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ABSTRACT

The Power to Discriminate*

This paper examines the relationship between labor market power and employer discrimination, providing new causal evidence on when and where discriminatory outcomes arise. We leverage mass layoffs and firm closures as a source of exogenous job search and combine this with an exact matching approach. We compare native–immigrant worker pairs who held the same job at the same firm, in the same occupation, industry, location, and wage prior to displacement. By tracking post-displacement outcomes across labor markets with differing levels of employer concentration, we identify the causal effect of labor market power on discriminatory behavior. We provide four main findings. First, wage and employment discrimination against immigrants is substantial. Second, discrimination is amplified in concentrated labor markets and largely absent in highly competitive ones. Third, product market power has no independent effect, consistent with the idea that wage-setting power is necessary for discriminatory outcomes. Fourth, gaps fade with sustained employer–immigrant interactions, consistent with belief-based discrimination and employer learning.

JEL Classification: J7, J61, J42, J63

Keywords: discrimination, immigration, market power

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1 Introduction

Employer power and labor market discrimination are two fundamental economic policy issues 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 sources of economic inequality are also strongly connected, as non-competitive market forces and the existence of supernormal profits have long been theorized as essential conditions for firms' ability to sustain discriminatory practices over time (e.g., Becker (1957)).

The well-established theoretical link between market power and employer discrimination has spurred extensive empirical research on how increased *product market* competition affects discriminatory practices (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)). Yet, despite decades of research, direct empirical evidence on the role of *labor market* power remains largely absent, leaving a critical gap in our understanding of how market power shapes discrimination. While both product and labor market power create supernormal profits that can offset the costs of discrimination, only labor market power allows firms to set wages and differentiate pay among equally productive workers. Consequently, the extent and nature of discrimination likely depends on which of the two forms of power firms possess. Against the backdrop of declining worker power (e.g., Stansbury and Summers (2020)), rising employer concentration (e.g., Azar et al. (2020b)), and persistent native-immigrant wage gaps (e.g., Amo-Agyei et al. (2020)), understanding the relationship between labor market competition and employer discrimination is critical.

This paper breaks new ground by providing novel empirical evidence of how labor market power shapes employer discrimination, unveiling a crucial but understudied facet of inequality. Using linked employer-employee data from Norway, we analyze real job seekers of equal productivity, identical labor market signals, and matching search conditions, but differing migration backgrounds. Leveraging quasi-random job search variation induced by involuntary job displacement—events entirely beyond workers' control—we compare the employment and earnings outcomes of native Norwegians and immigrants from Western countries (the in-group) to immigrants from non-Western countries (the out-group).¹

We measure labor market power using the Herfindahl–Hirschman Index of employment at the local labor market (commuting zone) by 3-digit occupation level in the year prior to

¹In the absence of discrimination, wages should be a function of a worker's marginal productivity. If wage gaps between equally qualified natives and immigrants emerge after they are induced to search because of a mass layoff or firm closure, this rejects the null hypothesis of no employer discrimination (e.g., Fryer et al. (2013)). If pre-search earnings partly reflect discrimination on the part of the employer or market, our estimates capture a lower bound on discrimination (e.g., Bohren et al. (2022)).

the displacement-induced search episode.² This is equal to the sum of the squared employment shares in a given occupation and geographic region. Our empirical models compare employment and wage gaps during the post-displacement search period between native and immigrant worker pairs with identical productivity and wages who were working in the same occupation and firm at the same time before separation. By contrasting these gaps in *concentrated* markets with those in *non-concentrated* markets in a triple-differences specification, we reveal how labor market power shapes discriminatory hiring and wage-setting practices. To validate our use of HHI as a proxy for market power, we also replicate our analysis on a subset of workers using an alternative measure of local monopsony power—wage mark-downs estimated from production functions following Yeh et al. (2022).

To illustrate our empirical approach, consider two equally qualified native-immigrant worker pairs— both working in the same occupation at the same firm, earning the same wage. Due to a mass layoff or firm closure, both worker pairs are induced to search for new jobs at the same time within their respective local labor markets. In the first local labor market, the market is highly concentrated, with a few dominant employers controlling most job opportunities for the occupations in which they were working. In the second local labor market, the market is more competitive, with a larger number of employers vying for talent in that occupation. In the concentrated market, employers control more of the local labor market, which may provide firms with more opportunities to engage in discriminatory practices due to the presence of wage-setting power and supernormal profits, regardless of whether the discrimination is based on their preferences (e.g., taste-based discrimination) or on their productivity beliefs about each group (e.g., statistical discrimination). In contrast, employers in competitive markets face intense competition for workers. In these markets, firms earn zero economic profits and cannot sustain the cost of discrimination without being driven out of the market.³ By comparing the employment and wage gaps between these equally qualified immigrant-native worker pairs in the two distinct labor markets, we can isolate how labor market power shapes the employment and earnings prospects of natives (the in-group) and immigrants (the out-group).

After establishing the relationship between labor market power and employer discrimination, we incorporate product market power (industry revenue concentration) into our framework. This addition is crucial, as product market power *also* enables firms to absorb the costs

²There are 160 commuting zones in Norway.

³In a finite pool of workers and under concave marginal productivity, excluding productive minority workers reduces the overall productivity of the firm's workforce. Profits among non-discriminatory firms rise and displace 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.

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 does not grant firms the ability to engage in wage discrimination if the labor market remains competitive. Specifically, unless a firm controls part of the labor market, it lacks wage-setting power and cannot discriminate on wages, as workers with a perfectly elastic labor supply will leave for better opportunities if wages drop below the competitive level. On the other hand, firms may still engage in employment discrimination by hiring less productive workers from favored groups over more productive workers from disfavored groups. In such cases, firms can use their product market profits to offset the costs of this discrimination.

Finally, we investigate the sources of discrimination underlying our results, focusing on the relative importance of belief-based versus preference-based discrimination. Theoretically, competition can reduce both types of discrimination, as firms incur costs from engaging in either type of discrimination that cannot be sustained in the absence of supernormal profits. However, the mechanisms through which competition reduces these forms of discrimination differ. For preference-based discrimination, competition increases the cost of maintaining animus toward members of the outgroup. Firms are incentivized to hire based on merit rather than bias, as failing to do so undermines their ability to compete effectively for workers. For belief-based discrimination, competition penalizes firms for inaccurate productivity assessments for individual workers. Relying on simple heuristics or group-based productivity assumptions becomes increasingly costly, prompting firms to absorb the cost of gathering more precise productivity information. This pressure leads firms to act on more accurate and comprehensive assessments of individual productivity, reducing reliance on decision-making based on uncertainty in hiring.

To explore this question, we draw on 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. In contrast, if discrimination stems from preferences (i.e., taste-based discrimination), these gaps are likely to persist over time.

To perform our analysis, we use Norwegian employer-employee linked register data and leverage shocks to employment between 2008 and 2012. 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). 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 must be true for our triple-differences approach to recover the causal effect of market power on discrimination? Crucially, we do not require labor market concentration (HHI) to be exogenous. The key identifying variation comes from mass layoffs and firm closures that exogenously force native and immigrant workers—employed in the same occupation, firm, and local labor market, and previously earning the same wage—into job search. Because we compare their post-displacement outcomes within each market, across levels of pre-existing HHI, we can isolate how firms with more or less market power treat equally productive workers in the same institutional and economic context. We fix HHI in the year prior to displacement to eliminate concerns about reverse causality. The causal interpretation relies not on what determines HHI, but on how it moderates employer behavior once workers are exogenously forced to search. In other words, we hold constant productivity, occupation, firm quality, and local labor market; only the degree of local employer power varies. To interpret our results causally, differences in relative outcomes across HHI must not be driven by differential selection into displacement that systematically varies with market concentration. This is unlikely by design and further ruled out using firm closures—where selection is mechanically eliminated—and via stable pre-trends in our event studies.

We present four core 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. Additionally, among those who secure a job, the annual earnings of the non-Western immigrants are substantially lower. Importantly, these wage disparities are not due to 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 for similar roles at firms of similar quality. These gaps also do not reflect differences in actual productivity between groups: prior to the displacement-induced search, the local labor market valued their productivity at an exactly equal rate, indicating that non-Western immigrants were *at least as productive* as their native and Western immigrant coworkers prior to the firm closing or mass layoff.

Second, discrimination is largely restricted to highly concentrated labor markets. Specifically, employment and wage gaps between equally productive natives and immigrants –ex-

ogenously induced to search for jobs after leaving the same firm and occupation simultaneously—are minimal in competitive markets but substantial in concentrated markets. This suggests that labor market competition serves as an efficient tool for reducing employer discrimination. However, it does not completely eliminate the gaps between non-Western immigrants and the rest.⁴ This finding is consistent with the canonical discrimination theories of Becker (1957), Arrow (1957), and Black (1995), which stipulate that competition will push economic profits to zero and eliminate firms’ ability to discriminate by driving up the cost of engaging in such practices. This direct relationship between labor market power and employer discrimination has never been documented empirically before.

Our secondary analyses rule out several potential alternative explanations and mechanisms for the identified employment gaps, including manager homophily, the density of social networks, social proximity to managers, differences in search intensity, differential sorting by occupation, local labor market, or firm quality, and differential selection of immigrants and natives on unobserved ability during mass layoffs (as shown through analyses restricted to individuals affected by firm closures). Moreover, for any of these or other unobserved mechanisms to explain our findings, they would need to systematically covary with labor market concentration within occupational groups in the same local area or across geographic areas within the same occupation — a scenario we view as improbable (see Section 6).

Third, when focusing on product market power (proxied by industry revenue concentration) while controlling for labor market concentration, we find no employer discrimination. This finding aligns with the idea that firms operating in competitive labor markets encounter a perfectly elastic labor supply curve and must compensate workers according to their marginal revenue product, 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 (not wage discrimination), we find no evidence in support of this in the Norwegian context. This paper is the first to empirically emphasize the importance of the source of employer power in explaining labor market discrimination through causal analysis, offering valuable insights into ongoing discussions about product market power and discrimination.⁵

⁴We 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.

⁵A 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 those results are driven by a strong within-firm correlation between labor market power and product market power (Marinescu et al., 2021a), such that

Finally, we reveal that the employment discrimination we observe is most consistent with belief-based factors, with minimal influence from preferences against non-Western immigrants. This conclusion is drawn from 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.⁶

To ensure these findings reflect continuous interactions with employers within the same firm rather than external networks, we conduct an auxiliary analysis using fixed effects for the firms where displaced workers are hired after their search. 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 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 non-Western immigrants tend to recruit less productive Norwegians, as measured by their worker fixed effects in an AKM model. 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 non-Western immigrants, (b) examining discrimination patterns against Western immigrants and second-generation non-Western immi-

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).

⁶For ease of exposition, “natives” includes immigrants from Western countries in our conceptual framework and our empirical estimation.

grants, and (c) assessing discrimination toward long-term non-Western immigrants. Our findings reveal that managers with greater recent experience working with non-Western immigrants are less likely to engage in discriminatory practices. Additionally, long-term non-Western residents face significantly less discrimination, while Western immigrants and second-generation non-Western immigrants 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 non-Western immigrants—who are most likely to have accurate productivity information for non-Western immigrants—are the managers most likely to hire non-Western immigrants in concentrated markets. These pieces of evidence strengthen our interpretation of the dynamic adjustment of employment and earnings gaps post-displacement.

In addition to documenting the presence and nature of discriminatory responses, we assess their economic consequences. Displaced non-Western immigrants experience substantial losses in earnings and employment relative to equally productive native peers, with these gaps persisting for multiple years. These dynamics also carry implications for firms. Using estimated worker fixed effects, we find that firms that avoided hiring displaced non-Western immigrants subsequently hire lower-productivity native workers. In contrast, firms that hire displaced immigrants tend to attract higher-productivity 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.

This paper delivers novel empirical evidence of how labor market power dynamics drive inequality and opportunity through employer discrimination. It also identifies the sources of employer power that fuel such discrimination and explores whether it stems from preferences or beliefs about disfavored groups. By integrating key trends in the modern labor market—declining worker power, rising firm dominance, increasing labor migration, and persistent wage gaps between natives and immigrants—this work makes a major contribution to advancing the literature.

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)).⁷ There is also rich

⁷Notably, 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

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)).⁸

We build on this literature by shifting the focus from whether discrimination exists to the conditions under which it 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, no empirical studies directly isolate the effect of labor market power—distinct from product market power—on discriminatory outcomes. Moreover, although belief-based and preference-based discrimination are conceptually well understood, little is known about how these mechanisms evolve over time in real labor markets. Our study addresses these gaps by examining how discrimination responds to variation in employer power and by tracing dynamic search outcomes across groups. In doing so, we offer a new perspective 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. While their analysis captures average gaps in wage offers, our contribution is to uncover when, where, and why such discrimination emerges. By leveraging variation in local labor market power, we provide the first causal evidence that discrimination is amplified by employer wage-setting power, or that, conversely, labor market competition reduces discrimination. We further distinguish labor from product market power, demonstrating that only the former enables discriminatory wage-setting. Finally, we go beyond static estimates by analyzing dynamic recovery patterns, offering novel evidence that belief-based discrimination diminishes with repeated employer–employee interactions, consistent with employer learning models. Taken together, our findings reveal the structural conditions under which discrimination persists—and when it is constrained.

may be one explanation for their findings.

⁸Related 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.

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. However, none of these studies have employed recent monopsony frameworks to examine a crucial prediction: that increased labor market competition should reduce and eventually eliminate discrimination. We expand on this literature by isolating the causal effects of labor market power on discrimination against minority workers, thereby advancing our comprehension of how market power and employer institutions influence inequality.

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 unique setting for this analysis, with rich data and a robust identification design. In addition, discrimination in Norway is traditionally considered relatively low, as Norwegians' strong egalitarian values make them one of the least accepting of inequality in the world in experimental settings (Almås et al., 2023). This suggests that the effects we identify likely represent conservative estimates relative to other settings. This section outlines the structure of local labor markets and immigration patterns, providing essential 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.⁹ 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 address any concerns about the 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.

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).

⁹Relative to other countries, the OECD ranks the level of employment protection in Norway similar to Italy (Salvanes et al., 2024).

2.2 Immigration in Norway

Norway has seen a significant rise in immigration over the past several decades, with immigrants now comprising more than 15 percent of the total 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 non-Western immigrants 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 to motivate the empirical analysis. The aim is not to introduce new theoretical insights, but to organize key ideas from canonical models of discrimination and market power in a way that clarifies the role of competition in

shaping discriminatory behavior. We use this framework to interpret our findings and to guide the empirical design.

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

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

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 will 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 dis-

crimination. 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 misperception; 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 imply stable 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, product market power as 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 critical. 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 Herfindahl-Hirschman indices (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 informative about 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 conclusive evidence of statistical discrimination, it is inconsistent with models of persistent taste-based or monopsonistic discrimination. Combined with the structure of the labor market and the design of our empirical comparisons, the dynamics of recovery provide insight into the forces driving immigrant-native disparities following displacement. In support of interpreting convergence as a sign of belief-based discrimination and correcting beliefs over time, we also examine other dynamics of hiring and earnings in supplementary analyses, including multiple characteristics of the managers and firms that do hire immigrants in concentrated markets relative to less concentrated 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 and were forced to search for new employment due to mass layoffs or firm closures. These events are exogenous to individual characteristics and generate a natural experiment in which workers with similar productivity are exposed to different labor market conditions. By comparing post-displacement outcomes across markets with varying degrees of labor and product market concentration, we test how employer market power shapes differential treatment of immigrants. We also examine the dynamics of recovery to assess whether observed disparities reflect persistent preferences or evolving beliefs. This approach provides a direct empirical analogue to the conceptual framework above and allows us to identify how competition mediates the extent and nature of discriminatory behavior.

In the absence of employer discrimination, equally qualified workers—separated from the same firm and earning the same pre-displacement wage—should receive comparable job offers and wages in subsequent employment. If gaps emerge across demographic groups following exogenous displacement, this rejects the null hypothesis of no discrimination (e.g., Fryer et al. (2013)). Moreover, if pre-displacement wages already reflect discriminatory treatment, our estimates reflect a lower bound on the extent of discrimination (e.g., Bohren et al. (2022)).

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.

To measure labor market power, we rely on a Herfindahl-Hirschman Index for each 3-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.¹⁰

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

¹⁰A 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 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.

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-7), and our main results hold closely when using markdowns in place of HHI (Appendix Table A-14).

4.2 Sample Construction and Empirical Method

Our empirical approach simulates an experiment comparing two immigrant-native worker pairs in the same occupation with identical productivity and labor market signals but different immigration backgrounds, both searching for jobs simultaneously. One pair of workers is placed in a monopsonistic labor market where firms have wage-setting power and earn supernormal profits via wage markdowns. The other pair of workers is placed in a competitive market where firms are wage-takers and make zero economic rents from workers because wages are driven up to workers’ marginal revenue product. We track their outcomes after they are exogenously displaced from their job (either due to a mass layoff or a firm closure) from the same firm and occupation, within the same labor market, at the same time, while previously earning the same amount. This ensures that their productivity, hours, and skills were recognized by the market as being the same before they began 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 off more than 30 percent of their workforce (e.g., Salvanes et al. (2024)).¹¹

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 reg-

¹¹To 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.

istry 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.

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.¹² 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 meet 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 that displaced workers are mechanically observationally equivalent.

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. Because we compare these workers within the same market and

¹²We also use a five-year tenure requirement and find identical effects. See Appendix Table A-9.

job context, 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.¹³

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.¹⁴ 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 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 finally by 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:

$$y_{ilsoty} = \alpha + \beta_1 \text{Earn}_{i,(t=0)} + \gamma_1 (P \times I)_{ity} + \gamma_2 (P \times H)_{itylso} + \gamma_3 (P \times I \times H)_{itylso} + \mu_1 I_i + \mu_2 (I_i \times H) + \tau_t + \phi_y + \psi_m + \varepsilon_{ilsoty}. \quad (1)$$

Here, y_{ilsoty} is the log of annual earnings. P is an indicator for the post-displacement pe-

¹³If the wage signals at their base firm also incorporate information on any differences in outside options or ex ante monopsonistic discrimination against non-Western immigrants, then it may be that non-Western immigrants are actually *more* productive than their coworkers at baseline, leading us to estimate a lower bound discrimination effect.

¹⁴The 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.

riod, 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 ($\mu_{NW I_i}$) 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. Building on the same sample of displaced workers and matching approach discussed above, we next estimate an even more saturated version of the specification that includes individual fixed effects (η_i). This version absorbs all baseline variation in individual characteristics, including earnings, firm, occupation, industry, education, birth cohort, nativity, and local labor market. It also accounts for any time-invariant unobserved heterogeneity. By conditioning on a rich set of pre-displacement characteristics and absorbing individual-level fixed differences, this approach isolates how employer behavior varies with market power in response to displacement-induced job search.

Augmented Matching Approach with Earnings-Specific Trends. Finally, we extend the model by 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.¹⁵ This flexibly controls for and rules 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.¹⁶ It also addresses issues related to mean reversion. Our full specification is:

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

This preferred model compares workers who were 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 and conservative 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:

¹⁵An alternative to this interaction would be to interact deciles of the pre-displacement earnings distribution with the relative time indicators. This approach yields nearly identical estimates (see Table A-1).

¹⁶We further augment our preferred specification with additional interactions between base occupation and industry with relative time indicators in Table A-2. 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.

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

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 an even more saturated version of our preferred event study specification that ensures individuals are observed in highly similar environments even *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 discrimination *in the same firm* even after controlling for these other margins 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:

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

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 highly saturated matched event study approach 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: whether non-Western immigrants experience larger earnings losses than comparable natives after displacement in more concentrated labor markets. The baseline matched triple-differences model compares displaced workers across immigration status and market concentration, before and after separation, ensuring that immigrants and natives were observationally equivalent at the time of displacement. The augmented matching model adds individual fixed effects, absorbing all time-invariant characteristics, including base firm, occupation, industry, location, education, birth cohort, and any other fixed characteristics relevant for wages and job search. Our preferred specification further allows for baseline earnings-specific trends, flexibly capturing heterogeneity in post-displacement trajectories by initial wage level. Finally, the post-displacement matching specifications condition not only on detailed pre-displacement characteristics but also on re-employment in the same post-displacement firm and occupation-local labor market cell. Taken together, these models provide a transparent and increasingly conservative test of how employer market power shapes post-displacement outcomes across worker groups.

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

Importantly, recent advances in the econometrics literature from Olden and Møen (2022) have shown that in the case of a triple-differences estimator, the parallel trends assumption need not hold for both lower-level layers of differences (e.g., high- versus low-concentration markets and native versus immigrant workers, considered separately). In other words, identification does not rest on two parallel trends assumptions, but one. Stability in the higher-order interaction ($P \times I \times H$) suffices because the triple differences estimator is the difference between two difference-in-differences estimators, and this differencing procedure accounts for any bias from lower-level interactions as long as the bias is the same for both.

Although the common trends assumption is fundamentally untestable (since the counterfactual outcomes for treated individuals are never observed), we provide suggestive evidence consistent with its validity. Specifically, we estimate event study models and verify that the triple-interaction coefficients for periods prior to treatment are statistically indistinguishable 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 distinguish between preference-based and belief-based discrimination, we explicitly show no evidence of violations in parallel trends prior to treatment.

The second assumption underlying our estimation approach is that, conditional on our matching strategy—requiring displaced native and immigrant workers to come from the same firm–occupation–industry–local labor market cell, including individual fixed effects, and controlling for potential earnings-specific trends—any remaining differences between these groups do not systematically vary across labor markets with differing concentration levels. Importantly, we do not require the displacement process itself to be unbiased between natives and immigrants overall; rather, our causal interpretation remains valid as long as residual differences in unobserved characteristics (such as skills, productivity, or preferences) after this exact matching do not correlate systematically with market concentration.

We note explicitly that this assumption is considerably more lenient than the standard selection assumption commonly employed in the literature, which typically requires mass layoffs and related job loss events to be fully orthogonal to worker characteristics (e.g., Jacobson et al. (1993); Stevens (1997); Davis and von Wachter (2011); Couch and Placzek (2010)), including in studies specifically from the Norwegian context (Salvanes et al. (2024); Huttunen et al. (2011b; 2018); Carneiro et al. (2023)). Although our more relaxed assumption is not directly testable, we provide additional empirical support by examining pre-displacement outcome trends. Specifically, we estimate Equation (3) separately for natives and immigrants to test whether their relative outcome trajectories differed systematically across markets prior to displacement. In Section 5.3, we confirm the absence of such differential pre-trends, lending further empirical credibility to 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. Importantly, 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-11 and Figure A-5). This reinforces the claim that our findings are not driven by endogenous displacement but reflect how employer power moderates reemployment outcomes among exogenously displaced workers.

The third assumption that must hold for us to interpret the results as causal 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). Specifically, general shocks, such as macroeconomic changes or policy interventions, may affect labor markets broadly, but there can be no nativity-specific shocks correlated with exogenous job loss events that systematically vary as a function of the competitiveness of the labor market and that also impact the outcomes of interest. If such shocks were to exist, they could introduce spurious variation unrelated to the treatment. To mitigate this risk, we include fixed effects for time and match workers on location, industry, firm, and occupation, which absorbs many potential sources of time-varying confounding. By focusing on relative differences across groups examining the robustness to using only firms closures to identify variation in search, we further reduce the likelihood that such shocks bias our estimates.¹⁷

¹⁷We find that controlling for relative time by occupation, relative time by industry, and relative time by occupation-industry results in identical estimates (Table A-2), so any occupation-specific or industry-specific

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 employment 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. Additionally, we fix labor market concentration measures to the pre-displacement year to mechanically rule out the possibility of general equilibrium effects to bias our estimates. Furthermore, when we directly control for the total incidence of separated workers in the base year in each occupation-LLM cell to characterize the intensity of all local separation events that year that could affect the worker, our estimates are identical (see Appendix Table A-12).

Overall, our various diagnostic and robustness tests provide strong evidence that the above assumptions hold and that the coefficients of interest in our model can be interpreted as the causal effect of labor market concentration and employer power on employer discrimination, which generates labor market gaps between equally productive workers. We rule out other explanations or mechanisms in Section 6.

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 also consider product market power, running horse races to uncover the relative importance of the two power sources in driving employer discrimination. Finally, we identify the relative importance of belief-based and preference-based discrimination, drawing on insights from Bohren et al. (2019); Fryer Jr (2007); Fryer et al. (2013) and analyzing the dynamic adjustment path over time following search.

5.1 Labor Market Power and Employer Discrimination

Difference-in-Differences Results. Table 2 shows the effect of job search induced by involuntary displacement 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

shocks are ruled out as explanations.

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 purely monopsonistic 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 equally productive 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, non-Western immigrants 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, revealing considerable native-immigrant labor market gaps in employment effects as a function of labor market concentration. For example, the negative employment effect is over 160 percent greater for non-Western immigrants 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 as a result of their search effort after having worked in the same job and firm with the same earnings before the search 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 mar-

ket 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.

While unlikely, our baseline results could theoretically reflect unobservable productivity differences between non-Western immigrants and natives within occupation-industry-firm-earnings cells, potentially masking factors unrelated to employer discrimination that also are correlated with post-search labor market outcomes. The individual fixed effect controls away any joint determinants of the outcome at baseline—observed or unobserved. The results in Table 2 show that this more saturated model produces identical results to our baseline model. Furthermore, our preferred estimates also control for any differential expected trends that vary across baseline earnings such as mean reversion or different recover paths over baseline earnings. This indicates that even after accounting for the full set of individual characteristics and prior employment histories, the differential impact of firm labor market power on non-Western immigrants relative to natives/Western immigrants remains robust. In other words, the persistent labor market gaps observed are not merely artifacts of unobservable productivity differences but reflect the systematic impact of employer discrimination and labor market concentration. That the annual earnings gaps are significantly larger than the employment and hours gaps suggests that even when employed, non-Western immigrants are paid significantly less than their native/Western immigrant counterparts, indicating that labor market power enables discrimination on both employment and wages as predicted in our conceptual framework above.

Marginal Effects. To better visualize the 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%. Note also that just as in Fryer et al. (2013), if pre-search earnings reflect both marginal productivity and differential treatment by demographic group, then interactions during and after the search period controlling for prior earnings will reflect a *lower* bound on the level of discrimination in the market.

The results displayed in Figure 1 are consistent with the notion of employer discrimination being a function of labor market power, where wage-setting power and supernormal profits bestow upon 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, labor market competition substantially reduces discrimination in the labor market, though it does not completely eliminate it.

5.2 The Role of Product Market Power

We expand our empirical framework to include product market power, an essential addition 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. Product market power by itself has no economically meaningful or statistically significant effect on the employer discrimination practices exercised by the firm: none 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. Us-

ing 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 our conclusion that product market power does not explain the observed discrimination patterns (Appendix Table A-3).

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 provides important insights into existing debates on product market power and discrimination that have previously been overlooked.

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 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 identified its prevalence as a function of 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 identify the relative importance of belief-based and 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 correct erroneous productivity beliefs and eliminate earnings and employment gaps—even in the presence of labor market power. 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 flexi-

ble than productivity beliefs in the face of new information.¹⁸ 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. 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 supports the idea of employer discrimination rooted in incorrect beliefs, suggesting that continuous exposure to workers can rectify 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 supports the common trends assumption required for a causal interpretation of our findings.¹⁹

The gradual convergence in immigrant-native outcomes following displacement is consistent with employer learning models, such as Altonji and Pierret (2001), in which firms initially rely on group-based priors but update their beliefs over time through direct performance observation. This dynamic fits our interpretation of discrimination as rooted in inaccurate beliefs, rather than persistent 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 (current 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. This result indicates that the closing of earnings gaps primarily 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 do not 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

¹⁸Current 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)).

¹⁹The 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.

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. 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.²⁰

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 equally 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

²⁰Our secondary analyses rule out manager homophily (Appendix Table A-4), the thickness of social networks or social proximity to managers (Appendix Tables A-6 and A-7), and post-layoff occupational sorting (Appendix Table A-5) as explanations or mechanisms for the employment gaps we find.

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-8 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 equally 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.²¹

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-8.

We find 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-9). 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 less efficient workers when they either rely on imprecise inferences about productivity, perhaps based on erroneous beliefs, or hold prejudices that prevent them from hiring members of the outgroup. Such practices can result in 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).

²¹The 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.

6 Robustness and Alternative Mechanisms

The matched triple differences specification, along with the more saturated augmented regression, offers substantial power to rule out alternative explanations and identification threats. For an alternative factor to explain our results, that factor would not only need to differ across natives and immigrants, but it would also need to systematically vary across the labor market concentration gradient. For instance, one might worry that differences in social networks or job search behaviors between natives and immigrants could explain our findings. While we rule out these mechanisms—along with homophily, occupational sorting, and the contact hypothesis—in this section, we note here that any such alternative explanation would require natives and immigrants to differ on these dimensions in a way that is specifically correlated with the market concentration they both encounter. For instance, if we are concerned that search differences between immigrants and natives drive our findings, then those differences (less search or worse search of immigrants) would have to be present in concentrated markets but not in competitive markets. Furthermore, those differences cannot be correlated with baseline features such as geographic local labor market, occupation or industry, or baseline earnings at the time the search commenced.

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 or language barriers—leading to slower transitions up the job ladder (Helm et al., 2023)? Four 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 systematic sorting into firms with lower firm fixed effects in more concentrated markets. If differential search effort among natives and non-Western immigrants 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 non-Western immigrants 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. 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.

Third, prior research highlights financial assets and consumption smoothing as critical factors influencing job search, with greater savings contributing to lower search effort (Lentz and Traaen, 2005; Lammers, 2014). Despite comparable productivity and earnings before displacement, non-Western immigrants hold fewer financial assets than natives or Western immigrants (Appendix Table A-10). This financial disparity suggests that non-Western immigrants 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.

Finally, if differential search efficiency is a driver of 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 transitions as they continue on-the-job search to attempt to leverage wage gains from the marginal search. 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-6).²²

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 after displacement? Evidence suggests this is unlikely. Specifically, Appendix Table A-5 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 non-Western immigrants 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.

Furthermore, when we control for current occupation and local labor market conditions in

²²An additional piece of evidence from the legal framework in Norway, receipt of unemployment benefits requires workers to register with NAV, attend meetings, provide evidence of job search activity, and accept suitable employment identified by the public employment office. These requirements ensure a minimum level of search effort among both natives and immigrants.

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 non-Western immigrants 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 strongly suggests that referral networks play a limited role 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-4). 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 non-Western immigrants 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-6) and high or low shares of non-Western immigrants (Appendix Table A-7), 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-8. 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.²³ 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-11. 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

²³This result in our setting contrasts with Agan and Starr (2020) but aligns with the results on employer neighborhood characteristics in Kline et al. (2022).

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.

Contact Hypothesis. So far, the alternative explanations discussed in this section have primarily explored various potential mechanisms underlying the relationship between competition and discrimination. Taken together, 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 non-Western immigrants 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 non-Western immigrants (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. How-

ever, 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-12.

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-period 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 non-Western immigrants 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-13. We find no statistically significant differences between non-Western immigrants and other workers in concentrated markets that would suggest program participation is responsible for our results.

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, non-Western immigrants 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 non-Western immigrants 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.²⁴ The results in Table A-14 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 entirely absent, leaving a critical gap in our understanding of how market power shapes discrimination.

We demonstrate that employer discrimination in the Norwegian labor market is significant and that increased competition can substantially reduce it, aligning with canonical the-

²⁴The 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.

oretical predictions. Additionally, we show that the source of employer power is critical. Specifically, we find that discrimination is driven by firms with substantial labor market power, and that 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 reveals that the observed discrimination is largely driven by inaccurate beliefs about immigrants' productivity, with preferences likely playing a much smaller role. Although discrimination gaps eventually close, we find strong evidence that the existence of discrimination in hiring and pay imposes significant 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. Understanding 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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Table 1: Sample Summary Statistics

	Non-Displaced Sample		Displaced Sample	
	(1)	(2)	(3)	(4)
	Mean	SD	Mean	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) 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	-0.237*** (0.044)	0.055** (0.022)	-0.484*** (0.088)	-0.237*** (0.047)	0.056** (0.023)	-0.484*** (0.093)	-0.195*** (0.048)	0.043* (0.024)	-0.405*** (0.097)
Post x NWI	-0.056*** (0.019)	0.005 (0.004)	-0.085*** (0.022)	-0.056*** (0.020)	0.005 (0.004)	-0.086*** (0.024)	-0.042** (0.019)	0.000 (0.004)	-0.056** (0.023)
Post x NWI x HHI	-0.383*** (0.133)	0.056 (0.056)	-0.582*** (0.207)	-0.383*** (0.142)	0.056 (0.060)	-0.587*** (0.225)	-0.415*** (0.142)	0.066 (0.059)	-0.656*** (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 Earn- ings
Post x NWI	-0.047*** (0.018)	0.010** (0.005)	-0.069*** (0.026)
Post x Revenue HHI	0.046** (0.023)	-0.018*** (0.003)	0.054*** (0.017)
Post x Occupation HHI	-0.185*** (0.056)	0.038 (0.027)	-0.264*** (0.079)
Post x Revenue HHI x NWI	0.020 (0.055)	-0.030*** (0.007)	0.050 (0.054)
Post x Occupation HHI x NWI	-0.491*** (0.184)	-0.005 (0.066)	-0.598* (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.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "NWII" refers to Non-Western Immigrants.

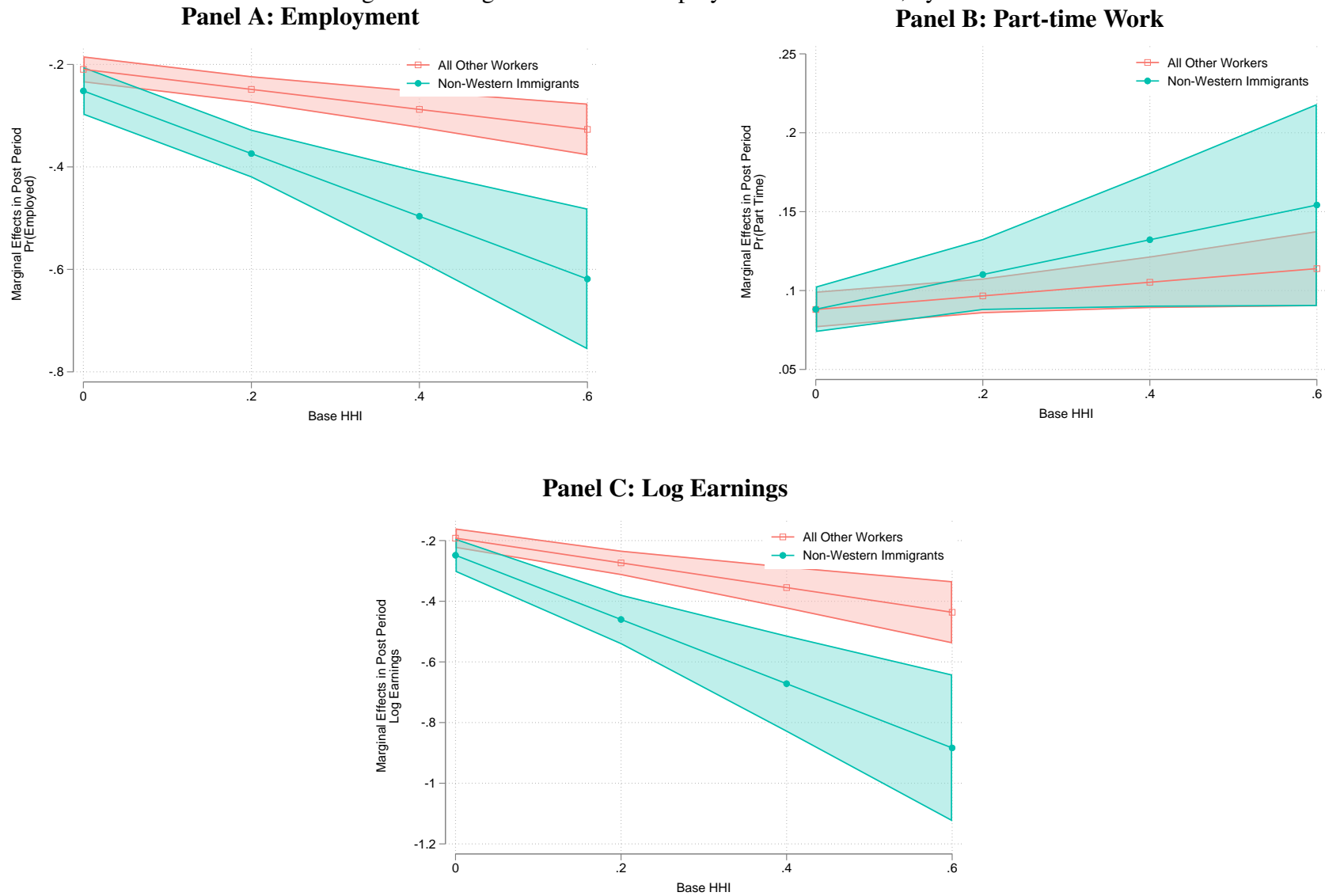
Table 4: Additional Evidence from Other Subgroups: Preferred Model

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

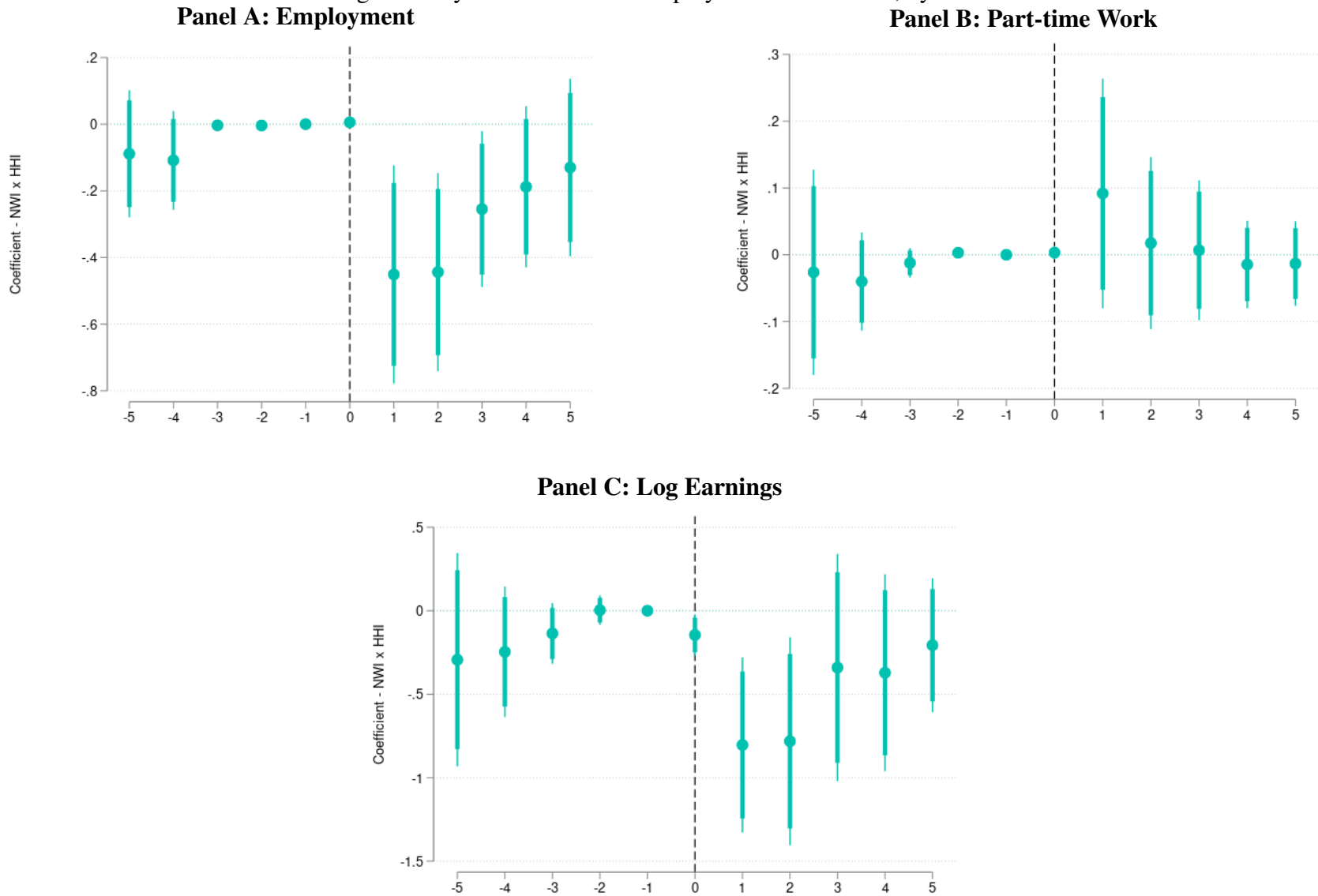
Figure 1: Marginal Effects of Employer Discrimination, by HHI



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. For a binarized event study that splits the interaction at $HHI > 0.25$, and yields similar results, see Figure A-4.

Online Appendix

Table A-1: Main Estimates with Baseline Earnings Decile by Relative Time Interactions

	(1) Employment	(2) Pr(Part Time)	(3) Log Earn- ings
Post x HHI	-0.155*** (0.045)	0.029 (0.021)	-0.313*** (0.090)
Post x NWI	-0.020 (0.020)	-0.008* (0.004)	-0.009 (0.025)
Post x NWI x HHI	-0.408*** (0.142)	0.060 (0.059)	-0.645*** (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-2: Difference-in-Differences Estimates with Base Occupation and Base Industry x Relative Interactions

	Base Industry x Relative Time		Base Occupation x Rel. Time			Base Occupation x Rel. Time Base Industry x Rel. Time			
	(1) Employment	(2) Pr(Part Time)	(3) Log Earn- ings	(4) Employment	(5) Pr(Part Time)	(6) Log Earn- ings	(7) Employment	(8) Pr(Part Time)	(9) Log Earn- ings
Post x HHI	-0.117** (0.046)	0.038* (0.023)	-0.301*** (0.090)	-0.030 (0.051)	0.040 (0.027)	-0.199** (0.098)	-0.012 (0.052)	0.041 (0.026)	-0.163* (0.096)
Post x NWI	-0.039** (0.017)	-0.005 (0.004)	-0.045** (0.021)	-0.030 (0.019)	-0.005 (0.004)	-0.019 (0.023)	-0.029 (0.018)	-0.007* (0.004)	-0.016 (0.022)
Post x NWI x HHI	-0.386*** (0.132)	0.070 (0.059)	-0.642*** (0.216)	-0.369*** (0.134)	0.054 (0.060)	-0.609*** (0.216)	-0.368*** (0.130)	0.055 (0.060)	-0.619*** (0.213)
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. 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-3: Main Estimates with Baseline Earnings Decile by Relative Time Interactions

	(1) Employment	(2) Pr(Part Time)	(3) Log Earn- ings
Post x NWI	-0.037*** (0.014)	0.005 (0.005)	-0.059** (0.023)
Post x Local Revenue HHI	0.001 (0.027)	-0.007 (0.007)	-0.012 (0.029)
Post x Occupation HHI	-0.195*** (0.060)	0.049** (0.022)	-0.385*** (0.106)
Post x Local Revenue HHI x NWI	-0.027 (0.085)	-0.021 (0.015)	0.007 (0.090)
Post x Occupation HHI x NWI	-0.393** (0.174)	0.081 (0.054)	-0.661*** (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-4: Manager Characteristics

	(1)	(2)	(3)	(4)
	Manager Share Fe- male	Manager Share Norwegian Female	Manager Share Norwegian	Manager Share NWI
Post x HHI	-0.020 (0.094)	-0.054 (0.074)	-0.301** (0.139)	0.059 (0.053)
Post x NWI	-0.000 (0.022)	0.005 (0.021)	-0.008 (0.014)	0.011 (0.010)
Post x NWI x HHI	0.055 (0.158)	0.046 (0.150)	0.134 (0.167)	0.001 (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-5: Difference-in-Differences Estimates, Occupational Sorting

	(1)	(2)
	Occupation Percentile Rank	AKM Occupation FE
Post x HHI	-2.550 (3.828)	-0.030 (0.025)
Post x NWI	0.428 (0.478)	-0.000 (0.004)
Post x NWI x HHI	-1.993 (5.164)	-0.032 (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-6: Neighborhood Characteristics - Manager Shares of Neighbors

	Employment		Log Earnings	
	Low Share in Management (1)	High Share in Management (2)	Low Share in Management (3)	High Share in Management (4)
Post x HHI	-0.167*** (0.052)	-0.215*** (0.061)	-0.361*** (0.112)	-0.382*** (0.116)
Post x NWI	-0.042* (0.022)	-0.031* (0.016)	-0.060** (0.028)	-0.029 (0.025)
Post x NWI x HHI	-0.401*** (0.151)	-0.475** (0.221)	-0.653*** (0.214)	-0.586 (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-7: Neighborhood Characteristics - NWI Shares of Neighbors

	Employment		Log Earnings	
	Low Share of NWI in Nhood (1)	High Share of NWI in Nhood (2)	Low Share of NWI in Nhood (3)	High Share of NWI in Nhood (4)
Post x HHI	-0.171*** (0.047)	-0.215*** (0.068)	-0.336*** (0.097)	-0.417*** (0.124)
Post x NWI	-0.022 (0.013)	-0.049** (0.022)	-0.024 (0.032)	-0.078*** (0.026)
Post x NWI x HHI	-0.446*** (0.135)	-0.418** (0.206)	-0.662*** (0.241)	-0.705** (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-8: Difference-in-Differences Estimates, Destination Firm Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Share NWI Workers	Share Western Immigrant Workers	Average Worker Age	Log(Firm Size)	Union Den- sity	Pr(Coworker from Neighbor- hood)
Post x HHI	0.016 (0.017)	0.021 (0.059)	-1.183 (0.741)	-0.616* (0.350)	-0.079* (0.044)	-0.306*** (0.075)
Post x NWI	0.008 (0.005)	0.032*** (0.007)	0.188 (0.152)	0.026 (0.058)	0.018*** (0.007)	0.011 (0.010)
Post x NWI x HHI	0.032 (0.059)	0.009 (0.066)	-0.465 (1.507)	0.781 (0.511)	-0.034 (0.079)	-0.233 (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-9: Difference-in-Differences Estimates, Five-Year Tenure Requirement

	Base Model			Individual Fixed Effects			Individual Fixed Effects + Base Earnings 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	-0.249*** (0.047)	0.048* (0.027)	-0.487*** (0.099)	-0.249*** (0.047)	0.048* (0.027)	-0.487*** (0.099)	-0.204*** (0.049)	0.034 (0.027)	-0.395*** (0.103)
Post x NWI	-0.071*** (0.023)	0.009* (0.005)	-0.111*** (0.028)	-0.071*** (0.023)	0.009* (0.005)	-0.112*** (0.029)	-0.058** (0.023)	0.005 (0.005)	-0.084*** (0.027)
Post x NWI x HHI	-0.238* (0.139)	0.096 (0.067)	-0.388 (0.262)	-0.238* (0.139)	0.096 (0.067)	-0.398 (0.271)	-0.274** (0.138)	0.107 (0.066)	-0.475* (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-10: 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-11: Difference-in-Differences Estimates, Firm Closures Only

	Employment (1)	Pr(Part Time) (2)	Log(Earnings) (3)
Post x HHI	-0.144** (0.071)	0.075** (0.036)	-0.440*** (0.141)
Post x NWI	-0.026* (0.015)	-0.001 (0.007)	-0.012 (0.035)
Post x NWI x HHI	-0.811** (0.357)	0.028 (0.152)	-1.535* (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-12: Main Estimates, Controlling for Size of Occupation Layoffs/Closures in Base Year

	(1)	(2)	(3)
	Employment	Pr(Part Time)	Log Earnings
Post x HHI	-0.207*** (0.049)	0.045** (0.022)	-0.337*** (0.095)
Post x NWI	-0.042** (0.019)	0.001 (0.004)	-0.057** (0.023)
Post x NWI x HHI	-0.416*** (0.142)	0.066 (0.059)	-0.651*** (0.224)
Shock Size	-0.015 (0.043)	0.074 (0.076)	0.077 (0.155)
Shock Size x Post	0.014 (0.031)	-0.003 (0.010)	-0.068 (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-13: Difference-in-Differences Estimates, Social Assistance Program Take-up

	Pr(Sick Leave) (1)	Pr(Unemployment Benefits) (2)	Pr(Disability Benefits) (3)
Post x HHI	0.035 (0.023)	0.301** (0.118)	0.018** (0.008)
Post x NWI	0.060*** (0.008)	0.043* (0.024)	-0.005*** (0.001)
Post x NWI x HHI	-0.036 (0.103)	0.268 (0.166)	-0.004 (0.020)
N	673,467	673,488	673,488

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.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-14: Difference-in-Differences Estimates, Occupation-LLM Average Markdown

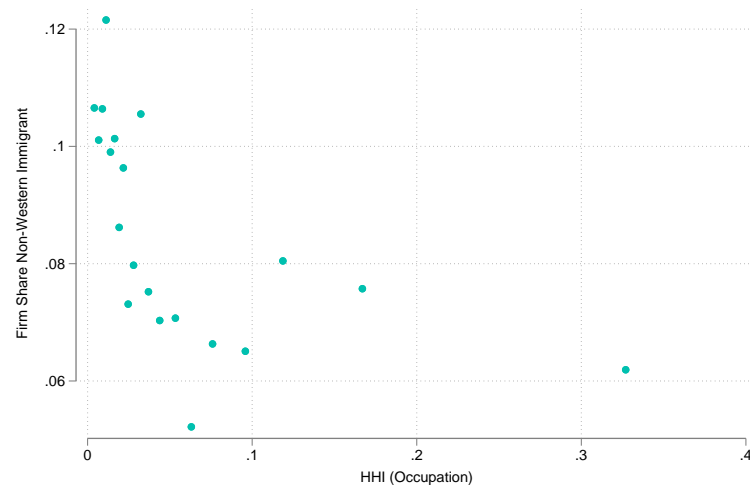
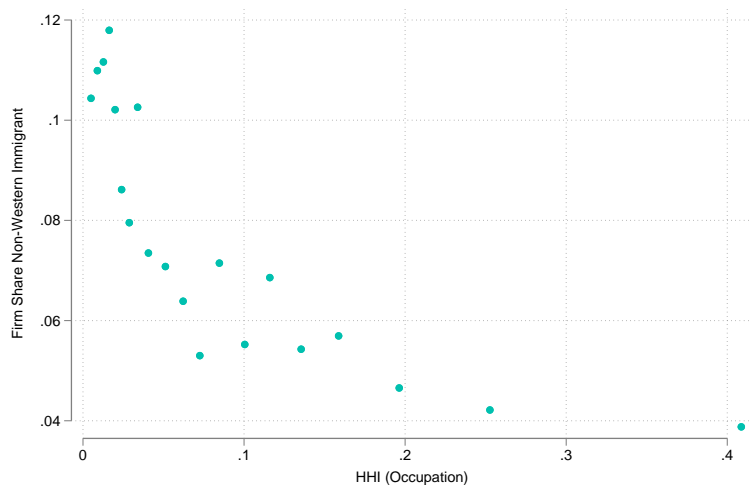
	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
Post x Avg Occ-LLM Markdown	-0.044*** (0.004)	0.006*** (0.002)	-0.059*** (0.009)
Post x NWI	0.058* (0.031)	-0.008 (0.013)	0.145** (0.060)
Post x NWI x Avg Occ-LLM Markdown	-0.034*** (0.011)	0.001 (0.005)	-0.061*** (0.022)
N	213,520	213,520	211,576

Source: Authors' calculations using Norwegian register data.

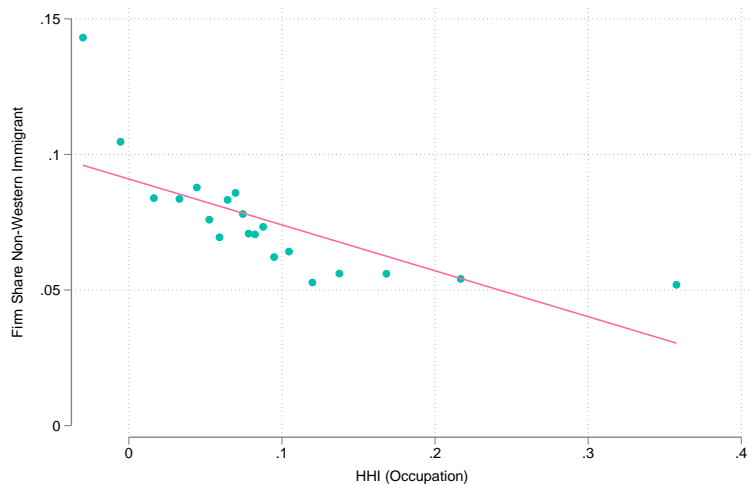
Notes: Estimates correspond to Equation 2 in the text and measure the difference-in-differences estimates over the first three years after the displacement event replacing occupation-LLM average wage markdowns. Calculations are for blue collar occupations in order to overlap with the sectors for which 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.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

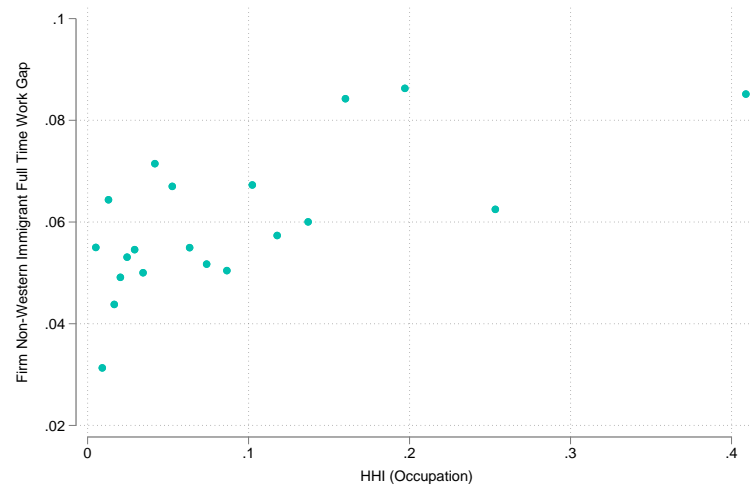
Figure A-1: Non-Western Immigrant Employment Gaps in Firm, by HHI
Panel A: Share NWI, All Sectors **Panel B: Share NWI, Private Sector**



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



Panel D: NWI Full-Time Status Gap within Firm

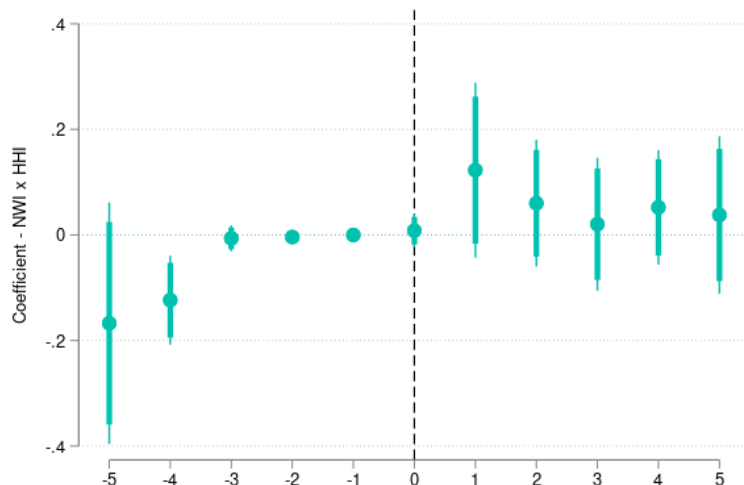


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

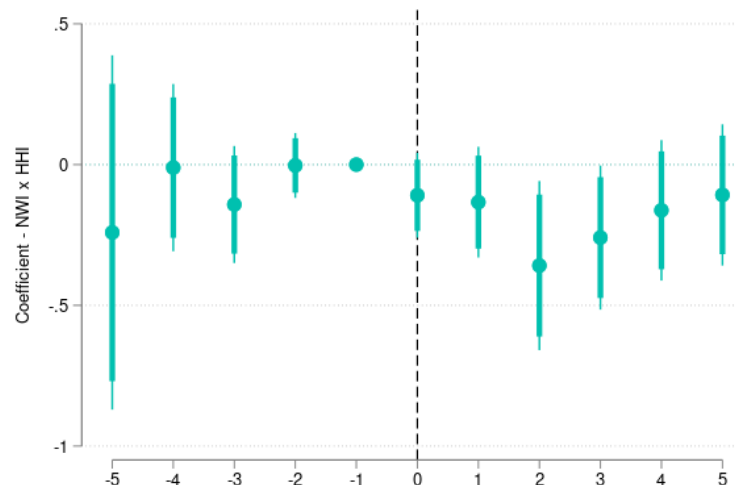
Notes: "NWI" refers to Non-Western Immigrants. In Panel D, we calculate the share of workers from each group within each firm that are full-time and plot the differences between the full-time share of natives and Western immigrants compared to non-Western immigrants.

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

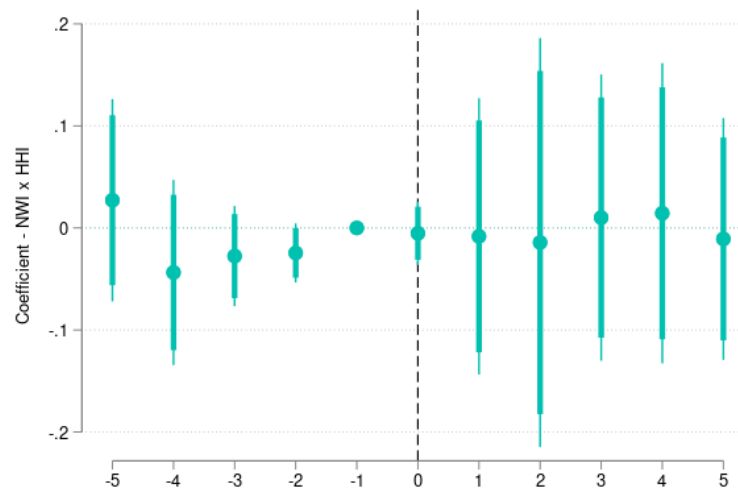
Panel A: Part Time Work



Panel B: Log Earnings



Panel C: Firm Fixed Effect of Destination Firm

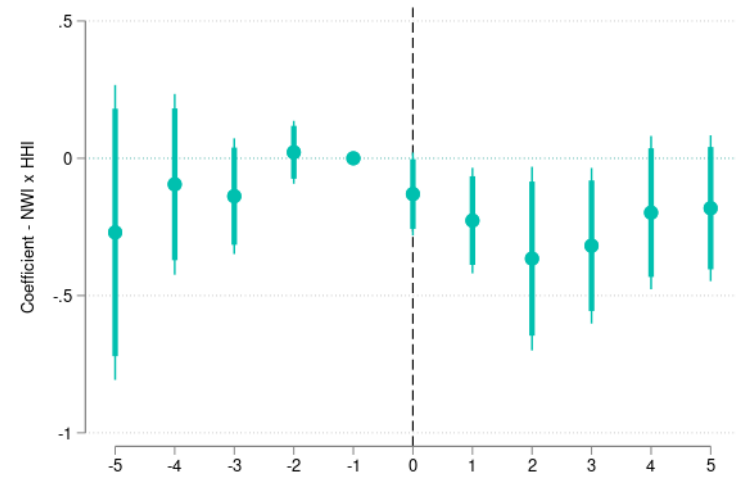
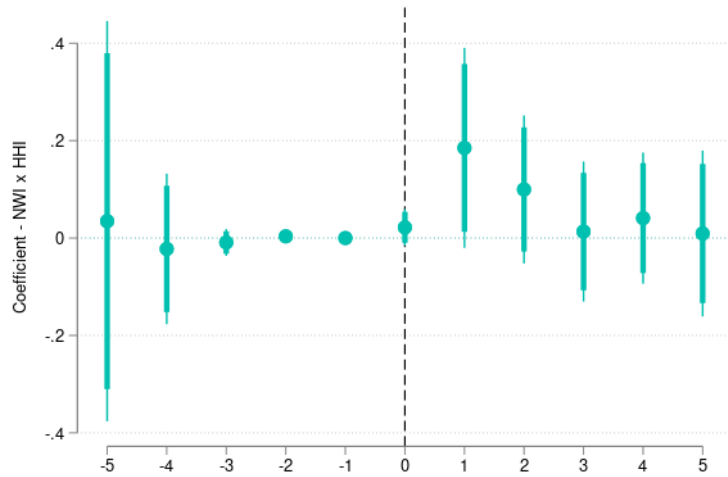


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

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.

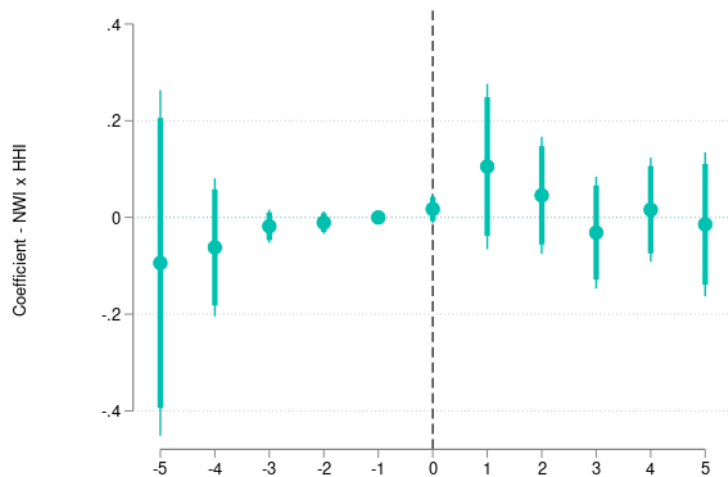
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

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**

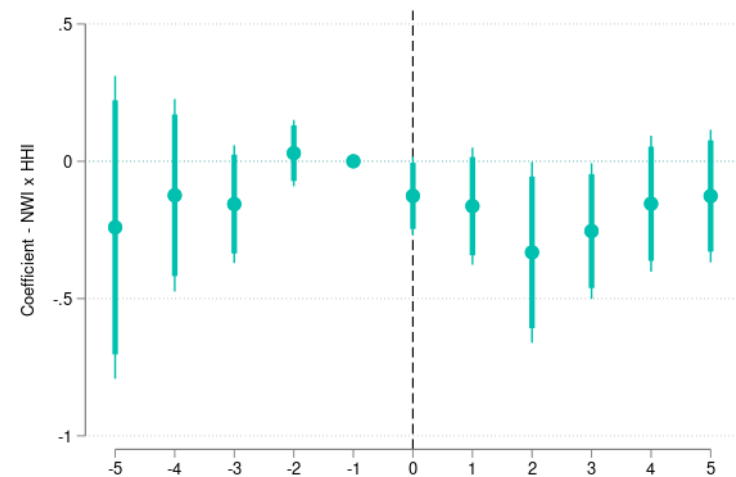


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Panel C: Current Occupation by LLM FE, Pr(Part Time)



Panel D: Current Occupation by LLM FE, Log Earnings

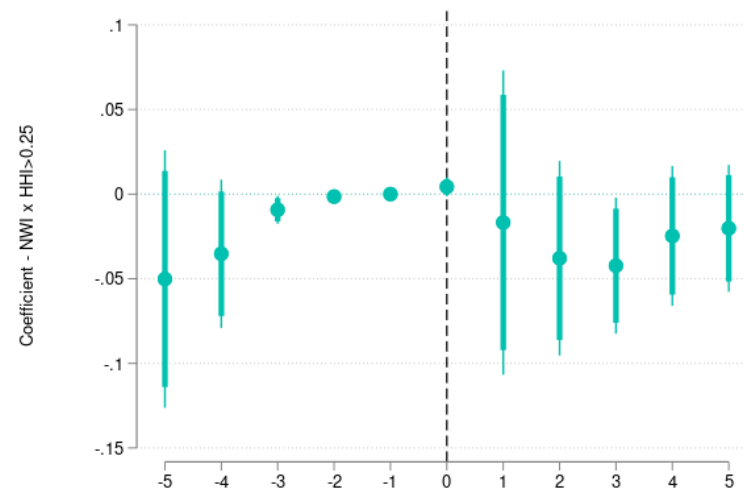
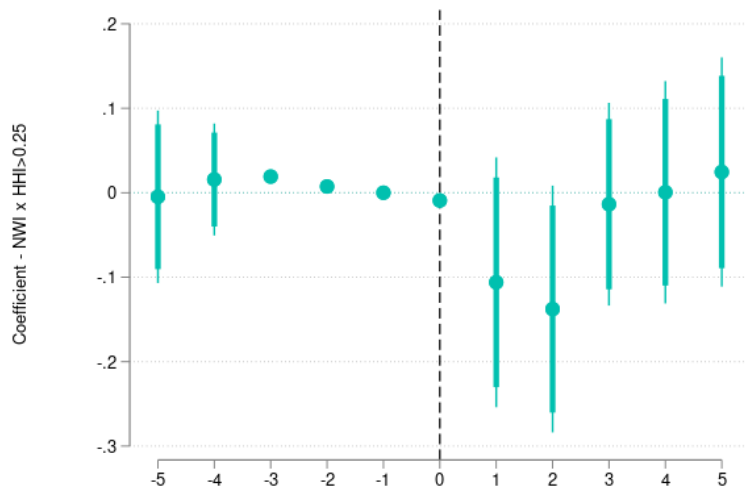


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

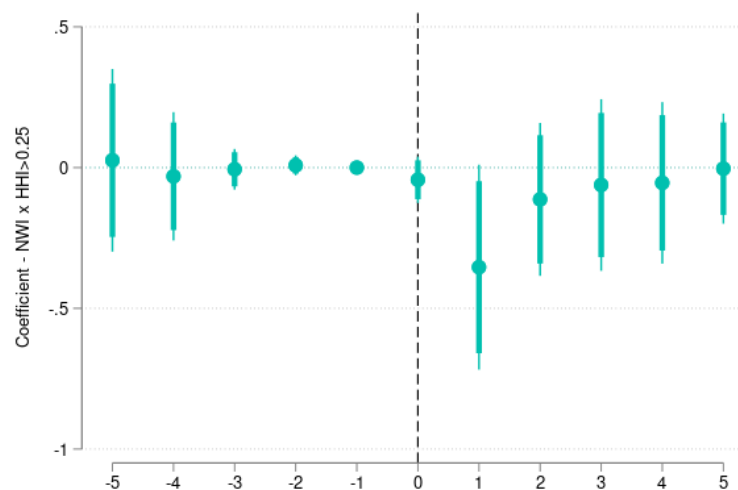
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 4 in the text, where ω_{pt} is a fixed effect for occupation (Panels A and B) or occupation by local labor market (Panels C and D)..

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 $HHI > 0.25$
Panel A: Employment **Panel B: Part-time Work**



Panel C: Log Earnings

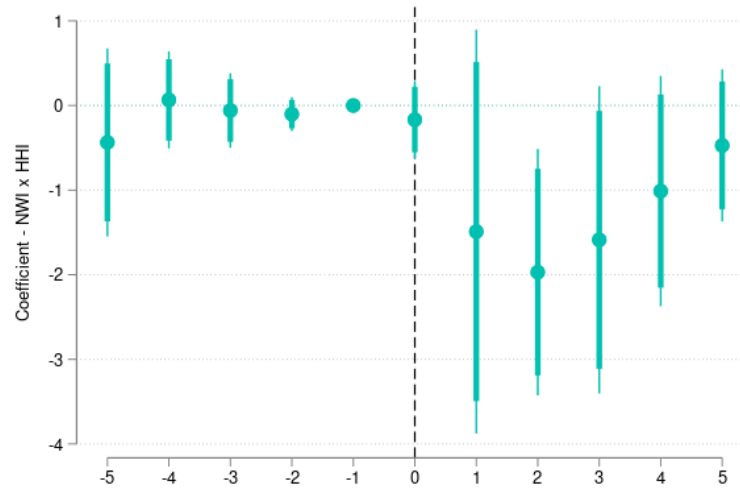


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

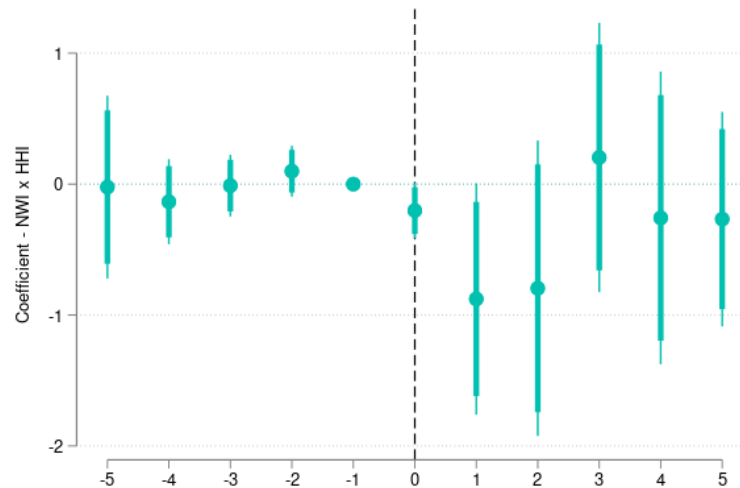
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 3 in the text, but replace the linear HHI term with an indicator for HHI above 0.25.

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

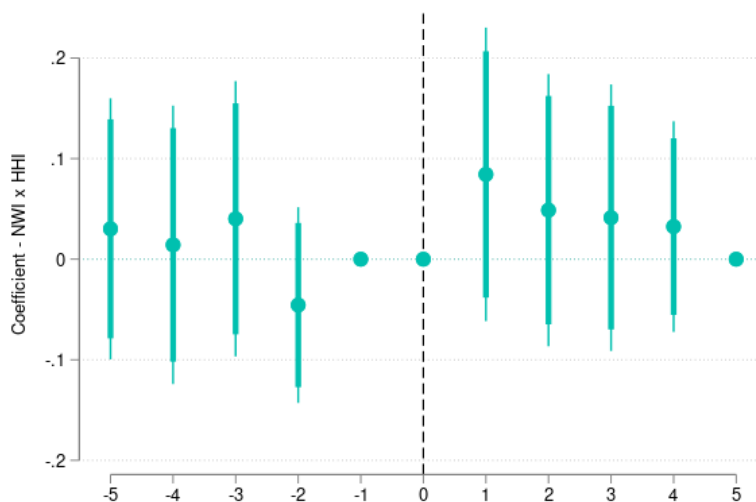


Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

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



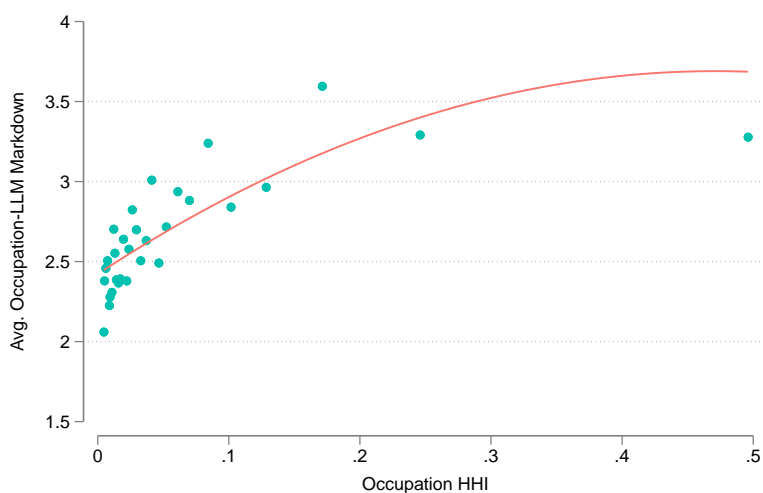
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

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.

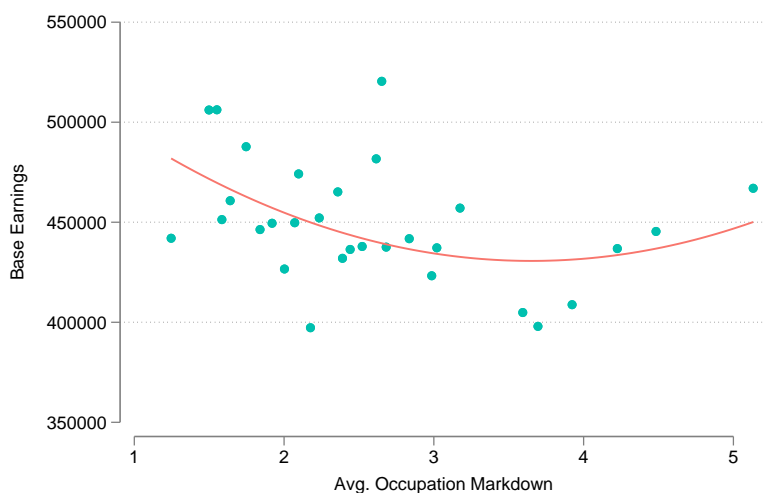
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-7: Relationships Between Concentration and Wage Markdowns (Manufacturing, Construction, and Mining)

Panel A: Occupation-LLM Average Markdown vs Concentration



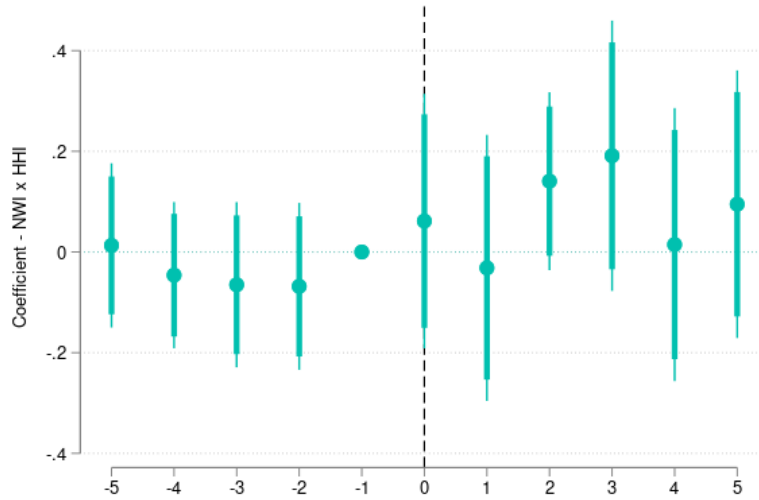
Panel B: Occupation-LLM Average Markdown vs Annual Earnings



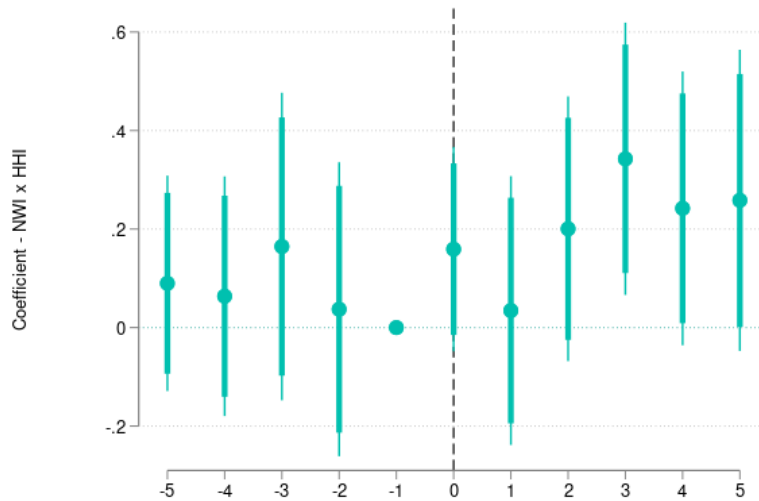
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.

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-8: Event Study Estimates: Firm-Level Average Worker Fixed Effects, by Group
 Panel A: Average Worker Fixed Effect of New (Non-Displaced) Norwegian Hires



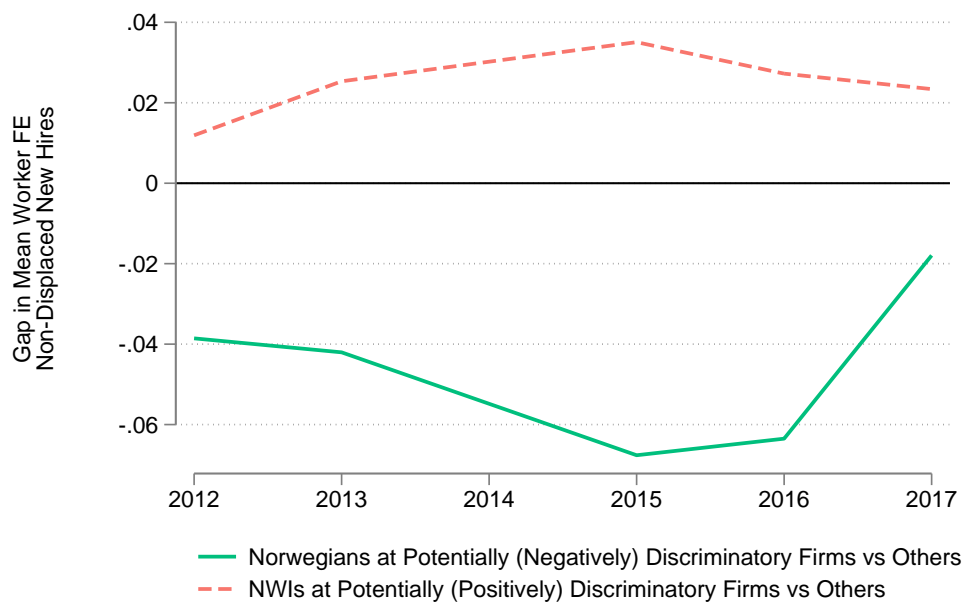
Panel B: Average Worker Fixed Effect of New (Non-Displaced) NWI Hires



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-9: (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.