The Costs of Job Displacement over the Business Cycle and Its Sources: Evidence from Germany

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We document the sources behind the costs of job loss over the business cycle using administrative data from Germany. Losses in annual earnings after displacement are large, persistent, and highly cyclical, nearly doubling in size during downturns. A large part of the long-term earnings losses and their cyclicality is driven by declines in wages. Key to these long-lasting wage declines and their cyclicality are changes in employer characteristics, as displaced workers switch to lower-paying firms. Changes in characteristics of workers or displacing firms explain little of the cyclicality, though nonemployment durations correlated with losses in employer effects play a role.
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Workers displaced during mass-layoffs experience large losses in annual earnings with long-term effects lasting up to 20 years (e.g., Jacobson, LaLonde and Sullivan 1993; Couch and Placzek 2010; Davis and von Wachter 2011) that are substantially higher in recessions (e.g., Davis and von Wachter 2011; Farber 2016).[1] A growing literature has examined the sources of such costly and persistent effects of job loss, focusing on the role of industry wage differentials (e.g., Katz and Dickens 1987; Krueger and Summers 1988), firm wage differentials (e.g., Lachowska, Mas and Woodbury 2020; Moore and Scott-Clayton 2019 building on the Abowd, Kramarz and Margolis 1999 - AKM - model), and of firm, industry, and occupation specific skills (e.g., Neal 1995; Poletaev and Robinson 2008; Huckfeldt 2022).

With displacement rates reaching 10-15% of employment during large recessions (e.g., Farber 2011; Song and Von Wachter 2014), job displacement is a particularly costly phe-

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[1]In addition to earnings and employment, recent literature has analyzed a large number of other potential effects of job loss on outcomes, such as health (Schaller and Stevens, 2015), mortality (Sullivan and Von Wachter, 2009), retirement (Chan and Huff Stevens, 2001) and the children of displaced workers (Lindo, 2011; Rege, Telle and Votruba, 2011). See Carrington and Fallick (2014) for a survey of the literature on the cost of job loss.

nomenon during recessions. Even though many studies analyze sources behind the average cost of job loss, few examine how the sources of the cost of job loss vary over the business cycle. Yet, several of the hypotheses considered by the existing literature are related to the business cycle. Recessions have been interpreted as periods of increased rate of potential costly job switches across sectors and occupations, or of reallocation of marginally productive jobs (e.g., Schumpeter 1942; Barlevy 2002; Foster, Grim and Haltiwanger 2016). It has also been long hypothesized that recessions are periods of cyclical downgrading (Okun 1973; McLaughlin and Bils 2001), because high-wage sectors have more cyclical job creation (e.g., Reynolds 1953; Reder 1955). A small but growing number of studies suggest a similar phenomenon occurs at the firm level (e.g., Moscarini and Postel-Vinay 2016; Haltiwanger et al. 2018), and hence could explain cyclical wage losses at job displacement. Longer unemployment spells due to slack demand in recessions could lead to lower reemployment wages through skill depreciation (e.g., Schmieder, von Wachter and Bender 2016; Nekoei and Weber 2017; Jarosch 2021).

In this paper, we study what determines displaced workers' earnings and wage losses over the business cycle using three decades worth of social security data from Germany. Our data and a new parsimonious empirical approach to studying job loss allow us to analyze the role of losses in establishment wage premia together with other channels such as lengthening unemployment spells and losses in occupation- and industry-specific skills over the business cycle.[2] At the same time, our approach allows us to control for changes in worker and firm composition over the business cycle. Our findings extend our understanding of the role that establishment wage premiums and other potential mechanisms play in the fluctuating cost of job loss over the business cycle (e.g., Davis and von Wachter 2011). The results also allow us to further connect the longstanding and hard-to-explain results from the job loss literature with the recently re-emerged literature on the role of employers in wage setting (e.g., Card, Heining and Kline 2013; Song et al. 2018; Bonhomme, Lamadon and Manresa 2019; Kline, Saggio and Sølvsten 2020).

Our data covers three decades of job displacements with detailed information about daily employment transitions, wages and employment, and firm and worker characteristics not typically available in U.S. data. Using information that spans multiple recessions, we provide an analysis of the long-term earnings losses of displaced workers in Germany, carefully ensuring comparability of our results to recent estimates using administrative data from the U.S. used in Davis and von Wachter (2011) and Lachowska, Mas and Woodbury (2020), among others. We then assess whether earnings losses and their cyclicality are due to lower employment rates, or whether wage losses themselves are cyclical as well, a crucial question for understanding the cyclical cost of job loss that is more difficult to answer with U.S. data.

In a final step, we analyze key sources of cyclical movements in wage losses highlighted in the literature, including changes in displaced workers' firm characteristics over the business cycle, indicators of specific skills, nonemployment duration, and changes in worker composition. We depart from the canonical estimation approach in the literature

[2]Note that we use the term firm throughout the paper for simplicity. In the data we can only identify establishments, which we cannot link up to the firm level.

on job displacement, and directly model the individual wage loss vis-a-vis a matched control observation. This allows us to effectively account for composition changes along several dimensions and to directly analyze various channels behind the cyclicality of the cost of job loss in a unified regression model.

Using a sample of male workers from West Germany, we obtain several key findings. First, as in comparable studies in the U.S., we find that workers in stable jobs separating from their main employer in the course of a mass-layoff during recessions suffer reductions in annual earnings of about 15% lasting at least 15 years. This suggests that job displacement has highly detrimental effects on earnings even in a labor market with a tighter social safety net and lower earnings inequality as in Germany. Also, consistent with typical U.S.-based findings, we find that there is a high degree of cyclicality in earnings losses in Germany, with losses in recessions more than double the losses in booms.

Our second finding is that the patterns of longer-term earnings losses after job displacement are to an important degree explained by cyclical wage losses. Reductions in reemployment rates explain an important part of the initial reductions in earnings, but play a small role for long-term earnings losses. While cyclicality in the incidence of nonemployment among displaced workers is expected, cyclical variation in reemployment wages is harder to explain.

Our third key finding is that losses in establishment wage premiums explain the majority of the level and the cyclicality in wage losses. We first establish that changes in the composition of job losers or displacing firms cannot explain cyclical wage losses. We then show that displaced workers experience substantial reductions in establishment size and establishment wage premiums at job loss. In the short run (1 year after job loss) 70 percent of wage losses are explained by losses of establishment effects, while in the long run the share explained increases to around 90 percent. We also show that these reductions are larger in recessions. Our accounting regressions suggest that the majority of average wage losses and a large part of their cyclicality are driven by reductions in establishment wage premiums.

Another important finding is that several commonly proposed channels of the cost of job loss correlate closely with losses in establishment effects. Hence, while we find a role for duration of nonemployment and various measures of changes in industry or occupation for explaining the average cost of job loss, part of their effect can be accounted for by losses in establishment effects. However, among all channels we considered, only nonemployment duration has explanatory power for the cyclicality of wage losses, consistent with recent causal estimates of the effect of nonemployment duration on wages in Schmieder, von Wachter and Bender (2016). Importantly, we find losses in establishment effects can explain almost half of the negative wage effect of nonemployment duration over the business cycle.[3]

Several papers have emphasized the problem of "limited mobility bias" in estimates

[3]We also show that payments from the German unemployment insurance system only replace about 25% of displaced workers' lost earnings and reduce the cyclicality of earnings losses by about 15%. While playing an important role to cover income losses among the unemployment, UI plays a smaller role for the average job loser since it does not insure for the short- and long-term wage losses.

of the AKM model,[4] which can be particularly problematic in short panels and in presences of small employers (Bonhomme, Lamadon and Manresa, 2019; Kline, Saggio and Sølvsten, 2020). While our very long panel and emphasis on displaced workers from large establishments reduces this concern, we also subject our analysis to thorough sensitivity checks: we introduce split-sample IV estimates to correct for bias, we re-estimate the IV analysis using a rolling window of fixed effects, and, following the spirit of Bonhomme, Lamadon and Manresa (2019), generate comparable findings based on a small number of firm-clusters. Our findings are robust to these and many other sensitivity checks. They are also very similar for women or when we include East Germany in the analysis.

These findings make several contributions to the literature on the effects of job loss and the wage structure. This is the first paper to systematically analyze the role of employer wage effects and a multitude of other sources behind the cyclicality of the cost of job displacement found in Davis and von Wachter (2011) and in Farber (2016). We find that changes in the composition of firms hiring job losers in recessions can explain the majority of the cyclicality in wage losses. This is consistent with studies showing that job creation in recession shifts to less attractive firms (e.g., Haltiwanger et al. 2018) and lower-wage industries (e.g., Reynolds 1953; Reder 1955; McLaughlin and Bils 2001). Our results are also reminiscent of findings that persistent earnings losses of entering the labor market in a recession can be partly explained by cyclical downgrading (e.g., Okun 1973; Oreopoulos, von Wachter and Heisz 2012).

Our paper also contributes to emerging literature studying the importance of establishment wage effects for the average cost of job loss (e.g., Lachowska, Mas and Woodbury 2020; Moore and Scott-Clayton 2019) and for wage losses at outsourcing Goldschmidt and Schmieder (2017). Fackler, Mueller and Stegmaier (2021) and Woodcock (2022) find that losses in firm effects explain a majority of wage loss at firm bankruptcy and after the Hartz reforms in Germany, respectively, and Bertheau et al. (2022) show that this holds for a wide range of European countries. By analyzing the effects of job loss by quintiles of pre-displacement establishment effects (Section IV.C), we partly reconcile our findings with those in Lachowska, Mas and Woodbury (2020) who find a smaller role of firm effects in explaining wage losses in the state of Washington. Overall, our findings underscore the importance of firm-specific components in wage setting shown by Card, Heining and Kline (2013) for Germany and also found in recent studies in the U.S. and other countries (e.g., Song et al. 2018; Kline, Saggio and Sølvsten 2020; Bonhomme, Lamadon and Manresa 2019), consistent with findings of the earlier literature that displaced workers lose industry- and occupation-specific rents (Krueger and Summers 1988; Katz and Dickens 1987). While large and persistent wage losses at job loss are hard to reconcile with a competitive model of the labor market, they arise naturally in the context of pervasive firm-specific rents and frictions.

A further contribution to this literature is that we show that low-wage job losers tend

[4] Establishment effects are only identified by movers between establishments and the number of movers per establishment tends to be relatively small, which leads to an upward bias in the estimated variance of establishment effects. E.g. Abowd et al. (2004); Andrews et al. (2008, 2012).

to experience larger and more cyclical wage losses. Consistent with the presence of outsourcing, they also suffer larger losses in establishment effects than higher-wage workers, even when exiting the same establishment. However, we do *not* find evidence that losses in establishment wage effects for low-wage workers intensify in recessions relative to high-wage workers.[5] The fact that such outsourcing does not intensify over the business cycle is further evidence that the cost of job loss is driven by the change in the type of firms that hires over the business cycle (e.g., Haltiwanger et al. 2018), not the type of firms that experiences layoffs. It also confirms evidence from the literature suggesting that in recessions low-wage workers suffer larger income and employment losses (e.g., Cutler and Katz 1991; Hoynes, Miller and Schaller 2012; Haltiwanger, Hyatt and McEntarfer 2018) but this does not seem to be due to larger moves down the job ladder to less productive firms. Overall, these findings further help to extend the stylized facts on the cost of job loss over the business cycle, and suggest that job search and job ladder models aiming at explaining these results would benefit from incorporating worker heterogeneity.

Another contribution of the paper is that we can jointly analyze key channels discussed in the literature, leading us to clarify how several channels are correlated with employer wage effects. In particular, our analysis reveals that losses in establishment effects is an important driver of the effect of nonemployment duration on wages (Schmieder, von Wachter and Bender, 2016), and a channel through which job instability can augment the cost of job loss (Jarosch, 2021). The analysis also shows how cyclical downgrading is more severe for the long-term unemployed, leading to amplify the cost of prolonged recessions (e.g., Song and Von Wachter 2014). Our results also confirm that both losses in rents and specific skills are likely relevant factors in explaining the average cost of job loss (e.g., Topel 1990). More importantly, they show that losses establishment wage effects explain a substantial fraction of the role of industry or occupation variables, shedding light on a commonly studied channel in the literature (e.g., Neal 1995; Poletaev and Robinson 2008; Huckfeldt 2022). The result that our industry and occupation variables contribute little to explaining the cyclicality of the cost of job loss is consistent with a large empirical literature showing that reallocation between industries or occupations does not appear to be a major source of employment fluctuation over the business cycle (e.g., Abraham and Katz 1986; Aaronson and Christopher 2004; Rothstein 2017).[6]

Last but not least, our approach provides a convenient, two-step estimation procedure that allows for an integrated treatment of a range of potential explanatory factors of the cost of job loss. Building upon Jacobson, LaLonde and Sullivan (1993)'s seminal contribution, most studies have analyzed the differences in the cost of job loss by worker and establishment characteristics by estimating large interacted distributed lag models. Here, we take cues from the literature on heterogeneous treatment effects as well as the

[5] This is consistent with Dorn et al. (2018), who do not find that domestic outsourcing increases in recessions.

[6] This does not preclude reallocation among other dimensions. Raposo, Portugal and Carneiro (forthcoming) estimate a model with firm, worker, and job title fixed effects in Portugal and find that both changes in job titles and changes in firm effects can explain the cost of job displacement. Hershbein and Kahn (2018) show that recessions increase the skill-requirements of new job postings. Huckfeldt (2022) develops a model of how fluctuation in skill requirements could explain part of the cyclical cost of job loss.

assumptions of the AKM model, and model individual-level wage losses with respect to a control observation as a function of worker and establishment variables. While the two approaches can be made isomorphic, our parsimonious approach allows us to easily control for changes in worker and firm composition over the business cycle, and better focus on the relative contribution of various explanatory factors, especially when analyzing the effect of job loss over the business cycle.

Section I describes our data, defines job displacement, and introduces our approach. Section II and III benchmark our findings to comparable estimates from the U.S. and analyze the effect of job loss on earnings, employment, and wages over the business cycle. Section IV analyzes how employer characteristics of displaced workers change over the business cycle. Section V assesses to what extent these changes can explain the large and cyclical wage losses we find. Section VI discusses potential mechanisms behind our findings.

I. Data and Methods

A. *German Administrative Data*

We use data from the social security system in Germany, provided by the Institute for Employment Research (IAB).[7] This data consists of complete day-to-day information on earnings and time worked in each employment spell. The data also contains basic demographic characteristics including education, as well as information on occupation, industry and receipt of unemployment insurance benefits. In addition, the worker-level data has been merged with information on employers at the establishment level to create a linked employer-employee panel spanning over 30 years from 1975 to 2009.[8]

B. *Measuring Job Displacement at Mass-Layoffs*

We follow the existing U.S. literature based on administrative data and define job displacement as an event when a worker with at least three years of tenure leaves a stable job at his main employer in the course of a mass-layoff. The analysis of workers leaving stable jobs has several advantages. It focuses on workers who in all likelihood expected to remain in their job in the absence of a mass-layoff, and thus were likely to be surprised by being displaced. Moreover, given the steep reduction in job mobility with even a few years of job tenure in Germany, very few of these workers were likely to have moved voluntarily. This reduces the measurement error in the definition of job displacement and helps establish a counterfactual outcome, since most of these workers would likely have remained in covered employment absent a job loss.

We work with two common definitions of a mass-layoff event: mass-layoffs where employment declines by at least 30 percent and permanent establishment closings. To

[7] We use extracts from the Integrated Employment Biographies (IEB) database prepared by IAB. Access to this data is regulated by Section 75 of the German Social Code (Book X).

[8] Self-employment and civil servants are not covered in this data. The lack of self-employment is comparable to other papers studying job displacement using administrative data, while the lack of information on civil servants, who benefit from extensive employment protection, is unlikely to matter for job losers.

make these definitions meaningful, we follow the literature and consider only workers whose employers had at least 50 employees in the year prior to the employment drop and did not have large employment fluctuations in the years before. For smaller establishments these measures of mass-layoff are less meaningful.[9] We also use information on worker flows between establishments to distinguish mass-layoffs and plant closings from other events such as outsourcing or ownership changes as in Hethey-Maier and Schmieder (2013). See Appendix Section 1 for details.

By focusing on job separations of high-tenured workers during mass-layoffs at medium-sized to large employers we obtain a clean measure of job displacement that is comparable to the existing literature. It is important to bear in mind that these definitions exclude many potential job losers, and our study does not intend to capture the experiences of these other job losers. Analyses in von Wachter, Song and Manchester (2011) and Hildreth, Handwerker and von Wachter (2009) have shown that in the U.S., these estimates are robust to relaxing the restrictions on job tenure, and moderate variations in the restrictions on firm size and size of the mass-layoff. Nevertheless, changes in the composition of job losers and firms displacing them over the business cycle may affect comparisons of the effects of job loss over time and over the business cycle. We address such potential composition changes explicitly in our analysis of the role of worker and employer characteristics in explaining the costs of job loss over the cycle.

C. Constructing a sample of displaced workers and a control group

Baseline restrictions We construct our analysis sample in two steps. First, based on a 25 percent random sample of all workers in Germany, we denote the year prior to displacement the "baseline year" $c-c$ and we choose for each baseline year all workers that satisfy the following restrictions on June 30th for that year: the individual is male, is between age 24 and 50, and works full time at a West German establishment with at least 50 employees, and has at least 3 years of tenure.[10] We define an individual as displaced (between year $c-1$ and c) if a) the individual leaves the establishment between $c-1$ and is not employed at the year $c-1$ establishment in any of the years $c, c+1, \ldots, c+10$ and b) the establishment has a mass-layoff (or plant closing) between year $c-1$ and c.

We focus our main analysis on men for two reasons: First, to facilitate comparisons with the earlier literature that has typically focused on men and, second, since the higher labor force attachment of men leads to less selection issues between in and out of the labor force, simplifying the interpretation of the results. We have, however, replicated the entire analysis for women. The results are very similar and will be discussed in Section V.D.

Our data covers a broader number of workers and labor force states than typical admin-

[9]Davis et al. (2016) show that at least in the U.S., at a 30% employment loss the majority of workers leaving the firm are laid off rather than voluntary quitters. Thus, this cutoff further helps in reducing measurement error from the presence of voluntary movers among the displaced.

[10]We also exclude individuals employed in the following sectors: mining, public administration, defense, activities of private households and extra-territorial organizations. These restrictions follow largely the existing literature. In addition, we drop workers younger than age 24, since they may not have fully entered the labor force. We also drop workers older than age 50, who had access to partial retirement programs in Germany during that period.

istrative data sets, since we observe receipt of unemployment insurance and means-tested unemployment assistance. Moreover, given that we have data on the national labor market, our sample includes individuals moving within Germany, and we impose relatively few restrictions on their presence in the labor market after job loss. There is a fraction of individuals that permanently drops out of our sample, among others because they stop working, work in self-employment, work in government jobs or move abroad. Here, we follow von Wachter, Song and Manchester (2011) who use national data in the U.S. and keep individuals in the sample even if they have zero earnings.[11]

Propensity Score Matching Displaced and non-displaced workers may differ in ways that make them difficult to compare. While differences in average outcomes are easily controlled for using worker fixed effects, controlling for differential trends among treatment and control groups is more difficult. We use propensity score matching to obtain a comparison group that provides the appropriate counterfactual earnings trends for the displaced workers in our design. Starting with our baseline restrictions, we use a two step-matching estimator where we match within baseline year and 1-digit industries based on a number of matching variables. Specifically for each baseline year and 1-digit industry, we estimate the propensity of being displaced using establishment size in year $c-1$, the worker's log daily wage in year $c-2$ and $c-3$, as well as education, job tenure, and age in year c as predictors. For each displaced worker we assign a single comparison worker, using the non-displaced worker with the closest propensity score (without replacement). Note that there is no restriction that workers in the comparison group have to stay at the same establishment between year $c-1$ and c, nor that they cannot be displaced in future years.

This yields a group of displaced workers and a very comparable set of non-displaced workers working at similar firms. This is in spirit closely related Abadie (2005) who suggests to use propensity score reweighting to create a control group with the same pre-displacement characteristics for which the parallel trends assumption is likely to hold. Since we create an explicit control group in each year by industry cell, and then stack the data from each cell, we also avoid the complications arising in difference in difference designs with multiple time periods pointed out by Goodman-Bacon (2021). Indeed our strategy of stacking matched treatment-control groups for each displacement year is very similar to the estimator proposed by Callaway and Sant'Anna (2021).

Table 1 displays average worker characteristics (Panel a) and employer characteristics (Panel b) of our sample of displaced workers (column 1), the matched sample of non-displaced workers (column 2), as well as a random sample of (unmatched) non-displaced workers (column 3). Even absent matching, Table 1 shows that observable characteristics between displaced workers (column 1) and non-displaced workers (column 3) prior to displacement are reasonably similar. Compared to the random sample, there is a small difference in pre-displacement daily wages, possibly due to lower job tenure. Figure

[11] Since some of these workers may be self-employed, this assumption likely overestimates the earnings losses of displaced workers. As an alternative, we have used information on individuals that work in covered employment or receive unemployment benefits at least once after job loss and drop them after the last observation in the dataset. The patterns for earnings losses are very similar. By construction this does not affect our main results on wages.

1 shows that the two groups exhibit almost identical pre-displacement trends in wages, earnings and employment. Table 1 and Figure 1 show that pre-displacement differences in wages, earnings, and job tenure are tiny in our matched sample. In the robustness section, we show that the results do not hinge on the use of propensity score matching, and hold when simply using a random sample of workers who satisfy the baseline restrictions as a control group.[12]

D. Empirical Approach

(1) Event study Analysis We first provide estimates of the effects of job loss on a variety of outcomes using an event study analysis. Let y_{itc} be the outcome of interest for a worker i, with baseline year c observed in year t. Furthermore, let Disp_i be an indicator variable for whether worker i is a displaced worker or belongs to the control group. We estimate the following regression model:

$$(1) \quad y_{itc} = \sum_{k=-5}^{15} \delta_k \times I(t = c+k) \times \text{Disp}_i + \sum_{k=-5}^{15} \gamma_k \times I(t = c+k) + \pi_t + \alpha_i + X_{it}\beta + \varepsilon_{itc}$$

The main coefficients of interest are δ_k, which measure the change in earnings of displaced workers with respect to the baseline year (c), *relative* to the evolution of earnings among non-displaced workers captured by year effects (π_t).[13] As we discuss in the appendix (Appendix Section 2), it is important to control for "year relative to baseline year" fixed effects (coefficients γ_k), since the tenure restriction in the baseline year leads to hump-shaped earnings profiles around the baseline year even for the control group that cannot be captured by year effects (included as π_t) alone. In addition, we control for individual effects α_i and time-varying control variables (X_{it}), chiefly worker age. Since our matching procedure implies that worker characteristics in the treatment and control groups are very similar at baseline, the inclusion of both the worker fixed effects and the X_{it} make little difference to the estimates.

(2) Decomposing the Sources of Cyclical Wage Losses using a Matched Diff-in-Diff Design The main goal of the analysis is to compare the effect of job loss on wages over the business cycle and assess the importance of potential determinants of the cost of job loss. Since the literature has shown that different types of workers can have different wage losses, to make the comparison over the cycle meaningful we need to control for potential changes in the composition of job losers over the cycle. We also would like to

[12] We also estimated the main results using alternative matching algorithms, such as not matching on industry, matching within counties, or matching on fewer variables and found almost identical results. Our results are not affected by whether a worker is displaced during a mass-layoff or a plant closing, so we do not differentiate between the source of layoff going forward. Appendix Table A-2 breaks down summary statistics by source of layoff, and we assess the role of plant closings for understanding cyclicality in Section V.D. Displacement rates over the business cycle are shown in Appendix Figure A-1.

[13] I.e., the specification omits δ_{t^*-5} and one of the year dummies to avoid collinearity. Essentially these are absorbed in the constant (γ_0). This means the δ_j can be interpreted as the difference between the two groups after taking out the initial difference in year $t^* - 5$.

assess whether the effect of job loss differs by worker and firm characteristics and which factors might help explain the fluctuations in the cost of job loss over the business cycle.

To control for composition effects and to study whether various factors, such as losses in establishment effects, can explain the wage losses at displacement, we construct an individual level measure of the wage loss after displacement. To do so, we use the fact that we have paired each job loser in our sample with a statistical twin via our matching procedure, and calculate an individual-level estimate of the wage loss at displacement:

$$\Delta_{dd} w_{ic} = \Delta_d w_{ic} - \Delta_{nd} w_{ic}$$

where $\Delta_d w_{ic}$ is the short-term individual wage change before (-5 to -1 years) and after (0 to 3 years) job displacement (and $\Delta_{nd} w_{ic}$ is the wage change for the matched control individual) for a displaced worker i with baseline year $c-1$. One can think of $\Delta_{dd} w_{ic}$ as an estimate of the individual treatment effect from job loss for each worker.

Based on this individual treatment effect of job loss on wages, it is then straightforward to investigate the role of composition changes over the business cycle or heterogeneity in the cost of job loss. To investigate the cyclicality of the cost of job loss we then estimate the following regression model for the individual cost of job loss:

$$\Delta_{dd} w_{ic} = \beta\, \Delta UR_c + \gamma\, \hat{\psi}_{J(i,c)} + \delta\, \hat{\alpha}_i + X_i \theta + c\pi_1 + c^2 \pi_2 + \varepsilon_{ic} \quad (2)$$

where our main measure of cyclicality will be the annual change in the national unemployment rate ΔUR_c. Given the high stock of long-term unemployed in Germany during the later part of our sample period, the change often provides a better measure of cyclical variation.[14] To control for a long-term trend in wage losses observed in the data, all models include a quadratic time trend.

The key parameter of interest is β, the cyclicality of the wage loss at job displacement. We begin by estimating the model without control variables. To then control for changes in worker and establishment composition, we control for $\hat{\alpha}_i$ (the estimated individual FE) and $\hat{\psi}_{J(i,c)}$ (the estimated establishment fixed effect *before* displacement). We also include quadratic terms of prior job tenure and of potential labor market experience.

The coefficient estimates on the covariates in regression model (2) are numerically equivalent to the short-term impact estimates of a fully interacted event-study model (e.g., as presented in Table 3 of Jacobson, LaLonde and Sullivan (1993) and many other papers). The advantage of the approach here is that it is a more parsimonious analysis of heterogeneity in the treatment effect. It is also easier to interpret the role of adding different covariates compared to models fully parametrizing the earnings path. Given our finding below and in Davis and von Wachter (2011) that what changes over the business cycle is mainly the short run effect, upon which a common recovery path follows, this approach is a natural way to proceed.

Finally, in order to assess the role played by the loss in establishment fixed effect we

[14] In particular, the change is more correlated with GDP growth, a key measure of the business cycle. We also replicated our entire analysis using the level of the unemployment rate instead of the change in the unemployment rate. The results are shown in our main tables and the appendix, and are qualitatively similar.

add to this model the change of the establishment effect at job loss relative to the control group $\Delta_{dd}\hat{\psi}_{J(i,t)}$. :

$$\Delta_{dd} w_{ic} = \beta\, \Delta UR_c + \gamma\, \hat{\psi}_{J(i,c)} + \delta\, \hat{\alpha}_i + \xi\, \Delta_{dd}\hat{\psi}_J + X_i\theta + c\pi_1 + c^2\pi_2 + \varepsilon_{ic} \tag{3}$$

Comparing the coefficient estimate with and without controlling for losses in establishment effects will indicate to what extent losses in establishment quality over the business cycle drive the cost of job loss. An additional advantage of our regression approach is that it makes it straightforward to add other channels of the cost of job loss that may change over time, such as nonemployment duration or changes in industry and occupation effects.

There are two important caveats to bear in mind. First, clearly post-displacement establishment characteristics (and other post-displacement career outcomes) included in regression model (3) may be endogenous. Although care has to be taken in interpreting these estimates as causal effect of changes on employer characteristics on earnings losses, the approach remains useful. The correlation is informative in its own right. Moreover, if workers are positively selected into firms with higher wages, the estimates serve as a *lower bound* of the remaining cost of job displacement. In the absence of credible exogenous variation in career outcomes, it has been common practice in the displacement literature to compare wage changes at different career outcomes (e.g., multiple displacements or changes in industry or occupation). An advantage in our setting is that we add the cyclical dimension. For example, we replicated the main result using purely annual variation in average changes in earnings and establishment characteristics, which are not affected by selective entry into establishments.[15] Another aspect relevant here is that in the framework of the AKM-model we discuss in the next section, worker and firm effects are sufficient to control for selective job mobility. The model appears to explain the German wage structure (Card, Heining and Kline 2013) well, lending some support for specifying an empirical model of the kind of regression (3). While the data-generating process underlying the basic AKM model posits that all of the loss in wages should be explained by a change in establishment effects, in practice likely other channels play a role as well.

A second caveat is that systematic wage differences across establishments may in principle not only capture rents, but other components of the wage structure, such as compensating differentials (Sorkin, 2018). While no conclusive evidence is available, the fact that establishment wage premiums are closely correlated with value added per worker supports the interpretation of wage premiums as rents (Card, Cardoso and Kline, 2016).

[15]This approach can be interpreted as using only the cyclical variation in market-wide firm quality to identify the estimates, or in other words as using year dummies as instruments for changes in firm quality. Given other career aspects vary over the business cycle, this might overstate the effect of changes in firm characteristics. In principle, one could control for the annual averages of these other career outcomes as well.

E. Outcome Variables and the AKM Model

A key advantage of our data is that it contains detailed information on daily wages (as recorded on June 30th each year) and days worked, which is not available in administrative U.S. data covering multiple business cycles. We use this data to decompose earnings losses over the business cycles into losses in daily wage and losses in the total number of days worked per year. We then analyze the effect of job loss on changes in employer attributes, chiefly the estimated establishment fixed effect, and decompose the wage loss over the business cycle into a range of factors as discussed in the previous section.[16]

To obtain estimates of persistent differences in employer daily wages, we estimate the AKM model using the universe of worker-establishment observations in Germany, following the implementation for Germany by Card, Heining and Kline (2013) [CHK]:

$$\ln(w_{it}) = \psi_{J(i,t)} + \alpha_i + \theta_t + x_{it}'\beta + \varepsilon_{it}, \tag{4}$$

where $\psi_{J(i,t)}$ represents a vector of establishment fixed effects, α_i a vector of individual fixed effects and θ_t and $X_{it}\beta$ are year effects and education specific experience profiles. Hence, the estimated establishment fixed effects $\psi_{J(i,t)}$ represent the adjusted log wage difference relative to a single omitted employer. Since our analysis pertains to changes in the establishment effect, the choice of the omitted employer does not affect the results. The residual, ε_{it}, captures purely transitory earnings fluctuations.

For our baseline measure of establishment effects, we estimate the AKM model pooling 30 years of data (1979 to 2009).[17] This reduces the problem of limited mobility bias in the AKM estimates of the establishment fixed effects mentioned in the introduction. In the sensitivity section we offer a range of robustness checks to address this concern. Another possible concern is that job losers themselves would contribute to the estimates of the establishment effect in the AKM model if we included them in the estimation and if job losers have wage losses for reasons outside the AKM model. To avoid this endogeneity, we exclude all post-displacement event observations for each worker who was displaced (and corresponding control workers) as well as for each establishment that had a mass-layoff.[18]

One concern about pooling 30 years of data for the AKM model is that the real establishment effects may not be constant over this long-time horizon. As an alternative to our main approach we therefore also employ a "rolling window" approach where we estimate a different AKM model for each cohort c of displaced workers, where we only use observations from year $c-5$ to year c (a 6 year window). These estimated establishment effects are then assigned to all future observations of workers in cohort c, so that we can assess post displacement outcomes. This has the advantage that establishment ef-

[16] All earnings, income, and wage measures have been deflated using the Consumer Price Index and represent Euros in 2000 prices.

[17] Appendix Table A-5 displays the basic AKM variance decomposition. The results are comparable to comparable findings in Card, Heining and Kline (2013).

[18] A related concern is that using the estimated worker effect as control variable may introduce a bias in our estimates. As further discussed in Section V.D and Appendix Section 4.3, we assessed the possible extent of such bias by instead using education as a control variable and in a Monte Carlo simulation and found little potential for bias from this source.

fects can vary over time across cohorts, while holding them constant within cohorts. To further allow for establishment effects that also vary within cohorts, we also replicated our main findings with time-varying establishment effects.

Another potential concern is that when used as control variables in equation (3), the sampling variation from the estimated AKM effects introduces measurement error. Following Goldschmidt and Schmieder (2017), we address the potential measurement error in establishment effects by implementing a split-sample instrumental variable (IV) estimator, where we randomly split the sample of workers, calculate two sets of AKM estimates, and use one set of establishment effects as instrumental variable for the other. We implement the split-sample IV estimator both for the full sample, as well as for the "rolling window" AKM estimates. A related concern is that using the estimated worker effect as control variable may introduce a bias from measurement error correlated with the establishment effects. Hence, we also estimated equation (3) using completed education as alternative control variable to capture permanent differences in worker productivity. In addition, as further discussed in Section V.D and Appendix Section 4.3, we assessed the possible extent of such bias in a simulation and found little potential for bias from this source.

Overall, the AKM model has proven to be an empirically successful extension of the standard human capital earnings function and has developed into the workhorse model for incorporating firm or establishment components into traditional earnings regressions. Despite well-known limitations, we believe that there is sufficient support for the model to treat the estimated establishment fixed effects as useful measures of employer characteristics. To benchmark our findings, we also use more common measures of firm type, such as establishment size and average establishment turnover rates.

II. The Long-Term Effect of Job Loss on Earnings and Wages

A. *Average Labor Market Outcomes of Displaced Workers*

We begin by providing benchmark estimates of the long-term effects of job displacement on earnings, wages, employment and unemployment up to 15 years after job loss. The results imply that job displacement leads to long-lasting earnings losses that have not faded 15 years after job loss. Strikingly, both the patterns and magnitudes confirm existing findings based on comparable data in the U.S. What is new here is that we also find that job displacement leads to very long-term effects on wages.

The first step is to confirm that our empirical strategy produces comparable control groups for displaced workers in our sample. The three left panels of Figure 1 shows average labor market outcomes in the three groups of workers - displaced workers, a random/unmatched group of non-displaced workers, and a matched group of non-displaced workers. Here, we pool workers who were displaced in any year between 1980 and 1994 as well as their respective non-displaced comparison workers, so that we observe individuals for the full post-displacement window.[19] Even absent matching, the evolution of earnings (Panel (a)), wages (Panel (c)), and employment (Panel (e)) among the

[19]Pooling all displacement cohorts leads qualitatively to the same results.

non-displaced is very similar to those among the displaced. Consistent with findings in the prior literature, a difference in levels remains. Yet, once we match based on the propensity score matching method, the pre-displacement trends up to year -2 are virtually identical in all three sub-figures. Hence, our matching procedure delivers a very comparable control group and the resulting differences yield readily interpretable results even without controlling for any variables (such as worker characteristics, calendar year effects, or displacement year effects).[20]

Comparing the evolution of earnings for treatment and (matched) control groups, Figure 1 Panel (a) reveals stark earnings losses in the year of displacement. Earnings are almost 10,000 Euro lower in year 0 for the displaced workers or slightly less than 30 percent relative to average earnings prior to displacement. While subsequent years show some recovery, it is slow and even after 10 years, displaced workers still have about 4,500 Euro lower earnings than non-displaced workers, a 15% reduction relative to the pre-displacement mean. Note that control group earnings are increasing up to year -1, but show a change in slopes from then onwards. This is partly explained by the fact that workers in both groups are by definition employed in the years prior to displacement but there is no restriction after year 0, and some people start dropping out of employment. This highlights the importance of a control group and motivates the regression analysis below to get causal estimates of the displacement effects.

Figure 1 (c) and (e) show how earnings losses are explained by employment losses and wage losses, respectively. Employment drops very sharply initially - by about 95 days in the post-displacement year compared to the control group, but also recovers faster than earnings. In the long run, displaced workers work around 30 days less per year than non-displaced workers. Wages on the other hand drop by about 8-9 percent initially with the gap only very gradually shrinking to about 6 percent. Thus, while the short-term earnings losses are largely driven by employment losses, in the long-run employment and wage losses play a similar role.[21]

B. Regression analysis of labor market outcomes of displaced workers

In order to obtain results of the effects of displacement relative to the matched group of non-displaced workers including additional control variables, we used our data to implement model (1) pooling all displacement years in our sample. The results are shown in Figure 1. Figure 1 Panel (b) implies that there is a strong initial effect of job loss on earnings, an ensuing recovery lasting 5-10 years, and a substantial long-term effect still visible 15 years after job loss.[22] From the results for daily wages (Panel (d)) and days worked (Panel (f)), it is clear that the strong short-term dip and the initial recovery

[20] Note that we are matching based on earnings in year -3 and -2, in order to allow for displaced worker to have diverging pre-displacement trends in year -1, e.g., due to the fact that they are in declining establishments.

[21] This statement depends on the amount of presence in the labor force required after job loss. If we require individuals to reappear in the sample at least once, the majority of the long-term effect is due to wage reductions. See further discussion in Section 3.2 and results in Appendix Figure A-4.

[22] The corresponding regression estimates based on a random control group that is directly comparable to estimates in Jacobson, LaLonde and Sullivan (1993) and Davis and von Wachter (2011) are shown in Appendix Figure A-3 and discussed further in Appendix Section 2. As found in other studies, there is a small pre-displacement dip in earnings, which can partly arise because the timing of the firm-level shock and the worker separation may deviate. Hence, workers

in earnings is chiefly driven by a sharp decline and swift but partial recovery in employment. Yet, despite an initial recovery, the reduction in wages is permanent and an important driver of the long-term earnings losses. This result is consistent with findings in Lachowska, Mas and Woodbury (2020) and in Jarosch (2021) for Washington State and Germany, respectively, who show that long-term earnings losses are to an important extent driven by losses in wages.

Given that we treat all unobserved individuals as working zero days and we are not observing self-employed workers, we may somewhat overstate the magnitude of long-term employment losses. In fact, if we only keep workers-year observations where workers are observed at least one day in UI or employment, employment (measured as days worked) converges faster and after ten years the gap shrinks to about ten days work per year (about a three percent drop compared to the control group). Thus, about two thirds of long-term earnings losses are driven by wage losses (See Appendix Figure A-4).

It is important to note that while the figure controls for individual fixed effects, it is also conditional on having found employment, and could understate the wage decline if workers who did not find jobs experienced even larger wage declines.[23] We do not find any effect on the propensity to working part time, our only measure of the number of hours worked (Appendix Figure A-5). Treating the effect of job loss on days worked as an estimate of the reduction in labor supply, we formally decomposed the percent decline in earnings due to job loss among workers with some presence in the labor force after job loss into the share due to the loss in wages, the loss in days worked, and the change in the covariance of wages and days worked (Appendix Section 3). The results confirm that while initially both losses in wages and days worked play a major role, over the long run losses in wages tend to dominate.[24]

Our findings for earnings resemble estimates for the U.S. (e.g, Jacobson, LaLonde and Sullivan, 1993; Couch and Placzek, 2010; von Wachter, Song and Manchester, 2011). Our results for wages are close to those in Lachowska, Mas and Woodbury (2020), who also find little short- or long-term recovery in wages. On the one hand, this may not be surprising, since we deliberately structured our analysis to replicate these studies in the way we defined displacements, our sample, and our estimation approach. The results are also consistent with findings in the empirical literature that the wage structure in the two countries exhibits important similarities, for example in the role of education and experience (e.g., Kane and Harhoff, 1997), the role of job mobility in wage growth

leaving in the year after the firm-level shock may have experienced a decline in earnings on the job. It may also be that there already is a reduction in days or hours worked at the establishment in the year before a separation.

[23]To assess the potential role of sample selection, we compared the difference in median log wages across treatment and control groups and find it to be similar to that shown in Figure 1 (Appendix Figure A-5). In Appendix Figure A-5, we also show that only the bottom 2 deciles of the distribution of days worked experiences a reduction in employment. Hence, the median regression yields an unbiased estimate of the change in median wages upon job loss. Note that women experience larger and longer losses in time worked and the incidence in working part time (see Appendix Section 8).

[24]See Appendix Figure A-6. As explained in Appendix Section 3, the decomposition requires information on both wages and days worked, and hence is based on a set of estimates that impose a minimum amount of presence in the labor market after job loss. Hence, the share of long-term earnings losses explained by employment losses is lower than for our main estimates. The figure also show that the correlation of wages and days worked rises temporarily, offsetting some of the short-term earnings loss. While the correlation pertains to intensive labor supply choices, it is consistent with the notion that there might be some selection in extensive labor supply as well.

(e.g., Giuliano and von Wachter, 2012), and the role of firms in wage setting (e.g., Card, Heining and Kline, 2013). On the other hand, much has been speculated about how the U.S., with more dynamic job creation, higher levels of job mobility, and less generous unemployment insurance may imply a faster recovery rate than a continental European labor market such as Germany. Yet, we find that the effect of job loss on time worked is of a similar order of magnitude in percentage terms as in Lachowska, Mas and Woodbury (2020), who find a reduction in hours worked of 10% five years after job loss.[25] Clearly, the composition of displaced workers and the types of shocks they suffer may be different in the two labor markets, and so the close correspondence should be interpreted with caution. But the congruence we observe in Figure 1 is nevertheless telling about how labor market shocks can have very detrimental and long-lasting effects on workers in different institutional settings.

III. The Effect of Job Loss on Wages and Employment Over the Business Cycle

A. Estimates of the Cyclicality of Earnings Losses

We next document a strong counter-cyclical pattern in earnings, employment, and wage losses at job displacement. Figure 2 Panel (a) shows earnings losses of displaced workers separately by year of displacement obtained by replicating the regression in equation (1) for each displacement year. For presentation purposes, we only show the first two years after job displacement. Vertical bars indicate recession years in Germany (defined as a year of negative GDP growth).

The figure reveals a strong cyclical pattern in the loss of annual earnings from job displacement. While losses were only about 5,000 Euro in the displacement year in 1979-1980, they were more than 10,000 Euro for workers displaced in the 1982 recession. After 1982 losses became smaller until they increased again during the 1993 recession. In the mid 1990s Germany entered a period of prolonged high unemployment rates and sluggish growth (sometimes termed eurosclerosis) and during this time period earnings losses of displaced workers stayed very high, only to come down briefly before the 2003 recession. After the 2003 recession (and the Hartz labor market reforms) earnings losses fell again as the economy and the labor market recovered.

Figure 3 and Table 2 explore the cyclicality of the effects of job loss further. Figure 3 Panel (a) plots the short-term effects of job loss on annual earnings for each displacement year directly against the prevailing change in the national unemployment rate (Appendix Figure A-7 shows the corresponding percent loss). Panel (b) shows the average short- and long-term earnings decline for job losses occurring in expansions and in recessions as measured by the change in the employment rate prevailing in the year of job loss. Workers losing their jobs in recessions experience larger and longer lasting earnings losses.

[25] Schmieder, von Wachter and Bender (2012a) discuss how difficult it is to compare unemployment duration across countries, and present evidence that nonemployment spells for unemployment insurance beneficiaries are similar in the U.S. and Germany. For a decomposition of life-time earnings losses into losses in wages and time worked in Germany see Jarosch (2021).

Going from peak to trough of the business cycle in Germany raises short-term earnings losses from -13% to -25% (Table 2 Panel A), strikingly similar to results in the U.S. (Davis and von Wachter 2011, Figure 5), who report an increase from -18% to -25%.[26] This results from a univariate regression corresponding to the displayed fitted lines in Panels (a) Figure 3, whose results are shown in Table 2 (row 1 and 2 of column 1, respectively). Column 5 of Table 2 Panel A displays the predicted change in the effect of job loss moving from a decreasing to an increasing unemployment rate (the corresponding levels are shown in columns 3 and 4).

B. Decomposition of Cyclical Component of Earnings Losses into Wage and Employment Losses

Turning to employment and wage losses, Figure 2 Panel (b) shows a highly cyclical pattern for number of days worked of displaced workers, with the largest losses for workers who lose their jobs during recessions or in the following year. Panel (d) of Figure 3 shows the average losses in days worked among job losses occurring in expansions and recessions. The panels indicate that an important part of the cyclicality of earnings losses at displacement are driven by employment losses. Figure 2 Panel (c) shows that wage losses are highly cyclical as well, with particularly large losses in the 2003 recession and its aftermath. Panel (f) of Figure 3 shows that the differences between expansions and recessions is particularly large for average wage losses. Understanding the source of the cyclicality of wage losses is thus crucial for understanding the cyclicality of the cost of job loss, something that we turn to in Section V.

To explore how much of the cyclicality of earnings losses is explained by losses in wages and days worked, we show corresponding scatter plots in Panels (c) and (e) in Figure 3 and regression estimates in rows 3 and 4 of Table 2. The results confirm that losses in earnings, in wages, and in time worked are all highly countercyclical. For each point higher change in the unemployment rate, Table 2 shows the earnings loss rises by about 6.3% (row 2), whereas wages are reduced by 3% (row 3). It is clear from Figure 3 and Table 2 that the relationships are precisely estimated. In the Appendix, we again formally decompose the earnings losses among those with some presence in the labor force into losses in wages, losses in days worked, and the correlation between the two. The wage and employment losses explain about half of the short-term effect on average, with employment losses being slightly more important in recessions. Over the long run, wage losses explain an increasingly larger losses of earnings losses as employment recovers.[27]

IV. Loss of Employer Characteristics and Implications for the Cost of Job Loss

In this section, we explore whether displaced workers lose quasi-rents provided by the firm or establishment, and whether this helps to explain the effect of job loss on

[26] To be directly comparable with results in Davis and von Wachter (2011), Panel B of Table 2 uses the level of the unemployment rate as an alternative measure of the state of the labor market.

[27] See Appendix Figure A-8 for the decomposition of the short-term loss for each year in our sample, and Appendix Figure A-9 for the decomposition of the short- and long-term earnings loss in booms and recessions.

wages. We cannot measure such rents directly, but instead use estimates of establishment fixed effects as described in Section I.E and establishment size as measures of rents or "firm quality". Establishment size has long been associated with higher wages and more pleasant work environments. Establishment fixed effects seek to directly estimate systematic wage differences across establishments paid to the same workers. Using these measures, we first analyze whether the incidence of job displacement differs by job type, and whether a job displacement changes the "quality" of a worker's employer. For the latter exercise, we simply estimate the same regression in equation (1) with two measures of establishment characteristics as outcome variables – log employment size of the establishment and the establishment fixed effects. In Section IV.B we then assess directly whether such changes in establishment characteristics can help to explain the effect of job displacement on wages and briefly compare their role with other channels behind the cost of job loss commonly studied in the literature. A more comprehensive assessment of different channels is relegated to Section V.C.

A. *The Effect of Job Loss on Employer Characteristics in the Cross Section*

In this section we show that workers coming from establishments with high fixed effects have much larger short- and long-term losses in both establishment fixed effects and daily wages. This is shown in various panels of Figures 4 and 5.

Figure 4 shows there is clearly a large decline in both employer fixed effect and establishment size relative to non-displaced workers. Establishment effects fall by about 6 points with very little recovery in the 15 years after job loss (Panel (a)). Establishment size drops by a full 100 log points with some recovery over time (Panel (b)).[28]

To better understand what happens to establishment effects, it is helpful to put job losers in the context of the wider population of workers. Figure 4 (c) shows histograms of establishment effects of job losers (before and after displacement) expressed as percentiles in the overall distribution of workers. Job losers come from the top of the overall distribution of establishment fixed effects, perhaps not surprisingly given our sample restrictions. However, despite a substantial shift to the left, job losers have higher establishment fixed effects than the average worker even after job loss.

The reductions in establishment effects suggest a strong pattern of mean reversion occurring at job loss. Figure 4 (d) explores this by showing a binned scatter plot of post- vs. pre-displacement establishment effects (broken into 20 equal sized bins). The regression line has a slope of 0.63, clearly smaller than 1, and falls mostly below the 45-degree line. This points to very substantial (though far from complete) mean reversion with much higher establishment effects losses for worker displaced from high establishment effect employers.[29]

[28]Correspondingly we see some rise in the incidence of job-to-job mobility among job losers compared to the control group (Appendix Figure A-24), but it is concentrated in the first five years after job loss. In addition, the recovery in establishment size may be partly due to job losers moving to small but younger and hence faster growing establishments.

[29]Appendix Figure A-11 (a) shows the change in establishment fixed effects by vintiles of the distribution of pre-displacement establishment effects. The figure shows that the change in establishment fixed effects at job loss is close to linear and negative. However, at -0.35 the regression coefficient is far from one, again implying that the initial advantage in establishment fixed effects is not lost. Appendix Figure A-11 (c) shows (split by quintiles of the full population

B. Employer-Level Determinants of the Average Cost of Job Loss

The findings in Section IV.A suggest that the substantial wage losses at job loss documented in Section II could be partly explained by losses in establishment characteristics. Figure 5 shows that losses in establishment effects indeed play an important role in explaining wage losses for displaced workers in Germany. Panels (a) and (b) show the average loss in wages and establishment effects for different quintiles of the (worker weighted) pre-displacement distribution of establishment effects among the population of displaced workers.

Panel (a) shows a clear ordering in the wage losses by pre-displacement establishment effects. There are no losses for the lowest quintile and more than 10 log point losses at the top. Panel (b) of Figure 5 plots the corresponding establishment effect losses for the same quintiles of the pre-displacement distribution of establishment effects. Comparing the two figures, it is immediately apparent that losses in wages closely correlate with losses in establishment effects. This is further shown in Panel (c) of Figure 5, which displays the binned scatter plot of 3-year losses in wages and 3-year losses in establishment effects. The two losses are proportional with a slope coefficient 0.77, indicating an important role of losses in establishment effects we will examine more closely below.

A comparison of Panels (a) and (b) of Figure 5 also reveals that the long-term loss in wages particularly reflects the loss in establishment effects, while in the short run other factors appear to play an additional role. Job losers at the bottom of the pre-displacement distribution of establishment effects (among job losers) experience neither a loss in establishment effects, nor a permanent reduction in wages. The middle 20% of the pre-displacement distribution of establishment fixed effects experience a short-run loss that is larger than their average loss in establishment effects (Panel (a)), but their long-run wage loss is equal to the loss in establishment effects. Job losers from the top 20% experience a permanent loss in wages without much recovery, and this loss is slightly larger than the loss in establishment fixed effects. Finally, Figure 5 Panel (c) reveals that the few lucky displaced workers that do experience increases in establishment fixed effects also experience increases in wages.

To assess the potential role of other factors and directly quantify the effect of establishment effects, we sequentially added a range of control variables to the event study regression for log of daily wages. When we pool all displacement years as in Section II, losses in establishment effects appear to explain the majority of earnings losses, and seem to be closely correlated with other changes in job characteristics. Consistent with the findings of past studies (e.g., Jacobson, LaLonde and Sullivan 1993; Neal 1995; Schoeni and Dardia 2002 and many others), Panel (d) of Figure 5 shows that, controlling for changes in industry and occupation, changes in establishment size, and average establishment wage also explains a non-negligible fraction of the effect of job loss on daily wages. However, when we include losses in establishment effects alone, they explain a large fraction of the wage loss, substantially more than each of the other changes, and more than all of them combined. When we force the establishment effect coefficient to

establishment effect distribution), that the losses are very persistent at all part of the distribution.

be 1, the losses in establishment effects explain about 70 percent of the wage losses after 1 year and 90 percent after 10 years. Our final specification shows that the remainder of the gap appears to be related to the duration of nonemployment immediately following displacement.[30]

Figure 6 explores the role of nonemployment duration further. Panel (a) shows that wage losses are highly correlated with the duration of the first nonemployment spell (if there is one) after job separation. Individuals who immediately find a job (nonemployment duration = 0), have only about a five log point short-term wage loss that mostly disappears over time. Short nonemployment spells of 1-3 months are associated with a loss of about ten log points in the short-term that decreases to a six log point loss after about five years. Longer nonemployment spells are associated with increasingly larger (and persistent) wage losses. Panel (b) shows comparable results for establishment effects. These mirror the relationship between wage losses and nonemployment durations closely, though the establishment effect losses tend to be somewhat smaller. The evidence suggests that nonemployment duration has a strong negative relationship with wage losses, a large part of which is explained by losses in establishment effects. Figure 6 c) and d) explores this further by showing the 3-year difference in difference estimates of log wage and establishment effect losses for different durations of the initial nonemployment spell. The figure underscores the strong negative association of nonemployment duration and with both losses in wages and establishment effects. Again, even conditional on establishment fixed effects the long-term unemploymed have somewhat lower wages.

These findings are broadly consistent with the assumption underlying the AKM model, according to which changes in the establishment effect explain most wage changes at job loss, an assumption directly validated using flows between firm effect classes in Card, Heining and Kline (2013) and Song et al. (2018). The results also suggest that changes in industry, occupation, establishment size, and nonemployment duration associated with wage losses should be substantially correlated with losses in establishment effects, something we return to below.

Given findings in Goldschmidt and Schmieder (2017) that workers experiencing domestic outsourcing move to establishments with lower fixed effects, a natural question is to what extent outsourcing (i.e. moving to business service firms) can also explain the average cost of job loss. Panels (a) and (b) of Appendix Figure A-17 show that indeed workers moving to food, cleaning, security, logistics, and temp agency (FCSLT) establishments have substantially higher establishment effect and corresponding wage losses than those that do not. However, less than 5% of the displaced workers in our sample move to FCLST establishments (Appendix Figure A-21) and while the trend is

[30]Appendix Table A-10 shows regressions of the short-term earnings loss pooling all years in our sample on a range of potential explanatory factors, with and without controlling for the change in establishment effects. Changes in the point coefficients demonstrate the correlation of establishment effects with several relevant career outcomes, such as nonemployment or a change in industry and occupation affiliation. Adding the change in establishment effects raises the R^2 from 11% to 34% when only tenure, experience, and worker fixed effects are included. Including all other explanatory characteristics raises this to 37% (alone, all other pre- and post-job loss characteristics explain 22% of the variance).

increasing transitions to FCSLT and young firms do not appear particularly cyclical.[31]

C. Comparison to Lachowska, Mas, and Woodbury (2020)

Our findings contrast those in Lachowska, Mas and Woodbury (2020), who find a smaller role for losses in firm effects for explaining the wage losses of job losers in Washington state. To analyze job losers' losses in employer effects in greater depth and to better compare our findings, we followed Lachowska, Mas and Woodbury (2020) and analyzed short-term losses in establishment effects and daily wages for workers experiencing up and downward transitions between employers falling in one of five quintiles of the distribution of establishment effects. The two panels of Table 3 show the incidence and changes in wages and establishment effects between quintiles for recessions (periods when unemployment rates were increasing) and expansions (periods when unemployment rates were decreasing). Here, we focus on general patterns that are similar in recessions and expansions, and return to differences over the cycle in the next section.

Table 3 clearly shows that losses in establishment effects and losses in wages are correlated throughout the matrix of firm transitions, consistent with the restrictions imposed by the AKM model. Moving from lower to higher establishment effect quintiles (above the diagonal) leads almost always to increases in wages and establishment effects; moving from higher to lower quintiles (below the diagonal) always leads to losses. Staying in the same quintile (the diagonal) typically leads to wage losses and reductions in establishment effects, although these are substantially smaller than for cross-quintile transitions. It is also worth noting that the loss (gain) in wages is generally greater (smaller) than the loss (increase) in establishment effects. As we discuss below, there are short-term wage losses occurring for reasons other than losses in establishment effects. However, wage and establishment effect losses are close to identical for many cells, especially for transitions out of the two highest establishment effect classes, and for larger downward transitions.

Comparing our transition matrices to the estimates from Washington state (Lachowska, Mas and Woodbury, 2020, Table 4, replicated in Appendix Table A-9), one can see two reasons for differences in the role of establishment effects in explaining average wage losses: differences in the incidence of up and downward moves after job loss, and differences in the changes of wages and establishment effects within given cells. Compared to our results in Table 3, a higher share of job losers in Lachowska, Mas and Woodbury (2020) is displaced from employers with low establishment effects. Overall, it appears the differences in shares across the 25 cells alone could explain a substantial fraction of the differences in the role of establishment effects. In the Appendix, we re-calculated our

[31] Appendix Figure A-21 also shows that not all FCLST sectors absorb job losers, apparent by the fact that the rate of entry into temp agencies (Panel (b)) is higher than for all FCSLT firms (Panel (a)). Panels (c) and (d) of Appendix Figure A-17 break the losses up by different types of business service firms, with workers moving to establishments food service firms and temp agencies experiencing particularly large losses. The change in establishment effects explains more than two thirds of the effect of entering an FCLST firm, half of the effect of entering a temporary help agency, and the entire effect of entering a new establishment (see columns 8 and 9 of Appendix Table A-10). Appendix Figure A-21 shows that entry into young firms (less than 5 years of age) is about as prevalent as entry into FCSLT firms. However, Appendix Figure A-17 shows there is no difference in losses in establishment effect by establishment age.

wage losses using the cell shares in Table 4 of Lachowska, Mas and Woodbury (2020). This reduces the percent of wage losses explained by losses in firm effects from roughly 75% to about 50%; if we instead reweight their wage and establishment losses with the average shares from our study, the percent wage loss explained rises from approximately 15% to a bit over 30%.[32] Hence, differences in the shares of transitions between different types of establishments can partly reconcile the contrasting findings.

In addition, compared to the results in Table 3, Lachowska, Mas and Woodbury (2020) find that displaced workers that experience increases in establishment effects tend to either still experience wage losses or see much smaller wage increases. Moreover, workers leaving lower paying firms (quintiles one and two) have substantially larger wage losses than losses in establishment effects. Hence, for these workers in the Washington data, the AKM model does not appear to adequately describe wages, whereas in Germany the model's predictions are borne out for job losers from all establishment groups. Together with the higher shares of these workers observed in Lachowska, Mas and Woodbury (2020), these differences can explain differences in our respective findings. It is possible that this may partly be a feature of the unusually deep 2008 recession and the ensuing prolonged slack labor market, an important avenue for future research.

V. Sources of Earnings Losses for Job Losers Over the Business Cycle

A. The Effect of Job Loss on Employer Characteristics Over the Business Cycle

The losses in establishment characteristics at job displacement in Section IV.A are more pronounced during recessions. Figure 7 shows changes in the establishment effects for job losers relative to the change in establishment effects of non-displaced workers over time. Panel (a) of Figure 8 shows that the loss in establishment effects correlates systematically with the change in the unemployment rate. Table 2 and Figure 8 b) show that the differences in average losses in establishment effects between expansions and recessions is substantial and long lasting.[33] Interestingly, the cyclicality in the loss of establishment characteristics is not due to a rise in job displacement from high-wage firms during recessions, as shown in Appendix Table A-4, where we regress pre-displacement establishment effects on the change and level of the unemployment rate and find virtually no cyclicality.[34] Instead, as we show in the next section, the key driver of the cyclicality

[32]These results are based on a transition table that averages recessions and expansions, see Appendix Tables A-8 and A-9. Appendix Figure A-15 displays 3-D figures of the shares of workers among worker and establishment effect bins in our data and in Washington state. There are several potential reasons why displaced workers in Germany come from establishments with higher fixed effects. To be part of our sample of job losers, workers have to come from establishments with at least 50 employees, whereas in Lachowska, Mas and Woodbury (2020) the restriction is imposed on firms. Hence, the Washington sample is likely to contain a larger number of smaller, lower paying establishments that are part of multi-establishment firms (e.g., such as fast food or retail chains). In addition, larger employers in Germany may be more constrained in laying off workers, potentially raising the incidence of mass-layoffs at larger, higher-paying establishments. Bertheau et al. (2022) confirm that in several other European countries displaced workers tend to come from the upper end of the distribution of establishment fixed effects.

[33]Figure 7 b), Appendix Figure A-20 b), and Table 2 also show that the decline in establishment size at job loss is larger in recessions, and correlates systematically with the change in the unemployment rate.

[34]Appendix Figure A-2 shows the fraction of establishments with at least 50 employees that experience a plant closing or mass-layoff in each year, depending on whether they are a high or low establishment fixed effect employer. Figure

of losses in establishment characteristics over the business cycle is a reduction in the quality of the post-displacement employer.

To complement our analysis of changes in firm characteristics in explaining the cost of job loss over the business cycle, we also examined the cyclical behavior of some of the key alternative channels highlighted in the literature. We find that several of the channels are highly pro-cyclical (shown in Appendix Table A-4). For example, not surprisingly, the average duration of nonemployment spells doubles in recessions compared to expansions (from a base of a little over two months). The incidence of changing industry (3 digit) or occupation (2 digit) also increases in recessions, but to a lesser degree (increasing by about a third from a base of 44 and 23 percentage points, respectively). We also examined whether the type of worker displaced changes over the cycle (Appendix Table A-4). We see only a small correlation of worker type with the business cycle, something that will be relevant for our findings in the next section. The lack of cyclicality of worker and displacing firm type may be surprising, but could be due to our focus on a sample of stable workers from mid-size to larger firms.

B. Employer-Level Determinants of the Cost of Job Loss Over the Business Cycle

To analyze determinants of changes in the effect of job losses on wages over the business cycle, we turn to a regression analysis of short-term wage losses. As discussed in Section I.D (2), we matched each displaced worker to a control observation, and regressed the resulting difference-in-difference estimates of the wage loss on a range of pre-displaced characteristics (model 2) and post-displacement characteristics (model 3). Table 4 contains our main results from these regressions for the short-term wage loss (3-year after displacement) in Panel A, and the long-term (10-year after displacement) wage loss in Panel B. The first column confirms that wage losses at job loss have a systematic cyclical component based on our main measure of cyclicality, the year-to-year change in the unemployment rate. To get a sense of the magnitudes, during our sample period the unemployment rate in Germany varied from between 3-4 percent in the early 1980s to over ten percent in the large recession in the mid-2000s. According to the estimate in column (1) of Panel A, in a year when the UR increases by two percentage points (not untypical in a recession), the log wage loss increases by around six points (to 13 percent), relative to an average loss of seven percent.

Part of this variation could in principle be explained by changes in the composition of job losers, something that is easily addressed in our regression framework. The overall finding for both 3-year and 10-year wage losses is that changes in worker composition have very little effect on the cyclicality of job loss. Column (2) confirms that workers coming from high-wage firms experience higher wage losses (see Section V.A). In addition, we find that lower-wage workers (as measured by pre-displacement worker fixed effects) experience larger wage losses as well, something we explore further in Section

A-2 Panel (a) shows that the mass-layoff rate is higher for establishments with fixed effects above the median but shows similar cyclicality. Panel (b) shows that plant closings are more cyclical among high establishment effect firms but the level is quite low.

VI.A.[35] However, controlling for changes in worker and establishment composition (Column 2) barely affects our main coefficient, implying that composition changes are not responsible for explaining the cyclicality of job loss we find.

We next show that a sizable portion of cyclicality of both short- and long-run wage losses is explained by changes in employer characteristics at job loss. Combined with our finding of little role for changes in the composition of displacing firms, this implies that changes in post-displacement employers drive a substantial portion of cyclicality. When we include the change in establishment fixed effects as a control variable in Column (3), our estimates of the cyclicality of job loss decline by around one half relative to the basic result in Column (1).[36] As a sensitivity check, in Column (4) we control for the change in the establishment fixed effect, but force the coefficient on the establishment fixed effect to be equal to 1 (as would be implied by the AKM model), in which case the change in the establishment fixed effect can explain even more of the cyclicality. Including pre-displacement worker and establishment effects does not affect these results (Columns (5) and (6)).

Overall, these results confirm the visual impression from Section IV that losses in establishment wage premiums are a key driver of the variation in the cost of job loss over the business cycle. To further assess the role of changes in employer quality over the business cycle, we also compared the entire transition matrix between quintiles of displacing and hiring firms between recessions and expansions in Panels A and B of Table 3, respectively. The comparison reveals that recessions are associated with increases in the shares of displaced workers moving to employers with lower establishment effects. Most of the declines appear driven by a rise in downward moves of workers displaced in the top and second quintiles of the establishment effect distribution, though we observe a rise in downward moves from the second and third quintiles as well. Consistent with our analysis of composition changes, we see no clear pattern in changes in shares on the side of displacing firms. Losses in establishment effects and wages remain highly correlated, and wage losses are somewhat larger in recessions than in expansions even within cells. This is likely due to factors other than establishment effects, which we turn to in the next section.

C. Other Explanations for the Cost of Job Loss Over the Business Cycle

In this section, we examine a range of other potential mechanisms behind the cyclical variation in the cost of job loss, as well as their correlation with losses in establishment fixed effects. From an empirical point of view, analyses of variation in the costs of job displacement from the prior literature can be grouped into roughly four categories: a) variation in cost of job loss by demographic characteristics, such as gender, education, or labor market experience; b) variation due to employer characteristics (e.g., establishment , establishment fixed effect); c) variation due to pre-displacement career outcomes, such

[35] We obtain the same result if we include completed years of education, education dummies, or even lagged wages as a measure of worker type (Appendix Table A-18).

[36] The change in the establishment effect represents the change in the adjusted average log wage difference between the pre and post job loss employer.

as job tenure, occupation tenure, industry tenure, prior occupation or prior industry; d) variation due to post-displacement career outcomes, such as nonemployment duration, switching primary industry or occupation, or recurring job loss.

We have explored including a range of variables from each of these categories in our regression model outlined in Section I.D (2). While we found several of these characteristics to matter in expected ways for explaining the cost of job loss, most do not help to further explain its cyclicality. For example, we find that job losers with longer labor market experience, higher job tenure, or lower education tend to experience larger earnings losses. However, we find that demographic factors and pre-displacement career background do not help to explain variation in the cost of job loss over the business cycle. Hence, we focused our main analysis on the role of post-displacement career outcomes.

The results from this exercise are shown in Table 5.[37] Column (1) replicates the baseline estimate of the cyclicality of the wage loss at job loss from Table 4. The table makes several points. Clearly, the effect of the duration of nonemployment after job loss plays an important role in explaining both the average cost of job loss and its cyclicality (Column (3)). This is consistent with findings in Schmieder, von Wachter and Bender (2016) that show that nonemployment duration has a negative causal effect on reemployment wages. Controlling for pre-displacement establishment effects does not affect this coefficient, implying that workers coming from high-wage firms do not search for jobs longer. In contrast, including the change in establishment effects in Column (4) explains about half of the effect of nonemployment duration (two thirds if we set the coefficient to one to address the concern of measurement error). This suggests that an important part of the loss in wages due to nonemployment duration found in Schmieder, von Wachter and Bender (2016) may be due to entry into lower paying firms.

As found in the prior literature, changes in industry and occupation (either measured as a change in job or tenure) increase the cost of job loss (Columns (5) and (6)). However, conditional on change in establishment effect, they have little effect on the degree of cyclicality. This suggests that changes in industry and occupation lead to large losses because they lead to reemployment at lower-wage firms. This is also consistent with the AKM model that suggest that conditional on worker and establishment effects, factors such as changes in industry or occupation, explain a moderate amount of the variation of wages and should not be correlated with the error term. Nevertheless, as discussed in Section I.D (2), a caveat throughout the analysis is that the associations in the depicted regressions do not necessarily imply causal relationships if workers with larger or smaller wage losses tend to select different employers after job loss.

A related question is whether outsourcing plays a greater role in explaining the cost of job loss in recessions than in expansions. While estimates displayed in Table 2 show that the incidence of moving into different types of business service firms (BSF) increases somewhat in recessions, the relationship is not estimated very precisely. As highlighted by Woodcock (2022), there is a sharp increase in movements to temporary help agencies after the Hartz reforms in 2004, but these are not very cyclical (Appendix Figure A-21). Table 5 Column (8) shows that moving to the classic low wage business service firms

[37]Corresponding findings for 10-year wage losses are shown in Appendix Table A-14.

(food, cleaning, security or logistics BSF or temp. agencies) is associated with larger wage losses. This is mostly explained by changes in establishment effects (Column 9), though a smaller negative effect remains. However, very little of the cyclicality of the cost of job loss can be explained by controlling for moves to business service firms, suggesting that outsourcing is not a key driver of the cyclicality of the cost of job loss, something we return to in Section VI.A.

D. Sensitivity Analysis

Our main findings are very robust to variation in samples, refinements of methodology, or in the way we measure the business cycle. Here we discuss a number of important robustness checks to our main analysis, a selection of which is shown in Table 6. In this table we focus on the main specifications of the cyclicality results that correspond to Table 4 columns (1), (2), and (5). Panel A of Table 6 shows the relationship of the change in the unemployment and the log wage loss; Panel B adds the pre-displacement establishment effect, worker effects, as well as experience and tenure to control for composition changes; Panel C adds the change in the establishment effect. Column (1) of Table 6 shows the baseline results from Table 4 for reference.

Robustness to Limited Mobility Bias As discussed in Section I, in order to maximize the precision of the estimated establishment fixed effects and to reduce the problem of limited mobility bias for our main analysis we estimate the AKM model pooling all years from 1979 to 2009. A concern with this approach is that it may be implausible that establishment effects are constant over such a long-time horizon. Column (2) of Table 6 therefore shows the results based on a rolling AKM model, where the AKM model is estimated using only observations within 6 years *prior* to the displacement event. In this model, adding the change in establishment FE appears to explain slightly less in the cyclicality (the coefficient on the Change in the UR is now -0.016 as opposed to -0.014 in Column (1)). Furthermore, the coefficient on the change in the establishment effect is slightly smaller (0.70 as opposed to 0.74). Note that we lose almost 12,000 observations in the rolling AKM model since we lose displaced workers who move to employers that did not exist prior to the baseline year (and thus are not in the AKM model). Furthermore, in this shorter panel we expect limited mobility bias to be significantly more problematic. The resulting measurement error is likely to bias the coefficient on the change in the establishment effect towards zero.[38]

Hence, our second strategy to address limited mobility bias is to correct for the resulting measurement error using a split sample IV.[39] We randomly split the data that is used in the AKM estimation into two equal sized samples (keeping person histories together) and estimate the AKM model separately in both subsamples. We then estimate the regression models (2) and (3) by using the estimated establishment effect from the first

[38]In a similar spirit to the rolling AKM model, column (6) of Table 6 replicates our main results with time-varying establishment effects, where the AKM model is estimated using fully interacted establishment by year effects as in Lachowska et al. (2022). The results are very similar to column (2).

[39]This was first proposed by Goldschmidt and Schmieder (2017).

sample instrumented with the estimated establishment effect from the second sample, which corrects for the bias from measurement error.[40] We instrument separately for the level and change in the establishment effect in these specifications. The result is shown in column (3) of Table 6 for the long AKM model. The number of observations drops slightly since using a smaller sample reduces the size of the largest connected group and we lose some small establishments where we do not get estimates of the establishment effect in both samples. The results of the split sample IV are in line with our expectations: the change in the establishment effect coefficient increases to 0.78 (consistent with measurement error bias) and it now explains slightly more of the cyclicality.

We also use the same split sample IV strategy for the rolling AKM model, where we now split the AKM data for each 6 year window. The results, shown in column (4), suggest that for the shorter AKM window the split sample IV matters more, consistent with the hypothesis that limited mobility bias should be worse in the smaller AKM window. In particular the coefficient on the change in the establishment effect increases from 0.70 (in column 2) to 0.90 in the split sample IV. The cyclicality that is explained in this model is essentially identical as in the long AKM window without split sample IV. In the appendix, we show that these different approaches also visually produce very similar degrees of cyclicality in estimated establishment fixed effects (Appendix Figure A-26).

As our third strategy of dealing with limited mobility bias, we use an idea inspired by Bonhomme, Lamadon and Manresa (2019) to partition establishments with less than 50 employees into 20 distinct clusters of establishments with a similar wage structure using the Kmeans clustering algorithm.[41] We then estimate the AKM model on the rolling window (6 years prior to job loss) by essentially treating each of the 20 clusters of small establishments as a single establishment. This is useful since limited mobility bias is much more problematic for small establishments that may have only few movers especially in a short panel. As one would expect from limited mobility bias, the resulting AKM decomposition of the variance of log wages (Appendix Table A-5 shows that the Kmeans model does attribute a smaller share to the variance of establishment effects (in 2009 about 15.5% as opposed to 27.6% in the long AKM model) but a larger share to the covariance term.[42] Yet, it appears substantial heterogeneity in establishment/cluster effects remains. Indeed, column (5) of Table 6 shows that using this approach establishment effects still explain a very similar share of the cyclicality, with a coefficient on the change in the unemployment rate of -0.016 Panel C. It is also reassuring that the estimated coefficient on the change in the establishment / cluster effect is substantially larger than in columns (1) and (2), as one would expect if the Kmeans approach reduces

[40] This produces unbiased estimates as long as the errors in the instrument are uncorrelated with the error in the diff-in-diff estimate of the wage loss $\Delta_{dd} w_{ic}$, which is for example the case if the error term in the AKM model is only serially correlated within individuals. We thank an anonymous referee for pointing this out to us.

[41] We use the Kmeans clustering algorithm to partition these smaller establishments into 20 distinct clusters based on the similarity of the discretized empirical CDF of their log wages. This approach dramatically reduces the number of estimated establishment (or rather "cluster") fixed effects. For example in 2009 the long AKM model estimates 1.02 million establishment effects, while the Hybrid Kmeans clustering AKM model only estimated 47,699 establishment effects plus 20 clusters effects (for the small firms).

[42] Interestingly, the result from Card, Heining and Kline (2013) that a large part of the increase in the variance of log wages in Germany can be attributed to the rise in the variance of establishment effects and the covariance term still appears to hold in the Kmeans model in Table A-4, though the weight is shifted a bit more towards the covariance term.

limited mobility bias.[43]

Other Robustness Checks We also conducted a number of other robustness checks to see whether our results are driven by specific methodological choices or our sample.

In Table 6, column (6) we replicate the standard regression approach, where each worker is simply assigned a random control observation (without matching), who satisfies the same baseline restrictions (tenure, establishment size, etc.). The results are very similar to our matching estimator in column (1).[44]

Column (8) shows the results for women. Women's wage losses actually exhibit stronger cyclicality (-0.039 in the raw correlation) but again controlling for composition and the change in the establishment effect reduces this coefficient by a similar amount. Appendix Section 8 replicates all main results for the paper for women, and finds them very robust. Finally, column (9) shows that the results are virtually identical if we pool displaced workers from East and West Germany. Appendix Table A-13 shows a number of further robustness checks, where we control for a wide range of additional pre-displacement characteristics (such as detailed occupation and industry codes, or occupation and industry tenure), displacement characteristics (such as whether the job loss occurred during a plant closing or mass-layoff), and post-displacement characteristics, such as changes in establishment size, part-time status, or the establishment turnover rate. Conditional on the establishment effect variables, none of these additional controls have a significant impact on the cyclicality of wage losses.[45]

VI. Additional Findings on Worker Heterogeneity and Unemployment Insurance

A. *Heterogeneity in the Role of Establishment Effects by Worker Type*

The work on outsourcing and the positive covariance in worker and establishment effects suggests that the role of establishment effects may differs by job losers' average wage level. To learn more, we examined how establishment fixed effects on wage losses differed between job losers with high and low worker fixed effects in Figure 9 (a) and Table 7.[46] Low-wage workers experience larger losses in establishment effects on average, and the highest-wage job losers experience hardly any loss in establishment wage

[43]To address the concern that the use of the estimated worker effect as control variable introduced bias, Appendix Table A-18 shows our results are robust if instead we control for measures of completed education. As an alternative, Appendix Table A-7 shows the results from a Monte Carlo analysis that explores the potential bias from using the estimated worker and establishment effects as regressors in the analysis (see also Appendix Section 4.3). The results point to very small if any bias arising from estimation of the worker fixed effects under reasonable assumptions.

[44]This is further explored in Appendix Table A-17. Our findings also hold if we dispense with the control group altogether (Appendix Table A-19). The cyclicality of wage changes increases, demonstrating the need for including a control group to reflect a counterfactual amount of cyclicality in wage changes.

[45]For example, switching to firm with higher turnover increases the cost of job loss, partly because high turnover firms have lower establishment effects, but this has no explanatory power for the cyclical nature of wage losses. Results shown in the appendix provide additional sensitivity by showing variation in our main specification by changing in the way we control for year effects (Appendix Table A-11, and by using the level instead of the change in the unemployment rate (Appendix Tables A-12).

[46]To make sure that the job loss event itself does not affect our measure of permanent skill, we use worker fixed effects that were estimated based on wage data *prior* to the displacement event.

effects at all. Figure 9 (b) shows that this is partly because low fixed effect workers have higher losses in establishment wage effects even coming from the same establishment, consistent with the analysis of outsourcing in Goldschmidt and Schmieder (2017). As a result, the correlation between worker and establishment fixed effects goes up in our sample of job losers. However, we did not find evidence that job losses reduce mismatch and improve sorting in the labor market.[47]

Turning to worker differences over the business cycle (Table 7), we find that as for the full sample for both high- and low-wage workers about half of the cyclicality in wage losses is explained by loss in establishment effects. Wage losses of low-wage workers are more cyclical than for high-wage workers, and losses in establishment effects explain a higher share of the variance of wage losses. Figures 9 (c) and (d) again compares the experience of high- and low-wage workers coming from the same type of firm. There appears to be no increase in the effect of outsourcing on wage losses during recessions, consistent with our analysis of the role of business service firms in Section V.C.[48]

B. The Role of UI in Buffering Job Losses Over Business Cycle

We also explored to what extent the relatively generous German UI system is able to dampen the effect of job loss and in particular its cyclicality. Perhaps not surprisingly, job loss leads to a spike in UI receipt in the year of job loss and its aftermath (Appendix Figure A-22). Yet, UI only replaces about 25 percent of the earnings losses in the first year, reflecting both partial take-up and the fact that part of the cost of job loss arises from reductions in hourly wages among those reemployed. The role of UI then declines quickly and the difference with respect to the control group effectively disappears after around 5 years, such that UI benefits do little to compensate long-term earnings losses for displaced workers.[49]

Given the important role of employment losses over the business cycle, we would expect that UI benefits may have some impact on the cyclicality of total income. Indeed, Table 2, row 7, panels A and B, show that UI benefits received in the first years after job loss rises significantly in recessions - for example from 550 Euro when the unemployment rate is falling to about 1100 Euro when the unemployment rate is increasing by 1 percentage point. In row 6 in Table 2, we directly analyze the cyclicality loss in total income at job displacement (defined as total annual earnings plus receipt of UI). Despite the large swings in benefit receipt, the cyclicality of the losses in annual earnings (row 2, column 1) change little once UI income is added (row 7, column 1). This partly re-

[47]It is a natural question whether job losses are concentrated among workers that were paid higher than their skill level would suggest, and hence whether job losses help return the labor market revert to a particular level of sorting. However, we do not find that 'over-placed' low-wage workers experienced the largest losses. We defined 'over-placed' workers as those whose wage effect at the displacing firm was above average than the mean establishment wage effect for workers with similar worker fixed effects. Yet, this measure neither predicted a higher likelihood of job loss nor a larger loss in establishment wage effects upon displacement.

[48]We also checked the differential effects of other potential channels by worker type over the business cycle and found they explain little of the cyclicality of the cost job loss, shown in the Appendix (Table A-16 and A-15).

[49]Contrasting Figure 1 (b) with Appendix Figure A-22 (f), it is apparent that the effect of UI on income declines rapidly and is effectively gone by 5 years after job loss. Schmieder, von Wachter and Bender (2012b) analyses the role of repeated job loss in explaining the persistent effect on nonemployment duration after job loss.

flects the fact that in Germany, in contrast to the U.S., neither the duration nor the level of UI benefits is extended in recession. A comparison of the total predicted increase in earnings losses in a recession compared to an expansion with and without UI – shown in rows 1 and 6 of column 5 – imply that the total cyclicality of earnings loss is only reduced by about 15% due to the presence of UI.

VII. Conclusion

In this paper we have used administrative data from Germany covering over three decades to analyze the sources behind large, persistent, and cyclical costs of job loss. This data allows us to make several important contributions to the existing literature. First, our data allow us to distinguish between losses in employment and losses in wages over the business cycle. Second, we can assess the contribution of employer characteristics in determining the cyclical wage losses we find. Third, we relate the role of changes in employer characteristics to other sources of the cost of job loss. Finally, we can establish the role of unemployment insurance in buffering the large and cyclical losses we find.

We obtain three main findings. First, earnings losses at job loss in Germany are large, persistent, and strongly countercyclical. The magnitude and cyclicality of the losses we find are surprisingly similar to comparable estimates from the U.S. While losses in employment play a large role, reduced wages play a larger role in long-term earnings losses and their cyclicality. Second, we find that a large part of wage losses and a substantial degree of their cyclicality can be explained by the reduction of establishment fixed effects of new employers. In contrast, characteristics of the pre-displacement employer explain little of the average cost of job loss or its cyclicality. Finally, while several factors may explain average wage losses at job loss in addition to changes in establishment effects, only nonemployment duration contributes to understanding the cyclicality of job loss. We show that losses establishment effects statistically explain a substantial share of the effect of nonemployment duration on wages.

These findings are consistent with an increasing literature documenting the existence of firm-specific wage components, their role in explaining the average cost of job loss, and their importance for explaining wage reductions at domestic outsourcing. Our results also highlight the importance in the variation of job characteristics over the business cycle for the career outcomes of job losers, consistent with their role in explaining career trajectories of young workers. Together with the important role of nonemployment durations over the business cycle that we document, these results indicate that variation of the state of the labor market driven by demand conditions play a key role in determining the outcomes of job losers. Bad luck in the timing of job loss of otherwise similar job losers creates persistent differences in wages and earnings levels lasting decades, explained to a large degree by losses in employer quality.

An important direction for future research would be to further understand the role of differences in worker skill that we have begun to document. Similarly, additional analysis of differences in the determinants of job loss by workers coming from different parts of the distribution of employer effects would be helpful, particularly in the U.S. Furthermore, it would be helpful to complement this analysis with evidence on the sources of cyclicality of the cost of job loss from other countries. Finally, more work is needed to integrate our findings in models that can explain variation in the incidence and cost of job loss over the business cycle.

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TABLE 1—WORKER AND EMPLOYER CHARACTERISTICS OF DISPLACED AND NON-DISPLACED WORKERS ONE YEAR PRIOR TO JOB DISPLACEMENT

	(1) Displaced workers	(2) Non-displaced workers matched	(3) Non-displaced workers random sample
Panel A: Average Individual Characteristics			
Fraction Non-German	0.14	0.10	0.10
	[0.35]	[0.30]	[0.30]
Years of education	10.9	11.0	11.0
	[2.2]	[2.3]	[2.4]
Potential experience	21.3	21.2	21.2
	[7.4]	[7.4]	[7.4]
Tenure with current employer	9.51	9.60	10.32
	[5.20]	[5.11]	[5.43]
Actual experience, but censored 1975	13.1	13.1	13.4
	[6.1]	[6.0]	[6.2]
Total annual earnings	32,354.8	33,824.4	36,017.2
	[10,033.9]	[9,573.6]	[9,433.1]
Days per year working full time	351.7	362.9	363.4
	[34.1]	[13.6]	[11.8]
Daily Wage on June 30th of year	91.6	93.0	98.9
	[26.0]	[25.9]	[25.6]
Log of wage on June 30th	4.48	4.50	4.56
	[0.28]	[0.28]	[0.26]
Panel B: Average Establishment Characteristics			
Number of employees	443.3	435.3	3,233.0
	[779.2]	[760.7]	[8,224.7]
Avg. years of education in estab.	10.8	10.8	10.9
	[1.0]	[1.0]	[1.1]
Establishment Fixed Effect	2.23	2.20	2.23
	[0.11]	[0.11]	[0.13]
Business Service Firm (FCSL)	0.026	0.023	0.021
	[0.159]	[0.151]	[0.144]
Temp. Agency	0.0046	0.0038	0.0017
	[0.0676]	[0.0612]	[0.0411]
Business Service Firm (non-FCSLT)	0.051	0.047	0.044
	[0.221]	[0.212]	[0.204]
New Establishment ($\leq$ 5 Years old)	0.057	0.040	0.029
	[0.233]	[0.197]	[0.169]
Number of Observations	95,492	95,492	102,468

Note: The table displays characteristics of male displaced and non-displaced workers in year prior to job loss who were displaced in West Germany between 1980 and 2005. Workers satisfy the following restrictions: age 24 to 50; in pre-displacement year workers have at least 3 years of job tenure and are employed full time at an establishment with at least 50 employees. See text for a definition of a displacement event. Non-displaced sample of workers in Column (1) are matched to displaced workers using propensity score matching within year and industry cells. Non-displaced sample of workers in Column (5) is a random sample of non-displaced workers (one observation per displaced worker, including workers for whom no match could be found in Column 1) that satisfy the same baseline restrictions. FCSL is an indicator variable for working at a Food, Cleaning, Security or Logistics firm. Temp. Agency is an indicator for working at a temporary employment agency. Business Service Firm (Non-FCSLT) is an indicator for working in the business service sector except for FCSL or a temp. agency. New Establishments are establishments less than 5 years old. Earnings are shown in Euros at 2000 prices.

TABLE 2—EFFECT OF UNEMPLOYMENT RATE ON DIFFERENT OUTCOMES FOR JOB LOSERS OVER 3 YEARS AFTER JOB DISPLACEMENT

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Regression of Effect of Job Loss on Year over Year Change in Unemployment Rate (UR)						
	Estimated Effect of Change in UR		Predicted Outcome for Change in UR		Difference going from -1% to +1% Change UR	Mean of dependent variable
Outcome:	Coefficient	Std. Err.	$\Delta UR = -1\%$	$\Delta UR = +1\%$		
Annual Earnings (in Euro)	-1897.4	[422.1]	-4254.1	-8048.9	-3794.8	-6625.8
Annual Earnings (Log points)	-0.063	[0.0099]	-0.13	-0.25	-0.12	-0.21
Log Wage Change	-0.030	[0.0073]	-0.036	-0.096	-0.060	-0.074
Annual Days Worked	-17.2	[2.91]	-37.6	-72	-34.4	-59.1
Establishment Fixed Effects	-0.020	[0.0044]	-0.031	-0.071	-0.040	-0.056
Annual Income (in Euro)	-1628.4	[392.5]	-3704.6	-6961.4	-3256.8	-5740.1
Annual UI Receipt (in Euro)	269.0	[43.0]	549.5	1087.5	538	885.7
Log Establishment Size	-0.15	[0.040]	-0.56	-0.86	-0.30	-0.75
Business Service Firm (FCSL)	0.0028	[0.0033]	0.016	0.022	0.0060	0.019
Temp. Agency	0.0041	[0.0039]	0.0079	0.016	0.0081	0.013
Business Service Firm (non-FCSLT)	0.0097	[0.0034]	0.023	0.043	0.020	0.035
New Establishment ($\leq$ 5 Years old)	-0.031	[0.023]	0.31	0.25	-0.060	0.27
Panel B: Regression of Effect of Job Loss on Unemployment Rate (UR)						
	Estimated Effect of Unemployment Rate		Predicted Outcome for Unemployment Rate		Difference going from 4% to 9% UR	Mean of dependent variable
Outcome:	Coefficient	Std. Err.	UR=4%	UR=9%		
Annual Earnings (in Euro)	-624.2	[157.2]	-4541.8	-7662.8	-3121	-6625.8
Annual Earnings (Log points)	-0.013	[0.0050]	-0.16	-0.23	-0.070	-0.21
Log Wage Change	-0.011	[0.0025]	-0.037	-0.092	-0.055	-0.074
Annual Days Worked	-3.59	[1.41]	-47.1	-65.0	-17.9	-59.1
Establishment Fixed Effects	-0.0056	[0.0018]	-0.036	-0.064	-0.028	-0.056
Annual Income (in Euro)	-583.8	[137.8]	-3790.4	-6709.4	-2919	-5740.1
Annual UI Receipt (in Euro)	40.3	[22.8]	751	952.5	201.5	885.7
Log Establishment Size	-0.017	[0.017]	-0.69	-0.77	-0.080	-0.75
Business Service Firm (FCSL)	-0.0024	[0.0011]	0.027	0.015	-0.012	0.019
Temp. Agency	0.0053	[0.00097]	-0.0038	0.023	0.027	0.013
Business Service Firm (non-FCSLT)	0.0045	[0.0011]	0.020	0.043	0.023	0.035
New Establishment ($\leq$ 5 Years old)	0.030	[0.0059]	0.16	0.31	0.15	0.27

Note: The sample is men with at least 3 years of tenure employed at an establishment of size greater or equal 50 displaced between 1980 and 2005. See Table 1 for additional detail and text for a definition of displacement event. Each row represents a separate worker-level regression of the average losses in the outcome variable over a 3 year period after job loss on the annual change in the unemployment rate (Panel A) and the unemployment rate (Panel B) are based on the national unemployment rate for West Germany and measured in percentage points.

TABLE 3—CHANGES IN DAILY WAGES AND IN ESTABLISHMENT FIXED EFFECTS UP TO 3 YEARS AFTER JOB LOSS FOR DISPLACED WORKERS MOVING BETWEEN QUINTILES OF ESTABLISHMENT FIXED EFFECTS

Fixed Effects Quintile of Origin Employer		Fixed Effects Quintile of Destination Employer 1	2	3	4	5	Row Totals*
Panel A : Recession - Change in Unemployment Rate ≥ 0							
1	Percent of Disp. Workers in Cell	**0.69**	0.50	0.26	0.14	0.075	**1.65**
	Mean Δ Wage	**-6.72**	1.64	5.85	16.9	26.9	**1.23**
	Mean Δ Estab. FE	**-8.69**	5.93	17.3	28.5	38.3	**4.96**
2	Percent of Disp. Workers in Cell	1.09	**2.09**	1.67	0.86	0.39	**6.10**
	Mean Δ Wage	-19.8	**-4.81**	-0.19	5.00	9.55	**-3.93**
	Mean Δ Estab. FE	-21.8	**-3.17**	5.30	14.1	25.6	**0.081**
3	Percent of Disp. Workers in Cell	1.46	3.36	**5.39**	4.24	1.90	**16.4**
	Mean Δ Wage	-31.4	-12.3	**-4.96**	1.98	7.83	**-5.54**
	Mean Δ Estab. FE	-32.9	-12.2	**-2.34**	5.97	17.0	**-2.70**
4	Percent of Disp. Workers in Cell	2.29	4.73	8.80	**13.0**	8.55	**37.4**
	Mean Δ Wage	-39.5	-19.9	-10.4	**-3.41**	2.51	**-8.01**
	Mean Δ Estab. FE	-43.3	-20.8	-10.2	**-1.28**	9.60	**-5.93**
5	Percent of Disp. Workers in Cell	2.17	3.59	6.47	10.9	**15.4**	**38.5**
	Mean Δ Wage	-50.5	-30.0	-19.9	-11.1	**-2.05**	**-12.9**
	Mean Δ Estab. FE	-52.5	-30.9	-19.6	-10.1	**1.66**	**-11.3**
Column Totals*	Percent of Disp. Workers	**1.78**	**3.59**	**6.69**	**10.5**	**11.9**	**100**
	Mean Δ Wage	**-35.4**	**-17.7**	**-10.9**	**-5.14**	**0.40**	**-9.09**
	Mean Δ Estab. FE	**-37.8**	**-17.8**	**-9.58**	**-2.93**	**5.81**	**-6.94**
Panel B : Boom - Change in Unemployment Rate < 0							
1	Percent of Disp. Workers in Cell	**0.78**	0.71	0.29	0.20	0.068	**2.05**
	Mean Δ Wage	**-4.60**	2.86	10.3	18.2	19.9	**3.17**
	Mean Δ Estab. FE	**-4.58**	10.2	16.8	27.8	37.1	**8.12**
2	Percent of Disp. Workers in Cell	0.83	**1.88**	1.86	0.96	0.45	**5.97**
	Mean Δ Wage	-12.2	**-2.86**	1.91	5.75	18.8	**0.33**
	Mean Δ Estab. FE	-18.6	**-3.22**	5.88	15.0	27.5	**2.71**
3	Percent of Disp. Workers in Cell	0.89	2.94	**4.97**	4.42	1.94	**15.2**
	Mean Δ Wage	-25.5	-9.19	**-1.87**	3.04	8.11	**-1.97**
	Mean Δ Estab. FE	-30.2	-11.1	**-1.81**	6.37	17.2	**-0.48**
4	Percent of Disp. Workers in Cell	1.53	3.78	7.97	**15.4**	7.84	**36.5**
	Mean Δ Wage	-35.8	-14.9	-8.12	**-2.19**	4.54	**-4.76**
	Mean Δ Estab. FE	-42.8	-19.9	-9.64	**-0.93**	10.2	**-4.16**
5	Percent of Disp. Workers in Cell	1.23	2.48	5.11	10.4	**21.1**	**40.3**
	Mean Δ Wage	-44.5	-26.4	-17.9	-9.34	**-0.56**	**-7.96**
	Mean Δ Estab. FE	-51.0	-30.3	-19.7	-9.71	**3.57**	**-6.57**
Column Totals*	Percent of Disp. Workers	**1.13**	**2.81**	**5.84**	**11.7**	**16.2**	**100**
	Mean Δ Wage	**-27.7**	**-12.9**	**-7.87**	**-3.46**	**1.57**	**-5.16**
	Mean Δ Estab. FE	**-33.1**	**-15.4**	**-8.45**	**-2.15**	**6.47**	**-3.91**

Note: The sample is men with at least 3 years of tenure employed at an establishment of size greater or equal 50 displaced between 1980 and 2005. See Table 1 for additional detail and text for a definition of displacement event. The table shows changes in daily wage and in establishment fixed effects (times 100) averaged over years 0 to 3 after job loss for displaced workers transitioning between quintiles of the overall establishment fixed effect distribution. The annual change in the unemployment rate is based on the national unemployment rate for West Germany and measured in percentage points.
* Row and column totals are the sum of the percentages of the respective row / column, and the weighted averages of the changes in wages and establishment fixed effects.

TABLE 4—THE CYCLICALITY OF LOG WAGE LOSSES WITH AND WITHOUT CONTROLLING FOR CHANGES IN ESTABLISHMENT EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Medium-run wage losses (Averaged Over 0-3 Years Post Displacement)						
Change in UR t-1 to t	**-0.030**	**-0.028**	**-0.015**	**-0.0095**	**-0.014**	**-0.0085**
	(0.0030)**	(0.0036)**	(0.0022)**	(0.0022)**	(0.0022)**	(0.0023)**
Establishment FE		-0.30			-0.020	0.079
		(0.030)**			(0.014)	(0.016)**
Worker FE		0.16			0.071	0.038
		(0.011)**			(0.010)**	(0.012)**
Change in Estab. FE			0.77	1	0.74	1
			(0.017)**		(0.019)**	
Coeff. on Chg. in Est. FE = 1				1		1
Mean of Dependent Variable	-0.077	-0.077	-.077	-0.077	-0.077	-0.077
R^2	0.016	0.10	0.33	0.034	0.34	0.039
Number of Observations	80917	80917	80917	80917	80917	80917
Panel B: Long-run wage losses (Averaged Over 0-10 Years Post Displacement)						
Change in UR t-1 to t	**-0.023**	**-0.019**	**-0.012**	**-0.0093**	**-0.0100**	**-0.0079**
	(0.0025)**	(0.0036)**	(0.0027)**	(0.0026)**	(0.0028)**	(0.0027)**
Establishment FE		-0.39			0.020	0.11
		(0.027)**			(0.017)	(0.023)**
Worker FE		0.18			0.088	0.069
		(0.012)**			(0.012)**	(0.013)**
Change in Estab. FE			0.84	1	0.82	1
			(0.021)**		(0.021)**	
Coeff. on Chg. in Est. FE = 1				1		1
Mean of Dependent Variable	-0.066	-0.066	-.066	-0.066	-0.066	-0.066
R^2	0.011	0.12	0.36	0.055	0.37	0.064
Number of Observations	61227	61227	61227	61227	61227	61227

Note: The sample is men with at least 3 years of tenure employed at an establishment of size greater or equal 50 displaced between 1980 and 2005. See Table 1 for additional detail and text for a definition of displacement event. The dependent variables is the individual difference-in-difference wage loss, i.e., the average change in log of daily wages compared to a matched control observation in the medium run (0-3 years) and long run (0-10 years), respectively. See Section I.D in the text for further explanation. The annual change in the unemployment rate (UR) is based on the national unemployment rate for West Germany and measured in percentage points. All regressions control for a quadratic in the calendar year; regressions in columns (2) to (6) also control for a quadratic in years of job tenure at displacement and a quadratic in years of potential labor market experience. Column (2) adds estimated fixed effect for the displaced worker and the pre-displacement establishment as controls. Column (3) adds the change in the estimated establishment fixed effect from the pre-displacement employer to the post-displacement employer, relative to the change in establishment fixed effects of the control group (constructed using the same approach as for the wage loss). Columns (4) and (6) force the coefficient on the change in the establishment fixed effect to be equal to 1. Statistical significance: * indicates $p \leq 0.05$ and ** indicates $p \leq 0.01$, SE are clustered on year level.

TABLE 5—THE CYCLICALITY OF LOG WAGE LOSSES UP TO 3 YEARS AFTER JOB LOSS CONTROLLING FOR NONEMPLOYMENT DURATION AND CHANGES IN JOB CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in UR t-1 to t	**-0.028**	**-0.014**	**-0.022**	**-0.010**	**-0.013**	**-0.011**	**-0.014**	**-0.019**	**-0.0098**
	(0.0036)**	(0.0022)**	(0.0032)**	(0.0021)**	(0.0022)**	(0.0021)**	(0.0022)**	(0.0030)**	(0.0021)**
Establishment FE	-0.30	-0.020	-0.30	-0.029	-0.030	-0.036	-0.026	-0.26	-0.047
	(0.030)**	(0.014)	(0.028)**	(0.013)*	(0.016)	(0.014)*	(0.013)	(0.033)**	(0.016)**
Change in Estab. FE		0.74		0.71	0.70	0.68	0.73		0.64
		(0.019)**		(0.018)**	(0.016)**	(0.017)**	(0.018)**		(0.013)**
Worker FE	0.16	0.071	0.13	0.057	0.058	0.060	0.070	0.10	0.047
	(0.011)**	(0.010)**	(0.0093)**	(0.0099)**	(0.011)**	(0.0097)**	(0.010)**	(0.0083)**	(0.010)**
Nonemp. Duration (post Disp)			-0.092	-0.052				-0.052	-0.032
			(0.0078)**	(0.0041)**				(0.0048)**	(0.0034)**
Occ. change					-0.023			-0.0088	-0.000032
					(0.0033)**			(0.0034)*	(0.0030)
Ind. change					-0.029			-0.015	-0.011
					(0.0025)**			(0.0037)**	(0.0023)**
Change in Industry Tenure						0.0042		0.0055	0.0031
						(0.00021)**		(0.00032)**	(0.00023)**
Change in Occupation Tenure						0.0029		0.0029	0.0021
						(0.00023)**		(0.00029)**	(0.00017)**
Part Time							-0.36	-0.36	-0.34
							(0.039)**	(0.048)**	(0.042)**
Business Service Firm (FCSL)								-0.074	-0.022
								(0.0085)**	(0.0061)**
Temp. Agency								-0.26	-0.11
								(0.020)**	(0.0086)**
Business Service Firm (Non-FCSLT)								0.013	0.018
								(0.0049)*	(0.0040)**
New Establishment ($\leq$ 5 Years old)								-0.0052	0.0068
								(0.0030)	(0.0011)**
Mean of Dependent Variable	-0.077	-0.077	-0.077	-0.077	-0.077	-0.077	-0.077	-0.077	-0.077
R^2	0.10	0.34	0.14	0.35	0.34	0.36	0.35	0.22	0.37
Number of Observations	80917	80917	80917	80917	68223	80917	80917	68223	68223

Note: This table adds control variables to our baseline estimates in Table 4. Column (1) replicates Column (2) in Table 4. See notes to Table 4 for basic variable definitions and controls. In addition: Nonemp. Duration is the duration in months from the date of job loss to the start of the first job after job loss. Occ. change is an indicator for switching 2-digit occupation after job joss. Ind. change is an indicator for switching 3-digit industry. Change in industry and occupation tenure is the change in tenure at the industry or occupation at the current job after job loss, based on 2-digit occupation and 3-digit industry codes. Part time is an indicator for working part time (less than 30 hours per week). FCSL is an indicator variable for working at a Food, Cleaning, Security or Logistics firm. Temp. Agency is an indicator for working at a temporary help agency. Business Service Firm (Non-FCSLT) is an indicator for working in the business service sector except for FCSL or a temp. agency. New Establishments are establishments less than 5 years old. Statistical significance: * indicates $p \leq 0.05$ and ** indicates $p \leq 0.01$, SE are clustered on year level.

TABLE 6—ROBUSTNESS OF MAIN RESULTS TO DIFFERENT STRATEGIES FOR ESTIMATING ESTABLISHMENT AND WORKER EFFECTS, ALTERNATIVE ESTIMATION APPROACHES, AND INCLUSION OF FEMALE AND EAST GERMAN JOB LOSERS

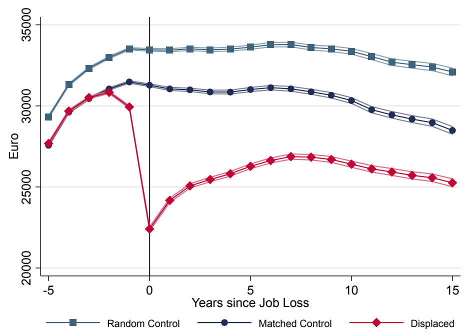
	(1) Baseline Results (Table 4)	(2) Est. FE from Rolling Window AKM Model	(3) Est. FE from Split Sample IV	(4) Est. FE from Rolling Window & Split Smpl IV	(5) Est. FE from Kmeans Clustering	(6) Est. FE from Estab.-by-Year FE	(7) Random Control Group (JLS Model)	(8) State Unemp. Rate	(9) Women West-Germany	(10) Men, Pooling West & East Germany
Panel A: Raw correlation (controlling for year and year squared only)										
Change in UR t-1 to t	-0.030	-0.030	-0.030	-0.027	-0.030	-0.030	-0.033	-0.027	-0.039	-0.030
	(0.0030)**	(0.0030)**	(0.0030)**	(0.0029)**	(0.0030)**	(0.0030)**	(0.0032)**	(0.0058)**	(0.0058)**	(0.0030)**
R^2	0.016	0.016	0.016	0.016	0.016	0.016	0.016	0.001	0.008	0.014
Number of Observations	80917	80917	79980	65219	80917	80917	86435	80917	24702	99442
Panel B: Controlling for composition effects (incl. expericence and tenure polynomial)										
Change in UR t-1 to t	-0.028	-0.027	-0.028	-0.022	-0.027	-0.030	-0.030	-0.024	-0.038	-0.027
	(0.0036)**	(0.0036)**	(0.0036)**	(0.0031)**	(0.0037)**	(0.0036)**	(0.0037)**	(0.0046)**	(0.0061)**	(0.0038)**
Establishment FE	-0.30	-0.35	-0.31	-0.44	-0.38	-0.18	-0.30	-0.30	-0.17	-0.23
	(0.030)**	(0.022)**	(0.032)**	(0.018)**	(0.027)**	(0.030)**	(0.034)**	(0.027)**	(0.048)**	(0.019)**
Worker FE	0.16	0.17	0.16	0.17	0.18	0.17	0.17	0.17	0.22	0.18
	(0.011)**	(0.011)**	(0.011)**	(0.011)**	(0.011)**	(0.012)**	(0.013)**	(0.012)**	(0.014)**	(0.0099)**
R^2	0.103	0.108	0.104	0.122	0.110	0.093	0.114	0.090	0.057	0.098
Number of Observations	80917	80909	79980	65219	80917	78477	86435	80917	24702	99442
Panel C: Controlling for change in establishment fixed effects (FE) and composition effects										
Change in UR t-1 to t	-0.014	-0.016	-0.013	-0.014	-0.016	-0.014	-0.015	-0.0089	-0.024	-0.013
	(0.0022)**	(0.0025)**	(0.0023)**	(0.0026)**	(0.0025)**	(0.0018)**	(0.0021)**	(0.0036)*	(0.0047)**	(0.0027)**
Worker FE	0.071	0.11	0.066	0.097	0.096	0.100	0.076	0.072	0.13	0.070
	(0.010)**	(0.0079)**	(0.010)**	(0.0081)**	(0.0083)**	(0.0099)**	(0.011)**	(0.010)**	(0.015)**	(0.010)**
Establishment FE	-0.020	-0.024	-0.018	0.018	0.077	-0.014	-0.023	-0.016	0.077	-0.012
	(0.014)	(0.016)	(0.014)	(0.016)	(0.026)**	(0.012)	(0.017)	(0.013)	(0.030)*	(0.0082)
Change in Estab. FE	0.74	0.70	0.78	0.90	0.85	0.71	0.75	0.74	0.68	0.75
	(0.019)**	(0.021)**	(0.018)**	(0.020)**	(0.020)**	(0.013)**	(0.020)**	(0.018)**	(0.023)**	(0.016)**
R^2	0.340	0.300	0.297	0.230	0.298	0.354	0.353	0.329	0.165	0.359
Number of Observations	80917	68482	79980	65219	68738	78477	86435	80917	24702	99442

Note: Each column in each panel represents a separate regression of the average change in log daily wages of an individual job loser compared to a matched control observation in the medium run (0 to 3 years after job loss) on the annual change in the national unemployment rate for West Germany. See notes to Table 4 for basic variable definitions. All regressions control for year and year squared. Panel (A) does not include other controls. Panel (B) controls for the worker fixed effect and the pre-displacement establishment fixed effect as well as tenure and experience polynomials. Panel (C) is the same as Panel (B) but adds the (difference-in-difference) change in the establishment fixed effect. Column 1 shows our baseline results from Columns 1, 2, and 5 of Table 4, respectively. Column 2 uses establishment and worker fixed effects that are estimated using a rolling AKM model that only uses observations within 5 years prior to the displacement event. Column 3 uses a split sample IV estimator to instrument for the establishment FE and the change in the establishment FE. Column 4 combines the split sample IV with the rolling AKM window. Column 5 uses the hybrid Kmeans clustering approach described in the text. Column 6 uses establishment effects that vary by year (AKM model with establishment-by-year fixed effects). Column 7 shows results using the random control group (instead of the matched control group) comparable to canonical approach in Jacobson, LaLonde and Sullivan (1993). Column 8 uses the change in the state unemployment rate as the business cycle indicator and controls for year effects. Column 9 shows our main result for women in West Germany. Column 10 shows the results for men pooling East and West Germany. Statistical significance: * indicates $p \leq 0.05$ and ** indicates $p \leq 0.01$, SE are clustered on year level.

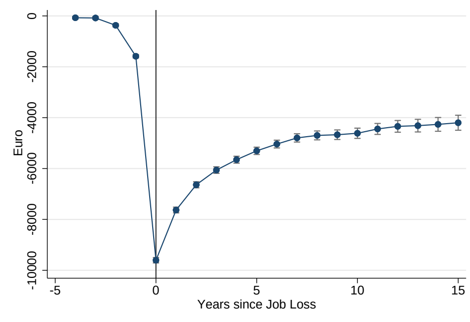
TABLE 7—THE CYCLICALITY OF LOG WAGE LOSSES FOR HIGH- AND LOW-WAGE WORKERS AS MEASURED BY WORKER FIXED EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Sample of Workers with High Worker Fixed Effects (Above Median)						
Change in UR t-1 to t	**-0.017**	**-0.016**	**-0.0073**	**-0.00011**	**-0.0071**	**0.000056**
	(0.0025)**	(0.0027)**	(0.0023)**	(0.0031)	(0.0023)**	(0.0030)
Establishment FE		-0.18			-0.0051	0.13
		(0.025)**			(0.017)	(0.023)**
Worker Fixed Effect (FE)		0.088			0.030	-0.016
		(0.015)**			(0.013)*	(0.015)
Change in Estab. FE			0.57		0.57	
			(0.016)**		(0.015)**	
Coeff. on Chg. in Est. FE = 1				1		1
Mean of Dependent Variable	-0.028	-0.028		-0.028	-0.028	-0.028
R^2	0.076	0.095	0.25	0.060	0.25	0.065
N	33141	33141	33141	33141	33141	33141
Panel B: Sample of Workers with Low Worker Fixed Effects (Below Median)						
Change in UR t-1 to t	**-0.036**	**-0.037**	**-0.018**	**-0.015**	**-0.018**	**-0.015**
	(0.0047)**	(0.0045)**	(0.0024)**	(0.0022)**	(0.0023)**	(0.0023)**
Establishment FE		-0.43			-0.031	0.047
		(0.034)**			(0.015)*	(0.013)**
Worker Fixed Effect (FE)		0.27			0.13	0.10
		(0.018)**			(0.014)**	(0.015)**
Change in Estab. FE			0.85		0.84	
			(0.017)**		(0.018)**	
Coeff. on Chg in Est. FE = 1				1		1
Mean of Dependent Variable	-0.11	-0.11		-0.11	-0.11	-0.11
R^2	0.040	0.079	0.37	0.024	0.38	0.027
Number of Observations	47764	47764	47764	47764	47764	47764

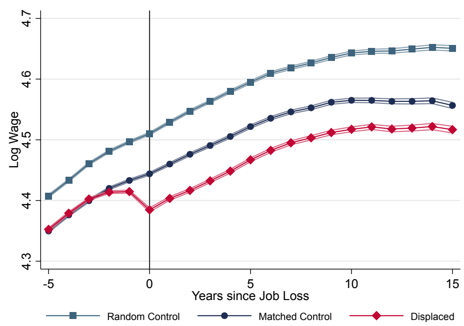
Note: This table replicates our main estimates from Table 4 for high and low wage workers. Panel A. restricts to workers whose worker fixed effect is above the median in the full population of workers in the AKM model. Panel B restricts to workers whose worker fixed effect is below the median. See Table 4 for variable definitions and additional controls. The annual change in the unemployment rate (UR) is based on the national unemployment rate for West Germany and measured in percentage points. Columns (4) and (6) regresses the log wage loss on the change in UR controlling for the change in the establishment effect, where the coefficient on the establishment effect is forced to be equal to 1. Statistical significance: * indicates $p \leq 0.05$ and ** indicates $p \leq 0.01$, SE are clustered on year level.



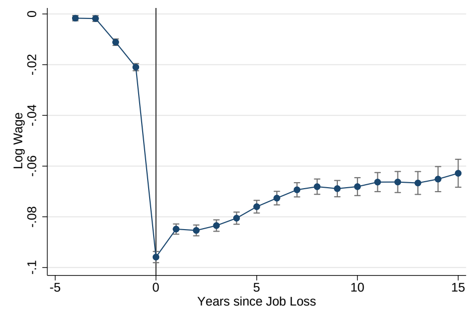
(a) Annual Earnings in Euros - Raw Means



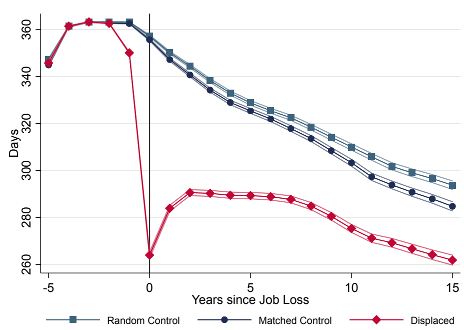
(b) Annual Earnings in Euros - Event Study



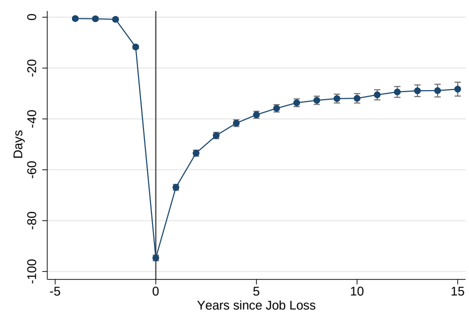
(c) Log Daily Wage - Raw Means



(d) Log Daily Wage - Event Study



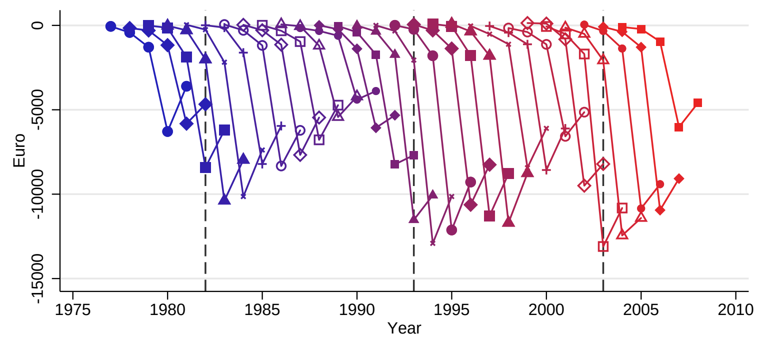
(e) Annual Days Worked - Raw Means



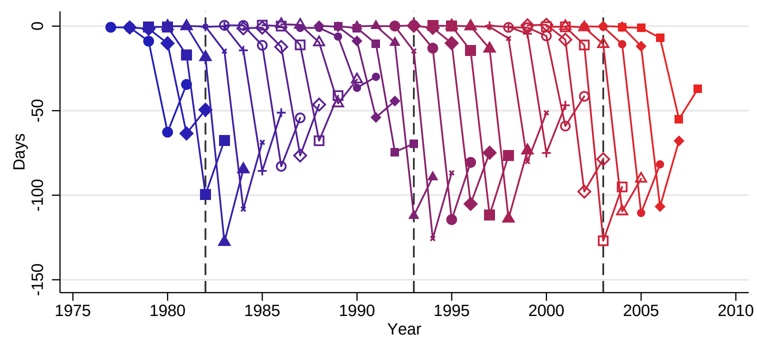
(f) Annual Days Worked - Event Study

FIGURE 1. LABOR MARKET OUTCOMES OF DISPLACED WORKERS BEFORE AND AFTER JOB LOSS

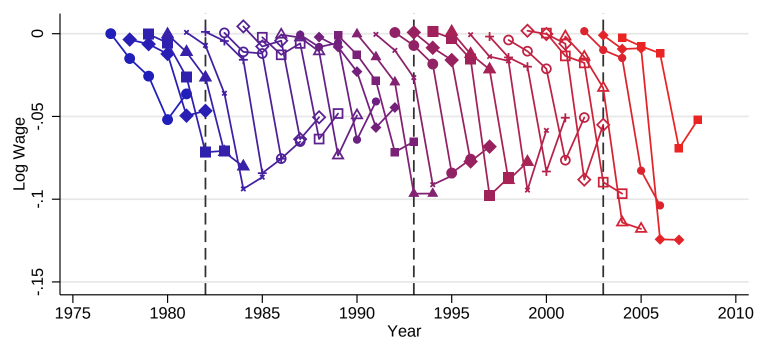
Note: Panels on the left of the figure show labor market outcomes for displaced workers (red diamond line), matched non-displaced workers based on the propensity score as described in text (navy circled line), and a random sample of non-displaced workers (blue squared line). Each point represents the average value in the respective worker group. Panels on the right of the figure show the corresponding estimates of the effect of displacement from event study regressions using matched sample as control group that control for age, year and individual fixed effects. All panels are constructed pooling workers displaced between 1979 and 1994, while the outcome data spans 1975-2009. See notes to Table 1 and text for definition of sample and displacement event. Earnings are shown in Euros at 2000 prices.



(a) Losses in Annual Earnings by Year in Euros



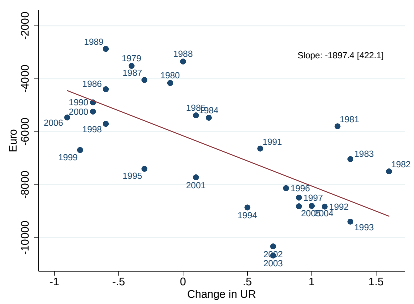
(b) Losses in Annual Days Worked by Year

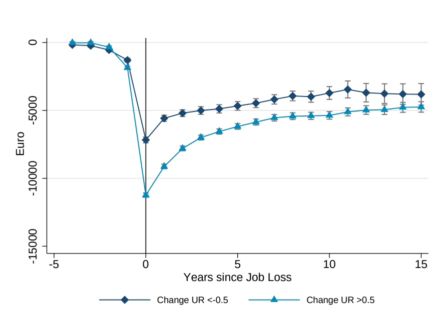


(c) Log Wage Losses by Year

FIGURE 2. LABOR MARKET OUTCOMES OF DISPLACED WORKERS BY YEAR OF JOB LOSS

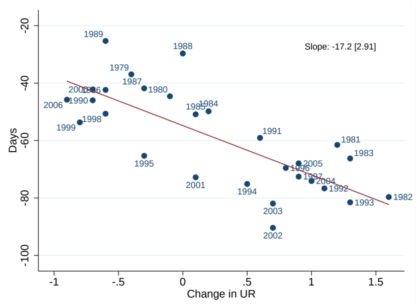
Note: Each line plots the coefficients from a separate event study regression estimating the effect of job displacement on the respective outcome. Regressions used matched control group and control for age, year and individual fixed effects. Dashed vertical lines show business cycle troughs. Earnings are shown in Euros at 2000 prices.

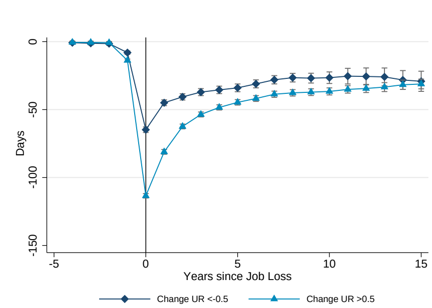




(a) Change in Annual Earnings vs. Change in Unemployment Rate

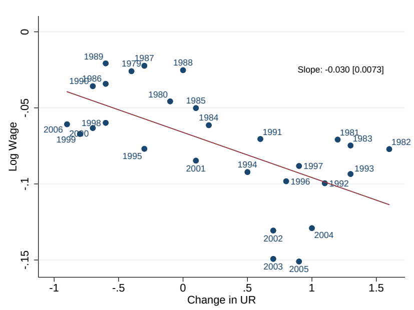
(b) Loss in Annual Earnings in Expansions vs. Recessions

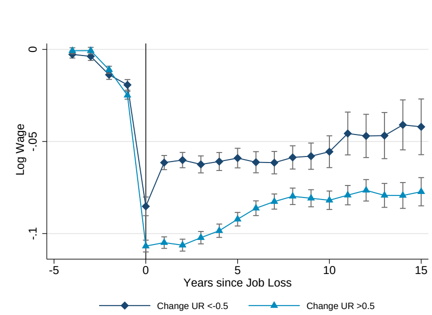




(c) Change in Annual Days Worked vs. Change in Unemployment Rate

(d) Loss in Days Worked in Expansions vs. Recessions



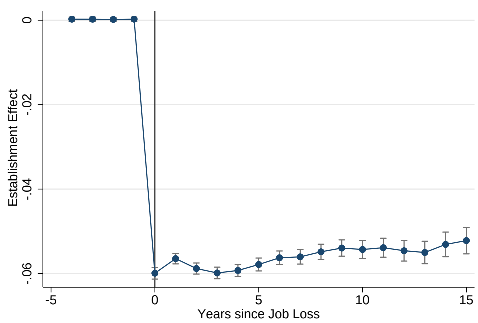


(e) Change in Log Daily Wage vs. Change in Unemployment Rate

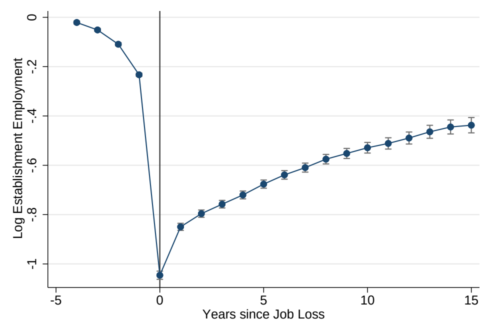
(f) Loss in Log Wage in Expansions vs. Recessions

FIGURE 3. EARNINGS, EMPLOYMENT AND WAGE LOSSES BY STATE OF LABOR MARKET

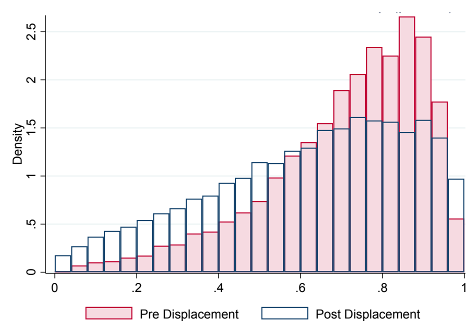
Note: Panels (a) to (c) shows scatterplots of the medium-term earnings, employment, and wage losses of job losers collapsed to the year level, relative to the year-over-year change in the unemployment rate based on the national unemployment rate for West Germany measured in percentage points. All outcomes are based on the changes for job losers relative to matched control observations over 0 to 3 years after job loss (see Table 4 and Section I.D). Panel (a) shows the effect on losses in annual earnings. Panel (b) the effect on losses in annual days worked. Panel (c) show the effect on log daily wages. The panels also shows the slope and standard error of the regression line. Panels (d) to (e) show results from corresponding event study regressions that pool job displacements occurring in expansions and recessions, respectively.



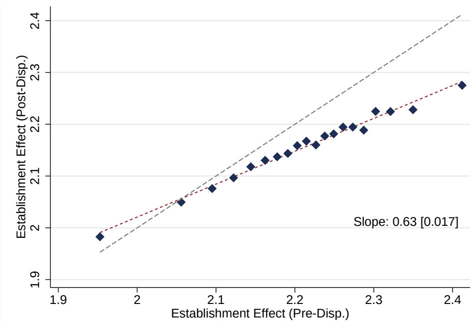
(a) Establishment Fixed Effect (FE)



(b) Log(Establishment Employment)



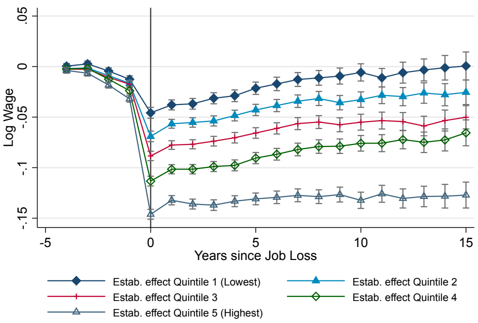
(c) Histogram of Estab. FE pctile Pre and Post Disp.



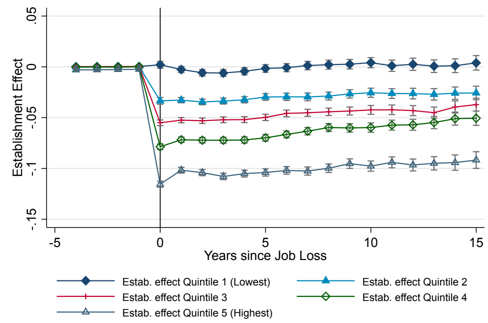
(d) Post-Disp. Estab. FE by Pre-Disp Estab. FE

FIGURE 4. ESTABLISHMENT CHARACTERISTICS AFTER JOB DISPLACEMENT

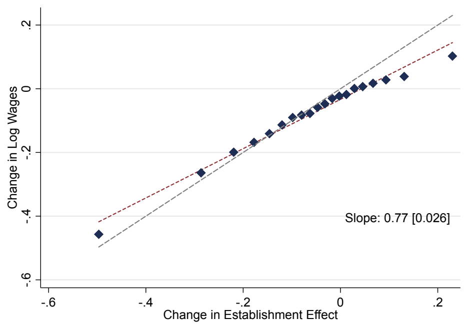
Note: Panels (a) and (b) show the effect of job loss on establishment fixed effects and log establishment size from event study regressions (see Figure 1).
Panel (c) shows histograms of the pre- and post-displacement distribution of establishment fixed effects, where the establishment fixed effects are normalized to percentiles of the overall distribution in the AKM sample (i.e., in a random sample the distribution would be flat). Panel (d) shows a binned scatter plot of post-displacement establishment fixed effects vs. pre-displacement establishment fixed effects among the displaced workers. Establishment effects are the average of the 3 years (5 years) prior (post) displacement. Bins are vintiles of the distribution of pre-displacement establishment fixed effects among displaced workers. The dashed gray line is 45-degree line, red line the regression line.



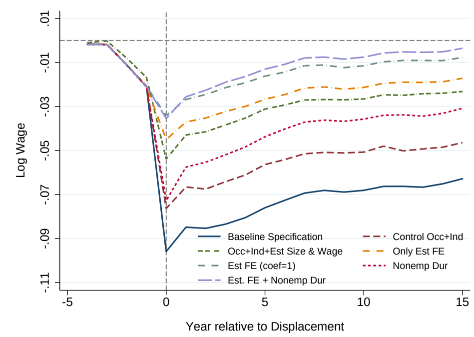
(a) Log Wages by Quintile Displacing Establishment FE



(b) Establishment FE by Quintile of Displacing Establishment FE



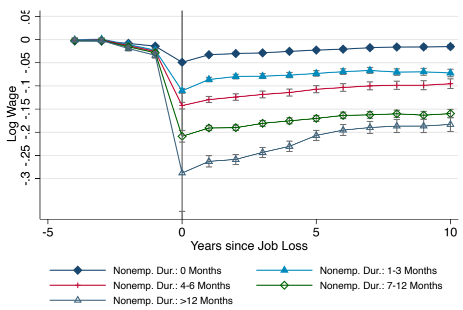
(c) Wage Loss by Establishment FE Loss



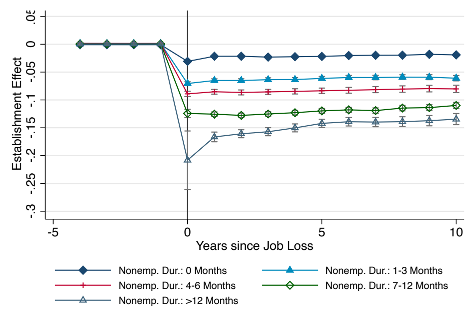
(d) Log Wage Losses Controlling for Post-Displacement Characteristics

FIGURE 5. THE RELATIONSHIP BETWEEN LOSSES IN ESTABLISHMENT FIXED EFFECTS (FE) AND WAGE LOSSES AT JOB DISPLACEMENT

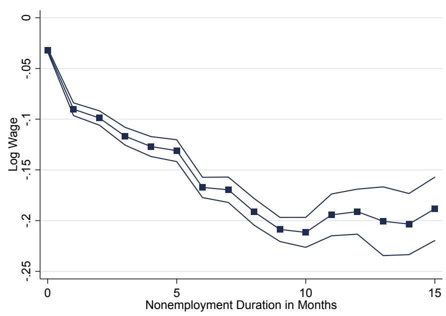
Note: Panels (a) and (b) show the effect of job loss on establishment fixed effects and log wages from event study regressions (see Figure 1) separately by quintiles of the pre-displacement establishment fixed effect. The quintiles are based on the distribution among displaced workers. Panel (c) shows a binned scatter plot of the difference-in-differences in log wages vs. the difference-in-differences in establishment fixed effects at job displacement (i.e., the change in outcome for a job loser relative to the change for a matched control observation, see Table 4 and Section I.D). The gray dashed line is the 45-degree line, the red line the regression line. Panel (d) shows the effect of job loss on log wages from an event study regression (see Figure 1) while consecutively adding more post-displacement controls: occupation and industry fixed effects, establishment size and establishment average wage, establishment fixed effects (from AKM model), establishment fixed effects (from AKM model) with coefficient constraint to 1, and duration of the post-displacement nonemployment spell. The baseline specification corresponds to Figure 1 (d).



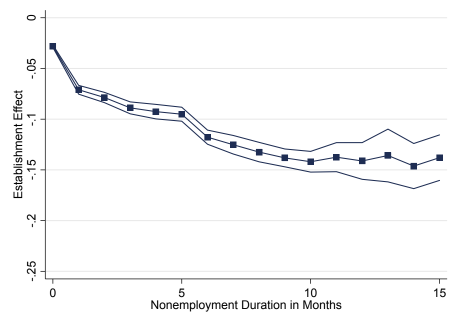
(a) Long-Term Wage Losses at Job Loss by Different Nonemployment Durations



(b) Long-Term Establishment Effect Losses by Different Nonemployment Durations



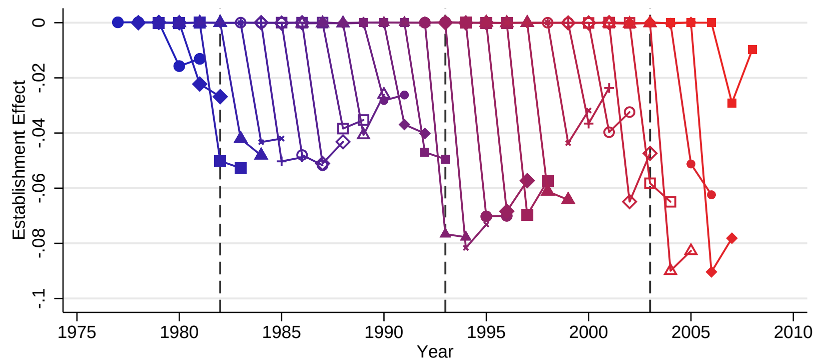
(c) Medium-Term Wage Loss by Month of Nonemployment Duration



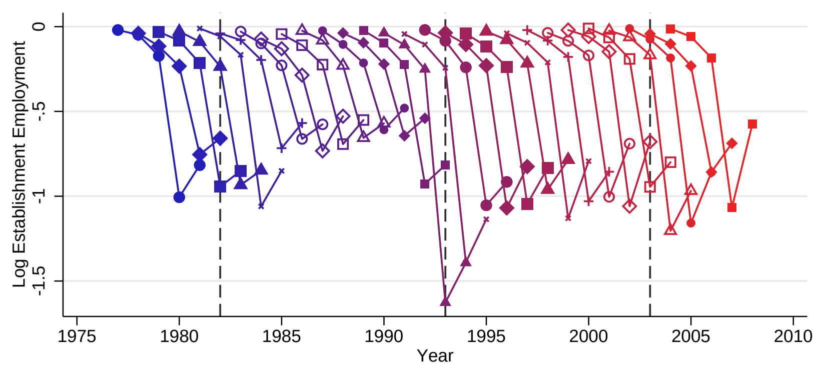
(d) Medium-Term Establishment Effect Loss by Month of Nonemployment Duration

FIGURE 6. LOSSES IN WAGES AND IN ESTABLISHMENT FIXED EFFECTS AT JOB DISPLACEMENT INCREASE WITH NONEMPLOYMENT DURATION

Note: The Figure shows the relationship between post-displacement outcomes conditional on the duration of the first nonemployment spell immediately after displacement for those not immediately finding a job. Panels (a) and (b) show event study estimates separately by categories of nonemployment duration (see Figure 1). Panels (c) and (d) use the difference-in-differences in the wage and establishment effect losses over the first 3 years after displacement relative to the pre-displacement period (compared to a matched control observation, see Table 4 and Section I.D), by months of nonemployment duration after job displacement.



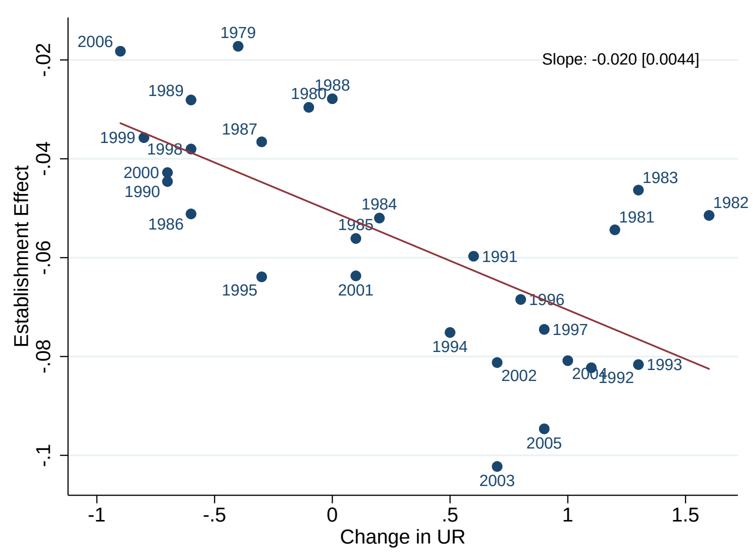
(a) Establishment Fixed Effect



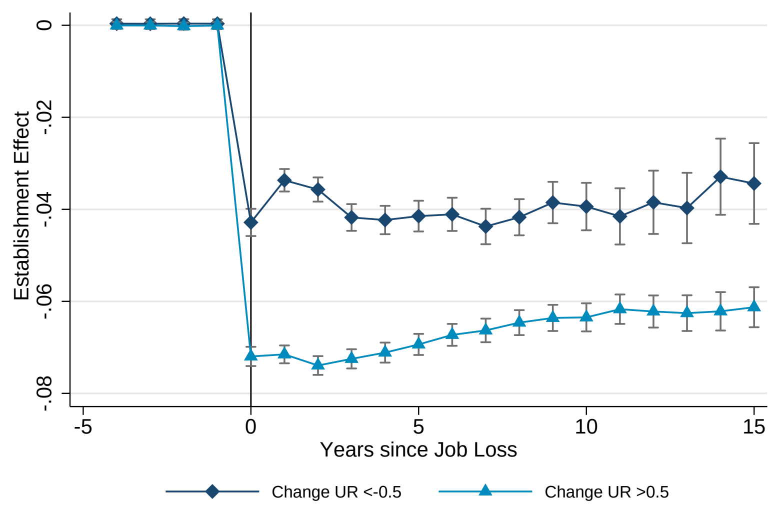
(b) Log Number of Employees at Establishment

FIGURE 7. EMPLOYER CHARACTERISTICS (NUMBER OF EMPLOYEES AND ESTABLISHMENT FIXED EFFECT) OF DISPLACED WORKERS BY YEAR OF JOB LOSS

Note: Each line plots the coefficients from a separate event study regression estimating the effect of job displacement on the respective outcome. Event study regressions use matched control group and control for age, year and individual fixed effects. Dashed vertical lines show business cycle troughs.



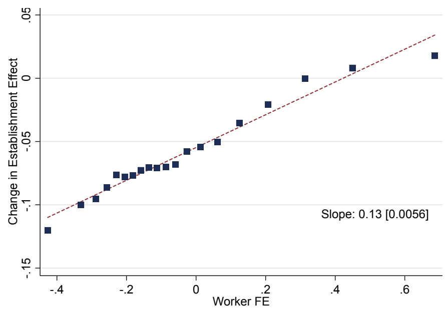
(a) Change in Establishment Effect vs. Change in Unemployment Rate



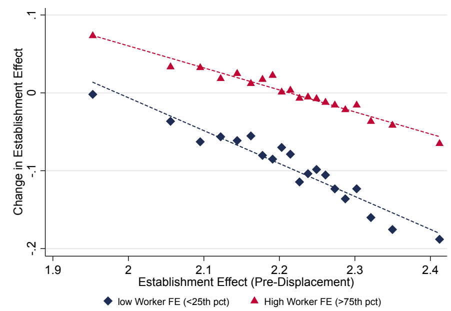
(b) Losses in Establishment Effects in Expansions vs. Recessions

FIGURE 8. THE EFFECT OF JOB DISPLACEMENT ON LOSSES IN ESTABLISHMENT FIXED EFFECTS OVER THE BUSINESS CYCLE

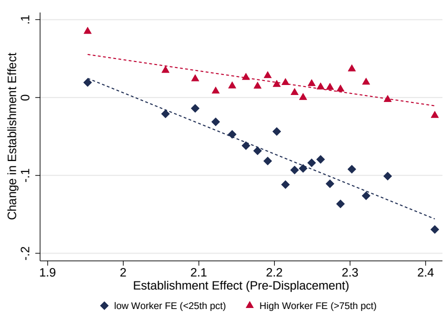
Note: The figure shows how losses in establishment fixed effects at job displacement vary over the business cycle. Panel (a) shows a scatterplot of the medium-term losses in establishment fixed effects of job losers collapsed to the year level, relative to the year-over-year change in the unemployment rate based on the national unemployment rate for West Germany measured in percentage points. The outcome is based on the difference-in-differences in the establishment effect losses over the first 3 years after displacement relative to the pre-displacement period (compared to a matched control observation, see Table 4 and Section I.D. Panel (b) shows results from corresponding event study regressions that pool job displacements occurring in expansions and recessions, respectively.



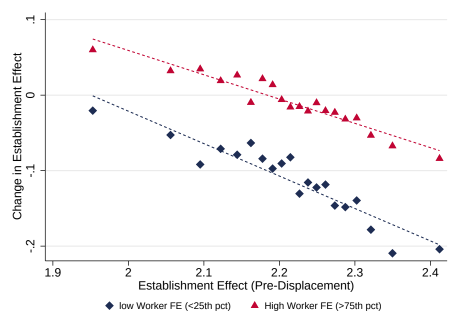
(a) Changes in Estab. FE by Worker FE



(b) Changes in Estab. FE by Pre-Disp Estab. FE



(c) Changes in Estab. FE by Pre-Disp Estab. FE, Change $UR < -0.5$ (Expansions)



(d) Changes in Estab. FE by Pre-Disp Estab. FE, Change $UR > 0.5$ (Recessions)

FIGURE 9. THE ROLE OF JOB DISPLACEMENT IN REALLOCATING LOW-WAGE WORKERS TO EMPLOYERS WITH LOW ESTABLISHMENT FIXED EFFECTS OVER THE BUSINESS CYCLE

Note: Panel (a) shows a binned scatter plot of the difference-in-differences in the establishment fixed effect losses at job loss (compared to a matched control observation, see Figure 5 and Section I.D) vs. the individual effect of the displaced worker. Panels (b)-(d) show binned scatter plots of the difference-in-differences in establishment fixed effects vs. the pre-displacement establishment fixed effect, splitting the sample into workers with high (above 75th percentile) and low (below 25th percentile) worker fixed effects. Panel (b) shows the overall relationships, while (c) and (d) further differentiate by expansions vs. recessions.