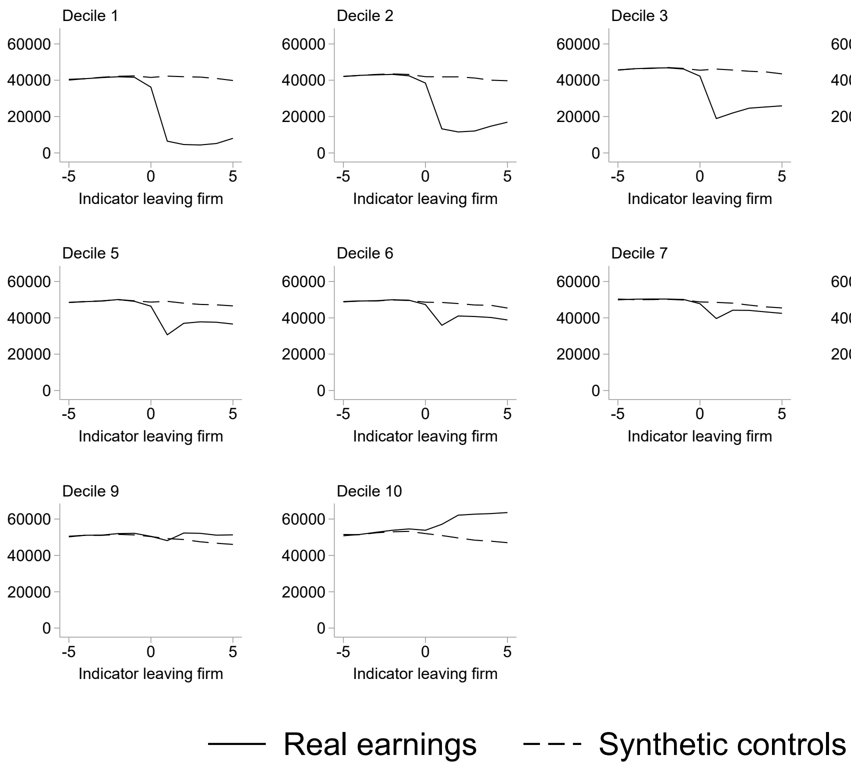

Figure 6: Average earnings for treatment and synthetic control group at deciles of the cumulative loss distribution

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 in 10 deciles according to the magnitude of their estimated cumulative earnings losses. 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.

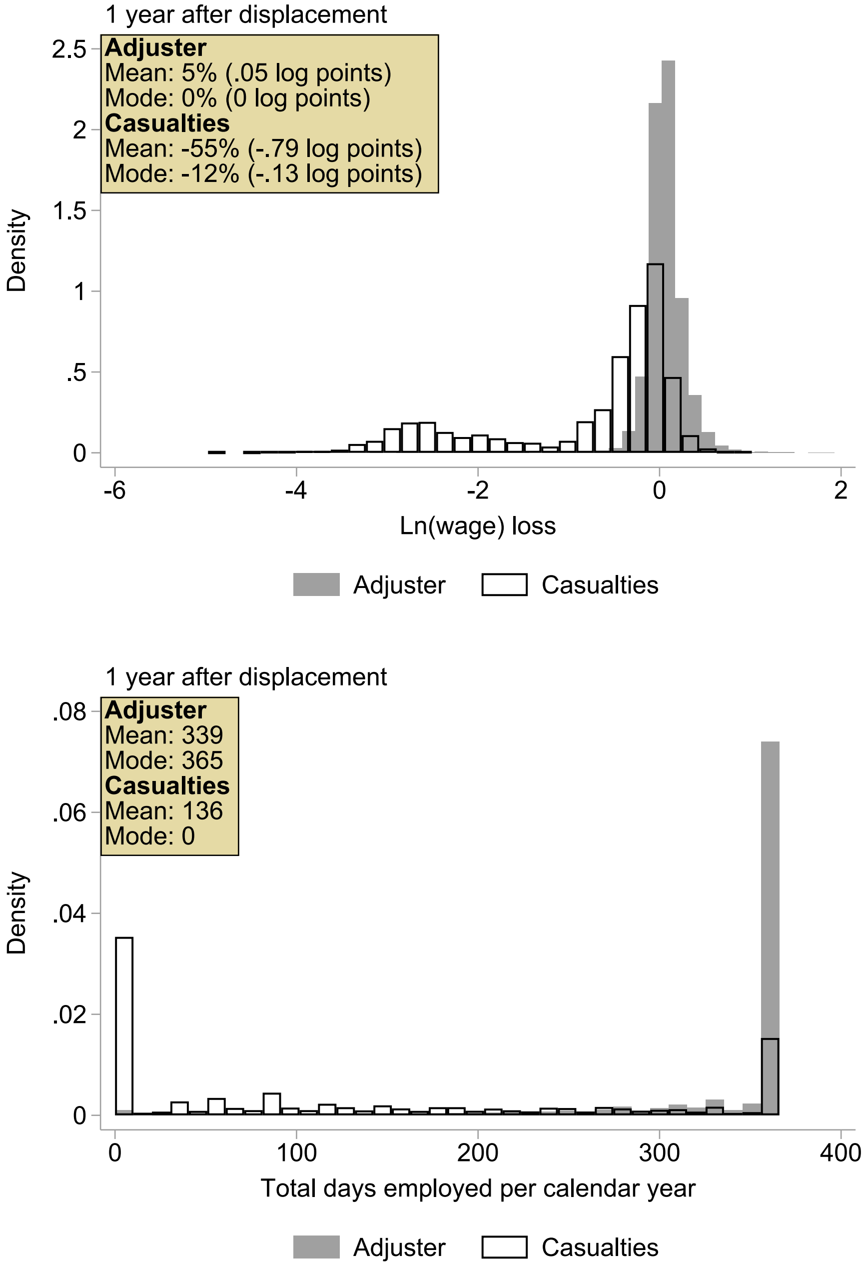

Figure 7: Wage losses of adjusters and casualties in the first year after firm closure

Notes: The top panel plots the distribution of wage losses following firm closure, calculated for each worker as the log difference between the worker's wage one year after firm closure and the worker's wage one year before firm closure. The bottom panel plots the distribution of changes in annual days of employment following firm closure, calculated for each worker as the difference between the worker's days of employment one year after firm closure and the worker's days of employment one year before firm closure. The figures plot histograms separately for "adjusters" (workers in the lowest quartile of earnings losses) and "casualties" (workers in the highest quartile of earnings losses).

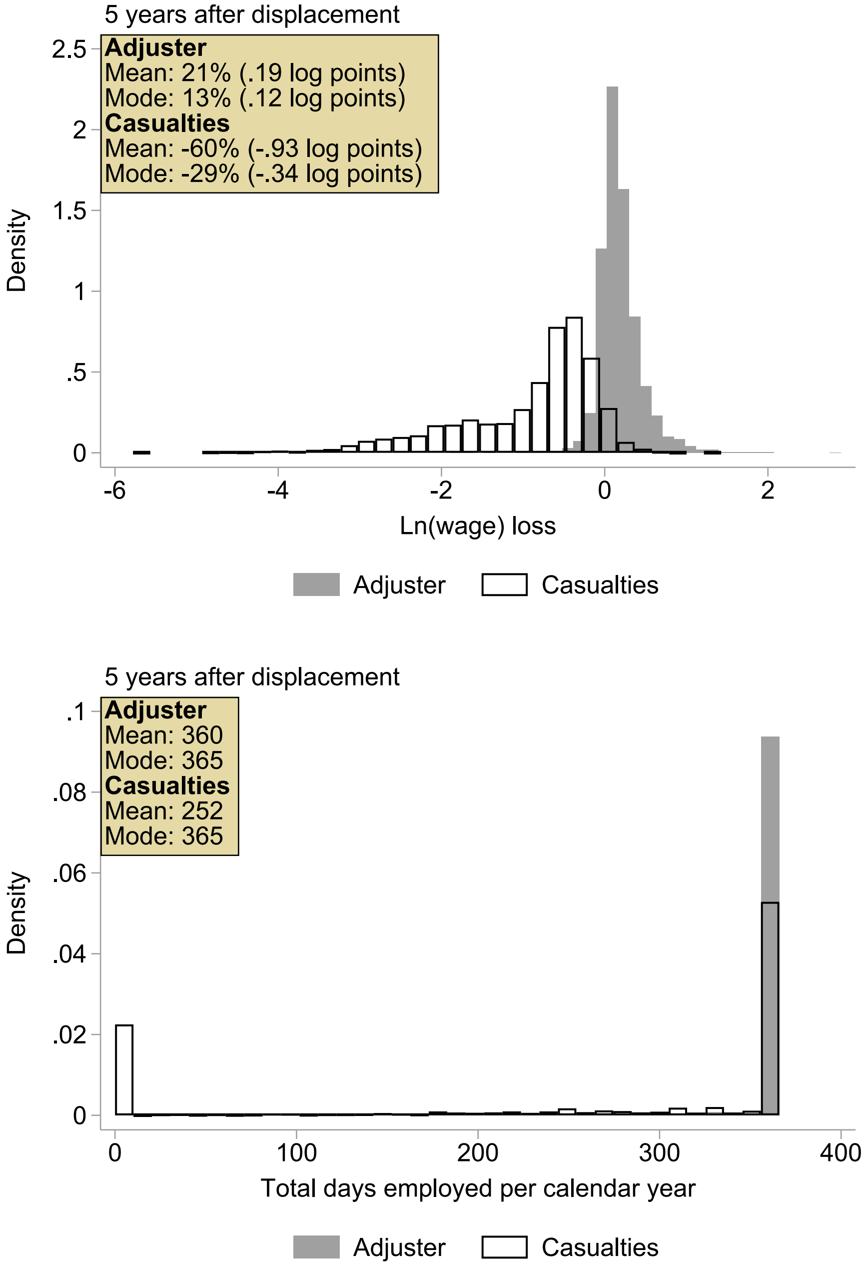

Figure 8: Wage losses of adjusters and casualties in the fifth year after firm closure

Notes: The top panel plots the distribution of wage losses following firm closure, calculated for each worker as the log difference between the worker's wage five years after firm closure and the worker's wage one year before firm closure. The bottom panel plots the distribution of changes in annual days of employment following firm closure, calculated for each worker as the difference between the worker's days of employment five years after firm closure and the worker's days of employment one year before firm closure. The figures plot histograms separately for "adjusters" (workers in the lowest quartile of earnings losses) and "casualties" (workers in the highest quartile of earnings losses).

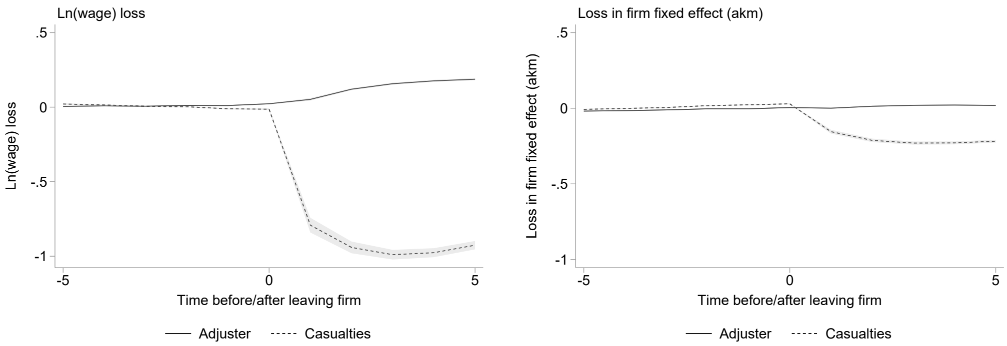

Figure 9: Contribution of closing firm fixed effect (AKM) to wage losses

Notes: The plots compare overall wage losses from firm closure to the change 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). The left panel plots the trend in average log wage for each of these groups relative to the worker's wage in the year pre-closure. The figure restricts to workers with positive wage. The right panel plots the firm AKM for these workers based on the firm at which the workers is employed. Bootstrap standard errors are plotted as the shaded region.

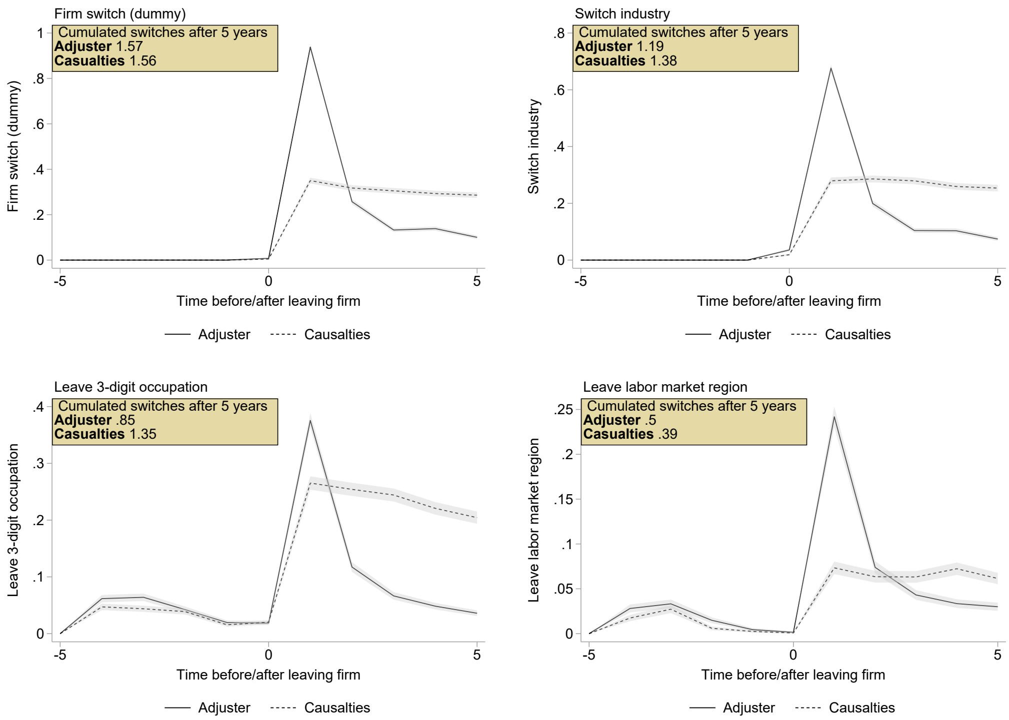

Figure 10: Fraction of workers who switch firm, industry, occupation, and region after firm closure

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 plots 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.

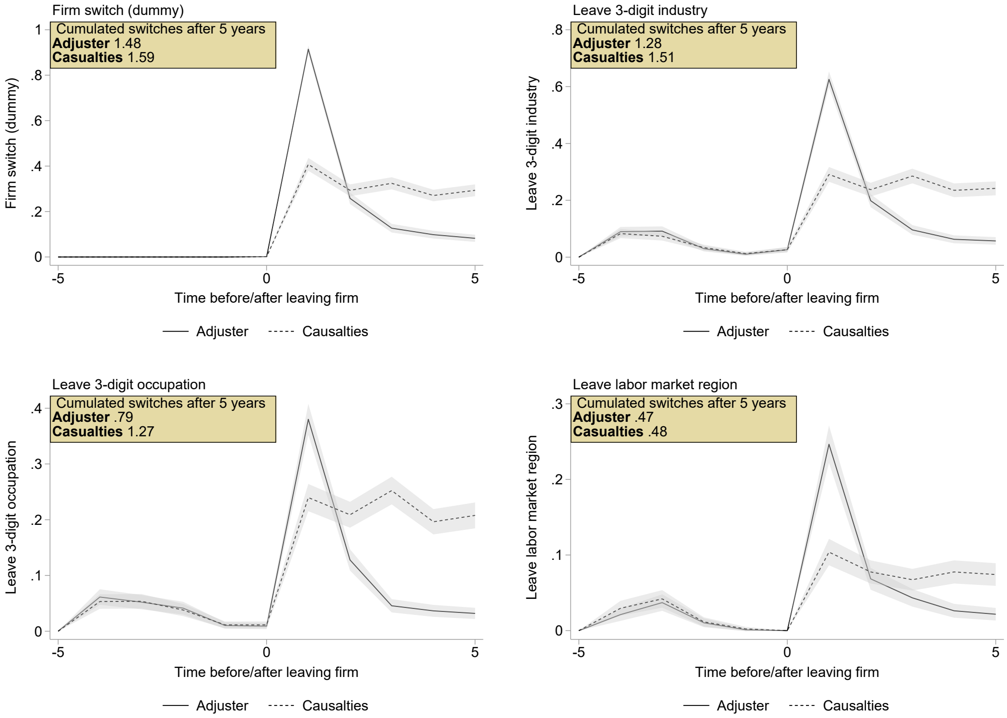

Figure 11: Fraction of workers who switch firm, industry, occupation, and region after firm closure (matched sample)

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 a firm that produces at least one "adjuster" and at least one "casualty." 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 plots 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.

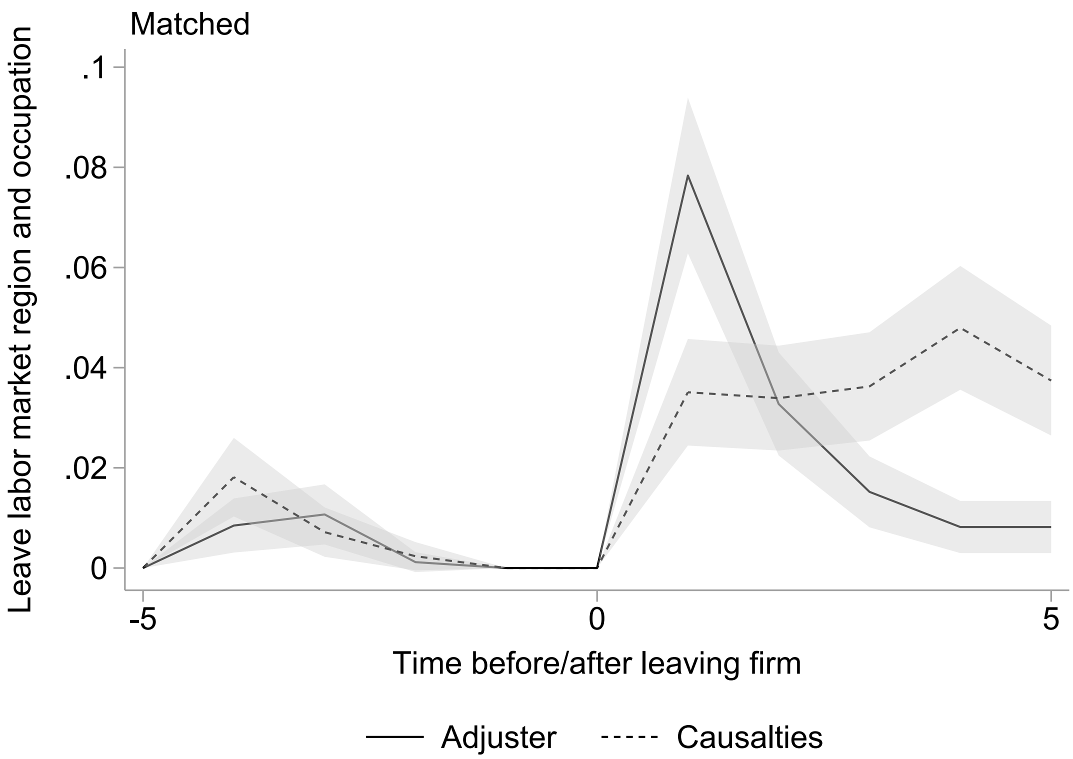

Figure 12: Share of workers who switch occupation and labor market region (matched sample)

Notes: This figure plots the share of workers who switch both occupation and region in a given year, plotted separately for "adjusters" (workers in the smallest quartile of earnings losses) and "casualties" (workers in the largest quartile of earnings losses). The figure restricts to the "matched sample" where each casualty is paired with an adjuster of the same gender, age category, and pre-displacement occupation (three-digit).

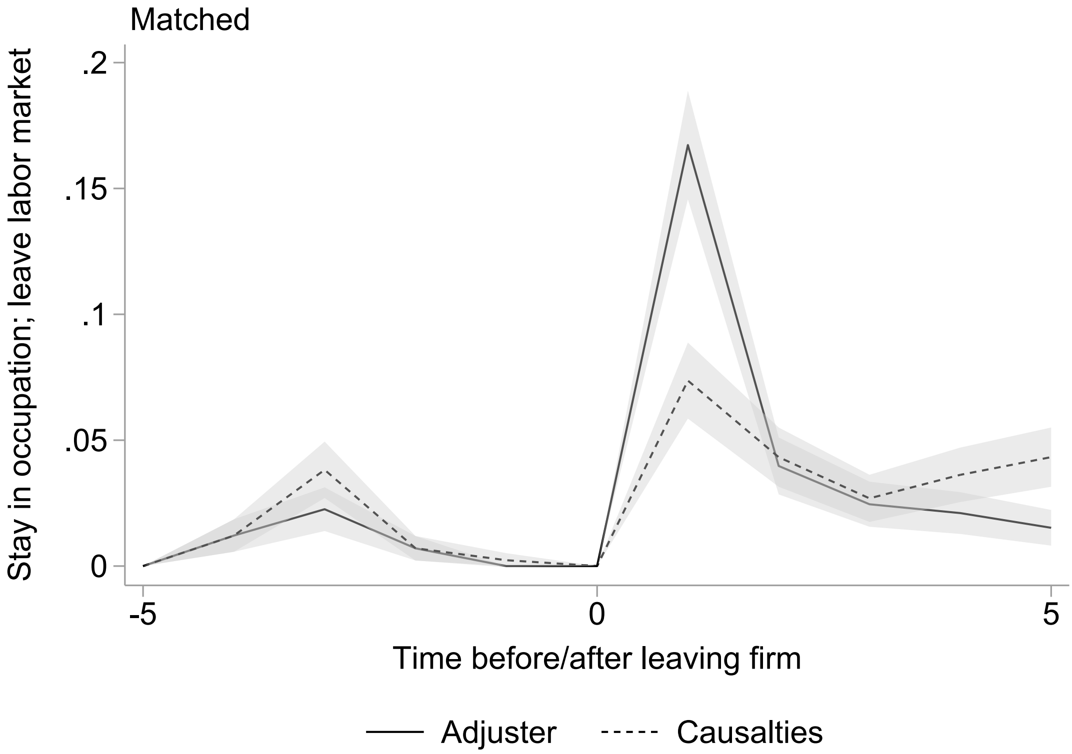

Figure 13: Share of workers who switch labor market region but not occupation (matched sample)

Notes: This figure plots the share of workers who switch region but remain in the same occupation in a given year, plotted separately for "adjusters" (workers in the smallest quartile of earnings losses) and "casualties" (workers in the largest quartile of earnings losses). The figure restricts to the "matched sample" where each casualty is paired with an adjuster of the same gender, age category, and pre-displacement occupation (three-digit).

Figure 14: Share of workers who switch occupation but not the labor market region (matched sample)

Notes: This figure plots the share of workers who switch occupation but remain in the same region in a given year, plotted separately for “adjusters” (workers in the smallest quartile of earnings losses) and “casualties” (workers in the largest quartile of earnings losses). The figure restricts to the “matched sample” where each casualty is paired with an adjuster of the same gender, age category, and pre-displacement occupation (three-digit).

Tables in text

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

	Non-displaced	Displaced	Difference	P-value
Total labor earnings per calendar year	48380.874	48525.941	-145.067	0.629
Gender	0.318	0.290	0.028	0.000
Real tenure	3.618	5.471	-1.853	0.000
Age (in years)	39.375	38.176	1.199	0.000
		Education:		
Low educated (no vocational degree)	0.190	0.139	0.051	0.000
Medium educated (apprenticeship degree)	0.746	0.837	-0.091	0.000
High educated (univerisity degree)	0.064	0.025	0.040	0.000
No. employees total	553.937	170.998	382.939	0.000
		Main industries of displaced workers:		
Manufacturing	0.457	0.449	0.008	0.042
Wholesale and retail	0.170	0.217	-0.047	0.000
Construction	0.093	0.165	-0.072	0.000
Individuals	567508	161,213		

Notes: The table presents Source: IEB 1984 – 2010.

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.

Table 2: Characteristics of relative loss distribution

	All	Education			Gender	
		Low	*Medium*	*High*	*Women*	*Men*
Mean	-1.275	-1.841	-1.196	-0.765	-1.612	-1.137
Mode	-0.489	-0.534	-0.482	-0.212	-0.520	-0.493
Skewness	-0.428	-0.037	-0.493	-0.476	-0.101	-0.551
P25	-2.354	-3.258	-2.188	-1.797	-3.105	-2.042
P75	-0.096	-0.486	-0.068	0.468	-0.208	-0.069
Loss $<$ 1 month	0.246	0.167	0.256	0.358	0.223	0.255
N	15960	2213	13364	383	4625	11335

Notes: The table presents Source: IEB 1984 – 2010.

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).

Table 3: Variance decomposition of displacement losses

	Estimated earnings losses		Synthetic counterfactual estimates	
	Variance comp.	Share of total	Variance comp.	Share of total
Total variance of loss	6430.151	1.000	11732.548	1.000
Individual char.	104.019	0.016	1188.151	0.101
Education	9.037	0.001	951.945	0.081
Firm f.e.	803.362	0.125	3103.390	0.265
Occupation f.e.	193.275	0.030	2013.387	0.172
Region f.e.	40.177	0.006	609.464	0.052
Citizenship	37.085	0.006	22.733	0.002
Residuals	5333.934	0.830	3414.879	0.291
Covariances	-90.738	-0.014	428.599	0.037

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

Table 4: Characteristics of adjusters and casualties workers

	Casualties	Adjuster	Difference	p-value
Gender	0.332	0.224	0.108	0.000
Real tenure	5.612	5.464	0.148	0.001
Age (in years)	38.836	37.925	0.911	0.000
Log firm wage	4.433	4.498	-0.065	0.000
No. employees total	161.176	183.043	-21.866	0.021
Log daily wages	4.550	4.651	-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
Quarter of leaving before closure:				
< 1	0.729	0.698	0.031	0.001
2	0.208	0.235	-0.027	0.002
$>= 3$	0.063	0.067	-0.004	0.390
Relative loss	-2.453	-0.064	-2.390	0.000
Observations	4,839	4,841		

Notes: This table compares the characteristics of adjusters and casualties in the matched sample. "Adjusters" are workers in the smallest quartile of earnings losses; "casualties" are workers in the largest quartile of earnings losses.

Table 5: Characteristics of adjusters and casualties (matched sample)

	Disrupted	Adjuster	Difference	p-value
Gender	0.373	0.373	0.000	1.000
Real tenure	5.400	5.368	0.032	0.825
Age (in years)	36.703	36.762	-0.059	0.904
Log firm wage	4.460	4.460	0.000	1.000
No. employees total	244.961	244.961	0.000	1.000
Log daily wages	4.499	4.542	-0.043	0.134
Quarter of leaving before closure	0.467	0.517	-0.050	0.282
Education				
Low	0.142	0.142	0.000	1.000
Medium	0.851	0.851	0.000	1.000
High	0.007	0.007	0.000	1.000
Quarter of leaving before closure:				
< 1	0.660	0.621	0.039	0.096
2	0.249	0.278	-0.029	0.170
$>= 3$	0.091	0.101	-0.009	0.511
Relative loss	-3.197	0.427	-3.625	0.000
Individuals	855	855		

Notes: This table compares the characteristics of 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). “Adjusters” are workers in the smallest quartile of earnings losses; “casualties” are workers in the largest quartile of earnings losses.

Table 6: Educational updating

	Low educated		Medium educated	
	Unmatched	*Matched*	Unmatched	*Matched*
Adjuster	-0.009**	-0.014**	-0.005***	-0.004
	(0.004)	(0.007)	(0.001)	(0.003)
Observations	13842	2262	71029	16095
R^2	0.004	0.007	0.002	0.003

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).

Appendix

NTD: FIX FIGURE NUMBERING

A Event study

Figure B.1 compares the results from a standard event study approach to the results from our synthetic control group approach. In more detail, we ran event studies that compare the earnings trajectories of workers who lost their jobs in firm closures to those of comparable workers who did not lose their jobs in firm closures. Before running the event studies, we used propensity score matching to align the displaced workers of the treatment group to the non-displaced workers of the control group. We match the treatment and control group workers on the same base line variables that we have used for the synthetic control group approach (i.e., age, gender, education, three-digit occupation, two-digit industry, and firm size). Moreover, we apply the same sample restriction as in our baseline data and only consider workers who worked in large firms with more than 50 employees and had at least two years of tenure before the respective displacement year. See the data section for more details.

Afterwards, we separately ran the following event studies for each displacement year between 2001 and 2005 and aggregate the coefficient estimates using the observations from each separate regression as weights.

$$Y_{it} = \alpha + \lambda_t + \sum_{k=-5}^{5} \delta_k + X_{it}\beta + \epsilon_{it} \tag{A.1}$$

where Y_{it} is the dependent variable of annual earnings, λ_t are the year fixed effects. $\sum_{k=-5}^{5} \delta_k$ are the event-time dummies that follow all treated workers throughout five years before until five years after the treatment. X_{it} denotes a set of control variables that we restrict to three age categories and ϵ_{it} is the error term. As we ran the event studies separately for each displacement year and only compare the treated displaced workers to the group of non-displaced workers who are never treated, we avoid common problems that arise in two-way fixed effects models with multiple treatment times that rely on varying observation periods and use *not yet treated* observations as controls.

Figure B.1 presents the results. The solid line shows the aggregated average displacement losses from our event studies. The dashed lines represent the average displacement losses from the synthetic control group approach on the individual level. Both approaches show qualitatively similar patterns. However, the event study approach resulted in slightly larger earnings losses, particularly in the longer run.

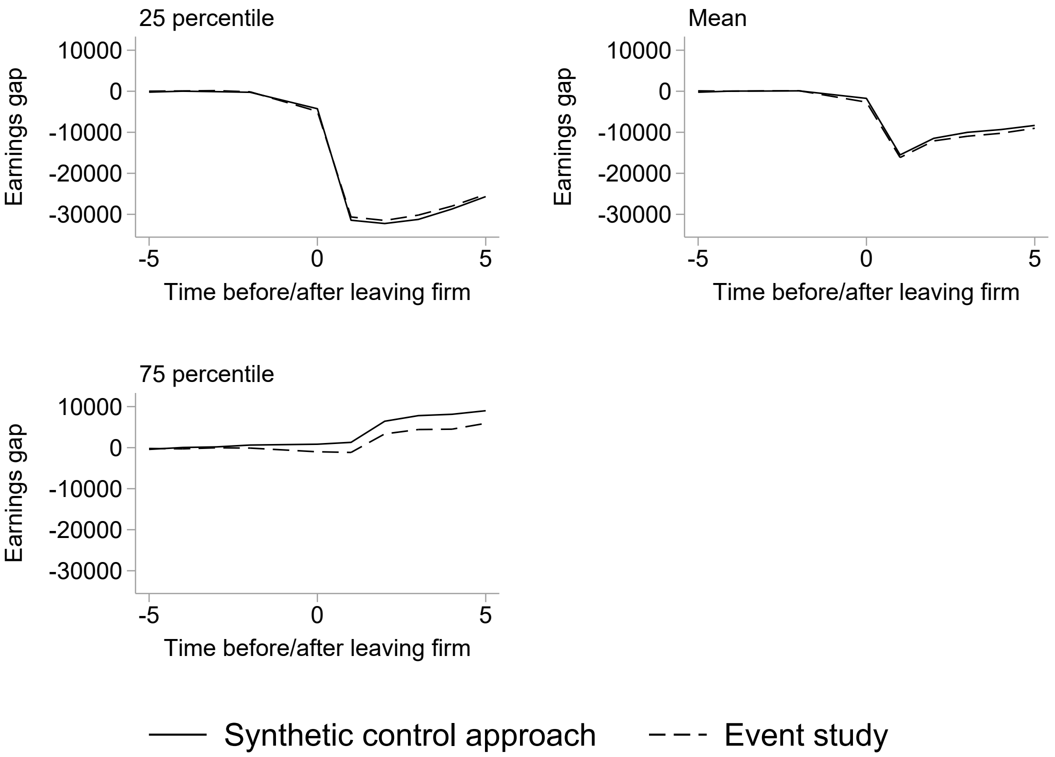

Figure A.1: Regular event study vs. synthetic control group approach

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; the figure plots the average of these estimates. The figure plots comparisons separately for the earnings losses at the 25th percentile, mean, and 75th percentiles.

B Event study

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 grey 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.

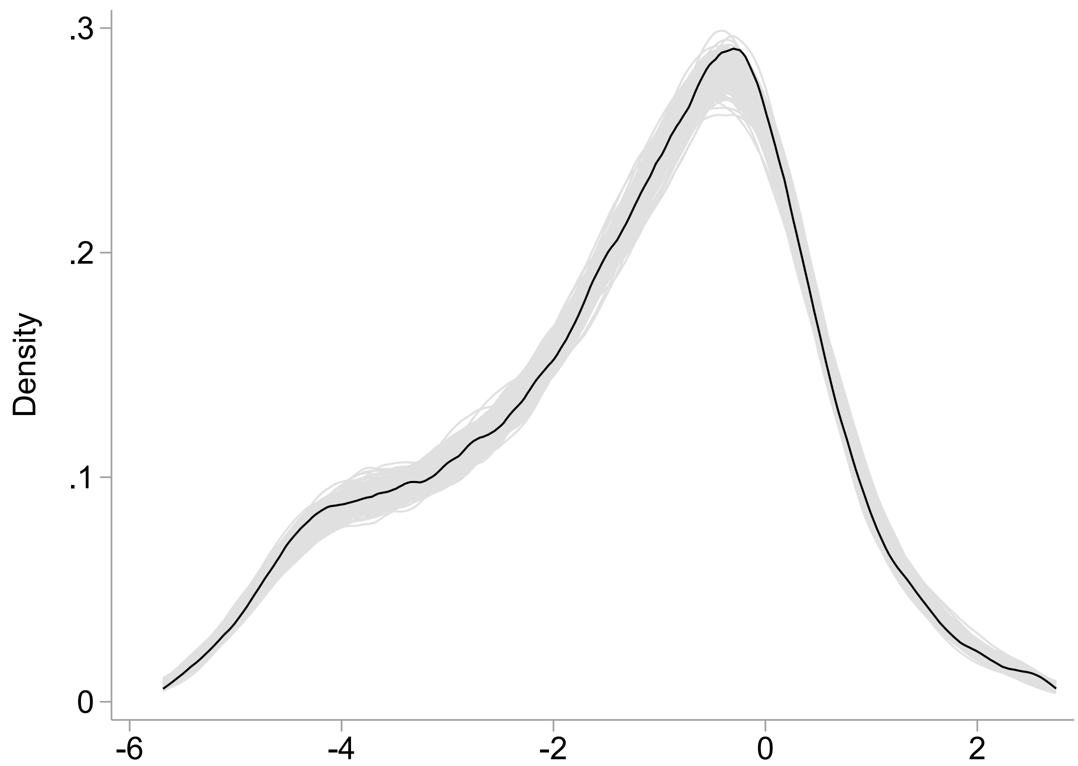

Figure B.1: Permutation of loss distribution on 100 ten percent samples

Notes: This figure plots the distribution of earnings losses derived from 100 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.