Employers and Refugee Economic Integration: The Effect of Early Employer Quality*

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Abstract

Drawing on matched employer–employee data from Denmark, we study the role of early employers in the labor market integration of refugees. First, using the two-way fixed effects model of Abowd, Kramarz, and Margolis (1999), we estimate firm-specific wage premia that we use as a proxy for workplace quality. Second, we leverage the role of social connections and a dispersal policy implemented between 1986 and 1998, which quasi-randomly allocated refugees across municipalities, to obtain exogenous variation in their exposure to the quality of first potential employers. We find that placement in a municipality where, at arrival, co-ethnics are employed by high-quality employers has positive and statistically significant effects on refugees' employment and earnings for up to ten years. We also present a set of novel stylized facts on refugees and the firm ladder, highlighting the lasting influence of first employers for this group of workers and discussing potential issues for two-way fixed effects models. Our results suggest that policymakers should consider the type of employers offering jobs to refugees as an additional determinant of their success in host countries. Incorporating our insights in a data-driven algorithm to optimally match refugees with Danish municipalities leads to a 46% increase in short-run employment probability relative to the status quo dispersal policy.

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1 Introduction

Success in the labor market is a crucial determinant of immigrant integration in host countries. Yet across a wide range of destination countries, refugees experience persistent gaps in their economic performance, even compared to otherwise similar migrants (Bratsberg, Raaum, and Roed, 2017; Brell, Dustmann, and Preston, 2020; Fasani, Frattini, and Minale, 2022).[1] These difficulties, combined with the near doubling of forcibly displaced people over the past decade, have placed increasing pressure on public authorities (UNHCR, 2025).[2] In particular, rapid concentration of refugees in specific destinations generates short-term crowding effects and fiscal costs related to border protection, administrative services, and provision of housing, food, and healthcare (Bahar, Brough, and Peri, 2024). Over the longer term, delayed integration imposes lasting social and political costs. By contrast, successful integration can yield substantial economic benefits for host countries, including greater labor market flexibility, improved fiscal sustainability, and support in counteracting population aging. Recognizing these stakes, an extensive body of research has developed to understand the determinants of refugee economic integration (see Arendt, Dustmann, and Ku (2022) and Foged et al. (2024) for reviews).

Despite the considerable attention given to many aspects of refugee integration, the role of employers has remained largely overlooked in the economics literature. This is surprising for two main reasons. First, workplaces vary widely across many dimensions, such as productivity, on-the-job training and management practices, and early career matches with different firms can have long-term consequences for employees (see von Wachter (2020) for a review). Second, mounting evidence from various countries indicates that firms play an important role in wage determination, with firm-specific pay policies identified as a significant source of wage inequality (Card, Heining, and Kline, 2013; Card, Cardoso, and Kline, 2016; Card, Cardoso, Heining, and Kline, 2018). In the context of immigrant–native earnings disparities, between-firm sorting accounts for a substantial share of the earnings gap. (Damas de Matos, 2017; Dostie, Li, Card, and Parent, 2023; Arellano-Bover and San, 2023; Åslund, Bratu, Lombardi, and Thoresson, 2025). Still, little is known about whether and to what extent an early match with a "good" firm provides refugees with a pathway to a better career. A key reason is that endogeneity and selection are pervasive in employer–employee matching, making the causal effect of employers on employees difficult to identify.

In this paper, we address this gap by investigating the impact of the quality of refugees' first accessible employers on their labor market outcomes, developing an identification strategy to answer this causal question. Utilizing multiple linked administrative datasets on individuals and employers provided by Statistics Denmark, we proceed in two stages. First, we estimate establishment-specific pay premia by following the methodology of Abowd, Kramarz, and Margolis (1999) (AKM), which models wages as a function of additive worker and

[1] These disadvantages often reflect refugees' unique experiences of conflict, persecution, and displacement. They suffer trauma, lose human capital during their journey, often come from countries with markedly different cultures and languages, and are generally less positively selected than other migrants in terms of education and skills (Foged, Hasager, and Peri, 2024).

[2] In 2024, approximately 105,000 refugees were resettled in the U.S., the largest number received by any host country. The U.S. was also the world's largest recipient of new individual asylum applications (over 700,000).

firm fixed effects to account for unobserved individual heterogeneity. These premia capture the wage component attributable to working at a given firm. Consistent with a broad empirical literature linking premia to firm productivity and desirability, we use these premia as a proxy for establishment quality.[3] Second, we leverage the conditionally random initial placement of refugees across Danish municipalities under a dispersal policy implemented from 1986 to 1998. Thus, conditional on individual characteristics known to authorities at the time of allocation, the set of establishments active in a municipality can be regarded as exogenous for newly arrived refugees, enabling us to construct various measures of early employer exposure.

Denmark offers a particularly advantageous setting to answer this question. On the one hand, the country has accepted refugees from a large variety of origins over the past four decades, frequently changing integration policies. As a consequence, it is widely considered the "ideal laboratory" to study refugee integration (Arendt et al., 2022). In addition, the country's unusually rich administrative data allow us both to estimate firm quality with precision and to track individual refugees over a 15-year period following arrival. On the other hand, the implementation of a conditionally random national dispersal policy helps address concerns about worker nonrandom sorting across locations and employers, which has long complicated the estimation of causal effects while addressing similar questions.

To motivate our empirical approach, we document three novel stylized facts about refugees and firms. These relationships, while descriptive, offer valuable insights given the limited existing evidence on this population. First, for refugees, starting one's career in the host country at a higher-quality firm—measured by the establishment-specific pay premium—is strongly correlated with higher hourly wages, annual earnings, and employment probabilities. These effects persist over the short (1–5 years), medium (6–10 years), and long run (11–15 years) after the first employment spell.[4] Second, employer quality is "sticky" for refugees. While we show that pay premia for natives who begin in low- or high-quality firms quickly converge, the initial advantage (or disadvantage) for refugees remain much more persistent, suggesting that lower mobility across rungs of the firm ladder amplifies the importance of early matches.[5] Even among refugees who experience at least one employer transition or one move across municipalities, the persistence of first-employer quality remains, consistent with portable human capital development rather than a mechanical effect. Third, social connections shape refugees' opportunities. Using a dyadic dataset of worker-establishment pairs, we show that having a co-ethnic employed at an establishment upon arrival significantly increases a refugee's probability of later employment there, highlighting the role of co-ethnic (or co-national, terms we use interchangeably) networks in the labor market.

Building on this descriptive evidence, we exploit the quasi-random assignment of refugees under the dispersal policy to account for nonrandom sorting. Conditional on the personal characteristics known to the placement authorities, such as family structure and origin coun-

[3]See Kline (2024) for a review of methodologies and interpretations of firm wage differences.

[4]This sample consists of refugees subject to the 1986–1998 dispersal policy in Denmark. A one standard deviation increase in the pay premium offered by the first employer is linked to a higher probability of being employed by 3 percentage points, and to higher yearly earnings by 2577 USD in the long run. Repeating the analysis on a larger sample of refugees arriving in Denmark in the same period yields the same results.

[5]Another implication is that applying AKM decompositions on subgroups with low mobility may exacerbate the limited mobility bias, reinforcing a concern we raise with a methodological contribution later.

try, the initial assigned location was uncorrelated with refugees' observable skills and locational preferences, allowing us to obtain causal estimates of the impact of quality of early employers on future outcomes. In practice, we proxy early employer quality through three different sets of establishments initially accessible to newly arrived refugees: (i) all establishments active in the municipality of assignment at the time of arrival, (ii) those active in the municipality of assignment at arrival that had ever hired other co-nationals, and (iii) those in the municipality of assignment employing members of the local co-ethnic network at the time of arrival. For each set of employers, we compute the average quality of the establishments.[6] Taken together, these three measures of early exposure progressively refine the set of first accessible employers for refugees, from a broader pool of potential employers to a narrower, potentially more relevant one.

We find that high-quality early employers significantly shape refugees' labor market integration. Placement in a municipality where, at arrival, members of the local co-ethnic network are employed by higher-quality firms, has positive effects on both employment and earnings. A one standard deviation increase in network employers' pay premia raises employment probability by 0.8 (1.3) percentage points and yearly earnings by 231 (558) USD in the short run (medium run).[7] The effects remain positive in the long run but lose statistical significance. In contrast, higher workplace premia among all firms in the municipality reduce employment and earnings in the short run, suggesting that social connections are critical for accessing certain employers that may otherwise be inaccessible. These magnitudes are economically meaningful. In the first five years at destination, the effect size corresponds to a 7.5% increase relative to the sample mean for both outcomes, and over the first decade our effects amount to between one fourth and one seventh of estimates for other integration policies, such as active labor market policies and language training, a remarkably large magnitude given that they arise solely from exposure rather than a direct intervention. Since the early years are a crucial integration period, when refugees often face very low employment rates (Brell et al., 2020), employer accessibility emerges as a promising policy lever. For the cohorts in our sample, we estimate that a one standard deviation increase in network employers' pay premia would close 5% of the refugee–low-skilled native earnings gap, measured ten years after arrival. We validate our evidence with extensive robustness checks, including alternative quality measures, a split-sample IV approach, and pseudo-placebo tests.

This paper makes several contributions. First, exploiting the policy experiment created by the refugee dispersal program together with our measure of employer quality, we develop an identification strategy that provides the *first* causal evidence on the role of employers in refugee integration. Our results suggest that successful integration depends not only on obtaining a job but also on the quality of the employer providing it. We document that early employers are especially consequential for refugees, thus revealing a new margin of difference with natives, for whom the quality of the first employer is far less relevant. Second, as a methodological contribution, we discuss how these persistent effects may generate dynamic patterns not captured by two-way fixed effects models commonly used to document firm wage

[6] Figure 1 outlines the steps involved in the construction of our main measure of exposure to employer quality.
[7] At 2015 prices.

differences, cautioning against their use with foreign-born subgroups without scrutiny. Third, while the importance of network size for integration has been widely emphasized, we instead focus on network quality, decomposing it into member and firm components through the two-way AKM structure, showing the latter to be the main driver of our results. Finally, we provide novel evidence on how co-ethnic networks connect refugees to employers and demonstrate how our insights can improve refugee assignment policies through a data-driven algorithm.

To illustrate the role of connections in more detail, we show evidence consistent with networks serving as key information-transmission mechanisms, facilitating the labor market integration of new members by connecting them with employers. We highlight two complementary channels: job referrals and broader information sharing. Refugees arriving in networks connected to high-quality firms are more likely to access high-quality firms themselves and to obtain jobs with greater communication and cognitive task content. Highly educated refugees, who are most exposed to skill downgrading, are those benefiting the most from these connections, which prove crucial in bridging credential gaps. In addition, we show evidence of industry sorting consistent with sector-specific knowledge transfer, and find that network effects are strongest in mid-sized networks but weaken in larger ones, consistent with information congestion (Wahba and Zenou, 2005; Beaman, 2012).[8] In contrast, we do not find that firms with high predicted willingness to hire refugees are those driving our results. Connections at high-quality firms matter most when these firms are only moderately likely to hire refugees, in line with our expectation that information flows exert the greatest impact at firms on the margin of hiring. We rationalize networks' role within a classic search-and-match model, proposing an augmented Diamond–Mortensen–Pissarides framework (see Appendix A).

We conclude the analysis by leveraging our insights on employer quality and networks to extend a data-driven algorithm combining machine learning methods with integer optimization to assign refugees optimally across localities. First, we show that incorporating information on the quality of firms connected through networks improves the predictive accuracy of a LASSO model relative to a baseline that relies only on individual characteristics. Second, we find that the optimized assignment raises refugees' average predicted probability of employment within five years to 42.1%, compared to 28.9% under the status quo dispersal policy. We then evaluate counterfactual assignment scenarios in Denmark, confirming substantial employment gains from this flexible, low-cost procedure. Operationally, such algorithms can be integrated into software tools and made available in real time to resettlement authorities, without requiring them to interpret the synergies detected by these designs, ultimately reducing inefficiencies from the information overload common in manual assignment processes.[9]

The results of our study are highly relevant for policymakers. With increasing rates of unexpected, forced migration worldwide, receiving countries urgently need effective strategies to support migrants' assimilation.[10] While the Danish dispersal policy aimed to evenly distribute refugees to allocate integration costs more equitably, we emphasize the importance of a thor-

[8] Beaman (2012) shows that larger networks can harm some cohorts by congesting job-information flows.

[9] One U.S. resettlement agency (HIAS) has already adopted a software (*Annie*™) based on a similar algorithm developed in Ahani, Andersson, Martinello, Teytelboym, and Trapp (2021).

[10] At the end of 2024, the number of people displaced internationally (refugees, asylum seekers, and other people in need of international protection) surpassed 45 million (UNHCR, 2025). This group accounted for more than one in seven international migrants. Figure B1 shows the trend over the past 40 years.

ough consideration of factors that can benefit both refugees and host communities. Beyond simply assigning refugees to economically stronger areas, our findings suggest a more nuanced implication: policymakers should consider locations where social connections provide access to high-quality employment opportunities. Because these decisions involve no direct monetary costs, they represent an effective, low-cost policy improvement, which can be fully automated and tailored to various policymakers' priorities, as we show.[11] More broadly, recognizing the role of early employers in shaping integration trajectories is essential, especially since successful refugee integration and continued arrivals can yield significant economic and fiscal benefits for host countries (Clemens, 2022; Bahar, Parsons, and Vézina, 2022).

Our paper relates to at least four main strands of literature. First, we add to the body of work on the determinants of refugees' integration. Extensive research has analyzed asylum policies implemented by host countries, including language training, active labor market policies, changes in welfare benefits, and regulations for permanent residency and employment.[12] Other studies have focused on local conditions at refugees' arrival, such as the size of co-ethnic networks, employment levels, crime rates, and urbanization, among others.[13] However, to the best of our knowledge, our study is the first to consider the causal impact of employers and their quality on refugees' labor market outcomes, bridging the gap between the literature on refugees' integration and job ladder models. Importantly, we examines effects over a 15-year period, focusing on refugees who do not emigrate during this time. We distinguish between short-run (1–5 years), medium-run (6–10 years), and long-run (11–15 years) effects, extending the time horizon of most previous studies.[14]

Second, our study engages with the literature on firm wage-setting policies and their role in shaping workers' outcomes. Specifically, we connect to the emerging strand that examines the immigrant-native earnings gap employing the AKM framework (e.g., Damas de Matos (2017) in Portugal, Dostie et al. (2023) in Canada, Arellano-Bover and San (2023) in Israel) or a firm productivity grouping (Åslund et al., 2025). These studies consistently find that a significant portion of this gap is attributable to natives earning higher average workplace premia, largely driven by between-workplace variation.[15] By contrast, we focus on migrants' initial exposure to different rungs of the job ladder and show how this can shape their individual long-term assimilation trajectories. Furthermore, unlike most of the AKM literature, we employ AKM

[11] Potential crowding-out effects in the labor market from a large influx should not be a significant concern, as there is limited evidence of native displacement due to refugee inflows in Denmark (Foged and Peri, 2016).

[12] For instance, see Arendt, Bolvig, Foged, Hasager, and Peri (2024), Foged, Kreuder, and Peri (2022), Arendt and Bolvig (2023), Abbiati, Battistin, Monti, and Pinotti (2025), Dustmann, Landersø, and Andersen (2024b), (Arendt, Dustmann, and Ku, 2023), Marbach, Hainmueller, and Hangartner (2018), and Fasani, Frattini, and Minale (2021).

[13] Examples in this strand of literature include Edin, Fredriksson, and Åslund (2003), Damm (2009a), Åslund and Rooth (2007), Azlor, Damm, and Schultz-Nielsen (2020), Damm and Dustmann (2014), Dustmann, Mertz, and Okatenko (2023), and Eckert, Hejlesen, and Walsh (2022). Some of these authors have used the same Danish dispersal policy to study the impact of initial characteristics.

[14] We are aware of only three works that explicitly consider refugees and firms together, each addressing questions distinct from ours. Foged et al. (2022) examine Denmark's "Industry Packages", which matched local labor demand with refugee labor supply. Ahrens, Beerli, Hangartner, Kurer, and Siegenthaler (2023) discuss firm wage-setting power as a mechanism through which restrictions on refugees' outside options affect wages. Loiacono and Silva-Vargas (2024) conduct an experiment in Uganda, pairing refugees with subsidized employers for internships to assess effects on firms' willingness to hire refugees.

[15] Rather than between-firm sorting, the pay premium gap could also reflect lower premia for immigrants relative to natives within the same firm (i.e., a pay-setting effect). We examine within-firm differentiation in Appendix C.

fixed effects in a second stage where, relying on the dispersal policy for identification, we obtain quasi-experimental estimates for our question that are unconfounded by sorting bias. At the same time, by documenting persistent effects of early employer quality on refugees' productivity in our first descriptive fact, we offer a methodological contribution to these two-way fixed effects models by cautioning against dynamic effects in the migration context.

Specifically, these effects can severely undermine the conventional AKM framework, which views wage determination as fundamentally static. Observing that young workers who spend time in larger cities in Spain earn more later, De La Roca and Puga (2017) first raised this concern by revealing a highly portable dynamic component in the big-city earnings premium that get embedded in workers' human capital.[16] Our case reveals a new source of this dynamic, originating from the persistent influence of the first employer fixed effect on future wages for a group of migrant workers. Contrary to their case, however, this time-dependence violates the separability of worker and firm fixed effects that underpins the AKM model.[17] Although our framework is unaffected, since refugees are excluded from the panel used in the AKM estimation, our results highlight the need for caution when applying AKM decompositions to subgroups such as refugees or immigrants, as done in some recent studies.[18] While a number of empirical papers similarly emphasize the long-term consequences of entry labor market conditions for young adults (Kahn, 2010; Oreopoulos, von Wachter, and Heisz, 2012; Altonji, Kahn, and Speer, 2016), and especially of employment at heterogeneous firms (Arellano-Bover, 2024; Arellano-Bover and Saltiel, 2024; Gendron-Carrier, 2025), to our knowledge, we are the first to document this distinction in the importance of early employers between natives and refugees, which seems economically reasonable. For refugees, the first workplace represents initial exposure to an unfamiliar labor market, offering opportunities for training, country-specific human capital development, and valuable connections, while also shielding against early negative shocks (Willis, 2025); for natives, local knowledge, mobility, and established networks mitigate such dependence on the first match.

Third, our work also relates to the extensive literature on the importance of social networks for labor market outcomes (Ioannides and Loury, 2004; Bayer, Ross, and Topa, 2008; Hellerstein, McInerney, and Neumark, 2011; Topa, 2019). Immigrants, in particular, have been shown to rely heavily on their networks when searching for work due to their limited knowledge of the language, cultural norms, or effective job search methods (Dustmann, Glitz, Schönberg, and Brücker, 2016; Goel and Lang, 2019). We document the role of these connections for refugees and use them to characterize their set of accessible employers. Studies analyzing co-ethnic networks have also focused on the effect of network size and, more rarely,

[16] Card, Rothstein, and Yi (2025) present augmented specifications that include quarters of previous employment in specific sets of commuting zones to account for dynamic place-specific experience effects, though they eventually conclude that on aggregate these effects are negligible in the U.S. Notably, Arellano-Bover (2024) and Arellano-Bover and Saltiel (2024) provide further evidence of dynamic effects from accumulation of portable skills when starting in larger firms or in firms offering greater for on-the-job learning.

[17] We find no evidence of initial positive assortative matching for refugees, and importantly, we show that such a relationship between first employer quality and future wages is not observed for comparable natives, in line with the aggregate result of Di Addario, Kline, Saggio, and Sølvsten (2023).

[18] Bonhomme, Lamadon, and Manresa (2019) note that the static formulation of the AKM framework limits its ability to capture mechanisms emphasized in the dynamic structural literature. They propose an alternative framework to model unobserved heterogeneity and document state dependence in earnings. We view our evidence of dynamic effects for refugees as a distinct contribution, as we argue in Section 5.

quality, typically defined by outcomes of co-ethnic members, such as employment and earnings (Edin et al., 2003; Damm, 2009a, 2014). Conversely, our measure of firm quality in the network, while in principle a dimension of network quality, is primarily based on employer characteristics. As such, it allows us to decompose network quality into the roles of members and firms. Although larger networks and those with higher member employment are associated with better assimilation, whether connections to high-quality firms are beneficial or detrimental remains an empirical question: such links could serve as stepping stones to better jobs, or prove disadvantageous if those firms are harder to access. Our results speak directly to this point.

This network dimension has not yet been explored, with the only exception of Schmutte (2015), who uses 2002–2003 U.S. data to study the effect of firm-based local network quality on pay premia from job transitions. Our work differs in several important ways. First, by leveraging the quasi-experimental design of the Danish dispersal policy, our identification strategy is arguably stronger, as it avoids the need to assume no endogenous sorting at the block level within neighborhoods. Second, we focus on non-native workers, and refugees in particular, who were absent from his study and rely differently on networks. Third, the scope of our data allows us to conduct a richer analysis, examining multiple labor market outcomes and following individuals over a 15-year period after arrival. Lastly, while our main measure of network quality is constructed similarly, we build it at arrival to capture early exposure and complement it with other AKM-based aggregations, such as averages over all active firms in the municipality, to provide alternative proxies for accessible employers.

Finally, our data-driven algorithm to optimally assign refugees to localities builds on a recent work at the intersection of economics and operations research that has proposed and designed automated processes for host countries' resettlement decisions. This literature spans data-driven assignment methods aimed at maximizing short-run integration outcomes (Bansak, Ferwerda, Hainmueller, Dillon, Hangartner, Lawrence, and Weinstein, 2018), the development of software tools to support resettlement agencies with matching (Ahani et al., 2021), and algorithmic designs that incorporate refugees' preferences over localities (Jones and Teytelboym, 2018; Nguyen, Nguyen, and Teytelboym, 2021; Delacrétaz, Kominers, and Teytelboym, 2023). We extend these methodologies by incorporating network measures into the prediction models underlying the data-driven assignment, explicitly accounting for the quality of accessible employers at arrival, and by examining the resulting employment gains under various policy counterfactuals.

The remainder of the paper is organized as follows. Sections 2 and 3 describe the institutional setting in Denmark and the data used to construct the sample for our analysis, respectively. Section 4 outlines the procedure used to build a proxy for establishment quality. Section 5 presents a set of novel stylized facts on refugees and establishments. In Section 6, we discuss our empirical framework, while Section 7 presents the main results and robustness checks. Section 8 provides evidence on the mechanisms. In Section 9, we develop a data-driven approach to implement an optimal assignment of refugees, considering alternative counterfactual scenarios. Finally, Section 10 concludes.

2 Institutional Setting

Denmark represents a compelling context to study the role of employers for refugees' integration due to both large inflows of displaced people and the implementation of a national dispersal policy during the period of study. The country once had some of the most liberal refugee immigration laws (Dustmann et al., 2024b), and 155,752 individuals were granted refugee status between 1984-2019 (Arendt et al., 2022). In 2000, at the end of the dispersal policy, the number of asylum seekers who were granted protection was 96 per 100,000 inhabitants, about 4 times the EU average at the time.

In 1956, the Danish Refugee Council (DRC) was established in response to Denmark's 1952 ratification of the 1951 United Nations Convention on the Status of Refugees.[19] The DRC was tasked with assisting asylum seekers in applying for refugee status and residence permits in Denmark. In response to a large inflow of refugees during the early 1980s, the Danish government implemented a dispersal policy in 1986 for individuals whose asylum cases had been approved (i.e., refugees). This policy was carried out by the DRC.

The dispersal policy consisted of two stages and was in effect from 1986 to 1998 (Damm and Dustmann, 2014; Dustmann, Vasiljeva, and Damm, 2019; Dustmann et al., 2023). Its aim was to distribute refugees arriving in Denmark across counties and municipalities in proportion to the population size of each locality. Prior to 1986, refugees were primarily housed in the largest cities (Hasager and Jørgensen, 2024). In the first stage of the policy, refugees were allocated to Denmark's 15 counties based on the population size of the county. In the second stage, they were further assigned to municipalities within those counties, again in proportion to each municipality's population. At the time, Denmark had 275 municipalities.[20] The goal of achieving proportional allocation at the municipal level within 3–5 years was pursued through a rotation scheme, with DRC offices rotating between towns within each county.

When refugees arrived in Denmark and sought asylum, they were initially placed in Red Cross reception centers across the country while waiting for their case to be adjudicated. These centers, both then and now, are usually located in sparsely populated areas. Until 2013, asylum seekers were not permitted to work while residing there. Consequently, most refugees likely had very little contact with the broader Danish population before being granted asylum. In the late 1990s, the average waiting time for a decision exceeded one year (Arendt et al., 2024).

Once granted asylum, refugees were relocated to temporary housing in one of Denmark's 15 counties within the first 10 days. Subsequently, the local DRC office assigned them to a municipality within the county and assisted them in securing permanent housing. During the policy period, the vast majority of refugees obtained permanent housing within 18 months. According to Damm and Dustmann (2014), only 0–4% did not secure permanent housing within this time frame. Because some refugees initially lived in temporary housing near the municipality to which they were later assigned, typically within 3 months and on average

[19] Denmark was the first country to sign and ratify the 1951 Refugee Convention (Gammeltoft-Hansen and Madsen, 2021).

[20] Pre-2006-reform Danish municipalities are best compared in the U.S. context to incorporated towns or cities, or to small counties.

after 6–7 months, we define the municipality of assignment as the municipality in which the refugee resided in the year following the receipt of their residence permit (Damm, 2009b).

Municipality allocation decisions were made by the DRC without face-to-face meetings with the refugees. However, after receiving asylum, refugees completed a questionnaire that collected personal information such as nationality and family size. These characteristics were used to guide the allocation process. To support the conditional quasi-random assignment to municipalities, Table 1 shows that refugee characteristics are orthogonal to municipality characteristics, conditional on nationality, family size, and year of arrival—the variables observed by the authorities.[21] In particular, we show that education measures, which are likely correlated with unobserved skills, are not correlated with municipality characteristics related to co-national presence, labor market outcomes, or employer quality.[22]

While there were no restrictions on mobility for refugees following their initial assignment to municipalities, they were incentivized to remain in the assigned municipality for the first 18 months. This was because the introduction program, which included courses in Danish language, culture, and job training, was offered only in the assigned municipality, despite the fact that eligibility for means-tested social benefits was not conditional on remaining there (Damm, 2005). Furthermore, reassignment requests were considered by the council only after the initial municipal assignment. Figure 2 (Panel A) shows the proportion of refugees residing in their assigned municipality in the years following asylum approval. In the early years, the majority remained in their assigned municipality, and even by year 15, more than 40% still resided there.[23] This persistence is important, as we emphasize the role of initial exposure to employers in the municipality of assignment.

Two additional clarifications regarding the context are in order. First, refugees were eligible to work immediately upon receiving asylum, so there were no formal restrictions on the ability to seek employment for refugees in our sample. Second, public attitudes toward refugees were not particularly hostile during this period in Denmark. If anything, sentiment began to shift in the early 2000s, when the Danish People's Party, a populist right-wing party, began supporting the governing coalition, and "Start Aid," a reform that reduced welfare benefits for refugees by around 40%, was implemented in 2002 (Dustmann et al., 2024b).

3 Data and Sample

Our study relies on Danish administrative data provided by Statistics Denmark. Denmark is among the countries offering the most detailed longitudinal data on refugees in recent decades, along with exceptionally rich information on multiple dimensions. The datasets we use cover the entire population of individuals and firms in the country and include detailed information on aspects such as residence location, demographic details, socioeconomic characteristics, and employer (establishment and firm) information. To analyze labor market outcomes, we use employer-employee matched data from the Integrated Database for Labor Market Research

[21] These variables are always included as controls in our analysis.

[22] Additional checks are provided in Damm and Rosholm (2010), Damm (2014) and Foged et al. (2024).

[23] In the long run, this mobility rate is comparable to that observed for the overall Danish workforce in the 20 years following the start of the dispersal policy (Panel B of Figure 2).

(IDA), which we link to the Income Register (IND) and the Register for Classification of Employment (AKM). Additionally, we draw demographic information from the Population Register (BEF) and data on educational attainment from the Education Register (UDDA). Finally, we use the Migration Register (VNDS) and the Country Admission Register (OPHG) to gather information on refugee admissions and the timing of their initial settlements.

As the main outcomes in the analysis, we consider employment probabilities and earnings for refugees following their arrival in Denmark. Employment is defined as a binary indicator equal to 1 if the individual was employed at any point during the year, and 0 otherwise. Earnings are measured as annual gross labor market income, expressed in thousands of US dollars, deflated to 2015 prices, and include zero earnings. For the analysis of short-term (1–5 years), medium-term (6–10 years), and long-term (11–15 years) outcomes, we calculate the simple average of individual yearly outcomes within each interval and use this average as the outcome variable.

Our analysis sample consists of refugees who were granted asylum between 1987 and 1998.[24] Since actual refugee status is not directly observable prior to 1997, we follow other studies and impute this status for the years 1987–1996 according to the following two criteria (Damm and Dustmann, 2014; Foged et al., 2024). An individual is considered as a refugee if: i) they arrived in Denmark from one of nine refugee-sending countries—Palestine, Ethiopia, Somalia, Afghanistan, Sri Lanka, Iraq, Iran, Lebanon, or Vietnam; and ii) they arrived unmarried to either someone from a non-refugee country or an immigrant from a refugee-sending country who had arrived one year or more earlier.[25] The second criterion ensures that the sample consists of refugees subject to the quasi-random dispersal policy, excluding subsequently arriving spouses, who were almost always assigned to the location of their preexisting family. Similarly, migrants from Yugoslavia are excluded from the set of refugee-sending countries because they were subject to non-random dispersal patterns upon arrival in Denmark as part of the Bosnian program during the 1990s. Lastly, we restrict our sample of refugees to those aged 18–55 at arrival and to those remaining in Denmark for at least 15 years in order to extend the time horizon of the analysis relative to most previous studies.

Table 2 presents summary statistics for the final analysis sample of refugees who were *de facto* randomly dispersed and observed for at least 15 years in Denmark.[26] The average age at arrival is approximately 30 years, the majority are male (58%), and one fourth of those reporting educational attainment have completed academic education prior to arrival. Figure 3 displays the size and the origin composition of the different arriving cohorts of refugees in our sample, which consists of approximately 15,570 individuals. In the period we study, the yearly number of arrivals ranged from around 900 to roughly 1,700.

[24]We start from the 1987 cohort because of data limitations.

[25]Conflicts (e.g., Iran and Iraq) and civil wars (e.g., Somalia) triggered an exodus from many of these countries.

[26]As we impose various restrictions on the initial sample of refugees, selection may raise concerns about the external validity of our conclusions. For instance, the requirement that individuals be observed for at least 15 years at destination may result in a particularly positively selected sample. To examine this, Table B1 reports summary statistics for the refugee sample *before* excluding those observed for fewer than 15 years and later-arriving spouses.

4 Establishment Quality

This section describes the high dimensional fixed effects method we use to estimate a proxy for establishment quality. We adopt the popular methodology originally proposed by Abowd et al. (1999), known as the AKM decomposition, and build on frontier refinements developed in the recent empirical literature. First, we lay out the AKM wage framework applied to Danish matched employer-employee data to estimate workplace pay premia, describing sample restrictions adopted and presenting variance decomposition results (Section 4.1). Then, we discuss the main identification assumption and present a variety of tests to show the consistency of our setting with it (Section 4.2). Because we estimate the model without imposing origin restrictions on the sample of workers, we also assess whether our estimates accurately reflect workplace qualities for migrants in Denmark. We leave details on this aspect to Appendix C.

4.1 Job ladder wage model

To proxy the quality of establishments available to refugees in Denmark, we fit a linear wage model with additive person and establishment fixed effects (Abowd et al., 1999). Using our data, we construct a job spell panel with person-year observations for N workers employed in J establishments during our period of interest (1986–1998). We assume that the log hourly wages y_{it} of worker i in year t are determined by the sum of a worker component α_i, an establishment component $\psi_{\mathbf{J}(i,t)}$, time-varying characteristics X'_{it}, and an error term r_{it}:

$$log(y_{it}) = \alpha_i + \psi_{\mathbf{J}(i,t)} + X'_{it}\beta + r_{it} \tag{1}$$

Following the standard interpretation of the AKM decomposition (Abowd et al., 1999; Card et al., 2013), the person effect α_i captures a combination of personal, permanent skills leading to different earnings capacity; the establishment effect ψ_j can be interpreted as the time-invariant pay premium offered by the employer to all employees working at j; and X_{it} is a vector of time-varying controls that affect worker productivity, which in our case include year dummies and age terms (quadratic and cubic). Importantly, the model assumes that ψ_j remains constant throughout the sample period from 1986 to 1998.[27]

For the estimation of equation 1, we adopt a common restriction in the literature and consider only private sector employees to obtain a set of comparable firms with market-based wage-setting. We winsorize hourly wages by excluding the bottom and top 0.5% in each year and further retain only observations indicated as high-quality by Statistics Denmark to handle outliers and avoid reporting errors. Moreover, although refugees represent a small group compared to the rest of the population, we exclude them from the panel used for estimation to avoid mechanical effects.[28] As is well known, workplace effects in this model can only be identified within a "connected set" of establishments linked by worker transitions, as movers provide the variation needed to disentangle α_i from ψ_j in wages. Consequently, we restrict the

[27] We consider this an innocuous assumption given the relatively short period analyzed. Nevertheless, Lachowska, Mas, and Woodbury (2020) provide evidence of persistency of firm effects.

[28] In Section 5 we present another important justification for this choice.

analysis to the largest connected set of establishments, reporting various statistics in Table 3 (Panel A). Our largest connected set includes 98% of workers and 90% of establishments, with the mean log hourly wage for observations within it equal to that of the full panel.

Consistent with this methodology, we interpret the vector of establishment fixed effects ψ_J as measures of the pay premia workers receive at the given establishment j. As noted in various studies, establishment fixed effects reflect employer-specific, time-invariant compensation policies (Baker, Gibbs, and Holmstrom, 1994) or, more broadly, advantages associated with being employed by a given employer (Card et al., 2013). These advantages can derive, for instance, from rent-sharing or efficiency wages (e.g., see Burdett and Mortensen (1998), Moscarini and Postel-Vinay (2012), Shapiro and Stiglitz (1984), and Akerlof and Yellen (1990) for theoretical explanations of pay premia). We use the estimated establishment effects as the main measure of employer quality in our analysis, consistent with a broad empirical literature showing that establishment fixed effects are strongly associated both with observable measures of firm productivity and desirability (Kline, 2024). We confirm that this is true in our context as well in Tables B2 and B3, which show a positive and significant correlation between the estimated establishment fixed effects and other establishment characteristics, such as value added per worker, establishment size, and the share of high-skill employees.

Panel B of Table 3 presents the resulting variance decomposition from estimating equation 1. The variance of hourly wages is divided into five components: worker effects, employer effects, year effects, covariance terms, and a residual. While worker fixed effects account for a large share of the variation in our outcome (44%), employer effects are also important, explaining approximately 16% of the variation. This is comparable to 13% reported by Lachowska et al. (2020) for Washington state and 18–21% reported by Card et al. (2013) for Germany.

4.2 Endogenous mobility

Consistent OLS estimation of equation 1 requires the assumption of conditional exogenous mobility. As discussed in Card et al. (2013), a sufficient condition for this assumption to hold is that, using h_j to denote establishment indicators, $E\left[h_j' r\right] = 0$ for each establishment j. This amounts to say that the error term r in equation 1 is conditionally independent of employer transitions, implying that the probability of being assigned to an establishment for a worker depends only on worker and plant characteristics.[29] Essentially, this condition prevents forms of "endogenous mobility" driven by specific characteristics of the worker-employer match that can be interpreted as "interaction effects".[30]

A variety of tests has been proposed to ensure that data are consistent with this exogenous mobility assumption (e.g., Card et al. (2013) for Germany, Card et al. (2016) for Portugal, Arellano-Bover and San (2023) for Israel, Dostie et al. (2023) for Canada, Song, Price, Guve-

[29] Worker-employer matching is assumed to be based on a combination of the permanent component of individuals' ability and the average pay premia offered by workplaces.

[30] To examine different forms of endogeneity, we can assume, as in Card et al. (2013), that the composite error can be rewritten as a sum of three separate random effects:

$$r_{it} = \eta_{ij(i,t)} + \zeta_{it} + \epsilon_{it}$$

where $\eta_{ij(i,t)}$ is a match component, ζ_{it} is a unit root component, and ϵ_{it} is a transitory error. Valid OLS estimates require that firm-to-firm transitions are not related to components of r_{it}.

nen, Bloom, and von Wachter (2019) for the US, among others). Below, we replicate some of these checks in our setting to rule out problematic forms of endogenous mobility. Overall, we consider the evidence provided to support the assumptions needed for the AKM decomposition to be a reasonable approximation of labor market dynamics in Denmark during our period of study.

Following Card et al. (2013), in Figure 4 we begin by presenting a simple event study analysis that examines the wage effects of job transitions, where origin and destination workplaces are classified into quartiles based on the mean wages of other workers at those workplaces.[31] The figure shows that different mobility groups exhibit distinct wage levels both before and after a move, consistent with expectations based on the quartile rankings. At the same time, there is strong evidence that moving to a job with higher-paid coworkers leads to a wage increase, while transitioning to a lower quartile results in a wage reduction. Such patterns of systematic wage changes indicates that different establishments pay different average wage premia to their employees.

A crucial feature of Figure 4 is the approximate symmetry (i.e., similar magnitude, opposite sign) of the wage gains and losses for workers moving between quartile 1 and quartile 4 establishments.[32] This symmetry is inconsistent with sorting based on the idiosyncratic match component of wages, a form of endogenous mobility that would introduce bias to the AKM approach.[33] Additional reassurance against this type of sorting is provided by Panel C of Table 3, which compares the adjusted-R^2 from the AKM model to that of a fully saturated model where log wages are regressed on an indicator for each worker-employer spell (the job match effects model from Card et al. (2013)). While the statistical fit is slightly better for the job match effects model (adjusted-$R^2 = 0.865$), the roughly 7 percentage-point difference between the adjusted-R^2 values suggests that the additively separable AKM model of wages is fairly accurate.

More broadly, violations of the separability assumptions in the AKM model can be assessed by examining residuals for specific types of matches. Figure 3 plots the mean wage residuals across 100 cells, defined by deciles of person effects and establishment effects, as in Card et al. (2013). While deviations are observed among the lowest-decile establishments, even the largest deviations are less than 0.5% in magnitude, strongly supporting the conclusion that the additive structure of equation 1 provide a good approximation of the wage-setting process.

Figure 4 also shows no indication of an Ashenfelter transitory dip in movers' wages prior to a move, effectively ruling out any connection between firm-wide shocks and mobility rates.[34] More importantly, the figure displays no evidence of systematic mobility patterns in which

[31] For clarity, we only report wage profiles for workers leaving quartile 1 and quartile 4 establishments.

[32] A transition from quartile 1 to quartile 4 is associated with a trend-adjusted wage gain of 32.7 log points, while a transition from quartile 4 to quartile 1 with a trend-adjusted loss of 30.6 log points.

[33] This evidence suggests that idiosyncratic match effects are not of great importance. If variation in wages across establishments reflects differences in average wage premia rather than sorting, then movers will experience systematic wage changes. Individuals who move to establishments where other workers are highly paid (low paid) will, on average, experience wage gains (losses). This is what is predicted by an additive model with exogenous mobility, and it is precisely what we observe in this exercise.

[34] As explained in Card et al. (2016), workers may be more likely to leave workplaces experiencing negative shocks and move to firms undergoing positive shocks. In such cases, we would expect to observe a systematic dip in the wages of workers about to leave.

workers moving to higher-wage firms exhibit different wage trends prior to their move compared to those transitioning to lower-wage firms. A second form of endogenous mobility would arise if the direction of firm-to-firm mobility were correlated with transitory wage shocks, ϵ_{it} (Card et al., 2016). However, our evidence suggests that such a correlation is not present.

Finally, we consider two aspects related to the reliability of our estimates. First, since AKM decompositions require large sample sizes to ensure sufficient mobility, Panel A of Table 3 also considers the extent to which limited mobility could pose an issue in our setting. The average number of movers per employer in the sample used to estimate equation 1 is above 10, comfortably exceeding the threshold of 6 suggested by Andrews, Gill, Schank, and Upward (2012), above which limited mobility bias is unlikely to be a concern. Nevertheless, to deal with limited mobility concerns and measurement error, in Section 7 we will also subject our analysis to a sensitivity check introducing split-sample instrumental variable (IV) estimates to correct for possible bias.

Second, we address the concern that, if employers engage in forms of within-firm differentiation between natives and immigrants, establishment effects estimated on the full sample of workers in Denmark may not capture the workplace quality actually experienced by foreign-born individuals, particularly refugees. This could arise from native-immigrant differences in bargaining power, available outside options, firm-specific labor supply elasticities, or reservation wages (Adda, Dustmann, and Görlach, 2022; Arellano-Bover and San, 2023; Dustmann, Ku, and Surovtseva, 2024a). While we find some evidence of within-firm differentiation, Appendix C shows that it does not threaten our estimates, as the workplace quality ranking is broadly similar for natives and immigrants. We therefore proceed to use the workplace effects estimated in equation 1, based on the full sample of natives and non-refugee immigrants, in the remainder of our analysis.

5 Stylized Facts

In this section, we document three novel stylized facts about refugees and their employers that motivate our empirical analysis. While these relationships may not be interpreted as causal, they are important to establish given the limited existing evidence on this group. To do so, we use the proxy for employer quality estimated with the AKM decomposition in the previous section and apply it to our refugee sample.

Fact 1. First, we examine the relationship between establishment quality and the labor market outcomes of refugees in our sample. Table 4 presents the correlation between the first employer's fixed effect and refugees' individual employment (columns 1-3) and annual earnings (columns 4-6). All panels include individual controls, but while Panel A includes the main set of fixed effects we use in the analysis, Panel B does not include any. Panel C employs the same specification of Panel A but on a smaller sample of refugee movers.

Two clear results emerge. First, the quality of the initial workplace is strongly correlated with employment probability and annual earnings of refugees in the short run (1–5 years), medium run (6–10 years), and long run (11–15 years). A one standard deviation increase in the pay premium offered by the first employer is linked to a higher probability of being employed by 3.6 percentage points, and to a higher yearly earnings by 3441 USD in the short run (Panel A). Second, the persistence of the effect up to 10 to 15 years after the beginning of the first employment spell is remarkable. Varying the fixed-effects specification leaves the magnitude and significance of the estimates unchanged, as does restricting the sample to a subset of refugees. Importantly, in Panel C we focus on refugees who had at least one transition between employers to rule out the possibility that these persistent effects are mechanically due to refugees staying with the same employer throughout the period, finding only small decreases in the magnitudes. While there is no causality in these regressions, as sorting of more productive workers to better firms is not completely accounted for, this descriptive evidence suggests that initial workplace quality is directly connected to refugees' outcomes.

Table 5 confirms that this long-run correlation also holds for refugee wages, the outcome typically used in AKM decompositions.[35] This result carries important implications for using two-way fixed effects models that include person and employer effects on specific subgroups, such as refugees and potentially other immigrants. A time-dependence effect suggesting that individual productivity is shaped by the first employer fixed effect would directly violate the fundamental assumptions of the AKM model. Specifically, the AKM framework views wage determination as a fundamentally static process, assuming that worker and firm effects enter the equation additively and separately. Not only does this framework ignore dynamic effects, but it cannot reliably estimate person and employer effects if past match quality has a lasting impact on an individual's future earnings capacity.[36]

Our observation is similar in spirit to some recent works. Arellano-Bover (2024) and Arellano-Bover and Saltiel (2024) find evidence of dynamic effects from starting in large firms or firms with greater on-the-job learning opportunities, and even earlier, De La Roca and Puga (2017) documented for Spain that young workers who spend time in larger cities earn more later in their career. A way to account for that dynamic is discussed in Card et al. (2025), where the authors estimate augmented AKM specifications that incorporate dynamic effects of spending time in large, high-wage US commuting zones.[37] However, the source of dynamic effects that we describe, arising from the fixed effect of past employers, cannot be easily accounted for with an augmented static AKM specification. Relatedly, Bonhomme et al. (2019) document an effect of the previous employer on workers' current earnings, conditional on the type of the current firm, using an alternative two-step empirical approach in which firms are grouped into classes via k-means clustering. By contrast, here we highlight a departure from AKM assumptions while remaining within the AKM framework, emphasizing the dynamics triggered by first employers rather than previous ones. Our evidence points to mechanisms

[35] We run the same regressions on a larger sample of refugees who arrived in Denmark during this period and find that the observed patterns are not driven by our restrictions of the analysis sample (see Table B4 in the Appendix).

[36] Rather than reflecting improvements in earnings capacity, the persistent first-employer effect could arise from network channels. Refugees who start at high-quality employers may do so because of stronger initial connections, and when they switch firms those connections steer them toward other high-quality employers. Hence, we re-estimate the same main specifications of Tables 4 and 5 on refugees who ever moved away from their initial municipality, and thus their initial network, and again find unchanged results (Table B5). While the precise mechanism is hard to pin down, this additional restriction suggest that the persistent first-employer effect cannot be fully driven by patterns induced by network connections either.

[37] They note that accounting for these effects has very little impact on their estimates of place effects in the aggregate, and therefore carry out most of their analysis with a simpler model without dynamics.

more consistent with human capital development than with an offer–counteroffer mechanism in the spirit of Postel–Vinay and Robin (2002), and pertains to a limited foreign-born group.

Importantly, Table 5 does not reveal the same dynamic pattern for a comparable sample of native labor market entrants, confirming the validity of our approach. On the one hand, this reassures us that our controls and fixed effects help ensure that our estimates are not solely capturing sorting on unobservable skills. If sorting was driving our results for refugees, then it would imply that natives are not sorting in the same way. This seems implausible because, if anything, we would expect natives to be able to sort more easily and effectively in the labor market.[38] On the other hand, this discrepancy provides further justification, beyond the rationale of avoiding mechanical effects and the arguments discussed in Appendix C, for estimating employer fixed effects using a job spell dataset that includes only natives and non-refugee immigrants, as we do in Section 4.1. We further rule out sorting as the sole driver of these estimates by estimating, in a separate AKM decomposition, individual fixed effects for refugees and then plotting them against their first employer fixed effects. Figure 6 shows no evidence of positive assortative matching.[39]

We interpret this difference in the role of the first employer as evidence of a novel and additional aspect that differentiates refugees from other workers. For refugees, the first workplace represents a critical point of entry into a new labor market. It carries greater potential for learning, acquiring context-specific human capital, and building professional connections. A better first employer may also buffer against negative initial shocks and offer stability in early years at destination. In contrast, natives possess local skills, face fewer barriers, can move freely, know the labor market better, including how and where to search for other jobs. As a result, while a large literature has emphasized the importance of entry labor market conditions for workers in general, the long-term consequences of an initial job match are likely to be even more consequential for refugees than for natives.

Fact 2. We then explore the stickiness of initial employer quality for refugees. Figure 7 plots the evolution of firm pay premia earned by quartile of the first employer quality distribution, separately for refugees and a comparable set of natives entering the labor market for the first time. We draw two key insights from this chart. First, refugees do not move up and down the firm ladder as easily as natives, whose average pay premium by quartile group converge rapidly over the first five years since the beginning of the first job spell. Second, this pronounced stickiness in initial quality for refugees appears to persist, as a sizeable gap remains open at years 10 and 15.

This is unsurprising given extensive evidence documenting the numerous obstacles refugees face, as well as the persistent labor market gaps between refugees and other groups of workers in host countries (Bratsberg et al., 2017; Schultz-Nielsen, 2017; Brell et al., 2020; Fasani et al., 2022). At the same time, these findings underscore the importance of the initial conditions refugees encounter upon arrival, including the quality of accessible workplaces. Our results align with Åslund et al. (2025), who show that immigrants in Sweden are less likely than na-

[38] Refugees' skill signals are likely much noisier at arrival, and their opportunities for sorting into better jobs are more limited.

[39] In fact, Figure 6 reveals some degree of negative assortative matching for refugees, consistent with the hypothesis that they face greater difficulty signaling their skills upon entering the host labor market.

tives to climb the productivity ladder and transition across firms. We confirm a similar pattern for refugees, whose mobility is particularly constrained.

Fact 3. Lastly, we assess the role of social connections in shaping job search outcomes for refugees. Specifically, we aim to determine whether a connection to a firm influences the probability that a refugee worker is hired by that firm. We begin by defining a connection as a co-national residing in the same municipality to which the refugee is initially assigned and employed at a given establishment when the refugee arrives. Following the approach of Eliason, Hensvik, Kramarz, and Skans (2023) and Åslund, Engdahl, and Willis (2024), we construct a dataset of refugee-establishment dyads, pairing each refugee i with every establishment j active in their municipality of assignment at the time of arrival. We then use the following specification to recover the effect of being connected to establishment j through a member of the local co-ethnic network at arrival on the probability of being hired by j:

$$[Pr(\text{Hired by } j)]_{ij} = \alpha + \beta D_{ij} + X_i'\gamma + \lambda_j + \epsilon_{ij} \tag{2}$$

where the outcome variable is an indicator for whether refugee worker i is employed at establishment j at any point in their working career, multiplied by 100, and D_{ij} is a dummy variable taking value 1 if refugee i has a connection at j. Importantly, we restrict our sample to dyads where establishment j has hired at least one refugee with a connection there and at least one without. This restriction eliminates pairs with no variation in the existence of a connection, making the cardinality of the sample tractable.

Table 6 reports estimates of parameter β using different sets of controls and fixed effects.[40] In column 1, we include a set of individual controls consisting of variables observed by Danish authorities when assigning refugees to municipalities as part of the dispersal policy. In column 2, we add education controls. In column 3, we also include establishment fixed effects to focus on within-establishment variation in connectedness, effectively controlling for factors such as differing hiring strategies across establishments. Column 4 presents the most restrictive specification, including establishment-by-cohort fixed effects to account for different hiring situations at the same establishment faced by different cohorts of refugees. Our preferred specification (column 3) indicates that having a local co-ethnic connection at arrival increases the probability of being hired at a given establishment by 0.06 percentage points for newly arrived refugees.

Overall, we interpret the evidence discussed in this section as suggesting that establishment quality is consequential for refugees' labor market outcomes, and that initial conditions influence their subsequent transitions across workplaces. While refugees move along the employer ladder, they do so sluggishly, relying on ethnic networks upon arrival when searching for a job. We will explore these dynamics more formally in the next sections.

[40] Regardless of the specification employed, our estimates likely represent a lower bound, as we do not exclude refugees who relocate to a municipality different from their initial assignment. While this relocation choice is endogenous, it mechanically reduces the likelihood that, after relocating, they will work at an establishment where they had a connection.

6 Empirical Approach and Identification

The main aim of this paper is to estimate the effect of employer quality on refugees' labor market outcomes. Ideally, one would randomly assign identical refugees to firms of varying quality upon arrival and track their subsequent integration. However, such an experiment is not feasible in practice. As a consequence, sorting across locations and employers complicates drawing causal conclusions, and the evidence presented in Section 5 cannot completely rule out the possibility that unobserved productivity drives matching.

In this section, we outline the empirical strategy used to circumvent this issue and estimate instead the effect of the quality of refugees' first accessible employers on their outcomes. To do so, it is important to address this question in Denmark, where we can rely on exogenous variation in refugees' exposure to employers. Specifically, we leverage the implementation of the 1986–1998 dispersal policy, which effectively randomized the allocation of refugees across Danish municipalities.

For this approach to credibly address sorting, avoiding that newly arrived migrants choose their residential location based on individual skills that also affect their outcomes, we require the identifying assumption that the placement policy is independent of unobserved individual characteristics. As discussed in Section 2, Table 1 presents a test of the conditional random assignment of refugees to municipalities. Family structure and country of origin are, by design, systematically correlated with initial location characteristics, since placement was conditional on them. Crucially, however, education at arrival, likely related to unobserved skills, does not appear to be correlated with municipality characteristics. This supports the assumption that individual refugee characteristics are independent of initial location characteristics, conditional on observables available to the authorities, which we control for throughout our analysis.

This randomization allows us to obtain quasi-experimental evidence on the impact of exposure to different employers.[41] While individual refugee-employer matches are not strictly random, the dispersal policy ensures that refugees were quasi-randomly exposed to the set of active establishments in their assigned municipality upon arrival, as well as to those that hired members of the co-national network they initially joined. After estimating AKM-based establishment fixed effects as described in Section 4, which serve as our proxy for establishment quality, we then proceed to aggregate them at three different levels of exposure, progressively refining our definition to identify the most relevant employers for newly arrived refugees.

First, we investigate the effect of "pure" geographic exposure by averaging individual establishment effects, ψ_j, across all active establishments in the municipality of assignment at the time of arrival.[42] Second, we assess the role of employer quality by focusing only on establishments in the municipality of assignment that, at the time of arrival, were either employing

[41] Other studies have exploited variation at highly disaggregated geographical levels, such as residential housing blocks, under the assumption of no endogenous sorting at the block level within neighborhoods (Boeri, De Philippis, Patacchini, and Pellizzari, 2015; Schmutte, 2015). However, the policy experiment created by spatial dispersal policies represents the methodologically most rigorous approach (Schüller and Chakraborty, 2022).

[42] Importantly, as showed in Card et al. (2025), AKM-based aggregations used to define location effects avoid bias from selective mobility that attenuated place effect estimates in earlier studies. Specifically, in studies with two-way fixed effects models that include person and place effects, patterns of selective mobility between establishments at different levels of the local job ladder for geographic movers introduce bias in place effects.

or had previously employed at least one co-ethnic. Finally, we estimate the effects of an exposure measure based on averaging individual establishment effects in the municipality of assignment for all establishments that, at the time of arrival, were employing at least one local co-ethnic network member. To address remaining endogeneity concerns, we construct these measures one year prior to the admission year of each newly arrived refugee.

Depending on the level of exposure considered, we use the function f to average individual establishment effects, $\hat{\psi}_j$, across these three different sets of establishments. While we will explore all three levels, we emphasize and use the last as our preferred measure of exposure, as it captures the effects of having co-ethnic network links to high-paying local firms at arrival. Thus, our main specification estimates the effect of exposure to employer quality at arrival on refugees' labor market outcomes as follows:

$$y_{ioc,t} = \alpha + \beta_1 f[\hat{\psi}_j]_{om,c-1} + \beta_2 Sh.Conat._{om,c} + \beta_3 Emp.NW_{m,c} + \boldsymbol{X}_{i,t} + \gamma_m + \delta_{o,c} + \epsilon_{ioc,t} \tag{3}$$

where $y_{ioc,t}$ denotes either employment probability or real annual earnings for refugee i from country of origin o at time t, who arrived in Denmark in year c (cohort) and was assigned to municipality m. Here, $f[\hat{\psi} j]_{om,c-1} \equiv [\bar{\hat{\psi}}_j]_{om,c-1}$ captures the average quality of establishments in municipality m employing other migrants from country o, one year prior the arrival (i.e., $c-1$) of refugee i.[43] The parameter of interest is β_1, which, given the research design, can be interpreted causally when estimated by OLS. To facilitate interpretation, we divide our main regressor by its standard deviation. To account for the fact that the average of $\hat{\psi} j$ is a generated regressor, we will also report p-values after bootstrapping standard errors (as in Bana, Bedard, Rossin-Slater, and Stearns (2023)).[44]

Equation 3 includes controls for the population share of co-nationals in the municipality of assignment at arrival, $Sh.Conat._{om,c}$, and for the employment rate of non-Western immigrants, $Emp.NW_{m,c}$. The former controls for country o's comparative advantage in m, which can influence geographic sorting by generating a greater inflow of co-nationals. The latter allows to control for broadly-defined local economic characteristics favoring non-Western migrants integration in general. Additionally, the vector $X_{i,t}$ contains individual background characteristics in the year of assignment, including those observed by the authorities during the dispersal process.[45]

Our preferred specification includes municipality of assignment fixed effects, γ_m, which account for time-constant differences in economic advantages across municipalities, effectively capturing location m's absolute advantage for all refugees. Municipality fixed effects are particularly important in our setting as the location of high-paying employers is not random but likely linked to other location-specific factors. We also include origin-by-cohort fixed effects, $\delta_{o,c}$, to control for selection along unobservable factors in inflows from origin o. Hence,

[43] Had we focused on the impact of geographic quality by averaging establishment effects of all establishments active in the municipality of assignment at the time of arrival, the main regressor in equation 3 would have been $f[\hat{\psi} j]_{m,c-1}$.

[44] We adopt a wild cluster bootstrap procedure, which perform better than traditional simpler bootstrap procedures (Cameron and Trivedi, 2022), adopting standard Rademacher weights and bootstrapping standard errors 9999 times.

[45] More specifically, we include age at entry (quadratic and cubic terms as well) and a dummy for gender, for being married, for having kids (in two age ranges), for having the spouse in Denmark.

we exploit both variation in employer quality for refugees from the same origin and cohort but assigned to different municipality (cross-municipality variation), and variation in quality for different origin-cohort refugee groups within the same municipality (within-municipality variation).[46] Robust standard errors are clustered at the municipality of assignment level throughout the analysis.

Our estimand should be interpreted as capturing *intention-to-treat effects*. We estimate the average impact of being exposed through the network to high-quality firms, regardless of whether individuals ultimately work in those firms or relocate after the initial assignment. In this sense, refugee movers do not pose a threat to our approach. On the contrary, mobility that depends on the initial quality of employers is itself an outcome that we can and will analyze. Still, as discussed in Section 2, the dispersal policy was consequential for refugees' settlement patterns: after 8 years, more than 50% remained in their municipality of assignment, and after 15 years, more than 40%.

In Figure B2, we explore the raw variation in our main treatment variable, the average accessible employer quality measured by the AKM establishment effects in the co-ethnic network, in the analysis sample.[47] The standard deviation of 0.11 log points roughly corresponds to an 11% wage gain linked to better employers' pay policies when moving from a network at the 25th percentile to one at the 75th percentile of the quality distribution.[48] This amount of variation is slightly lower but comparable to the standard deviation of U.S. location wage premia, measured by averaging establishment effects at the commuting zone level, which is 0.15 log points (Card et al., 2025).

Since municipality characteristics are often correlated, and our main network regressor is constructed by aggregating establishment pay premia, Table B7 explores the unconditional relationship between establishment-based network quality and other local characteristics. This helps assess the extent to which our measure captures a distinct dimension not already reflected in existing observable characteristics of municipalities. Reassuringly, network quality, averaged over countries of origin and years within each municipality for the purpose of this table, is not perfectly correlated with any single characteristic, and the signs of the observed correlations are broadly in line with expectations.

7 Results

7.1 Employment and earnings

Our main results derive from the estimation of equation 3 at the three different levels described in Section 6. Panel A in Table 7 shows the effects of geographic exposure to employers of differing quality on employment and earnings for our sample of refugees. Specifically, the table indicates that assignment to a municipality with higher-quality employers leads to a modest reduction in the probability of employment and earnings in the short run (1–5 years

[46] The quality of employers is identified because it varies across origin-by-cohort groups within municipalities.

[47] Table B6 also reports the residual variation after partialling out the portion of the treatment variable explained by the main set of fixed effects used in the analysis.

[48] Figure B3 shows that each of the three dimensions of the local co-ethnic network (municipality, year of arrival, origin country) contributes to the overall variation in quality.

after arrival). Ceteris paribus, being surrounded by high-paying firms seems to be detrimental immediately after arrival, as these establishments might be more difficult to access. Our estimates reveal no significant effects on employment or earnings in the medium run (6–10 years) or the long run (11–15 years) after arrival.

Since coefficients in Panel A are estimated using the full set of fixed effects from equation 3, employed also in Panel B and Panel C, we repeat these regressions focused on geographic effects by including only cohort fixed effects.[49] Table B8 in the Appendix reports results from this different and less restrictive specification, confirming the short-run negative effect found in Panel A from the main more saturated model.

In Panel B of Table 7, we present estimates of the effect of exposure to higher-quality employers who had previously employed, or were employing, at least one co-national at the time of arrival. We view this measure of exposure as a refinement of the geographic measure in Panel A, as these employers have already demonstrated a willingness to hire refugees, making them a better proxy for the set of first accessible employers. The estimates indicate a positive effect on employment and a weaker, less significant effect on income in the medium run (6–10 years after arrival), with no effects observed for either outcome during the 1–5 year or 11–15 year periods after arrival.

In Panel C in Table 7, we present our preferred estimates, which reflect the effects of exposure to higher-quality employers that employ co-nationals at the time of arrival. The results show positive, statistically significant effects on employment and earnings in the short run (1–5 years after arrival) and the medium run (6–10 years after arrival).[50] A one standard deviation increase in the average workplace pay premia earned by the local co-ethnic network at arrival leads to a higher probability of being employed by 0.8 percentage points in the short run and by 1.3 in the medium run, and to higher yearly earnings by 231 USD in the short run and 558 USD in the medium run. The effect remains positive in the long run (11–15 years after arrival) but without being statistically significant. These magnitudes are economically meaningful: for both employment probability and annual earnings, the effect size in the first five years corresponds to a 7.5% increase relative to the sample mean. Section 7.2 further discusses these magnitudes in relation to the existing literature.

Our main results show that having network links to high-paying firms benefits the labor market integration of refugees for up to ten years after arrival. As our measure of employment quality is refined to incorporate co-national network links, the effects of exposure shift from being negative to positive and from detectable only in the short run to detectable in both the short and medium run. This suggests that geographical exposure to higher-quality employers alone does not drive the labor market integration of refugees; rather, it is exposure to high-quality employers that employ co-nationals that provides these benefits. Furthermore, since the positive effects are larger for employers who employed co-nationals at the time of arrival, compared to those that employed co-nationals at any point before arrival, this indicates that the benefits are driven by contemporaneous links to high-quality employers rather than by

[49] Therefore, we are effectively comparing two refugees arrived in the same year and assigned to two different municipalities.

[50] Considering the p-values from the bootstrapping procedure, our estimates for the medium run are significant only at the 10% level.

employers' general willingness to hire refugees of a given nationality. In Section 8, we examine the specific network mechanisms that drive our main results.

Interestingly, comparing Panel C of Table 7 with the potentially endogenous estimates of Table 4, we can see that the effects are much smaller in magnitude. Two explanations are plausible. First, estimates from the initial exercise considering the actual first employer may include some upward bias, possibly through match-specific components between refugee workers and establishments that are not fully accounted for. While we did not find evidence supporting positive assortative matching between refugees and their first employers (see Figure 6), we cannot completely rule this possibility out. Second, the discrepancy may arise because of a difference in estimands: the first-employer specification captures a direct effect that accrues immediately upon employment for those hired, whereas our main estimates are intention-to-treat effects of network quality, measuring the average impact of exposure to higher-quality employers, which are estimated on a broader sample and may or may not materialize for each individual.

It is also important to note that, while virtually all refugees in our sample are assigned a measure of geographic exposure to employer quality, this is not the case when the measure is refined to account for the average quality of firms currently employing network members. Refugees assigned to municipalities without co-nationals do not receive an exposure measure, nor do those assigned to municipalities where co-nationals work for establishments outside the largest connected set, where a fixed effect could not be estimated. Table 8 reports the differences in characteristics between refugees matched with a network measure and those unmatched. As expected, these two groups differ along dimensions related to the likelihood of arriving in a location and year with an established network. Refugees in the matched group are more likely to reside in municipalities with a higher number of co-nationals and in larger, more educated, and more urbanized municipalities. The two groups also differ in some individual characteristics, all of which are important to consider when generalizing our results.[51]

In this light, the estimates from Panel C of Table 7 can be interpreted as the effect of firm quality within the network on refugees' outcomes *conditional* on having a measurable network measure. In Table B9, we compare outcomes for individuals with no observable network to those with networks of varying firm quality, split in terciles of the network distribution. This evidence suggests that medium- and high-quality networks in terms of employers are more beneficial than either having a low-quality network or no network at all, particularly for employment probabilities. In line with economic intuition, having a low-quality network or having no network seem to have comparable effects on labor market outcomes, likely reflecting some form of free disposal.

Finally, we examine whether the effect found on earnings is driven by higher earnings among those employed or by higher employment probability. Specifically, we want to make sure that higher firm quality in the network boosts earnings not only by bringing more refugees into work, but also through better jobs, higher wages, and/or more hours of work.[52] In Table

[51] In particular, refugees in the matched group are more likely to be female, less likely to come from predominantly Muslim countries, and less likely to be native speakers of Latin languages. They also tend to have higher initial labor income and are more likely to be employed in jobs with a higher complexity index.

[52] We do not observe hours of work.

B10, we estimate the effect on annual earnings conditional on refugees reporting positive earnings, expressed in thousands in columns 1 to 3 and in logs in columns 4 to 6. The positive and statistically significant pattern is confirmed for the short and medium run, particularly when using a less restrictive set of fixed effects to address the smaller sample size resulting from this restriction. The fact that the coefficient magnitudes did not decline when conditioning on positive earnings indicates that the increase in earnings documented in our main results is driven disproportionately by the intensive margin.

Influencing refugees' employment and income can generate spillover effects beyond the labor market, shaping social integration more broadly. One such dimension of integration is, for instance, crime outcomes for refugees and their families. As this is not the focus of our analysis, we leave this aspect for Appendix D.

7.2 Comparison with prior studies

Given the extensive literature that has studied the impact of various integration policies and differing initial conditions on refugees, it is important to frame our estimates within that context. Our analysis speaks more directly to the work on the role of initial labor market conditions for integration. This literature has mostly focused on two main variables, one capturing the strength of the labor market and the other capturing the availability of support of co-nationals, finding significant effects for both (e.g., Edin et al. (2003); Damm (2009a); Damm and Rosholm (2010); Azlor et al. (2020)). When analyzed together in Denmark, higher employment rates in the host local labor market, but not larger networks of co-nationals, are associated with better employment and earnings outcomes for refugees, in both the short run and the long run (Foged et al., 2024; Damm, 2014).

Unlike most of this literature, we do not focus on the coefficients of these variables, which we include as controls, and their lack of statistical significance does not contradict or invalidate earlier studies. By introducing municipality fixed effects in our main specification, we absorb part of the effects estimated in previous work, which, even though best seen as municipality-by-year of arrival effects, tend to be relatively stable over time. As a result, our findings should not be thought of as originating from general differences across municipalities; instead, they can be seen as being "on top of" most municipality effects documented by the literature, and thus complementing it by adding the employer dimension to the analysis.[53]

Specifically, we present evidence that efforts to promote labor market integration should not focus only on obtaining a job, as much of the existing literature has emphasized, given refugees' typically low labor market attachment (Brell et al., 2020). We show that the *quality of the employer* offering that job plays a crucial role. High-quality early employers not only increase the likelihood of employment but also shape its trajectory over time. In other words, improving refugees' integration requires attention not just to access to employment, but to the quality of the employers providing it. This idea complements the finding by Degenhardt and

[53]The early literature on the effects of living in co-ethnic enclaves on individual earnings (Edin et al., 2003; Damm, 2009a) also included municipality fixed effects. However, unlike our approach and the more recent dispersal policy literature discussed here, it estimated a contemporaneous relationship between ethnic concentration and individual earnings, rather than focusing on conditions at arrival, even though the importance of cumulative effects was already documented in Edin et al. (2003).

Nimczik (2025) that access to gig jobs in online food delivery initially accelerates job finding among refugees in Austria, but that refugee gig workers tend to remain in low-paid, unstable jobs with limited career prospects.

As for more general comparisons with integration policies, the language training policy of 1999 implemented in Denmark, which provided 400 extra hours of training to refugees, is considered the most effective intervention for refugees' employment probability and earnings in the long run (Foged et al., 2024). Relative to this policy, we observe that the magnitude of our main effects is meaningful but not huge. Our estimates are about one-seventh of the language reform effect in the short run and about one-fourth in the medium run. Nevertheless, we see the effects we find in the first 10 years since migration as important, given that the early years after arrival are a crucial period for refugees' integration (Brell et al., 2020).

Our results also align with the AKM literature on the role of firms in wage inequality, which attributes a substantial share of earnings gaps between groups to between-firm sorting (e.g., the gender gap in Card et al. (2016) and the native-immigrant gap in Dostie et al. (2023)). As discussed, our estimates should be treated as intention-to-treat effects, capturing the average impact of being exposed, through the network, to high-quality firms. This approach is appealing because it allows us to draw causal conclusions, but it limits direct comparability with AKM studies that focus on the consequences of direct employment in different firms without quasi-experimental designs. Still, Åslund et al. (2025), using a job ladder model based on a firm productivity grouping, estimate that working in the fifth decile of the firm productivity distribution yields earnings returns of 8.5 log points for natives and 12.1 log points for immigrants in Sweden, with the differential returns driven primarily by non-Western immigrants. While not identical in interpretation, our earnings effects are in the same ballpark. This proximity highlights that network-provided access to better firms can have economically important consequences, particularly for non-Western immigrants, who are precisely the focus of our study.

7.3 Decomposing network quality: employer or employee quality?

Our causal estimates indicate that refugees' labor market outcomes improve with the quality of employers accessible through their network. However, this measure may not fully isolate the role of firms if the employer quality in the network partly reflects the quality of network members employed there.[54] As a result, high firm quality in the network may also capture correlated member quality, which is itself a potential driver of individual labor market outcomes. Put differently, well-established and high-achieving ethnic peers can act as role models, potentially motivating newly arrived immigrants to invest in skill acquisition.[55]

Addressing this concern allows for a simple decomposition of the network quality effect. Previous studies on networks have documented that the returns to living in an ethnic en-

[54] Even though our AKM-based measure of firm quality by construction purges establishment fixed effects from worker sorting in the full economy, it does not rule out nonrandom sorting of the specific subset of workers in an individual's network.

[55] Examples of positive human capital externalities are documented in Åslund, Edin, Fredriksson, and Grönqvist (2011) and Chakraborty, Schüller, and Zimmermann (2019); negative effects detrimental to success in Borjas (1995), Cutler and Glaeser (1997), and Bertrand, Luttmer, and Mullainathan (2000).

clave increase with enclave quality, most commonly proxied by average annual earnings in the group, but also by alternative measures such as the share of self-employed or the share of highly educated (e.g., Edin et al. (2003); Damm (2009a)). In line with a linear fixed-effects model for individual earnings, the average earnings in network k at any given time can be expressed in terms of members' earnings capacity and the firm pay premia of their employers as follows:

$$\overline{log(y)}_k = \frac{1}{N_k}\sum_{i\in N_k} log(y_i) = \underbrace{\frac{1}{N_k}\sum_{i\in N_k}\alpha_i}_{\text{average member ability}} + \underbrace{\sum_{j\in J}\pi_{kj}\psi_j}_{\text{average firm pay premium}} + \overline{\mathbf{X}}'_k\beta + \nu_k \tag{4}$$

where i indexes individual members, N_k is the total number of members in the network, α_i is individual ability, ψ_j is establishment j's pay premium, π_{kj} are exposure shares to establishments with $\sum_j \pi_{kj} = 1$, and ν_k captures deviations due to estimation error and model misspecification. Hence, including both the average member ability and the average firm pay premia terms in our main specification allows for a cleaner assessment of the role of firms, while also contributing to the network literature by decomposing the aggregate measure of network quality.

We do so by constructing two network-level controls that capture the quality of network members. First, by including refugees into the same job spell dataset used in Section 4.1 and re-running the AKM decomposition over the dispersal policy period, we estimate a vector of individual fixed effects (α_i) that proxy for unobserved permanent ability related to earnings potential. We then average these coefficients within each network to create a measure of member quality. Second, we compute the share of network members with academic education prior to arrival, providing an alternative and more traditional measure. Each of these controls is included separately in our main specification 3.

Table 9 presents the results of this decomposition exercise. Panel A reports estimates for the effect of aggregate quality, using average real annual earnings in log in the network.[56] The coefficients are small, positive, but not statistically significant. Panels B to D include controls for average member quality (AKM-based individual effects in Panels B and C, and share of highly educated members in Panel D). While the inclusion of these controls reduces statistical significance in the medium run in some specifications, the magnitudes of the average firm quality coefficients remain positive and stable, while coefficients on average member quality are small, negative and insignificant. This suggests that our establishment-based network quality measure primarily reflects firm quality rather than the sorting of high-ability individuals, so that the positive impact of network quality previously documented in this setting is driven by connections to better-paying firms.

More broadly, while disentangling the relative importance of different channels behind the

[56] Edin et al. (2003) and Damm (2009a) examine the role of enclave quality using 2SLS specifications where the main effect of the quality measure was not separately identified from country-of-origin fixed effects. In their setting, the variables of interest were interactions between quality and ethnic concentration. In contrast, we can include and estimate the main effect directly in our design. Focusing on neighborhoods within municipalities, Damm (2014) estimates the main effect of quality of co-nationals, defined as the employment rate of men aged 18–60, their mean years of education, and log mean real annual earnings. Results for the latter, however, are not reported.

positive effects of co-ethnic enclaves is empirically challenging, these results suggest that our measure of firm quality primarily captures the opportunity-augmenting role of the network. Therefore, consistent with the idea of network providing a "warm embrace" (Borjas, 2000), the causal effects we documented previously seem to reflect social interactions as friction-reducing mechanisms in the job-search process, rather than human capital externalities. Put differently, networks are helpful insofar as they connect new members to strong labor demand; while high-quality members may provide that access, they may also generate stronger competition, so that ultimately employer quality matters more than employee quality. We will expand on this point in our discussion of mechanisms in Section 8.

7.4 Robustness

We perform several tests to ensure the robustness of our main results for refugees' employment and earnings. These checks are briefly summarized below, and the corresponding evidence is presented in Appendix E.

7.4.1 Aggregation of establishment quality

We begin by exploring alternative constructions of the network measure of firm quality, replacing the simple average used in equation 3 with different functions f to aggregate individual establishment effects $\hat{\psi}_j$ within networks. This allows us to assess whether our results depend on a particular aggregation choice. First, we consider a weighted average to aggregate establishment effects, where the weights are full-time equivalents at each establishment, reflecting the importance of employers based on their size and the ratio of part-time to full-time employment. We re-estimate equation 3 and report the results in Table E1. Panel A examines geographic exposure to firms, using the weighted average of all employers in the municipality, while Panel B instead considers network-level exposure, using the weighted average of local employers that employ co-nationals at the time of arrival. For this latter measure, estimates remain essentially unchanged in magnitude compared to those in Panel C of Table 7 and, if anything, become statistically significant also when considering the bootstrap p-values in the medium run.

Second, instead of averaging establishment fixed effects, we test the robustness of our network-based estimates by assigning a dummy variable equal to one for establishments in the top quartile of the Danish workplace pay premium distribution, and then calculating the average of this dummy within each network. This approach yields the proportion of top employers in the network. The estimates obtained using this alternative measure are reported in Panel C of Table E1 and confirm the pattern in the sign and significance of our main results. However, their magnitudes are larger, which we attribute to the emphasis on top employers in this specification.

7.4.2 Specifications and sample restrictions

We also estimate alternative specifications of equation 3. We begin by including both our geographic and network-level measures of exposure to firm quality, aiming to isolate their independent effects. The coefficients of interest for network quality, reported in Table E2, remain virtually unchanged in magnitude, sign, and statistical significance compared to the separate estimation. This suggests a significant degree of orthogonality between the effects of geographic and network-based quality measures on refugees' employment and earnings. This result is unsurprising in light of Figure 8, which shows that municipalities with higher average establishment quality do not necessarily coincide with those having higher average network-based establishment quality. The stability of the coefficients on network-based quality even after controlling for location quality reinforces our confidence that social connections play a primary role in facilitating refugee integration through access to better-quality employers.

One potential concern is that our results could be driven by the fact that establishments employing co-ethnics are those that provide particular types of jobs that network members are more likely to obtain, independent of the role of social connections in facilitating access. To address this possibility, we use industry information for each establishment to classify jobs held by employed network members into one of nine industry groups.[57] We then repeat our main analysis, controlling for the industry shares of the network, and report the estimates in Panel B of Table E2. Our main results remain very similar.

Next, we perform a falsification test by restricting our sample to refugees who relocate to a municipality different from their assigned one within their first five years after arrival. For this subset of refugees, whom we refer to as "early movers", we expect no effect, as networks, by construction, can only provide valuable connections to good firms in the municipality of assignment.[58] Consistently, the estimates for employment and earnings in the early movers sample, reported in Panel C of Table E2, are substantially smaller in magnitude and statistically insignificant compared to our main results.

Finally, we check the sensitivity of our results to sample restrictions. Estimates reported in Table E3 are obtained by trimming the top or bottom 10% of observations in our refugee sample based on size of co-ethnic network at arrival (Panel A), population in the municipality of assignment (Panel B), and municipality of assignment's share of national full-time equivalents (Panel C). Coefficients remain statistically significant and stable in magnitude across these restrictions, confirming that our results are robust to changes in sample selection and hold more broadly within the population of interest, rather than being driven by networks or municipalities of extreme sizes.[59]

7.4.3 Measurement of establishment quality

The accuracy of our estimates of individual establishment effects is crucial for our empirical exercise. As shown in Tables B2 and B3, these estimates are not purely driven by noise,

[57]The nine NACE industry categories are: Agriculture, fishing, and quarrying; Manufacturing; Electricity, gas, and water supply; Construction; Wholesale and retail trade, hotels, and restaurants; Transport, storage, and communication; Financial intermediation and business activities; Public and personal services; and Activity not stated. Unfortunately, we cannot explore the role of occupations of foreign-born workers, as occupation codes are unavailable in our data prior to 1991.

[58]The average duration of residence in the municipality of initial assignment for this group is 2.7 years. This duration is short enough that we do not expect good connections to have an impact.

[59]In Figure B4, we further rule out the possibility that our results are driven solely by the largest municipality of assignment.

as they display a positive and statistically significant correlation with other observable measures of establishment productivity and desirability, in line with the existing literature (Kline, 2024). Building on this, we introduce split-sample instrumental variable (IV) estimates to correct for measurement error that results from the well-known "limited mobility bias" in AKM model estimates (Kline, Saggio, and Sølvsten, 2020; Bonhomme et al., 2019).[60] In practice, we randomly split the sample of workers used in the estimation of equation 1 into two equal sized samples (keeping person histories together), calculate two sets of AKM estimates separately in both subsamples, aggregate establishment effects at the network level, and then use the network-level quality from one set of establishment effects as instrumental variable for the other in the same specification as equation 3.[61] Table E4 shows that the IV estimates are comparable in magnitude to our main results, indicating that our findings are robust to this correction.

7.4.4 Origin-permutation test

Another potential concern with our network-based measure of quality is that it may capture local confounding variables that make a municipality generally advantageous for refugees. If this were the case, we would expect the effects associated with the co-ethnic network not to differ from those of a network including all refugees. To assess whether co-ethnic network effects drive our findings, rather than other hidden local characteristics related to the treatment variable, we perform a permutation-based pseudo-placebo test.[62] Specifically, we run 1,000 replications of equation 3, each time assigning the network quality of one of the other eight origins to dispersed refugees from a given country of origin.[63] By permuting the treatment across countries of origin, we generate a distribution of placebo-like effects under the null hypothesis that the treatment has an attenuated effect. We repeat this procedure for each outcome for which we found an effect in our preferred specification (employment over 1–5 years after arrival, employment 6–10 years, earnings 1–5 years, and earnings 6–10 years) and plot the resulting distributions in Figures 9 and 10. In every case, the true effect estimated in Panel C of Table 7, depicted by the black solid line, is more extreme than the 95th percentile of its respective placebo-like distribution. This test confirms that the relevant connections come from the co-ethnic network rather than from an aggregate network of all refugees.

[60] Establishment effects are only identified through worker mobility. When the number of movers per establishment is small, the estimated effects in equation 1 can be noisy, leading to attenuation bias in our main estimates from equation 3. Since the bias is particularly problematic in short panels and in presences of small employers, we may be already partially insured against this issue: our panel spans a relatively long period (1986–1998), we aggregate single establishment effect at the network level, and mobility rates are reasonably high (Table 3).

[61] Similar strategies are implemented in Goldschmidt and Schmieder (2017), Schmieder, von Wachter, and Heining (2023) and Gerard, Lagos, Severnini, and Card (2021).

[62] Strictly speaking, a placebo test involves running the same analysis in a context where no true effect should exist. In our setting, we instead expect attenuated effects, rather than effects of exactly zero, across alternative permutations.

[63] In practice, we perform a derangement (i.e., no element appears in its original position) with repetitions. There are 14,833 alternative ways to reshuffle the origin-specific measures in this case.

7.4.5 Local skill-complementarity

We do not require network members to be randomly assigned to firms to recover causal estimates. However, in the absence of such randomization, there remains a concern that certain network-related factors might drive part of the observed effects. How serious is this concern? Suppose municipality m is particularly attractive to migrants from origin o because of the skill-complementarity between o-types and the local mix of the most productive firms. While the inclusion of a municipality fixed effect γ_m controls for any systematic differences in locations affecting all refugees, it does not account for such group-specific advantages.

Since the presence of such comparative advantages is likely to result in geographic sorting, our main specification accounts for this as we control for the network size ($Sh.Conat._{om,c}$). At the same time, our extensive battery of robustness checks has already ruled out several other important channels. To threaten our main findings, one would need unobservables that are specific to municipality-origin pairs to simultaneously increase the likelihood that migrants from a given origin enter high-quality firms and improve the integration of newly arrived co-nationals. Crucially, these factors would need to operate independently of individual unobserved abilities of network members or the industry composition of the network, both of which we have already accounted for in prior checks.

Nevertheless, to address this remaining concern, we perform two additional exercises. First, we compute our main establishment-based measure of network quality using only refugees who were randomly assigned to locations through the dispersal policy and remained in their assigned municipality. While this procedure restricts the number of networks we can observe and measure, it still allows to construct a meaningful sample and compare this more restrictive, arguably more exogenous quality measure with the baseline version. Figure E1 plots the bivariate relationship between the baseline network quality (x-axis) and the more restrictive version (y-axis). The two measures are strongly related and share substantial variation.

Second, we re-estimate equation 3 including an alternative set of fixed effects designed to absorb unobserved municipality-origin factors. Because origin-by-municipality fixed effects would be too demanding for our data, we instead include region-by-municipality and arrival cohort fixed effects. Specifically, we grouped origin countries by broad geographic region (Middle East, Africa, and Southwest Asia) and grouped cohorts into three 4-year cohort bins. This adjustment is also particularly relevant if, for instance, top firms in certain municipalities systematically discriminate against particular nationalities, as we would account for native attitudes shaped by visual or cultural stereotypes. Table E5 presents the results employing this alternative set of fixed effects and using both our main network quality measure (Panel A) and the alternative focused on firms in the top quality quartile (Panel B) as main regressors. While medium-run estimates in Panel A are affected (not in Panel B), short-run estimates for employment and earnings across both panels remain virtually unchanged in magnitude and statistical significance, further ruling out this type of bias.

8 Mechanisms

This section examines the mechanisms through which exposure to good employers provided by co-ethnic networks affect refugees' labor market integration in Denmark. The literature on social connections in the labor market identifies two primary ways in which connections benefit job seekers. First, network members may share information about openings they have knowledge of. Second, both workers and employers may use job referrals to reduce information asymmetry in the search process. In both cases, social networks act as information-transmission mechanisms for their members, which is particularly beneficial for immigrants, who often lack the country-specific knowledge necessary for a successful job search.[64] Below we evaluate various complementary channels.

8.1 Referrals

A compelling explanation is that connections to good firms provide employment in better firms and better jobs through job referrals. Although our data do not include direct information on referrals, we examine additional outcomes and patterns that may offer indirect evidence of their use.

First, we study whether having network connections to good employers affects refugees' job search outcomes. Specifically, in Table 10, we examine whether these connections (using our baseline exposure measure in Panel A, and the alternative based of top-quartile firms in Panel B) lead to access to better employers, as measured by establishment fixed effects. Due to the smaller sample size available, we do not include municipality fixed effects in these specifications, but we control for key municipality characteristics related to the presence of high-quality firms.[65] Our preferred estimates (columns 4-6) indicate that better employers in the network lead new refugees to access better workplaces themselves, with statistically significant effects emerging in the medium run. As discussed previously, establishment pay premia correlate positively with productivity. Hence, in columns 7 to 9, we complement this evidence by finding that better connections lead new refugees to access more productive workplaces, as measured by value added per capita, with statistically significant effects in the short run in Panel B.[66]

Moreover, we consider whether exposure to better firms improves access to better jobs too. We define job quality based on task content, constructing an index equal to the ratio of

[64]Several other factors not directly related to job search might also contribute to how networks accelerate immigrants' labor market integration. For example, family, friends, and colleagues can provide support and encouragement, as well as information about job training programs, language classes, and other resources that facilitate labor market integration (Åslund et al., 2024). However, since it is unlikely that this type of support varies systematically with the quality of employers in the network, and given that we control for network size and employment outcomes in our main specification, we are sufficiently confident these dynamics do not drive the results we observe.

[65]We include the municipality's average workplace pay premium, share of college-educated individuals aged 18-65, and population share of the country total aged 18-65.

[66]For a cleaner productivity measure, we isolate the permanent component in firm value added by estimating:

$$\ln(VA/N)_{ft} = \lambda_f + \lambda_t + \epsilon_{ft}$$

where $\ln(VA/N)_{ft}$ is log value added per capita for firm f in year t, λ_f are firm fixed effects, and λ_t are year fixed effects. We borrow this approach from Åslund et al. (2025). λ_f is used as outcome in columns 7 to 9 of Table 10.

communication and cognitive tasks to manual tasks. To do so, we draw information on task content from O*NET data, using the ISCO classification for jobs.[67] In Table 11, at least when using our exposure measure to employer quality based on the presence of top firms in the networks (Panel B), we find that refugees with better employer connections are more likely to access better jobs, with significant effects in both the short and medium run. Table 11 also examines the geographic mobility of refugees, which can be a channel for labor market success by facilitating movement towards job opportunities (Foged et al., 2024). It is perhaps unsurprising that we find no significant effect on this margin, as our regressor captures two countervailing forces at play. On the one hand, having connections to high-quality local firms may provide incentives to stay; on the other hand, early access to better jobs and firms may encourage subsequent moves in search of even better matches or amenities.

Complementing this evidence, we examine which individuals benefit most from these connections. Heterogeneity by age at arrival (Figure 11) and by education completed prior to arrival (Figure 12) reveals that middle-age refugees with academic education experience the largest gains in labor market outcomes. This pattern is consistent with highly educated immigrants from disadvantaged countries facing downskilling and benefiting most when employers learn about their true abilities. In this context, referrals may help bridge credential gaps.[68] Interestingly, for these groups of refugees, the employment and earnings effects are permanent, remaining positive and statistically significant also in the long run.

Second, we can examine the wage and turnover dynamics among refugees to assess whether they are consistent with implications of referral-based job matching. Consistent with the models of search with referrals presented in Dustmann et al. (2016) and Glitz and Vejlin (2021), where workers' match-specific productivity is more uncertain in the external labor market than in the referral market, we expect workers hired through referrals to exhibit higher initial wages and lower turnover due to the higher expected match quality at the outset. We also anticipate that these initial differences will decline as tenure increases, driven by continuous learning about match-specific productivity and selective separations.[69]

Following Glitz and Vejlin (2021), we construct a proxy for referrals by using an indicator variable equal to 1 if a newly arrived refugee i begins their current job in an establishment where at least one member of their initial local co-ethnic network is still present at the time of job start (τ).[70] We then estimate the following specification:

$$y_{i,t} = \alpha + \beta_1 \text{JR}_{i,\tau} + \beta_2(\text{JR}_{i,\tau} * \text{Tenure}_{i,t}) + X_{i,t}\gamma + \phi_t + \lambda_{\bar{j}} + \epsilon_{i,t} \tag{5}$$

where $y_{i,t}$ represents either (log) wages or an indicator variable for leaving one's current employer, while JR is our proxy for referrals. The model also includes a vector of individual and

[67] The US Department of Labor O*NET database (`https://www.onetcenter.org/db_releases.html`) measures the importance of various physical and language abilities for each occupation.

[68] Dostie et al. (2023) show that employment reallocation over time toward higher-paying employers drives the reduction in immigrant-native wage gaps in Canada. This effect is particularly significant for highly educated immigrants from disadvantaged countries, who often experience substantial downskilling but benefit as employers learn about their abilities. Our findings for refugees speaks directly to the importance of this mechanism.

[69] We expect $\beta_1 > 0$ and $\beta_2 < 0$ for wages, and $\beta_1 < 0$ and $\beta_2 > 0$ for turnover.

[70] In other words, we assume that when a refugee follows a network member into the same establishment, the new job is obtained through a referral.

establishment characteristics, $X_{i,t}$, year fixed effects (ϕ_t), and, in the preferred specification, terciles of establishment productivity fixed effects ($\lambda_{\bar{j}}$). Standard errors are clustered at the municipality of assignment level.[71]

Table 12 reports estimates from equation 5, with columns 1 to 4 presenting results for the wage regressions and columns 5 to 8 for the turnover regressions. Our preferred specifications, which account for the fact that different types of firms rely on referrals to varying degrees (e.g., low- and high-productivity firms, as discussed in Galenianos (2013)) by including terciles of establishment productivity fixed effects, are shown in columns 2 and 6. While the coefficients are generally not statistically significant, likely due to limited power for this demanding analysis, the pattern of signs is consistent with the predictions of referral-based models. This remains true in columns 3 and 7, where we restrict the sample to job spells that are the first in Denmark for each refugee. Remarkably, the estimate on turnover at the time of entry is consistently negative and also significant across specifications, suggesting the presence of higher initial expected match quality.[72]

Overall, we interpret this evidence to suggest that referrals are an important driver of our results for refugees, although other mechanisms may also be at work.

8.2 Information sharing

Could network members share information about vacancies, regardless of whether these openings are in firms where they themselves are employed, and could this help explain the improved labor market integration of newly arrived refugees? We find evidence suggesting that such information sharing among network members is taking place and influences both labor market outcomes and those beyond it. This process may complement and amplify the impact of referrals.

We begin by considering industry sorting. Specifically, we assess whether the industry concentration of the co-ethnic network at arrival, measured using a Herfindahl–Hirschman Index (HHI) based on the distribution of network members across industries, predicts industry sorting for newly arrived refugees.[73] Our outcome of interest is defined as the share of a refugee's network working in the same industry as the individual during the first years after arrival. Table F1 provides evidence supporting this mechanism.[74] Whether HHI is included as a continuous variable (columns 1 and 3) or as a binary indicator equal to one for networks with above-median HHI (columns 2 and 4), greater industry concentration within an individual's network is associated with stronger alignment between the individual's own industry and that of their network in the first five years since arrival (columns 1 and 2), consistent with a channeling mechanism influenced by shared information about job opportunities or sector-specific

[71] The controls in $X_{i,t}$ include tenure, tenure squared, age, age squared, accumulated experience in Denmark, accumulated experience squared, occupation-specific experience, occupation-specific experience squared, establishment size, industry dummies, education group dummies, and a gender dummy. All dummies are interacted with tenure and tenure squared to ensure that heterogeneous tenure profiles across subgroups that differentially rely on referrals do not bias the estimates (Glitz and Vejlin, 2021).

[72] We lose significance in column 8, where very likely the inclusion of establishment fixed effects substantially reduces the variation available to identify turnover effects.

[73] As in other classic antitrust applications, the HHI is computed as the sum of squared industry shares.

[74] All regressions include the standard set of controls. While we do not observe refugees' occupation prior to migration, we control for the education attainment at arrival.

knowledge. The magnitude of the effect declines when we consider sorting in the medium run (6–10 years after arrival).

We then expand the analysis to study the interaction between network quality and network industry concentration.[75] Columns 5 to 7 of Table F1 report the corresponding estimates using binary indicators for HHI and quality. The positive and significant coefficient on their interaction indicates that when high-quality networks are also concentrated, they become particularly powerful drivers of industry sorting, likely reflecting transmission of sector-specific job information. As before, the effect diminishes with time since arrival.

The sharing of information among network members might also pertain other aspects beyond the labor market. One of the most prominent theories explaining ethnic segregation is related to networks, as social connections may lead to a concentration of members of the same ethnic group in the same residential areas (or the same workplaces) through the exchange of information about residential (and job) opportunities (Glitz, 2014).[76] We then start by assessing whether the quality of networks, according to our definition, can be linked to refugees' outcomes outside the labor market, such as residential mobility decisions, suggesting a flow of information among network members. While proximity to contacts is itself valuable, social connections provide hard-to-find local information that is useful when choosing among alternative locations (Büchel, Ehrlich, Puga, and Viladecans-Marsal, 2020).

Hypothesizing that network members at better firms might be individuals more aware of job opportunities and better integrated in the host country search market, and hence more active in sharing relevant information, we test whether higher network quality predict more residential clustering. We construct an individual measure of residential clustering, or co-location rate, for each refugee by computing the ratio between co-ethnics living in their parish (minus self) and co-ethnics living in the whole municipality (minus self). We run the following specification:

$$\begin{aligned} y_{ioc,t} = \alpha + \beta_1 f[\hat{\psi}_j]_{om,c-1} + \sum_{\tau=2}^{15} \phi_\tau \mathbf{1}\{YSM_{it} = \tau\} + \sum_{\tau=2}^{15} \eta_\tau \mathbf{1}\{YSM_{it} = \tau\} * f[\hat{\psi}_j]_{om,c-1} \\ + \beta_2 Sh.Conat._{om,c} + \beta_3 Emp.NW_{m,c} + X_{i,t} + \gamma_m + \delta_{o,c} + \epsilon_{ioc,t} \end{aligned} \tag{6}$$

where $y_{ioc,t}$ represents this individual co-location rate capturing how locally embedded members become over time. Our coefficients of interest η come from the interaction between indicators for years since migration (τ) and initial network quality. The model also includes the set of controls and fixed effects used in the main analysis.

Figure F2 plots the total effect of network quality at each year since migration, showing a clear and significant pattern over time. Refugees assigned to high-quality co-ethnic networks at arrival are more likely to live near other co-ethnics, and this effect strengthens the longer they have been in the country. The increasing effect in later years may arise for two reasons. First, since refugees receive housing assistance and most of them use it, relocation is relatively costly in the initial years. Second, as more individuals move closer together, others are likely

[75] We first explore this descriptively in Figure F1, which shows the unconditional correlations.

[76] Various studies have documented the link between labor market networks and residential proximity (e.g., Hellerstein et al. (2011)).

to follow, giving rise to a cumulative mechanism consistent with increasing information flow within the network over time.

Finally, we complement this evidence with a heterogeneity analysis to explore whether the impact of network quality depends on network size. We categorize networks into size-based terciles and interact the three dummy variables with our network quality measure. Table 13 reports estimates of the effect on refugees' integration, using mid-sized networks as omitted category. The negative and statistically significant coefficients on the interaction of quality and the large network dummy suggests potential information congestion, further supporting the idea that information flow is a relevant mechanism (Beaman, 2012).[77]

All in all, referrals and information sharing represent plausible mechanisms through which the higher quality of co-ethnic networks improves refugees' outcomes. A model that would deliver this result is one in which employed members of the network pass job offers on to unemployed members, so that the better the firms that employ them, the better the offers they can supply. We develop one such model in Appendix A, where we rationalize the information-transmission role of networks within a classic search- and-match model, proposing an augmented Diamond–Mortensen–Pissarides framework.

8.3 Networks and employers' willingness to hire refugees

Referrals and information sharing may be less relevant if members of high-quality networks work at firms with a stronger propensity to hire refugees, since information flows add little beyond the access new refugee members would have obtained on their own. By contrast, if employers are on the margin, that is neither systematically open nor systematically closed to hiring refugee workers, information within the network should matter most. To explore this, we examine the relationship between firm willingness to hire and network quality.

We begin by predicting the probability of hiring a refugee for each establishment and year using a logit model with various predictors, including establishment size (log of full-time equivalents), industry, municipality, share of foreign employees, presence of a foreign manager, and AKM firm fixed effect. We do this separately for each year to account for employer learning, as firms may update their beliefs about refugees' skills based on past refugee hiring experiences (Loiacono and Silva-Vargas, 2024). Next, we compute the average of these scores across firms in each network, classifying networks into terciles with high, medium, or low predicted propensity to hire refugees. We then study the interaction of these categories with network employer quality in predicting refugees' labor market integration.

Table F2 reports estimates from regressions of refugees' labor market outcomes on dummies for terciles of network average willingness to hire and their interaction with network quality. The omitted category is the bottom tercile of openness to hire a refugee ("Low"). The rest of the specification is the same as in equation 3. Interestingly, the results show that the strongest significant effect of network quality on refugees' outcomes arises in networks whose employers display medium levels of predicted willingness to hire a refugee on average. In other words, connections at high-quality firms help refugees most when these firms are moderately likely to hire them, revealing a reversed U-shaped pattern. Connections with good

[77] On the other hand, small networks may not have enough information to share.

firms that on average are not likely to hire refugees are not as useful, nor are those with good firms that on average are very likely to hire them and access is less of a barrier. The concentration of effects among medium-willingness employers is in line with our expectations, as information flows are expected to have the greatest impact for firms most on the margin of hiring refugees. Overall, this relationship between predicted firm openness and network quality provides further support for the important role of information flows within networks.

8.4 Co-ethnic managers

Co-ethnic connections to higher-quality firms may be particularly valuable when they involve individuals employed as managers, who often have greater influence over hiring decisions (Åslund et al., 2025). We therefore examine whether the positive effects of these connections are stronger when the employed co-ethnic contacts hold managerial positions.[78]

We begin by identifying managers using occupation codes and classifying workers by task content. For 1987–1990, we adopt Statistics Denmark's definition of managers; for 1991–1998, we follow Bernard, Fort, Warzynski, and Smeets (2024), who classify workers into five broad categories, one of which is managers.[79] These definitions are fairly broad, encompassing salaried managers across small and large businesses, from small-store managers to managers of large corporations. We identify 743 distinct individuals from refugee-sending countries who held managerial positions and were part of the networks of the newly arrived refugees during the years of the dispersal policy we study.

Next, having classified managers in our data, we define an indicator equal to one if a refugee in our sample had at least one co-ethnic employed as a manager at an establishment in the municipality to which the refugee was initially assigned. We then interact this indicator with both our baseline measure of network employer quality, constructed from all co-ethnics employed in any position at local establishments, and indicators for years since arrival. In doing so, our goal is to estimate whether the effect of establishment quality varies with the presence of co-ethnic managers in the network.

Figure F3 shows the estimated annual differential effects of network establishment quality when co-ethnic managers are part of the network, relative to having no co-ethnic managers in the network at arrival. The effect of employer quality is stronger in networks with managers as such connections lead to higher employment (left panel) and annual earnings (right panel). The coefficients, however, are statistically significant only for some of the first five years after migration for earnings. Perhaps not surprisingly, the positive patterns then fade over time. Given the limited number of managers in the sample, these estimates are imprecise and should be interpreted with caution. Nonetheless, managerial connections may represent an additional mechanism contributing to the broader effects documented in our main results.

[78] A related perspective is offered by Dagnelie, Mayda, and Maystadt (2019), who study whether co-ethnic entrepreneurs, rather than managers, help refugees by hiring them.

[79] As DISCO-88 codes, which are occupation codes nearly identical to ISCO-88, are available in the registers only from 1991 onward, we use the variable NYSTGR to identify managers in earlier years. With NYSTGR, we apply Statistics Denmark's definition; with DISCO codes, we follow Bernard et al. (2024). In both cases, occupation codes come from the Employment Classification Module (AKM) register.

9 Optimal Assignment

We have estimated a positive and significant effect of being connected to high-quality firms through co-ethnic networks on refugees' labor market outcomes. Since the assignment of refugees to different resettlement locations within the host country is one of the earliest and most consequential policy decisions in the integration process, a natural question arises: *can our findings be leveraged to provide a cost-efficient policy tool that improves refugees' outcomes?* In this section, we integrate machine learning methods and integer optimization for refugee resettlement, building on methodologies from a recent series of papers and extending them by incorporating our empirical insights.

9.1 Methodology and results

Given robust empirical evidence that initial placement determines refugees' long-term outcomes, identifying optimal matches between refugees and localities is crucial. A recent strand of literature at the intersection of economics and operations research has proposed and designed automated processes for host countries' resettlement decisions (Bansak et al., 2018; Ahani et al., 2021; Delacrétaz et al., 2023). We contribute to this body of work by explicitly including network measures among the variables used to train prediction models underlying the data-driven assignment, accounting in particular for the quality of accessible employers at arrival through co-ethnic networks.[80] As shown in previous sections, this is an important refugee–municipality match-specific characteristic that would otherwise remain unobserved without either first estimating a fixed effects wage model or accessing additional firm-specific information. Using the generated predictions, we can then compare various assignment policy scenarios in Denmark, in the spirit of recent work for the United States and Switzerland (Bansak et al., 2018; Ahani et al., 2021).

We begin by presenting the problem for the optimal assignment of refugees. The integer optimization problem of matching refugees to municipalities takes the form of a multidimensional knapsack problem, and can be formulated as follows:[81]

$$\begin{aligned}
\max \quad & \sum_{i=1}^{|\mathcal{I}|} \sum_{m=1}^{|\mathcal{M}|} v_m^i z_m^i & & \text{(7a)}\\
\text{s.t.} \quad & \sum_{m=1}^{|\mathcal{M}|} z_m^i \leq 1, & \forall i, \quad & \text{(7b)}\\
& \underline{s}_k^m \leq \sum_{i=1}^{|\mathcal{I}|} s_k^i z_m^i \leq \bar{s}_k^m, & \forall m, \forall k, \quad & \text{(7c)}\\
& z_m^i \leq a_m^i, & \forall i, \forall m, \quad & \text{(7d)}\\
& z_m^i \in \{0,1\}, & \forall i, \forall m. \quad & \text{(7e)}
\end{aligned}$$

[80] Ahani et al. (2021) acknowledge that "additional information—such as housing information, social networks, or new job opportunities—likely exists to at least some degree at the local community level and could prove very useful in supplementing the decision process".

[81] We adopt a notation similar to Ahani et al. (2021).

where the objective is to maximize the sum of individual values v, a measure of individual integration, over all refugees i assigned Danish municipalities m. The binary variable z^i_m equals one if refugee i is matched to municipality m, and zero otherwise. Two constraints are specified. Constraint 7c ensures that each municipality receives at least a minimum number and no more than a maximum number of refugees, denoted by $\underline{s}^m_k$ and $\bar{s}^m_k$ respectively. We use the actual number of refugees originally assigned to each municipality as our inferred municipality quota. In practice, we almost always impose only this upper bound, but in some cases, municipality capacities are pre-approved, requiring that a minimum number of refugees also be assigned.[82] For binary support service $a^i_m \in \{0,1\}$, constraint 7d ensures that matches occur only when the municipality can accommodate specific needs. In our case, we ensure that the optimal allocation matches the actual number of refugees with specific family structures assigned to each location.[83] The variable domain specified in constraint 7e characterizes the problem as an integer optimization, where the optimal solution is the vector z^* of refugee-municipality matches.

For the integration metric in the objective function, we focus on individual employment probability, as in Bansak et al. (2018) and Ahani et al. (2021), and consider employment in the first five years after migration to align with the short-run horizon used in our main analysis. Crucially, however, employment probabilities are not observed for incoming refugees, nor for past refugees in municipalities other than the one to which they were actually assigned. Hence, to obtain the potential outcome distribution for each refugee, we rely on machine learning methods to estimate individual probabilities of employment at each municipality m from:

$$v^i_m = \mathbb{E}[y_i \mid \mathbf{X}_{i,m}] \tag{8}$$

where y_i is a binary outcome indicating whether individual i was ever employed within the first five years after migration. In the baseline version, which follows other recent works, $\mathbf{X}_{i,m}$ includes observable individual characteristics and their interactions with municipalities. In our alternative version, we augment instead the set of variables for statistical learning of employment probabilities with $\tilde{\mathbf{X}}^{network}_{i,m}$, which explicitly includes employer quality within the networks, so that:

$$v^i_m = \mathbb{E}[y_i \mid \mathbf{X}_{i,m}, \tilde{\mathbf{X}}^{network}_{i,m}] \tag{9}$$

We proceed in the following steps. First, because the potential outcome distribution is not observed, following Bansak et al. (2018) we begin with a modeling stage that predicts expected individual success in terms of early employment for a random 80% of refugees from cohorts 1987–1997 across all potential resettlement locations (training sample). We employ two supervised machine learning models in this stage: we introduce a Least Absolute Shrinkage and Selection Operator (LASSO) constraint to a logit model featuring individual characteristics

[82] s^i_k denotes the required units of service k. It is possible to define other constraints similar constraint 7d, where instead of municipality quotas, refugee processing capacity at the municipality or slots in language classes are considered. In our case, for municipality quotas, $s^i_k = 1\ \forall i$.

[83] We implement this by creating indicators for refugees with children in the 0–5 and 6–17 age ranges, consistent with the dummy variables used in our main analysis.

and their interactions with municipalities, and we also estimate a gradient boosted regression tree (GBRT), featuring the same set of predictors and municipality interactions as in the LASSO specification.

Next, we evaluate the performance of these models on a holdout (test) sample (the remaining random 20%), comparing them with two second-best alternatives: a naive constant estimator and a standard logit model that includes individual characteristics but does not account for municipality-specific effects. Based on predictive performance, we select LASSO and use this method to estimate the potential outcome distribution for the last cohort of refugees allocated under the dispersal policy (the 1998 cohort), for whom we implement the optimal assignment. Importantly, we estimate the potential outcome distribution with LASSO using, in turn, both the baseline in expression 8 and our refinement in expression 9. Further methodological details and performance comparisons are provided in Appendix G.

Finally, in a matching stage, we solve the maximization problem described above using our predictions for the 1,258 refugees in the 1998 cohort and evaluate the employment gains from the optimal assignment relative to the actual one. Restricting our exercise to this cohort offers three advantages. First, we rely on the integration of earlier-arriving refugees in the same municipalities to train our models. Second, we use networks formed in earlier periods to construct our measures and thus inform our models. Third, we avoid complications from multi-period assignments, which may become difficult to manage.[84] We use the CBC MILP solver within the PuLP Python modeling environment to find the optimal municipality for each of these refugees, i.e. vector z^*.

Intuitively, given the municipality quotas, the optimal allocation cannot assign all refugees to a handful of economically dynamic municipalities, no matter how beneficial these might be for integration. At the same time, when minimum capacities are pre-approved, weaker locations cannot be left without refugees. Respecting the constraints of problem 7 therefore requires balancing the advantages and disadvantages of different municipalities across different refugees. Our extension, by explicitly incorporating networks and employer quality, reinforces the idea that optimal assignments vary across immigrant types: different origins may benefit from different locations, and the best placement depends not only on individual characteristics and the overall economic activity of a municipality, but also on an origin-municipality match-specific component determined by the structure of accessible co-ethnic networks, as we show below.

Several important results emerge from solving problem 7. First, using baseline predictions from expression 8 and implementing the assignment solution raises the average predicted probability of ever being employed within five years since migration for the 1998 cohort by 16.6 p.p., from 28.9% to 45.5%. This optimized assignment increases the probability of finding employment across the entire distribution of refugees, as shown by the empirical cumulative distribution functions of refugees' predicted employment probabilities in Figure 13. The employment probability distribution after this optimization (dashed green line) first-order stochastically dominates the estimated distribution under actual assignment (solid red

[84] As noted by Ahani et al. (2021), "experiments with $n > 1$ placement periods introduce some additional nuances that required equally detailed implementation strategies".

line). These results are in line with employment improvements obtained in other works for the United States (+25 p.p.) and Switzerland (+11 p.p.) using very similar methodologies.[85]

Second, accounting for the role of co-ethnic networks, in particular the quality of employers accessible to newly arrived refugees through these connections, in predicting the potential outcome distribution as in expression 9 with the LASSO, improves predictive accuracy relative to baseline predictions from expression 8. Details are reported in Table G1. Using this refined distribution in problem 7, the optimized refugee-municipality matches are different relative to the solution obtained without accounting for network quality. This new solution to the assignment problem leads to an average predicted probability of ever being employed within five years of 42.1%, confirming the considerable improvement from algorithmic assignment over the status quo, with gains across the entire distribution (solid blue line in Figure 13).

Importantly, accounting for firms connected through the networks also improves on the baseline optimization. Had we used the more accurately predicted probabilities from expression 9, while holding fixed the matches prescribed by the first optimized assignment, we would have obtained an average predicted employment probability of 36.0%. In other words, accounting for network information in our data-driven approach updates the potential outcome distribution and leads to a slightly modified optimized assignment, which improves not only on the actual assignment under the dispersal policy but also on the optimized assignment that does not account for networks. Indeed, the new optimized assignment (solid blue line) first-order stochastically dominates both the estimated distribution under actual assignment (solid red line) and the baseline optimized one (solid green line).

Concrete examples illustrate how network quality shapes the optimized allocation, making some municipalities particularly attractive for some origins and less for others. For instance, about 17% of refugees from Vietnam in the 1998 cohort were assigned to Odense under the dispersal policy, even though the quality of employers connected to their co-ethnic network there was relatively low, despite Odense being Denmark's third-largest and one of its most economically active cities. In our optimized allocation, by contrast, no Vietnamese refugee is assigned to Odense. Another example is represented by Viborg, which in 1998 received around 7% of all randomly dispersed refugees, the second most after Copenhagen. About 15% of refugees assigned to Viborg under the dispersal policy were from Somalia, even though their network employers were not of particularly high quality. In the optimized allocation, however, two-thirds of these Somalis are reallocated elsewhere, many to Esbjerg, Denmark's fifth-largest city, where their network was substantially stronger and reported some of the highest quality values for this origin. At the same time, the number of Iranian refugees assigned to Viborg by our algorithm grows by a factor of four relative to the dispersal policy, consistent with their stronger network there. These reallocations occur while respecting municipality quotas, ensuring, for instance, that the overall number of refugees from the 1998 cohort in Viborg remains unchanged, and at the same time better aligning origins with localities to improve integration prospects.

[85] Bansak et al. (2018) find a 25 p.p. gain in median employment during the first 90 days for the United States in 2016 ($n = 919$), and a 11 p.p. gain in third-year employment for Switzerland in 2013 ($n = 888$). Ahani et al. (2021) find gains between 22% and 38%, depending on the constraints, in employment outcomes within 90 days since arrival for refugees in the United States in 2017 ($n = 498$).

Third, our next result is closely related to this point. Since incorporating networks into the algorithm changes the optimal matches for refugee–municipality pairs, we examine which refugees gain or lose when reallocated from municipalities chosen without accounting for networks to those selected optimally with them. It is important to document which types of refugees gain from explicitly accounting for our network connections in implementing an optimized assignment, as improving the condition of the most vulnerable individuals could be an important ancillary goal for policymakers. Figure G1 plots the predicted gain or loss for individuals who would be reassigned against their predicted employment in the actual assignment. We find heterogeneous gains across the distribution of individual predictions when comparing the optimal baseline and refined assignments. Notably, the refugees who stand to benefit most from our approach are those predicted to fare worst under the status quo.

Finally, compared to the status quo assignment, implementing the optimized assigned prescribed by our approach also yields heterogeneous gains in average predicted employment across receiving municipalities. Figure G2 shows gains and losses by municipality. Interestingly, the optimized assignment does not disproportionately benefit the largest or most economically active municipalities: the fourth-largest (Odense) records small losses, and even the two largest (Copenhagen and Aarhus) see gains that are not among the highest. This is consistent with the origin–municipality complementarities described above. Once network quality is accounted for, the optimal allocation differs across origins rather than concentrating all refugees in a few top cities.[86] This further highlights the importance of an approach that accounts for origin-municipality complementarities rather than treating municipalities in isolation, even if this complicates the design of resettlement policies. Overall, had the authorities relied on this optimized method for resettling refugees, most Danish municipalities would have experienced increases in the average predicted employment of their refugee population arriving in 1998.

9.2 Counterfactual policy scenarios

We conclude this section by comparing four counterfactual policy scenarios generated with our preferred employment-optimization procedure. These scenarios show the optimal assignment under different constraints and illustrate the sensitivity of employment gains to policy choices. The four scenarios we consider are:

1. *Optimized assignment subject to family structure restrictions and inferred municipality quotas (with no lower bound).*
2. *Optimized assignment subject to family structure restrictions and allowing for 110% of inferred municipality quotas (with no lower bound).*
3. *Optimized assignment subject to family structure restrictions and allowing for 150% of inferred municipality quotas (with no lower bound).*
4. *Optimized assignment subject to family structure restrictions and allowing for 110% of inferred municipality quotas, with a lower bound of 90%.*

[86] For instance, Esbjerg, mentioned earlier among our examples, records some of the largest gains in Figure G2.

Counterfactual 1 represents our preferred optimization setup, accounting for networks and featuring default constraints for family structure and inferred quotas for each municipality. Relaxing municipality quotas by allowing an additional 10% (counterfactuals 2 and 4) or an additional 50% (counterfactual 3) capacity increases the average predicted employment probability in all three scenarios. The average predicted probability of ever being employed within five years since migration rises to 43.6% in counterfactual 2, to 50.0% in counterfactual 3, and to 43.2% in counterfactual 4. Empirical cumulative distribution functions for refugees allocated under these scenarios are reported in Figure G3.

Counterfactual 4 also imposes a lower bound of 90% of inferred capacity that must be met by the optimal assignment in every municipality. This additional constraint reduces the average predicted employment probability relative to counterfactual 2, but only slightly. This is encouraging, as it indicates that our optimization performs well and delivers gains even under tighter distributional constraints. Ultimately, while increasing municipality quotas generates gains by allowing more refugees to benefit from favorable matches, the presence of family-structure restrictions in the form of binary service constraints and of minimum capacity requirements have only a modest impact on employment gains.

Overall, the optimization produced by this algorithm produces substantial employment gains while respecting important distributional considerations. Resettlement authorities can modify the constraints we specified or introduce new ones (e.g., limits on the number of refugees from certain origins), as this data-driven approach is highly flexible to accommodate policymakers' priorities. While one resettlement agency in the United States (HIAS) has already adopted a software based on a similar algorithm (*Annie*™, developed in Ahani et al. (2021)), refining these methodologies to consider which local features and complementarities offer the best potential to improve refugee outcomes as well as encouraging their wider adoption could enhance integration and reduce host-country costs for providing asylum to refugees.

10 Conclusion

This paper presents causal evidence on the effect of early employer quality on refugees' labor market integration. Using administrative data from Denmark, we first estimate establishment-specific fixed effects that capture employer pay-setting policies and serve as a proxy for employer quality. We then exploit co-ethnic connections and Denmark's 1986–1998 dispersal policy, which quasi-randomly assigned newly arrived refugees to Danish municipalities, to obtain exogenous variation in refugees' exposure to different levels of employer quality.

We contribute to the literature by providing causal estimates of the role of employers in refugees' integration, addressing nonrandom sorting with a quasi-experimental design that has not yet been exploited in works on firm wage differences. We document a lasting influence of early employers on refugees' outcomes, in contrast to other workers who face lower barriers and greater mobility, and we discuss potential violations of the widely used AKM decomposition that may affect recent studies. Our work also offers a new perspective on networks by disentangling the effect of the quality of connected firms from that of network members on newly arrived refugees' outcomes.

The results show that higher-quality employers in the local co-ethnic network at arrival improve employment and earnings outcomes for newly arrived refugees. While these effects are not statistically significant in the long run (11 to 15 years after arrival), we find a clear positive impact in the short run (1–5 years after arrival) and medium run (6–10 years after arrival). Job referrals and information sharing among network members appear to be key channels through which connections to good employers matter. The benefits are strongest for highly educated refugees, who may be subject to severe downskilling.

We conclude by extending a data-driven algorithm to optimally match refugees with municipalities in order to maximize early employment outcomes, incorporating information on employer quality within networks. In our setting, the optimized assignment raises employment within the first five years after arrival by about 46% relative to the dispersal policy, with substantial gains across counterfactual policy scenarios. Similar flexible approaches can be readily adopted by resettlement agencies to reduce inefficiencies of manual processes.

Our findings have important policy implications for host countries. We show that refugee integration is sensitive to employer quality and that the connections provided by co-nationals can offer valuable access to firms. Rather than focusing solely on logistical considerations or on broad economic characteristics of the local labor market, the design of placement policies—and of integration policies at destination in general—should also take into account the role played by employers, which has so far been largely overlooked. Improving migrants' access to better employers, encouraging stronger early matches with firms, and more broadly incorporating a wider range of demand-side considerations into dispersal policies can promote faster and more effective integration for refugees while limiting costly mismatches.

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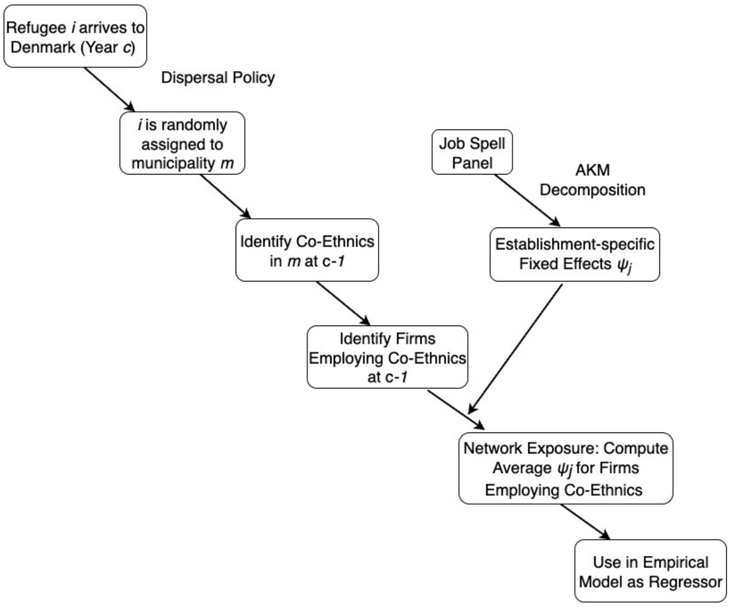
Figures and Tables

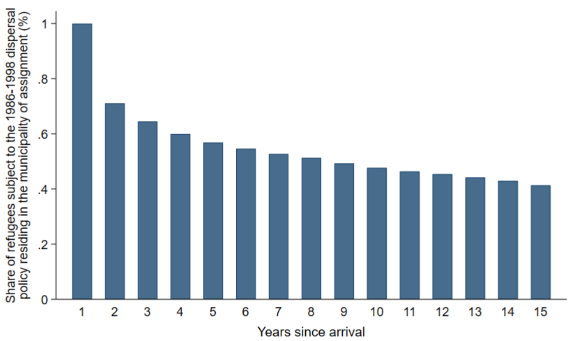
Figure 1: Construction of employer-based regressor



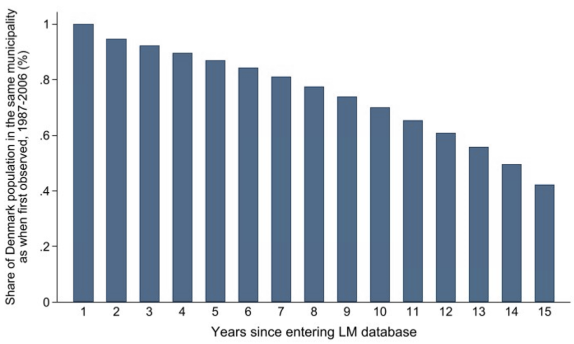
Notes: The diagram shows the methodology employed to construct the average quality of establishments employing members of the local co-ethnic network at the time of arrival, the regressor in our main empirical results.

Figure 2: Mobility from initial municipality

(a) Refugees subject to dispersal

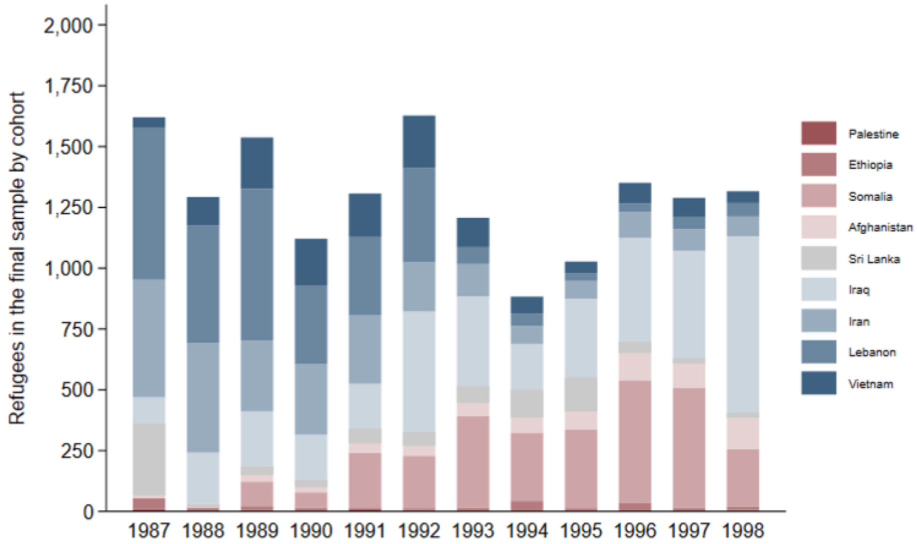


(b) Employer-employee matched data



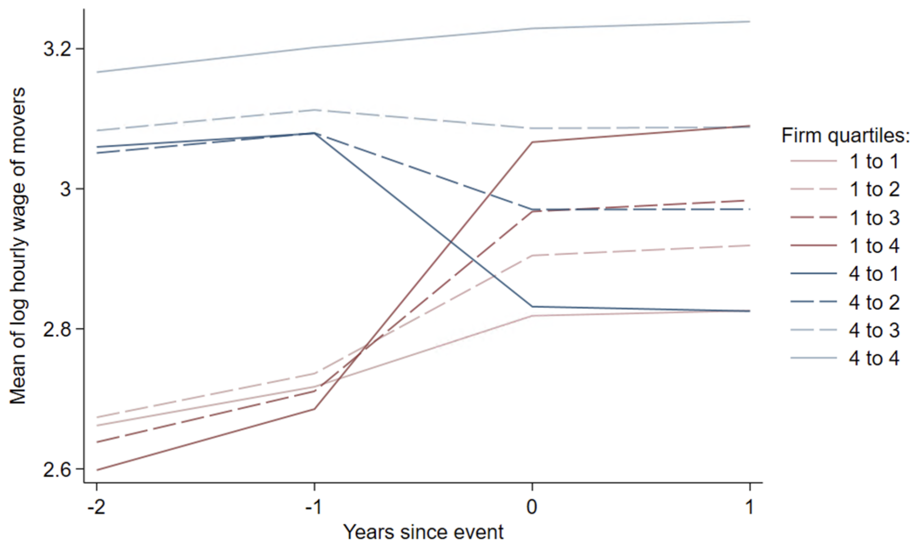
Notes: This figure plots the share of individuals who remain in the same municipality over time. Panel A considers the sample of refugees subject to the 1986–1998 dispersal policy that we use in the analysis (N=15,578) and plots the share who still reside in the municipality of initial assignment by year since arrival. Panel B considers the entire set of individuals included in the employer-employee matched dataset during the 20 years following the start of the dispersal policy and plots the share who still reside in the same municipality as the one they were first observed in the data.

Figure 3: Cohort size and composition



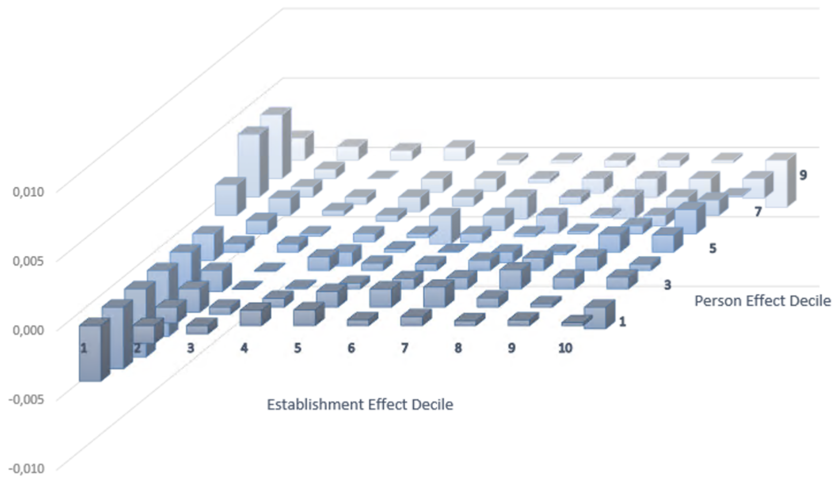
Notes: This figure plots the number of refugees subject to the 1986–1998 national dispersal policy by cohort and country of origin. We start from 1987 because of data limitations.

Figure 4: Wage changes for job movers by average wage quartile



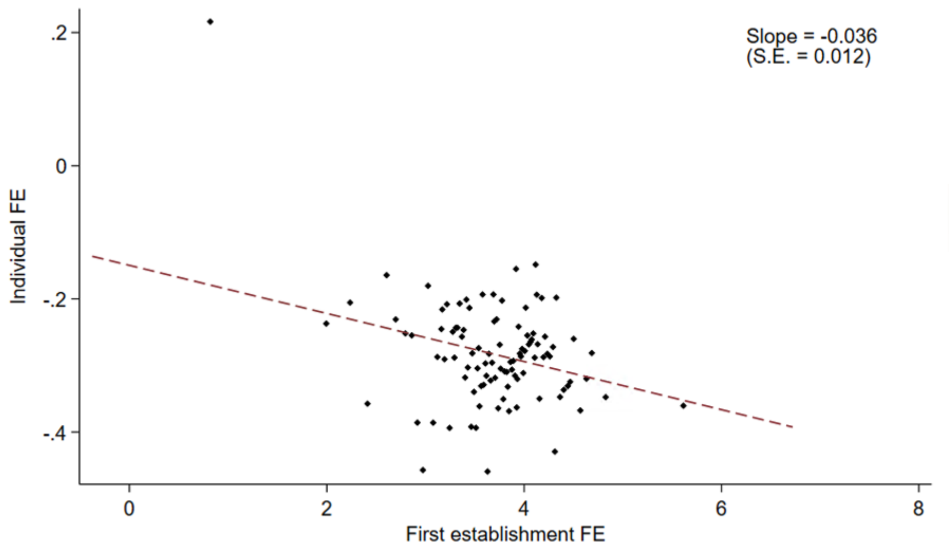
Notes: Event study analysis on the wage effects of job transitions. For any given worker, firms are categorized into quartiles based on the average wage of coworkers. Each point in the figure is the average wage by period, origin, and destination firm quartile, restricting the sample to workers who are employed for at least two years in both the origin and destination firms. The figure only displays transitions for workers leaving firms with the lowest-paid (quartile 1) and highest-paid (quartile 4) coworkers.

Figure 5: Mean residuals by person/establishment deciles



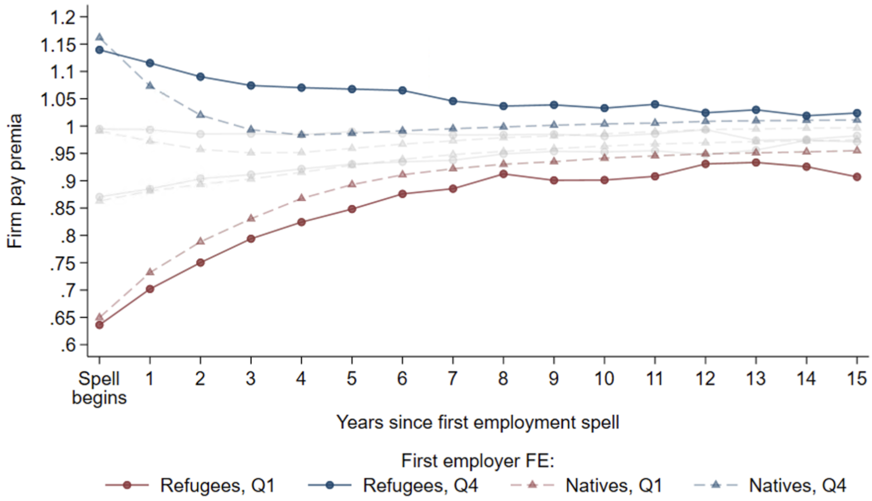
Notes: This figure plots the mean wage residuals from the AKM decomposition for specific types of matches. Meas wage residuals are displayed across 100 cells, defined by deciles of person effects (x-aixs) and establishment effects (z-axis).

Figure 6: Refugees' assortative matching



Notes: This figure plots the binned relationship between individual fixed effects for refugees, proxying for unobserved abilities, and the fixed effects of their first workplace, proxying for establishment quality. Both sets are estimated using the AKM decomposition. Each marker represents 18 individuals.

Figure 7: Stickiness of initial firm quality

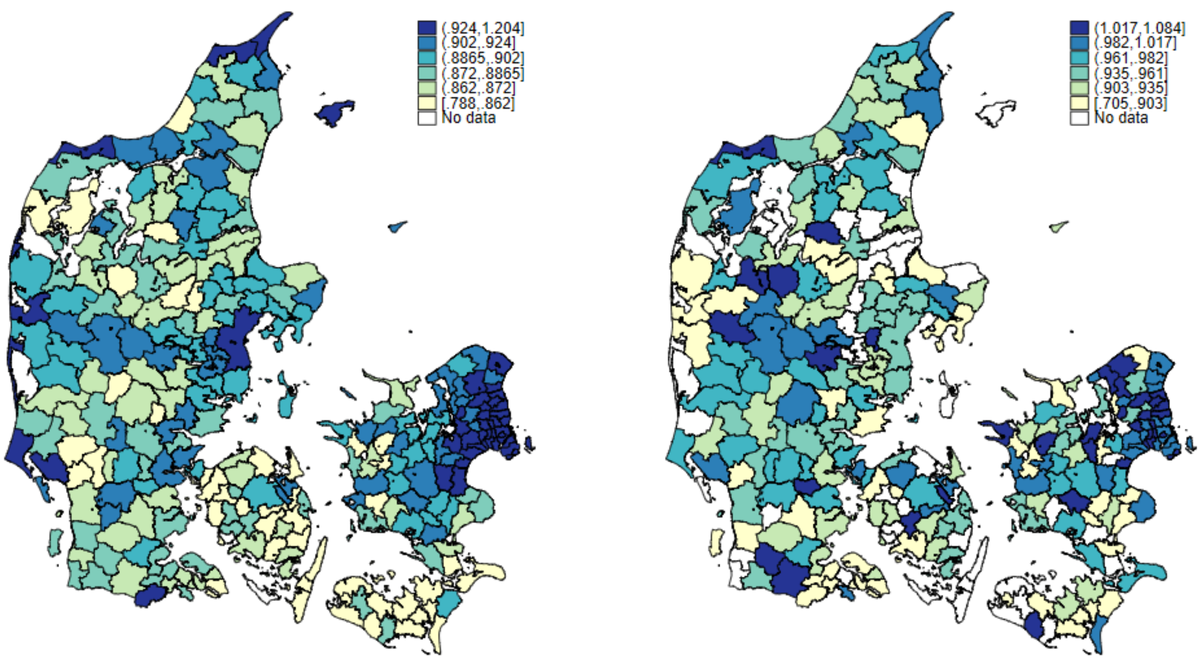


Notes: This figure plots the evolution of firm pay premia earned by natives and refugees. Both samples of native and refugee workers are categorized into quartiles based on the fixed effect of their first employer. Each point in the figure is the average firm pay premium by group (natives, denoted by triangle-shaped markers, and refugees, denoted by circle-shaped markers), by initial firm effect quartile (maroon for the bottom quartile, blue for the top quartile), and by year since first employment spell.

Figure 8: Geography of establishment and network

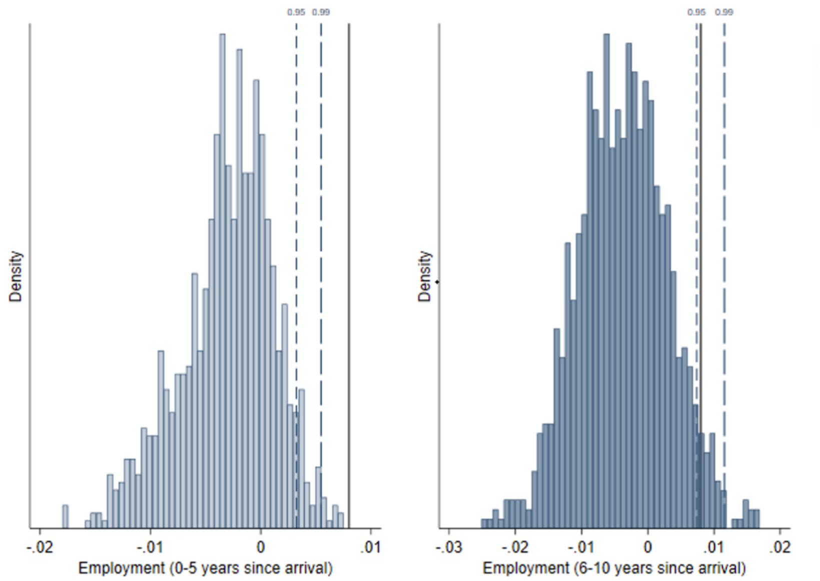
(a) Average establishment effects

(b) Average network quality



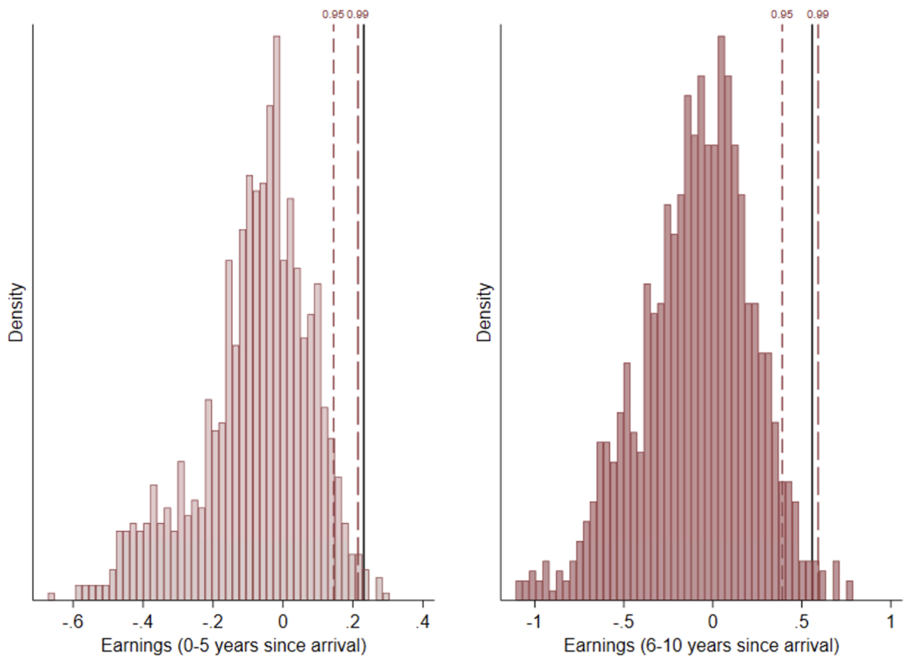
Notes: These maps display the average establishment effects (Panel A) and the average establishment effects within networks (also averaged over origin and year, Panel B) by sextiles across the 275 Danish municipalities (pre-2007 reform). In certain locations data have been suppressed to comply with Statistics Denmark's confidentiality rules.

Figure 9: Permutation-based test: Employment



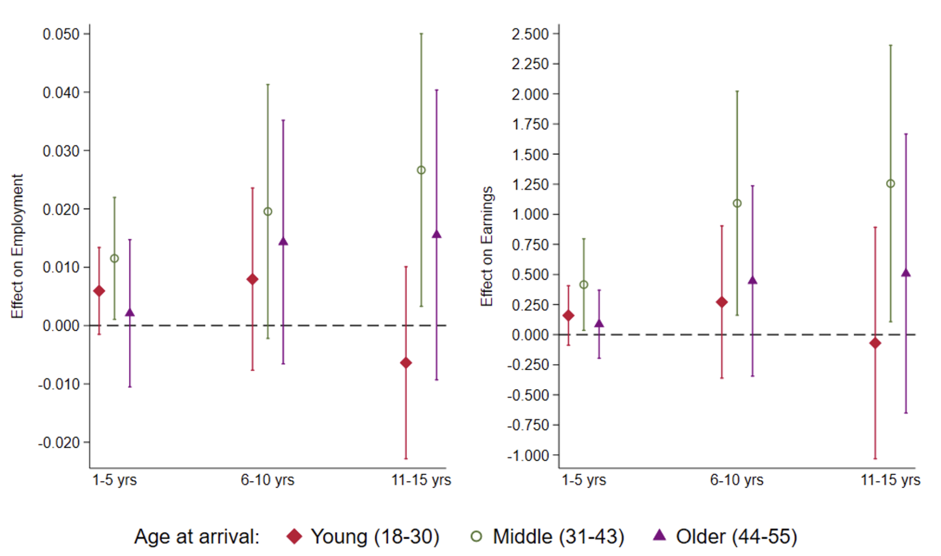
Notes: This figure plots the distribution of pseudo-placebo effects obtained by estimating coefficient β_1 in equation 3 1,000 times, using employment as outcome (short run for the left-hand side panel, medium run for the right-hand side panel). Every time we assign the network quality of one of the other eight origins to dispersed refugees from a given country of origin. The black solid line reflects the true effect estimated in our main results. The short-dotted line (long-dotted line) represents the 95$^{\text{th}}$ (99$^{\text{th}}$) percentile of the placebo distribution.

Figure 10: Permutation-based test: Earnings



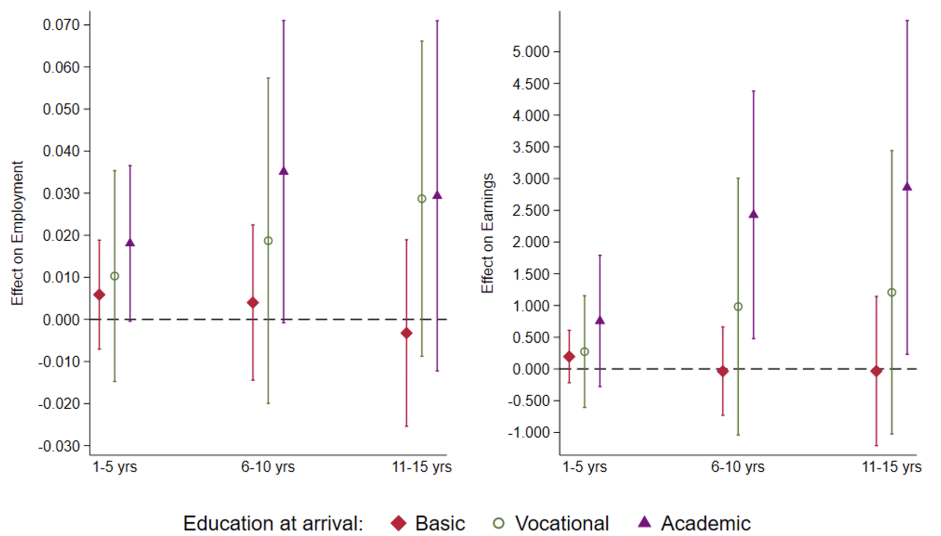
Notes: This figure plots the distribution of psuedo-placebo effects obtained by estimating coefficient β_1 in equation 3 1,000 times, using earnings as outcome (shor run for the left-hand side panel, medium run for the right-hand side panel). Every time we assign the network quality of one of the other eight origins to dispersed refugees from a given country of origin. The black solid line reflects the true effect estimated in our main results. The short-dotted line (long-dotted line) represents the 95$^{\text{th}}$ (99$^{\text{th}}$) percentile of the placebo distribution.

Figure 11: Heterogeneity analysis by age



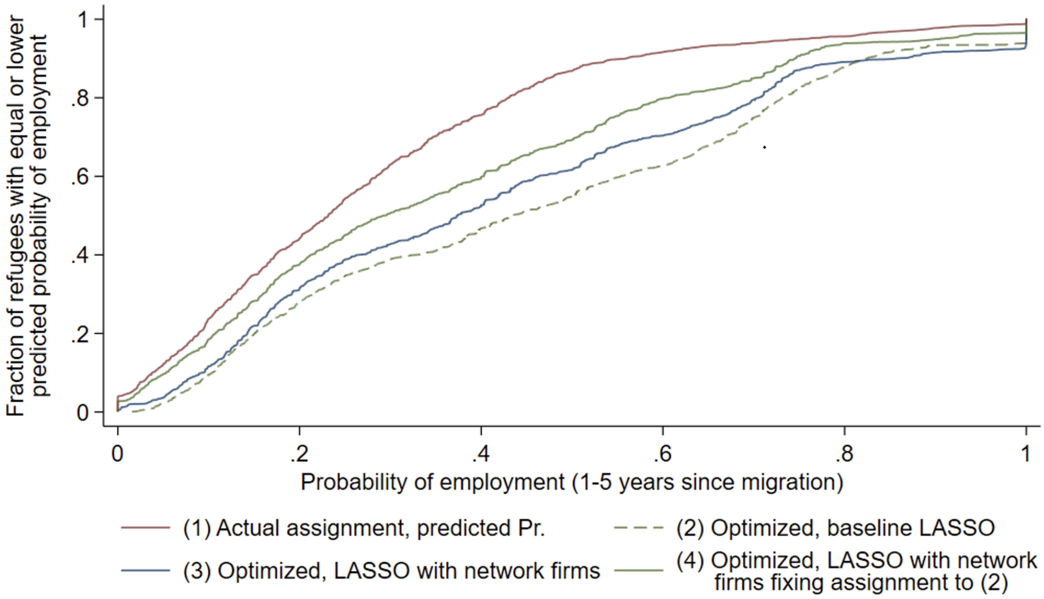
Notes: This figure plots the estimates and 95 percent confidence intervals of the differential effect of network quality (unweighted measure) on the main outcomes (employment in the left panel and earnings in the right panel) by age at arrival. Refugees are divided into three groups spanning the same number of years: young (18 to 30 years of age), middle-aged (31 to 43 years) and older (44 to 55 years). Each outcome is examined over three time intervals: short run (1 to 5 years since arrival in Denmark), medium run (6 to 10 years), and long run (11 to 15 years).

Figure 12: Heterogeneity analysis by education



Notes: This figure plots the estimates and 95 percent confidence intervals of the differential effect of network quality (unweighted measure) on the main outcomes (employment in the left panel and earnings in the right panel) by education completed prior to arrival. Refugees are divided into three groups: basic education, vocational education and academic education. Each outcome is examined over three time intervals: short run (1 to 5 years since arrival in Denmark), medium run (6 to 10 years), and long run (11 to 15 years). The sample is restricted to refugees reporting education attainment.

Figure 13: Empirical cumulative distribution functions under different assignments



Notes: This figure displays the empirical cumulative distribution functions (ECDFs) of the refugees' predicted employment probabilities within the first five years since migration under their actual and optimized assignments. The solid red line refers to predicted probabilities in municipality of assignment under the dispersal policy. The dashed green line refers to probabilities computed with the baseline LASSO (no network measures) in the corresponding optimized assignment. The solid blue line refers to probabilities computed with the refined LASSO (including network measures) in the new optimized assignment. The solid blue line refers to probabilities computed with the refined LASSO (including network measures) in the new optimized assignment. The solid green line refers to probabilities computed with the baseline LASSO in the new optimized assignment. All optimized cases employed family structure restrictions and inferred municipality quotas (with no lower bound) as constraints.

Table 1: Conditional random assignment of the policy

	Employment Rate of Non-Western Immigrants (1)	Share of Conationals (2)	Network quality (mean) (3)	Network quality FTE weights (mean) (4)
Age 30-39 years	-0.030*	-0.099**	0.026	0.002
	(0.016)	(0.038)	(0.037)	(0.038)
Age 40-49 years	-0.002	-0.095*	0.007	0.047
	(0.024)	(0.053)	(0.048)	(0.051)
Age 50-55 years	0.053	-0.205*	0.178**	0.140
	(0.038)	(0.110)	(0.091)	(0.093)
Female	-0.004	0.045***	-0.014	-0.011
	(0.005)	(0.010)	(0.012)	(0.012)
No. children, 0-2 yrs	0.003	-0.016	0.003	-0.001
	(0.008)	(0.014)	(0.020)	(0.020)
No. children, 3-5 yrs	-0.000	-0.028**	0.001	0.010
	(0.008)	(0.013)	(0.018)	(0.018)
No. children, 6-12 yrs	0.008	-0.012	-0.004	-0.005
	(0.005)	(0.009)	(0.011)	(0.011)
No. children, 13-17 yrs	-0.006	0.039***	-0.005	0.011
	(0.009)	(0.015)	(0.015)	(0.015)
Single	-0.000	-0.025*	-0.015	-0.019
	(0.007)	(0.013)	(0.016)	(0.016)
Africa	0.303*	0.424	0.502***	0.218**
	(0.163)	(0.330)	(0.142)	(0.090)
Asia	0.222**	-0.257	-0.297	-0.135
	(0.103)	(0.627)	(0.641)	(0.089)
Basic education	0.017	-0.002	0.031	0.013
	(0.011)	(0.023)	(0.025)	(0.026)
Academic education	0.007	0.014	0.003	-0.001
	(0.013)	(0.025)	(0.030)	(0.030)
Unknown education	0.016	0.002	-0.008	-0.025
	(0.010)	(0.021)	(0.024)	(0.025)
Obs.	15,571	15,571	10,271	10,246
Adj. R^2	0.776	0.557	0.442	0.521
Municipality FE	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes
F	1.105	0.240	1.865	2.012
Pr > F	0.346	0.868	0.133	0.110

Notes: This table reports a balancing test for the conditional random assignment of the dispersal policy. The sample is refugees from admission cohorts 1987 to 1998 and subject to the dispersal policy. Outcomes of these regressions are the main regressors in our preferred specification. Network quality represents the average employer quality of establishments employing at least one co-national in the municipality of assignment at the time of arrival (unweighted mean in column 3, weighted mean using full-time equivalents in column 4). Variables reflecting family structure and country of origin are not (nor are they expected to be) uncorrelated with initial location characteristics, as placement was conditional on these factors. F denotes the F-test statistic of joint insignificance of the dummies for educational attainment: basic education, academic education, and unknown education (vocational education is the omitted category of reference). $Pr > F$ denotes the corresponding p-value from the F-test. Robust SE in parentheses are clustered at the family level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 2: Sample of refugees: Summary statistics

	Observations	Mean	S.D.
Panel A: Individuals			
Age at Entry	15,578	29.8	8.25
Female	15,578	0.42	0.49
Married/cohabiting	15,578	0.65	0.48
Number of Children	15,578	1.06	1.62
Academic Education (prior to arrival)	6,566	0.26	0.44
Latin Alphabet of Mother Tongue	15,578	0.33	0.47
From Predominantly Muslim Country	15,578	0.14	0.35
Asia	15,578	0.15	0.36
Middle East	15,578	0.66	0.48
Africa	15,578	0.20	0.40
Employment (any)	15,578	0.22	0.27
Annual Earnings (thousand USD)	15,578	8244.6	12925.7
Avg. Hourly Wage Rate in Estab.	8,080	28.4	12.6
Complex Job (indicator)	15,578	0.12	0.22
Panel B: Worker years			
Agriculture	40,893	0.01	
Manufacturing	40,893	0.27	
Utilities	40,893	0.00	
Construction	40,893	0.02	
Trade and hospitality	40,893	0.18	
Transport	40,893	0.11	
Business and finance	40,893	0.08	
Public and personal services	40,893	0.33	
Not stated	40,893	0.00	

Notes: This table presents summary statistics for the sample of refugees used in the main analysis. Panel A reports individual and economic characteristics (observations are individual refugees). Panel B reports the industry distribution of the annual main jobs of employed refugees during the first fifteen years after arrival in Denmark (observations are worker years). Panel B includes only jobs with non-missing establishment and industry information. The nine NACE industry categories used are: Agriculture, fishing, and quarrying; Manufacturing; Electricity, gas, and water supply; Construction; Wholesale and retail trade, hotels, and restaurants; Transport, storage, and communication; Financial intermediation and business activities; Public and personal services; and Activity not stated.

Table 3: Estimation summary for AKM model

	Full panel (pooled)	Largest connected set (pooled)
Panel A: Summary statistics		
No. of worker-year obs.	15,836,448	15,703,133
No. of unique workers	2,344,821	2,311,622
No. of unique employers	291,386	264,706
No. of total moves	2,804,122	2,802,315
No. of unique movers	1,312,173	1,310,531
Log hourly wage (mean)	2.93	2.93
Average no. of moves by employer		10.59
Average no. of movers by employer		10.32
Panel B: Variance decomposition		
Total variance		0.148
Worker FEs (% explained)		0.065 (44.0)
Employer FEs (% explained)		0.023 (15.5)
Year FEs (% explained)		0.029 (19.3)
2cov (% explained)		-0.007 (-4.5)
Residual (% explained)		0.026 (17.3)
Panel C: Goodness of fit		
RMSE of AKM model		0.160
Adj. R^2		0.793
RMSE of CHK match model		0.141
Adj. R^2		0.865

Notes: This table reports summary results from OLS estimation of equation 1. Panel A reports the main statistics for the 1986–1998 yearly job spell panel of natives and non-refugee migrants employed in the AKM model. Panel B shows the variance decomposition for log hourly wages (share of total variance explained in parenthesis). Panel C presents the goodness of fit from the AKM specification and compares it with a job-match effect model that includes a separate dummy for each job (person-establishment pair).

Table 4: First employers and refugees' labor market outcomes

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main specification						
First employer quality	0.036***	0.035***	0.030***	3.441***	2.560***	2.577***
	(0.007)	(0.008)	(0.009)	(0.371)	(0.436)	(0.564)
boot. p-val.	[0.001]	[0.001]	[0.009]	[0.000]	[0.000]	[0.000]
Mean of Y	0.721	0.577	0.522	23.522	24.515	25.200
Obs.	4,189	4,082	3,855	4,189	4,087	3,868
Adj. R^2	0.075	0.096	0.126	0.111	0.085	0.102
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Alternative specification						
First employer quality	0.038***	0.037***	0.036***	3.560***	2.666***	2.753***
	(0.007)	(0.009)	(0.009)	(0.397)	(0.442)	(0.546)
boot. p-val.	[0.000]	[0.001]	[0.002]	[0.000]	[0.000]	[0.000]
Mean of Y	0.720	0.577	0.522	23.488	24.467	25.157
Obs.	4,217	4,111	3,885	4,217	4,115	3,897
Adj. R^2	0.052	0.075	0.100	0.068	0.053	0.065
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	No	No	No
Cohort-by-Origin FE	No	No	No	No	No	No
Panel C: At least one firm transition						
First employer quality	0.017*	0.028***	0.033***	2.942***	2.076***	2.757***
	(0.009)	(0.010)	(0.012)	(0.662)	(0.625)	(0.874)
boot. p-val.	[0.081]	[0.023]	[0.022]	[0.000]	[0.007]	[0.013]
Mean of Y	0.818	0.730	0.666	26.984	31.987	33.543
Obs.	2,102	2,093	2,024	2,102	2,093	2,029
Adj. R^2	0.047	0.038	0.087	0.119	0.061	0.066
Ever changed employer	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All panels report OLS estimates from regressions where the outcome variable is a measure of refugees' employment probability (columns 1-3) and annual earnings measured in 2015 USD (columns 4-6). We regress the outcome on the standardized quality measure of the first employer. The short-run (columns 1 and 4), medium-run (columns 2 and 5), and long-run (columns 3 and 6) effects are defined relative to the year of hiring by the first employer, normalized to 1. This sample consists of refugees subject to the 1986–1998 dispersal policy in Denmark. In Panel A we employ the main specification we use throughout the paper, while in Panel B we do not include fixed effects. In Panel C we restrict the sample to refugees with at least one transition between different employers. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 5: First employers and wages by group

	Ln(Hourly Wage)					
	Natives			Refugees		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
First employer quality	0.025***	0.001	0.001	0.097***	0.038***	0.027***
	(0.002)	(0.001)	(0.001)	(0.014)	(0.008)	(0.010)
boot. p-val.	[0.000]	[0.234]	[0.127]	[0.000]	[0.000]	[0.006]
Obs.	536,804	439,037	367,363	4,188	3,024	2,454
Adj. R^2	0.141	0.081	0.077	0.056	0.039	0.056
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Entry FE	Yes	Yes	Yes	-	-	-
Cohort-by-Origin FE	-	-	-	Yes	Yes	Yes

Notes: This table reports OLS estimates from regressions where the outcome variable is log of real hourly wages (2015 USD) for natives (columns 1-3) and refugees (columns 4-6). We regress the outcome on the standardized quality measure of the first employer. The short-run (columns 1 and 4), medium-run (columns 2 and 5), and long-run (columns 3 and 6) effects are defined relative to the year of hiring by the first employer, normalized to 1. The sample of refugees consists of individuals subject to the 1986–1998 dispersal policy in Denmark. The sample of natives consists of individuals entering the labor market for the first time in the same years of the dispersal policy. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 6: Effects of connections on firm hiring

	(1)	(2)	(3)	(4)
	Hired by j	Hired by j	Hired by j	Hired by j
Connection	0.269***	0.269***	0.0556***	0.108***
	(0.0322)	(0.0322)	(0.0193)	(0.0222)
Constant	0.340***	0.340***	0.199*	0.185*
	(0.117)	(0.117)	(0.114)	(0.111)
Observations	2,370,224	2,370,224	2,370,224	2,366,900
E[Y | D=0]	0.083	0.083	0.083	0.082
Individual controls	Yes	Yes	Yes	Yes
Education controls	No	Yes	Yes	Yes
Firm FE	No	No	Yes	-
Firm-by-Cohort FE	No	No	No	Yes

Notes: The table reports OLS estimates from a linear probability model where the outcome variable takes value 1 if refugee i is hired by establishment j and 0 otherwise, and the main regressor ("Connection") takes value 1 if one member of refugee's i local co-ethnic network at arrival works in establishment j and 0 otherwise. Observations are refugee-potential establishment dyads. Robust SE in parentheses are clustered at the establishment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 7: Effects of exposure to local firms

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Geographic exposure						
Avg. quality	-0.035**	0.014	-0.019	-1.313**	-1.002	-1.291
	(0.018)	(0.026)	(0.037)	(0.514)	(1.123)	(1.736)
boot. p-val.	[0.077]	[0.640]	[0.722]	[0.018]	[0.441]	[0.530]
Co-national share	-0.001	-0.006	-0.001	-0.022	-0.313	-0.194
	(0.004)	(0.005)	(0.006)	(0.127)	(0.232)	(0.372)
Emp. NW immigrants	0.005	0.002	0.005	0.008	-0.001	0.098
	(0.006)	(0.008)	(0.011)	(0.210)	(0.386)	(0.567)
Mean of Y	0.104	0.259	0.325	2.983	8.803	13.871
Obs.	15,554	15,062	14,701	15,554	15,062	14,701
Adj. R^2	0.176	0.204	0.196	0.151	0.159	0.148
Panel B: Ever employed one co-national						
Avg. quality	0.004	0.012**	0.006	0.081	0.527*	0.322
	(0.003)	(0.006)	(0.007)	(0.112)	(0.294)	(0.409)
boot. p-val.	[0.216]	[0.066]	[0.352]	[0.487]	[0.097]	[0.465]
Co-national share	-0.002	-0.005	-0.001	0.043	-0.125	0.139
	(0.004)	(0.006)	(0.007)	(0.131)	(0.303)	(0.404)
Emp. NW immigrants	-0.004	-0.003	0.006	-0.149	-0.320	0.424
	(0.011)	(0.012)	(0.016)	(0.361)	(0.560)	(0.824)
Mean of Y	0.106	0.261	0.328	3.034	8.949	14.040
Obs.	11,401	11,086	10,859	11,401	11,086	10,859
Adj. R^2	0.199	0.221	0.206	0.181	0.179	0.156
Panel C: Network exposure						
Avg. quality	0.008**	0.013**	0.005	0.231**	0.558**	0.342
	(0.003)	(0.006)	(0.007)	(0.107)	(0.253)	(0.379)
boot. p-val.	[0.017]	[0.085]	[0.511]	[0.037]	[0.052]	[0.400]
Co-national share	-0.002	-0.005	-0.004	0.075	-0.139	-0.001
	(0.004)	(0.007)	(0.008)	(0.140)	(0.319)	(0.435)
Emp. NW immigrants	-0.000	-0.001	0.002	0.094	0.211	0.399
	(0.012)	(0.014)	(0.019)	(0.408)	(0.603)	(0.974)
Mean of Y	0.107	0.262	0.331	3.080	8.979	14.156
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.205	0.226	0.210	0.185	0.180	0.159
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of the effect of exposure to employer quality on employment and earnings of refugees subject to the 1986–1998 dispersal policy. To compute effects in the short-run, medium-run, and long-run, we set the year of admission to Denmark to 1. The quality measure used in Panel A is the average establishment effect for establishments active in the municipality of assignment at the time of arrival. The quality measure in Panel B is the average establishment effect for establishments active in the municipality of assignment at the time of arrival that are hiring or have previously hired a co-national of the newly arrived refugee. The quality measure in Panel C is the average establishment effect for establishments active in the municipality of assignment at arrival that are hiring a co-national of the newly arrived refugee. Individual controls include variables observed by authorities in the dispersal process. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 8: Characteristics of individual refugees matched with network

	Unmatched			Matched			
	Count (1)	Mean (2)	SD (3)	Count (4)	Mean (5)	SD (6)	Diff. (7)
Age at entry	5,305	29.84	7.86	10,273	29.85	8.45	0.010
Female	5,305	0.38	0.48	10,273	0.44	0.50	0.062***
Married	5,305	0.74	0.35	10,273	0.73	0.36	-0.006
Pre-arrival academic education	5,305	0.11	0.32	10,273	0.10	0.31	-0.010
Latin mother tongue	5,305	0.39	0.49	10,273	0.30	0.46	-0.092**
Predominantly muslim country	5,305	0.83	0.30	10,273	0.74	0.39	-0.097***
Mean employment dummy	5,305	0.21	0.26	10,273	0.23	0.28	0.015
First (log) labor income	3,403	8.39	1.66	6,555	8.53	1.60	0.142**
Mean complex job dummy	5,305	0.11	0.21	10,272	0.13	0.22	0.017***
(log) Conationals in initial municipality	3,997	2.58	1.56	10,124	5.23	1.63	2.649***
Conationals share, initial municipality	5,305	0.08	0.12	10,273	0.27	0.22	0.188***
Urban, initial municipality	5,301	0.28	0.45	10,273	0.72	0.45	0.437***
Initial Municipality Pop. share of country total (18-65)	5,301	0.01	0.01	10,273	0.04	0.03	0.029**
Initial Municipality Empl. rate (18-65), Any Empl.	5,301	69.42	3.67	10,273	68.90	2.78	-0.523
Initial Municipality Empl. rate (Nonwestern imm., 18-65), Any Empl.	5,301	46.56	11.87	10,273	42.98	7.74	-3.587***
(log) Avg. labor income in initial municipality (USD 2015)	5,301	10.36	0.13	10,273	10.36	0.11	-0.002
Share college educated in initial municipality (18-65)	5,301	0.15	0.05	10,273	0.18	0.05	0.031***

Notes: This table displays characteristics (mean and standard deviation) for individual refugees subject to the 1986–1998 dispersal policy and included in our sample, separated into those not matched with a network (columns 1-3) and those matched matched with a network (columns 4-6). The difference in means is presented in column (7). Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 9: Network quality decomposition: Firms and members

	Employment			Earnings		
	Yr. 1-5 (1)	Yr. 6-10 (2)	Yr. 11-15 (3)	Yr. 1-5 (4)	Yr. 6-10 (5)	Yr. 11-15 (6)
Panel A: Average network earnings						
Avg ln(earnings)	0.003	0.005	0.005	-0.020	0.243	0.054
	(0.005)	(0.007)	(0.010)	(0.169)	(0.346)	(0.532)
Mean Y	0.104	0.257	0.321	2.965	8.870	13.804
Obs.	10,382	10,079	9,868	10,382	10,079	9,868
Adj. R^2	0.193	0.218	0.204	0.171	0.175	0.154
Panel B: Average of individual FEs as quality of members						
Avg. employer quality	0.009**	0.012*	0.001	0.248**	0.478	0.225
	(0.004)	(0.007)	(0.007)	(0.117)	(0.292)	(0.419)
boot. p-val.	[0.019]	[0.144]	[0.890]	[0.044]	[0.134]	[0.613]
Avg. member quality	-0.003	-0.003	-0.001	-0.068	-0.193	-0.004
	(0.003)	(0.005)	(0.006)	(0.111)	(0.217)	(0.315)
boot. p-val.	[0.417]	[0.595]	[0.920]	[0.565]	[0.412]	[0.990]
Mean Y	0.107	0.262	0.331	3.069	8.956	14.163
Obs.	9,496	9,234	9,062	9,496	9,234	9,062
Adj. R^2	0.208	0.226	0.212	0.183	0.180	0.160
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Average of individual FEs as quality of members						
Avg. employer quality	0.011***	0.017**	0.006	0.364***	0.730***	0.506
	(0.003)	(0.006)	(0.008)	(0.104)	(0.276)	(0.404)
boot. p-val.	[0.003]	[0.043]	[0.346]	[0.005]	[0.028]	[0.233]
Avg. member quality	0.006	0.008	-0.002	-0.032	-0.035	-0.045
	(0.015)	(0.023)	(0.025)	(0.123)	(0.279)	(0.345)
boot. p-val.	[0.575]	[0.994]	[0.774]	[0.795]	[0.887]	[0.880]
Mean Y	0.107	0.262	0.331	3.070	8.954	14.162
Obs.	9,498	9,236	9,064	9,498	9,236	9,064
Adj. R^2	0.192	0.220	0.207	0.167	0.174	0.156
Panel D: Share with academic education as quality of members						
Avg. employer quality	0.010***	0.017**	0.008	0.327***	0.626***	0.466
	(0.003)	(0.007)	(0.008)	(0.121)	(0.317)	(0.478)
boot. p-val.	[0.004]	[0.048]	[0.295]	[0.015]	[0.092]	[0.364]
Avg. member quality	0.006	0.027	0.003	-0.014	0.802	-0.075
	(0.015)	(0.023)	(0.025)	(0.552)	(1.089)	(1.392)
boot. p-val.	[0.538]	[0.123]	[0.625]	[0.819]	[0.189]	[0.392]
Mean Y	0.106	0.262	0.332	3.072	8.985	14.259
Obs.	9,137	8,830	8,712	9,137	8,830	8,712
Adj. R^2	0.190	0.215	0.203	0.165	0.168	0.152
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates for network measures of firm quality and member quality included together in our main estimating equation. Panel A reports estimates for the effect of aggregate network quality, using average earnings in log in the network. Panels B, C and D include controls for average member quality along with average firm quality (AKM-based individual effects in Panels B and C, and share of highly educated members in Panel D). Panel C and D use a less demanding set of fixed effects. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 10: Job search outcomes: Firm quality

	Firm FE			Firm FE			Firm Value Added		
	(1) Yr. 1-5	(2) Yr. 6-10	(3) Yr. 11-15	(4) Yr. 1-5	(5) Yr. 6-10	(6) Yr. 11-15	(7) Yr. 1-5	(8) Yr. 6-10	(9) Yr. 11-15
Panel A: Network quality, unweighted									
Avg. Quality	0.012**	0.011**	0.008**	0.009	0.008**	0.005	0.027	-0.014	-0.006
	(0.006)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.032)	(0.014)	(0.018)
boot. p-val.	[0.037]	[0.020]	[0.061]	[0.130]	[0.053]	[0.315]	[0.531]	[0.330]	[0.762]
Mean Y	0.965	0.969	0.977	0.965	0.969	0.977	0.063	0.095	0.088
Obs.	1,535	2,295	2,012	1,535	2,295	2,012	1,415	2,639	2,757
Adj. R^2	0.019	0.014	0.010	0.023	0.024	0.012	0.054	0.031	0.052
Panel B: Network quality									
Share Top-Quartile	0.026	0.040**	0.020	0.013	0.027**	0.010	0.198**	-0.004	0.008
	(0.012)	(0.016)	(0.013)	(0.013)	(0.014)	(0.010)	(0.096)	(0.046)	(0.055)
boot. p-val.	[0.131]	[0.009]	[0.122]	[0.455]	[0.039]	[0.524]	[0.080]	[0.927]	[0.876]
Mean Y	0.965	0.969	0.977	0.965	0.969	0.977	0.063	0.095	0.088
Obs.	1,535	2,295	2,012	1,535	2,295	2,012	1,415	2,639	2,757
Adj. R^2	0.018	0.015	0.009	0.022	0.024	0.012	0.058	0.031	0.052
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	No	No	No	No	No	No
Municipality controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports OLS estimates for network measures of firm quality in our main estimating equation, using as outcomes the AKM-based establishment fixed effect (columns 1 to 6) and firm value added (columns 7 to 9) of refugees' employers. The latter is computed by purging the log of firm value added per capita of year effects to capture the permanent component of firm-level productivity. To measure exposure to employers, Panel A uses the baseline average quality of firms in the network, while Panel B uses the share of top-quartile firms in the network. Specifications in columns 1 to 3 do not include municipality of assignment fixed effects or controls (we onl include individual-level controls), while columns 4 to 9 add municipality of assignment controls (average employer quality in the municipality, share of college-educated individuals aged 18-65, population share of the country total aged 18-65). Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 11: Job search outcomes: Job tasks and geographic mobility

	Complex Job			Mobility		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Network exposure, Unweighted						
Avg. quality	0.006	0.006	0.001	0.008	0.012	0.017*
	(0.006)	(0.007)	(0.007)	(0.011)	(0.010)	(0.010)
boot. p-val.	[0.306]	[0.372]	[0.893]	[0.495]	[0.275]	[0.123]
Mean Y	0.117	0.260	0.286	0.347	0.459	0.534
Obs.	10,245	9,970	9,778	10,245	9,970	9,778
Adj. R^2	0.040	0.114	0.129	0.184	0.209	0.238
Panel B: Network exposure						
Share Top-Quartile	0.029*	0.040**	0.030	-0.002	-0.004	0.011
	(0.017)	(0.018)	(0.019)	(0.037)	(0.033)	(0.035)
boot. p-val.	[0.114]	[0.035]	[0.140]	[0.953]	[0.905]	[0.768]
Mean Y	0.117	0.260	0.286	0.347	0.459	0.534
Obs.	10,245	9,970	9,778	10,245	9,970	9,778
Adj. R^2	0.040	0.114	0.129	0.184	0.209	0.238
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports OLS estimates for network measures of firm quality in our main estimating equation, where the outcome is instead an index of job quality (columns 1 to 3) and an indicator for geographic mobility after initial assignment (columns 4 to 6). Job quality is defined on the basis of task content, using an index equal to the ratio of communication and cognitive tasks to manual tasks. To measure exposure to employers, Panel A uses the baseline average quality of firms in the network, while Panel B uses the share of top-quartile firms in the network. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table 12: Use of job referrals: Evidence of implications

	Wages				Turnover			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Member Present	-0.025	0.033	0.049	0.065*	-0.106***	-0.131***	-0.108**	0.006
	(0.045)	(0.030)	(0.039)	(0.037)	(0.040)	(0.049)	(0.055)	(0.065)
Member * Tenure	0.004	-0.002	-0.006	-0.008*	0.003	0.009	0.008	-0.003
	(0.007)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)
Obs.	31,943	15,480	6,059	9,092	32,034	15,507	6,068	9,120
Adj. R^2	0.154	0.145	0.182	0.573	0.083	0.076	0.079	0.165
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Productivity Tercile FE	No	Yes	Yes	-	No	Yes	Yes	-
First Jobs Sample	No	No	Yes	Yes	No	No	Yes	Yes
Establishment FE	No	No	No	Yes	No	No	No	Yes

Notes: This table reports OLS estimates from equation 5. The outcome in columns 1 to 4 is log wages, while in columns 5 to 8 it is an indicator variable for leaving one's current employer. "Member present" is an indicator used as a proxy for job referral, equal to 1 if a newly arrived refugee starts a job in an establishment where at least one member of their initial local co-ethnic network is still employed at the time of job start. Individuals and establishment controls are listed in the text. Specifications in columns 2, 3, 6, and 7 include terciles of establishment productivity fixed effects, while columns 3, 4, 7, and 8 restrict the sample to job spells that are each refugee's first in Denmark. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table 13: Effect of network employer quality by network size

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Avg. quality	0.016***	0.011	0.000	0.476***	0.762**	0.340
	(0.004)	(0.008)	(0.008)	(0.159)	(0.318)	(0.472)
boot. p-val.	[0.001]	[0.175]	[0.965]	[0.010]	[0.025]	[0.515]
Small Network	0.164**	-0.101	-0.130	4.427	-1.516	-1.787
	(0.084)	(0.108)	(0.117)	(3.031)	(4.626)	(5.867)
Large Network	0.166**	0.055	-0.009	6.402***	10.263*	4.013
	(0.065)	(0.123)	(0.124)	(2.236)	(5.383)	(6.397)
Avg. quality*Small N.	-0.017*	0.013	0.016	-0.416	0.270	0.316
	(0.009)	(0.011)	(0.012)	(0.328)	(0.488)	(0.595)
boot. p-val.	[0.070]	[0.235]	[0.197]	[0.228]	[0.599]	[0.579]
Avg. quality*Large N.	-0.018**	-0.006	0.001	-0.692***	-1.106*	-0.435
	(0.008)	(0.014)	(0.013)	(0.260)	(0.634)	(0.697)
boot. p-val.	[0.087]	[0.712]	[0.933]	[0.024]	[0.163]	[0.567]
Obs.	10,097	9,827	9,637	10,097	9,827	9,637
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates from regressions of refugees' outcomes on average employer quality in the network interacted with dummy variables that capture different terciles of the network size distribution. Mid-sized network is the omitted category. Controls used in main equation 3 are included. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Appendix A Theoretical Model

This section develops an augmented Diamond-Mortensen-Pissarides framework to conceptualize our findings on the role of network quality in shaping refugees' labor market outcomes. We extend the classic DMP framework in three main directions. First, we allow for two types of workers: outsiders and insiders. We think of outsiders as newly arrived immigrants (refugees, in our specific case) who lack knowledge of the search process at destination. Insiders, in contrast, are immigrants who have already spent some time in the host country's labor market and know how and where to look for job opportunities. After ending their first employment spell, outsiders permanently become insiders. Second, we capture the important role of social networks by allowing insiders to share job opportunities with outsiders, should they receive information about vacancies that they choose not to accept. To model this, we introduce on-the-job search for employed insiders and allow outsiders to meet a firm only if its vacancy was previously rejected by an insider. Third, we link the quality of job opportunities available to outsiders to insider decisions, ensuring that insiders' employment conditions affect the labor market outcomes of outsiders. We then derive two predictions regarding the employment and wages of newly arrived network members.

A.1 Framework

Specifically, we consider the following extension to the DMP model. Time is continuous with an infinite horizon, and all agents discount future at rate r. The labor force is normalized to 1. Workers exit the labor market (retire or emigrate) at Poisson rate δ, and each worker who leaves is immediately replaced by a new worker, who one should think of as a newly arrived immigrant. Naturally, new workers enter the labor market as unemployed. This replacement is crucial in our model because these newly arrived immigrants will be a special category. There is a large mass of ex-ante homogeneous firms that can enter the labor market with one vacancy. As is standard, the measure of active firms in equilibrium is determined by free entry. Firms who decide to enter the labor market and search for workers must pay a flow recruiting cost c. Existing jobs get terminated at the job destruction rate λ. Generally, job matches produce an amount p of the numeraire good, but this general productivity will be affected by an idiosyncratic productivity x.

We now provide a description of the various types of workers. We will refer to newly arrived workers who just replaced a retired or emigrated worker as outsiders (denoted by o). Outsiders enter the model as unemployed and with a low knowledge of the host country's labor market. After leaving their first job, outsiders permanently become insiders (denoted by i). This implies that at any point in time there are $2^2 = 4$ types of workers. Workers can be unemployed or employed, and they can be insiders or outsiders. We assume that these workers are part of a network that allows for sharing of information about vacancies, flowing from insiders to outsiders.

Let us now turn to the details of the matching process. In particular, we need to account for on-the-job search of insider workers. We denote the aggregate matching function by $m = m(v, u + e)$, where u denotes unemployment, v vacancies, and e the number of employed job seekers. As usual, the rate at which workers arrive to vacancies is a function of the ratio of vacancies to all job seekers and is $q(\theta) \equiv m\left(1, \frac{u+e}{v}\right)$. Market tightness θ denotes the ratio $v/(u+e)$. We assume that employed and unemployed workers are equally good at finding jobs, so that jobs arrive to each searching worker at the same rate, which is equal to $\theta q(\theta)$. Importantly, since outsiders have only a very limited knowledge of the search process in the host country, we assume that unemployed outsiders can only bump into a vacancy that is rejected by employed insiders. Clearly, unemployed outsiders bumping into a vacancy will accept it, instead of passing the offer to their network.

An employed insider will bump into a vacancy with probability $\theta q(\theta)$. When the meeting happens, a value for the productivity of the new potential match is drawn for a general distribution $G(x)$, where the productivity of the new potential match, x', is independent of the current productivity, x, and is irreversible. The employed worker has the choice either to continue production at their current productivity level and firm, or to separate from their firm and accept the new offer. If the value of the new offer is not high enough, the employed insider passes the offer to an unemployed outsider in their network who will start producing at the already drawn productivity level x'. Since the decision by the employed insider depend on the level of the new productivity drawn, i.e., whether x' is above or below a threshold x_r, an employed insider will resign and accept the new offer with probability $[1-G(x_r)]\theta q(\theta)$ and an unemployed outsider will receive job offer with probability $[G(x_r)]\theta q(\theta)$. Since they are not insiders (yet), employed outsiders cannot engage in on-the-job search.

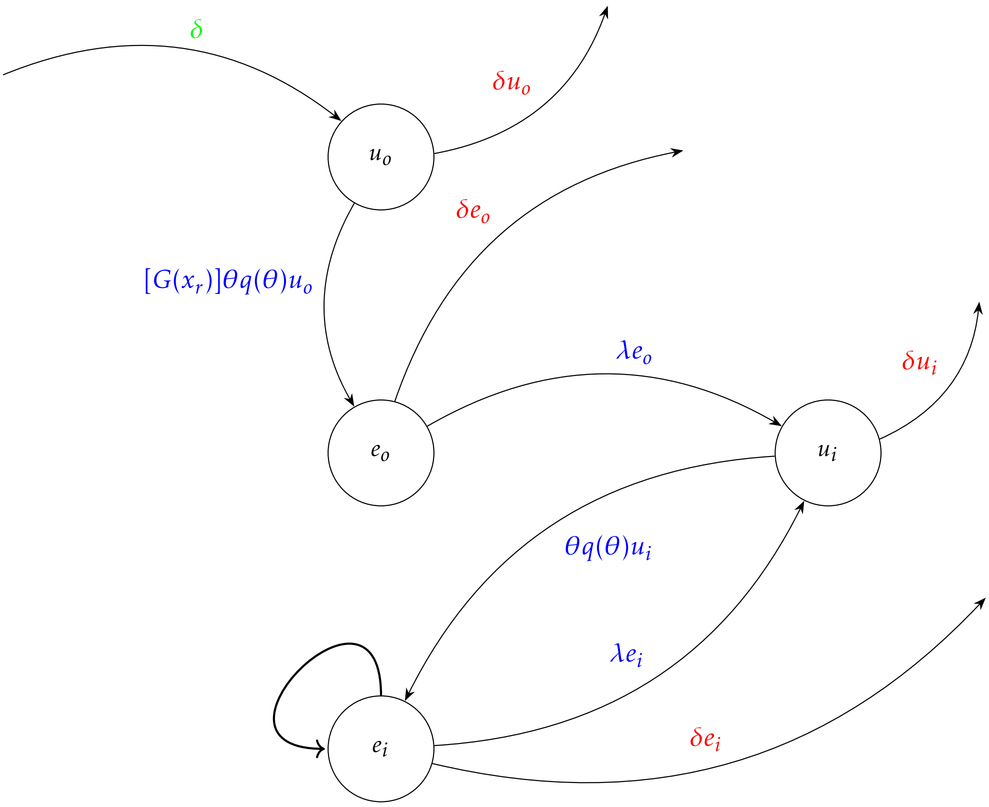
To retain tractability, we assume that on-the-job seekers who accept a new offer need to pay a relocation cost $\sigma(x)$. Intuitively, the higher the quality of their current match the more costly the separation will be. This assumption will result in a reservation threshold x_r that satisfies the following condition:

$$[1-G(x)]\theta q(\theta)[W_i(x')-W(x)] = \sigma(x) \tag{10}$$

For our purposes, we focus on the implications of an higher x_r. In other words, we will explore what happens to outsiders in a world when they arrive to a high-quality network (i.e, where insiders are matched with highly productive, high-paying firms, resulting in high x_r) vs. a world when they arrive to a low-quality network (i.e., low x_r).

We close the model with a few more standard assumptions. After the matching has concluded and firms have met the various types of workers, the two parties negotiate over the wage using Nash Bargaining. We will let $\beta \in [0,1]$ denote the bargaining power of the worker. All unemployed workers enjoy a benefit z, which we think of as the utility of leisure and/or the value of home production. Full description of worker flows, value functions, bargaining problems, and definition of equilibrium is discussed in the next sections.

A.2 Worker flows



A.3 Beveridge curves

We start with: $\dot{u}_o = \delta - \delta u_o - [G(x_r)]\theta q(\theta)u_o = 0$

$$u_o = \frac{\delta}{\delta + G(x_r)\theta q(\theta)} \tag{11}$$

We have: $\dot{e}_o = [G(x_r)]\theta q(\theta)u_o - \delta e_o - \lambda e_o = 0$

$$e_o = \frac{G(x_r)\theta q(\theta)\delta}{(\lambda+\delta)[\delta + G(x_r)\theta q(\theta)]} \tag{12}$$

We then have: $\dot{u}_i = \lambda e_o + \lambda e_i - \delta u_i - \theta q(\theta)u_i = 0$

$$u_i = \frac{\lambda(e_o + e_i)}{\delta + \theta q(\theta)} \tag{13}$$

So we solve: $\dot{e}_i = \theta q(\theta)u_i - \lambda e_i - \delta e_i = 0$

$$e_i = \frac{\theta q(\theta)u_i}{(\delta+\gamma)} = \frac{[1 - G(x_r)]\theta q(\theta)\lambda([G(x_r)]\theta q(\theta)\delta)\delta(\lambda+\delta+\theta q(\theta))}{(\delta+\gamma)(\delta+[G(x_r)]\theta q(\theta))} \tag{14}$$

And we also obtain:

$$u_i = \frac{\lambda}{\delta+\theta q(\theta)}\left(\frac{G(x_r)\theta q(\theta)\delta + [1-G(x_r)]\theta q(\theta)\lambda([G(x_r)]\theta q(\theta)\delta)\delta(\lambda+\delta+\theta q(\theta))}{(\lambda+\delta)[\delta+G(x_r)\theta q(\theta)]}\right) \tag{15}$$

A.4 Value functions

$$rW_i(x) = w(x) + \lambda(U_i - W_i(x)) + [1 - G(x_r)]\theta q(\theta)(W_i(x') - W(x)) - \delta W_i(x) \tag{16}$$

$$rW_o(x) = w_o(x) + \lambda(U_i - W_o(x)) - \delta W_o(x) \tag{17}$$

$$rU_i = z + \theta q(\theta)(W_i(x) - U_i) - \delta U_i \tag{18}$$

$$rU_o = z + G(x_r)\theta q(\theta)(W_i(x) - U_o) - \delta U_o \tag{19}$$

$$rV = -pc + q(\theta)[J_i(x)(1 - G(x_r)) + J_o(x)G(x_r)] \tag{20}$$

$$\begin{aligned} rJ_i &= p - w_i(x) + \lambda[V - J_i(x)] + [1 - G(x_r)]\theta q(\theta)[V - J_i(x)] + \delta[V - J_i(x)] \\ &= p - w_i(x) + (\lambda + \delta + [1 - G(x_r)]\theta q(\theta))[V - J_i(x)] \end{aligned} \tag{21}$$

$$rJ_o = p - w_o(x) + (\lambda + \delta)[V - J_o(x)] \tag{22}$$

We can rewrite equation 20 setting $V = 0$ to obtain the free-entry condition:

$$pc = q(\theta)[J_i(x)(1 - G(x_r)) + J_o(x)G(x_r)] \tag{23}$$

but more in general define q_i as the probability that a firm with a vacancy meets an insider and q_o as the probability that a firm with a vacancy meets an outsider.

A.5 Bargaining problem

We solve the standard Nash bargaining problem to determine wages of outsider and insider workers, so that each party in the trade will enjoy a fraction of the total surplus of the match, where that fraction will be equal to her bargaining power (β for workers and $1 - \beta$ for firms). For outsiders, the following condition must be satisfied:

$$(1 - \beta)(W_o(x) - U_o) = \beta J_o(x) \tag{24}$$

We let the probability that a firm with a vacancy meets an insider be $q_i = q(\theta)\left(1 - \frac{e_i}{e_i + u_i} G(x_r)\right)$, and the probability of meeting an outsider be $q_o = q(\theta)\frac{e_i}{e_i+u_i}G(x_r)$. After substituting, we obtain:

$$
\begin{aligned}
w_o(x) = \beta & \left[1 + \frac{(\lambda+n+\delta)\theta q(\theta) G(x_r)}{(r+\delta)} q_o c\right] p(x) \\
& + (1-\beta)\left(\frac{\lambda\theta q(\theta)}{(r+\delta)(r+\delta+\lambda+\theta q(\theta))} + 1\right) z \\
& - (1-\beta)\frac{\lambda\theta q(\theta)}{(n+\delta)(r+\delta+\lambda+\theta q(\theta))}[1-G(x_r)]\theta q(\theta)[W_i(x') - W_o(x)] \\
& + \left[\frac{(\lambda+r+\delta)G(x_r)\beta q_i q_o}{r+\delta+\lambda+\theta q(\theta)[1-G(x_r)]} - \frac{(1-\beta)\lambda}{r+\delta+\lambda+\theta q(\theta)}\right]\frac{\theta q(\theta)}{(r+\delta)} w_i(x)
\end{aligned}
\tag{25}
$$

Not surprisingly, $w_o(x)$ is increasing in both $p(x)$ and z. However, a couple of interesting relationships emerge. First, $w_o(x)$ is a function of $w_i(x)$, as seen from the last term of equation 25. When an outsider meets a firm, accepting a job there is the step that allows them to permanently become an insider. Hence, a higher (future) wage $w_i(x)$ makes the outsider more willing to accept a lower (current) $w_o(x)$ in order to leave their outsider status behind. At the same time, the network introduces a countervailing effect: the higher the wage of insiders, the more likely it is that offers, and especially better ones, will be passed down. This dynamic increases $w_o(x)$ and turns the relationship between $w_o(x)$ and $w_i(x)$ from negative to positive, for sufficiently high levels of x_r. Second, as seen from the third term of equation 25, $w_o(x)$ also negatively depends on $[1-G(x_r)]\theta q(\theta)[W_i(x') - W_o(x)]$, which further captures this channel independently. As insider wages rise, so does the probability of receiving better offers through the network. The behavior and outside option of insiders matter because outsiders will eventually become insiders themselves in the future.

For insiders, an analogous condition must be satisfied:

$$
(1-\beta)(W_i(x) - U_i) = \beta J_i(x) \tag{26}
$$

so that we obtain:

$$
\begin{aligned}
w_i(x) = & \left[\frac{\beta(r+\delta+[1-G(x_r)]\theta q(\theta)+\lambda)(r+\delta) + (1-\beta)(r+\delta+[1-G(x_r)]\theta q(\theta))}{(r+\delta+[1-G(x_r)]\theta q(\theta))(r+\delta+\beta\theta q(\theta)) + \lambda(r+\delta)}\right] p(x) \\
& + (1-\beta)\left[\frac{(r+\delta+[1-G(x_r)]\theta q(\theta)+\lambda)(r+\delta+[1-G(x_r)]\theta q(\theta))}{r+\delta+[1-G(x_r)]\theta q(\theta))(r+\delta+\beta\theta q(\theta)) + \lambda(r+\delta)}\right] z \\
& - (1-\beta)\frac{(r+\delta+[1-G(x_r)]\theta q(\theta)+\lambda)(r+\delta)}{r+\delta+[1-G(x_r)]\theta q(\theta))(r+\delta+\beta\theta q(\theta)) + \lambda(r+\delta)}[1-G(x_r)]\theta q(\theta) W_i(x')
\end{aligned}
\tag{27}
$$

A.6 Steady state equilibrium

A steady state equilibrium in this model is a list of wages for the two types of workers (w_i and w_o), a measure of vacant firms v, and measures of unemployed and employed workers in the various states (u_o, e_o, u_i, e_i), satisfying: the free entry condition (equation 23), the two wage curves (equations 25 and 27), and the four Beveridge curves reported above.

A.7 Equilibrium relationship

Refugees arrive into different networks. Higher levels of x_r correspond to high-quality networks, where members have highly productive matches that can be seen as employment in highly productive, high-paying firms. Our exercise of interest consists of considering a higher x_r to examine the implications for outsiders' outcomes, specifically employment and wages.

Proposition 1: *Higher network quality increases the employment level of outsiders.*

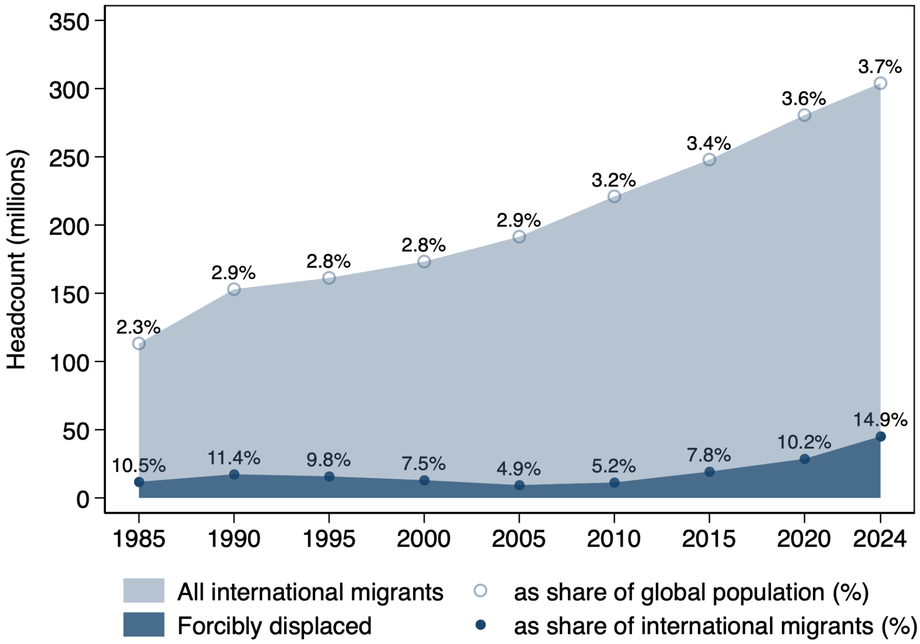
This follows directly from equation 12. Intuitively, since they are working at high-pay premium firms, employed insiders of high-quality networks are more likely to reject and pass an offer from a vacant firm to members of their network, increasing the employment of outsiders, compared to low-quality networks.

Proposition 2: *Higher network quality has a positive effect on wages of outsiders.*

This follows from the right-hand-side terms in equation 25. On the one hand, unemployed outsiders are willing to accept lower wages when employed insiders in their network have high-paying jobs, as they anticipate that working will allow them to abandon their status of outsiders and access high future wages (expectations channel). On the other hand, when insiders are employed in highly productive, high-paying firms, wage offers that are passed along rather than accepted are higher, shifting the distribution of wages available through the network to the right (stochastic dominance channel) and offsetting the first effect, for sufficiently high levels of x_r.

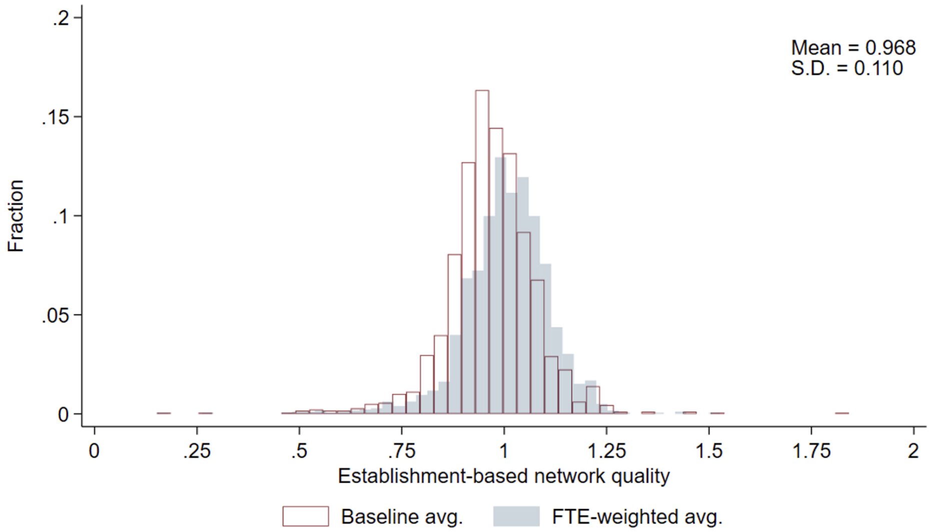
Appendix B Supplementary Analysis

Figure B1: Global forcibly displaced population



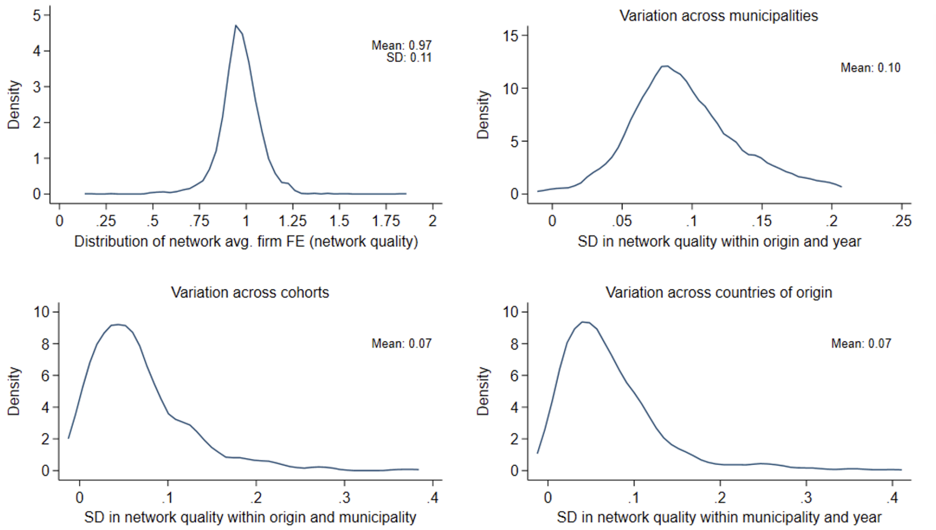
Notes: This figure plots the worldwide number of international migrants (light blue) and the number of forcibly displaced people, including refugees, asylum seekers, and others in need of international protection but excluding internally displaced persons (dark blue), from 1985 to 2024. It also reports the share of international migrants in the global population and the share of forcibly displaced group within the total international migrant population.
Source: United Nations High Commissioner for Refugees (UNHCR); United Nations Department of Economic and Social Affairs (UN DESA).

Figure B2: Variation in main treatment variables



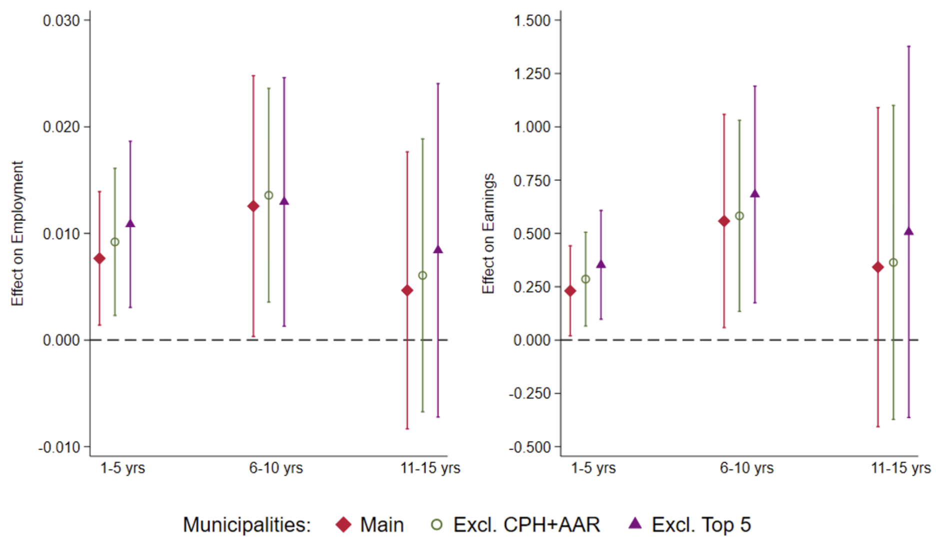
Notes: This figure displays the variation in our analysis sample for the main network quality measures (red-outlined bars for the unweighted average, light blue bars for the average weighted by full-time equivalents). The x-axis is measured in log wage points.

Figure B3: Dimensions of variation



Notes: This figure shows how the main treatment variable (average firm fixed effect in the network) varies across the three dimensions of the network (municipality, cohort, origin). The top left corner plots the overall variation. The top right panel shows the distribution of standard deviations in network quality across municipalities, holding origin and cohort (year) fixed. The bottom left (right) corner plots the distribution of the standard deviations in the network quality across cohorts (countries of origin), fixing municipality and origin (year).

Figure B4: Heterogeneity analysis by municipality



Notes: This figure plots the estimates and 95 percent confidence intervals of the differential effect of network quality (unweighted measure) on the main outcomes (employment in the left panel and earnings in the right panel) by municipality of assignment. Three groups of municipalities of assignment are considered: all municipalities; all excluding Copenhagen and Aarhus (top 2); and all excluding Copenhagen, Aarhus, Odense, Aalborg and Esbjerg (top 5). Each outcome is examined over three time intervals: short run (1 to 5 years since arrival in Denmark), medium run (6 to 10 years), and long run (11 to 15 years).

Table B1: Sample of refugees pre- and post-restrictions: Balance table

	Excluded			Analysis sample			
	Count	Mean	SD	Count	Mean	SD	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age at Entry	4,131	28.41	7.11	15,578	29.84	8.25	1.431***
Female	4,131	0.45	0.50	15,578	0.42	0.49	-0.031**
Married/cohabiting	4,131	0.68	0.47	15,578	0.65	0.48	-0.024**
Number of Children	4,131	0.91	1.54	15,578	1.06	1.62	0.148***
Academic Education (prior to arrival)	1,293	0.26	0.44	6,566	0.26	0.44	-0.004
Latin Alphabet of Mother Tongue	4,131	0.49	0.50	15,578	0.33	0.47	-0.165***
From Predominantly Muslim Country	4,131	0.23	0.42	15,578	0.14	0.35	-0.091***
Asia	4,131	0.07	0.25	15,578	0.15	0.36	0.081***
Middle East	4,131	0.48	0.50	15,578	0.66	0.48	0.171***
Africa	4,131	0.45	0.50	15,578	0.20	0.40	-0.252***
Employment (any)	4,131	0.15	0.22	15,578	0.22	0.27	0.074***
Annual Earnings (thousand USD)	4,131	5325	10075	15,578	8245	12926	2919***
Avg. Hourly Wage Rate in Estab.	1,575	28.24	12.28	8,080	28.42	12.56	0.187
Complex Job (indicator)	4,127	0.09	0.19	15,577	0.12	0.22	0.028***

Notes: This table displays characteristics (mean and standard deviation) for individual refugees subject to the 1986–1998 dispersal policy, separated into those observed for less than 15 years in Denmark and later-arriving spouses (columns 1-3), both excluded from the final analysis sample, and those included (columns 4-6). The difference in means is presented in column (7). Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table B2: Regressions of standardized AKM FE's on establishment characteristics

Specification:	Multivariate	Bivariate	Multivariate	Bivariate	Variable Mean / SD
Dependent Var.:	Establishment Quality				
	(1)	(2)	(3)	(4)	(5)
log(Value Added Per Worker)	0.046***	0.246***	0.036***	0.183***	12.711
	(0.005)	(0.004)	(0.005)	(0.004)	[0.769]
log(Average Wage Bill Per Worker)	0.256***	0.408***	0.261***	0.351***	11.997
	(0.006)	(0.005)	(0.006)	(0.005)	[0.744]
log(Employment)	0.072***	0.149***	0.077***	0.139***	2.053
	(0.002)	(0.002)	(0.002)	(0.002)	[1.286]
Exporter (1 = Yes)	–0.010	0.304***	0.017**	0.233***	0.408
	(0.007)	(0.006)	(0.007)	(0.006)	[0.491]
Export/Sales	0.234***	0.642***	0.078**	0.336***	0.075
	(0.023)	(0.044)	(0.019)	(0.029)	[0.212]
High-skill Share	0.372***	0.721***	0.204***	0.439***	0.102
	(0.018)	(0.018)	(0.019)	(0.019)	[0.204]
Mean of Y	0.860	0.860	0.860	0.860	
SD of Y	1.000	1.000	1.000	1.000	
Obs.	125,441	125,441	125,441	125,441	125,441
R-squared	0.108		0.170		
Industry FE	No	No	Yes	Yes	

Notes: The table reports regression results in which standardized average firm AKM fixed effects - calculated for each establishment and then averaged using worker weights - are regressed on firm characteristics for the years 1992–1998. Columns (1) and (3) display estimates from multivariate specifications, whereas columns (2) and (4) show estimates from bivariate regressions. Columns (1) and (2) are estimated without controlling for the 27-group industry codes, whereas columns (3) and (4) include these controls. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. Robust standard errors reported in parenthesis.

Table B3: Regressions of non-standardized AKM FE's on establishment characteristics

Specification:	Multivariate	Bivariate	Multivariate	Bivariate	Variable Mean / SD
Dependent Var.:	Establishment Quality				
	(1)	(2)	(3)	(4)	(5)
log(Value Added Per Worker)	0.012***	0.062***	0.00902***	0.046***	12.711
	(0.001)	(0.001)	(0.00121)	(0.001)	[0.769]
log(Average Wage Bill Per Worker)	0.074***	0.103***	0.0653***	0.088***	11.997
	(0.001)	(0.001)	(0.00156)	(0.001)	[0.744]
log(Employment)	0.018***	0.037***	0.0194***	0.035***	2.053
	(0.001)	(0.000)	(0.00057)	(0.001)	[1.286]
Exporter (1 = Yes)	-0.002	0.077***	0.00421**	0.058***	0.408
	(0.002)	(0.001)	(0.00166)	(0.002)	[0.491]
Export/Sales	0.059***	0.162***	0.0196***	0.084***	0.075
	(0.006)	(0.011)	(0.00367)	(0.007)	[0.212]
High-skill Share	0.094***	0.181***	0.0510***	0.110***	0.102
	(0.004)	(0.004)	(0.00483)	(0.005)	[0.204]
Mean of Y	0.220	0.220	0.220	0.220	
SD of Y	0.250	0.250	0.250	0.250	
Obs.	125,441	125,441	125,441	125,441	125,441
R-squared	0.108		0.170		
Industry FE	No	No	Yes	Yes	

Notes: The table reports regression results in which non-standardized average firm AKM fixed effects - calculated for each establishment and then averaged using worker weights - are regressed on firm characteristics for the years 1992–1998. Columns (1) and (3) display estimates from multivariate specifications, whereas columns (2) and (4) show estimates from bivariate regressions. Columns (1) and (2) are estimated without controlling for the 27-group industry codes, whereas columns (3) and (4) include these controls. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. Robust standard errors reported in parenthesis.

Table B4: First employers and wages on the extended sample

	Ln(Hourly Wage)			Ln(Hourly Wage)		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
First employer quality	0.102***	0.050***	0.032***	0.098***	0.047***	0.027***
	(0.008)	(0.005)	(0.005)	(0.008)	(0.005)	(0.006)
boot. p-val.	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Mean of Y	3.250	3.347	3.352	3.250	3.347	3.352
Obs.	10,029	7,927	6,313	10,016	7,915	6,289
Adj. R^2	0.078	0.065	0.041	0.097	0.085	0.075
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	Yes	Yes	Yes
Cohort-by-Origin FE	No	No	No	Yes	Yes	Yes

Notes: This table reports OLS estimates from regressions where the outcome variable is log of real hourly wages (2015 USD) and main regressor is the standardized quality measure of the first employer. The sample consists of refugees who arrived in Denmark during the 1986–1998 dispersal policy. We present a specification without fixed effects (columns 1-3) and one with the main set of fixed effects (columns 4-6). The short-run (columns 1 and 4), medium-run (columns 2 and 5), and long-run (columns 3 and 6) effects are defined relative to the year of hiring by the first employer, normalized to 1. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table B5: First employers and outcomes of refugee geographic movers

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
First employer quality	0.044***	0.044***	0.037***	3.338***	3.091***	2.908***
	(0.014)	(0.013)	(0.012)	(0.593)	(0.681)	(0.767)
boot. p-val.	[0.008]	[0.008]	[0.014]	[0.000]	[0.001]	[0.003]
Mean of Y	0.713	0.559	0.510	23.833	24.166	25.412
Obs.	1,753	2,371	2,549	1,753	2,373	2,558
Adj. R^2	0.058	0.066	0.104	0.095	0.071	0.095
Ever left assigned municipality	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports OLS estimates from regressions where the outcome variable is a measure of refugees' employment probability (columns 1-3) and annual earnings measured in 2015 USD (columns 4-6). We regress the outcome on the standardized quality measure of the first employer. The short-run (columns 1 and 4), medium-run (columns 2 and 5), and long-run (columns 3 and 6) effects are defined relative to the year of hiring by the first employer, normalized to 1. This sample consists of refugees subject to the 1986–1998 dispersal policy in Denmark who ever moved away from their municipality of assignment. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table B6: Variation in treatment variables: Raw and residualized

	Mean	S.D.	p25	p75
Network quality	0.968	0.110	0.914	1.028
Residuals: net of FEs	-0.000	0.083	-0.039	0.044
Network quality (FTE-weighted)	1.005	0.108	0.950	1.071
Residuals: net of FEs	0.000	0.079	-0.035	0.039
Share top-quartile	0.272	0.314	0.000	0.438
Residuals: net of FEs	0.000	0.243	-0.129	0.090
Observations	1,787			

Notes: This table presents summary statistics for the treatment variables used in the analysis, collapsed at the network level (municipality by year by country of origin). We report statistics for three variables: baseline average network quality (unweighted), average network quality weighted by establishment full-time equivalents, and the share of establishments in the network that fall within the top quartile of the overall quality distribution. For each variable, we report both raw variation and residualized variation after controlling for the main set of fixed effects used in the analysis (municipality fixed effects and origin-by-cohort fixed effects).

Table B7: Municipality characteristics: Correlation matrix

	Avg. Network Quality (1)	Empl. Rate (2)	Empl. Rate, Immigrants (3)	Annual Earnings (4)	Pop. Size (5)	Share of Non-Western Immigrants (6)	Share of Co-Nationals (7)	Urban Area (8)
Avg. Network Quality	1.00							
Empl. Rate	0.30***	1.00						
Empl. Rate, Non-Western Immigrants	0.29***	0.49***	1.00					
Annual Earnings	0.34***	0.72***	0.31***	1.00				
Pop. Size	0.08*	-0.18***	-0.12***	-0.11***	1.00			
Share of Non-Western Immigrants	0.23***	0.17***	0.02	0.29***	0.32***	1.00		
Share of Co-Nationals	-0.20***	-0.27***	-0.34***	-0.33***	-0.06	-0.03	1.00	
Urban Area	0.22***	0.12***	0.01	0.20***	0.44***	0.40***	-0.20***	1.00

Notes: This table presents a correlation matrix of municipality characteristics. Employment rates and earnings are measured as annual averages at the municipality level. For each refugee-sending country, we compute (i) the establishment-based average network quality and (ii) the share of co-nationals already residing in the municipality, both at the municipality–year level. We then average each of these measures across origin countries within every municipality-year cell. The analysis is restricted to municipality–years in which network quality is observed for at least one country of origin. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table B8: Geographic effects

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Geographic exposure, unweighted						
Avg. quality	-0.014***	-0.003	0.006	-0.357***	0.020	0.514*
	(0.004)	(0.006)	(0.006)	(0.131)	(0.209)	(0.280)
boot. p-val.	[0.011]	[0.676]	[0.230]	[0.037]	[0.929]	[0.078]
Co-national share	0.014**	-0.005	-0.007	0.505**	-0.273	-0.275
	(0.006)	(0.006)	(0.007)	(0.199)	(0.218)	(0.343)
Emp. NW immigrants	0.024***	0.016**	0.019**	0.773***	0.563**	0.931**
	(0.006)	(0.007)	(0.008)	(0.197)	(0.264)	(0.399)
Mean Y	0.105	0.259	0.325	2.984	8.803	13.871
Obs.	15,571	15,082	14,722	15,571	15,082	14,722
Adj. R^2	0.084	0.155	0.131	0.069	0.121	0.096
Panel B: Geographic exposure, weighted						
Avg. quality	-0.014***	-0.004	0.005	-0.396***	-0.101	0.371
	(0.003)	(0.006)	(0.006)	(0.109)	(0.203)	(0.289)
boot. p-val.	[0.002]	[0.578]	[0.320]	[0.004]	[0.661]	[0.206]
Co-national share	0.013**	-0.005	-0.006	0.498**	-0.262	-0.247
	(0.006)	(0.006)	(0.007)	(0.198)	(0.217)	(0.336)
Emp. NW immigrants	0.024***	0.017**	0.020**	0.787***	0.595**	0.962**
	(0.006)	(0.007)	(0.008)	(0.199)	(0.262)	(0.392)
Mean Y	0.105	0.259	0.325	2.984	8.803	13.871
Obs.	15,571	15,082	14,722	15,571	15,082	14,722
Adj. R^2	0.083	0.155	0.131	0.069	0.121	0.095
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	No	No	No	No	No
Origin FE	No	No	No	No	No	No
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of the effect of exposure to employer quality on employment and earnings of refugees subject to the 1986–1998 dispersal policy. To compute effects in the short-run, medium-run, and long-run, we set the year of admission to Denmark to 1. The quality measure used in Panel A is the average establishment effect for establishments active in the municipality of assignment at the time of arrival. Individual controls include variables observed by authorities in the dispersal process. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table B9: Effect of network quality in terciles relative to missing network

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Network exposure, Terciles based on unweighted average						
Low-quality	0.001	0.014	0.016	-0.037	0.582	0.404
	(0.006)	(0.013)	(0.015)	(0.211)	(0.539)	(0.808)
Mid-quality	0.011	0.031**	0.024	0.121	0.819	0.550
	(0.007)	(0.013)	(0.016)	(0.227)	(0.564)	(0.819)
High-quality	0.014*	0.019*	0.011	0.390*	0.663	0.330
	(0.007)	(0.012)	(0.014)	(0.235)	(0.531)	(0.725)
Panel B: Network exposure, Terciles based on weighted average						
Low-quality	0.003	0.014	0.018	0.012	0.476	0.508
	(0.006)	(0.011)	(0.014)	(0.179)	(0.500)	(0.765)
Mid-quality	0.016**	0.023*	0.015	0.324	0.721	0.254
	(0.008)	(0.012)	(0.017)	(0.241)	(0.580)	(0.905)
High-quality	0.011	0.025**	0.015	0.262	0.773	0.248
	(0.008)	(0.012)	(0.015)	(0.235)	(0.564)	(0.814)
Obs.	14,728	14,263	13,925	14,728	14,263	13,925
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates from regressions of refugees' outcomes on three dummy variables that capture different terciles of the network quality distribution. Missing network quality is the omitted category. Panel A uses the distribution of unweighted network measure, while Panel B uses the weighted one. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table B10: Effects on the intensive margin of annual earnings

	Earnings			ln(Earnings)		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Conditional on reporting positive earnings, Main FEs						
Avg. quality	0.334	0.789**	-0.090	0.091***	0.079*	-0.045
	(0.243)	(0.369)	(0.524)	(0.034)	(0.040)	(0.046)
boot. p-val.	[0.176]	[0.057]	[0.865]	[0.008]	[0.099]	[0.081]
Co-national share	0.086	0.107	-0.021	0.022	-0.004	-0.007
	(0.062)	(0.067)	(0.550)	(0.038)	(0.038)	(0.047)
Emp. NW immigrants	0.182	0.411	0.441	0.117	0.057	0.026
	(0.608)	(0.760)	(1.230)	(0.084)	(0.083)	(0.113)
Mean Y	8.410	17.579	26.903	1.135	2.021	2.481
Obs.	3,741	5,088	5,137	3,736	5,087	5,137
Adj. R^2	0.154	0.113	0.060	0.100	0.086	0.052
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Conditional on reporting positive earnings, Alternative FEs						
Avg. quality	0.426*	1.000**	0.057	0.094***	0.093**	-0.038
	(0.230)	(0.390)	(0.537)	(0.034)	(0.039)	(0.044)
boot. p-val.	[0.086]	[0.029]	[0.917]	[0.010]	[0.039]	[0.436]
Co-national share	0.050	0.147	-0.077	-0.027	0.020	-0.014
	(0.053)	(0.094)	(0.530)	(0.036)	(0.033)	(0.042)
Emp. NW immigrants	0.275	0.528	0.032	0.105	0.063	0.011
	(0.715)	(0.803)	(1.142)	(0.103)	(0.077)	(0.099)
Mean Y	8.403	17.566	26.892	1.134	2.020	2.478
Obs.	3,738	5,092	5,143	3,736	5,092	5,143
Adj. R^2	0.144	0.112	0.065	0.101	0.088	0.057
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of the effect of exposure to employer quality on earnings of refugees subject to the 1986–1998 dispersal policy, conditional on reporting positive earnings in the interval analyzed. To compute effects in the short-run, medium-run, and long-run, we set the year of admission to Denmark to 1. The quality measure used in both Panels is the average establishment effect for establishments active in the municipality of assignment at the time of arrival. Individual controls include variables observed by authorities in the dispersal process. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Appendix C AKM Model with Immigrant and Native Groups

A natural concern is that establishment effects estimated using the entire sample of workers active in Denmark may not reflect the actual workplace quality experienced by foreign-born individuals, and by refugees in particular. This could occur if employers engage in forms of within-firm differentiation between groups, potentially driven by differences in bargaining power, outside options, firm-specific labor supply elasticities, or reservation wages (Adda et al., 2022; Arellano-Bover and San, 2023; Dustmann et al., 2024a). Such a scenario would be problematic if the resulting workplace ranking of quality differ substantially between natives and immigrants.

To address this concern, and building on the previous AKM decomposition, we estimate a model with separate workplace effects by nativity for firms in the "dual-connected set"—i.e., the set of firms included in the connected sets for both natives and immigrants (Dostie et al., 2023; Drenik, Jäger, Plotkin, and Schoefer, 2023). In this model, y_{it} is generated by:

$$log(y_{it}) = \alpha_i + \psi^{B(i)}_{\mathbf{J}(i,t)} + x_{it}\beta + \epsilon_{it}, \tag{28}$$

where $\psi^{B(i)}_{j(i,t)}$ represents nativity-specific, time-invariant firm fixed effects, with birthplace $B(i)$ equal to N if worker i is native-born and to M if worker i is foreign-born—i.e., $B(i) \in N, M$. Pay premia offered by workplaces are allowed to vary by group but are assumed to be the same for all workers within each group. Clearly, since we only observe one fixed birthplace per worker, we cannot absorb potential average differences between native-born and foreign-born workers.

The magnitudes of the pay premia for native-born workers are only identified relative to those of immigrant workers by applying a normalization *across* the groups.[87] Therefore, we shift both the native-specific and immigrant-specific firm effect distributions by normalizing the mean of native-specific workplace effects to zero, and plot the resulting distributions in Figure C1. Workplace effects for immigrant workers are shifted downward compared to those for native workers. In the sample of "dual-connected" firms, the average pay premium for immigrants is -0.53 relative to the mean of workplace effects of native workers normalized to zero—i.e., the average pay premium is 53 log points lower for immigrants compared to natives. In other words, immigrant workers receive lower pay premia than native workers.

To examine whether firms extend their pay premia to immigrant labor, we compare the workplace pay premia earned by native workers and immigrant workers at the same workplace, following an approach similar to Arellano-Bover and San (2023) and Drenik et al. (2023). This relationship may reflect, for instance, the relative degree of rent sharing or the degree to which employers differentiate the pay of immigrant labor. We use the estimated workplace pay premia received by native workers, ψ^N, and compare those estimates to those

[87] This relies on the observation that person effects and pay premia are only identified up to a normalizing constant, such that adding a constant to all person effects and subtracting the same constant from all firm effects leaves the fitted values from the model unchanged (Abowd et al., 1999; Dostie et al., 2023). The relative pay-setting effect is identified only after normalizing the pay premia for natives and immigrants relative to each other.

for immigrant workers, ψ^M, at the same workplace j:

$$\psi_j^M = \alpha + \rho\psi_j^N + \nu_j \tag{29}$$

where ρ captures the elasticity of immigrant to native pay premia.[88] Figure C2 shows the binned relationship between native and immigrant workplace effects. The estimate of $\rho = 0.63$ implies that the pass-through of firm-level wage premium to immigrant workers is substantial, though not complete. For example, when firm A in the dual-connected set offers a 10% pay premium to its native workers compared with firm B, the corresponding pay premium for immigrant workers is 6.3% at A versus B, suggesting rather equal rent-sharing between firms and immigrant workers.[89]

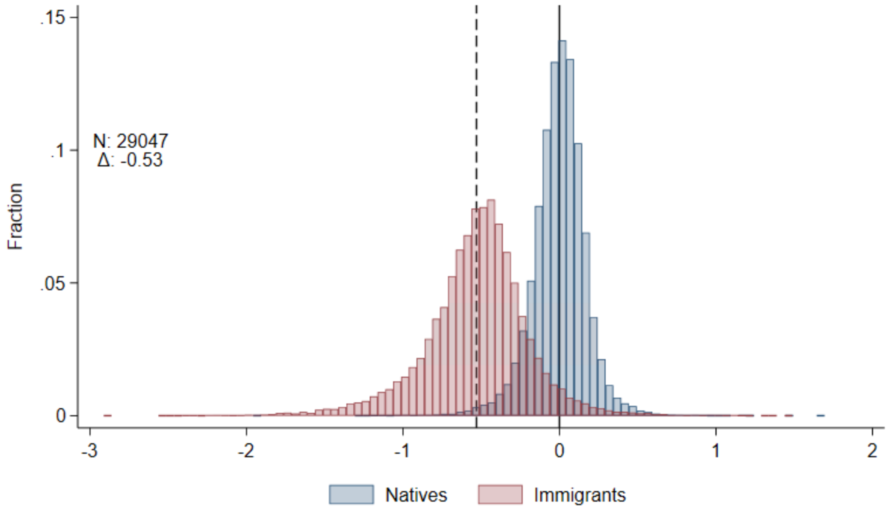
More importantly, despite evidence of imperfect pass-through, Figure C2 shows a strong alignment in the rankings of fixed effects across the two groups. In other words, workplaces that pay natives more also pay immigrants more. This provides additional reassurance that lower premia for immigrants relative to natives within the same firm do not threaten our AKM estimation. More broadly, Hermansen, Penner, Boza, Elvira, Godechot, Hällsten, Henriksen, Hou, Lippényi, Petersen, Reichelt, Sabanci, Safi, Tomaskovic-Devey, and Vickstrom (2025) document in a cross-country study that unequal access to higher-paying jobs is the primary driver (explaining around three fourths) of the immigrant-native earnings gap, rather than unequal pay for the same work at the same employer. Denmark, along with the U.S. and Sweden, stands out with the smallest immigrant–native differences, and in particular the smallest within-job pay gaps among the nine European and North American countries analyzed.

We complement our previous evidence with Figure C3, which plots the correlation between the establishment fixed effect estimated with the AKM and the establishment wage premium paid to immigrant workers. This correlation is virtually zero, ruling out that firms of good quality systematically pay a premium (or an under-premium) to their migrant employees. Overall, reassured by these findings, we then proceed to use the workplace effects estimated in equation 1 using the full sample of natives and non-refugee immigrants in our main analysis.

[88] As already noted, a normalization of workplace effects is necessary to interpret the elasticity as the proportion of workplace premia earned by native workers that immigrant workers receive at higher-paying firms (Card et al., 2016; Drenik et al., 2023). We follow these authors and normalize workplace effects to zero in the lowest vingtile, which does not affect the estimate of the slope ρ, as it would be absorbed by the constant.

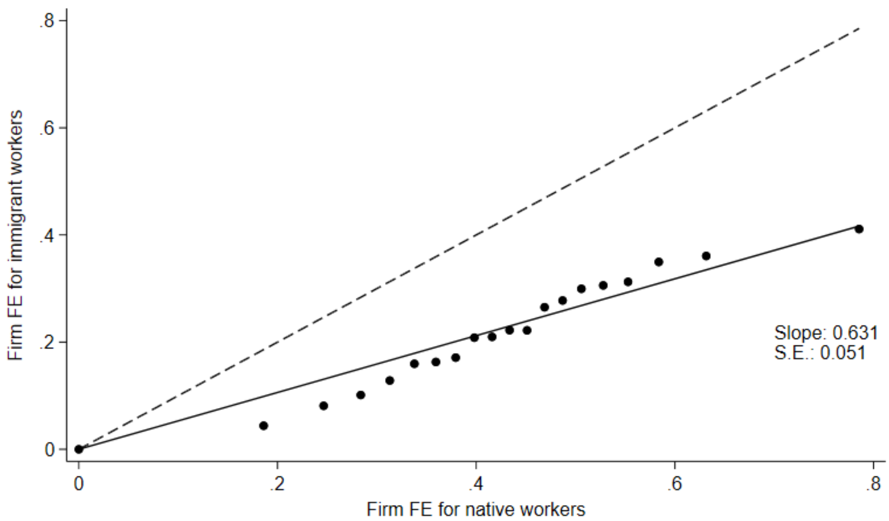
[89] Arellano-Bover and San (2023) find a very similar correlation in Israel.

Figure C1: Distribution of establishment premia by birthplace



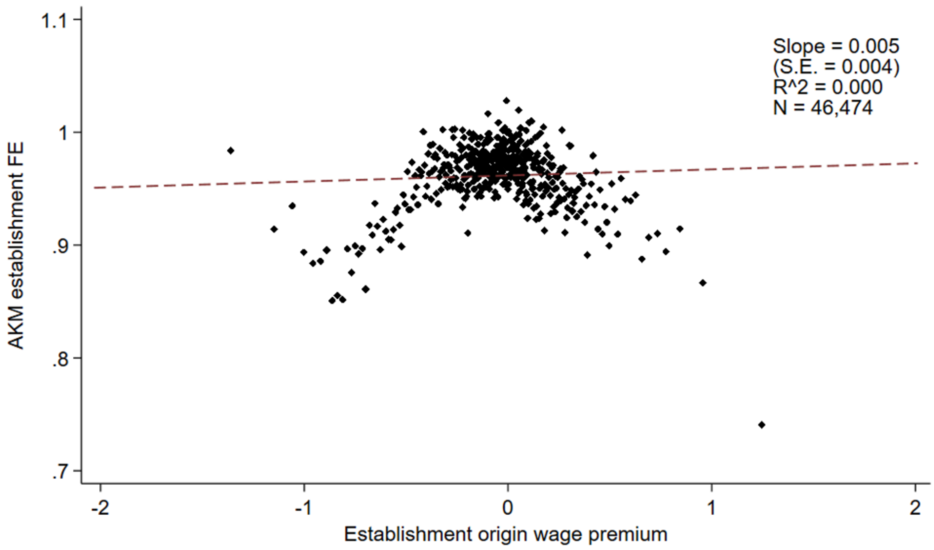
Notes: This figure plots the distribution of workplace pay premia by nativity. Native-specific (in red) and immigrant-specific (in blue) workplace effect distributions have been normalized to the mean of native-specific workplace effects. Nativity-specific workplace effects are computed for establishments in the dual-connected set (N=29,047).

Figure C2: Establishment pay premia sharing



Notes: This figure shows the binned relationship between native and immigrant workplace effects. We normalize workplace effects to zero in the lowest vingtile (bottom 5%). The slope of the regression line captures the elasticity of immigrant to native pay premia.

Figure C3: Establishment quality and origin premium paid



Notes: This figure plots the binned relationship between establishment fixed effects estimated with the AKM and the establishment-specific average origin premia. The latter is computed for each establishment with at least one native and one foreign-born worker as the log of the ratio between the average real hourly wage paid to immigrants and that paid to natives, averaged over the years included in the the AKM sample (1986–1998). Each marker represents 100 establishment.

Appendix D Other Social Outcomes

Our findings have broader policy implications for destination countries. Failures of refugee labor-market integration may increase the risk of participation in the informal economy or criminal activity (Bell, Fasani, and Machin, 2013; Pinotti, 2017). By contrast, effective labor-market integration can improve the material and psychological well-being of refugees and their families and foster broader social integration, thereby reducing social marginalization and, potentially, crime (Arendt et al., 2024). This dimension of integration is crucial for shaping natives' attitudes toward refugees, which in turn can either fuel opposition to immigration or strengthen support for additional refugee policies (Abbiati et al., 2025; Bansak, Hainmueller, and Hangartner, 2016).

In addition to examining employment and earnings, we now analyze crime outcomes for refugees and their children as measures of social integration. We study children who arrived in Denmark before age 18, no more than one year after their refugee parents (who are included in our main estimation sample), and who then lived in Denmark for at least 15 years. Our data on criminal convictions come from crime registers (KRAF, KRSI), which consist of nationwide police and court records and cover all criminal convictions in Denmark. Following earlier studies, we consider all criminal convictions (excluding traffic violations) and distinguish between two categories: property crime and violent crime. Weak integration and limited ties to Danish society may leave refugees feeling marginalized and their children feeling inferior at school, experiencing learning difficulties, and becoming more prone to violent behavior. Moreover, lower disposable income may prompt efforts to supplement family resources, increasing involvement in property crime (Arendt et al., 2024; Dustmann, Landersø, and Andersen, 2024c). Our crime categories follow Statistics Denmark's definitions. Violent crimes include offenses such as assault and homicide, while property crimes include theft and fraud.

Our results are reported in Tables D1 and D2. The former presents estimates of the effect of exposure to employer quality on crime outcomes for refugees covered by the 1986–1998 dispersal policy; the latter presents estimates for their children. We consider all crimes together (Panel A), property crimes (Panel B), and violent crimes (Panel C). For children, treatment is defined using the quality of firms in the network of the first-arriving parent.[90] We examine both the probability of conviction and the number of convictions, capturing the extensive and intensive margins. For adult refugees, we do not find statistically significant effects; magnitudes are small, consistent with earlier studies and with the fact that our treatment reflects exposure rather than direct intervention. By contrast, and consistent with other work on intergenerational consequences of welfare cuts and language training for refugees, we find long-run negative effects on violent crime for children, both in the probability of conviction and in the number of convictions, suggesting that labor market conditions propagate to other aspects of family integration.

[90] In regressions for refugee children, we control for characteristics of the first-arriving parent (or of the father if the parents arrived together), as well as the child's gender and age at arrival.

Table D1: Effects on criminal convictions

	Probability of Conviction			Number of Convictions		
	Yr. 1–5	Yr. 6–10	Yr. 11–15	Yr. 1–5	Yr. 6–10	Yr. 11–15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All crime						
Avg. quality	-0.010	-0.002	-0.003	-0.022	-0.003	-0.013
	(0.007)	(0.006)	(0.004)	(0.015)	(0.014)	(0.011)
boot. p-val.	[0.169]	[0.773]	[0.416]	[0.194]	[0.866]	[0.305]
Co-national share	-0.007	0.006	-0.006	-0.008	0.009	-0.021**
	(0.006)	(0.005)	(0.004)	(0.012)	(0.011)	(0.009)
Emp. NW immigrants	-0.008	-0.002	0.007	0.028	0.051	0.009
	(0.014)	(0.014)	(0.013)	(0.038)	(0.046)	(0.044)
Mean Y	0.173	0.112	0.093	0.279	0.202	0.166
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.049	0.045	0.045	0.035	0.031	0.027
Panel B: Property crime						
Avg. quality	-0.011*	-0.001	0.001	-0.016	0.000	-0.002
	(0.006)	(0.005)	(0.003)	(0.010)	(0.010)	(0.009)
boot. p-val.	[0.111]	[0.859]	[0.799]	[0.149]	[0.986]	[0.832]
Co-national share	-0.008	0.002	-0.008***	-0.008	-0.001	-0.021***
	(0.005)	(0.004)	(0.003)	(0.009)	(0.008)	(0.008)
Emp. NW immigrants	-0.027**	0.004	0.008	-0.028	0.031	0.018
	(0.014)	(0.012)	(0.013)	(0.023)	(0.033)	(0.035)
Mean Y	0.131	0.069	0.045	0.187	0.116	0.081
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.031	0.023	0.014	0.024	0.018	0.002
Panel C: Violent crime						
Avg. quality	-0.000	-0.004*	0.001	-0.001	-0.006**	0.001
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
boot. p-val.	[0.958]	[0.106]	[0.850]	[0.696]	[0.034]	[0.865]
Co-national share	-0.005***	-0.002	0.002	-0.005**	-0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Emp. NW immigrants	0.001	0.002	-0.004	0.003	0.006	-0.002
	(0.004)	(0.006)	(0.008)	(0.005)	(0.007)	(0.009)
Mean Y	0.022	0.020	0.019	0.026	0.023	0.021
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.017	0.013	0.013	0.010	0.010	0.010
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of the effect of exposure to employer quality on crime outcomes for refugees subject to the 1986–1998 dispersal policy. Panel A shows effects on all convictions, excluding traffic, Panel B shows effects on property crimes and Panel C shows effects on violent crimes. To compute effects in the short-run, medium-run, and long-run, we set the year of admission to Denmark to 1. The quality measure used is the average establishment effect for establishments active in the municipality of assignment at the time of arrival. Individual controls include variables observed by authorities in the dispersal process. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

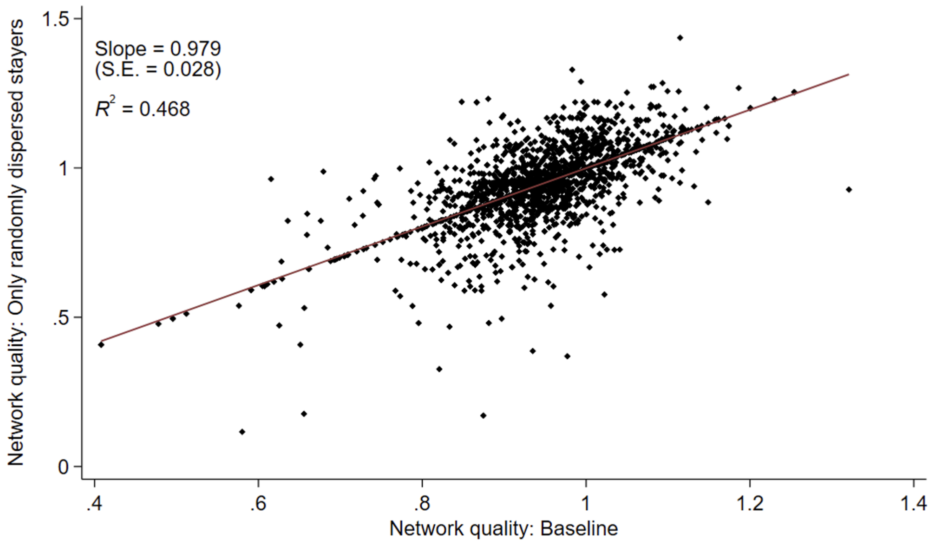
Table D2: Effects on criminal convictions for refugee children

	Probability of Conviction			Number of Convictions		
	Yr. 1–5	Yr. 6–10	Yr. 11–15	Yr. 1–5	Yr. 6–10	Yr. 11–15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All crime						
Avg. quality	0.000	-0.002	-0.010	0.008	0.013	-0.045
	(0.005)	(0.008)	(0.010)	(0.013)	(0.022)	(0.045)
boot. p-val.	[0.940]	[0.832]	[0.348]	[0.745]	[0.619]	[0.368]
Co-national share	-0.003	0.001	0.008	0.003	-0.029	-0.016
	(0.005)	(0.008)	(0.008)	(0.018)	(0.022)	(0.029)
Emp. NW immigrants	0.010	0.028	0.009	0.023	0.033	0.044
	(0.013)	(0.020)	(0.023)	(0.032)	(0.054)	(0.077)
Mean Y	0.046	0.125	0.193	0.077	0.273	0.467
Obs.	5,410	5,329	5,263	5,410	5,329	5,263
Adj. R^2	0.126	0.139	0.133	0.084	0.110	0.106
Panel B: Property crime						
Avg. quality	-0.003	-0.001	-0.005	0.006	0.011	0.006
	(0.004)	(0.008)	(0.008)	(0.010)	(0.017)	(0.024)
boot. p-val.	[0.535]	[0.872]	[0.541]	[0.707]	[0.594]	[0.810]
Co-national share	-0.004	0.004	0.007	-0.014	0.000	0.017
	(0.004)	(0.008)	(0.008)	(0.011)	(0.014)	(0.015)
Emp. NW immigrants	0.019*	0.018	0.012	0.015	0.015	0.006
	(0.011)	(0.021)	(0.020)	(0.021)	(0.040)	(0.041)
Mean Y	0.036	0.092	0.116	0.055	0.158	0.203
Obs.	5,410	5,329	5,263	5,410	5,329	5,263
Adj. R^2	0.100	0.085	0.067	0.066	0.072	0.053
Panel C: Violent crime						
Avg. quality	0.003	0.005	-0.017**	0.003	0.005	-0.025**
	(0.003)	(0.004)	(0.007)	(0.004)	(0.006)	(0.011)
boot. p-val.	[0.271]	[0.332]	[0.047]	[0.423]	[0.481]	[0.048]
Co-national share	-0.006	-0.004	0.003	-0.010	-0.010	0.005
	(0.003)	(0.004)	(0.005)	(0.006)	(0.009)	(0.009)
Emp. NW immigrants	0.001	0.002	0.014	0.010	0.015	0.025
	(0.009)	(0.011)	(0.011)	(0.013)	(0.020)	(0.017)
Mean Y	0.010	0.037	0.063	0.013	0.052	0.082
Obs.	5,410	5,329	5,263	5,410	5,329	5,263
Adj. R^2	0.028	0.069	0.069	0.029	0.079	0.059
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of the effect of exposure to employer quality on crime outcomes for the children of refugees subject to the 1986–1998 dispersal policy. Panel A shows effects on all convictions, excluding traffic, Panel B shows effects on property crimes and Panel C shows effects on violent crimes. To compute effects in the short-run, medium-run, and long-run, we set the year of admission to Denmark to 1. The quality measure used is the average establishment effect for establishments active in the municipality of assignment at the time of arrival. Individual controls include parental variables observed by authorities in the dispersal process along with age at arrival and gender of the children. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Appendix E Robustness Evidence

Figure E1: Construction of network measure, All co-nationals vs. randomly dispersed stayers



Notes: This figure plots the relationship between network quality based on all co-nationals in the municipality of assignment at arrival and network quality based only on co-nationals who were subject to the dispersal policy and remained in their assigned municipality. Each marker represents a local co-ethnic network, and the red solid line shows the line of best fit.

Table E1: Exposure to local firms, Alternative aggregations

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Geographic exposure						
Avg. quality (weighted)	0.006	0.022	0.031	-0.127	-0.226	0.176
	(0.009)	(0.021)	(0.036)	(0.363)	(1.012)	(1.861)
boot. p-val.	[0.465]	[0.476]	[0.588]	[0.740]	[0.838]	[0.946]
Co-national share	-0.001	-0.006	-0.002	-0.028	-0.316	-0.203
	(0.004)	(0.005)	(0.007)	(0.127)	(0.238)	(0.377)
Emp. NW immigrants	0.005	0.003	0.006	-0.021	-0.027	0.078
	(0.006)	(0.008)	(0.011)	(0.216)	(0.390)	(0.567)
Mean Y	0.104	0.259	0.325	2.983	8.803	13.871
Obs.	15,554	15,062	14,701	15,554	15,062	14,701
Adj. R^2	0.175	0.204	0.196	0.150	0.159	0.148
Panel B: Network exposure						
Avg. quality (weighted)	0.008**	0.015***	0.000	0.204**	0.630**	0.031
	(0.003)	(0.005)	(0.007)	(0.103)	(0.252)	(0.384)
boot. p-val.	[0.014]	[0.016]	[0.984]	[0.062]	[0.028]	[0.944]
Co-national share	-0.002	-0.005	-0.005	0.073	-0.144	-0.032
	(0.004)	(0.007)	(0.008)	(0.137)	(0.316)	(0.443)
Emp. NW immigrants	0.000	-0.000	0.002	0.111	0.240	0.403
	(0.012)	(0.014)	(0.019)	(0.406)	(0.595)	(0.977)
Mean Y	0.106	0.262	0.331	3.075	8.964	14.145
Obs.	10,222	9,947	9,757	10,222	9,947	9,757
Adj. R^2	0.205	0.226	0.211	0.184	0.181	0.160
Panel C: Network exposure						
Share Top-Quartile	0.026***	0.036**	0.029	0.855***	1.487**	1.293
	(0.008)	(0.016)	(0.019)	(0.280)	(0.644)	(1.167)
boot. p-val.	[0.001]	[0.067]	[0.171]	[0.003]	[0.035]	[0.312]
Co-national share	-0.002	-0.005	-0.004	0.069	-0.166	-0.011
	(0.004)	(0.007)	(0.008)	(0.140)	(0.323)	(0.444)
Emp. NW immigrants	0.001	-0.000	0.003	0.114	0.252	0.432
	(0.012)	(0.014)	(0.019)	(0.409)	(0.595)	(0.967)
Mean Y	0.107	0.262	0.331	3.080	8.979	14.156
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.206	0.226	0.210	0.185	0.180	0.159
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Panel A and Panel B of this table replicate, respectively, Panel A and Panel C of Table 7, replacing the original measures with weighted versions. Average quality is calculated as the weighted average of workplace effects, using full-time equivalents as weights. Panel C considers the share of top employers in the network, defined by assigning a value of one to establishments in the top quartile of the Danish workplace pay premium distribution and then averaging this indicator within each network. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table E2: Exposure to local firms, Alternative specifications and restrictions

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Exposure measures jointly						
Avg. geographic quality	-0.054**	-0.021	-0.028	-1.820**	-2.372	-1.729
	(0.027)	(0.043)	(0.057)	(0.731)	(1.843)	(2.582)
boot. p-val.	[0.090]	[0.776]	[0.832]	[0.039]	[0.380]	[0.712]
Avg. network quality	0.009***	0.013**	0.005	0.276***	0.616**	0.385
	(0.003)	(0.006)	(0.007)	(0.105)	(0.248)	(0.378)
boot. p-val.	[0.004]	[0.074]	[0.446]	[0.012]	[0.030]	[0.343]
Co-national share	-0.001	-0.005	-0.004	0.084	-0.130	0.006
	(0.004)	(0.007)	(0.008)	(0.140)	(0.310)	(0.428)
Emp. NW immigrants	0.000	-0.001	0.002	0.100	0.220	0.407
	(0.012)	(0.014)	(0.019)	(0.401)	(0.595)	(0.978)
Mean Y	0.107	0.262	0.331	3.080	8.979	14.156
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.206	0.226	0.210	0.185	0.181	0.159
Panel B: Network exposure - Including industry controls						
Avg. quality	0.007**	0.012**	0.003	0.187*	0.536**	0.300
	(0.003)	(0.006)	(0.007)	(0.113)	(0.256)	(0.379)
boot. p-val.	[0.017]	[0.085]	[0.511]	[0.037]	[0.052]	[0.400]
Co-national share	-0.001	-0.004	-0.002	0.109	-0.111	0.054
	(0.004)	(0.007)	(0.008)	(0.145)	(0.313)	(0.432)
Emp. NW immigrants	0.003	-0.002	0.002	0.163	0.190	0.344
	(0.012)	(0.014)	(0.019)	(0.401)	(0.616)	(0.987)
Mean Y	0.107	0.262	0.331	3.080	8.979	14.156
Obs.	10,246	9,971	9,781	10,246	9,971	9,781
Adj. R^2	0.205	0.226	0.210	0.185	0.180	0.159
Panel C: Network exposure - Early movers sample						
Avg. quality	0.000	-0.004	-0.007	0.060	-0.132	-0.232
	(0.005)	(0.008)	(0.010)	(0.204)	(0.348)	(0.493)
boot. p-val.	[0.976]	[0.596]	[0.505]	[0.785]	[0.714]	[0.650]
Co-national share	0.003	-0.010	0.013	0.368	0.225	1.202
	(0.008)	(0.011)	(0.015)	(0.352)	(0.703)	(1.032)
Emp. NW immigrants	-0.016	0.003	0.004	-0.791	0.083	0.353
	(0.015)	(0.017)	(0.026)	(0.566)	(0.864)	(1.228)
Mean Y	0.113	0.266	0.331	3.386	9.198	14.766
Obs.	3,545	3,468	3,388	3,545	3,468	3,388
Adj. R^2	0.273	0.246	0.223	0.243	0.196	0.165
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Panel A of this table reports estimates from regressions that include both main measures of exposure to firm quality as regressors (geographic exposure and network-based exposure to employers hiring at least one co-national at arrival). Panel B examines network-level quality controlling for the industry shares of the network. Panel C repeats the estimation for network-level quality restricting the sample to refugees who relocate to a municipality different from their assigned one within their first five years after arrival ("early movers"). All quality measures are unweighted (calculated as the unweighted average of workplace effects). Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table E3: Sample restrictions and sensitivity

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Network size						
Avg. quality (w/o bottom 10%)	0.007**	0.012*	0.003	0.207*	0.534**	0.289
	(0.003)	(0.006)	(0.007)	(0.110)	(0.256)	(0.382)
boot. p-val.	[0.033]	[0.097]	[0.694]	[0.077]	[0.063]	[0.498]
Obs.	10,097	9,827	9,637	10,097	9,827	9,637
Avg. quality (w/o top 10%)	0.007**	0.010*	0.003	0.204*	0.478*	0.261
	(0.003)	(0.006)	(0.006)	(0.110)	(0.258)	(0.377)
boot. p-val.	[0.060]	[0.131]	[0.655]	[0.078]	[0.096]	[0.539]
Obs.	8,729	8,512	8,350	8,729	8,512	8,350
Panel B: Municipality of assignment's population						
Avg. quality (w/o bottom 10%)	0.008***	0.012*	0.004	0.279***	0.570**	0.317
	(0.003)	(0.006)	(0.007)	(0.104)	(0.255)	(0.384)
boot. p-val.	[0.005]	[0.111]	[0.512]	[0.009]	[0.047]	[0.433]
Obs.	10,051	9,779	9,592	10,051	9,779	9,592
Avg. quality (w/o top 10%)	0.008**	0.015***	0.007	0.240**	0.640***	0.417
	(0.003)	(0.006)	(0.006)	(0.103)	(0.243)	(0.367)
boot. p-val.	[0.025]	[0.030]	[0.323]	[0.032]	[0.030]	[0.317]
Obs.	8,793	8,582	8,442	8,793	8,582	8,442
Panel C. Municipality of assignment's share of national FTE						
Avg. quality (w/o bottom 10%)	0.008***	0.012*	0.004	0.266**	0.571**	0.287
	(0.003)	(0.006)	(0.007)	(0.105)	(0.257)	(0.385)
boot. p-val.	[0.015]	[0.122]	[0.595]	[0.016]	[0.049]	[0.508]
Obs.	10,061	9,790	9,602	10,061	9,790	9,602
Avg. quality (w/o top 10%)	0.008***	0.018***	0.009	0.268***	0.761***	0.577
	(0.003)	(0.005)	(0.006)	(0.104)	(0.230)	(0.356)
boot. p-val.	[0.018]	[0.004]	[0.158]	[0.022]	[0.006]	[0.136]
Obs.	8,845	8,631	8,488	8,845	8,631	8,488
Main controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates for the network-level measure of firm quality after trimming the top or bottom 10% of observations in our refugee sample based on size of co-ethnic network at arrival (Panel A), population in the municipality of assignment (Panel B), and municipality of assignment's share of national full-time equivalents (Panel C). Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

Table E4: Split-sample IV estimates

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Network exposure, Unweighted average						
Avg. quality	0.012**	0.015	0.015	0.167	0.550	0.645
	(0.006)	(0.011)	(0.013)	(0.189)	(0.469)	(0.663)
Mean Y	0.106	0.262	0.331	3.070	8.960	14.181
Obs.	10,114	9,848	9,661	10,114	9,848	9,661
Adj. R^2	0.060	0.099	0.090	0.048	0.072	0.068
Kleib.-Paap F	69.36	67.88	67.53	69.36	67.88	67.53
Panel B: Network exposure, Weighted average						
Avg. quality	0.010*	0.020**	0.003	0.154	0.714*	0.089
	(0.005)	(0.009)	(0.013)	(0.157)	(0.380)	(0.577)
Mean Y	0.106	0.262	0.331	3.070	8.950	14.173
Obs.	10,093	9,827	9,640	10,093	9,827	9,640
Adj. R^2	0.061	0.099	0.090	0.049	0.073	0.069
Kleib.-Paap F	92.01	89.42	87.18	92.01	89.42	87.18
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports IV estimates from the following split-sample procedure. We randomly split the sample of workers used in the AKM estimation into two equal sized samples, calculate two sets of AKM estimates separately in both subsamples, aggregate establishment effects at the network level, and then use the network-level quality from one set of establishment effects as instrumental variable for the other in our main equation. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

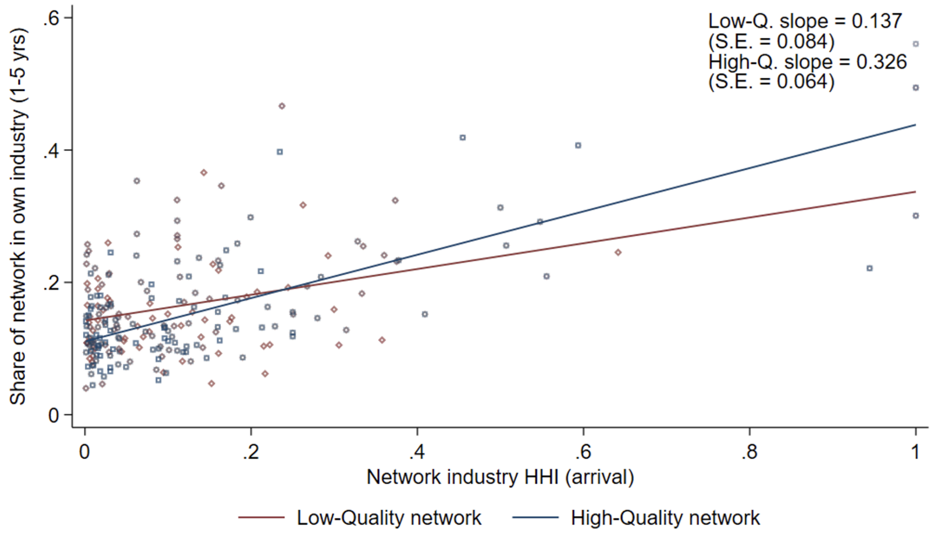
Table E5: Robustness with municipality-by-origin fixed effects

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Network exposure						
Avg. quality	0.008**	-0.002	-0.015	0.274**	0.024	-0.687
	(0.003)	(0.008)	(0.010)	(0.111)	(0.311)	(0.466)
boot. p-val.	[0.019]	[0.844]	[0.162]	[0.023]	[0.952]	[0.164]
Mean Y	0.106	0.262	0.330	3.074	8.975	14.140
Obs.	10,220	9,944	9,753	10,220	9,944	9,753
Adj. R^2	0.191	0.204	0.171	0.168	0.169	0.132
Panel B: Network exposure						
Share Top-Quartile	0.029***	0.013	0.002	0.824***	0.501	-0.505
	(0.010)	(0.027)	(0.030)	(0.319)	(0.964)	(1.373)
boot. p-val.	[0.006]	[0.686]	[0.949]	[0.015]	[0.666]	[0.763]
Mean Y	0.106	0.262	0.330	3.074	8.975	14.140
Obs.	10,220	9,944	9,753	10,220	9,944	9,753
Adj. R^2	0.192	0.204	0.171	0.168	0.169	0.132
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-by-Continent FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort bin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This tables replicates our main estimates using a different, more demanding set of fixed effects that include region-by-municipality and arrival cohort fixed effects. Specifically, we grouped origin countries by broad geographic region (Middle East, Africa, and Southwest Asia) and grouped cohorts into three 4-year cohort bins. Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1. P-values from wild cluster bootstrap are reported in square brackets.

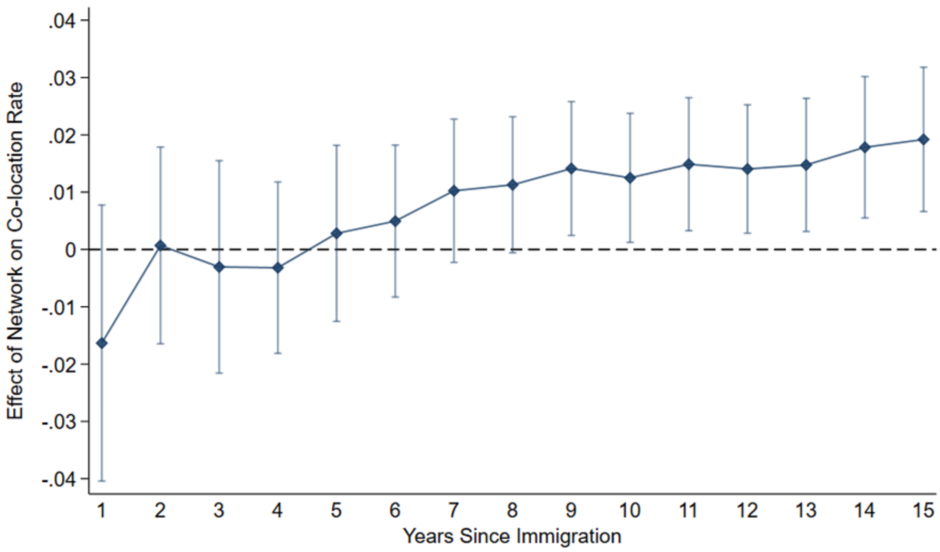
Appendix F Additional Results on Mechanisms

Figure F1: Industry concentration: Unconditional correlations



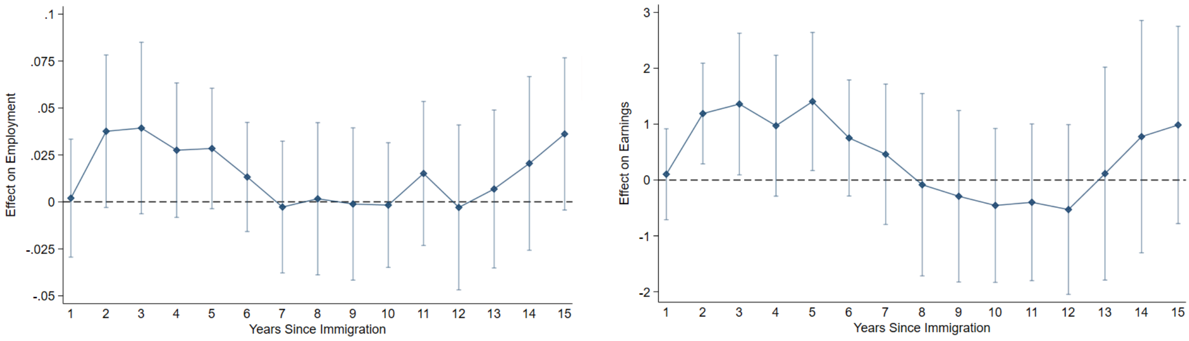
Notes: This figure plots the binned relationship between the share of an individual refugee's network in their own industry over the first five years after arrival and the network's industry Herfindahl–Hirschman Index (HHI) at arrival. Variable definitions are provided in the main text. Observations are split into two groups based on network quality (navy indicates networks with above-median quality, while maroon represents those below the median). Each marker represents 5 refugees.

Figure F2: Residential clustering



Notes: This figure plots the effect of network quality (unweighted measure) at each year since migration on refugees' individual co-location rate. The co-location rate is computed as the ratio between co-ethnics living in their parish (minus self) and co-ethnics living in the whole municipality (minus self). The coefficients are displayed with the 95% confidence intervals.

Figure F3: Role of co-ethnic managers



Notes: The figures display the yearly effects of co-ethnic links to firms employing co-ethnic managers. Variable definitions are provided in the main text. The left panel shows the effects on employment, while right panel presents the effects on earnings. Coefficients are shown with 95% confidence intervals.

Table F1: Industry concentration and network quality

	Network share in own industry						
	Yr. 1-5	Yr. 1-5	Yr. 6-10	Yr. 6-10	Yr. 1-3	Yr. 4-6	Yr. 7-10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Network HHI	0.281***		0.106**				
	(0.054)		(0.045)				
High network HHI		0.074***		0.033*	0.012	0.012	0.013
		(0.020)		(0.017)	(0.026)	(0.017)	(0.030)
High network quality					-0.021	-0.013	-0.025
					(0.026)	(0.017)	(0.030)
Interaction					0.069***	0.061**	0.023
					(0.026)	(0.017)	(0.030)
Mean Y	0.169	0.169	0.136	0.136	0.183	0.149	0.132
Obs.	1,285	1,285	1,111	1,111	600	1,202	821
Adj. R^2	0.271	0.183	0.109	0.095	0.195	0.135	0.082
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality quality	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Table F2: Network exposure: Employer quality and willingness to hire

	Employment			Earnings		
	Yr. 1-5	Yr. 6-10	Yr. 11-15	Yr. 1-5	Yr. 6-10	Yr. 11-15
	(1)	(2)	(3)	(4)	(5)	(6)
Avg. quality	-0.017	0.004	-0.004	-0.467	-0.034	-0.458
	(0.011)	(0.011)	(0.018)	(0.415)	(0.539)	(1.085)
Medium Will.	-0.321**	-0.219	-0.144	-8.697*	-11.417	-8.246
	(0.129)	(0.147)	(0.178)	(4.591)	(7.078)	(11.221)
High Will.	-0.228**	-0.058	0.032	-5.572	-3.699	-2.082
	(0.111)	(0.129)	(0.185)	(3.873)	(6.213)	(11.011)
Quality*Medium Will.	0.035***	0.027*	0.020	0.994**	1.428*	1.102
	(0.013)	(0.016)	(0.019)	(0.506)	(0.777)	(1.250)
Quality*High Will.	0.023**	0.006	-0.003	0.595	0.414	0.232
	(0.011)	(0.014)	(0.020)	(0.430)	(0.652)	(1.185)
Obs.	9,876	9,663	9,478	9,876	9,663	9,478
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-by-Origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust SE in parentheses are clustered at the municipality of assignment level. Significance levels: *** for p<0.01, ** for p<0.05, * for p<0.1.

Appendix G Additional Results on Optimal Assignment

We train and test two supervised machine learning models: a Least Absolute Shrinkage and Selection Operator (LASSO) constraint to a logit model, and a gradient boosted regression tree (GBRT). We also compare these models with two second-best alternatives: a naive constant estimator and a standard logit model that includes individual characteristics but does not account for municipality-specific effects.

The LASSO estimates a high-dimensional regression model by minimizing the sum of squared residuals subject to an ℓ_1 penalty on the coefficients, which shrinks some estimates toward zero and sets others exactly to zero, thereby performing variable selection. The resultant reduction in variability may be sufficiently large to offset the bias introduced by shrinkage, leading to a lower mean squared error. Our initial high-dimensional specification included individual refugee characteristics (e.g., age, gender, family composition, origin) and their interactions with municipalities. In our refined version, we further incorporate network measures to account for the potential influence of co-ethnic connections on employment outcomes. When applied to a logit specification for a binary outcome, the same penalization principle applies to the log-likelihood, resulting in a model that retains only the most relevant predictors. Importantly, the LASSO selects network measures among the predictors most useful for explaining the outcome.

GBRT builds an ensemble of classification trees sequentially, where each new tree is fit to the residuals from the previous stage to reduce prediction errors. In the binary outcome setting, the algorithm uses a logistic link so that predictions are probabilities. We apply it on the same specification as in the LASSO.

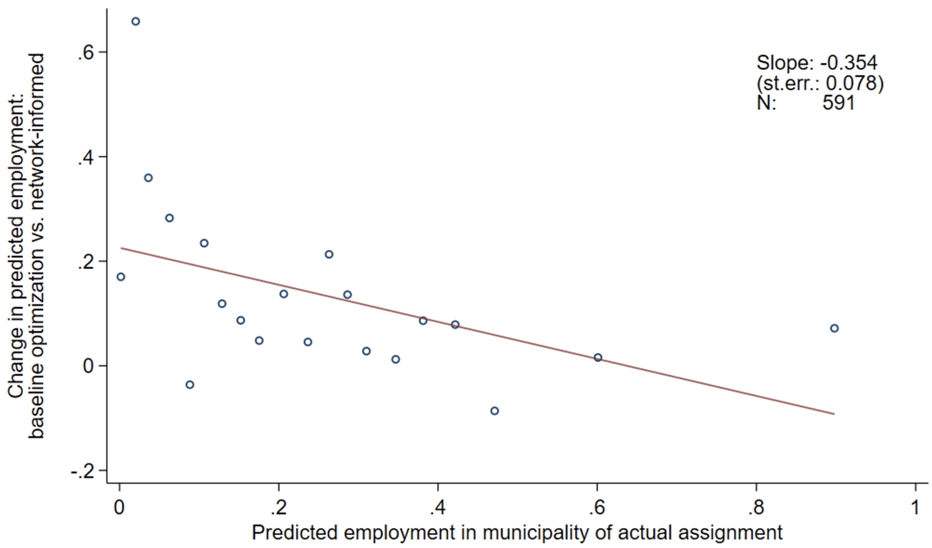
Table G1 reports the performance of all models on the holdout sample of refugees from the 1987–1997 cohorts across several metrics. Both the LASSO and GBRT outperform the second-best benchmarks on all measures, with the LASSO exceeding GBRT on every metric except precision. Incorporating network measures into the LASSO further improves prediction accuracy in the test set relative to the baseline LASSO on all metrics except deviance. Since we use the predicted probabilities from this refined LASSO as our preferred predictions, we finally check that our optimization problem is meaningful by ensuring sufficient variation in probabilities across municipalities for each refugee. We find an average within-individual standard deviation in predicted employment probability across municipalities of 0.195 (average individual predicted probability = 0.283), making the exercise policy relevant.

Table G1: Performance of machine learning models

Model	Test sample: Misc. error (1)	Recall (2)	Precision (3)	AUC-ROC (4)	Brier (5)	Deviance (6)
Constant	0.258	0.000	0.000	0.500	0.191	1.141
Logit	0.256	0.010	0.778	0.662	0.179	1.077
LASSO	0.240	0.246	0.578	0.730	0.166	0.959
GBRT	0.242	0.078	0.826	0.721	0.174	1.054
LASSO with netw.	0.227	0.284	0.592	0.739	0.161	0.970

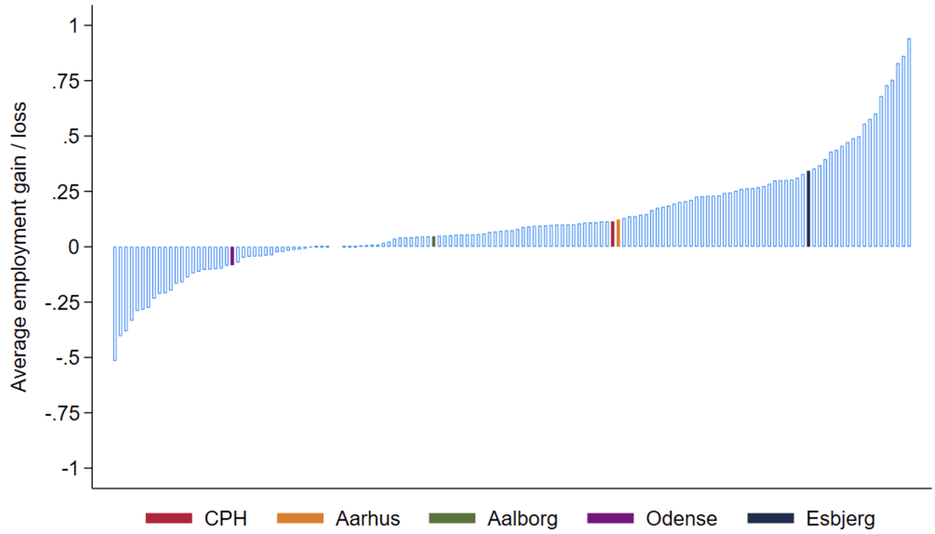
Notes: Misclassification (Misc.) error is the proportion of observations incorrectly classified. *Recall* measures the proportion of correctly predicted employed refugees among refugees actually employed (true positives over true positives plus false negatives). *Precision* measures the proportion of correctly predicted employment cases among all predicted employment cases (true positives over true positives plus false positives). All of these measures refer to a binary classification with a threshold set at the standard value of 0.5. Because our measure of quality scores uses predicted probabilities of employment, this specific threshold does not affect optimal allocations. Area under the receiver operating characteristic curve (*AUC-ROC*) measures the area under the receiver operating characteristic curve for each model. The *Brier* score measures the accuracy of probabilistic predictions for binary outcomes, computing the mean squared difference between the predicted probability and the actual outcome (coded as 0 or 1). *Deviance* also measures the quality of probabilistic predictions but penalizes errors logarithmically (i.e., it punishes high-confidence wrong predictions much more harshly than the Brier score).

Figure G1: Gains along the distribution of individual predicted probabilities



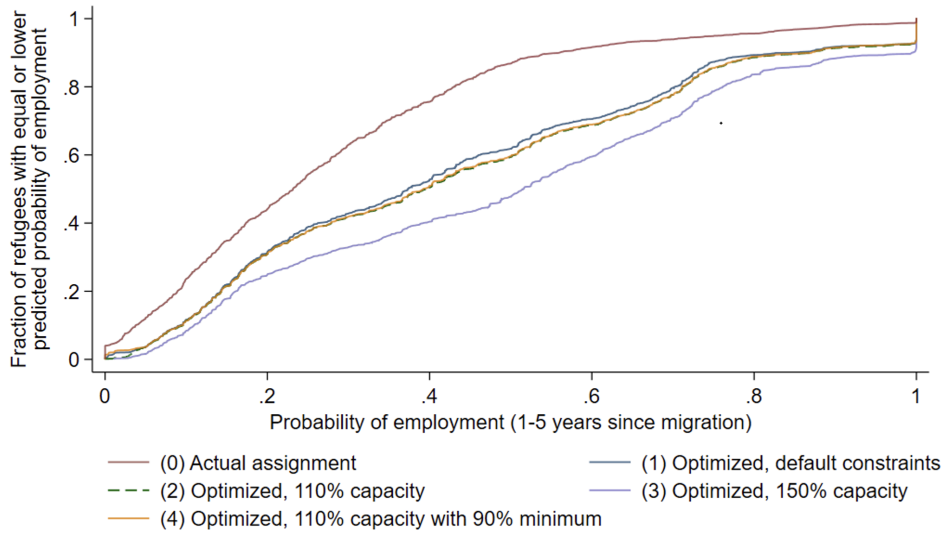
Notes: This figure shows the binned relationship between gains (or losses) in predicted employment probability for refugees who would be reassigned from municipalities optimally chosen without accounting for networks to those selected optimally with networks (y-axis) and their predicted probability in the municipality of actual assignment under the dispersal policy (x-axis). Each marker represents one-twentieth of the refugees subject to these hypothetical relocations with nonzero gains. The red solid line represents the line of best fit (slope and standard error are reported).

Figure G2: Average gains by municipality



Notes: This figure displays the average gains and losses in predicted refugee employment by municipality, comparing the status quo assignment under the dispersal policy with the optimized assignment generated using predictions that include network measures. The five largest municipalities (Copenhagen, Aarhus, Aalborg, Odense, and Esbjerg) are highlighted.

Figure G3: Counterfactual policy scenarios



Notes: This figure displays the empirical cumulative distribution functions (ECDFs) of the refugees' predicted employment probabilities within the first five years since migration under different assignment scenarios. In all scenarios, probabilities are computed with the refined LASSO that includes network measures. The solid red line refers to predicted probabilities in municipality of assignment under the dispersal policy. The solid blue line (counterfactual 1) refers to the optimized assignment using default constraints (municipality-specific family structure restrictions and inferred quotas). The dashed green line (counterfactual 2) refers to the optimized assignment subject to family structure restrictions and allowing for 110% of inferred municipality quotas (with no lower bound). The solid purple line (counterfactual 3) refers to the optimized assignment subject to family structure restrictions and allowing for 150% of inferred municipality quotas (with no lower bound). The solid yellow line (counterfactual 4) refers to the optimized assignment subject to family structure restrictions and allowing for 110% of inferred municipality quotas, with a lower bound of 90%.