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Abstract

This paper investigates how the size of co-ethnic networks at the time of arrival affect the economic success of immigrants in Germany. Applying panel analysis with a large set of fixed effects and controls, we isolate the association between initial network size and long-run immigrant outcomes. We also look at those who were assigned to an initial location independently of their choice allows a causal interpretation of our estimates. We find that immigrants initially located in places with larger co-ethnic networks are more likely to be employed at first, but have a lower probability of investing in human capital.

JEL-Codes: J240, J610, R230.

Keywords: networks, immigration, human capital, employment.

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1 Introduction

Labor market assimilation and economic integration of new immigrants are crucial to their overall success in the host country. What is the role of previously-arrived immigrants from the same country of origin, which we call co-ethnic network, in determining such economic success? Do new immigrants benefit from it when looking for jobs and when building their careers? Or are they diverted by such networks towards informal labor force connection and lower-quality jobs, which may discourage them from the acquisition of general human capital? How do the effects differ between the short-run and the long-run? This paper attempts to answer these questions using survey data on recent immigrants to Germany, matched with the universe of administrative records from the German social security archive. The merged dataset includes, quite uniquely among these type of data, pre-migration information on individual immigrants. In addition, the longitudinal structure of the data allows us to follow individuals after arrival in Germany, collecting yearly information on their labor market and education trajectories after arrival. Our analysis sheds some light on whether encouraging immigrants' spatial concentration, and hence local co-ethnic networks, is conducive to economic success for new immigrants in the short- and in the long-run.

The causal effect of the size of the co-ethnic network on immigrants' labor market success is not easy to assess. The main challenge is that the size of the local network is likely to affect immigrant selection. It is therefore likely to be correlated with observable and unobservable characteristics of new immigrants. Following post-migration outcomes for immigrants in areas with large and small co-ethnic communities would imply comparing individuals that are systematically different. The literature has already established that, in general, new immigrants tend to cluster where co-ethnic networks are settled. This is true both in the US (Cutler and Glaeser, 1997, and Borjas, 1998), and in Germany (Glitz, 2014) and this tendency may vary across ethnicity and immigrants characteristics. For instance, using social security data for Germany in 2008, Glitz (2014) computes measures of segregation and finds Western Europeans and Turks to be the groups with the highest segregation indexes. He also finds that less educated immigrants were more likely to locate near networks, relative to more educated ones. Hence these and other unobserved characteristics, likely correlated with the economic success of immigrants, would bias OLS estimates of the causal effect of local co-ethnic networks on immigrants' success.

In order to think more systematically about the role of co-ethnic networks for the short- and long-run employment and human capital investment of new immigrants, we present a simple theoretical framework. A partial-equilibrium search model illustrates the trade-off between employment and human capital investment after arrival in the destination country. We consider workers who can receive job offers through a formal search channel and through an informal/network-based channel. How successful the latter is depends on the size of the local co-ethnic network, while the effectiveness of the formal search method depends on an individual's human capital. Job opportunities have a wage distribution that can be different between formal and informal offers. The key prediction of our model is that, while larger networks have a positive effect on the probability of finding employment in the short run, they also discourage investment in human capital. Over time this implies that immigrants in locations with smaller networks catch up and converge to similar or higher employment probabilities compared to those with larger networks. The closing of the employment gap is due to the higher human capital investment of new immigrants in markets with small initial co-ethnic networks. Therefore, our model suggests that it is important to distinguish between short-run and long-run impacts of co-ethnic networks on employment and human capital accumulation. This distinction has not received much attention in the literature, due to the lack of data on training activities and school attendance by adult immigrants in the host country. In general there is a dearth of longitudinal datasets following individual migrants before and after migration and over time.

Our main empirical findings support the key predictions of our simple model. First, we find that immigrants in districts with larger initial co-ethnic networks are significantly more likely to find employment within their first three years in Germany. Second, we find that this advantage fades away over time and disappears completely after around five years. Third, the likelihood that immigrants invest in training/schooling/education within three years of migration decreases with the size of co-ethnic network at arrival. Those investments improve opportunities in the labor market, in terms of wage and employment, hence the initial advantage in employment probability due to large-networks fades away over time. We also find that immigrants with smaller initial co-ethnic networks are less likely to find their jobs through referrals, and these effects are largely driven by less educated immigrants. For immigrants with tertiary education, the size of the initial network does not seem to affect economic outcomes. Finally, when we restrict our analysis to the sample of refugees and *ethnic Germans* subject to dispersal policies,[1] we find estimates of the effects on employment and on human capital investments similar to those obtained from least squares panel estimation with the rich set of fixed effects and pre-migration controls. This is consistent with the panel estimation producing estimates of the causal effect of networks. We also perform a series of robustness checks and falsification exercises, including a different definition of the geographic level of the networks and a placebo exercise to rule out demand-side effects. These exercises confirm our main results and are comforting in terms of the validity of our identification strategy.

This paper contributes to the literature in three ways. First, we estimate the short-run and long-run effects of the size of the co-ethnic network at arrival on immigrants' employment by taking advantage of the panel nature of our dataset.[2] Second, we analyze the investment in human capital of new immigrants after arrival as an additional outcome. This turns out to be a crucial mechanism to understand the differences in outcomes between the short- and long-run. To the best of our knowledge, there is no other study that investigates the role of co-ethnic networks on human capital investment of first-generation immigrants.[3] Third, thanks to the novel survey data, we have direct information on job search methods and in particular on whether people have found jobs through

[1]See Section B of our Appendix for the institutional background. Glitz (2012) also uses the group of *ethnic Germans*, exploiting its random allocation as a source of exogenous variation in labor supply.

[2]To our knowledge Edin et al. (2003) is the only study shortly mentioning the dynamics of network effects, though the paper focuses entirely on the static mechanism.

[3]Investments in schooling and education are mentioned in other studies (for example in Edin et al. (2003) and Damm (2009)) as possible channels through which networks have an effect. They have never been studied directly, however, because of data limitations.

personal contacts or through other search channels. Hence in our study we can directly test the empirical importance of a network-based channel in finding a job.[4]

As mentioned above, the identification of a causal effect of network size on new immigrants' outcomes hinges on the ability of separating out immigrant selection and its potential impact on post-migration labor market outcomes. A first approach in reducing the selection bias is controlling for a very large set of local and individual characteristics. Existing research (e.g. Cutler and Glaeser (1997), Bertrand et al. (2000), and Dustmann and Preston (2001)), measures networks at the local labor market level and include several local controls. Pre-determined individual controls for immigrants (such as their schooling level) are sometimes introduced, although it is typically hard to find those in national administrative datasets, and we do not know of any study controlling for pre-migration labor market characteristics of immigrants. This strategy, therefore, does not fully address the problems of endogenous sorting on unobservable characteristics. Recent papers, including Edin et al. (2003), Damm (2009), and Beaman and Magruder (2012), have exploited a different strategy. Researchers have noticed that national and international dispersal policies, typically applied to refugees, generate an exogenous initial spatial distribution of refugees. These policies, by distributing individuals independently of their skills and labor market histories, have generated quasi-experimental variation in the initial exposure to co-ethnic networks, which could be used to identify a causal effect on later outcomes. Limiting attention to refugees makes this type of identification credible. However, this group may be very different from the rest of the immigrant population, in terms of skills and other individual characteristics, and this limits the external validity of such an exercise.[5] Refugees come from traumatic situations, often experience periods of non-employment before migration, are nationals of a specific set of countries, and might not have selected their country of destination. This might not correspond to the experience of the majority of other immigrants, usually attracted by family and the chances of stable employment. As a consequence, network effects for refugees may not be representative of those on other immigrants.

Our approach improves on these methods along a few dimensions. First, our survey data include a set of pre-migration characteristics of immigrants, such as employment status, work experience, education level and language proficiency before arrival. This allows us to control for several pre-determined characteristics (usually unobservable in previous studies) and to test how these pre-migration characteristics are correlated with the size of the local co-ethnic network at arrival. Second, we observe arrivals of different national groups in different districts over time, and hence we can include a large set of two-way controls in our analysis. Specifically, we can control for country-year fixed effects absorbing all the common traits from specific national cohorts of immigrants, and for district-year fixed effects, absorbing local economic conditions at arrival and their role in the selection of immigrants. Finally, we can identify in our sample those individuals who were subject to dispersal policies (the refugees and the ethnic Germans) and conduct a specific analysis for that group. By doing so we can evaluate how the estimated effects for all immigrants

[4]A rare study analyzing the channels through which people find jobs and relating them to network size is Dustmann et al. (2016), where the network is defined at the firm level.

[5]Table D.1 in our Appendix shows that in our data these differences are substantial.

compare with those for the group of refugees and ethnic Germans, after including fixed effects and pre-migration controls. This allows us to assess whether the effects estimated on the restricted group subject to quasi-experimental variation are similar to the effects estimated using the large set of fixed effects and pre-migration controls in the sample of all immigrants. Similar estimates using either method would imply that the panel analysis with a rich set of fixed effects and pre-migration controls addresses in a satisfactory way the issues of selection and omitted variable bias.

The rest of the paper is organized as follows. In Section 2 we review the literature and discuss our contribution. In Section 3 we present our theoretical framework. Section 4 describes our data sources and presents some summary statistics; Section 5 presents our main empirical specification and discusses identification challenges. Section 6 presents our empirical results, including robustness checks. Section 7 provides some concluding remarks.

2 Literature Review

Our paper is related to research on the effects of networks on job search and labor market outcomes. Much of this literature does not analyze immigrants *per se*, but rather focuses on the role of social networks on economic outcomes of individuals. Important theoretical contributions to the modeling of social networks and their effects on labor market outcomes build on Calvó-Armengol and Jackson (2004). Beaman (2012) develops a network model with multiple cohorts to investigate the relative importance of information transmission and competition in networks and their consequences on the labor market. Bayer et al. (2008) investigate the effect of living in the same city block on the likelihood of working in the same establishment, finding an important role for referrals in the labor market. Goel and Lang (2009) show that networks may bring about additional job offers, thereby raising the observed wages of workers in jobs found through formal channels relative to those in jobs found through the network.[6] Our model, which builds upon Goel and Lang (2009), introduces in a simple search model the choice of human capital investment. Several papers frame networks as alternative to search in the general labor market. The network provides an advantage in the probability of a match but it may be limited by the specificity and cost of referrals. Galenianos (2013, 2014) develop models where network and formal markets coexist and different individuals use either of them depending on relative costs and benefits. Our framework can be seen as a simple case within this set of models. Zaharieva (2015) discusses theoretically how social networks and referrals may affect employment, earnings and welfare in a search and matching model with on-the-job search.

As mentioned above, some studies use the exogenous dispersal of refugees upon arrival to achieve empirical identification of the effect of the co-ethnic networks on labor market outcomes. Edin et al. (2003) use data from a dispersal policy in Sweden and find positive effects of network size on earnings for less-skilled immigrants. They also point out that networks might have a positive effect on information and a negative effect on human capital acquisition. However, they are not

[6] Pellizzari (2010) develops a search model with a formal and an informal channel and match-specific productivity. He finds stronger wage effects from finding jobs through contacts in countries with less competitive labor markets.

able to investigate the empirical importance of this channel because their data do not include any measure of human capital investment. Similarly, Damm (2009) investigates the effects of ethnic enclaves on labor market outcomes in Denmark by taking advantage of a dispersal policy and finds a large positive static effect of ethnic enclaves on earnings after migration. Munshi (2003) looks at network effects for Mexican migrants in the US. He uses past rainfall in the origin community as an instrument for network size at destination, and finds positive effects of networks on employment and on the chance to work in high-wage occupations. Xie and Gough (2011) analyze the role of ethnic enclaves on labor market outcomes in the US, and find no evidence of a positive effect of ethnic enclaves on earnings of new immigrants. However, the analysis is mainly based on correlations. Hellerstein et al. (2011) look at the role of residential proximity on the chances that workers work at the same establishment.[7]

Recent work looks at the role of referrals on employment outcomes at the firm level. Dustmann et al. (2016) develop a model of job referrals by which current employees in a firm provide information on potential candidates, and test the main predictions of the model using information on ethnic origin of employees of a large metropolitan market in Germany. They find that firms tend to hire workers from ethnic groups that are already represented in the firm, and that hiring through referrals pays higher wages and exhibits lower turnover. This suggests that referrals may improve the quality of employer-employee matches. Similarly, Patacchini and Zenou (2012) analyze the effect of ethnic networks on job search methods, and find results that confirm a positive role of networks on the probability of finding a job through referral. Analysis from our survey confirms these findings.

Our paper has several original features, compared to the existing literature. First, we are unique in having data on the pre- and post-migration history of individual workers. Second, our measures of co-ethnic network in the place and time of arrival, are based on the universe of immigrant workers and hence they are more precise and detailed than that of previous studies. This enables the inclusion of a richer set of fixed effects, absorbing local time-varying factors that may confound identification. In addition, we include in our analysis a comparison of the group of all immigrants and a specific group of immigrants whose initial location was determined by a quasi-random dispersal policy. Being able to compare a group of randomly dispersed refugees and ethnic migrants with a representative sample of new immigrants is unique to this paper, and helps us understanding the role of possible lingering selection on unobservables which may be present for economic immigrants but not for dispersed refugees. Let us emphasize, however, that the rich set of pre-migration controls, and the variation across location, time and nationality for the network variable, allow us to assuage worries of migrant selection and its correlation with initial networks via the inclusion of a rich set of fixed effects and of pre–migration controls. Finally, and also new in the literature, because of the longitudinal nature of our data we can perform an analysis of the dynamic effects of initial networks

[7] Using Danish administrative data, Bennett et al. (2015) look at the role of attitudes as well as networks on educational attainments of teenagers with a migration background. Åslund et al. (2011) analyze the role of neighborhood characteristics on the school performance of immigrant children, using data from an exogenous refugee policy in Sweden. Using the mass migration wave to Israel as exogenous variation, Gould et al. (2009) look at the effects of high exposure to immigrants during elementary school on the long-term educational attainment of natives.

on employment and on human capital investment from one to six years after arrival. Uncovering the dynamic effects of initial networks allows better understanding of how those networks affect immigrants and their assimilation, and also makes us better equipped to discuss possible policy implications.

3 A Simple Theoretical Setup

Our simple framework builds upon Montgomery (1991) and Goel and Lang (2009). It helps to illustrate the trade-off between search and human capital investments in the presence of social networks. This generates the key insights for our empirical predictions. Let us consider two periods, $t = 1, 2$. At the beginning of $t = 1$ the agent (a newly-arrived immigrant) enters the local economy (the destination country) with a certain level of human capital, which we take as exogenous. The level of human capital in periods 1 and 2 are denoted by h_1 and h_2. We interpret human capital as the general set of skills that are valued in the host country labor market. The initial value of h is determined by its pre-migration level and its transferability. The size of co-ethnic network at the initial location is denoted as n_1. We denote a certain realization of h by $\bar{h}$ and a certain realization of n by $\bar{n}$. We assume all individuals to be initially unemployed.

There are two mechanisms through which workers receive job offers.[8] First, when searching for a job there is a certain probability that the worker receives an offer through the *formal channel*.[9] We denote this probability by p_f and assume that it depends positively on the human capital level of the individual, so that $\partial p_f(h)/\partial h > 0$, and also assume it does not depend on the size of the local network. The individual may also receive an offer from the co-ethnic *network channel* (or informal channel) with a probability p_i, which depends positively on the size of the co-ethnic network, such that $\partial p_i(\bar{n})/\partial n > 0$ and does not depend on the individual's human capital. We assume decreasing marginal returns for both channels, i.e. $\partial^2 p_i(n)/\partial n^2 < 0$ and $\partial^2 p_f(h)/\partial \bar{h}^2 < 0$.[10]

At the beginning of each period, the worker decides whether to search for a job or to invest in general human capital, engaging in activities that increase her human capital level h. If the individual looks for a job, she has some chances of getting an offer from either channel, as outlined above. We do not need to assume that wages are drawn from the same wage offer distribution in the formal and network channel. We assume the draws to be independent and that the two wage offer distributions have overlapping support.[11] For convenience, we assume that those distributions do not change between period 1 and period 2. We denote the common cumulative distribution of

[8] A more general model is that of van den Berg and van der Klaauw (2006), where the intensity of the search is endogenous. For simplicity, in our model the only endogenous choice is whether to search or to invest in human capital during the first period.

[9] One characterization of the "formal channel" would be a matching mechanism where applicants send applications with their resumes to employers or to an employment agency.

[10] Since p_i and p_f are probabilities, they are bounded between zero and one. However, we are not imposing the constraint that $p_f + p_i = 1$. This is because in our model an individual searching for a job can get either zero, one or two offers.

[11] This means that the highest possible offer from one of the two distributions cannot be lower than the lowest offer from the other distribution. In that case, there would be no gain in expectations from drawing two offers instead of one. That is a case we can deal with, but it is not of interest.

wage offers obtained in the formal channel as $F_f(w)$. Correspondingly, wage offers in the network channel are drawn from $F_i(w)$. Instead of searching for a job, the individual can increase her human capital endowment. Her human capital after education is $\bar{h}' > \bar{h}$. We assume that $\bar{h}' = \bar{h} + A$, with $A > 0$. This is equivalent to assuming that human capital increase is independent from its initial level. Combined with $\partial^2 p_f(h)/\partial\bar{h}^2 < 0$, this assumption implies that investing in education has larger marginal effects on labor market perspectives of individuals with low initial levels of human capital. At the beginning of period 2, an agent that has chosen human capital investment in the previous period is more likely to get offers through the formal channel (and therefore less likely to be unemployed) and has a higher expected wage (because of the possibility of receiving two offers).

The key decision for the agent is made at the beginning of period 1. If she searches for a job she will receive an offer through the formal channel with probability $p_f(\bar{h})$ and through the network channel with probability $p_i(\bar{n})$. If she receives no offer, she remains unemployed, receives unemployment payments b_u, and begins period 2 with the same level of human capital $h_2 = h_1 = \bar{h}$. If she receives one offer, from either channel, she will accept it if the wage is higher than b_u and reject it otherwise. We assume b_u to be time invariant and that the agent gets no utility from leisure, so the decision in the second period is equivalent to that in the first period. If the agent receives two offers, she will accept the higher offer if it is higher than b_u, and reject both otherwise. Instead, if she decides to get education, the individual receives b_h in period 1 and will enter period 2 with a higher level of human capital $h_2 = \bar{h}' > \bar{h}$. This allows her a higher probability to receive an offer from the formal channel at $t = 2$. We assume that $b_u \geq b_h$ to allow for some costs of education.[12]

3.1 Preferences

Our agent values consumption and discounts second period outcomes at the rate $0 < \beta < 1$. We assume utility to be linear in consumption,[13] such that we can write expected utility as $EU(c_1, c_2) = c_1 + \beta E(c_2)$. As a standard two-period model, the solution is best described using backward induction. We start by illustrating possible payoffs at period 2. At $t = 2$, human capital investment will not occur since $b_u \geq b_h$. Therefore, the individual will search for a job at $t = 2$ for all realizations of the exogenous parameters. If the agent acquired human capital in period $t = 1$, she will be able to search for a job with a higher probability of receiving an offer through the formal channel, and therefore also a higher probability of receiving two offers. If the agents searched in period 1, she will search again with the same human capital endowment as in $t = 1$.[14]

[12] While this assumption seems natural in this context, it is stronger than needed in our model as we only need assume that expected income is larger for those who look for a job at $t = 2$. None of the main propositions discussed below depend on this assumption.

[13] Implicitly, we are assuming that individuals are endowed with one unit of time/effort in each period, which they supply to education or search/work.

[14] We assume separation rates at the end of each period to be equal to one so that our problem is recursive. None of our qualitative results depends on this assumption. We are not investigating the possibility that employment can generate human capital as well. As long as growth in human capital is smaller when working than when in school, the main results of our model are robust to relaxing this assumption.

3.2 Value functions

At the beginning of $t = 2$, all individuals search for a job. If the agent has searched at $t = 1$ then $h_2 = h_1 = \bar{h}$, and her expected payoff from searching in period 2 is

$$\begin{aligned} S_2(\bar{n}, \bar{h}) = b_u &+ p_i(1-p_f)\int \max\{W_2(x_i) - b_u, 0\}dF_i(x_i) \\ &+ p_f(1-p_i)\int \max\{W_2(x_f) - b_u, 0\}dF_f(x_f) \\ &+ p_i p_f \int \max\{W_2(x_i) - b_u, W_2(x_f) - b_u, 0\}dF_i(x_i)dF_f(x_f) \equiv \mathcal{S}(\bar{n}, \bar{h}), \end{aligned} \tag{1}$$

where we omitted the dependence of p_i and p_f on network size $\bar{n}$ and human capital $\bar{h}$. Searching in period 2 means the agent gets at least b_u, and has a certain probability of receiving wage offers that are higher than b_u. The agent may instead enter period 2 after having invested in human capital in period $t = 1$. In this case her human capital is $\bar{h}' > \bar{h}$ and therefore the value of searching is $\mathcal{S}(\bar{n}, \bar{h}') > \mathcal{S}(\bar{n}, \bar{h})$ because of our assumption that $\partial p_f/\partial h > 0$.

At the beginning of period 1 the agent decides whether to make an educational investment or to search for a job. If the agent decides to search for a job in period $t = 1$ given an initial network size of $\bar{n}$ and initial human capital level $\bar{h}$ the value function can be simply written as:

$$S_1(\bar{n}, \bar{h}) = \mathcal{S}(\bar{n}, \bar{h}) + \beta\mathcal{S}(\bar{n}, \bar{h}) = (1+\beta)\mathcal{S}(\bar{n}, \bar{h}). \tag{2}$$

A searching individual receives the value of being unemployed plus the possible gain from employment. At the beginning of period 1 the individual may instead decide to invest in human capital. The corresponding value function is

$$H_1(\bar{n}, \bar{h}) = b_h + \beta\mathcal{S}(\bar{n}, h'). \tag{3}$$

Costs of education are incorporated in b_h.[15] Education increases future employment possibilities as the newly acquired skills are useful to find a job in the host economy. Therefore, the lower employment prospects are and the higher the discount rate (β) is, the more likely it is that an agent invests in human capital at $t = 1$.

3.3 Employment and Human Capital Investment

The simple structure described above is sufficient to illustrate the main trade-off faced by the agent. Human capital investment increases employment and expected wages in the future, at the cost of possibly foregoing current earnings. After observing her level of human capital and the size of the social network at the beginning of period 1, the individual decides whether to look for a job or to acquire human capital. The optimal decision between searching and acquiring human capital results from comparing $S_1(\bar{n}, \bar{h})$ and $H_1(\bar{n}, \bar{h})$. Next, we discuss how this optimal choice depends on $\bar{n}$ and

[15] Results may be different for a risk-averse agent since returns to education are stochastic in this model.

$\bar{h}$. We are able to make three simple predictions in a comparative statics exercise.

Proposition 1 *For each level of n_1 there is at most one "reservation" level of h_1 below which the agent will invest in human capital and above which the agent will search for a job in period 1.*

For a given level of n_1, both the value of searching and the value of investing in human capital are increasing, concave functions of h_1. Under our assumptions, the relative first and second derivatives are such that the two curves $S_1(\bar{n}, h)$ and $H_1(\bar{n}, h)$ will intersect at most once in the h space.[16] Following Proposition 1, for a given level of social networks individuals with higher human capital are more likely to be employed in period 1 and less likely to invest in further human capital. Individuals with lower human capital are more likely to get education and less likely to be employed soon after arrival. See Appendix A for additional discussion.

Proposition 2 *For each level of h_1 there is at most one "reservation" level of n_1 below which the agent will invest in human capital and above which the agent will search for a job in period 1.*

For a fixed value of $h_1 = \overline{h}$, $S_1(n, \overline{h})$ is increasing in the level of n_1, because n_1 positively affects offers' arrival rate via the network channel. It is only slightly more subtle to see why the value of human capital investment is lower at higher values of n_1. Let us imagine a case in which an individual with a very large social network decided to acquire further education in period 1. Despite the higher level of human capital, it would still be relatively likely for her to get an offer in the informal sector compared to the formal sector, and thus for her further human capital investment makes less of a difference.[17] Proposition 2 implies that individuals with larger co-ethnic networks are less likely to get further education and more likely to be employed in the first period. See Appendix A for additional discussion.

Proposition 3 *The magnitudes of the effects of networks on employment and human capital investment are lower if the individual has a higher initial human capital endowment.*

For a given network level, individuals with higher initial human capital endowment h_1 are relatively more likely to find a job through the formal channel compared to individuals with lower initial human capital endowment. The marginal effect of network size in the value functions of individuals with initially high human capital is therefore going to be smaller. While qualitative effects of network size are unaffected, effects on employment are quantitatively larger for individuals with lower initial human capital endowments.[18] See Appendix A for additional discussion.

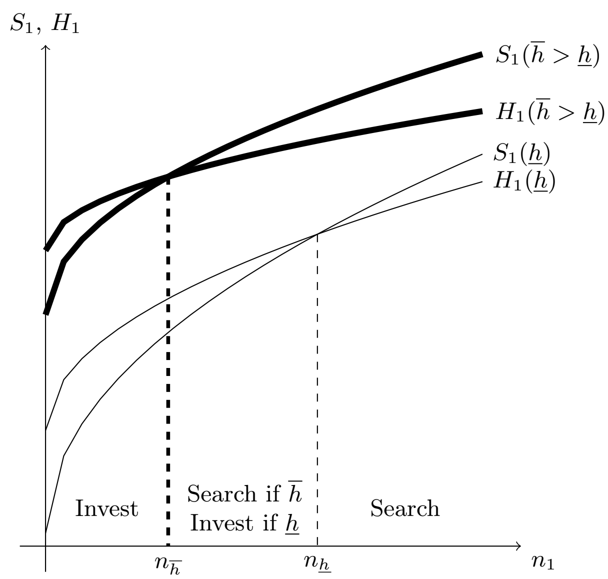
[16] Depending on functional form and support of h and n, corner solutions may exist: initial social networks n may be so large that the agent may find it optimal to search for a job irrespective of the level of h. We analyze the two functions S_1 and H_1 in more detail below and in Appendix A.

[17] Corner solutions may exist in this case as well: there might be levels of human capital that are high enough such that the agent searches for a job in period 1 for any possible level of social networks.

[18] In order to make predictions concerning whether we expect individuals with low initial human capital or individuals with high initial human capital to be more likely to invest in it, we need to give some structure to the returns to human capital. If returns to human capital are smaller for individuals with high initial human capital endowment, which is the standard assumption in the literature and has support in our data, individuals with lower initial human capital are more likely to invest in its improvements. Results would change if returns to human capital were larger for individuals with larger initial stocks. This case would be closer to Regets and Duleep (1999).

Summarizing, based on our model we expect individuals with larger initial co-ethnic networks to be more likely to find employment after arrival. However, our model also predicts the positive effect of a co-ethnic network on employment probability to decrease over time, because individuals with smaller co-ethnic networks "catch up" through human capital investment. Finally, the effect of network size on employment probability and on human capital investment after immigration are larger for individuals with lower initial human capital. Figure 1 summarizes the main features of the equilibrium of our model. It plots the value functions of an individual, S_1 and H_1, as a function of initial network size. An individual with lower initial human capital $\underline{h}$ will optimally decide to invest in human capital if her initial network size is below $n_{\underline{h}}$, and she will search for a job if it is larger. This illustrates Proposition 2 above. The two thicker curves in Figure 1 are instead drawn for an individual with higher human capital $n_{\overline{h}} > n_{\underline{h}}$. Both S_1 and H_1 are higher (because at higher human capital levels the expected utilities are higher due to higher probability of job offers) and flatter (reflecting the fact that marginal effects of network size are smaller at higher levels of human capital, because offers are more likely to come from the formal channel, making networks less relevant for labor market outcomes as in Proposition 3). The new threshold for network size below which the individual invests in human capital is now lower at $n_{\overline{h}}$, because the shift of the value function for search is larger than that of the value function for human capital investment. This shift from $\underline{h}$ to $\overline{h}$ is an illustration of Proposition 1 above. The figure shows a range of intermediate network sizes for which individuals with lower levels of initial human capital invest, while individuals with higher levels of initial human capital search for a job in the first period.

Figure 1: Searching for a Job and Human Capital Investment

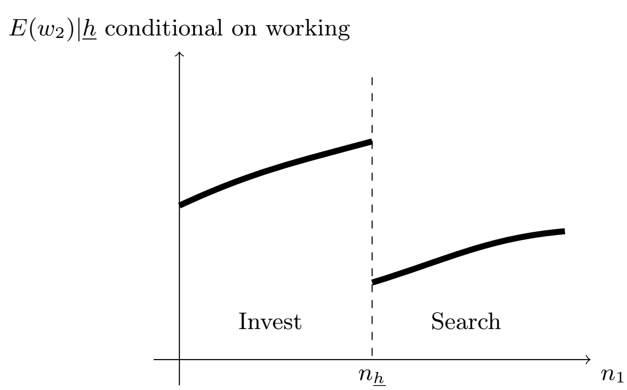


3.4 Wages

In the paragraphs above, we have discussed the implications of our model for employment and human capital investment. Next, we look at the effects on wages (wages of those who are employed). Even if the distributions of wages from each channel (market and network) are given, the realized wage of an individual depends on the probability of getting competing offers. When an individual has a higher chance of receiving two offers she also has a larger expected wage, but may not have a higher observed wage. Therefore, without additional assumptions on the wage distributions of the two channels our model cannot deliver any predictions on relative observed wages at $t = 1$, because more chances to draw from a distribution can lower *observed* wages of the employed. For the analysis below, we therefore further assume that the wage offer distribution of the formal channel and of the network channel have the same expected value. This rules out that a higher probability of receiving an additional offer depresses average wages. Under this assumption, observed wages at $t = 1$ are a monotonically increasing function of n: conditional on h_1, a higher $\bar{n}$ increases the likelihood of receiving two offers, which is associated with a higher expected wage.

The relationship between initial network size and observed wages at $t = 2$ is only slightly more complicated. Assuming that initial human capital is sufficiently low for an internal solution to exist, at low levels of n_1 the individual will acquire human capital and enter period 2 with $h_2 > h_1$. Observed wages at time $t = 2$ are increasing in n_1 because larger social networks increase the probability of receiving two offers. However, this effect exhibits a discontinuity at the level of social networks above which the individual does not invest in human capital at $t = 1$. If n_1 is high enough, the individual will not find it profitable to invest in human capital at $t = 1$ and her wages at $t = 2$ will be lower. For changes in initial network size that are large enough to affect human capital accumulation decisions, individuals with larger network are expected to have lower wages in the long run. Figure 2 depicts the relationship between the wage in the second period and the size of the network.[19] We expect this effect to be concentrated among those with relatively low initial human capital, for whom initial network size is more likely to affect human capital decisions.

Figure 2: Wages at $t = 2$ for a Given Level of Initial Human Capital



[19]Figure 2 is drawn under the assumption that initial human capital is low enough to imply positive human capital investment at $t = 1$ for low values of n.

4 Data

Our primary dataset is the IAB-SOEP Migration Sample, a recent survey of immigrants to Germany initiated in 2013 and conducted every year. The survey over-samples immigrants who arrived in Germany after 1994. We use the subsample of the survey that has been linked to social security data (which includes all workers covered by the social security system, excluding civil servants, self-employed, and military personnel),[20] selecting only foreign-born people aged 15-65 and excluding those entering with a job offer or as students (who, as a consequence, would not be looking for a job immediately after arrival). We also exclude individuals who reported to be self-employed or civil-servant in the first job in Germany or at the time of the survey or who had income from self-employment the year previuos to the survey.[21] We are able to observe several individual pre-migration characteristics, as well as the entire labor market history after migration to Germany. The final sample size consists of 1,148 individuals, and 11,788 person-year observations. The data on employment and wages are from IEB (*Integrierten Erwerbsbiografien*) and cover the period 1975-2014. A person is considered employed if she ever works within the year. We check the robustness of the results to this criterion by using alternative definitions of employment, that we discuss later. Wages are measured as log of real daily wages of the longest spell within the year considering only full-time spells. Our measure of human capital investment originates from the survey data. The survey provides a full account of each year spent in education as each individual is asked retrospectively to fill a life-long calendar and to report for each year, starting from age 15 and through to the current age, whether in that year she was in education.[22] We use this information to reconstruct an individual's life-long panel of spells of education, and we merge this with the individual administrative records.[23]

The variable capturing the co-ethnic network size at arrival for each immigrant is calculated as the number of workers from the same origin country-group[24] as shares of total employment in each district in the year when the immigrant first arrived in Germany.[25] This share is calculated using the full registry of employees in Germany (IEB). The number of German districts is 402, with an average size of 69,194 workers per district and a median size of 45,725. Our sample of immigrants is distributed across 238 districts. Our network measure has an average size of 0.01

[20]We use waves 1-3 of the survey (2013-2015), as they are currently the only ones linked to social security data.

[21]The survey is targeted at individuals with any migration background, including second generation immigrants. See Appendix C for details.

[22]It is possible to distinguish the type of education only in terms of two broad categories: vocational education, including also retraining or apprenticeship, and school-university education. We run the analysis considering an aggregated measure of human capital investment and then we report the main estimates also by type of education.

[23]To limit recall bias, we also use administrative data, setting the education variable to zero if the person in the corresponding year works at least 50 percent of the time. Later we discuss the robustness of the results to different values for this cut-off.

[24]Due to sample size considerations, we aggregate them into eight country groups: Western countries including Western Europe, Eastern Europe, South-Eastern Europe, Turkey, USRR, Asia and Middle East, Africa, Central and South America. The data are aggregated based on the workplace district.

[25]Since the information on country of birth is missing in German administrative data, immigration status is defined based on nationality. We define the living district as the one corresponding to the longest spell within the year, and impute this information from the workplace district in case of missing values.

with a standard deviation of 0.015 and a highest value of 0.08. The immigrants with the highest value of the average co-ethnic network size are those from Western Europe (0.033) followed by Turkish immigrants (0.029), and South-Eastern European immigrants (0.02).

4.1 Descriptive Statistics

Table 1 reports summary statistics for the main variables used in our empirical analysis. The top panel of this table reports averages for time-varying individual variables. The first of them, however, Netw_{cd_0} measures the size of the co-ethnic network at time of arrival (described above) and is fixed for an individual. The employment probability has an average of 68.2 percent in the individual-year observations.[26] The average real daily wage earned in the sample is around 62 Euros for full-time workers. Individuals in the sample are investing in education, i.e. spending some time in school or in training, in 4.9 percent of the individual-year observations. Education and training are more likely when an immigrant first arrives and the share of individual-year in education is higher during the early years of their stay in Germany: eleven percent among immigrants in Germany for two years or less was in education part of the year, while it decreased to one percent for immigrants in Germany for at least six years (see Table D.5). Symmetrically, employment rates increase with time since arrival. During the first two years since arrival only 47 percent of individuals worked, while after 10 years more than 80 percent were employed (see Table D.5). Our panel is unbalanced: the average number of years since migration observed is 6.97, whereas the median value is six years. Around 28 percent of observations involve individuals who lived between zero and two years in Germany, 21 percent for three to five years, and 51 percent have been in Germany six or more years. Our sample is comprised of relatively young individuals – the average age is 37 years old.

The bottom panel of Table 1 lists averages of time-invariant individual characteristics mainly relative to ethnicity, country of origin and pre-migration characteristics. These information are obtained from the IAB-SOEP-Migration survey. Our sample consists of 1,148 foreign born individuals[27] in working age (15-65 years old), who are linked to the registry data.[28] Among those immigrants, we select individuals whose date of arrival reported in the survey is within three years of their first appearance in the registry data.[29] As we do not have information on the district of arrival from the survey, we take the district of first registration in the administrative data as capturing the place of arrival of the new immigrant. In addition to the standard characteristics, such as gender, age, and region of origin, we have information on a set of pre-migration characteristics that we use throughout the analysis: education, work experience, language proficiency, and employment

[26]We define employment as working for any length of time during the year. This share falls to 56 percent if we count as employed only those working for at least 50 percent of the year.

[27]Using country of birth to identify immigrants is a much more precise definition, and this represents an improvement with respect to all previous papers using German administrative data, which can only identify immigrants via nationality. This is particularly important for Germany, where members of the large group of ethnic Germans are entitled to German nationality by law. To the best of our knowledge, Dustmann et al. (2016) is the only other study using these data in part of their analysis.

[28]For more details about the linkage please see Section C in the Appendix.

[29]Please see Section 6.7 for results using alternative ways of imputing the initial location of an immigrant

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	N
	Time-Variant Variables		
Netw_{cd_0}	0.010	0.015	11788
Employment	0.682	0.466	11788
Human Capital Investment	0.049	0.216	11753
Real Daily Wage	62.088	29.104	4827
Year since Migration:0-2	0.282	0.450	11788
Year since Migration:3-5	0.209	0.407	11788
Year since Migration:6+	0.509	0.500	11788
Age	37.322	10.820	11788
	Individual Variables		
West	0.095	0.293	1148
East Europe	0.153	0.360	1148
Turkey	0.047	0.212	1148
South and East Europe	0.220	0.415	1148
USSR	0.360	0.480	1148
Asia	0.071	0.256	1148
Africa	0.042	0.200	1148
Central and South America	0.012	0.110	1148
First Job Found through Contacts	0.596	0.491	644
Low Education before Migration	0.443	0.497	1148
Medium Education before Migration	0.312	0.463	1148
High Education before Migration	0.246	0.431	1148
Employed before Migration	0.699	0.459	1148
Language Proficiency before Migration	0.116	0.320	1148
Work Experience before Migration	9.257	8.608	1148
Age at Migration	31.370	9.918	1148

Source: IAB-SOEP Migration Sample, (bottom Panel) linked to IEB Data (top Panel).

status one year before migration. The survey data also report the job search method for the first job found in Germany. Our sample reflects the fact that people are relatively young when they migrate: age at migration is around 31 years on average with a median age of 29. Notice, as first indicator of the potential importance of networks for this group, that 60 percent of the immigrant sample found their first job in Germany through personal contacts[30] (65 percent among low-skilled immigrants, i.e. those with at most lower secondary education). The information on job search method is rarely available in data that collect labor market outcomes and we will use it more formally below.

[30] The question asked is the following: "How did you find your first job in Germany?". The possible answers are: Federal Employment Office, employment agency, employment agency for foreigners, private job agency, job advertisement in the newspaper, job advertisement on the internet, through business relationships in Germany, through friends/acquaintances/relatives (which we denote as "personal contacts"). For this answer we consider only the first two waves of the survey because in the third wave the question is asked differently. Since multiple answers are possible we select only the cases where the respondent reported only one method of search. In addition, we exclude a small group of individuals who never work in the administrative archive.

5 Empirical Specification and Identification

In order to estimate the effect of the size of the co-ethnic network at arrival on employment and human capital investment of new immigrants, we estimate the following linear equation:

$$Y_{icd_0t} = \alpha + \beta \mathbf{X_{it}} + \gamma_0 Netw_{cd_0} + \gamma_1 Netw_{cd_0} \times Ysm_{it} + \eta Ysm_{it} + \delta_{d_0} + \psi_{t_0} + \theta_c + \epsilon_{it}, \qquad (4)$$

where Y_{icd_0t} is an outcome for individual i who first arrived in district d_0 from country-group of origin c in year t. In our main regressions the variable Y will be, alternatively, a dummy for being employed or a dummy for being in school or training, which we call "investing in human capital". $\mathbf{X_{it}}$ is a vector of time-varying and time-invariant individual characteristics and includes a gender dummy, age and its square, age at migration and its square, and a set of pre-migration characteristics (education, working experience and its square, employment, and a binary indicator of German language proficiency).[31] The variable $Netw_{cd_0}$ captures the size of the co-ethnic network (previous working immigrants from the same country-group c as share of total employment) in the district of arrival d_0.[32] This measure varies across country-groups, districts and individuals. For each individual it is fixed at the value in year 0 (arrival year). The term δ_{d_0} captures a set of district-of-arrival fixed effects and θ_c captures a set of country-of-origin fixed effects. The term ψ_{t_0} denotes year-of-arrival fixed effects. The variable Ysm_{it} is a dummy that indicates the number of years since migration for individual i. In our analysis we use three dummies for "years since migration": $(Ysm^{0-2})_{it}$, $(Ysm^{3-5})_{it}$ and $(Ysm^{6+})_{it}$. These dummies take values one in the first three years since arrival, years 3-5, and more than five years from arrival, respectively.

The non-random initial location of immigrants may bias the estimates of the coefficients of interest ($\gamma's$) if unobserved individual characteristics affecting employment and human capital investments are also correlated at the local level with the initial size of the co-ethnic network. Controlling for pre-migration characteristics (which are usually not observed) and including district and country-of-origin fixed effects, which absorb systematic differences in economic performance across cities and ethnic groups, alleviates these issues substantially. Many non-observable individual features in previous studies may be proxied by pre-migration characteristics in this study. The variation over location, time and groups allows a rich set of fixed effects absorbing local non observable characteristics. In our main specification, we estimate equation (4) using OLS while absorbing location specific effects and pre-migration characteristics with the controls. We therefore only exploit differences in the size of initial co-ethnic network unrelated to pre-migration characteristics. We only use within-district, within country-of-origin variation, controlling for time-of-arrival effects. In addition, in the employment regressions we use a large external sample of immigrants from administrative data in order to estimate country-year, year-district, and country-district fixed effects on employment. This allows us to include a very "saturated" specification with two-way

[31] When we run the analysis on the restricted sample we also include a binary indicator for refugees in order to control for additional differences between refugees and ethnic Germans.

[32] We consider all workers observed at the 30^{th} of June in each year. We exclude from this sample all individuals in apprenticeship or in marginal employment.

(district-time) fixed effects.[33] Local district-time specific economic shocks that affect outcomes and, possibly, the characteristics of immigrants locating there, are absorbed by district-time effects. We estimate equation (4) first using all immigrants in our sample and then using a restricted sample, which consists of people who reported in the survey entering Germany as asylum seekers or refugees[34] and of ethnic Germans during the period of the *Residence Allocation Act*. Due to institutional arrangements, both of these groups were subject to a dispersal policy implemented by a central authority and hence are not subject to self-sorting (see Appendix B for details about the Institutional setting). Ethnic Germans (*Aussiedler*) were immigrants with German ethnicity mainly from Eastern Europe who were subject to allocation by local authorities as they entered Germany. This restricted and exogenously dispersed sample consists of 311 individuals, of whom about one-third are asylum seekers and two thirds ethnic Germans.

Our main identification strategy relies on the fact that, conditional on all fixed effects, the initial distribution of immigrants is not associated with sorting of their characteristics that is correlated with the size of their co-ethnic network. As we observe several pre-migration individual characteristics – included as controls in the main analysis – we will test whether they are correlated with the size of the co-ethnic networks, unconditionally. Then we can also test whether this correlation is reduced when we include our sets of fixed effects, which should certainly account for local and group features that may produce those correlations. We will perform this exercise first on the whole sample and then on the sample of refugees. The first sample should show some sorting of characteristics correlated with the network, when we do not control for fixed effects, while the second, if exogenously distributed, should not. If the sorting is across districts or across ethnic groups it should be controlled for by fixed effects and correlations should disappear when introducing those.

While testing the absence of conditional correlation of observable pre-migration immigrant characteristics and network intensity does not guarantee that unobservable characteristics are also uncorrelated, it does provide an important check of our identifying assumption. The test that the restricted sample does not exhibit unconditional correlations is also a check that that sample is spatially randomly distributed. If most of the omitted variable bias is eliminated by introducing a set of fixed effects or by using the restricted sample, we have two ways of producing coefficients that can be causally interpreted. Besides testing that, conditional on one-way fixed effects, immigrants' characteristics are not correlated with the size of the network, in the most demanding specifications we will also control for a full battery of two-way fixed effects: country-of-origin by year-of-arrival, year-of-arrival by district-of-arrival, and country-of-origin by district-of-arrival fixed effects.[35]

[33]We don't have an external sample with information on human capital investment for the estimation of double fixed effects, therefore in the specification for human capital we only use one-way fixed effects for location, time and country groups separately, as described in equation (4).

[34]In the following we sometimes denote this group as refugees for simplicity.

[35]In order to do this we need to rely on pre-estimated fixed effects computed using an external sample of immigrants obtained from administrative data as our small sample size does not allow us to estimate them reliably.

6 Results

6.1 A Test of Sorting

The immigrant survey includes information on pre-migration characteristics, specifically on age at migration, education, employment, working experience, and language proficiency. The existing literature analyzing post-migration outcomes of immigrants uses administrative data and does not have access to pre-migration information. In our main regressions, which we discuss below, we include them as controls. In this section, instead, we use them to test the initial sorting of immigrants across locations, which provides an idea of the potential concerns for omitted variable bias in our estimates. In particular, while initial characteristics of immigrants can be correlated with the size of co-ethnic networks at arrival because individuals are likely to differ in their preferences for networks, we test whether this correlation survives the inclusion of district-of-arrival, country-of-origin, and year-of-arrival fixed effects. If the correlations between pre-migration characteristics and size of the network are weak once we condition on our set of fixed effects, this would indicate that individual characteristics, at least those observable before migration, are not correlated with selection of places with large networks. If the vector of pre-migration observables is a proxy for unobserved characteristics affecting human capital investment and employment after migration, this test is a check of how severe the omitted variable bias can be in our regressions.

This orthogonality (balancing) test of pre-treatment characteristics is novel to the migration literature because typically the information about pre-migration variables is usually unavailable. It is however typical in several other fields–for instance, a similar approach is take in Guryan et al. (2009) to test the orthogonality between predetermined student characteristics and average class characteristics. In Table 2 we show the coefficients obtained by regressing the initial network size variable on all pre-migration variables of migrants into those locations, first without including other controls (Column 1), then adding country-of-origin, year-of-arrival, and district-of-arrival fixed effects (Column 2). While significant correlations exist (Column 1) showing in particular that large initial co-ethnic networks are associated with workers who worked less, or with workers with more language proficiency before migration, none of the pre-migration characteristics are correlated with the initial network size (neither individually nor jointly) once we condition on the district, year-of-arrival, and country-of-origin fixed effects (Column 2). Notice that the estimated coefficients on each immigrant characteristics in Column 2 of Table 2 are small and not statistically significant. The test of joint significance of all characteristics being correlated with the size of the local network cannot reject the null of no correlation at any significance level (p-value equal to 0.56).

We also estimate the same specifications on our restricted sample (Columns 3 and 4) to check whether this test is consistent with exogenous dispersal of refugees or ethnic Germans to local areas. In this case, even without any control, none of the pre-migration characteristics has a significant coefficient in explaining the size of the initial co-ethnic network. Nearly all point estimates (Column 3) are lower in magnitude compared to the full sample (Column 1) and none is significant. Including the set of fixed effects (Column 5) leaves estimates unaffected and increases somewhat the standard

Table 2: Test of Network Sorting

	Dependent Variable: Netw_{cd_0}			
	Full Sample		Restricted Sample	
Pre-Migration Variables	(1)	(2)	(3)	(4)
Language Proficiency	0.186*	-0.055	-0.046	-0.002
	(0.100)	(0.067)	(0.053)	(0.035)
Employment	-0.185**	-0.025	-0.137	-0.143
	(0.075)	(0.054)	(0.099)	(0.117)
Work Experience	-0.014	-0.000	-0.002	0.002
	(0.014)	(0.009)	(0.015)	(0.012)
Work Experience sq.	0.001	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Low Education	0.008	0.036	0.057	-0.069
	(0.075)	(0.050)	(0.047)	(0.068)
Medium Education	-0.170**	-0.058	0.012	-0.106
	(0.069)	(0.054)	(0.043)	(0.075)
Age at Migration	0.034	-0.012	0.003	-0.001
	(0.022)	(0.014)	(0.014)	(0.013)
Age at Migration sq.	-0.001**	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Individuals	1148	1148	311	311
Rsq	0.044	0.784	0.054	0.882
All Coefficients=0 (p-value)	0.000	0.567	0.017	0.807
District of arrival	no	yes	no	yes
Year of arrival	no	yes	no	yes
Country of origin	no	yes	no	yes

Note: The dependent variable is the network at migration, calculated as the number of workers by nationality as share of total employment in each district in the year in which the immigrant first arrives to Germany. The heading "Restricted sample" refers to refugees and ethnic Germans who were subject to a dispersal policy. Robust standard errors in parenthesis: * p<0.10, ** p<0.05, *** p<0.01

errors, confirming that the characteristics of the restricted sample were uncorrelated with destination and group. The evidence shown in Table 2 is consistent with the hypothesis that, once we include our set of fixed effects, immigrants' pre-migration characteristics are uncorrelated with initial location and hence omitted variable bias should not be significant if those characteristics are proxies for unobserved ability.

6.2 Job Search Methods

Our model assumes that initial locations with a large co-ethnic network can help employment probability through the role of the network in referring new immigrants to jobs. This channel is rarely directly tested in the literature because of data limitations.[36] In most datasets, it is not possible to know how people find a job, and hence it is impossible to produce evidence that search methods are affected by the size of social networks. In our data, as mentioned above, we have information on the way an individual found her first job in Germany. This allows us to investigate the relation between co-ethnic networks at arrival and the type of search method that

[36] Dustmann et al. (2016) is a notable exception. The authors use German administrative data to evaluate the effect of within-firm ethnic networks on wage growth and firm turnover. Part of their empirical analysis is based on the same survey that we use to show how the within-firm ethnic networks affect the probability of finding the job through contacts.

facilitated the first job in Germany for the immigrant. Table 3 shows the coefficients on the co-ethnic network variable in regression models whose dependent variable is set equal to one if the first job in Germany was found thanks to "personal contacts" (Columns 1-4), newspaper/internet (Column 5), or employment agency (Column 6). Looking at correlations of alternative channels with local network size allows us to investigate and rule out potential confounder mechanisms. For instance, one can imagine that the support offered by the public employment agency may be more effective for immigrants that are part of a larger local community because of linguistic and cultural reasons, and in that case larger networks will be associated with more jobs through employment agencies.

Table 3: Method of finding the first Job in Germany

	Contacts				News-Internet	Empl. Agency
	(1)	(2)	(3)	(4)	(5)	(6)
Netw_{cd_0} (a)	0.093***	0.050	-0.006	0.072	-0.034	0.026
	(0.019)	(0.051)	(0.058)	(0.053)	(0.047)	(0.046)
Netw_{cd_0}xLow Edu (b)			0.099**		-0.076	-0.007
			(0.049)		(0.052)	(0.039)
Netw_{cd_0}xHigh Edu (c)				-0.064		
				(0.055)		
Mean Dep. Var.	0.597				0.180	0.191
(a)+(b)			0.093		-0.110	0.020
(a)+(b)==0 (p-value)			0.079		0.001	0.653
(a)+(c)				0.009		
(a)+(c)==0 (p-value)				0.880		
Controls	no	yes	yes	yes	yes	yes
Clusters		193	193	193	193	193
Individuals	647	644	644	644	644	644
Rsq	0.032	0.441	0.446	0.443	0.415	0.395

Note: The dependent variable is a binary indicator for finding the first job in Germany through contacts (friends/acquaintances/relatives) (columns 1-3), news or internet (column 5) or employment agency (column 6). All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age at migration (and its square), country of origin fixed effects, average wage in the district of arrival, year and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level, * p<0.10, ** p<0.05, *** p<0.01.

Given that this information is only available for the first job in Germany, we estimate the same specification (4) as in the rest of the analysis, but using only one observation for each individual and, hence, we simply estimate the effect of network at arrival on the probability of finding the first job in Germany through networks. The network variables we use throughout the regression analysis are standardized to have mean zero and standard deviation one. The estimates in the row Netw_{cd0} contain the coefficient on the size of the co-ethnic network in the district of arrival. In Column 1 we show the simple correlation, without including controls. In Column 2 we introduce the full set of fixed effects and controls, and in Columns 3 and 4 we add the interaction of the network variable with a dummy for being low skilled or high skilled, respectively. In Columns 5 and 6 we estimate the same specification as in Column 3, and the dependent variable is a dummy equal to one if an individual found a job using online advertising, namely internet or newspaper (Column 5), or if she used an employment agency (Column 6). The results show a significant positive correlation between initial network size and the likelihood that the first job in Germany was found through personal contacts, and this is shown in Column 1 of Table 3. In particular, one standard deviation increase in the co-ethnic network size at arrival corresponds to a 9.3 percentage points greater likelihood of having found the first job through contacts. This unconditional correlation becomes smaller and not significant once we include all of our controls (Column 2). Columns 3, however, shows that networks have a positive effect on finding the first job through contacts for immigrants with low levels of education, even once we introduce all the controls and fixed effects. For less-educated immigrants, an increase in the network by one standard deviation increases the probability of finding a job through contacts (rather then agencies or internet) by nine percentage points. This magnitude corresponds to around 14 percent of the average (which is 65 percent for the less-educated). On the other hand, there is no effect of a larger network on the probability that immigrants with high education find a first job via contacts (Column 4). The increased reliance on personal contacts by individuals with lower levels of education arriving in districts with larger networks corresponds to a decreased reliance on newspaper/internet and employment agencies, despite the effect being only significant for the former.[37]

6.3 Employment

Our main empirical results are illustrated in Tables 4 and 5. The terms ($\text{Netw}_{cd_0}\text{xYsm}^{3-5}$) and ($\text{Netw}_{cd_0}\text{xYsm}^{6+}$) show the coefficients of the interactions of the initial network size with a dummy that is equal to one when individual i has been in Germany between three and five years or more than five years, respectively. The dynamic effects of the initial co-ethnic network on employment are estimated using a linear probability model, where our dependent variable is a dummy for being employed in the current year.[38] Results are reported in Table 4. In Column 1 we only include the non-interacted network size measure. This column is similar to the type of "static" estimates previously presented in the literature (see e.g. Edin et al., 2003 and Damm, 2009). On average, a larger co-ethnic network at arrival significantly increases the probability of employment, if one averages immigrants who have been in the country between zero and more than six years. In all other columns we include the interactions with years since arrival and control for all demographics and pre-migration characteristics as described in Section 4. Specifically, in Columns 2 and 3 we include district-of-arrival, year-of-arrival and country-of-origin fixed effects, whereas in Column 4 and 5 we include all the two-way effects, i.e. country of origin-by-year, country of origin-by-district fixed effects, and district-by-year fixed effects. In Columns 1,2 and 4 we use the full sample, whereas in Columns 3 and 5 we restrict the analysis to the restricted sample of refugees and ethnic Germans (denoted by R), as described in Section 4. This sample is smaller but has the advantage of being subject to an arguably exogenous initial allocation. In Columns 4 and 5, the fixed effects we use are estimated on a very large external sample of immigrants taken from our administrative data.[39]

Our estimates of the dynamic effects of networks on employment are consistent with the basic predictions of our model. Social networks have significantly positive effects on the probability of

[37] It is worth noting here that the results of these regressions do not refer to the relative time spent searching with each method. Rather, they capture only which one was the successful search methods. We do not know the extent

Table 4: Network at Arrival and Employment

	Dependent Variable: Employment (dummy)				
	(1)	(2)	(3)	(4)	(5)
Netw_{cd_0} (a)	0.022**	0.077***	0.120**	0.085***	0.109**
	(0.010)	(0.016)	(0.060)	(0.016)	(0.053)
Netw_{cd_0}xYsm3-5		-0.041***	-0.011	-0.042***	-0.011
		(0.015)	(0.062)	(0.015)	(0.059)
Netw_{cd_0}xYsm6+ (b)		-0.082***	-0.063	-0.082***	-0.058
		(0.021)	(0.059)	(0.021)	(0.053)
Mean Dep. Var.	0.682		0.652		
(a)+(b)		-0.005	0.058	0.002	0.050
(a)+(b)==0 (p-value)		0.715	0.076	0.851	0.126
Clusters	238	238	137	238	136
Obs	11788	11788	4230	11754	4212
Rsq	0.151	0.156	0.219	0.155	0.224
Single FEs (predicted)	yes	yes	yes	no	no
Double FEs (predicted)	no	no	no	yes	yes
Sample	full	full	R	full	R

Note: The dependent variable is a dummy for employment, defined as one if the individual works for any extent of time during the year. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Sample R includes only those who migrated to Germany as asylum seekers/refugees, or ethnic Germans migrating when the dispersal policy was in effect. Column (1) reports the average "static" effect of network. In columns (1)-(2)-(3) we include the following predicted fixed effects, that we estimated using an external sample of immigrants: year of arrival, district of arrival, and country group. In columns (4)-(5) we include the following externally predicted fixed effects: year of arrival-district of arrival, country-year of arrival, country-district of arrival. All predicted fixed effects are obtained using a random sample of 212,653 immigrants from the IEB data, corresponding to 1,991,245 person-year observation. In addition to year, country and district fixed effects, the estimating regression includes the following regressors: education, age and its square, and gender, and the standard errors are clustered at individual level. Standard errors in parenthesis are obtained with 500 bootstrap replications clustered at district level with significance level * p<0.10, ** p<0.05, *** p<0.01.

being employed in the first two years after arrival. When we do not interact the network variables with dummies for year since arrival (Column 1), we obtain a positive estimate on the network size that implies an increase in the probability of working by 2.2 percentage points (relative to an average employment rate of 68.2 percent) for an increase in the network size by one standard deviation.

However, when we estimate the effect interacted with years-since-arrival (Columns 2 to 5), we estimate a much larger increase in probability of employment (about 7.7 percentage points) in the first 2 years. This effect is reduced to 3.6 percentage points after three to five years and virtually disappears after six years. In all specifications we include also the full set of pre-migration

to which different search methods were used, but we know which one produced the first job.

[38]Below, we discuss robustness checks where we change the exact way in which we define this dummy variable.

[39]The external estimation sample is a sample of 212,653 randomly drawn individuals with non-German nationality from the two-percent IEB registry, corresponding to 1,991,245 person-year observations. The estimated regression includes gender, education, age, age squared and the fixed effects. Standard errors are clustered at individual level. Given the very high number of missing information for the education variable, the latter is imputed using the algorithm IP1 developed by Fitzenberger et al. (2005). We use the Stata command *reghdfe* to estimate these fixed effects and then we import them into the main sample as additional regressors. For all regressions where we include predicted fixed effects, we obtain the standard errors using 500 bootstrap replications.

characteristics and the average wage in the district-year of arrival as controls.[40] The results are very stable in magnitude also in our most demanding specification, in which we perform the analysis controlling for two-way fixed effects (Column 4). We obtain an increase around eight percentage points in the probability of working following an increase in our network size measure of one standard deviation. This effect is halved after 3-5 years and disappears after six years.

We also perform the analysis on the restricted sample of asylum seekers and ethnic Germans (Columns 2 and 5). The results are qualitatively similar to those obtained in the full sample. Even in this case we use the one-way fixed effects (in Column 3) and the two-way fixed effects (in Column 5). Quantitatively the point estimates are somewhat larger in magnitude for the 0-2 years effect. For an increase in the network size by one standard deviation, the probability of being employed rises by ten percentage points in the first two years since migration in the most conservative specification. There is also weak evidence that part of the positive effect may be persistent in the long-run for this sample, although this effect (equal to five percentage points of employment in magnitude) is not statistically significant. So for this group we estimate a slightly stronger positive initial employment effect, half of which may last for more than six years. Possibly, co-ethnic networks work harder to support and promote the employment of refugees. Alternatively, refugees are in particular need of the help of their co-ethnic network to find a first job, and so its size matters more for them. Moreover, as we show in Table D.1, refugees are different from other migrants in terms of their pre-migration characteristics: general human capital, language proficiency and labor market performance. This may enhance the initial job-finding effect of co-ethnics. All results remain significant after adopting a different level of clustering. In each table we report standard errors clustered at the district level, however they are stable with clustering at individual level as well.[41]

6.4 Human Capital Investment

Differences in employment rates associated with large initial networks disappear over time. Is this just the consequence of a temporary boost to job search provided to immigrants in location with large networks? Or are there specific offsetting factors at work for individuals arriving in places with smaller co-ethnic networks? Using the rich details of the surveys with information on the full history of human capital investments of new immigrants, we analyze whether there is a systematic relationship between social networks at arrival and investment in human capital. The main results of this regressions are presented in Table 5. We find relatively strong evidence of a negative effect of the size of the initial network on the likelihood of investing in human capital during the first five years since arrival. The estimates of Column 2 show that immigrants first arriving in cities with co-ethnic networks that are larger by one standard deviation are 2.9 percentage points less likely to invest in human capital during their first two years, where the baseline for people investing in human capital in our sample is around eleven percent. The average "static" effect estimated in

[40] An alternative control for labor demand factors would be the unemployment rate by district and year. Due to administrative changes in the registry data, we don't have this information for the aggregate data at district level for the years before 1999.

[41] Results are available upon request.

Column 1 without accounting for years from arrival is still negative and significant, albeit slightly lower in magnitude, corresponding to a reduction by around two percentage points.[42] These results are consistent with our model, which predicts that individuals with larger initial co-ethnic networks are more likely to work and less likely to pursue more education in Germany. In all regressions we include controls for pre-migration characteristics, one-way fixed effects for district, country-group of birth and year, as well as average wage at the district-year level. In Column 3 we present our analysis using the restricted sample of refugees and ethnic Germans. For this group, consistent with the slightly larger employment effect at arrival for those located in districts with large co-ethnic networks, we also find a larger decline in human capital investment in the first two years, corresponding to a 4.5 percentage points reduction in the probability of investing in human capital.[43] In this case the under-investment seems stronger only in the first two years. After six years refugees and ethnic Germans in location with large networks may have more than compensated for the lower initial investments. The estimates show a positive (but not statistically significant) investment after six years for refugees in large co-ethnic network locations.[44]

In Table 6 we report results by distinguishing the type of human capital investment in school-university education (Column 1) and vocational education (Column 2). The outcome is a dummy equal to one if an immigrant has pursued during the year that type of training/education. The finding of smaller human capital investment in large network locations are largely driven by investment in school/college education, which also seems to have stronger persistence. The negative effect of the network size at arrival on investment in school/formal education is long-lasting: a one-standard-deviation increase in network size at arrival translates into a 1.1 percentage point reduction in human capital investment in the long-run. The effects on vocational training are smaller and more temporary, and imply that refugees who may not have taken advantage of vocational training right away, when arriving in large network location, can do this later. To the contrary, if they do not go to school and take advantage of that opportunity in their earlier years, in large network locations, they probably do not compensate for such missed opportunity later.

6.5 Effects by Education Group

From existing research and anecdotal evidence we know that less-educated immigrants are especially likely to locate where networks are large, as they may be helped to a larger extent by the connection with peers for job finding purposes. It is then natural to investigate, therefore, whether our cleaner identification method shows that the effects of network in job-finding rates are stronger for less-educated migrants. Table 7 (Columns 1-3) breaks down the main sample by pre-migration

[42] To the best of our knowledge, this is the first estimate in the literature of the effect of co-ethnic networks at arrival on human capital investment of immigrants. Therefore, it is hard to compare the magnitude we find to any prior one may have.

[43] We cannot add pre-estimated two-way fixed effects in this specification as in the employment regressions because we lack a comparable bigger sample with information on human capital investments for immigrants.

[44] In additional regressions, available upon request, we show that results on employment and human capital investment are robust to (and larger in magnitude when) restricting the sample to individuals who were younger than 30 at migration. This is the group for which human capital investment has the largest return.

Table 5: Network at Arrival and Investment in Human Capital

	Dep. Var.: Investment in Human Capital (dummy)		
	(1)	(2)	(3)
Netw_{cd_0} (a)	-0.017***	-0.029***	-0.045**
	(0.006)	(0.010)	(0.021)
Netw_{cd_0}xYsm3-5		-0.001	0.037*
		(0.008)	(0.019)
Netw_{cd_0}xYsm6+ (b)		0.021**	0.079***
		(0.009)	(0.022)
Mean Dep. Var.	0.049	0.049	0.060
(a)+(b)		-0.008	0.035
(a)+(b)==0 (p-value)		0.229	0.104
Clusters	238	238	137
Obs	11753	11753	4214
Rsq	0.220	0.222	0.319
Sample	full	full	R

Note: The dependent variable is a dummy for being in education, defined as one if the individual reports to be in education and is not working in the same year more than 50 percent of the days. The survey provides a full account of each year spent in education as each individual is asked retrospectively to fill a life-long calendar and to report for each year, starting from age 15 and until the current age, whether in that year she was in education. From this information we construct a yearly binary indicator for each respondent. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Sample R includes only those who migrated to Germany as asylum seekers/refugees, or ethnic Germans migrating when the dispersal policy was in effect. Standard errors in parenthesis are clustered at district level, * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Network at Arrival and Investment by type of Human Capital

Dep. Var.	School-University	Vocational
	(1)	(2)
Netw_{cd_0} (a)	-0.022***	-0.010*
	(0.008)	(0.006)
Netw_{cd_0}xYsm3-5	-0.003	0.004
	(0.006)	(0.005)
Netw_{cd_0}xYsm6+ (b)	0.010	0.014**
	(0.007)	(0.006)
Mean Dep. Var.	0.029	0.021
(a)+(b)	-0.011	0.004
(a)+(b)==0 (p-value)	0.017	0.363
Netw_{t0} (1)=(2) (p-value)	0.1597	
Netw_{t0}+Netw_{t0}xYsm6+ (1)=(2) (p-value)	0.0129	
Clusters	238	238
Obs	11753	11753
Rsq	0.221	0.082

Note: from the survey we construct for each individual a yearly binary indicator for being in education, distinguishing by type of education: school-University (column 1) or vocational education (column 2) according to the heading. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level with * p<0.10, ** p<0.05, *** p<0.01.

educational levels. The three education categories are considered following the standard German classification: "lower education", corresponding to no vocational training, "medium education", corresponding to some form of post-secondary vocational study, and "higher education" corresponding to college education and above. The first three columns estimate the effect of the network and its interactions with years since migration on employment probability, separately by education group. From those estimates we clearly see that the positive initial effect of network on employment is stronger for low- and medium-skilled immigrants, while it is close to zero and not statistically significant for highly-educated immigrants. Confirming the findings of Table 4, we find that the effect disappears or is very strongly attenuated six years after arrival. For less-educated immigrants the effect of co-ethnic network on employment is substantial. Column 1 of Table 7 shows that moving to a district with one standard deviation larger co-ethnic network at arrival corresponds to a 12.6 percentage point greater probability of being employed, relative to an average 66 percent probability. This effect largely disappears five years after migration. The point estimate remains positive but is imprecisely estimated and not statistically significantly different from zero. The network effect is still positive, albeit lower (nine percentage points for a one-standard-deviation increase) for immigrants with intermediate levels of schooling (Column 2 of Table 7). Finally the effects is not significantly different from zero for the highly skilled (Column 3).

Table 7: Network at Arrival and Employment/Human Capital Investment by Education

Dep. Var.	Employment			Human Capital		
Education	Low	Medium	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
Netw_{cd_0} (a)	0.126***	0.089*	-0.002	-0.033	-0.044***	-0.022
	(0.045)	(0.052)	(0.059)	(0.021)	(0.013)	(0.021)
Netw_{cd_0}xYsm3-5	-0.035*	-0.013	-0.014	0.005	0.013*	-0.011
	(0.020)	(0.024)	(0.026)	(0.014)	(0.007)	(0.014)
Netw_{cd_0}xYsm6+ (b)	-0.078***	-0.042	-0.031	0.028*	0.031***	0.007
	(0.027)	(0.027)	(0.038)	(0.016)	(0.009)	(0.015)
Mean Dep. Var.	0.662	0.703	0.692	0.068	0.025	0.046
(a)+(b)	0.049	0.047	-0.033	-0.005	-0.013	-0.015
(a)+(b)==0 (p-value)	0.128	0.286	0.563	0.727	0.298	0.487
Clusters	177	153	131	177	153	131
Obs	5454	3969	2365	5435	3969	2349
Rsq	0.320	0.337	0.395	0.348	0.139	0.223

Note: The dependent variable is a dummy for employment (columns 1-3), or a dummy for being in education (columns 4-6). Each column refers to a different sample according to the education obtained before migration. Low education refers to lower secondary education. Medium and high refer to upper secondary, and tertiary education, respectively. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and its square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, working experience (and its square). Standard errors in parenthesis are clustered at district level, * p<0.10, ** p<0.05, *** p<0.01.

Columns 4-6 of Table 7 investigate the relationship between network size and human capital investment for individuals with different initial education levels. Results largely mirror those for employment, as it was true for the aggregate effects. They are again stronger, with a negative sign, for individuals with lower and medium levels of education (the highest point estimates is for the medium-skilled at 4.4 percentage points). The point estimates are the lowest for highly-educated workers and they are not statistically significant. The results across educational categories need to be taken with a grain of salt, as each group has a small number of individuals, and a formal test shows that the point estimates of the relevant coefficients are not statistically different across the three education categories, either in the short- or long-run. Overall, however, our findings suggest that, one to two years after arrival, less-educated immigrant workers arriving in districts with larger co-ethnic networks are more likely to find employment. These benefits of networks also dissipate over time. This may be because individuals in location with smaller networks invest more in human capital, and in the long-run have the same probability of being employed as the group that started with a larger co-ethnic network. Short-run effects on employment probability and human capital investment are larger for immigrants with low and medium levels of education. They are those who are most likely to rely on referrals, personal interactions and word of mouth to find a job. In the first two years after arrival they experience a lower probability of going back to school by 3.3-4.4 percentage points if they arrive in a district with one standard deviation larger co-ethnic networks. This difference disappears after 4-5 years, but leaves immigrants who arrived in large co-ethnic location with lower overall investments in human capital. In the sample of high-skilled migrants the effects are smaller in magnitude and are not statistically significant.

In additional regressions we analyze whether the presence of co-ethnic networks with the same level of skill yields a stronger impact on employment of skill groups. In that case we separate immigrants by skill and we construct separate networks for each of the three education groups adding co-ethnic network of the same level of skill in the district of arrival, and then standardizing for total employment by district-year. Table D.2 in our Appendix reports the results for each skill group and each co-ethnic network-skill group. The results show that the group of low-skilled migrants benefits in its short-run employment probability mainly from locating near large low-skilled co-ethnic groups, middle-skilled immigrants benefit from locating near large "low and middle skilled" co-ethnic groups, while high-skilled employment probability is not affected by proximity to any co-ethnic skill group.

6.6 Network Effects on Wages

Does a higher short-run employment probability for immigrants in areas with large co-ethnic groups imply lower wages for these migrants? A faster matching rate to local jobs may come at the expense of the quality of the match and hence produce lower wages. Alternatively, higher match probability may simply be a result of more opportunities/better information, and hence produce larger or unaffected wages for the new immigrants. In order to address these questions, Table 8 analyzes the impact of co-ethnic network size on wages of new immigrants. The table has the same structure as the previous one which reported coefficients for other outcomes. Columns 1, 2 and 4 show coefficients estimated on the full sample; Columns 3 and 5 refer to regressions including observations only from the restricted sample of refugees and ethnic Germans. In all regressions we include all control variables as described in Section 5. In Columns 1-3 we include one-way fixed effects (district-of-arrival, year-of-arrival, and country fixed effects), whereas in Columns 4-5 we use two-way externally estimated fixed effects (district-of arrival by country, year-of-arrival by district, and year-of-arrival by country fixed effects), using the same procedure as for the employment regression.[45] Column 1 shows the average "static" result, where the network variable is not interacted with years since migration. The point estimate is negative but small and not statistically significant, implying a less-than two log percentage difference in wages for each standard deviation change in the network size. Considering the dynamic effect, estimated on the full sample in Column 2, we obtain a positive and non-significant coefficient in the short-run (0-2 years), offset by a negative marginally significant effect in the medium- (3-5 years) and long-run (5 years or more), so that the effects are not significantly different from zero. The long-run effect is obtained summing the first and the third coefficient, and its value and significance is shown in the fifth and sixth row of the Table. Overall, the network size at arrival does not seem to have a significant effect on wages, either in the full sample or in the restricted sample.

Table 8: Network at Arrival and Wages

	Dependent Variable: Daily Wage (log)				
	(1)	(2)	(3)	(4)	(5)
Netw_{cd_0} (a)	-0.019	0.013	-0.061	0.028	-0.037
	(0.025)	(0.023)	(0.061)	(0.018)	(0.054)
Netw_{cd_0}xYsm3-5		-0.032**	0.077	-0.029*	0.076
		(0.016)	(0.060)	(0.015)	(0.055)
Netw_{cd_0}xYsm6+ (b)		-0.040*	0.044	-0.034	0.039
		(0.024)	(0.064)	(0.024)	(0.055)
Mean Dep. Var.	4.014		4.021		
(a)+(b)		-0.027	-0.017	-0.005	0.002
(a)+(b)==0 (p-value)		0.356	0.690	0.817	0.953
Clusters	209	209	113	209	112
Obs	4784	4784	1740	4770	1738
Rsq	0.436	0.437	0.486	0.439	0.476
Single FEs (predicted)	yes	yes	yes	no	no
Double FEs (predicted)	no	no	no	yes	yes
Sample	full	full	R	full	R

Note: The dependent variable is the (log) real daily wage corresponding to the longest spell in the year and considering only full time spells. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), a binary indicator for marginal employment, country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Sample R includes only those who migrated to Germany as asylum seekers/refugees, or ethnic Germans migrating when the dispersal policy was in effect. In columns (1)-(3) we include the following predicted fixed effects, that we estimated using an external sample of immigrants: year of arrival, district of arrival, and country group. In columns (4)-(5) we include the following externally predicted fixed effects: year of arrival-district of arrival, country-year of arrival, country-district of arrival. All predicted fixed effects are obtained using a random sample of 206,322 immigrants from IEB data, corresponding to 1,786,319 person-year observations. In addition to year, country and district fixed effects, the estimating regression includes the following regressors: education, age and its square, and gender. Standard errors in parenthesis are obtained with 500 bootstrap replications clustered at district level with significance level * p<0.10, ** p<0.05, *** p<0.01.

Table 9 shows the results separating individuals with low, medium and high levels of education.

[45] The external estimation sample includes 206,322 randomly drawn individuals with non-German nationality from the 2 percent IEB registry, corresponding to 1,786,319 person-year observations. The estimated regression includes gender, education, age, and age squared as controls, alongside the two-way fixed effects. Standard errors are clustered at the individual level. We impute the education variable as described above. For all regressions where we include predicted fixed effects, we obtain the standard errors using 500 bootstrap replications.

Here we only show the results with two-way fixed effects. There is suggestive evidence of a negative (albeit imprecisely estimated) medium- and long-term effect for the lower educated group, but the effect is not large (two to three log points) and not statistically significant. Notice that the last two lines of Table 10 show the estimates of the long-run effects, adding the relevant coefficients in the regression, and its p-value.[46] As described above, under some additional assumptions on the wage distribution of the two modes of job-finding our simple model delivers a positive relation between initial wage and network size in the short-run and, in the long-run (second period), a positive relation up to a certain level, with a discrete negative jump at that level, when individuals decide not to invest in human capital. Hence the regression results relative to those from the full sample seem consistent with the idea of a somewhat negative long-run impact of large networks due to under-investment in human capital. A slightly richer explanation, proposed by Bentolila et al. (2010), is that there is matching specific heterogeneity in productivity across the formal and informal channels. In particular, finding a job through the informal channel may be associated with a penalty due to imperfect matching. However, our results on the restricted sample and those with two-way fixed effects show much smaller effects in the long-run, so that "no wage effects" seems the more likely interpretation of the findings.

In a more general setup, one may expect that ethnic networks of immigrants may be particularly good in generating referrals for jobs that do not require a high level of formal education and are in specific labor market niches. These jobs may be more easily signaled to co-ethnic new arrivals, but they may also be an imperfect match for the specific abilities of a new immigrant and have low potential of generating a professional career, similarly to Bentolila et al. (2010). We investigate this mechanism by analyzing whether the larger probability of employment associated with a larger size of co-ethnic networks is also accompanied by a larger measure of job "mismatch". To do this, we construct a measure of job mismatch from the information in the survey. Individuals are asked about the type of education required in their current occupation, and this information is compared to the education that they effectively have. Individuals are classified as overqualified when the education level required is lower than their level of education. We use a dummy to indicate whether individuals are overqualified for their current jobs. In Table D.3 in the Appendix we find suggestive evidence that less-skilled individuals have a higher likelihood of being overqualified in the current job, relative to high-skilled individuals, when arriving in districts with larger co-ethnic networks. For low-skilled individuals, the increase in the initial network size by one standard deviation corresponds to a nine-percentage-point-higher likelihood of being overqualified for the current job with respect to highly-skilled migrants, and to a six-percentage-point-higher likelihood in absolute terms.

[46] The literature provides mixed evidence about the effect of social networks on wages also for the general population. In the seminal work of Granovetter (1973), the quality of the match depends on how close the person is to the social contact. More recently, Loury (2006) finds that the wage effect varies according to the type of contacts, whereas Pellizzari (2010) shows that the positive effect on wages of finding a job through the informal channel rises when the efficiency of the matching process in the formal channel increases as firms become more selective when hiring through the informal channel. Looking at the effect of co-ethnic networks on earnings, Damm (2009) finds a substantial positive effect, equivalent to 18 percent, irrespective of the skill level.

Table 9: Network at Arrival and Wages by Education

	Dependent Variable: Daily Wage (log)		
Education	Low	Medium	High
	(1)	(2)	(3)
Netw_{cd_0} (a)	0.037	0.020	0.010
	(0.030)	(0.029)	(0.034)
Netw_{cd_0}xYsm3-5	-0.058**	-0.027	0.036
	(0.027)	(0.038)	(0.038)
Netw_{cd_0}xYsm6+ (b)	-0.060	0.001	-0.024
	(0.042)	(0.037)	(0.044)
Clusters	140	121	95
Obs	2121	1721	928
Rsq	0.419	0.502	0.469
Mean Dep. Var.	3.967	3.999	4.157
(a)+(b)	-0.023	0.020	-0.014
(a)+(b)==0 (p-value)	0.446	0.571	0.783
Sample	full	full	full

Note: The dependent variable is the (log) real daily wage corresponding to the longest spell in the year and considering only full time spells. Each column refers to a different sample according to the education obtained before migration. Low education refers to lower secondary education. Medium and high refer to upper secondary, and tertiary education, respectively. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), a binary indicator for marginal employment, country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, working experience (and its square). In all columns we include the following externally predicted fixed effects: year of arrival-district of arrival, country-year of arrival, country-district of arrival. All predicted fixed effects are obtained using a random sample of 206,322 immigrants from IEB data, corresponding to 1,786,319 person-year observations. In addition to year, country and district fixed effects, the estimating regression includes the following regressors: education, age and its square, and gender. Standard errors in parenthesis are clustered at district level, * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

6.7 Placebo Exercise and Robustness Checks

In our baseline specification discussed above we calculate network size as the number of employed individuals from the same country group in the district of arrival divided by total district employment. A possible concern with our findings is that they may be in part driven by labor demand conditions for immigrants in a specific district at a particular point in time. Some districts might have labor market conditions that are favorable to immigrants, and these conditions might be persistently correlated with the size of ethnic communities. While in the main analysis the inclusion of a district-time effect (two-way fixed effects) should reduce this concern substantially, we also inquire further into this potential issue here. In Table 10 we perform a placebo-type analysis where we investigate the specific role of co-ethnic networks, as opposed to the effect of the overall share of immigrants in a district. In Columns 2 and 4 of Table 10, we use, as explanatory variable, (Netw_{cd_0}) the number of all immigrants (excluding co-nationals) as share of the employment of a district. In Columns 1 and 3 we use the standard definition of co-ethnic networks used in the rest of the paper. Columns 1 and 2 show the estimates of the network variable and its interactions when using employment probability as the dependent variable: Column 1 reports the baseline estimates and Column 2 shows the estimates using the share of foreign born excluding the co-nationals in the same district. The estimates of Column 2 are not significantly different from zero, and are much smaller in magnitude and even of the opposite sign. This is consistent with the view that co-ethnic networks, specifically, and *not* the generic presence of immigrants, are indeed the determinants of the employment effect we find in our baseline regression. The results presented are also consistent with the idea that the baseline estimates are not driven by generic correlations with labor market demand, proxied by a general measure of immigrant share in employment.

Table 10: Falsification Test: non co-Ethnic Network

Dependent Variable:	Employment		Human Capital	
Network	Baseline	Other	Baseline	Other
	(1)	(2)	(3)	(4)
Netw_{cd_0}	0.078***	-0.029	-0.029***	0.017
	(0.022)	(0.041)	(0.010)	(0.017)
Netw_{cd_0}xYsm3-5	-0.029*	-0.001	-0.001	0.002
	(0.015)	(0.013)	(0.008)	(0.008)
Netw_{cd_0}xYsm6+	-0.066***	-0.020	0.021**	0.001
	(0.019)	(0.015)	(0.009)	(0.010)
Clusters	238	238	238	238
Obs	11788	11788	11753	11753
Rsq	0.264	0.260	0.222	0.219

Note: The dependent variable is a binary indicator for employment (columns 1-2), and a binary indicator for being in education (columns 3-4). In column (2) and (4) the network variable is computed using all immigrants in the district of arrival excluding those from the country of origin of the individual. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and their square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level, * p<0.10, ** p<0.05, *** p<0.01.

Columns 3 and 4 perform an equivalent falsification test on the relationship between network size and human capital investment. Column 3 presents our baseline results. In Column 4 we define networks as the share of *non co-national* foreign-born in local employment. Effects in Column 4 are not significantly different from zero, smaller in magnitude, and of the opposite sign compared to baseline. The results of this exercise are consistent with the mechanism operating through co-ethnic networks and not through a more generic correlation with labor markets or with the overall share of immigrants. We find these results reassuring as they clearly support our main findings.

We produce a few additional robustness checks, reported in the Appendix. First, we check whether our results are robust to different geographical levels of aggregation. Throughout the paper we use districts as units, that seem a reasonable approximation of local labor markets. In Table D.6 in the Appendix D we perform the analysis using municipalities instead, which are smaller units (there are about 12,000 in Germany) and may capture interactions with a very local group of people. The results show that our main estimates are robust to this modification.[47]

We also test the robustness of results to different definitions of our binary employment variable. The baseline definition of employment corresponds to having at least an employment spell in the year. Our baseline definition of human capital investment is an indicator equal to one when an individual attends education/training and was not working in the same year more than 50 percent of the days. In Appendix Table D.4 we report the baseline results (Columns 1 and 5 for employment

[47]Other studies sometimes use small units when analyzing the role of networks. Bayer et al. (2008) for instance, use Census blocks, whereas Schmutte (2015) considers small neighborhoods.

and human capital, respectively) and we show that the results are robust to defining an individual as employed if she/he works at least 25, 50, or 75 percent of the year (Columns 2-4), or if we choose different cut-offs for working days in the definition of human capital (Columns 6-8). We also consider alternative length for the interval of time allowed after arrival, before the first appearance of the immigrant in the registry data. These checks reduces the number of immigrants for which we can impute the co-ethnic network at arrival with the benefit of reducing potential measurement error. Table D.7 shows that the results are robust to more restrictive imputation windows, e.g. considering only individuals within one or two years of their first appearance in the registry data, as opposed to the baseline three year period.

7 Concluding Remarks

This paper focuses on co-ethnic networks of immigrants in Germany and their role in helping the economic success of new immigrants. In particular, we investigate how the size of co-ethnic networks at arrival affects employment and human capital investments of immigrants over time, from the very first year after arrival up to six or more years after that. We frame our interpretation of the findings within a simple search model where individuals can search through a formal channel and an informal, network-based channel. In the informal channel, co-ethnic networks help individuals find employment by providing referrals. Such a model predicts an initial lower probability of employment for individuals initially placed in locations with smaller size of co-ethnic networks. Over time, however, our model predicts convergence to similar employment probability and possibly higher wages for those with smaller co-ethnic networks because they have larger incentives in investing in human capital and human capital help their chances of employment.

Our main dataset combines a recent survey of immigrants with administrative records of those individuals. This allows us to reconstruct their entire individual labor market history, beginning with information on their district-of-arrival in Germany. Our empirical evidence is consistent with the main implications of our model: individuals initially located in districts with larger co-ethnic networks are more likely to be employed soon after arrival relative to those first located in areas with small co-ethnic networks. However, they are also less likely to invest in human capital, in the form of schooling and college education, and are therefore not more likely to be employed a few years after arrival. These effects are stronger for immigrants who arrived with lower levels of education (especially those with lower secondary education).

Identifying the effect of co-ethnic networks on human capital investment of recent immigrants is a new contribution of this paper, and suggests that, while positive overall, co-ethnic networks may give a larger initial boost that attenuates over time and can cause under-investment in human capital. This is relevant when designing policies that should affect the integration and long-run success of immigrants, in general, and of refugees in specific. The benefits of a dense co-ethnic network can be only short-lived in terms of employment, and an unintended consequence of encouraging settlement in co-ethnic enclaves may be that new immigrants have lower incentives to obtain more education and training in the long-run. Previous empirical estimations of network effects for immigrants such as Edin et al. (2003) and Damm (2009) emphasized only the earnings effect and did not really develop a dynamic analysis. Those studies mostly found a positive impact of networks on earnings, and they argued that dispersal policies have high costs for immigrants, worsening their labor market outcomes. The implications from our results, however, suggest a more complex story. While in the short-run employment probability may be increased by the presence of co-ethnic networks, dynamically they may reduce human capital accumulation and lower the quality of job matches and (possibly, but not significantly in our analysis) wages. Ignoring those effects may result in overestimating the positive effects of placing refugees in locations with large co-ethnic networks. Thanks to a rich dataset, which includes several pre-migration characteristics, as well as a subsample of refugees and ethnic Germans exogenously dispersed, we contribute to better isolating genuinely causal effects of co-ethnic networks in this type of study. We find that panel estimates of immigrants' outcomes, controlling for a rich set of fixed effects and pre-migration characteristics are comparable to those obtained using the quasi-random settlement policies for refugees and ethnic Germans. This is consistent with a causal effect of co-ethnic networks on immigrants' outcomes.

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Appendices

A Analytical Discussion of our Propositions

In Section 3, we discuss three Prepositions with the three main implications of our simple model, which we compare to what we find in our empirical exercise. In this short Appendix we present a slightly more structured discussion as a way to motivate those prepositions. We first focus on Proposition 2, which concerns the effects of a changing level of n_1 on the choice of our agent between searching and human capital investment, taking h_1 as given. Here we will therefore look at the derivatives with respect to n, and then look at the effects of changing h on outcomes as a comparative statics exercise. In order to evaluate the effects of initial network size on the $t = 1$ choice between human capital investment (H_1) and searching for a job (S_1), lets us look at the first derivative of these functions with respect to n.

$$\frac{\partial H_1}{\partial n_1} = \beta \left(\frac{\partial \mathcal{S}(h')}{\partial n_1} \right) \tag{5}$$

Let us now look at the first derivative of the S_1 function with respect to n_1.

$$\frac{\partial S_1}{\partial n_1} = (1+\beta) \left(\frac{\partial \mathcal{S}(\bar{h})}{\partial n_1} \right) \tag{6}$$

In order to sign the above two derivatives we need to sign $\frac{\partial \mathcal{S}(h)}{\partial n_1}$, the next step. In order to mantain relatively compact notation, let us define:

$$\int \max\{x_i - b_u, 0\} dF_i(x_i) \equiv \mathcal{A} \tag{7}$$

$$\int \max\{x_f - b_u, 0\} dF_f(x_f) \equiv \mathcal{B} \tag{8}$$

$$\int \max\{x_i - b_u, x_f - b_u, 0\} dF_i(x_i) dF_f(x_f) \equiv \mathcal{C} \tag{9}$$

We can now write

$$\begin{aligned} \frac{\partial \mathcal{S}(h)}{\partial n} &= \frac{\partial p_i}{\partial n}(1-p_f(h))\mathcal{A} - p_f(h)\frac{\partial p_i}{\partial n}\mathcal{B} + \frac{\partial p_i}{\partial n}p_f(h)\mathcal{C} \\ &= \frac{\partial p_i}{\partial n}\left[(1-p_f(h))\mathcal{A} + p_f(h)(\mathcal{C}-\mathcal{B})\right] \end{aligned} \tag{10}$$

Assuming that these two distributions have common support, $\mathcal{C} > \mathcal{B}$ (if the common support assumption does not hold, this equation would hold as weak inequality). In other words, the value of drawing twice from two distributions that partially overlap and picking the better outcome is strictly better (in expectations) than drawing from one of those distributions only. Therefore, $\frac{\partial \mathcal{S}(h)}{\partial n} > 0$. This implies that both the value of searching and the value of human capital investment increase in n_1. The outcome from searching is positively affected by networks, which makes it more likely to find a job and increase the expected wage (because of the increased probability of drawing two offers and picking the higher one in that case).

What can help us understand how n_1 affects our individual's decision is to compare these two derivatives. We therefore compare $\frac{\partial \mathcal{S}(h')}{\partial n_1}$ with $\frac{\partial \mathcal{S}(\bar{h})}{\partial n_1}$ where $h' > \bar{h}$. Let us write the cross derivative of $\mathcal{S}$ with respect to n and h as

$$\begin{aligned}\frac{\partial^2\mathcal{S}(h)}{\partial n\partial h} &= -\frac{\partial p_i}{\partial n}\frac{\partial p_f}{\partial h}\mathcal{A} - \frac{\partial p_i}{\partial n}\frac{\partial p_f}{\partial h}\mathcal{B} + \frac{\partial p_i}{\partial n}\frac{\partial p_f}{\partial h}\mathcal{C} \\ &= \frac{\partial p_i}{\partial n}\frac{\partial p_f}{\partial h}(\mathcal{C} - \mathcal{A} - \mathcal{B})\end{aligned} \tag{11}$$

Under our assumption that both wage distributions have only non negative outcomes $\mathcal{C} < \mathcal{A} + \mathcal{B}$. This directly implies that $\frac{\partial \mathcal{S}^2(h)}{\partial n\partial h} < 0$, which in turn means that $\frac{\partial \mathcal{S}(h')}{\partial n} < \frac{\partial \mathcal{S}(\bar{h})}{\partial n}$. In words, when individuals have high levels of human capital in our setup, the marginal effect of network size on search outcomes is (positive but) smaller. Since $\beta > 0$, this means that $\frac{\partial \mathcal{S}}{\partial n} > \frac{\partial H_1}{\partial n}$. This result, together with the results on second derivative we move to next, helps us analyze the comparative statics of the choice outlined by our model intuitively and graphically.

Having established that both the value of human capital investment and the value of searching for a job are monotonically increasing in n_1 for a given level of h_1, we next look at the second derivative of the same two functions.

$$\frac{\partial^2 H_1}{\partial n^2} = \beta\left(\frac{\partial^2\mathcal{S}(h')}{\partial n^2}\right) \tag{12}$$

$$\frac{\partial^2 S_1}{\partial n^2} = (1+\beta)\left(\frac{\partial^2\mathcal{S}(\bar{h})}{\partial n^2}\right) \tag{13}$$

In order to sign these derivatives and compare them, we need to sign the terms in brackets. We evaluate that term for a generic h so that we can investigate how it is affected by the level of h.

$$\begin{aligned}\frac{\partial^2\mathcal{S}(h)}{\partial n^2} &= \frac{\partial^2 p_i}{\partial n^2}(1-p_f(h))\mathcal{A} - p_f(h)\frac{\partial^2 p_i}{\partial n^2}\mathcal{B} + \frac{\partial^2 p_i}{\partial n^2}p_f(h)\mathcal{C} \\ &= \frac{\partial^2 p_i}{\partial n^2}(1-p_f(h))\mathcal{A} + \frac{\partial^2 p_i}{\partial n^2}p_f(h)(\mathcal{C}-\mathcal{B}) < 0\end{aligned} \tag{14}$$

since $\mathcal{C} > \mathcal{B}$. So both our value functions (human capital investment and search, given initial human capital level) have negative second derivatives.

In order to accurately draw those functions and in particular to evaluate whether the single-crossing result holds, it is useful to look at whether and how the magnitude of this second derivative depends on h. It is useful to rewrite the equation above as

$$\frac{\partial^2\mathcal{S}(h)}{\partial n^2} = \frac{\partial^2 p_i}{\partial n^2}\mathcal{A} + \frac{\partial^2 p_i}{\partial n^2}\left(\mathcal{C}-\mathcal{A}-\mathcal{B}\right)p_f(h) \tag{15}$$

The first term of equation (15) above is unaffected by h. The second term is positive because $\frac{\partial^2 p_i}{\partial n^2} < 0$ and $\mathcal{C} - \mathcal{A} - \mathcal{B} < 0$. Since $\frac{\partial p_f(h)}{\partial h} > 0$, the overall derivative is increasing in h (it is less negative for larger values of h). We can now compare the second derivatives of the human capital investment and job searching functions:

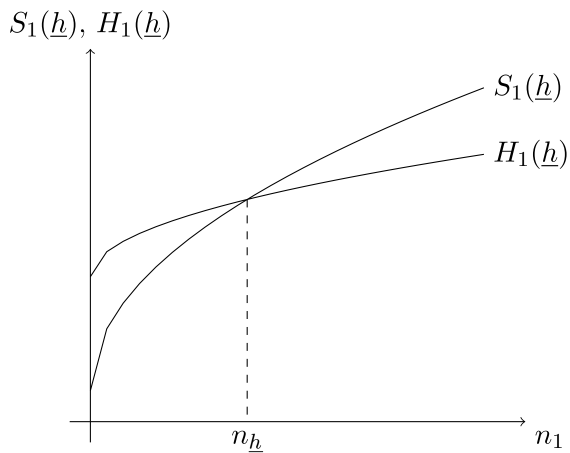
$$\begin{aligned}\frac{\partial^2 S(h')}{\partial n^2} &> \frac{\partial^2 S(\bar{h})}{\partial n^2} \\ \beta\frac{\partial^2 S(h')}{\partial n^2} &> (1+\beta)\frac{\partial^2 S(\bar{h})}{\partial n^2}\end{aligned} \tag{16}$$

where the second line follows from the fact that both second derivatives are negatives and β is positive. Therefore,

$$\frac{\partial^2 H_1}{\partial n^2} > \frac{\partial^2 S_1}{\partial n^2} \tag{17}$$

In words, we found that both the value function for human capital investment and for job search are monotonically increasing concave functions of network size (for a given level of human capital initial endowment), and that both first and second derivatives are smaller (in absolute value) for the human capital investment function. With the information above, we can draw the S_1 and the H_1 curves on a chart, which lets us evaluate the content of Propositions 1 and 2. To the left of the

Figure A.1: Value of search and of human capital investment



intersection point $n_{\underline{h}}$ in Figure A.1 the individual will decide to invest in human capital, because it give higher utility in expectations. To the right of $n_{\underline{h}}$ (i.e. for larger size of initial social network) she will decide to search for a job. Figure A.1 is drawn for a certain human capital $\underline{h}$. However, the relative position of the two curves depend on the level of initial human capital and on other parameters. For example, under certain parameter configurations there may be a corner solution where h is sufficiently high that the the value of search is higher than the value of human capital investment even on the vertical axis. In this case, the agent will decide to look for a job at $t = 1$ for all levels of n_1. From Figure A.1 above we can also investigate the effects of changing h_1 on the equilibrium values. Since each curve on the chart above is drawn for a certain level of h, curves will shift as we let h vary. Symmetrically to our analysis for n above, it is very easy to show that

$$\frac{\partial S_1}{\partial h} > \frac{\partial H_1}{\partial h} \tag{18}$$

while both derivatives are positive, the S_1 curve reacts more strongly than the H_1 curve to changes in h. Looking back at Figure A.1, as we increase h the equilibrium value of n that will make our individual indifferent between searching for a job and acquiring human capital at $t = 1$ will be lower. If we compare two individuals with the same social network but with different levels of initial human capital, the individual with higher initial human capital is more likely to start looking for a job earlier. Based on equation (11) above, at higher levels of h the effect of network on both curves is smaller is magnitude, and therefore equilibrium values respond less to changes in n at higher levels of h, which is what Proposition 3 states.

B Institutional Background: Dispersal Policies of Asylum Seekers and Ethnic Germans

The allocation of asylum seekers is regulated at Federal level by the Asylum Procedure Act (*Asylverfahrensgesetz*). According to the law asylum seekers are allocated first to initial reception facilities, the nearest in the Federal State of arrival and have no freedom to move. The allocation to a reception facility is determined by its current capacity. Each Federal State is assigned a defined quota (*Königsteiner Schlüssel*), which is calculated every year according to the tax receipts and population of the State. Consideration is also given to whether the branch office of the Federal Office responsible for the reception center deals with the asylum seeker's home country. After the first period in reception facilities, which can last up to a maximum of six months or until the application is processed, the asylum seekers are placed in a district within the state of the first allocation. The state authorities decide whether to place them in collective accommodations, or whether to grant the applicant a permit to take an apartment. This discretionary decision must take into account both the public interest and the asylum seeker's personal concerns. The residence obligation ends as soon as the Federal Office grants asylum or refugee status. In special cases, for example in cases of family reunification, asylum seekers may also be assigned to a different reception center at their own request. The average duration of the application procedure was around two years as of 2010.

In case of ethnic Germans the allocation was regulated by the so-called *Wohnortzuweisungsgesetz* (Residence Allocation Act). Starting from July 1989, ethnic Germans were granted a German visa upon application, and registered with a central authority. Those without a job (which comprised the vast majority of them) were distributed to one of the 16 Federal States according to pre-specified state quotas, based on the (*Königsteiner Schlüssel*), used also for distributing asylum seekers. The further allocation to districts was regulated within each Federal State, according to a state-specific allocation key that was based on the relative population share of each district. In some cases, the applicant could ask to join the relatives already living in the country. However, since the constraint of residence restriction turned out not to be enforced, a new law was passed in March 1996, which made the restriction stricter. Those who moved from the assigned district lost access to welfare benefits. The law was then abolished in December 2009. In our analysis we select only ethnic Germans entering Germany between 1996 and 2009, when the restriction on residence was stricter (see also Glitz, 2012 for a detailed description of the institutional background).

C Survey Data linked to Administrative records

The IAB-SOEP Migration Sample is a recent longitudinal survey of individuals with migration background who live in Germany. The survey is carried out jointly by the Institute for Employment Research (IAB) and the German Socio Economic Panel (GSOEP). The survey has a panel structure (first wave was carried out in 2013; second wave in 2014; third wave in 2015). The starting sample consisted of around 5,000 individuals. The head of each household was drawn from the German Social Security Archive (IEB), which implies that heads of households are individuals who have been at least once part of the labor force, registered as unemployed, or benefits recipient. All family members were also interviewed. A subsample of the original survey sample has been linked to the social security data (IEB), using the personal identifier for the household's heads and a matching procedure based on full name, birth date, gender, and address for the family members. At the end of the questionnaire, due to data protection, respondents are asked to give their consent for the record linkage. The overall approval rate amounts to around 50 percent and for the third wave only the heads of household have been linked. The final linked sample consists of 2,606 individuals: 1,992 from the first wave, 48 from the second wave, and 566 from the third wave. Our sample consists of 1,148 individuals: 772 from the first wave, 12 from the second wave, and 363 from the third wave. This final sample is obtained excluding second generation migrants, those with missing information in the variables of interest, those entering as students or with a job offer, or those who reported to be self-employed or civil-servant in the first job in Germany or at the time of the survey or who had income from self-employment the year previuos to the survey.

D Additional Tables

Table D.1: Comparing Refugees and non Refugees in Survey Data (share)

	Non Refugees	Refugees	P-value
Pre migration Employment	0.562	0.435	0.000
Pre Migration Edu: Low	0.547	0.717	0.000
Pre Migration Edu: Medium	0.236	0.146	0.000
Pre Migration Edu: High	0.217	0.137	0.000
Pre migration Language: Good	0.130	0.057	0.000

Source: IAB-SOEP Migration Sample (wave I-III). P-value: significant difference between two samples.

Table D.2: Network Skill and Own Skill

Dependent Variable	Employment (dummy)								
Network Skill		Low			Medium			High	
Own Skill	Low	Medium	High	Low	Medium	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Netw_{cd_0}	0.135***	0.084	0.025	0.094*	0.065	-0.000	0.013	0.009	0.003
	(0.037)	(0.074)	(0.082)	(0.051)	(0.043)	(0.044)	(0.027)	(0.061)	(0.021)
Netw_{cd_0}xYsm3-5	-0.037*	-0.009	-0.031	-0.027	-0.006	0.001	-0.007	-0.034	-0.017
	(0.020)	(0.027)	(0.039)	(0.021)	(0.021)	(0.023)	(0.024)	(0.035)	(0.015)
Netw_{cd_0}xYsm6+	-0.080***	-0.043	-0.053	-0.061**	-0.037	-0.014	-0.042	-0.085	-0.023
	(0.026)	(0.030)	(0.048)	(0.028)	(0.026)	(0.033)	(0.028)	(0.075)	(0.030)
Clusters	177	153	131	177	153	131	177	153	131
Obs	5454	3969	2365	5454	3969	2365	5454	3969	2365
Rsq	0.321	0.336	0.395	0.317	0.337	0.395	0.313	0.335	0.395

Note: the dependent variable is a binary indicator for employment. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and its square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level with * p<0.10, ** p<0.05, *** p<0.01.

Table D.3: Network at Arrival and Overqualification in the Current Job

	(1)	(2)
Low Edu	-0.039	-0.040
	(0.132)	(0.127)
Med Edu	0.343**	0.338**
	(0.158)	(0.155)
Netw$_{cd_0}$ (a)	0.032	-0.028
	(0.037)	(0.052)
Netw$_{cd_0}$xLow Edu (b)		0.093*
		(0.049)
Netw$_{cd_0}$xMedEdu		0.082
		(0.073)
Clusters	217	217
Individuals	743	743
Rsq	0.648	0.651
Mean Dep. Var.	0.402	
(a)+(b)		0.065
(a)+(b)==0 (p-value)		0.113

Note: The dependent variable is a binary indicator for being overqualified in the current job. All network variables are standardized tp have mean zero and standard deviation one. Controls: gender, age at migration (and its square), current age (and its square), current education, country of origin fixed effects, average wage in the district of arrival, year and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level, * p<0.10, ** p<0.05, *** p<0.01.

Table D.4: Different Definition of Employment and Human Capital

Dependent Variable	Employment (dummy)				Human Capital (dummy)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Netw$_{cd_0}$	0.100***	0.071***	0.062***	0.074***	-0.028**	-0.032***	-0.029***	-0.032***
	(0.019)	(0.021)	(0.020)	(0.021)	(0.013)	(0.008)	(0.010)	(0.011)
Netw$_{cd_0}$xYsm3-5	-0.077***	-0.025	-0.017	-0.014	-0.006	0.005	-0.001	-0.002
	(0.012)	(0.017)	(0.016)	(0.016)	(0.009)	(0.007)	(0.008)	(0.008)
Netw$_{cd_0}$xYsm6+	-0.105***	-0.059***	-0.045**	-0.043**	0.022**	0.025***	0.021**	0.022**
	(0.013)	(0.018)	(0.018)	(0.017)	(0.011)	(0.008)	(0.009)	(0.009)
Clusters	238	238	238	238	238	238	238	238
Obs	10453	11788	11788	11788	11744	11757	11753	11751
Rsq	0.263	0.275	0.287	0.290	0.268	0.213	0.222	0.229
Cut-off working days (%)	0	25	50	75	0	25	50	75

Note: the dependent variable is a binary indicator for employment, and a binary indicator for being in education according to the heading. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and its square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level with * p<0.10, ** p<0.05, *** p<0.01.

Table D.5: Employment and Human Capital Investment Over Time (share)

Years since Migration	Work	In Education
0-2	0.472	0.112
3-5	0.711	0.044
6-9	0.762	0.022
10+	0.804	0.012
Total	0.682	0.049

Source: IAB-SOEP Migration Sample linked to IEB data.

Table D.6: Robustness. Network Defined at District vs Municipality Level

Network Level:	District				Municipality			
Dependent variable	Employment		Human Capital		Employment		Human Capital	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Netw_{cd_0}	0.078***	0.078***	-0.029***	-0.029***	0.048**	0.048**	-0.019*	-0.019*
	(0.020)	(0.022)	(0.010)	(0.010)	(0.019)	(0.021)	(0.010)	(0.011)
Netw_{cd_0}xYsm3-5	-0.029**	-0.029*	-0.001	-0.001	-0.020	-0.020	-0.005	-0.005
	(0.013)	(0.015)	(0.008)	(0.008)	(0.013)	(0.014)	(0.008)	(0.007)
Netw_{cd_0}xYsm6+	-0.066***	-0.066***	0.021**	0.021**	-0.068***	-0.068***	0.019**	0.019**
	(0.014)	(0.019)	(0.009)	(0.009)	(0.014)	(0.016)	(0.008)	(0.008)
Clusters	1148	238	1148	238	1148	435	1148	435
Obs	11788	11788	11753	11753	11788	11788	11753	11753
Rsq	0.264	0.264	0.222	0.222	0.311	0.311	0.249	0.249
Cluster	I	D	I	D	I	M	I	M

Note: the dependent variable is a binary indicator for employment, and a binary indicator for being in education according to the heading. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and its square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects (columns 1-4), or municipality fixed effects (columns 5-8). Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis, are clustered as denoted in the Table. "I" denotes the individual level, and "D" denotes the district level, "M" denotes municipality level, with * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table D.7: Network, Employment and Investment in Human Capital. District Imputation at different lags

Dep. Variable	Employment (dummy)			Human Capital (dummy)		
Imputation	t-1	t-2	t-3 (Baseline)	t-1	t-2	t-3 (Baseline)
Netw_{cd_0}	0.085***	0.091***	0.078***	-0.023**	-0.019*	-0.029***
	(0.023)	(0.023)	(0.022)	(0.010)	(0.011)	(0.010)
Netw_{cd_0}xYsm3-5	-0.052***	-0.041***	-0.029*	-0.000	-0.007	-0.001
	(0.015)	(0.015)	(0.015)	(0.007)	(0.008)	(0.008)
Netw_{cd_0}xYsm6+	-0.087***	-0.074***	-0.066***	0.018**	0.014	0.021**
	(0.019)	(0.019)	(0.019)	(0.008)	(0.009)	(0.009)
Clusters	221	233	238	221	233	238
Obs	9459	10890	11788	9424	10855	11753
Rsq	0.257	0.258	0.264	0.172	0.206	0.222

Note: the dependent variable is a binary indicator for employment, and a binary indicator for being in education according to the heading. All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age and age at migration (and its square), country of origin fixed effects, year at migration fixed effects, average wage in the district of arrival, and district at migration fixed effects. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Standard errors in parenthesis are clustered at district level with * $p<0.10$, ** $p<0.05$, *** $p<0.01$.