The Power to Discriminate

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March 2025

Abstract

This paper provides the first empirical analysis of the relationship between employer discrimination and labor market power. It studies equally productive native and immigrant worker pairs with identical labor market signals and wages who are forced into job search due to mass layoffs and firm closures. By comparing re-employment gaps between these worker pairs in concentrated versus competitive labor markets, we isolate the role of labor market power from individual employment determinants. Our findings reveal four key results. First, wage and employment discrimination against immigrants is substantial. Second, labor market power amplifies discrimination, with far less discrimination taking place in highly competitive markets. Third, product market power has no independent effect on employer discrimination, consistent with the notion that wage-setting power is necessary for wage discrimination. Fourth, discrimination is rooted in employers' incorrect beliefs about immigrant productivity, and sustained employer-immigrant interactions gradually eliminate wage and employment disparities.

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

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1 Introduction

Employer power and labor market discrimination are two fundamental economic policy issues with widespread implications for economic efficiency, equity, and the trade-off be-

tween 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 study, 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. 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. (2020a)), 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 the first 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 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

[1] In 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 Jr 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)).

[2] There are 160 commuting zones in Norway.

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, we reveal how labor market power shapes discriminatory hiring and wage-setting practices (akin to a difference-in-differences specification).

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 hold more power, 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 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

[3] In 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.

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 investigate 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.

We present four core results. First, we find clear evidence of significant wage and em-

ployment 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 –exogenously 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 mechanisms to account for our findings, they would need to systematically correlate with concentration within occupational sub-segments of the same geographic labor market and spatial sub-segments of the national occupational market—a scenario we consider improbable (see Section 6 for further

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

details).

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]

Finally, we reveal that the employment discrimination we observe is primarily driven by 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.

[5] 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).

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

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 immigrants, 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.

This paper delivers the first empirical analysis 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); Bohren et al. (2022)). There is also rich empirical literature studying the implications of employer discrimination in various contexts (e.g., Bertrand and Mullainathan (2004); Fryer Jr and Levitt (2004); List (2004); Lang and Manove (2011); Lang and Spitzer (2020); Gerard et al. (2021); Benson and Lepage (2024)), and the role of product market power in driving employer discrimination (e.g., Black (1999); Levine et al. (2014); Hirata and Soares (2020); Meireles et al. (2021)). Notably, Kline et al. (2022) find that racial discrimination in job callback rates in the US context is driven by a relatively small share of firms. While the authors do not tie this to the competitive landscape, if most firms are exposed to relatively competitive local labor markets, they likely have little power to discriminate, which may be one explanation for their findings.

We build on these existing literatures and break new ground in understanding the dynamics of discrimination in labor markets across multiple dimensions. First, we provide novel evidence of the relationship between *labor market competition and discrimination*, a connection long predicted by theory but absent in empirical investigations. Second, while extensive research has examined discrimination in labor markets, no study has yet utilized real-world data outside laboratory settings to *disentangle the relative impacts of belief-based versus preference-based discrimination*—a crucial distinction for understanding the mechanisms of discrimination. Third, prior empirical studies have failed to *differentiate between product and labor market power* when assessing competition's impact on employer discrimination. Through these contributions, we highlight a new dimension of employer behavior in labor markets with important implications for policymakers and researchers alike.

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.

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 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.[7] 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 condi-

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

tional 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).

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: Employer Discrimination

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

3.1 A Basic Model of Discrimination

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

$$u_e = \pi - d(L_{nw}), \tag{1}$$

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

The profit function is:

$$\pi = f(L_n + L_{nw}) - (MCL_n)L_n - (MCL_{nw})L_{nw}, \tag{2}$$

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

The marginal cost of labor (MCL) represents the cost to the employer of hiring an additional worker. In a competitive market, firms are wage-takers and face a perfectly elastic labor supply, so wages equal the marginal cost of labor (MCL_n and MCL_{nw}), which corresponds to the worker's marginal revenue product (MRP). In monopsonistic markets, firms face an upward-sloping labor supply curve, meaning that the marginal cost of labor exceeds the wage. This creates a wedge between wages and MRP, allowing firms to set wages below MRP through a wage markdown (M_n and M_{nw}).[8]

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

3.2 First-Order Conditions

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

$$\max_{L_n, L_{nw}} u_e = f(L_n + L_{nw}) - (MCL_n)L_n - (MCL_{nw})L_{nw} - d(L_{nw}). \tag{3}$$

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

$$\frac{\partial u_e}{\partial L_n} = f'(L_n + L_{nw}) - MCL_n = 0. \tag{4}$$

$$\frac{\partial u_e}{\partial L_{nw}} = f'(L_n + L_{nw}) - MCL_{nw} - \frac{\partial d}{\partial L_{nw}} = 0. \tag{5}$$

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

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

When the marginal worker from each group are equally productive ($MRP_n = MRP_{nw}$), the firm will only hire a non-Western immigrant when $M_n - M_{nw} > d(L_{nw})$.

3.3 Implications of Discrimination

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

Belief-Based Discrimination. If discrimination arises from beliefs about productivity, the disutility from hiring non-Western immigrants is given by $d(L_{nw})$, where $d(L_{nw}) = g(E[L_n] - E[L_{nw}])$, with $E[L_n]$ and $E[L_{nw}]$ representing the employer's expectations of native versus non-Western immigrant productivity. Non-Western immigrants may face lower hiring probabilities and wages if employers underestimate their productivity relative to natives, conditional on true productivity. Hiring a non-Western immigrant would, therefore, entail additional perceived risk. Since obtaining individual productivity information entails costs (c), firms may rely on group-based beliefs rather than invest in additional information, provided that $MRP_n - MRP_{nw} - c > g(E[L_n] - E[L_{nw}]) + (M_n - M_{nw})$. In other words, firms would be willing to rely on these beliefs rather than invest in more information until the difference in workers' actual productivity minus the cost of acquiring that information is larger than the utility cost of the uncertainty around their expectations for immigrants' productivity plus any differences in their wage markdowns.[9]

3.4 Introducing Competition

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

For workers, discrimination based on beliefs is also costly. High-ability workers from a group perceived to be less productive may be overlooked in favor of less productive workers

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

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

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

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

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

3.5 Source of Power

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

As discussed above, in perfectly competitive labor markets, firms face a perfectly elastic labor supply, meaning they cannot set different wages for different workers based on their group membership and cannot mark down wages below the worker's marginal revenue product (MRP). This eliminates the ability to engage in wage discrimination for the same productivity. In addition, they cannot engage in employment discrimination because foregoing the most productive labor hurts the firm's profitability directly and pushes them out of the market in the long run.

Unlike in a competitive labor market, a monopsonistic employer faces an upward-sloping labor supply curve. Firms can exploit this by paying workers less than their marginal revenue product ($M_n > 0$ and $M_{nw} > 0$) and they can differentially mark down wages by subgroup even for equally productive workers, regardless of its product market power. The productivity costs of hiring less productive members of the favored group can be absorbed by the supernormal profits that the firm makes from wage markdowns. Thus, observationally identical natives and non-Western immigrants who are induced to search for jobs in such markets may have very different outcomes in *both* employment and earnings depending on the discrimination practices or preferences of employers. This is true irrespective of whether the discrimination is belief-based, preference-based, or monopsonistic.[10]

In a setting where a firm holds power in the product market but no power in the labor market, firms can discriminate, but with considerable limitations. In particular, if the firm pays disfavored workers less than their marginal revenue product, these workers, exhibiting a perfectly elastic labor supply, can simply leave for better options. In other words, the lack of wage-setting power places strong limits on wage discrimination in the market by forcing $M_n = 0$ and $M_{nw} = 0$. However, as discussed above, the firm can still engage in employment discrimination by only hiring natives, covering the cost of the productivity differences between the marginal native and marginal immigrant with its supernormal product market profits. Thus, we would expect labor market gaps after search for natives and non-Western

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

immigrants in this type of setting to only arise in the context of employment, not wages.[11]

3.6 Bohren et al. (2019) and Dynamics of Discrimination

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

However, once workers are hired, repeated interactions significantly reduce the cost of acquiring individualized information about productivity, effectively pushing $c \to 0$. Over time, this dynamic implies that discrimination stemming from incorrect productivity beliefs will diminish, assuming the worker is hired and meaningful interactions occur between the employer and the employee. If discrimination was present in the initial hiring choice, so-called "belief flipping" (Fryer Jr, 2007) might occur. This is a situation in which the disfavored group overtakes the favored group in wage and employment outcomes over time as the true productivity of positively selected members of the disfavored group is revealed.

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

3.7 Conceptual Framework and Overview of Empirical Analyses

To examine the relationship between competition and employer discrimination, we focus on a setting where equally productive individuals face the same employer decisions

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

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

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

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

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

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

Overall, our empirical analysis provides a comprehensive understanding of employer discrimination in what is ostensibly one of the most equal countries in the world. We examine whether such discrimination is driven by beliefs or preferences, how it is influenced by market power, and what policies could mitigate these practices effectively.

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 down.

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 if they are attached to a firm in the labor register. 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.

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. While the HHI does not capture all aspects of firms' labor market power, it serves as a strong and credible proxy. 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).

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 profits because wages are driven up to workers' marginal revenue product. We track their outcomes after they were 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.

In terms of technical implementation, our estimation approach utilizes a stacked layoff design based on involuntary displacement events that induce job search among observationally equivalent workers who are separated in differentially concentrated labor markets. This approach relies on examining four levels of differences, akin to a quadruple difference-in-differences design. Specifically, we compare the changes in outcomes after displacement (difference 1) for workers who are displaced relative to those who are not (difference 2) in more or less concentrated markets (difference 3) among natives/Western immigrants in one group and non-Western immigrants in the other (difference 4).

We define involuntary job loss events if establishments either close down or terminate more than 30 percent of their workforce (e.g., Salvanes et al. (2024)).[12] To overcome issues associated with staggered difference-in-differences designs, we follow Cengiz et al. (2019) and "stack" the dataset into base year panels by displacement year (2008 through 2012).[13] For each base year panel, we follow workers employed in a private-sector firm in the base year for a total of 11 years, from event year $t-5$ through $t+5$, using data from 2003-2017.

We fix our HHI measure in the year prior to the displacement-induced search. This allows us to overcome any potential endogeneity issues caused by the displacement events directly

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

[13]Recent advances in difference-in-difference estimation have shown that these estimators can be biased when there are time-varying treatments and treatment effects (Goodman-Bacon (2021); De Chaisemartin and d'Haultfoeuille (2020)). The bias stems from post-treatment outcomes being used as control observations without accounting for the time-varying nature of the treatment effect. Our approach overcomes this problem by using stacked panels (Cengiz et al. (2019)), where the control group in each panel consists solely of untreated workers (i.e., those who do not experience a mass separation in the base year).

impacting the HHI of the local market.[14]

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.[15] 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.

For individual i in local labor market l, industry s, occupation o, relative time t, base year b and year y, we estimate the following quadruple difference-in-differences specification:

$$\begin{aligned} y_{ilsotby} = {} & \alpha + \beta_1 P_{it} + \beta_2 I_i + \beta_3 D_i + \beta_4 H_{ilso} + \gamma_1 (P \times I)_{it} + \gamma_2 (P \times D)_{it} + \gamma_3 (P \times H)_{itlso} \\ & + \gamma_4 (I \times D)_i + \gamma_5 (I \times H)_{ilso} + \gamma_6 (D \times H)_{ilso} + \delta_1 (P \times I \times D)_{itlso} + \delta_2 (P \times I \times H)_{itlso} \\ & + \delta_3 (I \times D \times H)_{ilso} + \delta_4 (P \times D \times H)_{itlso} + \zeta_1 (P \times I \times D \times H)_{itlso} + \psi Earnings_{ib} \\ & + \eta_l + \theta_s + \kappa_o + \tau_t + \phi_y + \omega_b + \epsilon_{ilsotby}, \end{aligned} \tag{6}$$

where P denotes the post displacement search period, I denotes immigrant status, H denotes HHI in the base year, and D denotes displacement (treatment) status. We include fixed effects for baseline (pre-displacement) local labor market, baseline industry, baseline occupation, year relative to baseline (layoff) year, and current year. We also control for baseline earnings. We cluster the standard errors at the baseline firm.

We also estimate a more saturated model in which we include individual fixed effects and examine the outcomes only for those that were affected by a mass layoff or firm closure. This model simplifies to a triple-differences approach, comparing the displaced sample before and after the displacement-induced job search (first difference) across HHI (second difference) and between non-Western immigrants and others (third difference). The individual fixed effects subsume variation based on baseline earnings, baseline firm, education, birth cohort, local labor market, occupation, industry, overall treatment status, and displacement year. It also absorbs any unobserved time-invariant productivity differences across individuals. This

[14]The average displacement event induces less than 3% of the local market to search for a job, such that the direct effect on HHI or general equilibrium employment demand is negligible. Further, since we are interested in the relative effect across demographic groups, this is less of a concern.

[15]We also use a five-year tenure requirement and find identical effects. See Appendix Table A-6.

specification, therefore, allows us to compare natives and immigrants who had the same employment history and earnings and were laid off from the same occupation in the same firm at the same time. Thus, at baseline, each of these workers is mechanically equivalent in the determinants of their outcomes both on observed and unobserved characteristics. This is our preferred specification:

$$y_{ilsoty} = \alpha + \gamma_1(P \times I)_{ity} + \gamma_2(P \times H)_{itylso} + \gamma_3(P \times I \times H)_{itylso} + \eta_i + \tau_t + \phi_y + \epsilon_{ilsoty}. \quad (7)$$

Finally, we take advantage of the longitudinal dimension of our data by estimating an event study model using the following specification (based on the individual fixed effects approach):

$$y_{ilsoty} = \alpha + \sum_{\tau=-5,\, \tau\neq-1}^{5} [\delta_\tau(1|t=\tau) \times H_{ylso}] + \sum_{\tau=-5,\, \tau\neq-1}^{5} [\varphi_\tau(1|t=\tau) \times I_i] + \sum_{\tau=-5,\, \tau\neq-1}^{5} [\theta_\tau(1|t=\tau) \times H_{ylso} \times I_i] + \eta_i + \tau_t + \phi_y + \varepsilon_{ilt}. \quad (8)$$

There are two key benefits to the event study specification. First, 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 tracking individuals over time and 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.

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 the observed differences in outcomes are attributable to the treatment effects of concentration rather than pre-existing differences or confounding factors. In this subsection, we discuss each of these assumptions and describe how we examine the validity of them using the available data and empirical framework.

The first assumption is that of parallel trends. In our saturated individual-level triple difference-in-differences framework, causal identification requires that the third-level interaction—the combination of labor market concentration, immigration status, and time—exhibits parallel trends in the absence of treatment.

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 untestable in practice (since we can never observe the counterfactual outcome of treated individuals), we can obtain suggestive evidence that is consistent with this assumption. This is achieved by empirically estimating event study models and asserting that pre-treatment coefficients for the triple interaction are statistically indistinguishable from zero. In our empirical framework, this is equivalent to estimating Equation (8) and plotting the θ_τ terms for the periods before their search began. In Section 5.3, when we estimate the dynamic treatment effects to differentiate between preference-based and belief-based discrimination, we demonstrate that there is no evidence of violations of the parallel trends assumption in the pre-search period.

The second assumption underlying our estimation approach is that the job loss events we exploit to induce search are orthogonal to individual characteristics that influence post-displacement outcomes. This assumption ensures that displaced workers are not systematically different from non-displaced workers in ways that could bias the estimated effects. This selection assumption is common to all job loss papers in the literature, and several studies in the Norwegian context lend strong support for this assumption (e.g., Salvanes et al. (2024); Huttunen et al. (2011b; 2018); Carneiro et al. (2023)). However, it is important to note that our setting actually relies on a much more lenient selection assumption. Specifically, since we are interested in the native-immigrant search gaps across markets with different levels of labor market concentration, we can permit differential selection into the treatment sample across natives and immigrants as long as such selection does not vary systematically across markets with different concentration levels (the level of our third difference). While this assumption is hard to validate, we note that the job loss literature is expansive and has generally accepted these events as orthogonal to worker characteristics (e.g., Jacobson et al. (1993); Stevens (1997); Davis and von Wachter (2011); Couch and Placzek (2010)). We also note that the event study results obtained through the estimation of Equation (8) help examine pre-treatment relative trends across natives and immigrants as a function of labor market power. The absence of divergent trends in the pre-period across these groups suggests that there is no differential selection into treatment that could risk bias our results. To further 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 (rather than focusing both on mass layoffs and firm closures). This is an interesting exercise because in the event of firm closures, there is no ability of employers to decide who is dismissed and who is not, thereby mechanically ruling out any concerns associated with selection.[16]

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 at 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, location, industry, and occupation, which absorb many potential sources of time-varying confounding. By focusing on relative differences across groups, incorporating base-year panel stacking, and examining the robustness to using only firms closures to identify variation in search, we further reduce the likelihood that such shocks bias our estimates.

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

[16]Theoretically, like in a hiring situation, during a mass layoff in a competitive market, employers cannot discriminate on the dimension of who retains their job for the risk of laying off their more productive workers. There is more room to do so in a concentrated market. However, empirically, we find similar effects in the case of firm closures as we find for mass layoffs. We discuss these results after discussing our main findings.

(see Appendix Table A-9).

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 Jr 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 1 shows the effect of job search induced by involuntary displacement on employment and earnings using our quadruple difference-in-differences design (Equation (6)) as well as our triple-differences model with individual fixed effects (Equation (7)) 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 of the dynamics from five years prior to displacement until five years after displacement.

Starting with the results based on Equation (6), 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 DD 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 $DD \times HHI$ 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 $DD \times NWI$ 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, $DD \times NWI \times HHI$, 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 market power enables firms to engage in differential hiring and wage-setting based on group membership. This power allows firms to set different wages for equally qualified workers, rely on group-based assumptions about productivity, or exclude workers from the disfavored group. Moreover, the ability to earn supernormal profits enables firms to absorb the costs of discriminatory hiring, whether by forgoing more productive out-group workers whose true productivity is costly to assess or due to employer bias. These profits from wage markdowns make discriminatory practices more viable in concentrated labor markets.

While unlikely, our baseline results could theoretically reflect unobservable productivity differences between non-Western immigrants and natives within occupation-industry-experience-earnings cells, potentially masking factors unrelated to employer discrimination that also are correlated with post-search labor market outcomes. To address this, we estimate more saturated models by incorporating individual fixed effects into the regression framework and comparing changes in displaced workers' outcomes (Equation (7)). The individual fixed effect controls away any joint determinants of the outcome at baseline–observed or unobserved. The results in Table 1 show that this more saturated model produces identical

results to our baseline model. 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 the estimates in Equation 6 (corresponding with Columns (1) - (3) in Table 1).

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 Jr 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 2. 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 $DD \times NWI \times RevenueHHI$ 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 1.

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 Jr 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 flexible than productivity beliefs in the face of new information.[17] Results from our event study regressions (Equation (8)) 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.[18]

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

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

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

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.

To collect further evidence of preference-based and belief-based discrimination, we conduct three auxiliary analyses: (a) we use information on managers to measure their past experience with non-Western immigrants in their workplace as an outcome in our models (Panel D of Table 3); (b) we examine discrimination patterns toward second-generation non-Western immigrants and Western immigrants (Panels A and B of Table 3); and (c) we assess discrimination against non-Western immigrants who have been in the country the longest (Panel C of Table 3). 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 5 of Table A-1). 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 longer 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.[19]

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

[19]Our secondary analyses rule out manager homophily (Appendix Table A-1), the thickness of social networks or social proximity to managers (Appendix Tables A-3 and A-4), and post-layoff occupational sorting (Appendix Table A-2) as explanations or mechanisms for the employment gaps we find.

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 gaps narrow over time, we find strong evidence of costs to allocative efficiency and productivity. To examine this, we analyze firms that we call likely "negative discriminators," defined as firms that hire a displaced Norwegian but no displaced non-Western immigrant in our sample. We then examine the quality of the native-born Norwegian new hires who were not displaced in our sample. To measure quality, we estimate an AKM model of earnings with worker and firm fixed effects and then extract the worker fixed effect for each new hire. We calculate the gap in the average worker fixed effect among new hires for firms that were likely discriminators versus those that were not. We also 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.

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-6). 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).

6 Robustness and Alternative Mechanisms

The quadruple difference-in-difference specification, along with the more saturated individual fixed effects 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 job search among non-Western immigrants in highly concentrated markets? Four pieces of evidence suggest this is unlikely. First, the earnings gaps persist even after controlling for destination firm fixed effects. 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 reenter 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 Tranaes, 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-7). 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, 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.

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-2 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 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-1). 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-3) and high or low shares of non-Western immigrants (Appendix Table A-4), 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-5. 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.[20] 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.

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

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

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 individual fixed effect difference-in-differences models using only those displaced via a firm closure. These results are in Table A-8. 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. This implies that any underlying 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 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 3 shows no significant relationship between neighborhood composition and the likelihood of non-Western immigrants being hired, suggesting that non-work contact does not play a role.

Second, if initial discriminatory preferences among managers influenced outcomes but

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

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

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

More Severe Labor Demand Shocks. Another possibility is that non-Western immigrants and natives were employed in occupations and locations that experienced different levels of local labor demand shocks, which initially triggered the firm closures or layoffs. If these shocks were systematically correlated with search outcomes for non-Western immigrants in concentrated markets, they could potentially explain our findings. While we consider this scenario unlikely, we test for it directly in Table A-9.

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-10. 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 1. 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.

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 theoretical 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: Difference-in-Differences Estimates

	Base Model			Individual Fixed Effects		
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
DD	-0.073***	0.027***	-0.078***	–	–	–
	(0.005)	(0.003)	(0.007)	–	–	–
DD × HHI	-0.244***	0.060***	-0.488***	-0.237***	0.056**	-0.484***
	(0.045)	(0.023)	(0.089)	(0.047)	(0.023)	(0.093)
DD × NWI	-0.048***	0.003	-0.086***	-0.056***	0.005	-0.086***
	(0.018)	(0.004)	(0.023)	(0.020)	(0.004)	(0.024)
DD × NWI × HHI	-0.390***	0.060	-0.583***	-0.383***	0.056	-0.587***
	(0.132)	(0.056)	(0.202)	(0.142)	(0.060)	(0.225)
Observations (N)	4,428,176	4,428,176	4,411,883	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equations 6 and 7 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 2: Labor Market Power vs. Product Market Power

	Employment	Pr(Part Time)	Log Earnings
	(1)	(2)	(3)
DD × NWI	-0.065***	0.016***	-0.110***
	(0.018)	(0.005)	(0.026)
DD × Revenue HHI	0.042**	-0.017***	0.044*
	(0.017)	(0.005)	(0.024)
DD × Occupation HHI	-0.235***	0.053**	-0.378***
	(0.058)	(0.027)	(0.087)
DD × NWI × Revenue HHI	0.026	-0.032***	0.064
	(0.058)	(0.007)	(0.060)
DD × NWI × Occupation HHI	-0.450**	-0.018	-0.501
	(0.185)	(0.066)	(0.324)
Observations (N)	506,312	506,312	502,798

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 7 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.

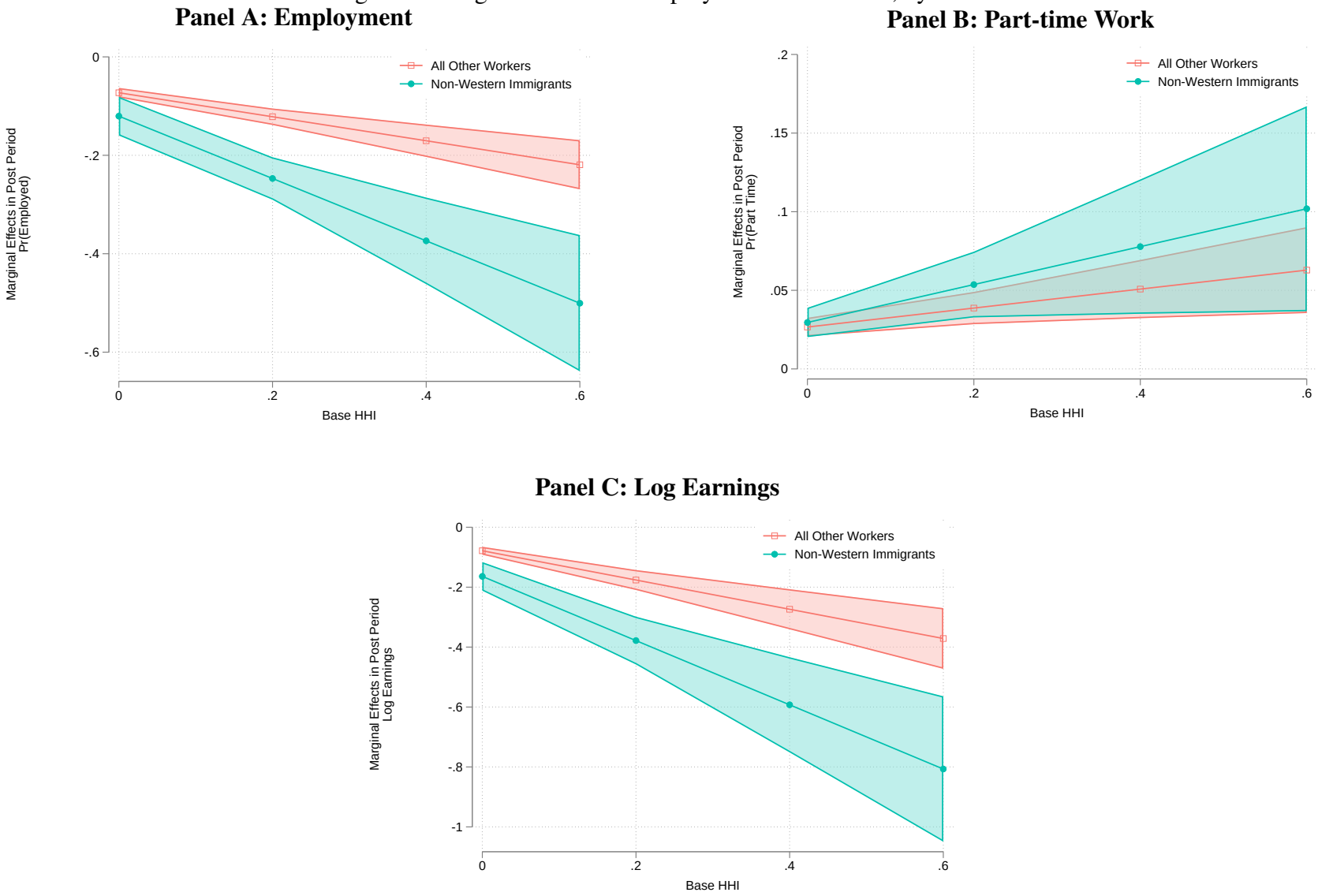
Table 3: Additional Evidence: Individual Fixed Effects Model

	Panel A: Second-Generation Immigrants		
	(1) Employment	(2) Pr(Part Time)	(3) Log Earnings
DD × HHI	-0.237***	0.056**	-0.483***
	(0.047)	(0.023)	(0.094)
DD × 2nd Gen NW	0.012	0.001	0.097***
	(0.011)	(0.007)	(0.023)
DD × 2nd Gen NW × HHI	0.072	-0.046	0.165
	(0.119)	(0.115)	(0.189)
Observations (N)	634,280	634,280	630,237
	Panel B: Western Immigrants vs Natives		
DD × HHI	-0.231***	0.054**	-0.485***
	(0.047)	(0.023)	(0.094)
DD × Western Imm.	0.002	-0.000	0.016
	(0.007)	(0.004)	(0.013)
DD × Western Imm. × HHI	-0.160	0.050	0.052
	(0.126)	(0.056)	(0.162)
Observations (N)	634,280	634,280	634,280
	Panel C: NWI, Long Time in Norway (>P75)		
DD × HHI	-0.236***	0.055**	-0.483***
	(0.047)	(0.023)	(0.093)
DD × NWI	-0.040*	0.001	-0.110***
	(0.022)	(0.006)	(0.032)
DD × NWI × HHI	-0.272	0.019	0.210
	(0.210)	(0.087)	(0.248)
Observations (N)	645,640	645,640	645,640
	Panel D: Destination Firm, Manager Average Experience with NWI		
	Manager Experience, t-3 to t-1	Manager Experience, t-1	Manager Neighborhood Share NWI
DD × HHI	0.024	0.026**	0.007
	(0.019)	(0.013)	(0.032)
DD × NWI	-0.020	-0.019**	0.003
	(0.013)	(0.010)	(0.007)
DD × NWI × HHI	0.197***	0.125**	-0.023
	(0.073)	(0.059)	(0.040)
Observations (N)	351,396	351,396	344,098

Source: Authors' calculations of Norwegian register data.

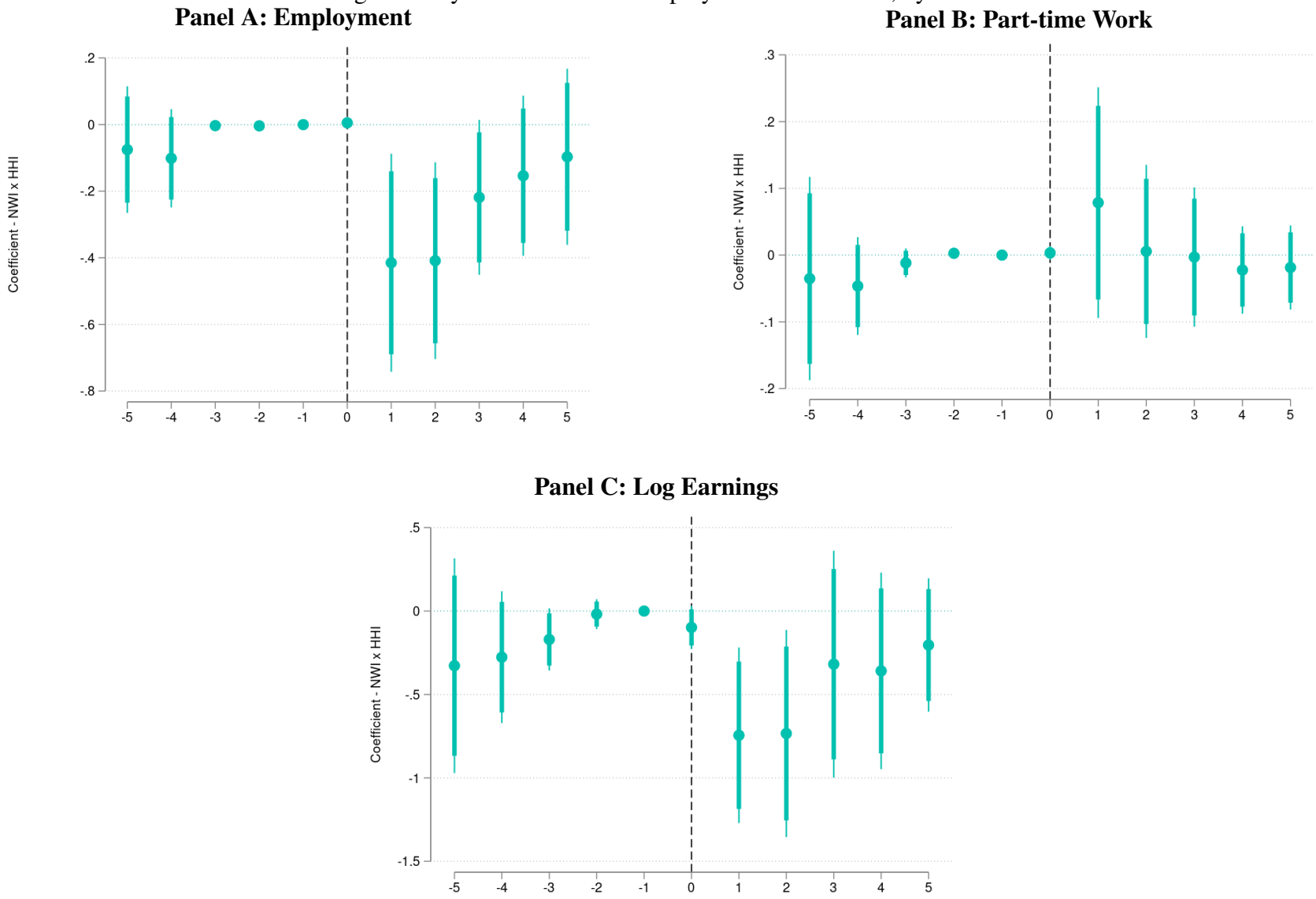
Notes: Estimates correspond to Equation 7 in the text over the first three years after the layoff event. Standard errors clustered at the base firm level. * p<0.10, ** p<0.05, *** p<0.01. "NWI" refers to Non-Western Immigrants. The 75th percentile of time in Norway among non-Western immigrants in the 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 three years (or one year) 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 7 in the text and those presented in Table 1. 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 8 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: Manager Characteristics

	Manager Share Female	Manager Share Norwegian Female	Manager Share Norwegian	Manager Share NWI	NWI Share in Manager's Neighborhood
	(1)	(2)	(3)	(4)	(5)
DD × HHI	-0.016	-0.051	-0.294**	0.057	0.007
	(0.092)	(0.072)	(0.135)	(0.050)	(0.032)
DD × NW Immigrant	0.001	0.006	-0.007	0.011	0.003
	(0.022)	(0.022)	(0.015)	(0.011)	(0.007)
DD × NW Immigrant × HHI	0.053	0.045	0.135	0.003	-0.023
	(0.157)	(0.150)	(0.168)	(0.076)	(0.040)
Observations (N)	353,585	353,585	353,585	353,585	344,098

Source: Authors' calculations using Norwegian register data.

Notes: Estimates correspond to Equations 6 and 7 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.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column 5 values are calculated by averaging across all managers in the share of non-Western immigrants in each manager's neighborhood.

Table A-2: Difference-in-Differences Estimates, Occupational Sorting

	Base Model		**Individual Fixed Effects**	
	Occupation Percentile Rank	AKM Occupation Fixed Effects	Occupation Percentile Rank	AKM Occupation Fixed Effects
	(1)	(2)	(3)	(4)
DD	0.383	0.009***	–	–
	(0.289)	(0.003)	–	–
DD × HHI	-0.166	-0.017	-2.154	-0.023
	(3.726)	(0.025)	(3.850)	(0.025)
DD × NWI	0.651	0.003	0.557	0.002
	(0.466)	(0.004)	(0.478)	(0.004)
DD × NWI × HHI	-0.410	-0.031	-2.296	-0.037
	(5.069)	(0.040)	(5.163)	(0.042)
Observations (N)	4,216,197	4,202,959	617,737	616,528

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equations 6 and 7 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 Equations 6 and 7.

Table A-3: 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)
DD × HHI	-0.213***	-0.254***	-0.456***	-0.457***
	(0.051)	(0.061)	(0.111)	(0.115)
DD × NW Immigrant	-0.061***	-0.041**	-0.101***	-0.049*
	(0.023)	(0.017)	(0.029)	(0.026)
DD × NW Immigrant × HHI	-0.370**	-0.446**	-0.585***	-0.534
	(0.151)	(0.221)	(0.217)	(0.483)
Observations (N)	344,336	329,152	341,686	327,122

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 7 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-4: Neighborhood Characteristics - NWI Shares of Neighbors

	Employment		**Log Earnings**	
	Low Share of NWI in Nhood	High Share of NWI in Nhood	Low Share of NWI in Nhood	High Share of NWI in Nhood
	(1)	(2)	(3)	(4)
DD × HHI	-0.220***	-0.251***	-0.433***	-0.490***
	(0.047)	(0.068)	(0.095)	(0.122)
DD × NW Immigrant	-0.037***	-0.062***	-0.053*	-0.104***
	(0.013)	(0.023)	(0.032)	(0.027)
DD × NW Immigrant × HHI	-0.416***	-0.387*	-0.603**	-0.641*
	(0.134)	(0.205)	(0.244)	(0.334)
Observations (N)	325,472	348,016	323,314	345,494

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 7 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-5: Difference-in-Differences Estimates, Destination Firm Characteristics

	Share NWI Workers (1)	Share Western Immigrant Workers (2)	Average Worker Age (3)	Log(Firm) Size (4)	Union Density (5)	Pr(Coworker from Neighborhood) (6)
DD × HHI	0.017	0.030	-1.063	-0.570*	-0.067	-0.307***
	(0.017)	(0.059)	(0.740)	(0.344)	(0.044)	(0.074)
DD × NWI	0.009	0.034***	0.226	0.041	0.022***	0.010
	(0.005)	(0.007)	(0.155)	(0.061)	(0.007)	(0.011)
DD × NWI × HHI	0.031	0.003	-0.564	0.740	-0.045	-0.2232
	(0.059)	(0.066)	(1.508)	(0.515)	(0.079)	(0.0148)
Observations (N)	512,634	459,558	512,634	512,634	512,634	512,634

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equations 6 and 7 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-6: Difference-in-Differences Estimates, Five-Year Tenure Requirement

	Base Model			**Individual Fixed Effects**		
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
DD	-0.073***	0.027***	-0.078***	–	–	–
	(0.005)	(0.003)	(0.007)	–	–	–
DD × HHI	-0.244***	0.060***	-0.488***	-0.237***	0.056**	-0.484***
	(0.045)	(0.023)	(0.089)	(0.047)	(0.023)	(0.093)
DD × NW Immigrant	-0.048***	0.003	-0.086***	-0.056***	0.005	-0.086***
	(0.018)	(0.004)	(0.023)	(0.020)	(0.004)	(0.024)
DD × NW Immigrant × HHI	-0.390***	0.060	-0.583***	-0.383***	0.056	-0.587***
	(0.132)	(0.056)	(0.202)	(0.142)	(0.060)	(0.225)
Observations (N)	4,428,176	4,428,176	4,411,883	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equations 6 and 7 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-7: 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-8: Difference-in-Differences Estimates, Firm Closures Only

	Employment	Pr(Part Time)	Log(Earnings)
	(1)	(2)	(3)
DD × HHI	-0.192***	0.089**	-0.545***
	(0.073)	(0.036)	(0.148)
DD × NW Immigrant	-0.040***	0.003	-0.042
	(0.015)	(0.007)	(0.034)
DD × NW Immigrant × HHI	-0.793**	0.023	-1.485*
	(0.361)	(0.152)	(0.865)
Observations (N)	173,304	173,304	171,966

Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 7 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. Standard errors clustered at the base firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-9: Main Estimates, Controlling for Size of Occupation Layoffs/Closures in Base Year

	Base Model			**Individual Fixed Effects**		
	Employment	Pr(Part Time)	Log Earnings	Employment	Pr(Part Time)	Log Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
DD	-0.072***	0.026***	-0.075***	–	–	–
	(0.005)	(0.003)	(0.007)	–	–	–
DD × HHI	-0.225***	0.049**	-0.413***	-0.232***	0.053**	-0.386***
	(0.043)	(0.023)	(0.085)	(0.048)	(0.022)	(0.093)
DD × NWI	-0.048***	0.003	-0.086***	-0.056***	0.005	-0.085***
	(0.018)	(0.004)	(0.023)	(0.020)	(0.004)	(0.024)
DD × NWI × HHI	-0.389***	0.060	-0.581***	-0.383***	0.056	-0.583***
	(0.132)	(0.056)	(0.203)	(0.142)	(0.060)	(0.226)
Shock Size	0.007***	-0.007*	0.020***	-0.015	0.029	-0.004
	(0.001)	(0.004)	(0.007)	(0.039)	(0.064)	(0.253)
Shock Size × Post	-0.021***	0.013	-0.081***	-0.005	0.003	-0.105**
	(0.007)	(0.009)	(0.015)	(0.031)	(0.011)	(0.053)
Observations (N)	4,428,176	4,428,176	4,411,883	673,488	673,488	668,808

Source: Authors' calculations using Norwegian register data.

Notes: Estimates correspond to Equations 6 and 7 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 × 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.

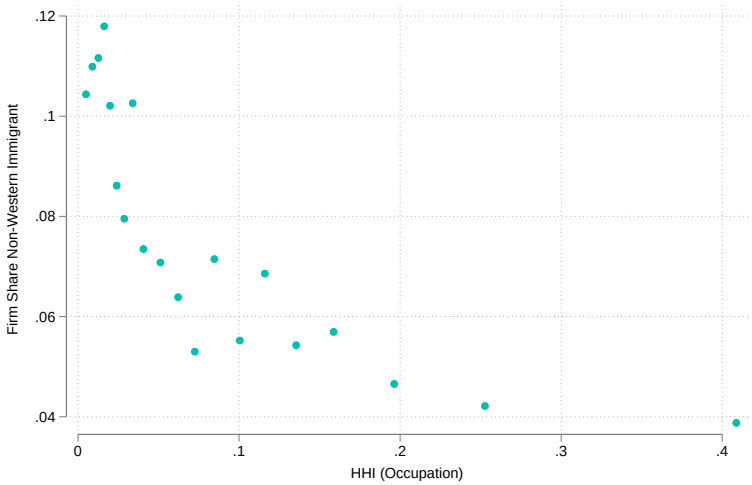
Table A-10: Difference-in-Differences Estimates, Social Assistance Program Take-up

	Individual Fixed Effects Model		
	Pr(Sick Leave)	Pr(Unemployment Benefits)	Pr(Disability Benefits)
	(1)	(2)	(3)
DD × HHI	0.047*	0.324***	0.025***
	(0.024)	(0.117)	(0.008)
DD × NWI	0.064***	0.051**	-0.003***
	(0.008)	(0.024)	(0.001)
DD × NWI × HHI	-0.046	0.250	-0.010
	(0.103)	(0.167)	(0.020)
Observations (N)	673,488	673,488	673,488

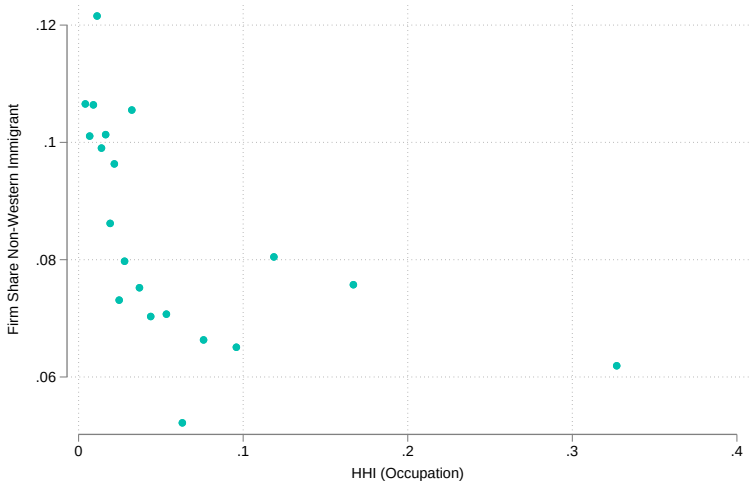
Source: Authors' calculations using Norwegian register data.
Notes: Estimates correspond to Equation 7 in the text and measure the difference-in-differences estimates over the first three years after the displacement event. Standard errors clustered at the base firm level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

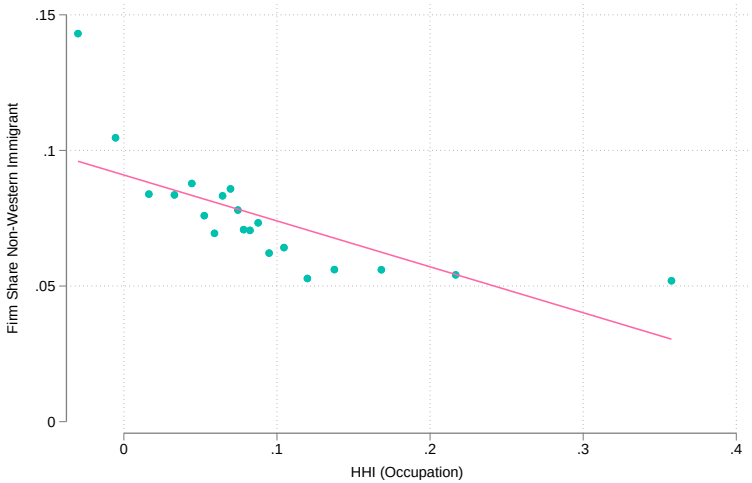
Panel A: Share NWI, All Sectors



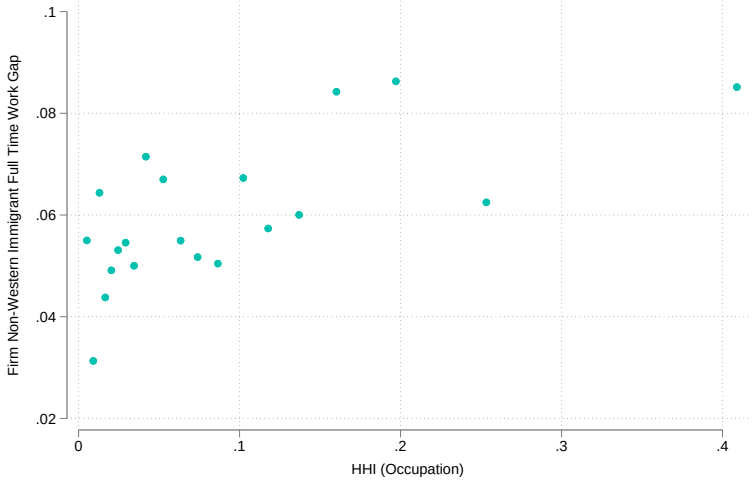
Panel B: Share NWI, Private Sector



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



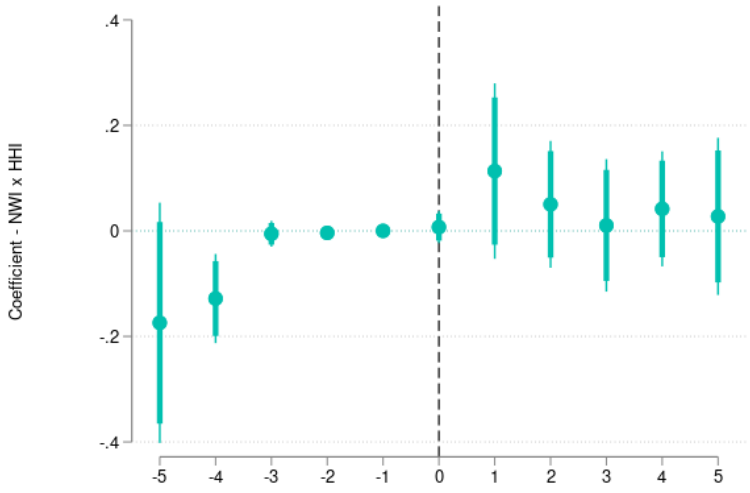
Panel D: NWI Full-Time Status Gap within Firm



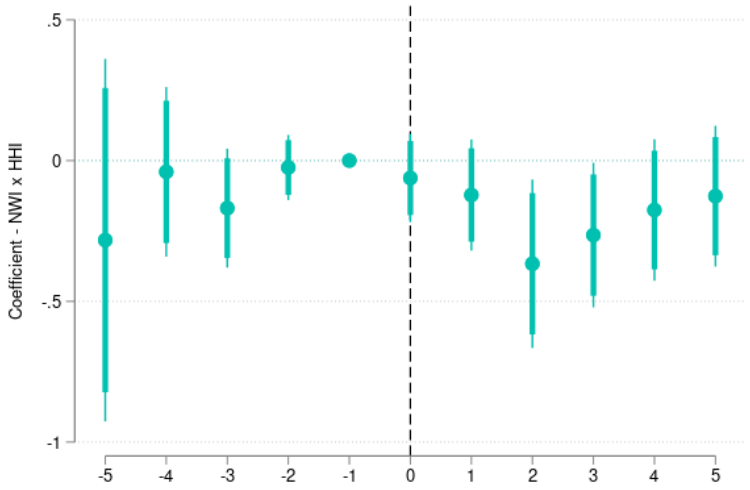
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. In Panel D, we calculate the share of workers from each group within each firm that are full-time and plot the differences between the full-time share of natives and Western immigrants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

Figure A-2: Dynamic Effects of Employer Discrimination with Current Firm Fixed Effects

Panel A: Part Time Work



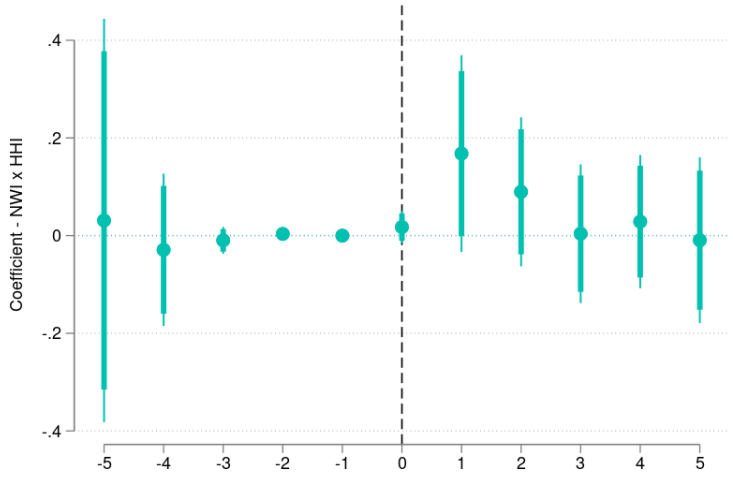
Panel B: Log Earnings



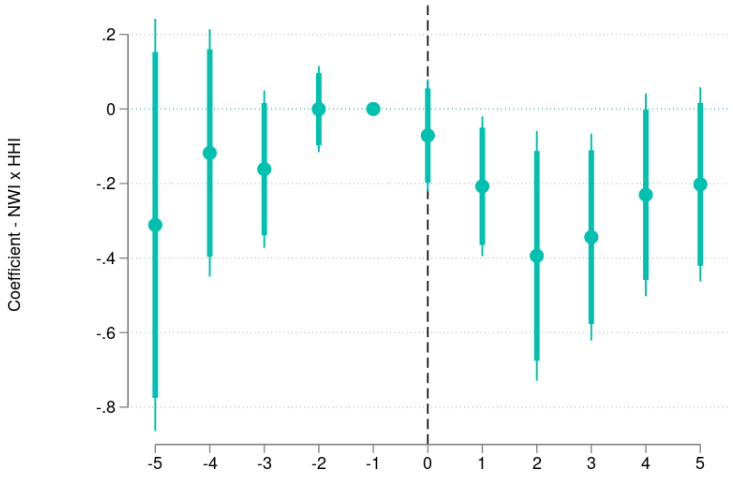
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 8 in the text, except this model adds a fixed effect for each worker's *current* firm.
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

Figure A-3: Dynamic Effects of Employer Discrimination with Additional Fixed Effects

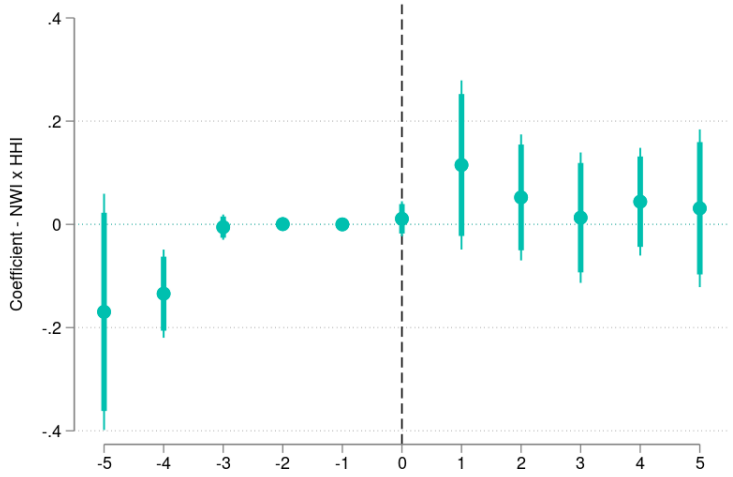
Panel A: Current Occupation and LLM FE, Pr(Part Time)



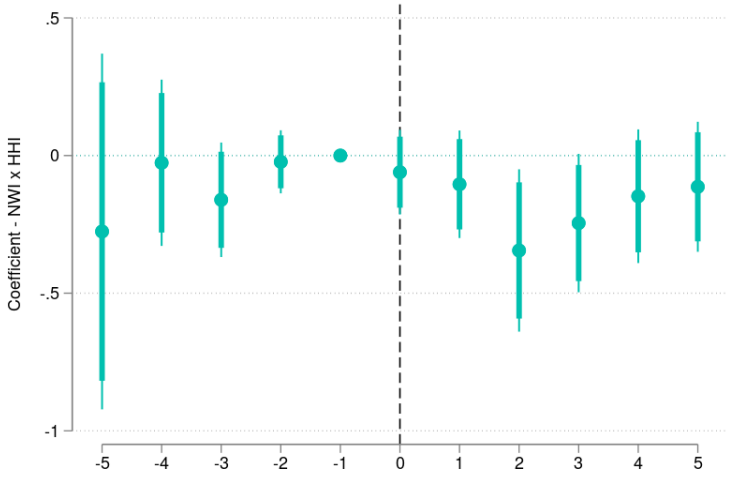
Panel B: Current Occupation and LLM FE, Log Earnings



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

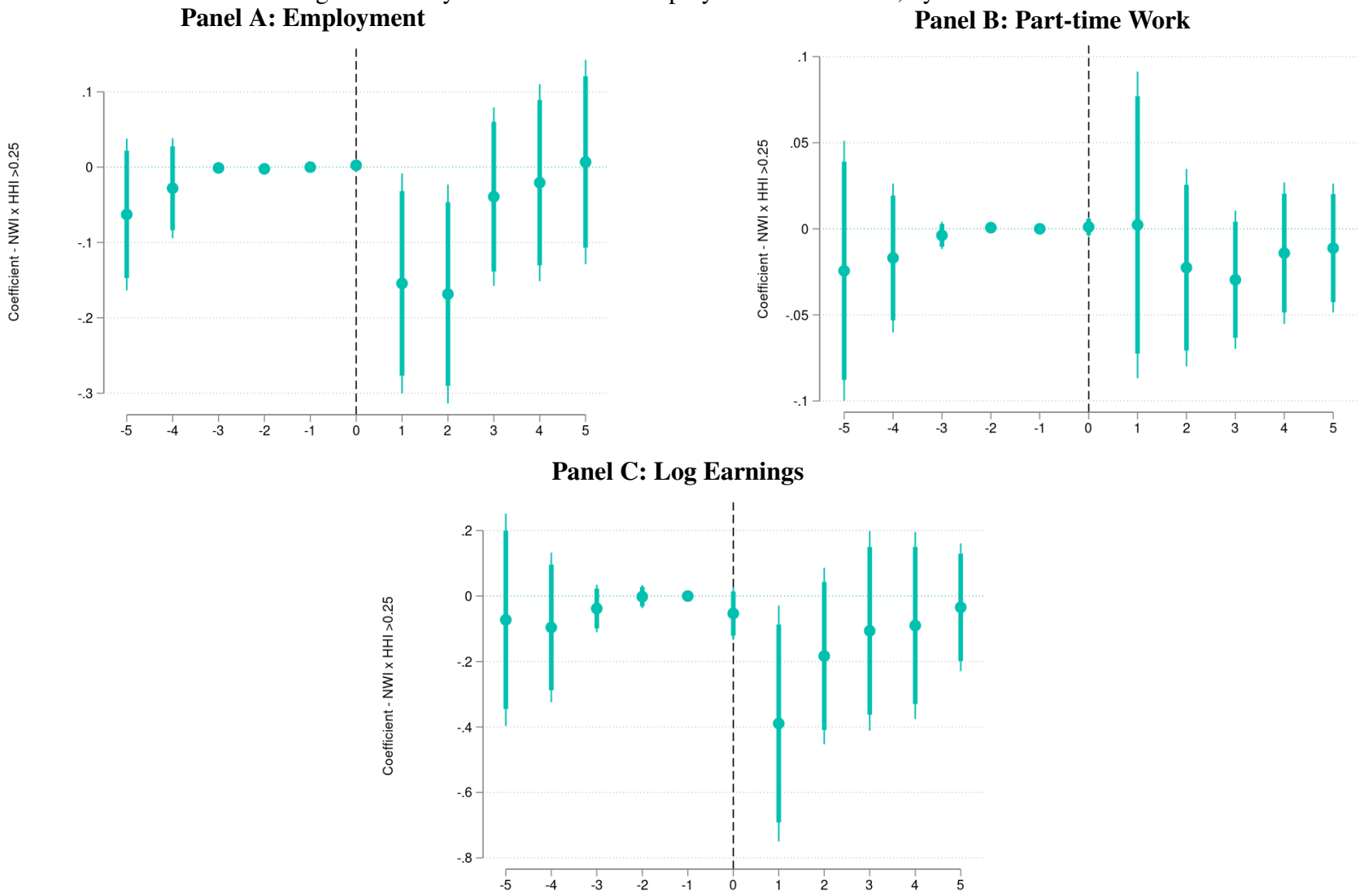


Panel D: Current Occupation, Firm, LLM FE, Log Earnings



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 8 in the text.
This model adds a fixed effect for each worker's current occupation and local labor market (Panels A and B) or current firm, occupation, and local labor market (Panels C and D).
Bars represent the 90% and 95% confidence intervals for standard errors clustered at the base firm level.

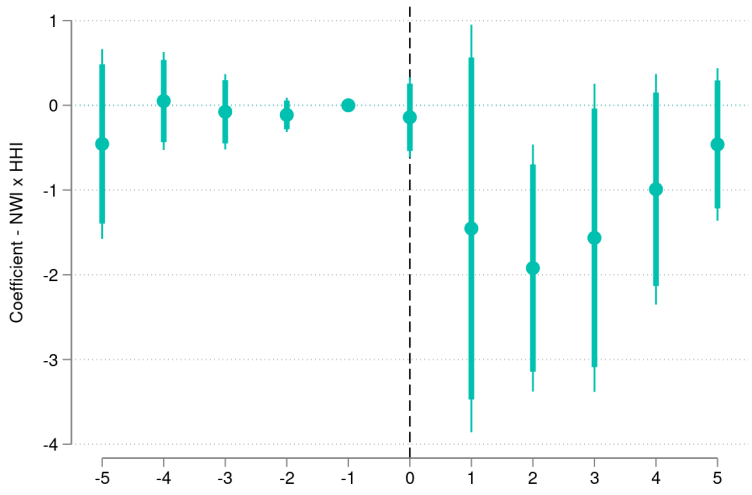
Figure A-4: Dynamic Effects of Employer Discrimination, by HHI > 0.25



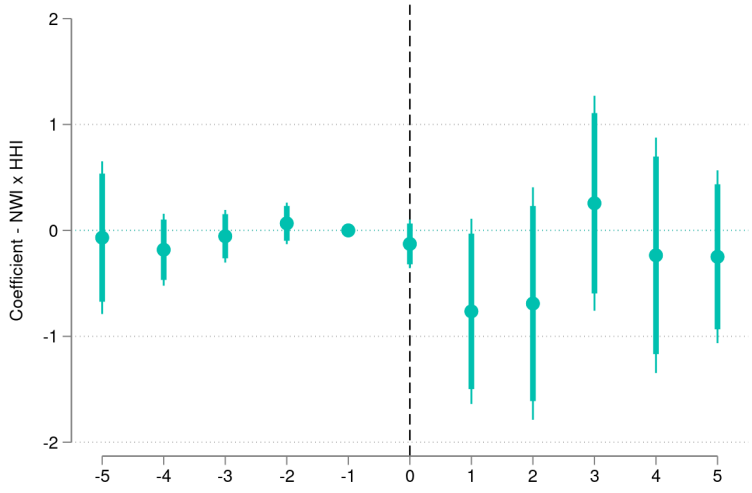
Source: Authors' calculations of Norwegian registry data from 2003 to 2017.
Notes: "NWI" refers to Non-Western Immigrants. Estimates correspond to the θ parameters of Equation 8 in the text, but replace the linear HHI term with an indicator for HHI above 0.25.
Bars represent the 90% and 95% confidence 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

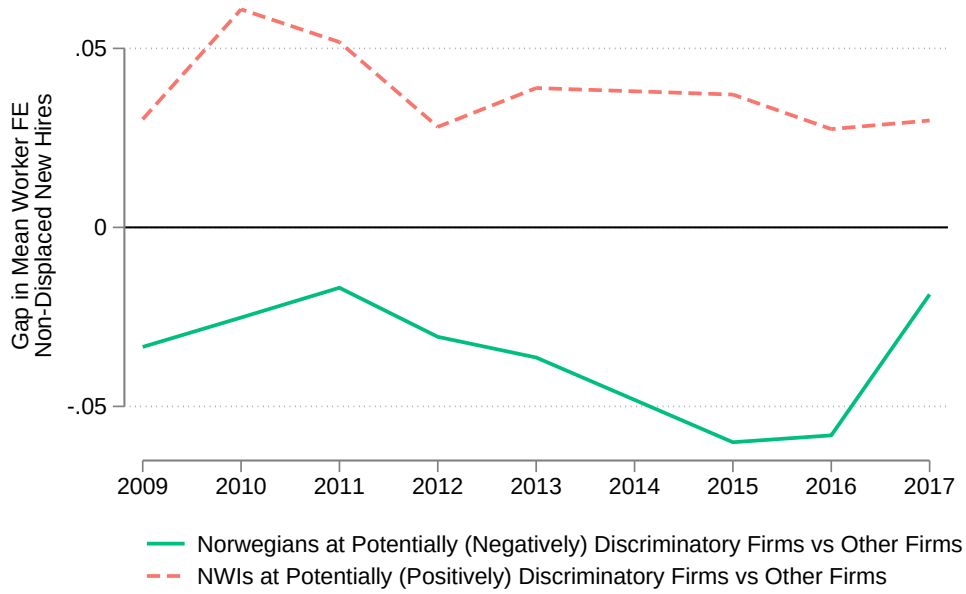


Panel B: Log Earnings, Mass Layoffs Only



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 8 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: (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.