# Job Displacement and Migrant Labor Market Assimilation*

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**Abstract**

This paper sheds new light on the barriers to migrants' labor market assimilation. Using administrative data for Germany from 1997-2016, we estimate dynamic difference-in-differences regressions to investigate the relative trajectory of earnings, wages, and employment following mass layoff separately for migrants and natives. We show that job displacement affects the two groups differently even when we systematically control for pre-layoff differences in their characteristics: migrants have on average higher earnings losses, and they find it much more difficult to find employment. However, those who do find a new job experience faster wage growth compared to displaced natives. We examine several potential mechanisms and find that these gaps are driven by labor market conditions, such as local migrant networks and labor market tightness, rather than migrants' behavior.

**Keywords:** Immigration, Job Displacement, Job Search
**JEL codes:** J62, J63, J64

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# 1 Introduction

In the face of their demographic crises, many developed countries turn to immigration as a solution. Historically, countries have been using immigration as a way to increase their human capital and relieve labor shortages in particular sectors and occupations, and the policy debate has focused on how to attract migrants with the right set of education, experience, and skills. However, much of the success of immigration policies hinges on how well the migrants integrate into the host economy. Migrants who struggle to enjoy the same opportunities as natives not only suffer from lower earnings and standards of living, but their host country is also missing out on their full economic potential.

There is an extensive body of research analyzing whether, and how, migrants' labor market outcomes converge to natives' over time (e.g., Chiswick (1978); Borjas (1985); Lubotsky (2007); Algan, Dustmann, Glitz, and Manning (2010); Blau, Kahn, and Papps (2011); Abramitzky, Boustan, and Eriksson (2014)). These studies document important patterns of migrants' earnings growth by cohort, migrant generation, and source country characteristics. However, they can say little about the drivers of this convergence. Do migrants' outcomes improve because they build social networks, learn to navigate the labor market, or because they benefit from changing societal attitudes? Answering these questions doesn't just expand our understanding of the assimilation process, but is also of crucial importance to policymakers looking to help migrants fully utilize their skills in the host country.

In this paper, we shed light on the *process* of migrant assimilation by studying how migrants and natives in Germany respond to unemployment. We use the aftermath of involuntary job displacement as a laboratory to observe how the two groups fare under identical circumstances, something that isn't possible when comparing migrants and natives in general. We focus on workers with at least 3 years of tenure at the time of displacement, such that we look at migrants who, on paper at least, might be considered to be already well-integrated in the labor market. We estimate the relative trajectory of their earnings, wages, and employment following a job loss, applying a reweighting scheme that ensures the migrants and natives are comparable in terms of observable characteristics. We then analyze how these gaps vary under different labor market conditions and in response to worker behavior to identify the obstacles to migrant assimilation in the labor market. Our results show that both displaced migrants and natives face substantial earnings losses after job displacement, with migrants catching up with natives only 3 years after displacement. We moreover find indicative evidence that it's the labor market conditions and environment, rather than worker behavior as such, that contributes to migrants' lagging behind the natives.

There are several reasons why the German institutional setting is well-suited to analyze migrant-native earnings differentials. First, the German labor market is characterized by low levels of informality, such that switching to illegal employment is only a marginal outside option for most migrants.[1] Second, migrants with at least one year of work experience in Germany – and thus essentially all migrants in our sample – face the same conditions concerning UI benefit receipt as natives. Finally, even though migrants have become increasingly important for the German labor market, their share rising from 9% in 2005 to almost 13% in 2015[2], they still struggle to take up employment.[3]

We start by estimating the cost of job loss in terms of earnings, employment, and wages for migrants and natives on German administrative data in the years 1996-2016. To obtain causal estimates of job loss, we focus on migrants and natives who lost their job involuntarily in mass layoffs. Following the literature on job displacement (e.g., Jacobson, LaLonde, and Sullivan (1993); Schmieder, von Wachter, and Heining (2023)), we use propensity score matching to find a comparable non-displaced worker for each displaced migrant and native, and then use an event study design to estimate the impact of displacement on the labor market outcomes separately for natives and migrants. To allow for a direct comparison of the cost of displacement between the two groups, we re-weight the sample of migrants to make them observationally identical to natives, using the reweighting algorithm in DiNardo, Fortin, and Lemieux (1996). Unlike the existing work on migrant-native outcome gaps, which focused on linking them to the differences between migrants' and natives' characteristics such as age and education, our estimates effectively control for these differences. We thus come very close to the ideal yet unfeasible experiment in which two otherwise identical workers with different migration statuses lose their jobs.

We find that both migrants and natives experience a large dip in earnings following a mass layoff, with about 13% higher losses for migrants in the year after displacement. Importantly, while this gap dissipates about 3 years after the negative shock, it consists of two persistent, opposing trends in employment and wages. First, migrants significantly struggle to find employment compared to natives: even 5 years after the layoff, migrant workers are 3 percentage points less likely to be employed than observationally similar natives. At the same time, however, those migrants who do find a job experience a relatively faster wage growth compared to natives, so that 5 years after the layoff the migrant-native wage gap is positive (2 log points difference).[4] We find that wage growth is more pronounced for firm movers compared to stayers, suggesting that it is driven by migrants who seize better opportunities, rather than firms updating their beliefs about migrant workers' productivity. Overall, these results confirm that labor market shocks affect migrants and natives differently: while a negative shock hits immigrants harder, we also find tentative evidence

[1] Note that Germany has a small shadow economy relative to GDP, ranking 7th among all OECD countries in 2022 (Schneider and Boockmann, 2022).

[2] Own computations based on a 2% sample of worker biographies provided by the Institute for Employment Research.

[3] For example, in August 2022, only 53% of migrants in Germany were employed, compared to 69.2% of natives (Brücker, Hauptmann, Keita, and Vallizadeh, 2022).

[4] Note that both migrants and natives experience an absolute loss in earnings, wages, and employment after a layoff that persists beyond 5 years after the displacement. The migrant-native gaps describe here refer to the relative sizes of these displacement effects.

that some migrants may benefit from being forced to search again.

In the second part of the paper, we examine *why* labor market shocks impact migrants differently to natives. To do this, we draw on the rich set of information available in the administrative data set, including job search of UI recipients, coworker networks, initial labor market conditions, and unobservable productivity of both the workers and their employers as estimated with AKM fixed effects. For each potential mechanism, we analyze whether a particular factor – such as workers' job search objectives and behaviors – can explain some of the migrant-native gaps in employment and wages.

First, we look at whether the negative employment gap and in particular the positive wage gap, can be explained mechanically by the reset to worker sorting across firms. If new immigrants find it more difficult to match with more productive employers, a reset of the matching in terms of a mass layoff could lead to relatively better matches for migrants (and relatively worse matches for natives). This would lead to a regression to the mean pattern in migrants' and natives' outcomes – generating a positive wage gap for migrants. However, we find that firm sorting of this kind does not explain the wage gap. While a significant part (15-20%) of the cost of job displacement is driven by a fall down the firm ranking in terms of firms' AKM fixed effects, this happens equally likely for the migrants and natives in our sample. Similarly, even though we find that the faster post-displacement wage growth is driven by migrant job-switchers, these switches do not correspond to a significantly faster sorting into better firms.

Second, we focus on the role of worker behavior. Using the data on the job search of UI recipients, we find that migrants and natives broadly search for similar jobs, so it is not the case that migrants struggle to find employment because they systematically misunderstand labor market conditions or search for unfeasible jobs. What does seem to matter for their job search is their networks. The administrative data allow us to calculate the share of migrants both in their pre-layoff workplace and in their area of residence. We find that having a larger migrant network – living in an ethnic enclave – substantially lowers migrants' chances of finding employment and better-paid jobs. Because this effect does not apply to natives living in the same location, the difference in social networks explains a large share of the observed negative gaps in labor market outcomes.

Third, we test the hypothesis that the negative gap between migrants and natives is caused by labor market discrimination. Direct measures of migrant discrimination on the aggregate level are rare, so we address this point indirectly by comparing the size of migrant-native outcome gaps across labor markets of different tightness. Our identifying assumption is that discrimination is more costly in labor markets with many vacancies chasing relatively few unemployed workers. In line with our predictions, we find that both wage and employment gaps disappear in labor markets in the upper half of the tightness distribution.

As a final step of our analysis, we use the worker AKM fixed effects to examine the diverging labor market outcomes *within* the migrant group, between the migrants who do and do not find re-employment. Re-estimating the wage and employment gaps separately for migrants with above- and below-median worker FE, we show that the within-migrant differences are driven entirely by unobserved migrant productivity pre-displacement. The above-median migrants track natives closely in terms of their earnings and employment; the migrant-native gap in earnings and employment is driven by below-average migrants.[5]

Our main takeaway from the mechanism analysis is that the differential impact of mass layoffs on migrants – and hence their assimilation – is mostly driven by their environment rather than their behavior. Furthermore, the comparison of outcomes between migrants suggests that to address the migrant-native gap the policymakers should focus on interventions that close the differences within the migrant group, by learning from the above-median migrants who already navigate the German labor market successfully.

Our analysis isn't without its caveats. We do not measure worker behavior exhaustively. For example, we lack information on job search effort and intensity: the negative migrant-native employment gap might be the result of migrant workers searching less, or the positive wage gap might be because migrants search more. There is also the question of unobserved differences between migrants and natives: both groups self-select into their status which might lead to systematic differences in characteristics such as risk aversion or productivity (Borjas, 1987). We present evidence that selective return migration *under*-estimates the migrant-native gap in our data, but addressing these issues more broadly, e.g. the question of selection on the unobservables into migration is outside of the scope of this paper.

Our main contribution is to the literature on migrant assimilation into the labor market (e.g., Chiswick (1978); Borjas (1985); Algan et al. (2010); Dustmann, Frattini, and Lanzara (2012); Borjas (2015); Cadena, Duncan, and Trejo (2015)). The pioneering work in this area has focused on documenting wage assimilation profiles and how they develop over time for different cohorts of migrants. We extend this literature by systematically controlling for the observable differences between migrants and natives and exploring the drivers of their assimilation. Our empirical strategy allows us to show that the differences in characteristics between migrants and natives alone cannot explain why migrants often take decades to catch up with natives. Furthermore, by examining various alternative mechanisms, we can shed some light on what slows down the assimilation of migrants observationally identical to natives.

Our empirical strategy draws on the literature investigating how sensitive migrants are to adverse economic shocks. Borjas and Cassidy (2021) find that migrants particularly suffered from displacement during the early phase of the Covid-19 pandemic, partly because they are less likely to work in jobs that can be performed remotely. In the same spirit, other studies have shown that

[5]We do not use the worker fixed effects to compare unobservable productivity between migrants and natives. Because AKM fixed effects are calculated using (pre-displacement) wages, they reflect not just the productivity differences between workers, but also any labor market discrimination that might lead to lower pay. Worker FE can be used for within-migrant comparisons under the assumption that all migrants are discriminated against in equally.

migrants' entry wages during recessions are lower than natives' (see, e.g., (Kahn, 2010; Kondo, 2015; Speer, 2016)) and that migrants' or African Americans' unemployment rate is particularly sensitive to business cycle conditions and local unemployment rates (e.g., (Altonji and Blank, 1999; Bratsberg, Barth, and Raaum, 2006; Dustmann, Glitz, and Vogel, 2010; Hoynes, Miller, and Schaller, 2012)). Closest to our paper, Bratsberg, Raaum, and Røed (2018) and Hardoy and Schøne (2014) analyze the raw gap in job loss costs for migrants and natives in Norway, without controlling for ex-ante differences between the two groups. In their study, displacement unambiguously leads to large negative gaps between migrant and native outcomes. In contrast, we show that controlling for observable differences can substantially change the result: for migrants who are comparable to natives, the wage gap is decreasing and turns positive four years out.

We also add to the growing literature on migrants' sorting across firms. Dostie, Li, Card, and Parent (2023) show that differences in firm wage premiums explain an important part of the migrant-native earnings differential in Canada, and that part of migrants' wage assimilation is accounted for by moves to better employers. Similar patterns are observed in Sweden (Åslund, Bratu, Lombardi, and Thoresson, 2021) and Israel (Arellano-Bover and San, 2020). We show that, while important for explaining absolute differences in pay, such upward moves are not due to a reset after job displacement: sorting across firms post-displacement does not explain the difference in wage losses between migrants and natives when holding pre-displacement characteristics constant.

Finally, we contribute to the broader literature on the cost of job displacement. Previous studies have shown that displacement comes at much higher costs for women (Meekes and Hassink, 2022; Illing, Schmieder, and Trenkle, 2021), workers in routine-intensive occupations (Blien, Dauth, and Roth, 2021), and low-wage workers in the manufacturing sector (Helm, Kügler, and Schönberg, 2021). Bertheau, Acabbi, Barcelo, Gulyas, Lombardi, and Saggio (2022) have shown that the costs of job displacement can vary substantially across countries, with workers displaced in Southern Europe facing much higher costs than workers in Northern Europe. While there is emerging literature on the costs of job loss by worker type, no study to date analyzes detailed post-displacement labor market outcomes for migrant workers.

The rest of this paper proceeds as follows. In Section 2, we describe the German administrative data and the sample of working-age men we use for our analysis. In Section 3, we estimate the migrant-native gaps in labor market outcomes following an involuntary job displacement. We examine the potential drivers of these gaps in Section 4. Section 5 concludes.

## 2 Data

For our empirical analysis, we use a 12.5% sample of the universe of workers subject to social security in 1997-2016, provided by the Institute for Employment Research (IAB). Following the previous literature on job displacements (e.g., Jacobson et al. (1993); Schmieder et al. (2023)), we focus on a baseline sample of male workers who fulfill the following restrictions: They have at least 3 years of tenure, are full-time employed, aged 25-50, and work for an establishment with at least 50 employees.

The workers in our sample thus have relatively stable employment biographies before they are laid off, which facilitates the comparison with the control group. We also ensure that firms are large enough for displacement to be exogenous, i.e. unaffected by an individual worker's productivity. These restrictions moreover help us to compare our results with findings from existing research. Since previous research has shown that costs of job displacement differ by gender (e.g., Meekes and Hassink (2022); Illing et al. (2021)), in Appendix C we replicate our main results for a sample of women.[6]

We merge this data with information on mass layoff events, where we define mass layoffs as establishments either i) completely closing down or ii) reducing their workforce by at least 30% between June 30 in t=-1 and June 30 in t=0. One threat to the identification of mass layoffs in administrative data is mergers, takeovers, spin-offs, and ID changes. To eliminate such events from our data and thus avoid measurement error, we construct a matrix of worker flows between establishments by year following Hethey-Maier and Schmieder (2013). If more than 30 percent of displaced workers move to the same successor establishment, we exclude this establishment from our sample. We moreover exclude establishments where the workforce increased by more than 30% in at least one of the two years preceding the layoff.

Workers in our sample are displaced in 2001-2011; restricting our observation period to 1997-2016 thus ensures that we can follow workers for at least five years before and five years after displacement, as long as they are registered in the social-security data during this period.

Table 1 presents summary statistics of displaced workers in our sample in t=-1, the baseline year. Column (1) shows the characteristics of a random 2-percent sample of migrants in Germany, which we compare to our baseline sample of migrants (column (2)) and migrants after reweighting (column (3)) (we explain the reweighting procedure in detail in Section 3). Migrants in our sample have substantially higher tenure, wages, and earnings than the random sample of migrants. A similar pattern holds for a random sample of native workers (column (4)) compared to native workers in our sample (column (5)). This reflects our baseline restrictions, which ensure that we focus on a sample of high-tenured workers with a strong attachment to the labor market. Note, however, that our results are robust to relaxing the tenure restriction to one or two years, respectively (see Table 6).

[6] We find that the main patterns for employment, earnings, and wages broadly hold for female migrants. Unlike men, female migrants after reweighting experience no difference in post-displacement wages compared to natives. As a result, for women, the entire migrant-native gap in earnings after displacement is driven by the lower employment probablities of female migrants.

# 3 The migrant-native earnings gap

## 3.1 Empirical strategy

We aim to identify the causal effect of job displacement on the earnings losses of migrants and natives. Identifying this effect is difficult due to a potential selection bias arising from the fact that migrants and natives may lose their jobs for different reasons. For example, firms may fire workers if they are bad matches, and because of fewer skills acquired in the destination country, this may be more often the case for migrant workers. To hold the reason for losing one's job constant, we follow the seminal study by Jacobson et al. (1993) and investigate the labor market effects of being exogenously displaced in a mass layoff. To estimate the clean treatment effect of job displacement, we would ideally compare the labor market outcomes of the same worker on two occasions: in the event that he is losing his job, and in the event that he is not. Since this is not possible, we compare each displaced worker to a matched, non-displaced worker. The non-displaced worker matches are statistical twins for the displaced workers; our identifying assumption is that in the absence of job displacement, displaced workers' labor market trajectories would have evolved in the same way as those of their non-displaced matches.

**Propensity score matching: finding a control for each displaced worker** Simply comparing displaced to non-displaced workers in our sample would likely lead to biased estimates of the cost of displacement because the two groups differ in their individual and job characteristics. To address this, we assign each displaced worker a suitable non-displaced control worker match within cells. Both displaced and non-displaced workers also have to satisfy our baseline restrictions in a given baseline year. We match on the following variables: establishment size in t=-1, log wage in t=-3 and t=-4, years of education in t=-1, tenure in t=-1, and age in t=-1. We assign each worker a control worker with the closest propensity score (without replacement).

**Re-weighting: making natives and migrants comparable** Migrants differ from natives in observable individual characteristics (e.g. age and education), and they work in different 1-digit occupations and for different firms. All of these may shape their post-layoff labor market recovery. To understand which part of the migrant-native earnings gap is due to such differences in observables, we complement our regressions with a reweighting scheme first proposed by DiNardo et al. (1996) and applied in the context of job displacement by Illing et al. (2021). We, therefore, reweight migrants to native workers in terms of the following observable characteristics, all measured in the year before job loss: Age, tenure, education, 1-digit occupations, a dummy for city residency, firm size, and 1-digit industries.[7] Migrants who are more similar to natives on characteristics such

[7]Note that while some of the variables used in the reweighting exercise overlap with those used for the propensity score matching, there are some differences. For example, we decided to include a dummy on city residency in the set

as years of education and tenure receive a higher weight. The intuition is that after reweighting migrants to natives, we can attribute the differences in their outcomes after job loss to how they respond to displacement or to the difficulties they face, rather than to their characteristics.[8] As a result, we apply reweighting to all regressions in the paper unless explicitly noted.

Comparing our reweighted sample of migrants in Table 1 (column (3)) to baseline native workers (column (5)) shows that they are very similar in terms of observable characteristics. After reweighting, hardly any differences between migrants and natives remain in terms of individual characteristics such as years of education, age, and earnings. Migrants in the reweighted sample moreover work in more similar establishments than natives, even though differences remain in terms of firm size (larger for natives) and the share of migrant workers (larger for migrants). In addition to our baseline results, in Section 3.3 we therefore also present results from an additional analysis where we compare migrants and natives displaced from the same establishment.

**Event studies** To estimate the treatment effects from job displacement, we apply a dynamic difference-in-differences regression model with worker and time-fixed effects. Specifically, we estimate the following regression specification separately for migrants and natives:

$$\begin{aligned} y_{itc} = & \sum_{j=-5, j\neq -3}^{j=5} \alpha_j \times I(t = c+1+j) \times Disp_i \\ & + \sum_{j=-5, j\neq -3}^{j=5} \beta_j \times I(t = c+1+j) \\ & + \pi_t + \gamma_i + X_{it}\beta + \varepsilon_{itc} \end{aligned} \tag{1}$$

where the dependent variable $y_{itc}$ denotes average labor market outcomes (e.g., earnings, log daily wages, employment, number of days worked) of individual $i$, belonging to cohort $c$ in year $t$.[9] $Disp_i$ is a dummy indicating whether a worker is displaced. We interact with dummies $I(t = c+1+j)$ for 5 years before and after the job loss. We omit period $t = -3$ as the reference category. The coefficients of interest are $\alpha_j$, which present the evolution of displaced workers' labor market outcomes relative to the non-displaced control group. We moreover include dummies $I(t = c+1+j)$ for the year since displacement to account for the fact that due to the baseline tenure restriction, matched workers are

of reweighting variables, because migrants are much more likely than natives to live in cities (see Table 1. While the objective of the propensity score matching is to make displaced workers comparable to non-displaced workers, the objective of the reweighting is to make displaced migrants comparable to displaced natives.

[8]Note that we drop matched worker pairs where the displaced migrant had a propensity score above the 99th percentile. If we kept these workers, they would receive very large weights such that our reweighted estimates would be driven by a few migrant workers in our sample. Our results are thus conservative relative to what we would get if we used the full reweighted sample. In the Appendix, we replicate Figure 1 for the full sample of workers, showing both larger positive wage and negative employment effects for migrants, but estimated with much higher statistical uncertainty.

[9]For all workers laid off in year $t = 0$, the baseline year is $t = -1$, which is also their cohort $c$.

on an upward earnings profile (Schmieder et al., 2023). In addition, $\pi_t$ comprises year-fixed effects, $\gamma_i$ are individual fixed effects, and $X_{it}$ is a vector of time-varying age polynomials. We cluster standard errors at the worker level and re-weight migrant estimates to make them comparable to natives, as described above.

**Difference-in-differences** While the event study regression results are informative about the long-term dynamics of labor market trajectories, a broader comparison between pre- and post-layoff labor market outcomes helps us to dig deeper into the mechanisms underlying our event study regression results. We, therefore, follow Schmieder et al. (2023) and estimate a simple difference-in-differences (DID) type of regression model, where we proceed in two steps. In the first step, within each matched worker pair, we construct an individual-level cumulative measure of earnings losses (and other outcomes), which we call the DID outcome. For this purpose, we compute the mean difference in earnings (and other outcomes) before and after job loss within each displaced and non-displaced worker match:

$$\Delta y_{DID,ic} = \Delta y_{DP,ic} - \Delta y_{NDP,ic} \tag{2}$$

where $\Delta y_{DP,ic}$ reports the difference in average earnings for displaced worker $i$ in cohort $c$ before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss. $\Delta y_{NDP,ic}$ reports the corresponding measure for the matched non-displaced worker. $\Delta y_{DID,ic}$ then indicates the extent to which these differences in means vary within matched worker pairs: the "individual treatment effect" from job loss.

In the second step, we regress the outcome variable $\Delta y_{DID,ic}$ on different sets of variables, for example, search preferences or deciles of labor market tightness. In addition, we always use our baseline weights to control for pre-displacement differences in observable characteristics between migrants and natives. We cluster standard errors at the baseline establishment or baseline county level and re-weight migrant estimates to make them comparable to natives.

## 3.2 Baseline results

We present the results of our main event study design in Figure 1. In the four panels of the figure, we plot the treatment effect of displacement on yearly earnings[10], daily log wages, employment status and the number of days worked per year separately for natives and migrants. We present both weighted and unweighted estimates for migrants to highlight how controlling for the differences in observable characteristics changes the migrant-native gap in post-displacement outcomes.

[10]We show yearly earnings measured relative to earnings in t=-2. This measure helps us to include observations with 0 earnings and is more easily interpretable than log(earnings +1). Note that we exclude outlier pairs where the relative earnings measure exceeds 100 at least once during the observation period. This affects less than 0.7% of our sample.

We find that, in a raw comparison, migrants lose out more following an involuntary job loss than natives. The gap between migrants' and natives' displacement costs is negative and persistent for all four labor market outcomes. 5 years after an involuntary job loss, workers suffer from lower employment, wages, and total earnings, but this cost is significantly greater for migrants: 1 year after displacement, they have 15 percentage points higher relative earnings losses than natives, a gap that decreases to 5 percentage points 5 years out.

However, these patterns change significantly when we re-weight the migrant treatment effects to take into account the differences in observable characteristics. Controlling for worker characteristics reduces all relative treatment costs considerably. In Panel (a), we show that the negative and persistent migrant-native gap in earnings becomes statistically indistinguishable from natives' 3 years after the displacement when re-weighting is applied.

Re-weighting similarly changes our quantitative and qualitative estimates of the wage and employment costs. In particular, we find that the temporary negative gap in earnings is driven by significantly more persistent trends in employment than wages. In Panel (c), we show that the cost of displacement in terms of employment is significantly greater for migrants than natives: 5 years after the negative shock, migrants are 2 percentage points less likely to be employed than natives. This pattern is similar, albeit somewhat smaller, for the number of days worked (Panel (d)).

At the same time, however, the migrants who manage to find employment experience a relatively *better* wage trajectory than comparable natives. While the annual wage gap is significantly and persistently negative for the raw comparison, it is significantly smaller – and turns positive in year 4 – for re-weighted migrants. As a result, while the cumulative cost in wages is somewhat larger for natives, displaced migrants switch to a faster wage growth compared to natives. In Figure B4 in the Appendix, we confirm that this pattern is not driven by a changing composition of employed migrants (e.g. migrants with higher reservation wages both take longer to search and have higher wages upon re-employment), since the pattern holds when we restrict our sample to individuals who found re-employment within 1 year of the mass layoff.

We explore the wage gap between migrants and natives in greater detail in Table 2. In Panels A and B of the table, we calculate the average starting (re-employment) wage and subsequent wage growth for migrants relative to natives. Both unweighted and weighted estimates show that migrants earn lower starting wages, but they subsequently experience faster wage growth. One explanation of this pattern could be that firms need longer to learn about migrants' productivity; this might also explain why migrants take longer to find re-employment. However, the wage data does not suggest this is the case. As we show in columns (3) and (4), both patterns are primarily driven by job movers rather than growth within the same employment spell. Migrants who switch jobs during the 5 years after the mass layoff both earn higher starting wages, and experience faster wage growth. We return to the question of different migrant outcomes in Section 4.6.

### 3.3 Within-firm results

As the next step, we replicate our baseline estimates of the migrant-native displacement gap for workers who were laid off from the same establishment. While we explore the role of firms in the displacement gap in more detail in Section 4.1, we first want to exclude the possibility that our estimates are driven entirely by migrants and natives working at (and being laid off from) different firms.

We re-estimate the earnings, wage, and employment outcomes for migrants and natives laid off from the same establishment, in the same 3-digit occupation, and plot them in Figure 2.[11] We find that the overall negative earnings gap replicates: migrants' earnings are significantly lower following a job displacement compared to natives. The gap closes more slowly within this sample, by year 5 instead of year 3 in the full sample. It is primarily driven by a persistently negative gap in re-employment. However, we also find a persistently negative gap in wages conditional on employment. This suggests that while the overall finding of a negative migrant-native gap in outcomes post-displacement holds, the positive wage gap we find by year 5 in the full sample is driven by the displaced migrants and natives working in different firms pre-displacement.

### 3.4 Robustness

**Robustness of baseline estimates** We conduct several checks to test the robustness of our baseline earnings, wage, and employment gaps. Their results are summarized in Table 6. Overall, we find that our results replicate for a variety of alternative matching variables as well as for a longer time frame. We conduct robustness checks using our weighted difference-in-differences specification which estimates the cumulative cost of job displacement 5 years after the event. Column (1) summarizes our baseline estimate of significant negative cumulative gaps in earnings, employment, and wages.[12]

**Baseline Tenure Restrictions** Our baseline analysis focuses on a sample of workers who are highly attached to the labor market (3 years of tenure). This could bias the migrant-native gap if high-tenure migrants are particularly well-integrated into the German labor market, and their re-employment probability is thus higher than that of other migrants. In this case, we would underestimate the gap. In Columns (2) and (3) of Table 6, we, therefore, relax the tenure restriction to 1 and 2 years, respectively. The estimates of the earnings and employment migrant-native gap stay virtually unchanged; the wage gap becomes statistically insignificant from 0 when we set tenure to 2 years but remains negative for 1-year tenure requirement. We also show that our findings are robust to a related concept of tenure length in the German labor market (rather than in the layoff

[11] See Appendix Section A for more detail on how we construct the sample.

[12] While we find a diminishing-to-positive wage gap in the event study, the cumulative wage loss for migrants is still greater than for natives.

firm). We plot, in Figure B10, the cumulative cost of displacement by the number of years a worker has been recorded for in the administrative data. We find that the gap between migrants and natives is invariant to overall tenure in the labor market.

**Propensity Score Matching** In the next 3 columns, (4) - (6), we vary the set of variables we use in the propensity score matching (to find a counterfactual for each displaced worker). We exclude pre-displacement wages from the matching set; we match on values in period $t-4$ only instead of the combination of several years we use at the baseline; and we match on counties instead of 1-digit industries. The estimate for the earnings gap varies somewhat in terms of its magnitude, but it remains significantly negative (not matching on wages produces an especially large migrant-native earnings gap). The estimates for wages and employment are robust both in terms of their sign and magnitude.

**Time Window** In column (7), we extend the time frame of our study to 10 years after the displacement. While we lose some of our observations as a result (both because of attrition and because some workers were laid off towards the end of our time frame), this robustness check allows us to confirm whether the observed patterns persist beyond the initial 5-year window. We find that, in line with the baseline patterns, the wage gap becomes statistically indistinguishable from 0 as faster wage growth of migrants makes up for the initial negative gap. However, the gap in migrants' re-employment probability remains depressed even 10 years after the job loss: it decreases from 3.9% after 5 years to 2.8% after 10 years, remaining significantly negative.

**Reweighting** In column (8), we invert the re-weighting procedure and reweight natives to resemble migrants. This is essentially a test for our reweighting algorithm: it should yield similar results, regardless of the direction of reweighting. To show this, we use the same reweighting algorithm as described in Section 3.1. The only difference is that instead of a dummy for native workers as an outcome variable in our probit regression, we now regress a dummy for *migrant* workers on a set of pre-displacement individual characteristics and establishment characteristics. The resulting coefficients are very comparable to the baseline estimates, except for the wage gap, which more than doubles in size.

**Financial Crisis, East Germany** Migrants particularly suffer during recessions, so the financial crisis, which is included in our period of analysis, may bias our results in the direction of particularly large earnings losses for migrants. We thus estimate Equation 1 only for baseline years up to 2007, ensuring that none of the workers in our analysis sample lose their jobs during the financial crisis. Reassuringly, Table B7 in the Appendix shows that our results are robust to excluding the financial crisis years. Migrants displaced in 2001-2007 face substantially larger earnings losses (columns (1) and (2)), wage losses (columns(3) and (4)), employment losses (columns (5) and (6)), and losses in yearly days worked (columns (7) and (8)) than native workers.[13] Similarly, we run a robustness check where we exclude workers displaced from East German establishments from our sample. Our observation period starts only six years after German reunification and covers a time when East Germany underwent major economic transitions. This could lead to different displacement effects for workers in East Germany, and for migrants in East Germany, reintegration into the labor market could be particularly difficult. Reassuringly, our results are robust to estimating our regression based on a sample of workers displaced in West Germany only (Appendix Table B9).

**Return migration** A key difference between displaced natives and migrants is that migrants are more readily able to move out of Germany. This is likely to lead to a negative out-selection from our sample as the least successful migrants return to their home countries, but it might also lead to a loss of some of the ablest migrants who, after experiencing a negative shock in Germany, choose to move to another destination. Our baseline estimates thus may both under and over-estimate the true extent of the migrant-native gap in displacement costs.

We do not observe return (or other) emigration in our data, but we can compare attrition rates across migrants and natives after displacement. We plot the difference in Figure B3, where the first coefficient represents the probability a migrant leaves the sample, compared to a native. We find that the probability of leaving the sample is significantly higher for migrants (8%) than for natives (5%). As explained above, while these patterns are in line with the return migration of migrants, they may also be driven by e.g. a greater propensity to drop out of the labor force.[14]

As a second robustness check, we re-estimate our baseline costs of displacement for the full sample of displaced migrants, including those who later disappear from the administrative data. Since we don't observe their outcomes, we assume that they remain unemployed throughout (and their wages and earnings are hence 0). As a result, we are overestimating migrants' job loss compared to natives, providing a lower bound estimate. We present these plots in Figure B5 in the Appendix. They show that our results are robust to return migration. We find the same qualitative pattern of a persistent negative employment gap, and a decreasing-to-positive wage gap for displacement loss of migrants compared to natives. The overall gap in earnings still disappears by the end of our 5-year horizon. Quantitatively, the initially estimated gaps are very similar, but they close more slowly when (potential) return migrants are included - driven by our conservative assumption that return

[13] Since our post-job-loss period spans five years, restricting the observation period to 2007, the year before the financial crisis, could not suffice - the crisis could also have affected job search success in the post displacement years. We therefore run an additional robustness check, where we only include matched worker pairs with baseline years up to 2003 in our sample (see Table B8 in the Appendix). The resulting patterns are very similar to our main results: Migrants face larger earnings and employment losses.

[14] Table B4 shows that both migrants and natives who drop out of the sample are positively selected: They earn slightly higher wages than workers who remain in our sample. They are also older and worked somewhat fewer days per year before displacement. It is reassuring that the direction of the selection of drop-outs does not differ by migration status.

migrants would remain unemployed throughout. This also drives the fact that the unweighted and weighted migrant estimates are more similar in Figure B5 than in our baseline results.

Finally, since the baseline sample includes dropouts until they leave the register data, we restrict our sample and remove the dropouts entirely. We again re-estimate our baseline event study specification for this sample and plot the results in Figure B6. It shows that our baseline results are robust to this exclusion.

# 4 Mechanisms

In the previous section, we have shown that after an exogenous job loss, migrants and natives experience significantly different employment and wage trajectories even when we control for the observable differences between the groups. Migrants are significantly more likely to remain unemployed but conditional on finding a job, their wages grow relatively faster than natives'. In this section, we explore several potential mechanisms driving this result. Overall, our analysis suggests that the differential impact of job loss on migrants compared to natives is primarily driven by their circumstances – such as the usefulness of their networks and the conditions in the labor market – rather than by their mobility or search behavior.

## 4.1 Firm quality

We start by examining the role of workers sorting into firms of different quality. Our point of departure is the rich literature documenting the importance of firms in explaining wage heterogeneity between otherwise similar workers (Card, Cardoso, Heining, and Kline, 2018); the observation that climbing the "firms ladder" is a major driver in wage growth of an individual (Topel and Ward, 1992); and the evidence that interruptions to this process, such as job displacement, have long-term consequences on workers' labor market outcomes precisely because the affected worker struggles to find re-employment at high-wage firms (Schmieder et al., 2023).

In light of this literature, we consider the possibility that the observed gaps in displacement outcomes between migrants and workers are the consequence of differential sorting into firms pre-displacement, and the subsequent "re-shuffling" caused by the displacement. The migrant-native gap would then be given "mechanically" by this sorting process. If, for example, there is strong positive assortative matching between workers and firms, any gap between migrants and natives that existed before the mass layoff would carry through to their post-layoff firm placements. If the sorting process is for any reason less efficient for migrants, we would observe a widening gap between migrants and natives after the displacement. On the other hand, if the sorting of similar workers across firms is mostly driven by luck, job displacement will result in a reversion to the mean in worker-firm matches: the workers who were lucky before the displacement are less likely to repeat their lucky draw, whereas the workers previously working at lower-quality firms benefit from the push to draw again. If native workers are in general more likely to be employed at higher-quality firms – perhaps because they have longer labor market histories in Germany than migrants – they will lose out relatively more from the displacement, and we would expect their post-displacement wages to be lower than those of migrants.

Our estimates of post-displacement wages from Section 3 paint a mixed picture. While migrants earn relatively lower wages in their first re-employment spell, their wages grow significantly faster, so the migrant-native displacement gap in wages turns positive by year 4.

Table 2, Panels A and B, provide additional evidence for this pattern. Column (2) shows that migrants who are comparable to natives earn substantially lower market entry wages, but experience significantly faster wage growth than natives. This wage growth is higher for migrants both within and between firms: In Panel B, column (4), we show that wage growth is almost five times as large for migrants who switch between employers, but it is also 1.2 pp higher for migrants who stay with the same employer than for natives.

To analyze the role of firm sorting – and luck – more formally, we use the firm fixed effect estimates available in the administrative data and analyze how they change post-displacement for migrants and natives.[15] These results are summarized in the bottom two panels of Table 2, where we replicate our analysis for entry wages and wage growth for firm fixed effects. In Panel C of the table, we show that, once we apply the re-weighting to make the two groups observationally comparable, firm FE in the first post-displacement job is only weakly lower for migrants compared to natives. This means that differences in sorting into firms cannot fully explain the statistically significant difference in starting wages between the two groups.

This finding is further supported by the results for subsequent wage growth, examined in Panel D. We observe no significant differences in the growth of firm quality as measured by fixed effects. Importantly, this holds when we look separately at job-stayers and job-switchers, which we found (in Panel B) to be driving the relative wage growth of migrants post-displacement.

An alternative way to demonstrate the weak relevance of firm quality on the relative migrant-native post-displacement outcomes is to replicate our baseline event study for firm fixed effects and compare them to the trajectory of wages. We present these results in Panel (a) of Figure B2 in the Appendix. It shows that, while both migrants and natives experience a drop in their firm quality after the displacement, this drop is the same for both groups and doesn't follow the pattern of wages. As a result, the change in firm FE cannot explain the differences in post-displacement wages of migrants and natives. We elaborate on this point further in Panel (b) of the same figure, which plots the re-employment firm FE as a function of pre-displacement firm FE, separately for migrants and natives. We document a strong negative relationship – workers from high-quality pre-displacement firms lose out and vice versa –, but the two lines overlap almost perfectly.

[15]See Appendix Section A for an overview on how the AKM data is constructed

To summarize, we find that an exogenous employment shock leads to a strong reassignment of workers across firms, and most workers lose out in this process (highlighting the role of luck in pay determination). However, this process affects migrants and natives in the same way, and thus it cannot explain the significant difference in the cost of displacement.

## 4.2 Job search

In this section, we turn to the role of workers' job search behavior. If migrants look for different jobs, or if they search differently from natives, we would expect the two groups to have different labor market outcomes. To answer this question, we make use of additional data on the job search preferences and objectives of UI benefit recipients.[16]

One caveat in using this data is that not all displaced workers receive UI benefits, primarily because not all displaced workers become unemployed. As a result, the search patterns analyzed here describe the more negatively selected displaced workers who likely struggled to find employment in the first place. Importantly, however, the second coefficient in Figure B3 in the Appendix shows that after conditioning on the observables, migrants and natives are equally likely to ever receive UI benefits and thus to sort into this sample.

The UI benefit recipient data collects workers' stated preferences and objectives as recorded by their caseworker at the employment office at the start of their unemployment spell. It contains rich information on their target occupations, whether they are looking for permanent or fixed-term positions, full- or part-time. The unemployed also signal the geographic scope of their search. In Figure B3, we show that migrants and natives differ only marginally in their search objectives. The two groups are equally likely to search for full-time positions, and migrants do not search across larger geographic areas. Migrants are significantly more likely to search for temporary positions, and they also search across a somewhat broader set of occupations. If anything, however, these occupations are paid on average more than their core 3-digit occupation.

To test whether these differences can explain the gap in outcomes, or whether the same search behavior has different returns for migrants relative to natives, we regress the individual-level difference-in-differences estimate for employment and wages on the worker's search preferences. The results are summarized in Table 3. Column (1) estimates the baseline migrant-native gap. In columns (2) - (6), we test each measure individually, and in column (7) we add all measures jointly. We find that the negative coefficient on the migrant dummy for both wages and employment is unaffected by the inclusion of search preference variables, and none of these variables are statistically significant.

Overall, while we do find some systematic differences in migrants' and natives' search objectives and preferences, these differences cannot explain any of the differences in wages and employment between the two groups.

[16]See Appendix Section A for more details on the data

## 4.3 Networks

The fact that migrants and natives are relatively similar in what they're searching for doesn't mean they search in the same way. While we do not possess any data on workers' search efforts or activity, the administrative data makes it possible to construct proxy measures of their social networks. Given the prominence of social connections in job search (Dustmann, Glitz, Schönberg, and Brücker, 2015; Glitz, 2017; Saygin, Weber, and Weynandt, 2021), we might expect differences in social networks to translate into gaps in labor market outcomes post displacement.

We measure workers' networks at two levels: at the workplace, and in the place of residence. At the workplace, we calculate the share of co-workers from the same region of origin (see Appendix A for the details on origin groups) within the establishment and the same 3-digit occupation – and thus essentially within the same team – in the year before the mass layoff. At the place of residence, we calculate the share of the working-age population of the same migration status. If we assume that similar individuals are more likely to form a network, we would expect migrants to benefit from higher shares of other migrants in their workplace or neighborhood, and inversely natives to benefit from lower migrant or ethnic shares.[17]

To estimate the impact of a worker's network on the migrant-native outcomes gap, we follow the same methodology as for job search preferences above: we regress the difference-in-differences estimate of wages and employment on a migrant dummy and our measures of the social network. To reflect the fact that migrants' and natives' networks are complements, we also include an interaction term between the migrant dummy and the size of the network.

We summarize the results of our analysis in Table 4. In Panel A of the table, we focus on social networks as proxied by the migrant status of co-workers. In all three regressions – for earnings, employment, and wages – we find that a higher share of migrant co-workers has a negative effect on the labor market outcomes for all workers. This suggests that firms which employ a larger share of migrants are significantly different from those which employ primarily natives, either because of different worker sorting across these firms or because of firm characteristics as such. However, once we control for this baseline difference, the coefficient on the interaction between the migrant share and the migrant dummy can be interpreted as the differential impact of the same co-worker network on migrants relative to natives.

We find that contrary to expectations, a higher migrant share within an establishment weakly *worsens* a migrant's labor market outcomes compared to natives. The coefficient on the interaction term is negative and insignificant for employment, and negative and statistically significant for

[17]Our measure focuses on the share of same-origin workers in the same firm, rather than looking at the shares of same-origin past colleagues at other firms as in some prior literature (e.g. Glitz (2017)). Our reason for this approach is twofold. First, since a mass layoff is defined as a 30% drop in firm-level employment, our measure will capture the potential size of the remaining network for an unemployed individual. Second, our focus on migrants means that, for a non-negligible share of individuals, the layoff follows the first employment spell in Germany. These individuals may not have been in the system for long enough to have a significant network of past colleagues. Using current colleagues thus allows us to compare like for like for migrants and natives.

wages. This suggests that for migrants, any benefits from having a larger network are weakly outweighed by the usefulness of that network for job search.

In Panels B and C of Table 4, we turn our attention to networks based on the worker's location of residence. We split the sample into two groups based on whether the individual lives in an area with above- or below-median share of individuals with the same origin group (migrant or native). We then regress individual cost of displacement on migrant dummy. This allows us to compare whether the cost of displacement in terms of earnings, employment and wages is higher or lower in areas with larger individual-specific networks, and whether that pattern varies for migrants vs natives.

We find that, similar to workplace-based networks, residence-based networks also worsen relative migrant outcomes after a displacement. Migrants living in migrant enclaves experience significantly greater loss in earnings, wages, and employment compared to migrants living in predominantly native areas. In the case of wages, for example, there is no migrant-native post-displacement gap in majority native areas, but migrants do earn 8.4% less due to the displacement when they reside in areas with above-median migrant shares.

An alternative interpretation of these results is that migrants and natives reside in different areas, and it is the different labor market conditions, rather than network as such, that drive these differences in displacement costs. This explanation is particularly plausible because we don't observe individuals' networks directly and instead only measure them through a proxy. If, for example, migrants tend to cluster in worse labor markets (perhaps because housing is cheaper), and the displacement cost is higher under worse local conditions, we would also find that the displacement gap is larger in areas with high migrant share. To address this point, we add controls for county fixed effects, so that we effectively compare individuals with different same-origin shares living in the same location. The results are presented in columns (2), (4) and (6). We find that the impact on migrants becomes somewhat larger, not smaller, suggesting that the negative impact of ethnic enclaves cannot be explained by the differential labor market conditions since migrants tend to settle in better-performing labor markets on average.

The combination of these results means that a large fraction of the migrant-native displacement gap is driven by the worse social networks of migrants living in ethnic enclaves. This result is somewhat surprising if we assume that migrants living in ethnic enclaves have larger networks than natives living in the same location, suggesting that the quality of the network matters more than its size for labor market outcomes. Given that, as we have demonstrated in Section 3 of this paper, migrants find it harder to find well-paying jobs, or to search for jobs in general, these difficulties will transmit through the social network and worsen the employment chances of other migrants negatively too.

## 4.4 Labor market conditions

As a final potential mechanism, we explore the role of labor market conditions: The lower re-employment chances and lower (initial) pay may be explained by migrants facing a worse work environment than natives. This might occur for two reasons: migrants settling in local labor markets with worse opportunities, or migrants being less able to take advantage of these opportunities. In this section, we test for the latter mechanism by comparing the cost of displacement for migrants and natives living in the same type of local labor market. In the next section, we look explicitly at whether migrants and natives differ in where they reside.

We capture the state of a local labor market by looking at its tightness, defined as the log ratio of vacancies to unemployed job-seekers. We calculate the labor market tightness of all German counties for every month in our dataset. The relevant measure for a displaced worker is the relative tightness in his local labor market at the time of displacement, which we measure as the county's decile in that month's tightness distribution. We then test whether the migrant-native displacement gap varies across different local labor markets by re-estimating the migrant-native gap in outcomes for annual earnings, employment probabilities, and wages by decile of labor market tightness (measured in t=-1). In other words, we compare the outcomes for natives and workers displaced in counties in the same decile of tightness distribution.

We plot these gaps in Panels (a) - (c) of Figure 4. The main pattern across all panels is that the migrant-native gap significantly declines in labor market tightness. The gap in earnings is largest, almost 100%, for the counties in the lowest decile of market tightness. The gap closes quickly as labor market tightness increases; it becomes statistically indistinguishable from 0 for counties above the 4th tightness decile. We observe the same steep pattern of convergence for wages. For employment, the gap is relatively smaller in the least tight counties and remains more constant across the tightness distribution, although here too it becomes statistically insignificant for counties at the 4th decile. Importantly, these patterns are entirely driven by migrants struggling relatively more in slack local labor markets: the cost of displacement for natives is the same in all deciles of labor market tightness.

Overall, our findings demonstrate that local labor market conditions play a significant role in migrants' labor market experience compared to natives. There are several mechanisms that could explain these findings. The migrants laid off in slack labor markets might have different unobservable ability to the migrants laid off when market tightness is high; a tighter labor market might make it easier to search formally, without the use of social networks, which would benefit migrants more; or it might lessen discrimination against migrants by making it more difficult (costly) to only employ natives. While our data doesn't allow us to cleanly distinguish between these channels, we can dig deeper by looking at whether firms hire different workers as labor market tightness varies.

To do this, we re-visit our results on firm AKM fixed effects by estimating the migrant-native gap in post-displacement firm FE at different levels of labor market tightness. These estimates are plotted in Panel (d) of Figure 4. We find that the firm FE – tightness relationship is relatively flat for natives, but upward-sloping for migrants, so migrant workers are more likely to be matched with better-quality firms in tighter labor markets. These results are consistent with the idea that labor market discrimination is more costly in tighter markets, but it is also possible that hiring into high-pay firms becomes less dependent on networks when tightness is high. Of course, these two explanations are not mutually exclusive – recruitment via networks can be one way to discriminate against migrants.

### 4.5 Geographic mobility

So far, our analysis of the various mechanisms suggests that the differential impact of job loss on migrants compared to natives is primarily driven by their circumstances – such as the usefulness of their networks and the situation in their local labor market – rather than by their search behavior. However, the fact that workers' circumstances are tightly linked to their local labor market raises the question of how much of the migrant-native outcome gap is driven – or alleviated – by geographic mobility. As we've shown, migrants do better in areas with higher labor market tightness and lower shares of migrants; if migrants actively move to such areas following a job displacement, the observed migrant-native outcome gap would be under-estimating the true extent of the initial disparities between migrants and natives. In this respect, mobility would also be an important *mechanism* for reducing the migrant-native gap, changing our interpretation of the estimated role of initial networks and labor market conditions.

To examine migrants' mobility patterns, we compare the change in mobility after displacement between migrant and natives. We start by looking at workplace mobility between German states in column (1) of Table 5. We find that, conditional on finding a new job, displaced migrants are weakly less likely to move than displaced natives. In column (2), we additionally control for the initial distribution of migrants and natives across German counties by including baseline county fixed effects. The estimated difference in mobility rates more than halves and becomes insignificant.

The measure of cross-state mobility in columns (1) and (2) is only available for workplace moves in our data, such that it also reflects the migrant-native gap in the take-up of employment after job loss. To include the unemployed in our analysis, we next look at mobility across job agency branches, which are recorded for all individuals in the register data.[18] We repeat our mobility regressions for moving across job agency branches in columns (3) and (4). In line with the literature on the estimated differences in mobility between migrants and natives (Borjas, 2001; Cadena and Kovak, 2016; Basso, D'Amuri, and Peri, 2019; Schündeln, 2014), we find that migrants are somewhat more mobile than natives (1.6ppt) across the smaller geographic units of job agency branches.

[18] Germany has about 1000 job agency branches, which is slightly less more than counties (401) and federal states (16). Job agencies are responsible for recipients of *Arbeitslosengeld 1*, which individuals receive in their first year of unemployment.

The actual impact of mobility on the migrant-native displacement gap depends on where migrants reside and where they move to. Looking at the descriptive statistics in Table 1, we see that at the baseline, migrants are more likely to reside in weakly better local labor markets than natives. This is true for both migrants and natives overall, as well as for our sample of displaced workers: Migrants and natives experience on average the same labor market tightness, 80% of displaced migrants live in cities (compared to 44% of natives), and they are significantly less likely to live in East Germany.

To understand whether migrants' greater mobility hinfers or further improves their labor market conditions, we compare the difference between their average pre- and post-displacement labor market tightness to the matched counterfactual. The resulting dependent variable thus tells us how the post-displacement market tightness change relative to counterfactual non-displaced workers. The results are presented in the last two columns of Table 5. We find no statistically significant differences between displaced migrants and natives. This suggests that, despite their marginally higher overall mobility, displaced migrants are not significantly more likely to relocate to regions with better labor market conditions.

There are two different ways to interpret these results. On the one hand, one could argue that mobility doesn't explain the observed migrant-native outcome gap post-displacement because the mobility patterns of the two groups are almost identical: while migrants are somewhat more mobile on average, they do not move to systematically better regions of Germany. On the other hand, the *lack* of migrant mobility does contribute to the gap in outcomes because we find that migrants are more sensitive to their local labor market conditions than natives. If displaced migrants relocated to commuting zones with smaller ethnic enclaves and higher market tightness, the migrant-native gap in outcomes would be smaller.

## 4.6 Understanding within-migrant differences

So far, we have focused on explaining the difference between migrants and natives. The persistent negative gap in employment, but a diminishing to positive gap in wages suggests that post-displacement outcomes vary widely between those migrants who managed to find re-employment and those who do not. As the last step of our analysis, we therefore look at whether there are any systematic differences between these two groups of migrants.

To do this, we make use of worker AKM fixed effects estimated for the whole population of workers before the displacement (for more detail on the calculation of worker FE, see Appendix Section A). While the presence of labor market discrimination makes it impossible to use worker FE for an unbiased comparison of "quality" between migrants and natives, we argue that insofar as all migrants are subject to the same overall level of discrimination, worker FE *can* be used to make within-group comparisons.

We split the sample of displaced migrants into two groups based on whether their individual fixed effect lies below or above the (migrant) median, and then re-estimate our baseline event study separately for these two groups. These estimates, plotted in Figure 3, show that there is a stark difference in outcomes based on migrants' unobserved quality. The migrant-native displacement gap in overall earnings is almost entirely driven by below-median migrants: the displacement gap in earnings of above-average migrants is the same as that for native workers. The same holds for employment and the number of days worked, where it's the below-median migrants who drive most of the initial gap in employment probabilities, as well as the gradual convergence towards the natives. The picture is somewhat different for wages: while the entire initial gap is again driven by below-median migrants, the subsequent wage growth is shared by all migrants. As a result, the below-median migrants significantly reduce their negative wage gap compared to natives, and the above-average migrants earn relatively more than natives (i.e. their displacement gap in wages is smaller) across all periods, almost catching up with their pre-displacement wages by year 5.[19]

These results imply a strong persistence in migrant outcomes. This has important implications for inequality both within the migrant group and between migrants and natives. The above-median migrants return to their pre-displacement outcomes within 5 years, but below-median migrants experience persistently lower employment and pay that goes beyond this time frame. As a consequence, we see that exogenous job loss exacerbates the inequality between migrants, with below-median migrants falling even further behind. At the same time, these results mark a clear way to address the migrant-native outcomes gap. Instead of trying to help all migrants to become more like natives, the policymakers should focus on interventions that allow below-median migrants to become more similar to above-median migrants, who already navigate the German labor market successfully.[20]

# 5 Conclusion

In this paper, we use the aftermath of mass layoffs as a laboratory to study the assimilation of migrants into the German labor market. By following native and migrant workers after job displacement, we can observe how the two groups navigate the labor market under near-identical

[19]In Figure B8, we additionally split natives into above- and below-median worker FE (within the native distribution). This comparison shows that (i) the gap in post-displacement outcomes between above- and below-median workers is similarly large for natives and migrants, and (ii) above-median migrants fare as well as below-median natives.

[20]We find that the cost of displacement differs between migrants of different origin groups. In Figure B9, we plot the cumulative cost of displacement (from our difference-in-differences estimates) in earnings, employment and wages by 8 main origin groups (and and "other" category). We find significant heterogeneity in post-layoff outcomes by migrants' region of origin. Migrants from former USSR republics recover better than natives, recording a *positive* gap in earnings and employment. Workers from the rest of Europe and Central and South America do only marginally worse compared to natives. Migrants from developing countries (Turkey, Africa and Asia and Middle East) on the other hand, suffer more than natives, especially due to the large negative gap in wages.

circumstances. The fact that the displaced migrants were employed for at least 3 years in a large firm suggests they were already relatively integrated into the labor market, making the two groups even more comparable.

Nevertheless, we find that an exogenous employment shock affects migrants and natives very differently. Compared to natives, migrants struggle to find employment more than 5 years after the mass layoff. At the same time, however, the migrants that succeed in finding a job experience a faster wage growth than natives, so that by year 5 the migrant-native gap in wages is positive.

We examine several potential drivers of these gaps. We find that they seem to be driven by workers' circumstances rather than their direct behavior. We find little difference in job search and geographic mobility, but a significant role of social networks, especially at the residence level. We also find that the migrant-native gap is driven by labor markets with relatively low labor market tightness, suggesting a potentially large role in labor market discrimination.

We believe this analysis offers insights about the assimilation process more generally, both by highlighting how idiosyncratic shocks can slow down – or reverse – labor market integration, and by shedding light on its key mechanisms. Our results suggest that policymakers wishing to aid the economic integration of migrants should focus on helping migrants with job search, given their weaker social networks, and on strengthening anti-discrimination legislation. Moving subsidies would make it easier for migrants to relocate to more favorable labor markets. Alternatively, the policy could draw on the experience of the migrants who already navigate the post-displacement labor market more successfully than natives.

An important contribution of this paper is that, unlike the existing studies, we systematically control for the differences in migrant-native characteristics throughout our analysis. As a result, we complement the findings emphasizing the importance of migrants catching up to natives in terms of their skills, education, experience, and language skills by identifying the barriers to assimilation that remain after such a convergence. Given the increasing shift by developed countries toward attracting skilled, highly-educated immigrants, this type of assimilation is likely to become more and more relevant.

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# 6 Tables

Table 1: Displaced Worker Characteristics in t=-1

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Migrants | | | Natives | |
| | Random 2-percent | Baseline | Weighted | Random 2-percent | Baseline |
| **Panel A: Individual Characteristics** | | | | | |
| Years of Education | 11.2 | 11.2 | 12.4 | 12.0 | 12.3 |
| | [2.05] | [1.61] | [2.12] | [1.94] | [1.77] |
| Age | 37.8 | 37.9 | 39.1 | 40.4 | 39.4 |
| | [12.5] | [6.68] | [6.57] | [13.3] | [6.71] |
| Tenure | 2.37 | 6.38 | 6.12 | 2.93 | 6.21 |
| | [2.07] | [2.57] | [2.41] | [2.17] | [2.43] |
| Real Daily Wage | 57.5 | 89.3 | 102.7 | 68.7 | 102.3 |
| | [48.8] | [30.8] | [37.2] | [53.0] | [36.8] |
| Total Yearly Earnings | 13620.3 | 30208.5 | 35034.1 | 20661.7 | 35479.6 |
| | [16493.5] | [11829.3] | [14100.7] | [18855.8] | [14191.0] |
| Days per year working | 214.8 | 335.7 | 338.0 | 281.9 | 344.3 |
| | [158.6] | [53.7] | [51.5] | [135.1] | [45.6] |
| **Panel B: Regional Characteristics** | | | | | |
| Lives in City | 0.64 | 0.80 | 0.65 | 0.44 | 0.57 |
| | [0.48] | [0.40] | [0.48] | [0.50] | [0.50] |
| Lives in East Germany | 0.063 | 0.042 | 0.059 | 0.19 | 0.25 |
| | [0.24] | [0.20] | [0.24] | [0.39] | [0.43] |
| Tightness | . | 0.15 | 0.16 | . | 0.15 |
| | | [0.084] | [0.082] | | [0.081] |
| **Panel C: Establishment Characteristics** | | | | | |
| Size of Establishment | 1000.3 | 290.8 | 292.2 | 782.1 | 344.1 |
| | [3922.8] | [490.1] | [549.2] | [3473.1] | [634.3] |
| Share Migrant Workers | 0.30 | 0.24 | 0.19 | 0.053 | 0.074 |
| | [0.27] | [0.19] | [0.18] | [0.086] | [0.095] |
| Share High-Skilled Workers | 0.099 | 0.079 | 0.13 | 0.13 | 0.12 |
| | [0.16] | [0.12] | [0.18] | [0.17] | [0.16] |
| Share Marginally Employed Workers | 0.21 | 0.059 | 0.051 | 0.17 | 0.041 |
| | [0.28] | [0.13] | [0.11] | [0.26] | [0.095] |
| Displaced from Complete Closure | . | 0.32 | 0.32 | . | 0.32 |
| | | [0.47] | [0.47] | | [0.46] |
| Number of Observations | 574167 | 17503 | 17214 | 5882551 | 128744 |

**Notes:** This table summarizes characteristics of different samples of (displaced) migrants and natives. Columns (1) and (4) show the characteristics of a random 2-% sample of all workers subject to social security employment in Germany from 2000-2010, stemming from the Sample of Integrated Employment Biographies (SIAB). Columns (2) and (5) represent all displaced workers in our baseline sample (i.e. workers who are matched to a non-displaced control worker and who fulfill the baseline restrictions). They are part of our 12.5% random draw of workers from 1997-2016. Column (3) reports migrants in the baseline sample reweighted to natives, and trimmed at the 99th percentile of the propensity score. We measure characteristics in the year before displacement (t=-1). Standard deviations in brackets.

Table 2: Entry Wages and Wage Growth

| | (1) No Weights | (2) Weights | (3) Stayers | (4) Movers |
|---|---|---|---|---|
| **Panel A:** Entry Wage Gap | | | | |
| Migrant | -0.043*** | -0.023*** | -0.0098*** | -0.031*** |
| | (0.0029) | (0.0027) | (0.0036) | (0.0036) |
| Observations | 132019 | 132019 | 51530 | 78633 |
| $R^2$ | 0.120 | 0.189 | 0.211 | 0.262 |
| Mean Dep. Var (Native) | -0.053 | -0.053 | -0.035 | -0.065 |
| **Panel B:** Wage Growth | | | | |
| Migrant | 0.055*** | 0.032*** | 0.012*** | 0.051*** |
| | (0.011) | (0.0097) | (0.0044) | (0.016) |
| Observations | 114453 | 114453 | 49654 | 62966 |
| $R^2$ | 0.075 | 0.076 | 0.340 | 0.117 |
| Mean Dep. Var (Native) | 0.065 | 0.065 | 0.029 | 0.095 |
| **Panel C:** Entry Gap in AKM Estab FE | | | | |
| Migrant | -0.22** | -0.17* | 0.068 | -0.30*** |
| | (0.090) | (0.091) | (0.063) | (0.097) |
| Observations | 129321 | 129321 | 49330 | 78146 |
| $R^2$ | 0.225 | 0.227 | 0.254 | 0.293 |
| Mean Dep. Var (Native) | -0.64 | -0.64 | -0.33 | -0.85 |
| **Panel D:** Growth in AKM Estab FE | | | | |
| Migrant | -0.25 | 0.13 | -0.25 | 0.87 |
| | (0.84) | (0.52) | (0.19) | (1.27) |
| Observations | 110502 | 110502 | 48280 | 60399 |
| $R^2$ | 0.061 | 0.073 | 0.114 | 0.142 |
| Mean Dep. Var (Native) | -0.42 | -0.42 | -0.70 | -0.19 |

**Notes:** This table reports the migrant-native gap on different wage and AKM establishment fixed effects outcomes. All regressions control for displacement establishment and year FE. We cluster standard errors at the displacement establishment level. The entry Wage Gap is the gap between a displaced worker's first post-displacement wage and his wage in the year before displacement ('baseline wage'), as a share of the baseline wage. Wage Growth is the gap between a displaced worker's last observed wage post-displacement and his first observed wage post-displacement, as a share of the first observed wage. Entry AKM Estab. FE Gap is the gap between a displaced worker's first post-displacement establishment wage premium and his establishment wage premium in the year before displacement ('baseline establishment premium'), as a share of the baseline establishment wage premium. AKM Estab. FE Change is the gap between a displaced worker's last observed AKM establishment FE post-displacement and his first observed AKM establishment FE post-displacement, as a share of the first observed AKM establishment FE. Column (1) reports coefficients without weighting, column (2) reports coefficients when reweighting migrants to natives as described in Section 3. In column (3), stayers are workers where the first wage establishment is the same as the last wage establishment. In column (4), movers are workers who switch establishments post-displacement. ***, **, and * refer to statistical significance at the 1, 5, and 10 percent levels, respectively. Workers in our sample are displaced in 2001-2011, and they are observed from 1996 to 2017.

Table 3: Explaining the Gap in Employment and Wage Losses by Search Behavior

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| **Panel A:** Employment | | | | | | | |
| Migrant | -0.025*** | -0.025*** | -0.025*** | -0.025*** | -0.025*** | -0.025*** | -0.025*** |
| | (0.0088) | (0.0088) | (0.0088) | (0.0088) | (0.0089) | (0.0088) | (0.0090) |
| Better Wage Rank | | 0.00084 | | | | | -0.0015 |
| | | (0.012) | | | | | (0.013) |
| Same Wage Rank | | -0.0012 | | | | | 0.0024 |
| | | (0.011) | | | | | (0.017) |
| Searching for Different 3-Digit Occupation | | | 0.00027 | | | | 0.0038 |
| | | | (0.0087) | | | | (0.015) |
| Searching within Daily Commuting Distance | | | | 0.00072 | | | 0.0014 |
| | | | | (0.052) | | | (0.052) |
| Searching for Any Employment Contract | | | | | 0.0032 | | -0.0084 |
| | | | | | (0.021) | | (0.024) |
| Searching for Full-time or Part-time Contract | | | | | | 0.073 | 0.076 |
| | | | | | | (0.058) | (0.058) |
| Searching for Full-time Contract | | | | | | 0.039 | 0.043 |
| | | | | | | (0.057) | (0.057) |
| Observations | 23347 | 23347 | 23347 | 23347 | 23347 | 23347 | 23347 |
| $R^2$ | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.003 |
| Mean Dep. Var (Native) | -0.096 | -0.096 | -0.096 | -0.096 | -0.096 | -0.096 | -0.096 |
| **Panel B:** Log Wages | | | | | | | |
| Migrant | -0.066*** | -0.066*** | -0.066*** | -0.066*** | -0.066*** | -0.066*** | -0.066*** |
| | (0.023) | (0.022) | (0.023) | (0.022) | (0.023) | (0.023) | (0.023) |
| Better Wage Rank | | 0.051 | | | | | 0.061 |
| | | (0.037) | | | | | (0.042) |
| Same Wage Rank | | 0.030 | | | | | 0.046 |
| | | (0.032) | | | | | (0.059) |
| Searching for Different 3-digit Occupation | | | 0.010 | | | | 0.021 |
| | | | (0.024) | | | | (0.051) |
| Searching within Daily Commuting Distance | | | | -0.086 | | | -0.093 |
| | | | | (0.14) | | | (0.14) |
| Searching for Any Employment Contract | | | | | 0.021 | | -0.011 |
| | | | | | (0.045) | | (0.050) |
| Searching for Full-time or Part-time Contract | | | | | | 0.17 | 0.18 |
| | | | | | | (0.13) | (0.13) |
| Searching for Full-time Contract | | | | | | 0.16 | 0.16 |
| | | | | | | (0.12) | (0.12) |
| Observations | 21606 | 21606 | 21606 | 21606 | 21606 | 21606 | 21606 |
| $R^2$ | 0.002 | 0.003 | 0.002 | 0.004 | 0.002 | 0.002 | 0.005 |
| Mean Dep. Var (Native) | -0.19 | -0.19 | -0.19 | -0.19 | -0.19 | -0.19 | -0.19 |

**Notes:** This table shows the role of various search outcomes in explaining the migrant-native gap in employment and wage losses after job displacement. Each column consecutively adds new search preferences as controls. The outcome variables are based on the individual difference-in-differences estimate which measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss for displaced vs. non-displaced workers, as defined in Equation 2. The search controls describe workers' preferences as stated in their first meeting with their caseworker after the layoff. Better/same wage rank indicates whether workers search for a 3-digit occupation with higher/same mean wages compared to their occupation in t=-1. Searching for any employment contract indicates whether workers are indifferent between taking up a fixed-term vs. permanent contract. In all regressions, we reweight migrants to natives as described in Section 3. We have to restrict the sample to workers who appear in the job seeker dataset, with non-missing search information. We cluster standard errors at the baseline county level. ***, **, and * refer to statistical significance at the 1, 5, and 10 percent levels, respectively. Workers in our sample are displaced from 2001-2011, and they are observed from 1996 to 2017.

Table 4: The Role of Networks

| | (1) Earnings Rel. To t=-2 | (2) Earnings Rel. To t=-2 | (3) Log Wages | (4) Log Wages | (5) Employment | (6) Employment |
|---|---|---|---|---|---|---|
| **Panel A:** Within-Establishment Network | | | | | | |
| Migrant | -0.047*** | -0.039*** | -0.037** | -0.022 | -0.032*** | -0.029*** |
| | (0.0093) | (0.0096) | (0.015) | (0.016) | (0.0045) | (0.0048) |
| Migrant Coworker Share | | -0.26*** | | -0.25*** | | -0.078*** |
| | | (0.044) | | (0.046) | | (0.016) |
| Migrant × Migrant Coworker Share | | -0.028 | | -0.17** | | -0.019 |
| | | (0.044) | | (0.068) | | (0.022) |
| Baseline Establishment FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 132044 | 132044 | 122614 | 122614 | 132044 | 132044 |
| $R^2$ | 0.214 | 0.215 | 0.222 | 0.224 | 0.223 | 0.224 |
| Mean Dep. Var (Native) | -0.23 | -0.23 | -0.19 | -0.19 | -0.096 | -0.096 |
| **Panel B:** Ethnic Enclaves: Share of Same Origin Above Mean | | | | | | |
| Migrant | -0.093*** | -0.10*** | -0.084*** | -0.10*** | -0.051*** | -0.052*** |
| | (0.016) | (0.017) | (0.021) | (0.021) | (0.0095) | (0.0098) |
| Baseline County FE | No | Yes | No | Yes | No | Yes |
| Observations | 130156 | 130156 | 121118 | 121118 | 130156 | 130156 |
| $R^2$ | 0.003 | 0.024 | 0.003 | 0.026 | 0.005 | 0.031 |
| Mean Dep. Var (Native) | -0.23 | -0.23 | -0.19 | -0.19 | -0.096 | -0.096 |
| **Panel C:** Ethnic Enclaves: Share of Same Origin Below Mean | | | | | | |
| Migrant | -0.063*** | -0.079*** | -0.016 | -0.031** | -0.034*** | -0.038*** |
| | (0.010) | (0.012) | (0.018) | (0.016) | (0.0062) | (0.0063) |
| Baseline County FE | No | Yes | No | Yes | No | Yes |
| Observations | 132404 | 132404 | 123137 | 123137 | 132404 | 132404 |
| $R^2$ | 0.002 | 0.027 | 0.000 | 0.033 | 0.003 | 0.032 |
| Mean Dep. Var (Native) | -0.23 | -0.23 | -0.19 | -0.19 | -0.096 | -0.096 |

**Notes:** This table shows the role of networks in explaining the migrant-native gap in earnings, wages, and employment after job displacement. The outcome variables are based on the individual difference-in-differences estimate which measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss for displaced vs. non-displaced workers, as defined in Equation 2. In Panel A, we show the effect of having more migrants in one's team on labor market outcomes after displacement. We define the migrant coworker share (centered around its mean) as the number of migrants employed in the same 3-digit occupation in the baseline establishment by the total number of coworkers in the same 3-digit occupation, measured in the year before displacement. In Panels B and C, we show how the migrant-native earnings gap differs for migrants who were displaced from a county with above or below average same origin group shares, respectively. Same origin group share is a variable that indicates the working age population share of a given nationality group (as defined in Table B10) by the overall working age population in that county on December 31, measured in the year before displacement (see Appendix A.1 for details). In all regressions, we reweight migrants to natives as described in Section 3. We cluster standard errors at the baseline county level. ***, **, and * refer to statistical significance at the 1, 5, and 10 percent levels, respectively. Workers in our sample are displaced from 2001-2011, and they are observed from 1996 to 2017.

Table 5: The Displacement Gap in Geographic Mobility

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Moves State | | Moves Job Agency Office | | Labor Market Thickness Change | |
| Migrant | -0.020* | -0.0085 | 0.019*** | 0.016*** | 0.0080 | -0.0017 |
| | (0.012) | (0.012) | (0.0066) | (0.0059) | (0.052) | (0.052) |
| Observations | 133694 | 133694 | 143376 | 143376 | 121799 | 121798 |
| $R^2$ | 0.004 | 0.095 | 0.004 | 0.030 | 0.001 | 0.096 |
| Mean Dep. Var (Native) | 0.18 | 0.18 | 0.033 | 0.033 | -0.12 | -0.12 |
| Baseline County FE | No | Yes | No | Yes | No | Yes |

**Notes:** This table shows the migrant-native gap in geographic mobility. The outcome variables are based on the individual difference-in-differences estimate which measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss for displaced vs. non-displaced workers, as defined in Equation 2. The outcome variable in columns (1) and (2) measures whether a worker moved his workplace to a different federal state (conditional on finding new employment). The outcome variable in columns (3) and (4) measures whether a worker moved their residence to a different local branch office of the Federal Employment Agency district (unconditional on employment). There are about 1000 distinct offices in our sample. We define the outcome variable in columns (5) and (6) as the share of employed individuals in a given 3-digit occupation by all employed in that commuting zone, divided by the respective share for all of Germany. In all regressions, we reweight migrants to natives as described in Section 3. We cluster standard errors at the baseline county level. ***, **, and * refer to statistical significance at the 1, 5, and 10 percent levels, respectively. Workers in our sample are displaced from 2001-2011, and they are observed from 1996 to 2017.

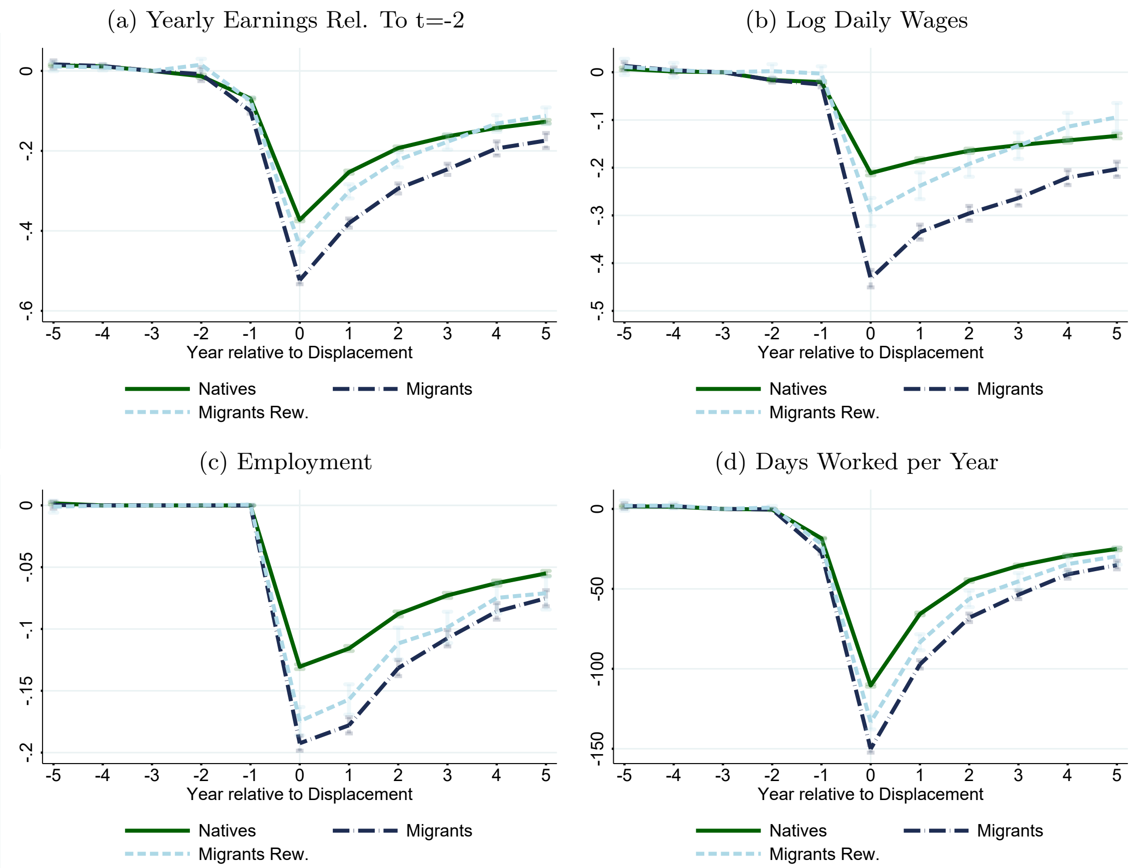
Table 6: Robustness Checks

| | (1) Baseline | (2) 1 Year Tenure Restr. | (3) 2 Years Tenure Restr. | (4) Matching Without Wages | (5) Matching on t=-4 Only | (6) Exact Matching on Counties | (7) Longer Time Window | (8) Reweight. Natives to Migrants |
|---|---|---|---|---|---|---|---|---|
| **Panel A:** Earnings Rel. to Year -2 | | | | | | | | |
| Migrant | -0.071 | -0.074 | -0.051 | -0.16 | -0.073 | -0.077 | -0.053 | -0.059 |
| | (0.0095)** | (0.011)** | (0.0088)** | (0.014)** | (0.015)** | (0.011)** | (0.0097)** | (0.0093)** |
| Observations | 143647 | 195001 | 167483 | 226977 | 157329 | 138115 | 141777 | 131802 |
| $R^2$ | 0.003 | 0.014 | 0.001 | 0.003 | 0.001 | 0.004 | 0.001 | 0.002 |
| Mean Dep. Var Men | -.226 | -.24 | -.227 | -.257 | -.253 | -.233 | -.189 | -.194 |
| | (.002) | (.002) | (.003) | (.004) | (.003) | (.002) | (.002) | (.002) |
| **Panel B:** Log Wages | | | | | | | | |
| Migrant | -0.039 | -0.051 | -0.0098 | -0.031 | -0.039 | -0.041 | -0.012 | -0.088 |
| | (0.013)** | (0.011)** | (0.013) | (0.011)** | (0.013)** | (0.014)** | (0.013) | (0.012)** |
| Observations | 133388 | 181493 | 155732 | 206824 | 146200 | 128270 | 134217 | 123292 |
| $R^2$ | 0.001 | 0.026 | 0.000 | 0.000 | 0.001 | 0.001 | 0.000 | 0.004 |
| Mean Dep. Var Men | -.194 | -.192 | -.195 | -.192 | -.204 | -.199 | -.18 | -.168 |
| | (.002) | (.002) | (.003) | (.002) | (.002) | (.002) | (.002) | (.002) |
| **Panel C:** Employment | | | | | | | | |
| Migrant | -0.039 | -0.036 | -0.038 | -0.049 | -0.037 | -0.043 | -0.028 | -0.030 |
| | (0.0052)** | (0.0045)** | (0.0043)** | (0.0043)** | (0.0046)** | (0.0052)** | (0.0047)** | (0.0037)** |
| Observations | 143647 | 195001 | 167483 | 226977 | 157329 | 138115 | 141777 | 131802 |
| $R^2$ | 0.004 | 0.023 | 0.004 | 0.005 | 0.004 | 0.005 | 0.002 | 0.003 |
| Mean Dep. Var Men | -.096 | -.093 | -.095 | -.094 | -.096 | -.096 | -.079 | -.083 |
| | (.001) | (.001) | (.001) | (.001) | (.001) | (.001) | (.001) | (.001) |
| **Panel D:** Days Worked Full-time | | | | | | | | |
| Migrant | -25.6 | -26.3 | -23.2 | -38.8 | -26.7 | -30.1 | -20.9 | -23.6 |
| | (2.49)** | (2.13)** | (2.24)** | (2.48)** | (2.75)** | (2.72)** | (2.47)** | (2.20)** |
| Observations | 143647 | 195001 | 167483 | 226977 | 157329 | 138115 | 141777 | 131802 |
| $R^2$ | 0.007 | 0.030 | 0.006 | 0.013 | 0.007 | 0.010 | 0.004 | 0.006 |
| Mean Dep. Var Men | -66.63 | -64.52 | -65.611 | -62.849 | -66.2 | -67.417 | -52.742 | -56.996 |
| | (.412) | (.37) | (.527) | (.366) | (.412) | (.417) | (.439) | (.411) |

**Notes:** Each column in this table represents a different robustness check of a weighted difference-in-differences regression. All outcome variables are based on the individual difference-in-differences estimate which measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss for displaced vs. non-displaced workers. Column (1) reports the baseline coefficients. Columns (2) and (3) report results when relaxing the baseline tenure restriction to 1 and 2 years, respectively. Columns (4) and (5) report results of a matching specification where we do not match on trends in wages and where we match on characteristics in t=-4, only. Column (6) reports results when we match exactly on counties (NUTS 3 regions) instead of 1-digit industries in t=-1. Column (7) reports results for a longer time window (10 years pre vs. 10 years post displacement), and Column (8) reports results when we reweight natives to migrants. We cluster standard errors at the county level at the time of displacement (constant within matched worker pairs). * and ** correspond to 5 and 1 percent significance levels, respectively. Workers in our sample are displaced from 2001-2011, and they are observed from 1996 to 2017.

# 7 Figures

Figure 1: Labor Market Outcomes by Migration Status



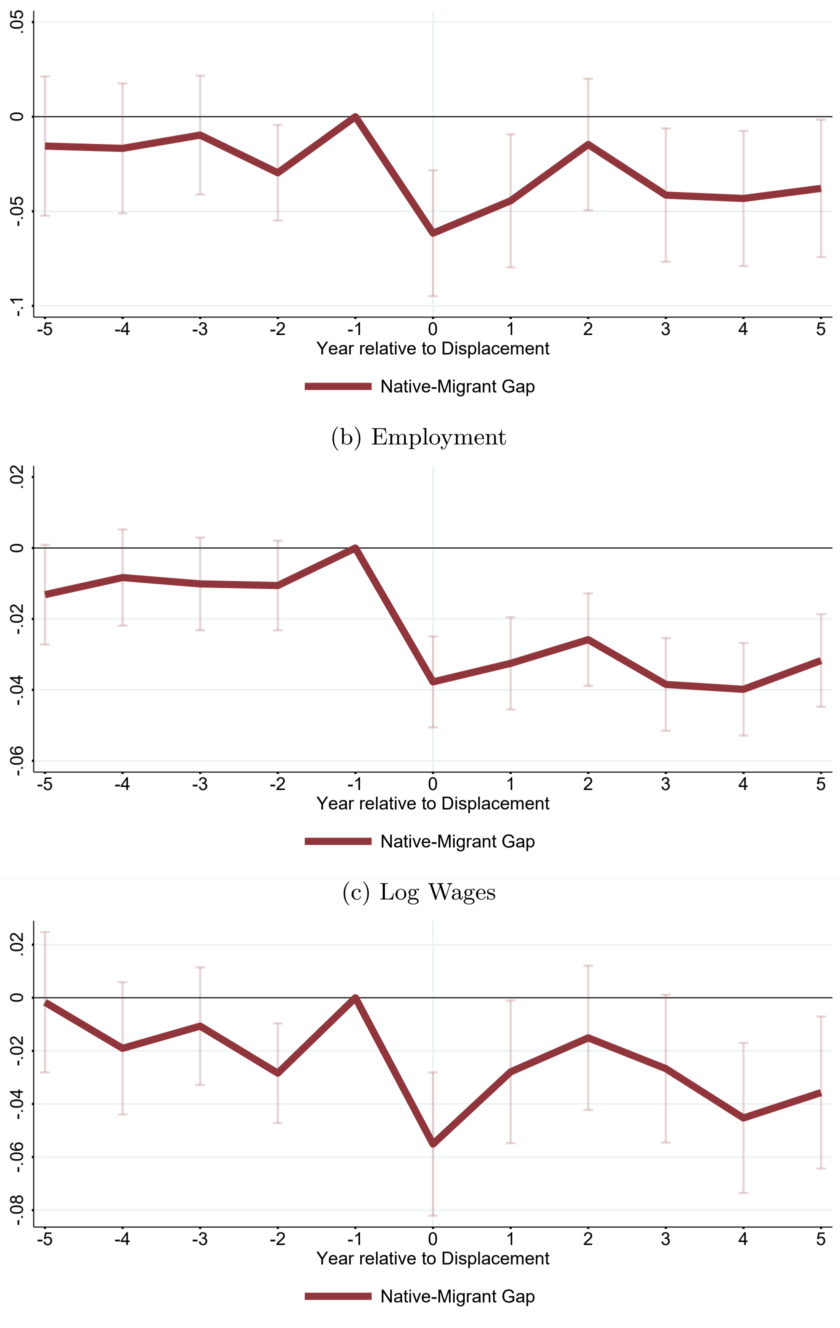

**Notes:** This figure plots event study regression coefficients showing the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel (a)), log wages (Panel B(b)), employment (Panel (c)), and days worked per year (Panel (d)). The solid green line plots coefficients for the baseline sample of native workers, the dashed blue line plots coefficients for the baseline sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. See Section 3 for details on the reweighting. We compute all estimates using the event study regression equation (1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 2: Within-Establishment Migrant-Native Displacement Gap

(a) Log Earnings

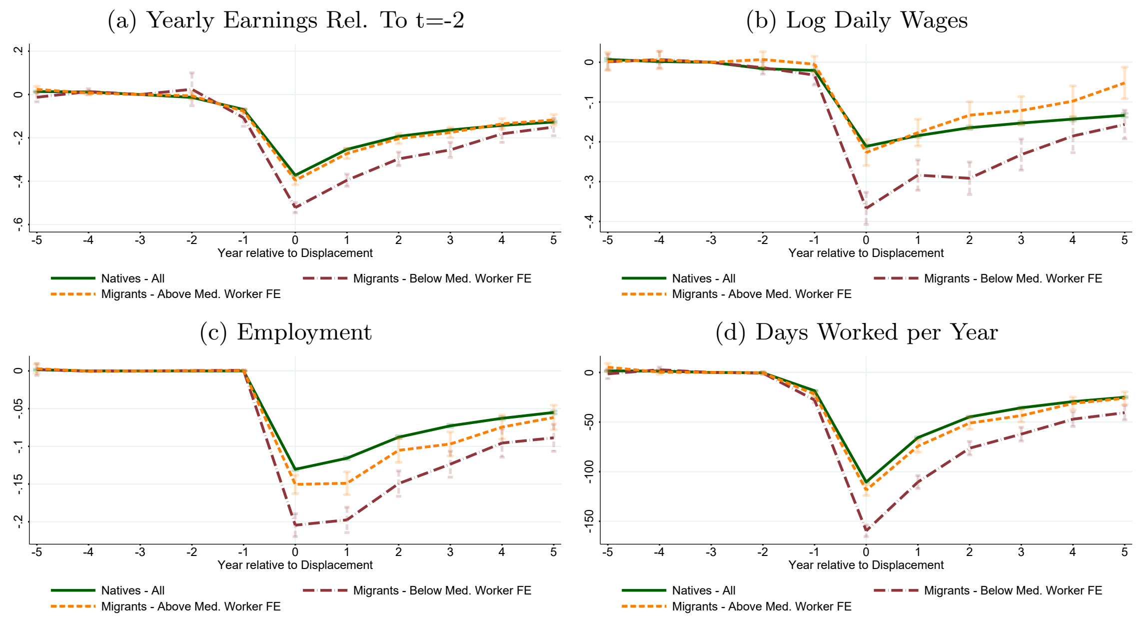
(b) Employment

(c) Log Wages



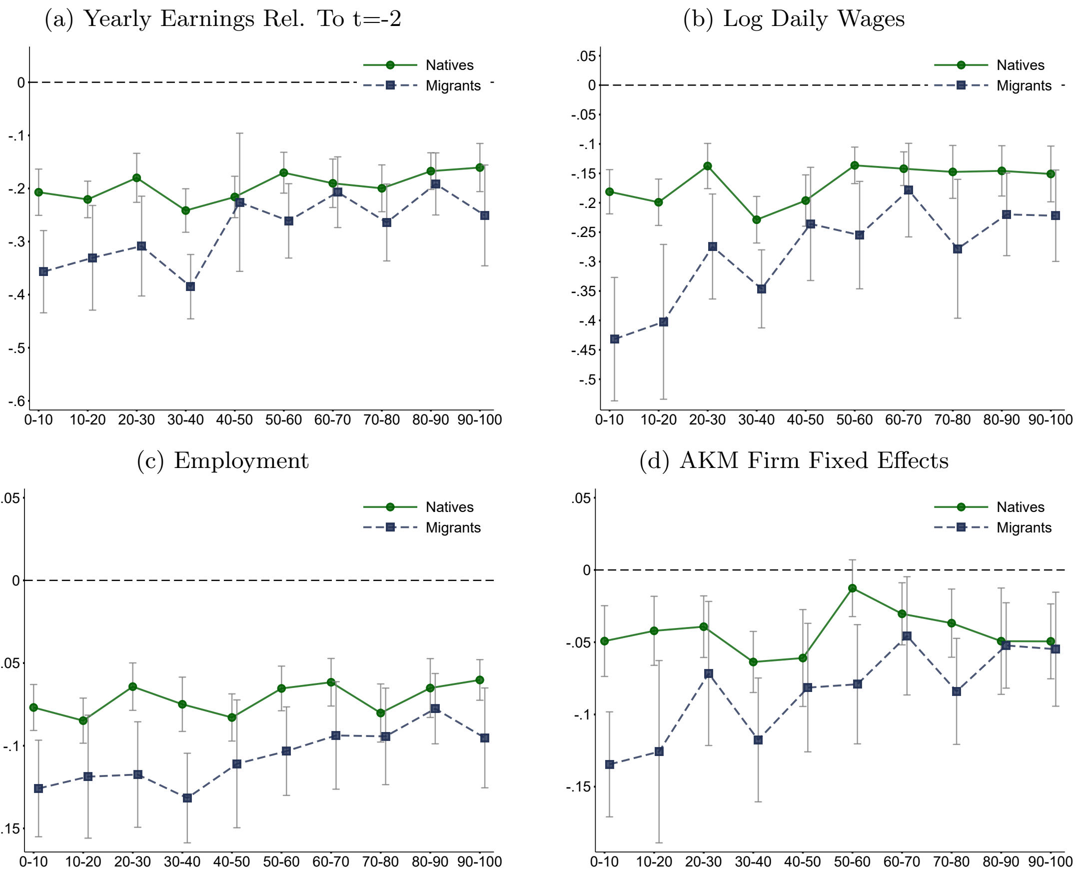

**Notes:** This figure plots the migrant-native earnings, employment, and wage gap for a sample of displaced workers who were displaced from the same establishment and 3-digit occupation in the same year. In addition, we require workers to have the same age at the time of displacement, the same years of tenure, and the same years of education. Workers moreover need to be in a full-time job before displacement. We plot coefficients from a regression of the outcome variable on migrant status interacted with time since displacement, time since displacement dummies, year dummies, and individual fixed effects. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 3: Labor Market Outcomes by Baseline Worker AKM Fixed Effects




**Notes:** This figure plots event study regression coefficients showing the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel (a)), log wages (Panel (b)), employment (Panel (c)), and days worked per year (Panel (d)). The solid green line plots coefficients for the sample of native workers, the dashed brown line plots coefficients for the sample of migrant workers with below median baseline worker productivity, and the dashed orange line plots coefficients for the sample of migrant workers with above-median baseline worker productivity. We use the IAB's AKM dataset to measure worker productivity in the baseline year, see Appendix A.2 for details. See Section 3 for details on the reweighting. We compute all estimates using the event study regression equation (1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 4: Migrant-Native Gap by Pre-Displacement Labor Market Tightness




**Notes:** This figure shows how costs of job displacement differ by decile of labor market tightness, measured in t=-1. We define labor market tightness as the log ratio of vacancies by unemployed job-seekers at the baseline county level. Each panel plots coefficients from a separate OLS regression where we regress workers' individual difference-in-differences outcomes on dummies for the 10 deciles. Panel (a) reports yearly earnings relative to t=-2, Panel (b) reports log wages, Panel (c) reports the probability to be employed, and Panel (d) reports AKM establishment fixed effects. Each difference-in-differences outcome measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=5) job loss for displaced vs. non-displaced workers, within matched worker pairs. All regressions control for age, age squared, tenure, experience, full-time work status, firm size, 1-digit industry dummies, 1-digit occupation dummies (all measured in t=-1), and log wages (in t=-3). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

# A Data

## A.1 Population Data

In order to analyze the role of local ethnic shares, we use the data set *Population and Employment, Foreign Population, Results of the Central Register of Foreigners, Destatis, 2019*. It is based on official records from the German foreigners' registration office and is thus highly reliable.

This data set reports the population in Germany on December 31. It thus contains the exact population of a given nationality by age and county. We have access to this data for each year in the period 1998-2017. To construct our ethnic share measure, we restrict the data to the working-age population, i.e. individuals aged 15-65. We then divide nationalities into groups of origin according to **?**see also Table B10). To give one example: Rather than analyzing the share of Polish citizens by itself, we group them into a cluster of Central European countries (Polish, Czech, Hungarian, Slovakian, and Slovenian citizens). The idea is that on the one hand, individuals from these countries have a similar educational background and are thus likely substitutes for each other. On the other hand, these countries are culturally closely related, and Central European citizens may thus form ethnic clusters. In the last step, we divide the number of each nationality group in a given county by the overall working age population in that county on December 31:

$$Share_{oct} = \frac{P_{oct}}{P_{oct} + N_{ct}} \tag{3}$$

where $P_{oct}$ is the number of working-age citizens from a given nationality group $o$, in county $c$, and at time $t$. $N_{ct}$ is the number of working-age natives in county $c$ and at time $t$. Figure B11 shows how the share of the same-nationality working-age population is distributed among displaced workers. Not surprisingly, it takes much higher values for Germans (60-100%) than migrants (0-10%). Even though the share is skewed towards 0 for migrant workers, there is substantial variation in the ethnic share: About 16% of displaced migrants live in counties with an ethnic share of 5% or more, and one-third of displaced migrant workers live in counties with an ethnic share of at least 3%.

Note that the population data comes with a drawback: For the majority of foreigners' registration offices, the jurisdictions coincide with German counties. However, in the federal states of Saarland, Hesse, and Brandenburg, a county-specific assignment of data is not always possible. Therefore, it is not possible to determine the percentage of the working-age population of a certain nationality for all German counties over the whole period. For instance, in the year 2017, 10 out of 401 German counties could not be merged (Kassel city and the county of Kassel, all six counties of Saarland, Cottbus, and the county of Spree-Neiße). This is only a minor issue for our analysis, as the vast majority of counties - especially the five largest metropolitan areas: Berlin, Cologne, Frankfurt, Hamburg, and Munich - are included in the sample.

## A.2 AKM Data

For the analysis of worker and establishment AKM effects, we use a data set provided by the Institute for Employment Research (IAB) and described in Lochner, Wolter, and Seth (2023). These data cover the years 1985-2021 and contain both worker and establishment fixed effects averaged over sub-periods of 7 years each: [1985-1992; 1993-1999; 2000-2006; 2007-2013; 2014-2021]. We can use a unique worker or establishment ID to link these data to our baseline sample. In general, we proceed as follows: If a worker works for establishment A in 1998, we assign him the establishment fixed effect for the given establishment that is available for the year range 1993-1999. If she switches to establishment B in 2001, we assign him the establishment fixed effect for the respective establishment in the year range 2000-2006.

For the analysis of worker-fixed effects presented in Figure 3, we have to ensure that the baseline worker-fixed effects we assign are not yet influenced by the displacement itself. Thus, for the baseline years 2001-2005, we always assign a worker his worker fixed effect computed for the years 1993-1999. For all other baseline years, we assign the worker fixed effect from 2000-2006.

## A.3 Tightness Data

The tightness indicator, which we construct from information on vacancies and unemployed job seekers in a given county, is based on data provided by the Statistical Office of the German Federal Employment Agency (BA). The number of vacancies in this data stems from the number of open positions that employers register with the Federal Employment Agency. It comprises both positions with a social-security contract as well as marginal employment (mini-jobs). The Statistical Office of the BA records these data on a monthly level, but we are using a yearly aggregate based on June 30.

The information on unemployed job seekers stems from the number of individuals registered in the job agencies. It comprises both individuals who receive unemployment benefits type 1 (usually in the first year after job loss) and those who receive unemployment benefits type 2 (beyond one year of unemployment). The data does not include unemployed individuals in training programs.

To construct our tightness indicator, we collect both vacancies and the number of unemployed job seekers at the county level. We then construct the following variable:

$$Log(Tightness_{ct}) = Log(\frac{Vacancies_{ct}}{Unemployed_{ct}}) \tag{4}$$

where $c$ corresponds to a given county and $t$ corresponds to a given year. We measure tightness in the baseline year, i.e. in the year before job displacement. Note that due to data limitations, we can construct the tightness measure from the year 2007 onwards, only.

## A.4 Job Search Data

For our measurement of job search preferences, we draw on the *jobseeker history panel*, which is an administrative dataset provided by the IAB. We use the versions *ASU V06.11.00-201904* and *XASU V02.03.00-201904*. These data are based on the information the caseworker enters into the Federal Employment Agency's online system once the unemployed job seeker is registered for the first time.

We use the following indicators available in this data: The unemployed job seeker's preferred 3-digit occupation, a dummy indicating whether he is willing to search for a job outside of the daily commuting distance range, a dummy indicating a job seeker's willingness to accept any employment contract (vs. accepting only a permanent contract), and his willingness to accept a full-time, part-time or any type of job. One drawback of the data is that the information on the geographic scope of search is available for spells starting before July 2006, meaning that we have to restrict the time frame of our sample for the job search analysis.

## A.5 Data for Within-Firm Analysis

For the within-firm analysis described in Section 3.3, we construct a different sample than for our baseline analysis. More specifically, we draw on a dataset of the universe of workers who were employed at a layoff firm in the year before layoff in any of the baseline years 2001-2011. This has the advantage that we observe the full workforce of these establishments (in the year before displacement) and we can therefore zoom into teams at a given workplace.

We then use an exact matching algorithm, where we match displaced migrants to displaced natives within the same establishment, 3-digit occupation, baseline year, age, tenure, education, full-time employment status, and gender. In cases where we assign more than one native worker to a migrant, we randomly keep one match. While this is a demanding matching algorithm, it ensures that we essentially compare workers displaced from the same team within the same establishment, and with very comparable individual characteristics. Our underlying assumption is that these workers had very similar careers before displacement and that any differences in their post-displacement outcomes can therefore be attributed to migration status rather than the status quo pre-layoff.

Note that as Appendix Table B5 shows, this matching procedure comes with the drawback of a reduced sample size. This is less of an issue for the migrants in our sample, where the sample size drops from 17,500 displaced migrant workers in the baseline sample to 14,300 in the within-firm analysis sample. For native workers, however, the number of observations is reduced from almost 130,000 in the baseline sample to 14,500 in the within-firm analysis sample.[21] Similarly, workers in the within-firm analysis sample present a negative selection relative to workers in the baseline sample: They earn lower wages and have lower tenure. Not surprisingly, they also work in substantially larger firms - it is more likely that we find an exact match for a given worker in larger firms. All of these are important to keep in mind when interpreting the results of the within-firm analysis.

[21]Note that some workers (less than 1%) are displaced twice during the layoff period; in that case, we only count the first displacement event in the table.

# B Appendix Tables and Figures

Table B1: Displaced and Non-displaced Workers in the Baseline Sample in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| **Panel A: Individual Characteristics** | | | | |
| Years of Education | 11.2 | 11.2 | 12.3 | 12.3 |
| | [1.68] | [1.61] | [1.76] | [1.77] |
| Age | 37.9 | 37.9 | 39.4 | 39.4 |
| | [6.83] | [6.68] | [6.82] | [6.71] |
| Tenure | 6.37 | 6.38 | 6.20 | 6.21 |
| | [2.60] | [2.57] | [2.47] | [2.43] |
| Real Daily Wage | 91.3 | 89.3 | 104.1 | 102.3 |
| | [30.1] | [30.8] | [36.2] | [36.8] |
| Total Yearly Earnings | 33649.7 | 30208.5 | 38024.4 | 35479.6 |
| | [11161.1] | [11829.3] | [13474.8] | [14191.0] |
| Days Worked in Year | 362.8 | 335.7 | 362.9 | 344.3 |
| | [14.9] | [53.7] | [13.9] | [45.6] |
| **Panel B: Regional Characteristics** | | | | |
| Lives in City | 0.77 | 0.80 | 0.55 | 0.57 |
| | [0.42] | [0.40] | [0.50] | [0.50] |
| Lives in East Germany | 0.031 | 0.042 | 0.22 | 0.25 |
| | [0.17] | [0.20] | [0.42] | [0.43] |
| Local UR Change | 0.014 | 0.028 | 0.019 | 0.035 |
| | [0.14] | [0.14] | [0.13] | [0.14] |
| **Panel C: Establishment Characteristics** | | | | |
| Establishment Size | 276.6 | 290.8 | 324.2 | 344.1 |
| | [529.4] | [490.1] | [714.3] | [634.3] |
| Share Migrant Workers | 0.21 | 0.24 | 0.065 | 0.074 |
| | [0.19] | [0.19] | [0.086] | [0.095] |
| Share High-Skilled Workers | 0.079 | 0.079 | 0.12 | 0.12 |
| | [0.12] | [0.12] | [0.16] | [0.16] |
| Share Marginally Employed Workers | 0.078 | 0.059 | 0.054 | 0.041 |
| | [0.15] | [0.13] | [0.11] | [0.095] |
| Displaced from Complete Closure | 0.000057 | 0.32 | 0.000070 | 0.32 |
| | [0.0076] | [0.47] | [0.0084] | [0.46] |
| Number of Observations | 17503 | 17503 | 128744 | 128744 |

**Notes:** This table shows the characteristics of displaced and non-displaced male workers in the year prior to displacement. Workers satisfy the following baseline restrictions: male, aged 24 to 50, working full-time, at least 3 years of tenure, working in an establishment with at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells (see Section 3 for more details). Columns (1) and (2) report summary statistics for non-displaced and displaced migrants. Columns (3) and (4) report summary statistics for non-displaced and displaced natives. Standard deviations in brackets.

Table B2: Worker Distribution Across Industries in Baseline Sample in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| Agriculture | 0.00023 | 0.00023 | 0.00085 | 0.00085 |
| | [0.015] | [0.015] | [0.029] | [0.029] |
| Mining, Energy | 0.033 | 0.033 | 0.020 | 0.020 |
| | [0.18] | [0.18] | [0.14] | [0.14] |
| Food Manufacturing | 0.064 | 0.064 | 0.037 | 0.037 |
| | [0.25] | [0.25] | [0.19] | [0.19] |
| Consumption Goods | 0.10 | 0.10 | 0.070 | 0.070 |
| | [0.30] | [0.30] | [0.25] | [0.25] |
| Production Goods | 0.12 | 0.12 | 0.084 | 0.084 |
| | [0.33] | [0.33] | [0.28] | [0.28] |
| Investment Goods | 0.16 | 0.16 | 0.15 | 0.15 |
| | [0.37] | [0.37] | [0.36] | [0.36] |
| Construction | 0.039 | 0.039 | 0.086 | 0.086 |
| | [0.19] | [0.19] | [0.28] | [0.28] |
| Retail | 0.11 | 0.11 | 0.13 | 0.13 |
| | [0.32] | [0.32] | [0.34] | [0.34] |
| Traffic, Telecommunication | 0.075 | 0.075 | 0.069 | 0.069 |
| | [0.26] | [0.26] | [0.25] | [0.25] |
| Credit, Insurance | 0.0043 | 0.0043 | 0.015 | 0.015 |
| | [0.066] | [0.066] | [0.12] | [0.12] |
| Restaurants | 0.021 | 0.021 | 0.0053 | 0.0053 |
| | [0.14] | [0.14] | [0.072] | [0.072] |
| Education | 0.0022 | 0.0022 | 0.020 | 0.020 |
| | [0.047] | [0.047] | [0.14] | [0.14] |
| Health | 0.0051 | 0.0051 | 0.012 | 0.012 |
| | [0.072] | [0.072] | [0.11] | [0.11] |
| Commercial Services | 0.23 | 0.23 | 0.24 | 0.24 |
| | [0.42] | [0.42] | [0.43] | [0.43] |
| Other Services | 0.022 | 0.022 | 0.028 | 0.028 |
| | [0.15] | [0.15] | [0.16] | [0.16] |
| Non-Profit | 0.0093 | 0.0093 | 0.013 | 0.013 |
| | [0.096] | [0.096] | [0.11] | [0.11] |
| Public Administration | 0.0022 | 0.0022 | 0.018 | 0.018 |
| | [0.047] | [0.047] | [0.13] | [0.13] |
| Number of Observations | 17503 | 17503 | 128744 | 128744 |

**Notes:** This table shows the distribution across industries of displaced and non-displaced male workers in the year prior to the displacement year. Workers satisfy the following baseline restrictions: Aged 24 to 50, working full-time in the pre-displacement year, at least 3 years of tenure, and the establishment has at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells. The non-displaced sample of workers is a random sample of workers (one per displaced worker) who satisfy the same baseline restrictions. Standard deviations in brackets.

Table B3: Worker Distribution Across Occupations in Baseline Sample in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| Agriculture, gardening, work with animals | 0.0066 | 0.0042 | 0.0073 | 0.0041 |
| | [0.081] | [0.065] | [0.085] | [0.064] |
| Simple, manual tasks | 0.42 | 0.46 | 0.22 | 0.23 |
| | [0.49] | [0.50] | [0.41] | [0.42] |
| Qualified, manual tasks | 0.18 | 0.17 | 0.24 | 0.26 |
| | [0.38] | [0.38] | [0.43] | [0.44] |
| Technician | 0.025 | 0.029 | 0.072 | 0.074 |
| | [0.16] | [0.17] | [0.26] | [0.26] |
| Engineer | 0.017 | 0.015 | 0.043 | 0.038 |
| | [0.13] | [0.12] | [0.20] | [0.19] |
| Simple services | 0.23 | 0.20 | 0.15 | 0.12 |
| | [0.42] | [0.40] | [0.35] | [0.33] |
| Qualified services | 0.013 | 0.012 | 0.019 | 0.017 |
| | [0.11] | [0.11] | [0.14] | [0.13] |
| Semi-professions | 0.0050 | 0.0047 | 0.016 | 0.015 |
| | [0.071] | [0.068] | [0.13] | [0.12] |
| Professions | 0.0039 | 0.0042 | 0.0084 | 0.011 |
| | [0.062] | [0.064] | [0.091] | [0.10] |
| Simple commercial and administrative tasks | 0.023 | 0.021 | 0.039 | 0.035 |
| | [0.15] | [0.14] | [0.19] | [0.18] |
| Qualified commercial and administrative tasks | 0.065 | 0.061 | 0.16 | 0.16 |
| | [0.25] | [0.24] | [0.37] | [0.36] |
| Manager | 0.010 | 0.012 | 0.029 | 0.029 |
| | [0.10] | [0.11] | [0.17] | [0.17] |
| Not classified | 0.0025 | 0.0025 | 0.0032 | 0.0030 |
| | [0.050] | [0.050] | [0.056] | [0.054] |
| Number of Observations | 17503 | 17503 | 128744 | 128744 |

**Notes:** This table shows the distribution across occupations according to Blossfeld (1987) of displaced and non-displaced male workers in the year prior to the displacement year. Workers satisfy the following baseline restrictions: Aged 24 to 50, working full-time in the pre-displacement year, at least 3 years of tenure, and establishment has at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells. The non-displaced sample of workers is a random sample of workers (one per displaced worker) who satisfy the same baseline restrictions. Standard deviations in brackets.

Table B6: The Importance of Reweighting Variables in Explaining the Migrant-Native Gap in Earnings Losses Relative to t=-2

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Migrant | -0.093** | -0.087** | -0.087** | -0.087** | -0.084** | -0.084** | -0.081** | -0.077** |
| | (0.011) | (0.010) | (0.010) | (0.010) | (0.010) | (0.0099) | (0.0095) | (0.0091) |
| Age in t-1 | | -0.0015** | -0.0014** | -0.0017** | -0.0017** | -0.0017** | -0.0016** | -0.0020** |
| | | (0.00033) | (0.00032) | (0.00030) | (0.00030) | (0.00029) | (0.00028) | (0.00026) |
| Education in t-1 | | 0.0075** | 0.0074** | 0.0044** | 0.0045** | 0.0046** | 0.0042** | 0.0030* |
| | | (0.0013) | (0.0013) | (0.0014) | (0.0014) | (0.0013) | (0.0012) | (0.0012) |
| Tenure in t-1 | | | -0.00074 | -0.0013 | -0.0013 | -0.0011 | -0.00098 | 0.00016 |
| | | | (0.0013) | (0.0013) | (0.0013) | (0.0012) | (0.0012) | (0.0010) |
| Log wage in t-3 | | | | 0.010 | 0.0093 | 0.0055 | 0.0048 | 0.018** |
| | | | | (0.0066) | (0.0066) | (0.0065) | (0.0067) | (0.0067) |
| Log wage in t-4 | | | | 0.014* | 0.014* | 0.013* | 0.013* | 0.019** |
| | | | | (0.0059) | (0.0059) | (0.0058) | (0.0059) | (0.0059) |
| City Resident in t-1 | | | | | -0.0077* | -0.0098** | -0.011** | -0.015** |
| | | | | | (0.0038) | (0.0032) | (0.0033) | (0.0029) |
| Log(Firmsize) in t-1 | | | | | | 0.016** | 0.016** | 0.0061 |
| | | | | | | (0.0052) | (0.0054) | (0.0045) |
| Observations | 266136 | 266136 | 266136 | 266136 | 264576 | 264576 | 264576 | 264576 |
| Occupation Controls | No | No | No | No | No | No | Yes | Yes |
| Industry Controls | No | No | No | No | No | No | No | Yes |

Notes: Each column in each panel returns the coefficients from the OLS regression. The dependent variable is earnings relative to earnings in t=-2. The displacement occurred between t=-1 and t=0. Controls correspond to our reweighting variables (all measured in t=-1 if not stated otherwise): Age, education (in years), tenure (in years), log(wage) (in t=-3 and t=-4), a dummy for city residency, log(firm size), 1-digit occupations, and 1-digit industries. We cluster standard errors at the displacement establishment level (constant within matched worker pairs). * and ** correspond to 5 and 1 percent significance levels, respectively. Source: IEB.

Table B4: Sample Attrition: Summary Statistics for Stayers vs. Drop-Outs

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Migrants | | Natives | |
| | Stayers | Drop-outs | Stayers | Drop-outs |
| **Panel A: Individual Characteristics** | | | | |
| Years of Education | 11.2 | 11.3 | 12.3 | 12.6 |
| | [1.59] | [1.81] | [1.76] | [2.02] |
| Age | 37.7 | 39.6 | 39.4 | 40.5 |
| | [6.67] | [6.61] | [6.71] | [6.67] |
| Tenure | 6.36 | 6.69 | 6.20 | 6.42 |
| | [2.55] | [2.72] | [2.42] | [2.54] |
| Real Daily Wage | 89.0 | 92.3 | 101.9 | 109.9 |
| | [30.6] | [33.4] | [36.6] | [39.8] |
| Total Yearly Earnings | 30266.2 | 29487.1 | 35454.8 | 35949.4 |
| | [11737.8] | [12899.5] | [14121.5] | [15442.9] |
| Days Worked in Year | 337.1 | 317.6 | 345.3 | 325.3 |
| | [52.4] | [65.3] | [44.4] | [61.3] |
| **Panel B: Regional Characteristics** | | | | |
| Lives in City | 0.80 | 0.77 | 0.57 | 0.58 |
| | [0.40] | [0.42] | [0.50] | [0.49] |
| Lives in East Germany | 0.041 | 0.043 | 0.25 | 0.23 |
| | [0.20] | [0.20] | [0.43] | [0.42] |
| Local UR Change | 0.028 | 0.021 | 0.035 | 0.031 |
| | [0.14] | [0.14] | [0.14] | [0.14] |
| Number of Observations | 16205 | 1298 | 122286 | 6458 |

**Notes:** This table shows the characteristics of displaced workers in the year prior to displacement. Workers satisfy the following restrictions in the pre-displacement year: male, aged 24 to 50, working full-time, at least 3 years of tenure, working in an establishment with at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells. Columns (1) and (2) report summary statistics for displaced migrants who stay in the German administrative data (column (1)) and who leave the German administrative data post-layoff without returning (column (2)). Columns (3) and (4) report summary statistics for displaced natives who stay and drop out of the data, respectively. Standard deviations in brackets.

Table B5: Displaced Worker Sample Statistics

| | (1) Migrants Eventfirm | (2) Migrants Displaced | (3) Migrants Matched | (4) Natives Eventfirm | (5) Natives Displaced | (6) Natives Matched |
|---|---|---|---|---|---|---|
| **Panel A: Individual Characteristics** | | | | | | |
| Years of Education | 11.6 | 11.5 | 11.4 | 12.3 | 12.2 | 11.4 |
| | [1.81] | [1.77] | [1.53] | [1.97] | [1.91] | [1.53] |
| Age | 34.2 | 33.6 | 31.7 | 35.5 | 34.5 | 31.7 |
| | [10.0] | [10.1] | [10.6] | [11.6] | [11.7] | [10.7] |
| Tenure | 3.16 | 2.78 | 2.46 | 3.78 | 3.27 | 2.47 |
| | [3.10] | [2.92] | [2.97] | [3.33] | [3.05] | [2.97] |
| Real Daily Wage | 60.4 | 57.5 | 57.8 | 71.3 | 65.8 | 58.1 |
| | [38.8] | [38.1] | [36.7] | [48.9] | [47.8] | [37.9] |
| Total Yearly Earnings | 20034.2 | 18531.4 | 18173.2 | 24442.0 | 22015.3 | 18488.7 |
| | [14274.7] | [13772.1] | [13696.9] | [18107.3] | [17508.8] | [14468.9] |
| Days per year working | 314.1 | 304.2 | 295.6 | 326.2 | 316.5 | 296.3 |
| | [76.6] | [80.6] | [82.4] | [67.5] | [72.9] | [84.0] |
| **Panel B: Regional Characteristics** | | | | | | |
| Lives in City | 0.67 | 0.67 | 0.68 | 0.45 | 0.45 | 0.64 |
| | [0.47] | [0.47] | [0.47] | [0.50] | [0.50] | [0.48] |
| Lives in East Germany | 0.076 | 0.081 | 0.14 | 0.31 | 0.32 | 0.14 |
| | [0.26] | [0.27] | [0.34] | [0.46] | [0.47] | [0.35] |
| **Panel C: Establishment Characteristics** | | | | | | |
| Size of establishment | 226.6 | 227.9 | 514.3 | 250.2 | 260.8 | 510.8 |
| | [403.7] | [412.8] | [788.2] | [475.1] | [495.2] | [784.7] |
| Share Migrant Workers | 0.27 | 0.27 | 0.18 | 0.075 | 0.075 | 0.17 |
| | [0.22] | [0.22] | [0.14] | [0.10] | [0.10] | [0.13] |
| Share High-Skilled Workers | 0.073 | 0.072 | 0.084 | 0.11 | 0.11 | 0.085 |
| | [0.12] | [0.12] | [0.12] | [0.15] | [0.15] | [0.13] |
| Number of Observations | 165976 | 131111 | 14344 | 1069836 | 807202 | 14546 |

**Notes:** This table shows the characteristics of male workers in the data that we use for the within-firm analysis (see Section 3.3 and Appendix Section A). Column (1) shows all migrants in event firms, that is all migrants who were employed in a layoff firm in the respective baseline year, where layoffs occur in 2001-2011. Column (2) shows the sub-sample of migrants who were displaced in the layoff. Column (3) shows the sub-sample of displaced migrants for which we find an exact match using the matching procedure described in Appendix Section A. Columns (4)-(6) repeat the same for natives. We show averages for the respective baseline years. Standard deviations in brackets.

Table B7: Restricting the Sample to Baseline Years up to 2007 (Pre Financial Crisis)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Log (Earnings) | | Log Wage | | Employment | | Days Worked | |
| | Natives | Migrants | Natives | Migrants | Natives | Migrants | Natives | Migrants |
| Year (Disp) t-5 | 0.015** | 0.036** | 0.0053** | 0.0014 | 0.0024** | -0.0012 | 2.13** | 6.02** |
| | (0.0018) | (0.010) | (0.0016) | (0.010) | (0.00052) | (0.0033) | (0.31) | (1.91) |
| Year (Disp) t-4 | 0.014** | 0.015* | 0.00039 | 0.011 | -0.000023 | -0.00045** | 1.46** | 2.95* |
| | (0.0011) | (0.0072) | (0.0014) | (0.0081) | (0.000024) | (0.00016) | (0.17) | (1.22) |
| Year (Disp) t-2 | -0.011** | -0.014** | -0.016** | 0.0089 | 0.0014** | 0.00046 | 0.11 | -0.65 |
| | (0.00091) | (0.0039) | (0.0013) | (0.0097) | (0.00028) | (0.00087) | (0.16) | (0.60) |
| Year (Disp) t-1 | -0.085** | -0.11** | -0.020** | -0.0063 | -0.0000051 | 0.00092** | -18.8** | -24.1** |
| | (0.0011) | (0.0062) | (0.0015) | (0.0094) | (0.000036) | (0.00033) | (0.17) | (1.01) |
| Year (Disp) t | -0.58** | -0.70** | -0.22** | -0.24** | -0.14** | -0.18** | -114.1** | -135.2** |
| | (0.0032) | (0.019) | (0.0026) | (0.016) | (0.0011) | (0.0068) | (0.45) | (2.86) |
| Year (Disp) t+1 | -0.36** | -0.50** | -0.19** | -0.23** | -0.12** | -0.17** | -68.9** | -92.3** |
| | (0.0032) | (0.021) | (0.0024) | (0.018) | (0.0011) | (0.0078) | (0.47) | (3.16) |
| Year (Disp) t+2 | -0.27** | -0.37** | -0.17** | -0.21** | -0.094** | -0.14** | -47.4** | -67.6** |
| | (0.0032) | (0.020) | (0.0025) | (0.017) | (0.0011) | (0.0077) | (0.48) | (3.09) |
| Year (Disp) t+3 | -0.23** | -0.30** | -0.16** | -0.17** | -0.078** | -0.11** | -37.5** | -53.8** |
| | (0.0032) | (0.019) | (0.0026) | (0.016) | (0.0012) | (0.0076) | (0.49) | (3.12) |
| Year (Disp) t+4 | -0.19** | -0.24** | -0.15** | -0.12** | -0.067** | -0.092** | -30.8** | -42.4** |
| | (0.0032) | (0.021) | (0.0027) | (0.022) | (0.0012) | (0.0075) | (0.49) | (3.05) |
| Year (Disp) t+5 | -0.17** | -0.17** | -0.14** | -0.10** | -0.059** | -0.089** | -26.1** | -36.1** |
| | (0.0032) | (0.019) | (0.0027) | (0.017) | (0.0012) | (0.0083) | (0.48) | (3.31) |
| Observations | 2215070 | 265244 | 2144405 | 254099 | 2311627 | 282494 | 2311627 | 282494 |
| $R^2$ | 0.104 | 0.115 | 0.050 | 0.049 | 0.072 | 0.104 | 0.153 | 0.190 |
| Mean of dep. var | 10.4 | 10.2 | 4.62 | 4.42 | 0.96 | 0.94 | 332.5 | 321.3 |

Notes: The table returns coefficients $\alpha_j$ from regression equation (1). The displacement occurred between t=-1 and t=0. The sample is restricted to pre-financial crisis baseline years, e.g., all years up to 2007. Year $t = -3$ is omitted as the baseline category. The outcome variables are log (earnings) (columns 1 and 2), log wage (columns 3 and 4), employment (columns 5 and 6), and days worked (columns 7 and 8). In all columns, we control for a year since displacement, year, and age polynomials. Standard errors are clustered at the individual level. Migrants are reweighted to natives using individual characteristics, industries, and occupations. ** and * refer to statistical significance at the 1 and 5 percent levels, respectively. Source: IEB.

Table B8: Restricting the Sample to Baseline Years up to 2003 (Pre Financial Crisis)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Log (Earnings) | | Log Wage | | Employment | | Days Worked | |
| | Natives | Migrants | Natives | Migrants | Natives | Migrants | Natives | Migrants |
| Year (Disp) t-5 | 0.018** | 0.042** | 0.0042* | -0.0027 | 0.0029** | 0.0031 | 2.66** | 8.44** |
| | (0.0023) | (0.014) | (0.0019) | (0.012) | (0.00066) | (0.0040) | (0.39) | (2.47) |
| Year (Disp) t-4 | 0.014** | 0.022* | -0.00071 | 0.0099 | -0.000048 | -0.00042* | 1.55** | 4.43** |
| | (0.0015) | (0.0100) | (0.0016) | (0.010) | (0.000030) | (0.00019) | (0.22) | (1.67) |
| Year (Disp) t-2 | -0.0084** | -0.017** | -0.014** | 0.0077 | 0.0022** | 0.00067 | 0.51* | -0.91 |
| | (0.0012) | (0.0051) | (0.0017) | (0.012) | (0.00041) | (0.0013) | (0.21) | (0.84) |
| Year (Disp) t-1 | -0.081** | -0.11** | -0.024** | -0.016 | -0.0000073 | 0.0013* | -17.6** | -23.0** |
| | (0.0013) | (0.0083) | (0.0018) | (0.012) | (0.000040) | (0.00052) | (0.21) | (1.29) |
| Year (Disp) t | -0.59** | -0.69** | -0.20** | -0.20** | -0.15** | -0.18** | -119.9** | -138.2** |
| | (0.0041) | (0.025) | (0.0031) | (0.019) | (0.0013) | (0.0093) | (0.56) | (3.89) |
| Year (Disp) t+1 | -0.37** | -0.51** | -0.18** | -0.22** | -0.13** | -0.17** | -72.6** | -96.1** |
| | (0.0041) | (0.029) | (0.0030) | (0.022) | (0.0014) | (0.011) | (0.60) | (4.32) |
| Year (Disp) t+2 | -0.27** | -0.38** | -0.16** | -0.20** | -0.10** | -0.15** | -50.0** | -73.1** |
| | (0.0041) | (0.027) | (0.0031) | (0.020) | (0.0015) | (0.011) | (0.62) | (4.26) |
| Year (Disp) t+3 | -0.23** | -0.30** | -0.15** | -0.15** | -0.083** | -0.12** | -39.5** | -56.6** |
| | (0.0041) | (0.026) | (0.0033) | (0.020) | (0.0015) | (0.010) | (0.62) | (4.26) |
| Year (Disp) t+4 | -0.19** | -0.25** | -0.14** | -0.11** | -0.071** | -0.095** | -32.0** | -45.3** |
| | (0.0041) | (0.028) | (0.0033) | (0.031) | (0.0015) | (0.010) | (0.62) | (4.09) |
| Year (Disp) t+5 | -0.17** | -0.16** | -0.13** | -0.11** | -0.063** | -0.097** | -27.0** | -38.2** |
| | (0.0040) | (0.025) | (0.0034) | (0.022) | (0.0015) | (0.011) | (0.62) | (4.51) |
| Observations | 1469255 | 150594 | 1418583 | 143951 | 1540502 | 161467 | 1540502 | 161467 |
| $R^2$ | 0.103 | 0.112 | 0.049 | 0.046 | 0.077 | 0.109 | 0.162 | 0.198 |
| Mean of dep. var | 10.3 | 10.2 | 4.62 | 4.45 | 0.95 | 0.93 | 329.7 | 317.8 |

Notes: The table returns coefficients $\alpha_j$ from regression equation (1). The displacement occurred between t=-1 and t=0. The sample is restricted to the pre-financial crisis baseline years, e.g., all years up to 2003. Year $t = -3$ is omitted as the baseline category. The outcome variables are log (earnings) (columns 1 and 2), log wage (columns 3 and 4), employment (columns 5 and 6), and days worked (columns 7 and 8). In all columns, we control for a year since displacement, year, and age polynomials. Standard errors are clustered at the individual level. Migrants are reweighted to natives using individual characteristics, industries, and occupations. ** and * refer to statistical significance at the 1 and 5 percent levels, respectively. Source: IEB.

Table B9: Restricting the Sample to a Workplace in West Germany at the Time of Displacement

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Log (Earnings) | | Log Wage | | Employment | | Days Worked | |
| | Natives | Migrants | Natives | Migrants | Natives | Migrants | Natives | Migrants |
| Year (Disp) t-5 | 0.025** | 0.0081 | 0.014** | -0.056 | 0.0037** | -0.0043 | 3.40** | 11.3 |
| | (0.0030) | (0.037) | (0.0022) | (0.034) | (0.00081) | (0.019) | (0.49) | (8.19) |
| Year (Disp) t-4 | 0.018** | 0.059 | 0.0039* | 0.017 | -0.00012** | -0.00030 | 2.11** | 12.0* |
| | (0.0018) | (0.034) | (0.0018) | (0.025) | (0.000043) | (0.00079) | (0.27) | (5.18) |
| Year (Disp) t-2 | -0.015** | -0.018 | -0.020** | -0.0017 | 0.00068 | 0.0033 | -0.35 | 1.55 |
| | (0.0014) | (0.017) | (0.0017) | (0.022) | (0.00040) | (0.0022) | (0.24) | (1.76) |
| Year (Disp) t-1 | -0.091** | -0.10** | -0.026** | -0.031 | 0.000026 | 0.00026 | -20.0** | -22.3** |
| | (0.0016) | (0.019) | (0.0019) | (0.021) | (0.000064) | (0.0015) | (0.27) | (2.96) |
| Year (Disp) t | -0.61** | -0.79** | -0.20** | -0.20** | -0.14** | -0.20** | -120.4** | -148.8** |
| | (0.0049) | (0.092) | (0.0037) | (0.052) | (0.0016) | (0.023) | (0.66) | (12.3) |
| Year (Disp) t+1 | -0.35** | -0.56** | -0.17** | -0.28** | -0.12** | -0.17** | -70.2** | -91.9** |
| | (0.0049) | (0.095) | (0.0034) | (0.058) | (0.0017) | (0.031) | (0.71) | (13.5) |
| Year (Disp) t+2 | -0.26** | -0.41** | -0.15** | -0.27** | -0.093** | -0.15** | -48.0** | -66.9** |
| | (0.0048) | (0.069) | (0.0035) | (0.052) | (0.0017) | (0.023) | (0.72) | (10.5) |
| Year (Disp) t+3 | -0.22** | -0.42** | -0.14** | -0.20** | -0.078** | -0.13** | -38.3** | -67.4** |
| | (0.0049) | (0.083) | (0.0036) | (0.049) | (0.0018) | (0.023) | (0.73) | (11.4) |
| Year (Disp) t+4 | -0.19** | -0.28** | -0.13** | -0.15** | -0.069** | -0.15** | -32.0** | -58.4** |
| | (0.0048) | (0.062) | (0.0037) | (0.050) | (0.0018) | (0.028) | (0.73) | (11.6) |
| Year (Disp) t+5 | -0.17** | -0.21** | -0.12** | -0.15** | -0.061** | -0.13** | -27.8** | -47.6** |
| | (0.0047) | (0.061) | (0.0038) | (0.046) | (0.0018) | (0.025) | (0.73) | (11.0) |
| Observations | 1021363 | 23973 | 983096 | 22811 | 1068103 | 25903 | 1068103 | 25903 |
| $R^2$ | 0.110 | 0.135 | 0.054 | 0.061 | 0.074 | 0.118 | 0.164 | 0.209 |
| Mean of dep. var | 10.2 | 10.1 | 4.43 | 4.33 | 0.96 | 0.93 | 328.7 | 313.8 |

Notes: The table returns coefficients $\alpha_j$ from regression equation (1). The displacement occurred between t=-1 and t=0. The sample is restricted to workers employed in West Germany at the time of displacement. Year $t = -3$ is omitted as the baseline category. The outcome variables are log (earnings) (columns 1 and 2), log wage (columns 3 and 4), employment (columns 5 and 6), and days worked (columns 7 and 8). In all columns, we control for a year since displacement, year, and age polynomials. Standard errors are clustered at the individual level. Migrants are reweighted to natives using individual characteristics, industries, and occupations. ** and * refer to statistical significance at the 1 and 5 percent levels, respectively. Source: IEB.

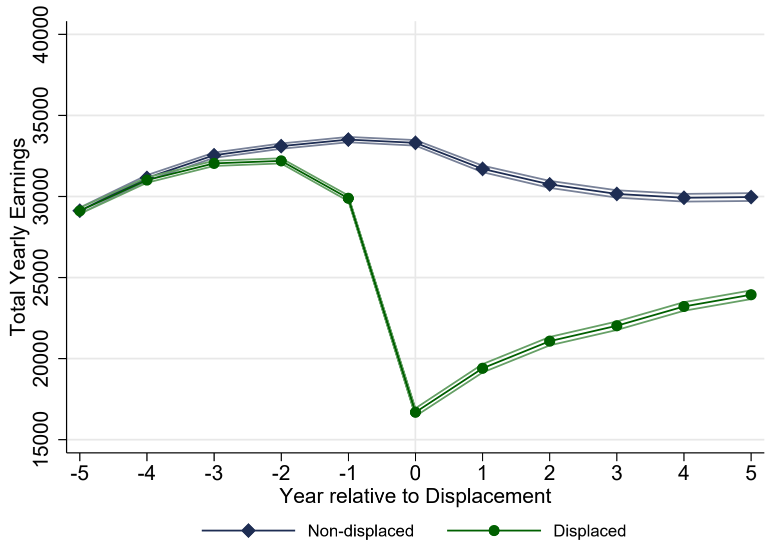
Table B10: Overview of Origin Groups as in Battisti et al. (2021)

| | (1) Group name | (2) Countries | |
|---|---|---|---|
| 1 | Germany | Germany | |
| 2 | Western incl. Western European Countries | Australia | New Zealand |
| | | Austria | Norway |
| | | Canada | Portugal |
| | | Denmark | Samoa |
| | | Finland | Spain |
| | | France | Sweden |
| | | Greece | Switzerland |
| | | Italy | United Kingdom |
| | | Ireland | USA |
| | | Netherlands | |
| 3 | Eastern Europe | Czech Republic | Slovakia |
| | | Hungary | Slovenia |
| | | Poland | |
| 4 | South-Eastern Europe | Albania | Former Jugoslavia |
| | | Bosnia and Herzegovina | Northmazedonia |
| | | Bulgaria | Mazedonia |
| | | Kosovo | Romania |
| | | Croatia | Serbia |
| 5 | Turkey | Turkey | |
| 6 | Former USSR | Armenia | Lithuania |
| | | Azerbaijan | Moldova |
| | | Belarus | Russian Federation |
| | | Estonia | Tajikistan |
| | | Georgia | Turkmenistan |
| | | Kazakhstan | Ukraine |
| | | Kyrgyzstan | Uzbekistan |
| | | Latvia | |
| 7 | Asia and Middle East | | |
| 8 | Africa | | |
| 9 | Central and South America | | |
| 10 | Other | | |

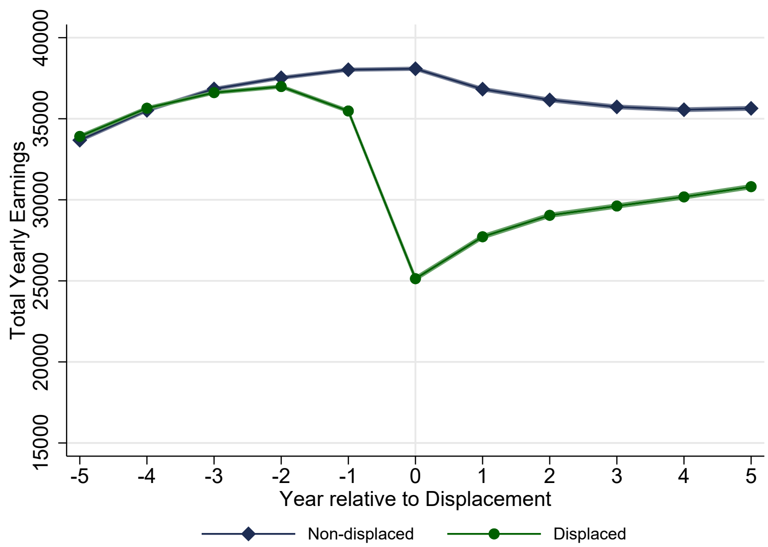
Notes: This table shows how we assign migrants to origin groups following Battisti, Felbermayr, Peri, and Poutvaara (2018). The category "Other" contains origin countries that rarely appear in our data. These include, amongst other islands, the Fiji Islands, the Marshall Islands, and Andorra.

Figure B1: Raw Earnings Losses by Migration Status

(a) Total Yearly Earnings - Migrants



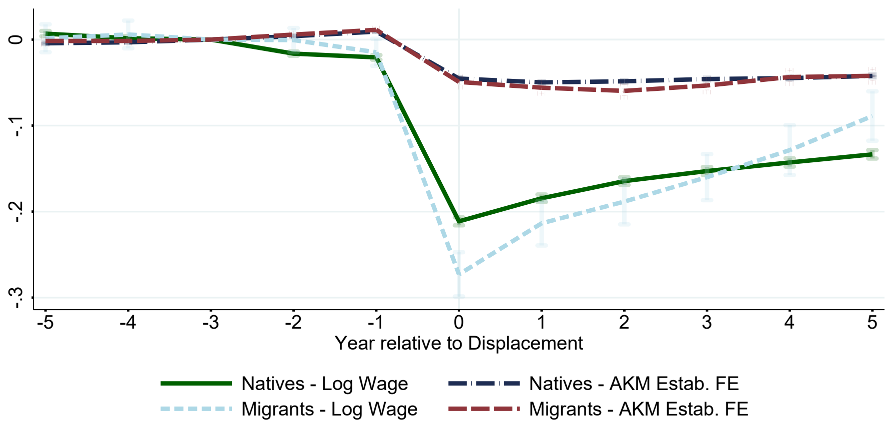

(b) Total Yearly Earnings - Natives



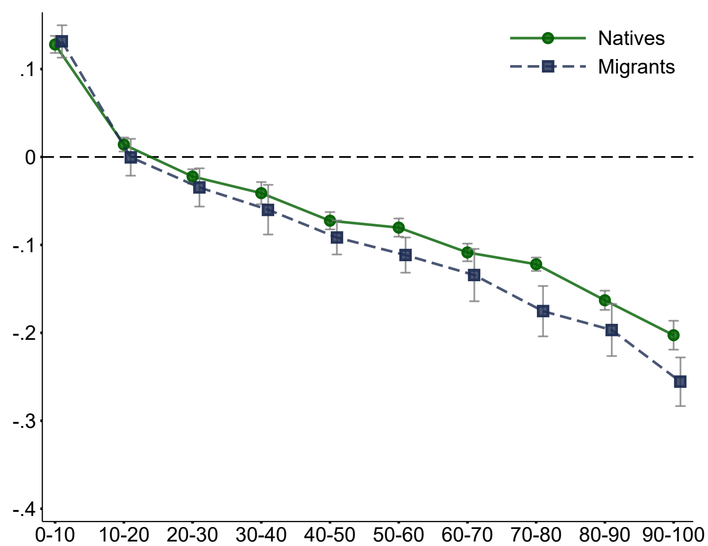

**Notes:** Panel (a) plots the raw means in total yearly earnings (EUR) for non-displaced (blue diamonds) and displaced (green dots) migrants. Panel (b) plots the raw means in total yearly earnings for (EUR) for non-displaced (blue diamonds) and displaced (green dots) natives. Vertical bars indicate the 95% confidence interval. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B2: The Role of AKM Establishment Fixed Effects

(a) Log Wages and AKM Estab. Fixed Effects

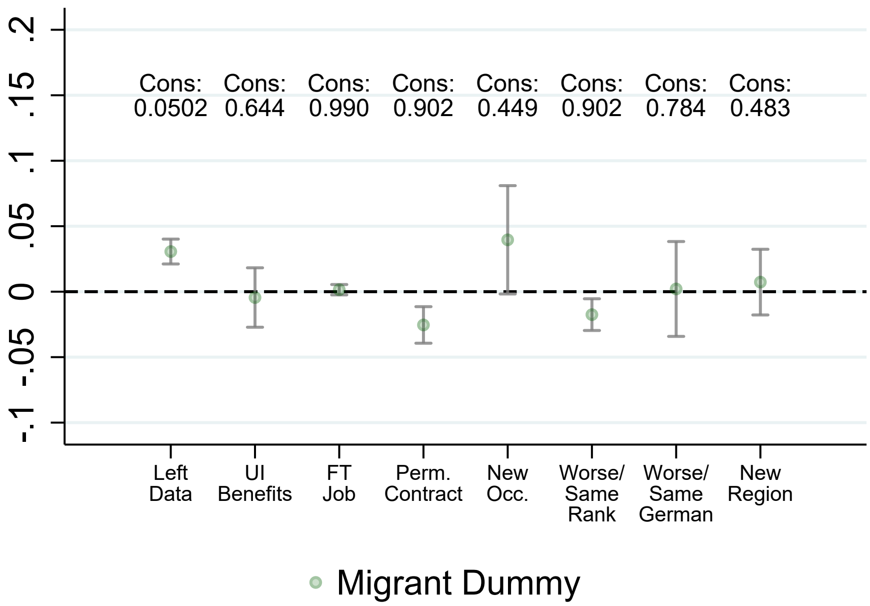



(b) AKM Estab. Fixed Effects by Pre-Displ. AKM Decile



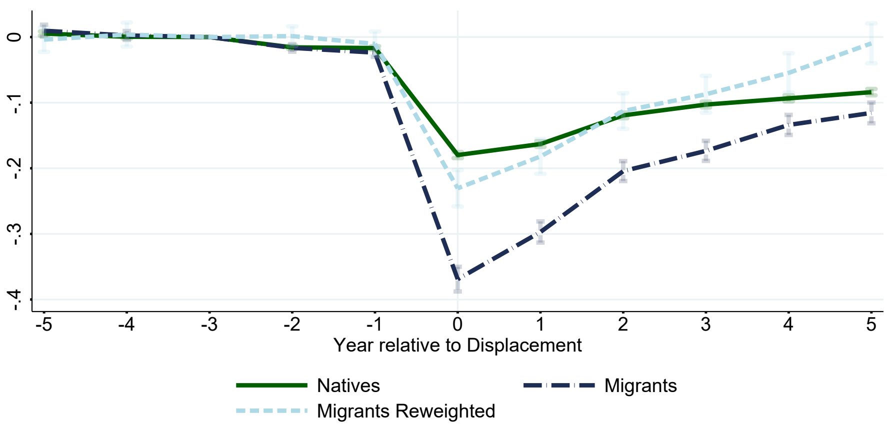

**Notes:** Panel (a) plots event study regression coefficients on the differential evolution of log wages for natives (dark green, solid line) and migrants (light-blue, dashed line), as well as AKM establishment fixed effects for natives (dark blue, dashed line) and migrants (red dashed line). We reweight migrants to natives based on both individual and establishment characteristics. Panel (b) plots losses of AKM establishment fixed effects by pre-displacement decile of AKM establishment fixed effect. We plot coefficients from an OLS regression where we regress workers' individual difference-in-differences AKM establishment fixed effects on dummies for the 10 deciles, as well as individual and establishment controls. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B3: Search Behavior by Migration Status




**Notes:** This figure plots search outcomes for displaced workers. We plot coefficients from an OLS regression where we regress workers' search outcomes on a migrant dummy using our baseline weights. The constant reports the average effect for natives, while the green dots present the coefficient on the migrant dummy. The first outcome variable "left data" is one if a worker leaves our sample and does not return within our observation period. The second outcome variable "UI benefits" is one if a worker ever registers as an unemployed job-seeker, conditional on not leaving the data. The remaining coefficients are conditional on registering as an unemployed job seeker. They show workers' likelihood to search for a full-time job ("FT Job"), for a permanent as opposed to a fixed-term contract ("Perm. Contract"), for a different 3-digit occupation ("New Occ."), for an occupation with a worse or same wage rank ("Worse/Same Rank"), for an occupation which requires worse or the same German skills ("Worse/Same German"), and whether workers are willing to accept job offers outside commuting distance ("New Region"). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

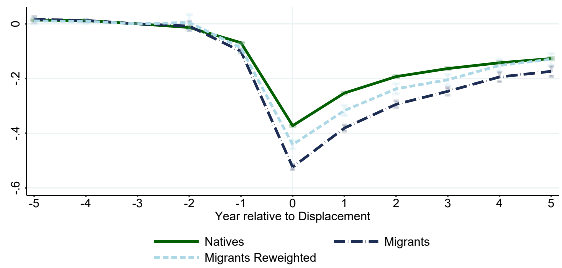
Figure B4: The Evolution of Wages for Workers Re-Employed in t=1



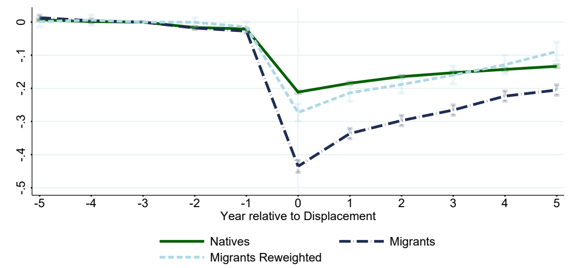

**Notes:** This figure replicates Panel (b) of Figure 1 from the main paper, but restricting to a sample of workers who were re-employed in t=1. The figure plots event study regression coefficients on the differential evolution of log wages for displaced vs. non-displaced workers. The solid green line plots coefficients for the sample of native workers, the dashed blue line plots coefficients for the sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. We reweight based on both individual and establishment characteristics. Individual characteristics are: Age, tenure, education (all in years), and 1-digit occupations (all measured in t=-1). Establishment characteristics are: Log establishment size, the share of migrant workers, the share of high-skilled workers, the share of full-time workers, and city residency (all measured in t=-1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced in 2001-2011, and they are observed from 1997-2016.

Figure B5: Labor Market Outcomes by Migration Status - Including Return Migrants

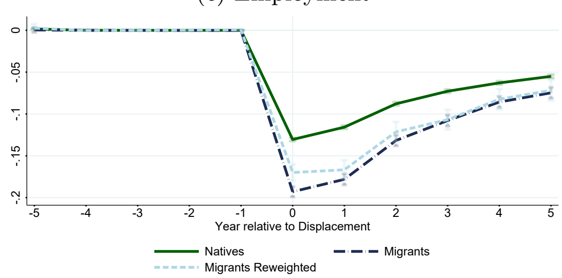
(a) Yearly Earnings Rel. To t=-2



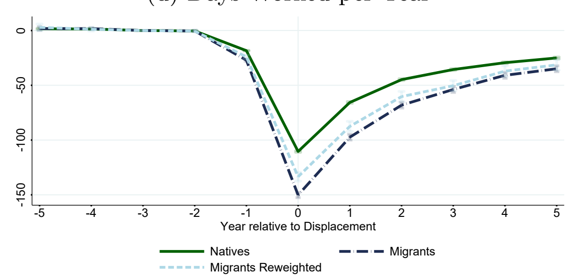

(b) Log Daily Wages




(c) Employment

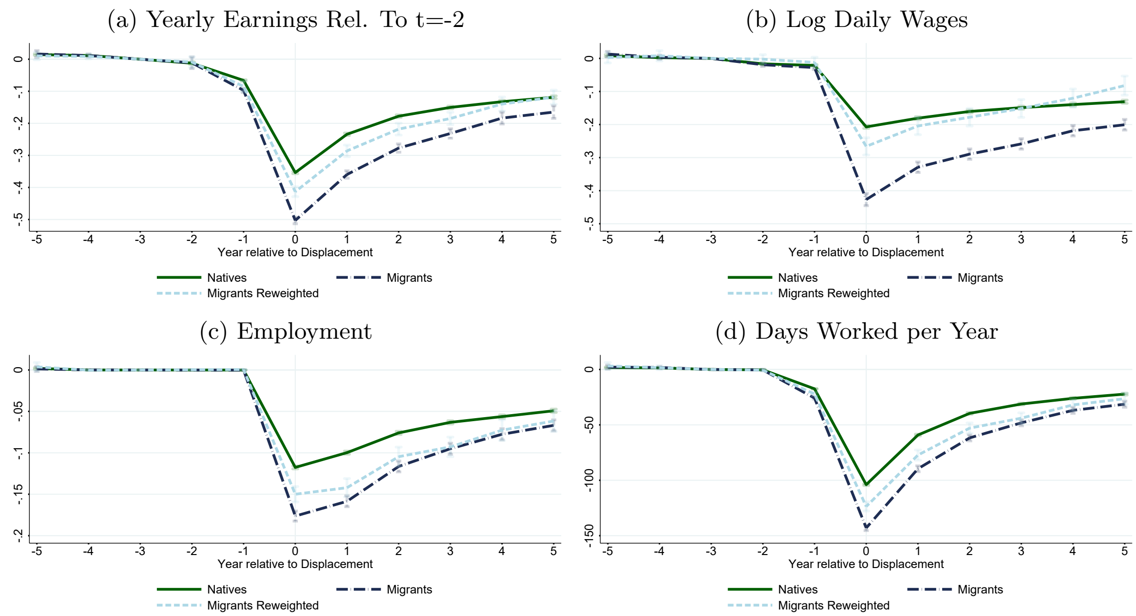



(d) Days Worked per Year



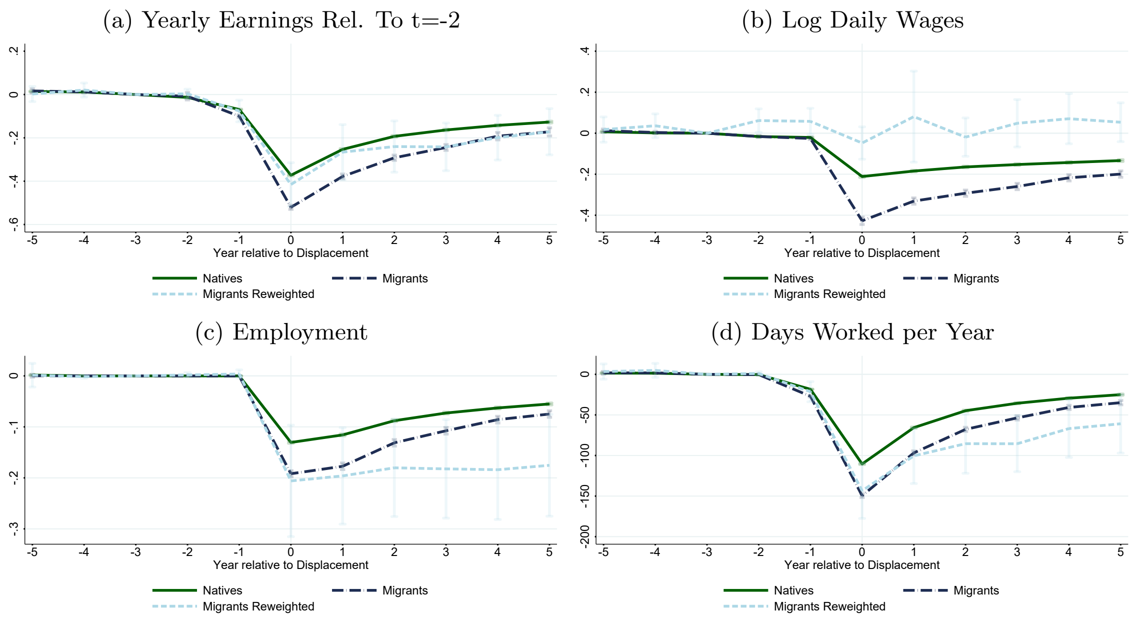

**Notes:** This figure replicates Figure 1 from the main paper but includes return migrants. Technically, we do this by assigning workers 0 earnings, employment, and days worked whenever they are not observed in the data. We thus pretend that workers who drop out of the data (for whatever reason) are unemployed with 0 earnings. The figure plots event study regression coefficients on the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel A), log wages (Panel B), employment (Panel C), and days worked per year (Panel D). The solid green line plots coefficients for the sample of native workers, the dashed blue line plots coefficients for the sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. We reweight based on both individual and establishment characteristics. Individual characteristics are Age, tenure, education (all in years), and 1-digit occupations (all measured in t=-1). Establishment characteristics are Log establishment size, the share of migrant workers, the share of high-skilled workers, the share of full-time workers, and city residency (all measured in t=-1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B6: Labor Market Outcomes by Migration Status - Excluding Return Migrants for the Full Observation Period



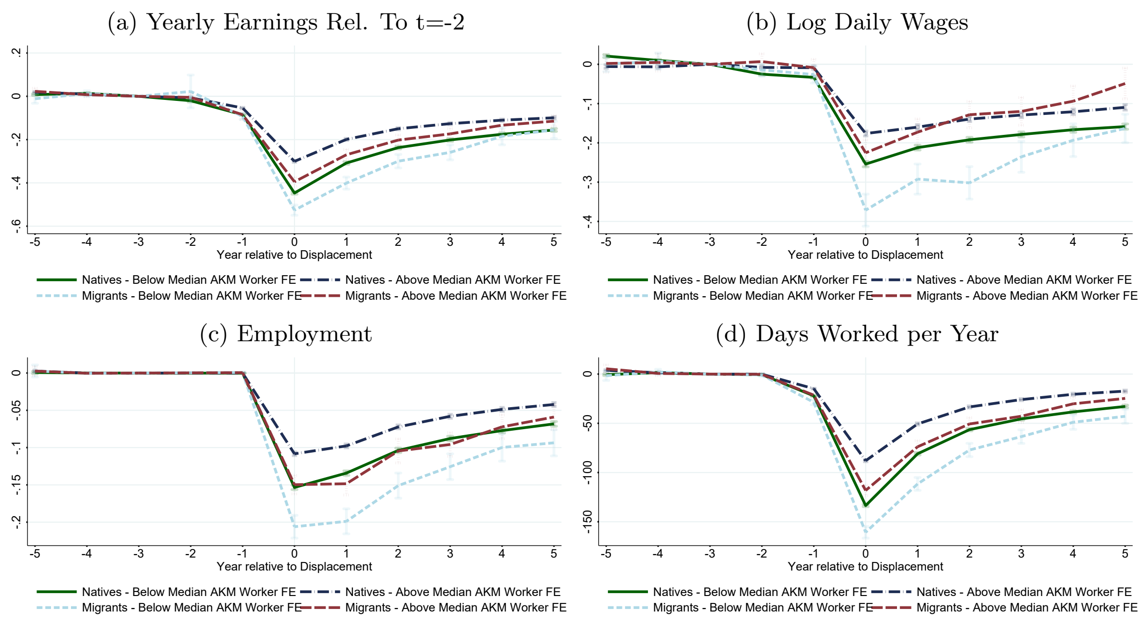

**Notes:** This figure replicates Figure 1 from the main paper but excludes return migrants for the full observation period. That is, while in the baseline results we keep workers in the sample up to the last year that we observe them in the German administrative data, here we drop workers who disappear from the data without returning from the sample. This means that we drop workers who are displayed in columns (2) and (4) in Table B4, and their respective matches. The figure plots event study regression coefficients on the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel A), log wages (Panel B), employment (Panel C), and days worked per year (Panel D). The solid green line plots coefficients for the sample of native workers, the dashed blue line plots coefficients for the sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. We reweight based on both individual and establishment characteristics. Individual characteristics are Age, tenure, education (all in years), and 1-digit occupations (all measured in t=-1). Establishment characteristics are Log establishment size, the share of migrant workers, the share of high-skilled workers, the share of full-time workers, and city residency (all measured in t=-1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B7: Labor Market Outcomes by Migration Status - Untrimmed Sample



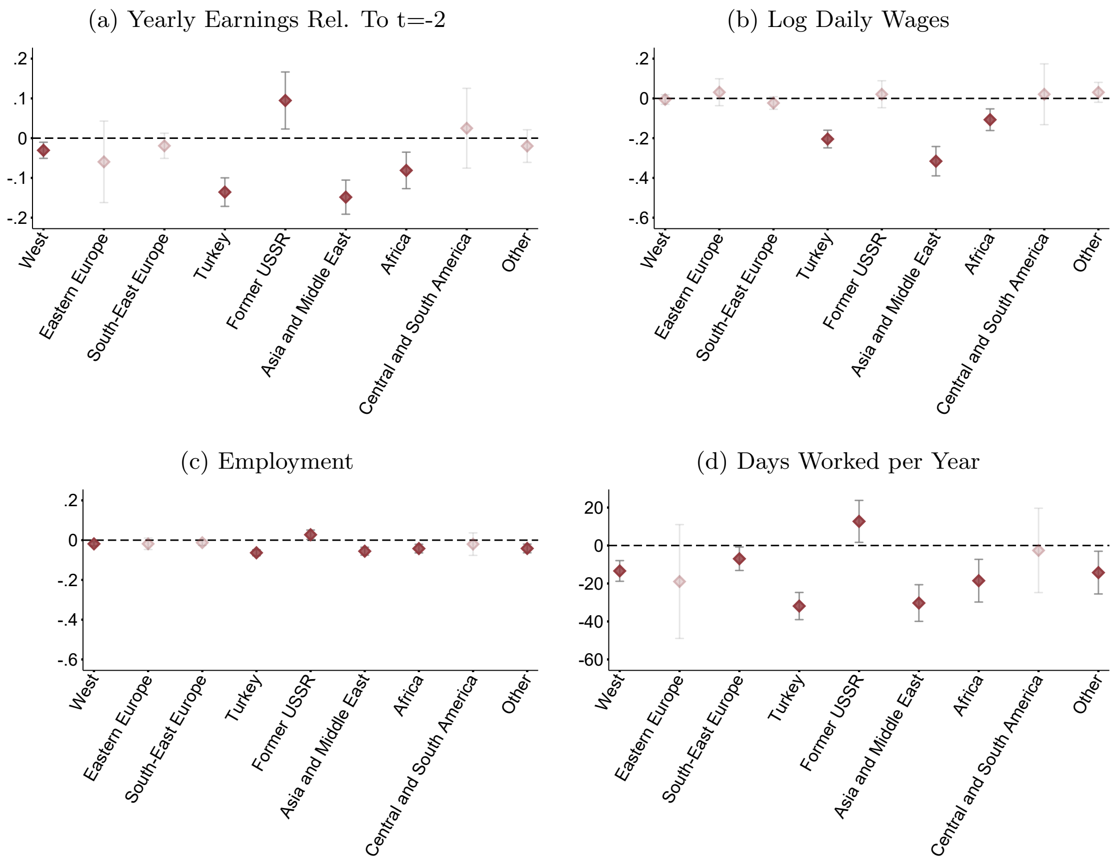

**Notes:** This figure replicates Figure 1 from the main paper, but without trimming the reweighted sample of migrants at the 99th percentile. The figure plots event study regression coefficients showing the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel (a)), log wages (Panel B(b)), employment (Panel (c)), and days worked per year (Panel (d)). The solid green line plots coefficients for the baseline sample of native workers, the dashed blue line plots coefficients for the baseline sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. See Section 3 for details on the reweighting. We compute all estimates using the event study regression equation (1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B8: Labor Market Outcomes by Baseline Worker Productivity - Natives vs. Migrants



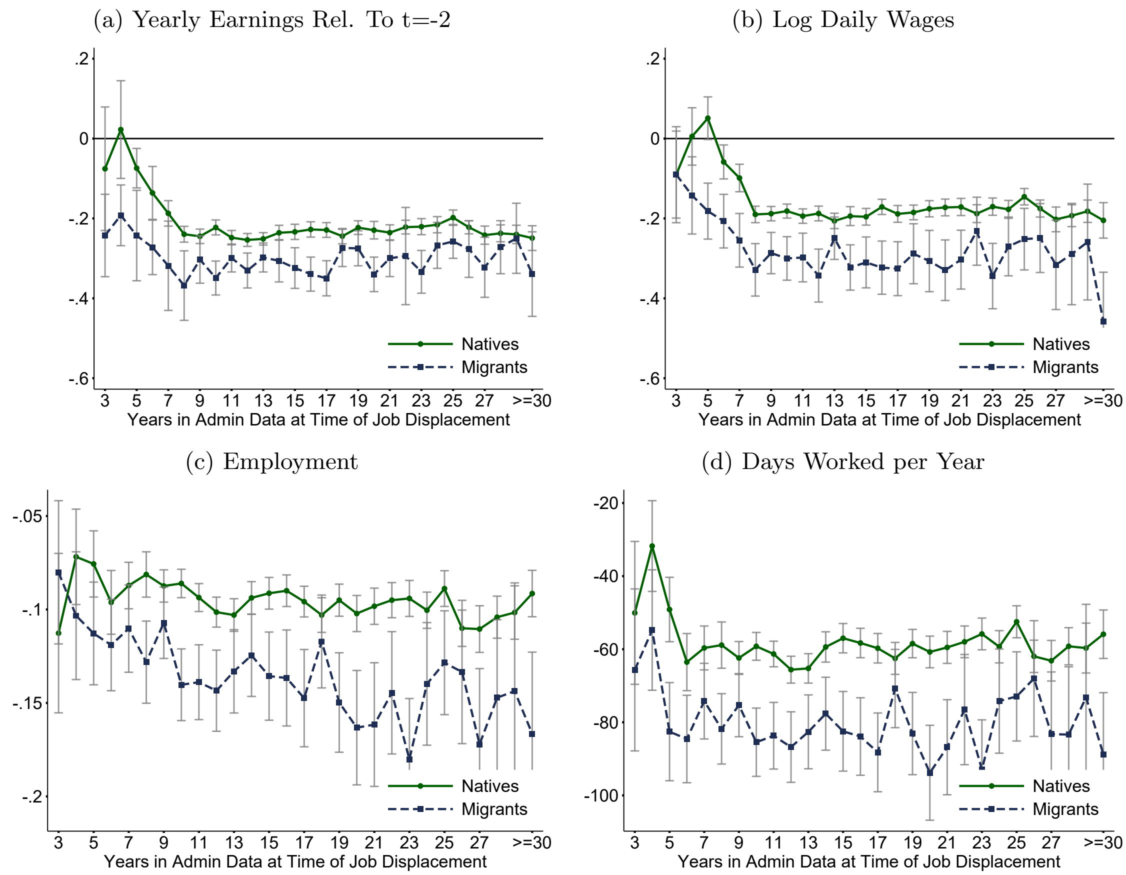

**Notes:** This figure plots event study regression coefficients showing the differential evolution of the following outcomes for displaced vs. non-displaced workers: earnings relative to t=-2 (Panel (a)), log wages (Panel (b)), employment (Panel (c)), and days worked per year (Panel (d)). The solid green line plots coefficients for the sample of native workers with below median baseline worker productivity, the dashed darkblue line plots coefficients for natives with above median baseline worker productivity, the dashed lightblue line plots coefficients for the sample of migrant workers with below median baseline worker productivity, and the dashed red line plots coefficients for the sample of migrant workers with above median baseline worker productivity. We use the IAB's AKM dataset to measure worker productivity in the baseline year, see Appendix A.2 for details. See Section 3 for details on the reweighting. We compute all estimates using the event study regression equation (1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced in 2001-2011, and they are observed from 1997-2016.

Figure B9: Costs of Job Displacement by Origin Group



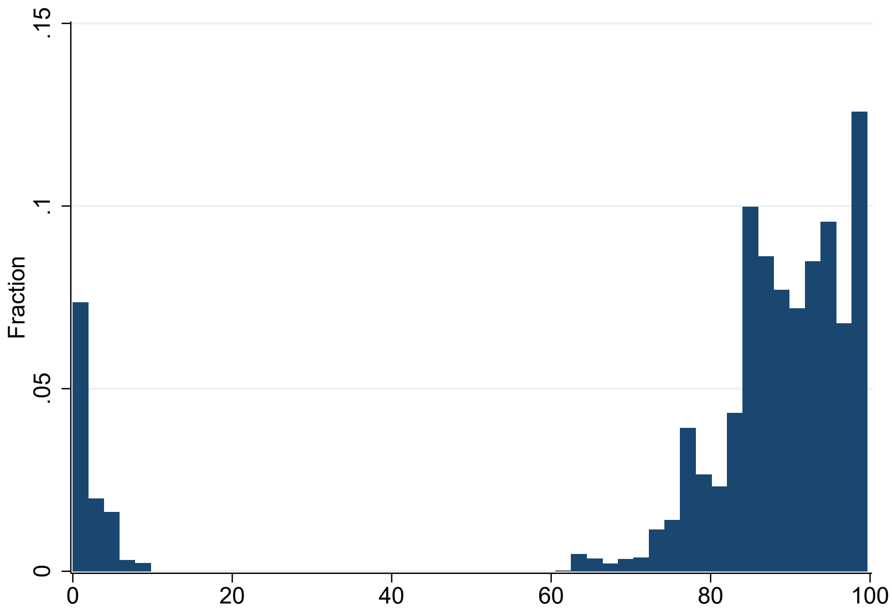

**Notes:** This figure shows how the costs of job displacement differ by origin group. Each panel plots coefficients from a separate OLS regression where we regress workers' individual difference-in-differences outcomes on dummies for the 9 origin groups, with "German origin" as the omitted category. Panel A reports yearly earnings relative to t=-2, Panel B reports log wages, Panel C reports the probability to be employed, and Panel D reports the number of days worked per year. Each difference-in-differences outcome measures differences in the outcome before (t=-5 to t=-2) vs. after (t=0 to t=3) job loss for displaced vs. non-displaced workers, within matched worker pairs. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the displacement establishment level. All regressions control for individual characteristics (age, age squared, years of education, tenure, experience, full-time work, log wage in t=-3, and log establishment size), 1-digit industries, and occupations according to Blossfeld (1987) in the year before displacement.

Figure B10: Costs of Job Displacement by Years in Administrative Data




**Notes:** This figure shows how costs of job loss differ by the time (in years) a worker has been registered in the German administrative data in the year before displacement (t=-1). Panel A reports yearly earnings relative to t=-2, Panel B reports log wages, Panel C reports the probability to be employed, and Panel D reports the number of days worked per year. We regress workers' individual difference-in-differences outcomes on dummies for years in admin data (x-axis), as well as individual, industry, and occupation controls. The solid green line reports coefficients for our sample of native workers, and the dashed blue line reports coefficients for our sample of migrant workers. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the displacement establishment level. Regressions control for individual characteristics (age, age squared, years of education, tenure, experience, full-time work, log wage in t=-3, and log firm size), 1-digit industries, and occupations according to Blossfeld (1987) in the year before displacement.

Figure B11: Distribution Share Same-Nationality Working Age Population in County in t=-1




**Notes:** This figure shows the distribution of the share of same-nationality working-age population in a county in $t = -1$ for our sample of displaced workers. For migrants, the share ranges from 0-10%; for natives, it ranges from 60-100%. Workers in our sample are displaced in the period 2001-2011, and they are observed from 1996 to 2017. In $t = -1$, we observe 17,605 displaced migrants and 129,701 displaced natives. Source: Destatis.

# C Replication of Main Results for Women

Table C1: Displaced and Non-displaced Female Workers in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| **Panel A: Individual Characteristics** | | | | |
| Years of Education | 11.2 | 11.1 | 12.2 | 12.2 |
| | [1.78] | [1.75] | [1.81] | [1.79] |
| Age | 38.5 | 38.6 | 39.3 | 39.4 |
| | [7.51] | [7.47] | [7.29] | [7.16] |
| Tenure | 6.08 | 6.09 | 6.13 | 6.15 |
| | [2.49] | [2.45] | [2.44] | [2.37] |
| Real Daily Wage | 75.5 | 74.6 | 90.1 | 88.8 |
| | [30.8] | [31.4] | [34.0] | [34.0] |
| Total Yearly Earnings | 27883.6 | 24755.6 | 32927.3 | 30692.0 |
| | [11291.0] | [11584.8] | [12537.0] | [12874.7] |
| Days Worked in Year | 362.1 | 328.0 | 363.1 | 342.7 |
| | [18.4] | [59.8] | [14.1] | [48.6] |
| **Panel B: Regional Characteristics** | | | | |
| Lives in City | 0.75 | 0.77 | 0.60 | 0.59 |
| | [0.43] | [0.42] | [0.49] | [0.49] |
| Lives in East Germany | 0.044 | 0.053 | 0.31 | 0.37 |
| | [0.20] | [0.22] | [0.46] | [0.48] |
| Local UR Change | 0.019 | 0.032 | 0.019 | 0.029 |
| | [0.14] | [0.14] | [0.12] | [0.13] |
| **Panel C: Establishment Characteristics** | | | | |
| Establishment Size | 238.1 | 248.9 | 586.3 | 620.2 |
| | [374.2] | [343.3] | [1177.7] | [944.9] |
| Share Migrant Workers | 0.21 | 0.23 | 0.057 | 0.060 |
| | [0.19] | [0.20] | [0.082] | [0.090] |
| Share High-Skilled Workers | 0.092 | 0.085 | 0.15 | 0.14 |
| | [0.14] | [0.13] | [0.17] | [0.16] |
| Share Marginally Employed Workers | 0.11 | 0.093 | 0.076 | 0.062 |
| | [0.17] | [0.17] | [0.14] | [0.13] |
| Displaced from Complete Closure | 0 | 0.28 | 0.00015 | 0.24 |
| | [0] | [0.45] | [0.012] | [0.43] |
| Number of Observations | 5812 | 5812 | 64871 | 64871 |

**Notes:** This table shows the characteristics of displaced and non-displaced female workers in the year prior to displacement. Workers satisfy the following baseline restrictions: male, aged 24 to 50, working full-time, at least 3 years of tenure, working in an establishment with at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells (see Section 3 for more details). Columns (1) and (2) report summary statistics for non-displaced and displaced migrants. Columns (3) and (4) report summary statistics for non-displaced and displaced natives. Standard deviations in brackets.

Table C2: Female Workers' Distribution Across Industries in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| Agriculture | 0.00052 | 0.00052 | 0.0015 | 0.0015 |
| | [0.023] | [0.023] | [0.039] | [0.039] |
| Mining, Energy | 0.00017 | 0.00017 | 0.0026 | 0.0026 |
| | [0.013] | [0.013] | [0.051] | [0.051] |
| Food Manufacturing | 0.13 | 0.13 | 0.050 | 0.050 |
| | [0.33] | [0.33] | [0.22] | [0.22] |
| Consumption Goods | 0.12 | 0.12 | 0.097 | 0.097 |
| | [0.33] | [0.33] | [0.30] | [0.30] |
| Production Goods | 0.063 | 0.063 | 0.040 | 0.040 |
| | [0.24] | [0.24] | [0.20] | [0.20] |
| Investment Goods | 0.16 | 0.16 | 0.081 | 0.081 |
| | [0.36] | [0.36] | [0.27] | [0.27] |
| Construction | 0.0052 | 0.0052 | 0.015 | 0.015 |
| | [0.072] | [0.072] | [0.12] | [0.12] |
| Retail | 0.15 | 0.15 | 0.17 | 0.17 |
| | [0.36] | [0.36] | [0.38] | [0.38] |
| Traffic, Telecommunication | 0.043 | 0.043 | 0.044 | 0.044 |
| | [0.20] | [0.20] | [0.21] | [0.21] |
| Credit, Insurance | 0.014 | 0.014 | 0.028 | 0.028 |
| | [0.12] | [0.12] | [0.17] | [0.17] |
| Restaurants | 0.031 | 0.031 | 0.013 | 0.013 |
| | [0.17] | [0.17] | [0.11] | [0.11] |
| Education | 0.010 | 0.010 | 0.15 | 0.15 |
| | [0.10] | [0.10] | [0.35] | [0.35] |
| Health | 0.031 | 0.031 | 0.042 | 0.042 |
| | [0.17] | [0.17] | [0.20] | [0.20] |
| Commercial Services | 0.19 | 0.19 | 0.14 | 0.14 |
| | [0.39] | [0.39] | [0.35] | [0.35] |
| Other Services | 0.032 | 0.032 | 0.030 | 0.030 |
| | [0.18] | [0.18] | [0.17] | [0.17] |
| Non-Profit | 0.0069 | 0.0069 | 0.021 | 0.021 |
| | [0.083] | [0.083] | [0.14] | [0.14] |
| Public Administration | 0.0095 | 0.0095 | 0.074 | 0.074 |
| | [0.097] | [0.097] | [0.26] | [0.26] |
| Number of Observations | 5812 | 5812 | 64871 | 64871 |

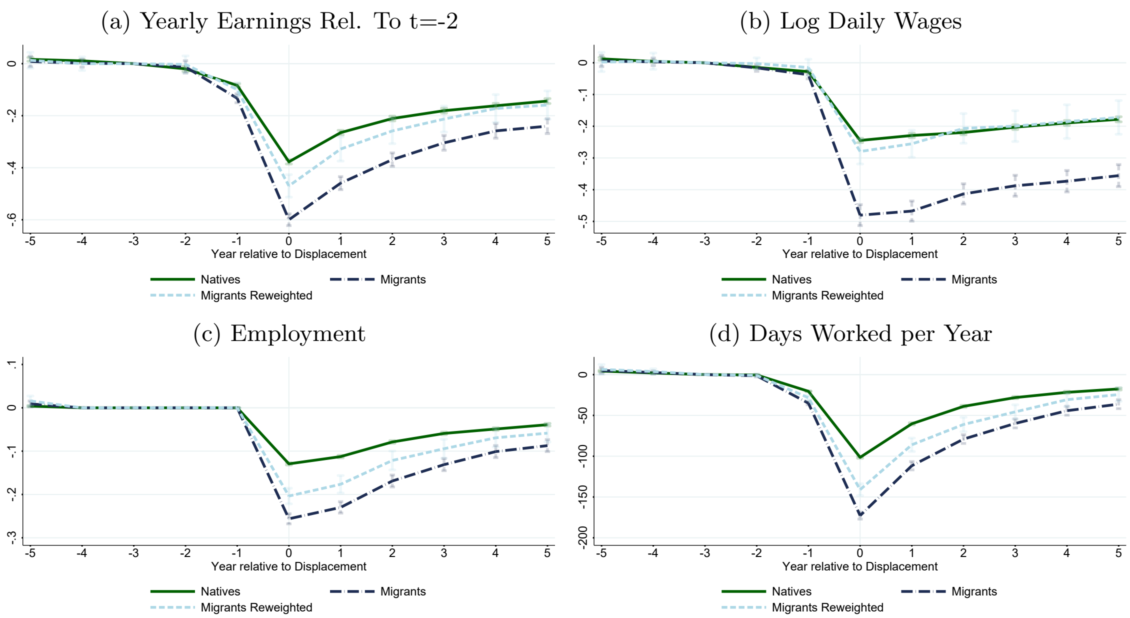
**Notes:** This table shows the distribution across industries of displaced and nondisplaced female workers in the year before the displacement year. Workers satisfy the following baseline restrictions: Aged 24 to 50, working full-time in predisplacement year, at least 3 years of tenure, and the establishment has at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells. The non-displaced sample of workers is a random sample of workers (one per displaced worker) who satisfy the same baseline restrictions. Standard deviations in brackets.

Table C3: Female Workers' Distribution Across Occupations in t=-1

| | (1) Non-Displaced Migrants | (2) Displaced Migrants | (3) Non-Displaced Natives | (4) Displaced Natives |
|---|---|---|---|---|
| Agriculture, gardening, work with animals | 0.0015 | 0.0010 | 0.0046 | 0.0037 |
| | [0.039] | [0.032] | [0.068] | [0.061] |
| Simple, manual tasks | 0.38 | 0.43 | 0.13 | 0.15 |
| | [0.48] | [0.49] | [0.34] | [0.36] |
| Qualified, manual tasks | 0.073 | 0.079 | 0.037 | 0.042 |
| | [0.26] | [0.27] | [0.19] | [0.20] |
| Technician | 0.016 | 0.019 | 0.041 | 0.039 |
| | [0.12] | [0.14] | [0.20] | [0.19] |
| Engineer | 0.0086 | 0.0072 | 0.011 | 0.0091 |
| | [0.092] | [0.085] | [0.10] | [0.095] |
| Simple services | 0.17 | 0.16 | 0.069 | 0.060 |
| | [0.38] | [0.36] | [0.25] | [0.24] |
| Qualified services | 0.022 | 0.019 | 0.026 | 0.022 |
| | [0.15] | [0.14] | [0.16] | [0.15] |
| Semi-professions | 0.028 | 0.030 | 0.095 | 0.080 |
| | [0.16] | [0.17] | [0.29] | [0.27] |
| Professions | 0.0058 | 0.0033 | 0.015 | 0.015 |
| | [0.076] | [0.057] | [0.12] | [0.12] |
| Simple commercial and administrative tasks | 0.093 | 0.078 | 0.11 | 0.11 |
| | [0.29] | [0.27] | [0.32] | [0.31] |
| Qualified commercial and administrative tasks | 0.19 | 0.17 | 0.43 | 0.44 |
| | [0.39] | [0.37] | [0.50] | [0.50] |
| Manager | 0.010 | 0.010 | 0.023 | 0.021 |
| | [0.10] | [0.10] | [0.15] | [0.14] |
| Not classified | 0.0038 | 0.0022 | 0.0021 | 0.0029 |
| | [0.061] | [0.047] | [0.046] | [0.054] |
| Number of Observations | 5812 | 5812 | 64871 | 64871 |

**Notes:** This table shows the distribution across occupations according to Blossfeld (1987) of displaced and non-displaced female workers in the year before the displacement year. Workers satisfy the following baseline restrictions: Aged 24 to 50, working full-time in the pre-displacement year, at least 3 years of tenure, and the establishment has at least 50 employees. Non-displaced workers are matched to displaced workers using propensity score matching within a year and industry cells. The non-displaced sample of workers is a random sample of workers (one per displaced worker) who satisfy the same baseline restrictions. Standard deviations in brackets.

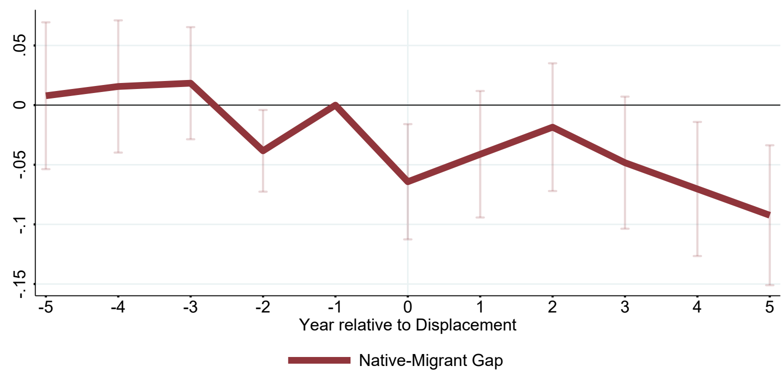
Figure C1: Labor Market Outcomes by Migration Status - Female Workers



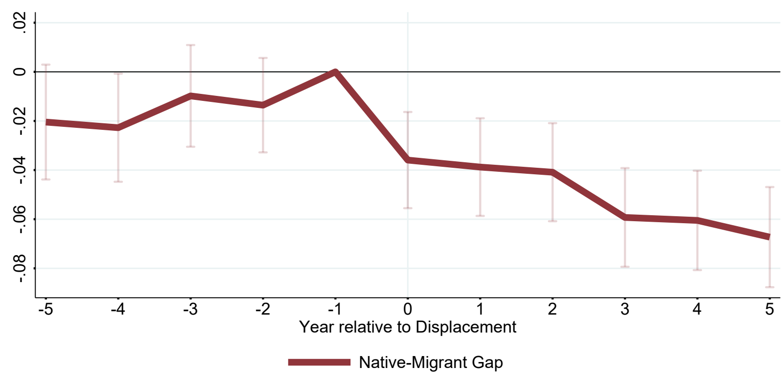

**Notes:** This figure plots event study regression coefficients showing the differential evolution of the following outcomes for a sample of female displaced vs. non-displaced workers: earnings relative to t=-2 (Panel (a)), log wages (Panel B(b)), employment (Panel (c)), and days worked per year (Panel (d)). The solid green line plots coefficients for the baseline sample of native workers, the dashed blue line plots coefficients for the baseline sample of migrant workers, and the light blue line plots coefficients for the sample of reweighted migrant workers. See Section 3 for details on the reweighting. We compute all estimates using the event study regression equation (1). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure C2: Replication of the Main Results for Within-Firm Data - Women

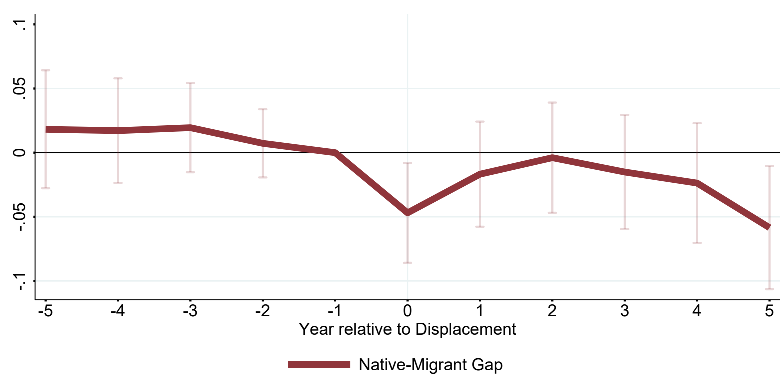
(a) Log Earnings




(b) Employment




(c) Log Wages




**Notes:** This figure plots the migrant-native earnings, employment, and wage gap for a sample of female displaced workers who were displaced from the same establishment and 3-digit occupation in the same year. In addition, we require workers to have the same age at the time of displacement, the same years of tenure, and the same years of education. Workers moreover need to be in a full-time job before displacement. We plot coefficients from a regression of the outcome variable on migrant status interacted with time since displacement, time since displacement dummies, year dummies, and individual fixed effects. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.