The Labor Market Costs of Job Displacement by Migrant Status*

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Abstract

This paper examines the differential impact of job displacement on migrants and natives. Using administrative data for Germany from 1997-2016, we identify mass layoffs and estimate the trajectory of earnings and employment of observationally similar migrants and natives displaced from the same establishment. Despite similar pre-layoff careers, migrants' earnings losses are about 23% higher in the first 5 years after displacement. This gap arises from both lower re-employment probabilities and post-layoff wages and is not driven by selective return migration. Key mechanisms include sorting into lower-quality firms and relying on lower-quality coworker networks during job search.

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1 Introduction

Migrant earnings and employment rates are consistently below those of natives in the majority of the OECD.[1] Yet understanding why has proven surprisingly difficult. The standard approach — comparing migrants' and natives' earnings and tracking their convergence over time — faces two key issues. First, it is notoriously difficult to control for the array of differences between migrants and natives. Second, both migrants and natives adapt their behavior in response to labor market conditions, making it difficult to isolate the causal impact of migrant status on outcomes.

In this paper, we address both of these issues. Drawing on German administrative data between 1997 and 2016, we compare the trajectory of earnings and employment of observationally similar migrants and natives who were displaced from the same job and the same establishment in the same mass layoff. The rich administrative data allows us to control for many of the observable and unobservable differences that usually drive the migrant-native gap in outcomes, and our focus on mass layoffs means we are comparing the two groups under the same exogenous circumstances. As a result, this setting allows us to provide evidence on migrant-native gaps for migrants with strong labor market attachment and who have observationally already converged to natives. Any gaps in outcomes post-displacement should thus help us identify persistent barriers to migrant integration that are driven by migrant identity itself.

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.[2] Second, migrants with at least one year of work experience in Germany – and thus all migrants in our sample – are eligible for the same type of unemployment insurance (UI) benefit in the first year following the layoff. 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, they still struggle to take up employment.[3]

Our empirical strategy is based on comparing the post-displacement outcomes of migrants to outcomes of similar native workers laid off from the same establishment.[4] While we are unable to

[1]For example, in 2023, migrant employment was 5.7ppt lower EU-wide, and 1.3 ppt lower for the whole of the OECD. All but 8 OECD countries recorded higher rates of relative poverty for migrants compared to natives, and in all but 6 migrants experienced higher unemployment rates than natives. (OECD and European Commission, 2023).

[2]Note that Germany has a small shadow economy relative to GDP that was below average compared to other OECD countries in 2022 (Schneider and Boockmann, 2022).

[3]Own computations based on a 2% sample of worker biographies provided by the Institute for Employment Research. In August 2022, only 53% of migrants in Germany were employed, compared to 69.2% of natives (Brücker et al., 2022).

[4]While our main analysis focuses on estimating the additional cost of displacement for migrants compared to natives, we also replicate the standard estimates of the cost of job loss by comparing the outcomes of displaced natives and migrants to a matched counterfactual of non-displaced workers. See Appendix Sections A.4 and A.5 for details.

control for all potentially important characteristics – such as migrants' language skills or job search behavior – our empirical strategy allows us to remove a large set of potential drivers of migrant-native differences. Using matched employer-employee data for Germany, we identify all mass layoffs between 2001 and 2011, and estimate individual-level event studies of migrants' wages and employment, relative to displaced natives, up to five years after the displacement. To do this, we use a 2-step matching procedure to find a displaced native counterfactual to each displaced migrant worker, matching on education, 3-digit occupation, demographic characteristics, and pre-layoff wages. Importantly, the migrant-native pairs are laid off from the same establishment in the same year, allowing us to implicitly control for a further set of observable and unobservable characteristics at the establishment level such as productivity and local labor market conditions. Overall, we argue our estimates are very close to capturing the labor market impact of "migrant identity" as such.

We find that migrants experience a large and significant *additional* cost of job displacement on top of the layoff costs for natives. In our matched sample of displaced migrants and natives, natives lose on average 39% of their earnings in the 5 years post-displacement. Migrants face the loss of additional 9 percentage points (ppt). This gap in earnings is driven by both a relatively lower re-employment probability (a gap of 6ppt in the year of the layoff) and lower relative post-displacement wages (a gap of 13 log points in the year of the layoff). These estimates are robust to a wide range of alternative sample restrictions and matching procedures.

To investigate whether our results are driven by return migration, we complement our baseline analysis with a detailed study on the rates at which different types of migrants and natives drop out of the administrative data.[5] We start by documenting that migrants are indeed more likely to drop out than natives following a job displacement. By year 5, migrants are 5.6ppt more likely to not be recorded in the German administrative data and we show that this impacts our estimates of the migrant-native gap in unemployment probability. While migrants are more likely to be registered as unemployed in the years after the layoff, this gap declines starting in year 2 after the layoff. This decline is entirely driven by migrants leaving the administrative data (and potentially Germany) at a faster rate than natives, leaving the gap in employment constant over time.

We find only weak evidence of systematic selection into return migration by worker productivity, as proxied by AKM worker fixed effects (Abowd et al., 1999). We do, however, find a strong pattern in return migration as a function of workers' outside options in their home country. Controlling for worker fixed effects, we show that migrants from countries with a higher net income

[5] Administrative data does not record whether an individual left the country. We use leaving the administrative sample – i.e. being no longer registered as employed or unemployed in the German social-security data – as a proxy for return migration. More broadly, our analysis of sample attrition allows us to evaluate the impact of the changing composition of workers on our baseline estimates regardless of the underlying reason for attrition.

are significantly more likely to drop out of the administrative data. While we lack data on outcomes outside of Germany, this pattern suggests that return migration is more likely to be driven by migrants leaving for better opportunities.

Following the literature (Lubotsky, 2007; Rho and Sanders, 2021), we also provide an alternative estimate of our baseline results for a fixed sample of workers. We restrict our sample to migrants and natives who are always registered in the administrative data (as employed or unemployed) in the 10 years around the layoff. The resulting migrant-native gap in employment is somewhat smaller and shrinks at a faster rate; by year 5, it is only half the size of the gap in the baseline sample (4ppt vs. 8ppt). The wage gap, on the other hand, remains virtually the same. This suggests that return migration can explain only a limited part of the difference in the total cost of job displacement between migrants and natives.

To understand why migrant earnings and employment are relatively worse after job displacement, we turn to the other leading supply-side and demand-side explanations studied in the literature: job search and labor market discrimination. Furthermore, by analyzing firm sorting and the role of coworker networks, we test whether the migrant-native gap can be explained by the matching process in the labor market.

We start by testing whether the negative employment and wage gaps can simply be explained by migrants searching for different types of jobs. Drawing on the rich information in German social security data, we are able to compare the reported job preferences of all job seekers in our baseline sample. We find no evidence that displaced migrants target different jobs or types of employment: In particular, migrants are not less likely to search for full-time positions, nor are they more reluctant to search outside their core occupation or commuting zone. Nevertheless, despite similar job preferences, migrants are more likely to take-up part-time work after displacement, which contributes to their higher loss in daily wages.

Next, we focus on the role of local labor market conditions. Previous literature has shown that the cost of job displacement depends on worker's outside options and general macroeconomics conditions (Bana, 2021; Schmieder et al., 2023); we test whether migrants' labor market outcomes are especially sensitive to these. We find no significant variation with local unemployment rates and only a weak and imprecisely estimated positive effect of occupational market thickness. An alternative way to interpret these results is through the lens of anti-immigrant discrimination. If this discrimination is driven by employer preferences, a profit-maximizing firm should discriminate less in labor markets where hiring is more costly — such as when the unemployment rate is low or labor market thickness is high. As such, our results provide little evidence of this type of discrimination. We also do not find any variation across occupations requiring different degrees of physical proximity, as we would expect if the migrant-native gap stems from taste-based discrimination on the side of employees or consumers. That said, discrimination can take on patterns uncorrelated with labor market conditions, and we do find significant variation in the cost of job displacement by demographic characteristics such as ethnic and cultural proximity to Germany. Moreover, following Sorkin (2025), we also find some evidence of statistical discrimination, especially on the hiring margin. Overall, while it is difficult to draw firm conclusions without direct measures of discrimination, our analysis shows that migrant experience after a job displacement varies significantly across different groups of migrants in ways that make it difficult to rule out anti-immigrant discrimination.

Finally, we examine the matching process itself. Given the importance of firms in pay setting, we test whether negative labor market shocks disrupt workers' progression up the "firms ladder" (Schmieder et al., 2023) for migrants to a greater extent than for natives. Using AKM firm fixed effects to capture firms' role in pay, we show that displaced migrants are on average re-employed in lower-wage firms than their native coworkers. This difference in firms explains about 16% of the average migrant-native gap in wages.[6] The subsequent closing of the wage gap is partially driven by firm sorting: We show that the relatively more productive migrants climb the firm's ladder faster than their native coworkers, which explains about a tenth of their wage growth and catch-up.

We moreover find suggestive evidence that part of the reason why migrants sort into worse firms stems from differences in their use of coworker networks. Existing literature has documented that networks are more important to migrants than to natives when searching for jobs (McKenzie and Rapoport, 2010; Dustmann et al., 2015; Glitz, 2017), which makes them a natural candidate for explaining the migrant-native gap in post-displacement outcomes. To test this hypothesis, we adopt a measure of coworker networks à la Caldwell and Harmon (2019) which captures the employment opportunities at firms employing former colleagues of the displaced worker. We replicate the finding that networks matter more for labor market outcomes of migrants compared to natives, and we show that migrants benefit especially from former migrant coworkers. At the same time, however, we show that migrant networks are smaller and of lower quality than native networks – migrant coworkers work at worse firms and in lower-paying jobs. The overall impact on the migrant-native gap is thus mixed: the migrants who are able to take advantage of their networks do better than their peers, but the worse quality of migrant networks, and the migrants' inability to fully benefit from their native networks, contribute to the migrant-native gap in outcomes.

This paper speaks to several literatures. First, we contribute to the broad literature on economic outcomes and integration of migrants.[7] A number of studies have documented the relatively slow

[6] The average additional loss of wages is 12 log points, and the additional loss in earnings due to the difference in firm FE is 2 log points, i.e. 16% of the average additional loss of wages.

[7] See Berbée and Stuhler (2024) for a comprehensive analysis of migrants' labor market integration into the German labor market since 1976.

economic assimilation of migrants into their destination countries, often driven by large initial differences in education, skills, and the type of jobs migrants sort into (Borjas, 1985; Algan et al., 2010; Abramitzky et al., 2014). Existing literature has also demonstrated that migrants are more prone to, and impacted by, adverse shocks.[8] In general, the large and persistent migrant-native gap in earnings and employment that we find corroborates the large and persistent outcome gaps between migrants and natives more broadly and helps to explain why it often takes decades for migrants to fully catch up with natives. In addition, however, we demonstrate that the migrant-native gap following job displacement goes beyond the observable differences between the two groups, and cannot be easily rationalized by the differences in previous employers, labor market conditions, or worker sorting.

Our finding regarding the differential sorting of migrants across firms is in line with the recent work of Dostie et al. (2023) who 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.[9] Similarly to Patacchini and Zenou (2012), Åslund et al. (2014), Dustmann et al. (2015), and Glitz (2017), we document the importance of professional networks for migrant outcomes, and show that migrants might gain by accessing native coworker networks (Åslund et al., 2024).

Finally, we build on the literature on the cost of job displacement. Studies have documented that displaced workers struggle to get re-employed immediately, and it can take years for their wages to catch up to that of their peers in continuous employment (Jacobson et al., 1993; Schmieder et al., 2023). There is growing evidence that this cost varies significantly across workers.[10] Our paper add to this by estimating the relative cost of job displacement for migrants, a population relatively more exposed to negative labor market shocks.

Closest to our paper, Bratsberg et al. (2018) and Hardoy and Schøne (2014) analyze the gap in job loss costs for migrants and natives in Norway, controlling for migrant-native differences in demographics and firm characteristics. Our approach builds on these studies and goes one step further. By comparing migrants and natives displaced from the same layoff event, 3-digit occupation, and with comparable pre-layoff wages, we ensure that we compare outcomes of displaced *coworkers* with similar ex-ante productivity. In addition, by analyzing a broader set of outcomes,

[8] Migrants' entry wages during recessions are lower than natives' (see, e.g., 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., Dustmann et al. (2010); Hoynes et al. (2012)).

[9] Similar patterns are observed in Sweden (Åslund et al., 2021) and Israel (Arellano-Bover and San, 2024).

[10] Displacement comes at much higher costs for women (Meekes and Hassink, 2022; Illing et al., 2024), workers in routine-intensive occupations (Blien et al., 2021), and low-wage workers in the manufacturing sector (Helm et al., 2023); Athey et al. (2024) present a comprehensive overview of the heterogeneity in the cost of displacement for workers in Sweden. Bertheau et al. (2023) 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.

such as sorting and coworker networks, we also speak to the mechanisms on why migrants' and natives' costs of job displacement are different.

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

2 Data and Institutional Context

For our empirical analysis, we use worker-level data provided by the Institute for Employment Research (IAB), in particular the *Integrated Employment Biographies, v14*. We draw the universe of workers employed at a mass layoff establishment in the year before the layoff, for all displacement events occurring in 2001-2011. We observe workers' employment biographies from 1997 to 2016. We only consider workers born in 1950 or later.

From the worker-level spell data, we construct a yearly panel as of June 30 in a given year, based on the code provided by Dauth and Eppelsheimer (2020). We impute missing education information following Fitzenberger et al. (2006) and we compute years of education based on information on workers' educational attainment: no vocational training, vocational training, or university degree. Whenever an individual is not observed in the data, we assign them zero earnings and employment.

We moreover impute wages based on polynomials of age, tenure, and migrant status following Dustmann et al. (2009). We create a linked employer-employee dataset by merging the worker-level data with establishment-level data on establishments' average wages and workforce composition from the *Establishment History Panel, BHP 7521, v1*. We also add information on worker and establishment fixed effects provided by Lochner et al. (2024). Put together, the sample provides a rich set of information on workers' employment history, demographic background, and pay.

Migrant Status We define migrants based on the first citizenship recorded in the IAB data. For each worker in our sample, we therefore add information on the citizenship recorded in their first entry in the social-security records. Whenever a worker has non-German citizenship in their first social-security record, we classify them as migrant workers. Note that, because the administrative data does not record ethnicity, our definition of migrant status is based on the workers' citizenship rather than their ethnicity; the German immigration system is described in detail in Appendix A.1. We discuss the potential bias to our estimates arising from this distinction in Section 3.3.

Most of the migrants in our sample keep their citizenship up to the baseline year. 5% of our baseline sample, or 774 migrants, have naturalized by the time of the layoff. In Figure B9, we show that their earnings, wage, and employment losses after displacement are very comparable to those of non-naturalized migrants. This reassures us to keep our definition of migrant status based on the first reported citizenship in the administrative data.

Our baseline sample includes return migrants and any workers who drop out of the administrative data after displacement (we record them as unemployed, with zero earnings). We include these observations for two reasons. First, leaving the administrative record – becoming economically inactive – might be the outcome of job displacement, and as such something we want to study explicitly. Second, the administrative records do not allow us to cleanly identify return migration from other reasons of leaving the dataset. In Appendix C, we re-estimate our results using a balanced sample of displaced workers who were continuously observed in the administrative dataset. The migrant native gap in earnings and employment is somewhat lower, but comparable to the baseline results.

Mass Layoffs and Unemployment We define mass layoffs following standard practice in the literature 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 $t = 0$. We follow Hethey-Maier and Schmieder (2013) and drop mergers, takeovers, spin-offs, and ID changes. For this purpose, we construct a matrix of worker flows between establishments by year. If more than 30 percent of displaced workers move to the same successor establishment, we exclude this establishment from our sample. To ensure that we focus on establishments without large employment fluctuations immediately before the layoff, we exclude establishments where the workforce *increased* by more than 30% in at least one of the two years preceding the layoff.

A displaced worker is a worker who leaves a layoff establishment as part of a layoff event and does not return to this establishment for at least 5 years. Workers in our sample were displaced in 2001-2011; restricting our observation period to 1997-2016 thus ensures that we can follow workers for five years before and five years after displacement, as long as they are registered in the social-security data during this period. In general, the displaced individuals in our sample will be eligible for receiving 60-67% of their pre-displacement income for at least a year after the layoff in the case of unemployment. In Appendix A.2, we describe in detail the unemployment insurance and benefits available to displaced migrants and natives. Importantly, our sample restrictions (see below) mean that, conditional on pre-displacement wages and age, migrants and natives are eligible for the same duration and value of UI.[11]

[11] An important exception is the fact that immigrants from outside of the EU may be required to leave Germany if they do not find re-employment within a year of the layoff. This might drive (involuntary) return migration and skew our estimate of the employment gap. We discuss this mechanism in Section 3.4.

Baseline Restrictions We follow the standard baseline restrictions in the job displacement literature (e.g., Schmieder et al. (2023)) and only consider displaced workers who fulfill the following on June 30 in the baseline year t-1: They are full-time employed on June 30 at a firm with at least 50 workers, they have at least 3 years of tenure, and they are aged 24-50 years. Table B6 shows that our main results hold if we relax these restrictions.

The baseline restrictions ensure that displaced workers are of prime working age and have relatively stable employment biographies before they are laid off, and therefore likely did not expect the layoff. We also ensure that firms are large enough for displacement to be exogenous, i.e. unaffected by an individual worker's productivity.

Given the positive selection of baseline migrants and the negative selection of baseline natives documented in Table 1, our results likely present lower bounds compared to the effects we would estimate on the full population of migrants and natives in Germany. We present the results of an alternative analysis estimating the cost of job displacement for all displaced migrants and natives in Appendix Section A.4.

3 The Migrant-Native Earnings Gap

3.1 Empirical Strategy

The ideal experiment to estimate the migrant-native difference in costs of job loss would be the following: Imagine two workers, m and n, who are working at the same establishment at exactly the same job. Workers m and n are identical in their demographics, skills, and experience on the job, except for one characteristic: m is a migrant, while n is not. Then imagine both workers are displaced in the same layoff event. Comparing m's earnings, employment, and wages to n's before and after layoff would give us the causal effect of migrant status on the cost of displacement.

The above experiment is not feasible, but having access to all displacement events in the German social-security data allows us to come close to it. In particular, we can match migrants to similar displaced natives who lose their job at the exact same establishment in the same 3-digit occupation, and in the same year, and use these pairs for our comparison.

Comparison to Job Loss Literature Our empirical strategy differs from the empirical strategy commonly used in the literature on job displacement (e.g., Jacobson et al. (1993); Schmieder et al. (2023)) in several ways.

First, while most job loss papers calculate displacement costs of displaced workers relative to a non-displaced worker match, we focus only on displaced workers. This is because we do not aim to quantify the absolute costs of job displacement; instead, we quantify the *additional* cost of job displacement for migrants relative to native workers. This decision guides our matching strategy. We match displaced migrants to displaced natives to ensure we are comparing workers who are as similar as possible – including being laid off from the same establishment – except for their migration status. In contrast, the standard approach, comparing displaced workers to non-displaced workers, would not control for differences in the characteristics of migrants and natives which may contribute to the migrant-native gap. It also does not allow for matching on establishment.

A potential disadvantage of our approach is that it does not control for any underlying trends in the labor market outcomes of migrants and natives. For example, if migrants' earnings and employment are deteriorating compared to natives in general, our estimate of the migrant-native gap in the cost of job loss would overestimate the true differences between displaced migrants and natives. To address this issue, we estimate the cost of job displacement in a triple difference-in-differences model which interacts the time since displacement with displacement and migrant status, on a sample of matched displaced and non-displaced migrant and native workers, as a robustness check.

Second, compared to most of the existing studies (see e.g., Helm et al. (2023) or Schmieder et al. (2023)), our analysis is more demanding in terms of the variables we match on. We compare workers laid off in the same year, from the same establishment, and from the same 3-digit occupation. This helps us to compare individuals with very similar jobs, and similar outside options in the local labor market.

Finally, unlike most of the previous literature, we pool men and women in our baseline analysis to make our results more generalizable. While there is evidence that the cost of job displacement varies by gender (Illing et al., 2024), we find that the *relative* cost between migrants and natives does not significantly vary by gender (see Figure 1).

Exact Matching Combined with Propensity Score Matching To assign a unique displaced native worker match to each displaced migrant worker, we use 1:1 propensity score matching without replacement combined with exact matching.[12] We proceed as follows. We match within cells of baseline year, establishment, 3-digit occupation, and gender. Since workers may still differ within these cells, we use propensity score matching to assign the closest match *within each cell* on the following characteristics: log wages in t-3, log wages in t-4, age in t-1, years of education in t-1[13],

[12] To implement exact matching without replacement, the algorithm must move sequentially, matching each displaced migrant to a shrinking set of displaced natives. As a result, the order of matching matters. We examine the robustness of our findings to this order in Table B7.

[13] Note that in the baseline matching, we exclude workers with missing values in the (imputed) education variable. This affects only 5.6% of natives but 14.15% of migrants in the pre-matching sample. In a robustness check, we therefore modify the baseline matching algorithm in two ways. First, we modify the baseline coding by assigning missing values in the education variable to the lowest category (10 years of education). Second, instead of matching

and tenure in t-1.

Matching on these characteristics allows us to control both for pre-layoff productivity and skill/experience profiles.[14] Matching on wages has the additional advantage that we implicitly control for the amount of UI benefits displaced individuals receive post-layoff since these depend on their last net wage (see Section A.2 for details). Moreover, matching exactly on the layoff establishment means that we implicitly control for a set of other observable – and unobservable – characteristics at the establishment and local labor market level, such as the productivity of the establishment, worker sorting, and local labor market conditions. Our baseline sample contains 15,638 matched pairs.

Summary Statistics Table 1 shows how displaced workers in our matched baseline sample compare to a 2% random sample of full-time workers in Germany.[15] The table yields two key takeaways: First, migrants and natives in our baseline sample are different compared to the overall population. They have higher tenure (about 3 years), a difference stemming from our baseline restrictions where we condition on displaced workers with at least 3 years tenure. While migrants in the baseline sample earn higher wages compared to the average migrant (3.6 EUR/day), baseline natives earn lower wages than the average native (5.3 EUR/day).

Workers in our baseline sample moreover work in smaller establishments with a lower share of high-skilled workers, and a lower share of workers in a minijob.[16] Baseline migrants work in establishments with a lower share of migrant workers compared to the random sample of migrants (19% vs. 28%), while baseline natives work in establishments with a higher share of migrant workers compared to the random sample of natives (19% vs. 5.4%). We discuss selection in more detail in Section 3.3.

Second, although migrants and natives in the random sample differ significantly, our matching algorithm effectively makes displaced migrants and natives comparable: They have very similar years of education (11.3 vs. 11.5), age (37.9 vs. 38.3), and tenure (6.38 vs. 6.41 years). Their real daily wages are comparable (EUR 89.2 vs. 91.5), and by construction, displaced migrants and natives work in exactly the same establishments.

on education, we match on an occupation's skill requirement, defined as the last digit of the 5-digit occupational code (see Paulus and Matthes (2013) for details). Figure B15 presents the results, which indicate slightly smaller employment losses but are otherwise largely similar to the baseline findings. More generally, migrants may face limited international transferability of skills (Chiswick and Miller, 2009), which could lead to a downward bias in our estimates if their educational attainment is more likely to be underreported.

[14] As Table B7 shows, our results are robust to alternative matching specifications, such as not matching on wages.

[15] See Tables B1 and B2 for an overview of the distribution across industries and occupations.

[16] Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours of work per week, and a maximum of EUR 538 total monthly income (as of 2024). See Gudgeon and Trenkle (2024) for more detail.

Note that while we are able to control for a large set of observable differences between migrants and natives, we are still far from the "ideal" experiment described above: For example, we do not have information on migrants' language skills. Moreover, while we only compare migrants and natives displaced from the same 3-digit occupation, they may still carry out different tasks within this occupation, which is something we cannot control for.

Event Study To estimate the differential effect of being displaced by migrant status, we apply a dynamic difference-in-differences regression model with worker and time-fixed effects. Specifically, we estimate the following regression specification on our baseline sample of displaced workers:

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

where the dependent variable y_{itc} denotes average labor market outcomes (e.g., earnings, log daily wages, employment)[17] of individual i, belonging to cohort c in year t.[18] $Migrant_i$ is a dummy which is equal to 1 if a worker has non-German citizenship in their first spell in the admin data. We interact it with dummies $I(t = c+1+j)$ for 5 years before and after the job loss, and we omit period $t = -3$ as the reference category. The coefficients of interest are α_j, which quantify the evolution of displaced migrants' labor market outcomes relative to displaced natives. 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 on an upward earnings profile (Schmieder et al., 2023). In addition, π_t comprises year fixed effects, γ_i are individual fixed effects, and X_{it} is a vector of controls: baseline year (cohort) interacted with time since displacement and a fourth-order polynomial in age. We cluster standard errors at the worker level.

Cross-Sectional Analysis 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. For some of the below analyses, we therefore construct a cross-sectional

[17]Note that as our main earnings variable, we use earnings relative to earnings in $t = -2$. We winsorize the variable at the 99th percentile.

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

sample that allows us to study heterogeneity (e.g., by origin group or local labor market conditions) of the migrant-native earnings gap in a transparent way.

In a first step, within each matched worker pair, we construct a match-specific measure of earnings losses (and other outcomes), which we call the difference-in-differences (DID) outcome. For each unique matched pair p, this measure gives us the difference in the average outcome pre- vs. post-layoff for migrants vs. natives:

$$\Delta y_{DID,p} = \Delta y_{migrant,p} - \Delta y_{native,p} \tag{2}$$

where $\Delta y_{native,p}$ is defined as follows:

$$\Delta y_{native,p} = \overline{y}_{native,p,post} - \overline{y}_{native,p,pre} \tag{3}$$

$\Delta y_{native,p}$ thus reports the difference in average earnings, or a different outcome, for displaced native of matched pair p before ($t = -5$ to $t = -2$) vs. after ($t = 0$ to $t = 5$) job loss. $\Delta y_{migrant,p}$ is defined analogously for displaced migrants. $\Delta y_{DID,p}$ then indicates the extent to which the pre-post difference in mean outcomes varies within matched worker pairs, giving us the match-specific migrant earnings penalty.

In the second step, we regress $\Delta y_{DID,p}$ on different sets of dummy variables, for example, deciles of labor market characteristics in $t = -1$ or migrants' origin group:

$$\Delta y_{DID,p} = \delta Z_p + \gamma Cohort_p + X_{migrant}\beta + \epsilon_p \tag{4}$$

where the coefficient of interest, δ, tells us how the migrant-native gap varies for matched pairs with different baseline characteristics[19]. $Cohort_p$ controls for a matched pair's baseline year, i.e., the calendar year before the layoff. $X_{migrant}$ controls for a fourth-order polynomial in age, measured for the migrant worker. We cluster standard errors at the baseline establishment level.

3.2 Baseline Results

We start by looking at the raw patterns: What is the evolution of annual earnings for migrants and natives after displacement? Panels (a) and (b) of Figure 1 plot this data for our baseline sample of men and women, respectively. Before displacement, the earnings of matched migrants and natives working in the same firm develop in steps, with small or insignificant differences between the two.

[19]Baseline characteristics in Z_p, e.g. AKM worker fixed effects, are often defined in deciles or quartiles. Unless otherwise noted, quantiles are computed by year using the full available sample. Deciles of worker fixed effects are instead computed once over the full sample across all years, and deciles of the county-level share of the same-nationality working-age population are computed using the baseline sample.

After the layoff, however, a gap opens up. As shown previously (Jacobson et al., 1993; Schmieder et al., 2023), there is a significant cost of job displacement in terms of workers' earnings. Compared to two years pre-displacement, a native man loses 15,000 EUR on average in the first year after displacement, and this loss of earnings is highly persistent: 5 years after the layoff, a displaced native man is still earning 10,000 EUR less than before displacement. Panel (b) shows this pattern is very similar for native women. The earnings loss for migrants is even larger. The average income of a migrant male worker is 18,000 EUR lower the year after displacement compared to two years earlier (16,000 EUR for migrant women), and converges similarly slowly, to a loss of about 12,000 EUR 5 years post-displacement (14,000 EUR for migrant women).

The patterns in the raw data suggest a large and significant difference in the cost of job loss between migrants and natives. Our event study results – which allow us to control for the age profile of earnings along with individual- and time-fixed effects – confirm this result. In Panels (c) and (d) of Figure 1, we plot the differential treatment effect of displacement on the annual earnings of migrants relative to natives. We find that the cost of displacement for an average migrant is 2,500 EUR larger than that of a similar native worker laid off from the same firm, equivalent to 23% higher cost of job displacement. Moreover, this gap is persistent, staying constant for up to 10 years after displacement.

This migrant-native gap in post-displacement earnings can be driven by two factors: migrants earning relatively lower wages, or being relatively less likely to find employment post-layoff. Our event study results for daily wages and employment probability suggest that the observed gap in earnings is driven by both. In Panel (a) of Figure 2 we estimate our event study for a binary indicator of employment, and find that displaced migrants are on average 8ppt less likely to be employed after layoff. This lower employment probability stays virtually flat – or widens somewhat – over the 5 years after displacement. Panel (a) of Figure 3 shows the analogous result for wages. Migrants' wages are 13 log points lower than natives' immediately after the layoff; this gap approximately halves over the next 5 years, but remains statistically and economically significant.[20] Overall, our baseline results reveal a large, statistically significant, and persistent gap in post-layoff labor market outcomes for migrants vs natives. In the next sections, we examine the robustness of this result both in terms of alternative specifications and measures, and in relation to the patterns of self-selection into work after displacement, especially return migration.

[20]Results for days worked per year (available upon request) show a negative but flat gap after displacement. The gradual closing of the earnings gap is thus driven by the increase in wages, and somewhat offset by the small widening of the employment gap.

3.3 Robustness

We conduct several checks to test the robustness of our baseline earnings, wage, and employment gaps. Overall, we find that our results replicate for a variety of alternative matching and estimation strategies, sample restrictions, and a longer time frame.

Comparison to non-displaced workers Our matching strategy differs from the empirical strategy commonly used in the literature on job displacement in that we compare displaced migrants to displaced workers as opposed to using non-displaced workers within each group as a counterfactual. A benefit of this approach is that it forces the displaced migrants and natives to be directly comparable via our matching procedure, including matching on establishment so that we compare displaced migrants and natives laid off from the same firm. A potential disadvantage is that it does not control for any broader trends in migrants' labor market outcomes compared to natives, so that the estimated migrant-native gap in the cost of job loss might be over-estimating the true differences. As a robustness check, we re-estimate our results using this more standard procedure in a triple-difference-in-differences model which interacts time since treatment with both displacement and migrant status.

We estimate two such models, reflecting the different approaches to matching. First, we follow the classic approach à la Schmieder et al. (2023) which matched displaced migrants (natives) to their non-displaced counterfactuals (see Appendix Section A.4 for details). The results, presented in Figure B1 and Table D2, show no significant pre-trend differences between migrants and natives. The estimated migrant-native gaps for wages and employment are somewhat larger than our baseline results, likely reflecting the differences in the characteristics of migrants and natives.[21]

Our second model addresses this concern by re-estimating the triple-difference regression using a restricted version of the baseline sample. Specifically, we implement an additional matching step: After completing the baseline matching of displaced migrants and natives, we further assign to each displaced migrant (native) a non-displaced matched counterpart (see Appendix Section A.5 for details). In contrast to the classic approach, this ensures that migrants and natives in the sample are comparable. The results in Figure B2 and Table D3 are very similar to our baseline findings.

Matching and Sample Restrictions We test the robustness of our findings with several alternative matching strategies and sample restrictions. In column (5) of Table B7, we exclude pre-displacement wages from our matching set, and in column (6), we switch the direction of the matching algorithm from finding natives for migrants to finding migrants for natives. Both columns show

[21] The gap in employment closes somewhat faster, which might be reflective of return migration (which we discuss in Section 3.4).

that our estimates remain virtually unchanged compared to the baseline.

Regarding our sample choices, in column (4) of Table B7, we relax the firm size cutoff of 50 employees to include displaced workers from firms with 30 workers or more. In Table B6, we re-estimate the gap in costs of displacement when excluding migrants from western Europe, Australia, New Zealand, and the USA (column 2); from the top decile of worker ability as measured by worker AKM fixed effects (column 3); and excluding all workers displaced from an East-German establishment (column 5). In Appendix C, we replicate our results for a balanced sample of migrants and natives, i.e. only including workers that were observed in the administrative data for the whole duration of their displacement window. We will discuss these results further as a part of our discussion of return migration.

Finally, we also test the role of interactions between time since displacement and other variables that affect labor market outcomes: age, education, tenure and gender. If workers with, e.g., different education levels experience different labor market trajectories after displacement, and if migrants and natives display different levels of education, some of migrant-native gap in post-displacement outcomes might be in fact driven by education rather than migrant status. To control for this possibility, we add interactions between the time since treatment and each of these four covariates to our baseline event study regression. The results are summarized in Table B8. The migrant-native gap estimates deviate from the baseline only weakly, suggesting a limited role of other covariates.

Length of Tenure 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 B7, we therefore relax the tenure restriction to 1 and 2 years, respectively. The estimates of the earnings, daily wages, and employment migrant-native gaps are comparable to the baseline results in column (1) of the table.

A related question is one of tenure in the German labor market. About 5% of migrants in our baseline sample have become naturalized German citizens in the year before the layoff (compared to their first entry in the admin data), so we re-estimate our difference-in-difference regression excluding these workers. The results, presented in column (4) of Table B6, are robust to this exclusion (see also Figure B9 for a direct comparison of naturalized and non-naturalized migrants).

Cohort Heterogeneity During our baseline layoff period, 2001-2011, Germany and much of the world economy went through the full business cycle. This raises the possibility that our estimates of the migrant-native displacement gap might be driven by a particular layoff cohort (i.e. a cohort of workers laid off in particular years) and might thus not reflect a general pattern in the economy. In our baseline specification, we allow for cohort-specific trajectories in labor market outcomes by controlling for the interaction between the baseline year and time since the layoff. However, we implement several further robustness checks of our baseline results to particular cohorts or time periods.

First, in column (6) of Table B6, we restrict our sample to workers with baseline years between 2000 and 2003 so that their 5-year post displacement outcomes are not affected by the economic downturn of 2008. We show that the estimates of the migrant-native gap for total earnings, daily wage, and employment are very similar to our baseline estimates. Further, in Figures B3 and B4, we estimate the event study of total earnings for each layoff cohort separately. We do observe some cyclical variation: in general, it seems that strong cyclical changes, such as the recession and the subsequent recovery in economic activity, closed the migrant-native gap somewhat[22]. However, the gap is statistically significant and persistent for all cohorts. These results also show that our baseline findings are not driven by bias which might arise in difference-in-differences estimators with staggered treatment and heterogeneous treatment effects (Callaway and Sant'Anna, 2021). In this respect, the cohort-specific regressions in Figures B3 and B4 correspond to disaggregated stacked difference-in-differences regressions.

Finally, we show that our findings are robust to a related concept of tenure length in the German labor market (rather than in the layoff 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 overall earnings gap between migrants and natives varies only weakly with the length of stay in German labor market. Migrants who have been in the country for longer suffer greater relative loss of earnings: they are less likely to find re-employment and earn somewhat lower daily wages. This appears to be driven by the greater likelihood of the more recent migrants to drop out of the administrative data (possibly due to return migration). Despite this, the earnings gap is negative and significant for all but the most recent migrants (less than five years in the administrative data) and those with the longest tenure (more than 30 years in the administrative data), for whom it is negative and statistically insignificant.

Long-Term Outcomes In Figure B5, we extend the post-layoff period to understand how persistent is the initial migrant-native gap. We find that the trends observed within the first five years continue steadily for ten years after displacement. The gap in relative earnings stays constant, about

[22]The workers laid off during the recession experienced a smaller migrant-native gap in year t=0, but this gap grew over time in contrast to other cohorts where it stayed constant or diminished somewhat.

10% a year, even 10 years later. The trend for employment is the same: the small additional decline in years 1-3 after layoff vanishes by year 4, and the migrant-native employment gap in year 10 (8ppt) is larger than immediately after the layoff. The upward pattern for the daily wage gap similarly extends beyond the first five years: the initial gap of about 13 log points is closing steadily and reaches 7 log points in year 10 after displacement.

Definition of Migrant The definition of migrant we use in this paper is based on an individual's citizenship rather than their ethnicity. As a result, we are likely undercounting second-generation migrants and any migrants who gained German citizenship before the start of our study window. The way these individuals fare in the labor market, and the nature of selection into naturalization, may bias our estimates of the migrant-native gap.

We argue that coding some individuals with non-German ethnicity as natives likely leads to an underestimate of the true migrant-native gap insofar as this group is likely to face similar labor market discrimination and reduced information about job opportunities. At the same time, the lack of data on ethnicity may lead us to overestimate the gap if naturalized Germans are drawn from the upper half of the migrant productivity (or wage) distribution. Reassuringly, our analysis of the migrants who became German citizens during our study does not suggest large differences in post-displacement outcomes (see Figure B9). There is one exception: Migrants who naturalize are equally likely as natives to drop out of the admin data. The administrative data does not record ethnicity, which means we are unable to determine the size of the potential bias empirically.

3.4 Return Migration

A key difference between displaced natives and migrants is that migrants are more readily able to move out of Germany. Depending on whether the selection into return migration is positive or negative, our baseline estimates might underestimate or overestimate the true extent of the migrant-native gap in displacement costs. If following a layoff, less productive migrants emigrate to their home country (or elsewhere), we might underestimate the true migrant-native gap in the cost of job displacement. If, on the other hand, the self-selection is positive, the negative migrant-native gap might be a "statistical construct" driven entirely by the changing composition of the migrant group. In this section, we look in detail at the available data about the patterns of return migration in our sample and evaluate their impact on our estimates of the migrant-native gap.[23]

[23] Note that we only focus on static implications of return migration. Several studies (e.g. Adda et al. (2022)) point out the dynamic implications of return migration intentions. For example, individuals who are planning to leave Germany might invest less in their language skills, leading to lower wages both before and after displacement. We abstract from these considerations.

Measuring Return Migration Because the administrative data only records when workers leave the administrative records, not when they leave the country, we use information on whether a worker drops out of the administrative data as a proxy for return migration. Workers might be leaving the administrative sample for a variety of reasons besides emigration, such as becoming self-employed, starting education, or retiring. This means we measure return migration with a degree of measurement error. In particular, if migrants are more likely to become self-employed (an employment status that is not recorded in the administrative data) or work illegally, we might be over-estimating the migrant-native gap in the cost of job displacement. Unfortunately, the administrative data does not record workers' reasons for leaving the social-security system reliably, making it difficult to understand differences in post-attrition destinations.[24] However, studying the patterns in attrition from the administrative data is still informative about the role of changing composition on our estimate of the migrant-native gap.

Drop-out Patterns How much more likely are migrants to drop out of the administrative data after displacement? We take dropping out of the sample as an outcome of displacement, and plot this event study in Panel (b) of Figure 2. We find that migrants are indeed significantly more likely to drop out of our sample, and the gap increases over time: In the year immediately after the layoff, migrants are 1.4ppt more likely to be without record in the administrative data. 5 years out, this gap has increased to 5.6ppt.[25]

Panels (a) and (c) of Figure 2 help to illustrate how these different attrition rates influence our estimates of the migrant-native gap unemployment. If migrants who struggle to find employment emigrate from Germany, this will increase the share of migrants who drop out of the administrative data, but reduce the migrant-native gap in unemployment probability. The three panels of Figure 2 suggest that this is likely happening in our sample: the decreasing gap in unemployment probability (Panel c) does not translate into a smaller employment gap (Panel a), because it is instead entirely driven by displaced migrants leaving the administrative record (Panel b). The partial closing of the wage gap (Panel a, Figure 3) might be driven by the same pattern if migrants choose to leave

[24] For most individuals, the recorded reason is the "end of contract" with the employer or the employment agency (i.e., end of eligibility for unemployment insurance).

[25] Importantly, attrition is not driven by a large outflow within the first year after displacement, which we would expect if migrant attrition is primarily driven by visa revocation after a year of unemployment. 33% of migrants who drop out of the sample within 5 years post-layoff leave within the first year; the corresponding number for natives is 27%. This corresponds to 4.7% of all displaced migrants and 3.9% of all displaced natives. More broadly, Figure B10, Panel (f), does show that migrants with less than 11 years of tenure in the German labor market, who are less likely to be eligible for permanent residency, are 5-6ppt more likely than natives to drop out of the data. For migrants with 11-25 years of tenure, the gap is only about 2.5ppt. The German immigration system therefore does seem to have some impact on migrants' return behavior, but much of the attrition behavior is driven by other factors (see Appendix A.1 and A.2 for details on German immigration and unemployment insurance systems).

Germany rather than accept relatively lower wages (see Section 4.4 for a discussion).

Selection of Drop-outs To understand which migrants are leaving the dataset, we first compare the average characteristics of migrant (and native) "stayers" and "drop-outs" in the year before the layoff. Table B3 presents these results. Migrants who leave the administrative data are about 7 months older than the stayers, earn 3.6% a year more, but have marginally shorter tenure in the firm (3 months difference). They also tend to work in somewhat larger firms with a better-skilled workforce and a lower share of coworkers in a minijob. Importantly, however, these differences between migrant stayers and drop-outs follow the same pattern as between native stayers and drop-outs from the administrative data. In other words, even though displaced migrants are more likely to be neither employed nor unemployed (and thus not appear in the administrative data), the selection of observables into dropping out does not differ from that of natives.

We next investigate drop-out rates by our proxy for worker productivity: pre-displacement AKM worker fixed effects. We then contrast this analysis with a study of how drop-out rates vary by net income of the origin country, which we interpret as a measure of migrant workers' outside options. Figure 4 presents the results. Panels (a) and (b) show the migrant-native gap in dropping out of the admin data, averaged over the whole post-displacement period. We find a weak but statistically insignificant U-shaped pattern in AKM worker FE (Panel a): both less productive migrants and more productive migrants are weakly more likely to leave the data. The exception are migrants in the top two deciles of the productivity distribution, whose likelihood of dropping out of the sample is about 1/3 higher than for the average migrant, and this difference is statistically significant. We exclude migrants in the top productivity decile from our baseline sample for a robustness check (Table B6, column 3), which yields very comparable results.

We find that attrition patterns are better explained by migrants' outside options rather than worker productivity. In Figure 4, Panel (b), migrants in the top 3 deciles of origin country net income measured at baseline are significantly more likely to drop out of the sample, and the likelihood to drop out increases sharply with better outside options.[26] Migrants in the top decile are 15ppt more likely to leave the admin data post-displacement, compared to a 0 gap for migrants with median outside options. Note that to ensure that this pattern is not driven by a correlation between migrants' outside options and their productivity, we always control for baseline AKM worker FE in these regressions.

Panels (c)-(f) of Figure 4 show the gap in unemployment probability and in earnings relative to earnings in $t = -2$. For migrants in the top half of the productivity distribution, there is no

[26]The same analysis but by migrants' origin group, rather than country income (Panel f of Figure B11) corroborates this picture. We show that the migrants relatively most likely to drop out of the administrative data are from the West, in contrast to immigrants from poorer regions such as former Soviet states, Eastern Europe, or Africa.

gap in unemployment rates and relative earnings; for the bottom half, there is a linear relationship between outcomes and worker productivity (Panels (c) and (e)). Similarly, migrants in the top two deciles of origin country net income face a small or negative unemployment gap (i.e. their unemployment rates are lower than that of their native colleagues). The pattern on earnings is less clear; this is because we assign zero earnings for migrants who are not in the admin data (and thus disproportionally those in the top decile of origin country net income). Panel (d) of Figure B8 shows that there is a zero wage gap for migrants from the top two deciles of origin country net income.

What does this suggest for the potential bias induced by return migration? We conclude that selection into return migration by worker productivity is less of an issue, significantly affecting only the top end of the migrant productivity distribution. We do find a strong selection pattern driven by the economic situation in migrants' origin country which potentially applies to the 30% of migrants with better outside options. The effect of this selection on our estimates of the migrant-native gap is nuanced. On the one hand, these return migrants are probably negatively selected compared to other migrants *of the same origin* who decided to stay in Germany: their better outside options allow them to leave Germany rather than face low wages or unemployment. On the other hand, insofar as the better outside option translate into better outcomes in Germany, the selection is positive, leading us to over-estimate the migrant-native gap.[27] To fully evaluate the bias in our estimate of the migrant-native gap, we would need to make assumptions about the unobservable counterfactual in Germany, which is beyond the scope of this paper.

Conditioning on Balanced Panel In our baseline analysis, we assign workers who are not observed in the admin data zero earnings and employment, which may lead us to overestimate the gap if some of these migrants are in reality employed in their home country. To compare our baseline results with more conservative estimates, we re-estimate all our key results on a restricted sample excluding any migrants or natives who left the administrative data at any point in the 10 years around the layoff.

These results, reported in Appendix C, replicate our baseline estimates both qualitatively and quantitatively. The gap in total earnings in the balanced sample is somewhat smaller (7ppt instead of 9ppt), driven by slightly higher employment probabilities (5ppt vs 6.5ppt gap) in the year of displacement. These gaps also close faster – we observe a decrease in both compared to the constant earnings and employment gap in the full sample. These results further support the hypothesis that return migration serves as an exit route for displaced migrants who would otherwise struggle with

[27]In our sample, migrants from higher net-income countries earn on average the same as other migrants. This presents suggestive evidence towards negative selection in this pattern of return migration.

re-employment.

We prefer to not apply this restriction for the baseline results since it means that we are conditioning on an outcome measured post-treatment – the probability of leaving the administrative data. However, this restricted sample of migrants (and natives) may be particularly relevant for policy-makers, given that these are the migrants who remain in Germany.

4 Mechanisms

In the previous section, we have documented that migrants fare significantly worse after job displacement than natives. Compared to their native coworkers, observationally similar migrants, laid off from the same establishment, are 8ppt less likely to have become re-employed five years out. If they do find another job, they initially earn 13 log points less; and while this gap narrows over time, migrants' total earnings are 10 ppt lower than that of similar displaced natives even 10 years out. We have shown that these results are unlikely to be driven by positive selection into return migration, and might be an underestimate of the true gap given the observed patterns of emigration from Germany. In this section, we explore several potential mechanisms driving this result: differences in stated job preferences, worker-firm sorting patterns, the role of labor market conditions and discrimination, and coworker networks.

4.1 Reported Job Search Preferences

If migrants look for different jobs compared to natives, it would be expected for the two groups to have different post-displacement outcomes. To examine the role of reported job preferences in explaining differences in the costs of displacement, we use additional data on the job search preferences and objectives of individuals who register as job seekers with the Federal Employment Agency.[28]

One caveat in using this data is that not all displaced workers register as job seekers, primarily because some manage to find re-employment after the layoff is announced but before they are actually laid-off. As a result, the search patterns analyzed here describe negatively selected displaced workers. Importantly, however, this negative selection seems to be similar for migrants and natives: The two groups are equally likely to be registered as job seekers at any point after the displacement (Panel A of Table 2).[29]

[28] See Appendix Section A.3 for more details on the data.

[29] To square this result with the estimated higher unemployment of migrants relative to natives after the layoff, note that the *duration* of unemployment is higher for displaced migrants. Moreover, the likelihood of receiving UI estimated in Table 2 includes all incidences of unemployment spells, including for employed individuals receiving assistance from

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 Panel B of Table 2, we compare the stated search preferences of migrants (column 2) relative to natives (column 1). We find that the two groups do not differ in the kind of jobs they are searching for; specifically, migrants are not less likely to target full-time or permanent jobs which might come with a pay penalty. They are also equally willing to consider vacancies outside of their core occupation category or commuting zone.

Despite their very similar reported job preferences, migrants do take up different types of jobs compared to their native coworkers. Panel C of Table 2 shows that after displacement, migrants are significantly more likely to work part-time, which might drive some of the observed gap in wages. Compared to natives, they are also less likely to switch to a different occupation. This is particularly significant given that the geographic mobility of the two groups is the same (Panel C): Migrants' lower overall (geographic and occupational) mobility might contribute to their lower re-employment rates.

Note that all of this analysis comes with the caveat that we only observe search preferences for those displaced workers who registered as job seekers after the mass layoff. As a result, our results cannot tell us whether differences in search preferences or strategies drive the initial gap in employment upon displacement; we can only conclude they cannot explain the migrant-native gap in re-employment for the workers who are selected into the sample.

4.2 Labor Market Conditions

The displaced migrants and natives in our sample face the same local labor market conditions at baseline.[30] However, migrants might be less able to take advantage of the existing job opportunities or struggle more in a high-unemployment labor market. In this section, we test whether the migrant-native gap in post-displacement outcomes depends on the state of the local labor market. We focus on two different measures of local labor market conditions: unemployment rate and occupation-specific thickness.

the employment agency and those who become unemployed in later years after the displacement. Workers can register as job seekers with their local job agency even if they are employed – for example when anticipating an unemployment spell, or when they are unhappy with their current employment and want to receive job search assistance from their caseworker.

[30] 16% of all displaced workers in our sample move to a different federal state in the 5 years after the layoff, so not all migrants and natives displaced from the same firm will face the same local labor market. However, we found no evidence of migrants moving to significantly different local labor markets (Panel C of Table 2).

Panels (a) and (b) in Figure B12 plot the migrant-native gap for wages and employment by local (county) unemployment rates. There is no statistically significant difference by quartiles of local unemployment rates.

Next, we look at occupation-specific labor market thickness. This measure, based on Jäger et al. (2024), captures how much a given local labor market specializes in the worker's occupation.[31] Holding labor demand constant, thicker labor markets imply greater competition for jobs and might translate into relatively worse post-displacement outcomes for migrants if they are less competitive or find it more difficult to navigate a competitive labor market. On the other hand, market specialization happens for a reason: the thickness of a labor market is a strong agglomeration force, attracting employers and making firms and workers more productive. This might have the opposite effect on relative migrant outcomes if migrants do better in markets with strong labor demand and many outside options.[32]

Panels (c) and (d) in Figure B12 show a weak positive U-shaped relationship with both employment and wages. The wage gap in the top quartile (10 log points) is similar to that in the bottom quartile (although the latter comes with large standard errors), and lower than in the second and third quartile (around 25 log points), although the difference is not statistically significant.[33] These results suggest that the agglomeration effect dominates the competition effect weakly more for migrants. Given that market thickness operates through occupational specialization, this finding might be one reason why migrants are less likely than natives to switch occupations following a displacement.

4.3 Discrimination

Our results on labor market conditions might be mediated by – or interpreted as evidence of – discrimination against migrants in the German labor market. In the absence of a direct measure of discrimination, we rely on a set of proxy measures and comparisons that shed some light at this issue.

We start by estimating the migrant-native gap by different migrant origin groups in Figure B11. We find that migrants from Africa, Asia (including Turkey) and the Middle East suffer significantly

[31] We calculate thickness as the share of an occupation's employment in a commuting zone compared to the nationwide share of the occupation's employment. See Appendix A.3 for more details.

[32] Jäger et al. (2024) show that the cost of substituting a worker is lower in thicker markets, highlighting the advantage such markets present to the employer.

[33] Moretti and Yi (2024) estimate the effect of mass layoffs on US workers across labor markets of different thicknesses. They find that the cost of job displacement is smaller in thicker markets, both in terms of employment and wages. These results are not directly comparable with ours because we estimate the differential impact on the migrant-native gap rather than the impact on level outcomes. They are, however, consistent in that we find a relatively lower wage gap in the thicker markets.

larger cost of job displacement compared to migrants from regions ethnically, culturally and racially more similar to Germany, such as Western and Eastern Europe.[34] This may potentially hint at discrimination from the side of firms, but the larger gaps for migrants from more culturally distant regions could also be driven by supply side factors, such as worse language skills.

To further explore potential employer taste-based discrimination, we draw on Becker (1971): employer taste-based discrimination should be less prevalent in more competitive labor markets where finding another employee is more costly. We test this by comparing migrants' relative outcomes across measures of outside options (labor market thickness) and local unemployment rates in Figure B12. We fail to find significant variation in outcomes, making it less likely that this type of discrimination against migrants plays a large role in driving the migrant-native gap; at the same time, however, our measure of competitiveness might be too coarse, or anti-immigrant discrimination might not respond strongly to market forces.

Next, we examine whether the migrant-native gap might be driven by taste-based discrimination on the side of consumers or other employees. In Panels (e) and (f) of Figure B12, we compare the gap in employment and wages between occupations requiring different degrees of physical proximity, as taken from Mongey et al. (2021) paper on social distancing during the COVID-19 pandemic. The data does not reveal any clear patterns: if anything, the gaps in both employment probability and wages are somewhat lower for displaced migrants in the top quartile of physical proximity requirement.

Finally, we follow Sorkin (2025) and conduct an alternative test of employer taste-based and statistical discrimination based on the dynamics of employer behavior. Statistical discrimination should wane with tenure as employers learn about worker productivity over time. As a result, any remaining gap in migrant-native outcomes for high-tenure workers is indicative of taste-based discrimination by the employer (which is assumed to be constant over time).[35]

Our estimates, plotted in Figure B7 broadly support these patterns. First, panel (a) shows that the migrant-native gap in wages decreases in pre-displacement tenure, suggesting a degree of statistical discrimination (and employer learning). Second, the gap becomes statistically indistinguishable from 0 for workers with tenure of 7 years or longer, potentially suggesting little employer taste-based discrimination on average. However, panels (b) and (c) reveal a considerable degree of heterogeneity in these estimates. Migrants from western countries (including much of western Europe and majority English-speaking countries) experience on average no migrant-native gaps in wages even at very low tenure length, although the standard errors are quite large. Migrants from

[34] The comparison of the migrant-native gaps by migrants' naturalization status in Figure B9 suggests that these differences are unlikely to be driven by potential differences in migrants' legal status.

[35] To follow the methodology in Sorkin (2025), we restrict our attention to average wages in the first full-time job after displacement.

all other countries, on the other hand, consistently close the gap only at tenure of 7 years or longer. These results point to a likely significant role of statistical discrimination for at least some groups of migrants.

These results can be driven by two different mechanisms: discrimination may distort the hiring patterns of migrants compared to natives; or there might be direct differences in wages paid to migrants and natives within the same firm. In panel (d) of Figure B7, we specifically look at co-movers – matched migrants and natives who were also re-employed at the same firm, and so were not subject to differential hiring patterns. The chart shows that, as time goes on, migrants are paid somewhat more by the same employer than their native counterparts. Importantly, as our results include both low- and high-tenure workers, this may reflect employer learning and the unwinding of statistical discrimination; but it also highlights the role of hiring patterns in driving the migrant-native gap in wages.

Overall, the evidence on the role of anti-immigration discrimination is mixed. We do not find any evidence that migrant-native gaps are smaller in markets and occupations where employer discrimination might be more costly, and our test of consumer or employee taste-based discrimination also returns a null result. Nevertheless, this evidence is indirect, and we cannot rule out that anti-migrant discrimination does not drive our results. In fact, the migrant-native gap is larger for migrants originating from specific regions. While this might reflect cultural or language disadvantages on the side of the migrants, the patterns are consistent with a degree of statistical discrimination (and employer learning), which seems to operate especially on the hiring margin.

4.4 Sorting across Firms

One of the key factors explaining wage heterogeneity between otherwise similar workers, both in levels and growth, are firms (Card et al., 2018). Existing work has shown that interruptions to climbing the "firms ladder", such as job displacement, have long-term consequences on workers' labor market outcomes because the affected worker struggles to find re-employment at high-wage firms (Schmieder et al., 2023).

Our data shows that displaced migrants were already employed in relatively higher-paying firms pre-layoff. Panel C in Table 1 shows that the median wage of layoff firms compared to other firms employing migrants is 1.1% higher. As a result, the negative migrant-native wage gap could be explained by migrant workers converging with the rest of the migrant population as a result of the layoff. On the other hand, if displacement causes all workers – migrants and natives alike – to fall down the "firms ladder", the migrant-native gap in wages should be 0 after displacement.

We test this hypothesis in Panel (b) of Figure 3. We find that the average displaced migrant finds re-employment in significantly lower-paying firms than their displaced native coworkers, suggesting that job displacement disrupts firm-worker sorting more for migrants than for natives. This pattern is corroborated by Figure B6. Migrants are significantly more likely to be re-employed in firms with a higher share of workers employed in a minijob, and a higher share of migrant employees, both of which are associated with lower pay. Overall, the differential sorting of workers across firms explains about 16% of the migrant-native wage gap in the year of displacement.

The firm FE gap stays constant throughout, in contrast to the wage gap that falls by a third in the 5 years post-displacement (Panels a and b in Figure 3). However, to understand whether sorting actually contributes to closing of the wage gap, we need to separate the individual-level outcomes from the changing composition of the re-employed migrants.

We start by examining the role of the composition effect. In Panel (c) of Figure 3, we plot separately the wage gaps for workers who became re-employed 0, 1, 2, and 3 years after the layoff. Compared to the continuously employed (re-employment in year 0), the wage gaps of the subsequent re-employment cohorts are much larger, and close more slowly, if at all. The baseline migrant-native wage gap is thus larger, and closes more slowly, because of the changing composition of employment among displaced migrants.[36]

To understand the role of sorting within similar migrants, in Panel (d) of Figure 3 we plot the wage gap and firm FE gap for displaced migrants who found immediate re-employment. Similarly to the full migrant sample (Panel a), these migrants experience a negative wage gap compared to natives, although this gap is smaller and closes fully within 5 years after the layoff. The pattern of the firm FE gap, however, is significantly different to the aggregate gap for the full sample. As shown in Panel (d) of Figure 3, this gap starts at 0 and weakly increases. These patterns have several implications for the role of sorting on pay. First, the negative wage gap but 0 firm FE gap in year 0 suggests that displaced migrants initially struggle to be re-employed at the same pay level as before even if they sort into similar firms as their native coworkers, possibly because of employer discrimination or greater uncertainty about migrants' productivity. Second, the closing of the wage gap is accompanied by a small increase in migrant's relative firm FE. This implies that, in addition to firms learning about, and rewarding, migrants' true productivity over time, the growth in wages is also driven partly by migrants switching jobs to climb the firms ladder faster.[37]

[36] Panel (c) also highlights the importance of the duration of non-employment for post-displacement wage convergence: workers who managed to find a job faster face a smaller wage penalty that closes faster compared to those who were out of employment for longer. Further analysis (available on request) shows that the negative relationship between post-displacement wages and non-employment holds for both migrants and natives, in line with Fallick et al. (2025).

[37] We can compare these results with panel (d) of Figure B7 which compares pay in a specific subsample of workers who were re-employed by the same firm (co-movers) in a full-time position. The wage gap between migrants and natives in this subsample is 0 from t=0. There are several explanations for this difference. First, the broader sample

Finally, an important aspect of workers sorting across firms is whether sorting results in more productive workers matching with more productive firms. While we abstain from using worker FE to make comparisons between migrants and natives, we can use them to understand wage differences *within* displaced migrants. Panel (e) of Figure B8 shows that the firm FE gap converges to zero for more productive migrants, suggesting positive assortative matching. Moreover, a comparison with Panel (b) of the figure suggests that the closing of the firm FE gap for the top 50% productive migrants translates into the closing of the wage gap for these workers.

Overall, this analysis suggests a nuanced role of firm sorting in explaining the migrant-native wage gap. High-productivity migrants find re-employment quickly and in similarly productive firms to the natives. However, this is not enough to close their wage gap over time: they achieve this by employer learning and workers switching over to higher-productivity firms at a faster rate than natives. Lower-productivity migrants, on the other hand, fall down the firms ladder and struggle to recover their position, which drives their lower wages – and resulting in a relatively slow closing of the aggregate migrant-native wage gap.

4.5 Networks

Existing literature has shown that networks and social connections are an important mechanism for job search (Dustmann et al., 2015; Glitz, 2017; Glitz and Vejlin, 2021; Saygin et al., 2021). The impact of networks in our context is a priori ambiguous. On the one hand, networks might matter more for migrants, serving as an important source of information if migrants are less well-informed about local job opportunities and existing job-search assistance. On the other hand, migrants' networks might be smaller, less diversified, and of lower quality: our data shows that migrants on average are more likely to be unemployed and work in lower-paying firms which would reduce their ability to refer to, or inform about, good job opportunities.

Our measure of coworker networks draws on the network-driven outside options introduced by Caldwell and Harmon (2019). We define a worker's network as all coworkers in the same 3-digit occupation who worked at the establishment in the 3 years prior to the layoff but moved to a different establishment by the year of the layoff. We exclude coworkers who are part of the baseline

in Figure 3 includes part-time workers who might have switched to full-time later on; and second, it highlights that working in the same firm is different from working in a firm with the same AKM FE. Co-movers seem to be better at relaying their true productivity to the employer compared to the broader sample. This may be driven by networks, which we discuss next.

sample of matched workers.[38] [39] We then define network-driven outside options as follows:

$$\Omega_{p,t=0} = \sum_j ShareCoworkers^2_{pj,t=0} \times EstablishmentGrowth_{jt} \tag{5}$$

where $ShareCoworkers_{pj,t=0}$ is the share of a matched pair's coworkers p employed at establishment j in year $t = 0$. Our proxy for an establishment's labor demand, $EstablishmentGrowth_{jt}$ measures the net growth of establishment j, averaged across the 3 pre-layoff years. We square the share of former coworkers to reflect the fact that larger firm-specific networks likely matter more.[40]

We use this measure to explain the variation in our difference-in-differences measure of relative post-displacement outcomes. We then regress the difference in log wages or employment post- vs. pre- displacement, $\Delta y_{i,p}$, for a given worker i of matched pair p (see equation 2), on our measure of network-based outside options:

$$\Delta y_{i,p} = \gamma Migrant + \alpha\Omega_{p,t=0} + \beta\Omega_{p,t=0} \times Migrant + X_{i,t=0}\gamma + \epsilon_p \tag{6}$$

α captures the average effect of network quality on all displaced workers. β, our coefficient of interest, measures the additional effect of the same network on migrant outcomes. $X_{i,t=0}$ contains dummies for baseline year and a fourth-order polynomial in age. To further test whether the type of network matters, we run separate regressions for the share of *migrant* coworkers and for the share of *native* coworkers in one's network. A worker has many outside options if a large share of her former coworkers are employed in expanding firms. Note that by construction, $\Omega_{p,t=0}$ is constant within matched migrant-native pairs p.

We start by describing the characteristics of coworker networks in Table B9. Panel A shows that networks comprising of migrants are much smaller and less diversified across a large set of measures – firms, regions, occupations, and industries. Furthermore, the characteristics of the network members reflect the relatively worse outcomes of migrants in the labor market (Panel B): they are less likely to work in a full-time job, more likely to hold a minijob and earn less. Nevertheless, these differences are not primarily driven by the establishments the network members work at: migrant and native former coworkers are employed at establishments with near-identical AKM fixed effects, shares of high-skilled workers, and offering virtually the same average wage.

[38] This restriction ensures that our measure of coworker networks is not endogenous to the post-layoff outcomes.

[39] While our baseline sample of displaced workers stems from *IEB, version 14*, the sample of coworkers stems from the more recent *IEB, version 16*. For a small set of 413 matched displaced worker pairs, at least one worker's ID changed across the two versions. We exclude these matched pairs from our coworker analysis sample to minimize measurement error.

[40] Dustmann et al. (2015) show, on a sample of German migrants, that the probability of being hired via one's social network increases in the share of the migrants' potential social network in the firm.

Put together, these findings suggest that compared to networks comprising of native coworkers, migrant networks provide access to similar establishments but the network itself tends to be smaller and consist of relatively worse-placed individuals.

We present our results of regression 6 in Table 3. Column (2) shows that, as expected, better outside options arising from coworker networks have a positive effect on post-displacement wages (significant) and employment (insignificant). The interaction effect is positive and significant for both wages and employment. Since the displaced migrants and natives share the same network of former coworkers, this result suggests the same network has a particularly important effect on migrants. However, the results in columns (3) and (4) suggest that the composition of the network itself (migrant vs native) matters little – except for migrant wages, where we do see that they respond particularly strongly to larger (and better) migrant networks. One possible reason why the type of network might not matter is the difference in network quality described above: if migrant networks are more important for migrants but are of lower quality, their overall effect might not be any different from a network comprising natives.

Next, to understand better the mechanism behind networks, we examine whether better networks directly result in displaced workers switching over to network-connected firms – as opposed to improving workers' outcomes through better labor market information. The three Panels of Figure 5 plot the additional probability a displaced migrant becomes employed at an establishment employing a former coworker, relative to the probability for displaced natives. Panel (a) of the figure shows that in any year after the layoff, migrants are indeed significantly (5-6ppt) more likely to be re-employed in firms with a network member. Panel (b) shows that this relative probability is almost twice as large if the former coworker is also a migrant; however, the switching gap is also positive for native networks (Panel c). These results support the referral-based interpretation of coworker networks à la Dustmann et al. (2015), and we similarly find evidence of the particular importance of migrant networks.

In Table B10, we look at whether referrals impact wages. We estimate regression 6 separately for wages of workers employed in network-connected (Panel A) and workers employed in not connected (Panel B) establishments. We find that all of the positive effects of networks on pay estimated in Table 3 are driven by individuals who switched over to connected establishments. Panel A shows that migrant wages increase in any type of coworker network, as long as the network results in the displaced worker getting a job in the connected establishment. In contrast, we do not find evidence for a broader information effect: Panel B shows no impact of networks on wages for individuals who were not hired in a connected establishment.

Overall, our findings suggest that coworker networks play an important role in migrant-native employment and wage gaps. Even though displaced migrants and natives in our setting have the same coworker networks, these networks matter more for the labor market outcomes of displaced migrants. However, the network that matters more – that of other migrant coworkers, as evidenced by the role of job-switching – is of lower quality. As we show in Panel A of Table B10, the wage premium of finding a job via a migrant coworker is half of the wage premium of a job referral from a native coworker. As a result, the lower quality of migrant networks self-perpetuates worse labor market outcomes for displaced migrants.

Finally, we broaden our definition of a network to all individuals living in the same county. In Figure B13, we compare migrant-native gaps across counties with different shares of working age population migrants of the same nationality. The patterns suggest that the broader environment matters: displaced migrants in the counties in the 3rd and 4th decile of migrant-shares fare as well as their native counterparts after displacement. Wages compared to natives are significantly lower, and unemployment compared to natives significantly higher, in counties with both higher and lower shares of migrants. However, Panel (d) of the figure shows that the better-than-average outcomes in the 3rd and 4th decile are likely driven by higher return migration from these areas. We hypothesize that, rather than being the "Goldilocks" areas for migrant assimilation, these counties do not offer sufficiently large migrant networks to help the displaced workers find re-employment.

5 Discussion & Conclusion

In this paper, we show that job loss affects migrants more than natives. Following an exogenous job displacement, migrants are 8 percentage points less likely to be re-employed and their wages are 13 log points lower. Over the 5 years after the displacement, this corresponds to an average additional loss of earnings of 23% per year compared to native workers. These numbers are not only statistically significant but economically meaningful.

Importantly, our estimates cannot be easily explained by migrants being substantially different from natives in terms of their education, experience, or job. In contrast to the literature on migrant-native outcomes which highlights the differences – and convergence – in observable characteristics between the two groups, we employ several strategies to ensure we make a like-for-like comparison. We use a two-step matching algorithm to assign each displaced migrant a near-identical displaced native. Furthermore, we restrict our attention to migrant and native pairs displaced from the same layoff event, allowing us to implicitly control for a further set of characteristics at the employer level. Our focus on mass layoff events ensures that we compare workers leaving employment for the same reason and in the same circumstances.

We have explored a range of potential mechanisms for the large, negative, and persistent migrant-native gap in post-displacement outcomes. We can rule some mechanisms out: the migrant-native gap is not driven by different job preferences or geographic mobility, and stays relatively robust across migrant's tenure in German labor market. We find some evidence of others: We see that worker sorting across firms explains some of the migrants' lower wages, and identify an important role of coworker networks for wages as well as employment, hinting at the central role of the matching process for migrant outcomes. The main takeaway from our mechanism analysis, however, is that the large, negative, and persistent gap between migrant and native outcomes cannot be simply explained by a single channel.

There are also some mechanisms we are unable to rule out, and where more data is needed to fully understand their impact on the cost of job displacement. Perhaps most importantly, we lack data on workers' job search activity: it might be the case that migrants are searching less, or do not direct their search efficiently. This also complicates our interpretation of the search preferences mechanism: if migrants need to send more or different applications to catch up on their pre-displacement income, equal job preferences may in fact drive the migrant-native gap. The fact that we are unable to offer concrete conclusions on the role of labor market discrimination carries similar consequences. While we do not observe migrants' relative outcomes to be better in more competitive labor markets, our measure of competitiveness might be too coarse, or anti-immigrant discrimination might not follow the patterns suggested by Becker (1971). As a result, we cannot determine whether, and how much, of the observed migrant-native gap is driven by this mechanism.

Nevertheless, our paper carries several important implications for policy. We show that the process of migrant assimilation is not linear, and needs to continue even after the migrant finds a permanent position alongside natives: all the displaced migrants in our sample had held for several years native-like permanent jobs in large companies, but their post-displacement labor market trajectory was still different from that of their native coworkers. Policymakers may thus consider tailoring job search assistance and training programs to migrants even after their initial assimilation. Our paper offers some guidance in that respect too. One of our main findings is the relatively large degree of heterogeneity within migrants themselves. In particular, some migrant groups – the more productive and highly-skilled – struggle very little with keeping up with their native coworkers despite their migrant status. This suggests that support programs might be more effective if they target lower-skilled migrants who face the greatest barriers to integration, rather than treating all migrants as a homogeneous group.

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6 Tables

Table 1: Matched Displaced Workers vs. a Random Sample of Full-Time Workers in t-1

	(1) All Workers Migrants	(2) Baseline Sample Migrants	(3) All Workers Natives	(4) Baseline Sample Natives
Panel A: Individual Characteristics				
Years of Education	11.5	11.3	12.4	11.5
	[2.07]	[1.68]	[1.93]	[1.57]
Age (Years)	38.9	37.9	40.9	38.3
	[10.7]	[7.09]	[10.6]	[6.96]
Tenure (Years)	3.43	6.38	3.92	6.41
	[2.78]	[2.52]	[2.87]	[2.52]
Real Daily Wage (EUR)	85.6	89.2	96.8	91.5
	[38.9]	[30.4]	[41.9]	[31.7]
Total Yearly Earnings (EUR)	29802.3	33683.1	34126.2	35675.7
	[14618.2]	[32190.5]	[14984.4]	[37575.2]
AKM Worker FE	4.39	4.33	4.53	4.35
	[0.36]	[0.28]	[0.39]	[0.29]
Panel B: Regional Characteristics				
Lives in City	0.63	0.65	0.45	0.62
	[0.48]	[0.48]	[0.50]	[0.49]
Works in East Germany	0.050	0.057	0.21	0.057
	[0.22]	[0.23]	[0.40]	[0.23]
Panel C: Establishment Characteristics				
Size of Establishment	1288.3	323.1	934.5	323.1
	[4600.1]	[527.4]	[3952.0]	[527.4]
Share Migrant Workers	0.28	0.19	0.054	0.19
	[0.25]	[0.15]	[0.083]	[0.15]
Share High-Skilled Workers	0.11	0.089	0.13	0.089
	[0.16]	[0.13]	[0.17]	[0.13]
Share in Minijob	0.091	0.050	0.084	0.050
	[0.16]	[0.11]	[0.14]	[0.11]
Full-time Daily Wage (Median, EUR)	83.7	84.6	88.6	84.6
	[39.8]	[29.1]	[39.4]	[29.1]
Number of Observations	300,092	15,638	3,995,776	15,638

Notes: This table presents differences in average characteristics for our baseline matched sample of displaced migrants and natives compared to a 2% random sample of full-time workers from the IAB's Integrated Employment Biographies (IEB). Columns (1) and (3) show characteristics of a 2% random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent the displaced migrants and natives in the baseline matched sample. The matching variables are: baseline year, establishment, 3-digit occupation, gender (exact matching); and log wages in t-3, log wages in t-4, age in t-1, education in t-1, and tenure in t-1 (propensity score matching). We report displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours of work per week, and a maximum of EUR 538 total monthly income (as of 2024). We define high-skilled workers as holding a university degree. Standard deviations in brackets.

Table 2: Reported Preferences vs. Realized Outcomes

	(1) Natives Mean		(2) Migrants Gap		(3) Number of Observations
	Mean	Std. Err.	Gap	Std. Err.	
Panel A: Any Search					
Any Job Seeker Spell?	0.54	[0.0066]	0.0010	[0.0057]	31,276
Panel B: Reported Preferences					
Full-Time Job	0.99	[0.00096]	-0.00031	[0.0014]	13,492
Permanent Contract	0.87	[0.0089]	0.0020	[0.0053]	13,454
Outside Commuting Distance	0.47	[0.0070]	0.015	[0.0094]	11,294
Different 3-digit Occ.	0.47	[0.012]	0.0038	[0.017]	3,292
Panel C: Realized Outcomes (Post-Pre)					
Full-Time Job	-0.34	[0.0075]	**-0.11**	[0.0071]	8,395
Log Wage	-0.31	[0.011]	**-0.15**	[0.014]	6,546
Different 3-digit Occ.	0.56	[0.012]	**-0.017**	[0.0074]	7,895
Moves State	0.16	[0.0077]	-0.010	[0.0059]	6,588
Commutes	0.041	[0.0091]	0.018	[0.010]	6,329
Labor Market Thickness	-0.57	[0.21]	0.044	[0.045]	5,944

Notes: This table shows how reported preferences and realized outcomes differ for displaced migrants compared to displaced natives. Column (1) reports the mean for displaced natives; column (2) reports the additional gap for migrant workers. In Panel A, the outcome variable is a dummy indicating whether a worker ever appeared in the UI search records within the 5 years after job loss. In Panels B-C, we restrict the sample to individuals with at least one UI search record. The search outcomes in Panel B are dummies for the types of jobs individuals report searching for in their first meeting with the caseworker after displacement. Panel C reports realized job and mobility outcomes. The mean for natives reports the difference in a given outcome post-layoff (t=0 to t=5) vs. pre-layoff (t=-5 to t=-2), corresponding to the term defined in Equation 3. The gap for migrants reports the within-matched-pair difference post- vs. pre-layoff, corresponding to the term defined in Equation 2. *Outside Commuting Distance* is a dummy that is equal to 1 if an individual is willing to take up a job that requires relocation. *Moves State* is a dummy that is equal to 1 if a worker moves to a job in a different federal state compared to the baseline year. *Commutes* is a dummy that is equal to 1 if a worker lives and works in a different county. Following Jäger et al. (2024), *labor market thickness* measures the share of employed workers in a given 3-digit occupation, year, and commuting zone relative to the national share of employed workers in a given 3-digit occupation and year. We cluster standard errors at the baseline establishment level. Workers in our sample were displaced in 2001-2011, and they were observed from 1997-2016. Coefficients in bold are statistically significant at the 5%-level.

Table 3: The Role of Coworker Networks

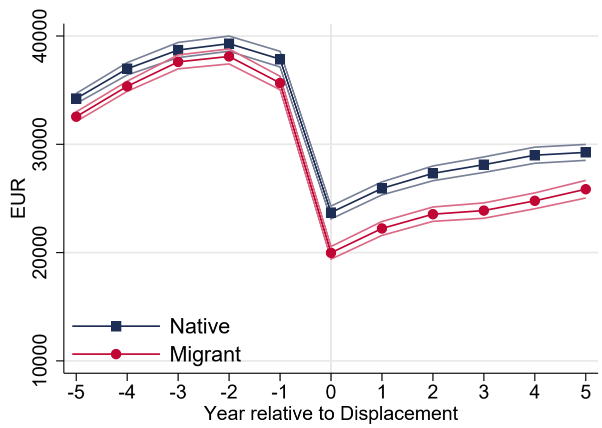
	(1)	(2)	(3)	(4)
	Baseline		Former Coworkers	
		All	Migrants	Natives
Panel A: Log Wages				
Migrant	-0.13	-0.13	-0.14	-0.13
	(0.016)***	(0.016)***	(0.016)***	(0.016)***
$\Omega_{pt,\text{Former Coworkers}}$		0.0066	0.00020	0.0014
		(0.0038)*	(0.0023)	(0.0016)
Migrant $\times$ $\Omega_{pt,\text{Former Coworkers}}$		0.012	0.017	0.0043
		(0.0069)*	(0.0072)**	(0.0025)*
$X_{i,t=0}$	Yes	Yes	Yes	Yes
Observations	22472	22472	22472	22472
Panel B: Employment				
Migrant	-0.081	-0.081	-0.081	-0.081
	(0.0056)***	(0.0054)***	(0.0055)***	(0.0056)***
$\Omega_{pt,\text{Former Coworkers}}$		0.00047	-0.00010	0.00037
		(0.0042)	(0.0025)	(0.00099)
Migrant $\times$ $\Omega_{pt,\text{Former Coworkers}}$		0.0067	0.0044	0.0022
		(0.0026)**	(0.0036)	(0.0013)
$X_{i,t=0}$	Yes	Yes	Yes	Yes
Observations	25354	25354	25354	25354

Notes: This table presents γ, α and β coefficients from regression equation 6, where we regress the difference in a given outcome post- vs. pre-displacement, $\Delta y_{i,p}$, on the network-based outside options measure, a dummy for migrant worker, and an interaction of the two. $X_{i,t=0}$ contains baseline year dummies and a fourth-order polynomial in age. We restrict the sample to matched worker pairs for which all 3 network measures are defined. For a given matched pair p, $\Omega_{p,t=0}$ reports our (standardized) proxy of establishment demand, weighed by the share of former coworkers employed at that establishment j in t=0. Column (1) reports the baseline coefficients. Column (2) reports results where $\Omega_{p,t=0}$ is based on all coworkers. Column (3) reports results where $\Omega_{p,t=0}$ is based on all migrant coworkers. Column (4) reports results where $\Omega_{p,t=0}$ is based on all native coworkers. We cluster standard errors at the baseline establishment level. ***, ** and * correspond to 10, 5 and 1 percent significance levels, respectively. Workers in our sample are displaced in 2001-2011, and they are observed from 1996 to 2017.

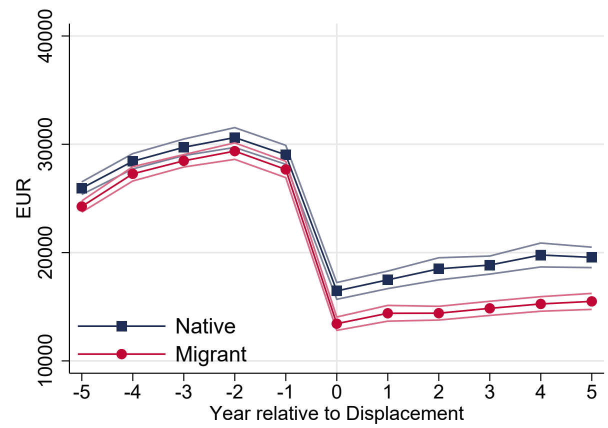
7 Figures

Figure 1: The Migrant-Native Earnings Gap

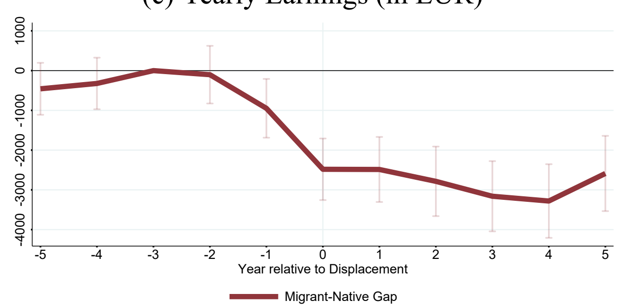
(a) Yearly Earnings (in EUR) - Men



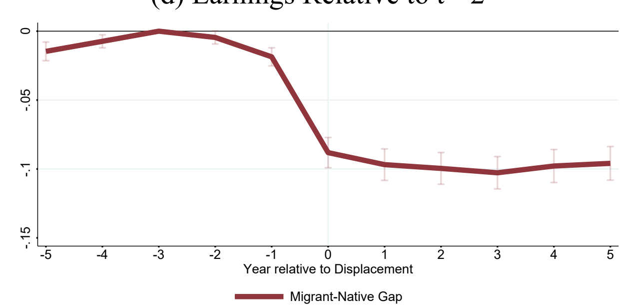
(b) Yearly Earnings (in EUR) - Women



(c) Yearly Earnings (in EUR)



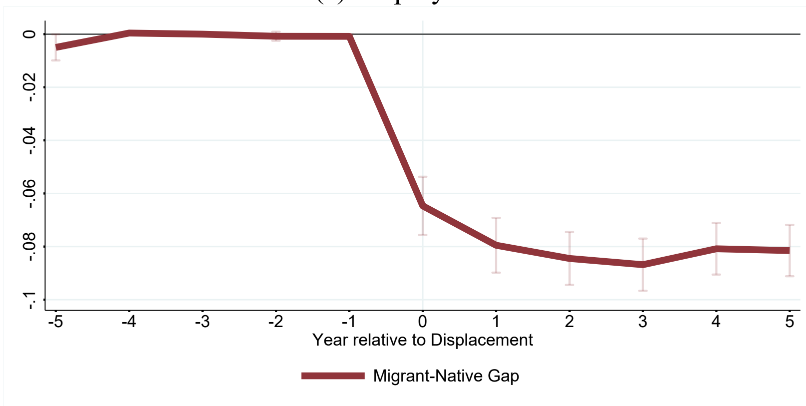
(d) Earnings Relative to t=-2



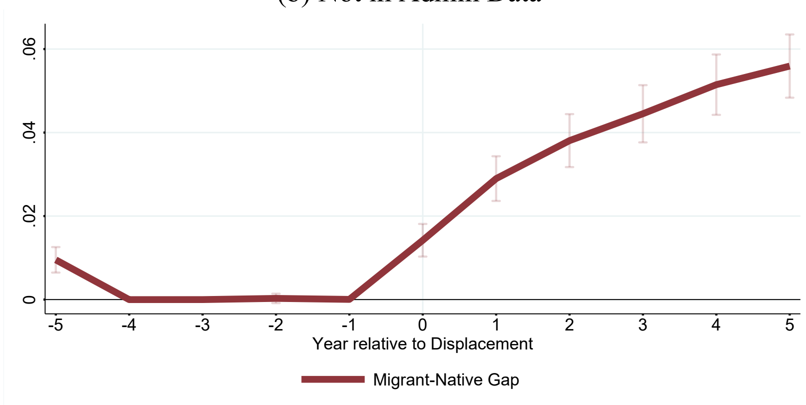
Notes: This figure plots raw means and event study regression coefficients for yearly earnings pre- and post-layoff. Panel (a) plots the raw earnings trajectory for men, separately for natives (blue squares) and migrants (red dots). Panel (b) plots the raw earnings trajectory for women, separately for natives (blue squares) and migrants (red dots). Panel (c) plots the α_j coefficients from regression equation 1 for total yearly earnings (in EUR), pooling men and women. Panel (d) plots the α_j coefficients from regression equation 1 for earnings relative to earnings in t=-2, pooling men and women. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Appendix Table D1, columns (1) and (2) for the corresponding regression output.

Figure 2: The Migrant-Native Employment Gap

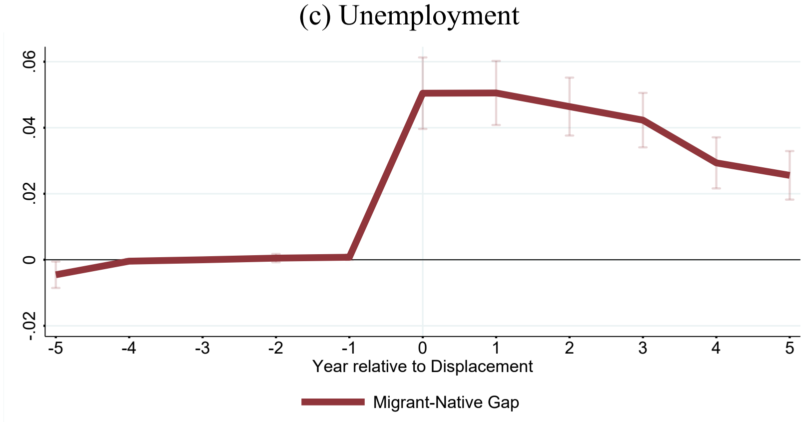
(a) Employment



(b) Not in Admin Data

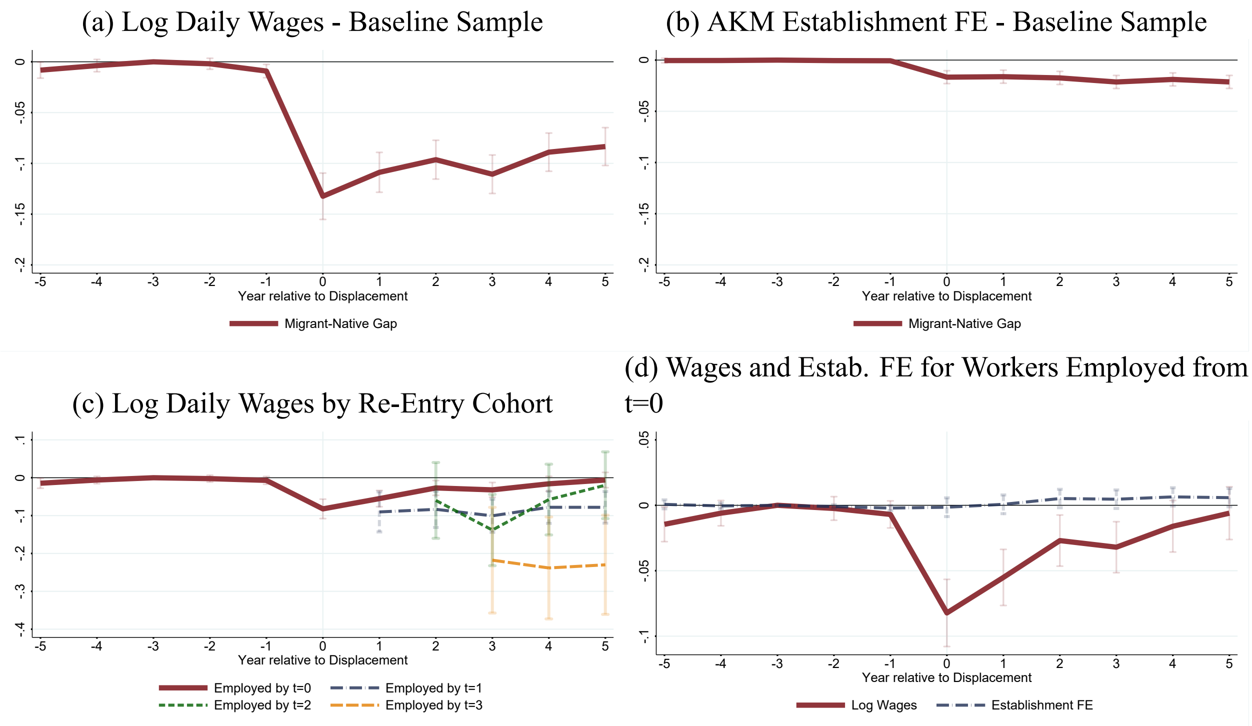


(c) Unemployment



Notes: This figure plots the α_j coefficients from regression equation 1 for the migrant-native employment gap. In Panel (a), the outcome variable is employment, including 0s when there is no administrative record. In Panel (b), the outcome is a dummy taking the value 1 whenever a worker does not have a social-security record (either employment or unemployment), and 0 otherwise. In Panel (c), the outcome is a dummy taking the value 1 whenever a worker is registered as unemployed in the social security data. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Appendix Table D1, columns (5)-(7) for the corresponding regression output.

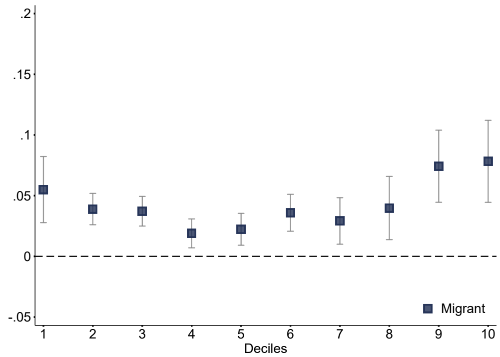
Figure 3: The Migrant-Native Gap in Wages and Establishment Sorting



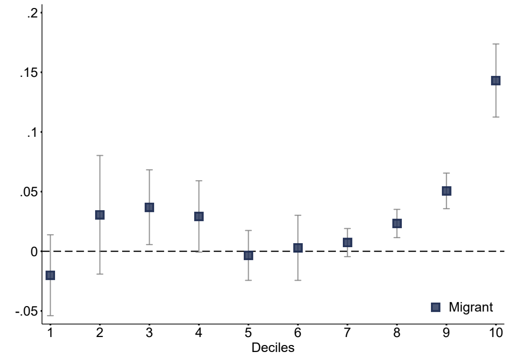
Notes: This figure plots the α_j coefficients from regression equation 1 for the migrant-native wage gap and for establishment sorting. Panels (a) and (b) present coefficients estimated from regressions on the baseline sample. In Panel (b), the outcome variable is the AKM establishment fixed effect provided by Lochner et al. (2024). In Panel (c), we plot wage trajectories for different cohorts of workers: Workers who become re-employed (i) by t=0 (red line), (ii) by t=1 (dark blue line), (iii) by t=2 (green line), and (iv) by t=3 (orange line). In all cohorts, these workers continue to be employed through t=5. In Panel (d) we restrict the sample to displaced workers who become re-employed in t=0 and continue to be employed through t=5. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Appendix Table D1, columns (3)-(4) and Appendix Table D4 for the corresponding regression output.

Figure 4: Migrant-Native Gaps by Pre-Displacement AKM Worker FE and Origin Country Net Income

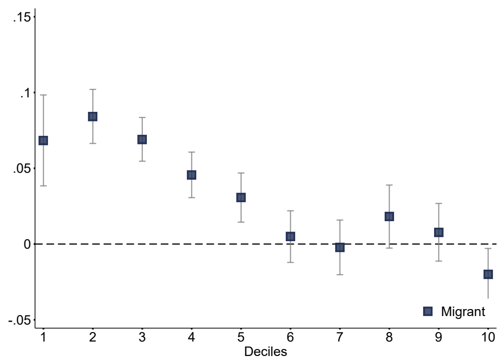
(a) Not in Admin Data - AKM Worker FE



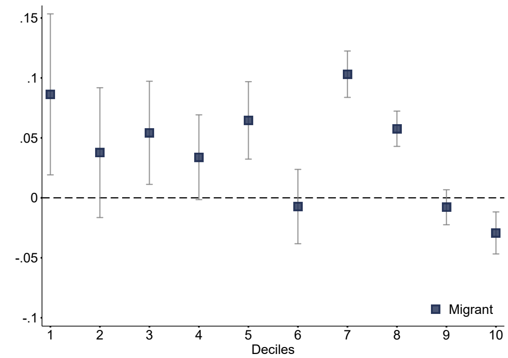
(b) Not in Admin Data - Origin Country Net Income



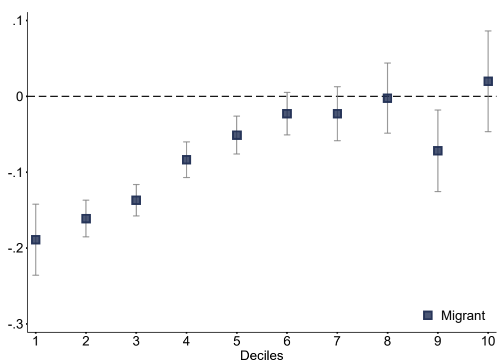
(c) Unemployment - AKM Worker FE



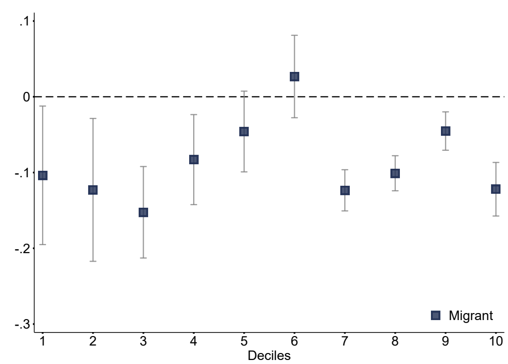
(d) Unemployment - Origin Country Net Income



(e) Relative Earnings - AKM Worker FE



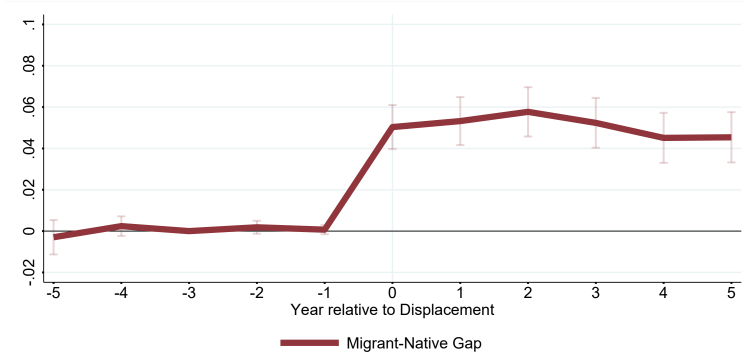
(f) Relative Earnings - Origin Country Net Income



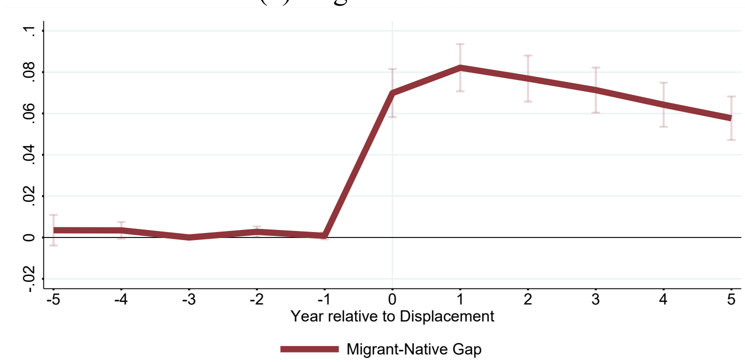
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by migrants' decile of pre-displacement AKM worker FE (Panels a, c, e) and origin country net income (Panels b, d, f), all measured in t=-1. We use the AKM worker FE measure provided by Lochner et al. (2024) and we collect data on "adjusted net income" by country from the World Bank's World Development Indicators (World Bank, 2024). Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. The regressions for Panels (b), (d), and (f) control for pre-displacement worker FE. "Not in admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Appendix Tables D5 and D6 for the corresponding regression output.

Figure 5: Job Switches to Connected Establishments

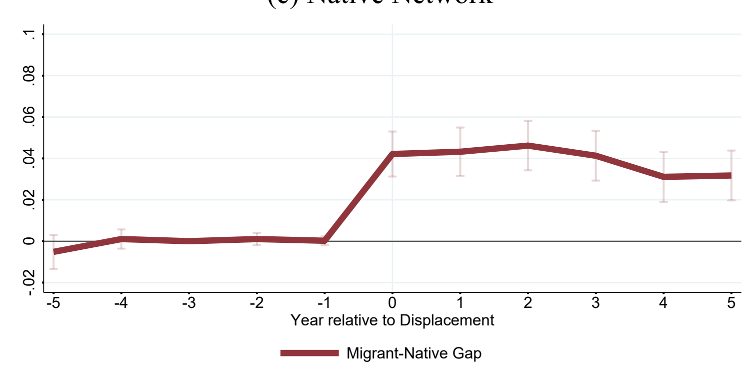
(a) All



(b) Migrant Network



(c) Native Network



Notes: This figure plots the α_j coefficients from regression equation 1 for the migrant-native gap in moving to establishments that are part of their network. We control for AKM worker FE measured in t=-1. In Panel (a), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous coworker. In Panel (b), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous *migrant* coworker. In Panel (c), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous *native* coworker. Coworkers are all workers who were employed at the displacement establishment in the same 3-digit occupation at least once in the 3 years before the layoff and have moved to a different establishment. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Appendix Table D7 for the corresponding regression output.

A Background, Data, Additional Analyses

The below section adds information on the background of the German UI system and on immigrants in the German labor market. It also describes additional data sets and our definition of indicators such as local thickness. Finally, it details the alternative matching specifications that we use for the triple-differences estimations.

A.1 The German Immigration System

Migrants from non-EU member states In the 2000s, Germany had quite a restrictive immigration system for non-EU workers. Two separate institutions had to approve the request for a work visa: The *Ausländerbehörde* (foreigners' registration office) and the Federal Employment Agency. Employers had to prove that no German applicants (or workers without German citizenship, but with equal labor market rights) were available for the job (so-called *Vorrangprüfung*).

Before January 1, 2005, migrants were subject to the *Ausländergesetz* or "aliens act". Depending on their permit, they were entitled to (temporarily) work in Germany. They had to live and work in Germany for a minimum of 8 years before they were eligible to apply for a residence permit.

On January 1, 2005, the so-called *Aufenthaltsgesetz* was introduced. This somewhat modernized the status quo, e.g., by reducing the barriers for ICT workers. The duration in Germany needed to apply for a residence permit was reduced from 8 to 5 years. Still, migrants needed a permit from the foreigners' registration office and the Federal Employment Agency to start working in Germany.

Migrants from EU countries Access to the German labor market was (and is) much easier for migrants from EU member states. EU law requires citizens from other EU countries to be treated like German citizens in the German labor market ("free movement of labor" principle). This means that citizens from these countries do not need a visa or work permit to start working in Germany, and firms do not have to prove that no similarly qualified German applied for the job. Note that while Central and Eastern European countries such as the Czech Republic and Poland entered the EU in 2004, their citizens were granted the "free movement" status from 2011, only. They were thus treated as third-party nationals for all but the last year of our mass layoff period.

A.2 Unemployment Insurance in Germany

In Germany, every worker who worked for at least 12 months in the 24 months before becoming unemployed is entitled to receive *Arbeitslosengeld I (ALG I, unemployment in-*

surance type I) benefits. Individuals in the ALG I scheme receive 60% (or 67% if there are children in the household) of their last net income. They need to be registered as unemployed job seekers with their local employment agency and actively look for jobs. Individuals aged 49 or younger are entitled to receive ALG 1 for up to 1 year; older individuals can receive the benefits for up to 2 years.[41]

Once eligibility for ALG I expires, individuals received the less generous *Arbeitslosenhilfe* (job seeker's allowance, pre-2005) or *Arbeitslosengeld II (ALG II, unemployment insurance type II)* benefits. The pre-2005 job seeker's allowance policy had a net income replacement rate of 53% (57% with kids). Only individuals who previously received ALG I were eligible; i.e., recipients of the job seeker's allowance had to have worked for at least 12 months in the 24 months before unemployment to be eligible.

From 2005, the ALG II policy meant substantially reduced benefits but eligibility was not attached to previous employment. For individuals without a family, the monthly base benefit was EUR 345 in 2005 (this had increased to EUR 364 by 2011). On top of this, there are benefits for rent and utilities and additional benefits for kids. There is no time limit - individuals are entitled to receive these benefits until they take up a new job.

In the first year of unemployment, migrants are subject to the same rules as natives. For our baseline sample, which includes individuals with three years of tenure before job displacement, this means that migrants and natives are entitled to the same level of benefits. However, there is one important distinction: non-EU migrants must leave Germany if they can no longer "assure their livelihood." In practice, this means they can stay as long as they receive ALG I but are not eligible for ALG II. Therefore, once their ALG I eligibility expires, they must either leave the country, find a new job, or secure financial support from someone else.

As with most policies, there are exceptions. Individuals from a country deemed "unsafe" by the German government cannot be deported. There are also exceptions for individuals with a German spouse or with children who have German citizenship.

A.3 Additional Data

Population Data In order to analyze the role of local same-nationality working age population shares, we use the data set *Population and Employment, Foreign Population, Results of the Central Register of Foreigners, Destatis, 2019* (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 of a given year. It 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 the same-nationality share

[41] Individuals older than 49 years make up only a small part of our sample: 3.3%.

measure, we restrict the data to the working-age population, i.e. individuals aged 15-65. In the last step, we divide the number of each nationality in a given county by the overall working-age population in that county on December 31:

$$Share_{oct} = \frac{P_{oct}}{P_{oct} + N_{ct}} \tag{7}$$

where P_{oct} is the number of working-age citizens from a given origin 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 B16 shows how the share of the same-nationality working-age population is distributed among displaced workers.

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.

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 et al. (2024). 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 her 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 her the establishment fixed effect for the respective establishment in the year range 2000-2006.

Physical Proximity Our physical-proximity measure is constructed following the high physical-proximity measure $\overline{HPP}_j$ defined by Mongey et al. (2021). This indicator, derived from a variable from O*Net labor data, measures the requirement for physical proximity in an occupation on a scale from 1 to 5, with 5 indicating the highest degree of need for physical proximity. Additionally, Mongey et al. (2021) create a binary physical-proximity indicator HPP_j^*, which is 1 if $\overline{HPP}_j$ is above the "employment-weighted median across OCC occupations of physical proximity", and 0 otherwise. We use the continuous version of the measure for our analysis.

Job Search Data For our measurement of job search preferences, we draw on the *Job-seeker 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 job seeker is registered for the first time.

We use the following indicators available in this data: The 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 only available for spells starting before July 2006, meaning that we have to restrict the time frame of our sample for part of the job search analysis.

Thickness Indicator Panels (c) and (d) of Figure B12 plot the migrant-native gap in employment and wage losses by quartiles of labor market thickness. We follow Jäger et al. (2024) and define labor market thickness in the following way:

$$Thickness_{cz,occ,t} = \frac{Workers_{cz,occ,t}}{Workers_{cz,t}} \div \frac{Workers_{DE,occ,t}}{Workers_{DE,t}} \tag{8}$$

where $\frac{Workers_{cz,occ,t}}{Workers_{cz,t}}$ is the share of employed workers in a given commuting zone and 3-digit occupation in a given year; $\frac{Workers_{DE,occ,t}}{Workers_{DE,t}}$ is the share of employed workers in a given 3-digit occupation in a given year in Germany.

A.4 Alternative Matching Approach 1

Our analysis differs from the seminal papers on job displacement à la Jacobson et al. (1993) in that we compare displaced migrant and native workers to each other, rather than matching a migrant (native) displaced worker to a similar migrant (native) non-displaced worker. To test whether our main results hold with the "classic" approach, we follow Schmieder et al. (2023) and employ an alternative empirical strategy where we assign each displaced worker a non-displaced worker match within cells of migrant status. This means that each native displaced worker gets assigned a similar native, non-displaced control twin, and each migrant displaced worker gets assigned a similar migrant, non-displaced control twin.

In addition, we match exactly on gender, workplace in East vs. West Germany, county, 3-digit occupation, and 3-digit industry (all measured in t=-1). If there are several potential controls for a displaced worker within these cells, we select the closest match based on propensity score matching on log wages (t=-3 and t=-4), age, years of education, years of tenure, and establishment size (all measured in t=-1). We apply the same baseline restrictions as in the main analysis.

To get at the treatment effects from job displacement, we then estimate the following triple-differences regression:

$$
\begin{aligned}
y_{itc} = & \sum_{j=-5, j\neq -3}^{5} \alpha_j \times I(t = c + 1 + j) \times Disp_i \times Migrant_i \\
& + \sum_{j=-5, j\neq -3}^{5} \beta_j \times I(t = c + 1 + j) \times Disp_i \\
& + \sum_{j=-5, j\neq -3}^{5} \omega_j \times I(t = c + 1 + j) \\
& + \pi_t + \gamma_i + X_{it}\beta + \varepsilon_{itc}
\end{aligned} \tag{9}
$$

where the dependent variable y_{itc} denotes average labor market outcomes (e.g., log daily wages) of individual i, belonging to cohort c in year t. $Disp_i$ is a dummy indicating whether a worker is displaced. As in equation 1, we interact with dummies $I(t = c+1+j)$ for 5 years before and after the job loss and omit period $t = -3$. In equation (9), α_j represents the triple-difference effect of job displacement on migrants relative to natives, at relative time j from the displacement event. The coefficients β_j capture the evolution of labor market outcomes for displaced workers relative to the non-displaced control group. We add controls for time-since-displacement, year-fixed effects π_t, and individual fixed effects γ_i. X_{it} includes baseline year (cohort) interacted with time since displacement and a fourth-order polynomial in age. We cluster standard errors at the worker level.

Table B4 shows how matched migrants and natives differ from a 2% random sample of full-time workers in the German social-security data. While the displacement sample is, with a few exceptions such as tenure, relatively comparable to the random worker sample, there are substantial differences between displaced migrants and natives. For example, displaced migrants work in firms with much higher shares of migrant workers (26%) compared to displaced natives (7%). They also earn substantially lower wages pre-layoff, pointing to lower productivity. When drawing conclusions from this analysis, it is therefore important to keep in mind that here, in contrast to our main analysis, we do not control for observational differences between displaced natives and displaced migrants. It is therefore unclear to what extent the differences between the two groups are driven by observational differences, such as differential sorting across firms or occupations.

Even in this alternative analysis, however, the displaced migrants and natives have substantially higher tenure compared to the average full-time worker in Germany. While this difference means that our baseline estimate of the migrant-native gap in the cost of job displacement might not reflect the migrant-native gap for the whole population, it is motivated by the necessary restrictions placed by our methodology. We focus on displacement due to mass layoffs to ensure we only compare workers who become unemployed for the same, exogenous, reason. The job displacement literature (Jacobson et al., 1993; Schmieder et al., 2023), which we follow closely, usually focuses on workers with at least three years of tenure at the time of displacement, to ensure that displaced workers had stable careers before the lay-off. Several robustness checks concerning our baseline restrictions, for example reducing the tenure requirement to one year, confirm that the migrant-native gap that we document is prevalent in different displaced worker samples (see Table B7).

Figure B1 and Table D2 present the results. The gaps are substantially larger than in the baseline results: Migrants face an additional relative earnings gap of around 19%. This is both because they have higher wage losses (24% gap in the layoff year and 19% gap five years out) and because they struggle more to become re-employed (an approximately 13ppt difference post layoff). Given the observational differences observed in Table B4, it is not surprising that this naive comparison of migrants and natives yields larger gaps. We therefore complement this analysis with an additional, *double matching* approach in Section A.5.

A.5 Alternative Matching Approach 2

The matching approach outlined in Section A.4 has one important disadvantage: It does not control for observational differences between (non-)displaced migrants and natives. We therefore additionally apply a *double matching* approach, where we extend our baseline matching to include a second matching step.

Recall that in our baseline matching (detailed in Section 3), we match displaced migrants to displaced natives. For the *double matching* approach, we additionally assign each displaced migrant (native) in the baseline sample a non-displaced migrant (native) counterpart from our pool of non-displaced control workers. In the second step, we cannot compare workers from the same establishment; instead, we match exactly on county and 3-digit occupation.

If there are several potential controls for a displaced worker within these cells, we select the closest match based on propensity score matching on log wages (t=-3 and t=-4), age, years of education and years of tenure (all measured in t=-1)[42]. We apply the same baseline restrictions as in the main analysis. We then estimate triple-difference regressions as specified in Equation 9. Figure B2 and Table D3 show that the costs of job loss that we estimate using this alternative strategy are very comparable to the baseline results.

Table B5 shows how matched migrants and natives differ from a 2% random sample of full-time workers in the German social-security data. Note that one disadvantage of the *double matching* is that we lose workers from the baseline sample for which we did not find a counterfactual, non-displaced worker. We lose disproportionally more migrants: We retain roughly 73% of baseline sample migrants, and 92.6% of baseline sample natives.

A comparison of Table 1 and Table B5 shows that adding the second matching step leaves us with a slightly more positively selected sample of migrants. This holds in terms of education, wages, earnings, and worker fixed effects. In addition, the alternative matching approach 2 more often kicks out migrants living in rural areas, and in larger establishments with a lower share of high-skilled workers. Similar patterns hold for natives.

[42]Note that these are the same variables that we use in the baseline matching.

B Appendix Tables and Figures

Table B1: 1-Digit Industries of Displaced Workers vs. a Random Sample

	(1) All Workers Migrants	(2) Baseline Sample Migrants	(3) All Workers Natives	(4) Baseline Sample Natives
1-Digit Industries				
Agriculture	0.012	0.00045	0.0079	0.00045
	[0.11]	[0.021]	[0.089]	[0.021]
Mining, Energy	0.0079	0.036	0.017	0.036
	[0.088]	[0.19]	[0.13]	[0.19]
Food Manufacturing	0.034	0.074	0.026	0.074
	[0.18]	[0.26]	[0.16]	[0.26]
Consumption Goods	0.041	0.11	0.041	0.11
	[0.20]	[0.32]	[0.20]	[0.32]
Production Goods	0.091	0.11	0.066	0.11
	[0.29]	[0.31]	[0.25]	[0.31]
Investment Goods	0.19	0.16	0.15	0.16
	[0.40]	[0.37]	[0.35]	[0.37]
Construction	0.054	0.038	0.059	0.038
	[0.23]	[0.19]	[0.24]	[0.19]
Retail	0.11	0.14	0.14	0.14
	[0.31]	[0.34]	[0.35]	[0.34]
Traffic, Telecommunication	0.062	0.074	0.062	0.074
	[0.24]	[0.26]	[0.24]	[0.26]
Credit, Insurance	0.014	0.0083	0.040	0.0083
	[0.12]	[0.091]	[0.20]	[0.091]
Restaurants	0.082	0.015	0.022	0.015
	[0.27]	[0.12]	[0.15]	[0.12]
Education	0.014	0.0042	0.025	0.0042
	[0.12]	[0.064]	[0.16]	[0.064]
Health	0.054	0.013	0.091	0.013
	[0.23]	[0.11]	[0.29]	[0.11]
Commercial Services	0.17	0.18	0.15	0.18
	[0.38]	[0.38]	[0.36]	[0.38]
Other Services	0.037	0.025	0.034	0.025
	[0.19]	[0.16]	[0.18]	[0.16]
Non-Profit	0.010	0.0086	0.014	0.0086
	[0.10]	[0.092]	[0.12]	[0.092]
Public Administration	0.013	0.0061	0.056	0.0061
	[0.11]	[0.078]	[0.23]	[0.078]
Number of Observations	300,092	15,638	3,995,776	15,638

Notes: Differences in the distribution across 1-digit industries for our baseline sample of displaced migrants and natives compared to a 2% random sample of full-time workers from the IAB's Integrated Employment Biographies (IEB) in t=-1. Columns (1) and (3) show characteristics of a 2% random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent the displaced migrants and natives in the baseline matched sample. The matching variables are: baseline year, establishment, 3-digit occupation, gender (exact matching); and log wages in t-3, log wages in t-4, age in t-1, education in t-1, and tenure in t-1 (propensity score matching). We report displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Standard deviations in brackets.

Table B2: 1-Digit Occupations of Displaced Workers vs. a Random Sample

	(1) All Workers Migrants	(2) Baseline Sample Migrants	(3) All Workers Natives	(4) Baseline Sample Natives
1-Digit Occupations				
Agriculture, Gardening, Work with Animals	0.022	0.0049	0.016	0.0049
	[0.15]	[0.070]	[0.12]	[0.070]
Raw Material Extraction, Production, Manufacturing	0.37	0.49	0.28	0.49
	[0.48]	[0.50]	[0.45]	[0.50]
Construction, Architecture, Building Services Engineering	0.074	0.084	0.069	0.084
	[0.26]	[0.28]	[0.25]	[0.28]
Natural Sciences, Geography, Computer Sciences	0.037	0.038	0.040	0.038
	[0.19]	[0.19]	[0.20]	[0.19]
Transport, Logistics, Security	0.19	0.22	0.12	0.22
	[0.39]	[0.41]	[0.33]	[0.41]
Commercial Services, Retail, Sales, Hospitality	0.085	0.053	0.086	0.053
	[0.28]	[0.22]	[0.28]	[0.22]
Business Administration, Accounting, Law, Public Admin	0.096	0.079	0.24	0.079
	[0.30]	[0.27]	[0.42]	[0.27]
Health, Social Services, Education	0.065	0.023	0.11	0.023
	[0.25]	[0.15]	[0.32]	[0.15]
Humanities, Economics, Media, Arts	0.013	0.0079	0.017	0.0079
	[0.11]	[0.088]	[0.13]	[0.088]
Military	0	0	0	0
	[0]	[0]	[0]	[0]
Not classified	0.052	0	0.023	0
	[0.22]	[0]	[0.15]	[0]
Number of Observations	300,092	15,638	3,995,776	15,638

Notes: This table presents differences in the distribution across 1-digit occupations according to the German Classification of Occupations (KldB 2010, see Paulus and Matthes (2013)) for our baseline sample of displaced migrants and natives compared to a 2% random sample of full-time workers from the IAB's Integrated Employment Biographies (IEB). Columns (1) and (3) show characteristics of a 2% random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent the displaced migrants and natives in the baseline matched sample. The matching variables are: baseline year, establishment, 3-digit occupation, gender (exact matching); and log wages in t-3, log wages in t-4, age in t-1, education in t-1, and tenure in t-1 (propensity score matching). We report displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Standard deviations in brackets.

Table B3: 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	11.3	11.4	11.5	11.7
	[1.60]	[1.85]	[1.52]	[1.75]
Age (Years)	37.7	38.4	38.0	39.2
	[6.94]	[7.39]	[6.84]	[7.27]
Tenure (Years)	6.47	6.16	6.44	6.28
	[2.55]	[2.43]	[2.55]	[2.44]
Real Daily Wage (EUR)	88.8	90.1	91.2	92.4
	[28.9]	[33.5]	[30.9]	[34.4]
Total Yearly Earnings (EUR)	33319.5	34510.0	35506.1	36254.5
	[30434.4]	[35855.4]	[36577.0]	[40798.1]
Panel B: Regional Characteristics				
Lives in City	0.67	0.62	0.62	0.61
	[0.47]	[0.49]	[0.49]	[0.49]
Works in East Germany	0.057	0.056	0.058	0.053
	[0.23]	[0.23]	[0.23]	[0.22]
Panel C: Establishment Characteristics				
Size of Establishment	318.7	333.1	315.1	350.2
	[517.8]	[548.6]	[512.6]	[574.5]
Share Migrant Workers	0.18	0.20	0.19	0.20
	[0.14]	[0.15]	[0.14]	[0.15]
Share High-Skilled Workers	0.085	0.098	0.086	0.097
	[0.12]	[0.15]	[0.13]	[0.15]
Share Marginally Employed Workers	0.052	0.046	0.051	0.047
	[0.12]	[0.11]	[0.12]	[0.11]
Number of Observations	10,862	4,776	12,094	3,544

Notes: This table shows the characteristics of displaced workers in our baseline sample in the year prior to displacement. Stayers (columns 1 and 3) are workers who are always observed in the admin data throughout our observation period. Drop-outs (columns 2 and 4) are workers who drop out of the admin data for at least one year during our observation period. Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours of work per week, and a maximum of EUR 538 total monthly income (as of 2024). We define high-skilled workers as holding a university degree. Standard deviations in brackets.

Table B4: Summary Statistics for Alternative Matching Approach 1

	(1)	(2)	(3)	(4)	(5)	(6)
		Migrants			Natives	
	All Workers	Baseline Sample		All Workers	Baseline Sample	
		Displaced	Non-Displaced		Displaced	Non-Displaced
Panel A: Individual Characteristics						
Years of Education	11.5	11.3	11.2	12.4	12.3	12.4
	[2.07]	[1.67]	[1.66]	[1.93]	[1.79]	[1.79]
Age (Years)	38.9	37.3	38.1	40.9	38.7	39.0
	[10.7]	[6.99]	[7.06]	[10.6]	[6.95]	[6.94]
Tenure (Years)	3.43	6.18	6.44	3.92	6.19	6.37
	[2.78]	[2.44]	[2.46]	[2.87]	[2.42]	[2.46]
Real Daily Wage (EUR)	85.6	85.3	89.0	96.8	100.0	102.7
	[38.9]	[31.4]	[31.2]	[41.9]	[36.6]	[36.3]
Total Yearly Earnings (EUR)	29802.3	32502.3	37363.3	34126.2	43715.2	47191.1
	[14618.2]	[33278.0]	[43476.2]	[14984.4]	[52184.9]	[53897.3]
AKM Worker FE	4.39	4.33	4.37	4.53	4.50	4.54
	[0.36]	[0.29]	[0.29]	[0.39]	[0.34]	[0.34]
Panel B: Regional Characteristics						
Lives in City	0.63	0.83	0.82	0.45	0.61	0.62
	[0.48]	[0.38]	[0.38]	[0.50]	[0.49]	[0.49]
Works in East Germany	0.050	0.063	0.063	0.21	0.30	0.30
	[0.22]	[0.24]	[0.24]	[0.40]	[0.46]	[0.46]
Panel C: Establishment Characteristics						
Size of Establishment	1288.3	302.4	413.8	934.5	404.1	458.4
	[4600.1]	[462.4]	[748.4]	[3952.0]	[678.1]	[774.6]
Share Migrant Workers	0.28	0.26	0.25	0.054	0.070	0.065
	[0.25]	[0.20]	[0.20]	[0.083]	[0.096]	[0.090]
Share High-Skilled Workers	0.11	0.084	0.086	0.13	0.13	0.14
	[0.16]	[0.13]	[0.14]	[0.17]	[0.16]	[0.16]
Share in Minijob	0.091	0.083	0.082	0.084	0.051	0.048
	[0.16]	[0.17]	[0.16]	[0.14]	[0.11]	[0.11]
Full-time Daily Wage (Median, EUR)	83.7	81.4	82.5	88.6	88.5	89.7
	[39.8]	[32.3]	[30.5]	[39.4]	[34.2]	[33.4]
Number of Observations	300,092	8,142	8,142	3,995,776	97,610	97,610

Notes: This table presents differences in average characteristics for our alternative sample of (non-)displaced migrants and natives compared to a 2% random sample of full-time workers from the IAB's Integrated Employment Biographies (IEB). See Appendix Section A.4 for an overview on how we construct the alternative sample of (non-)displaced workers. Columns (1) and (4) show characteristics for a 2% random sample of workers in Germany 2000-2010. Columns (2) and (5) show characteristics for all displaced migrants and natives, respectively. Columns (3) and Columns (6) show characteristics of matched control migrants and natives, respectively. We report displaced and matched non-displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours of work per week, and a maximum of EUR 538 total monthly income (as of 2024). We define high-skilled workers as holding a university degree. Standard deviations in brackets.

Table B5: Summary Statistics for Alternative Matching Approach 2

	(1)	(2)	(3)	(4)	(5)	(6)
		Migrants			Natives	
	All Workers	Baseline Sample		All Workers	Baseline Sample	
		Displaced	Non-Displaced		Displaced	Non-Displaced
Panel A: Individual Characteristics						
Years of Education	11.5	11.4	11.3	12.4	11.6	11.6
	[2.07]	[1.70]	[1.65]	[1.93]	[1.58]	[1.52]
Age (Years)	38.9	37.7	37.7	40.9	38.2	38.6
	[10.7]	[7.10]	[7.28]	[10.6]	[6.96]	[7.13]
Tenure (Years)	3.43	6.32	6.56	3.92	6.37	6.46
	[2.78]	[2.49]	[2.54]	[2.87]	[2.50]	[2.56]
Real Daily Wage (EUR)	85.6	90.2	94.8	96.8	92.0	96.6
	[38.9]	[31.2]	[30.3]	[41.9]	[32.0]	[31.3]
Total Yearly Earnings (EUR)	29802.3	34581.7	39354.1	34126.2	36248.3	40066.4
	[14618.2]	[34762.9]	[40725.4]	[14984.4]	[38784.3]	[39229.1]
AKM Worker FE	4.39	4.35	4.41	4.53	4.36	4.43
	[0.36]	[0.29]	[0.28]	[0.39]	[0.29]	[0.29]
Panel B: Regional Characteristics						
Lives in City	0.63	0.70	0.71	0.45	0.63	0.62
	[0.48]	[0.46]	[0.46]	[0.50]	[0.48]	[0.49]
Works in East Germany	0.050	0.058	0.058	0.21	0.060	0.060
	[0.22]	[0.23]	[0.23]	[0.40]	[0.24]	[0.24]
Panel C: Establishment Characteristics						
Size of Establishment	1288.3	288.1	951.0	934.5	324.9	875.1
	[4600.1]	[447.8]	[2853.5]	[3952.0]	[523.7]	[2591.5]
Share Migrant Workers	0.28	0.19	0.18	0.054	0.19	0.12
	[0.25]	[0.15]	[0.15]	[0.083]	[0.15]	[0.12]
Share High-Skilled Workers	0.11	0.094	0.10	0.13	0.091	0.10
	[0.16]	[0.14]	[0.14]	[0.17]	[0.13]	[0.13]
Share in Minijob	0.091	0.054	0.056	0.084	0.052	0.055
	[0.16]	[0.12]	[0.12]	[0.14]	[0.12]	[0.11]
Full-time Daily Wage (Median, EUR)	83.7	85.8	90.2	88.6	85.0	90.4
	[39.8]	[30.9]	[31.1]	[39.4]	[29.6]	[29.9]
Number of Observations	300,092	11,411	11,411	3,995,776	14,478	14,478

Notes: This table presents differences in average characteristics for our alternative sample of (non-)displaced migrants and natives compared to a 2% random sample of full-time workers from the IAB's Integrated Employment Biographies (IEB). See Appendix Section A.5 for an overview on how we construct the alternative sample of (non-)displaced workers. Columns (1) and (4) show characteristics for a 2% random sample of workers in Germany 2000-2010. Columns (2) and (5) show characteristics for all displaced migrants and natives, respectively. Columns (3) and Columns (6) show characteristics of matched control migrants and natives, respectively. We report displaced and matched non-displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours of work per week, and a maximum of EUR 538 total monthly income (as of 2024). We define high-skilled workers as holding a university degree. Standard deviations in brackets.

Table B6: Robustness: Different Sample Restrictions

	(1) Baseline	(2) No Western Migrants	(3) No Top AKM Decile	(4) No Naturalized Migrants	(5) No East Germany	(6) Baseline Years 2000-2003
Panel A: Earnings Relative to t=-2						
Migrant	-0.088	-0.092	-0.092	-0.091	-0.083	-0.10
	(0.0056)**	(0.0064)**	(0.0072)**	(0.0078)**	(0.0063)**	(0.0089)**
Observations	15638	11415	15007	14864	14746	7448
$\Delta y_{native,p}$	-.39	-.391	-.401	-.393	-.38	-.438
	(.004)	(.004)	(.004)	(.004)	(.004)	(.005)
Panel B: Log Wages						
Migrant	-0.12	-0.15	-0.13	-0.13	-0.11	-0.14
	(0.011)**	(0.013)**	(0.015)**	(0.015)**	(0.012)**	(0.021)**
Observations	12395	9159	11912	11765	11766	5656
$\Delta y_{native,p}$	-.389	-.42	-.409	-.393	-.377	-.441
	(.007)	(.008)	(.007)	(.007)	(.007)	(.01)
Panel C: Unemployment						
Migrant	0.042	0.061	0.045	0.043	0.039	0.053
	(0.0033)**	(0.0042)**	(0.0044)**	(0.0041)**	(0.0034)**	(0.0067)**
Observations	15638	11415	15007	14864	14746	7448
$\Delta y_{native,p}$	.227	.251	.233	.228	.219	.269
	(.002)	(.003)	(.002)	(.002)	(.002)	(.004)
Panel D: Not in Admin data						
Migrant	0.036	0.018	0.034	0.038	0.036	0.037
	(0.0031)**	(0.0027)**	(0.0037)**	(0.0038)**	(0.0038)**	(0.0044)**
Observations	15638	11415	15007	14864	14746	7448
$\Delta y_{native,p}$	.1	.07	.1	.1	.1	.1
	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)

Notes: Each column in this table presents our main results for a different sample. The gap for migrants reports the regression-adjusted within-matched-pair difference post- vs. pre-layoff, corresponding to the term defined in Equation 2. We control for baseline year and a fourth-order polynomial in age. $\Delta y_{native,p}$ reports the mean change for natives, see Equation 3 for the exact definition. Column (1) reports the baseline gap. Column (2) reports results when excluding Western migrants. Column (3) reports results when excluding migrants in the top AKM worker FE decile (measured at t=-1). Column (4) reports results when excluding migrants who become German citizenship between their first spell in the German admin data and the employment spell at the layoff firm in t=-1. Column (5) reports results when excluding workers displaced from an establishment located in East Germany. Column (6) reports results for a sample of workers displaced in 2000-2003 (and thus well before the financial crisis). ”Not in admin data” is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. We cluster standard errors at the baseline establishment level. * and ** correspond to 5 and 1 percent significance levels, respectively. Workers in our sample are displaced in 2001-2011, and they are observed from 1996 to 2017.

Table B7: Robustness: Different Baseline Restrictions and Matching

	(1) Baseline	(2) 1 Year Tenure	(3) 2 Years Tenure	(4) Firmsize >= 30	(5) No Matching on Wages	(6) Match Migrant to Native
Panel A: Earnings Relative to t=-2						
Migrant	-0.088	-0.073	-0.077	-0.083	-0.084	-0.084
	(0.0057)**	(0.0069)**	(0.0069)**	(0.0063)**	(0.0067)**	(0.0065)**
Observations	15638	21370	18315	18012	16182	16179
$\Delta y_{native,p}$	-.39	-.325	-.358	-.378	-.379	-.379
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Panel B: Log Wages						
Migrant	-0.12	-0.098	-0.11	-0.12	-0.11	-0.12
	(0.011)**	(0.015)**	(0.014)**	(0.013)**	(0.014)**	(0.014)**
Observations	12395	17141	14631	14259	12777	12771
$\Delta y_{native,p}$	-.389	-.312	-.349	-.385	-.385	-.385
	(.007)	(.006)	(.006)	(.006)	(.007)	(.007)
Panel C: Unemployment						
Migrant	0.042	0.038	0.039	0.041	0.040	0.041
	(0.0033)**	(0.0040)**	(0.0035)**	(0.0040)**	(0.0042)**	(0.0043)**
Observations	15638	21370	18315	18012	16182	16179
$\Delta y_{native,p}$	.227	.213	.219	.219	.22	.22
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Panel D: Not in Admin data						
Migrant	0.036	0.033	0.035	0.035	0.035	0.035
	(0.0029)**	(0.0031)**	(0.0036)**	(0.0034)**	(0.0037)**	(0.0038)**
Observations	15638	21370	18315	18012	16182	16179
$\Delta y_{native,p}$	.1	.1	.1	.1	.1	.1
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)

Notes: Each column in this table presents our main results for a sample with different baseline restrictions or a different matching algorithms. The gap for migrants reports the regression-adjusted within-matched-pair difference post- vs. pre-layoff, corresponding to the term defined in Equation 2. We control for baseline year and a fourth-order polynomial in age. $\Delta y_{native,p}$ reports the mean change for natives, see Equation 3 for the exact definition. Column (1) reports the baseline gap. Columns (2) and (3) report results when relaxing the baseline tenure restriction to 1 and 2 years (instead of 3 years), respectively. Column (4) reports results when relaxing the baseline firm size restriction to 30 workers (instead of 50 workers). Column (5) reports results for a matching algorithm where we do not match on pre-displacement wages. Column (6) reports results for a matching algorithm where instead of finding a 1:1 native worker match for each displaced migrant, we find a 1:1 migrant worker match for each displaced native. "Not in admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. We cluster standard errors at the baseline establishment level. * and ** correspond to 5 and 1 percent significance levels, respectively.

Table B8: Robustness: Additional Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Relative Earnings		Employment		Log Wages	
Migrant × t=-5	-0.015	-0.014	-0.0050	-0.0050	-0.0082	-0.0084
	(0.0034)***	(0.0033)***	(0.0025)**	(0.0024)**	(0.0040)**	(0.0039)**
Migrant × t=-4	-0.0073	-0.0073	0.00042	0.00011	-0.0037	-0.0037
	(0.0024)***	(0.0024)***	(0.00032)	(0.00030)	(0.0030)	(0.0031)
Migrant × t=-2	-0.0045	-0.0037	-0.00079	-0.00038	-0.0019	-0.00088
	(0.0024)*	(0.0024)	(0.00084)	(0.00084)	(0.0027)	(0.0027)
Migrant × t=-1	-0.019	-0.015	-0.00082	-0.00019	-0.0091	-0.0068
	(0.0033)***	(0.0033)***	(0.00040)**	(0.00036)	(0.0033)***	(0.0033)**
Migrant × t=0	-0.088	-0.079	-0.065	-0.057	-0.13	-0.12
	(0.0055)***	(0.0055)***	(0.0055)***	(0.0055)***	(0.011)***	(0.011)***
Migrant × t=1	-0.097	-0.088	-0.080	-0.074	-0.11	-0.097
	(0.0057)***	(0.0057)***	(0.0052)***	(0.0052)***	(0.0098)***	(0.0097)***
Migrant × t=2	-0.100	-0.091	-0.084	-0.079	-0.096	-0.085
	(0.0058)***	(0.0058)***	(0.0050)***	(0.0050)***	(0.0096)***	(0.0094)***
Migrant × t=3	-0.10	-0.094	-0.087	-0.081	-0.11	-0.099
	(0.0059)***	(0.0059)***	(0.0049)***	(0.0049)***	(0.0095)***	(0.0093)***
Migrant × t=4	-0.098	-0.089	-0.081	-0.075	-0.089	-0.077
	(0.0060)***	(0.0060)***	(0.0049)***	(0.0049)***	(0.0094)***	(0.0093)***
Migrant × t=5	-0.096	-0.086	-0.082	-0.075	-0.083	-0.070
	(0.0061)***	(0.0061)***	(0.0048)***	(0.0049)***	(0.0093)***	(0.0092)***
Observations	344036	344036	344036	344036	285499	285499
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes	No	Yes

Notes: This table presents α_j coefficients from Equation 1. Columns (1), (3), and (5) report coefficients for the baseline regression, where X_{it} controls for baseline year (cohort) and a fourth-order polynomial in age. In Columns (2), (4), and (6), we in addition control for baseline year (cohort) interacted with age, education, tenure, and gender. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Table B9: Characteristics of Coworker Networks in t=0

	(1) All Coworkers	(2) Native Coworkers	(3) Migrant Coworkers
Panel A: Connections			
Network Size	65.9	57.6	12.1
	[148.0]	[135.7]	[30.1]
Distinct Establishments	28.1	25.1	6.22
	[44.6]	[40.0]	[12.2]
Distinct 3-Digit Occ.	9.95	9.24	3.45
	[8.20]	[7.70]	[3.87]
Distinct 2-Digit Ind.	11.4	10.6	3.76
	[9.52]	[9.04]	[4.46]
Distinct Counties	10.1	9.51	3.14
	[13.4]	[12.9]	[4.34]
Distinct Federal States	3.43	3.31	1.63
	[2.80]	[2.74]	[1.28]
Panel B: Coworker Characteristics			
Migrant Share	0.13	0	1
	[0.18]	[0]	[0]
Share Full-Time Employed	0.76	0.76	0.71
	[0.21]	[0.22]	[0.32]
Age	39.4	39.5	37.9
	[5.63]	[5.73]	[8.51]
Daily Wage (EUR)	79.7	80.1	73.3
	[32.8]	[33.0]	[37.7]
Any Minijob in Year	0.12	0.11	0.16
	[0.13]	[0.12]	[0.24]
Commutes (County-Level)	0.48	0.48	0.43
	[0.24]	[0.25]	[0.36]
Panel C: Firm Characteristics			
Size of Establishment	303.0	294.2	370.6
	[771.4]	[689.2]	[1226.4]
Mean Full-Time Wage	94.1	94.1	93.2
	[32.2]	[32.2]	[38.3]
AKM Establishment FE	0.12	0.12	0.11
	[0.15]	[0.15]	[0.21]
Share Migrant Workers	0.12	0.10	0.22
	[0.10]	[0.085]	[0.17]
Share High-Skilled Workers	0.12	0.12	0.12
	[0.13]	[0.13]	[0.16]
Share in Minijob	0.12	0.12	0.14
	[0.12]	[0.12]	[0.18]
Number of Distinct Networks	5527	5494	3948

Notes: This table summarizes the characteristics of displaced workers' coworker networks at the time of layoff. Coworkers are all workers who were employed at the displacement establishment in the same 3-digit occupation at least once in the 3 years before the layoff and have moved to a different firm by t=0. We exclude coworkers who are part of our baseline sample of matched workers. Standard deviations in brackets.

Table B10: The Role of Coworker Networks: Outcomes by Comoving Status

	(1)	(2)	(3)	(4)
	Baseline	Former Coworkers		
		All	Migrants	Natives
Panel A: Log Wages - Comovers				
Migrant	-0.13	-0.13	-0.13	-0.13
	(0.016)***	(0.015)***	(0.016)***	(0.015)***
$\Omega_{pt,all}$		0.0085		
		(0.0047)*		
Migrant × $\Omega_{pt,all}$		0.020		
		(0.0096)**		
$\Omega_{pt,migrant}$			0.0011	
			(0.0027)	
Migrant × $\Omega_{pt,migrant}$			0.018	
			(0.0071)**	
$\Omega_{pt,native}$				0.0018
				(0.0019)
Migrant × $\Omega_{pt,native}$				0.031
				(0.012)***
$X_{i,t=0}$	Yes	Yes	Yes	Yes
Observations	19559	19559	19559	19559
Panel B: Log Wages - Non-Comovers				
Migrant	-0.10	-0.10	-0.10	-0.10
	(0.024)***	(0.024)***	(0.024)***	(0.024)***
$\Omega_{pt,all}$		0.010		
		(0.010)		
Migrant × $\Omega_{pt,all}$		-0.0066		
		(0.010)		
$\Omega_{pt,migrant}$			-0.00093	
			(0.013)	
Migrant × $\Omega_{pt,migrant}$			-0.011	
			(0.021)	
$\Omega_{pt,native}$				0.012
				(0.0098)
Migrant × $\Omega_{pt,native}$				-0.010
				(0.0099)
$X_{i,t=0}$	Yes	Yes	Yes	Yes
Observations	2913	2913	2913	2913

Notes: This table presents γ, α and β coefficients from regression equation 6. We restrict the sample to matched worker pairs for which all 3 network measures are defined. For a given matched pair p, $\Omega_{p,t=0}$ reports our (standardized) proxy of coworker networks. $X_{i,t=0}$ contains baseline year dummies and a fourth-order polynomial in age. Panel A conditions on comovers, i.e. displaced workers that switch to an establishment where at least one of their former coworkers is working at any time post-displacement. Panel B conditions on non-comovers. We cluster standard errors at the baseline establishment level. ***, **, and * correspond to 10, 5, and 1 percent significance levels, respectively.

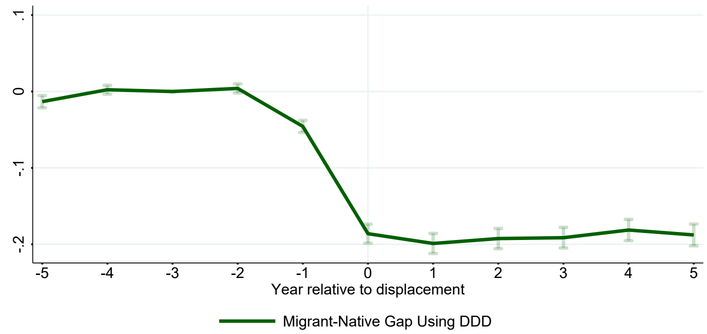
Table B11: Overview of Origin Groups as in Battisti et al. (2022)

	(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 Yugoslavia
		Bosnia and Herzegovina	North Macedonia
		Bulgaria	Macedonia
		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		

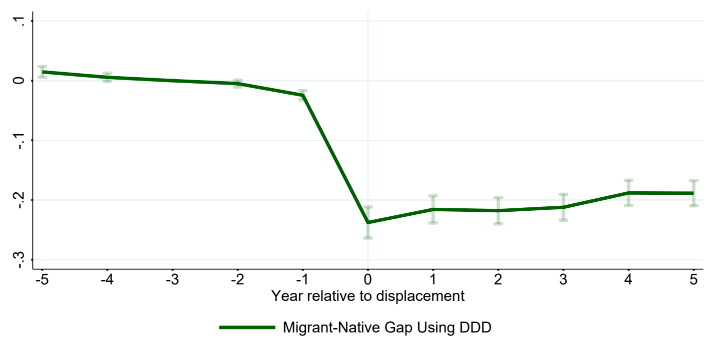
Notes: This table shows how we assign migrants to origin groups following Battisti et al. (2022). The category ”Other” contains origin countries that rarely appear in our data (e.g., the Fiji Islands, the Marshall Islands, and Andorra) and migrants with ”unclear” citizenship.

Figure B1: Migrant-Native Gaps When Comparing Displaced to Non-displaced Workers

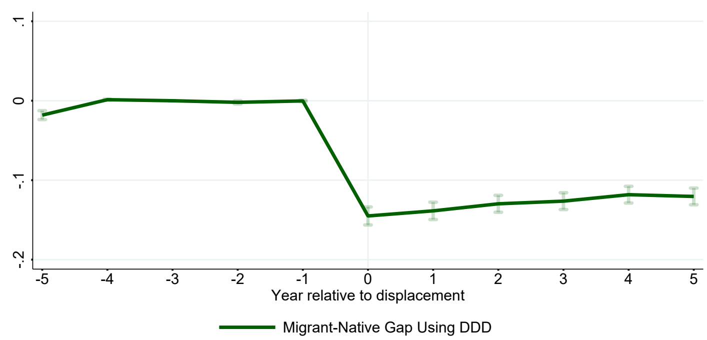
(a) Relative Earnings



(b) Log Wages



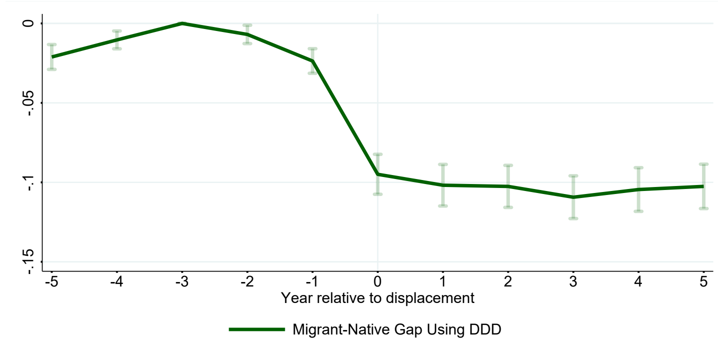
(c) Employment



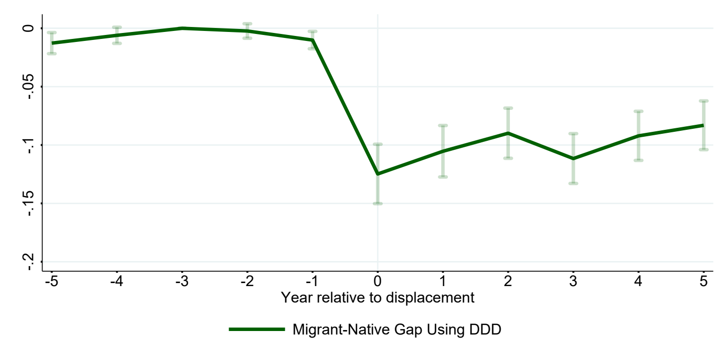
Notes: This figure plots event study coefficients for an alternative analysis where we match displaced migrants and natives *separately* to similar non-displaced workers (*Alternative Matching Approach 1*). We plot triple-differences coefficients α_j from Equation 9, where we interact migrant status with year relative to displacement and a dummy that is 1 for displaced workers. Panel (a) shows earnings relative to earnings in t=-2, Panel (b) shows log wages, and Panel (c) shows employment. See Section A.4 for a description of the matching algorithm and the regression equation. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Table D2 for the corresponding regression output.

Figure B2: Migrant-Native Gaps When Comparing Displaced to Non-displaced Workers and Matching Migrants to Natives

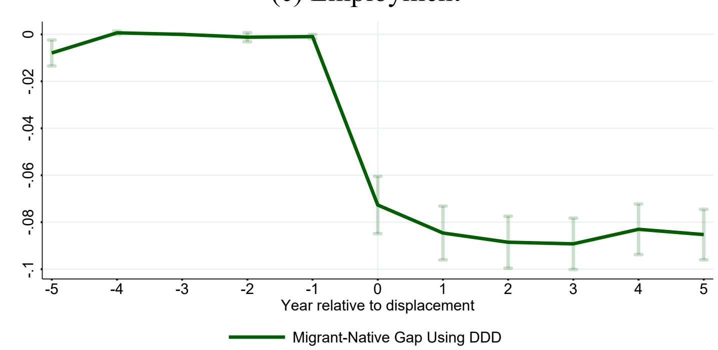
(a) Relative Earnings



(b) Log Wages

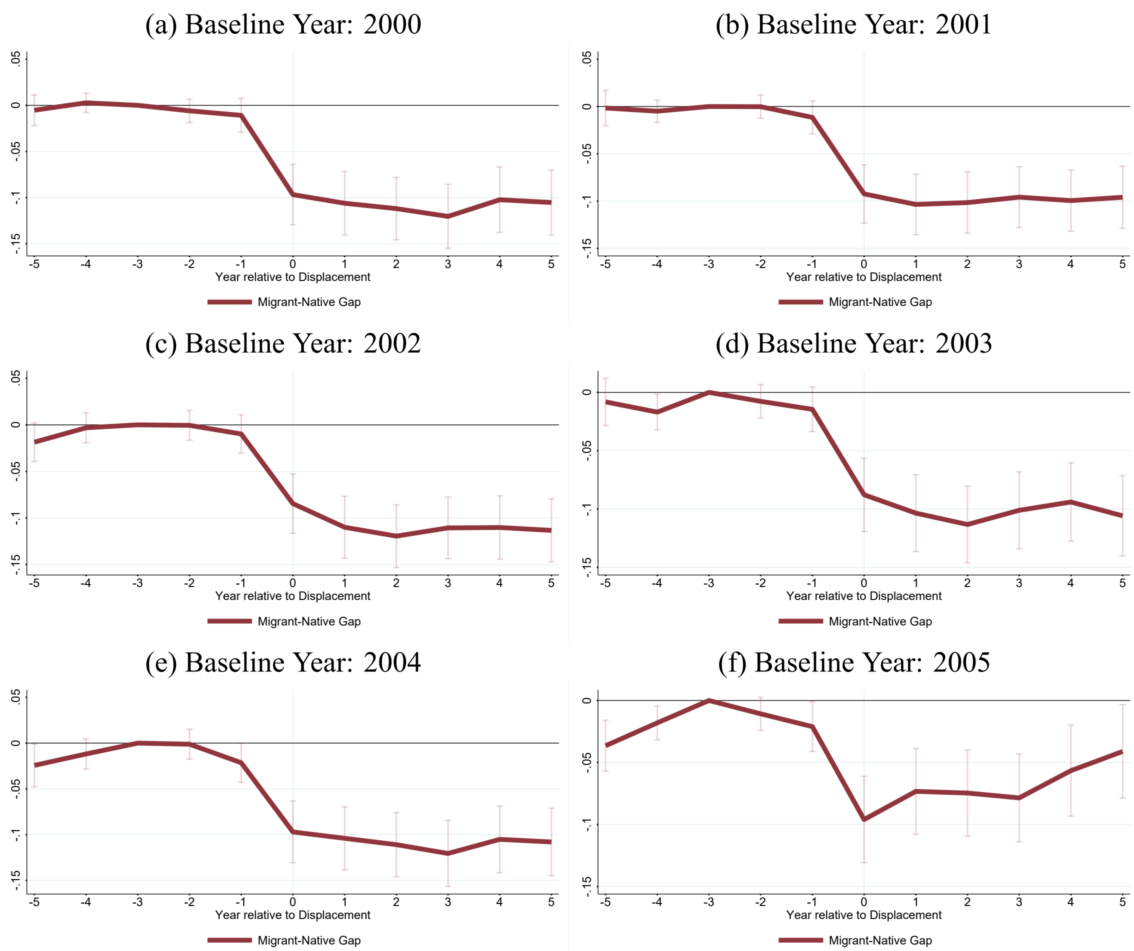


(c) Employment



Notes: This figure plots event study coefficients for an alternative analysis where we match non-displaced workers to our baseline sample of displaced migrants and natives (*Alternative Matching Approach 2*). We plot triple-differences coefficients α_j from Equation 9, where we interact migrant status with year relative to displacement and a dummy that is 1 for displaced workers. Panel (a) shows earnings relative to earnings in t=-2, Panel (b) shows log wages, and Panel (c) shows employment. See Section A.5 for a description of the matching algorithm and the regression equation. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. See Table D3 for the corresponding regression output.

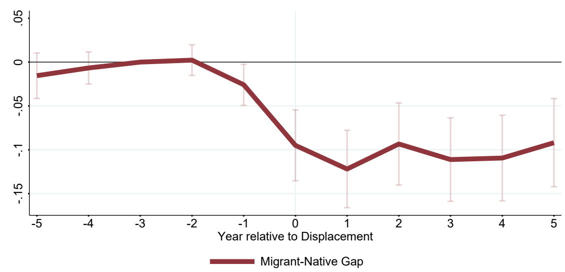
Figure B3: The Migrant-Native Earnings Gap by Baseline Year



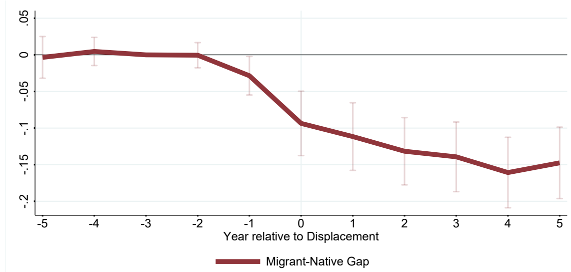
Notes: This figure plots the α_j coefficients from regression equation 1 for earnings relative to earnings in t=-2. In each Panel, we restrict the sample to matched pairs laid-off in a different baseline year. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B4: The Migrant-Native Earnings Gap by Baseline Year, Continued

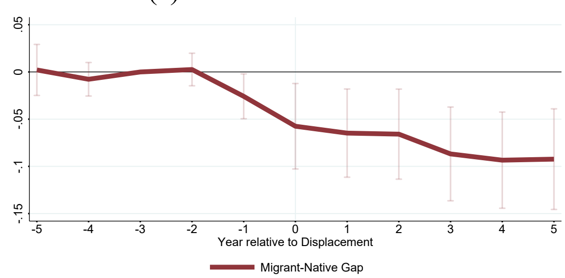
(a) Baseline Year: 2006



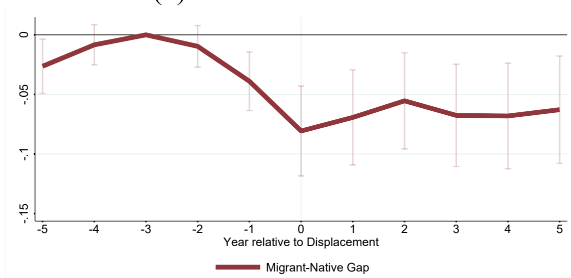
(b) Baseline Year: 2007



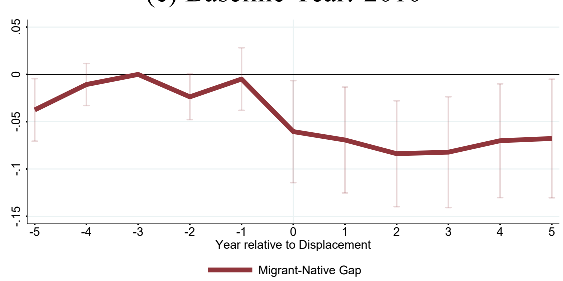
(c) Baseline Year: 2008



(d) Baseline Year: 2009



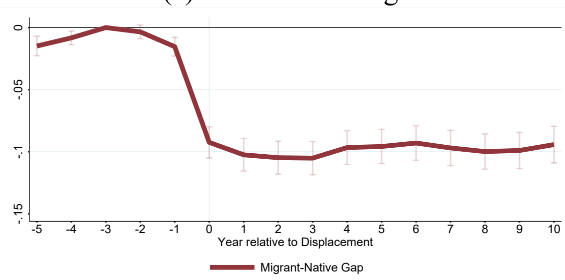
(e) Baseline Year: 2010



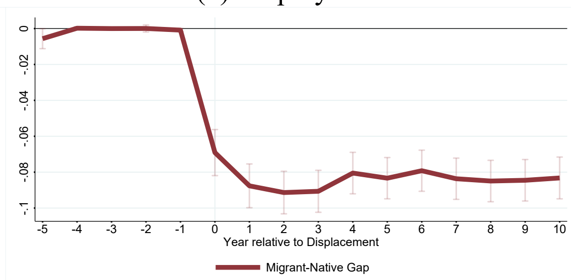
Notes: This figure plots the α_j coefficients from regression equation 1 for earnings relative to earnings in t=-2. In each Panel, we restrict the sample to matched pairs laid-off in a different baseline year. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B5: Main Results - Long-Term

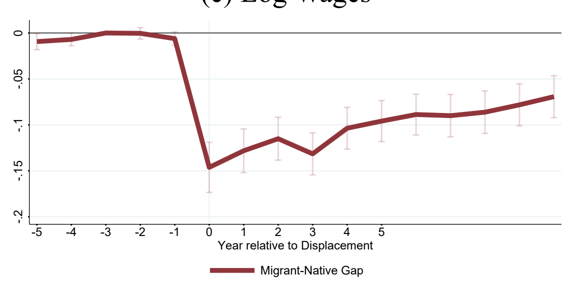
(a) Relative Earnings



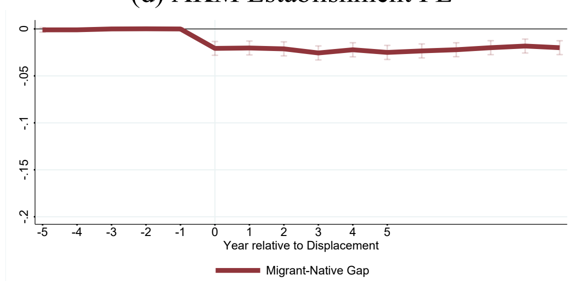
(b) Employment



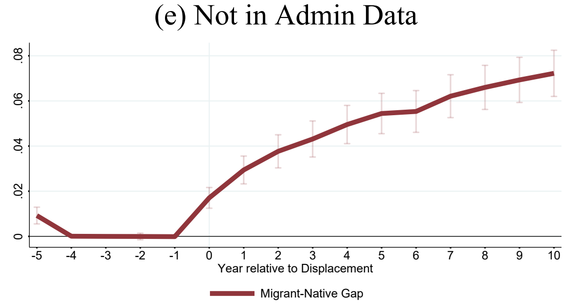
(c) Log Wages



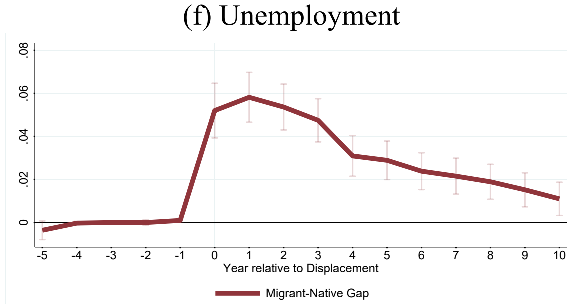
(d) AKM Establishment FE



(e) Not in Admin Data



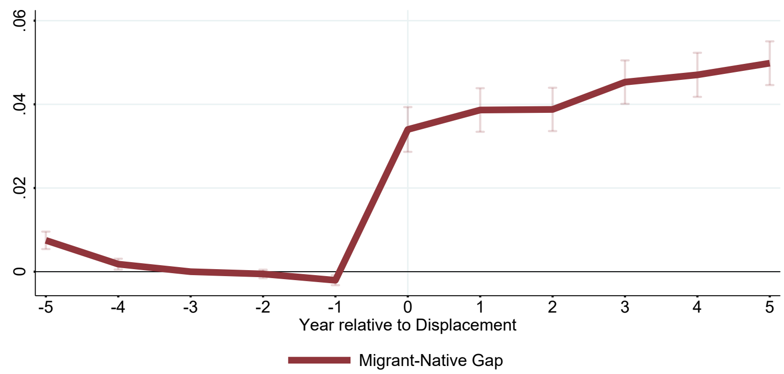
(f) Unemployment



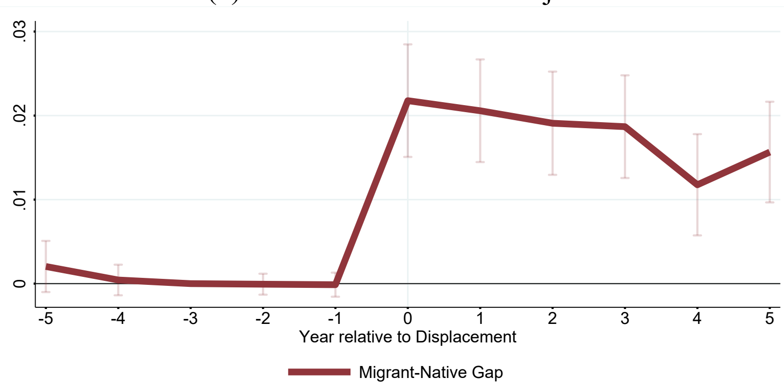
Notes: This figure plots the α_j coefficients from regression equation 1 for our main outcome variables and for all layoffs for which we observe up to 10 years post-event (i.e., all layoffs up to the baseline year 2006). In Panel (a), the outcome variable is earnings relative to earnings in t=-2. In Panel (b), the outcome variable is employment. Panels (c) and (d) plot wages and AKM establishment FE as provided by Lochner et al. (2024), respectively. Panels (e) and (f) plot a dummy that is equal to 1 if there is no admin data record, and unemployment, respectively. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2006, and they are observed from 1997-2016.

Figure B6: Establishment Characteristics

(a) Share of Migrant Workers



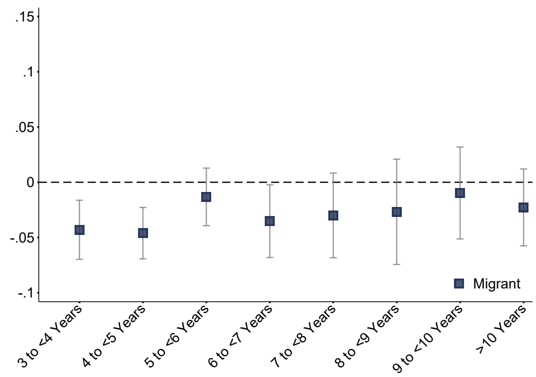
(b) Share of Workers in Minijob



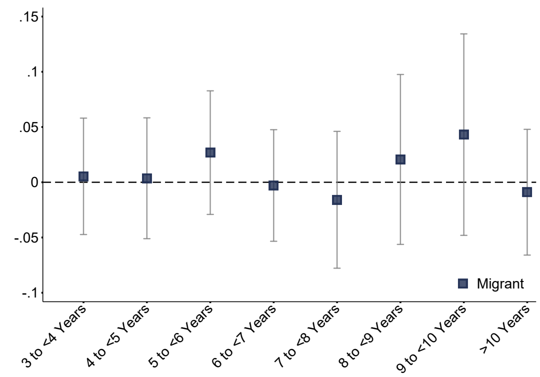
Notes: This figure plots the α_j coefficients from regression equation 1 for establishment sorting. In Panel (a), the outcome variable is the leave-one-out share of migrant workers. In Panel (b), the outcome variable is the leave-one-out share of workers in a minijob. Minijobs, or marginal employment, are a specific type of job in the German labor market. They are exempt from social-security contributions, allow a maximum of 10 hours work per week, and a maximum of EUR 538 total monthly income (as of 2024). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B7: The Migrant-Native Wage Gap in First Full-Time Position after Displacement

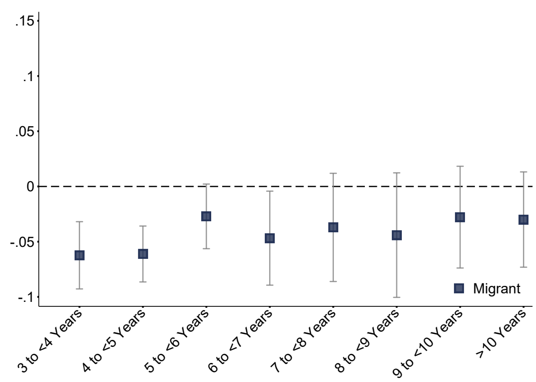
(a) Full-time Log Wages by Tenure at Baseline, All Countries



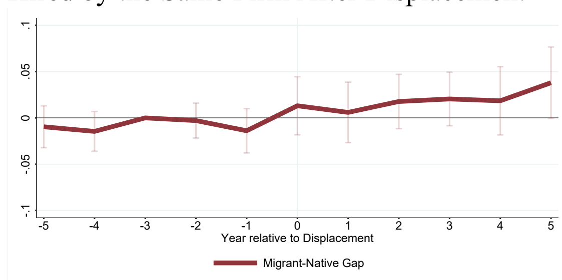
(b) Full-time Log Wages by Tenure at Baseline, Western Countries



(c) Full-time Log Wages by Tenure at Baseline, All Other Countries



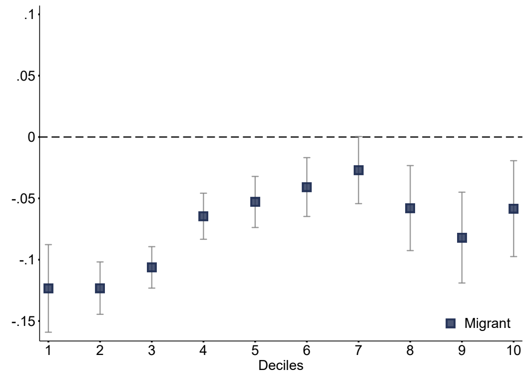
(d) Full-Time Log Wages for Matched Pairs Hired by the Same Firm After Displacement



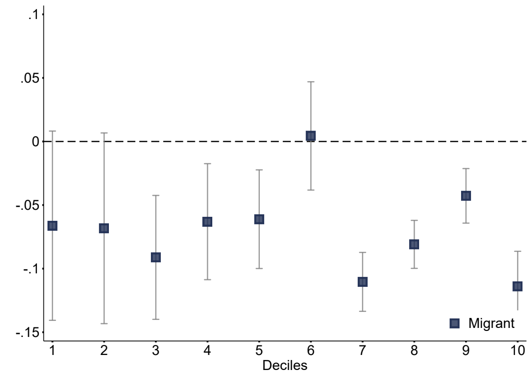
Notes: Panels (a)-(c) plot δ coefficients from a variation of Equation 4. Panel (a) plots the migrant-native gap in full-time log wages by tenure at baseline (t=-1). Panel (b) and (c) plot the migrant-native full-time log wage gap by tenure for migrants from Western countries vs. migrants from all other countries. See Table B11, row 2, for a list of all Western countries. Panel (d) plots the α_j coefficients from regression equation 1 for full-time log wages of migrant-native pairs who end up in the same post-layoff establishment. We only take into account a workers' first post-layoff employer. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the displacement establishment (Panels (a)-(c)) or individual (Panel (d)) level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B8: Migrant-Native Gaps by Pre-Displacement AKM Worker FE and Origin Country Net Income - Additional Outcomes

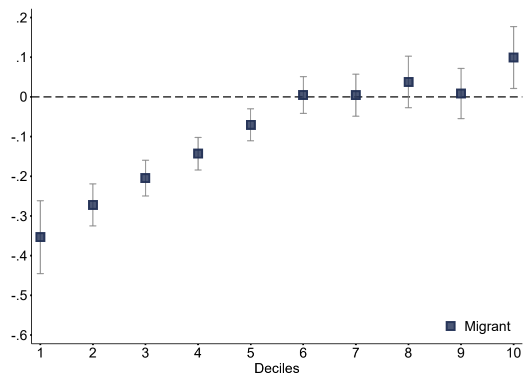
(a) Employment - AKM Worker FE



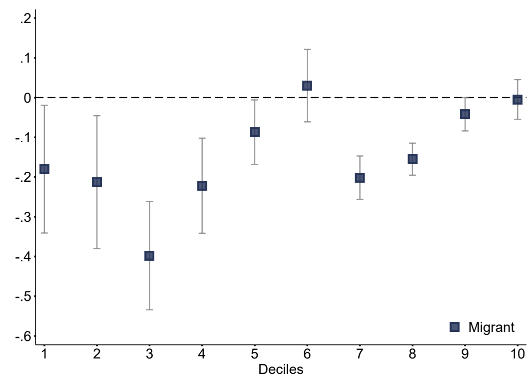
(b) Employment - Origin Country Net Income



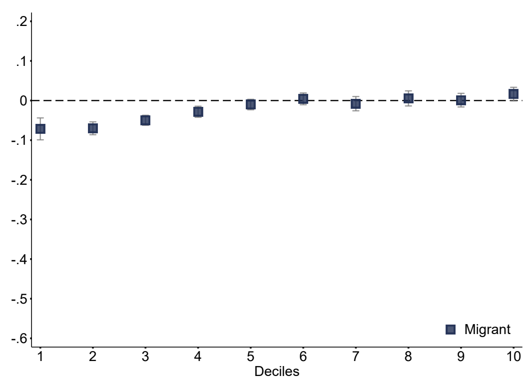
(c) Log Daily Wages - AKM Worker FE



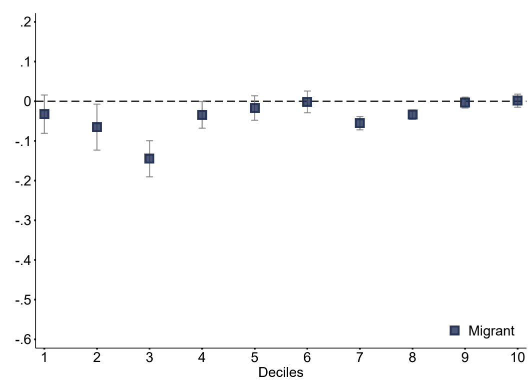
(d) Log Daily Wages - Origin Country Net Income



(e) AKM Establishment FE - AKM Worker FE

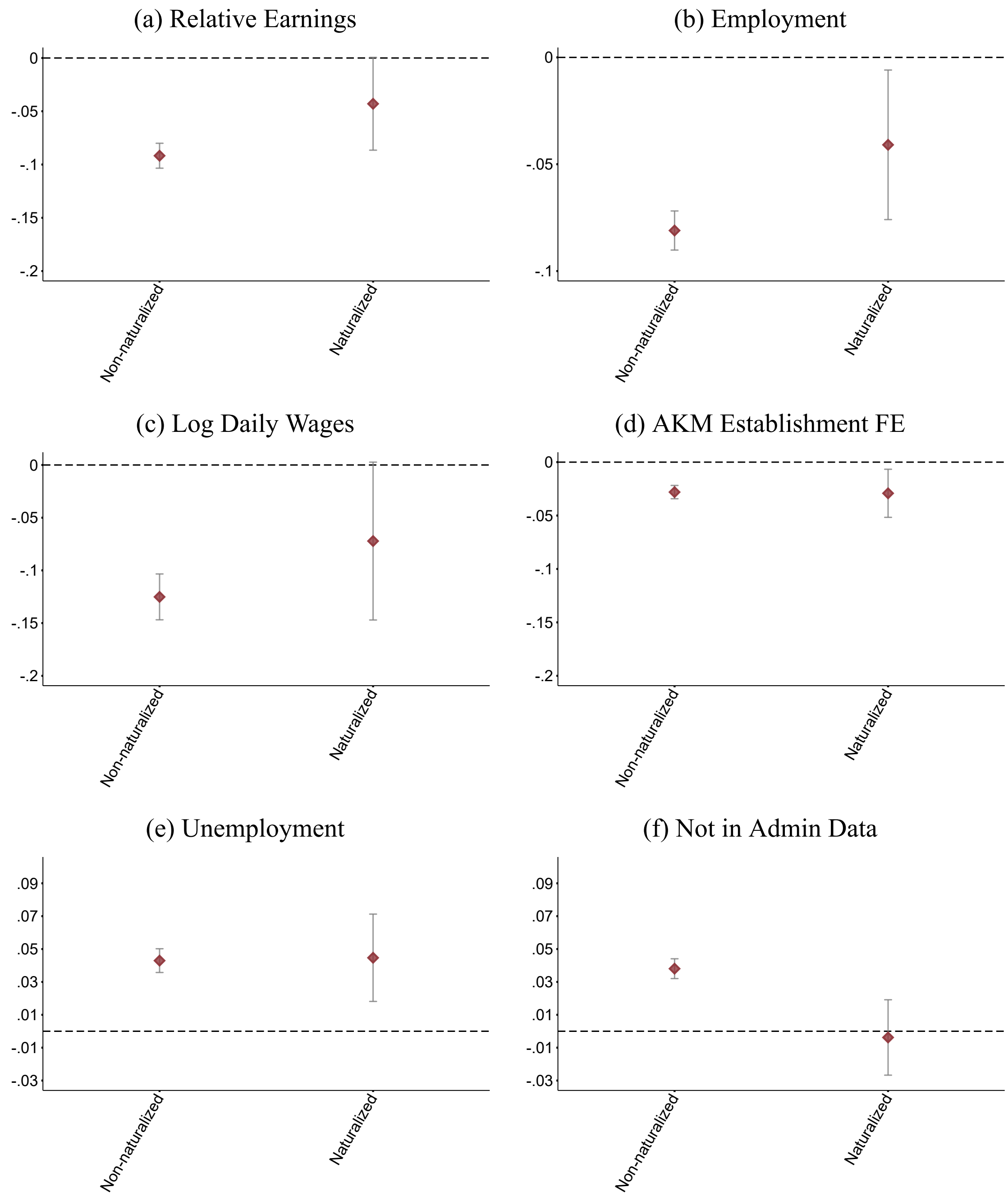


(f) AKM Establishment FE - Origin Country Net Income



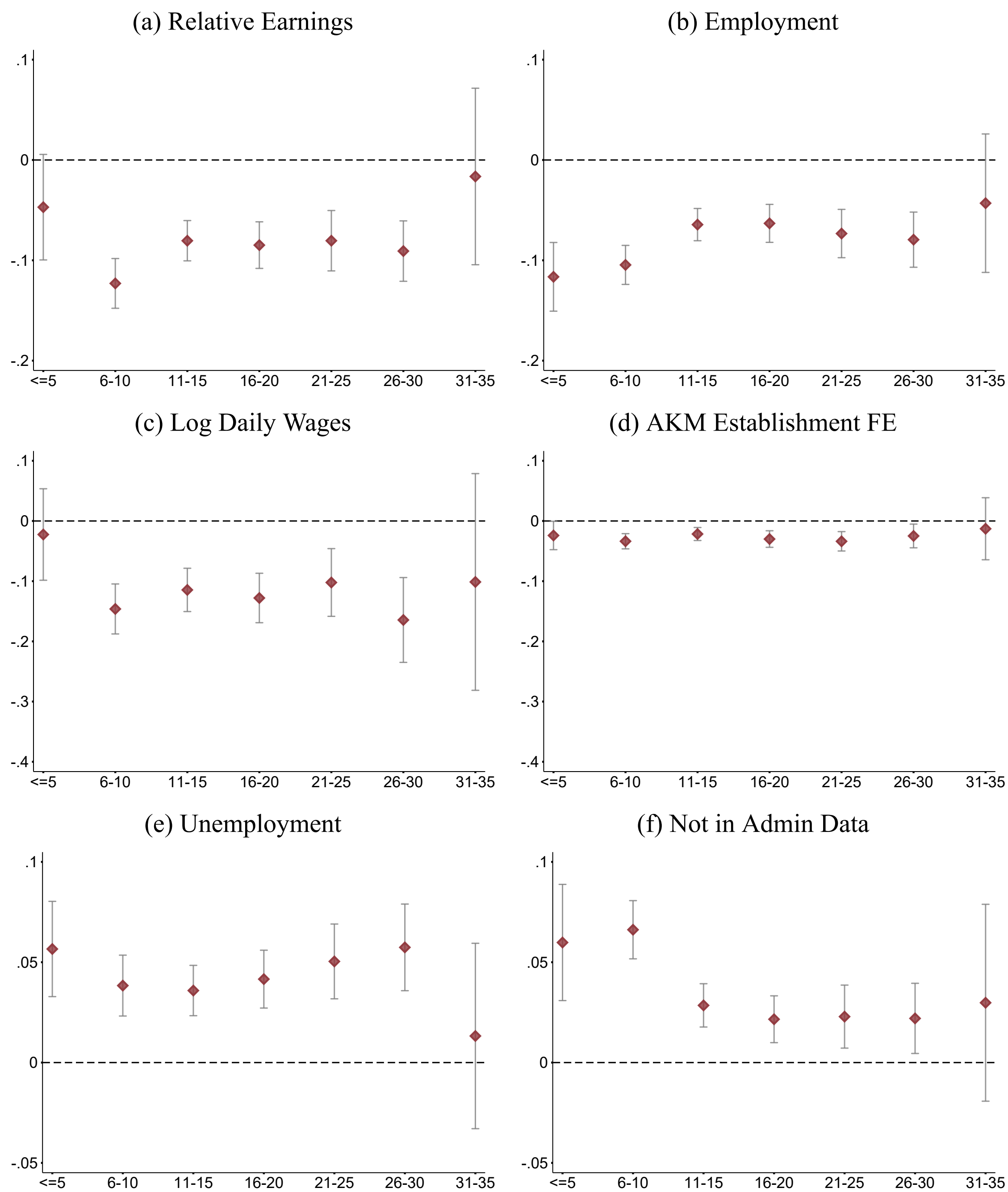
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by migrants' decile of pre-displacement AKM worker FE (Panels a, c, e) and origin country net income (Panels b, d, f), all measured in t=-1. We use the AKM worker FE measure provided by Lochner et al. (2024) and we collect data on "adjusted net income" by country from the World Bank's World Development Indicators (World Bank, 2024). Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. The regressions for Panels (b), (d), and (f) control for pre-displacement worker FE. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B9: Migrant-Native Gap by Naturalization



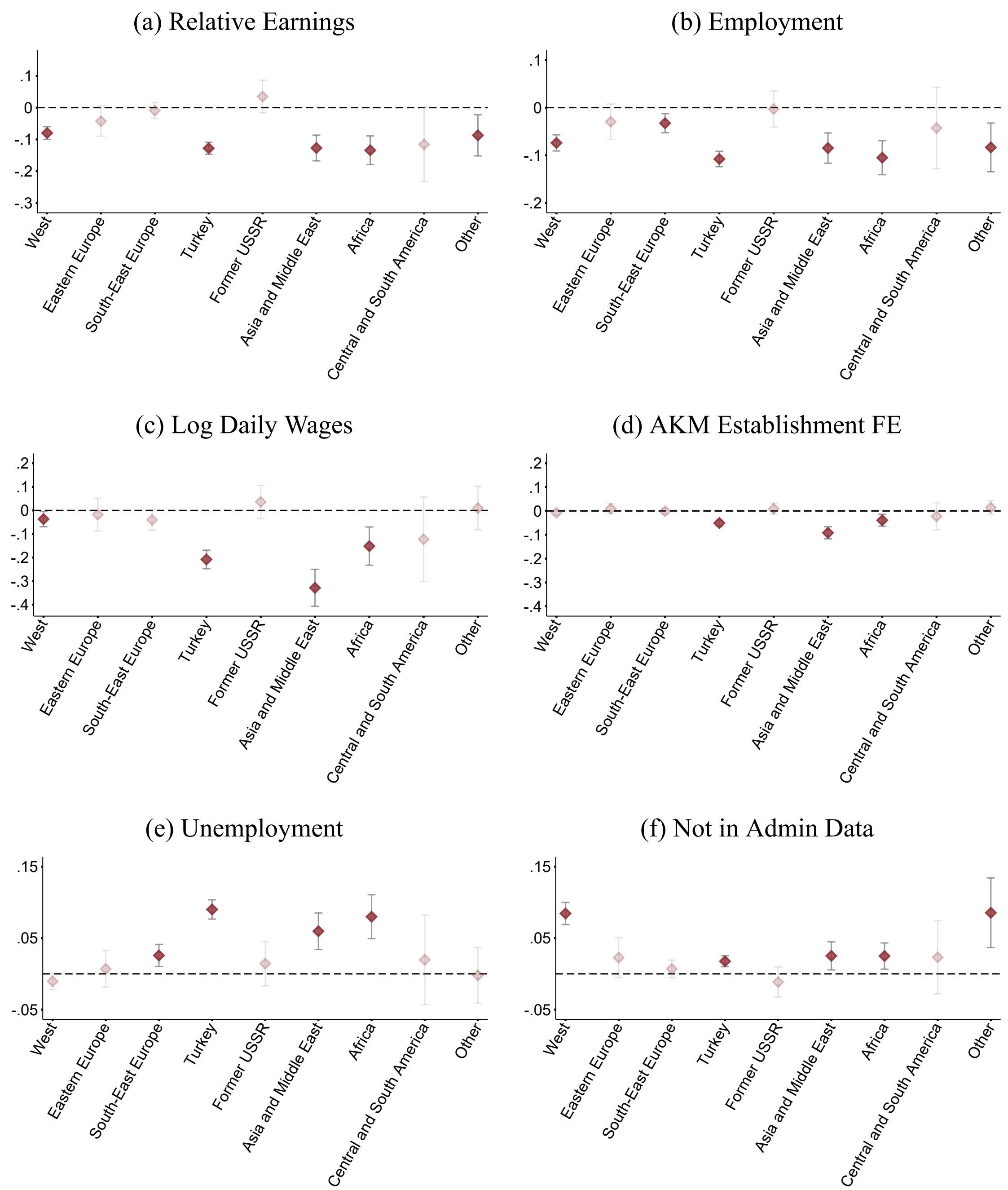
Notes: This figure shows how the migrant-native gap in costs of job displacement differs depending on whether individuals have acquired German citizenship by the time of layoff. We classify migrants as naturalized if they had non-German citizenship in their first social-security record, and German citizenship in the year before the layoff. Each Panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on the naturalization dummy. All regressions control for origin group and AKM worker FE at baseline. Panel (a) reports earnings relative to earnings in t=-2, Panel (b) reports log wages, Panel (c) reports unemployment, and Panel (d) reports a dummy that is equal to 1 whenever a worker does not have a social-security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B10: Migrant-Native Gap by Years in Admin Data



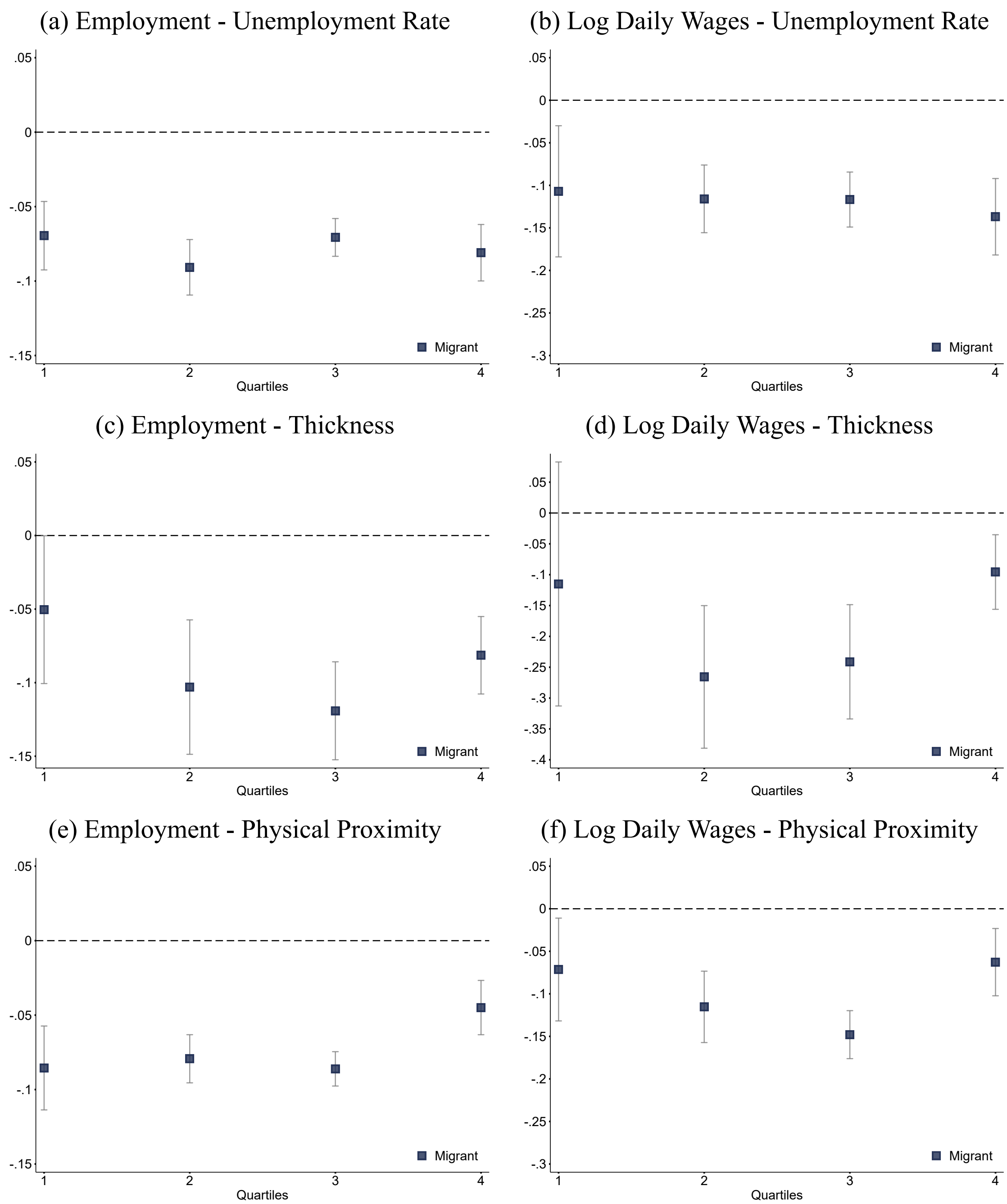
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by the number of years since the first record in the German admin data, measured at t=-1. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 7 categories. All regressions control for origin group and AKM worker FE at baseline. Panel (a) reports earnings relative to earnings in t=-2, Panel (b) reports log wages, Panel (c) reports unemployment, and Panel (d) reports a dummy that is equal to 1 whenever a worker does not have a social-security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B11: Migrant-Native Gap by Origin Group



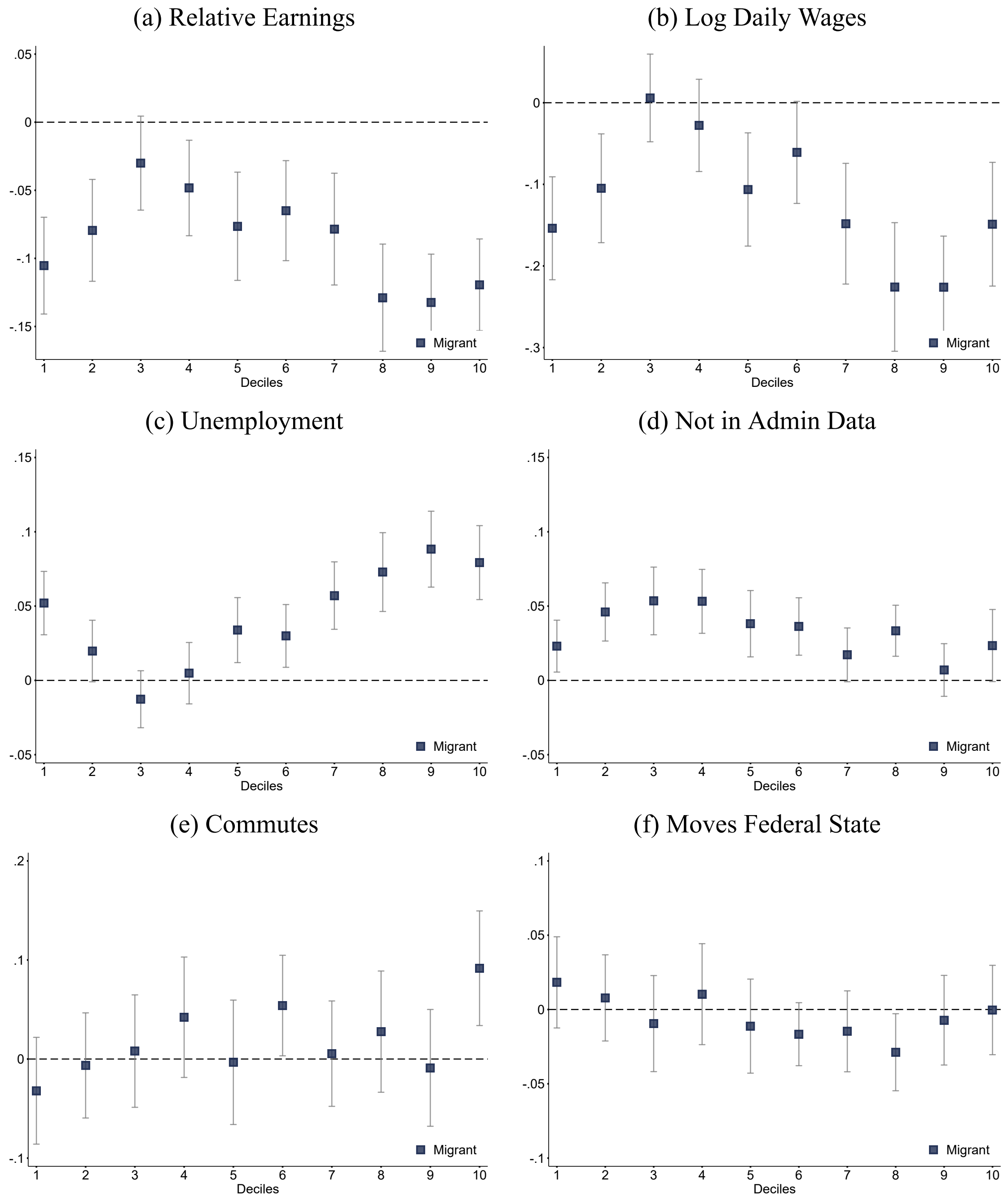
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by origin group measured in the first admin data record, as defined by Battisti et al. (2022). See Table B11 for details. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 9 groups. All regressions control for AKM worker FE at baseline. Panel (a) reports earnings relative to earnings in t=-2, Panel (b) reports log wages, Panel (c) reports unemployment, and Panel (d) reports a dummy that is equal to 1 whenever a worker does not have a social-security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B12: Migrant-Native Gaps by Pre-Displacement Local Labor Market and Occupation Characteristics



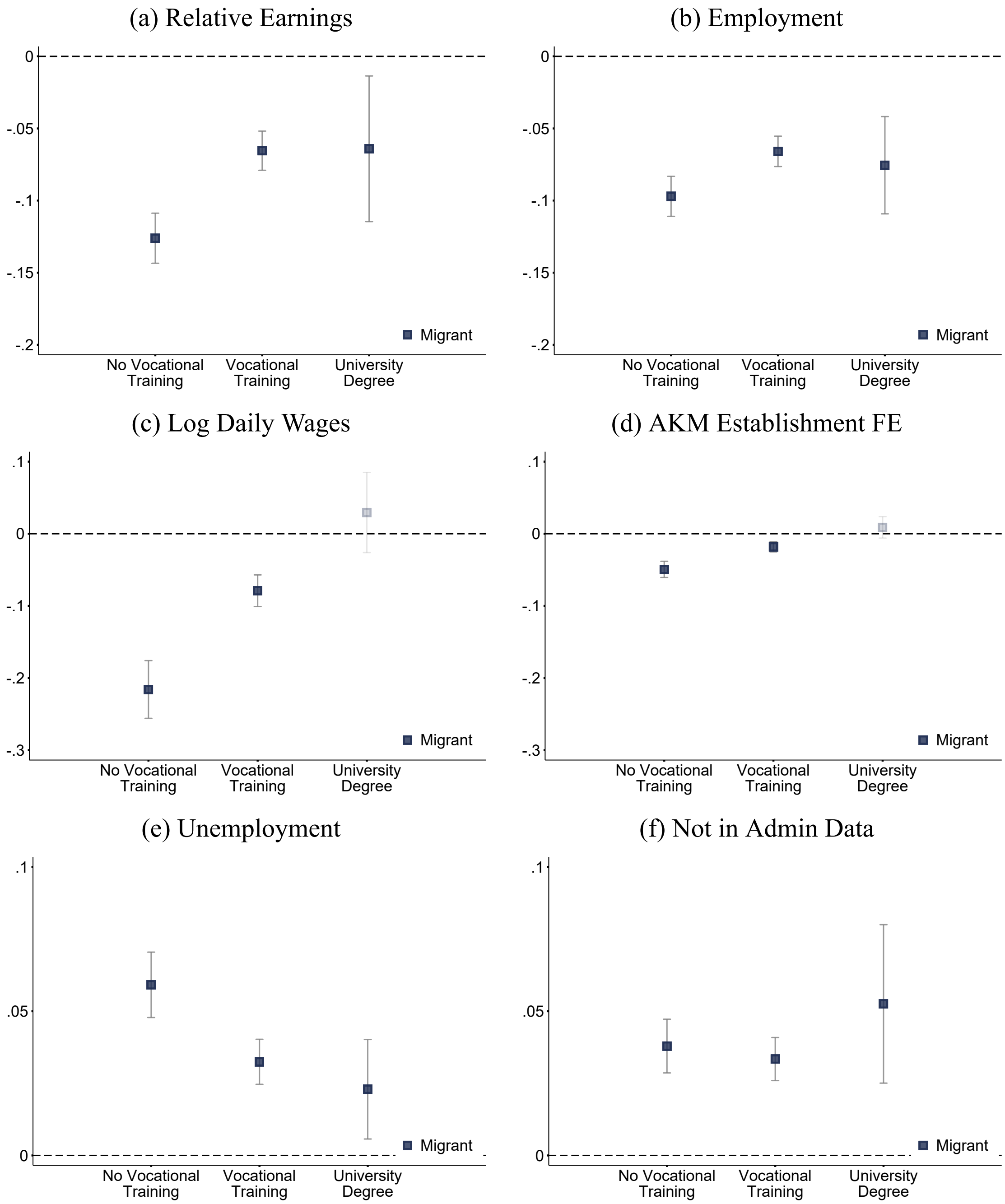
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by quartile of county unemployment rate (Panels a and b), local labor market thickness (Panels c and d), and the 2-digit occupation's physical proximity indicator (Panels e and f), all measured in t=-1. We follow Jäger et al. (2024) and define labor market thickness as the share of employed workers in a given 3-digit occupation, year, and commuting zone by the national share of employed workers in a given 3-digit occupation and year. We base our measure of an occupation's "physical proximity" on an indicator used by Mongey et al. (2021) based on O*NET data, creating a cross-walk to the German occupational data. See Appendix A.3 for details. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 4 quartiles. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B13: Migrant-Native Gap by County Share of Same-Nationality Working Age Population



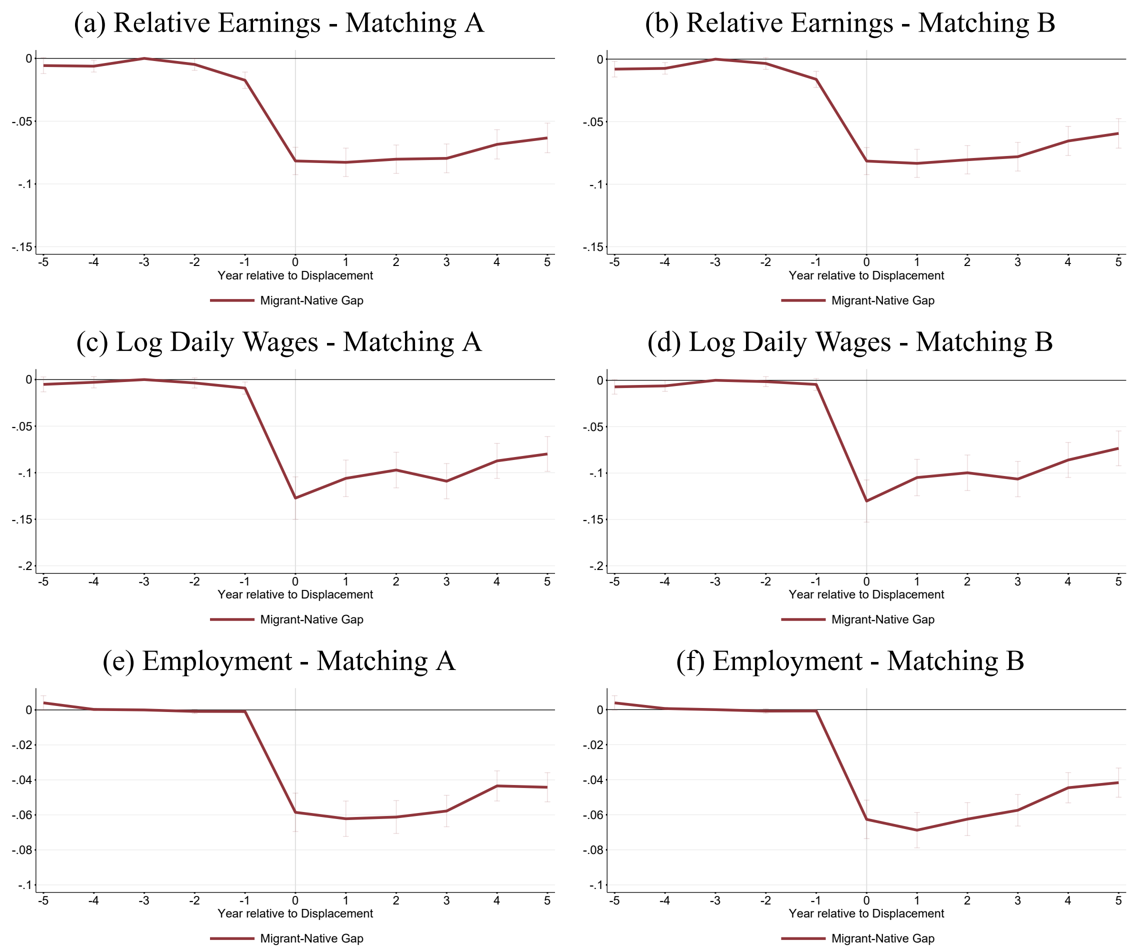
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by deciles of the county share of same-nationality working age population, measured at t=-1. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. Panel (a) reports earnings relative to earnings in t=-2, Panel (b) reports log wages, Panel (c) reports unemployment, Panel (d) reports a dummy that is equal to 1 whenever a worker does not have a social-security record, Panel (e) reports a dummy indicating whether a worker is commuting across county, and Panel (f) reports whether a worker moved federal state relative to the baseline year. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B14: Migrant-Native Gap by Skill Group



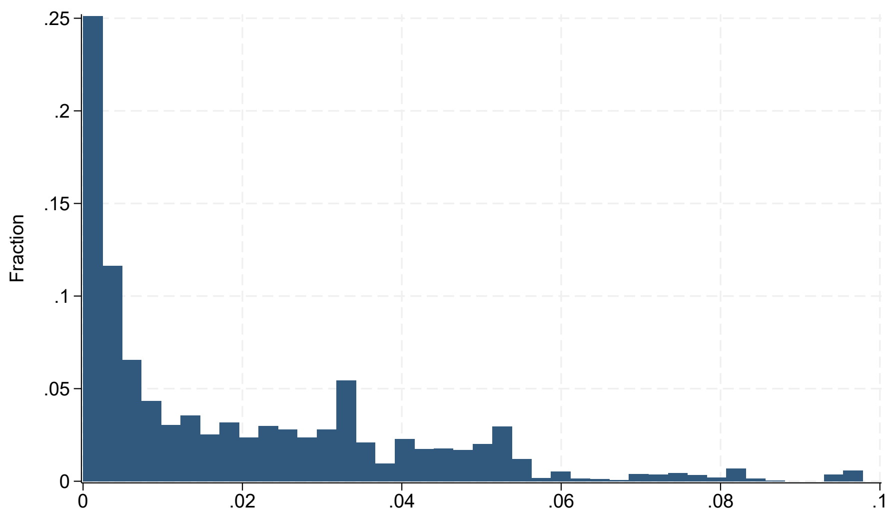
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by skill group. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 3 skill groups. Panel (a) reports earnings relative to earnings in t=-2, Panel (b) reports employment, Panel (c) reports log of daily wages, Panel (d) reports establishment AKM fixed effects, Panel (e) reports unemployment rates, and Panel (f) reports a dummy that is equal to 1 whenever a worker does not have a social-security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B15: Main Labor Market Outcomes with Matching on Different Education Variables



Notes: This figure presents the main migrant–native gaps in the costs of job displacement under two alternative matching specifications. Each figure plots the α_j coefficients from regression equation 1 for the migrant-native earnings, wage, and employment gap. *Matching A* deviates from the baseline matching algorithm by replacing missing values in the years-of-education variable with the lowest category (10 years of education). *Matching B* deviates from the baseline by substituting the years-of-education variable with the skill requirement measure (defined as the last digit of the 5-digit occupational code). Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

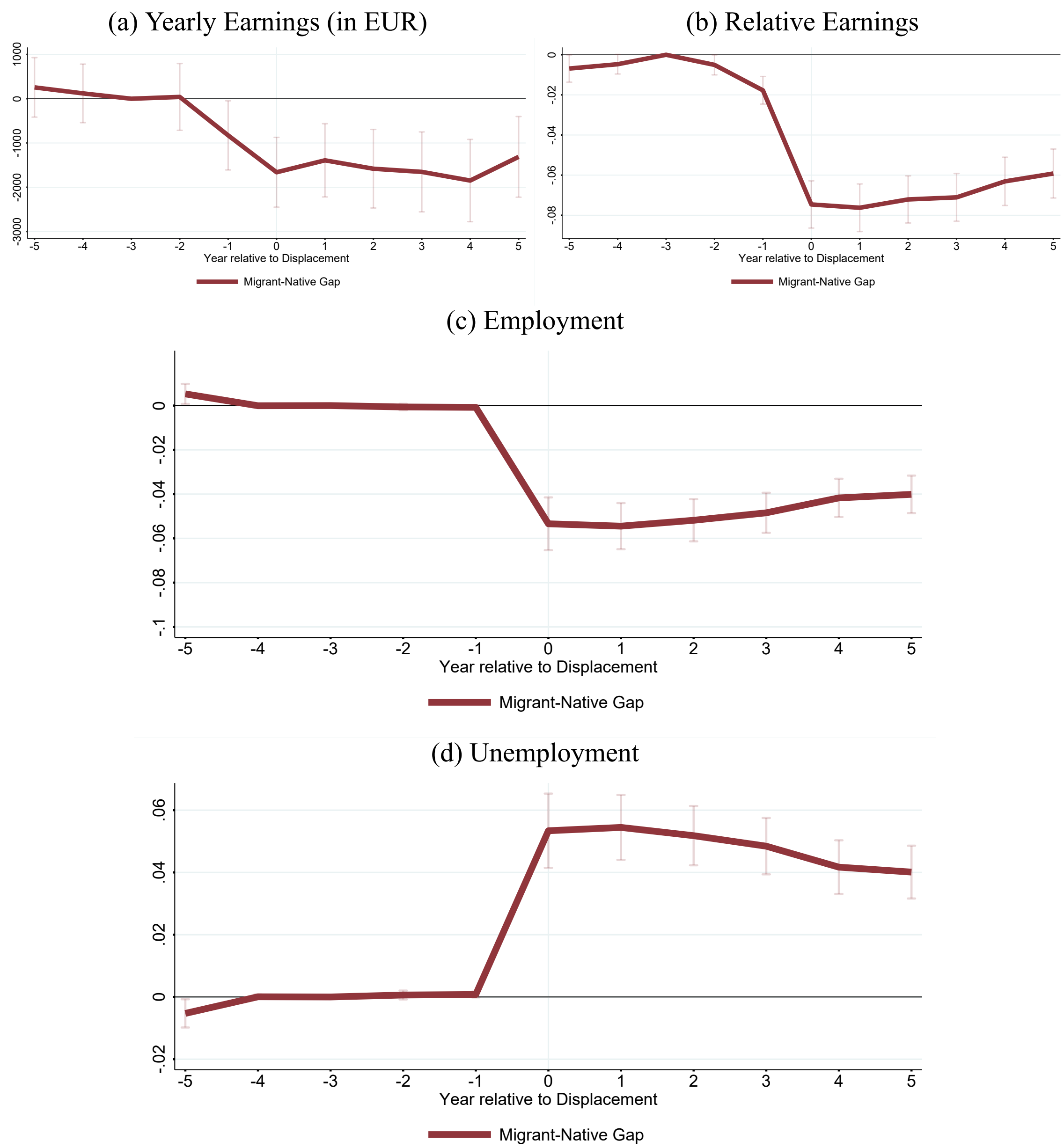
Figure B16: Distribution Share Same-Nationality Working Age Population in County in t=-1



Notes: This histogram shows the distribution of the share of same-nationality working-age population in a county at $t = -1$ for our sample of displaced migrants. Workers in our sample are displaced in the period 2001-2011, and they are observed from 1996 to 2017. Data source: Destatis (2019).

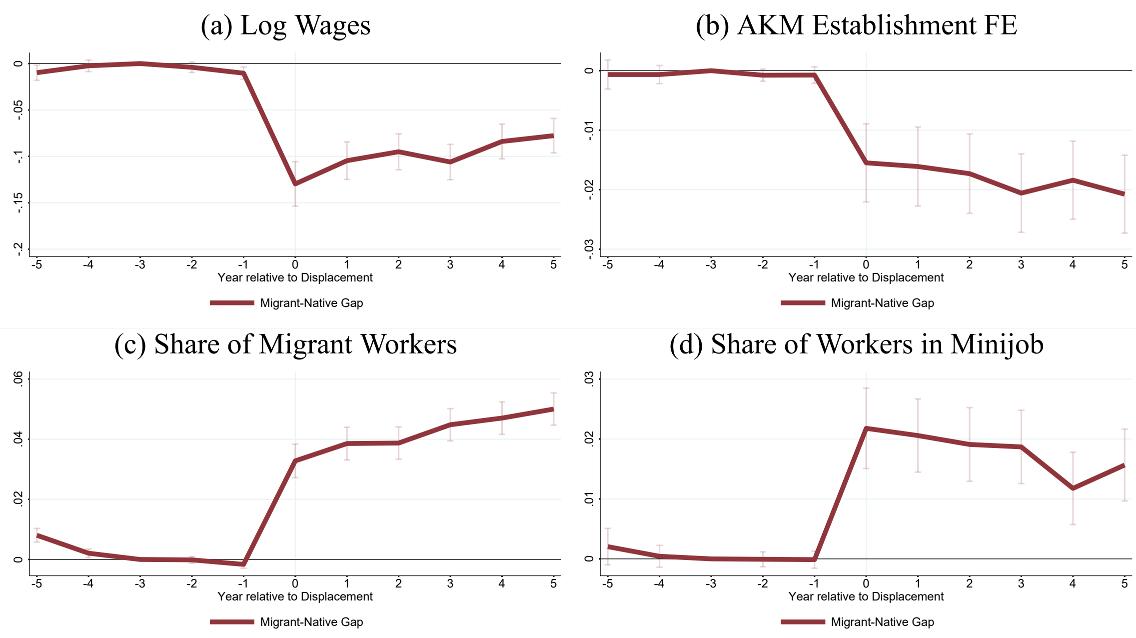
C Replication of Main Results for a Balanced Panel of Workers

Figure C1: The Migrant-Native Earnings and Employment Gap - Balanced Panel



Notes: This figure plots the α_j coefficients from regression equation 1 for total yearly earnings (Panel a), earnings relative to earnings in t=-2 (Panel b), employment (Panel c), and unemployment (Panel d). The sample is restricted to individuals with a record in the German admin data from t=-5 through t=5. Unemployment is defined as being a UI benefit recipient or a training program participant. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure C2: The Migrant-Native Gap in Wages and Establishment Sorting - Balanced Panel



Notes: This figure plots the α_j coefficients from regression equation 1 for the migrant-native wage gap and for establishment sorting. The sample is restricted to individuals with a record in the German admin data from t=-5 through t=5. In Panel (a), the outcome variable is log wages. In Panel (b), the outcome variable is the AKM establishment fixed effect, using the dataset provided by Lochner et al. (2024). In Panel (c), the outcome variable is the leave-one-out share of migrant workers. In Panel (d), the outcome variable is the leave-one-out share of workers in a minijob. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

D Regression Tables for Main Figures

Table D1: Regression Output for Baseline Analysis

	(1) Earnings Rel. To T-2	(2) Earnings (EUR)	(3) Log Wages	(4) AKM Estab. FE	(5) Employ-ment	(6) Unemploy-ment	(7) Not In Admin Data
Migrant × -5	-0.015	-457.6	-0.0082	-0.00044	-0.0050	-0.0045	0.0095
	(0.0034)***	(326.8)	(0.0040)**	(0.0011)	(0.0025)**	(0.0020)**	(0.0015)***
Migrant × -4	-0.0073	-323.1	-0.0037	-0.00040	0.00042	-0.00041	-0.0000055
	(0.0024)***	(324.4)	(0.0030)	(0.00069)	(0.00032)	(0.00033)	(0.000026)
Migrant × -2	-0.0045	-102.0	-0.0019	-0.00050	-0.00079	0.00051	0.00028
	(0.0024)*	(362.7)	(0.0027)	(0.00045)	(0.00084)	(0.00063)	(0.00056)
Migrant × -1	-0.019	-948.4	-0.0091	-0.00065	-0.00082	0.00077	0.000050
	(0.0033)***	(369.6)**	(0.0033)***	(0.00062)	(0.00040)**	(0.00040)*	(0.000051)
Migrant × 0	-0.088	-2482.6	-0.13	-0.017	-0.065	0.050	0.014
	(0.0055)***	(387.3)***	(0.011)***	(0.0032)***	(0.0055)***	(0.0054)***	(0.0020)***
Migrant × 1	-0.097	-2487.0	-0.11	-0.016	-0.080	0.051	0.029
	(0.0057)***	(409.1)***	(0.0098)***	(0.0032)***	(0.0052)***	(0.0049)***	(0.0027)***
Migrant × 2	-0.100	-2785.6	-0.096	-0.017	-0.084	0.046	0.038
	(0.0058)***	(437.5)***	(0.0096)***	(0.0032)***	(0.0050)***	(0.0044)***	(0.0032)***
Migrant × 3	-0.10	-3160.7	-0.11	-0.021	-0.087	0.042	0.045
	(0.0059)***	(441.8)***	(0.0095)***	(0.0032)***	(0.0049)***	(0.0041)***	(0.0034)***
Migrant × 4	-0.098	-3280.9	-0.089	-0.019	-0.081	0.029	0.051
	(0.0060)***	(463.4)***	(0.0094)***	(0.0032)***	(0.0049)***	(0.0039)***	(0.0036)***
Migrant × 5	-0.096	-2588.2	-0.083	-0.021	-0.082	0.026	0.056
	(0.0061)***	(473.3)***	(0.0093)***	(0.0032)***	(0.0048)***	(0.0037)***	(0.0038)***
Observations	344036	344036	285499	277169	344036	344036	344036
Dep. Var Mean Natives $t-1$	-0.034	35675.7	4.49	0.22	1	0	0
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Since Disp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the α_j coefficients from regression equation 1 for the main outcome variables, for the baseline analysis. "Not in Admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. Cohort FE includes cohort interacted with time since displacement controls. All regressions moreover control for a fourth-order polynomial in age. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. See Figures 1, 2, and 3 for the corresponding event study graphs.

Table D2: Regression Output for Triple-Diff Analysis with Standard Mass Layoff Approach

	(1) Earnings Rel. To T-2	(2) Earnings (EUR)	(3) Log Wages	(4) AKM Estab. FE	(5) Employ-ment	(6) Unemploy-ment	(7) Not In Admin Data
Migrant × Disp. × -5	-0.013	1134.9	0.015	-0.0051	-0.018	0.0011	0.017
	(0.0040)***	(348.5)***	(0.0045)***	(0.0013)***	(0.0028)***	(0.0022)	(0.0019)***
Migrant × Disp. × -4	0.0023	911.7	0.0056	-0.0036	0.0013	-0.00059	0.000014
	(0.0029)	(323.5)***	(0.0034)	(0.00079)***	(0.00039)***	(0.00056)	(0.000020)
Migrant × Disp. × -2	0.0040	-600.0	-0.0049	0.00066	-0.0020	0.00079	0.0011
	(0.0030)	(351.7)*	(0.0028)*	(0.00053)	(0.0010)*	(0.00077)	(0.00072)
Migrant × Disp. × -1	-0.046	-1845.7	-0.024	0.0037	-0.00025	0.00027	-0.000077
	(0.0039)***	(378.2)***	(0.0038)***	(0.00068)***	(0.00043)	(0.00045)	(0.000032)**
Migrant × Disp. × 0	-0.19	-3557.0	-0.24	-0.048	-0.14	0.14	0.0096
	(0.0062)***	(396.5)***	(0.013)***	(0.0037)***	(0.0057)***	(0.0056)***	(0.0023)***
Migrant × Disp. × 1	-0.20	-4114.8	-0.22	-0.055	-0.14	0.11	0.031
	(0.0066)***	(414.9)***	(0.011)***	(0.0037)***	(0.0055)***	(0.0051)***	(0.0032)***
Migrant × Disp. × 2	-0.19	-4210.7	-0.22	-0.060	-0.13	0.091	0.040
	(0.0066)***	(426.2)***	(0.011)***	(0.0038)***	(0.0053)***	(0.0047)***	(0.0037)***
Migrant × Disp. × 3	-0.19	-4598.7	-0.21	-0.062	-0.13	0.077	0.049
	(0.0068)***	(460.6)***	(0.011)***	(0.0038)***	(0.0053)***	(0.0044)***	(0.0039)***
Migrant × Disp. × 4	-0.18	-4402.7	-0.19	-0.050	-0.12	0.061	0.057
	(0.0070)***	(484.0)***	(0.011)***	(0.0037)***	(0.0053)***	(0.0041)***	(0.0042)***
Migrant × Disp. × 5	-0.19	-4342.3	-0.19	-0.051	-0.12	0.060	0.061
	(0.0071)***	(523.0)***	(0.011)***	(0.0036)***	(0.0052)***	(0.0040)***	(0.0043)***
Observations	2326544	2326544	2103871	2061516	2326544	2326544	2326544
Dep. Var Mean Natives $t-1$	0.041	45453.2	4.62	0.16	1	0	0
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Since Disp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the α_j coefficients from regression equation 9 for the main outcome variables under *alternative matching approach 1*. "Not in Admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. Cohort FE includes Cohort interacted with time since displacement controls. All regressions moreover control for a fourth-order polynomial in age. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. See Figure B1 for the corresponding event study graphs.

Table D3: Regression Output for Triple-Diff Analysis with Baseline Sample

	(1) Earnings Rel. To T-2	(2) Earnings (EUR)	(3) Log Wages	(4) AKM Estab. FE	(5) Employ- ment	(6) Unemploy- ment	(7) Not In Admin Data
Migrant × Disp. × -5	-0.021	-952.7	-0.013	0.00096	-0.0079	-0.0024	0.010
	(0.0039)***	(373.1)**	(0.0045)***	(0.0012)	(0.0028)***	(0.0022)	(0.0017)***
Migrant × Disp. × -4	-0.010	-580.8	-0.0060	-0.00018	0.00066	-0.00066	0.0000017
	(0.0028)***	(374.9)	(0.0035)*	(0.00078)	(0.00037)*	(0.00037)*	(0.000018)
Migrant × Disp. × -2	-0.0069	-183.2	-0.0023	-0.00072	-0.0012	0.00071	0.00048
	(0.0029)**	(422.8)	(0.0031)	(0.00051)	(0.00097)	(0.00071)	(0.00066)
Migrant × Disp. × -1	-0.024	-1160.8	-0.010	0.000094	-0.00094	0.00089	0.000049
	(0.0039)***	(433.9)***	(0.0037)***	(0.00070)	(0.00045)**	(0.00044)**	(0.000038)
Migrant × Disp. × 0	-0.095	-3059.9	-0.12	-0.016	-0.073	0.058	0.015
	(0.0063)***	(448.7)***	(0.013)***	(0.0036)***	(0.0061)***	(0.0060)***	(0.0022)***
Migrant × Disp. × 1	-0.10	-3034.3	-0.11	-0.016	-0.085	0.052	0.032
	(0.0065)***	(466.9)***	(0.011)***	(0.0035)***	(0.0057)***	(0.0054)***	(0.0030)***
Migrant × Disp. × 2	-0.10	-3169.4	-0.090	-0.018	-0.089	0.047	0.041
	(0.0066)***	(510.1)***	(0.011)***	(0.0036)***	(0.0055)***	(0.0049)***	(0.0036)***
Migrant × Disp. × 3	-0.11	-3634.4	-0.11	-0.024	-0.089	0.042	0.047
	(0.0067)***	(522.2)***	(0.011)***	(0.0036)***	(0.0055)***	(0.0046)***	(0.0039)***
Migrant × Disp. × 4	-0.10	-3735.2	-0.092	-0.020	-0.083	0.028	0.055
	(0.0069)***	(533.4)***	(0.011)***	(0.0036)***	(0.0054)***	(0.0043)***	(0.0041)***
Migrant × Disp. × 5	-0.10	-3008.6	-0.083	-0.021	-0.085	0.027	0.058
	(0.0070)***	(553.2)***	(0.010)***	(0.0036)***	(0.0054)***	(0.0041)***	(0.0042)***
Observations	569558	569558	507456	496535	569558	569558	569558
Dep. Var Mean Natives $t-1$	0.0057	38157.3	4.53	0.21	1	0	0
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Since Disp. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the α_j coefficients from regression equation 9 for the main outcome variables under *alternative matching approach 2*. "Not in Admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. Cohort FE includes Cohort interacted with time since displacement controls. All regressions moreover control for a fourth-order polynomial in age. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. See Figure B2 for the corresponding event study graphs.

Table D4: Regression Output for Figure 3, Panels (c)-(d)

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Wages for Workers Employed by				Workers Employed from t=0	
	t=0	t=1	t=2	t=3	Log Wages	AKM Estab. FE
Migrant × -5	-0.015				-0.015	0.00073
	(0.0066)**				(0.0066)**	(0.0017)
Migrant × -4	-0.0060				-0.0060	-0.00045
	(0.0048)				(0.0048)	(0.0011)
Migrant × -2	-0.0024				-0.0024	-0.00095
	(0.0045)				(0.0045)	(0.00071)
Migrant × -1	-0.0070				-0.0070	-0.0021
	(0.0052)				(0.0052)	(0.00098)**
Migrant × 0	-0.082				-0.082	-0.0015
	(0.013)***				(0.013)***	(0.0036)
Migrant × 1	-0.055	-0.090			-0.055	0.00075
	(0.011)***	(0.026)***			(0.011)***	(0.0036)
Migrant × 2	-0.027	-0.083	-0.060		-0.027	0.0052
	(0.0098)***	(0.024)***	(0.050)		(0.0098)***	(0.0035)
Migrant × 3	-0.032	-0.10	-0.14	-0.22	-0.032	0.0047
	(0.0097)***	(0.022)***	(0.047)***	(0.070)***	(0.0097)***	(0.0035)
Migrant × 4	-0.016	-0.078	-0.058	-0.24	-0.016	0.0064
	(0.0098)	(0.021)***	(0.047)	(0.067)***	(0.0098)	(0.0035)*
Migrant × 5	-0.0060	-0.078	-0.020	-0.23	-0.0060	0.0058
	(0.010)	(0.021)***	(0.044)	(0.066)***	(0.010)	(0.0035)*
Observations	140481	37071	12375	5738	140481	137870
Dep. Var Mean Natives $t-1$	4.49	4.49	4.49	4.49	4.49	0.22
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time Since Disp. FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the α_j coefficients from regression equation 1 for the main outcome variables, for the baseline analysis. Columns (5) and (6) restrict the samples to workers who found a new job within a year from the layoff. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. Cohort FE includes Cohort interacted with time since displacement controls. All regressions moreover control for a fourth-order polynomial in age. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. See Figure 3 for the corresponding event study graphs.

Table D5: Regression Output for Figure 4, Panels (a), (c), (e)

	(1) Not in Admin Data	(2) Unemployment	(3) Earnings Rel. To t-2
AKM Worker FE in t-1			
Q1	0.055	0.068	-0.19
	(0.014)**	(0.015)**	(0.023)**
Q2	0.039	0.084	-0.16
	(0.0065)**	(0.0090)**	(0.012)**
Q3	0.037	0.069	-0.14
	(0.0061)**	(0.0073)**	(0.010)**
Q4	0.019	0.046	-0.084
	(0.0060)**	(0.0076)**	(0.012)**
Q5	0.022	0.031	-0.051
	(0.0066)**	(0.0081)**	(0.012)**
Q6	0.036	0.0049	-0.023
	(0.0076)**	(0.0086)	(0.014)
Q7	0.029	-0.0022	-0.023
	(0.0096)**	(0.0091)	(0.018)
Q8	0.040	0.018	-0.0023
	(0.013)**	(0.010)	(0.023)
Q9	0.074	0.0078	-0.072
	(0.015)**	(0.0095)	(0.027)**
Q10	0.078	-0.020	0.020
	(0.017)**	(0.0085)*	(0.033)
Observations	15416	15416	15416

Notes: This table reports the migrant-native gap in costs of job displacement by migrants' decile of pre-displacement AKM worker FE, measured in t=-1. We use the AKM worker FE measure provided by Lochner et al. (2024). Each column shows the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. "Not in Admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. * and ** correspond to 5 and 1 percent significance levels, respectively. See Figure 4 for the corresponding figures.

Table D6: Regression Output for Figure 4, Panels (b), (d), (f)

	(1) Leaves Admin Data	(2) Unemployment	(3) Earnings Rel. To t-2
Origin Country Net Income in t-1			
Q1	-0.014	0.084	-0.12
	(0.017)	(0.033)*	(0.045)**
Q2	0.033	0.049	-0.15
	(0.025)	(0.026)	(0.046)**
Q3	0.037	0.058	-0.17
	(0.015)*	(0.021)**	(0.029)**
Q4	0.030	0.035	-0.090
	(0.015)*	(0.017)*	(0.029)**
Q5	-0.00043	0.067	-0.055
	(0.010)	(0.016)**	(0.027)*
Q6	0.0044	-0.0076	0.025
	(0.014)	(0.015)	(0.027)
Q7	0.0078	0.11	-0.13
	(0.0058)	(0.0098)**	(0.014)**
Q8	0.023	0.057	-0.10
	(0.0058)**	(0.0075)**	(0.012)**
Q9	0.051	-0.0098	-0.042
	(0.0073)**	(0.0073)	(0.013)**
Q10	0.14	-0.042	-0.086
	(0.014)**	(0.0080)**	(0.017)**
Observations	13218	13218	13218

Notes: This table reports the migrant-native gap in costs of job displacement by migrants' origin country net income, measured in t=-1. We collect data on "adjusted net income" by country from the World Bank's World Development Indicators (World Bank, 2024). Each column shows the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. "Not in Admin data" is a dummy that is equal to 1 whenever a worker does not have a social security record. Unemployment is defined as being a UI benefit recipient or a training program participant. Relative earnings is defined as earnings in t relative to earnings in $t = -2$. Standard errors (in brackets) are clustered at the baseline establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. * and ** correspond to 5 and 1 percent significance levels, respectively. See Figure 4 for the corresponding figures.

Table D7: Regression Output for Figure 5, Panels (a), (b), (c)

	(1) All	(2) Native Network	(3) Migrant Network
Migrant × -5	-0.0030	-0.0052	0.0035
	(0.0042)	(0.0041)	(0.0037)
Migrant × -4	0.0024	0.0010	0.0034
	(0.0024)	(0.0023)	(0.0020)*
Migrant × -2	0.0018	0.0010	0.0027
	(0.0016)	(0.0015)	(0.0013)**
Migrant × -1	0.00070	0.00022	0.00080
	(0.0011)	(0.0010)	(0.00086)
Migrant × 0	0.050	0.042	0.070
	(0.0053)***	(0.0054)***	(0.0058)***
Migrant × 1	0.053	0.043	0.082
	(0.0058)***	(0.0058)***	(0.0057)***
Migrant × 2	0.058	0.046	0.077
	(0.0060)***	(0.0060)***	(0.0056)***
Migrant × 3	0.052	0.041	0.071
	(0.0060)***	(0.0060)***	(0.0055)***
Migrant × 4	0.045	0.031	0.064
	(0.0061)***	(0.0060)***	(0.0053)***
Migrant × 5	0.045	0.032	0.058
	(0.0061)***	(0.0060)***	(0.0053)***
Observations	318364	318364	318364
Dep. Var Mean Natives $t-1$	0	0	0
Individual FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Time Since Disp. FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes

Notes: This table reports the α_j coefficients from regression equation 1 for the migrant-native gap in moving to establishments that are part of their network. We control for AKM worker FE measured in t=-1. In column (1), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous coworker. In column (2), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous *migrant* coworker. In column (3), the outcome variable is a dummy indicating whether the displaced worker works at an establishment to which they are connected via a previous *native* coworker. Coworkers are all workers who were employed at the displacement establishment in the same 3-digit occupation at least once in the 3 years before the layoff and have moved to a different establishment. Standard errors (in brackets) are clustered at the worker level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016. *, ** and *** correspond to 10, 5 and 1 percent significance levels, respectively. See Figure 5 for the corresponding event study graphs.

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