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 lose an additional 9% of their earnings 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 depending on lower-quality coworker networks during job search.

Keywords: Immigration, Job Displacement, Job Search
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1 Introduction

Immigration is often hailed as a solution to the demographic crisis of high-income countries. The influx of foreign labor can relieve labor shortages in particular sectors and occupations and reverse or delay the consequences of declining fertility rates. In reality, however, many countries struggle to make the most of migrants' economic potential: migrant earnings and employment rates are below those of natives in the majority of the OECD.[1]

Existing studies have identified a host of causes for the differences between migrant and native outcomes: differences in human capital and job credentials; linguistic, cultural, and institutional distance from the host country; individuals' legal status and the country's assimilation policy (Duleep and Regets, 1999; Borjas, 2015; Gould and Klor, 2015; Gathmann and Keller, 2018; Govind, 2021; Lippens et al., 2023; Foged et al., 2024). Typically, these findings come from comparing the convergence of migrants' and natives' wages over time. However, two issues stand out. First, it is notoriously difficult to control for the array of differences between migrants and natives. Second, migrants' wage convergence is not necessarily linear, and understanding the obstacles to convergence is as important as understanding its drivers.

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 circumstances. Overall, this setting allows us to provide evidence on an important impediment to wage convergence: the cost of job loss for migrants.

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 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[3], they still struggle to take up employment.[4]

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.[5] While we are unable to 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 native counterfactual

[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.

[4]For example, in August 2022, only 53% of migrants in Germany were employed, compared to 69.2% of natives (Brücker et al., 2022).

[5]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 displacement by comparing the outcomes of displaced natives and migrants to a matched counterfactual of non-displaced workers. See Appendix A.4 and Figure B1 for details.

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 52% of their earnings in the 5 years post-displacement. Migrants face an additional loss of 9%. 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% 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 (Lubotsky, 2007; Adda et al., 2022), 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. We start by documenting that migrants are indeed more likely to drop out than natives following a job displacement. By year 5, migrants are 5.5ppt 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 estimated using the standard model of 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 are significantly more likely to drop out of the administrative data. We conclude that if return migrants are leaving Germany to pursue better opportunities, their post-displacement outcomes in Germany would have likely been worse than those of other migrants, leading us to underestimate the migrant-native gap.

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, suggesting that return migration can explain only some of the difference in the total cost of job displacement between migrants and natives.

To summarize, we show that migrants face a significant additional cost of job displacement compared to natives. Our empirical strategy means we already control for many of the usual explanations for worse economic outcomes of migrants, such as education, employer, and previous career. 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. Finally, 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 they are more reluctant to search outside of their core occupation and commuting zone.

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 (Gathmann et al., 2020; 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. While we lack any direct measures of discrimination in the German labor market, we would expect employers to discriminate less in labor markets where hiring is more costly – such as where the unemployment rate is high or labor market thickness is low. As such, our results provide little direct evidence of labor market discrimination. We also do not find any variation across occupations requiring different degrees of physical proximity, as we'd expect if the migrant-native gap stems from taste-based discrimination on the side of employees or customers. While we do find significant variation in the cost of job displacement by ethnic and cultural proximity to Germany, these might be driven by some of e.g. local knowledge and language skills which are not captured in our data.

Since migrants' worse employment outcomes do not appear to be driven by the leading supply- and demand-side factors, 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 12% of the initial migrant-native gap in wages. 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 Danieli (2024) and adjust it to reflect the fact that larger networks are more likely to result in labor market referrals (Dustmann et al., 2015). 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: while the migrants who are able to take advantage of their networks do better than their peers, this effect in itself is not large enough to fully close the gap, and the greater sensitivity to migrant networks leads to perpetuating worse labor market outcomes for migrants.

Overall, while our paper documents the importance of the matching mechanism for migrant outcomes, we cannot explain the gap in full. In part, this reflects the limits of our analysis. We do not measure worker behavior exhaustively: for example, we lack information on job search effort and intensity. We similarly lack measures of labor market discrimination that would allow us to estimate its impact directly. There is also the broader issue of unobserved differences between migrants and natives: both groups self-select into their status which might lead to systematic differences in characteristics such as risk aversion and productivity (Borjas (1987)). Nevertheless, our paper highlights that our understanding of why migrants suffer relatively more after job displacement is tightly linked to the question of what makes a worker a "migrant": Cultural and labor market know-how? Physical appearance? Preferences? Skills? Answering this question is likely the first step to designing policy to improve migrants' relative outcomes. This paper carries several other implications for policymakers. We show that wage convergence is not a linear process, and job-seeking and training support for migrants might need to continue even after their initial assimilation. We also show that migrants' outcomes are not just a function of their characteristics and behavior: *how* labor markets function matters, too.

This paper speaks to several literatures. First, we contribute to the broad literature on economic outcomes and integration of migrants. A number of studies have documented the relatively slow 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 (Chiswick, 1978; Borjas, 1985; Lubotsky, 2007; Algan et al., 2010; Blau et al., 2011; Abramitzky et al., 2014). Existing literature has also demonstrated that migrants are more prone to, and impacted by, adverse shocks (Altonji and Blank, 1999; Dustmann et al., 2010; Kahn, 2010; Hoynes et al., 2012; Kondo, 2015; Speer, 2016; Borjas and Cassidy, 2023).[6] 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 employer, labor market conditions, or worker sorting. 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 but with a focus on raw differences, without controlling for variation between the two groups.

Our finding on 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.[7] 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. Previous 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; Couch and Placzek, 2010; Schmieder et al., 2023). Displacement comes at much higher costs for women (Meekes and Hassink, 2022; Illing et al., 2024), black workers in the US (Sorkin, 2023), workers in routine-intensive occupations (Blien et al., 2021), and low-wage workers in the manufacturing sector (Helm et al., 2021); Athey et al. (2024) present a comprehensive overview of the heterogeneity in the cost of displacement for workers in Sweden.[8]

[6] Borjas and Cassidy (2023) find that migrants particularly suffered from displacement during the early phase of the Covid-19 pandemic, partly because they are less likely to work in jobs that can be performed remotely. In the same spirit, other studies have shown that migrants' entry wages during recessions are lower than natives' (see, e.g., (Kahn, 2010; Kondo, 2015; Speer, 2016)) and that migrants' or African Americans' unemployment rate is particularly sensitive to business cycle conditions and local unemployment rates (e.g., (Altonji and Blank, 1999; Bratsberg et al., 2006; Dustmann et al., 2010; Hoynes et al., 2012)).

[7] Similar patterns are observed in Sweden (Åslund et al., 2021) and Israel (Arellano-Bover and San, 2020).

[8] 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.

Our paper contributes to this literature by estimating in detail the relative cost of job displacement for migrants, a population relatively more exposed to negative labor market shocks, and by examining a host of possible mechanisms that drive our result.

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 0 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. (2023). 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' legal standing 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 B7, 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 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 at least 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.[9]

Baseline Restrictions We follow the job displacement literature (e.g., Jacobson et al. (1993); 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 B5 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.

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.

[9] A potentially important point 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.

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); von Wachter et al. (2011); 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. Within these, we match migrants to natives. In contrast to the existing literature, our estimates therefore do not quantify the absolute costs of job displacement; instead, they quantify the *additional* cost of job displacement for migrants relative to native workers. In Appendix Section A.4, we show that we can replicate our main results using the estimates of the cost of job displacement (following a version of the baseline matching in Schmieder et al. (2023)).

Second, compared to most of the existing studies (see e.g., Helm et al. (2021) 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. 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, education in t-1, and tenure in t-1.

Matching on these characteristics allows us to control both for pre-layoff productivity and skill/experience profiles.[10] 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 workers in Germany.[11] The table yields two key takeaways: First, migrants and natives in our baseline sample are positively selected compared to the overall population. They have higher tenure and wages, and work more days per year. They also work in smaller establishments with a lower

[10] As Table B4 shows, our results are robust to alternative matching specifications, such as not matching on wages.

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

share of high-skilled workers, and a lower share of workers in a minijob.[12] Given this positive selection of individuals in our baseline sample, our results likely present lower bounds compared to the effects we would estimate on the full population of migrants and natives in Germany.

Second, while migrants and natives in the random sample are very different, our matching algorithm does a good job of making 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), even though migrants work slightly fewer days per year (334.4 vs. 338.5). By construction, displaced migrants and natives work in exactly the same establishments.

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:

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

where the dependent variable y_{itc} denotes average labor market outcomes (e.g., earnings, log daily wages, employment) of individual i, belonging to cohort c in year t.[13] $Migrant_i$ 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 time-varying age polynomials. 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 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.

[12] 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 Tazhitdinova (2020) or Gudgeon and Trenkle (2024) for more detail.

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

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.

$$\Delta y_{DID,p} = \delta Z_p + \epsilon_p \tag{4}$$

where the coefficient of interest, δ, tells us how the migrant-native gap varies for matched pairs with different baseline characteristics. 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. After the layoff, however, a gap opens up. As shown previously (Jacobson et al., 1993; Couch and Placzek, 2010; 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 2,400 EUR larger than that of a similar native worker laid off from the same firm, equivalent to 18% higher cost of job displacement. Moreover, this gap diminishes only slowly over time: five years after displacement, it has closed by only by a third.

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% lower than natives' immediately after the layoff; this gap approximately halves over the next 5 years, but remains statistically and economically significant.[14] 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.

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 strategies, sample restrictions, and a longer time frame.

Matching and Sample Restrictions We test the robustness of our findings with several alternative matching strategies and sample restrictions. In column (5) of Table B4, 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 that our estimates remain virtually unchanged compared to the baseline.

Regarding our sample choices, in column (4) of Table B4, we relax the firm size cutoff of 50 employees to include displaced workers from firms with 30 workers or more. In Table B5, 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). Finally, 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.

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 B4, 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

[14] 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.

in the admin data), so we re-estimate our difference-in-difference regression excluding these workers. The results, presented in column (4) of Table B5, are robust to this exclusion (see also Figure B7 for a direct comparison of naturalized and non-naturalized migrants).

Cohort Heterogeneity During our baseline layoff period, 2001-2010, 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.[15]

We test this possibility in several ways. First, in column (6) of Table B5, we restrict our sample to workers displaced in the four years between 2000 and 2003 so that their 5-year 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 not different from our baseline estimates. Further, in Figures B2 and B3, 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[16]; the gap is statistically significant and persistent for all cohorts.

We also show that our findings are robust to a related concept of tenure length in the German labor market (rather than in the layoff firm). We plot, in Figure B8, the cumulative cost of displacement by the number of years a worker has been recorded for in the administrative data. We find that the earnings gap between migrants and natives is invariant to overall tenure in the labor market.

Long-Term Outcomes In Figure B4, 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 total annual earnings decreases from 14% 5 years out to 9% 10 years out. 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 at the same level as immediately after the layoff.

The pattern for the daily wage gap similarly extends beyond the first five years: the initial gap of about 13% is closing steadily and reaches 4% 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

[15] Recent methodological advances in the difference-in-difference literature have also highlighted that heterogeneous treatment effects may lead to bias in the difference-in-differences estimator. The source of bias in the standard setup of dynamic difference-in-differences comes from making "forbidden" comparisons of already-treated units from different cohorts, as well as from comparing treated and control units from cohorts with very different treatment effects. We avoid these pitfalls by only comparing displaced migrants and natives from the same layoff event: the treated (migrant) and control (native) units belong to the same event cohort, and we do not use not-yet-treated units as controls. However, our baseline estimate could still mask large differences in treatment effects by cohorts, which is why we estimate our event studies separately by cohort in Figures B2 and B3.

[16] The workers laid off during the recession experienced the smallest migrant-native gap, of about 7% on average for the five years post-displacement.

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 B7). 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.[17]

Drop-out Patterns We start by evaluating the likely extent of return migration: how much more likely are migrants to drop out of the administrative data after displacement?[18] 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 5.5ppt.[19]

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

[17]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.

[18]Workers might be leaving the administrative sample for a variety of reasons besides emigration, such as becoming self-employed, starting education, or retiring. Unfortunately, the administrative data does not record the different choices reliably: 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). As a result, we cannot compare return migration to other reasons for attrition. However, the aim of our analysis is to understand the role of changing composition on our estimate of the migrant-native gap, regardless of the reason for attrition.

[19]Importantly, attrition is not driven by a large outflow within the 1st 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 B8, 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).

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 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 1.3% a year more, but have marginally shorter tenure in the firm (3 months). They also tend to work in somewhat larger firms with better-skilled workforce and a lower share of marginally attached coworkers. 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. There is no clear pattern by AKM worker FE (Panel a): both less productive migrants and more productive migrants are only marginally more likely to leave the data. One exception is migrants in the top productivity decile, who are about a third more likely to leave the data. We therefore exclude them from our baseline sample for a robustness check (Table B5, 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 substantially more likely to drop out of the sample, and the likelihood to drop out increases sharply with better outside options.[20] Migrants in the top decile are 12ppt 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 log yearly earnings. For migrants in the top 3 productivity deciles, there is no gap in unemployment rates and log yearly earnings, but the gap almost linearly increases with decreasing worker productivity (Panels c and e). Similarly, migrants in the top 3 deciles of origin country net income face no unemployment gap and a smaller gap in log yearly earnings compared to migrants with worse outside options.

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 decile of the migrant productivity distribution. We do find a strong selection pattern driven by the economic situation in migrants' origin country which applies to 40% of migrants. This suggests that return migrants are negatively selected: the better outside options of some migrants allow them to leave Germany rather than face low wages or unemployment. However, 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

[20]The same analysis but by migrants' origin group, rather than country income (Panel f of Figure B9) 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.

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 0 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 (15% instead of 18%), driven by slightly higher employment probabilities (5ppt vs 7ppt gap) in the year of displacement. We also observe a faster closing of the employment and earnings gap when we exclude return migrants – further supporting the hypothesis that return migration serves as an exit route for displaced migrants who would otherwise struggle with 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. Observationally similar migrants, laid off from the same establishment, are 8ppt less likely to become re-employed than their displaced native coworkers. If they do find another job, they initially earn 13% less; and while this gap narrows over time, migrants' total earnings are 15% 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 the different costs of displacement, we use additional data on the job search preferences and objectives of UI benefit recipients.[21]

One caveat in using this data is that not all displaced workers receive UI benefits, primarily because some manage to find employment before the layoff. 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).[22]

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

[22] 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

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 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.[23] 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.

Panels (a) and (b) in Figure 5 plot the migrant-native gap for wages and employment by local (county) unemployment rates. The wage gap is somewhat smaller for the counties in the lowest quartile of unemployment, suggesting that migrants are able to keep up with their native coworkers when labor market conditions are better, but this difference is not statistically significant.

Next, we look at occupation-specific labor market thickness. This measure, based on Jäger and Heining (2022), captures how much a given local labor market specializes in the worker's occupation.[24] 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

includes all incidences of unemployment spells, including for employed individuals receiving assistance from 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.

[23] 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).

[24] 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.

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.[25]

Panels (c) and (d) in Figure 5 paint a mixed picture. We do not find any significant effects of labor market thickness on employment: the positive and negative effects either perfectly balance each other, or thickness does not affect migrants' employment differentially to the natives. We do find a weak positive relationship with wages. The wage gap in the top quartile (10%) is a third lower than in the bottom quartile of commuting zones (15%), although the difference is not statistically significant.[26] 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, one way to study this mechanism indirectly is by comparing migrants' relative outcomes across different labor market conditions. Drawing on Becker (1971), employers should discriminate less in tighter labor markets where finding another employee is more costly.

In the light of this hypothesis, our finding of a weak and insignificant relationship between measures of outside options and local labor market conditions in Figure 5 means we fail to find strong evidence that discrimination against migrants plays a large role in driving the migrant-native gap.

Next, we look at whether the migrant-native gap might be driven by taste-based discrimination on the side of consumers, employers, or other employees. In Panels (e) and (f) of Figure 5, 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: while both employment probability and wages are somewhat lower for displaced migrants in the third quartile of physical proximity requirement, the profile across the other three quartiles is virtually flat.

Finally, we estimate the migrant-native gap by different migrant origin groups in Figure B9. We find that migrants from Africa, Asia (including Turkey) and the Middle East suffer significantly larger cost of job displacement compared to migrants from regions ethnically, culturally and racially more similar to Germany, such as Western and Eastern Europe.[27] 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.

Overall, the evidence on the role of anti-immigration discrimination is weak. While we do find that migrants originating from certain regions experience a larger migrant-native gap, this might reflect racial discrimination as well as any cultural or language advantage on the side of the migrants. Similarly, we do not find any evidence that migrant-native gaps are smaller in markets and occupations where employer

[25] Jäger and Heining (2022) show that the cost of substituting a worker is lower in thicker markets, highlighting the advantage such markets present to the employer. Dauth et al. (2022) show that larger and thicker labor markets experience stronger positive assortative matching, leading to better matches and higher productivity.

[26] 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.

[27] The comparison of the migrant-native gaps by migrants' naturalization status in Figure B7 suggests that these differences are unlikely to be driven by potential differences in migrants' legal status.

discrimination might be more costly. Nevertheless, our evidence is indirect, and we cannot rule out that anti-migrant sentiment does not drive our results.

4.4 Sorting across Firms

One of the key factors explaining wage heterogeneity between otherwise similar workers, both in levels and growth, are firms (Topel and Ward, 1992; 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 have already been employed in relatively higher-paying firms pre-layoff. Panel C in Table 1 shows that while the median wage of layoff firms is in line with the rest of the firm population for native workers, these firms do pay more than other firms employing migrants (median migrant wage is 6.2% higher in layoff firm compared to all firms). 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 B5. 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 12% 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 approximately halves 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.

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 increases to .5% by year 5. 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 the growth in wages is driven partly by migrants switching jobs to climb the firms ladder faster, in addition to firms learning about, and rewarding, migrants' true productivity over time.

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 B6 shows that the firm FE gap is smaller – converging to 0 – 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 30% 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 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; 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 Danieli (2024). 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 sample of matched workers.[28] 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.[29]

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

[28] 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.

[29] 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.

options:

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

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. 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 3. 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. 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 4. 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 of 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 6 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 B6, 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 4 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, they 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 B6, 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 B10, 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 quartile 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 quartile 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.[30]

4.6 Discussion

In the previous sections, we have explored a host 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. 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 sentiment 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

[30] Another explanation is the spatial sorting of different migrant types or migrants of different origins. While even the most migrant-heavy counties in Germany are majority-native (see Figure B12), migrants from different backgrounds might be more or less likely to settle alongside their compatriots. If migrants from the West are less likely to see out ethnic enclaves, as well as overall more likely to emigrate from Germany, we would observe the same pattern as we find in Figure B10. Similarly, the migrants who do not need their ethnic networks may settle in most native countries.

mechanism.

The lack of a single leading mechanism does not mean the data offers no suggestions on how to reduce the cost of job displacement for migrants. Figures B6 and B11 show that relative migrant outcomes vary strongly with migrants' characteristics. Migrants who are more productive and have higher education in general experience relatively better post-displacement outcomes, in some cases closing the gap with natives entirely. As we show in Panels (f) of the figures, much of this better performance is not driven by return migration. From a policy perspective, instead of trying to help all migrants to adopt the economic behavior of natives, the policymakers could focus on interventions that allow less educated, less productive migrants to close the gap with other migrants who already navigate the German labor market successfully.

5 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% lower. Over the 5 years after the displacement, this corresponds to an average additional loss of earnings of 20% per year compared to natives 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.

Having established our main finding of a significant, negative, and persistent gap in labor market outcomes between displaced migrants and natives, we conduct a range of exercises to explain it. We start by showing that the gap is unlikely to be the result of a selective pattern of return migration. We then turn to the literature and explore a range of leading drivers of migrants' labor market outcomes. We can reject the suggestion that migrants simply search for different kinds of jobs, and we find little evidence that the gap is driven by variations in local labor market conditions. We do find evidence of two key channels: firm sorting and coworker networks. We show that job displacement disrupts the "firms ladder" progression of migrants more, and they have to climb it faster in order to re-establish pay parity with natives. In terms of networks, we find that they both worsen and ameliorate the relative cost of job loss for migrants. On the one hand, migrants benefit from networks more, which helps to close the gap somewhat. On the other hand, migrants rely more on the smaller and lower-quality migrant networks, which perpetuates their worse labor market outcomes.

There are also several important channels our paper cannot speak to. While our analysis of local labor market conditions fails to find any evidence of employer discrimination against migrants, this analysis is based on predictions of behavioral patterns a la Becker (1971) that might not correspond to real-life discrimination. We lack the data to test other plausible mechanisms, such as job search behavior, language skills, or return intentions which might drive workers' incentives to accumulate human capital. Our paper highlights the importance of understanding what it means to be a migrant in order to fully explain the obstacles to their assimilation.

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-seeking 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. A policymaker wishing to make the most of migrants' economic potential might find it easier to focus on closing the differences within the migrant group, by learning from migrants who already navigate the German labor market successfully.

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6 Tables

Table 1: Characteristics of Displaced Workers vs. A Random Sample 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	11.2	11.3	12.0	11.5
	[2.00]	[1.68]	[1.92]	[1.57]
Age	37.5	37.9	40.1	38.3
	[12.4]	[7.09]	[13.2]	[6.96]
Tenure	2.47	6.38	3.17	6.41
	[2.52]	[2.52]	[2.75]	[2.52]
Real Daily Wage	60.2	89.2	69.9	91.5
	[44.0]	[30.4]	[48.0]	[31.7]
Total Yearly Earnings	14163.7	33683.1	20772.5	35675.7
	[16398.6]	[32190.5]	[18518.9]	[37575.2]
Days per Year Working	219.4	334.4	283.1	338.5
	[157.0]	[55.3]	[133.7]	[51.1]
AKM Worker FE	.	4.33	.	4.35
		[0.28]		[0.29]
Panel B: Regional Characteristics				
Lives in City	0.63	0.65	0.44	0.62
	[0.48]	[0.48]	[0.50]	[0.49]
Works in East Germany	0.061	0.057	0.19	0.057
	[0.24]	[0.23]	[0.39]	[0.23]
Panel C: Establishment Characteristics				
Size of Establishment	1044.6	323.1	793.9	323.1
	[4035.7]	[527.4]	[3530.8]	[527.4]
Share Migrant Workers	0.30	0.19	0.053	0.19
	[0.27]	[0.15]	[0.087]	[0.15]
Share High-Skilled Workers	0.094	0.089	0.12	0.089
	[0.15]	[0.13]	[0.17]	[0.13]
Share in Minijob	0.19	0.050	0.16	0.050
	[0.27]	[0.11]	[0.25]	[0.11]
Median Full-time Wage	79.6	84.6	85.6	84.6
	[37.7]	[29.1]	[37.4]	[29.1]
Displaced from Complete Closure	.	0.32	.	0.32
		[0.47]		[0.47]
Number of Observations	774921	15638	8062766	15638

Notes: This table presents differences in average characteristics for our baseline sample of displaced migrants and natives compared to a random sample of workers. Columns (1) and (3) show characteristics of a random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent all displaced migrants and natives in the baseline sample. We report displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Standard deviations in brackets.

Table 2: Reported Preferences vs. Realized Outcomes

	(1) Natives Mean		(2) Migrants Gap		(3) Number of
	Mean	Std. Err.	Gap	Std. Err.	Observations
Panel A: Any UI 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 and Heining (2022), *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 displacement establishment level (constant within matched worker pairs). 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: 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 Medium-Skilled Workers	0.70	0.71	0.65
	[0.13]	[0.13]	[0.17]
Share Full-Time Workers	0.74	0.74	0.72
	[0.17]	[0.17]	[0.23]
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. Column (1) presents summary statistics for the full network, column (2) presents summary statistics for the network of native coworkers, and column (3) presents summary statistics for the network of migrant coworkers. Panel A summarizes the average number of connections, Panel B summarizes the average characteristics of coworkers, and Panel C summarizes the characteristics of firms where coworkers are employed in t=0. Note that the number of distinct networks is smaller than the number of matched pairs in our baseline sample (Table 1) because several matched pairs can be part of the same layoff event. Standard deviations in brackets.

Table 4: The Role of Coworker Networks

	(1)	(2)	(3)	(4)
	Baseline		Former Coworkers	
		All	Migrants	Natives
Panel A: Log Wages				
Migrant	-0.12	-0.13	-0.13	-0.13
	(0.016)***	(0.016)***	(0.016)***	(0.016)***
$\Omega_{pt,all}$		0.0088		
		(0.0040)**		
Migrant × $\Omega_{pt,all}$		0.014		
		(0.0080)*		
$\Omega_{pt,migrant}$			0.00083	
			(0.0029)	
Migrant × $\Omega_{pt,migrant}$			0.017	
			(0.0077)**	
$\Omega_{pt,native}$				0.0022
				(0.0021)
Migrant × $\Omega_{pt,native}$				0.0046
				(0.0031)
Observations	22472	22472	22472	22472
Mean Dep. Var Natives	-.333	-.333	-.333	-.333
	(.005)	(.005)	(.005)	(.005)
Panel B: Employment				
Migrant	-0.079	-0.080	-0.080	-0.079
	(0.0057)***	(0.0054)***	(0.0056)***	(0.0057)***
$\Omega_{pt,all}$		0.0043		
		(0.0055)		
Migrant × $\Omega_{pt,all}$		0.0074		
		(0.0031)**		
$\Omega_{pt,migrant}$			0.0013	
			(0.0034)	
Migrant × $\Omega_{pt,migrant}$			0.0042	
			(0.0037)	
$\Omega_{pt,native}$				0.0015
				(0.0017)
Migrant × $\Omega_{pt,native}$				0.0023
				(0.0015)
Observations	25354	25354	25354	25354
Mean Dep. Var Natives	-.289	-.289	-.289	-.289
	(.002)	(.002)	(.002)	(.002)

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. 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

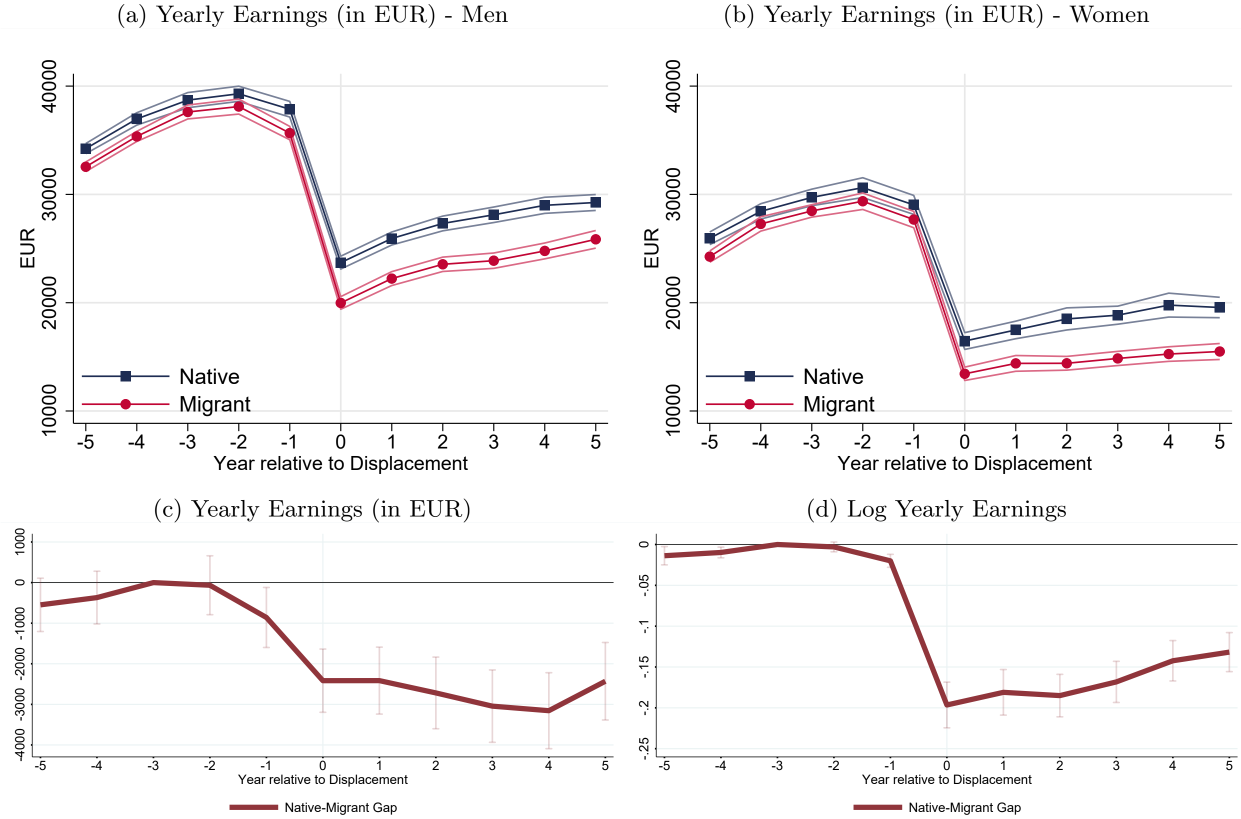
Figure 1: The Migrant-Native Earnings Gap

(a) Yearly Earnings (in EUR) - Men

(b) Yearly Earnings (in EUR) - Women

(c) Yearly Earnings (in EUR)

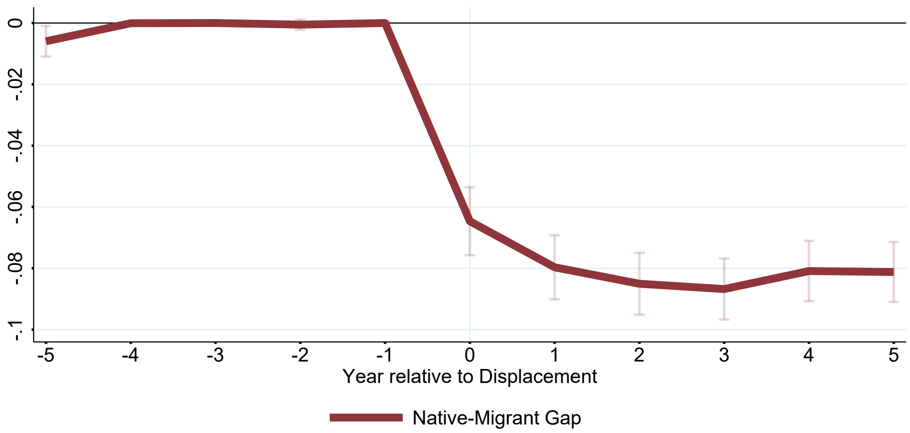
(d) Log Yearly Earnings



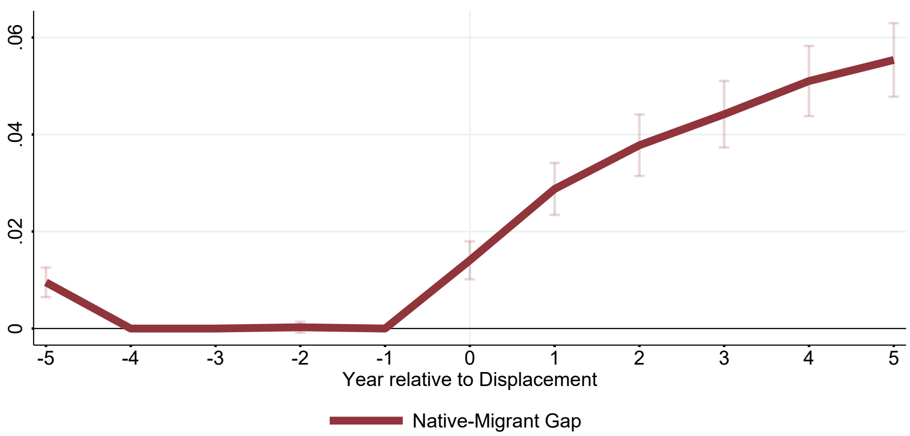
Notes: This figure plots raw means and event study regression coefficients for yearly earnings pre- and post-layoff. Panel (a) plots the raw earnings trajectory for men, separately for natives (blue squares) and migrants (red dots). Panel (b) plots the raw earnings trajectory for women, separately for 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 log earnings, pooling men and women. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 2: The Migrant-Native Employment Gap

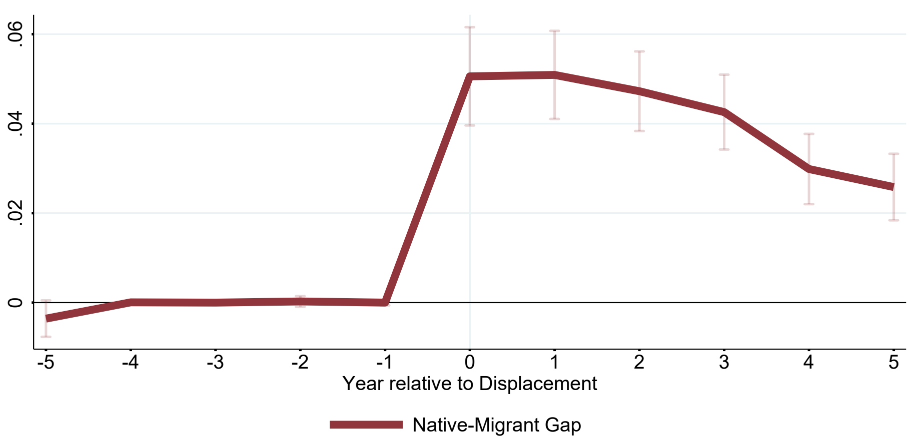
(a) Employment



(b) Not in Admin Data

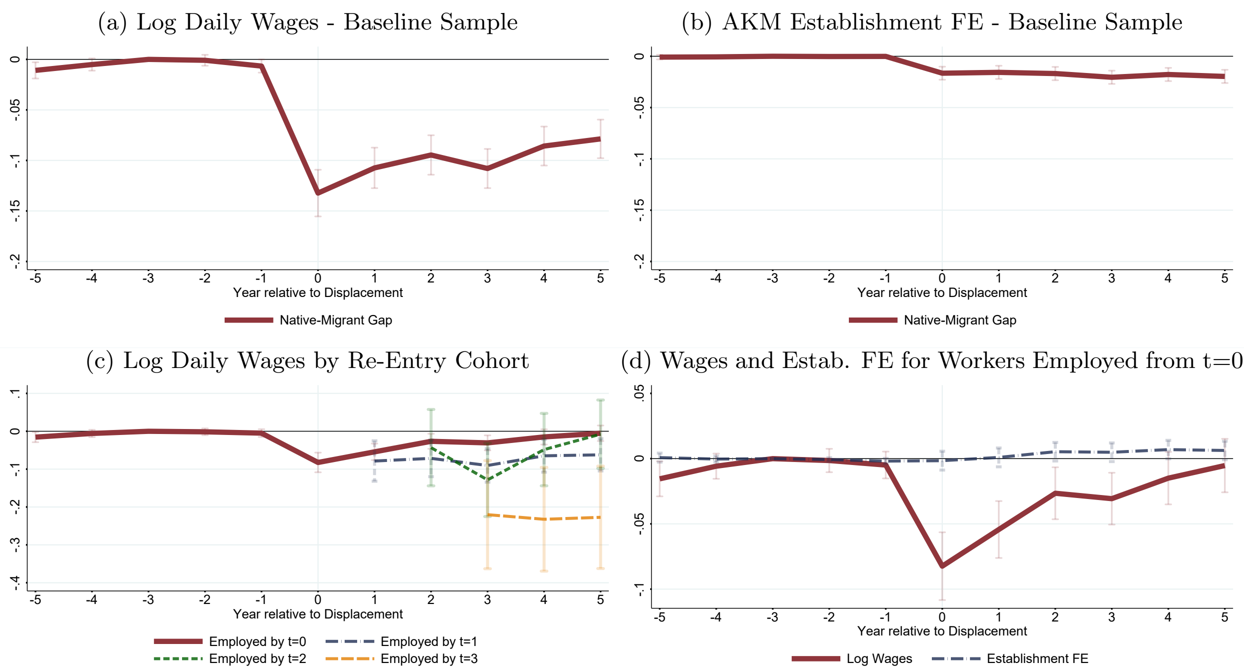


(c) Unemployment



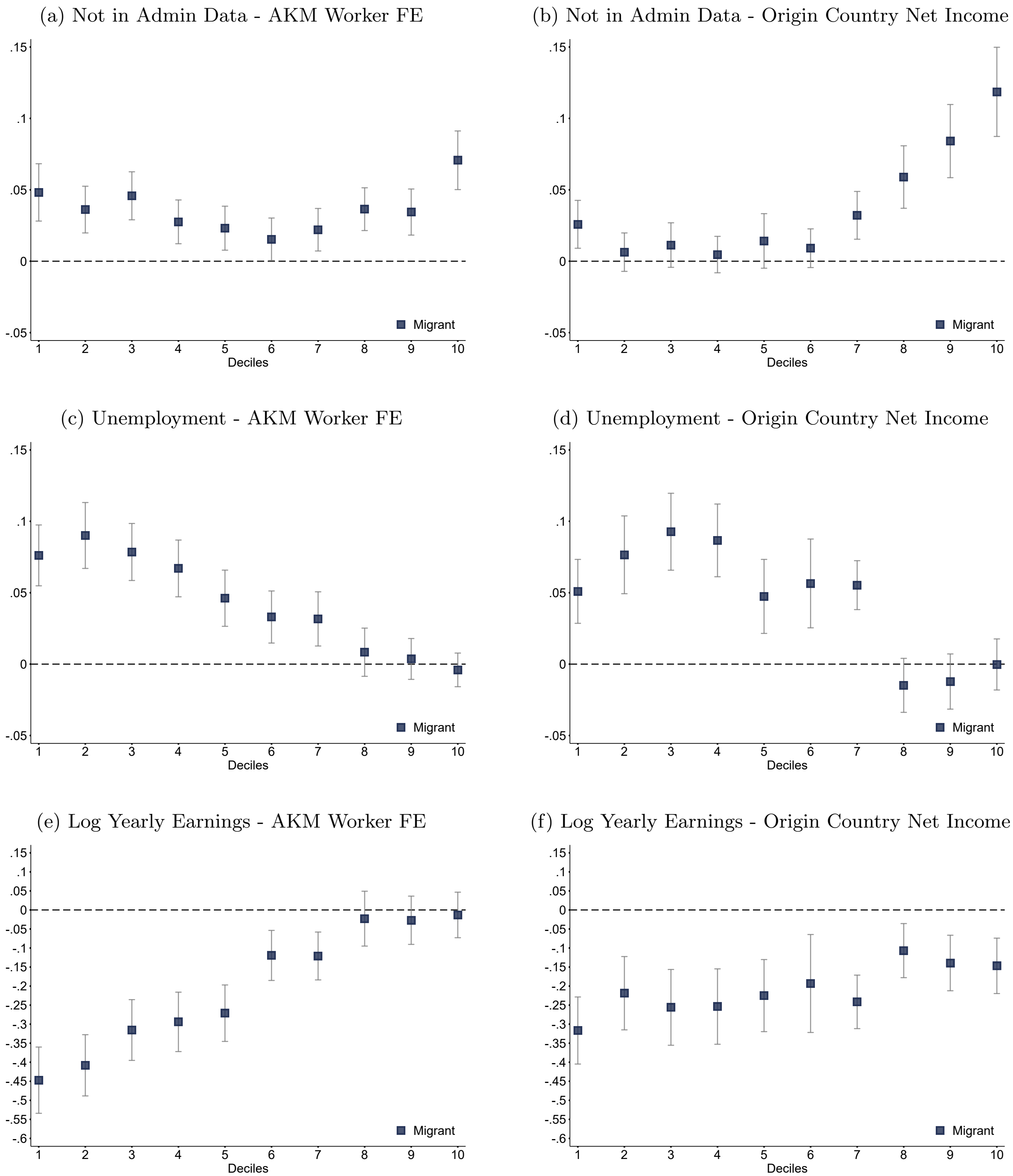
Notes: This figure plots the α_j coefficients from regression equation 1 for the migrant-native employment gap. In Panel (a), the outcome variable is employment, including 0s when there is no administrative record. In Panel (b), the outcome is a dummy taking the value 1 whenever a worker does not have 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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 3: 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. (2023). 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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

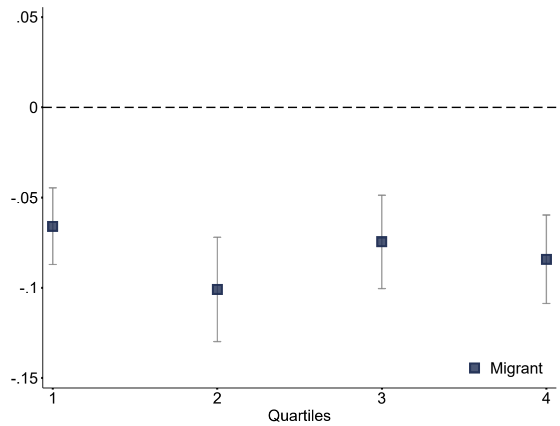
Figure 4: Migrant-Native Gaps by Pre-Displacement AKM Worker FE and Origin Country Net Income



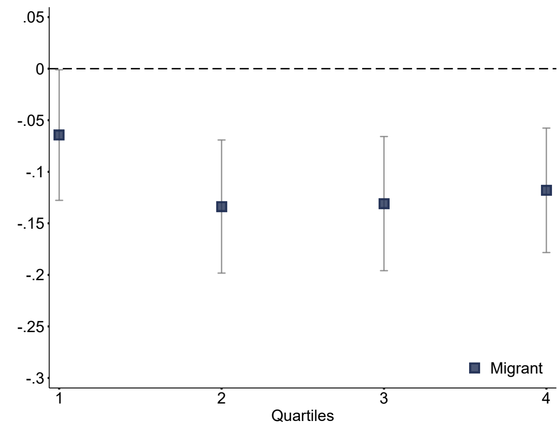
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by migrants' decile of pre-displacement AKM worker FE (Panels a, c, e) and origin country net income (Panels b, d, f), all measured in t=-1. We use the AKM worker FE measure provided by Lochner et al. (2023) and 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. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 5: Migrant-Native Gap by Pre-Displacement Local Labor Market and Occupation Characteristics

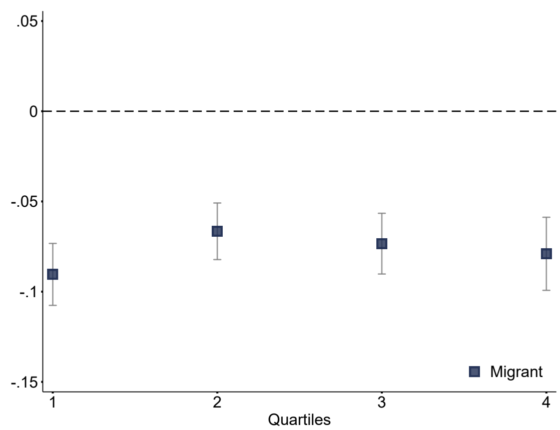
(a) Employment - Unemployment Rate



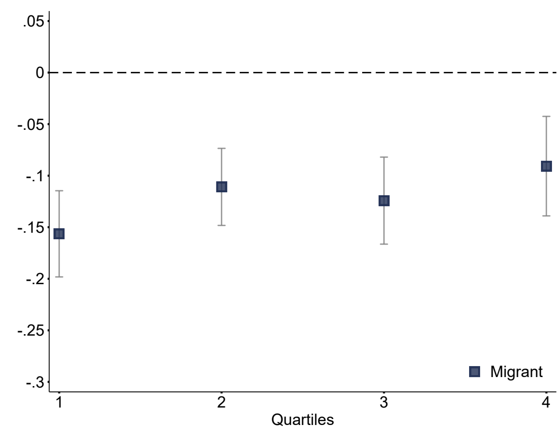
(b) Log Daily Wages - Unemployment Rate



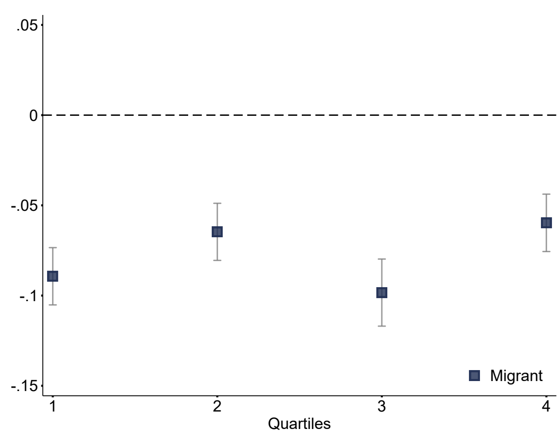
(c) Employment - Thickness



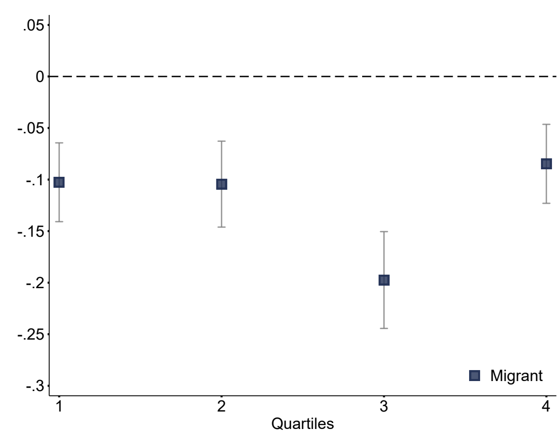
(d) Log Daily Wages - Thickness



(e) Employment - Physical Proximity



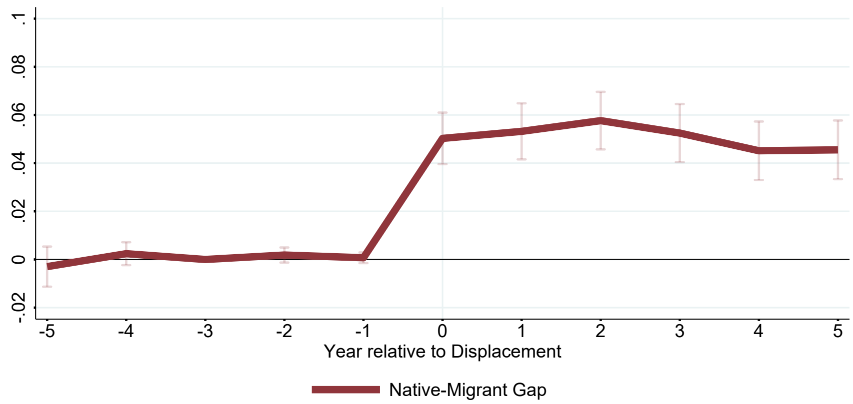
(f) Log Daily Wages - Physical Proximity



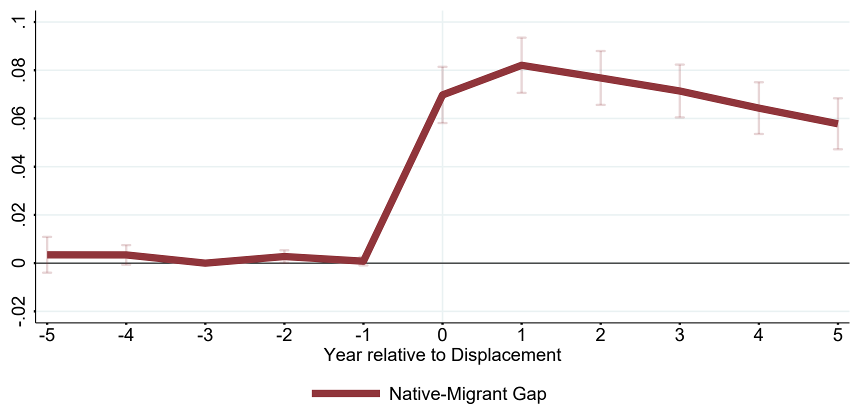
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by quartile of county unemployment rate (Panels a and b), local labor market thickness (Panels c and d), and the 2-digit occupation's physical proximity indicator (Panels e and f), all measured in t=-1. We follow Jäger and Heining (2022) 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure 6: Job Switches to Connected Establishments

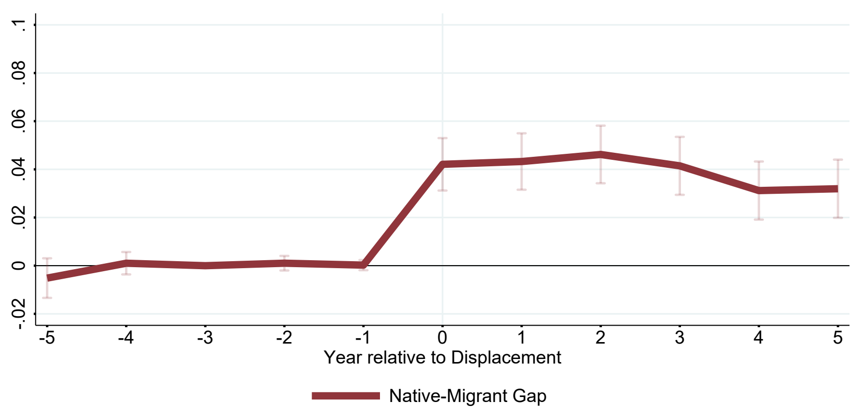
(a) All



(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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

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 specification that we use to create Figure B1.

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 *Auslaenderbehoerde* (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 *Vorrangpruefung*).

Before January 1, 2005, migrants were subject to the *Auslaendergesetz* 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 insurance type I)* benefits. Individuals in the ALG I scheme receive 60% (or 67% if there are kids 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.

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 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 B12 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. (2023). These data cover the years 1985-2021 and contain both worker and establishment fixed effects averaged over sub-periods of

7 years each: [1985-1992; 1993-1999; 2000-2006; 2007-2013; 2014-2021]. We can use a unique worker or establishment ID to link these data to our baseline sample. In general, we proceed as follows: If a worker works for establishment A in 1998, we assign him the establishment fixed effect for the given establishment that is available for the year range 1993-1999. If she switches to establishment B in 2001, we assign him the establishment fixed effect for the respective establishment in the year range 2000-2006.

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, they 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 *Jobseeker History Panel*, which is an administrative dataset provided by the IAB. We use the versions *ASU V06.11.00-201904* and *XASU V02.03.00-201904*. These data are based on the information the caseworker enters into the Federal Employment Agency's online system once the 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 5 plot the migrant-native gap in employment and wage losses by quartiles of labor market thickness. We follow Jäger and Heining (2022) 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

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, district, 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 regression specification separately for migrants and natives:

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

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 $t = -3$. The coefficients α_j present the evolution of displaced workers' labor market outcomes relative to the non-displaced control group. We add year-fixed effects π_t, individual fixed effects γ_i, and time-varying age polynomials X_{it} and we cluster standard errors at the worker level.

Figure B1 presents the results. It shows that both migrants and natives have substantial costs of job displacement: In the year immediately after displacement, displaced natives are about 30 percentage points less likely to be employed relative to the non-displaced control group (Panel a). At about 45 percentage points this gap is significantly larger for migrants (Panel b). Both natives and migrants catch up with their non-displaced controls over time, migrants less so than natives.

Panels (c) and (d) show a similar picture for log daily wages. Re-employed displaced natives earn around 19 log points less than non-displaced natives; while this difference closes somewhat over time, it remains significant at 9 log points 5 years out. Displaced migrants lose even more - the difference to their non-displaced controls is around 40 log points in t=0, and 19 log 5 years out.

When drawing conclusions from Figure B1, it is important to keep in mind that in contrast to our main analysis, this analysis does not take into account 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 into occupations.

B Appendix Tables and Figures

Table B1: Distribution across 1-Digit Industries of Displaced Workers vs. A Random Sample in t=-1

	(1) All Workers Migrants	(2) Baseline Sample Migrants	(3) All Workers Natives	(4) Baseline Sample Natives
1-Digit Industries				
Agriculture	0.014	0.00045	0.0073	0.00045
	[0.12]	[0.021]	[0.085]	[0.021]
Mining, Energy	0.0035	0.036	0.010	0.036
	[0.059]	[0.19]	[0.10]	[0.19]
Food Manufacturing	0.020	0.074	0.021	0.074
	[0.14]	[0.26]	[0.14]	[0.26]
Consumption Goods	0.023	0.11	0.032	0.11
	[0.15]	[0.32]	[0.18]	[0.32]
Production Goods	0.043	0.11	0.040	0.11
	[0.20]	[0.31]	[0.20]	[0.31]
Investment Goods	0.090	0.16	0.091	0.16
	[0.29]	[0.37]	[0.29]	[0.37]
Construction	0.029	0.038	0.041	0.038
	[0.17]	[0.19]	[0.20]	[0.19]
Retail	0.088	0.14	0.14	0.14
	[0.28]	[0.34]	[0.34]	[0.34]
Traffic, Telecommunication	0.041	0.074	0.047	0.074
	[0.20]	[0.26]	[0.21]	[0.26]
Credit, Insurance	0.0084	0.0083	0.030	0.0083
	[0.091]	[0.091]	[0.17]	[0.091]
Restaurants	0.079	0.015	0.030	0.015
	[0.27]	[0.12]	[0.17]	[0.12]
Education	0.019	0.0042	0.033	0.0042
	[0.14]	[0.064]	[0.18]	[0.064]
Health	0.048	0.013	0.097	0.013
	[0.21]	[0.11]	[0.30]	[0.11]
Commercial Services	0.15	0.18	0.13	0.18
	[0.36]	[0.38]	[0.33]	[0.38]
Other Services	0.032	0.025	0.036	0.025
	[0.18]	[0.16]	[0.19]	[0.16]
Non-Profit	0.0084	0.0086	0.016	0.0086
	[0.091]	[0.092]	[0.13]	[0.092]
Public Administration	0.011	0.0061	0.049	0.0061
	[0.10]	[0.078]	[0.22]	[0.078]
Number of Observations	774921	15638	8062766	15638

Notes: This table presents differences in the distribution across 1-digit industries for our baseline sample of displaced migrants and natives compared to a random sample of workers. Columns (1) and (3) show characteristics of a random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent all displaced migrants and natives in the baseline sample. We report displaced workers' characteristics in t=-1 (pooling baseline years 2000-2010). Standard deviations in brackets.

Table B2: Distribution across 1-Digit Occupations of Displaced Workers vs. A Random Sample in t=-1

	(1) All Workers Migrants	(2) Baseline Sample Migrants	(3) All Workers Natives	(4) Baseline Sample Natives
1-Digit Occupations				
Agriculture, Gardening, Work with Animals	0.020	0.0043	0.015	0.0043
	[0.14]	[0.065]	[0.12]	[0.066]
Simple, Manual Tasks	0.18	0.43	0.10	0.41
	[0.38]	[0.49]	[0.30]	[0.49]
Qualified, Manual Tasks	0.12	0.17	0.12	0.18
	[0.32]	[0.37]	[0.33]	[0.39]
Technician	0.013	0.030	0.039	0.035
	[0.11]	[0.17]	[0.19]	[0.18]
Engineer	0.013	0.015	0.023	0.015
	[0.12]	[0.12]	[0.15]	[0.12]
Simple Services	0.22	0.18	0.15	0.18
	[0.42]	[0.38]	[0.35]	[0.38]
Qualified Commercial and Administrative Tasks	0.065	0.098	0.19	0.098
	[0.25]	[0.30]	[0.39]	[0.30]
Manager	0.010	0.0083	0.024	0.0083
	[0.100]	[0.091]	[0.15]	[0.091]
Not Classified	0.015	0	0.016	0
	[0.12]	[0]	[0.13]	[0]
Number of Observations	774921	15638	8062766	15638

Notes: This table presents differences in the distribution across 1-digit occupations as defined by Blossfeld (1987) for our baseline sample of displaced migrants and natives compared to a random sample of workers. Columns (1) and (3) show characteristics of a random sample of migrants and natives in Germany 2000-2010, respectively. Columns (2) and (4) represent all displaced migrants and natives in the baseline sample. 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	37.7	38.4	38.0	39.2
	[6.94]	[7.39]	[6.84]	[7.27]
Tenure	6.47	6.16	6.44	6.28
	[2.55]	[2.43]	[2.55]	[2.44]
Real Daily Wage	88.8	90.1	91.2	92.4
	[28.9]	[33.5]	[30.9]	[34.4]
Total Yearly Earnings	33319.5	34510.0	35506.1	36254.5
	[30434.4]	[35855.4]	[36577.0]	[40798.1]
Days per Year Working	339.7	322.4	341.4	328.5
	[50.2]	[63.7]	[48.0]	[59.5]
Panel B: Regional Characteristics				
Lives in City	0.67	0.62	0.62	0.61
	[0.47]	[0.49]	[0.49]	[0.49]
Lives 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]
Displaced from Complete Closure	0.33	0.29	0.33	0.29
	[0.47]	[0.46]	[0.47]	[0.45]
Number of Observations	10862	4776	12094	3544

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. Standard deviations in brackets.

Table B4: 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: Log Earnings						
Migrant	-0.20	-0.16	-0.17	-0.19	-0.18	-0.19
	(0.014)**	(0.019)**	(0.017)**	(0.018)**	(0.019)**	(0.019)**
Observations	13619	18743	15996	15664	14079	14074
$\Delta y_{native,p}$	-.74	-.631	-.681	-.722	-.722	-.722
	(.009)	(.008)	(.008)	(.008)	(.009)	(.009)
Panel B: Log Wages						
Migrant	-0.12	-0.098	-0.11	-0.12	-0.11	-0.12
	(0.011)**	(0.015)**	(0.014)**	(0.014)**	(0.014)**	(0.015)**
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
	(0.0035)**	(0.0041)**	(0.0037)**	(0.0041)**	(0.0044)**	(0.0044)**
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: Leaves the Admin Data						
Migrant	0.036	0.033	0.035	0.035	0.035	0.035
	(0.0031)**	(0.0032)**	(0.0035)**	(0.0035)**	(0.0038)**	(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 within-matched-pair difference post- vs. pre-layoff, corresponding to the term defined in Equation 2. $\Delta y_{native,p}$ 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. We cluster standard errors at the displacement establishment level (constant within matched worker pairs). * and ** correspond to 5 and 1 percent significance levels, respectively. Workers in our sample are displaced from 2001-2011, and they are observed from 1996 to 2017.

Table B5: 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: Log Earnings						
Migrant	-0.20	-0.23	-0.22	-0.20	-0.18	-0.23
	(0.014)**	(0.018)**	(0.022)**	(0.021)**	(0.016)**	(0.026)**
Observations	13619	10092	12242	12928	12884	6343
$\Delta y_{native,p}$	-.74	-.777	-.797	-.747	-.718	-.827
	(.009)	(.011)	(.009)	(.009)	(.009)	(.013)
Panel B: Log Wages						
Migrant	-0.12	-0.15	-0.14	-0.13	-0.11	-0.14
	(0.011)**	(0.014)**	(0.017)**	(0.017)**	(0.013)**	(0.021)**
Observations	12395	9159	11176	11765	11766	5656
$\Delta y_{native,p}$	-.389	-.42	-.434	-.393	-.377	-.441
	(.007)	(.008)	(.007)	(.007)	(.007)	(.01)
Panel C: Unemployment						
Migrant	0.042	0.061	0.047	0.043	0.039	0.053
	(0.0035)**	(0.0044)**	(0.0048)**	(0.0044)**	(0.0036)**	(0.0067)**
Observations	15638	11415	14096	14864	14746	7448
$\Delta y_{native,p}$	.227	.251	.241	.228	.219	.269
	(.002)	(.003)	(.002)	(.002)	(.002)	(.004)
Panel D: Leaves the admin data						
Migrant	0.036	0.018	0.032	0.038	0.036	0.037
	(0.0031)**	(0.0028)**	(0.0040)**	(0.0039)**	(0.0040)**	(0.0045)**
Observations	15638	11415	14096	14864	14746	7448
$\Delta y_{native,p}$	.1	.07	.09	.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 within-matched-pair difference post- vs. pre-layoff, corresponding to the term defined in Equation 2. $\Delta y_{native,p}$ 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 2001-2004 (and thus well before the financial crisis). Unemployment is defined as being a UI benefit recipient or a training program participant. We cluster standard errors at the displacement establishment level (constant within matched worker pairs). * 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 B6: 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	-0.12	-0.12	-0.12	-0.12
	(0.016)***	(0.015)***	(0.016)***	(0.015)***
$\Omega_{pt,all}$		0.011		
		(0.0044)**		
Migrant × $\Omega_{pt,all}$		0.024		
		(0.0098)**		
$\Omega_{pt,migrant}$			0.0021	
			(0.0034)	
Migrant × $\Omega_{pt,migrant}$			0.017	
			(0.0076)**	
$\Omega_{pt,native}$				0.0027
				(0.0024)
Migrant × $\Omega_{pt,native}$				0.038
				(0.011)***
Observations	19559	19559	19559	19559
Mean Dep. Var Natives	-.363	-.363	-.363	-.363
	(.005)	(.005)	(.005)	(.005)
Panel B: Log Wages - Non-Comovers				
Migrant	-0.11	-0.11	-0.11	-0.11
	(0.025)***	(0.025)***	(0.025)***	(0.025)***
$\Omega_{pt,all}$		0.011		
		(0.013)		
Migrant × $\Omega_{pt,all}$		-0.0061		
		(0.013)		
$\Omega_{pt,migrant}$			-0.0019	
			(0.016)	
Migrant × $\Omega_{pt,migrant}$			-0.0023	
			(0.022)	
$\Omega_{pt,native}$				0.010
				(0.013)
Migrant × $\Omega_{pt,native}$				-0.0087
				(0.013)
Observations	2913	2913	2913	2913
Mean Dep. Var Natives	-.132	-.132	-.132	-.132
	(.01)	(.01)	(.01)	(.01)

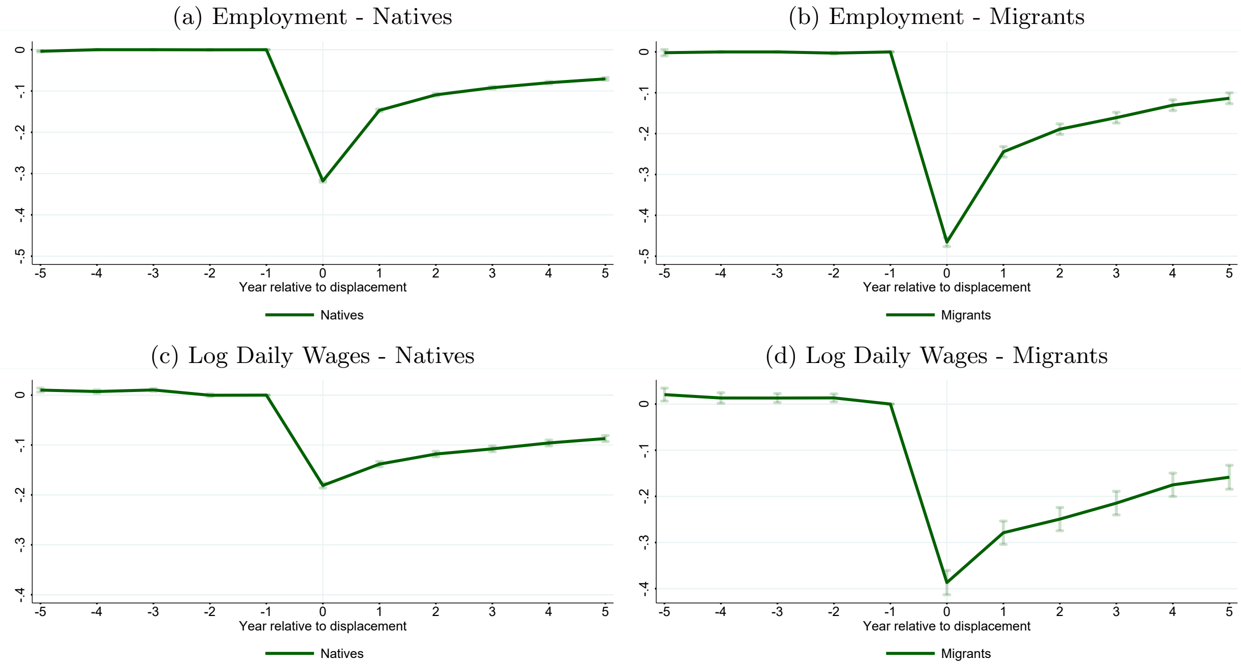
Notes: This table presents γ, α and β coefficients from regression equation 6. We restrict the sample to matched worker pairs for which all 3 network measures are defined. For a given matched pair p, $\Omega_{p,t=0}$ reports our (standardized) proxy of coworker networks. Panel A conditions on comovers, 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. 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 from 2001-2011, and they are observed from 1996 to 2017.

Table B7: 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 Jugoslavia
		Bosnia and Herzegovina	Northmazedonia
		Bulgaria	Mazedonia
		Kosovo	Romania
		Croatia	Serbia
5	Turkey	Turkey	
6	Former USSR	Armenia	Lithuania
		Azerbaijan	Moldova
		Belarus	Russian Federation
		Estonia	Tajikistan
		Georgia	Turkmenistan
		Kazakhstan	Ukraine
		Kyrgyzstan	Uzbekistan
		Latvia	
7	Asia and Middle East		
8	Africa		
9	Central and South America		
10	Other		

Notes: This table shows how we assign migrants to origin groups following Battisti 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



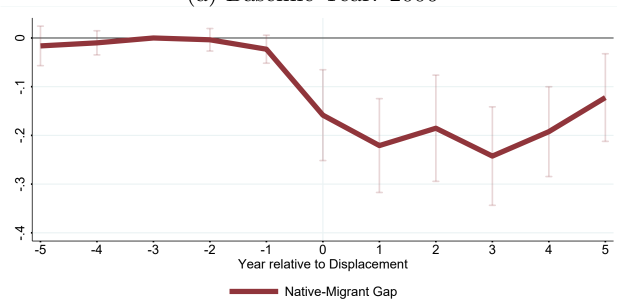
Notes: This figure plots event study coefficients for an alternative analysis where we match displaced migrants and natives *separately* to similar non-displaced workers. Panel (a) plots the difference in employment for displaced vs. non-displaced native workers. Panel (b) plots the difference in employment for displaced vs. non-displaced migrant workers. Panels (c) and (d) plot log wages for displaced vs. non-displaced natives and migrants, respectively. See Section A.4 for a description of the matching algorithm. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B2: The Migrant-Native Earnings Gap by Baseline Year

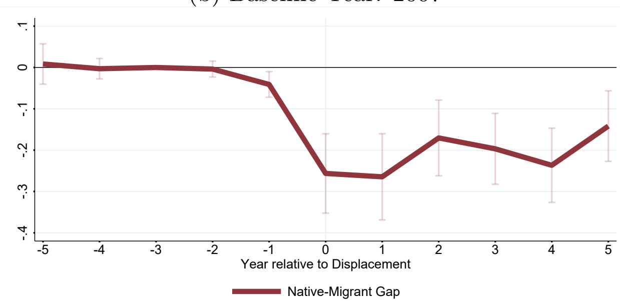
Notes: This figure plots the α_j coefficients from regression equation 1 for log earnings. In each Panel, we restrict the sample to matched pairs laid-off in a different baseline year. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level.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

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

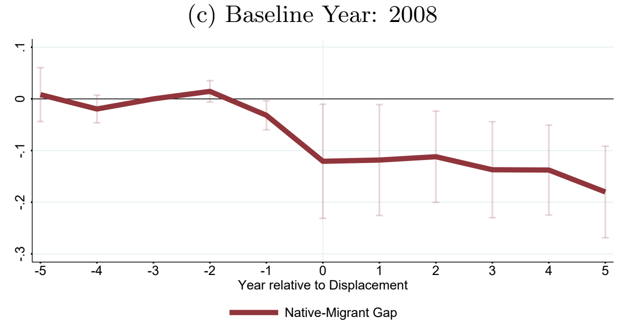
(a) Baseline Year: 2006



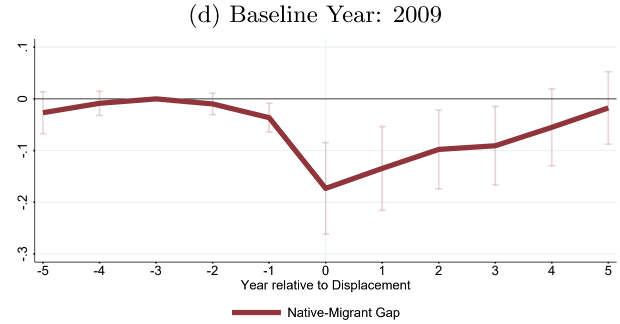
(b) Baseline Year: 2007



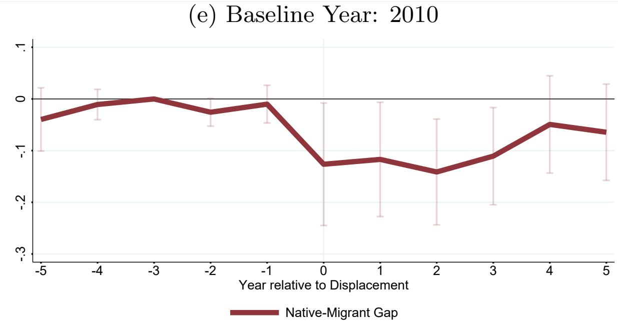
(c) Baseline Year: 2008



(d) Baseline Year: 2009



(e) Baseline Year: 2010



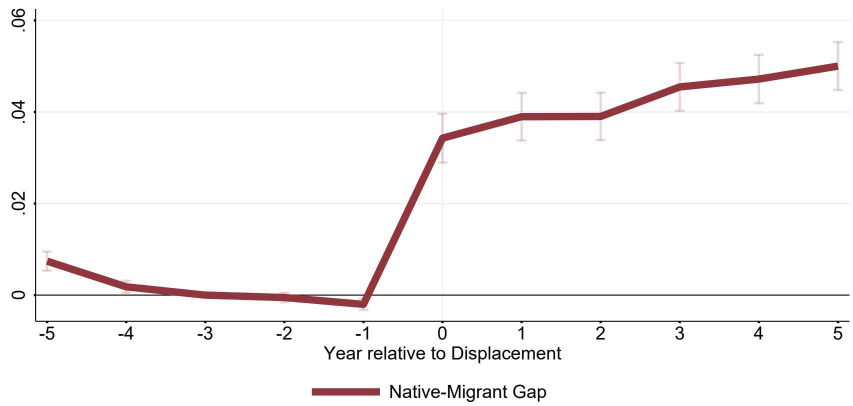
Notes: This figure plots the α_j coefficients from regression equation 1 for log earnings. In each Panel, we restrict the sample to matched pairs laid-off in a different baseline year. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level.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

Figure B4: Main Results - Long-Term

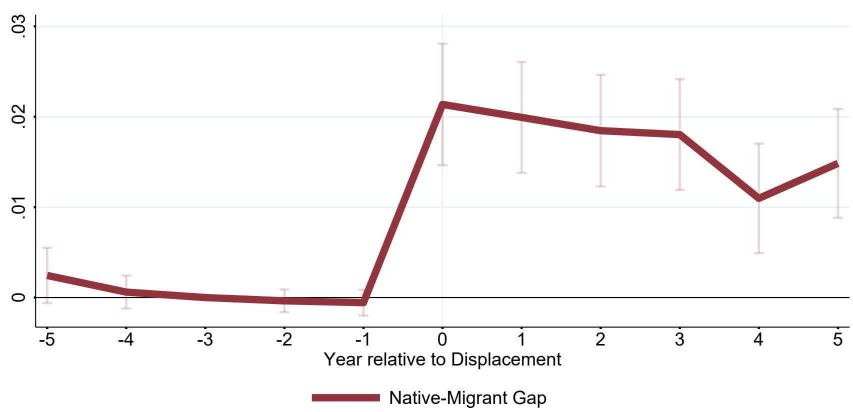
Notes: This figure plots the α_j coefficients from regression equation 1 for our main outcome variables and for all layoffs for which we observe up to 10 years post-event (i.e., all layoffs up to the baseline year 2006). In Panel (a), the outcome variable is log earnings. In Panel (b), the outcome variable is employment. Panels (c) and (d) plot wages and AKM establishment FE as provided by Lochner et al. (2023), 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. Vertical bars indicate the estimated 95% confidence interval based on standard errors clustered at the individual level. Workers in our sample are displaced from 2001-2006, and they are observed from 1997-2016.

Figure B5: Establishment Characteristics

(a) Share of Migrant Workers



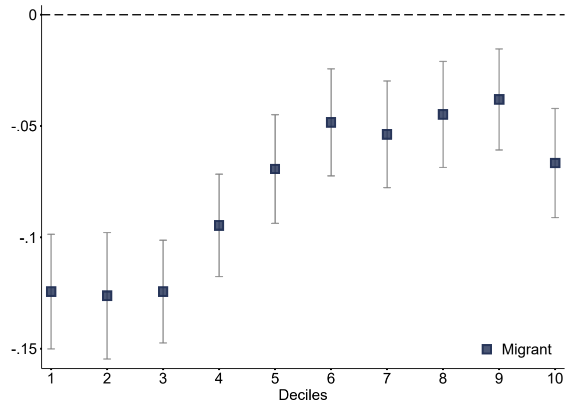
(b) Share of Workers in Minijob



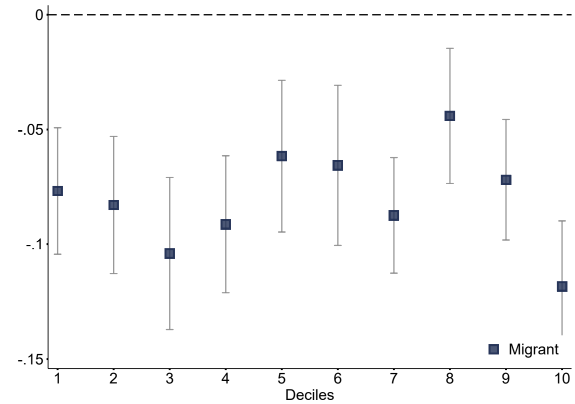
Notes: This figure plots the α_j coefficients from regression equation 1 for establishment sorting. In Panel (a), the outcome variable is the leave-one-out share of migrant workers. In Panel (b), the outcome variable is the leave-one-out share of workers in a minijob. Minijobs, or marginal employment, 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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B6: Migrant-Native Gaps by Pre-Displacement AKM Worker FE and Origin Country Net Income - Additional Outcomes

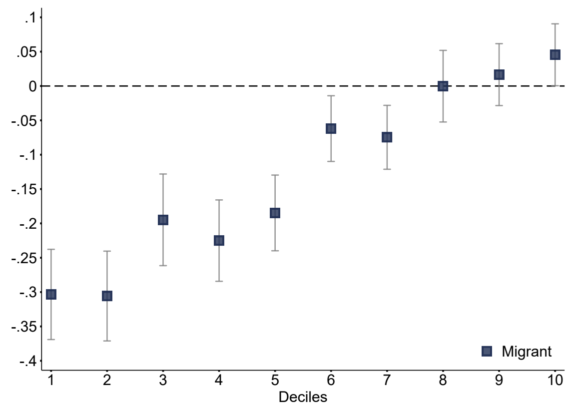
(a) Employment - AKM Worker FE



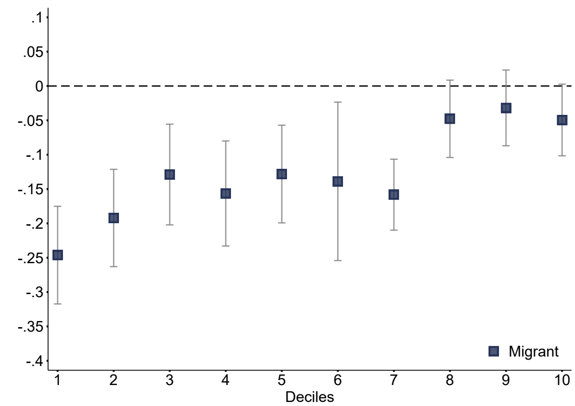
(b) Employment - Origin Country Net Income



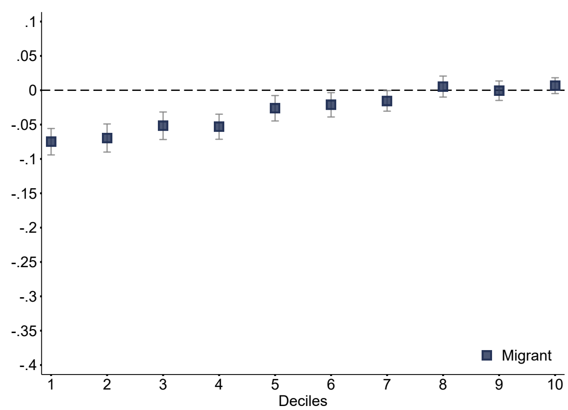
(c) Log Daily Wages - AKM Worker FE



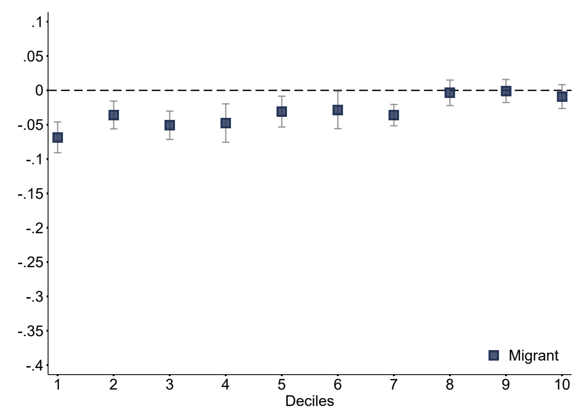
(d) Log Daily Wages - Origin Country Net Income



(e) AKM Establishment FE - AKM Worker FE

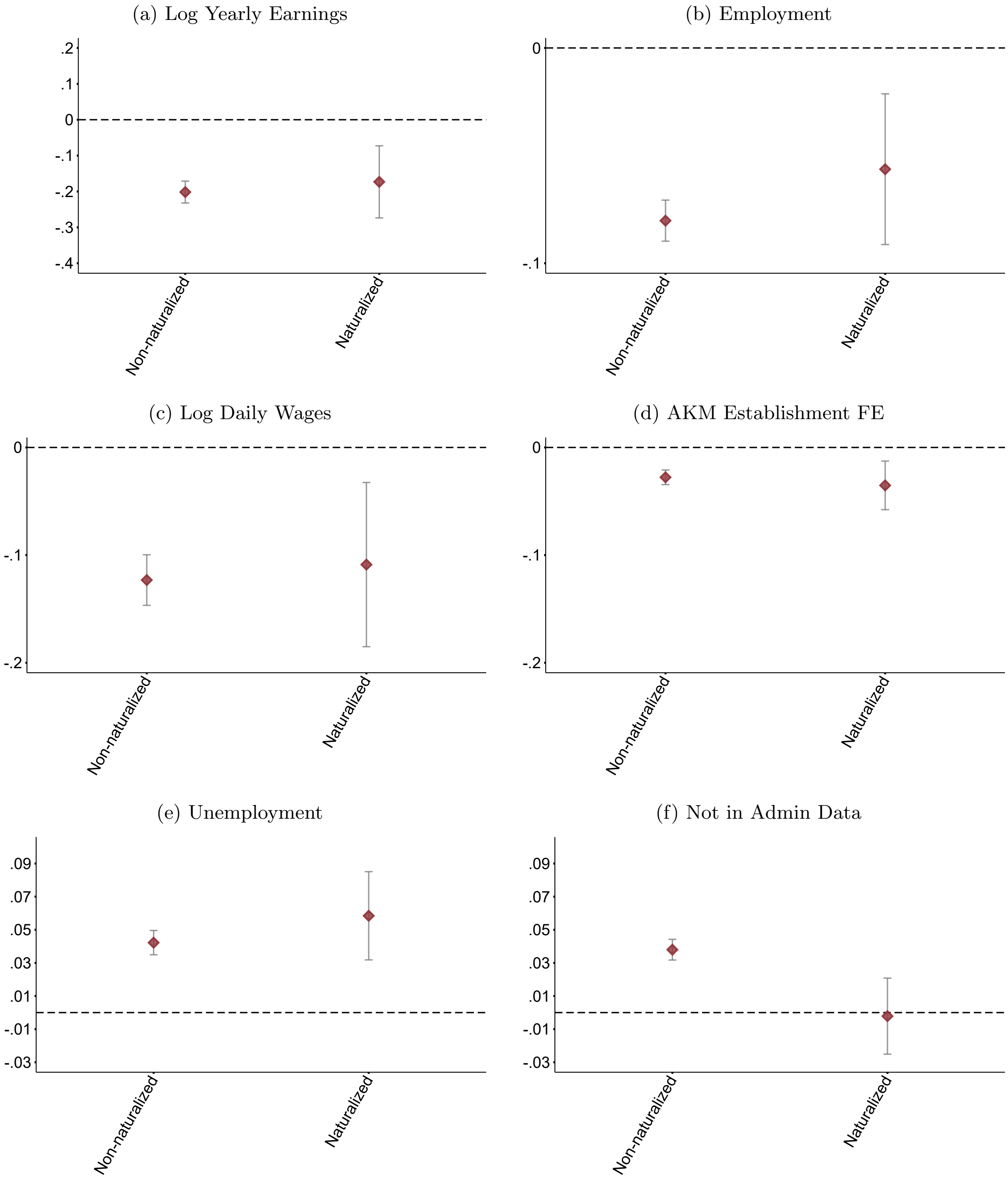


(f) AKM Establishment FE - Origin Country Net Income



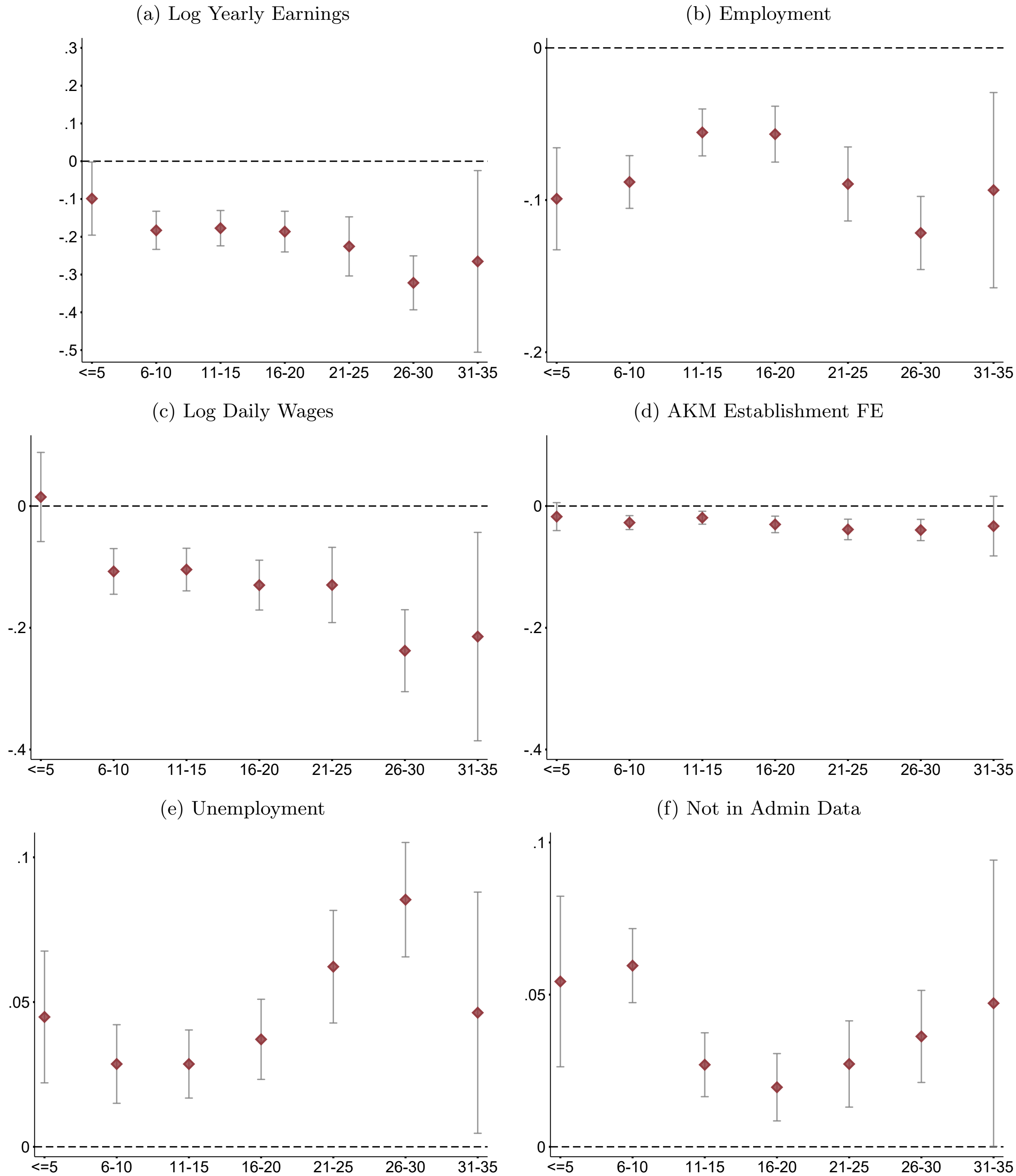
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by migrants' decile of pre-displacement AKM worker FE (Panels a, c, e) and origin country net income (Panels b, d, f), all measured in t=-1. We use the AKM worker FE measure provided by Lochner et al. (2023) and 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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B7: Migrant-Native Gap by Naturalization



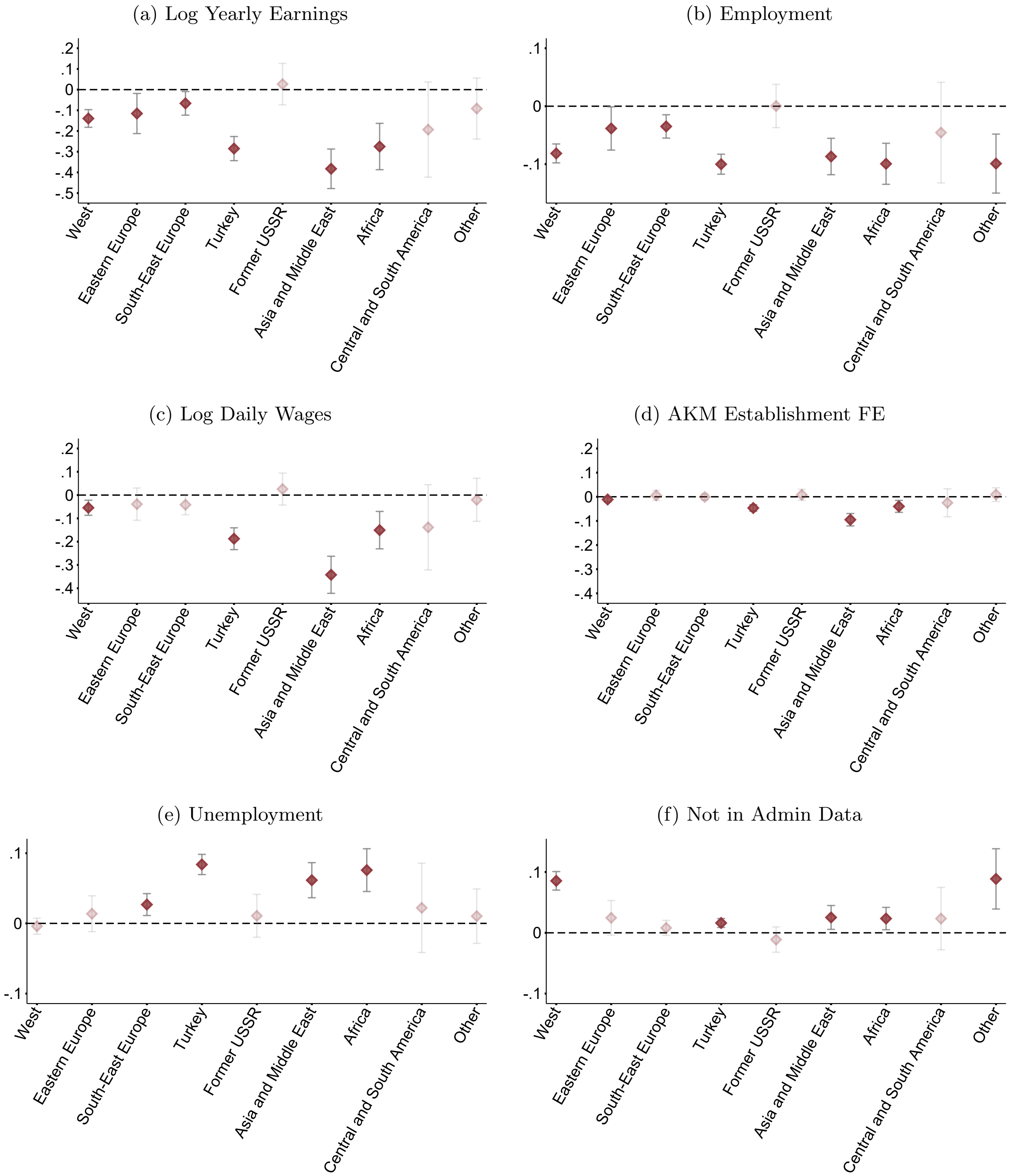
Notes: This figure shows how the migrant-native gap in costs of job displacement differs depending on whether individuals have acquired German citizenship by the time of layoff. We classify migrants as naturalized if they had non-German citizenship in their first social-security record, and German citizenship 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 log yearly earnings, 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B8: Migrant-Native Gap by Years in Admin Data



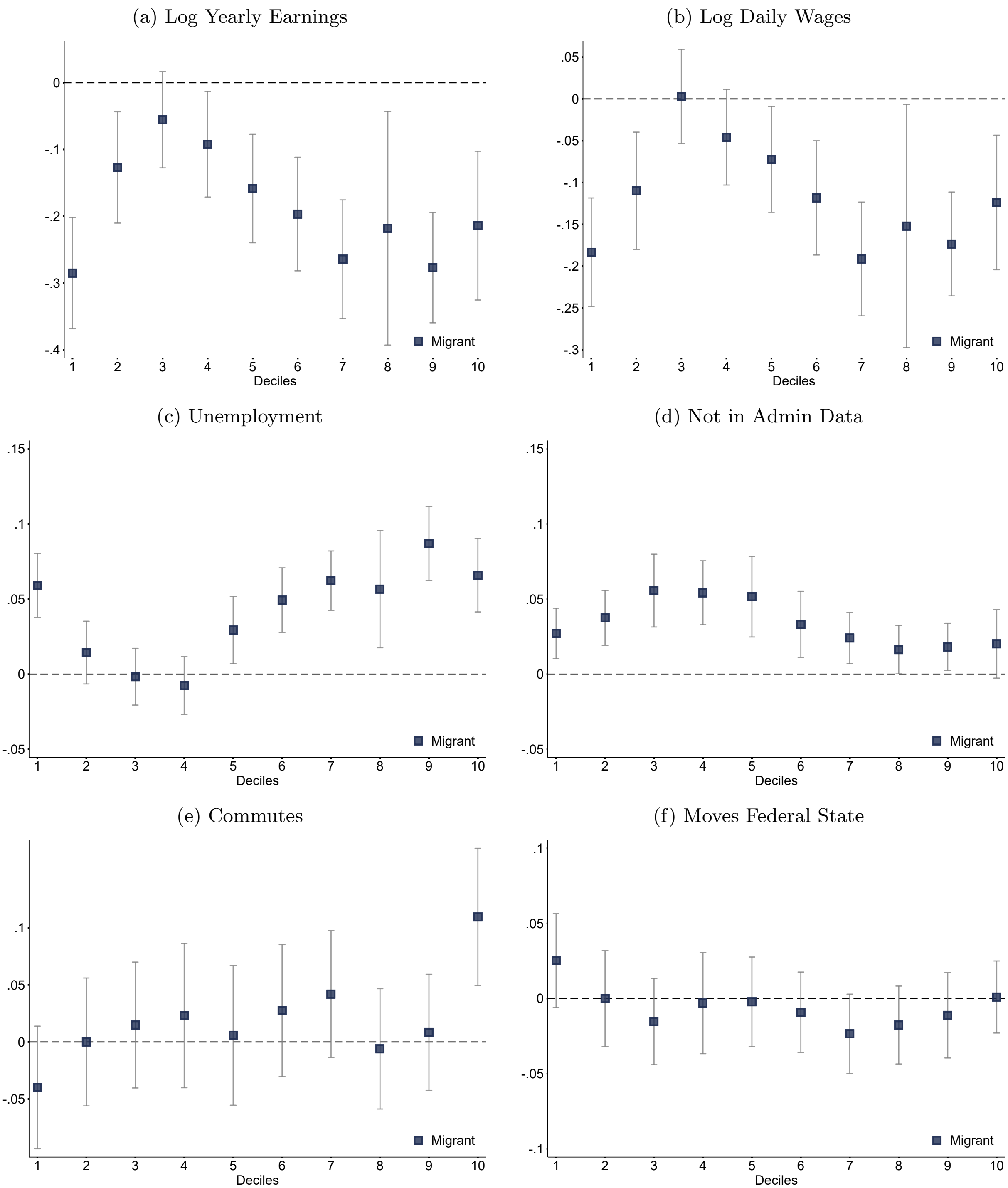
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by the number of years since the first record in the German admin data, measured at t=-1. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 7 categories. All regressions control for origin group and AKM worker FE at baseline. Panel (a) reports log yearly earnings, 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B9: Migrant-Native Gap by Origin Group



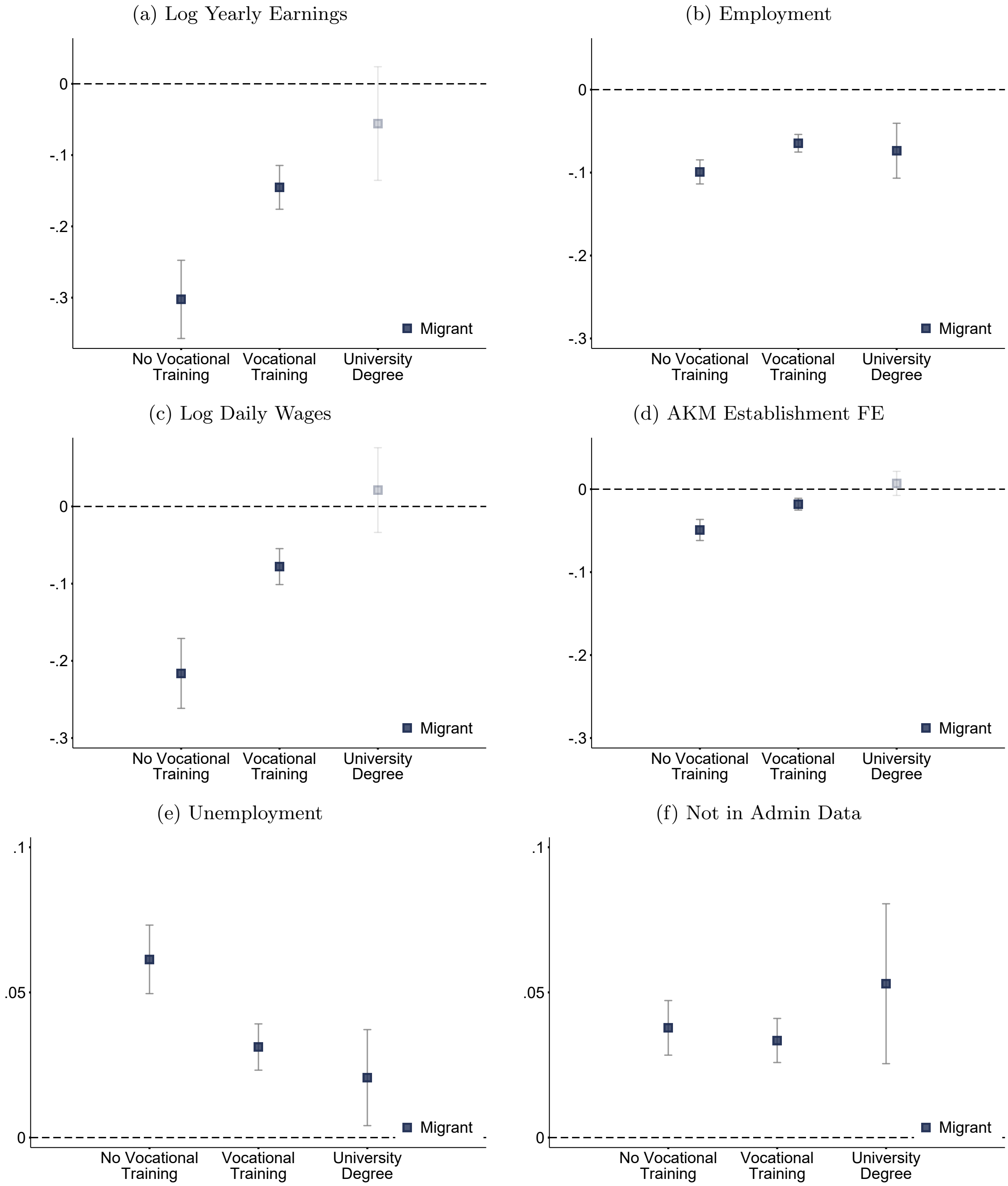
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by origin group measured in the first admin data record, as defined by Battisti et al. (2022). See Table B7 for details. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 9 groups. All regressions control for AKM worker FE at baseline. Panel (a) reports log yearly earnings, 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B10: Migrant-Native Gap by County Share of Same-Nationality Working Age Population



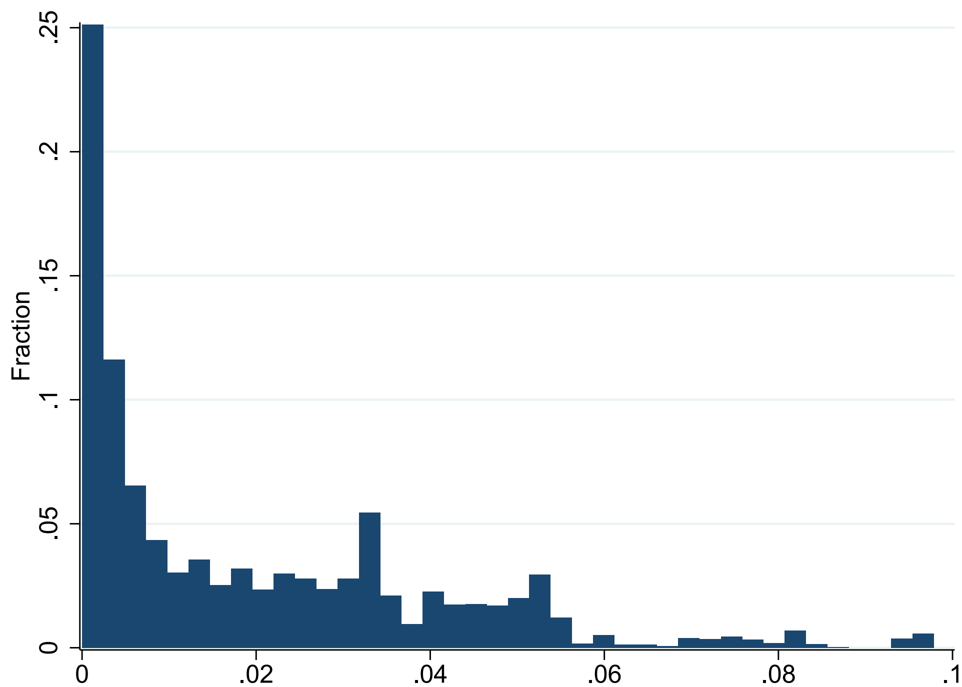
Notes: This figure shows how the migrant-native gap in costs of job displacement differs by deciles of the county share of same-nationality working age population, measured at t=-1. Each panel plots the δ coefficients from a variation of Equation 4 where we regress the match-specific diff-in-diff outcome on dummies for the 10 deciles. Panel (a) reports log yearly earnings, 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B11: 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 log yearly earnings, 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 displacement establishment level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure B12: Distribution Share Same-Nationality Working Age Population in County in t=-1

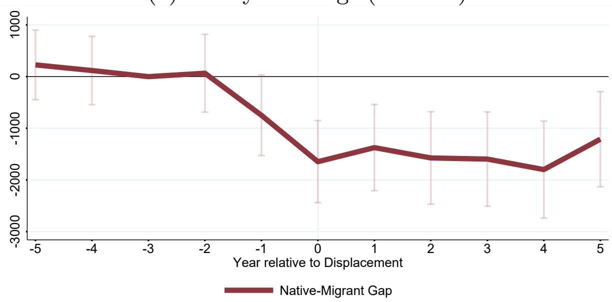


Notes: This histogram shows the distribution of the share of same-nationality working-age population in a county at $t = -1$ for our sample of displaced migrants. Workers in our sample are displaced in the period 2001-2011, and they are observed from 1996 to 2017. Data source: Destatis (2019).

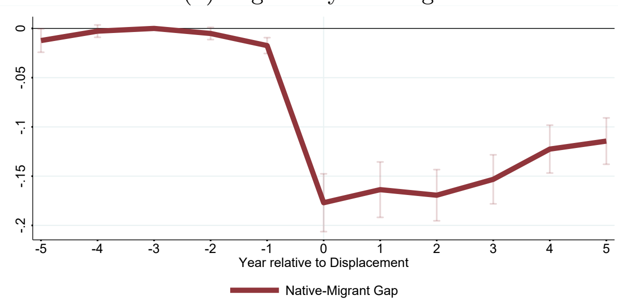
C Replication of Main Results for a Balanced Panel of Workers

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

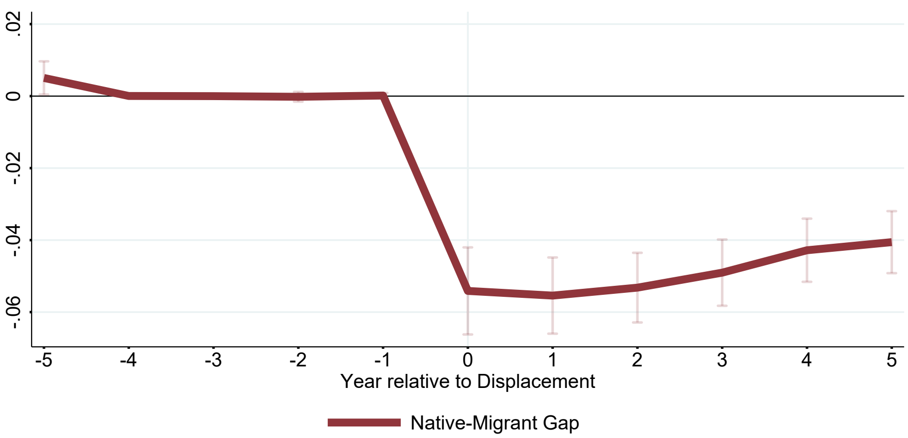
(a) Yearly Earnings (in EUR)



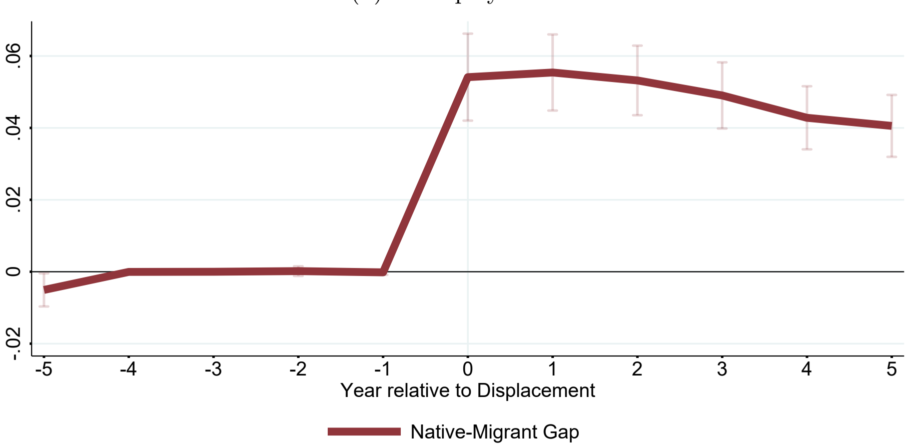
(b) Log Yearly Earnings



(c) Employment

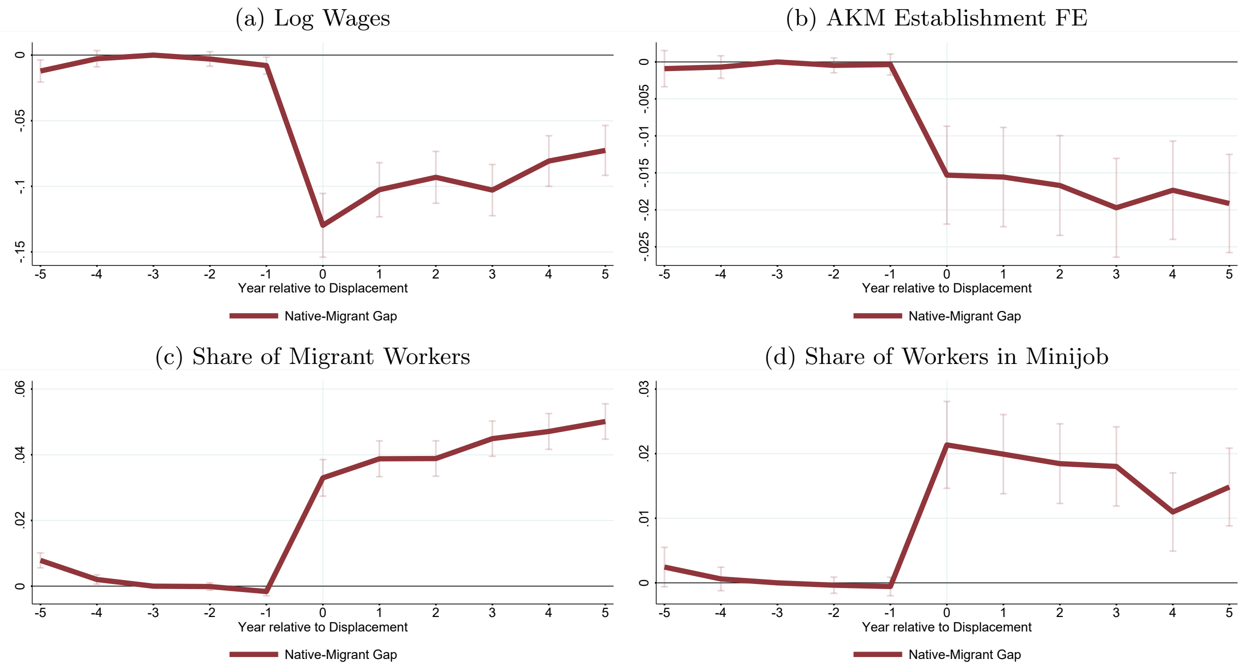


(d) Unemployment



Notes: This figure plots the α_j coefficients from regression equation 1 for total yearly earnings (Panel a), log yearly earnings (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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.

Figure C2: 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. (2023). 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 individual level. Workers in our sample are displaced from 2001-2011, and they are observed from 1997-2016.