The Power to Discriminate

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May 2026

Abstract

Economic theory has long linked employer power to discrimination, but theory and empirical applications have seldom considered which form of power matters. We distinguish between labor market and product market power and design our study to isolate the role each plays in allowing discrimination to persist. Our setting leverages job displacements from mass layoffs and firm closures as a source of exogenous job search, combined with exact matching of native–immigrant worker pairs who held the same job at the same firm, in the same occupation, industry, location, tenure, and wage prior to displacement. By tracking post-displacement outcomes across labor markets with differing levels of employer concentration, we identify how labor market power affects discriminatory hiring and wage-setting. We find that wage and employment discrimination against immigrants is amplified in concentrated labor markets and largely absent in highly competitive ones. By contrast, product market power has little independent relationship with these gaps once labor market concentration is accounted for. Earnings gaps also narrow over time following re-employment, particularly within firms, consistent with employer learning and belief-based mechanisms. Together, the results suggest that discriminatory outcomes are closely linked to labor market structure and employer wage-setting power, with implications for theories of discrimination and labor market competition.

JEL Codes: J7; J61; J42; J63
Keywords: Discrimination; Immigration; Market Power

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1 Introduction

Employer power and labor market discrimination are two central issues in economics, with widespread implications for economic efficiency, equity, and the trade-off between the two (e.g., Smith (1776); Robinson (1933); Becker (1957); Arrow (1973); Card (2022)). These two phenomena are also closely linked, as non-competitive market forces and the existence of supernormal profits have long been understood as essential conditions for firms' ability to sustain discriminatory practices over time (e.g., Becker (1957)).

In Becker's (1957) canonical framework, discrimination is costly: firms that discriminate sacrifice profits relative to firms that hire and compensate workers according to productivity. Discriminatory behavior can therefore persist only insofar as firms earn supernormal profits that can absorb these costs. Since Becker develops this argument under the assumption of competitive labor markets, these rents originate from the product market. Competition disciplines discrimination by eroding product market rents through entry, substitution, and profit pressure. Consistent with this mechanism, the empirical literature examining whether competition disciplines discrimination has operationalized competition through variation in *product market* conditions—such as deregulation, entry, or industry concentration (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)).

Robinson's (1933) monopsony framework highlights why this omitted margin may matter. In Robinson's framework, firms can depart from competitive wages only when they possess labor market power. In competitive labor markets, firms must pay workers their marginal revenue product regardless of their pricing power in output markets. This distinction suggests that the ability to sustain discriminatory pay may depend not only on whether firms earn rents, but also on whether they possess wage-setting power in local labor markets. While product market power may generate profits, wage discrimination requires the ability to set wages below competitive levels. The relative importance of product market rents and labor market power in sustaining discriminatory outcomes therefore remains empirically unresolved. This distinction is especially relevant in today's labor market, where employer labor market concentration is rising, worker bargaining power is declining (e.g., Stansbury and Summers (2020); Azar et al. (2020b)), and wage gaps between natives and immigrants remain persistent (e.g., Amo-Agyei et al. (2020)). Understanding how labor market power shapes discrimination is thus critical for both equity and efficiency.

We provide new causal evidence on how labor market power shapes employer discrimination. Using linked employer-employee register data from Norway, we study workers induced to search for new jobs following involuntary displacement events caused by mass layoffs and firm closures—events entirely outside workers' control. Our empirical design compares the post-displacement labor market outcomes of native and immigrant workers who were previously employed in the same firm, occupation, industry, local labor market, and who earned the same wage

prior to displacement. We then examine whether the resulting immigrant-native gaps in employment and earnings differ systematically across labor markets with different levels of employer concentration. This setting allows us to study how employers respond to workers with observationally identical pre-search labor market histories and productivity signals, but different migration backgrounds: native Norwegians and Western immigrants (the in-group) versus non-Western immigrants ("NWIs", the out-group).

We measure labor market power using the Herfindahl–Hirschman Index (HHI) of employment within commuting-zone-by-occupation cells in the year prior to displacement.[1] We estimate a triple-differences framework that compares immigrant-native gaps following displacement-induced job search across labor markets with different levels of concentration. Our interest lies not in the displacement event itself, but in the search process it triggers, which allows us to observe employer behavior toward otherwise similar workers in markets with differing degrees of employer power. To validate our use of HHI as a proxy for monopsony power, we also replicate the analysis using wage markdowns estimated from production functions following Yeh et al. (2022).

To illustrate our identification strategy, consider two equally qualified native–immigrant worker pairs—both employed in the same occupation at the same firm and earning the same wage. Following a mass layoff or firm closure, both pairs are exogenously forced to search for new jobs, but they search in local labor markets that differ in employer concentration. In the concentrated market, a small number of firms control most employment opportunities, allowing employers with wage-setting power and supernormal profits to sustain discriminatory behavior—whether driven by biased preferences or inaccurate beliefs. In the competitive market, firms face tighter constraints: profits are competed away, and discriminatory behavior becomes more costly to sustain.[2]

By comparing post-displacement outcomes across these settings, we examine whether immigrant-native gaps widen systematically with labor market concentration. This stylized example captures the core logic of our identification strategy: conditional on similar pre-search productivity, exogenous separation, and comparable search conditions, systematic differences in post-displacement outcomes are difficult to reconcile with supply-side explanations alone and instead point toward differential employer treatment, following the logic of Fryer et al. (2013). In the absence of discrimination, wages should be positively related to a worker's marginal productivity. To the extent that discrimination was already reflected in pre-displacement wages, our estimates understate the true productivity of non-Western immigrants, implying that our results should be interpreted as a lower bound on employer discrimination (e.g., Bohren et al. (2022)).[3]

[1] There are 160 commuting zones in Norway.

[2] In a finite labor pool and under concave marginal productivity, excluding productive minority workers reduces firm output. Non-discriminatory firms gain a competitive edge and displace firms that discriminate.

[3] One possibility is that pre-layoff wages partly reflect favorable immigrant–firm match effects rather than persistent worker productivity. In that case, layoffs would destroy these match-specific rents, and immigrants could appear underpaid relative to observationally similar natives after displacement for reasons unrelated to discrimination. However, our empirical tests of firm-specific match effects in Section 6 suggest that, in more concentrated markets, implied

After establishing the relationship between labor market power and employer discrimination, we extend our framework to incorporate product market power, measured as industry revenue concentration. This matters because product market power can also enable firms to absorb the costs of discrimination through supernormal profits (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)). However, product market power alone may not be sufficient to sustain wage discrimination if the labor market remains competitive. Without control over the labor market, firms face a highly elastic labor supply and must pay workers their marginal product, otherwise workers leave for better-paying opportunities. Still, firms may engage in employment discrimination—favoring less productive workers from preferred groups–so long as their product market rents can cover the efficiency losses. Identifying which type of power drives discrimination is important not only for policy but also for economic theory: it speaks directly to the long-standing prediction that some form of market power is a precondition for sustained discrimination and suggests that wage-setting power in labor markets may play a more central role than product market pricing power in sustaining discriminatory wage-setting. This distinction calls for different policy tools such as strengthening labor mobility, collective bargaining, or antitrust in labor markets if labor market power is the driver, and also highlights the importance of distinguishing between different sources of market power in models of employer discrimination.

Finally, we investigate the sources of discrimination underlying our results, focusing on the relative importance of belief-based and preference-based mechanisms. Theoretically, competition can reduce both forms of discrimination because each imposes costs on firms that become harder to sustain as supernormal profits are competed away. However, the mechanisms through which competition operates differ. For preference-based discrimination, competition increases the cost of maintaining animus toward members of the out-group: firms that hire based on bias rather than merit weaken their ability to attract and retain talent, and in doing so, their ability to compete and survive. For belief-based discrimination, competition penalizes firms for relying on group-based heuristics when assessing productivity. As the cost of misjudging candidates rises, firms are incentivized to invest in more precise information about individual workers. This shifts hiring decisions away from uncertainty and toward more accurate, individualized assessments of productivity.

To explore this question, we borrow insights from Bohren et al. (2019) and analyze the dynamic adjustment process following displacement-induced search episodes. If our findings are driven by belief-based discrimination, ongoing employer interactions with workers from disfavored groups should directly help correct inaccurate productivity beliefs and reduce the cost of acquiring more accurate productivity information about the individual worker. This would gradually eliminate earnings and employment gaps, even in the presence of labor market power. If discrimination stems from fixed preferences (i.e., taste-based discrimination), these gaps are more likely to persist.

match effects move in the *opposite* direction of the HHI gradient we estimate. This both helps assuage concerns that match effects drive our results and implies that our estimates likely represent lower bounds.

To perform our analysis, we use Norwegian employer-employee linked register data and leverage shocks to employment between 2008 and 2012 to induce workers to search for a new job. We follow individuals for 11 years in the window before and after a separation event through a stacked event-study design (Cengiz et al. (2019)). Workers are categorized into two groups: non-Western immigrants (the out-group) and the rest, namely native-born Norwegians and Western immigrants (the in-group).[4] This classification aligns with standard conventions in the Nordic context, where segregation and discrimination occur against immigrants who physically, linguistically, or culturally differ from the majority population (e.g., Böhlmark and Willén (2020)). Throughout the paper, we focus on three core outcomes: employment, part-time work, and annual labor earnings.

What assumptions must hold for us to interpret post-displacement differences as evidence of discrimination against the out-group in our sample? We draw theoretical insight from Fryer et al. (2013) to answer. From the supply side, workers must be similar in individual productivity (including accounting for match effects), and search behavior and costs. Our sample design—conditioning on workers who held the same job in the same firm at the same time and earned the same wage—ensures that workers are highly comparable along a rich set of labor market dimensions correlated with these baseline conditions. Requiring at least three (or five) years of continuous full-time work further reduces concerns about differences in labor market attachment, core language skills, or access to support systems. A wide range of robustness checks further supports the comparability of supply-side factors across groups. Under these conditions, systematic differences in post-displacement outcomes are difficult to reconcile with supply-side explanations alone and instead point toward differential treatment on the demand side.[5]

We present four main sets of results. First, we find clear evidence of significant wage and employment discrimination in the Norwegian labor market. After controlling for pre-displacement earnings, employment history, and layoff from the same firm and occupation within the same labor market at the same time, non-Western immigrants are hired at significantly lower rates than their native or Western immigrant counterparts. Among those who secure a job, the annual earnings of the non-Western immigrants are substantially lower. Importantly, these wage disparities are not driven by systematic differences in the types of occupations, local labor markets, or firms that immigrants and natives enter after a successful search. Rather, they reflect unequal compensation within similar roles at firms of similar quality. Prior to the displacement-induced search, the local labor market priced the output of these workers similarly across immigration status.

Second, discrimination is largely restricted to highly concentrated labor markets. Between what are arguably equally productive natives and immigrants who were exogenously induced to search

[4]For ease of exposition, "natives" includes immigrants from Western countries in our conceptual framework and our empirical estimation.

[5]We directly address several factors correlated with neighborhood location and composition, occupation, industry, age, firm-specific tenure before displacement, mobility, social capital and homophily, match quality, job ladders, attrition, or baseline earnings (e.g., mean reversion).

for jobs after leaving the same firm and occupation simultaneously, employment and wage gaps are minimal in competitive markets but substantial in concentrated markets. This suggests that labor market competition may play an important role in reducing employer discrimination. However, it does not completely eliminate the gaps between NWIs and others.[6] To our knowledge, this provides the first direct empirical evidence linking labor market power to employer discrimination.

Third, when focusing on product market power (proxied by industry revenue concentration both nationally and locally) while controlling for labor market concentration, we find little evidence that product market power independently contributes to employer discrimination. This finding aligns with the idea that firms operating in competitive labor markets face highly elastic labor supply and have limited scope to sustain discriminatory wage-setting, irrespective of their ability to raise prices in output markets. Although it is theoretically possible for firms with product market power but without labor market power to engage in employment discrimination (but not wage discrimination), we find little evidence in support of this in the Norwegian context. To our knowledge, this paper is the first to empirically distinguish the role of labor market power from product market power in explaining employer discrimination through a quasi-experimental analysis, helping make progress in understanding the respective roles of product market and labor market power in employer discrimination.[7]

Finally, we reveal that the employment discrimination we observe is most consistent with belief-based factors, with minimal influence from direct preferences against non-Western immigrants. This interpretation is supported by multiple lines of evidence. We begin by analyzing the dynamic adjustment of employment and earnings gaps following displacement, building on insights from Bohren et al. (2019). Our analysis reveals substantial labor market gaps in employment and earnings between identical natives/Western immigrants and non-Western immigrants in concentrated labor markets during the first 2–3 years after displacement. These gaps gradually diminish over time and disappear within five years. This pattern supports the idea that ongoing interactions with workers from disfavored groups can correct erroneous productivity beliefs and reduce the cost of acquiring additional productivity information, thereby reducing discrimination-driven gaps (Fryer et al., 2013; Altonji and Pierret, 2001).

To examine whether these findings reflect continuous interactions with employers within the same firm, we conduct an auxiliary analysis using fixed effects for the firms where displaced work-

[6] We recognize that there may be labor market frictions (job search, mobility, institutional features) that provide firms with limited power to discriminate even in low-HHI markets. Therefore, even in non-concentrated markets, some unobserved labor market power may still persist, allowing firms to sustain a small degree of discrimination.

[7] A natural question is how these results align with prior work on product market power and employer discrimination, which typically finds a non-negligible relationship (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)). One possibility is that some of those estimates partly reflect the within-firm correlation between labor market power and product market power (Marinescu et al., 2021a), such that part of the estimated product market effect may capture unobserved labor market power. This is consistent with concurrent theoretical work suggesting that, for example, import competition disciplines local labor markets primarily when firms hold labor market power (Mertens, 2020).

ers are employed after their search (i.e., the destination firm). This approach compares the earnings and hours trajectories of a native-born Norwegian and a non-Western immigrant who were laid off from the same occupation and firm with the same earnings and subsequently hired at the same firm after initial displacement.

We find that non-Western immigrants rehired into the same firm as their native counterparts initially experience significantly lower earnings in concentrated labor markets during the first 2–3 years after displacement. However, these gaps close over time, suggesting that the overall convergence in earnings predominantly occurs *within* firms through repeated interactions with colleagues and managers. Despite the reduction in the employment and wage gaps over time between the non-Western immigrants and the rest, our findings reveal substantial efficiency costs for firms associated with belief-based discriminatory practices. Specifically, firms that hire displaced Norwegians but not displaced NWIs tend to recruit Norwegians with lower estimated worker effects based on an AKM estimation. These results have important implications for economic growth, productivity, and labor market efficiency.

To further investigate and differentiate between preference-based and belief-based discrimination, we conduct three auxiliary analyses: (a) leveraging information on managers to measure their recent work experience with NWIs, (b) examining discrimination patterns against Western immigrants and second-generation NWIs, and (c) assessing discrimination toward NWIs who have been in the country for more than 20 years. Our findings reveal that managers with greater recent experience working with NWIs are more likely to hire displaced NWIs in concentrated markets. Additionally, long-term non-Western residents face significantly less discrimination, while Western immigrants and second-generation NWIs experience no detectable discrimination in the labor market. These results support the concept of belief-based discrimination, where decision-makers, such as hiring managers, rely on group averages in the absence of complete information about individual workers. Managers with more work experience with NWIs—who are most likely to have accurate productivity information for non-Western immigrants—are the managers most likely to hire NWIs in concentrated markets but *not* in competitive markets.

In addition to examining the presence and nature of discriminatory responses, we assess their economic consequences. Displaced NWIs experience substantial losses in earnings and employment relative to observationally similar and arguably equally productive native peers, with these gaps persisting for multiple years. These dynamics also have implications for firms. Using estimated worker fixed effects, we find that firms that avoided hiring displaced NWIs subsequently hire lower-fixed effect native workers. In contrast, firms that hire displaced immigrants tend to attract higher-fixed effect native and immigrant workers. These patterns point to allocative inefficiencies arising from discriminatory behavior, suggesting that distorted beliefs or preferences can impose meaningful costs on both workers and firms.

We have systematically examined a wide range of alternative explanations that could plausibly

contribute to our results. **Table C-1** summarizes each hypothesized mechanism and the corresponding evidence; the main text contains separate subsections (see Section 6) detailing more than a dozen auxiliary analyses. We highlight two particularly important possibilities here: (i) that non-Western immigrants (NWIs) search differently and accept worse matches after displacement, and (ii) that NWIs sort into more concentrated labor markets on unobservables correlated with weaker post-displacement outcomes. For either explanation to account for our findings, these mechanisms would need to operate primarily for NWIs (and not for natives, Western immigrants, or second-generation NWIs), in high-HHI markets, within firm–occupation–local labor market cells, and conditional on identical baseline earnings and tenure. Our analyses include conditioning on the same destination firm; comparing public- and private-sector workers; examining job quality measures, mobility patterns, and baseline asset holdings; reweighting on pre-displacement characteristics; controlling for Generalized Propensity Scores to account for selection into pre-displacement concentration; limiting the sample to firm closures; inverse probability reweighting low-HHI NWIs to match the observable characteristics of high-HHI NWIs; and comparing NWIs from different regions or with different job tenure who likely face different search frictions. Across these exercises, the concentration penalty for NWIs in employment and earnings remains remarkably stable.[8] Taken together, the evidence strongly points away from heterogeneous search, unobserved selection, and other factors as drivers of our results.

1.1 Related Literature and Contribution

There is an extensive theoretical literature on employer discrimination, covering preference-based, belief-based, monopsonistic, and systemic discrimination (e.g., Robinson (1933); Becker (1957); Arrow (1957); Aigner and Cain (1977); Black (1995); Coate and Loury (1993); Bertrand et al. (2005); Kline et al. (2022); Bohren et al. (2022); Lepage (2024)).[9] There is also rich empirical literature studying the implications of employer discrimination in various contexts (e.g., Bertrand and Mullainathan (2004); Fryer Jr and Levitt (2004); List (2004); Lang and Manove (2011); Lang and Spitzer (2020); Gerard et al. (2021); Benson and Lepage (2024)), and the role of product market power in driving employer discrimination (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)).[10]

[8] A paired movers design (Sorkin and Warwar, 2026) also helps rule out differential match effects over concentration driving our results, and AKM model estimates rule out ex ante sorting on worker effects into concentrated markets by NWIs.

[9] Notably, Kline et al. (2022) find that racial discrimination in job callback rates in the US context is driven by a relatively small share of firms. While the authors do not tie this to the competitive landscape, if most firms are exposed to relatively competitive local labor markets, they likely have little power to discriminate, which may be one explanation for their findings.

[10] Related work by Balgova and Illing (2023) examines migrant-native gaps using German data and shows that migrants experience larger earnings declines following job loss. As an auxiliary analysis, they consider labor market tightness and find no differential effect across groups. However, tightness affects hiring frictions generally rather than the ability of employers to exercise discriminatory preferences. Tightness and labor market power (from, for example, concentration) are theoretically and statistically distinct.

We build on this literature by shifting the focus to the conditions under which discrimination persists. Theoretical work has long emphasized that firm behavior—discriminatory or not—is shaped by market structure and strategic incentives. Yet, while prior research has emphasized the role of competition, relatively little empirical work isolates the role of labor market power—distinct from product market power—in shaping discriminatory outcomes. Moreover, although belief-based and preference-based discrimination are conceptually well understood, less is known about how these mechanisms evolve over time in real labor markets. Our study contributes to these discussions by examining how discrimination responds to variation in employer power and by tracing dynamic search outcomes across groups. In doing so, we provide new evidence on how structural features of labor markets shape inequality.

More concretely, our study builds on the conceptual insight of Fryer et al. (2013), who argue that wage offer gaps conditional on prior earnings can reveal a lower bound on discrimination. While their analysis relies on survey data and productivity proxies, our approach leverages quasi-random mass layoffs to induce job search among exactly matched native–immigrant worker pairs who are identical in firm, occupation, location, and pre-displacement earnings (and more factors in robustness tests). While their analysis captures average gaps in wage offers, our contribution is to examine when, where, and under what conditions such discrimination emerges. In a similar vein, contemporaneous work by Sorkin (2025) studies Black and white workers in the U.S. to analyze earnings gaps after mass layoffs, interpreting the patterns through employer learning and emphasizing the importance of tenure matching.[11] His work provides valuable evidence on post-displacement disparities in the U.S. context and the conditions under which these gaps can be interpreted as evidence of discrimination. Our analysis instead focuses on how market structure shapes firms' ability to discriminate and on the relative roles of labor market and product market power. We find that discriminatory gaps are concentrated in settings where firms possess wage-setting power, while product market power appears to play a more limited independent role. In doing so, we contribute new evidence on the conditions under which discrimination can persist and provide suggestive evidence that belief-based discrimination diminishes with repeated employer–employee interactions, consistent with employer learning models (Lepage, 2024).

There is also a rapidly growing literature on monopsony power in labor markets (e.g., Schubert et al. (2020); Azar et al. (2020b); Qiu and Sojourner (2019); Rinz (2018); Prager and Schmitt (2021); Azar et al. (2020a); Benmelech et al. (2022); Marinescu et al. (2021a); Hershbein et al. (2018); Bassanini et al. (2022); Dodini et al. (2024a)). This literature has deepened our understanding of labor market imperfections and their impact on wage-setting. We contribute to this literature by applying recent monopsony frameworks to examine how labor market competition shapes discriminatory outcomes. More specifically, we study whether labor market power affects discrimination against minority workers and whether discriminatory gaps are concentrated in set-

[11] In our estimates, matching on tenure or including flexible interactions with tenure yield nearly identical results.

tings where firms possess wage-setting power.

Finally, we contribute to literature on integrating immigrants into host labor markets (e.g., Rica et al. (2015); Chin and Cortes (2015); Martín et al. (2016); Becker and Ferrara (2019); Dorn and Zweimüller (2021); Brell et al. (2020); Arendt and Bolvig (2020); Arendt et al. (2020); Lochmann et al. (2019); Battisti et al. (2022); Bratu et al. (2020); Dodini et al. (2024b); Butschek and Walter (2014); Ottosson (2022); Bratsberg et al. (2017); Silliman and Willén (2024)). We show how weak labor market competition exacerbates native-immigrant gaps, especially when firms rely on uncertain beliefs about immigrants' productivity. Relatedly, recent work by Dostie et al. (2023) highlights the importance of employer behavior in explaining immigrant-native wage gaps, showing that both sorting across firms and differential within-firm pay policies contribute to persistent disparities. Our approach provides a complementary perspective by holding firm characteristics fixed at the point of displacement and focusing on how labor market structure shapes employer behavior during search, rehiring, and earnings progression.

2 Background

Norway provides a useful setting for this analysis due to its rich administrative data and institutional environment. In addition, discrimination in Norway is often viewed as relatively limited, and Norwegians exhibit comparatively strong egalitarian preferences in experimental settings (Almås et al., 2023). To the extent that these attitudes translate into labor market behavior, the effects we identify may constitute conservative estimates relative to other settings. This section outlines the structure of local labor markets and immigration patterns, providing context for our empirical approach and findings.

2.1 Labor Markets in Norway

Local labor markets in Norway exhibit lower concentration than those in the US and the UK. However, labor market concentration remains prevalent, with several local labor markets exhibiting HHIs that exceed the thresholds established by the EU, UK, and US to indicate significant monopsony power (Dodini et al., 2024a). These thresholds, typically set at or above 2,500, mark areas of concern among regulators about diminished competition and heightened employer power in wage-setting, which may contribute to suboptimal labor market outcomes for workers.

In terms of employment contracts, Norway has a high degree of employment protection, making it difficult to terminate workers without cause.[12] Typically, the standard employment contracts mandate a three-month notice period for termination, and there is no general legal requirement for severance pay (Salvanes et al., 2024). In cases of mass layoffs or firm closures, which we use in our empirical models to induce search, firms can shorten the notice period considerably. There is no mandated order for laying off workers (Salvanes et al., 2024). In our empirical models, to

[12]Relative to other countries, the OECD ranks the level of employment protection in Norway similar to Italy (Salvanes et al., 2024).

address concerns about potential differential selection of immigrants and natives into the layoff sample as a function of labor market power, we also present results focusing exclusively on firm closures, where concerns about selective displacement are substantially reduced.

Upon contract termination, workers have access to Norway's generous unemployment insurance system. This system offers financial support to individuals for up to 2 years during periods of unemployment. The benefits are available to all workers who have been laid off or experienced a reduction in their work hours greater than 50 percent. Benefits are conditional on having worked the four months leading up to the termination and having earned a minimum income in the year prior to termination ($16,500 in 2019). The replacement rate is 62.4% of the previous year's pay or 62.4% of the average pay over the last 3 years.

To receive unemployment benefits, individuals must actively seek work and provide evidence of search activities to the Norwegian Labor and Welfare Administration (NAV) every 14 days, be registered with NAV and participate in regularly scheduled follow-ups, and cannot refuse employment offers provided via NAV without valid reasons (Johnsen et al., 2022). Additionally, they are required to participate in approved training programs if requested by NAV to support their reintegration into the labor market. Support includes career guidance, job matching services, and skill development opportunities designed to help individuals transition back into the workforce. Given these requirements to participate in significant job search activity to be eligible for UI benefits, significant differences in the intensity of search behavior across groups are unlikely to explain employment and wage gaps during the search process. This is especially unlikely to explain relative differences in search outcomes across labor markets with different levels of concentration. Approximately 78% of displaced workers in our sample are re-employed one year after displacement (Huttunen et al., 2011a).

While Norway has a coordinated wage bargaining system and a relatively compressed wage distribution due to its unions (Mogstad et al., 2025), there is considerable flexibility at the local level, allowing firms to differentiate pay based on productivity and market conditions. As documented by Bhuller et al. (2022), sectoral agreements coexist with firm-level bargaining, resulting in meaningful wage variation even within narrowly defined labor markets. Similar evidence is provided by Dodini et al. (2023; 2022), who show that a large share of wage variation is determined locally.

2.2 Immigration in Norway

Norway has seen a large rise in immigration over the past several decades, with immigrants now making up more than 15 percent of the population, a rate comparable to the United States. While migration was initially dominated by Western immigrants, non-Western migrants have steadily increased and now make up half of the immigrant population. This shift has sharpened the visible and linguistic distinction between immigrants and natives over time, heightening the risk of employer discrimination (e.g., Chiswick and Miller (2005)). Although ethnic and racial segregation

manifests differently across the world, in Scandinavia, it is most pronounced in the disparities between non-Western immigrants and other groups (e.g., Böhlmark and Willén (2020); Aldén et al. (2015); Grand and Szulkin (2002)). These divisions are believed to stem from non-Western immigrants' physical differences, distinct cultural backgrounds, and unfamiliar languages (Chiswick and Miller, 2005).

We define Western immigrants as those born in Sweden, Denmark, Finland, Iceland, Belgium, France, Ireland, Luxembourg, the Netherlands, Great Britain and Northern Ireland, Germany, Austria, Switzerland, Israel, the United States, Canada, or Oceania (Böhlmark and Willén (2020); Aldén et al. (2015); Korpi et al. (2023)). The most common immigrant countries include Poland (97,197), Lithuania (37,638), Sweden (36,315), Somalia (28,696), Germany (24,601), Iraq (22,493), Syria (20,823), the Philippines (20,537), Pakistan (19,973), and Eritrea (19,957). The immigrant population is relatively evenly distributed across the country, though there is a slightly higher density of immigrants in the country's four major metropolitan areas: Oslo, Bergen, Stavanger, and Trondheim. While residential segregation within cities between NWIs and the rest is widespread, it has declined significantly in the last few years (Kornstad et al., 2018).

An important contextual factor related to immigration in Norway is that those without a right to live and work in the country (non-EU citizens) must pass a language exam (currently level A2) and reside in the country for at least three years before they can receive permanent residency, though they can renew temporary status on an annual basis. Because of this, and since we are primarily interested in labor market gaps between non-Western immigrants and the rest, we require that immigrants and natives in our analysis have held full-time employment in Norway for at least three years before they are induced to search for a job. This ensures that they all meet the minimum stay requirement for permanent residency.

3 Conceptual Framework

This section outlines a simple conceptual framework that motivates the empirical analysis. The goal is not to introduce new theoretical insights, but rather to organize canonical ideas from the discrimination and market power literatures in a way that clarifies how competition shapes discriminatory behavior. The framework guides both the interpretation of our findings and the empirical design. Appendix A presents an alternative formulation that develops the same core mechanisms in a slightly more structured setting, drawing more directly on Becker (1957), Phelps (1972), Arrow (1973), and Bohren et al. (2019), though we note that none of these theories brings together discrimination, labor market power, product market power, and statistical discrimination.[13]

We begin with a stylized model of a firm that hires two types of workers: natives and immigrants. Each worker supplies one unit of labor (equally productive), and the firm chooses how

[13]Becker (1957) consciously abstracts from labor market power and assumes perfect labor competition. Robinson (1969) discusses imperfect labor competition, but not its implications for discrimination. Arrow (1973), Phelps (1972), and Bohren et al. (2019) discuss statistical discrimination and its dynamics but not competition.

many workers of each type to employ. Firms may hold biased beliefs about immigrant productivity or exhibit a taste-based preference for native workers. Let $f(L_n, L_m)$ denote the firm's production function, where L_n and L_m are the number of native and immigrant workers, respectively. The firm chooses employment levels to maximize profits, taking into account both wages and any discriminatory distortions. The profit function can be written as:

$$\Pi = f(L_n, L_m) - w_n L_n - w_m L_m - dL_m,$$

where w_n and w_m are the wages paid to natives and immigrants, and $d \geq 0$ captures the (implicit or explicit) cost of discrimination—whether due to biased beliefs or preferences. When $d > 0$, firms behave as if immigrants are less productive than they truly are, either because of incorrect priors (as in models of statistical discrimination; (Phelps, 1972; Arrow, 1973)) or utility penalties from hiring them (as in Becker (1957)).

In a perfectly competitive labor market, firms have no ability to set wages and must pay workers their marginal product. If they do not, workers will simply leave for outside options that pay their marginal product. In such a setting, discrimination is costly. Firms that undervalue immigrant workers will be outcompeted by those willing to hire them at their true productivity. As a result, standard models predict that discrimination cannot persist in competitive environments unless preferences are extreme or coordination failures prevent efficient hiring. By contrast, in labor markets with concentration and employer market power, firms face upward-sloping labor supply curves and can set wages below marginal product. These wage-setting rents create scope for inefficient allocations, including discriminatory ones, to persist. The key insight is that market power reduces the cost of discrimination, making it more likely to be observed where competition is limited (Robinson, 1933; Manning, 2003; Berger et al., 2022).

Types of Discrimination. Discrimination can take multiple forms. In canonical models, taste-based discrimination reflects employer preferences for one group over another (Becker, 1957), while statistical discrimination arises from uncertainty about worker productivity, leading firms to rely on group-level priors (Phelps, 1972; Arrow, 1973). Both mechanisms can generate persistent wage or hiring gaps and are captured in the framework above by a wedge $d > 0$ between wages and marginal product for immigrant workers.

In the context of imperfect competition, a third mechanism arises: monopsonistic discrimination. This idea—originating in the work of Robinson (1933) and formalized in labor economics by Boal and Ransom (1997) and Manning (2003)—suggests that firms may apply different markdowns to different groups based on the elasticity of their labor supply. If immigrants face more constrained mobility and fewer outside options due to language barriers, legal restrictions, or limited networks, firms may find it optimal to offer them lower wages than equally productive natives. Unlike taste-based or statistical discrimination, this mechanism does not require bias or misper-

ception; it reflects profit-maximizing behavior in the presence of heterogeneity in outside options. Notably, labor market competition reduces firms' ability to engage in this kind of discrimination by collapsing gaps in workers' outside options.

Empirically, distinguishing these mechanisms is challenging. However, our setting allows us to make progress. By focusing on workers displaced from the same firm and occupation, and conditioning on pre-displacement earnings, we mitigate concerns about differential mobility/outside options, firm-specific human capital, or pre-layoff productivity. Systematic differences in post-displacement trajectories—particularly as a function of labor market concentration—are more plausibly attributed to firm behavior than to compositional differences in worker-side frictions. Moreover, the dynamics of outcomes allow us to further differentiate these mechanisms: monopsonistic and taste-based discrimination is more naturally associated with persistent income wedges between natives and immigrants, while discrimination based on (correct or incorrect) beliefs about productivity implies convergence as firms update and correct their productivity beliefs.

Sources of Market Power. Market power can arise from different sources, each with distinct implications for discrimination. Our focus is on labor market concentration, which directly enables firms to set wages below marginal product. We measure this using the Herfindahl-Hirschman Index (HHI) at the occupation-region level, consistent with recent empirical work linking labor market concentration to wage-setting and employment outcomes (Azar et al., 2020b; Rinz, 2018). High HHI values indicate thinner labor markets with fewer outside options, where firms may be able to exercise wage-setting power over displaced workers.

This form of market power differs fundamentally from product market concentration. Firms with product market power—such as those operating in oligopolistic industries—may earn supernormal profits, but this does not automatically translate into wage-setting power unless they also dominate the local labor market. Product market concentration affects firms' pricing and profitability, but if the labor market is competitive, firms still have to offer market wages to attract and retain workers. In this case, wage discrimination is not sustainable. However, product market power can still allow discrimination on the extensive margin. That is, firms may choose not to hire certain groups—such as immigrants—if doing so aligns with managerial preferences or beliefs, and if their hiring needs are not tightly constrained. As long as firms can substitute across workers or leave positions unfilled without affecting output significantly, firms can cover the costs of substitution or unfilled positions using product market rents.

By contrast, labor market power enables firms to engage in both wage and hiring discrimination. In markets where workers have few outside options—due to constraints like high concentration, mobility frictions, or licensing barriers—firms can set wages below marginal product and selectively exclude certain worker types with minimal economic cost. These distortions are particularly relevant when discrimination is based on biased priors, as firms with market power may choose not to invest in resolving uncertainty or may persist in misjudging certain groups. Labor

market competition increases the cost of persistently getting productivity signals wrong.

Distinguishing between labor and product market power is therefore important. Product market rents may increase the capacity to discriminate, but only labor market power provides the mechanism for systematically underpaying or excluding certain workers. This distinction underpins our empirical approach: we separately measure labor market concentration using employment-based HHIs and product market concentration using industry revenue-based HHIs. By comparing their effects, we assess whether the observed immigrant-native gaps are driven by employer control over hiring and wages, rather than by profitability or general firm-level rents. Our findings suggest that discriminatory outcomes following displacement are closely tied to labor market structure rather than product market dominance.

Dynamics of Discrimination. The dynamics of immigrant-native disparities are helpful for better understanding the underlying mechanisms. If discrimination reflects taste, there is little reason to expect it to change over time; at least not in the short run. Employer preferences are typically stable and unlikely to immediately respond to additional information. Similarly, if what we observe is monopsonistic discrimination, then the firm profit maximizing behavior implies that whatever initial wedge is created will remain over time. In contrast, if discrimination reflects mistaken beliefs, then repeated interaction between employers and workers should lead to updating and, eventually, the elimination of the gap. This idea is central to models of employer learning, where firms revise their expectations based on observed performance (Farber and Gibbons, 1996; Altonji and Pierret, 2001). Convergence in outcomes over time, particularly in wages, is a key prediction of these models when beliefs are initially biased.

Our empirical setting allows us to test for such convergence directly. We follow displaced workers over time, comparing native and immigrant outcomes across labor markets with varying degrees of concentration. If discrimination is belief-based, we expect the initial immigrant-native gap to be larger in more concentrated labor markets but to narrow as firms update their priors. If, instead, discrimination is driven by tastes or reflects stable differences in search efficiency or outside options, the gap should remain stable.

While convergence is not definitive evidence of belief-based discrimination, it is more naturally consistent with models in which employers update beliefs over time than with models of persistent taste-based discrimination. Combined with the structure of the labor market and the design of our empirical comparisons, the recovery dynamics provide suggestive evidence about the mechanisms underlying immigrant–native disparities following displacement. To further examine these mechanisms, we also analyze supplementary evidence on hiring and earnings dynamics, including the characteristics of managers and firms that hire immigrants in concentrated versus less concentrated labor markets.

Empirical Analyses. Our empirical strategy operationalizes these ideas by comparing native and non-Western immigrant workers who are observationally equivalent prior to displacement. We

focus on individuals who held the same job in the same firm at the same time and were forced into job search due to mass layoffs or firm closures—events that are plausibly exogenous to individual characteristics and generate a setting in which similarly productive workers are exposed to different labor market environments. By comparing post-displacement outcomes across markets with varying degrees of labor and product market concentration, we examine how employer market power is associated with differential treatment of immigrants. We also trace outcomes over time to assess whether the observed disparities are more consistent with persistent preferences or with models in which employers update beliefs over time. This approach provides a direct empirical analogue to the conceptual framework and allows us to examine how labor market competition shapes the extent and persistence of discriminatory behavior.

What assumptions must hold for our strategy to identify discrimination against non-Western immigrant workers? To address this question, we borrow theoretical insight from Fryer et al. (2013). From the supply side, the key requirement is that workers are comparable in productivity, search behavior, and search costs prior to displacement. Our design—comparing individuals who held the same job in the same firm at the same time and earned the same wage prior to displacement—substantially limits differences between displaced native and non-Western immigrant workers along observable labor market dimensions. Moreover, by focusing on individuals with at least three years of full-time tenure (five years in robustness checks), we reduce concerns about differential labor market attachment or language barriers, and we ensure that all workers have equal access to welfare and unemployment insurance programs. Importantly, the occupations most commonly represented in our layoff sample among NWIs are blue-collar occupations–those in which Norwegian language skills are far less important than for white-collar workers, so language barriers are unlikely to play a major role in our estimates. These workers have therefore already demonstrated sustained labor market attachment and access to key labor market institutions, making it less likely that the estimated differences are primarily driven by unobserved supply-side factors. A broad set of robustness checks further supports this interpretation. If the supply-side conditions hold and if firms treat equally productive workers the same regardless of group identity, the workers would have equal outcomes.

If the supply-side conditions outlined above approximately hold, then post-displacement gaps across demographic groups are difficult to reconcile with purely supply-side explanations and instead point toward differential treatment on the demand side–discrimination. This logic mirrors the framework in Fryer et al. (2013), who show that gaps in wage offers conditional on prior earnings provide evidence of discrimination, since prior wages proxy for worker productivity. Our design strengthens this identification by comparing individuals within the same firm–occupation–year–earnings cell prior to displacement, effectively holding job role, employer, timing, and pre-displacement productivity constant at a granular level.[14]

[14]There are two ways in which worse post-displacement outcomes for immigrants could be consistent with lower

4 Data and Method

4.1 Data

Our primary data come from linked employer-employee records that cover all Norwegian residents. A unique person identifier enables us to follow workers over time, and unique firm and establishment identifiers allow us to observe their employers and identify whether establishments are downsizing or closing.

We merge these data with information from the demographic and immigration register, the tax register, and the social security register. Combined, these data provide us with detailed information on immigration status, source country, occupation, employment, employer, neighborhood and work location, earnings, and contract hours for every individual between 2003 and 2017. Each worker's neighborhood is defined by their *grunnkrets* (basic statistical units), which are analogous to US Census block groups and typically include approximately 500 inhabitants.

Our measure of earnings is individual annual pre-tax labor income, which includes regular labor income and income from self-employment. Employment status is defined based on whether they are attached to a firm in the labor register, which is determined in October of each calendar year. Hours worked are based on a categorical variable that indicates whether the worker is contractually employed as a part-time or a full-time worker.[15]

To measure labor market power, we rely on a Herfindahl-Hirschman Index for each 2-digit occupation in each local labor market in Norway. Local labor markets are defined based on commuting distance and divide Norway into 160 regions (Gundersen and Jukvam, 2013). The HHI is the sum of the squares of the employment shares across establishments within the occupation and local labor market. We scale this to range from 0 to 1, where 1 indicates a single monopsonist in the market. Hence, the HHI measures the concentration of labor demand for a given occupation *across establishments* in a local labor market.[16]

unobserved portable productivity. First, pre-displacement firms may have stronger diversity or affirmative-action preferences than the firms immigrants match with after displacement. Second, immigrants may benefit from disproportionately favorable firm-specific match effects that are destroyed by layoff. In our setting, however, the first explanation would additionally require affirmative-action preferences to be strongly correlated with labor market concentration. We view this as unlikely and inconsistent with existing evidence on affirmative-action policies (Fryer et al., 2013). Regarding match effects, we formally test for them using a paired-movers design that compares wage gaps among workers who move between the same pair of firms (Sorkin and Warwar, 2026). We find that implied match effects move in the *opposite* direction of our estimated HHI discrimination gradient—that is, match effects appear less important in concentrated markets (see Table A-18).

[15] Workers may have temporary work throughout the year, but the labor register captures formal employment relationships and attachment to firms as employees. If workers are not counted as "employed" in our data, they may still have earnings reported to the tax authority, which we do capture in our data. Earnings are, therefore, not conditional on the "employment" outcome, which is why we have few zeroes for earnings.

[16] A potential concern is that market concentration may partly reflect underlying differences in productivity across markets (Schubert et al., 2020). However, our identification strategy does not require HHI to be exogenous. By conditioning on identical pre-displacement characteristics and exploiting exogenous job loss events, we focus on how pre-existing concentration moderates employer behavior, not what determines it. Furthermore, our identification strategy closely matches on local labor market-occupation-industry-firm cells, so any underlying correlation between

We acknowledge that labor market frictions such as job search costs, mobility constraints, and institutional factors can allow firms some degree of discriminatory power even in low-HHI markets. Following Boal and Ransom (1997), we interpret market concentration as an empirically tractable proxy for employer wage-setting power in imperfectly competitive labor markets. While HHI does not capture all sources of monopsony power, it aligns with canonical models of oligopsonistic competition and provides a useful lens for studying how market structure affects wage and hiring behavior. Research shows it effectively reflects firms' influence over labor, which negatively affects employment and wages (e.g., Dodini et al. (2024a); Azar et al. (2020b); Marinescu et al. (2021b)). Descriptive data indicate a strong negative relationship between market concentration and firms' employment of the outgroup, non-Western immigrants (Figure A-1). To validate our use of HHI as a proxy for market power, we also replicate our analysis using an alternative measure of monopsony power—wage markdowns estimated from production function residuals following Yeh et al. (2022). For the subset of firms where these estimates are feasible, we find a strong correlation between local concentration and markdowns (Appendix Figure A-10), and our main results hold closely when using markdowns in place of HHI (Appendix Table A-25). HHI is also strongly correlated with an outside-options index constructed from Norwegian worker surveys measuring perceived outside labor market opportunities (see Silliman and Willén (2025)).

4.2 Sample Construction and Empirical Method

Our empirical approach is analogous to an experiment comparing two immigrant–native worker pairs in the same occupation with highly similar labor market histories and productivity signals, but different immigration backgrounds, who search for jobs simultaneously following displacement. One pair searches in a labor market with greater employer market power, where firms can sustain wage markdowns and earn rents from employment relationships. The other pair searches in a more competitive market, where firms face stronger competition for workers and wages are more closely tied to workers' marginal revenue product. We track outcomes after workers are exogenously displaced from the same firm and occupation, within the same labor market and at the same time, following either a mass layoff or firm closure, while earning similar pre-displacement wages. This comparison holds constant a rich set of pre-displacement labor market signals, including job match quality, employer, occupation, timing, and prior earnings, thereby substantially narrowing potential productivity differences before workers begin their job search.

Our estimation approach uses an event study design built around involuntary displacement events that induce job search among observationally equivalent workers. We define involuntary job loss events as instances in which establishments of at least ten workers either shut down or lay

productivity and sorting across these cells is addressed non-parametrically. Finally, we are interested in the differences between immigrants and natives, so to bias our results, such a correlation would have to disproportionately affect only non-Western immigrants and not other groups, which is unlikely.

off more than 30 percent of their workforce (e.g., Salvanes et al. (2024)).[17]

To implement our event study design, we follow the approach of Cengiz et al. (2019) and construct stacked panels by displacement year (2008 through 2012), tracking all displaced workers for eleven years surrounding the displacement event ($t-5$ through $t+5$) using registry data from 2003–2017. While recent critiques of staggered difference-in-differences designs highlight potential biases when treatment timing varies across units (Goodman-Bacon (2021); De Chaisemartin and d'Haultfoeuille (2020)), these concerns are not directly relevant here since our sample includes only treated individuals. Nonetheless, the stacked panel structure facilitates estimation and visualization of dynamic treatment effects across multiple cohorts while maintaining a consistent event time framework.[18]

To ensure that we are capturing the effects of concentration on similar types of workers regardless of immigration status, we limit our sample to those who worked consistently at full-time status in the three years before the displacement event.[19] This ensures that our sample is not skewed by recent arrivals or seasonal workers in the country and that all immigrants meet the work requirement for permanent residency in Norway, regardless of their origin country. In addition, it allows us to compare workers with a similar work history regardless of their immigration background. The work requirement also means that each of these individuals meets the requirements set out for using UI, ensuring that our results are not driven by differential selection into eligibility for unemployment benefits (which may impact search patterns) after separation. We limit the sample to those exposed to a layoff event when they are between the ages of 25 and 65, representing the period after college and before retirement.

Table 1 reports summary statistics for displaced workers in the year of displacement and for a random sample of non-displaced workers selected using the same criteria (e.g., at least three years of full-time work at a firm with ten or more employees). Our HHI variables in the displaced worker sample are defined across 2,343 occupation–local labor market (LLM) cells. Non-Western immigrants in Norway are more likely to work in blue-collar occupations and have lower average earnings than natives and Western immigrants. However, their firm-specific tenure, age, and gender composition (female share) are broadly comparable. Notably, baseline differences in these characteristics do not pose a threat to identification, as our matching strategy eliminates cross-sectional differences in firm, occupation, industry, and location sorting—as well as baseline earnings—such

[17]To address any concerns about potential differential selection of immigrants and natives into the layoff sample as a function of labor market power—which we find no evidence of and consider unlikely—we also present results focusing exclusively on firm closures, where such selection is mechanically ruled out. These estimates yield similar results.

[18]This period overlaps with the Great Recession in the United States. However, the financial crisis in Norway was limited to parts of 2009, when Norway had a relatively small and short-lived GDP contraction. However, Norway experienced robust economic growth in every other year in the period. Our results are robust to dropping the 2009 layoffs, and layoff counts in our sample are well balanced across the 2008-2012 period.

[19]We also use a five-year tenure requirement and find identical effects. See Appendix Table A-17.

that displaced workers are mechanically observationally equivalent.[20]

A key strength of our empirical strategy is that it does not require labor market concentration (HHI) to be exogenous. The identifying variation comes from mass layoffs and firm closures that exogenously force observationally equivalent workers—native and immigrant, employed in the same occupation, firm, and local labor market, and previously earning the same wage—into job search. However, it is still instructive to examine potential balance between baseline HHI (used as an interaction in our models) and observable characteristics. Table A-2 shows the correlation between baseline HHI in our layoff sample and observable characteristics of the workers and local labor markets. In column (1), we show unconditional correlations. In column (2), we show correlations after residualizing on our matching variables. The unconditional correlations are weak across characteristics in our sample. Even the strongest correlation is relatively modest at 0.25 for local labor market size. In columns (3) and (4), we regress baseline HHI on observables with no other correlates (column 3) and conditional on the fixed effects in our model (column 4). Despite the unconditional correlations in column (1), the relationships are noticeably weak. For example, a one standard deviation in unemployment benefits in a local labor market is associated with higher HHI of only 0.006; conditional on our fixed effects, this relationship switches sign. Because we compare these workers within the same market and job context, the operative relationship in our models corresponds to columns (2) and (4), and any differential post-displacement outcomes across levels of pre-existing HHI capture how firms with varying degrees of market power treat equally productive workers during the hiring process. Overall, there is little unconditional or conditional correlation between key observable characteristics and baseline HHI, supporting our interpretation of the results

To address concerns about reverse causality, we fix HHI in the year prior to displacement, ensuring that it is not affected by the layoff itself.[21] Importantly, the causal interpretation of our estimates does not rely on what determines HHI, but rather on how pre-existing market concentration moderates employer behavior once workers are displaced. That is, we hold constant worker productivity, job type, firm quality, and market location, and allow only the degree of employer

[20] Appendix Table A-1 provides additional information on the distribution of employment by nativity and HHI ranges. On average, non-Western immigrants are more likely to be employed in blue-collar occupations and are slightly more concentrated in the manufacturing and hospitality sectors. Occupation–LLM cells with higher HHI values also tend to have higher shares of blue-collar employment and manufacturing. However, these patterns do not differ meaningfully by nativity. Importantly, our empirical models hold these cross-sectional differences fixed: our identifying variation comes from comparisons between natives and immigrants within the same firm–occupation–industry–LLM–earnings cells. In Table A-4, we further include industry-by-relative-time and occupation-by-relative-time fixed effects, ensuring that any cross-sectional sorting cannot mechanically explain either the level or time-path differences in outcomes by HHI and nativity. As an additional robustness check, we also estimate models that flexibly interact base occupation, base industry, and base earnings with relative time and HHI. The resulting estimates are nearly identical, as shown in Table A-5.

[21] The average displacement event induces less than 3% of the local market to search for a job, such that the direct effect on HHI or general equilibrium employment demand is negligible. Further, since we are interested in the relative effect across demographic groups, this is not a concern.

market power to vary.

To estimate how displacement interacts with labor market concentration and immigrant status, we start with a baseline triple-differences model and incrementally build on it to strengthen identification and improve comparability. Each step adds structure—first by controlling for wage-relevant pre-displacement information, then by conditioning on detailed job characteristics through an exact-matching approach, then by absorbing permanent worker heterogeneity via individual fixed effects and allowing for flexible earnings trajectories across baseline income levels. This progression ensures that differences in post-displacement outcomes are not driven by pre-existing disparities in job quality, productivity, or recovery potential.

Baseline Triple-Differences Specification. Our baseline specification compares how the employment and earnings of immigrant and native workers evolve during the search process across labor markets with varying levels of employer concentration once they are induced to search by job displacement. It exploits three sources of change: changes (i) before and after job displacement, (ii) across markets with different levels of labor market concentration, and (iii) between non-Western immigrants and other workers.

For displaced individual i in local labor market l, sector (industry) s, occupation o, relative event time t, and calendar year y, we estimate:

$$\begin{aligned} y_{ilsoty} =& \alpha + \beta_1 Earn_{i,(t=0)} + \gamma_1 (P \times I)_{ity} + \gamma_2 (P \times H)_{itylso} + \gamma_3 (P \times I \times H)_{itylso} \\ &+ \mu_1 I_i + \mu_2 (I_i \times H) + \tau_t + \phi_y + \psi_m + \varepsilon_{ilsoty}. \end{aligned} \tag{1}$$

Here, y_{ilsoty} is the log of annual earnings. P is an indicator for the post-displacement period, I denotes non-Western immigrant status, and H captures whether the individual was displaced from a high-concentration labor market, based on the pre-displacement Herfindahl-Hirschman Index. We include fixed effects for being a non-Western immigrant (μNWI_i) and the interaction with base HHI, relative event time τ_t, and calendar year ϕ_y. The relative time dummies subsume any baseline "Post" effect in the model (the first difference). Standard errors are clustered at the baseline firm level.

Matching Approach. To ensure that workers are comparable at baseline, we rely on two key strategies. First, we control for baseline earnings ($\beta_1 Earn_{i,(t=0)}$), which reflect all wage-relevant information the labor market observes prior to displacement—such as productivity, outside options, and bargaining power. Second, we exactly match each worker to the same firm-occupation-industry-local labor market cell prior to displacement. We force these matched comparisons by including a fixed effect for each match stratum (ψ_m), which is an indicator for each firm-occupation-industry-LLM cell. These fixed effects force comparisons between workers who held the same job at the same firm in the same industry and location earning the same wage prior to displacement. While our baseline specification already ensures comparability across detailed pre-displacement

characteristics, this version imposes a much more stringent standard. This exact matching design implemented within a regression framework isolates post-displacement differences across levels of labor market concentration and immigrant status among workers who were observationally indistinguishable at baseline.

Variation in concentration comes across multiple dimensions: across space (some locations have a higher density of employers in general), across occupations (some occupations have more employers than others), across industries (some industries have more local firms than others), and across firms (some firms employ a different mix of occupations than others in the same industry). Our fixed effects, however, ensure that no single source of variation like local labor market size determines HHI. This assuages concerns about omitted variable bias in the dynamics of the effects being driven by, for example, city size, occupational prestige, or any other unobservable determinant of baseline earnings or search dynamics that might be correlated with baseline earnings, occupation, industry, location, or firm.

Augmented Matching Approach with Earnings-Specific Trends. Building on the matching approach above, we also include individual fixed effects (η_i), which absorbs all baseline variation in individual characteristics, including earnings, firm, occupation, industry, education, birth cohort, nativity, and local labor market. We also including additional interactions between the relative event time indicators (τ_t) and each worker's baseline earnings ($\beta_1 Earn_{i,(t=0)}$), allowing for differential earnings trajectories by initial wage level. This specification includes individual fixed effects and retains the rich set of match-stratum fixed effects (ψ_m), maintaining the conservative comparison framework described above. The added interaction absorbs potential differences in pre- and post-displacement earnings trends that are correlated with baseline wage levels, further tightening identification and addressing concerns that dynamic effects may reflect heterogeneous earnings paths rather than responses to displacement and market concentration. An alternative to this interaction would be to match on earnings decile by interacting deciles of the pre-displacement earnings distribution with the relative time indicators. This approach yields nearly identical estimates (see Table A-3). Both approaches flexibly control for and rule out any dynamic effects around displacement that might be correlated with baseline earnings if, for example, higher-wage workers would be expected to recover more quickly from a displacement event than lower-wage workers.[22] It also addresses issues related to mean reversion. Our full specification is:

$$y_{ilsoty} = \alpha + \gamma_1(P \times I)_{ity} + \gamma_2(P \times H)_{itylso} + \gamma_3(P \times I \times H)_{itylso} + \mu_1 I_i + \mu_2(I_i \times H) + \tau_t + \phi_y + \eta_i + \psi_m + \lambda_t \times Earn_{i,(t=0)} + \varepsilon_{ilsoty}. \quad (2)$$

[22] We further augment our preferred specification with additional interactions between base occupation and industry with relative time indicators in Table A-4. This means that any differential time patterns correlated with a worker's baseline earnings, occupation, or industry cannot explain any of the differential patterns across immigrant status that we find.

This preferred model compares workers who were observationally identical in pre-displacement job, firm, industry, location, and wage; controls for all stable individual characteristics; and allows for rich heterogeneity in recovery patterns by baseline earnings. It provides a transparent test of whether employer market power amplifies post-displacement earnings losses, and whether this amplification disproportionately affects non-Western immigrants due to discrimination.

Event Study. To examine dynamic patterns in earnings around displacement and assess the validity of the parallel trends assumption, we estimate an event study version of our preferred model with all of the same controls and fixed effects as Equation 2. This specification traces the evolution of outcomes in the years before and after job loss, separately for immigrants and natives in high- and low-concentration markets. Specifically, we estimate:

$$\begin{aligned} y_{ilsoty} = \alpha &+ \sum_{\tau=-5,\, \tau\neq-1}^{5} [\delta_\tau(1|t=\tau) \times H_{ylso}] + \sum_{\tau=-5,\, \tau\neq-1}^{5} [\varphi_\tau(1|t=\tau) \times I_i] \\ &+ \sum_{\tau=-5,\, \tau\neq-1}^{5} [\theta_\tau(1|t=\tau) \times H_{ylso} \times I_i] \\ &+ \mu_1 I_i + \mu_2(I_i \times H) + \tau_t + \phi_y + \eta_i + \psi_m + \lambda_t \times Earn_{i,(t=0)} + \varepsilon_{ilsoty}. \end{aligned} \tag{3}$$

There are two key benefits to the event study specification. First, the key coefficients in the model— the θ_τ terms—allow us to determine if immigrants and natives trended similarly in their outcomes in concentrated labor markets prior to separation, thus supporting the common trends assumption required for a causal interpretation of our results. Below, we discuss this assumption in detail as it pertains to our triple-difference framework. Second, by examining the dynamic adjustment path during the search period, we can differentiate between preference-based and belief-based discrimination using the insights from (Bohren et al., 2019) that productivity beliefs can adjust over time leading to closing gaps.

Post-Displacement Event Study Matching Specification. Finally, to further strengthen the comparability of displaced workers across groups, probe robustness of our findings, and uncover mechanisms behind our results, we estimate another saturated version of our preferred event study specification that ensures individuals are observed in highly similar environments after displacement. While a worker's post-displacement firm, occupation, or location are likely endogenous to the discrimination measured in our main specifications, controlling for these intermediate outcomes allows us to more precisely measure wage gaps and discrimination in the same firm even after controlling for the extensive margin of discrimination. It also allows us to rule out various other explanations and enables us to examine if the time dynamics we observe primarily occur *within* firms or *across* firms.

Using the displaced worker sample, we estimate the following specification:

$$y_{ilsoty} = \alpha + \sum_{\tau=-5,\,\tau\neq-1}^{5} [\delta_\tau(1|t=\tau) \times H_{ylso}] + \sum_{\tau=-5,\,\tau\neq-1}^{5} [\varphi_\tau(1|t=\tau) \times I_i]$$
$$+ \sum_{\tau=-5,\,\tau\neq-1}^{5} [\theta_\tau(1|t=\tau) \times H_{ylso} \times I_i] + \mu_1 I_i + \mu_2(I_i \times H)$$
$$+ \tau_t + \phi_y + \eta_i + \psi_m + \lambda_t \times Earn_{i,(t=0)} + \omega_{pt} + \varepsilon_{ilsoty}. \quad (4)$$

This is our event study model (Equation 3), estimated by matching on pre- and post-displacement strata ω_{pt}. We consider three definitions of this stratum (estimated via separate regressions): (1) the same firm, (2) the same occupation, and (3) the same occupation–local labor market (LLM) cell. The underlying comparison for this model is to examine the differences in the dynamics after displacement between two pairs of workers, each consisting of one immigrant and one native. Both pairs are displaced from the same firm, having earned the same wage while holding the same job in the same industry and local labor market. Following displacement, each pair is re-employed in: (1) the same firm, (2) the same occupation, and (3) the same occupation and location. For interpretation, it is important to remember that this control conditions on employment (being matched to a firm in the register) and thus mechanically excludes any post-displacement earnings differences between NWIs and other workers that are due to differences in the probability of being employed.

This post-separation matched event study provides a high-dimensional robustness test for our identification strategy and offers a demanding comparison of earnings trajectories among displaced workers with identical job histories and labor market placements. Matching on post-displacement firm means that upon being re-hired into the same firm, earnings differences are attributable to differential treatment by the same (new) employer despite having identical labor market signals prior to their job search. Matching on occupation and occupation-LLM removes variation in outcomes attributable to different market sorting over occupation or location.

To summarize, all specifications presented in this section estimate the same core effect but with different levels of controls and residual variation: whether non-Western immigrants experience larger earnings losses than comparable natives after displacement in more concentrated labor markets.

4.2.1 Empirical Model Assumptions

Our empirical estimation method relies on a set of key assumptions that must hold for us to interpret the results as causal. These assumptions ensure that observed outcome differences reflect genuine treatment effects of labor market concentration rather than pre-existing differences or confounding factors. Here, we outline each assumption and discuss how we validate them empirically.

The first key assumption is parallel trends. In our saturated, individual-level triple difference-in-differences approach, causal identification hinges on the assumption that, absent treatment, out-

come trajectories for the triple interaction—labor market concentration, immigration status, and time—would follow parallel paths.

Importantly, recent work in the econometrics literature (Olden and Møen, 2022) shows that identification in a triple-differences design does not require separate parallel trends assumptions for each lower-order comparison (e.g., high- versus low-concentration markets and native versus immigrant workers considered independently). Instead, the key identifying restriction concerns the stability of the higher-order interaction, $P \times I \times H$. Intuitively, the triple-differences estimator can be viewed as the difference between two difference-in-differences estimators, so lower-order biases are differenced out as long as they evolve similarly across the relevant comparison groups.

Although the common trends assumption cannot be directly verified—since the counterfactual outcomes for treated individuals are unobserved—we provide evidence supporting its plausibility. Specifically, we estimate event study models and examine whether the triple-interaction coefficients in the pre-treatment periods are statistically distinguishable from zero. In our framework, this involves estimating Equation (3) and plotting the θ_τ terms for pre-search periods. In Section 5.3, where we examine whether the observed patterns are more consistent with preference-based or belief-based discrimination, the event-study estimates likewise do not suggest any violations of parallel trends prior to treatment.

The second assumption underlying our estimation approach is that, conditional on our matching strategy—which requires displaced native and immigrant workers to come from the same firm–occupation–industry–local labor market cell, includes individual fixed effects, and controls for potential earnings-specific trends—any remaining differences between these groups do not vary differentially across labor markets with different concentration levels. Importantly, our approach does not require the displacement process itself to be unbiased between natives and immigrants overall. Rather, the causal interpretation requires that any residual differences in unobserved characteristics after this exact matching procedure—such as search behavior, skills, productivity, or preferences—are not systematically related to labor market concentration.

This assumption is less restrictive than the standard selection assumption commonly used in the displacement literature, which typically requires mass layoffs and related job loss events to be orthogonal to worker characteristics (e.g., Jacobson et al. (1993); Stevens (1997); Davis and von Wachter (2011); Couch and Placzek (2010)), including in studies from the Norwegian context (Salvanes et al. (2024); Huttunen et al. (2011b; 2018); Carneiro et al. (2023)). Although this assumption cannot be tested directly, we provide additional evidence by examining pre-displacement outcome trends. In Section 5.3, we find little evidence of such differential pre-trends, which provides additional support for this assumption.

To further probe concerns about selection into displacement events, we estimate our preferred specifications using only the subset of workers displaced due to complete firm closures, where no employer discretion exists over who is laid off. These closure events mechanically eliminate

concerns about differential selection across groups, allowing for a more stringent test of our identification strategy. The results using only closures are nearly identical to those from the full sample, and in some cases exhibit even steeper immigrant-native gaps across the concentration gradient (see Table A-22 and Figure A-5).

The third assumption required for a casual interpretation is that there are no contemporaneous shocks that differentially impact immigrants and natives in differentially concentrated labor markets at the time the job displacement events occur and that also impact the outcomes of interest (wages, employment, hours worked). More specifically, while aggregate shocks—such as macroeconomic changes or policy interventions—may affect labor markets broadly, there cannot be nativity-specific shocks correlated with job displacement events that also vary systematically with labor market concentration. To mitigate this concern, we include time fixed effects and match workers on location, industry, firm, and occupation, which absorbs many potential sources of time-varying confounding.[23]

Finally, the stable unit treatment value assumption (SUTVA) must hold. This requires that the treatment status of one worker does not influence the outcomes of others. In our context, this means that displacement events should not generate significant spillover effects across markets with different levels of labor market concentration, such as changes in local wages or labor demand, that could affect untreated workers differentially depending on whether they work in a concentrated or a non-concentrated market.

The relatively small scale of individual displacement events, which on average affect less than 3% of the local labor market annually (irrespective of the concentration level of the local market), minimizes the potential for such spillovers. In addition, we fix labor market concentration measures to the pre-displacement period, which helps reduce concerns that contemporaneous general equilibrium adjustments bias the estimates. Finally, when we directly control for the total incidence of displaced workers in the base year within each occupation-by-local-labor-market cell, the estimates remain nearly unchanged (Appendix Table A-23).

5 Results

In this section, we present our core results. We begin by discussing the effect of labor market power on employer discrimination. We then expand the framework to consider product market power, running horse races to assess the relative importance of the two sources of market power in shaping employer discrimination. Finally, we examine whether the observed patterns are more consistent with belief-based or preference-based discrimination, drawing on the framework developed in Bohren et al. (2019); Fryer Jr (2007); Fryer et al. (2013) and the dynamic adjustment path

[23] We find that controlling for relative time by occupation, relative time by industry, and relative time by occupation-industry results in nearly identical estimates (Table A-4), ruling out occupation- or industry-specific shocks as likely explanations. We show in Table A-2 that baseline concentration is not strongly correlated with observable baseline characteristics before or after conditioning on our estimation controls.

following job search.

5.1 Labor Market Power and Employer Discrimination

Difference-in-Differences Results. Table 2 shows the effect of involuntary-displacement-induced job search on employment and earnings using our matched triple differences design (Equation (1)) in columns 1-3 , our augmented model with individual fixed effects in columns 4-6, and our preferred model (Equation (2)) in columns 7-9 for the first three years after displacement. In Figure 2 (Figure A-4 for a binarized version in which we compare "high" and "low" HHI markets split at an HHI of 0.25), we show event study estimates from Equation (3) of the dynamics from five years prior to displacement until five years after displacement.

Starting with the results based on Equation (1), columns (1) through (3) show that search efforts after a job displacement result in negative employment, hours, and wage effects for natives/Western immigrants in non-concentrated (HHI = 0) markets (the $Post$ coefficient). These results are economically meaningful, with a drop in employment probability of 7 percentage points, an increase in the probability of part-time work by almost 3 percentage points, and a reduction in earnings by 0.08 log points.

The coefficient on $Post \times HHI$ reveals that the negative outcomes of a search after job displacements are approximately four times larger for natives/Western immigrants in highly concentrated labor markets compared to those in non-concentrated labor markets. This result aligns with prior research identifying the detrimental effects of labor market concentration and firm market power on earnings and employment (e.g., Dodini et al. (2024a)). The rationale underlying this result is related to the upward-sloping labor supply curve in concentrated markets that suppresses employment and wage levels relative to more competitive markets.

In terms of differential impacts on non-Western immigrants relative to other workers, the coefficient on $Post \times NWI$ shows that observationally identical non-Western immigrants are penalized in their search result in non-concentrated markets compared to natives/Western immigrants, with coefficients of -0.073 versus -0.121 in terms of employment, and -0.078 versus -0.164 in terms of log earnings. In other words, NWIs experience worse search outcomes by the amount of a 4.8 percentage point lower employment rate and an 8.6 percent earnings penalty. This indicates that even in more competitive/less concentrated labor markets, discrimination may generate labor market gaps between demographic groups.

The key coefficient of interest in our empirical analysis, $Post \times NWI \times HHI$, shows that the effect of firm labor market power has a much stronger impact on non-Western immigrants than it has on natives/Western immigrants, implying substantially larger labor market penalties for non-Western immigrants in employment effects as a function of labor market concentration. For example, the negative employment effect is over 160 percent greater for NWIs in fully concentrated markets than for natives or Western immigrants in those markets (-0.244 - 0.390). For log annual earnings, non-Western immigrants in fully concentrated labor markets earn more than 0.58 log

points less than natives/Western immigrants following displacement, despite having worked in the same firm and occupation and having similar pre-displacement earnings (and despite not being statistically significantly more likely to work part-time).

The significantly steeper effect among non-Western immigrants—who were previously paid the same as their peers for identical jobs at the same firm—suggests that labor market power enables firms to engage in differential hiring and wage-setting based on group membership. This power allows firms to set different wages for equally qualified workers, rely on group-based assumptions about productivity, or exclude workers from the disfavored group. Moreover, the ability to earn supernormal profits enables firms to absorb the costs of discriminatory hiring, whether by forgoing more productive out-group workers whose true productivity is costly to assess or due to employer bias. These profits from wage markdowns make discriminatory practices more viable in concentrated labor markets.

One alternative explanation for the baseline results is that they reflect persistent unobserved productivity differences between non-Western immigrants and natives within occupation–industry–firm–earnings cells that are also correlated with post-search labor market outcomes. Our augmented specifications address this concern in several ways. First, the individual fixed effects absorb all time-invariant determinants of labor market outcomes at baseline, whether observed or unobserved. Table 2 shows that this more saturated specification produces very similar estimates to the baseline model. Second, our preferred specification additionally controls for differential expected trends across baseline earnings levels, accounting for factors such as mean reversion or heterogeneous recovery paths by prior earnings. Taken together, these results suggest that the differential impact of labor market concentration on non-Western immigrants is unlikely to be fully explained by persistent unobserved worker differences.

Finally, the fact that the annual earnings effects are substantially larger than the corresponding employment and hours effects suggests that differential treatment may operate through both employment and wage-setting margins, consistent with the conceptual framework provided above. Even conditional on employment, non-Western immigrants experience substantially larger earnings losses following displacement in concentrated labor markets relative to otherwise similar native and Western immigrant workers.

Marginal Effects. To better visualize the estimated employer discrimination effects and examine the significance of the native-immigrant gaps in employment and earnings, Figure 1 plots the marginal effects of displacement-induced job search among natives and immigrants across the distribution of labor market concentration from our difference-in-differences estimates.

Across all three outcomes (employment, part-time status, and earnings), we observe small differences in effects between natives and non-Western immigrants in non-concentrated markets ($HHI = 0$), but substantial differences as the labor market power of firms increases. For two of the three outcomes – employment and earnings – these differences are highly statistically significant,

and the magnitude of the differences is substantial. Specifically, in the most concentrated markets, the negative employment and earnings effects among non-Western immigrants are more than twice the size of those experienced by natives. At an HHI of 0.25—classified as "highly concentrated" by the UK Competition Authority, the EU Commission, and the US DOJ—the employment gap between non-Western immigrants and natives/Western immigrants is about 15 percentage points, while the earnings gap is 0.23 log points, or 26%.[24]

The results displayed in Figure 1 are consistent with the idea that employer discrimination is amplified by labor market power, where wage-setting power and supernormal profits gives firms the ability to engage in discriminatory practices because (1) they have the power to engage in differential wage-setting and wage markdowns, and (2) they can absorb cost disadvantages associated with employer discrimination. Put differently, the results suggest that greater labor market competition substantially limits discrimination in the labor market, though it does not eliminate it entirely.

5.2 The Role of Product Market Power

We expand our empirical framework to include product market power since product market power also influences firms' ability to absorb the costs of discrimination through supernormal profits. However, unlike labor market power – which directly allows firms to set different wages for workers via the upward-sloping labor supply curve it faces – product market power alone does not allow firms to engage in wage discrimination. This is because the lack of control over the labor market means that the firm still faces a perfectly elastic labor supply, having to pay the marginal revenue product to the workers it decides to hire. However, product market power remains an important factor as it facilitates employment discrimination by allowing firms to absorb the costs of employing less productive workers from a specific group through supernormal profits.

The results from running a horse race between local labor market concentration and workers' base industry revenue concentration are shown in Table 3.[25]

Interestingly, the coefficients on $Post \times RevenueHHI$ are positive for employment and earnings, suggesting that workers in more concentrated product markets may experience somewhat better average post-displacement outcomes overall. One possible explanation is that concentrated product markets are characterized by larger or more stable firms, higher rents shared with workers, or lower search frictions following displacement. Importantly, however, these average effects do not translate into larger immigrant-native gaps once labor market concentration is accounted for.

Specifically, product market power by itself is not associated with economically meaningful or statistically significant effects on the employer discrimination practices exercised by the firm: none

[24] While relatively few workers are exposed to extremely concentrated markets, these thresholds are widely used by competition authorities and provide a useful benchmark.

[25] In our data, 2-digit industry revenue concentration and local labor market concentration are moderately positively correlated at 0.17 (see Figure A-12).

of the coefficients on $Post \times NWI \times RevenueHHI$ are statistically significant in the expected direction, and each is not economically meaningful. Notably, none of the point estimates for the occupation-based employment HHI significantly change from Table 2. As a robustness check, we also consider an alternative measure of product market power based on local industry revenue concentration. Because we do not observe exactly where firms generate sales geographically, we allocate national revenues across locations in proportion to each firm's employment footprint. Using this measure, we construct local industry revenue HHI at the LLM level and re-estimate our models. The results are nearly identical to those using national revenue concentration, reinforcing the conclusion that product market power is unlikely to explain the observed discrimination patterns (Appendix Table A-10).

This finding is consistent with the notion that firms are limited in their ability to discriminate based solely on product market power. Specifically, unless a firm also controls part of the labor market, it cannot practice *wage discrimination* because workers with a perfectly elastic labor supply will simply seek better opportunities elsewhere. While it is theoretically possible for firms with product market power but no labor market power to engage in *employment discrimination*, we see no evidence of this in the Norwegian context. This suggests that the source of employer power is critical in explaining labor market discrimination and helps clarify the role of different sources of firm power in shaping labor market discrimination.

A natural question to ask is how these results align with prior work on product market power and employer discrimination, which typically do find a non-negligible relationship (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)). One possibility is that those results are driven by a strong within-firm correlation between labor market power and product market power (Marinescu et al., 2021a), such that those effects are actually coming from unobserved labor market power held by these firms. This is consistent with concurrent theoretical work on the topic suggesting that, for example, import competition disciplines local labor markets only when firms hold labor market power (Mertens, 2020).

5.3 Dynamic Effects and Discrimination Type

Having examined employer discrimination in the Norwegian economy and documented how it varies with labor market power, the next important step is to better understand the source of that discrimination—whether it is driven by preferences or beliefs. To better understand whether the observed patterns are more consistent with belief-based or preference-based discrimination in our setting, we draw on insights from Bohren et al. (2019); Fryer Jr (2007); Fryer et al. (2013) and analyze the dynamic adjustment path over time following displacement episodes. If our findings are rooted in belief-based discrimination, continuous employer interactions with workers from the disfavored group should lead earnings and employment gaps to narrow over time—even in the

presence of labor market power.[26] Conversely, if discrimination is driven by preferences (i.e., taste-based discrimination), these gaps are likely to persist over time, at least in the short run, as preferences are far less flexible than productivity beliefs in the face of new information.[27] Results from our event study regressions (Equation (3)) are shown in Figure 2.

The event studies reveal significant gaps in employment rates and earnings between natives and non-Western immigrants during the first two years of the search period.[28] However, these gaps gradually narrow, and by five years after the initial search activity, there are no statistically significant differences in labor market outcomes between the two groups. This dynamic adjustment path is more consistent with employer discrimination rooted in incorrect beliefs, suggesting that continuous exposure to workers can rectify possible misconceptions about productivity and lower the cost of acquiring correct information about individuals' productivity. It is also worth noting that the relative pre-treatment trends are moving in a parallel fashion across all outcomes. This evidence is consistent with the common trends assumption required for a causal interpretation of our findings.[29]

The gradual convergence in immigrant-native outcomes following displacement is consistent with employer learning models, such as Altonji and Pierret (2001); Fryer et al. (2013); Lepage (2024), in which firms initially rely on group-based priors but update their beliefs over time through direct performance observation.[30] This dynamic is more consistent with discrimination rooted in inaccurate beliefs than with persistent taste-based preferences. To ensure that continuous interactions with employers from the same firm rather than networks or external factors drive this result, we also estimate our event study model with *current* firm fixed effects in Figure A-2. We observe nearly identical time paths to Figure 2. Controlling for this intermediate outcome/collider (current

[26] As Fryer et al. (2013) phrase it, "Over time, however, employers learn workers' true marginal product... within the firm, racial differences in wages should narrow with tenure."

[27] Current evidence suggests that tastes shifting enough to elicit changes in taste-based discrimination behavior typically require extreme circumstances such as violence or war (e.g. Moser (2012)). In experimental settings, subjects quickly update their beliefs in the face of new information about quality without necessarily changing their underlying preferences, even in the case of contentious inter-religious or inter-caste conflicts (e.g. Moorthy (2024)).

[28] We note that the coefficients on periods t-5 to t-1 are not statistically significant and are small relative to the post-separation effects in concentrated markets. Despite the fact that the trend is positive and would bias our difference-in-differences estimates toward zero, it is worthwhile to consider controlling for this phenomenon. To do this, we also match on the t-5 to t-1 change in earnings for each worker and estimate both Equation 2 and 3, controlling for this change interacted with relative time indicators, which is similar to our earnings by relative time interactions. The resulting estimates in Table A-6 closely match those in Table 2 and are slightly larger in magnitude. The time dynamics in Panel D of Figure 2 nearly identically match those in Panel C.

[29] The resulting pattern is the same when we instead estimate the interaction with a binary indicator of being in a "highly concentrated" market, using the US Department of Justice threshold of 0.25 (Appendix Figure A-4). Note that similar thresholds have been used both by the UK Competition authority as well as the European Commission when discussing mergers and acquisitions with respect to labor market power.

[30] Following the idea of asymmetric information (Kahn, 2013), the worker's base firm may have private information about immigrants' productivity that is lost after the displacement. That the market is slow to uncover this information *only* in concentrated markets underscores the fact that labor market competition gives firms a strong incentive to uncover all relevant productivity information faster. This leads to faster dissemination of private information to the full market. Thus, less private information in a competitive labor market is likely a function of competition itself.

firm) conceptually limits comparisons to instances where a native is hired in the *same firm* as a non-Western immigrant after their search and where both were initially separated from the same firm.[31] This result suggests that much of the convergence in earnings gaps occurs within firms over time. Furthermore, in Panel C of Figure A-2, we observe no systematic difference in the firm fixed effect of destination firms for NWIs in high HHI markets. This suggests that differences in firm quality at the destination are unlikely to explain any post-displacement wage differences between NWIs and other workers.

To collect further evidence of preference-based and belief-based discrimination, we conduct three auxiliary analyses: (a) we use information on managers to measure their past experience with non-Western immigrants in their workplace and neighborhood as outcomes in our models (Panel D of Table 4); (b) we examine discrimination patterns toward second-generation non-Western immigrants and Western immigrants (Panels A and B of Table 4); and (c) we assess discrimination against non-Western immigrants who have been in the country the longest (Panel C of Table 4). Our findings show that managers with more experience working with non-Western immigrants–i.e. those more likely to possess accurate information about the productivity of non-Western immigrants–are less likely to engage in discrimination and are disproportionately more likely to hire non-Western immigrants in concentrated labor markets. The firms that hire non-Western immigrants after the layoff have managers with more past work experience with non-Western immigrants. This is consistent with the idea that these firms hold more accurate beliefs—shaped by prior exposure (Lepage, 2024) and private information about immigrant productivity (Kahn, 2013)—and therefore rely more heavily on this information in concentrated markets. However, these managers are not meaningfully different in their non-work contact with non-Western immigrants as proxied by the managers' neighborhood composition (Column 3 of Panel D). We interpret this as evidence that these managers are not likely to have different taste preferences for personal contact with non-Western immigrants based on their non-work surroundings. Furthermore, we show that non-Western immigrants who have resided longest in the country, Western immigrants, and second-generation non-Western immigrants—i.e. immigrants that look more similar to natives on a range of observable characteristics and for whom there exists more information that the employer can extract—face little or no employer discrimination on the dimensions we examine.

The findings from our auxiliary analyses are consistent with belief-based discrimination, where decision-makers rely on inferences from group averages in the absence of full information, and managers with more prior experience possess better signals of productivity as a result of their experience. These results align well with our analysis of the dynamic adjustment path of employment and earnings gaps post-displacement.

[31] Generally, conditioning on a collider results in a "bias" in that it removes the effect of a treatment variable operating through the mediator being controlled for–discrimination on the hiring margin. However, it is still useful because we are interested in the effects of discrimination within the same post-displacement firm.

5.3.1 What Are the Costs?

To a non-Western immigrant who is subject to these discriminatory labor market dynamics, the costs are clear: as they search for a job after an unexpected employment shock, they cumulatively lose a significant amount of employment experience and income relative to their similarly productive native peers for at least three years despite eventually closing these gaps. Imperfect information about the productivity distributions of these groups as well as little information about individual productivity weigh significantly on the opportunities available to these workers.

These dynamics can also be costly to the firm. Although we observe that discrimination gaps narrow over time, we find strong evidence of costs to allocative efficiency and productivity. To examine this, we analyze the quality of firms' new hires. To measure quality, we estimate an AKM model of earnings with worker, firm, and year fixed effects and then extract the worker fixed effect for each non-displaced new hire at each firm.

We use these worker fixed effects in two ways. First, we calculate the average worker fixed effect at the firm level for new hires, split between native Norwegians and NWIs. We then estimate our event study model (Equation 3) using these firm-level means as outcome variables. Although NWIs are not hired into higher-quality firms (Figure A-2, Panel C), Figure A-11 shows that, relative to Norwegians in concentrated labor markets, NWIs in the same markets are hired by firms that are better at screening, attracting, and ultimately employing higher-quality Norwegian and NWI workers. In other words, even though NWIs are similarily productive as their native peers, in labor markets where discrimination is less costly due to concentration, the marginally hired NWI is brought into a cohort of peers who are, on average, higher quality than the marginal Norwegian hire. Conversely, firms that may have discriminated against NWIs in high-HHI environments tend to have marginally lower-quality Norwegian new hires.[32]

Second, we next calculate the gap in the average worker fixed effect among new hires for firms that were likely discriminators versus those that were not. We define likely "positive discriminators" as those that hire a displaced non-Western immigrant but no displaced Norwegian, and we examine the average worker fixed effect of their NWI new hires compared to other firms. While this evidence is only suggestive, it is supportive of the results in A-11.

We find suggestive evidence that "likely discriminator" firms, on average, hire less productive non-displaced Norwegians (as measured by their worker fixed effects) than firms that are not likely discriminators (Appendix Figure A-13). Conversely, likely "positive discriminators" tend to hire more productive non-displaced non-Western immigrants than those that are not positive discriminators. Thus, firms appear to hire workers with lower estimated worker effects when they either (1) rely on imprecise inferences about productivity, perhaps based on erroneous beliefs, or (2) hold prejudices that prevent them from hiring members of the outgroup. Such practices can result in

[32]The results from directly controlling in our regression for the quality of the worker's peer at their destination firm are indistinguishable from Figure A-2. Peer quality cannot explain the convergence in wages we find.

suboptimal workforce compositions, which ultimately hinder overall productivity in the labor market as well as firm performance. These findings align with recent audit studies showing that racial gaps in the callback rate for those with distinctively black names are negatively associated with firm profits per worker at large US employers (Kline et al., 2022).

6 Robustness and Alternative Mechanisms

In this section, we systematically stress-test the interpretation of our main results by examining a broad set of alternative mechanisms and identification concerns. The matched triple-differences specification, together with a series of highly saturated robustness specifications, allows us to assess whether the concentration gradient in immigrant-native labor market gaps can plausibly be explained by differential sorting, search behavior, labor market attachment, match quality, neighborhood characteristics, occupational mobility, labor demand shocks, or other correlated baseline differences.[33]

For an alternative mechanism to explain our results, it would not be sufficient for natives and immigrants to differ on that dimension on average. Instead, those differences would also need to vary systematically with labor market concentration. For example, if differences in job search behavior or social networks were driving our findings, then immigrant-native differences in search intensity, search quality, or network access would need to be substantially larger in concentrated labor markets than in competitive ones. In addition, these differences would need to operate independently of the detailed baseline characteristics absorbed by our matching strategy and robustness specifications, including local labor markets, occupations, industries, firms, and baseline earnings.

The goal of this section is to carefully evaluate which interpretations are most consistent with the full set of empirical patterns in the data. To provide a transparent summary of the alternative mechanisms considered in this section, **Table C-1** presents each interpretation alongside the corresponding empirical evidence and discussion.

Differential Search. Are the effects across the concentration gradient driven by less intensive or different job search behavior among non-Western immigrants in highly concentrated markets? Specifically, could it be that immigrants search less efficiently than natives—due to weaker job networks, language barriers, or spatial mismatch—leading to slower transitions up the job ladder (Helm et al., 2023)? Nine pieces of evidence suggest this is unlikely. First, the earnings gaps persist even after controlling for destination firm fixed effects, and we also do not find any system-

[33] Our additional specifications include: fully saturated interactions between base industry, occupation, and firm with relative time indicators (Table A-4); interactions between these and HHI and relative time (Table A-5); matching on *firm-specific tenure* of at least three years, age, and neighborhood (Table A-8); a separate split between Eastern Europeans and non-Western immigrants from outside Eastern Europe, which reveals no statistically significant differences (Table A-9); and conditioning on at least five years of full-time work experience (Table A-17). These analyses make it unlikely that our findings are driven by mechanisms related to ex ante sorting or selection on overall labor market attachment, labor market slack, urban/rural social divides, neighborhood, occupation, industry, firm, age, firm-specific tenure, mean reversion, different places on the job ladder, match quality, continental European background, market concentration, and any other baseline characteristics correlated with these dimensions.

atic sorting into firms with lower firm fixed effects in more concentrated markets. If differential search effort among natives and NWIs in concentrated markets were a key factor, we would expect pronounced sorting across firms and that immigrant-native gaps across the concentration gradient would be eliminated once we condition on the firms workers sort into after being induced to search. Furthermore, if NWIs were searching less intensively yet still ended up at the same firms as natives, this would imply positive selection on unobservable characteristics into these firms for immigrants. In that case, we might even expect non-Western immigrants to earn more conditional on employment. However, our core findings of negative labor market gaps in concentrated markets persist even among natives and immigrants with similar productivity signals displaced from the same occupations and firms, who re-enter the labor market at the same new firm (Figure A-2).

Second, we observe no significant differences in the quality of destination occupations or local labor markets across natives and non-Western Immigrants in the more concentrated markets. If natives and immigrants engaged in differential search, we would expect this to manifest in the quality of the occupations or local markets where they find employment. The absence of such differences suggests that search effort is not driving our findings (Figure A-3).

Third, an informative placebo test is to examine workers laid off from the public sector. In this setting, there is limited scope for discrimination due to highly structured hiring procedures, strong oversight of recruitment practices, and the absence of a profit motive. However, job search behavior could still matter—so if systematic immigrant–native differences in search across market concentration levels were driving our results, we should observe similar negative HHI effects here as when we consider those attached to the private sector at baseline. We find no differential effects between natives and immigrants in concentrated markets when considering those attached to the public sector, which is inconsistent with this broader search explanation (Table A-20).

Fourth, our results do not change when we include neighborhood by relative time fixed effects. This specification subsumes all correlates of spatial characteristics associated with a worker's neighborhood, including distance to job centers or neighborhood networks. This rules out spatial mismatch as a determinant of search (Table A-8).

Fifth, prior research highlights financial assets and consumption smoothing as critical factors influencing job search, with greater savings contributing to lower search effort (Lentz and Tranaes, 2005; Lammers, 2014). Despite comparable productivity and earnings before displacement, NWIs hold fewer financial assets than natives or Western immigrants (Appendix Table A-21). This financial disparity suggests that NWIs are incentivized to search more intensely, not less. Such behavior contradicts the differential search hypothesis necessary to explain our results—especially regarding the extensive margin employment findings.

Sixth, non-Western immigrants are more mobile across space than their native counterparts, particularly in areas with higher levels of labor market concentration. In other words, immigrants tend to engage in wider, not narrower searches. Figure A-6 shows the relationship prior

to displacement between measures of mobility (the probability of moving residences, working in a municipality other than one's home municipality, and beginning work in a new municipality in a particular year. Examining these differences in mobility as outcomes in our regression models in Table A-7 reveals that in high HHI labor markets, displaced non-Western immigrants are more likely to move residences to another municipality and to get a job in a new municipality. While the point estimates imply large effects relative to the $Post \times HHI$ interactions, the differences are not statistically significant. Thus, NWIs in highly concentrated labor markets are at least as as mobile and have a search radius as least as wide as their native counterparts.[34]

Seventh, consider the possibility that search behavior or effectiveness differs for workers with longer tenure at their pre-separation firm. Longer tenure could give employers clearer signals of a worker's ability—either through a more consistent record of past performance or through stronger workplace networks—which might, in turn, affect how these workers fare in the post-separation labor market. Tenure also might signal different places on the job ladder. If this were the case, we would expect the relationship between labor market concentration (HHI) and post-separation outcomes to differ for high-tenure versus low-tenure workers. However, when we interact tenure length with HHI, we find no such differences in the concentration gradient (Figure A-7). This indicates that conditioning on tenure—whether to account for stronger signals of productivity or better networks—does not alter our main results, making it unlikely that our findings are driven by differences in search effectiveness and instead pointing toward discrimination as the explanation.

Eighth, if differences in search behavior were the key driver of our results, we might expect heterogeneity within the non-Western immigrant (NWI) group. In particular, NWIs from former Soviet bloc countries in Eastern Europe could plausibly differ in search methods or network access from NWIs originating outside Europe, potentially making them more similar to native Norwegians or Western immigrants (e.g., workers from Poland compared to East Africa). If search behavior mattered, the concentration–outcome relationship should differ between these two subgroups. In practice, we find no statistically significant differences in effects between Eastern European NWIs and NWIs from outside Europe (Table A-9). This makes it unlikely that differences in search behavior are driving the NWI–native gap, and is more consistent with discrimination than with differential search behavior as the underlying mechanism. Consequently, these results also support the argument against discrimination being due to animus if members of the in-group hold more animus against those from ethnic backgrounds even further away from their own or based on phenotype or ethnicity.

Finally, if differential search efficiency, match quality, job ladder differences, or cyclical labor market slack were driving the closing of the gap between natives and immigrants, we would expect non-Western immigrants in concentrated markets to have more frequent firm-to-firm tran-

[34] Schündeln (2014) finds evidence that migrants are more mobile and more responsive to labor market differentials than natives in Germany, implying wider search areas.

sitions as they continue on-the-job search to attempt to leverage wage gains from the marginal search.[35] However, we do not see any evidence of this behavior, as differences between NWIs and other workers in concentrated markets are not statistically different in any year before or after displacement (see Appendix Figure A-9).[36]

Recent work raises the possible importance of match effects when considering earnings at baseline as a suitable measurement of productivity because a worker's wages at firm A might be different from those at firm B for reasons unrelated to the fixed firm and worker components of that match. To robustly test if such match effects might explain our results, we follow Sorkin and Warwar (2026) and implement a paired movers design directly in the Norwegian employer-employee dataset. After residualizing log earnings on a quartic in age and year fixed effects and implementing their IV strategy, we obtain a baseline coefficient of 0.782, and, scaling by the signal to noise ratio yields a variance of the match effect of 0.0281, a value comparable to their results in Brazil (see Table A-18). This means that match effects, in general, may play a notable role in the Norwegian labor market. However, the critical test for our setting is whether the portability of wages differs across concentration for non-Western immigrants, or in other words, if there is a negative HHI gradient for this group. We test this in a triple interaction in column 2 in which we interact base firm wage gaps on the right hand side with HHI, with an NWI indicator, and with $NWI \times HHI$. For match effects to explain the negative earnings trajectory, we would expect $NWI \times HHI$ to be strongly negative and significant. When we run this specification, we find a baseline value of 0.806 for Norwegians at HHI = 0. For NWIs in competitive markets, this is reduced by a value of -0.085, consistent with NWIs possibly being precluded (via discrimination or other pathways) to more advantageous matches on a consistent basis. Critically, the triple interaction between the origin wage gap, NWI status, and HHI is positive and significant at 0.332, meaning NWI wages become relatively more portable as market concentration increases. This is opposite-signed relative to what would be expected if match effects explained the HHI gradient in our empirical models.[37]

[35] Measures of local unemployment benefit receipt, which proxy for labor market slack, are, at most, only weakly correlated with HHI at baseline, and conditional on our matching exercise, are negatively correlated.

[36] Another search option for displaced workers is self-employment. In our sample of displaced workers, only about 3% of workers are self-employed in any given year after displacement. In our models, there is no statistically significant difference across concentration for NWIs in the likelihood of self-employment (coefficient of 0.011 with a standard error of 0.009 for the $NWI \times HHI$ interaction, and a coefficient for post $\times$ NWI of -0.002 with a standard error of 0.001). Another theoretical option is a return to their county of origin. However, according to the address and migration register, only 38 NWIs from our layoff sample emigrated from Norway after the layoff event, making this an extreme rare outcome. This is unsurprising given the high levels of attachment to the Norwegian labor market in our sample.

[37] Similar to the paired movers design, our specifications with post-displacement firm fixed effects non-parametrically control for fixed worker characteristics as well as baseline and destination firms. The convergence in earnings occurs primarily within destination firms rather than through mobility across firms (see Figure A-2). Explaining these convergence patterns through match quality would thus require a time-varying within-firm improvement affecting immigrants but not natives, and only in concentrated markets. We discuss match effects further in Table C-1.

Occupational Sorting. Could these effects be explained by differences in the types of occupations that non-Western immigrants versus natives enter in more concentrated markets relative to less concentrated markets before or after displacement? Evidence suggests this is unlikely.

Are non-Western immigrants differentially likely to *ex ante* sort into higher HHI occupations and markets relative to their native and Western immigrant counterparts, and might this explain our results? To test for this, we use the baseline distribution of local labor markets, occupations, industries, education, and earnings to construct a Generalized Propensity Score (Hirano and Imbens, 2004). We estimate baseline occupation HHI for NWIs via OLS as a function of education dummies (less than high school, high school, and Bachelor's degree or more), industry, occupation, and local labor market dummies, and base annual earnings. We use these parameters to predict HHI for the entire sample and use these predictions to construct a generalized propensity score for the whole sample from the predicted values and residuals of the selection model for non-Western immigrants. We then include this GPS and its square as controls in our model. This exercise controls for the possibility of baseline differential sorting into occupations, labor markets, industries, and baseline earnings across the HHI distribution for non-Western immigrants. The results suggest that there is minimal selection on these observables over HHI by non-Western immigrants. Such selection cannot explain our results. If anything, the inclusion of the GPS in the regression strengthens makes the point estimates for the interaction between NWI and HHI stronger, though none of the coefficients is statistically difference from our preferred model.

Finally, we note that in an AKM model setting, worker fixed effects tend to decline over HHI similarly for non-Western immigrants and their native counterparts, suggesting that *differential* ex ante sorting of lower-ability immigrants into higher-HHI markets does not play a notable role (see Appendix Figure A-8).[38]

In terms of occupational sorting after displacement across the HHI distribution, Appendix Table A-13 demonstrates that our core findings are not driven by systematic differences in the rank or earnings potential of the occupations that non-Western immigrants take up post-displacement relative to natives in more versus less concentrated markets. Neither the occupational rank of the workers nor the occupation fixed effect in an AKM-style regression significantly changed after their search. Instead, the differences in wage earnings for NWIs stem from disparities in the compensation they receive for performing the same job as their native/Western immigrant counterparts. This suggests that the issue lies less in occupational sorting and more in the valuation of their labor within these occupations.

[38]Sorkin and Warwar (2026) note that in an AKM model, worker effects may partially reflect match history. If NWIs face worse match access in concentrated markets due to discrimination, their worker effect will be more downward-biased as HHI rises. This makes Figure A-8 a conservative test of productivity selection. Contamination of the worker fixed effect from match effects may reflect discriminatory restrictions on match access throughout a worker's career, in which case lower estimated worker quality is itself a manifestation of discrimination rather than an alternative explanation.

Furthermore, when we control for current occupation and local labor market conditions in the post-separation period, we find that the dynamics mirror our main results (see Appendix Figure A-3 and its resemblance to Figure 2). This robustness indicates that occupational sorting does not play a central role in driving the observed gaps.

Finally, even when we introduce additional controls for firm-level effects in the period after the initial separation event—accounting for both the occupation, local labor market, and destination firm—we continue to observe significant earnings gaps for non-Western immigrants. These gaps are most pronounced in the first few years after displacement, gradually narrowing over time, particularly in highly concentrated labor markets. This pattern further supports the conclusion that differences in compensation, rather than occupational sorting, are the primary driver of the observed earnings gaps.

Social Networks/Capital and Homophily. Considering the critical role of referral networks in job search (e.g., Dustmann et al. (2016); Pallais and Sands (2016)), another potential explanation for the results we find is that natives and NWIs have, and utilize, social networks differently in concentrated relative to non-concentrated markets. We believe this is unlikely because the persistence of earnings gaps that we document across the concentration gradient persist even after conditioning on the firm or occupation where separated workers are hired. This suggests that referral networks are unlikely to be the primary driver in shaping the search-induced gaps that we uncover.

To further examine this mechanism, we analyze the role of social capital by estimating models using manager demographic characteristics as outcomes (Appendix Table A-11). This exercise is designed to test whether there is evidence of preferential hiring or clustering of non-Western immigrants under demographically similar managers, which might suggest a role for homophily or social network effects. The analysis indicates that there is no systematic relationship between the demographic attributes of managers and the rates at which NWIs are hired after their search. This finding strengthens the argument against the idea that immigrant homophily and within-group social networks are driving different rates of employment or wages.

Additionally, we examine whether neighborhood characteristics influence outcomes by estimating models separately for workers whose neighborhoods at the time of their search have high or low shares of workers in management occupations (Appendix Table A-14) and high or low shares of NWIs (Appendix Table A-15), with "high" and "low" determined based on the sample median. This analysis aims to uncover whether access to neighborhood-level social capital, either through proximity to managerial connections or immigrant communities, impacts the observed outcomes. Across both models, there is no economically meaningful or statistically significant difference in the coefficients for the $NWI \times HHI$ interaction term between subgroups. Interestingly, non-Western immigrants residing in neighborhoods with a higher concentration of managers or natives/Western immigrants experience slightly *worse* outcomes in concentrated labor markets compared to those with fewer management-focused neighbors, despite potentially having greater

social capital advantages.

Finally, we also investigate the characteristics of the firms that hire workers after their search in Appendix Table A-16. This reveals that there is no systematic sorting of non-Western immigrants in concentrated labor markets into firms that have more non-Western immigrant employees or that differ in the average age of workers at the firm.[39] In other words, workers are not sorting into firms with workers with similar backgrounds. Even though natives are less likely to work with a neighbor after displacement, NWIs are just as likely as natives to be employed at the same firm as a neighbor, regardless of market concentration. This suggests that social capital through neighborhood connections does not play a significant role in their employment outcomes. There is suggestive evidence that non-Western immigrants may be more likely to be hired by larger firms as a result of their search. This aligns with the idea that larger firms may have more formalized hiring process that are a step removed from the immediate preferences of owners and other managers via a formalized human resources department. However, the estimates are imprecise. Finally, Panel C of Figure A-2 shows that the firm fixed effects of destination firms—capturing average pay premiums in an AKM framework—do not systematically differ for NWIs in highly concentrated markets. This suggests that differences in the quality of hiring firms, at least as measured by wage premia, do not explain any observed post-displacement earnings gaps for NWIs in these areas.

These results suggest that social networks and homophily do not appear to drive the native-immigrant search-induced labor market gaps in concentrated relative to non-concentrated markets, helping to strengthen our interpretation that these effects are driven by discrimination arising from firm power in labor markets.

Selection of Separated Workers. Our estimate use mass layoffs and firm closures to induce search in our empirical models. While firm closures result in all workers in the firm being forced to search for a new job, mass layoffs involve some choices on the part of the worker's baseline firm. Because of this, there could theoretically be a difference between immigrants and natives laid off from the same firm in some characteristics the firm can observe but that is unobserved in the data. To address this, we estimate our preferred model using only those displaced via a firm closure. These results are in Table A-22. The results show that in the case of firm closures, the HHI gradient in re-hiring rate for non-Western immigrants and natives/Western immigrants is even steeper than in our overall model, though the differences from our main estimates are not statistically significant. This implies that any underlying (negative) selection process of non-Western immigrants during mass layoffs does not systematically vary over HHI, let alone enough to explain our results. Figure A-5 confirms the time dynamics of both forms of search-inducing separation lead to similar time dynamics in the point estimates, with some additional noise from a smaller sample size.

[39]This result in our setting contrasts with Agan and Starr (2020) but aligns with the results on employer neighborhood characteristics in Kline et al. (2022).

Contact Hypothesis. So far, the alternative explanations discussed in this section have primarily explored various potential mechanisms underlying the relationship between competition and discrimination. We interpret the evidence presented as strongly pointing to employer discrimination as the primary mechanism driving the concentration gradient gaps we document.

Another key finding of this paper, building on insights from Bohren et al. (2019) and supported by a range of robustness tests, is that this discrimination appears to stem from incorrect beliefs about the productivity of non-Western immigrants. Here, we consider an alternative explanation for the dynamic adjustment we observe: whether the narrowing of the earnings gap between NWIs and other workers in concentrated markets over time could result from changes in firm or management preferences driven by increased exposure to non-Western immigrant coworkers.

While we cannot directly observe preferences, this contact hypothesis explanation seems unlikely for two key reasons. First, if non-work contact with non-Western immigrants influenced managerial preferences, we would expect managers living in neighborhoods with a higher share of NWIs (a measure of revealed preference) to be more likely to hire these workers early in the post-layoff period (i.e., within the first 2–3 years). However, Panel D of Table 4 shows no significant relationship between neighborhood composition and the likelihood of non-Western immigrants being hired, suggesting that non-work contact does not play a role.

Second, if initial discriminatory preferences among managers influenced outcomes but evolved over time, we would expect to see differential occupational sorting within firms during the early post-layoff period. Specifically, disfavored workers would likely be placed in lower-earning roles (possibly with less direct contact with management or coworkers), with subsequent adjustments as preferences changed. However, the evidence indicates that differential occupational sorting does not account for the observed results, even in the initial years following displacement.

The contact hypothesis, if applicable in this context, would require firms to act on their initial animus against NWIs in setting NWI wages lower than their native/Western immigrant coworkers, resulting in wage discrimination after hiring them. Over time, continuous exposure and interaction would then shift these preferences and reduce their disutility. However, this explanation would have to entail an unusual dynamic: firms in concentrated labor markets that hire NWIs at lower wages would need to harbor biases strong enough to discriminate on wages after initially hiring them to recoup their utility losses, but those biases are *not* strong enough to prevent them from incurring those utility losses by hiring out-group members in the first place.

Taken together, the lack of evidence for neighborhood-based non-work contact effects, the absence of differential occupational sorting, and the implausible nature of the hypothesized mechanism suggest that changes in firm or managerial preferences are unlikely to explain the observed narrowing of the earnings gap.

More Severe Labor Demand Shocks. Another possibility is that non-Western immigrants and natives were employed in occupations and locations that experienced different levels of local

labor demand shocks, which initially triggered the firm closures or layoffs. If these shocks were systematically correlated with search outcomes for non-Western immigrants in concentrated markets, they could potentially explain our findings. While we consider this scenario unlikely, we test for it directly in Table A-23. To account for this, we control for the total number of workers who were separated due to mass layoffs or firm closures in each local labor market-occupation cell, measured as a share of total workers, in every base year. Additionally, we allow this shock size to differentially impact post-displacement outcomes by interacting it with a $Post$ indicator. The results remain virtually unchanged, confirming that differences in the size of local labor demand shocks do not influence our conclusions.

Differential Take-up of Social Assistance Programs Another potential concern is that non-Western immigrants may be more likely to rely on social assistance programs during their job search, which could either prolong their search or lead them to exit the labor force entirely. However, for this to explain our findings, not only would NWIs need to have higher take-up rates of these programs, but this pattern would also need to differ systematically between concentrated and non-concentrated markets. To investigate this, we analyze social program participation in Table A-24. We find no statistically significant differences between NWIs and other workers in concentrated markets that would suggest program participation is responsible for our results. Furthermore, at baseline high HHI markets do not exhibit stronger labor market slack as proxied by per-capita unemployment benefit receipt, and, conditional on our matching procedure, are *negatively* correlated with slack.

Looking more closely, the results suggest the opposite of what would be needed for social assistance to explain our findings. In column 2, the relationship between unemployment benefit take-up and non-Western immigrant status is weaker than their employment effect in Table 2. This means that, among those who are not employed, NWIs in concentrated markets may actually be *less* likely to claim unemployment benefits compared to natives, despite being equally eligible under our sample construction. Though we cannot statistically rule out effects proportional to their employment disadvantage, this pattern runs counter to the idea that program participation prolongs their job search.

Similarly, in column 3, we show that non-Western immigrants are consistently less likely across all levels of market concentration to receive disability pension payments, which are typically linked to partial or full labor force exit. If anything, this suggests that NWIs are more likely to continue searching for work rather than exiting the labor force entirely. Together, these findings indicate that differences in social program participation not only fail to explain our results but, if anything, would push against the patterns we observe in workers' employment outcomes.

Finally, we estimate our main models using an alternative measure of labor market power, namely estimated wage markdowns, for certain sectors. Following Yeh et al. (2022), we estimate a Cobb-Douglas production function of firm-level revenue using capital, labor, and materials inputs.

Assuming that raw materials are a flexibly supplied input, we can separate wage markdowns in sectors that make significant use of materials, namely manufacturing, construction, and mining. We estimate these production functions for each industry separately and then calculate the average wage markdown in each occupation-LLM cell across all blue collar occupations and replace HHI with this average local wage markdown in our main model.[40] The results in Table A-25 show that a 1 unit increase in markdowns reduces earnings and employment for NWIs by 6.1% and 3.4 percentage points, respectively. These effects are nearly identical to a 0.10 unit change in HHI in our main models. This exercise provides secondary support of our identification strategy and our use of concentration as a measure of labor market power.

7 Discussion

Discrimination remains a persistent and widespread issue in many labor markets. While theory suggests that competition should reduce discrimination by increasing its costs and driving discriminatory firms out of the market, real-world markets rarely operate in perfect competition (Card, 2022). Most firms possess some degree of market power—both in setting prices and wages. Despite extensive theoretical discussions about the relationship between market power and discrimination, direct empirical evidence on the role of *labor market* power remains limited, leaving a critical gap in our understanding of how market power shapes discrimination.

We document substantial immigrant-native gaps consistent with employer discrimination in the Norwegian labor market and that greater labor market competition appears to substantially limit these gaps. Additionally, we show that the source of employer power is critical. Specifically, the patterns we document are concentrated among firms with substantial labor market power, and product market power alone has little impact on employer discrimination. We show that this is likely because firms with labor market power can set wages and absorb the costs of discrimination through supernormal profits, allowing discrimination to persist. Conversely, employer discrimination is constrained in firms with product market power but no labor market power, as these firms lack wage-setting power. While they can still engage in employment discrimination by hiring more from the preferred demographic group, we find no evidence of this once we account for labor market power.

Finally, our analysis suggests that the observed patterns are more consistent with inaccurate beliefs about immigrant productivity than with persistent taste-based preferences. Although discrimination gaps eventually close, we find evidence consistent with meaningful costs to allocative efficiency and firm productivity in addition to the obvious costs to workers.

In core economic theory, social well-being is maximized when resources are allocated efficiently and workers are hired and compensated based on their productivity and merit. Under-

[40]The markdowns are calculated as the wedge between the output elasticity of labor and the labor share of revenues divided by the same wedge as applied to *materials* inputs. See Yeh et al. (2022) for more detail.

standing the factors that hinder this process is therefore of key policy relevance. In particular, it is crucial to examine the relationship between employer power and discrimination, identify the specific sources of employer power, and distinguish between preference-based and belief-based discrimination. Clarifying these dynamics can help policymakers address unfair compensation practices, mitigate persistent inequalities across groups, and promote broader social goals such as market efficiency and economic growth.

In terms of policy implications, our findings underscore the importance of addressing labor market power in efforts to reduce employer discrimination and close persistent wage and employment gaps. While competition can mitigate discriminatory behavior, firms with substantial labor market power retain the ability to sustain discrimination through wage-setting and profit absorption. Importantly, our results show that not all forms of firm power contribute equally to discrimination: labor market power, rather than product market power, is the primary driver in our setting. This distinction suggests that policies aimed at reducing discrimination should focus on labor market concentration and wage-setting power rather than targeting product market competition alone. Measures that enhance worker mobility, reduce barriers to occupational entry, increase transparency in hiring and wage-setting, and promote competitive labor markets could help curb discriminatory practices. Additionally, since much of the observed discrimination appears to stem from inaccurate employer beliefs about immigrant productivity rather than pure preferences, policies that improve information flows—such as credential recognition programs or structured hiring practices—may be particularly effective.

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Tables and Figures

Table 1: Sample Summary Statistics

	Non-Displaced Sample		Displaced Sample	
	(1) Mean	(2) SD	(3) Mean	(4) SD
HHI - Overall	0.0406	0.0820	0.0343	0.0690
HHI - Natives + Wester Imm.	0.0410	0.0827	0.0379	0.0777
HHI - NWI	0.0343	0.0690	0.0305	0.0639
Unique Occupation-LLM	3,170		2,343	
Overall				
Earnings	529,966	371,643	488,166	329,720
Age	43.35	10.55	41.96	10.71
Tenure at Firm (After 2001)	7.03	2.14	6.34	2.23
Blue Collar Share	0.33		0.34	
Share Female	0.32		0.32	
# Workers in Displaced Sample	168,628			
Natives + Wester Imm.				
Earnings	537,274	376,988	498,927	336,435
Age	43.56	10.59	42.22	10.80
Tenure at Firm (After 2001)	7.08	2.12	6.40	2.21
Blue Collar Share	0.31		0.32	
Share Female	0.31		0.32	
# Workers in Displaced Sample	155,584			
NWI				
Earnings	410,762	240,370	360,760	195,339
Age	40.01	9.23	38.93	9.12
Tenure at Firm (After 2001)	6.20	2.26	5.57	2.27
Blue Collar Share	0.52		0.51	
Share Female	0.37		0.34	
# Workers in Displaced Sample	13,134			

Source: Authors' calculations using Norwegian register data.
Notes: "NWI" refers to Non-Western Immigrants.

Table 2: Difference-in-Differences Estimates

	Base Model			Individual Fixed Effects			Individual Fixed Effects + Base Earnings x Relative Time		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.237***	0.055**	-0.484***	-0.237***	0.056**	-0.484***	-0.195***	0.043*	-0.405***
	(0.044)	(0.022)	(0.088)	(0.047)	(0.023)	(0.093)	(0.048)	(0.024)	(0.097)
Post x NWI	-0.056***	0.005	-0.085***	-0.056***	0.005	-0.086***	-0.042**	0.000	-0.056**
	(0.019)	(0.004)	(0.022)	(0.020)	(0.004)	(0.024)	(0.019)	(0.004)	(0.023)
Post x NWI x HHI	-0.383***	0.056	-0.582***	-0.383***	0.056	-0.587***	-0.415***	0.066	-0.656***
	(0.133)	(0.056)	(0.207)	(0.142)	(0.060)	(0.225)	(0.142)	(0.059)	(0.222)
N	673,488	673,488	668,808	673,488	673,488	668,808	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table 3: Labor Market Power vs. Product Market Power

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x NWI	-0.047***	0.010**	-0.069***
	(0.018)	(0.005)	(0.026)
Post x Revenue HHI	0.046**	-0.018***	0.054***
	(0.023)	(0.003)	(0.017)
Post x Occupation HHI	-0.185***	0.038	-0.264***
	(0.056)	(0.027)	(0.079)
Post x Revenue HHI x NWI	0.020	-0.030***	0.050
	(0.055)	(0.007)	(0.054)
Post x Occupation HHI x NWI	-0.491***	-0.005	-0.598*
	(0.184)	(0.066)	(0.319)
N	506,312	506,312	502,798

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table 4: Additional Evidence from Other Subgroups: Preferred Model

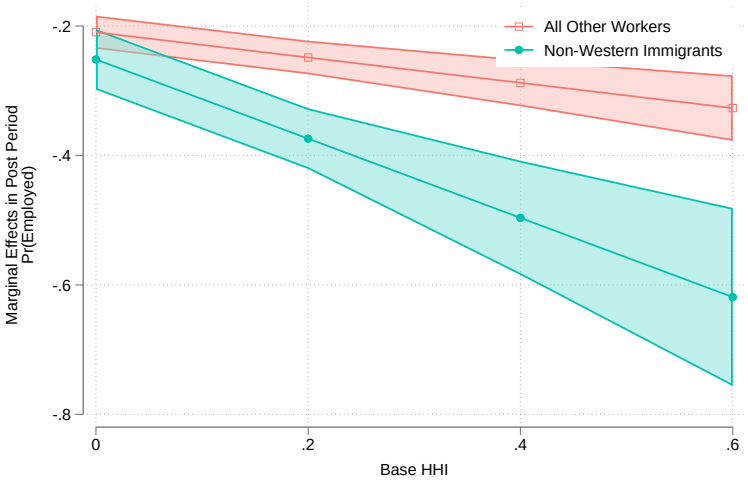
	(1) Employment	(2) Pr(Part Time)	(3) Log Labor Earnings
Panel A: 2nd Generation Immigrants vs All Others (Excl 1st Gen NWI)			
Post x HHI	-0.196***	0.043*	-0.401***
	(0.048)	(0.023)	(0.097)
Post x 2nd Gen NWI	0.016	0.000	0.105***
	(0.011)	(0.007)	(0.023)
Post x 2nd Gen NWI x HHI	0.053	-0.040	0.127
	(0.118)	(0.113)	(0.185)
N	634,280	634,280	630,237
Panel B: Natives vs Western Immigrants			
Post x HHI	-0.191***	0.041*	-0.405***
	(0.047)	(0.023)	(0.096)
Post x West. Imm.	-0.003	0.001	0.006
	(0.007)	(0.004)	(0.013)
Post x West. Imm. x HHI	-0.158	0.049	0.055
	(0.126)	(0.055)	(0.158)
N	634,280	634,280	630,237
Panel C: NWI, Long Time in Norway (>P75)			
Post x HHI	-0.195***	0.042*	-0.401***
	(0.048)	(0.023)	(0.096)
Post x NWI	-0.031	-0.002	-0.091***
	(0.022)	(0.006)	(0.031)
Post x NWI x HHI	-0.299	0.028	0.159
	(0.207)	(0.087)	(0.244)
N	645,640	645,640	641,497
Panel D: Destination Firm, Manager Average Experience with NWI			
	All Manager Experience, t-1	All Manager Experience, t-3 to t-1	Managers' Average NWI Neighborhood Share
Post x HHI	0.029	0.029**	0.003
	(0.019)	(0.013)	(0.028)
Post x NWI	-0.018	-0.018*	0.001
	(0.013)	(0.010)	(0.006)
Post x NWI x HHI	0.194***	0.124**	-0.020
	(0.074)	(0.059)	(0.038)
N	350,548	350,548	343,258

Source: Authors' calculations of Norwegian register data.

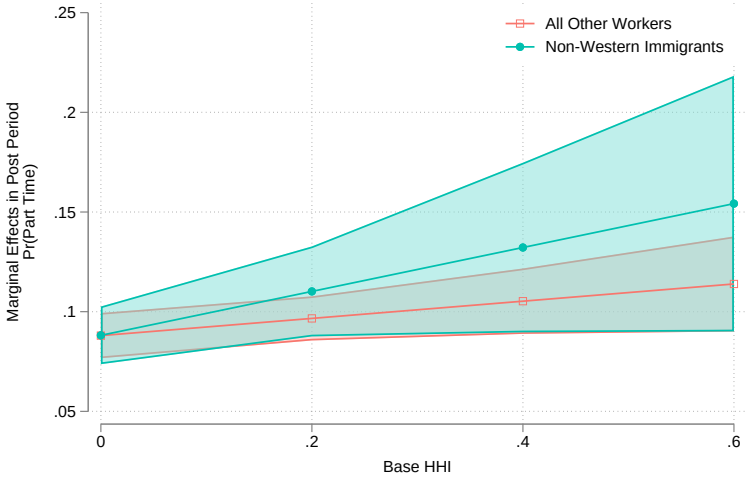
Notes: Estimates correspond to Equation 2 in the text over the first three years after the layoff event. Standard errors clustered at the base firm level. * p<0.10, ** p<0.05, *** p<0.01. "NWI" refers to Non-Western Immigrants. The 75th percentile of time in Norway among non-Western immigrants in the sample was 23 years at the time of the displacement event. Manager experience with NWI is measured by taking the average share of all workers the manager has worked with at their firm for the prior year (or three years) who are NWIs, averaging over all managers at the firm. Manager neighborhood share NWI is the percent of residents that are NWIs in each manager's neighborhood averaging over all managers at the firm.

Figure 1: Marginal Effects of Employer Discrimination, by HHI

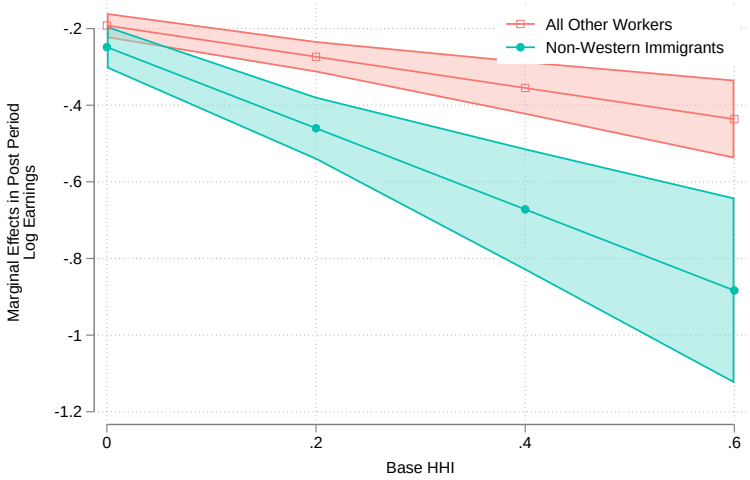
Panel A: Employment



Panel B: Part-time Work

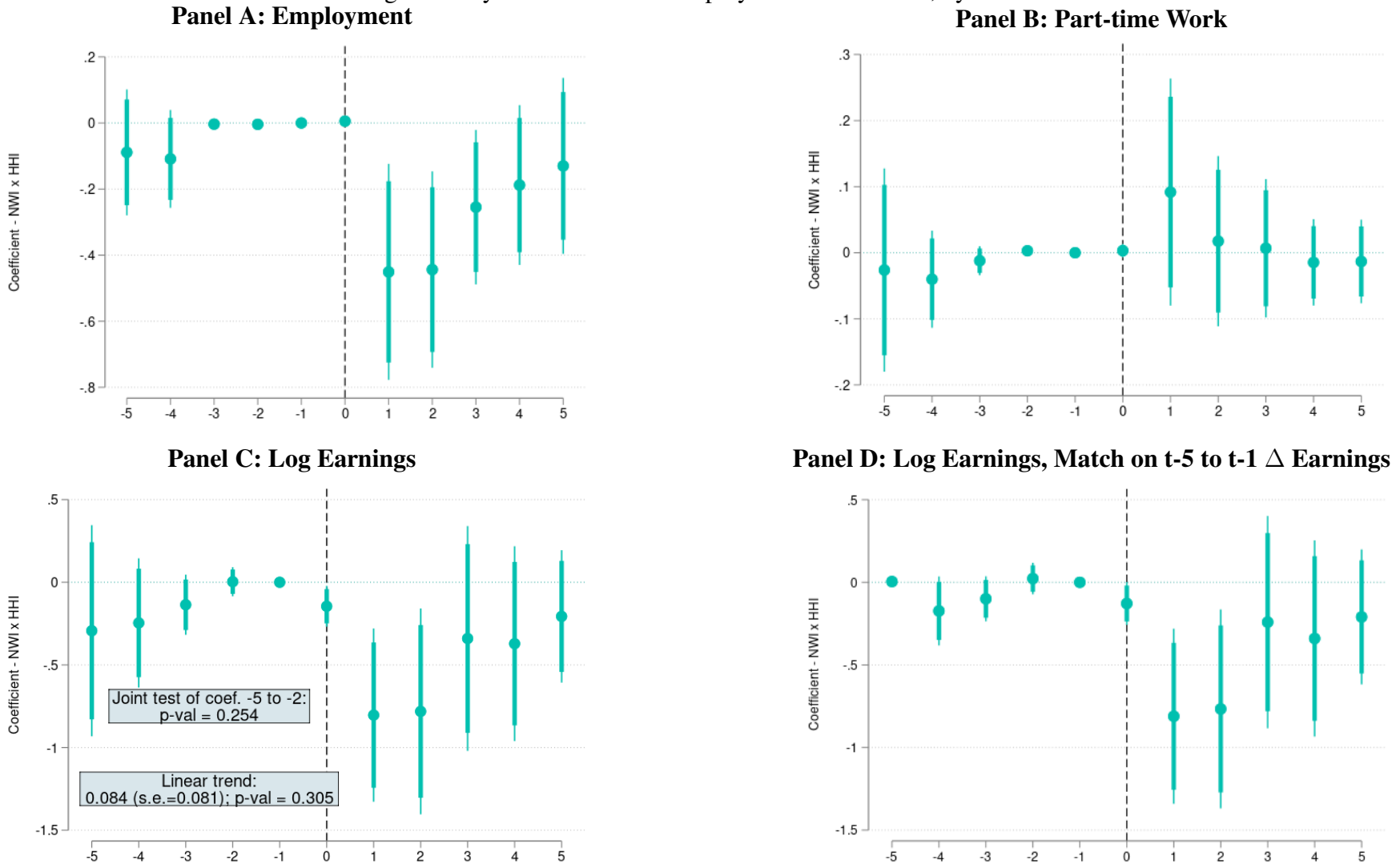


Panel C: Log Earnings



Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to Equation 1 in the text and those presented in Table 2. Shaded areas represent the 95% confidence intervals for standard errors clustered at the base firm level.

Figure 2: Dynamic Effects of Employer Discrimination, by HHI



Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 3 in the text.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level. Panel D adds an additional match variable interacting each worker's change in their earnings from t-5 to t-1 with relative time indicators. For a binarized event study that splits the interaction at $HHI > 0.25$ and yields similar results, see Figure A-4. The coefficients for t-3 to t-1 are zero by construction for Panel A because we condition on three years of continuous work prior to displacement.

A Online Appendix: Conceptual Framework

In this section, we provide an overview of the relationship between employer discrimination and competition through a conceptual framework. The objective is not to develop new theoretical insights but rather to draw upon existing models to help clarify key ideas and inform the setup of our empirical analysis. We begin with the foundational model of Becker (1957), extend it to incorporate statistical discrimination along the lines of Phelps (1972) and Arrow (1973), and then analyze the role of competition alongside different sources of market power; both in terms of the labor market and the product market. Next, we integrate insights from Bohren et al. (2019) to distinguish between belief-based and preference-based discrimination within imperfectly competitive markets. Finally, we connect this simple framework to the empirical hypotheses guiding our analysis.

A.1 A Basic Model of Discrimination

In the basic framework, employers derive utility from profits but may also face costs or benefits when hiring workers from different demographic groups. To link this framework to our empirical analysis, we classify these groups as natives (n) and non-Western immigrants (nw), noting that "natives" will also include Western immigrants. Let the employer's utility function be:

$$u_e = \pi - d(L_{nw}), \tag{5}$$

where u_e is the employer's utility and π is the profit function. The term $d(L_{nw})$ represents the cost (or disutility) associated with employing non-Western immigrant workers, capturing either taste-based discrimination ($d > 0$) or belief-based discrimination, where d depends on expected productivity differences based on statistical generalizations of the groups.

The profit function is:

$$\pi = f(L_n + L_{nw}) - (MCL_n)L_n - (MCL_{nw})L_{nw}, \tag{6}$$

where $f(\cdot)$ is the production function, reflecting the output as a function of total labor. The terms MCL_n and MCL_{nw} are the marginal costs of labor of natives and non-Western immigrants, and the terms L_n and L_{nw} are the numbers of natives and non-Western immigrants employed.

The marginal cost of labor (MCL) represents the cost to the employer of hiring an additional worker. In a competitive market, firms are wage-takers and face a perfectly elastic labor supply, so wages equal the marginal cost of labor (MCL_n and MCL_{nw}), which corresponds to the worker's marginal revenue product (MRP). In monopsonistic markets, firms face an upward-sloping labor supply curve, meaning that the marginal cost of labor exceeds the wage. This creates a wedge between wages and MRP, allowing firms to set wages below MRP through a wage markdown (M_n and M_{nw}).[41]

Employers maximize utility by choosing L_n and L_{nw}, subject to the profit function and any discriminatory preferences or beliefs against non-Western immigrants.

A.2 First-Order Conditions

To maximize utility, the employer solves the following optimization problem:

[41] Formally, in such concentrated markets, the marginal cost of labor is given by $MCL = w + \frac{\partial w}{\partial L} dL$, where $\frac{\partial w}{\partial L} dL$ represents the elasticity-adjusted wage effect, capturing how wages increase when firms expand employment and the resulting cost of raising wages for all inframarginal workers. In a perfectly competitive labor market, this term is zero, as firms face a perfectly elastic labor supply. It is this upward-sloping dynamic that generates the wage markdown $w = MRP - M$, which we use to capture this dynamic.

$$\max_{L_n, L_{nw}} u_e = f(L_n + L_{nw}) - (MCL_n)L_n - (MCL_{nw})L_{nw} - d(L_{nw}). \tag{7}$$

The first-order conditions for utility maximization with respect to L_n and L_{nw} are:

$$\frac{\partial u_e}{\partial L_n} = f'(L_n + L_{nw}) - MCL_n = 0. \tag{8}$$

$$\frac{\partial u_e}{\partial L_{nw}} = f'(L_n + L_{nw}) - MCL_{nw} - \frac{\partial d}{\partial L_{nw}} = 0. \tag{9}$$

The first condition states that natives are employed up to the point where their marginal productivity equals their marginal cost of labor. The second condition specifies that non-Western immigrants are employed until their marginal productivity equals their marginal cost of labor plus any utility costs due to discrimination. For a firm with labor market power, there is a wedge between workers' marginal revenue product (MRP) and their actual wage, so that $MCL_n = w_n = MRP_n - M_n$ and $MCL_{nw} = w_{nw} = MRP_{nw} - M_{nw}$. Thus, for any $d(L_{nw}) > 0$, an employer hires a marginal non-Western immigrant only if the post-markdown wage is sufficiently low, such that $(MRP_n - M_n) - (MRP_{nw} - M_{nw}) < d(L_{nw})$. When the marginal worker from each group are equally productive ($MRP_n = MRP_{nw}$), the firm will only hire a non-Western immigrant when $M_n - M_{nw} > d'(L_{nw})$.

A.3 Implications of Discrimination

Taste-Based Discrimination. If discrimination is driven by preferences, the disutility from hiring non-Western immigrants is given by $d(L_{nw}) = d_e L_{nw}$, where $d_e > 0$ is a constant disutility parameter. Employers treat non-Western immigrants as less desirable, effectively raising their perceived cost of employment. This leads to a lower demand for L_{nw} until $(MRP_n - M_n) - (MRP_{nw} - M_{nw}) < d_e$.

Belief-Based Discrimination. If discrimination arises from beliefs about productivity, the disutility from hiring non-Western immigrants is given by $d(L_{nw})$, where $d(L_{nw}) = g(E[L_n] - E[L_{nw}])$, with $E[L_n]$ and $E[L_{nw}]$ representing the employer's expectations of native versus non-Western immigrant productivity. Non-Western immigrants may face lower hiring probabilities and wages if employers underestimate their productivity relative to natives, conditional on true productivity. Hiring a non-Western immigrant would, therefore, entail additional perceived risk. Since obtaining individual productivity information entails costs (c), firms may rely on group-based beliefs rather than invest in additional information, provided that $MRP_n - MRP_{nw} - c > g(E[L_n] - E[L_{nw}]) + (M_n - M_{nw})$. In other words, firms would be willing to rely on these beliefs rather than invest in more information until the difference in workers' actual productivity minus the cost of acquiring that information is larger than the utility cost of the uncertainty around their expectations for immigrants' productivity plus any differences in their wage markdowns.[42]

A.4 Introducing Competition

An employer with discriminatory preferences (i.e., taste-based discrimination) faces two types of costs when hiring workers. First, the employer incurs a utility cost $d(L_{nw}) > 0$ from hiring minority workers, as their biases against the minority group generate disutility from working with them. Second, to avoid this disutility, the employer may opt to hire less productive workers from the preferred group, resulting in labor misallocation. This happens because the employer prefers certain workers over others based on personal biases, even when the productivity of workers from the two groups is identical. This misallocation leads to reduced firm profits, as the employer sacrifices productivity to satisfy their discriminatory preferences. Therefore, discrimination imposes

[42]The utility cost of the firms' expectations will depend on the risk aversion within the utility function $g(\cdot)$.

both a utility cost d_e (from hiring minorities) and an economic cost (from inefficiency in the hiring process).

For workers, discrimination based on beliefs is also costly. High-ability workers from a group perceived to be less productive may be overlooked in favor of less productive workers from a group perceived to be more productive, even if both groups have identical underlying productivity distributions. The foregone worker might subsequently sort into lower-quality jobs if there is a finite number of outside options. This distorts the allocation of labor across firms and negatively influences the firm's profit function, with the firm employing less productive workers from the majority group at the expense of more productive workers from the minority group. The cost to the firm of this reliance on beliefs is the difference in actual productivity between the hired native and the foregone non-Western immigrant. Assuming diminishing marginal productivity for labor from each group, employers who discriminate may hire fewer non-Western immigrants than is optimal, resulting in a suboptimal labor mix. This misallocation reduces overall output ($f(L_n + L_{nw})$) and profits (π).

In order to compensate for these misallocation or utility costs, in an imperfectly competitive labor market, the differentially marked-down wages of the immigrant and the native must be sufficiently large to offset these utility or misallocation costs, i.e. $(MRP_n - M_n) - (MRP_{nw} - M_{nw}) > d_e$ in the case of taste-based discrimination, or $MRP_n - MRP_{nw} - c > g(E[L_n] - E[L_{nw}]) + (M_n - M_{nw})$ for belief-based discrimination. In other words, a firm with labor market power relying on group-based productivity inference can afford to get productivity signals wrong by differentially marking down the wages of the workers they view as less productive.

In competitive labor markets, the lack of firm wage-setting power means that wages are equal to the marginal product of the worker's labor such that there is no markdown M for either worker. If the workers are equally productive, this means that $(MRP_n - M_n) - (MRP_{nw} - M_{nw}) \to 0 < d_e$. Under these conditions, firms with $d(L_{nw}) > 0$ will choose only to hire natives and pursue discrimination through the employment channel.

In a finite pool of workers and under concave marginal productivity, in such cases, excluding productive minority workers reduces the overall productivity of the firm's workforce. Non-discriminatory firms gain a competitive advantage, gradually displacing discriminatory firms. Thus, labor market competition helps drive discriminatory firms out of the market by allocating more productive labor to the less prejudiced firms or the firms with better information about immigrants' productivity. Competition, thus, steepens the consequences of either forgoing the best workers out of animus or forgoing the best labor out of incorrect beliefs about group-based differences such that firms will be willing to incur the cost of uncovering true individual productivity signals. Due to the lack of supernormal profits, the increased cost or inefficiency of discrimination cannot be sustained by the firm in the long run due to perfectly competitive markets. Ultimately, this will push discriminatory firms out of the market.

A.5 Source of Power

Firms can derive power and supernormal profits from two distinct sources: product markets (through price markups above marginal costs for products) and labor markets (through wage markdowns below marginal productivity in wages). The nature of the firm's power significantly influences the extent to which firms can engage in discriminatory practices, but this has received little attention in empirical work.

As discussed above, in perfectly competitive labor markets, firms face a perfectly elastic labor supply, meaning they cannot set different wages for different workers based on their group mem-

bership and cannot mark down wages below the worker's marginal revenue product (MRP). This eliminates the ability to engage in wage discrimination for the same productivity. In addition, they cannot engage in employment discrimination because foregoing the most productive labor hurts the firm's profitability directly and pushes them out of the market in the long run.

Unlike in a competitive labor market, a monopsonistic employer faces an upward-sloping labor supply curve. Firms can exploit this by paying workers less than their marginal revenue product ($M_n > 0$ and $M_{nw} > 0$) and they can differentially mark down wages by subgroup even for equally productive workers, regardless of its product market power. The productivity costs of hiring less productive members of the favored group can be absorbed by the supernormal profits that the firm makes from wage markdowns. Thus, observationally identical natives and non-Western immigrants who are induced to search for jobs in such markets may have very different outcomes in *both* employment and earnings depending on the discrimination practices or preferences of employers. This is true irrespective of whether the discrimination is belief-based, preference-based, or monopsonistic.[43]

In a setting where a firm holds power in the product market but no power in the labor market, firms can discriminate, but with considerable limitations. In particular, if the firm pays disfavored workers less than their marginal revenue product, these workers, exhibiting a perfectly elastic labor supply, can simply leave for better options. In other words, the lack of wage-setting power places strong limits on wage discrimination in the market by forcing $M_n = 0$ and $M_{nw} = 0$. However, as discussed above, the firm can still engage in employment discrimination by only hiring natives, covering the cost of the productivity differences between the marginal native and marginal immigrant with its supernormal product market profits. Thus, we would expect labor market gaps after search for natives and non-Western immigrants in this type of setting to only arise in the context of employment, not wages.[44]

A.6 Bohren et al. (2019) and Dynamics of Discrimination

Belief-based discrimination, unlike preference-based discrimination, stems from statistical generalizations about groups rather than ingrained personal biases. As highlighted in Bohren et al. (2019), beliefs about worker productivity can be refined and adjusted over time through repeated interactions and the accumulation of individualized information. This process enables employers to move beyond group averages and make more informed decisions, thereby mitigating discriminatory practices. Specifically, acquiring information about individual productivity entails costs, and the employer will hesitate to acquire information when the cost c is greater than the expected benefit from knowing the difference in productivity $g(E[L_n] - E[L_{nw}])$. This is particularly true in less competitive labor markets, where supernormal profits allow firms to absorb the costs of incorrect belief-based discrimination, provided this does not drive profits below zero.

However, once workers are hired, repeated interactions significantly reduce the cost of acquiring individualized information about productivity, effectively pushing $c \to 0$. Over time, this dynamic implies that discrimination stemming from incorrect productivity beliefs will diminish,

[43] Monopsonistic discrimination is characterized by differentially marking down the wages of some workers versus others under the belief that they have different labor supply elasticities and/or different outside options. Perfect competition driving wages up to marginal productivity reduces and eventually eliminates the firm's leverage to do this.

[44] In some models (e.g. Black (1995)), discrimination can spill over from prejudiced firms to non-prejudiced firms and grant the non-prejudiced firms monopsony power to mark down wages by eliminating a sufficient share of that worker's outside options. This implies a close tie between product market power and labor market power and implicitly assumes a finite number of firms competing for labor and high levels of resulting labor market concentration, a scenario different from a competitive labor market proposed above.

assuming the worker is hired and meaningful interactions occur between the employer and the employee. If discrimination was present in the initial hiring choice, so-called "belief flipping" (Fryer Jr, 2007) might occur. This is a situation in which the disfavored group overtakes the favored group in wage and employment outcomes over time as the true productivity of positively selected members of the disfavored group is revealed.

In contrast, preference-based discrimination, rooted in emotional aversion and entrenched biases, is often more resistant to change. Unlike productivity beliefs, which can be updated through evidence and experience, preferences are far less flexible, making them harder to overcome through increased exposure or interaction alone. Of course, positive experiences with minority workers can, in some cases, be strong enough to alter employer preferences. However, such shifts typically require substantial time and sustained exposure (we discuss changes in preferences in Section 6).

A.7 Conceptual Framework and Overview of Empirical Analyses

To examine the relationship between competition and employer discrimination, we focus on a setting where equally productive individuals face the same employer decisions across markets with varying levels of competition. Specifically, we compare native and non-Western immigrant worker pairs who (i) are valued as equally productive in the labor market as evidenced by identical earnings (such that $MRP_n - MRP_{nw} = 0$), (ii) hold the same occupation in the same industry and firm, and (iii) are induced to search for new jobs due to reasons orthogonal to individual characteristics. This approach ensures that any differences in the outcomes we examine are attributable to the competitive environment, not unobservable worker characteristics.

We leverage mass layoff events and firm closures to induce job search among these workers. These events are exogenous to individual labor market decisions, forcing workers with identical observable characteristics to seek re-employment via search. By comparing native-immigrant worker pairs displaced from the same occupation and firm at the same time, we create a controlled environment to study how market competition influences discriminatory hiring practices.

Our analysis focuses on two distinct scenarios: workers searching for jobs in *competitive* markets (i.e., non-concentrated) versus *concentrated* markets, defined in terms of both labor and product market competition. We measure post-search employment and earnings gaps between native and immigrant workers in these settings to uncover how competition impacts employer discrimination on employment and wages.

To disentangle the roles of labor market power and product market power in driving discrimination, we conduct a "horse-race" comparison between measures of each type of market power. This allows us to identify whether discriminatory hiring practices are more closely tied to monopsonistic labor markets or monopolistic product markets.

Additionally, drawing on insights from Bohren et al. (2019), we analyze the dynamic adjustment path following displacement events to differentiate between belief-based and preference-based discrimination. Belief-based discrimination arises from statistical generalizations and is expected to diminish with repeated interactions, whereas preference-based discrimination, driven by deep-seated biases, is more persistent and less responsive to information about worker's productivity. Importantly, since the market values their productivity equally before they start job searching, non-Western immigrants are likely just as productive as native workers. This rules out statistical discrimination based on accurate beliefs about their productivity. We supplement this approach with a series of auxiliary analyses.

Overall, our empirical analysis provides a comprehensive understanding of employer discrimination in what is ostensibly one of the most equal countries in the world. We examine whether

such discrimination is driven by beliefs or preferences, how it is influenced by market power, and what policies could mitigate these practices effectively.

Online Appendix: Tables and Figures

Table A-1: Share of Employment of Nativity Group in Most Common Occupations and Industries, by HHI, Displaced Sample

	Blue Collar	Manufacturing	Construction	Wholesale and Retail	Hotels and Restaurants	Professional Services
	(1)	(2)	(3)	(4)	(5)	(6)
			Overall Displaced Sample			
Norwegians	0.328	0.199	0.125	0.143	0.028	0.070
West. Imm.	0.268	0.153	0.122	0.139	0.064	0.104
NWI	0.525	0.233	0.091	0.091	0.155	0.045
			HHI<0.05			
Norwegians	0.272	0.155	0.131	0.163	0.028	0.072
West. Imm.	0.238	0.123	0.129	0.154	0.062	0.102
NWI	0.503	0.194	0.100	0.100	0.165	0.043
			HHI>0.05, <0.15			
Norwegians	0.561	0.372	0.107	0.048	0.033	0.058
West. Imm.	0.423	0.298	0.081	0.024	0.081	0.117
NWI	0.658	0.457	0.029	0.034	0.106	0.045
			HHI>0.15, <0.25			
Norwegians	0.659	0.476	0.083	0.042	0.028	0.072
West. Imm.	0.447	0.362	0.064	0.085	0.106	0.128
NWI	0.671	0.529	0.029	0.014	0.057	0.157
			HHI>0.25			
Norwegians	0.725	0.553	0.048	0.036	0.025	0.050
West. Imm.	0.818	0.682	0.023	0.045	0.023	0.114
NWI	0.815	0.783	0.022	0.043	0.033	0.043

Source: Authors' calculations using Norwegian register data.
Notes: Cells represent the share of each nativity group in the displaced sample in each category, separated by HHI ranges.

Table A-2: Unconditional and Conditional Correlations of Baseline HHI with Observable Characteristics in Layoff Sample

	(1) Unconditional Correlation	(2) Correlation, Residualized HHI	(3) Simple Regression, Standardized Coefficients	(4) Regression Standardized Coefficients, with Controls
Baseline Earnings	-0.063	-0.001	0.00168***	0
Baseline Age	0.017	0.000	0.000540	5.12e-05
Baseline Firm-Specific Tenure	0.013	-0.019	-0.00205***	-4.60e-05
Female	-0.048	0.007	0.00123**	1.20e-05
Bachelors' Degree or More	-0.077	0.000	0.00470***	4.60e-05
Less than High School	0.026	0.007	-0.000662	1.27e-05
LLM Size	-0.248	0.000	-0.0152***	3.60e-05
Mass Layoff Size (% Occ-LLM)	-0.016	0.019	-0.00423**	4.88e-06
Blue Collar Occupation	0.231	0.000	0.00701***	0
Layoff Firm Size	0.024	0.003	0.00260*	0.000982***
EU/EEA Citizen	0.022	-0.010	0.00109*	2.83e-05
Non-Western Immigrant	-0.021	0.000	-0.00107***	-1.47e-05
Mean Unemployment Benefits in LLM	0.150	-0.030	0.00586***	-0.00206*

*** p<0.01, ** p<0.05, * p<0.1

Source: Authors' calculations using Norwegian register data.

Notes: Conditional correlations conditional on residualizing baseline HHI on our fixed effects and controls in our main models, namely a fixed effect for baseline firm-industry-occupation-LLM cell and baseline earnings. The regressions in columns (3) and (4) regress baseline occupation-LLM HHI on standardized versions of the variables with mean 0 and standard deviation 1.

Table A-3: Main Estimates with Baseline Earnings Decile by Relative Time Interactions

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.155***	0.029	-0.313***
	(0.045)	(0.021)	(0.090)
Post x NWI	-0.020	-0.008*	-0.009
	(0.020)	(0.004)	(0.025)
Post x NWI x HHI	-0.408***	0.060	-0.645***
	(0.142)	(0.059)	(0.215)
N	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text replacing the linear earnings-by-relative time interactions with earnings decile by relative time interactions. The table presents difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-4: Difference-in-Differences Estimates with Base Industry, Occupation, Firm x Relative Interactions

	Base Industry x Relative Time			Base Occupation x Rel. Time Base Industry x Rel. Time			Base Firm x Rel. Time Base Occupation x Rel. Time Base Industry x Rel. Time		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.117**	0.038*	-0.301***	-0.012	0.041	-0.163*	0.016	0.026	-0.030
	(0.046)	(0.023)	(0.090)	(0.052)	(0.026)	(0.096)	(0.054)	(0.033)	(0.086)
Post x NWI	-0.039**	-0.005	-0.045**	-0.029	-0.007*	-0.016	-0.014	-0.012***	0.010
	(0.017)	(0.004)	(0.021)	(0.018)	(0.004)	(0.022)	(0.010)	(0.003)	(0.017)
Post x NWI x HHI	-0.386***	0.070	-0.642***	-0.368***	0.055	-0.619***	-0.254***	0.047	-0.492***
	(0.132)	(0.059)	(0.216)	(0.130)	(0.060)	(0.213)	(0.083)	(0.059)	(0.151)
N	673,488	673,488	668,808	673,488	673,488	668,808	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text with additional fixed effects as noted in each model label and measure difference-in-differences estimates over the first three years after the layoff event. The differences in the coefficients between columns 7-9 are not statistically significantly different from those in columns 1-6. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table A-5: Estimates Adding Baseline Occupation, Industry, Earnings by Relative Time by HHI Interactions

	Adding Base Occupation x Rel. Time x HHI Base Industry x Rel. Time x HHI Base Earnings by Rel. Time x HHI		
	(1)	(2)	(3)
	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-	-	-
	-	-	-
Post x NWI	-0.031*	-0.006	-0.021
	(0.017)	(0.004)	(0.022)
Post x NWI x HHI	-0.321***	0.036	-0.503**
	(0.122)	(0.060)	(0.201)
N	673,480	673,480	668,800

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text with the addition of base occupation by relative time, base occupation by relative time by HHI, base industry by relative time, base industry by relative time by HHI, base earnings by relative time, and base earnings by relative time by HHI interacted effects. The $Post \times HHI$ interaction is subsumed by the interactions between relative time and HHI mentioned above and thus are omitted. Estimates are not statistically significantly different from the baseline specification in Table 2. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-6: Difference-in-Differences Estimates with Matching on t-5 to t-1 Earnings Trends

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.187***	0.041*	-0.357***
	(0.047)	(0.024)	(0.091)
Post x NWI	-0.059***	0.002	-0.113***
	(0.020)	(0.004)	(0.024)
Post x NWI x HHI	-0.397***	0.072	-0.717***
	(0.142)	(0.057)	(0.238)
N	665,176	665,176	661,910

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text with additional controls for relative time dummies interacted with each worker's earnings changes from time t-5 to t-1, which matches on earnings trends as well as earnings levels at baseline.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table A-7: Difference-in-Differences Estimates on Mobility Outcomes

	(1) Moved Municipalities	(2) Worked in a Different Municipality from Home	(3) Work in a New Municipality
Post x HHI	0.028***	0.133*	0.117***
	(0.007)	(0.073)	(0.039)
Post x NWI	-0.005*	0.003	-0.004
	(0.003)	(0.038)	(0.009)
Post x NWI x HHI	0.045	-0.014	0.093
	(0.051)	(0.218)	(0.094)
N	673,488	644,728	673,488

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table A-8: Difference-in-Differences Estimates with Additional Conditions

	Condition: Three Years at the Same Firm Prior to Displacement			5-Year Age Bins x Relative Time			Base Neighborhood x Relative Time		
	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings	(4) Employment	(5) Pr(Part Time)	(6) Log Earnings	(7) Employment	(8) Pr(Part Time)	(9) Log Earnings
Post x HHI	-0.197***	0.040	-0.335***	-0.183***	0.041*	-0.358***	-0.202***	0.025	-0.326***
	(0.056)	(0.026)	(0.092)	(0.050)	(0.024)	(0.108)	(0.039)	(0.017)	(0.074)
Post x NWI	-0.041*	0.000	-0.050*	-0.064***	0.003	-0.110***	-0.043***	0.006	-0.072***
	(0.023)	(0.005)	(0.027)	(0.020)	(0.004)	(0.024)	(0.017)	(0.004)	(0.023)
Post x NWI x HHI	-0.480***	0.029	-0.661**	-0.433***	0.067	-0.671***	-0.352***	0.039	-0.519**
	(0.172)	(0.067)	(0.299)	(0.145)	(0.059)	(0.226)	(0.133)	(0.060)	(0.233)
N	524,656	524,656	520,852	673,488	673,488	668,808	659,928	659,928	655,190

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text with additional fixed effects as noted in each model label and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table A-9: Difference-in-Differences Estimates: European vs Non-European NWIs

	European NWI (Eastern Europe)			Non-European NWI		
	(1)	(2)	(3)	(4)	(5)	(6)
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.195***	0.043*	-0.407***	-0.195***	0.043*	-0.407***
	(0.048)	(0.024)	(0.097)	(0.048)	(0.024)	(0.097)
Post x NWI	-0.046***	0.007	-0.056**	-0.041	-0.003	-0.057*
	(0.012)	(0.005)	(0.023)	(0.026)	(0.005)	(0.030)
Post x NWI x HHI	-0.412**	-0.093	-0.544	-0.414***	0.143*	-0.712***
	(0.181)	(0.073)	(0.369)	(0.158)	(0.075)	(0.224)
N	647,928	647,928	643,694	659,840	659,840	655,351

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text with additional fixed effects as noted in each model label and measure difference-in-differences estimates over the first three years after the layoff event. European NWIs refer to immigrants born in countries in eastern Europe, mostly from former Soviet bloc countries, e.g. Poland, Lithuania, Bulgaria, etc. The differences in the coefficients between columns 4-6 are not statistically significantly different from those in columns 1-3. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWI" refers to Non-Western Immigrants.

Table A-10: Main Horserace Estimates with Local Labor Market - Industry Revenue HHI

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x NWI	-0.037***	0.005	-0.059**
	(0.014)	(0.005)	(0.023)
Post x Local Revenue HHI	0.001	-0.007	-0.012
	(0.027)	(0.007)	(0.029)
Post x Occupation HHI	-0.195***	0.049**	-0.385***
	(0.060)	(0.022)	(0.106)
Post x Local Revenue HHI x NWI	-0.027	-0.021	0.007
	(0.085)	(0.015)	(0.090)
Post x Occupation HHI x NWI	-0.393**	0.081	-0.661***
	(0.174)	(0.054)	(0.241)
N	672,736	672,736	668,067

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 with additional interactions for local industry revenue concentration. To allocate firm revenues to local labor markets, we allocate the share of the firm's revenues to each local labor market based on the share of its own employment in the local labor market. The table presents difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-11: Manager Characteristics

	(1) Manager Share Female	(2) Manager Share Norwegian Female	(3) Manager Share Norwegian	(4) Manager Share NWI
Post x HHI	-0.020	-0.054	-0.301**	0.059
	(0.094)	(0.074)	(0.139)	(0.053)
Post x NWI	-0.000	0.005	-0.008	0.011
	(0.022)	(0.021)	(0.014)	(0.010)
Post x NWI x HHI	0.055	0.046	0.134	0.001
	(0.158)	(0.150)	(0.167)	(0.073)
N	352,733	352,733	352,733	352,733

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-12: Difference-in-Differences Estimates, Controlling for Generalized Propensity Score

Panel A: GPS, GPS Squared Controls			
	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.193***	0.042*	-0.397***
	(0.047)	(0.024)	(0.094)
Post x NWI	-0.037**	-0.003	-0.053**
	(0.017)	(0.004)	(0.021)
Post x NWI x HHI	-0.430***	0.108*	-0.722***
	(0.147)	(0.060)	(0.246)
N	667,880	667,880	663,305
Panel B:Reweighting High-HHI NWIs to Low-HHI NWIs			
	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.196***	0.043*	-0.402***
	(0.048)	(0.024)	(0.096)
Post x NWI	-0.043**	0.001	-0.058**
	(0.019)	(0.004)	(0.023)
Post x NWI x HHI	-0.414***	0.065	-0.651***
	(0.142)	(0.059)	(0.223)
N	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. GPS refers to the Generalized Propensity Score in which we estimate baseline occupation HHI for non-Western immigrants via OLS as a function of education dummies (less than high school, high school, and Bachelor's degree or more), industry, occupation, and local labor market dummies, and base annual earnings. We use these estimates to predict HHI for the entire sample and use these predictions to construct a generalized propensity score for the whole sample from the predicted values and residuals of the selection model for non-Western immigrants. We then include this GPS and its square as a control in Panel A. In Panel B, we estimate a propensity score among NWIs on the probability of being in a low-HHI environment (<0.05) and generate inverse probability weights for NWIs only, after which we estimate our main model incorporating these weights.

Table A-13: Difference-in-Differences Estimates, Occupational Sorting

	(1) Occupation Percentile Rank	(2) AKM Occupation FE
Post x HHI	-2.550	-0.030
	(3.828)	(0.025)
Post x NWI	0.428	-0.000
	(0.478)	(0.004)
Post x NWI x HHI	-1.993	-0.032
	(5.164)	(0.042)
N	617,737	616,528

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Occupation percentile ranks are based on the average annual earnings in each occupation, fixed at the base year in which the layoff event occurred (1 = highest paid).
AKM occupation fixed effects are extracted from an AKM-style regression of log annual earnings on worker and occupation fixed effects and subsequently used as the outcome variable in Equation 2.

Table A-14: Neighborhood Characteristics - Manager Shares of Neighbors

	Employment		**Log Earnings**	
	Low Share in Management	High Share in Management	Low Share in Management	High Share in Management
	(1)	(2)	(3)	(4)
Post x HHI	-0.167***	-0.215***	-0.361***	-0.382***
	(0.052)	(0.061)	(0.112)	(0.116)
Post x NWI	-0.042*	-0.031*	-0.060**	-0.029
	(0.022)	(0.016)	(0.028)	(0.025)
Post x NWI x HHI	-0.401***	-0.475**	-0.653***	-0.586
	(0.151)	(0.221)	(0.214)	(0.481)
N	344,336	329,152	341,686	327,122

Source: Authors' calculations using Norwegian register data.

Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The manager shares of neighbors value is defined as the share of workers within a displaced worker's neighborhood that were in a management occupation at any firm during the year in which the worker was displaced. Low and high shares are defined by a split at the median.

Table A-15: Neighborhood Characteristics - NWI Shares of Neighbors

	Employment		**Log Earnings**	
	Low Share of NWI in Nhood	High Share of NWI in Nhood	Low Share of NWI in Nhood	High Share of NWI in Nhood
	(1)	(2)	(3)	(4)
Post x HHI	-0.171***	-0.215***	-0.336***	-0.417***
	(0.047)	(0.068)	(0.097)	(0.124)
Post x NWI	-0.022	-0.049**	-0.024	-0.078***
	(0.013)	(0.022)	(0.032)	(0.026)
Post x NWI x HHI	-0.446***	-0.418**	-0.662***	-0.705**
	(0.135)	(0.206)	(0.241)	(0.335)
N	325,472	348,016	323,314	345,494

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. NWI shares of neighbors are defined as the share of workers within a displaced worker's neighborhood who were non-Western immigrants during the year in which the worker was displaced. Low and high shares are defined by a split at the median.

Table A-16: Difference-in-Differences Estimates, Destination Firm Characteristics

	(1) Share NWI Workers	(2) Share Western Immigrant Workers	(3) Average Worker Age	(4) Log(Firm Size)	(5) Union Density	(6) Pr(Coworker from Neighborhood)
Post x HHI	0.016	0.021	-1.183	-0.616*	-0.079*	-0.306***
	(0.017)	(0.059)	(0.741)	(0.350)	(0.044)	(0.075)
Post x NWI	0.008	0.032***	0.188	0.026	0.018***	0.011
	(0.005)	(0.007)	(0.152)	(0.058)	(0.007)	(0.010)
Post x NWI x HHI	0.032	0.009	-0.465	0.781	-0.034	-0.233
	(0.059)	(0.066)	(1.507)	(0.511)	(0.079)	(0.148)
N	512,634	459,557	512,634	512,634	512,634	512,634

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-17: Difference-in-Differences Estimates, Five-Year Tenure Requirement

	Base Model			Individual Fixed Effects			Individual Fixed Effects + Base Earnings x Relative Time		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.249***	0.048*	-0.487***	-0.249***	0.048*	-0.487***	-0.204***	0.034	-0.395***
	(0.047)	(0.027)	(0.099)	(0.047)	(0.027)	(0.099)	(0.049)	(0.027)	(0.103)
Post x NWI	-0.071***	0.009*	-0.111***	-0.071***	0.009*	-0.112***	-0.058**	0.005	-0.084***
	(0.023)	(0.005)	(0.028)	(0.023)	(0.005)	(0.029)	(0.023)	(0.005)	(0.027)
Post x NWI x HHI	-0.238*	0.096	-0.388	-0.238*	0.096	-0.398	-0.274**	0.107	-0.475*
	(0.139)	(0.067)	(0.262)	(0.139)	(0.067)	(0.271)	(0.138)	(0.066)	(0.269)
N	469,376	469,376	467,199	469,376	469,376	467,199	469,376	469,376	467,199

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-18: Paired Movers Test of Match Effects Following Sorkin and Warwar (2026)

	(1) Full Population	(2) Interacted Model
Gap At Origin Firm	0.782***	0.806***
	(0.00535)	(0.00581)
Gap at Origin Firm x HHI		-0.231***
		(0.0356)
Gap at Origin Firm x NWI		-0.0853***
		(0.0149)
Gap at Origin Firm x NWI x HHI		0.332***
		(0.119)
Scaled Variance | Signal to Noise Ratio	0.0281	
Observations	130,365	130,365

Source: Authors' calculations using Norwegian register data.
Notes: Following Sorkin and Warwar (2026), we match a set workers in the full population of workers to others who made the same move between a baseline firm and their next firm during the same year. If multiple movers in the group exist, we randomly pair each worker. We residualize earnings on controls for a quartic in age well as year fixed effects. We instrument the year t-1 earnings gap with the earnings gap in year t-2 and estimate the earnings gap in the current firm as a function of the earnings gap at their prior firm. Standard errors clustered at the match pair level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-19: Heterogeneous Effects by Baseline Union Density

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.178***	0.070***	-0.398***
	(0.042)	(0.024)	(0.083)
Post x NWI	-0.041**	-0.001	-0.051**
	(0.020)	(0.004)	(0.023)
Post x NWI x HHI	-0.366**	0.037	-0.639**
	(0.165)	(0.065)	(0.292)
Post x HHI x 1SD Union Density	-0.063	-0.010	-0.053
	(0.041)	(0.018)	(0.084)
Post x NWI x 1SD Union Density	0.011	-0.004	0.029
	(0.011)	(0.004)	(0.018)
Post x NWI x HHI x 1SD Union Density	-0.137	0.072	-0.136
	(0.149)	(0.071)	(0.298)
N	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event with additional controls and fully saturated interactions between each of the post-layoff measures and the worker's baseline firm's union density, standardized with mean zero and standard deviation 1. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-20: Difference-in-Differences Estimates, Displaced from Public Sector Employment

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings	(4) Re-Employed in Public Sector
Post x HHI	-0.026	0.010	-0.028	0.118**
	(0.019)	(0.011)	(0.030)	(0.052)
Post x NWI	0.026**	-0.008	0.032*	0.007
	(0.012)	(0.007)	(0.016)	(0.013)
Post x NWI x HHI	-0.031	0.057	0.178	-0.104
	(0.069)	(0.067)	(0.137)	(0.082)
Post-Separation Pr(Public)		0.578		
N	251,488	251,488	250,050	251,488

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure difference-in-differences estimates over the first three years after the layoff event only for those displaced from a public sector position. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-21: Median Assets and Wealth by Subgroup at Baseline

	Full Sample at Baseline		**Displaced Sample**	
	Natives / Western Immigrants (1)	Non-Western Immigrants (2)	Natives / Western Immigrants (3)	Non-Western Immigrants (4)
Taxable Real Capital	451,373	350,062	433,974	336,715
Gross Financial Capital	103,303	46,333	91,638	41,525
Gross Wealth	582,000	360,000	544,000	329,000
State and Municipal Property Tax	5,750	3,399	5,236	2,636

Source: Authors' calculations using Norwegian register data.

Table A-22: Difference-in-Differences Estimates, Firm Closures Only

	Employment	Pr(Part Time)	Log(Earnings)
	(1)	(2)	(3)
Post x HHI	-0.144**	0.075**	-0.440***
	(0.071)	(0.036)	(0.141)
Post x NWI	-0.026*	-0.001	-0.012
	(0.015)	(0.007)	(0.035)
Post x NWI x HHI	-0.811**	0.028	-1.535*
	(0.357)	(0.152)	(0.857)
N	173,304	173,304	171,966

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the displacement event, including only those displaced by a firm closure. While the point estimates on Post x NWI x HHI are larger than our overall model, they are not statistically different from our main estimates. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-23: Main Estimates, Controlling for Size of Occupation Layoffs/Closures in Base Year

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x HHI	-0.207***	0.045**	-0.337***
	(0.049)	(0.022)	(0.095)
Post x NWI	-0.042**	0.001	-0.057**
	(0.019)	(0.004)	(0.023)
Post x NWI x HHI	-0.416***	0.066	-0.651***
	(0.142)	(0.059)	(0.224)
Shock Size	-0.015	0.074	0.077
	(0.043)	(0.076)	(0.155)
Shock Size x Post	0.014	-0.003	-0.068
	(0.031)	(0.010)	(0.053)
N	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the layoff event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The shock size variable is defined as the sum of all separated workers via a mass layoff or firm closure in each base year-LLM-occupation cell divided by the total number of employed workers in that cell in the base year, or in other words, the size of the separated pool as a share of workers.
"Shock size $\times$ post" controls for the differential effects of this shock after the layoff or closure events. That our main effects persist indicates that differences in the severity of the local labor market shocks that generate layoffs or firm closures do not affect our results.

Table A-24: Difference-in-Differences Estimates, Social Assistance Program Take-up

	Panel A: Social Assistance Take-up		
	(1)	(2)	(3)
	Pr(Sick Leave Benefits)	Pr(Unemployment)	Pr(Disability Pension)
Post x HHI	0.035	0.301**	0.018**
	(0.023)	(0.118)	(0.008)
Post x NWI	0.060***	0.043*	-0.005***
	(0.008)	(0.024)	(0.001)
Post x NWI x HHI	-0.036	0.268	-0.004
	(0.103)	(0.166)	(0.020)
N	673,467	673,488	673,488
	Panel B: Main Outcomes Controlling for Sick Leave and Disability Payments		
	(1)	(2)	(3)
	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.186***	0.041*	-0.370***
	(0.048)	(0.024)	(0.099)
Post x NWI	-0.046**	0.001	-0.070***
	(0.019)	(0.004)	(0.023)
Post x NWI x HHI	-0.420***	0.066	-0.692***
	(0.139)	(0.059)	(0.220)
N	673,467	673,467	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the displacement event. Standard errors clustered at the base firm level. Estimates in Panel B control for the total annual ammount of sick leave and disability pension payments during the year.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

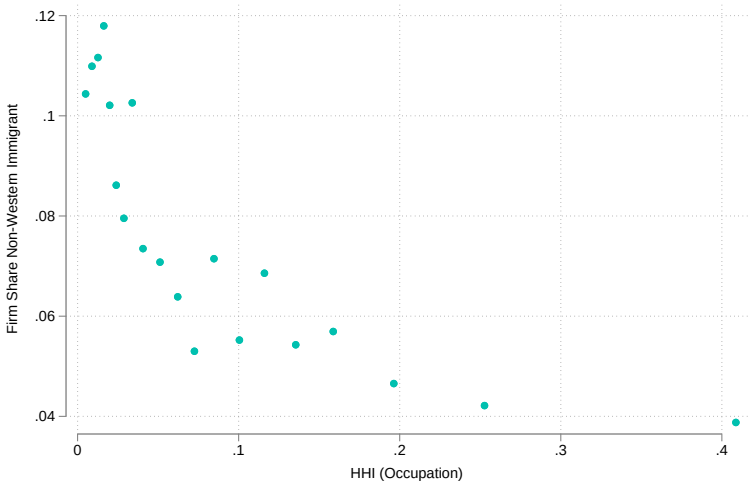
Table A-25: Difference-in-Differences Estimates, Occupation-LLM Average Markdown

	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x Avg Occ-LLM Markdown	-0.044***	0.006***	-0.059***
	(0.004)	(0.002)	(0.009)
Post x NWI	0.058*	-0.008	0.145**
	(0.031)	(0.013)	(0.060)
Post x NWI x Avg Occ-LLM Markdown	-0.034***	0.001	-0.061***
	(0.011)	(0.005)	(0.022)
N	213,520	213,520	211,576

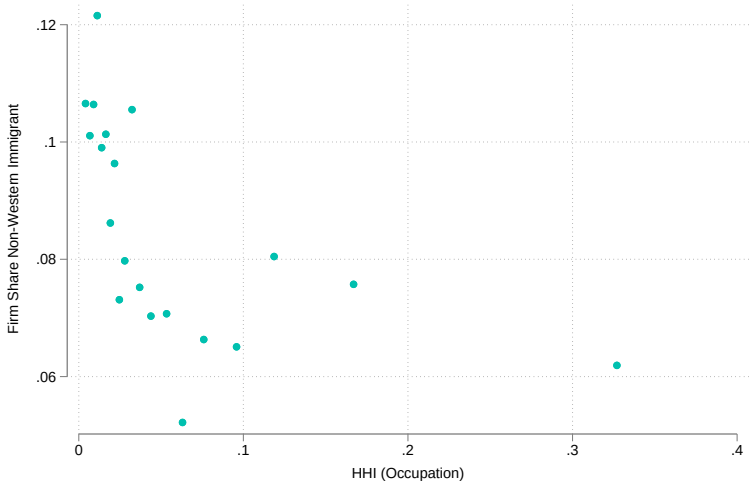
Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the displacement event replacing occupation-LLM average wage markdowns. Calculations are for blue collar occupations in order to overlap with the sectors for which production functions can be reliably estimated following Yeh et al. (2022) using raw materials as the flexible and competitively supplied input. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A-1: Non-Western Immigrant Employment Gaps in Firm, by HHI

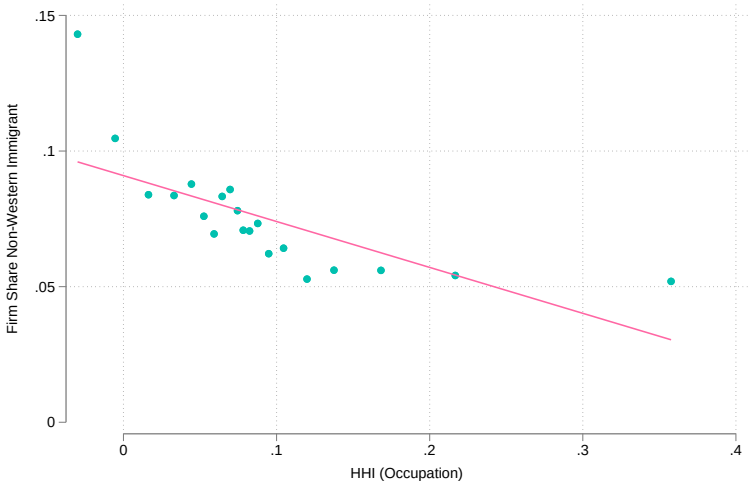
Panel A: Share NWI, All Sectors



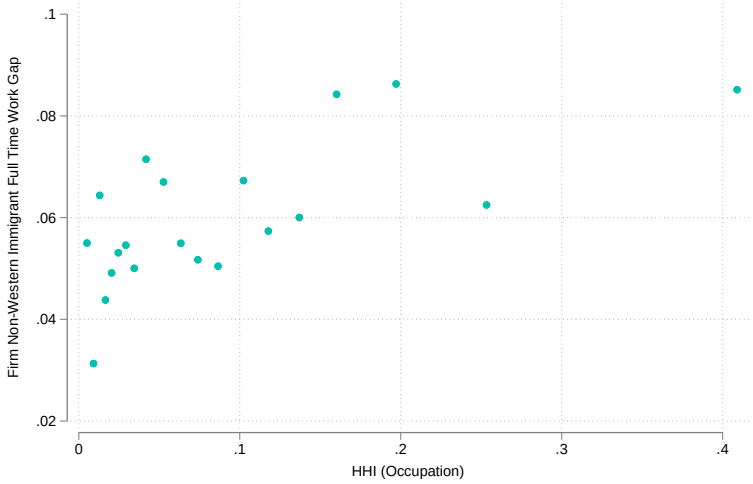
Panel B: Share NWI, Private Sector



Panel C: Share NWI, All Sectors with Industry Fixed Effects



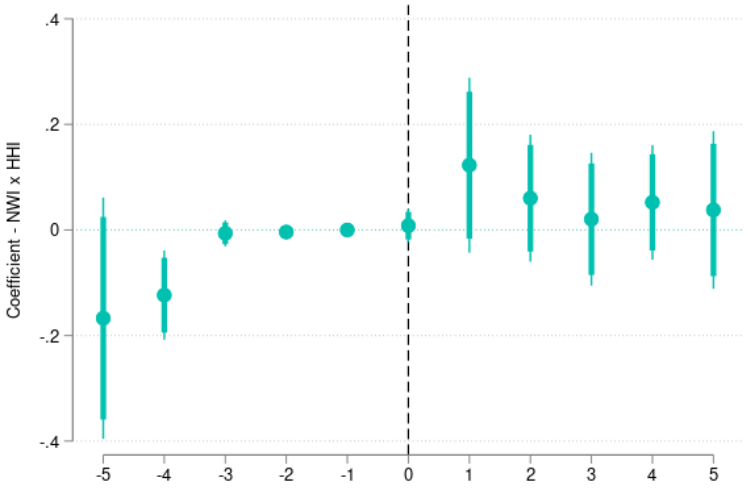
Panel D: NWI Full-Time Status Gap within Firm



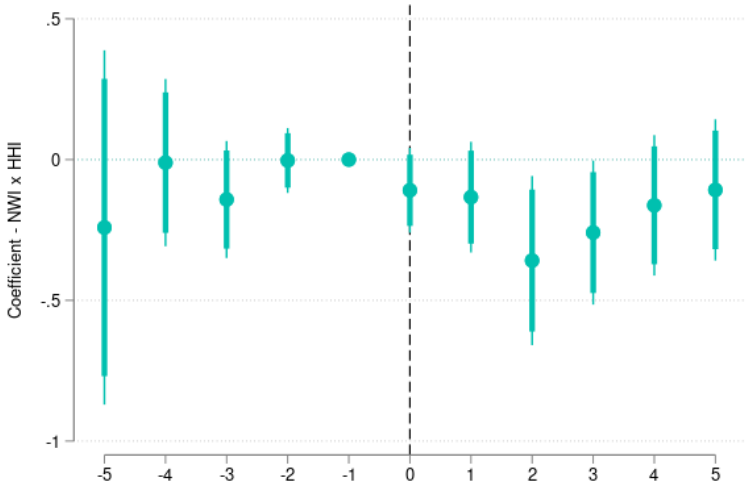
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. In Panel D, we calculate the share of workers from each group within each firm that are full-time and plot the differences between the full-time share of natives and Western immigrants compared to non-Western immigrants.

Figure A-2: Dynamic Effects of Employer Discrimination Matching on Destination Firm

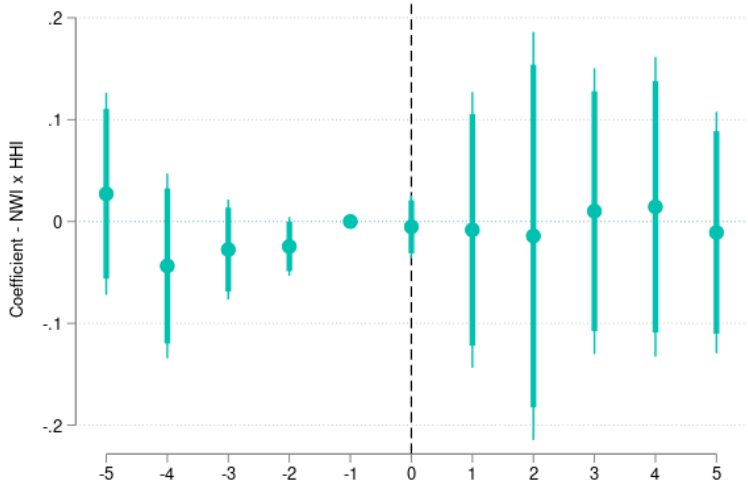
Panel A: Part Time Work



Panel B: Log Earnings



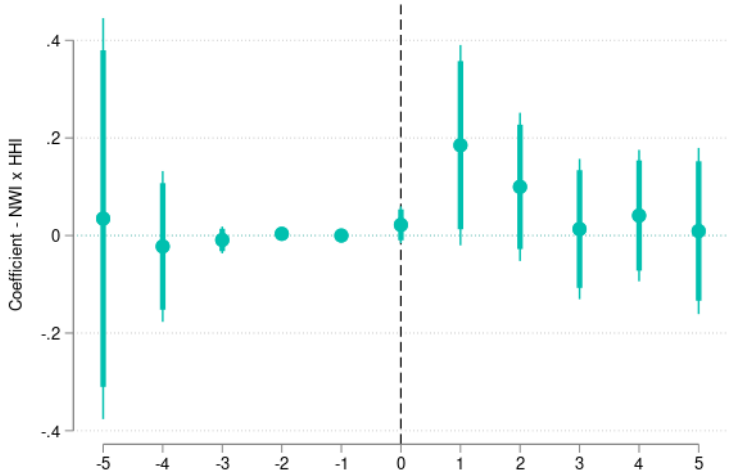
Panel C: Firm Fixed Effect of Destination Firm



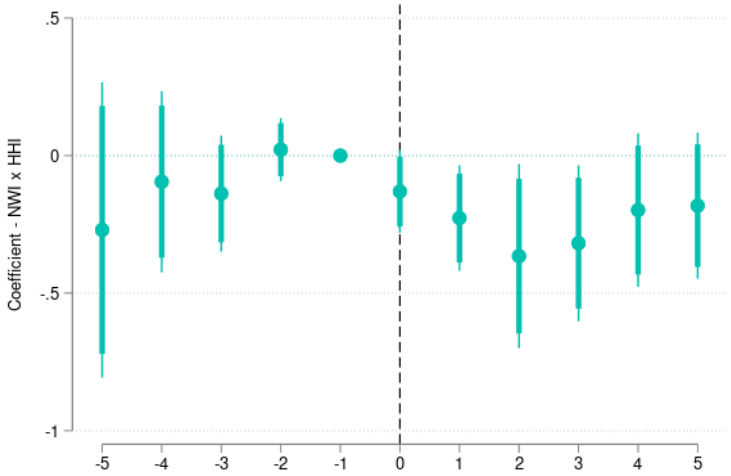
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates in Panels A and B correspond to the θ parameters of Equation 4 in the text, where ω_{pt} is a firm fixed effect. For Panel C, the estimates correspond to Equation 3 using the destination firm's fixed effect as the outcome variable, omitting the post-layoff firm fixed effect.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-3: Dynamic Effects of Employer Discrimination Matching on Post-Displacement Occupation, Local Labor Market

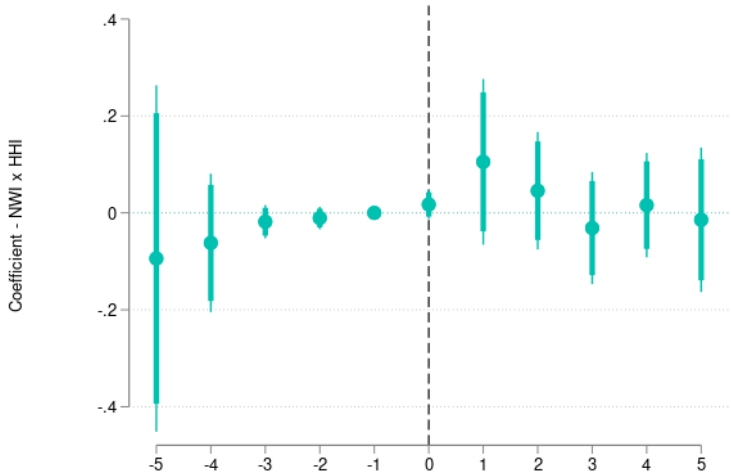
Panel A: Current Occupation FE, Pr(Part Time)



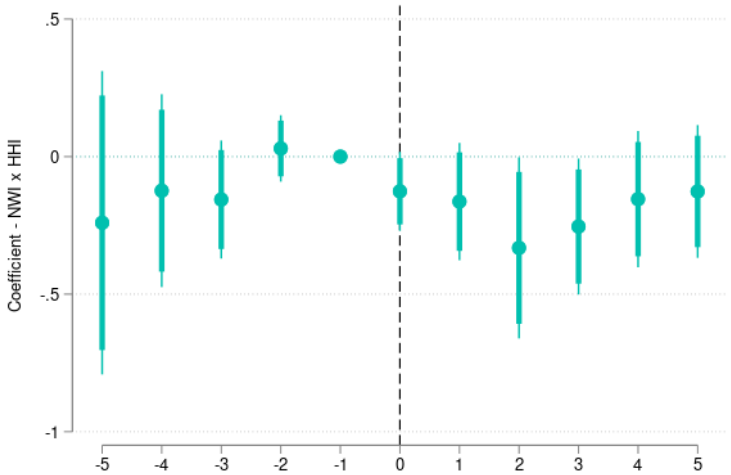
Panel B: Current Occupation FE, Log Earnings



Panel C: Current Occupation by LLM FE, Pr(Part Time)



Panel D: Current Occupation by LLM FE, Log Earnings

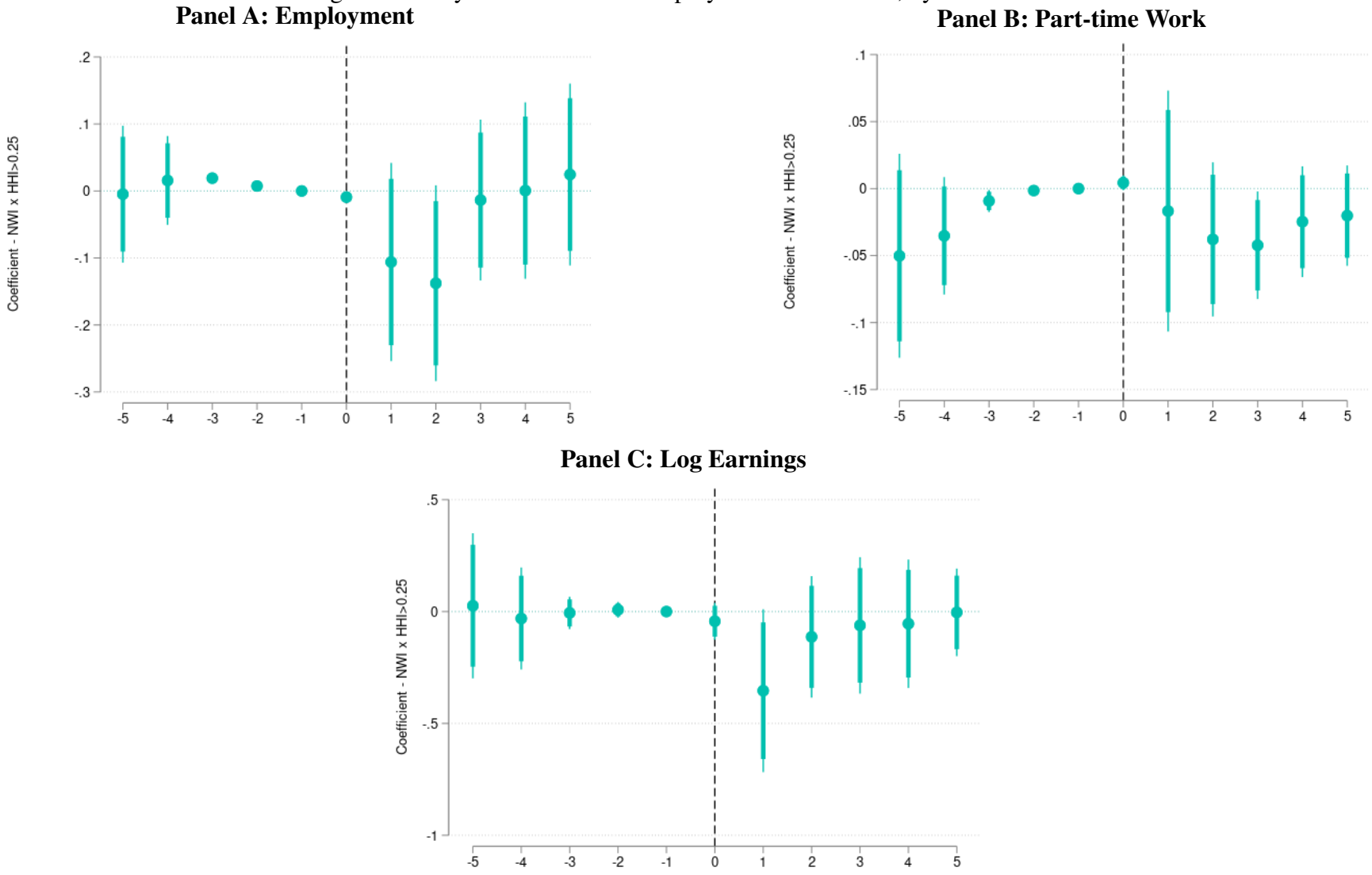


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 4 in the text, where ω_{pt} is a fixed effect for occupation (Panels A and B) or occupation by local labor market (Panels C and D).

Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

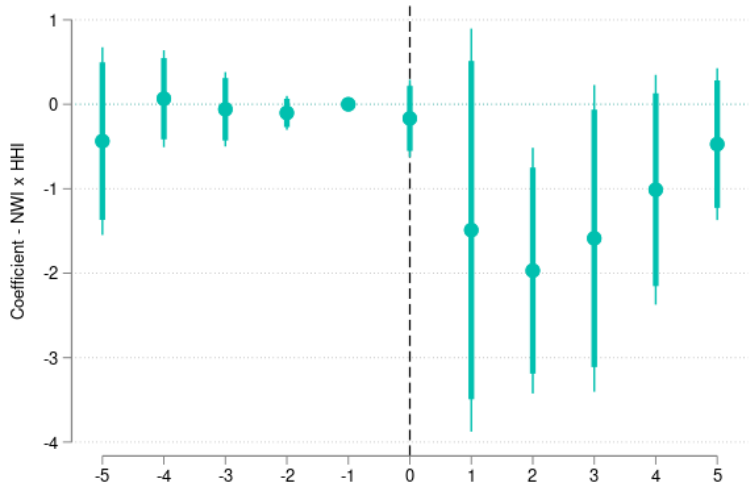
Figure A-4: Dynamic Effects of Employer Discrimination, by HHI > 0.25



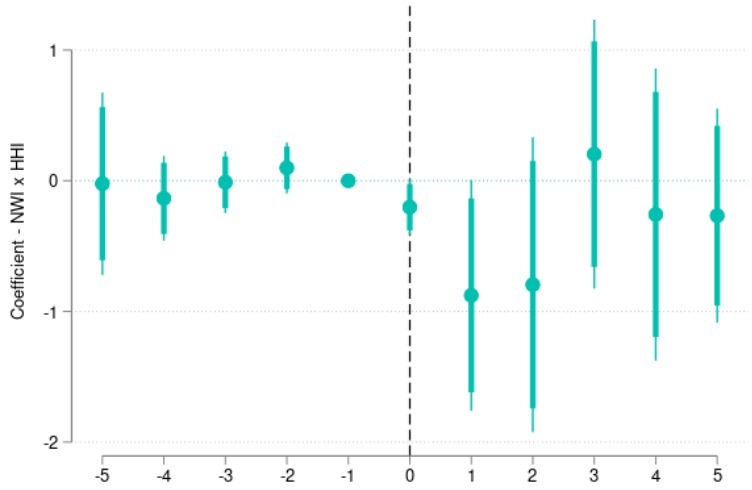
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 3 in the text, but replace the linear HHI term with an indicator for HHI above 0.25.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-5: Dynamic Effects of Discrimination by HHI; Mass Layoffs versus Firm Closures

Panel A: Log Earnings, Firm Closures Only



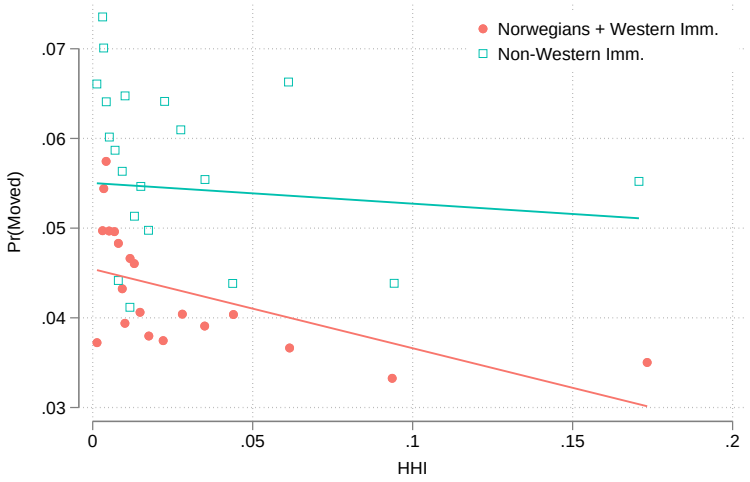
Panel B: Log Earnings, Mass Layoffs Only



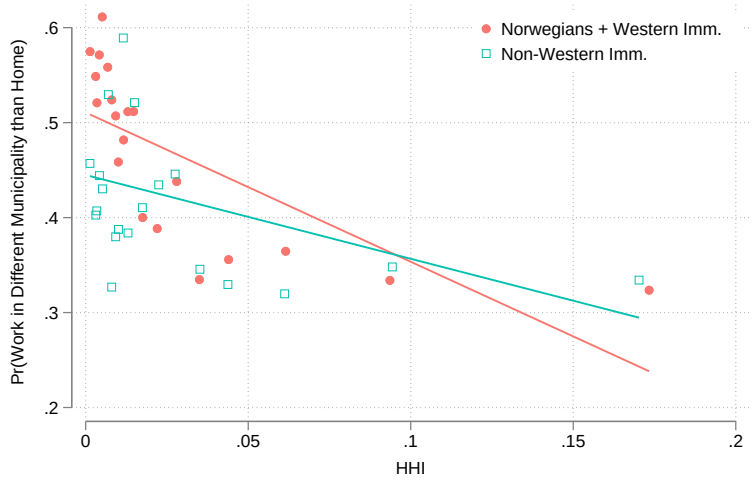
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 3 in the text, estimated separately for firm closure events and mass layoff events.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-6: Measures of Mobility Prior to Displacement by HHI, NWI Status

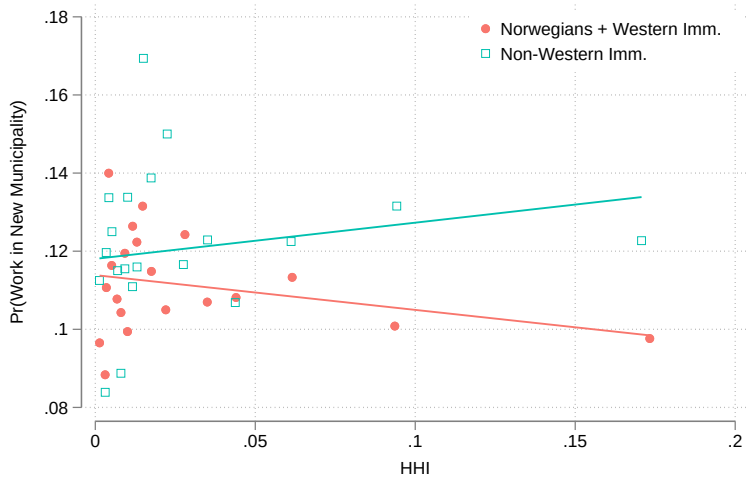
Panel A: Pr(Move Residence)



Panel B: Pr(Work in Municipality Other than Home)

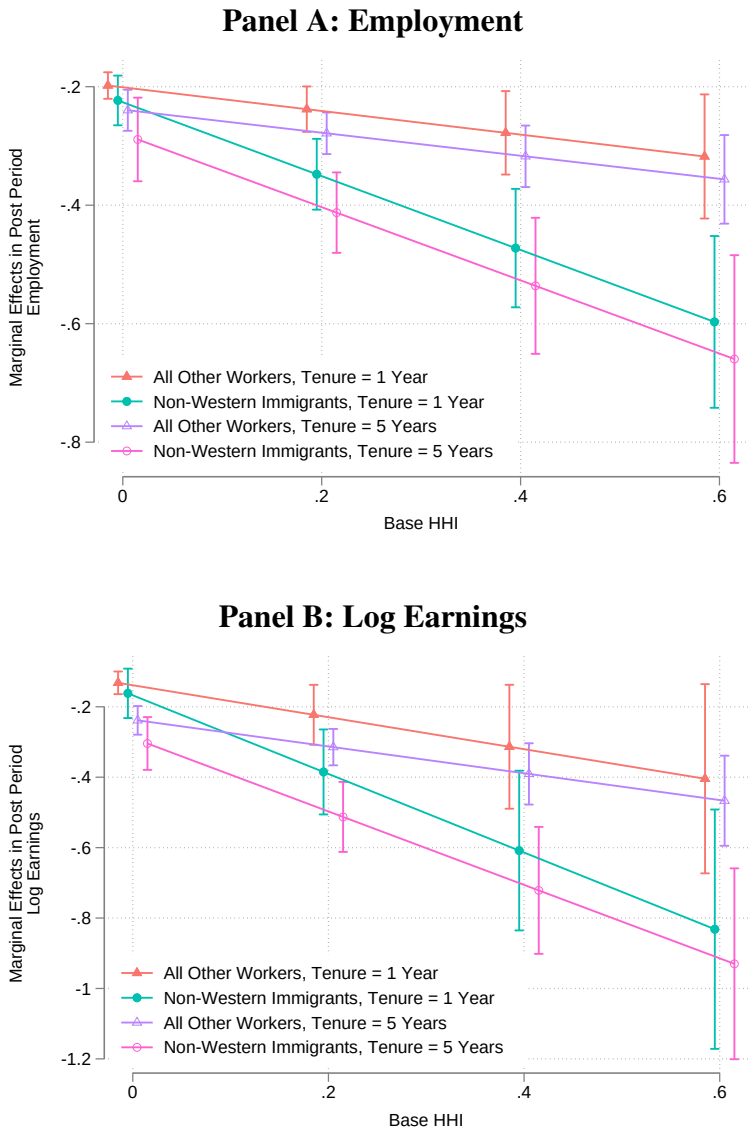


Panel C: Pr(Move Municipality of Work)



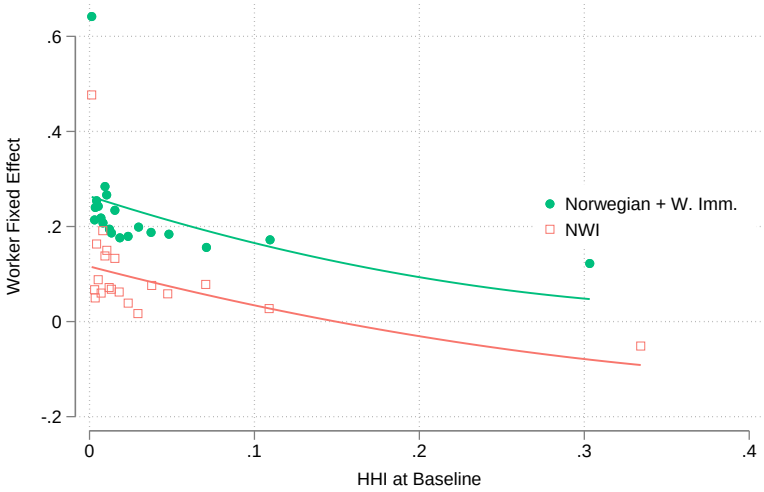
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants.

Figure A-7: Marginal Effects of Employer Discrimination by HHI, Firm-Specific Tenure Prior to Separation



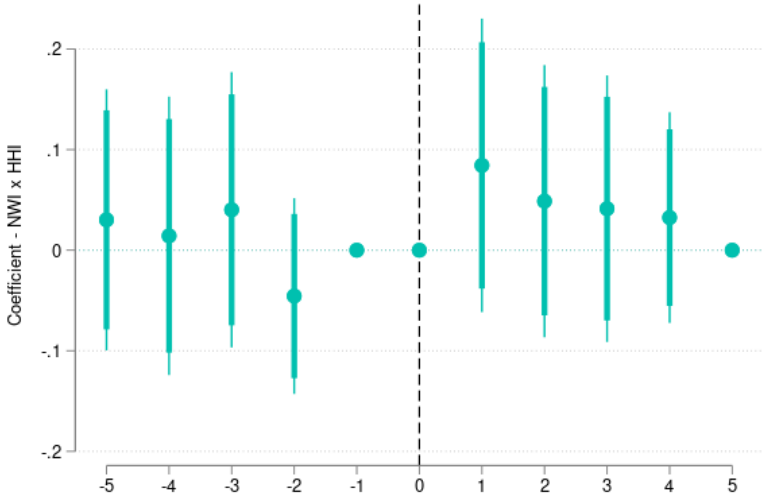
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the marginal effects of Equation 1 with additional interactions for each term with pre-layoff tenure from time $t-1$ to $t-5$.
Bars represent 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-8: AKM Worker Fixed Effects by HHI, Immigration Status



Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Worker fixed effects are extracted from an AKM model of log annual earnings on worker and firm fixed effects over the sample period.

Figure A-9: Dynamic Effects of Discrimination by HHI on Pr(Firm-Firm Transition)



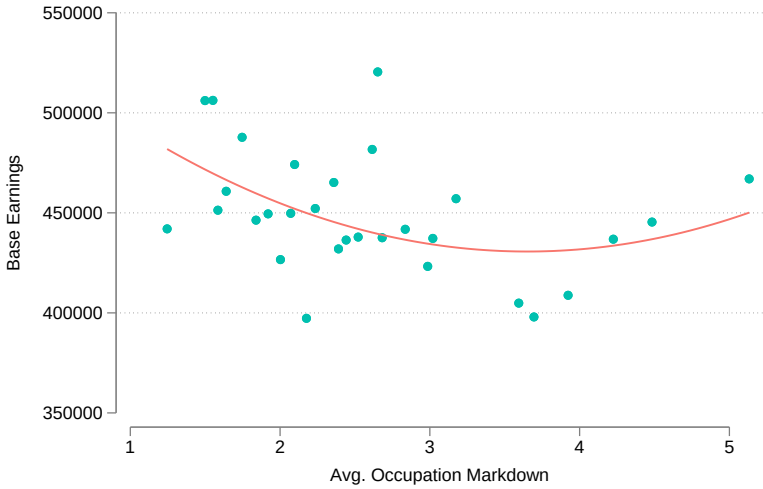
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 3 in the text. The displacement year (year 0) is mechanically omitted because all workers make a transition out of their firm in that year by design. Year 5 is omitted because the sample ends in year 6.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-10: Relationships Between Concentration and Wage Markdowns (Manufacturing, Construction, and Mining)

Panel A: Occupation-LLM Average Markdown vs Concentration

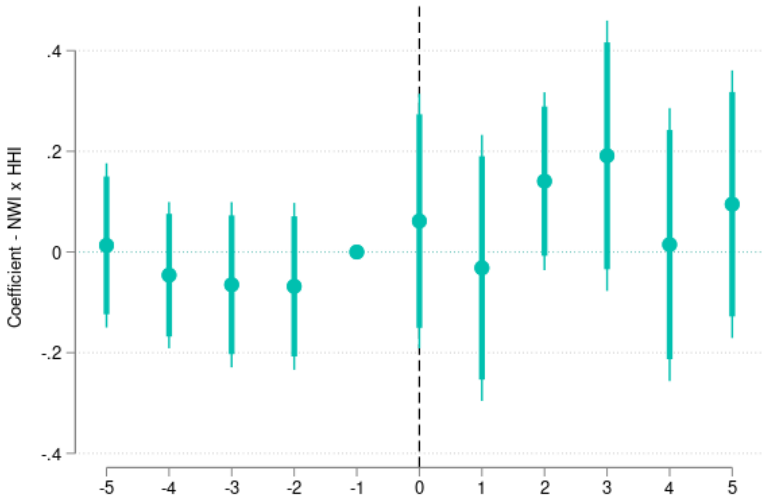


Panel B: Occupation-LLM Average Markdown vs Annual Earnings

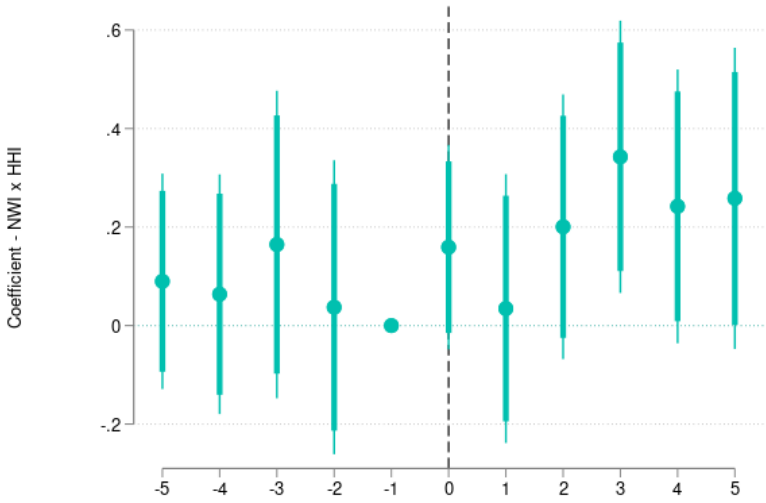


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: Markdowns are estimated using a production function for each industry group (manufacturing, construction, mining). Calculations are for blue collar occupations in order to overlap with the sectors for which production functions can be reliably estimated following Yeh et al. (2022) using raw materials as the flexible and competitively supplied input.

Figure A-11: Event Study Estimates: Firm-Level Average Worker Fixed Effects, by Group
Panel A: Average Worker Fixed Effect of New (Non-Displaced) Norwegian Hires

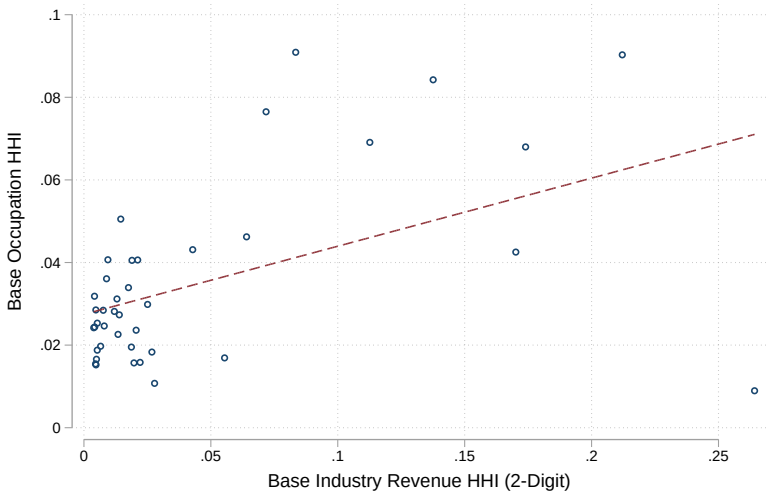


Panel B: Average Worker Fixed Effect of New (Non-Displaced) NWI Hires



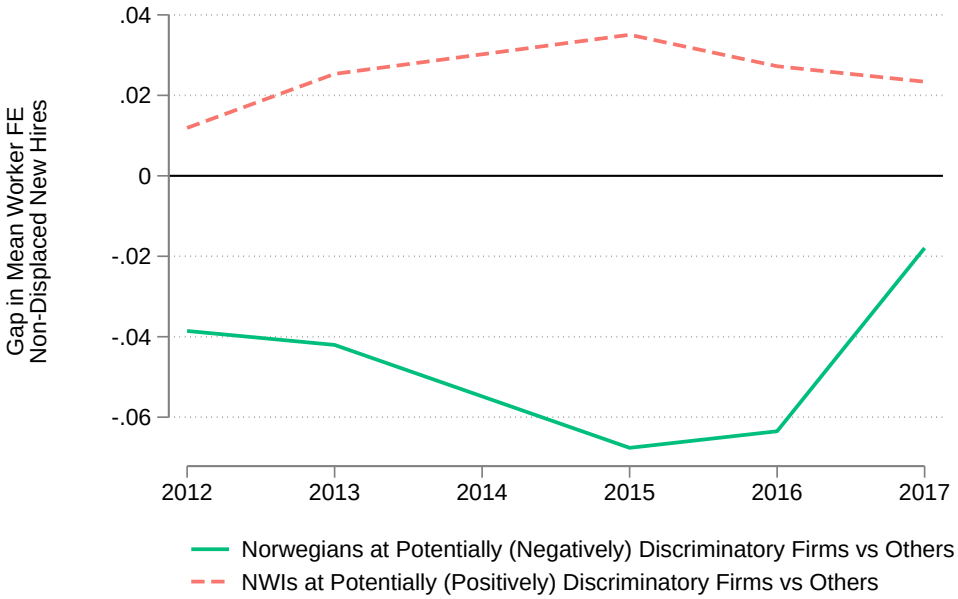
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: Estimates correspond to Equation 3 with the average of firm-level worker fixed effects for new hires as the outcome variables. Estimates correspond to the θ parameters of Equation 3. Worker fixed effects are extracted from an AKM model of log annual earnings on fixed effects for worker, firm, and year. The outcome variables are the mean values for new hires at the firm, defined as those who joined during the previous year.

Figure A-12: Baseline LLM-Occupation Employment HHI vs Baseline 2-Digit Industry Revenue HHI



Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

Figure A-13: (Negatively) Discriminatory Firms Hire Less Productive Norwegians, (Positively) Discriminatory Firms Hire More Productive Immigrants



Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. The graph is calculated by first estimating an AKM model with worker and firm fixed effects between 2003 and 2017 for all workers in Norway. We then divide workers into demographic groups (i.e. Norwegians and Non-Western immigrants), displacement status groups (formerly displaced by a mass layoff or firm closure versus not), and new hire status. We then calculate the average worker fixed effect from the AKM model among new hires that were not previously displaced in each firm-year cell for each demographic group.
We identify potentially negatively discriminatory firms as those that hired a displaced Norwegian from our layoff sample that year and no displaced non-Western immigrants. We identify potentially positively discriminatory firms as those that hired a displaced non-Western immigrant in that year and no displaced Norwegians. We then calculate the gap in the average worker fixed effect among these hires for firms that were likely to discriminate (positively or negatively) versus those that were not.
The results suggest that there is an allocative efficiency cost of the belief-based discrimination we document: firms that hired displaced Norwegians over non-Western immigrants hired marginally less productive Norwegians from the non-displaced pool. Firms that likely had better information on the productivity of non-Western immigrants and hired a displaced non-Western immigrant instead of displaced Norwegians ended up hiring more marginally more productive non-Western immigrants from the non-displaced pool. This may be in response to being able to hire marginally more productive non-Western immigrants who may have been subject to discrimination. These findings imply that firms engaging in belief-based discrimination suffer efficiency losses by hiring less productive natives, while those that do not achieve efficiency gains by hiring more productive workers from diverse backgrounds.

B Alternative Explanations and Tests

Because our triple-difference design compares NWIs to natives and Western immigrants from the same pre-displacement firm, in the same occupation and region, with similar baseline earnings and tenure, and includes rich covariates and individual fixed effects—along with robustness to conditioning on the same post-displacement firm—any alternative explanation must operate differentially by group, only in concentrated markets, within firms and occupations, conditional on observables, and affect NWIs but not Western immigrants or second generation NWIs.

Table C-1: Summary of Explanations and Tests

Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)

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Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Differential Search	Conditional on being laid off from the same firm, occupation, and LLM—with identical tenure and baseline earnings—NWI must search less effectively or accept worse matches than natives/Western immigrants (or second-generation NWI), but only in concentrated markets.	• Restricting to same post-displacement firm/occupation/occupation–LLM cell → eliminates the scope for differences in search effort to operate through sorting into different firms, occupations, or local labor markets.(Figure A-2). • No differential mobility effect: concentration is associated with greater cross-municipality employment for NWIs than for natives → suggests more intensive search by NWIs, not less; no evidence of NWIs returning to home country more in concentrated markets; no evidence of NWI being more likely to take up self-employment in concentrated markets (Figure A-6, Table A-7). • If search behavior or effectiveness differed systematically by tenure at the pre-displacement firm because longer-tenured workers may have clearer ability signals or stronger networks, we would expect the post-displacement HHI gradient to vary with tenure. In particular, longer-tenured workers should show smaller (or different) concentration effects if such factors matter. We find no such interaction effect: the HHI gradient is similar across tenure (Figure A-7). • Similar to tenure, we might expect non-Western immigrants from former Soviet bloc countries in Eastern Europe to differ in search methods or networks from NWI originating outside Europe—potentially making Europeans marginally more similar to native Norwegians or Western immigrants (e.g., Poland compared to East Africa). The absence of any statistically significant difference in effects between these two subgroups (Table A-9) suggests that search behavior is unlikely to drive the NWI–native gap (Table A-9). • Generalized propensity score (GPS) adjustment for baseline HHI among NWIs → explicitly models and adjusts for baseline sorting patterns of natives and NWI across markets with different levels of concentration. The results are unchanged, so post-displacement gaps are not driven by pre-existing differences or initial sorting behavior of where NWIs work at baseline, and, therefore, where they are likely to search next (Table A-12, Panel A). • Reweighting NWIs in concentrated markets to look like NWIs in competitive markets yields nearly identical results → rules out the possibility that differences in characteristics of NWI across these markets drive search-related outcome differences (Table A-12, Panel B). • No unobserved productivity gradient: worker fixed effect in an AKM-style model with firm–immigrant interaction vs. concentration shows no differential productivity–HHI slope for NWIs → rules out the possibility that, in more concentrated markets, NWIs have systematically lower underlying productivity than natives—something that could otherwise explain worse post-displacement matches without invoking discrimination (Figure A-8). • No differential effects are found between NWI and natives attached to the public sector, which operates under highly formalized and transparent recruitment procedures with limited scope for informal networks or discretionary screening → If differences in search behavior across HHI levels by immigration status were driving the results, we would expect similar gaps to persist even in this setting, yet we observe none (Table A-20). • Including neighborhood × time fixed effects → rules out spatial mismatch or neighborhood-based search frictions (Panel C of Table A-8). • NWIs have fewer financial assets than natives → stronger incentive to search intensively, not less. (Table A-21); are also less likely to take up social welfare benefits following displacement episodes (Table A-24). • No evidence of excess job-to-job mobility among NWIs post-hire → rules out gradual search catch-up explanation (Figure A-9).

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Selection into HHI	Non-Western immigrants may self-select into more concentrated labor markets (high HHI) based on unobserved characteristics—such as lower productivity, weaker networks, or worse language skills—that also affect post-displacement outcomes. For this mechanism to explain our results, it would also have to be the case that second-generation NWIs, Western immigrants, and long-tenured NWIs do not engage in the same selection.	• Generalized propensity score (GPS) adjustment for baseline HHI among NWIs → explicitly models and adjusts for baseline sorting patterns across markets with different levels of concentration; results unchanged, so post-displacement gaps are not driven by pre-existing differences in where NWIs work (Table A-12, Panel A). • Reweighting NWIs in concentrated markets to look like NWIs in competitive markets yields nearly identical results → rules out differences in selection on observed characteristics for NWIs in concentrated markets (Table A-12, Panel B). • Exact matching on pre-displacement firm × occupation × industry × llm × earnings → forces comparisons between observationally identical workers; rules out sorting on observables (Table 2). • Worker fixed effects and AKM slope tests → no evidence of differential unobserved productivity gradients across HHI for NWIs vs. natives and Western immigrants (Figure A-8). • No concentration gradient among second-generation NWIs → rules out selection based on cultural or inherited disadvantage (Table 4, Panel A). • No gradient for Western immigrants → rules out broad foreign-born or migration-based selection patterns (Table 4, Panel B). • Results replicate in firm closure subsample (not looking at mass layoffs where there is a theoretical risk of selection into treatment) → rules out endogenous selection into displacement or correlated shocks with HHI (Figure A-5).
Occupational Sorting	Non-Western immigrants may, after displacement, be more likely to transition into low-wage, low-mobility, or high-HHI occupations. If these post-displacement occupations also offer worse outcomes—particularly for NWIs in concentrated markets—this could generate the observed gaps without requiring employer discrimination.	• Post-displacement occupation quality unchanged across groups → no evidence that NWIs transition into lower-ranked occupations or occupations with lower AKM-estimated fixed effects relative to natives in concentrated markets (Table A-13). • If the wage gap were driven by sorting into different types of jobs after displacement, it should disappear when comparing workers re-employed in the same occupation at the same firm. In fact, NWIs still earn less than natives in these within-firm, within-occupation comparisons, indicating that the gap reflects differences in how employers value otherwise identical workers rather than differences in the jobs they obtain (Figure A-3). • Controlling for current occupation and local labor market in the post-period does not affect results → Appendix Figure A-3 closely resembles core event study, indicating sorting post-displacement is not driving the gap.

Table C-1: Summary of Explanations and Tests

Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Mean Reversion / Uneven Recovery from Layoff	Patterns of recovery might differ systematically across the distribution of earnings prior to the layoff event. If Non-Western immigrants are consistently lower in the earnings distribution and lower-earning workers recover differently from the layoff, this might affect the measured gaps later. On the other hand, if those higher in the income distribution are simply at a temporary peak, they might revert back to their typical earnings after the layoff.	• In all out augmented match specifications, we control for earnings by event time interactions (both in linear interactions as well as earnings decile), which flexibly controls for any differential recovery paths that might occur across the pre-layoff earnings distribution. These comprehensive controls do not reduce our coefficients but make them slightly larger in magnitude (though not statistically significantly different) (Table 2 and A-3).
Wage Compression, Internal Pay Equity, or Morale Concerns at Baseline Firm	If NWIs are less productive than their native counterparts, pay equity or morale concerns might lead to compression in the wage distribution (i.e., less-productive NWIs making the same amount as their native counterparts are overpaid relative to their productivity), then the layoff reveals those differences in productivity to the market, and the post-layoff firm pays NWIs closer to their true productivity at a lower wage.	• While pay equity and morale constraints cannot be observed in the data, for these constraints at the baseline firm to meaningfully explain our results, higher HHI at baseline (as well as larger wage markdowns within the firm) would need to be positively correlated with internal pay equity constraints within firms, and such pay equity constraints would need to be specific to NWIs within the same occupations. It would be curious if morale concerns were a meaningful constraint on firm pay-setting at the same time that workers in the local labor market are paid significantly less than their marginal product (Table A-25). • If internal pay equity or morale concerns were a driving factor, we might expect the results to differ based on pre-layoff union density, since union density in Norway has been shown to compress the wage distribution (e.g., Dodini et al. (2022)). When we estimate our models including a interaction between all of our key variables with the baseline firm's union density (standardized to mean 0 standard deviation 1), the interaction coefficient with post x NWI x HHI is not statistically significant and is small relative to the main NWI x HHI x Post interaction (Table A-19).

Table C-1: Summary of Explanations and Tests

Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Different Positions on the Job Ladder / Longer Job Ladder	Non-Western immigrants might be at a different place in their job ladder than native Norwegians and Western immigrants (over prejudiced firms or career stages), meaning that minorities would get a smaller gain to an Employed-Employed job transition. Convergence in E-E transitions might indicate being at a similar place in their job ladder (Sorkin, 2025).	• In the full employment register, E-E transition rate gaps between Norwegians and Non-Western immigrants are below 3% of the mean and statistically indistinguishable from zero when simply conditioning on earnings or age. When we condition on occupation and firm, as in all of our empirical models, immigrants of all kinds are slightly less likely to make an E-E transition. • Non-Western immigrants do not have a different post-layoff experience in terms of occupational sorting on occupation earnings rank (Table A-13). Conditioning on post-layoff occupation does not alter the earnings gaps (Figure A-3). • Non-Western immigrants in more concentrated markets are not meaningfully more likely to make a firm-firm transition after the initial layoff event (Figure A-9), consistent with a similar job ladder and position on the ladder. • If job ladder position is correlated with age, controlling for age trends while also conditioning on baseline occupation should alter the coefficients, but they do not (Table A-8 columns (4)-(6) show gaps grow slightly larger but not statistically significantly larger relative to baseline).

Table C-1: Summary of Explanations and Tests

Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Match Quality/Match Effects	Non-Western immigrants might simply generate worse match quality before and after their layoff events. Lower match quality makes the firm reward the workers less at first. These might be a concern if match effects are a notable feature of the market.	• If initial match quality were an issue after the mass layoff/firm closure, we would expect Non-Western immigrants to have greater firm-to-firm transitions after the event as they continue on-the-job search. However, differences between NWIs and other workers in concentrated markets are not statistically different in any year before or after displacement (Figure A-9). • Following Sorkin and Warwar (2026), we implement a paired movers design directly in the Norwegian employer-employee data. After residualizing log earnings on a quartic in age and year fixed effects and implementing their IV strategy, we test for the presence of match effects over the HHI distribution. The critical test for our setting is whether the portability of wages differs across concentration for non-Western immigrants, or in other words, if there is a negative HHI gradient for this group. We test this in a triple interaction in which we interact base firm wage gaps on the right hand side with HHI, with an NWI indicator, and with $NWI \times HHI$. For match effects to explain the negative earnings trajectory, we would expect $NWI \times HHI$ to be strongly negative and significant. The triple interaction between the origin wage gap, NWI status, and HHI is positive and significant at 0.332, meaning NWI wages become relatively *more* portable as market concentration increases. At HHI levels in the highly concentrated range (≥ 0.25), NWIs have equal or greater portability than natives. This is "wrong-signed" for match effects to explain our HHI gradient. If anything, the relative importance of match effects falls for NWIs in concentrated markets (Table A-18). • Our specifications with post-displacement firm fixed effects non-parametrically control for fixed worker characteristics as well as baseline and destination firms. The convergence in earnings occurs primarily within destination firms rather than through mobility across firms. (Figure A-2). • Explaining these convergence patterns through match quality would thus require a time-varying within-firm improvement affecting immigrants but not natives, and only in concentrated markets—a mechanism without clear theoretical grounding. Such a story would further imply systematic initial undermatching of immigrants (or vastly overpaying them) in high-HHI markets despite identical prior earnings, again pointing to belief-based mechanisms at hiring. This would imply either that baseline firms in high-HHI markets knowingly overpaid lower-productivity immigrant workers relative to natives, or that prospective employers uncover latent productivity differences at job search in a way tightly correlated with market concentration and capable of reproducing the dynamic convergence patterns we document within post-displacement firms. We view both scenarios as implausible.
Selective Attrition	If the least productive Non-Western immigrants drop out of the sample and selectively attrit as they are unable to find work, the earnings gap would reflect this selective attrition.	• Our inclusion of post-displacement firm fixed effects rules out differential exit from destination firms as a driver of the convergence patterns. Most of the convergence in earnings happens *within* firms. (Figure A-2) • If employment gains were driven by higher-ability immigrants being hired first, the marginal immigrant hired at the same firm would be of lower ability, which would mechanically imply earnings *divergence* rather than convergence. The employment dynamics we observe, therefore, move earnings in the opposite direction of what selective attrition of lower-ability workers from employment would predict.

Table C-1: Summary of Explanations and Tests

Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Urban/Rural Divides as a Proxy for Prejudice	If HHI proxies for urban/rural divides (i.e., smaller geographic areas are more concentrated) and if rural areas are more likely to be prejudiced against immigrants, then HHI interactions capture rural prejudice rather than market power mechanisms.	• Our inclusion of fixed effects in our matching scenario for local labor market (LLM) ensures that we are leveraging comparisons within the same labor market. Identifying variation in HHI includes within-LLM, cross-occupation, cross-industry differences in market power, which is inconsistent with alternative explanations correlated with the size of the local labor market. • Similarly, a mass layoff of the same nominal size would have a disproportionate impact on smaller labor markets, so controlling for the size of the mass layoffs in the base year as a share of total local employment would likely shift our estimated coefficients, but this does not happen (Table A-23). • Importantly, if HHI were simply a proxy for prejudice, our analysis would not be robust to changing the metric of market power to measured local average wage markdowns. However, when using production functions to estimate markdowns, the negative effects of market power persist, and, in fact, are of a similar magnitude as our HHI estimates (Figure A-10, Table A-25). • If prejudice from rural populations were driving our results, the inclusion of flexible neighborhood by relative time fixed effects would also strongly attenuate our estimates, but our estimates are unchanged when we include these controls (Table A-8).
Labor Market Slack	If HHI proxies for underlying labor market slack per se rather than labor market power, the differences between the effects of HHI on NWIs and natives may reflect cyclical factors rather than competition.	• If HHI is a proxy for underlying labor market slack, HHI should be strongly correlated with measures of, for example, unemployment at the local level. However, even the unconditional correlations between the intensity of unemployment benefits receipt in the local labor market and measures of HHI are weak. In regression format, a standard deviation increase in the average unemployment benefits per worker in a local labor market is only associated with a 0.006 point increase in our HHI measure. Conditional on our matching variables, the direction of this relationship flips sign, meaning that underlying slack is conceptually different from concentration, and this slack is unlikely to explain our results (Table A-2).

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Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Social Networks / Homophily / Group-Specific Amenities	Non-Western immigrants may lack access to native networks or face exclusion due to ethnic homophily in hiring. These network disadvantages could reduce job-finding success, lead to worse matches, or different group-specific amenities across firms would generate compensating differentials. For this mechanism to explain our results, it would have to operate only in concentrated markets and apply to non-Western immigrants—but not to Western immigrants or second-generation NWIs.	• Post-displacement earnings gaps persist even when comparing workers rehired into the same destination firm or the same occupation. This rules out explanations in which referral networks or homophily operate primarily by steering workers into different firms or occupations of differing quality. It also holds constant amenities that could differ across firms and influence the sorting behavior across firms. The persistence of the gap within the same firm and occupation instead points toward differential treatment of otherwise identical workers once hired (Figure A-2 and A-3). • No relationship between manager demographics and NWI hiring rates → no evidence of demographic-based preferential hiring or ethnic clustering under similar managers (Table 4, Panel D and Table A-11). • Neighborhood composition does not affect concentration gradient → NWIs in neighborhoods with more managers or fewer co-ethnics do not experience smaller gaps; if anything, outcomes are slightly worse (Table A-14 and A-15). • No sorting into firms with more NWIs or demographically distinct coworkers → rules out within-group network clustering as well as group-specific amenities as a driver of job matching (Table A-16). • NWIs equally likely as natives to work with a neighbor post-displacement → neighborhood-based social capital is not systematically stronger or weaker across groups. (Table A-16) • Destination firms for NWIs do not differ in average wage premia → no systematic difference in firm quality by concentration × nativity. (Figure A-2 Panel C)
Contact Hypothesis	The contact hypothesis suggests that exposure to diverse coworkers reduces prejudice and improves treatment of minority groups. If immigrants are more likely to work alongside natives in competitive (low-HHI) markets—and more segregated in concentrated (high-HHI) markets—this could explain differential treatment. For this explanation to hold, it must operate only for non-Western immigrants, only in concentrated markets, and not apply to Western immigrants, second-generation NWIs, or long-tenured NWIs.	• No effect of neighborhood immigrant share on early post-layoff hiring → managers living in areas with more NWIs are not more likely to hire NWIs, suggesting non-work exposure does not affect preferences (Table 4, Panel D and Table A-15). • No evidence of differential occupational sorting in early post-displacement years → NWIs are not pushed into lower-contact or lower-paying roles initially and then reallocated, undermining the exposure-driven belief adjustment story (Table A-13 and Figure A-3). • Wage gaps exist within the same firms immediately after hire → implies that firms discriminate after hiring, which is inconsistent with initial animus being overcome through contact (Figure A-2).

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Labor Demand Shocks	Any local labor demand shocks that generated the need for a mass layoff or firm closure might be more severe in more concentrated markets and disproportionately harm the prospects of NWIs. Larger shocks might drive worse outcomes in high-HHI areas for NWIs, even conditional on displacement.	• The layoff design leverages differences in HHI across occupations within local labor markets as well as differences in HHI within occupations across local labor markets. If higher-HHI local labor markets consistently had worse economic conditions, the local labor market match effects would average out these effects across all occupations. • Generalized propensity score (GPS) adjustment for baseline HHI among NWIs → explicitly models and adjusts for baseline sorting patterns across markets with different levels of concentration; results unchanged, so post-displacement gaps are not driven by pre-existing differences in where NWIs work (Table A-12, Panel A). • Reweighting NWIs in concentrated markets to look like NWIs in competitive markets yields nearly identical results → rules out differences in selection on unobserved characteristics for NWIs in concentrated markets (Table A-12, Panel B). • Control for total layoff intensity in occupation–local labor market cells → accounts for the severity of displacement shocks that might differentially affect NWIs in high-HHI areas. The post-displacement concentration gradient for NWIs remains unchanged after including these controls → indicates that differential severity of local demand shocks do not drive the observed gaps. If they did, we would expect the NWI × HHI interaction to attenuate when accounting for shock severity. (Table A-23).
Social Assistance Take-Up	If first-generation NWIs are more likely to access social assistance after displacement—and if those benefits are more generous or more accessible in concentrated markets—this could reduce their earnings mechanically through lower search effort or earnings-related eligibility thresholds.	• Estimate post-displacement program participation across groups and markets → no systematic differences that align with earnings gaps; if anything, NWIs in concentrated markets are *less* likely to receive unemployment benefits. (Table A-24) • NWIs less likely to claim disability benefits across all markets → contradicts the idea that they exit the labor force more readily than natives. (Table A-24, Panel A) • Controlling for social assistance in main regressions (sick leave and disability payments, i.e. those not directly tied to employment outcomes) does not attenuate NWI × HHI effects → differences in benefit take-up do not explain employment or earnings gaps. (Table A-24, Panel B)

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Alternative Explanation	Summary of Concern	Evidence Against Mechanism + Relevant Table(s) and/or Figure(s)
Language Barriers	Weaker Norwegian or English proficiency among NWIs could limit post-displacement job opportunities or wages—especially in concentrated markets where lower mobility reduces outside options—and thus generate the observed concentration gradient without employer discrimination. For this explanation to hold, it would need to affect NWIs but not Western immigrants, second-generation NWIs, or long-tenured NWIs.	• Substantial share of sample NWIs are blue-collar workers, where Norwegian language demands are minimal and tasks are less communication-intensive → reduces scope for language proficiency to drive large wage gaps. (Table A-1). • No concentration gradient for NWIs in the public sector, where Norwegian proficiency is a strong formal requirement → rules out language barriers as an explanation, since this is precisely the setting where language differences should matter most. (Table A-20). • No statistically significant difference in concentration effects between Eastern European NWIs (e.g., Poland, former Soviet bloc) and non-European NWIs (e.g., East Africa), despite likely differences in Norwegian proficiency / English proficiency → inconsistent with language barriers driving the NWI–native gap. (Table A-9). • Our estimates condition on pre-layoff earnings, occupation, and firm. If language was a real barrier to worker productivity, conditioning on pre-layoff earnings means that, by construction, any such language barriers were not sufficient to generate pre-layoff differences in earnings for NWIs and their native coworkers.