Employer Quality, Social Networks, and Economic Integration of Refugees*

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Abstract

We study the role of employers in the labor market integration of refugees. Drawing on comprehensive matched employer–employee data from Denmark, we estimate firm-specific wage premia that we then use as a proxy for workplace quality. Since sorting complicates establishing causal links, we leverage a dispersal policy implemented between 1986 and 1998 that quasi-randomly allocated refugees across municipalities to obtain exogenous exposure to sets of accessible employers. Our findings reveal that being placed in a municipality where, at arrival, members of the co-ethnic network are employed by high-quality employers has positive and statistically significant effects on refugees' employment and earnings for up to 10 years after arrival. We identify information sharing about job vacancies through social connections as the mechanism driving this result, as we find no evidence of direct use of job referrals. Additionally, we document novel insights on refugees and the firm ladder. Our results suggest that policymakers should consider access to high-quality employers as an additional factor contributing to refugees' labor market success when designing placement policies in host countries.

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1 Introduction

Success in the labor market is a crucial determinant of immigrant integration in host countries. Nevertheless, extensive research highlights that refugees—an inherently vulnerable group of migrants—experience persistent gaps in economic performance, even vis-à-vis similar migrant groups. (Bratsberg, Raaum, and Roed, 2017; Brell, Dustmann, and Preston, 2020; Fasani, Frattini, and Minale, 2022).[1] This evidence has motivated a growing body of research aimed at understanding the determinants of refugee economic integration. Some studies have evaluated a wide range of policies implemented by host countries, including regulations for permanent residency, employment support, language training, welfare benefits, among others. Other studies have focused on the local conditions refugees encounter upon arrival (for comprehensive reviews, see, for example, Arendt, Dustmann, and Ku (2022) and Foged et al. (2024a)).

Despite the significant attention given to many aspects of refugee integration, employers have been virtually absent from the discussion.[2] This is surprising given the significant role that firms and their pay policies play as a source of wage inequality (Card, Heining, and Kline, 2013). Importantly, in the context of immigrant-native earnings disparities, several studies have shown that between-workplace variation accounts for a substantial share of the earnings gap using job ladder models and firm productivity grouping (Damas de Matos, 2017; Dostie, Li, Card, and Parent, 2023; Arellano-Bover and San, 2023; Åslund, Bratu, Lombardi, and Thoresson, 2023). A natural implication is that employers might be crucial in shaping the labor market outcomes of refugees, as a match with a good firm can provide a pathway to a better career, higher pay, and improved protection against negative shocks. However, sorting across locations and employers complicates the estimation of any causal link.

[1] It is well known that refugees face a unique set of challenges. They have been displaced, experienced trauma, lost human capital during their journey, and often come from countries with cultures and languages that are markedly different from those of their destination. Moreover, they are generally less positively selected than other migrants in terms of education and skills (Foged, Hasager, and Peri, 2024a).

[2] We are aware of two notable exceptions. Foged, Kreuder, and Peri (2022) examine the impact of "Industry Packages", introduced in Denmark between 2013 and 2018, which aimed to improve employment outcomes of migrants by incorporating the needs of firms. By connecting employers with refugees, this policy matched local labor demand with the labor supply from recently arrived refugees. Similarly, Loiacono and Silva-Vargas (2024) conduct a randomized experiment in Uganda, pairing each refugee worker with a sample of Ugandan employers who were subsidized to offer a week-long internship at no cost. The study evaluates the impact of this intervention on firms' future willingness to hire refugees.

In this paper, we address a gap in the literature on refugee integration by investigating the impact of early exposure to "good" employers on the labor market outcomes of refugees in Denmark. Utilizing multiple linked administrative datasets on individuals and employers provided by Statistics Denmark, we proceed in two steps. First, we follow the methodology originally proposed by Abowd, Kramarz, and Margolis (1999) (AKM), which accounts for individual unobserved heterogeneity and models wages as a function of additive worker and firm fixed effects, and estimate establishment-specific pay premia to serve as our proxy for establishment quality. Second, building on a range of earlier studies, we leverage the conditionally random initial placement of refugees across Danish municipalities under a dispersal policy implemented from 1986 to 1998.[3] Consequently, conditional on individual characteristics known to the authorities at the time of allocation, we can treat the set of establishments active in a municipality as exogenous for newly arrived refugees, enabling us to construct various measures of exposure to employers.

To estimate the effect of exposure to high-quality employers, we would ideally compare a refugee with an identical twin, each randomly matched with firms of different quality upon arrival. While such an experiment cannot be conducted, a naïve alternative is to regress refugees' employment and earnings on the estimated quality of their first employer—in our case, the employer's pay premium. As problematic as this approach is, the results reported in Table 1 show a significant and persistent role of initial employers, with the quality of the initial workplace being strongly correlated with employment probability and earnings of refugees in the short run (1–5 years), medium run (6–10 years), and long run (11–15 years).[4]

Motivated by this evidence, we exploit the quasi-randomization provided by the dispersal policy to refine the analysis by constructing a proxy for the employers accessible to newly arrived refugees. Assuming that locations and co-ethnic networks are relevant channels for firm exposure, in practice we investigate whether the average quality of establishments active in the municipality of assignment, of establishments in that municipality that have previously hired other refugees, and of establishments employing members of the local co-ethnic network at the time of arrival affect em-

[3]Several authors have used this dispersal policy to study the impact of initial characteristics, such as (Damm and Dustmann, 2014; Eckert, Hejlesen, and Walsh, 2022; Dustmann, Mertz, and Okatenko, 2023; Foged et al., 2024a)

[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 2 percentage points, and to a higher yearly earnings by 1697 USD in the long run.

ployment and earnings of newly arrived refugees.[5] We expect our measures of early exposure to local employers to be relevant for refugees' outcomes, given that initial conditions have been shown to have persistent scarring effects, that the set of accessible firms likely depends on the location of residence, and that social connections are valuable for sharing vacancy information and job referrals.[6]

Nevertheless, we document three novel stylized facts about refugees and firms that provide *prima facie* evidence supporting the validity of our approach. First, we show that workplace-specific pay premia matter for refugees, as transitions between establishments of different quality significantly impact their earnings and duration of employment spells. Second, we document that refugees climb the firm ladder very slowly, achieving a statistically significant improvement in workplace quality only by year 10 after arrival. This improvement then persists until the end of our observation period. However, the magnitude of these gains is modest, which is not surprising given the well-documented economic gaps between refugees and other groups of workers. This finding reaffirms the importance of initial conditions in shaping long-term outcomes. Third, we show that social connections matter for refugees as well: having a local co-ethnic connection employed in a given firm at the time of arrival significantly increases a refugee's probability of being hired by that firm in their future career.

Our main finding is that being placed in a municipality where, at arrival, members of the local co-ethnic network are employed by high-quality employers has positive and significant effects on refugees' employment and earnings. 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 but without being statistically significant. On the contrary, a higher average workplace pay premia offered by employers active in the municipality at arrival is linked with a lower employment probability and decline in earnings, statistically significant in the short run. This suggests that social connections play an important role in providing access to certain employers, as being surrounded by high-paying firms might even be detrimental if they are difficult to access. We validate this

[5] We see these three levels of exposure as progressively refining the set of first accessible employers.

[6] These factors have been extensively documented by the migration literature but also by other studies. For instance, Kahn (2010), Oreopoulos, von Wachter, and Heisz (2012) and Altonji, Kahn, and Speer (2016) link entry labor market conditions (i.e., recession periods) to future outcomes. The importance of social networks has been discussed, among others, by Ioannides and Loury (2004) and Topa (2019).

evidence with several robustness checks.

We rationalize these findings by extending the theoretical framework proposed by Beaman (2012). Within an overlapping generations framework, social networks help new cohorts in the job search by sharing information about vacancies. Networks are efficient when job offers are passed on until all information is used, and inefficient when there is a constraint on the amount of time available to share job information, as jobs fill quickly. We expand this framework by endogenizing the level of efficiency and relating it to the quality of firms employing network members at the beginning of each period. As high-wage jobs tend to attract more applicants, increasing employer screening time, network members receiving information about vacancies with high-wage offers–if they decide not to accept them–have more time to pass on the information within the network.

Two key predictions have been tested in our analysis. First, higher network quality increases the employment level of new members. Since they are working at high-pay premium jobs, employed members of high-quality networks pass on higher wage offers to their network, which require more time to be filled, reducing information loss and increasing employment levels of the new cohort, compared to low-quality networks. Second, higher network quality has an ambiguous effect on the expected wage of new members. Since network members are employed in high-pay premium jobs, in a high-quality network higher wage offers are passed along rather than accepted, shifting the distribution of wages available through the network to the right (stochastic dominance channel). At the same time, a quality-induced reduction in inefficiency allows more offers, including lower-paying ones, to be passed on within the network, increasing the presence of lower wages (efficiency channel). We confirm empirically the prediction for employment and we find evidence that for expected wages, at least in our setting, the stochastic dominance channel dominates the inefficiency channel.[7]

The results of our study are highly relevant for policymakers. With increasing rates of unexpected, forced migration worldwide (Figure 1), receiving countries are in need of effective strategies to support migrants' assimilation. While the Danish dispersal policy aimed to evenly distribute refugees in order to allocate integration costs more equitably, we emphasize the importance of a thorough 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. We show that it is crucial for policymakers to consider locations where refugees' social

[7] Our empirical results refer to earnings rather than wages.

connections provide access to high-quality employment opportunities. As these decisions involve no direct monetary costs, we view this as an effective and low-cost policy improvement.[8]

Contribution to the literature. Our paper contributes to at least three main strands of literature. First, we primarily 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 (Foged, Hasager, Peri, Arendt, and Bolvig, 2024b), active labor market policies (Foged et al., 2022; Arendt and Bolvig, 2023), changes in welfare benefits (Dustmann, Landersø, and Andersen, 2024b), and regulations for permanent residency (Arendt, Dustmann, and Ku, 2023) and employment (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, and Minale, 2021). Other studies have focused on local conditions at refugees' arrival, such as the size of co-ethnic networks (Edin, Fredriksson, and Åslund, 2003; Damm, 2009), employment levels (Åslund and Rooth, 2007; Azlor, Damm, and Schultz-Nielsen, 2020), crime rates (Damm and Dustmann, 2014), and urbanization (Eckert et al., 2022), among others.

Our work contributes to the literature on the determinants of refugees' integration by emphasizing the importance of exposure to different employers for their labor market outcomes. To the best of our knowledge, it is the *first* to focus specifically on employers and their quality, proxied by pay-setting policies, bridging the gap between the literature on refugees and job ladder models. Importantly, our analysis examines effects over a 15-year period, focusing on refugees who do not emigrate during this time. Following Foged et al. (2024a), 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 previous studies.

Second, by focusing on employers, our study also engages with the literature analyzing the role of firm wage-setting policies in shaping workers' outcomes. Specifically, our work contributes to the small but fast-growing strand that employs the AKM framework to understand the role of firms in explaining the immigrant-native earnings gap (Damas de Matos (2017) in Portugal, Dostie et al. (2023) in Canada, Arellano-Bover and San (2023) in Israel), as well as Åslund et al. (2023), who examine earnings

[8]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).

differences through a job ladder model based on firm productivity groupings. These studies consistently find that an important portion of the earnings gap (approximately one-fifth) is attributable to natives earning higher average workplace premia.[9] Rather than decomposing this gap to understand the roles of differential sorting and differential pay setting in explaining immigrants' disadvantages relative to natives, our paper demonstrates that initial exposure to different rungs of the job ladder can shape the long-term assimilation trajectories of individual refugees.

We also provide complementary findings to this strand of literature. Dostie et al. (2023) show that employment reallocation over time toward higher-paying employers drives the reduction in immigrant-native wage gaps. 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. This dynamic appears to be especially relevant for refugees. Our findings indicate that arriving in a context with a stronger established network in terms of ties to higher-quality employers, leads to improved outcomes for refugees, highlighting the importance of this mechanism. Additionally, our results align with evidence from Åslund et al. (2023), who find that in Sweden, immigrants are less likely than natives to climb the productivity ladder and transition across firms. Our exploration of firm ladder climbing among refugees confirms the same pattern for this specific subgroup, as expected, given the numerous challenges they face.

Third, our work also relates to the extensive literature documenting the importance of social networks in determining labor market outcomes (Ioannides and Loury, 2004; 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). While most studies analyzing the role of co-ethnic networks for refugees have focused on the effects of network size and quality based on co-national outcomes, such as employment and earnings (Edin et al., 2003; Damm and Schultz-Nielsen, 2008), we construct a measure of network quality based on employer characteristics. Although larger networks and networks with

[9] Dostie et al. (2023) documents that the share of the gap explained by workplace premia derives almost entirely from between-firm sorting (i.e., the differential sorting of natives and immigrants across employers), with no evidence of higher-paying firms compressing their premia for immigrants relative to natives (i.e., a pay-setting effect). Conversely, Arellano-Bover and San (2023) find that the pay premium gap arises not only from the sorting of immigrants and natives across distinct firms but also because, within firms, pay premiums awarded to immigrants are lower than those awarded to natives.

higher employment rates among their members are generally associated with better assimilation, the impact of the quality of firms employing network members has not yet been explored.[10] Whether having connections at high-quality firms is beneficial or detrimental remains an empirical question. Such connections could either serve as stepping stones to better jobs or prove disadvantageous if those firms are more difficult to access. Our study addresses this question in the context of refugees in Denmark.

A study closely related to ours is Schmutte (2015), which uses employer-employee matched data from 2002 and 2003 in the United States to examine the effect of local referral network quality on the pay premium from job transitions. The study finds that the impact on the quality of job search outcomes is nearly twice as strong for non-native workers compared to natives. Our work differs in several important ways. First, we specifically focus on refugees in a different context and examine employment and earnings over a long time horizon as our main outcomes, rather than pay premia at the new job. Second, while we construct our main measure of network quality in a similar manner—by averaging AKM-derived employer fixed effects among network members—we do not restrict this to individuals living in the same neighborhood or block. Instead, we explore different aggregations at the time of arrival, such as over active firms in same the municipality, over firms that employed co-nationals in the past, or over those employing co-ethnic members in the same municipality, aiming to proxy early exposure to accessible employers. Third, our identification strategy is arguably stronger, as it leverages the quasi-experimental design of the Danish dispersal policy rather than relying on individual residence in residential blocks within larger neighborhoods and local fixed effects. Lastly, the quality and scope of our data enable us to incorporate a time dimension into the analysis, allowing us to track individuals over a 15-year period following their arrival.

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 stylized facts on refugees and establishments. In Section 6, we discuss our empirical framework, while Section 7 presents and interprets our main results. Section 8 introduces the theoretical framework used

[10] Beaman (2012) examines the dynamic implications of social network size for the outcomes of resettled refugees in the United States, showing that an increase in network size can negatively affect some cohorts within a network due to congestion in job information transmission.

to conceptualize these findings. Finally, Section 9 concludes.

2 Institutional Setting

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.[11] The council was tasked with assisting asylum seekers in applying for refugee status and residence permits in Denmark. In 1986, in response to the large inflow of refugees during the early 1980s, the Danish government implemented a dispersal policy for individuals whose asylum cases had been approved (refugees), which was implemented by the council.

The policy consisted of two stages and was in effect from 1986 to 1998, (Damm and Dustmann, 2014; Dustmann, Vasiljeva, and Piil Damm, 2019; Dustmann et al., 2023). Its aim was to distribute refugees arriving in Denmark across counties and municipalities in proportion to population sizes of the localities. In the first stage of the policy, refugees were allocated to the 15 Danish counties based on the population size of each county. In the second stage, they were further allocated to municipalities within the counties, also in proportion to the population size of each municipality. During this period, Denmark had 275 municipalities. The allocation of refugees in proportion to the population size of municipalities was intended to be achieved over 3–5 years and was implemented through a rotation scheme, with council offices rotating between towns within the counties.

When refugees arrived in Denmark and sought asylum, they were initially placed in Red Cross reception centers across the country. As a first step, once their asylum applications were approved, refugees were relocated to temporary housing in one of Denmark's 15 counties within 10 days. Subsequently, the local office of the council 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 find permanent housing within this time frame. Allocation decisions made by the council were made without face-to-face meetings with the refugees, but after receiving asylum, refugees completed a questionnaire that collected personal information such as nationality and family size. These characteristics were used by the

[11] Denmark was the first country to sign the 1951 Refugee Convention, see also https://maint.loc.gov/law/help/refugee-law/denmark.php

council to allocate housing. However, testing the conditional random assignment to municipalities, in Table 2 we show the orthogonality of refugee characteristics and location characteristics, conditional on nationality, family size, and year of arrival, which are the variables observed by the authorities.[12] In particular, we show that education measures, likely correlated with unobservable skills, are not correlated with municipality characteristics linked to co-nationals' presence, labor market success and employer quality.[13]

While there were no restrictions on mobility for refugees following their initial assignment to municipalities, they were incentivized to remain in the assigned municipality their first 18 months. This was because the introduction program, which included courses in Danish language, culture, and job training, was only offered in the assigned municipality, even though eligibility for means-tested social benefits was not conditional on refugees remaining in their assigned municipality (Damm, 2005). Further, reassignment requests were only taken under consideration by the council after the initial assignment. Figure 2 shows the proportion of refugees residing in their assigned municipality over the years following their asylum approval (Panel A). In the initial years, the majority of refugees remained in their assigned municipality, and even by year 15, more than 40% still resided there.[14] This is important as we emphasize the role of initial exposure to employers in the municipality of assignment.

3 Data and Sample

Our study utilizes Danish administrative data provided by Statistics Denmark. The datasets we use cover the entire population of individuals and firms in Denmark and includes detailed information on various 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 (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

[12] We always control for these variables in our analysis.

[13] Additional checks are performed in Damm and Rosholm (2010); Damm (2014); Foged et al. (2024a).

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

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, building on Foged et al. (2024a). 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. Since actual refugee status is not directly observable prior to 1997, we follow other studies and impute refugee status for the years 1987-1998 (Damm and Dustmann, 2014; Foged et al., 2024a). An individual is considered as a refugee if they meet the following criteria: 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. The second criterion is applied to ensure that the sample consists of refugees subject to the quasi-random dispersal policy.[15] We restrict our sample of refugees to those that were aged 18-55 at arrival and drop subsequently arriving spouses. Lastly, we restrict our analysis to refugees remaining in Denmark at least 15 years. Figure 3 displays the size and the origin composition of the different arriving cohorts of refugees in our sample. In the period we study, the yearly number of arrivals ranged from around 900 to roughly 1,700.

4 Measurement of Establishment Quality

This section describes how we proxy for establishment quality. First, we lay out the AKM framework for wages used to estimate workplace pay premia, describing sample restrictions adopted and variance decomposition results (Section 4.1). We then

[15]Notably, Yugoslavians 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.

proceed to discuss the main identification assumption, presenting a variety of tests to show the consistency of our setting with it (Section 4.2). Finally, because we estimate the model without imposing origin restriction on the sample of active workers, we consider whether our estimates accurately reflect workplace qualities for migrants in Denmark (Section 4.3).

4.1 Job ladder model

To proxy the quality of establishments available to refugees in Denmark, we fit a linear AKM model with additive person and establishment fixed effects, building on 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 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.[16]

For the estimation of equation 1, we adopt a common restriction in the literature and consider only private sector employees. 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. 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. 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

[16] 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.

ψ_j in wages. Consequently, we restrict the analysis to the largest connected set of establishments, reporting various statistics in Table 3. 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) and Shapiro and Stiglitz (1984) for theoretical explanations of pay premia). We use the estimated establishment effects as the main measure of employer quality in our analysis.

Table 4 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 f_j to denote establishment indicators, $E\left[f_j' r\right] = 0$ for each establishment j. This amounts to say that the error term r 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.[17] Essentially, this condition prevents forms of "endogenous mobility" driven by specific characteristics of the worker-employer match that can be interpreted as "interaction

[17] 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.

effects".[18]

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, Cardoso, and Kline (2016) for Portugal, Arellano-Bover and San (2023) for Israel, Dostie et al. (2023) for Canada, Song, Price, Guvenen, 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.[19] 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.[20] 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. Additional reassurance against this type of sorting is provided by the rightmost columns of Table 4, which compare 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

[18] 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}.

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

[20] 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.

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.[21] More importantly, the figure displays no evidence of systematic mobility patterns in which 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, since AKM decompositions require large sample sizes to ensure sufficient mobility, 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.

4.3 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. 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

[21] 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.

(Adda, Dustmann, and Görlach, 2022; Arellano-Bover and San, 2023; Dustmann, Ku, and Surovtseva, 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:

$$log(y_{it}) = \alpha_i + \psi^{B(i)}_{\boldsymbol{J}(i,t)} + x_{it}\beta + \epsilon_{it}, \tag{2}$$

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.[22] 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 6. 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 the approach of Arellano-Bover and San (2023) and Drenik et al. (2023). This relationship may reflect, for instance, the relative degree of rent

[22] 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.

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

$$\psi_j^M = \alpha + \rho\psi_j^N + \nu_j \tag{3}$$

where ρ captures the elasticity of immigrant to native pay premia.[23] Figure 7 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.[24]

More importantly, despite evidence of imperfect pass-through, Figure 7 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. Reassured by this finding, we then proceed to use the workplace effects estimated in equation 1 using the full sample of natives and immigrants in the remainder of our analysis.

5 Stylized Facts

In this section, we document three novel stylized facts about the refugees in our sample and their establishments. First, we examine whether transitions between establishments of different quality influence the earnings and tenure of refugees. Figure 8 presents changes in annual earnings (left panel) and tenure (right panel) for refugees who, during their first 15 years in Denmark, move between quartile 1 and quartile 4 jobs—i.e., those with the lowest-paid and highest-paid coworkers, regardless of origin. Refugees transitioning from quartile 1 to quartile 4 jobs experience significant gains in both earnings and tenure in their new jobs. These gains are nearly twice as large as those observed for refugees moving to a new job within the same quartile. Conversely,

[23] 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.

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

refugees transitioning from quartile 4 to quartile 1 jobs experience declines in both earnings and tenure. While these mobility groups are small due to the limited number of refugees in our sample, and while we do not restrict the maximum time allowed between leaving one job and starting another, this evidence suggests that workplace quality plays an important role in shaping refugees' outcomes.

We then ask whether refugees climb the firm ladder over time. To explore this question, we use the estimated firm effects as the outcome variable in the following regression:

$$\hat{\psi}_j = \left[\sum_{\tau=1}^{15} \beta_\tau \mathbf{1}\{YSM_{it} = \tau\}\right] + X'_{it}\gamma + \delta_{t'} + \epsilon_{it}, \tag{4}$$

where the set of parameters β_τ captures the trajectory of refugees over 15 years since migration. We include a vector of individual controls (age terms, gender dummy, family structure variables), cohort fixed effects, and cluster the standard error at the individual level. We plot the estimates of parameters β_τ in Figure 9, drawing two key insights from the evidence. First, refugees climb the firm ladder very slowly, showing a statistically significant improvement in workplace quality only by the tenth year after arrival. This improvement persists through the end of the observation period. Second, the magnitude of these gains is modest. This is unsurprising given extensive evidence documenting persistent labor market gaps between refugees and other groups of workers in host countries, as well as the numerous obstacles refugees face (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.

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:

$$[Pr(\text{Hired by } j)]_{ij} = \alpha + \beta D_{ij} + X_i'\gamma + \lambda_j + \epsilon_{ij} \tag{5}$$

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. 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 5 reports estimates of parameter β using different sets of controls and fixed effects.[25] 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 this evidence as suggesting that establishment quality is consequential for refugees' labor market outcomes, and that initial conditions influence their subsequent transitions across workplaces. While we have shown that refugees climb the employer ladder, they do so very gradually and modestly, relying on ethnic networks upon arrival when searching for a job. We will explore these dynamics more formally in the next sections.

[25] 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 Model

In this section, we outline the empirical strategy used to estimate the effect of employer quality on the labor market outcomes of refugees in Denmark. Specifically, we leverage the implementation of the 1986–1998 dispersal policy, which effectively randomized the allocation of refugees across Danish municipalities. This randomization allows us to obtain quasi-experimental evidence on the impact of exposure to different employers at the time of arrival. 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.

After estimating establishment AKM fixed effects as described in Section 4, we proceed to aggregate these effects 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. 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 or had previously employed at least one co-national. 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 function f to average individual establishment effects, ψ_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, 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 our measures of exposure to employer quality at arrival as follows:

$$y_{iom,t} = \alpha + \beta_1 f[\hat{\psi}_j]_{om',t'-1} + \beta_2 Sh.Conat._{om',t'} + \beta_3 Emp.NW_{m',t'} + X_{i,t} + \gamma_{m'} + \delta_{o,t'} + \epsilon_{iom,t} \quad (6)$$

where $y_{iom,t}$ denotes employment and earnings outcomes for refugee i from country of origin o living in municipality m at time t, who arrived in Denmark in year t' and was assigned to municipality m'. Here, $f[\hat{\psi}j]_{om',t'-1}$ captures the average quality of

establishments in municipality m' employing other migrants from country o, one year prior the arrival of refugee i.[26] The causal parameter of interest is β_1.

Equation 6 includes controls for the population share of co-nationals in the municipality of assignment at arrival, $Sh.Conat._{om',t'}$, and for the employment rate of non-Western immigrants, $Emp.NW_{m',t'}$. 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-specific characteristics, including those observed by the authorities in the dispersal process.[27]

Our preferred specification also includes municipality fixed effects $\gamma_{m'}$, which account for time-constant differences in economic advantages across municipalities, effectively capturing location m''s absolute advantage for all refugees, as well as origin-by-year cohort fixed effects $\delta_{o,t}$, which control for selection along unobservable factors in inflows from origin o.[28] Hence, 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). Robust standard errors are clustered at the municipality of assignment level throughout the analysis. 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)).[29]

7 Main Results

Our main results derive from the estimation of equation 6 at the three different levels described in Section 6. Panel A in Table 6 shows the effects of geographic expo-

[26] 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 6 would have been $f[\hat{\psi}j]_{m',t'-1}$.

[27] 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.

[28] 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.

[29] 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.

sure 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 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.

In Panel B of Table 6, 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 6, 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).[30] 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.

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

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

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 employers' general willingness to hire refugees of a given nationality. In Section 7.2, we examine the specific network mechanisms that drive our main results.

Interestingly, if we compare the results from Panel C in Table 6 with the endogenous estimates of Table 1, we can see that the effects are much smaller in magnitude. This fact suggests that our initial estimates from the naive exercise of considering the actual first employer of refugees were indeed biased, and the bias was positive, possibly reflecting positive assortative matching between workers and establishments.

It is 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 7 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.[31]

7.1 Robustness checks

We perform several robustness checks of our main results. First, we estimate the same specification as in equation 6, using a weighted average to aggregate individual establishment effects $\hat{\psi}_j$. The weights are full-time equivalents at each establishment, reflecting the importance of employers within municipalities and networks based on

[31] 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.

their size and the ratio of part-time to full-time employment. We report the estimates from this exercise in Table 8 for the measure of geographic exposure to all employers in the municipality (Panel A) and for the measure of exposure to local employers that employ co-nationals at the time of arrival (Panel B). Estimates for the latter remain essentially unchanged in magnitude, compared to those in Panel C of Table 6, but become statistically significant also when considering the bootstrap p-values in the medium run.

Second, we estimate an alternative specification that includes both our geographic and network-based measures of exposure to employer quality, aiming to isolate their independent effects. The coefficients on network quality, reported in Table 9, 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 10, 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 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 with our network-based measure of quality is that it may capture local confounding variables that make a municipality generally advantageous for refugees. To assess whether network effects drive our findings rather than other local characteristics related to the treatment variable, we perform a permutation-based placebo test. Specifically, we run equation 6 1,000 times, each time assigning the network quality of one of the other eight origins to dispersed refugees from a given country of origin.[32] By permuting the treatment across countries of origin, we generate a distribution of placebo effects under the null hypothesis that the treatment has no effect. We repeat this procedure for each outcome for which we found an effect in our preferred specification (Employment 1–5 years, Employment 6–10 years, Earnings 1–5 years, and Earnings 6–10 years) and plot the resulting distributions in Figures 11 and 12. In every case, the true effect estimated in Panel C of Table 6, depicted by the black solid line, is more extreme than the 95^{th} percentile of its respective placebo distribution.

Nevertheless, our network measure might simply capture differences in the types

[32] In practice, we perform a derangement with repetitions.

of jobs that firms offer. In other words, 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. To address this concern, we use industry information for each establishment to classify jobs held by employed network members into one of nine industry groups.[33] We then repeat our main analysis, controlling for the industry shares of the network, and report the estimates in Panel A of Table 10. Our main results remain unchanged.

We conduct two additional robustness checks. In Panel B of Table 10, we test the robustness of our estimates for the network-based measure using an alternative quality definition. Instead of averaging establishment fixed effects, we assign a dummy variable equal to one for establishments in the top quartile of the Danish workplace pay premium distribution, and then calculate the average of this dummy within each network to obtain the proportion of top employers in the network. The estimates obtained using this alternative function f in the original specification from equation 6 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.

Finally, we restrict 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. Consistently, the estimates for employment and earnings in the early movers sample, reported in Panel C of Table 10, are substantially smaller in magnitude and statistically insignificant compared to our main results.

7.2 Mechanisms

The literature on social connections in the labor market identifies two primary ways in which social connections benefit job seekers. First, members of a social network may share information about openings at firms where they are employed, have previously worked, or simply have knowledge of. Second, both workers and employers may use

[33]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.

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.[34]

Unfortunately, our data do not permit a direct test of whether the sharing of information about vacancies drives our results. However, we can examine the dynamics of wages and turnover among refugees to test for the use of job referrals. 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. 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).[35] We then estimate the following specification:

$$y_{i,t} = \alpha + \beta_1 \text{JR}_{i,\tau} + \beta_2 (\text{JR}_{i,\tau} * \text{Tenure}_{i,t}) + X_{i,t}\gamma + \phi_t + \delta_{\bar{\imath}} + \lambda_j + \epsilon_{i,t} \tag{7}$$

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 establishment characteristics, $X_{i,t}$, year fixed effects (ϕ_t), cohort fixed effects ($\delta_{\bar{\imath}}$), and, in the preferred specification, establishment fixed effects (λ_j). Standard errors are clustered at the municipality of residence level.[36]

[34]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 confident these dynamics do not drive the results we observe.

[35]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.

[36]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

Table 11 reports estimates from equation 7, with columns 1 to 3 presenting results for the wage regressions and columns 4 to 6 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 establishment fixed effects, are shown in columns 3 and 6. While the pattern of signs is somewhat consistent with expectations (with the exception of β_2 in column 6), the coefficients are not statistically significant. Therefore, we interpret this evidence to suggest that referrals are not the primary driver of our results. Instead, we believe our findings are more likely explained by standard practices of vacancy information sharing among network members.

8 Theoretical Model

In this section, we conceptualize our findings within the theoretical framework developed by Beaman (2012), which draws on earlier work by Calvó-Armengol and Jackson (2004). We derive two novel predictions regarding the employment and wages of network members. In the model, networks function as mechanisms for information transmission, where agents who receive job offers pass them along to unemployed network members if they are already employed and their current wage exceeds the offer. Networks are considered efficient when job offers are continuously passed along until all available information is utilized, and inefficient when jobs are filled quickly, limiting the time available to share job information. We extend Beaman's framework by endogenizing the network's efficiency level and linking it to the quality of the firms employing network members.

The basic structure of this overlapping generations framework is as follows. The economy is populated by a continuum of agents (i.e., refugees in our case) who live and work for S periods. An agent's cohort is defined by the year the refugee arrives in the host economy. Each cohort c consists of N_c identical agents. In every period, agents randomly receive information about job openings with probability a: if an agent is unemployed and receives job information, they fill the position; if the agent is already employed, they pass along the information to a randomly selected unemployed network member. At the very beginning of each period, an employed agent might lose their job with probability b (the exogenous breakup rate). For individuals already in

tenure profiles across subgroups that differentially rely on referrals do not bias the estimates (Glitz and Vejlin, 2021).

the market for at least one period, the probability of being employed is:

$$s_c^t = (1-b)s_c^{t-1} + (1-(1-b)s_c^{t-1})(a+v^t) \quad \text{if } c \le t \le c+(S-1) \tag{8}$$

where v^t is the probability of receiving information about a vacancy from network members. This probability can be expressed as:

$$v^t = \sum_{k=t-S+1}^{t-1} \frac{aN_k(1-b)s_k^{t-1}}{\sum_{k=t-S+1}^{t} N_k - (1-b)\sum_{k=t-S+1}^{t-1} N_k s_{t-1}^k} \tag{9}$$

Agent i receives information about a vacancy that includes an attached wage offer, w_{ict}^0. Unemployed agents always accept the job, while employed individuals accept it only if the offered wage exceeds their current wage ($w_{ict}^0 > w_{ict}$). Otherwise, the offer is passed on to a randomly selected network member who is either unemployed or employed at a wage lower than the offer. All wage offers are i.i.d. draws from a distribution $F(w)$. The cumulative distribution function of accepted wages is denoted by $G^t(w)$, while $H(w)$ represents the cumulative distribution function of wages available to be passed within the network; both are endogenous. In particular, it can be shown that:

$$1 - H(x) = \frac{a\hat{E}^{t-1}\left(1-\hat{G}^{t-1}(x)\right)(1-F(x))}{\bar{N}^t - \hat{E}^{t-1}\left(1-\hat{G}^{t-1}(x)\right)} * \frac{\bar{N}^t - \hat{E}^{t-1}}{a\hat{E}^{t-1}} \tag{10}$$

which represents the fraction of wage offers greater than a given level x that are still available to be passed on within the network.

Inefficiency is expressed in terms of information-passing technology. Since jobs may fill quickly, imposing a constraint on the amount of time available to pass along job information results in a limited number of rounds for job information transmission. For instance, when information can only be passed once, individuals who receive multiple offers accept the highest offer and discard the lower-wage ones, leading to the loss of some available offers within the network. Let $\hat{E}^{j-1}$ denote the number of network members already employed at the beginning of period j. The percentage of available information in the network lost due to inefficiency in period j is:

$$L^j = \frac{\overline{N}^j}{a\hat{E}^{j-1}} \sum_{z=2}^{a\hat{E}^{j-1}} \binom{a\hat{E}^{j-1}}{z} \left(\frac{1}{\overline{N}^j}\right)^z \left(\frac{\overline{N}^j - 1}{\overline{N}^j}\right)^{a\hat{E}^{j-1}-z} (z-1) \tag{11}$$

Introducing inefficiency has implications for both employment and wages within the network. In particular, the employment rate for a given cohort entering the market is:

$$s_j^j = a + \sum_{k=j-S+1}^{j-1} \frac{aN_k(1-b)s_k^{j-1}}{\overline{N}^j - (1-b)\sum_{k=j-S+1}^{j-1} N_k s_{j-1}^j}(1-L^j) \tag{12}$$

As a result, the employment level declines due to the inefficiency. However, the average wages of those employed increase relative to the efficient case, as low-wage draws from $H(w)$ are now discarded.

We expand this framework by considering a continuum of possible efficiency levels within a network. We assume that network members who receive information about vacancies with high wage offers—and choose not to accept them—have more time to pass on the information within the network. This assumption aligns with empirical evidence showing that high-wage jobs tend to attract more applicants (e.g., Banfi and Villena-Roldán (2019); Marinescu and Wolthoff (2020)), which is likely to result in longer screening times among high-paying employers, *ceteris paribus*. As a consequence, the quality of firms employing network members at the beginning of period j—proxied in our analysis by the level of workplace pay premia—determines the efficiency level of the information transmission technology. Since they are currently employed in better-paying jobs, members of high-quality networks pass on higher wage offers to their network. These higher offers take more time to be filled, thereby reducing information loss. By endogenizing the efficiency level of a network and linking it to the quality of firms employing its members, we can rewrite the percentage of available information in the network lost in period j as follows:

$$L^j = \frac{\overline{N}^j}{a\hat{E}^{j-1}} \sum_{z=2}^{a\hat{E}^{j-1}} \binom{a\hat{E}^{j-1}}{z} \left(\frac{1}{\overline{N}^j}\right)^z \left(\frac{\overline{N}^j - 1}{\overline{N}^j}\right)^{a\hat{E}^{j-1}-z} \frac{(z-1)}{q} \tag{13}$$

where quality $q \equiv f(\hat{\psi}^{j-1})$ reflects the pay-premia of firms employing network members. We can then derive two simple implications.

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

This follows directly from equations 12 and 13. The employment level of cohort j is negatively impacted by the inefficiency level of the network, which, in turn, decreases with the quality of employers of other members, as explained above.

Proposition 2: *Higher network quality has an ambiguous effect on expected wages of new members.*

Given that we use pay premia as a proxy for firm quality, networks of high quality ($\overline{q}$) inherently have a distribution of accepted wage offers ($G_{\overline{q}}^{t-1}(x)$) that first-order stochastically dominates the distribution of accepted wage offers ($G_{\underline{q}}^{t-1}(x)$) of lower-quality networks, $\underline{q}$. Formally, this can be expressed as: $G_{\overline{q}}^{t-1}(x) \leq G_{\underline{q}}^{t-1}(x)$ for all x, with strict inequality at some x. This implies that, as the quality q increases, the fraction of wage offers greater than a given level x that remain available to be passed within the network becomes larger, i.e., $1 - H_q(x)$ increases for all x. The intuition is that, by being employed in high pay-premium workplaces, network members in high-quality networks pass on offers that they would otherwise have accepted if they were earning lower wages at their current jobs. Hence, on one hand, the expected wage of members of the new cohort is higher in high-quality networks. On the other hand, the quality-induced reduction in inefficiency allows more offers, including lower-paying ones, to be passed within the network, thereby increasing the presence of lower wages in the network. These two opposing channels are captured by rewriting equation 10 in terms of q:

$$1 - H(x) = \frac{a\hat{E}^{t-1}\left(1 - \hat{G}_q^{t-1}(x)\right)(1 - F(x))}{\bar{N}^t - \hat{E}^{t-1}\left(1 - \hat{G}_q^{t-1}(x)\right)} * \frac{\bar{N}^t - \hat{E}^{t-1}}{a\hat{E}^{t-1}}(1 - g(L^j)) + g(L^j)(1 - F(x)) \quad (14)$$

where both $\hat{G}_q^{t-1}(x)$ and $g(L^j)$ are influenced by q. The term $g(L^j)$ represents the proportion of low offers passed along, acting as a weighting factor that shifts $H(x)$ closer to $F(x)$ as the level of inefficiency decreases. Which channel dominates remains an empirical question that we tested in our setting, finding positive effects of network quality on earnings of newly arrived refugees.

9 Conclusion

In this paper, we present causal evidence on the effect of different employer pay-setting policies on refugees' labor market integration. Using administrative data from Denmark, we estimate workplace-specific fixed effects, which we use as a proxy for employer quality, to construct an average quality measure that newly arrived refugees are exposed to. We leverage Denmark's 1986–1998 dispersal policy, which quasi-

randomly assigned refugees to Danish municipalities, to obtain exogenous variation in exposure to employer quality.

The results show that higher-quality employers in the local co-national network at arrival lead to better employment and earnings outcomes for newly arrived refugees. While this evidence is not significant in the long run (10 to 15 years after arrival), we find a notable positive effect in the short run (1-5 years after arrival) and medium run (6-10 years after arrival).

The paper's findings have important policy implications for host countries. We show that refugees' integration is sensitive to employers of different quality and the connections provided by co-nationals. Rather than focusing solely on logistic considerations or on broad economic characteristics of the local labor market, the design of placement policies, and of integration policies at destination more 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 better matches with firms, and ultimately incorporating a wider range of demand side considerations into dispersal policies can promote a faster and more effective integration for refugees.

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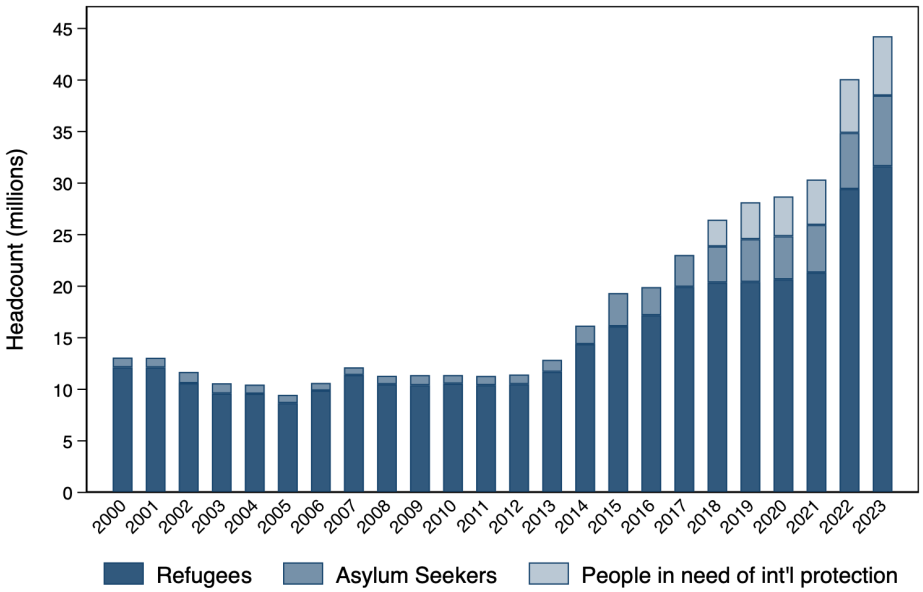
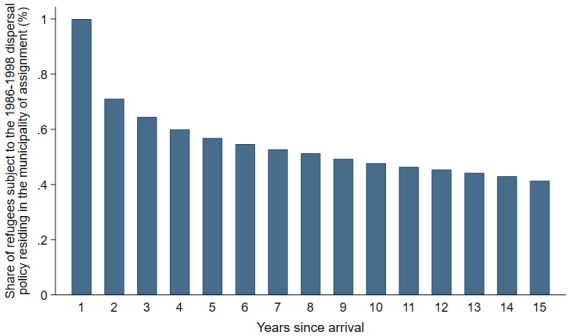
Figure 1: Global forcibly displaced population



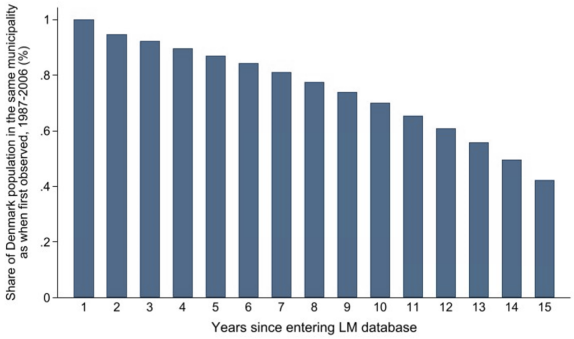
Notes: The histogram plots the number of forcibly displaced population worldwide by group (refugees, asylum seekers, and people in need of international protection). We have excluded from the count internally displaced people.
Source: United Nations High Commissioner for Refugees (UNHCR).

Figure 2: Mobility from initial municipality

(a) Refugees subject to dispersal

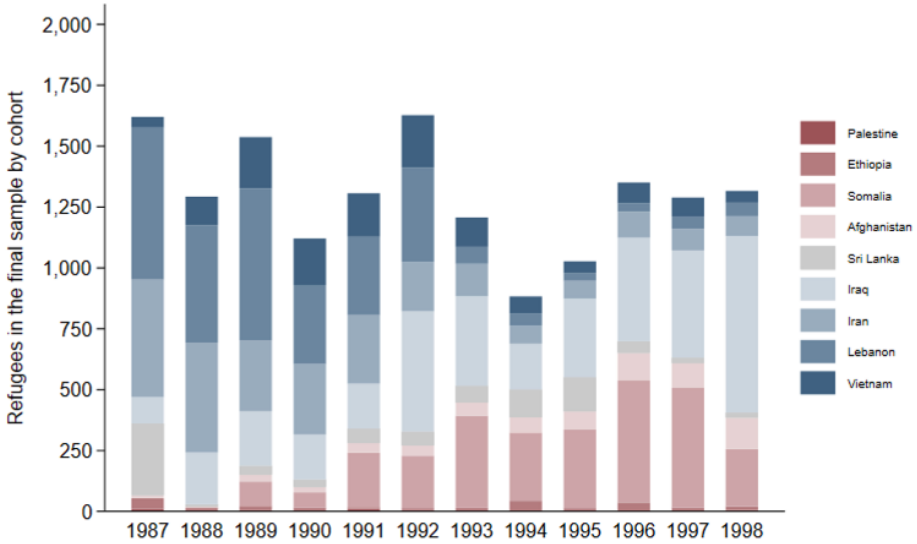


(b) Employer-employee matched data



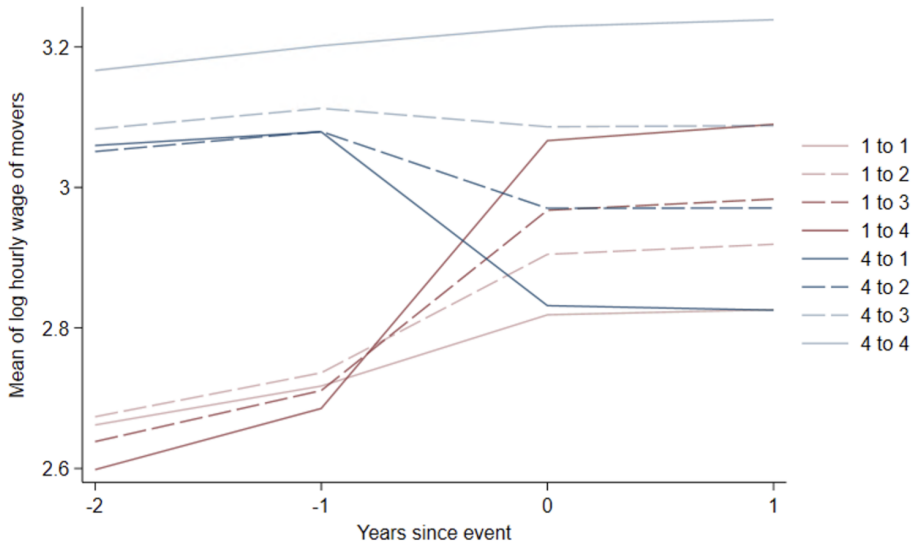
Notes: This figure plots the share of individuals who remain in the same municipality over time. Panel A considers the sample of refugees subject to the 1986-1998 dispersal policy that we use in the analysis (N=15,578) and plots the share who still reside in the municipality of initial assignment by 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



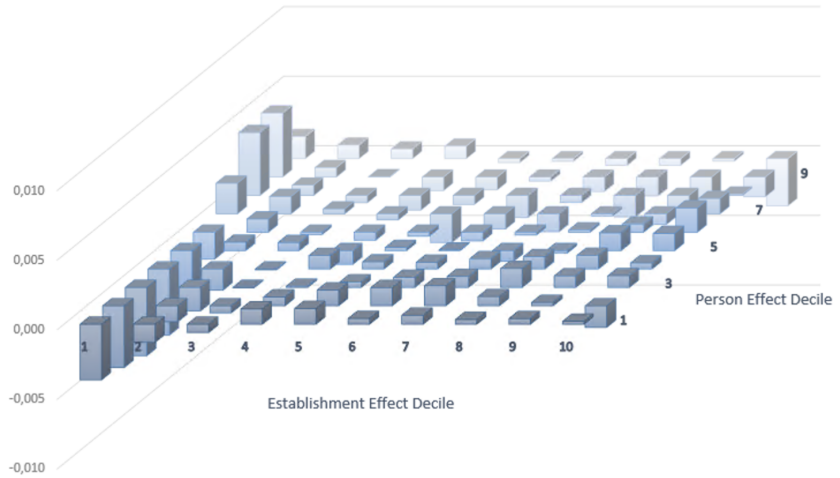
Notes: This figure plots the number of refugees subject to the 1986-1998 dispersal policy by cohort and country of origin.

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



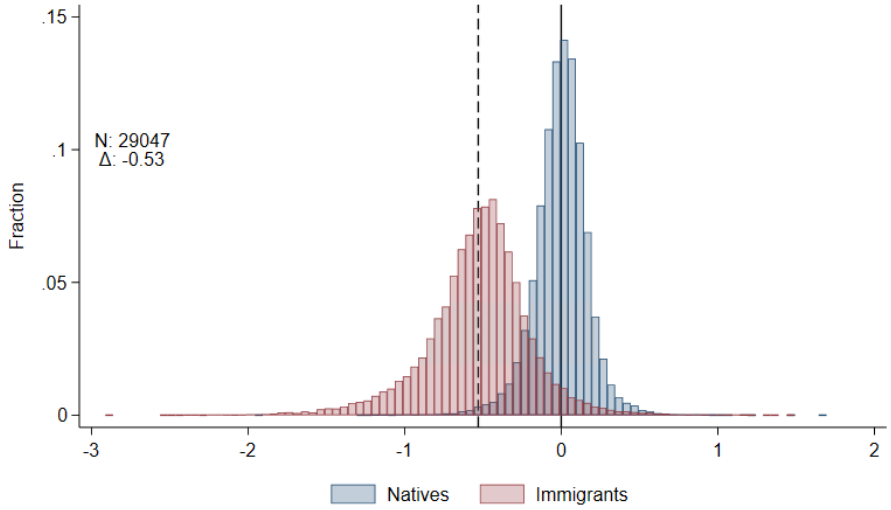
Notes: Event study analysis on the wage effects of job transitions. For any given worker, firms are categorized into quartiles based on the average wage of coworkers. Each point in the figure is the average wage by period, origin, and destination firm quartile, restricting the sample to workers who are 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



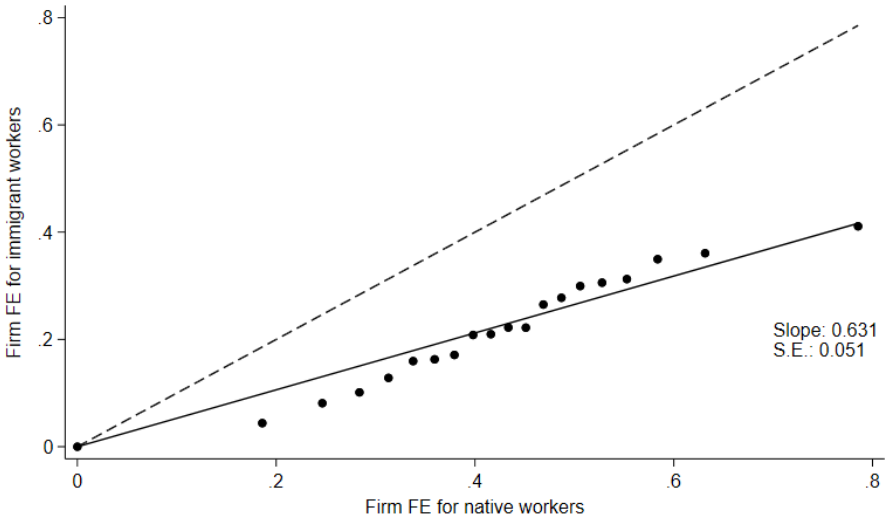
Notes: This figure plots the mean wage residuals from the AKM decomposition for specific types of matches. Meas wage residuals are displayed across 100 cells, defined by deciles of person effects (x-aixs) and establishment effects (z-axis).

Figure 6: 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).

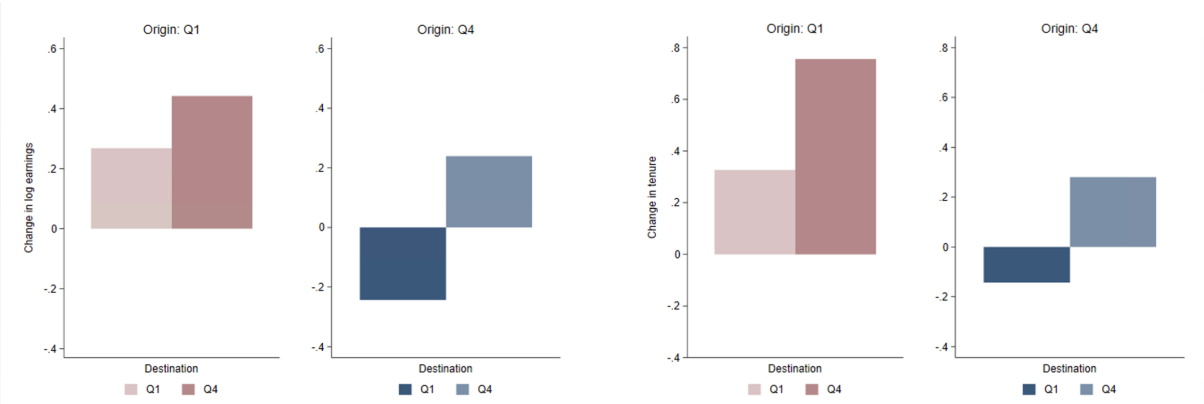
Figure 7: Establishment pay premia sharing



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.

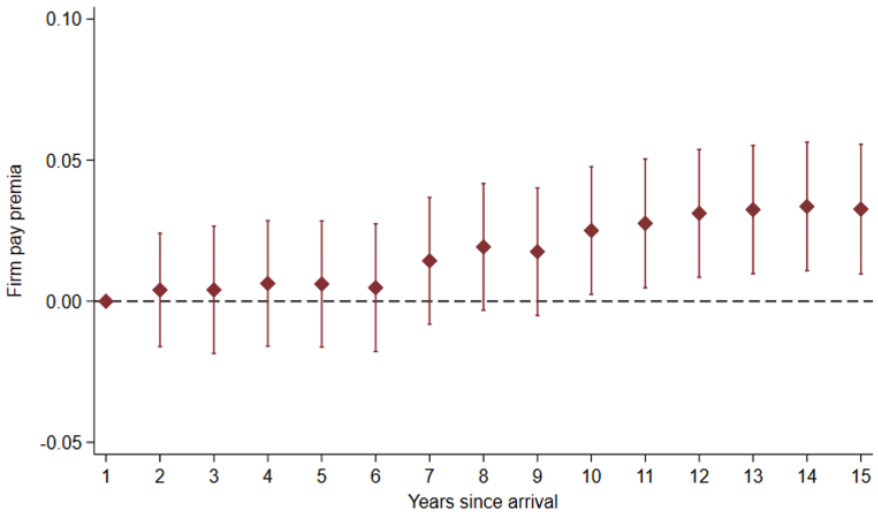
Figure 8: Refugee movers and establishment quality

(a) Earnings (b) Tenure



Notes: This figure plots changes in (log) earnings (Panel A) and in tenure (Panel B) for refugees moving from one establishment to another. The figure only displays the effect of transitions for workers leaving establishment with the lowest-paid (quartile 1) and highest-paid (quartile 4) coworkers, who then obtain a job in an establishment with the lowest-paid (quartile 1) and highest-paid (quartile 4) coworkers. We do not restrict on the maximum time allowed between leaving one job and starting another. Changes in earnings are computed as the difference between 2-year pre- and post-transition averages. Tenure is measured in years.

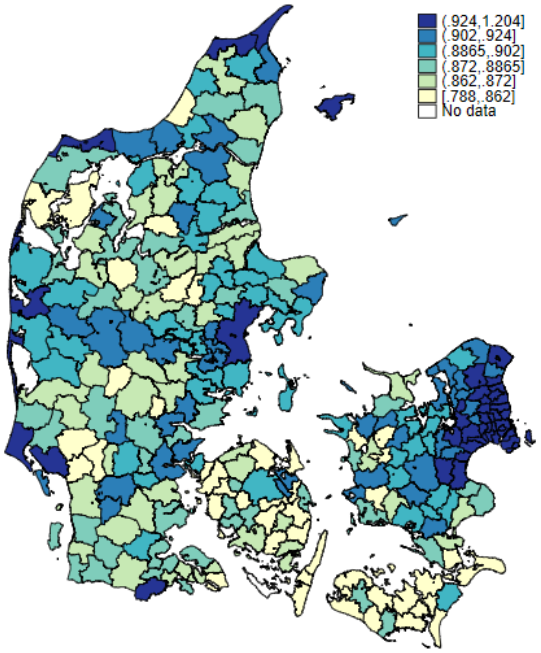
Figure 9: Climbing up the firm ladder



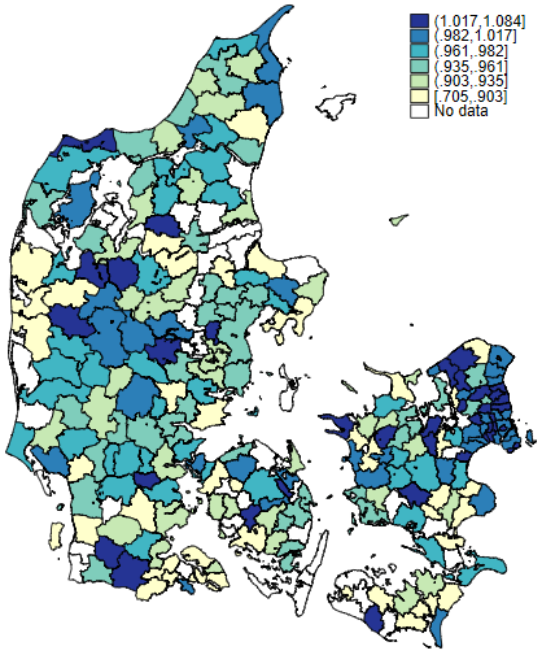
Notes: This figure plots point estimates and 95% confidence intervals of parameters β_τ from equation 4. Standard errors are clustered at the person level.

Figure 10: Geography of establishment and network

(a) Average establishment effects

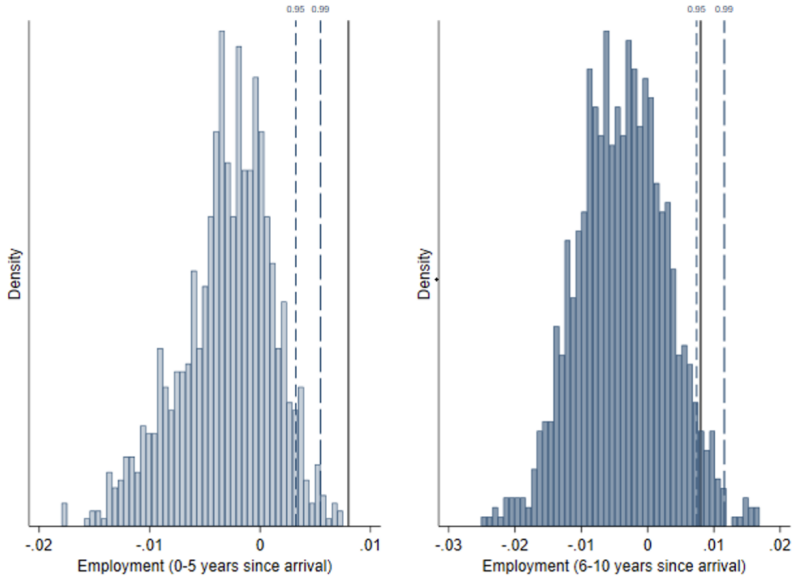


(b) Average network quality



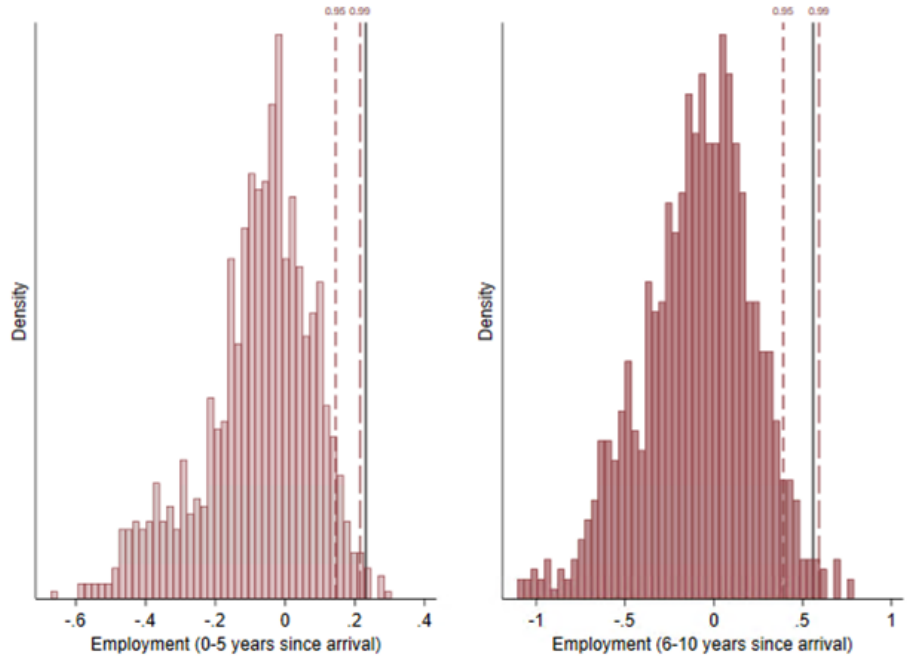
Notes: These maps display the average establishment effects (Panel A) and the average establishment effects within networks (also averaged over origin and year) by sextiles across the 275 Danish municipalities (pre-2007 reform). In certain locations data have been suppressed to comply with Statistics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

Figure 11: Permutation-based placebo test: Employment



Notes: This figure plots the distribution of placebo effects obtained by estimating coefficient β_1 in equation 6 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^{th} (99^{th}) percentile of the placebo distribution.

Figure 12: Permutation-based placebo test: Earnings



Notes: This figure plots the distribution of placebo effects obtained by estimating coefficient β_1 in equation 6 1,000 times, using earnings as outcome (short-run for the left-hand side panel, long-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^{th} (99^{th}) percentile of the placebo distribution.

Table 1: First employer and refugees' integration

	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	0.024***	0.023***	0.020***	2.267***	1.686***	1.697***
	(0.005)	(0.005)	(0.006)	(0.245)	(0.287)	(0.372)
boot. p-val.	[0.001]	[0.001]	[0.009]	[0.000]	[0.000]	[0.000]
Obs.	4189	4082	3855	4189	4087	3868
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

Notes: This sample consists of refugees subject to the 1986-1998 dispersal policy in Denmark. To compute effects in the short-run, medium-run, and long-run, we set the year of hiring by the first employer 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 2: 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.	15571	15571	10271	10246
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 3: Job spell panel for AKM estimation

	Full panel	Largest connected set
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

Table 4: Variance decomposition

Decomposition						AKM fit		CHK match fit	
Total variance	Worker FEs	Employer FEs	Year FEs	2cov	Residual	Adj. R^2	RMSE	Adj. R^2	RMSE
0.148	0.065	0.023	0.029	-0.007	0.026	0.793	0.160	0.865	0.141
	0.440	0.155	0.193	-0.045	0.173				

Notes: The table present the variance decomposition for log wages. Figures in the bottom row represent the share explained.

Table 5: Effects of connections on firm hiring

	(1) Hired by j	(2) Hired by j	(3) Hired by j	(4) 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: Observations are refugee-potential establishment pairs. 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 6: 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.	15554	15062	14701	15554	15062	14701
Adj. R^2	0.176	0.204	0.196	0.151	0.159	0.148
Panel B: Hiring or previously hiring at least 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.	11401	11086	10859	11401	11086	10859
Adj. R^2	0.199	0.221	0.206	0.181	0.179	0.156
Panel C: Hiring at least one co-national						
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.	10246	9971	9781	10246	9971	9781
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 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 7: Characteristics of individual refugees matched with network

	Unmatched			Matched			
	n	mean	sd	n	mean	sd	Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age at entry	5305	29.84	7.86	10273	29.85	8.45	0.010
Female	5305	0.38	0.48	10273	0.44	0.50	0.062***
Married	5305	0.74	0.35	10273	0.73	0.36	-0.006
Pre-arrival academic education	5305	0.11	0.32	10273	0.10	0.31	-0.010
Latin mother tongue	5305	0.39	0.49	10273	0.30	0.46	-0.092**
Predominantly muslim country	5305	0.83	0.30	10273	0.74	0.39	-0.097***
Mean employment dummy	5305	0.21	0.26	10273	0.23	0.28	0.015
First (log) labor income	3403	8.39	1.66	6555	8.53	1.60	0.142**
Mean complex job dummy	5305	0.11	0.21	10272	0.13	0.22	0.017***
First establ. avg. wage (ref. excl.)	2698	28.27	22.06	5382	27.71	23.82	-0.562
(log) Conationals in initial municipality	3997	2.58	1.56	10124	5.23	1.63	2.649***
Conationals share, initial municipality	5305	0.08	0.12	10273	0.27	0.22	0.188***
Urban, initial municipality	5301	0.28	0.45	10273	0.72	0.45	0.437***
Initial Municipality Pop. share of country total (18-65)	5301	0.01	0.01	10273	0.04	0.03	0.029**
Initial Municipality Empl. rate (18-65), Any Empl.	5301	69.42	3.67	10273	68.90	2.78	-0.523
Initial Municipality Empl. rate (Nonwestern imm., 18-65), Any Empl.	5301	46.56	11.87	10273	42.98	7.74	-3.587***
(log) Avg. labor income in initial municipality (USD 2015)	5301	10.36	0.13	10273	10.36	0.11	-0.002
Share college educated in initial municipality (18-65)	5301	0.15	0.05	10273	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 8: Effects of exposure to local firms: Weighted measures

	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: Geographic exposure						
Avg. quality	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.	15554	15062	14701	15554	15062	14701
Adj. R^2	0.175	0.204	0.196	0.150	0.159	0.148
Panel B: Hiring at least one co-national						
Avg. quality	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.	10222	9947	9757	10222	9947	9757
Adj. R^2	0.205	0.226	0.211	0.184	0.181	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

Notes: This table reproduces Panel A and Panel C of Table 6 using weighted measures. Average quality measures are calculated as the weighted average of workplace effects, using full-time equivalents as weights. 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 9: Robustness: Exposure measures jointly

	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)
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]
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.	10246	9971	9781	10246	9971	9781
Adj. R^2	0.206	0.226	0.210	0.185	0.181	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 reports estimates from regressions that include both main measures of exposure to quality as regressors (geographic exposure and network-based exposure to employers hiring at least one co-national at arrival). 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 10: Robustness: Other checks

	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: Hiring at least one co-national - Including industry controls						
Avg. quality	0.008**	0.013**	0.006	0.230**	0.574**	0.365
	(0.003)	(0.006)	(0.007)	(0.114)	(0.253)	(0.385)
boot. p-val.	[0.017]	[0.085]	[0.511]	[0.037]	[0.052]	[0.400]
Co-national share	-0.002	-0.005	-0.004	0.088	-0.086	0.049
	(0.004)	(0.007)	(0.008)	(0.142)	(0.314)	(0.421)
Emp. NW immigrants	0.001	0.002	0.009	0.078	0.234	0.603
	(0.012)	(0.014)	(0.020)	(0.414)	(0.618)	(0.995)
Mean Y	0.107	0.262	0.331	3.080	8.979	14.156
Obs.	10246	9971	9781	10246	9971	9781
Adj. R^2	0.205	0.226	0.210	0.184	0.180	0.159
Panel B: Hiring at least one co-national - Top-quartile employers						
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.	10246	9971	9781	10246	9971	9781
Adj. R^2	0.206	0.226	0.210	0.185	0.180	0.159
Panel C: Hiring at least one co-national - 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.	3545	3468	3388	3545	3468	3388
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: 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: Evidence on the use of job referrals

	Wages			Turnover		
	(1)	(2)	(3)	(4)	(5)	(6)
Member Present	0.005	0.002	0.060	-0.112**	-0.132***	0.008
	(0.074)	(0.076)	(0.075)	(0.044)	(0.043)	(0.059)
Member * Tenure	-0.015	-0.018	-0.028	0.111**	0.113**	0.027
	(0.030)	(0.029)	(0.027)	(0.044)	(0.045)	(0.049)
Obs.	31993	31993	28864	32085	32085	28937
Adj. R^2	0.153	0.154	0.475	0.075	0.084	0.148
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	No	Yes	Yes
Establishment FE	No	No	Yes	No	No	Yes

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