Professional networks and the labour market assimilation of immigrants

Olof Åslund
Mattias Engdahl
Sébastien Willis

IFAU | INSTITUTE FOR
EVALUATION OF
LABOUR MARKET AND
EDUCATION POLICY

Professional networks and the labour market assimilation of immigrants[*]

Olof Åslund[†]   Mattias Engdahl[‡]   Sébastien Willis[§]

February 6, 2024

Abstract

We study how professional networks are related to immigrant labour market integration. Matched employer-employee data for Sweden show that networks grow with time in the host country and that their composition changes from immigrant toward native network members. A firm-dyadic analysis of re-employment of displaced workers suggests that conational connections have a much larger positive effect than native connections. However, the employment effect of native connections grows with years since migration. Furthermore, native connections tend to be associated with higher earnings and increased hires in connected local industries. After 20 years in Sweden, the built-up connections raise immigrant re-employment rates by 0.7 to 1.1 percentage points, amounting to 10–20 percent of the observed difference by years since migration. Our findings indicate complete assimilation in the total productivity of professional connections for displaced workers.

Keywords: labour market integration of immigrants, networks, job search
JEL codes: J60, J20, J15

[†]Uppsala University, IFAU, UCLS, CReAM, IZA. olof.aslund@nek.uu.se
[‡]Institute for Evaluation of Labour Market and Education Policy (IFAU). mattias.engdahl@ifau.uu.se
[§]Uppsala University and Uppsala Center for Labor Studies (UCLS). sebastien.willis@nek.uu.se

1   Introduction

Social networks are an important determinant of individual labour market outcomes (Ioannides and Loury, 2004; Topa, 2019) and immigrants use their social networks particularly intensively when searching for work (Dustmann et al., 2016a; Goel and Lang, 2019). We study the development of professional connections and the dynamics of their impact in the context of immigrant labour market integration using matched employer-employee data from Sweden. A large literature finds that immigrant employment rates and earnings show substantial (but incomplete) convergence to those of comparable natives with time spent in the host country.[1] Networks are likely an important, but so far understudied, component of this labour market assimilation process.

The theoretical literature highlights two main ways in which workers and firms use social networks in the job search process. First, workers searching for a job may hear about vacancies from their connections (Boucher and Goussé, 2019; Calvó-Armengol and Jackson, 2004; Granovetter, 1995; Montgomery, 1992). Second, both workers and employers may use referrals provided by their social network to reduce uncertainty about the other side of the market (Barr et al., 2019; Dustmann et al., 2016a; Glitz and Vejlin, 2021; Montgomery, 1991; Burks et al., 2015). These mechanisms are likely to be particularly important for immigrant workers, who often have limited country-specific human capital, helping them receive either more or better job offers. Additionally, immigrants may draw on their networks for information about the labour market in their new country; e.g. job requirements, workplace or industry cultural norms, or characteristics of specific employers (including values and attitudes towards migrants). All of these might improve immigrants' chances of finding a job and the quality of any job match.[2]

Despite the potential importance of personal networks for immigrant job search, the role of networks changing with time spent in a receiving country remains sparsely studied. Our study provides several pieces of novel information. We first describe how job-related networks develop with time in the receiving country. In the main part of the study, we then focus on the job search of workers displaced by a plant closure to estimate the causal effect of different types of connections on immigrants' job search outcomes. We analyse the role of homophily (the tendency of individuals to form stronger relationships with others who share similar characteristics) in the productivity of networks and how this changes as immigrants spend time in Sweden. We consider re-employment in connected firms and local industries, as well as earnings and turnover among the re-employed. Finally, we combine the results of these analyses to quantify the total

---

[1] Since Chiswick (1978), a large literature has consistently documented this pattern in immigrants' labour market outcomes relative to natives across national contexts and time periods. A non-exhaustive list of contributions includes Borjas (1985, 1995), Eckstein and Weiss (2004), Card (2005), Lubotsky (2007), Algan et al. (2010), Dustmann and Görlach (2015), and Rho and Sanders (2021).

[2] There are of course also other reasons unrelated to job search for why personal networks might speed up the economic integration of immigrants. Having a supportive network of friends and colleagues can provide support and encouragement as immigrants work to establish themselves in their new country. Personal networks may also provide immigrants with information on job training programs, language classes, and other resources that can help them integrate into the labour market.

contribution of changes in the composition and use of networks to the convergence of displaced immigrants' re-employment probability to that of natives after a plant closure.

The analysis draws on administrative data covering all 16–64 year-olds in Sweden 1985–2018. With the help of individual and workplace identifiers, workers can be linked to employers and coworkers, past and present. To quantify meaningful work-related networks, we create a measure based on former coworkers from the most recent past workplace. Firms currently employing former colleagues are considered connected to the focal individual.

Consistent with a gradually improving labour market position, we find that coworker networks grow with time spent in the host country. The composition also shifts from countrymen and other migrants toward a dominance of native connections. The number of firms migrants are connected to through natives approaches but never quite reaches the number for native workers. The fact that immigrants have a larger absolute number of connections through foreign-born workers than do natives indicates workplace segregation.

To study the effect of professional networks on labour market outcomes, we relate different measures of the size and composition of coworker networks to the outcomes of job search. To overcome methodological challenges connected to the endogeneity of job search, we focus on workers who are forced to search for a job following displacement after a plant closure.[3] Building on the empirical strategy of Eliason et al. (2023), we first compare workers displaced by the same plant closure to estimate how connections to firms through past coworkers affect the probability of being hired by that firm. Workers are substantially more likely to move into a firm where they have a connection, compared to non-connected workers subject to the same displacement.

We find that the value of a connection is strongly related to origin. For immigrants, the impact of connections from their own origin group is around seven times larger than that of native connections, and also much larger than the effect of other migrant connections. However, the effect of native connections grows over time, doubling over the first twenty years an immigrant spends in Sweden. For natives, native connections matter most; the effect of native connections on the hiring probability is twice as large for displaced natives than it is for displaced immigrants.

To verify that the identifying assumptions hold, we perform a placebo analysis providing precise zero estimates. In other words, the main analysis, and in particular the differential effect of connections by nationality, most likely captures the causal effects of connections on re-employment rather than e.g. typical transition patterns across firms and local industries, or labour market segregation not related to workers' use of networks.

We then extend the approach of Eliason et al. (2023) and document that connections matter beyond hiring into specific firms. There is an even larger increase in the probability that workers are hired in a given local industry when they have a connection to the industry through a past coworker. When considering industry connections, we also find a smaller gap in the effect of conational and native connections on displaced immigrants, and greater convergence in the

---

[3]For example, having a small or low-quality social network will likely dissuade workers from searching for a job. This strategy has previously been used in studies on networks and job search by, e.g., Cingano and Rosolia (2012), Glitz (2017), or Eliason et al. (2023).

effect of native connections to the effects observed for displaced natives. It therefore appears that immigrants with longer durations in the host country do indeed benefit from increased information flows from former native coworkers, even when they are not necessarily hired by that coworker's current employer.

To better understand what differences might exist in the information provided to displaced immigrants by native or immigrant past coworkers, we then study how earnings and subsequent turnover differ between connected and unconnected hires. Consistent with models of job search with referrals (Dustmann et al., 2016a; Glitz and Vejlin, 2021), displaced immigrants hired at firms where they have connections have higher wage earnings and lower turnover than unconnected hires in the short run, but there are no long-run differences. These effects are largest for native connections, and the short-term effect of such connections increases with years spent in Sweden by the immigrant at the time of displacement. The effect of conational connections is smaller and less sensitive to time in Sweden. This suggests that, while displaced immigrants are less likely to get a job through their native connections than through their conational connections, such connected hires, when they do happen, are quite likely to come with referrals to firms, with benefits for wages and turnover. Connections at other firms in the local industry where a displaced worker is hired, on the other hand, are associated with small and insignificant effects on earnings and turnover. We therefore infer that the differential effect of industry connections on hiring, relative to firm connections, can mostly be explained by past coworkers providing information about vacancies at other firms in the local industry in which they work, rather than through, say, the provision of referrals.

Finally, we quantify the total contribution of changes in the size and use of professional networks to observed improvements over time in Sweden in the re-employment probability of displaced immigrants. Immigrants who have been in Sweden 20 years are around six percentage points more likely to be employed in the year after the plant closure than immigrants displaced in their first years in Sweden. To calculate the total effect of displaced immigrants' networks on the hiring probability, we multiply the causal effect of a given type of connection on the probability of being hired at a given firm by the average number of connections of that type. We find that immigrants' connections increase their total re-employment probability by 0.7 percentage points after 20 years in Sweden when considering only firm connections, or by 1.1 percentage points when considering industry connections. These figures correspond to 10–20 per cent of the increase in the re-hiring probability over time spent in Sweden and are at the same level as the total effect of natives' networks.[4] Thus, at face value, the results suggest complete assimilation in the value of professional networks among displaced workers.

Our paper provides novel insights that relate to at least two strands of literature: (i) the labour market impact of ethnic networks for immigrants and the impact of social networks more generally; (ii) the economic assimilation of immigrants. Regarding the first strand of research, a large literature has documented the importance of initial ethnic social networks,

---

[4]Although substantial, this impact is a lower bound. Of course, our network measure does not capture all potentially valuable professional contacts.

i.e. at the time of migration, for an immigrant's successful labour market integration (Battisti et al., 2022; Beaman, 2012; Damm, 2009; Edin et al., 2003; Munshi, 2003). We expand on this literature in two ways. First, we focus on individuals with whom immigrants are known to have interacted (former coworkers) and study the role of ethnicity in strengthening or weakening a given connection, allowing this role to change over time. Second, rather than treating networks as fixed at arrival and studying how a given measure of their quality affects integration in the long term, we study how changes in networks over time spent in Sweden improve the labour market outcomes of immigrants. The only partial exception we are familiar with is Glitz (2017), who allows the effect of the employment rate of past coworkers to differ by the ethnicity of coworkers, but does not differentiate between recent and established immigrants. Our study of the impact of networks following displacement also provides a contribution to the growing literature on differential resilience to economic shocks among immigrants and natives.[5]

Regarding the second strand of literature, the initial earnings gap between immigrants and native-born workers is often explained by a depreciation of skills accumulated by immigrants in their home country, a phenomenon known as downgrading (Dustmann et al., 2013, 2016b). The convergence of immigrant wages to native wages is likely to partly reflect the accumulation of host country-specific human capital (such as language).[6] But increasing returns over time to pre-existing skills is also an important channel (Eckstein and Weiss, 2004). Our analysis of professional networks provides novel evidence both on the accumulation of host country capital in terms of professional connections, and of the changing returns to such capital. The expansion of networks over time explains a non-negligible part of the economic assimilation of immigrants. Our results indicate that the employment rate and earnings of immigrants would be non-trivially lower without the build-up of networks after immigration.[7]

The paper is structured as follows. Section 2 outlines the data and central definitions. In Section 3 we present some descriptions of the build-up of coworker networks and summary statistics on displaced workers and the relationship between network size and re-employment post-displacement. The causal effect of a connection on the probability that a displaced worker is hired by either a connected firm or a connected local industry is discussed in Section 4. Finally, in Section 5 we relate the results on the causal effect of connections to changes in re-employment probability over years in Sweden. We conclude in Section 6.

---

[5]Studies of plant closures typically find that immigrants suffer greater earnings losses compared to natives (Hardoy and Schøne, 2014; Bratsberg et al., 2018; Illing and Koch, 2021), but there is also evidence suggesting that controlling for pre-event outcomes may alter the picture (Balgova and Illing, 2023). The latter study also provides suggestive evidence that differences in social networks may contribute to immigrants' reduced resilience to job losses compared to natives.

[6]Immigrants who master the language of the host country find jobs easier and are more productive on the job (see e.g. Foged et al. 2022, Auer 2018 or Chiswick and Miller 2015).

[7]Different causes of the earnings gap are not mutually exclusive. Immigrants who do not speak (or read) the local language are less likely to have broad social and information networks (Chiswick and Miller, 2015). Recognition of foreign credentials also plays a part in earnings convergence (Brücker et al., 2021) and is likely complementary to larger social networks.

2 Data

2.1 Data sources

The analysis is based on administrative data for the entire Swedish population 16–64 years old during the period 1985–2018. Data from different registries held by Statistics Sweden are linked by pseudonymised personal identifiers.

A compilation of labour market statistics (RAMS, *Registerbaserad arbetsmarknadsstatistik*) contains essential information and matches employees to firms and workplaces. This includes statutory income statements filed annually for each worker by each employer (firm and establishment) to the Swedish Tax Agency. The employment register also includes information on job spells, which we use to verify that the displaced workers and their connections from previous workplaces (or the closing establishment) indeed had overlapping work spells. We discuss how coworker networks are defined in more detail below. Private sector firms (and establishments) are identified by a firm register.

We also use the database Longitudinal integrated database for health insurance and labour market studies (LISA) from which we collect information on gender, birth year and level of education for the entire population. The county of birth for the Sweden-born population and the country of birth (group) for the foreign-born population are collected from a separate table based on the Total population register (*Registret över totalbefolkningen*, RTB).[8] A table containing the most recent immigration (and emigration) dates from the Total Population Register is also used to define the year of immigration for the foreign-born population.

2.2 Sample and definitions

2.2.1 Annual employment panel

The RAMS data are provided as employment spells, measured on a monthly basis. We transform these data into an annual panel of employment information based on an individual's main job over the course of the year. The main job is defined as the workplace where the individual is employed in the month of November from which the individual has the highest annual earnings. If an individual is not employed anywhere in the month of November, no main job is defined. All subsequent analyses build on the annual panel of main jobs to construct firm characteristics, individual networks, identify closures, etc.

2.2.2 Definition of coworker networks

We are interested in the effect of networks of former coworkers that an individual may draw on when searching for a job following a plant closure. To do so, in each year t, we first identify an individual's most recent employer during the previous five years, limiting attention to main jobs at workplaces with non-missing identifiers. We also drop individuals whose prior workplace

---

employed more than 100 workers.[9] This is both for computational reasons, since building the network data and conducting analyses for very large networks is prohibitively computationally intensive, as well as for conceptual reasons. Individuals who work in a large workplace are not likely to interact intensively with all their coworkers, so most links defined in this way are likely spurious. Dropping individuals altogether, rather than dropping only overly large components of their network, ensures we do not artificially deflate the size of the network of individuals with past employment in a larger workplace.

We then identify coworkers from the past workplace who are also no longer working at the past workplace in year t. Finally, we identify the set of firms an individual is connected to via their past coworkers pre-displacement, in year t. In general, when we refer to a worker's connections below, these will be connections to firms (via past coworkers), rather than connections to individuals. We characterise connections to firms using the characteristics of past coworkers who connect the focal individual to a firm.

2.2.3 Identification of closing workplaces

We use the dataset of main jobs to identify likely workplace closures. We define a closure between November of year t and November of year $t+1$ if a workplace employs some individuals in November of t but not in November of $t+1$ or $t+2$. To ensure that we do not misclassify either mergers or changes of workplace identifier (e.g. due to changes of ownership) as closures, we further apply the same restriction as Hethey-Maier and Schmieder (2013) to worker flows from the closing workplace. Specifically, we require that no workplace in $t+1$ employs more than 30 per cent of the workforce in year t of the closing firm. This definition of a closure, combined with the other data requirements for our analysis, implies that we study closures of firms whose last year of operation lies in 1990–2016.

To further ensure that closures are well-defined, we apply the following restrictions, following Eliason et al. (2023). We limit our attention to firms (i) that were employing at least 4 workers in the final year of operation, year t; (ii) composed of a single establishment; (iii) where firm and plant identifiers are non-missing; and (iv) operating in the private sector. We also drop firms that are defined as closing more than once in the data.[10] There are 50,193 closure events satisfying the above restrictions in our dataset.

---

[9]The share of observations censored due to either missing workplace identifiers or overly large workplaces falls across time. While around 19 per cent of the individuals are dropped in 1990, the share is around 11 per cent in 2018 (see Figure A.1). Our estimates are not sensitive to instead including these individuals in the sample and recording them as having no connections.

[10]We use the terms firm, workplace and establishment somewhat interchangeably. To be precise, all networks are defined at the workplace (i.e. establishment or plant) level, using the workplace identifier in the RAMS data, as are connections and hires. When studying closures, we limit ourselves to closures of single-workplace firms, making the distinction superfluous.

3 Descriptive results

3.1 Networks in the population

Figures 1a and 1b describe the gradual build-up of connections for immigrants to Sweden in our sample. In the first figure we have calculated the number of connections based on all immigrants present in Sweden in 1990–2016, including individuals without any connections (for example individuals who were never employed) and excluding individuals whose network information is censored. The number of connections increases sharply during the first couple of years in the new host country, after that the accumulation process slows down. In comparison with natives, for whom the number of native and immigrant connections is shown as horizontal lines, immigrants are connected to fewer firms throughout the first 25 years in Sweden. We also see that immigrants make up a larger share of the network members of immigrants in comparison with natives. Figure 1b shows that the qualitative pattern is similar if we condition on having been employed at some point in the five years prior to the year of observation (although at somewhat higher levels).

Figure 1: Number of connections by years since immigration

(a) All individuals   (b) Previously employed individuals
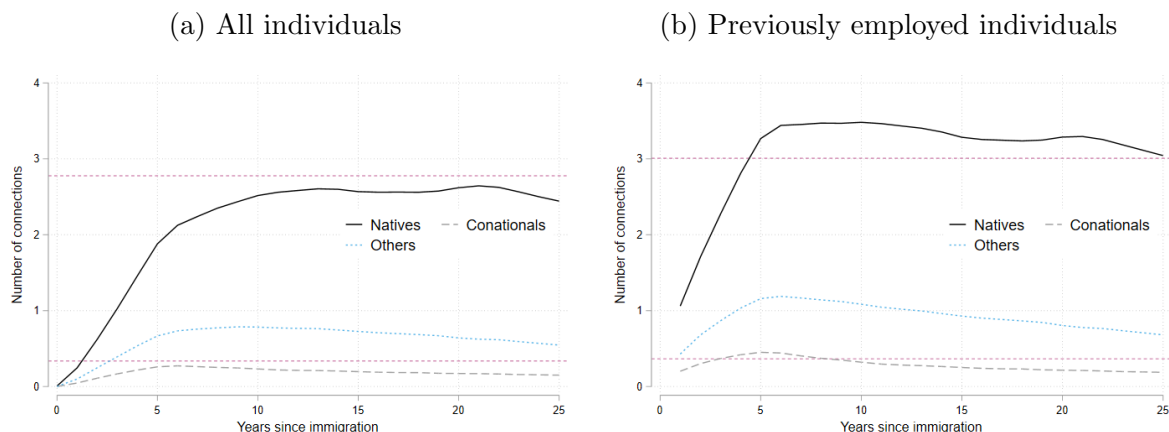
Notes: Number of connections (connected firms) for all individuals for whom the network is defined, including individuals with no connections, for t in 1990–2016. For natives, the average number of native and immigrant connections are shown as horizontal lines, for immigrants, the average number of connections is shown by years since immigration. "Others" refers to non-conational immigrants.

Given our definition of connections, changes in average network size depend on several factors: employment rates of immigrants (the number of connections is by definition zero for those never employed) and their former coworkers; mobility across firms and into and out of employment; the number of coworkers at previous workplaces. Figure A.2 in the appendix shows that entry into employment explains the largest share of network growth, though even established immigrants would have larger networks if they were employed as frequently as natives. On the other hand, immigrants, and in particular recent immigrants, have higher job transition rates than natives. This implies that immigrants would have smaller networks if they had the same

job mobility as natives, conditional on having been employed in the past five years.

3.2 Displaced workers

Our main sample of displaced workers is presented in Table 1. Workers are differentiated by place of birth (Sweden or elsewhere) and, in the case of immigrants, by number of years since immigration to Sweden. Immigrants who experience a displacement in their first 5 years in Sweden are on average 4.7 years younger and 5 percentage points less likely to be females than displaced natives. These differences are consistent with established differences between immigrants and natives in female labour force participation and with the fact that immigrants tend to arrive while young.

The differences in educational attainment are also substantial. The educational distribution of recent immigrants is more polarised, as immigrants are both more likely to have compulsory education and more likely to have tertiary education than natives. These differences are, however, less pronounced for immigrants with longer periods of residence in Sweden. Natives also have a stronger attachment to the labour market and their firm at the time of displacement: on average, natives have worked at 2 distinct workplaces in the five years before the displacement, have been employed for 4.2 out of those five years and have been employed at the closing firm for on average 1.7 years. For immigrants, the corresponding numbers are always lower, and more so the more recently an immigrant arrived in Sweden.

3.3 Networks and re-employment

Table 1 shows that there are large differences between immigrants and natives in the re-employment probability of displaced workers. Immigrants who have been in Sweden fewer than five years are 13.7 percentage points less likely to be employed in November of year $t + 1$ than natives. These differences become less pronounced with years since migration, but an immigrant who has been in Sweden 20–24 years is still 7.7 percentage points less likely to be employed one year later than a displaced native.

In Figure 2, we explore how network size and the re-employment probability change with years since immigration at displacement.[11] The number of connections increases rapidly in the first five years after immigration. Thereafter, the number of native connections increases more slowly, approaching the average number of native connections displaced native workers. The average number of immigrant connections, both conational and from other countries plateaus after 5–10 years, and then declines slightly. The re-employment probability follows a similar pattern to the increase in the number of native connections: the fraction employed in November of $t + 1$ increases from 55 per cent of those displaced between their first and second year in Sweden to around 60 per cent for those losing their job after five years in Sweden. Thereafter, the re-employment probability plateaus around 62–63 per cent, around 7–8 percentage points

---

[11]In Figure 1a, the full population is included. In Figure 2, the focus is on displaced workers only.

Table 1: Summary statistics for displaced workers in year t

| | Native | Years since immigration | | | | |
		0–4	5–9	10–14	15–19	20–24
Female	0.360	0.310	0.329	0.335	0.312	0.320
	(0.480)	(0.463)	(0.470)	(0.472)	(0.463)	(0.466)
Age	37.57	32.88	35.27	36.60	37.20	37.98
	(12.22)	(8.385)	(8.620)	(9.260)	(10.35)	(11.14)
Compulsory education	0.226	0.416	0.312	0.314	0.304	0.291
	(0.418)	(0.493)	(0.463)	(0.464)	(0.460)	(0.454)
Secondary education	0.657	0.325	0.453	0.493	0.552	0.592
	(0.475)	(0.469)	(0.498)	(0.500)	(0.497)	(0.491)
Post-Secondary education	0.117	0.258	0.236	0.193	0.144	0.117
	(0.321)	(0.438)	(0.424)	(0.395)	(0.351)	(0.321)
Number of workplaces, [y-5,y-1]	2.001	0.724	1.682	1.801	1.849	1.957
	(1.068)	(0.778)	(1.090)	(1.096)	(1.100)	(1.110)
Years employed during [y-5,y-1]	4.156	0.993	2.950	3.330	3.472	3.695
	(1.321)	(1.130)	(1.735)	(1.686)	(1.650)	(1.568)
Tenure in firm at y	1.746	0.493	1.105	1.277	1.332	1.380
	(1.961)	(0.821)	(1.505)	(1.701)	(1.752)	(1.794)
Employed one year later	71.18	57.47	62.38	62.63	62.01	63.46
	(45.29)	(49.44)	(48.45)	(48.38)	(48.54)	(48.16)
N native connections	4.337	1.117	2.945	3.397	3.631	3.901
	(6.916)	(3.673)	(5.996)	(6.451)	(6.688)	(6.907)
N migrant connections	0.564	0.715	1.597	1.578	1.488	1.393
	(1.471)	(2.235)	(3.205)	(3.148)	(3.106)	(2.913)
N conational connections	0	0.243	0.480	0.423	0.367	0.380
		(0.993)	(1.372)	(1.171)	(1.123)	(1.193)
N other migrant connections	0	0.487	1.146	1.180	1.142	1.033
		(1.770)	(2.580)	(2.662)	(2.643)	(2.381)
N connections	4.813	1.793	4.442	4.859	4.994	5.177
	(7.675)	(4.968)	(7.663)	(8.061)	(8.302)	(8.403)
N local industry connections	3.766	1.381	3.389	3.697	3.769	3.888
	(5.597)	(3.585)	(5.484)	(5.781)	(5.927)	(5.853)
Observations	263396	17199	10093	7598	7321	7602

Note: Mean coefficients; standard deviations in parentheses. The network variables measure the number of firms a displaced worker is connected to through different types of past coworkers. A worker may be connected to a firm through more than one past coworker, so different types of connections do not sum to the total number of connections.

lower than the average for natives.

Figure 2: Connections and re-employment over time in host country

(a) Number of connections in t (b) Re-employment in $t+1$
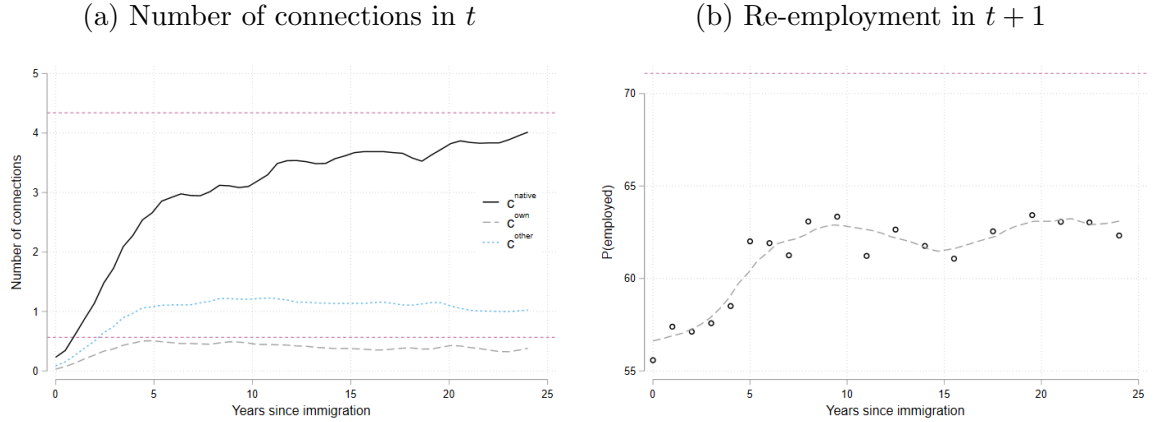
Notes: Number of connections at displacement by years since immigration (Figure 2a) and re-employment probability of displaced workers as a function of years since immigration (Figure 2b). Kernel-weighted local averages are reported, calculated using the Epanechnikov kernel and a default bandwidth. The average number of native connections, immigrant connections, and re-employment probability for natives are reported as horizontal lines. In Figure 2b we also report a binned scatter plot for immigrants, where ventiles of years since immigration are used as bins.

The associations in Figure 2 suggest that the accumulation of networks may contribute to improvements in immigrants' employment outcomes after a displacement. However, such an association may not be causal. Indeed, the descriptive statistics in Table 1 show that more recent immigrants are different from established immigrants along various observable dimensions. Established immigrants may also be more employable for otherwise unobserved reasons. We therefore now turn to an analysis of the causal effect of different types of connections on displaced immigrants' job search outcomes.

4 Networks and job search outcomes of displaced workers

4.1 Main empirical strategy

To understand the causal effect of connections on job search outcomes and whether this effect varies between immigrants and natives or changes with time in Sweden, we analyse the causal effect of a connection to a firm on the probability that a displaced worker is hired by the connected firm. We adapt the approach proposed by Eliason et al. (2023). For all workers i displaced by a plant closure in a given year, we first form all individual, i, by potential hiring firm, k, dyads. Importantly, there will be variation in whether or not displaced worker i is connected to firm k via a past coworker. Next, we use the following specification to estimate

the effect of being connected to firm k via a past coworker on the probability that i is hired by firm k following a displacement:

$$Y_{ijk} = \sum_{h \in H} \sum_{n \in N} \gamma^{nh} c_{ik}^n c_i^h + \alpha_{jk} + X_{ik}'\beta + \epsilon_{ijk}. \tag{1}$$

Y_{ijk} is an indicator for displaced worker i, employed at firm j in November of year t, being employed at firm k in November of year $t + 1$, multiplied by 100. γ^{nh} is the main effect of interest; the effect of a type-n connection at firm k, captured by the indicator variable c_{ik}^n, on the probability that displaced worker i of type h, captured by the indicator variable c_i^h, is hired by k. In the simplest case, $H = \{native, immigrant\}$ and $N = \{any\}$. In this case, we will estimate the effect of having a connection of any type on the hiring probability, allowing the effect to vary by the nativity of the displaced worker. In the main specification, we will further divide both N and H, distinguishing, for example, between connections to firms via either native or immigrant past coworkers.

Intuitively, our approach involves comparing individuals i and i', both of whom are displaced by the closure of firm j, and who differ in that one displaced individual, i, is connected to a firm k through a past coworker. The specification in Equation (1) will estimate the causal effect of a connection on the probability of being hired by k provided that connections to firm k are quasi-randomly assigned to displaced workers i, conditional on the included controls. Equation (1) therefore includes a firm pair fixed effect α_{jk}. The identifying assumption then becomes that, for a given pair of closing firm j and potential hiring firm k, connections to the hiring firm k are as good as randomly assigned across workers at the closing firm j. This assumption appears reasonable, since worker i does not choose the job mobility patterns of their past coworkers. However, the assumption may nevertheless be violated if displaced workers more actively search for jobs in the industry or municipality where they were previously employed, where their former coworkers may still be working. In most specifications we therefore further restrict ourselves to comparisons of workers with a similar work history. This is achieved by interacting the fixed effect α_{jk} with an indicator for whether worker i's prior job, where i interacted with their past coworkers, was in the same industry and municipality as firm k.

Equation (1) also includes a vector of displaced worker i by hiring firm k covariates, X_{ik}, that might predict having a connection at k. This vector includes the interaction of a set of dummies for the region of origin of the displaced worker and the region of origin of the manager at firm k, as prior research has shown that managers are disproportionately likely to hire workers of their own origin (Åslund et al., 2014).[12] X_{ik} also includes an indicator for whether i worked at firm k in the past, or in the industry or municipality where k is located, as well as measures of the composition of the workforce at firm k in year t: the share of co-nationals of i working at the firm, as well as the share of immigrants from other countries of origin.

The inclusion of the jk firm pair fixed effect implies the effect of a connection is identified

---

[12]Our data do not include occupation codes; we define managers as the firm owner, when one is identified in the data, or as the highest-paid worker in the workplace otherwise.

from firm pairs where there is variation across workers at closing firm j in the connectedness to k. Firm pairs for which there is no variation in the existence of a connection across workers at j are therefore dropped from the sample.[13] To avoid bias potentially introduced by being connected to a firm by more than one worker, we also drop displaced individuals connected to firm k by more than one type of connection (e.g., we drop individual i who is connected to firm k through both a native past coworker and an immigrant past coworker). We investigate whether there is heterogeneity in effects by the total number of connections a displaced worker has. Finally, standard errors are clustered by firm pair, jk.

To verify that the analysis outlined above captures causal effects of dyadic connections, rather than reflecting e.g. typical transitions between firm types or worker-manager similarity, we also perform a placebo analysis using Equation (1) based on connections that "could have been but were not". The procedure is described in section 4.2.2.

4.2 Hiring at connected firms

4.2.1 Results

Table 2 shows estimates of the impact of having a connection at a given firm on the probability of being hired there. Column 3 contains the main specification (columns 1 and 2 present simpler versions for comparison; columns 4 and 5 add further restrictions). There are two main messages: (i) connections matter; (ii) the impact of a connection depends on the origin of the displaced worker and the connection. The estimates presented in the main column (3) suggest that for displaced natives, having a native connection increases the probability of being hired at a given destination firm by 0.15 percentage points. This is an economically significant effect, similar to the main result of Eliason et al. (2023).[14] By way of comparison, the probability that an unconnected worker is hired by one of the K firms included in the sample is on average 0.044 per cent. For immigrants, on the other hand, conational connections increase the hiring probability by as much as 0.48 percentage points while native connections only increase the hiring probability for displaced immigrants by 0.07 percentage points. Consistent with homophily in the value of connections, immigrant connections matter less than native connections for natives, and other-group connections have a smaller impact for immigrants.

We test how robust the results are to different possible threats to the identification strategy across the columns of Table 2. Columns 1 and 2 use the less restrictive firm-pair fixed effect, α_{jk}, that does not account for the displaced worker's work history. Compared to the least extensive specification in column 1, in column 2 we add controls for potential confounds that

---

[13]This makes the estimation computationally tractable; otherwise the set of potential hiring firms $k \in K$ would include all firms in a given labour market, or even in the whole country and the set of dyadic observations, with cardinality IK, where $i \in I$ is the set of displaced individuals, would be intractably large.

[14]Eliason et al. (2023) find an effect of 0.25 percentage points using a more restrictive definition of a connection. Our closest specification to theirs is in column 1, where the effect of a native connection for displaced natives is 0.19.

Table 2: Effects of connections on firm hiring

	(1) Hired by k	(2) Hired by k	(3) Hired by k	(4) Hired by k	(5) Hired by k	(6) Hired by k
$c^{native} \times$ Native	0.19***	0.18***	0.15***	0.14***	0.15***	0.15***
	(0.0059)	(0.0061)	(0.0059)	(0.0067)	(0.0060)	(0.0062)
$c^{immigrant} \times$ Native	0.14***	0.13***	0.11***	0.11***	0.11***	0.100***
	(0.015)	(0.016)	(0.016)	(0.018)	(0.016)	(0.017)
$c^{native} \times$ Immigrant	0.092***	0.080***	0.067***	0.065***	0.059***	0.061***
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)
$c^{own} \times$ Immigrant	0.60***	0.58***	0.48***	0.48***	0.48***	0.51***
	(0.078)	(0.078)	(0.077)	(0.084)	(0.085)	(0.080)
$c^{other} \times$ Immigrant	0.12***	0.11***	0.100***	0.100**	0.097**	0.078*
	(0.026)	(0.027)	(0.028)	(0.034)	(0.030)	(0.031)
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed Effects	jk	jk	$jk \times IM$	$jk \times I, jk \times M$	$jk \times IMN$	$jk \times IM$
$\hat{\mathrm{E}}[Y(0)|c^{any}=0]$	0.046	0.046	0.044	0.044	0.044	0.043
N	18546253	18546253	18393690	17996169	18119174	11508128

Note: The dependent variable, an indicator for being hired by firm k, is scaled by 100. Controls are share native, conational, or same education as i, indicators for i having worked in same industry or municipality as k or sharing birth country with k's manager, all interacted with an indicator for i's nativity, and categories for region of birth. FE denoted by jk = firm pair, $I = i$ previously worked in k's industry, $M = i$ previously worked in k's municipality, $N = i$'s nativity. Column 6 restricts the sample to individuals with at least one connection. Standard errors clustered by jk firm pair are reported in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

could increase both the likelihood of having a connection to firm k and the probability of being hired by firm k: homophily with the manager at k, similarity with the workforce, or prior work experience in the industry or location of firm k. The point estimates decrease slightly, but are not materially affected by the inclusion of these covariates.

Compared to the main estimates in column 3, in column 4 we consider separate interactions between the jk fixed effect and various combinations of indicators for i having previously worked in the same industry or municipality as firm k. In column 5 we further restrict the comparison made in column 3 to workers of the same nativity. Again, these refinements decrease the point estimates slightly, but do not affect the main qualitative results. Native connections are more useful to displaced natives, and conational connections are more useful than native connections for displaced immigrants. Finally, in column 6 we only compare individuals who have a connection to at least one hiring firm. Dropping individuals with connections to no firms, whether because of low prior attachment to the labour market, or long tenure in the closing firm, does not materially affect the point estimates.

In Figure 2, we documented that the re-employment probability for immigrants increases with time in Sweden and suggested that this might be linked to increases in the size of immigrant networks. Another possibility is that immigrants' networks, in particular native networks, become more productive over time in Sweden as immigrants acquire complementary host country-specific human capital. To explore this possibility, we interact the connectedness indicators for displaced immigrants in Equation (1) with a cubic polynomial in the number of years since immigration.[15] The effect of a connection on the hiring probability is plotted in Figure 3. The estimates show that the higher productivity of own-group connections persists for at least two decades. However, there is a notable increase in the effect of native connections. Going from 0.04 percentage points after five years in Sweden to 0.09 percentage points after 20 years means that half of the gap to the effect of native connections for natives is closed (cf. column 3 of Table 2).[16]

Individuals who have been in Sweden longer at the time of displacement on average arrived in Sweden at a younger age than immigrants with a shorter tenure in Sweden at the time of displacement. To ensure the time profile of effects in Figure 3 does not simply reflect differences in age of arrival, we drop all individuals who first arrived in Sweden before the age of sixteen and report the results in Figure A.3a. The growth in the effect of national connections is greater in this case; the effect of native connections converges completely to the effect for displaced natives after twenty years. Earlier arrival cohorts will also be more heavily represented among displaced immigrants with a long tenure in Sweden. We check that the time profile of effects is

---

[15]We also include the main effect of the polynomial in years since immigration, interacted with a dummy for the displaced worker being an immigrant.

[16]We can only speculate regarding mechanisms behind the apparent increased productivity of own-group connections over the first 15 years. One explanation can be that previous interactions on average are longer, or that the accumulation of country-specific human capital enhances the value also of this type of connection. In any event, time differences in the productivity of immigrant connections are not statistically significant.

Figure 3: Effects for displaced immigrants by years since immigration
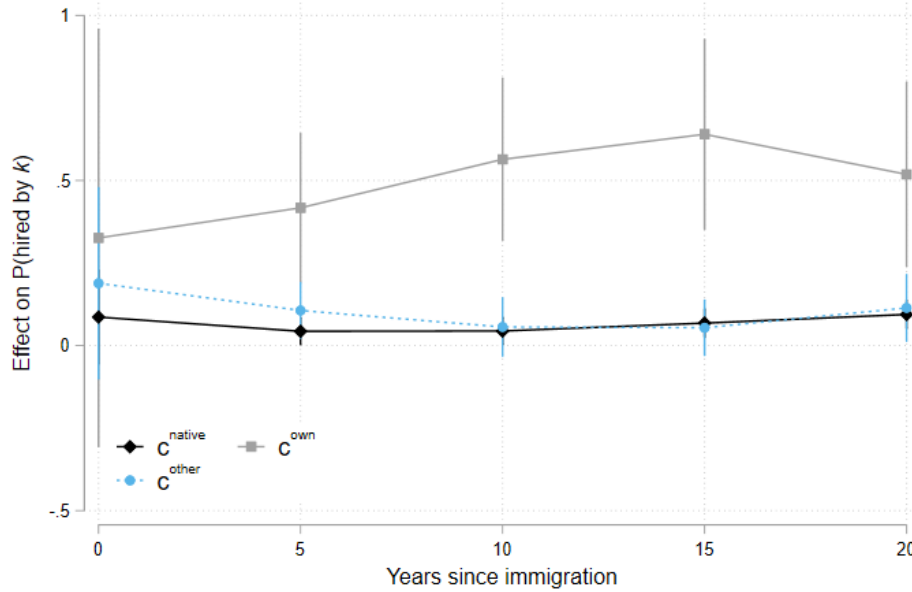
Notes: Heterogeneous effects of connections on the probability a displaced immigrant is hired by firm k, estimated by interacting the connectedness indicators for displaced immigrants with a cubic polynomial in years since migration at the time of displacement, including main effects. Marginal effects of a connection for selected values of years in Sweden are shown. Standard errors are clustered by firm pair jk, 95 per cent confidence intervals are reported.

not capturing cohort effects by interacting the firm-dyad fixed effect with a cohort fixed effect.[17] Estimates from these within-cohort analyses are shown in Figure A.3b. The growth in the effect of native connections is very similar to the original specification, where we compared individuals both within and across cohorts: The effect of a native connection increases from 0.01 percentage points after five years to 0.08 percentage points after 20 years.

4.2.2   Do the identifying assumptions hold?

To establish that our estimates have a valid causal interpretation, we conduct a placebo analysis. The differential effect of conational connections found in Section 4.2 could, for example, represent strong segregation by nationality in the labour market due to factors other than the use of networks in job search. More generally, the effect of a connection could potentially represent unobserved similarity between the connected displaced worker and their past coworker that makes the connected worker more attractive to the hiring firm than the unconnected displaced worker. The idea of the placebo analysis is to perform the main analysis again, only based on simulated connections drawn from contexts related to the actual connections. If the effect of a connection is spurious, and simulated connections are sufficiently similar to true connections, we

---

[17]We define the following cohorts by first immigration date: prior to 1980, 1980–1989, 1990–1999, from 2000; natives are considered a separate cohort.

should observe an "effect" for simulated connections too. If the identifying assumption holds, on the other hand, there should be no effect of simulated connections.

For each worker (with an identified, non-zero network in the displacement year, t) we identify the municipality and two-digit industry of their last prior firm, where they interacted with their past coworkers, as well as the nativity of the manager of that firm. We then randomly draw another firm employing 100 or fewer workers, operating in the same local industry in the same year, with a manager of the same nativity as the displaced worker's true past firm. The employees of this firm are the displaced individual's "placebo past coworkers". We use the same procedure to construct placebo connections from the placebo coworkers as we used with the true coworkers. We identify their workplace in year t, and say that the displaced worker has a placebo connection if the placebo coworker is working in a firm that is neither the closing workplace, nor their past workplace (the displaced worker's placebo workplace).[18]

Table 3: Effect of a placebo connection

	(1) Hired by k	(2) Hired by k	(3) Hired by k
$c^{native} \times$ Native	0.034*** (0.0047)	0.012 (0.0069)	0.011 (0.0060)
$c^{immigrant} \times$ Native	0.0087 (0.011)	-0.011 (0.014)	-0.0073 (0.015)
$c^{native} \times$ Immigrant	0.0012 (0.0090)	-0.0072 (0.010)	-0.0081 (0.011)
$c^{own} \times$ Immigrant	0.068 (0.083)	0.034 (0.089)	-0.024 (0.10)
$c^{other} \times$ Immigrant	0.022 (0.018)	0.014 (0.024)	0.011 (0.029)
Controls	No	Yes	Yes
Fixed Effects	jk	jk	$jk \times IM$
$\hat{E}[Y(0)|c^{any}=0]$	0.026	0.026	0.026
N	7052670	7052670	6921352

Note: IM is an indicator for having previously worked in the industry and municipality of firm k in a displaced worker's previous job. Standard errors clustered by closing firm, j, 95 per cent confidence intervals are reported.

We estimate the same set of dyadic specifications using our placebo sample as we did for the true sample and report the results in Table 3. The results clearly show that placebo connections do not affect the hiring probability. In the simplest specification, without controls and with

---

[18]This placebo exercise is slightly different from the one conducted by Eliason et al. (2023), a point we return to in Section 4.4.1.

only a firm-pair fixed effect (column 1), we only find a significant effect for the native placebo connections of displaced native workers. This effect is very small and not robust to including controls (column 2) or interacting the firm-pair fixed effect with an indicator for the displaced worker having worked in the local industry of the hiring firm in their previous job (column 3). The effects of placebo connections for displaced immigrants are zero for all types of connections and these effects are precisely estimated. The results of the placebo analysis provide strong evidence that the differential effect of different connection types is not due to differences by nativity in "typical" worker flows. In particular, the large effect of the conational connections for displaced immigrants is not due to differential persistence by nationality in the municipality or industry of employment. [19]

4.3   Robustness

4.3.1   Invariance to network size

First, the specification in Equation (1) assumes that the effect of a connection on the hiring probability does not vary with the total number of connections a displaced worker has. We test this assumption by interacting the connectedness indicators for displaced immigrants with the total number of either native or conational connections, also including the main effect of the number of connections of a given type. The results are reported in Figure 4.

Figure 4: Effect for displaced immigrant by number of connections
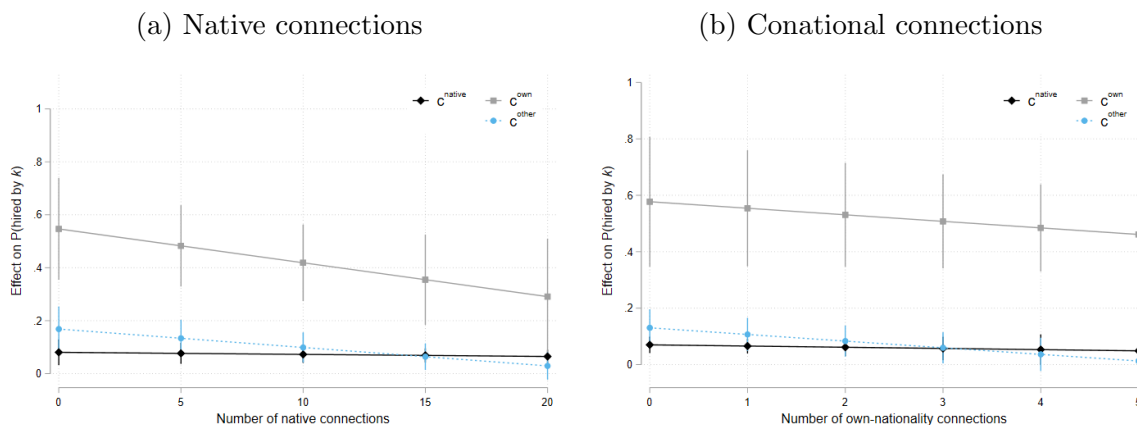
(a) Native connections

(b) Conational connections

Notes: Heterogeneous effects of connections on the probability a displaced immigrant is hired by firm k, estimated by interacting the connectedness indicators for displaced immigrants with the total number of either native or conational connections, including main effects. Marginal effects for a given network size are reported. Standard errors are clustered by jk firm pair, 95 per cent confidence intervals are reported.

---

[19]Note that the baseline probability of being hired in one of the placebo connected firms is 0.026 per cent, compared with a baseline hiring probability of 0.044 per cent in the set of true connected firms. This suggests that our placebo connected firms are comparable to the true connected firms, when it comes to hiring flows, even if the true connected firms are perhaps somewhat more likely to attract workers from the displaced firm.

The downward-sloping lines for immigrant connections (own and other) of Figures 4a and 4b suggest that the marginal value of these connections decreases with the total number of connections. The differences are, though, only statistically significant when comparing extremes of the range of the number of connections. The marginal impact of a native connection does not seem to be related to the number of native or conational connections. Taken together, the evidence in Figure 4 suggests that the assumption of a constant marginal effect of a connection appears reasonable for the number of connections typically observed in our sample.

4.3.2   Heterogeneity by other connecting worker characteristics

The results in Section 4.2 suggest that homophily in country of birth is a very strong predictor of the effect of a connection on the probability a displaced worker is hired by a given firm. To investigate whether homophily in country of birth is proxying for homophily in other dimensions, we further differentiate connections by demographic characteristics potentially shared with the displaced worker. We consider gender, education level, and whether the age difference between the two workers is greater than or less than five years. Figure A.4 reports the results, along with the original estimates.

Two results stand out. First, the effect of a connection on the hiring probability almost always increases when the displaced and connecting workers share one of the demographic traits considered. Second, the effect of homophily in other dimensions is of second-order importance compared to the effect of homophily in country of birth. For example, native connections with the *same* education level increase the hiring probability for displaced immigrants by 0.07 percentage points, while conational connections with a *different* education level increase the hiring probability for displaced immigrants by 0.43 percentage points.

We define connections to firms based on past coworkers' employment in year t, while firm closures happen between t and $t + 1$. This leaves open the possibility that the past coworker has left the hiring firm before the displaced worker searches for work. If there are differences by nativity in the persistence of employment, these could lead to differences in the effect of different types of connections. We check that this is not the case in Figure A.4e by differentiating between connections who have left the hiring firm by $t+1$ and those who haven't. Past coworkers persistently employed at the hiring firm have a much larger effect on hiring, regardless of nativity. This result also lends support to our interpretation of the effect of connections as reflecting information transmission, rather than unobserved similarity between the displaced worker and their past coworker. If the latter were the case, we should observe a similar effect of a connection on the probability of a hire, regardless of whether the past coworker continues to be employed at the hiring firm.

4.3.3   Heterogeneity by other displaced worker characteristics

The results presented above suggest that the impact of connections to some degree varies with years of residence in Sweden. We now consider whether there is greater convergence in the

productivity of different types of connections over years spent in Sweden for specific subgroups of immigrants. We consider the role of gender, education, and region of birth. To this end, we interact the connectedness indicators for immigrants with characteristics of the displaced worker as well as an indicator for whether a displaced worker is a recent (fewer than ten years in Sweden) or established (more than ten years) immigrant.

Results are reported in Figure A.5. While some confidence bands are quite wide, there are some notable patterns. The effect of a connection tends to be greater for males than for females (both for immigrants and natives), and smaller for Western European migrants than for others, indifferent of time in Sweden. For natives, the impact of connections tends to decline with education, but less clearly so for displaced immigrant workers. The finding that own-group connections matter more holds for almost all subgroups, and there is some tendency for established migrants experiencing a greater effect of any given connection. However, there is no evidence of greater convergence for any specific subgroup of immigrants.

Displaced immigrants' prior workplaces are on average smaller (the median number of coworkers is 11) than natives' prior workplaces (the median number of coworkers is 13). To check that differences in the effect of connections between immigrants and natives do not reflect differences in the size of the prior workplace, we interact the connectedness indicators with indicators for being in a small (fewer than 20 coworkers) or large prior workplace. The results are shown in Figure A.5e. While the effect of a given connection type is always larger when the prior workplace was small, the relative value of connection types is constant across size classes for the prior workplace. As a result, the effect of a conational connection from a large workplace is still 2.5 times the effect of a native connection from a small workplace.

## 4.4   Hires in local industries, wages and turnover

So far we have investigated and established the causal effect of connections on the probability a displaced worker is hired by a given firm. However, hiring by connected firms may not fully capture all the ways displaced workers use their networks to find a job. To present a more complete picture of how immigrants use their professional connections to find work, and to understand whether there are changes over time in the use of immigrants' networks, we now turn to analysing how connections affect other outcomes of displaced workers' job search.

4.4.1   Hires in a connected local industry

Past coworkers may provide information about vacancies in third firms, where those past coworkers are not currently working. This information channel is not captured by hires at connected firms, and may vary across displaced natives and immigrants. To test whether this is the case, we extend the dyadic approach described in Section 4.1 and consider how connections via past coworkers to a local industry increase the probability of a displaced worker being hired by a firm

in that industry.[20] Formally, we continue to estimate a version of Equation (1), however now k refers to a local industry and α_{jk} is a closing firm by potential hiring industry fixed effect. The set of controls, in this analysis, includes dummies for the region of origin of the displaced worker, as well as indicators for the displaced worker having previously worked in either the two-digit industry, municipality, or interaction of industry and municipality that define the local industry k.[21]

Figure 5: Effect of a connection on hires by firms and local industries

(a) Constant effects $\qquad$ (b) Heterogeneity by years in Sweden
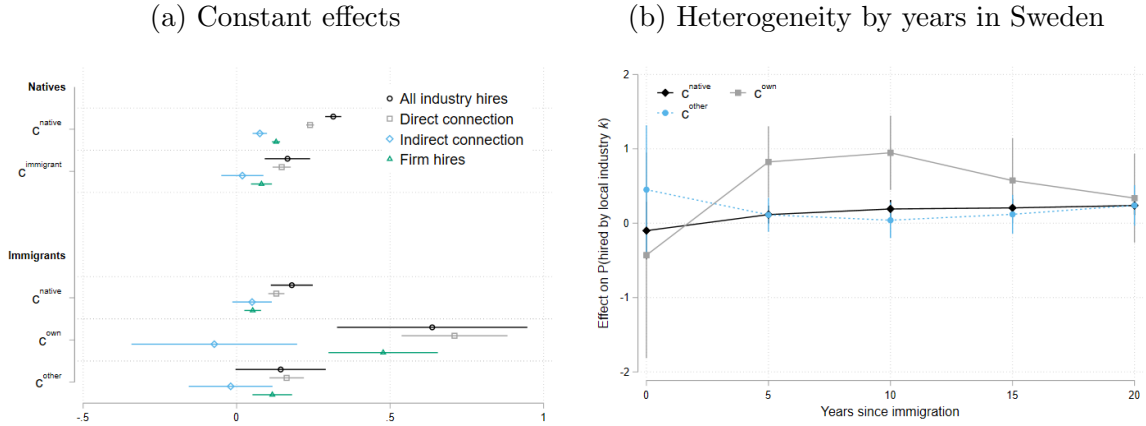
Notes: Coefficients estimated using Equation (1). Heterogeneous effects are estimated by interacting the connectedness indicators for displaced immigrants with a cubic polynomial in years since migration at the time of displacement, including main effects. Standard errors are clustered by jk pair, where k refers to either the hiring firm or the hiring local industry; 95 per cent confidence intervals are reported. Details for the regression reported in (a) are available in Table A.1.

Figure 5a reports the estimates for industry hires.[22] In addition to the main specification ("All industry hires"), we also separate between direct and indirect connections to firms within a local industry, where the latter means hirings in a connected local industry but at firms where there is no former colleague working. For comparison, we also report the effect of a connection on firm-level hiring, using the same individuals as we used to estimate the effect of connections on industry hiring. A first main message is that in particular native connections have a significantly larger effect on the probability of being hired by a local industry than by a given firm. For displaced immigrants (in this sample), a native connection raises the probability of a displaced immigrant getting hired at the connected firm by 0.05 percentage points, while the probability of getting hired in the connected local industry increases by 0.18 percentage points. For displaced natives in this sample, the corresponding effects are 0.13 and 0.31 percentage points. A second message of Figure 5a is that indirect native connections also seem to affect

---

[20]Local industries are defined as the aggregation of all firms working in the same two-digit industry by municipality.

[21]We now drop individuals who are connected to local industry k by more than one type of connection, leading to a smaller sample than when considering firm hiring, and cluster standard errors by closing firm j by local industry k.

[22]For regression estimates see Table A.1.

hirings outside the specific firm, whereas immigrant connections only appear to lead to hires in the connected workplaces themselves.[23]

There are also changes over time in Sweden in the effect of native connections on the probability of being hired by a given local industry, and these are also larger than the changes in the effect of native connections on firm hiring. Figure 5b shows that the effect of a native connection on the probability of being hired in the connected industry increases from 0.12 percentage points for immigrants displaced from a job five years after arriving in Sweden to 0.24 percentage points for immigrants displaced after 20 years, approaching the effect of a native connection for a displaced native. We again confirm that this time profile is not explained by age of arrival or cohort effects in Figure A.6.

We argue that the effect of industry connections on hiring in the industry in excess of the hiring at the connected firms reflects information transmission about vacancies at other firms in the local industry, or assistance in applying for these vacancies. Another possibility might be that there is simply persistence in the local industry that both the displaced individual and their past coworkers work in, and our industry analysis captures this. However, the placebo analysis conducted in Section 4.2.2 invalidates this interpretation. Under this alternative explanation, our placebo connections, which are measures of "typical" job mobility for the displaced workers, given their prior industry experience and location, should predict the hiring of the displaced workers. We did not find any association between the presence of placebo connections and actual hires.[24]

4.4.2   Wage and turnover effects

To understand whether homophily affects the type of information a connection typically shares, or whether connected hires in local industries reflect different types of information flows, relative to connected hires in firms, we now study how the wages and turnover of connected hires

---

[23]While we limit attention to hiring local industries where a worker has at most one connection type, workers may have multiple connections (of the same type) to different firms in the same local industry. To confirm this does not explain the larger industry effects, in column 4 of Table A.1 we restrict the sample to industries where the displaced connection has at most one past coworker working. Although coefficients are slightly smaller in this case, the main picture is unchanged. In particular, the effect of native connections for displaced immigrants is much larger than in the firm specification.

[24]The difference between our placebo analysis and that of Eliason et al. (2023) is particularly insightful here. They define a placebo workplace as another randomly chosen workplace in the same industry and municipality as the potential hiring firm, k and estimate the effect of a true connection on hiring at the placebo firm. In contrast, we define placebo coworkers as individuals who in the past worked at another randomly chosen firm in the same industry and location, with a manager of the same nativity, in the same year as the true past coworkers and estimate the effect of a placebo connection on hiring at the placebo coworkers' true firm in year t. The placebo analysis of Eliason et al. (2023) will conflate the effect of typical job mobility for workers employed in a given local industry in the past with the provision of information about vacancies or other help applying for jobs in a past coworker's current local industry. They find a positive effect of connections on hiring at the placebo firm, albeit substantially smaller than the effect on hiring at the true firm, as should be expected if workers do indeed share information about vacancies at third firms similar to their own firm. Our placebo analysis, on the other hand, will only identify whether a connection to a firm proxies for typical job mobility patterns for workers previously employed in a given past industry; we fail to find an effect of such placebo connections on hiring.

differ from those of unconnected hires. Dustmann et al. (2016a) and Glitz and Vejlin (2021) analyse a model of referral hires, where referrals lower the uncertainty to the employer about the productivity of a match with the referred worker. As a result, starting wages are higher for referred workers than for non-referred workers, and turnover is lower, since the firm makes fewer wrong decisions in hiring workers. However, over time, initial uncertainty about the unobserved productivity of workers is resolved. Bad matches are endogenously terminated and productive workers hired without a referral are able to renegotiate their wages up. As a result, initial differences between referred and non-referred workers disappear over time.

To test which type of connection is providing more referrals to displaced immigrants, as opposed to information about vacancies, we consider the sample of workers i who were displaced by the closure of j and who are employed at firm k in November of year $t+1$. We construct a panel of outcomes for these workers over the years $\tau \in [t+1; t+5]$ and estimate versions of the following equation, following Glitz and Vejlin (2021):

$$y_{ijk\tau} = \alpha_j + \sum_{s=1}^{5} \gamma_s c_{ik} \mathbf{1}(\tau = t+s) + \sum_{s=1}^{5} \beta_s \mathbf{1}(\tau = t+s) + X'_{ijk\tau}\beta + \epsilon_{ijk\tau}. \tag{2}$$

The outcomes we consider are log annual earnings in year τ and an indicator for leaving one's current employer between November of year τ and November of $\tau + 1$.[25] Importantly, we do not condition on individuals continuing to be employed at the firm k that hired them in $t+1$ at any subsequent point in the five years covered by the panel. c_{ik} is an indicator for hired worker i having a connection to hiring firm k of some sort; we will study the effect of connections through past coworkers to both the hiring firm and other firms in the same local industry and we will allow the effect of connections to vary by the degree of homophily between the hired worker and their past coworker. We include a closing firm j fixed effect and large set of pre-displacement demographic and employment history covariates $X_{ijk\tau}$. In particular, the set of covariates includes a quadratic in labour earnings in year t, i.e. pre-displacement, as well as indicators for having recently worked in either firm k or the two-digit industry of firm k and for having the same nativity as the manager at firm k. The effect of these covariates is allowed to vary with time since initial hire, τ. A causal interpretation of this analysis is subject to caution, since the sample selection necessarily conditions on displaced workers having found a job by November of $t+1$. However, such an analysis is still insightful and provides valuable information on whether connections referred the displaced worker to the hiring firm or not.

Figure 6 reports effects disaggregated by the homophily in nativity between the hired worker and their connection at the hiring firm.[26] For natives, the earnings estimates are positive and significant over the five-year window. The results also suggest lower turnover. Although somewhat noisy (due to the small number of immigrants hired with native referrals), estimates suggest substantial short-term earnings gains for immigrants hired through native connections. Neither

---

[25]Hourly wages or monthly hours worked are only observed for a subset of workers, so we study earnings rather than wages.

[26]The average effect of a firm-level connection on earnings and turnover is reported in Table A.2.

Figure 6: Labour market effects of firm connections
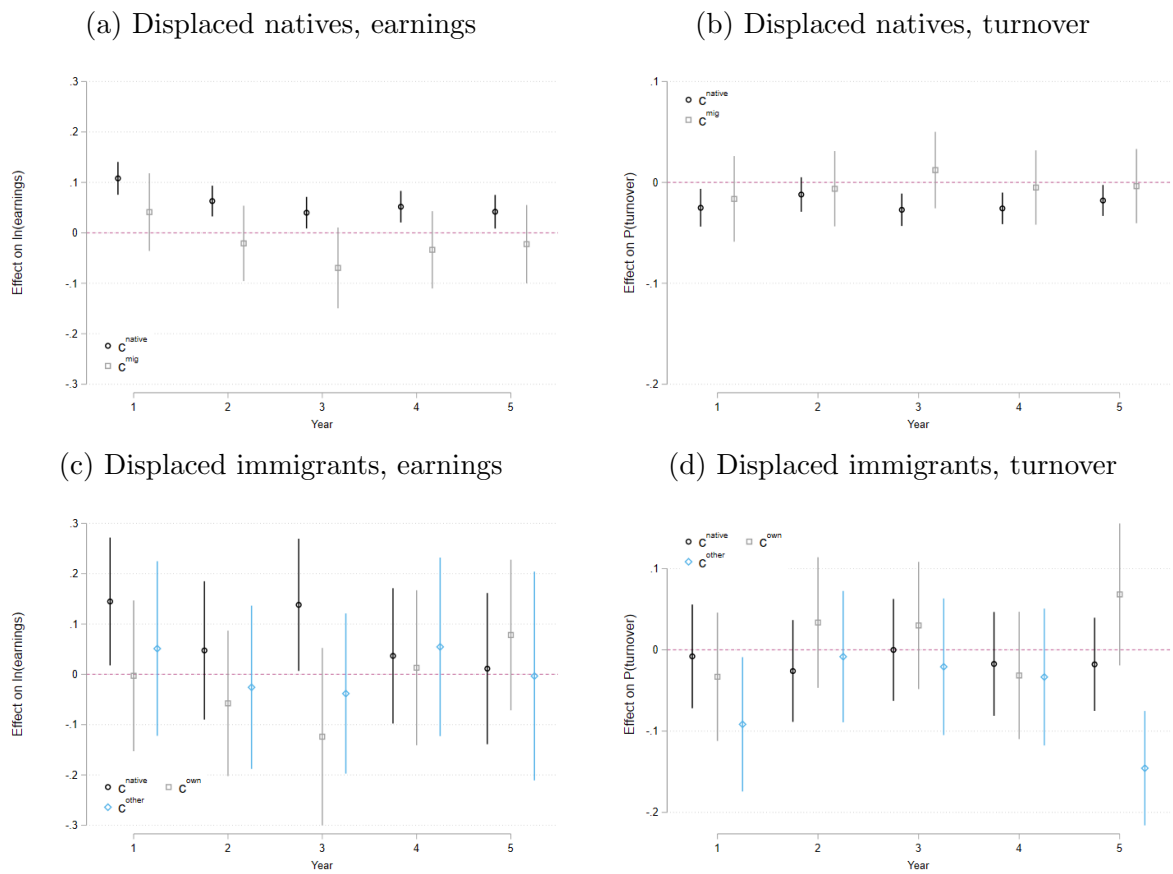
(a) Displaced natives, earnings

(b) Displaced natives, turnover

(c) Displaced immigrants, earnings

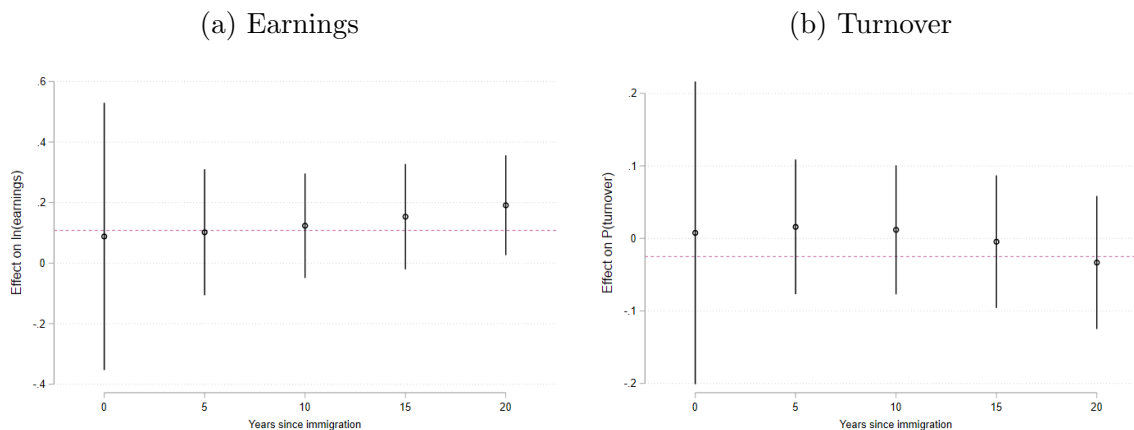(d) Displaced immigrants, turnover

Notes: Coefficients estimated using Equation (2), allowing the effect of a connection at the hiring firm on subsequent outcomes to vary by the nativity of the displaced and connecting workers. Standard errors are clustered by closing firm j and 95 per cent confidence intervals are reported.

23

group appears to benefit strongly from immigrant connections when hired. The time pattern of effects—short-term increase in earnings and possible decrease in turnover—is consistent with those connections providing referrals (Glitz and Vejlin, 2021).

In Figure 7, we consider how the effect of a native connection varies with years since immigration, measured at the time of the displacement. The analysis interacts the indicator for a native connection with a quadratic in years since immigration. The estimates are imprecise, again due to few immigrants being hired in firms through native connections. However, the increase in the effect of native connections on earnings and turnover is economically significant. The point estimate of the effect of a native connection on immigrant earnings or turnover becomes larger than the average effect of a connection for a hired native after 15–20 years in Sweden.

Figure 7: Effect of native connections for immigrants by years since immigration

(a) Earnings

(b) Turnover

Notes: Coefficients estimated using Equation (2), allowing the effect of a connected hire on subsequent outcomes to vary by the nativity of the displaced and connecting workers as well as years since immigration. Estimates only reported for first post-displacement year, $t + 1$. Effects of a native connection for a hired native are reported as horizontal lines. Standard errors are clustered by closing firm j and 95 per cent confidence intervals are reported.

Finally, we include an indicator for having a connection in the same local industry, but at a different firm in the specification. We report effects disaggregated by the nativity of the hired worker in Figure A.7.[27] In contrast to firm connections, connections elsewhere in the industry have a much smaller effect on earnings and turnover for hired natives. For hired immigrants, the effect is both statistically and economically insignificant, suggesting that industry connections are only providing information about vacancies, rather than referrals to displaced immigrants searching for work.

Taken together, the results in this section make for an interesting contrast with the effect of connections on the hiring probability. While conational connections were found to be more valuable in the sense of being more likely to lead to a hire, we have shown here that native connections are valuable in the sense that they have larger positive effects on the labour market

---

[27]We do not distinguish connections by nativity, due to lack of power. The average effect of such a connection is reported in Table A.3.

outcomes of displaced immigrants who are able to get hired at firms where they have a native connection. This is particularly true for immigrants who have been in Sweden for 15 or more years at the time of displacement.

5 Networks and assimilation profiles

We showed in Figure 2 that both the size of immigrants' networks and the re-employment probability increase with years since immigration. In Section 4.2 we considered the causal effect of a connection on the probability that a displaced worker is hired by a given firm or industry. Now we combine these pieces of information to understand what proportion of the re-employment gap between immigrants and natives (shown in Figure 2b) is explained by differences in the size or use of professional networks.

Let c_k be an indicator for a displaced worker having a connection to firm k. The probability that a displaced worker is employed in November of $t+1$ can be decomposed, using the law of total probability, as follows:

$$
\begin{aligned}
P(\text{employed}) &= \sum_{k \in K} P(\text{hired by } k) \\
&= \sum_{k \in K} P(c_k = 1) P(\text{hired by } k | c_k = 1) + P(c_k = 0) P(\text{hired by } k | c_k = 0) \\
&= \sum_{k \in K} P(c_k = 1) \gamma + P(\text{hired by } k | c_k = 0) \\
&= \gamma \times \sum_{k \in K} c_k + \sum_{k \in K} P(\text{hired by } k | c_k = 0),
\end{aligned}
\tag{3}
$$

where the third equation follows from the assumption, embedded in Equation (1), that connections raise the probability of being hired by a firm by some constant $\gamma = P(\text{hired by } k | c_k = 1) - P(\text{hired by } k | c_k = 0)$. $\sum_{k \in K} c_k$ is simply the displaced worker's total number of connections.[28] Differences between types of displaced workers, for example immigrants and natives or recent and established immigrants, in the probability of being employed in November of $t+1$ can therefore be decomposed into: (i) a term that is a function of the size and composition of their networks as well as the productivity of connections; and (ii) a term that is not a function of their networks.

We quantify how much of the increase in re-employment rates over years in Sweden, documented in Figure 2b, can be explained by the accumulation of professional networks, documented in Figure 2a, or by changes with years in Sweden in the productivity of connections, documented

---

[28]For ease of exposition, we have abstracted here from different types of connections. When there are two or more types of connections, e.g. immigrant and native connections, one will have to differentiate between the different possible combinations of connections to firm k in the second equation of the above derivation. If the effect of multiple different connection types to a firm on the hiring probability is additively separable into the effect of each connection type, then the first term in Equation (3) is simply replaced by the sum of the effect of each connection type multiplied by the number of connections of that type.

in Figure 5b. To do so, we calculate the contribution of networks to the re-hiring probability for a given type of displaced worker by multiplying the effect of a connection of a given type by the average number of connections that type of displaced worker has and then summing over connection types. The results are shown in Figure 8.

Figure 8: Contribution of networks to assimilation in re-employment after displacement

(a) Firm connections (b) Industry connections
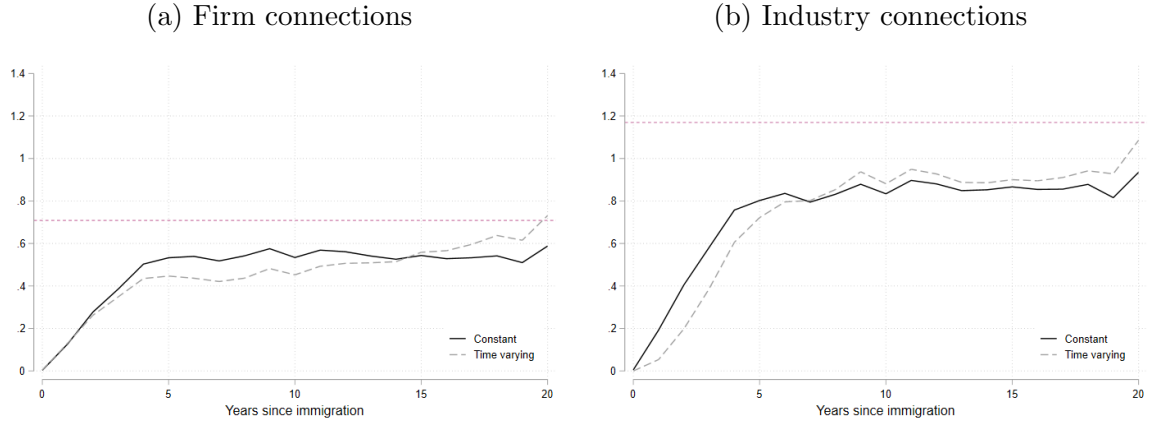
Notes: Contribution of networks to re-employment in November of $t+1$, defined according to Equation (3). Both constant and varying-by-years-in-Sweden estimates of the effect of a connection are considered. The contribution of natives' networks to their re-employment probability is reported as a horizontal line.

Figure 8a reports results based on the estimated effects of firm connections. When we assume that the effect is constant over time, contributions to the re-employment probability rise rapidly for a few years but then remain constant at a level below the average impacts for natives. When we instead allow for time-varying effects, there is instead a long-term increase and a complete assimilation to the native average. According to the estimates, firm connections increase the re-employment probability by around 0.7 percentage points for immigrants displaced after 20 years in Sweden. The difference between the solid and dashed lines reflects the increasing productivity of native connections.

Corresponding estimates of the effect of industry connections are presented in Figure 8b. The time patterns are qualitatively very similar to 8a, but at higher levels. Immigrants' industry connections increase their re-employment probability by 1.1 percentage points on average after 20 years, when the effect of connections is allowed to vary with years in Sweden. The increase in the re-employment that can be directly attributed to networks corresponds to 10–20 per cent of the total increase in the re-employment probability, depending on whether one considers the effect of the network on firm hiring or on industry hiring.

6   Conclusion

Social networks play an important role in the functioning of labour markets. For immigrant workers, professional networks are almost by definition absent at arrival. This paper studies the

development and importance of connections to former coworkers for immigrant labour market integration. We focus on professional networks established through interactions at workplaces and use administrative data from Sweden to create time- and type-varying characterizations of individual networks. First, we document that networks grow with time in the host country. This aspect of the immigrant integration process has received very little attention in the literature. Furthermore, we find that the composition of networks changes from countrymen and other immigrants toward native workers.

We then implement a dyadic regression approach exploiting variation in connectedness among workers displaced by the same plant closure to estimate the causal impact of connections on where a displaced worker is hired, following Eliason et al. (2023). We allow the effect to vary both by displaced worker and connection origin, and also explore heterogeneity in other dimensions. Conational connections have a much larger effect on re-employment rates for displaced immigrants than do native connections. As natives gain more from native connections, results thus suggest that homophily in origin is important. The findings are consistent with previous evidence on the value of ethnic networks, and robust to several robustness checks including a placebo analysis showing that the differential effect of conational connections cannot be explained through nationality-specific patterns of persistence in the local industry or manager nativity of firms employing immigrants.

However, we also show that native connections become more valuable with time spent in Sweden. The effects on re-employment increase and so does the impact on earnings for those finding work quickly after displacement. Native connections also seem to impact hirings in connected local industries but outside the connection's own firm. Of course, an important issue for further research is to understand the mechanisms underlying the changing productivity of native connections. For now, we can only speculate whether it should be attributed to improved language skills, cultural knowledge, reciprocal exposure, or just knowledge of relevant jobs.

Finally, our findings provide a rare example of complete long-term assimilation, in the sense that the total productivity of professional networks through former co-workers for displaced immigrants attains a level comparable to that observed for displaced natives. The analysis suggests that immigrants displaced 20 years after arrival in Sweden are 0.7 percentage points more likely to be re-employed immediately after a displacement thanks to their connections to firms, or 1.1 percentage points when considering their connections to industries. The latter aggregate effect corresponds to around 20 per cent of the increase in the re-employment probability documented over years spent in Sweden by the displaced immigrant. Since our definition of networks admittedly only captures part of the professional connections relevant to workers' job search and transitions, the estimates strongly emphasize the importance of informal connections in the labour market.

A primary contribution of the study is thus an improved understanding of the factors and mechanisms influencing immigrant labour market integration as well as job search outcomes in general. In a broad sense there are also implications for policy. The prevalence of ethnic workplace segregation documented in previous work (Hellerstein and Neumark, 2008; Åslund

and Skans, 2010; Glitz, 2014; Andersson et al., 2014) may not be a concern in the short-term; in fact, the higher productivity of conational connections suggests that working with peers generates protective resources for recent migrants. In the longer term, however, reaping the increasing employment and earnings benefits of professional links to natives becomes more essential. Thus, policies promoting immigrant employment might arguably wish to consider also how the workforce composition of firms offering employment opportunities will affect immigrants' ability to search for subsequent jobs.

References

ALGAN, Y., C. DUSTMANN, A. GLITZ, AND A. MANNING (2010): "The Economic Situation of First and Second-Generation Immigrants in France, Germany and the United Kingdom," *Economic Journal*, 120, F4–F30.

ANDERSSON, F., M. GARCÍA-PÉREZ, J. HALTIWANGER, K. MCCUE, AND S. SANDERS (2014): "Workplace Concentration of Immigrants," *Demography*, 51, 2281–2306.

ÅSLUND, O., L. HENSVIK, AND O. N. SKANS (2014): "Seeking Similarity: How Immigrants and Natives Manage in the Labor Market," *Journal of Labor Economics*, 32, 405–441.

ÅSLUND, O. AND O. N. SKANS (2010): "Will I see you at work: Ethnic Workplace Segregation in Sweden, 1985-2002," *ILR Review*, 63, 471–493.

AUER, D. (2018): "Language roulette – the effect of random placement on refugees' labour market integration," *Journal of Ethnic and Migration Studies*, 44, 341–362.

BALGOVA, M. AND H. ILLING (2023): "Job Displacement and Migrant Labor Market Assimilation," IZA Discussion Paper 16349.

BARR, T., R. BOJILOV, AND L. MUNASINGHE (2019): "Referrals and Search Efficiency: Who Learns What and When?" *Journal of Labor Economics*, 37, 1267–1300.

BATTISTI, M., G. PERI, AND A. ROMITI (2022): "Dynamic effects of co-ethnic networks on immigrants' economic success," *Economic Journal*, 132, 58–88.

BEAMAN, L. A. (2012): "Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the U.S." *Review of Economic Studies*, 79, 128–161.

BORJAS, G. J. (1985): "Assimilation, changes in cohort quality, and the earnings of immigrants," *Journal of Labor Economics*, 3, 463–489.

——— (1995): "Assimilation and changes in cohort quality revisited: what happened to immigrant earnings in the 1980s?" *Journal of Labor Economics*, 13, 201–245.

BOUCHER, V. AND M. GOUSSÉ (2019): "Wage dynamics and peer referrals," *Review of Economic Dynamics*, 31, 1–23.

BRATSBERG, B., O. RAAUM, AND K. RØED (2018): "Job Loss and Immigrant Labour Market Performance," *Economica*, 85, 124–151.

BRÜCKER, H., A. GLITZ, A. LERCHE, AND A. ROMITI (2021): "Occupational recognition and immigrant labor market outcomes," *Journal of Labor Economics*, 39, 497–525.

BURKS, S. V., B. COWGILL, M. HOFFMAN, AND M. HOUSMAN (2015): "The value of hiring through employee referrals," *Quarterly Journal of Economics*, 130, 805–839.

CALVÓ-ARMENGOL, A. AND M. O. JACKSON (2004): "The effects of social networks on employment and inequality," *American Economic Review*, 94, 426–454.

CARD, D. (2005): "Is the New Immigration Really so Bad?" *Economic Journal*, 115, F300–F323.

CHISWICK, B. R. (1978): "The Effect of Americanization on the Earnings of Foreign-born Men," *Journal of Political Economy*, 86, 897–921.

CHISWICK, B. R. AND P. W. MILLER (2015): "International migration and the economics of language," in *Handbook of the economics of international migration*, ed. by B. R. Chiswick and P. W. Miller, Elsevier, vol. 1, 211–269.

CINGANO, F. AND A. ROSOLIA (2012): "People I Know: Job Search and Social Networks," *Journal of Labor Economics*, 30, 291–332.

DAMM, A. P. (2009): "Ethnic Enclaves and Immigrant Labor Market Outcomes: Quasi-Experimental Evidence," *Journal of Labor Economics*, 27, 281–314.

DUSTMANN, C., T. FRATTINI, AND I. P. PRESTON (2013): "The effect of immigration along the distribution of wages," *Review of Economic Studies*, 80, 145–173.

DUSTMANN, C., A. GLITZ, U. SCHÖNBERG, AND H. BRÜCKER (2016a): "Referral-based job search networks," *Review of Economic Studies*, 83, 514–546.

DUSTMANN, C. AND J.-S. GÖRLACH (2015): "Selective out-migration and the estimation of immigrants' earnings profiles," in *Handbook of the Economics of International Migration*, ed. by B. R. Chiswick and P. W. Miller, North-Holland, vol. 1, 489–533.

DUSTMANN, C., U. SCHÖNBERG, AND J. STUHLER (2016b): "The Impact of Immigration: Why Do Studies Reach Such Different Results?" *Journal of Economic Perspectives*, 30, 31–56.

ECKSTEIN, Z. AND Y. WEISS (2004): "On the Wage Growth of Immigrants: Israel 1990-2000," *Journal of the European Economic Association*, 2, 665–695.

EDIN, P.-A., P. FREDRIKSSON, AND O. ASLUND (2003): "Ethnic enclaves and the economic success of immigrants—evidence from a natural experiment," *Quarterly Journal of Economics*, 118, 329–357.

ELIASON, M., L. HENSVIK, F. KRAMARZ, AND O. N. SKANS (2023): "Social connections and the sorting of workers to firms," *Journal of Econometrics*, 233, 468–506.

FOGED, M., L. HASAGER, G. PERI, J. N. ARENDT, AND I. BOLVIG (2022): "Language Training and Refugees' Integration," *Review of Economics and Statistics*, 1–41.

GLITZ, A. (2014): "Ethnic segregation in Germany," *Labour Economics*, 29, 28–40.

——— (2017): "Coworker networks in the labour market," *Labour Economics*, 44, 218–230.

GLITZ, A. AND R. VEJLIN (2021): "Learning through Coworker Referrals," *Review of Economic Dynamics*, 42, 37–71.

GOEL, D. AND K. LANG (2019): "Social ties and the job search of recent immigrants," *ILR Review*, 72, 355–381.

GRANOVETTER, M. (1995): *Getting a job: a study of contacts and careers*, University of Chicago Press, 2 ed.

HARDOY, I. AND P. SCHØNE (2014): "Does the impact of plant closure on labour market attachment differ between immigrants and native workers across the business cycle?" *Empirical Economics*, 46, 229–252.

HELLERSTEIN, J. K. AND D. NEUMARK (2008): "Workplace Segregation in the United States: Race, ethnicity, and skill," *The Review of Economics and Statistics*, 90, 459–477.

HETHEY-MAIER, T. AND J. F. SCHMIEDER (2013): "Does the Use of Worker Flows Improve the Analysis of Establishment Turnover? Evidence from German Administrative Data," *Schmollers Jahrbuch : Journal of Applied Social Science Studies / Zeitschrift für Wirtschafts- und Sozialwissenschaften*, 133, 477–510.

ILLING, H. AND T. KOCH (2021): "Who Suffers the Greatest Loss? Costs of Job Displacement for Migrants and Natives," Working Paper 8/2021, IAB-Discussion Paper.

IOANNIDES, Y. M. AND L. D. LOURY (2004): "Job information networks, neighborhood effects, and inequality," *Journal of economic literature*, 42, 1056–1093.

LUBOTSKY, D. H. (2007): "Chutes or Ladders? A Longitudinal Analysis of Immigrant Earnings," *Journal of Political Economy*, 115, 820–867.

MONTGOMERY, J. D. (1991): "Social networks and labor-market outcomes: Toward an economic analysis," *American Economic Review*, 81, 1408–1418.

——— (1992): "Job search and network composition: Implications of the strength-of-weak-ties hypothesis," *American Sociological Review*, 57, 586–596.

MUNSHI, K. (2003): "Networks in the modern economy: Mexican migrants in the U. S. labor market," *Quarterly Journal of Economics*, 118, 549–599.

RHO, D. AND S. SANDERS (2021): "Immigrant earnings assimilation in the united states: A panel analysis," *Journal of Labor Economics*, 39, 37–78.

TOPA, G. (2019): "Social and spatial networks in labour markets," *Oxford Review of Economic Policy*, 35, 722–745.

A Supplementary figures and tables

Figure A.1: Share of individuals where the network is censored
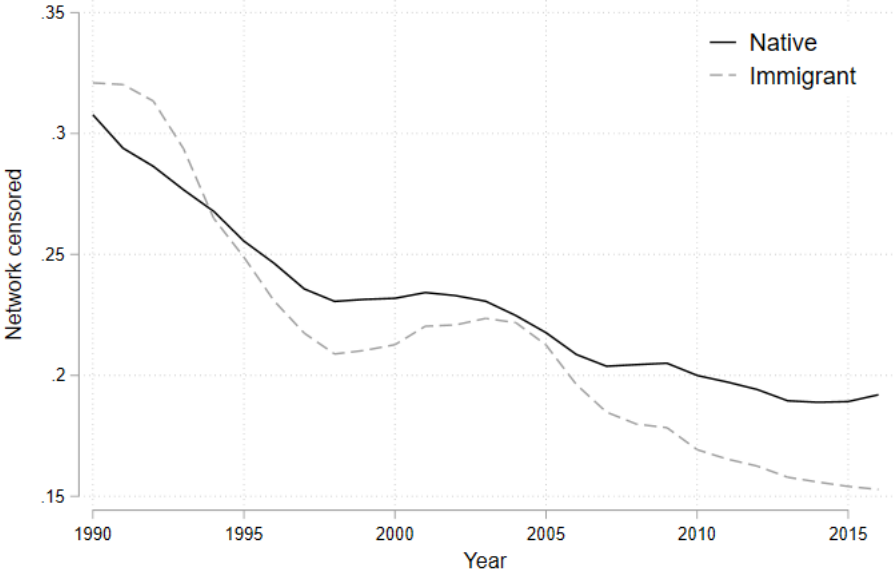
Notes: Share of individuals for whom the network is censored, i.e. a past workplace employed over 100 individuals or had a missing workplace identifier, over the period 1990–2016. The decline is largely due to a decline in the share of missing workplace identifiers, rather than changes in workplace size.

Figure A.2: Sources of growth in immigrants' professional networks

(a) Native comparison   (b) Established immigrant comparison
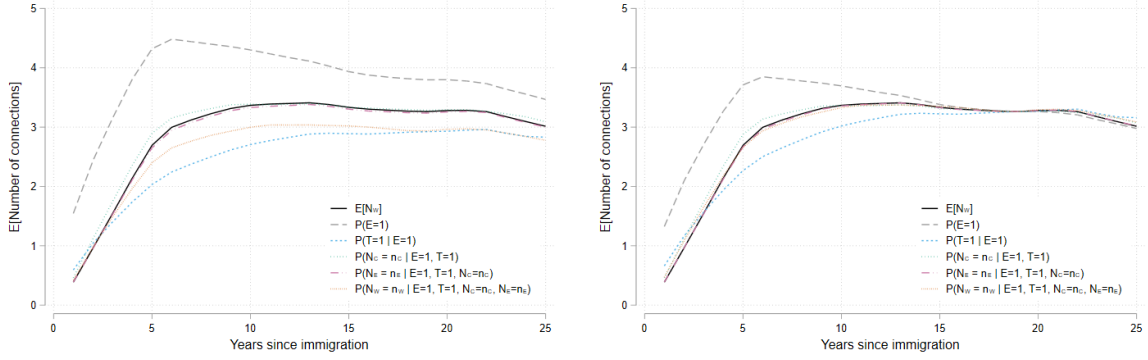
Notes: The true average number of connected workplaces (N_W) for a given number of years since immigration, $E[N_W]$, and hypothetical average numbers of connected workplaces simulated using a counterfactual distribution of a given determinant of the number of connections. The distribution of the true number of connections can be factorised as:

$$P(N_W = n_W) = P(E = 1) \times P(T = 1|E = 1)$$
$$\times \sum_{n_c \leq 100} P(N_c = n_c|E = 1, T = 1)$$
$$\times \sum_{n_E \leq n_c} P(N_E = n_E|E = 1, T = 1, N_c = n_c)$$
$$\times \sum_{n_W \leq n_E} P(N_W = n_W|E = 1, T = 1, N_c = n_c, N_E = n_E),$$

where E is an indicator for having been employed in the previous five years, T is an indicator for having left the prior workplace, N_c is the number of workers in that workplace, and N_E is the number of past coworkers currently employed. Counterfactual values for each distribution in the above equation are taken from the true distributions for either natives (Figure A.2a) or immigrants with 25 years in Sweden (Figure A.2b).

Figure A.3: Effect of firm connections by years since immigration
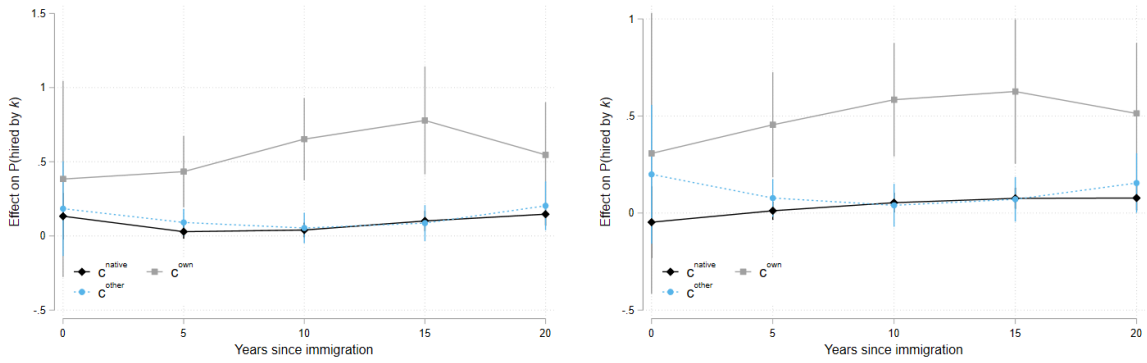
(a) Age at migration ≥ 16 (b) Within-cohort estimates

Notes: Heterogeneous effects of connections on the probability a displaced immigrant is hired by firm k, estimated by interacting the connectedness indicators for displaced immigrants with a cubic polynomial in years since migration at the time of displacement, including main effects. Marginal effects of a connection for selected values of years in Sweden are shown. The sample is restricted to immigrants aged at least 16 at the time of migration in Figure A.3a. The firm-pair fixed effect is interacted with a set of cohort dummies in Figure A.3b. Standard errors are clustered by firm pair jk, 95 per cent confidence intervals are reported.

Figure A.4: Heterogeneity by other connecting worker characteristics

(a) No heterogeneity
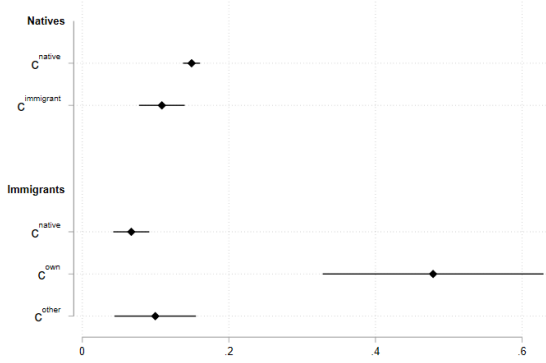
(b) Gender homophily
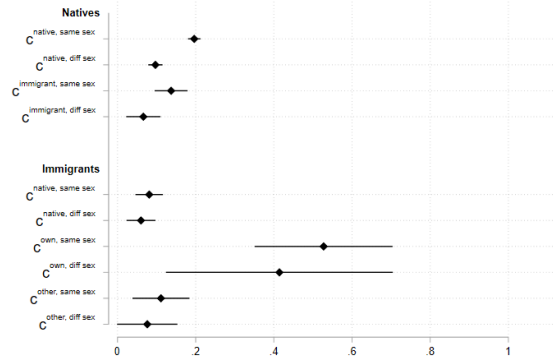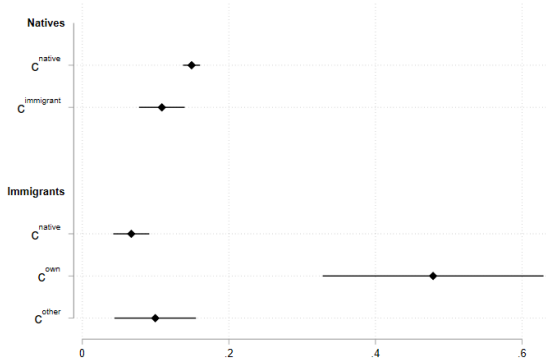

(c) Education homophily
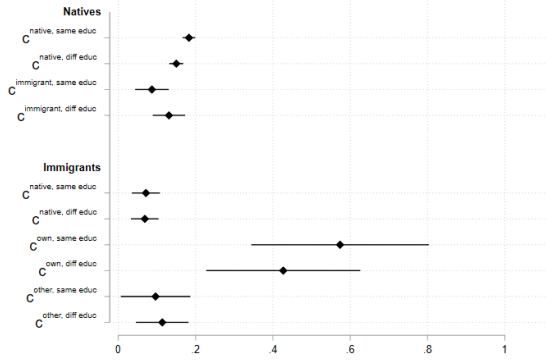
(d) Age homophily
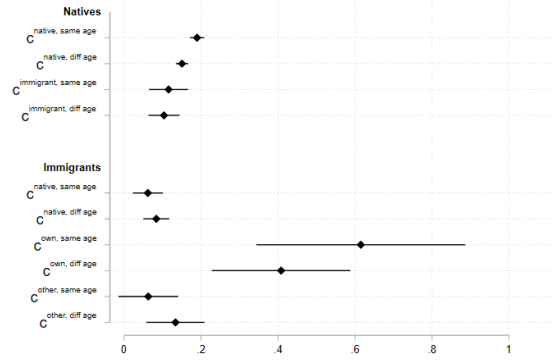
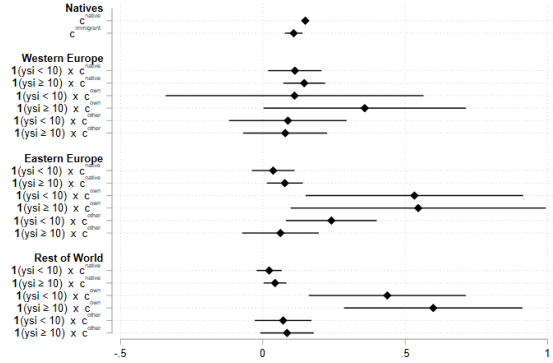(e) Connection to k in t and $t+1$
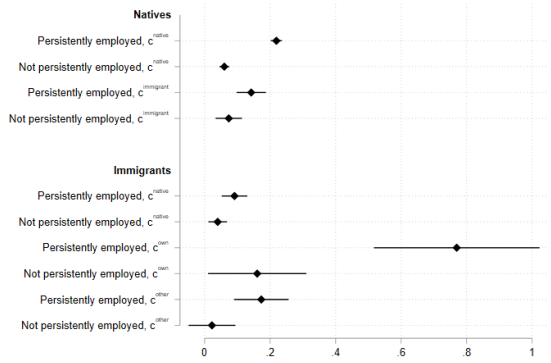
Notes: Coefficients estimated using Equation (1), allowing the effect of a connection on the hiring probability to vary by the nativity of the displaced and connecting workers as well as by homophily between both workers along other dimensions or whether the connecting worker was persistently employed at the hiring firm in both t and $t+1$. Standard errors are clustered by jk firm pair and 95 per cent confidence intervals are reported.

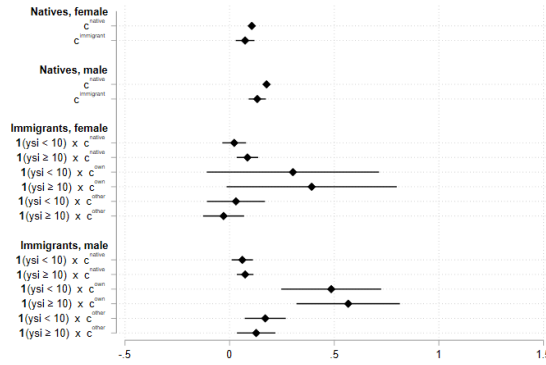Figure A.5: Heterogeneity by other displaced worker characteristics
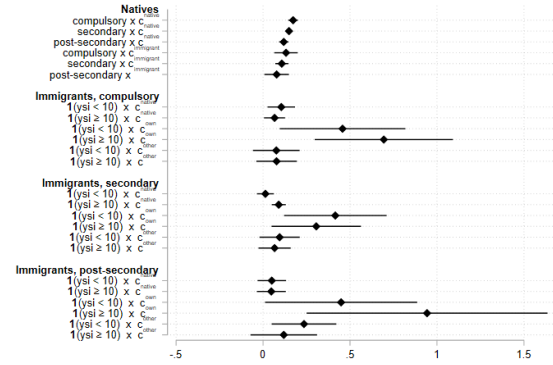
(a) No heterogeneity
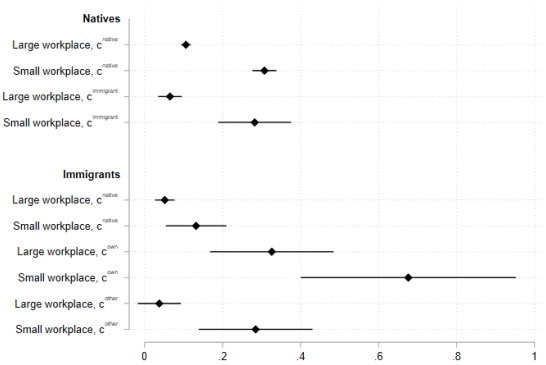

(b) Country of birth

(c) Gender

(d) Education


(e) Prior workplace size

Notes: Coefficients estimated using Equation (1), allowing the effect of a connection on the hiring probability to vary by the nativity of the displaced and connecting workers as well as by other demographic characteristics of the displaced worker and years since immigration. Western Europe includes the US and Canada, Eastern Europe includes Turkey and former members of the USSR. Standard errors are clustered by jk firm pair and 95 per cent confidence intervals are reported.

Figure A.6: Effect of industry connections by years since immigration
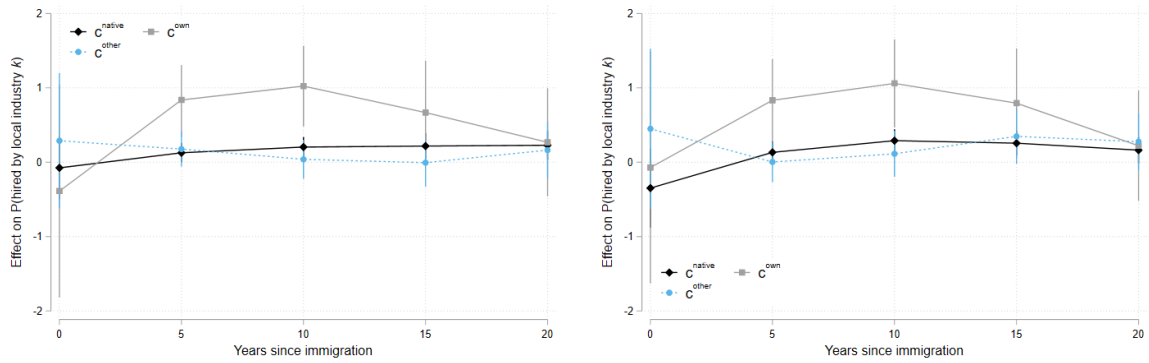
(a) Age at migration ≥ 16 (b) Within-cohort estimates


Notes: Heterogeneous effects of connections on the probability a displaced immigrant is hired by industry k, estimated by interacting the connectedness indicators for displaced immigrants with a cubic polynomial in years since migration at the time of displacement, including main effects. Marginal effects of a connection for selected values of years in Sweden are shown. The sample is restricted to immigrants aged at least 16 at the time of migration in Figure A.6a. The firm-hiring industry fixed effect is interacted with a set of cohort dummies in Figure A.6b. Standard errors are clustered by firm hiring industry pair jk, 95 per cent confidence intervals are reported.

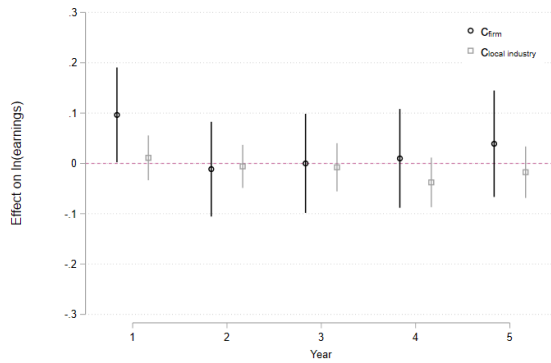Figure A.7: Labour market effects of local-industry connections

(a) Displaced natives, earnings
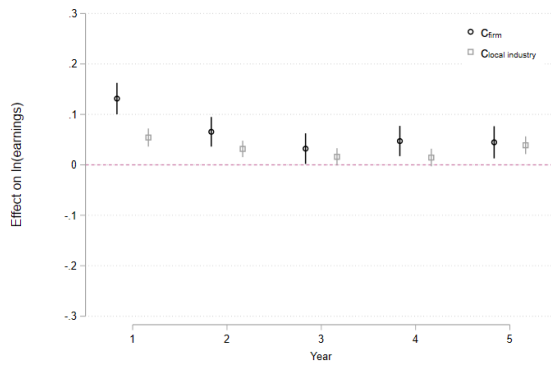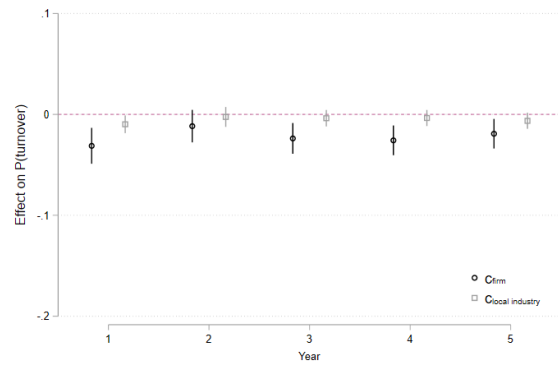
(b) Displaced natives, turnover

(c) Displaced immigrants, earnings

(d) Displaced immigrants, turnover
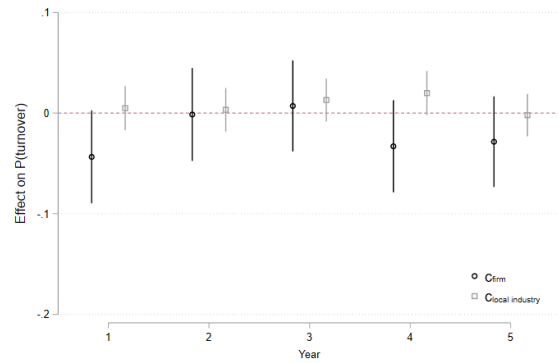
Notes: Coefficients estimated using Equation (2), allowing the effect of a connection at the hiring firm on subsequent outcomes to vary by the nativity of the displaced worker and whether the connection is at the hiring firm or at another firm in the same local industry. Standard errors are clustered by closing firm j and 95 per cent confidence intervals are reported.

Table A.1: Effects of connections on industry hiring

	(1) Hired in k	(2) Hired in k^C	(3) Hired in k^{UC}	(4) Hired in k	(5) Hired by k
$c^{native} \times$ Native	0.31***	0.24***	0.076***	0.21***	0.13***
	(0.013)	(0.0064)	(0.012)	(0.012)	(0.0067)
$c^{immigrant} \times$ Native	0.17***	0.15***	0.019	0.18***	0.081***
	(0.038)	(0.015)	(0.035)	(0.036)	(0.017)
$c^{native} \times$ Immigrant	0.18***	0.13***	0.050	0.13***	0.052***
	(0.035)	(0.013)	(0.033)	(0.032)	(0.014)
$c^{own} \times$ Immigrant	0.64***	0.71***	-0.073	0.49**	0.48***
	(0.16)	(0.088)	(0.14)	(0.15)	(0.091)
$c^{other} \times$ Immigrant	0.14	0.16***	-0.019	0.16*	0.12***
	(0.075)	(0.029)	(0.070)	(0.072)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	$jk \times IM$	$jk \times IM$	$jk \times IM$	$jk \times IM$	$jk \times IM$
$\hat{E}[Y(0)|c^{any}=0]$	0.32	0	0.32	0.31	0.041
N	11249994	11249994	11249994	11113371	15022513

Note: The dependent variable in columns 1-4 is an indicator for being hired in industry k, in column 5 it is an indicator for being hired by firm k. The sample of in column 4 is restricted to industries k that a displaced worker is connected to by at most one past coworker. The dependent variable is scaled by 100. Controls are share native, conational, or same education as i, indicators for i having worked in same industry or municipality as k or (in the firm regression) sharing birth country with k's manager, all interacted with an indicator for i's nativity, and categories for region of birth. FE denoted by $jk = I = i$ previously worked in k's industry, $M = i$ previously worked in k's municipality. Standard errors clustered by jk pair are reported in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Firm connections at hiring and post-displacement outcomes

	(1) ln (earnings)	(2) turnover	(3) ln (earnings)	(4) turnover
$t + 1 \times c_{firm}$	0.188*** (13.33)	-0.0332*** (-4.86)	0.113*** (7.45)	-0.0299*** (-3.51)
$t + 2 \times c_{firm}$	0.0906*** (6.16)	-0.00505 (-0.73)	0.0495*** (3.48)	-0.00966 (-1.25)
$t + 3 \times c_{firm}$	0.0580*** (3.83)	-0.0144* (-2.05)	0.0253 (1.71)	-0.0202** (-2.74)
$t + 4 \times c_{firm}$	0.0639*** (4.15)	-0.0226** (-3.14)	0.0413** (2.86)	-0.0258*** (-3.65)
$t + 5 \times c_{firm}$	0.0533*** (3.39)	-0.0154* (-2.10)	0.0349* (2.27)	-0.0187** (-2.65)
controls	No	No	Yes	Yes
Observations	772951	794188	763849	784188

Note: Included controls are indicators for having previously worked in the hiring firm or the hiring firm's industry, an indicator for having a conational manager at the hiring firm, the share of coworkers at the hiring firm with the same education as the displaced worker, the share of natives, the share of conationals and a quadratic in pre-displacement earnings, all interacted with dummies for the observation year post-hire, as well as gender-specific dummies for level of education and birth region as well as a quadratic in age, and nativity-specific terms for tenure at the time of displacement, the number of years worked of the five years prior to displacement, and the n umber of distinct workplaces in the five years prior to displacement. Standard errors clustered by closing firm, j, 95 per cent confidence intervals are reported.

Table A.3: Firm connections at hiring and post-displacement outcomes

	(1) ln (earnings)	(2) turnover	(3) ln (earnings)	(4) turnover
$t+1 \times c_{firm}$	0.208***	-0.0351***	0.128***	-0.0327***
	(14.68)	(-5.10)	(8.34)	(-3.83)
$t+2 \times c_{firm}$	0.0998***	-0.00417	0.0576***	-0.0106
	(6.74)	(-0.60)	(4.00)	(-1.35)
$t+3 \times c_{firm}$	0.0627***	-0.0130	0.0291*	-0.0207**
	(4.12)	(-1.83)	(1.96)	(-2.80)
$t+4 \times c_{firm}$	0.0675***	-0.0215**	0.0436**	-0.0263***
	(4.36)	(-2.98)	(2.98)	(-3.68)
$t+5 \times c_{firm}$	0.0606***	-0.0150*	0.0437**	-0.0198**
	(3.84)	(-2.03)	(2.81)	(-2.79)
$t+1 \times c_{localindustry}$	0.0992***	-0.00869**	0.0479***	-0.00827
	(14.70)	(-2.65)	(5.60)	(-1.96)
$t+2 \times c_{localindustry}$	0.0412***	0.00658*	0.0269***	-0.00179
	(5.83)	(1.98)	(3.43)	(-0.38)
$t+3 \times c_{localindustry}$	0.0196**	0.00802*	0.0134	-0.00180
	(2.70)	(2.36)	(1.63)	(-0.46)
$t+4 \times c_{localindustry}$	0.0146*	0.00620	0.00855	-0.000785
	(1.97)	(1.79)	(1.01)	(-0.21)
$t+5 \times c_{localindustry}$	0.0318***	-0.000492	0.0319***	-0.00569
	(4.18)	(-0.14)	(3.80)	(-1.49)
controls	No	No	Yes	Yes
Observations	772842	794058	763740	784058

Note: Included controls are indicators for having previously worked in the hiring firm or the hiring firm's industry, an indicator for having a conational manager at the hiring firm, the share of coworkers at the hiring firm with the same education as the displaced worker, the share of natives, the share of conationals and a quadratic in pre-displacement earnings, all interacted with dummies for the observation year post-hire, as well as gender-specific dummies for level of education and birth region as well as a quadratic in age, and nativity-specific terms for tenure at the time of displacement, the number of years worked of the five years prior to displacement, and the number of distinct workplaces in the five years prior to displacement. Standard errors clustered by closing firm, j, 95 per cent confidence intervals are reported.