Dynamic Effects of Co-Ethnic Networks on Immigrants' Economic Success *

Short title: *Co-Ethnic Networks and Immigrant Success*

Michele Battisti[†] Giovanni Peri [‡] Agnese Romiti[§]

Abstract

This paper investigates how co-ethnic networks affect the economic success of immigrants. Using longitudinal data of immigrants in Germany and including a large set of fixed effects and pre-migration controls to address the possible endogeneity of initial location, we find that immigrants in districts with larger co-ethnic networks are more likely to be employed soon after arrival. This advantage fades after four years, as migrants located in places with smaller co-ethnic networks catch up due to greater human capital investments. These effects appear stronger for lower-skilled immigrants, as well as for refugees and Ethnic Germans.

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[†]University of Glasgow, CESifo, IZA and CReAM. E-mail: michele.battisti@gla.ac.uk.

[‡]University of California, Davis, CESifo, NBER and IZA. E-mail: gperi@ucdavis.edu.

[§]University of Strathclyde-Glasgow and IZA. E-mail: agnese.romiti@strath.ac.uk.

1 Introduction

Labour market success is a critical component of the economic integration of new immigrants in host countries. What is the role of settled immigrants from the same country of origin, i.e. the co-ethnic network, in helping or hurting integration of newcomers into local labour markets? Do new immigrants benefit from this network when looking for their first job? Or does this network divert them towards available but lower-quality jobs at the expense of additional human capital accumulation? How do these effects differ between the short run and the long run?

In order to understand how employment and human capital investment of newcomers may be linked, we develop a simple search model in which workers may receive job offers through a formal search channel (e.g. replying to a job ad or submitting job applications) and through a network-based channel (e.g. word-of-mouth through friends, family, acquaintances, and other personal contacts). The frequency of opportunities through the formal search channel depends on an individual's human capital, as more-educated people attract more job offers and/or navigate the search better. On the other hand, the frequency of opportunities through the network channel depends on the size of the local co-ethnic network. Our simple model delivers two key predictions. First, larger local co-ethnic networks have a positive effect on the probability of individuals finding employment in the short run. Second, under some conditions larger networks discourage investments in general human capital. This implies that, soon after arrival, immigrants are more likely to be employed if they "land" in a location with large co-ethnic network. However, over time those who land in locations with smaller networks will catch up and converge to similar (or potentially higher) employment rates. The closing of the employment gap is due to higher human capital investment among new immigrants in markets with small initial co-ethnic networks. These investments offset the initial job-finding advantage from larger networks: the short-run impact of co-ethnic networks on employment probability may only be temporary and offset by less human capital accumulation.

We then test these predictions on data from Germany. Estimating the causal effect of the size of the co-ethnic network on immigrant outcomes is challenging. The main reason is that the presence of co-ethnic migrants in a location may generate selection of newcomers into that location. Immigrant selection across locations is likely to be correlated with characteristics affecting the economic success of immigrants. This would bias OLS estimates of the effect of local co-ethnic networks on immigrants' success. While we focus on the network of co-ethnic individuals in the location of first arrival, selection of immigrants to their initial locations would result in the comparison of individuals who are systematically different, generating spurious results. In principle, the identification of the causal effects of network size could be achieved by eliminating omitted variable bias on post-migration outcomes controlling for a very large set of location and individual characteristics, including those producing the selection. In particular, one should control for characteristics measured before migration which are potentially correlated with the initial choice. Potential selection on unobservables constitutes a limitation of this method.

An alternative avenue to identify causality, followed by Edin et al. (2003), Damm (2009) and Beaman (2012), takes advantage of the fact that dispersal policies applied to refugees often distribute individuals across locations independently of most of their characteristics, rather than allowing them to choose their location. This generates quasi-experimental variation across initial locations, and hence a lack of correlation between refugees' characteristics and sizes of their co-ethnic networks. This variation can then be used to identify a causal effect on later outcomes. While this type of identification is credible, it suffers from an external validity issue. The only groups subject to this policy are refugees and they are often different from the rest of the immigrant population in terms of skills, recent experience and other characteristics. As a consequence, co-ethnic network effects on refugees' outcomes may not be representative of those on other immigrants.

In this paper we use both methods described above. We begin by using survey data on recent immigrants to Germany linked to individual administrative records from the German social security archive. These data have the unique feature, relative to previous studies, of including rich information on individual pre-migration characteristics, which we include in our analysis, as well as detailed post-migration information on initial location, working history, schooling and training acquired in Germany. Including pre-migration characteristics can substantially reduce omitted variable bias. Moreover, we can test how these pre-migration characteristics are correlated with the size of the initial co-ethnic network, and hence determine the type and severity of selection along the dimensions we observe (and the importance of controlling for them). Specifically, as our data include immigrant arrivals from many different countries into different German districts in different years, the co-ethnic network size varies at the country-district-year level. This allows us to include a large set of fixed effects in our analysis, absorbing all the systematic effects of ethnicity, location-of-arrival, and year-of-arrival on outcomes, allowing identification of the effects only on more idiosyncratic variation. In our most demanding specification, we can control for country-year fixed effects by absorbing all the common traits from specific national cohorts of immigrants, for district-year fixed effects by absorbing local economic conditions at arrival, and for arrival district-country of origin fixed effects by absorbing specific bilateral features associated with channels of migration.

Additionally, we can restrict our analysis to those individuals who were subject to dispersal policies, i.e. refugees and Ethnic Germans (*Aussiedler*).[1] Estimates for this group should be close to causal due to the quasi-randomness of the dispersal. We can also test the external validity of these estimates by checking whether they are similar to those estimated when including the large set of pre-migration controls in the full sample of immigrants. Similar estimates for the two groups would suggest that our panel analysis with a rich set of pre-migration controls satisfactorily addresses the issues of selection/omitted variable bias. Differences in these estimates would provide a quantification of the possible bias in the full sample estimates, or of the heterogeneity of the effects between refugees and other immigrants.

Our main empirical findings support the key predictions of our simple model. First, we find that immigrants arriving in districts with larger co-ethnic networks are significantly more likely to find employment within three years after arrival. Second, we find that this advantage fades away over time and disappears after around four to six years. Third, the likelihood that immigrants spend time in training/schooling/education in the first three years after migration decreases with the size of co-ethnic network upon arrival. As investments in human capital improve employment opportunities, the initial advantage in employment probability fades away over time. We also find that immigrants with smaller initial co-ethnic networks are less likely to find their jobs through referrals. All of these effects are stronger for immigrants with lower levels of education. For immigrants with tertiary education, the size of the initial network does not seem to affect economic outcomes.

Restricting our analysis to the sample of refugees and Ethnic Germans subject to dispersal policies, we find estimates of the effects on employment and on human capital investments which are similar (sometimes a bit larger) to those obtained from least squares panel estimation with the rich set of fixed effects and pre-migration controls. This suggests that fixed effects and pre-migration

[1] Ethnic Germans are descendants of German citizens that had resided in areas formerly part of Germany until the end of World War II, when forced resettlement across Europe made many of them refugees. Most of them were from Eastern European countries and the former Soviet Union.

controls are largely effective in addressing omitted variable bias. The slightly larger estimates for refugees suggest that either there is still some negative bias in the estimates for the full sample, or that refugees experience a particularly strong network effect both increasing their employment and decreasing their schooling/training in the three years after arrival.

An additional innovation of this paper, relative to much of the existing literature, is that we can separately analyse short- and long-run outcomes and choices after migration. The distinction between short-run and long-run effects has not received much attention in this literature (with the notable exception of Edin et al. 2003, discussed in details below), due to the lack of genuinely longitudinal data. Moreover the analysis of human capital investment was also neglected due to a lack of data on training activities and school attendance by adult immigrants in the host country. Framing our analysis in dynamic terms and extending it to several years after arrival also makes clear that it is only possible to legitimately identify the causal impact of the *initial* (arrival) network size on subsequent outcomes. As individuals stay in the destination country and choose to relocate and move, the contemporaneous network size depends on their choices and therefore is co-determined with the outcomes of interest (employment, education). Therefore in our analysis the contemporary size of the co-ethnic network is not controlled for, as it is likely to be an endogenous variable, and the mobility choice after arrival is considered as an additional outcome. A third and perhaps smaller contribution is to explore some channels through which the co-ethnic network affects immigrants' employment. In our survey data, we have direct information on the channel through which new immigrants find jobs. Hence, we can directly test the impact of a larger network on the probability of finding a job via personal contacts.[2]

The first paper to credibly estimate the causal effect of initial network on labour market outcomes of immigrants (specifically on earnings) was Edin et al. (2003). Using data from Sweden, the authors use exogenous variation in the *initial* size of the co-ethnic network (which they classify as an enclave if it is larger than a certain threshold) due to a refugee dispersal policy. They then use it to instrument the *contemporary* size of the co-ethnic network, acknowledging the endogenous nature of the latter due to selective mobility. That study focuses on the impact eight years after immigration and on (log) earnings outcomes. Edin et al. (2003) find a non-significant average effect

[2] A rare study analysing 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.

of co-ethnic enclave on log earnings of refugees, consistent with our findings where no significant effect is found on either wages or employment after six years. When splitting the sample they find no effects on people with high education levels and borderline-significant effects on people with less than ten years of education. Again, this is consistent with what we find. Most of their paper is focused on the effect at a point in time, namely eight years after migration. Table 5 of Edin et al. (2003), however, shows the effects from two to eight years since migration, hence providing the only other analysis we are aware of which looks at some labour market dynamics. The estimates are rather noisy and mostly negative and non-significant. In particular, they do not show a short-run positive effect of enclave. Damm (2009) implements an identification strategy and empirical analysis that are quite similar to Edin et al. (2003) using Danish data, and focuses only on the effect seven years after arrival. Relative to these papers we do a large number of new things. First, we analyse employment probability and wages separately. Second, we are more systematic in analysing the trade-off between employment and human capital investment among new immigrants.[3] Third, we focus on the dynamic analysis looking at the evolution of employment, wages, and human capital in the short and long run. Fourth, we analyse the potential role of learning the local language on outcomes. Finally, and importantly, similarly to Edin et al. (2003) and Damm (2009) we compare the estimates obtained using the random dispersal approach with a saturated panel analysis.

More broadly, this paper contributes to a large literature estimating the effects of co-ethnic networks on new immigrants' labour market outcomes. Seminal works in this field include Cutler and Glaeser (1997), Bertrand et al. (2000), and Dustmann and Preston (2001). These papers analyse partial correlations and include several local controls and some individual controls at the time of migration, such as the schooling level of immigrants. They do not include any pre-migration individual controls, as those variables are not included in administrative datasets. They do not address, therefore, the problems of omitted variable bias generated by selection of initial location based on omitted (unobserved) individual characteristics, correlated with labour market outcomes. An important related contribution is Munshi (2003) who 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

[3] Investments in schooling and education are mentioned in those studies as possible channels through which co-ethnic networks have an effect but have not been studied specifically because of data limitations.

in high-wage occupations. Xie and Gough (2011) analyse the role of ethnic enclaves on labour market outcomes in the US, and find no evidence of a positive effect on earnings of new immigrants. However, the analysis is mainly based on correlations.[4]

This paper is also related to the literature on co-ethnic networks and job finding/job performance. Dustmann et al. (2016) look at the role of referrals on employment outcomes at the firm level. The authors 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. Similarly, Patacchini and Zenou (2012) analyse 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. More generally, past research has analysed the effects of networks on job search and labour market outcomes of workers. Important theoretical contributions to the modeling of social networks and their effects on labour 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 labour 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 labour market. Goel and Lang (2019) 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 found through the network. Galenianos (2013, 2014) develop models where network and formal markets coexist and different individuals use either depending on relative costs and benefits. Several of the above papers frame networks as an alternative to search in the general labour market, as we do. The network provides an advantage in the probability of a match, but it may be limited by the specificity and cost of referrals. Zaharieva (2015) discusses how social networks and referrals may affect employment, earnings, and welfare in a search and matching model with on-the-job search.

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

[4] Using Danish data, Bennett et al. (2015) look at the role of attitudes as well as networks on educational attainments of migrant teenagers. Åslund et al. (2011) analyse the role of neighbourhood characteristics on the school performance of immigrant children, using data from an exogenous refugee policy in Sweden.

2 A Simple Theoretical Framework

Our framework draws on Montgomery (1991) and Goel and Lang (2019). Its main goal is to illustrate the trade-off between search and human capital investments in the presence of social networks. 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). She is initially unemployed and has an exogenous level of human capital, h_t, representing a set of skills as they are valued in the host country labour market. h_1 is determined by its pre-migration level and its transferability. The size of the co-ethnic network at the initial location is denoted by n_1 so that specific realizations of h and n are denoted as $\bar{h}$ and $\bar{n}$. Following Goel and Lang (2019), there are two sources of job offers.[5] First, there is a certain probability that the worker receives an offer through the *formal channel*.[6] 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 does not depend on the size of the local network. The individual may also receive an offer from the co-ethnic *network-based channel* (or network channel) with a probability p_i. This depends positively on the size of the co-ethnic network, so that $\partial p_i(n)/\partial n > 0$ 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 $\partial^2 p_f(h)/\partial h^2 < 0$.[7]

At the beginning of each period, the worker decides whether to search for a job or to invest in human capital and 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. Draws from the two channels are independent of one another and the two wage offer distributions can be different but have overlapping support.[8] For convenience, we assume that those distributions do not change between period 1 and period 2. We denote the common cumulative distribution of wage offers obtained in the formal channel by $F_f(w)$. 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 investment would be $\bar{h}' > \bar{h}$. We assume that $\bar{h}' = \bar{h} + A$,

[5] A more general model is that of van den Berg and van der Klaauw (2006), where search intensity is endogenous. For simplicity, we do not model search intensity.

[6] Examples of "formal channel" offers are those obtained in response to applications by sending resumes or from an employment agency.

[7] Since p_i and p_f are probabilities, they are bounded between zero and one. 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.

[8] This means that the highest 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.

with $A > 0$.[9] Combined with $\partial^2 p_f(h)/\partial h^2 < 0$, this implies that investing in education has larger marginal effects on labour market perspectives of individuals with low initial levels of human capital. At the beginning of period 2, an agent that has invested in human capital in period 1 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 implies a higher probability of receiving an offer from the formal channel in period 2. We assume that $b_u \geq b_h$ to allow for some costs of education.[10] Our agent values consumption only, and discounts future outcomes at rate $0 < \beta < 1$. We assume utility to be linear in consumption,[11] 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 $t = 1$.[12]

[9] This assumption is stronger than necessary. All we need is an upper bound on the correlation between initial human capital and returns to human capital investment.

[10] 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.

[11] Implicitly, we assume individuals to be endowed with one unit of time/effort in each period, which they supply to education or search/work.

[12] 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

2.1 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 $\bar{h}$ for simplicity. 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 the value of searching is $\mathcal{S}(\bar{n}, \bar{h}') > \mathcal{S}(\bar{n}, \bar{h})$. 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 her value function is

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

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. Results may be different for a risk-averse agent since returns to education are stochastic. The lower initial employment prospects are, and the higher the discount rate (β) is, the more likely it is that an agent invests in human capital.

generate human capital as well (learning by doing). As long as growth in human capital is smaller when working than when in school, the qualitative implications of our model are robust to relaxing this assumption.

2.2 Employment and Human Capital Investment

The structure described above illustrates the main trade-off faced by the agent: human capital investment increases future employment and expected wages at the cost of foregoing current expected earnings. After observing her human capital level and network size at the beginning of period 1, the individual decides whether to search for a job or to acquire human capital. The optimal decision 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 $\bar{h}$. 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.[13] For a given level of social networks, individuals with lower human capital are more likely to get education and less likely to be employed soon after arrival. See Online 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 n_1, since 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. Imagine a case in which an individual with a 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 network-based sector compared to the formal sector, and thus, for her, further human capital investment makes less of a difference.[14] 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 Online Appendix A for additional discussion.

[13] 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 analyse the two functions S_1 and H_1 in more detail below and in Online Appendix A.

[14] Corner solutions may exist: 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.

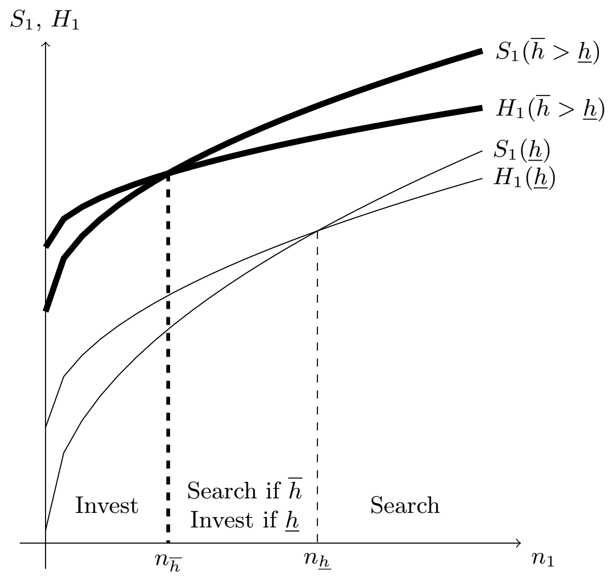
Proposition 3 *The magnitudes of the effects of networks on employment and human capital investment are lower if the individual has a higher initial human capital endowment.*

For a given network size, 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 for individuals with initially high human capital is therefore going to be smaller. While qualitative effects are unaffected, effects on employment are quantitatively larger for individuals with lower initial human capital endowments.[15]

Summarising, based on our model we expect individuals with larger initial co-ethnic networks to be more likely to find employment after arrival. However, the positive effect of a co-ethnic network on employment is expected 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 summarises 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 (at higher human capital levels expected utilities are higher due to higher probability of job offers) and flatter (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 labour 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 only individuals with lower levels of initial human capital invest in additional human capital in the first period.

[15]In order to make predictions concerning the relationship between the level of initial human capital and the probability of human capital investment, 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, then individuals with lower initial human capital are more likely to invest in its improvements. Results are different if returns to human capital are larger for individuals with larger initial stocks. This case would be closer to Regets and Duleep (1999). See Online Appendix A for additional

Figure 1: Searching for a Job and Human Capital Investment



2.3 Wages

In the paragraphs above we discussed the implications of our model for employment and human capital investment. Next, we briefly discuss effects on wages. Even if the distributions of wages from each channel (formal and network-based) 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 average realised 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, which may be restrictive. 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

discussion.

exist, at low levels of n_1 the individual acquires human capital and enter period 2 with $h_2 > h_1$. Observed wages at time $t = 2$ are locally increasing in n_1 because larger social networks increase the probability of receiving two offers. However, this effect exhibits a discontinuity: if n_1 is sufficiently high, the individual does not find it profitable to invest in human capital at $t = 1$ and wages at $t = 2$ may be lower. For changes in initial network size that are large enough to affect human capital accumulation decisions, individuals with larger networks are expected to have lower wages in the long run.[16] Because of this non-monotonicity, our model does not deliver clear predictions on the effects of networks on wages. Our empirical results on wages are also relatively imprecise and only partially aligned with the theory. They are presented and discussed in Section D of our Online Appendix.

3 Data

Our primary data source is the IAB-SOEP Migration Sample (see Brücker et al. (2017) for more details), a yearly survey of immigrants in Germany that started in 2013 and is carried out by the Institute for Employment Research (IAB) and the German Socio-Economic Panel (SOEP). In our main analysis, we use a subsample of individuals from waves 1-3 (2013-2015) that have been linked to the IEB (*Integrierten Erwerbsbiografien*), the German social security archive. Our data include all workers covered by the social security system, excluding civil servants, self-employed, and military personnel and cover the period 1975-2014. The final linked sample consists of 2,606 individuals. Our sample consists of 1,147 individuals, and it is obtained by excluding second generation migrants, those entering as students or with a job offer, as well as those who reported self-employment or civil service as the first job in Germany or at the time of the survey or who had income from self-employment the year previous to the survey,[17] and those with missing information in the variables of interest. We include foreign-born individuals aged 15-65.[18] For the individuals included in this sample we are able to observe all migration related variables and several pre-migration characteristics obtained from the survey. In addition, we can observe the entire labour market history after

[16] Figure A.2 in our Online Appendix depicts the relationship between wages in the second period and network size.

[17] This exclusion is motivated by the fact that we can use the information provided by the survey for some years (year of first employment, year previous to the survey) to make sure that missing spells in the administrative data do not correspond to spells of self-employment or civil service.

[18] The survey over-samples immigrants who arrived in Germany after 1994 and includes other individuals with a migration history in their family. See Online Appendix C for details.

migration to Germany that is available from the administrative data (IEB) and this crucially allows us to investigate both short-run and long-run effects of networks at arrival, as cannot be done in other studies. Wages are measured as the log of real daily wages of the longest spell within the year, considering only full-time spells and excluding apprenticeship and marginal employment. Human capital investment is measured by whether the person was engaged in learning activities, as reported in the survey. The survey provides a full account of each year spent in school or training as each individual is asked retrospectively to fill a life-long calendar and to report for each year, starting from age 15 and until age 65 or the current age, whether in that year she was in school/college, or in vocational education (including apprenticeship). To limit recall bias, we complement the survey information on education with the administrative data. In particular, we set the education investments in a year to zero if the person worked more than 50 percent of the time in the corresponding year.[19] Education is further split into two broad categories: vocational education including re-training and apprenticeship, and school/college education.

The variable capturing the co-ethnic network size at arrival for each immigrant is calculated, from the full registry of employees in Germany (IEB), as the number of workers from the same country-group as a share of total employment in the district-year of arrival in Germany. We aggregate immigrant origins into eight country groups: Western countries (Western Europe, North America, New Zealand, and Australia), Eastern Europe, South-Eastern Europe, Turkey, Countries from the former Soviet Union, Asia and Middle East, Africa, Central and South America.[20] The geographic units considered are 402 districts, with an average size of 69,194 workers per district and a median size of 45,725. Immigrants in our sample are distributed across 239 of those districts.

3.1 Descriptive Statistics

Table 1 shows summary statistics for the main variables in the empirical analysis. The final sample size consists of 11,771 yearly observations (slightly fewer for the human capital investment variable, due to some missing observations) for 1,147 foreign-born individuals, including refugees and Ethnic Germans, in working age (15-65 years old) and who are linked to the registry data.[21] As we do

[19] In Section 5.7, we discuss the robustness of the results to different values of this cut-off.

[20] Differently from our individual sample, which includes place of birth, the full registry only includes information on nationality that we use to identify immigrants and construct the network.

[21] For more details about the linkage please see Section C of our Online Appendix.

not have direct information on the district of arrival from the survey, we take the district of first appearance in the administrative data as capturing the place of arrival of the new immigrant. If the information of the district is not present in the administrative data in the year of arrival we use the district from the following years as long as the date of first appearance in the administrative data is within three years after the arrival year reported in the survey. Immigrants may take a while to obtain first employment or be officially registered as unemployed. Section 5.7 shows that our results are robust to alternative ways to assign districts of arrival.

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 status one year before migration. The survey data include the job search method for the first job found in Germany. The distribution across countries-of-origin shows a significant share of individuals from countries included in the former USSR. This is partly due to the survey design oversampling individuals migrating after 1994. To be sure that this group does not drive the results, we replicate the analysis excluding them. We also test that the results are robust to excluding the Western countries group, as for this group the role of the network might be less relevant. Results are robust to these modifications.[22] Our sample reflects the fact that people are relatively young when they migrate. Age at the time of migration is around 31, and in our sample the average age is 37. Note, as an 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 (65 percent among low-skilled immigrants, i.e. those with at most lower-secondary education).[23] The information on job search method is rarely available in data on labour market outcomes and we will use it more formally below.

The top panel of Table 1 reports the summary statistics for the key explanatory variable, "Network at Arrival", which measures the size of the co-ethnic network at time of arrival and the summary statistics for other time-varying individual variables. This network variable is fixed for each individual immigrant, and has an average size of 0.011 with a standard deviation of 0.014. This means that

[22] All results are available upon request.

[23] 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.

the average immigrant moved to a district where just over one percent of the working population was from her country-group of origin. The immigrant group with the highest value of the average co-ethnic network size are those from Western countries, i.e. Western Europe, North America, Australia and New Zealand (0.032) followed by Turkish immigrants (0.026), and South-Eastern European immigrants (0.019). Employment rates are 68.3 percent on average. 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. The average real daily wage earned in the sample is around 64 Euros for full-time workers. Individuals in the sample are investing in education, i.e. spending some time in school or in training, in 5 percent of the individual-year observations, distributed between vocational training (2.1 percent) and school/university (2.9 percent). Education and training are more common during the first years after arrival and the share of individual-year in education is higher during the early years of their stay in Germany: among those in Germany for three years or less, 10.2 percent were in education (of these, 4 percent were in vocational training, whereas 6.5 percent were at school or in university, see Column 2 of Table E.2).[24] This share decreased to one percent for immigrants in Germany for at least ten years. Symmetrically, employment rates increase with time since arrival. During the first three years since arrival only 52 percent of individual-year observations corresponded to employment, while after ten years this percentage raised to over 80 percent (Column 1 of Table E.2).

Our panel is unbalanced: the average number of years since migration observed is 6.97, whereas the median value is six. Around 36 percent of observations involve individuals who lived between zero and three years in Germany, 19 percent for four to six years, and 45 percent have been in Germany seven or more years. The bottom panel of Table 1 lists summary statistics of time-invariant individual characteristics mainly relative to ethnicity, country-of-origin, and pre-migration characteristics. These are obtained from the IAB-SOEP-Migration survey.

[24]Corresponding to 171 and 273 observations, respectively.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	N
	Time-Variant Variables		
Network at Arrival	0.010	0.015	11771
Employment	0.683	0.465	11771
Human capital investment	0.050	0.217	11664
Real daily wage	63.646	28.415	4682
Years since migration:0-3	0.361	0.480	11771
Years since migration:4-6	0.189	0.391	11771
Years since migration:7+	0.451	0.498	11771
Age	37.305	10.817	11771
	Time-Constant Variables		
West	0.095	0.293	1147
East Europe	0.153	0.361	1147
Turkey	0.047	0.212	1147
South-East Europe	0.221	0.415	1147
Former USSR	0.359	0.480	1147
Asia & Middle East	0.071	0.256	1147
Africa	0.042	0.200	1147
Central & South America	0.012	0.110	1147
First job found through contacts	0.597	0.491	643
	Pre-Migration Variables		
Low education	0.443	0.497	1147
Medium education	0.312	0.464	1147
High education	0.245	0.430	1147
Employment	0.699	0.459	1147
German proficiency	0.249	0.433	1147
Work experience	9.635	9.402	1147
Age	31.362	9.918	1147

Source: IAB-SOEP Migration Sample and IEB Dataset. The category "West" refers to Western Europe, North America, Australia, and New Zealand.

4 Empirical Specification and Identification

To estimate the effect of co-ethnic network at arrival on employment and human capital investment of new immigrants, we estimate the following equation:

$$Y_{icd_0t} = \alpha + \beta \mathbf{X_{it}} + \gamma \text{Netw}_{cd_0} \times \text{Ysm}_{it} + \eta \text{Ysm}_{it} + \delta_{d_0} + \psi_{t_0} + \theta_c + \epsilon_{it}, \quad (4)$$

where Y_{icd_0t} is an outcome in year t since first arrival for individual i who first arrived in district d_0 from country-group c. In our main regressions the variable Y will be, alternatively, a dummy for being employed or a dummy for attending school or training, which we call "investing in human capital". The vector $\mathbf{X_{it}}$ includes time-varying and time-invariant individual characteristics: a gender dummy, age and its square, age at migration and its square, and a set of pre-migration characteristics: education, work experience and its square, employment, and a binary indicator of German language proficiency. The survey asks respondents to report German proficiency separately in reading, speaking, and writing. Our indicator of proficiency is set equal to one if the individual reports at least sufficient proficiency in all three dimensions (each dimension takes a value between one and five, each value corresponding to "very poor", "poor", "sufficient", "good", "very good").[25] The variable Netw_{cd_0} captures the size of the co-ethnic network (previous working immigrants from the same country-group c as a share of total employment) in the district of arrival d_0.[26] This measure varies across country-groups, districts and year-of-arrival. For each individual it is fixed at the value of the year-of-arrival (which is $t = 0$). The term δ_{d_0} captures district-of-arrival fixed effects and θ_c captures 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": $(\text{Ysm}_{0-3})_{it}$, $(\text{Ysm}_{4-6})_{it}$ and $(\text{Ysm}_{7+})_{it}$. These dummies take a value of one in the year interval considered, and zero otherwise.[27] Additionally, we estimate a more detailed specification where the network variable, Netw_{cd_0} is interacted with one-year dummies, one for each year since migration. We show the estimated coefficients and confidence intervals for that specification in Figure 2 and 3.

The non-random initial location of immigrants may bias the estimates of the coefficients of interest (γ) if unobserved individual characteristics affecting employment and human capital investments are also correlated with the initial size of the co-ethnic network. Controlling for pre-migration characteristics 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

[25] 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.

[26] We consider all workers observed at the 30^{th} of June in each year.

[27] Notice that specification (4) is a panel with several observations for each individuals, one for each year in Germany. In this respect it differs from specifications estimated in Edin et al. (2003) which are cross sections, with outcomes fixed at year $t + 8$. In our estimation we cannot, however, introduce individual fixed effects, as the coefficient of interest—the ethnic network at arrival—does not vary for an individual over time.

the controls. We therefore only exploit differences in the size of initial co-ethnic network unrelated to pre-migration characteristics and occurring within-district and within country-of-origin, controlling for time-of-arrival effects. In addition, in the employment (and wage) 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 (and wages). This allows us to include a very "saturated" specification with all possible two-way fixed effects, albeit estimated on an external sample.[28] 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[29] and Ethnic Germans (*Aussiedler*) 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 Online Appendix B for details about the institutional setting). This restricted sample consists of 311 individuals, of whom about one-third are asylum seekers and two thirds Ethnic Germans.

Our identification strategy relies on assuming that, conditional on all fixed effects, the initial distribution of immigrants does not involve a sorting of their characteristics which is correlated with the size of their co-ethnic network. Evaluating whether sorting is taking place is therefore important for our analysis. As we are able to include several pre-migration individual characteristics, we can test whether they are correlated with the size of the co-ethnic networks. Then we can also test whether this correlation is reduced when we include our sets of fixed effects, which should account for local and group-specific features that may produce those correlations. We perform this exercise first on the whole sample and then on the sample of refugees. While there could still be unobservable characteristics correlated with the local network, the fact that those we do observe are not correlated with it provides an important check of our identifying assumption. If most of the omitted variable bias is eliminated by introducing the set of fixed effects or by using the restricted sample, we have two ways of producing coefficients that can be causally interpreted.

[28]We do not have an external sample with information on human capital investment for this estimation. Therefore, in the specification for human capital we can only use one-way fixed effects for initial location, time and countries.

[29]We sometimes denote this group as "refugees" for simplicity.

5 Results

5.1 A Test of Sorting

In this section we use all the variables observed before migration—namely age, education, employment, work experience, and language proficiency, to test the initial sorting of these characteristics across locations. Moreover we test whether this correlation survives the inclusion of district-of-arrival, country-of-origin, and year-of-arrival fixed effects. If most of the differences in initial location, correlated with skills, are driven by country-of-origin or specific districts, the fixed effects should absorb them. This exercise allows us to analyse whether the fixed effects absorb the sorting of abilities which can induce correlation between location and economic outcomes. While we cannot do the same check with unobserved abilities, if fixed effects control for sorting on observable skills, they may also control for non-observable ones.

To the best of our knowledge, this test of orthogonality in pre-treatment (pre-migration) characteristics of immigrants is novel to the migration literature, simply because the information about pre-migration variables is typically unavailable.[30] Table 2 presents estimates obtained by regressing the initial network size variable on all pre-migration variables of migrants into those locations, first without including any additional control (Column 1), then adding country-of-origin, year-of-arrival, and district-of-arrival fixed effects (Column 2). The network variables used throughout the regression analysis are standardized to have a mean of zero and a standard deviation of one. Significant correlations exist in Column 1, revealing in particular that large initial co-ethnic networks attract workers who were less likely to work before migration, and who were less likely to have intermediate levels of education. This reveals that locations with large co-ethnic enclaves attract people who may have a less continuous working history. Moreover, a test of joint significance rejects the null of no joint correlation. Column 2, however, shows that none of the pre-migration characteristics are correlated with the initial network size (either individually or jointly) once we condition on the fixed effects (Column 2). Notice that the estimated coefficients on each immigrant characteristic in Column 2 of Table 2 are very 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

[30] Similar tests have been used in other fields, however. For instance, a similar approach is taken in Guryan et al. (2009) to test the orthogonality between predetermined ability of a player and average ability of a player's partners participating in the same tournament.

Table 2: Test of Network Sorting

	Dependent Variable: Netw_{cd_0}			
	Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)
German proficiency	-0.004	-0.029	-0.051	0.048
	(0.064)	(0.043)	(0.047)	(0.040)
Employment	-0.195**	-0.026	-0.137	-0.160
	(0.077)	(0.055)	(0.092)	(0.126)
Work experience	-0.004	-0.004	0.001	0.002
	(0.012)	(0.008)	(0.010)	(0.008)
Work experience sq.	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Low education	-0.002	0.037	0.066	-0.060
	(0.076)	(0.051)	(0.047)	(0.072)
Medium education	-0.174**	-0.060	0.023	-0.095
	(0.070)	(0.055)	(0.043)	(0.077)
Age	0.023	-0.005	0.000	0.002
	(0.027)	(0.017)	(0.017)	(0.020)
Age sq.	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Individuals	1147	1147	310	310
R-squared	0.040	0.784	0.055	0.883
P-value (all coeff==0)	0.000	0.327	0.013	0.700
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

correlation at any significance level (p-value equal to 0.33). This reveals that the most systematic selection of immigrants on characteristics is driven by differences in network size across ethnicity and district of arrival. Once those are accounted for, the within-ethnic group, within-district variation over time shows observable skills that are uncorrelated with the density of the co-ethnic network.

We also estimate the same specifications on a restricted sample including only refugees and Ethnic Germans. This is to check whether this test is consistent with exogenous dispersal of refugees and Ethnic Germans to local areas. Results are in Columns 3 and 4 of Table 2. Consistent with our discussion on the dispersal policy in place for this group, even with no controls none of the pre-migration characteristics has a significant coefficient in explaining the size of the initial co-ethnic network for this group. Nearly all point estimates (Column 3) are lower in magnitude compared to the full sample (Column 1). Including the set of fixed effects (Column 5) leaves point estimates largely unaffected while increasing the standard errors, confirming that the characteristics of immigrants in the restricted sample were uncorrelated with destination, year and group fixed effects. The evidence shown in Table 2 is consistent with the hypothesis that, once we include the full set of fixed effects, immigrants' pre-migration characteristics are uncorrelated with initial location and hence omitted variable bias might not be too severe if those characteristics are proxies for observed and unobserved ability.

5.2 Employment

The main empirical results relative to the impact of initial co-ethnic networks on the probability of employment are shown in Table 3 and in Figure 2. The variables $\text{Netw}_{cd_0}\text{xYsm}_{0-3}$, $\text{Netw}_{cd_0}\text{xYsm}_{4-6}$ and $\text{Netw}_{cd_0}\text{xYsm}_{7+}$ denote interactions of the initial network size with dummies that equal one when individual i has been in Germany between zero and three years, four and six years, and seven or more years, respectively. The dynamic effects of the initial co-ethnic network on employment are estimated using a linear probability model, where the dependent variable is a dummy for being employed in the current year.[31] We include the full set of demographics and pre-migration characteristics and the average wage in the district-year of arrival as controls in all columns, as described in Section 3.[32] In Columns 1, 2 and 7 of Table 3 we include district-of-arrival, year-of-arrival and country-of-origin fixed effects. In Columns 3 and 8, the single fixed effects we include as controls are estimated on an external large sample of immigrants taken from administrative data. Using a much larger sample allows us to estimate the fixed effects with more precision. The external estimation sample includes 188,129 randomly drawn individuals with non-German nationality from the two-percent IEB registry, corresponding to 2,206,932 person-year observations.[33] We include as immigrants those with non-German nationality in their first observation, even when the individual acquires German nationality later on. The estimated regression includes gender, education, age, age squared as controls and the single or two-way fixed effects. Standard errors for external regressions are clustered at individual level.[34] Standard errors are clustered at district level in all

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

[32] Unemployment rate by district and year is not available prior to 1999, so we cannot use it as a control.

[33] We use the Stata command *reghdfe* to estimate these fixed effects and then we import them into the main sample as additional regressors. Dustmann et al. (2016) is another example of importing pre-estimated fixed effects from administrative data into regressions based on survey data.

[34] Given the very high number of missing values for the education variable, the latter is imputed using the algorithm IP1 developed by Fitzenberger et al. (2005).

tables reporting the results bases on the linked survey.[35] For all regressions where we include predicted fixed effects, we obtain the standard errors using 500 bootstrap replications. In Columns 4 and 9 we include all the two-way pre-estimated fixed effects, (country-of-origin-by-year-of-arrival, country-of-origin-by-district and district-by-year-of-arrival). In Columns 5 and 10 we additionally control for pre-estimated current district-year fixed effects to account for potential contemporaneous determinants of economic success at the district level. While the choice of current district is endogenous, and therefore this could be a "bad control", if mobility in response to local economic conditions is not too large then adding contemporaneous controls can be a way of controlling for exogenous evolution of economic conditions. In Column 6 we replicate the analysis of Column 5 but restricting the sample to individuals who never moved from the district-of-arrival (70 percent).[36]

While Columns (1)-(6) refer to the full sample, in Columns (7)-(10) we restrict the analysis to the sample of refugees and Ethnic Germans (i.e. the "Restricted" sample), as described in Section 4. In Column 1 we include our network size measure, capturing the effect of the arrival network averaged across years since migration.[37] On average, a larger co-ethnic network at arrival increases the probability of employment, when one averages the effects across different years after arrival. In the other columns, instead, we include the interactions of initial network with years since arrival.

[35]We performed also clustering at individual level in alternative specifications. The main results are available in Table E.8 of our Online Appendix, whereas the full set of results is available upon request.

[36]In order to estimate Columns 8-10 we first impute missing districts, either because they are unobserved in cases when they refer to spells out of the labour force or because they are unreported in the administrative archive. We impute the missing districts by carrying-forward the non-missing district until a new district is reported. We use this imputation also for estimating mobility between districts as an outcome (see the discussion in Section 5.7).

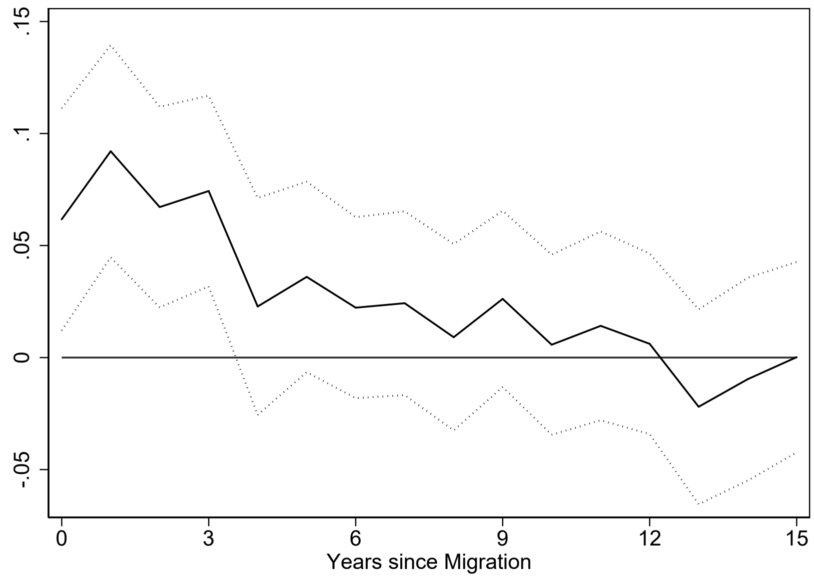
[37]This specification is not comparable to the one in Edin et al. (2003) as we are estimating the effect of network at arrival on outcomes after arrival, while they Edin et al. (2003) fix a specific period, eight years after arrival, and estimate the impact of current co-ethnic network, instrumented with initial network, on outcomes in that period.

Table 3: Network at Arrival and Employment

	Dependent Variable: Employment (dummy)									
	Full Sample					Non Movers	Restricted Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\text{Netw}_{cd_0}\text{xYsm}_{0-3}$		0.078***	0.070***	0.076***	0.076***	0.067***	0.114	0.124***	0.129***	0.128***
		(0.021)	(0.013)	(0.013)	(0.013)	(0.017)	(0.080)	(0.047)	(0.047)	(0.047)
$\text{Netw}_{cd_0}\text{xYsm}_{4-6}$		0.031	0.014	0.019	0.019	0.014	0.101	0.108***	0.114***	0.114***
		(0.020)	(0.014)	(0.013)	(0.013)	(0.017)	(0.093)	(0.039)	(0.035)	(0.035)
$\text{Netw}_{cd_0}\text{xYsm}_{7+}$		0.007	-0.012	-0.005	-0.005	-0.025	0.049	0.051	0.063*	0.062*
		(0.018)	(0.012)	(0.012)	(0.012)	(0.018)	(0.082)	(0.033)	(0.033)	(0.033)
Netw_{cd_0}	0.035**									
	(0.016)									
Observations	11771	11771	11771	11755	11750	7281	4213	4213	4213	4213
Clusters	239	239	239	239	239	161	138	138	138	138
R-squared	0.246	0.250	0.147	0.145	0.145	0.155	0.359	0.196	0.211	0.211
Mean dep var	0.683	0.683	0.683	0.683	0.683	0.658	0.654	0.654	0.654	0.654
Single FEs	yes	yes					yes			
Single FEs (predicted)			yes					yes		
Double FEs (predicted)				yes	yes	yes			yes	yes

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), average wage in the district of arrival. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Restricted sample includes only those who migrated to Germany as asylum seekers/refugees, or Ethnic Germans migrating when the dispersal policy was in effect. Single FEs refer to district at arrival, year of arrival, and country fixed effects. Double FEs refers to district at arrival-country, year-district of arrival, country-year of arrival. Columns (6) and (10) additionally include current year-district fixed effects, predicted from an external sample. Column (1) reports the average "static" effect of network. All predicted fixed effects are obtained using a random sample of 188,129 immigrants from the IEB data, corresponding to 2,206,932 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. Standard errors in parenthesis are clustered at district level in columns (1)-(2), and (7), and obtained with 500 bootstrap replications in all other columns, with significance level * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure 2: Network at Arrival and Employment



Note: The figure shows the coefficients of the yearly dummies interacted with the network variable, Netw_{cd_0} obtained from specification (4) where year since migration is expressed as yearly dummies. The dependent variable is defined as in Table 3. Solid lines refer to regression coefficients, dotted lines refer to 95% confidence intervals obtained using clustered standard errors at district level. The regression is estimated using the baseline sample as in Column 1 of Table 3. Additional controls are those used in Column 1 of Table 3: gender, age and age at migration (and their square), average wage in the district of arrival. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Single FEs: district at arrival, year of arrival, and country fixed effects. The figure is cut at year since migration equal to 15 for presentation purposes.

Several results from Table 3 are worth discussing. First, the estimates of the dynamic effects of networks on employment are consistent with the basic predictions of our model. Social networks have positive and significant effects on the probability of being employed in the first three years after arrival. When we do not interact the network variables with dummies for years-since-arrival (Column 1), we obtain a positive estimate on the network size that implies an increase in the probability of working by 3.5 percentage points (relative to an average employment rate of 68.3 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 10), we find a larger increase in probability of employment (around 7.8 percentage points in Column 2) for the first three years. This effect is reduced to around three percentage points for four to six years-since-arrival, but is not significant. The total effect is not statistically different from zero after seven years. The short-run results are stable in magnitude and remain significant in the most demanding specification, in which we perform the analysis controlling for the pre-estimated two-way fixed effects (Column 4), as well as when we additionally include the current year-district fixed effects (Column 5). Interestingly, the results are robust to controlling for mobility, as shown in Column 6. By restricting the analysis to non-movers only, the point estimates are very similar to the results obtained from the same specification on the full sample (Column 5). Figure 2 shows the point estimates and confidence intervals of a more flexible specification where we interact the network variable with yearly dummies since migration, estimating the same specification as in Column (1) of Table 3. The results are consistent with the ones reported in Table 3 using the same set of controls of Column (1).[38] The results show that an increase of one standard deviation in initial co-ethnic network size translates into an increase in employment of 9.3 percentage points in the first year after migration. This positive effect begins to shrink, becoming small and losing significance after three years since migration.

We then investigate effects for the restricted sample of asylum seekers and Ethnic Germans (Columns 7-10). Results are qualitatively similar to those for the full sample. Standard errors are larger and the reversal of the employment effects over time is somewhat attenuated. As above, we show results when we include one-way fixed effects estimated in sample (Column 7), one-way fixed effects pre-estimated externally (Column 8), two-way fixed effects pre-estimated externally (Column 9), and current district-year fixed effects pre-estimated externally (Column 10). Results are similar across these specifications.[39] Quantitatively, the point estimates are somewhat larger in magnitude for the 0-3 years effect. For a one standard deviation increase in the network size, the most conservative specification shows the probability of being employed rises by around 13 percentage points in the first three years after migration. The effect remains positive and significant in the medium-term (4-6 years after migration), slightly decreasing to 11 percentage points. We also find weak evidence that part of the positive effect may be persistent in the long-run for this sample, equal to around six percentage points of employment; thus for this group we estimate a slightly stronger positive initial employment effect, half of which may last for more than seven years.

Given the demanding specification and the fact that including fixed effects should control for selection bias (as shown in the previous section), differences in estimates between the full and the

[38]Results are very similar if we estimate the specification using pre-estimated fixed effects, as in Column 3.

[39]As shown in the test of sorting (Table 2), for the restricted sample pre-migration characteristics are uncorrelated with district, year and group fixed effects. We also test that the results for employment are robust to excluding all regressors such that for this group the co-ethnic network effect is not driven by individual characteristics or selection.

restricted sample may indicate a genuine different impact for refugees (hence imperfect external validity for this sample). Refugees may benefit significantly more from the help of their co-ethnic network to find a first job, and so the network size matters more for them. Moreover, as we show in Table E.1 in our Online Appendix, refugees are different from other migrants in terms of their pre-migration characteristics: general human capital, language proficiency, and labour market performance are lower for refugees. This is consistent with our heterogeneity results, which we present below and which show that less-skilled workers benefit more from co-ethnic networks to find their first job.

If we compare the dynamic effects estimated in this section with the only other dynamic estimates of the effect of co-ethnic networks on economic integration of immigrants in the literature, namely with Edin et al. (2003), Figure II, some similarities and few differences emerge. Their estimates are not directly comparable with ours as they analyse only (log) earnings and not employment, and as they condition on employed people, which select a rather special group. Still, they estimate a non-significant and rather noisy effect of the network up to seven years after migration, and then a negative effect. This contrasts with the initial positive estimates we find for the first years, later declining to zero. A few differences between Swedish refugees and the sample of German immigrants can explain this. First, at the time Swedish refugees were not legally allowed to work for the first two years after migration. This could substantially reduce the role of the initial network in helping to find the first job. All immigrants included in the German analysis could work since their arrival. Second, Edin et al. (2003) separate the effect of network from that of network "quality", the latter of which they measure as average income of co-ethnic migrants. When accounting for the effect of quality, their estimates of network on (log) earnings is negative in the early years but become zero and then small and positive after 4-5 years (see Figure II). Thus even in their case the short-run effect of network is attenuated in the long run. Our finding that the positive employment effect of the co-ethnic network is temporary and declines to zero, when considering the dynamic analysis, complements and qualifies the findings in Edin et al. (2003) of a positive log earning effect of co-ethnic network in the medium run, which was mainly obtained using a static approach.

Table 4: Network at Arrival and Method of Finding First Job in Germany

	Dependent Variable: Job Finding Method				
	Contacts			News-Internet	Empl. Agency
	(1)	(2)	(3)	(4)	(5)
Netw_{cd_0}	0.092***	0.057			
	(0.019)	(0.051)			
Netw_{cd_0}xLow Education			0.099*	-0.119***	0.012
			(0.054)	(0.034)	(0.046)
Netw_{cd_0}xMedium Education			-0.009	-0.060	0.063
			(0.085)	(0.056)	(0.073)
Netw_{cd_0}xHigh Education			0.013	-0.046	0.017
			(0.059)	(0.061)	(0.059)
Observations	643	643	643	643	643
Clusters		192	192	192	192
R-squared	0.031	0.442	0.446	0.420	0.414
Mean dependent variable	0.597	0.597	0.597	0.179	0.213

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 4) or employment agency (Column 5). All network variables are standardized: the relevant coefficient corresponds to the effect of an increase by one standard deviation. Controls: gender, age at first job (and its square), age at migration (and its square), average wage in the district of arrival, country of origin fixed effects, year and district at arrival 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.

5.3 Job Search Methods

Large co-ethnic networks can boost employment of new immigrants by referring them to jobs. This channel is rarely directly tested in the literature because of data limitations.[40] Our data include information on the way individuals found their first job in Germany. Table 4 shows the coefficients on the co-ethnic network variable in linear probability models where the dependent variable is equal to one if the first job in Germany was found thanks to "personal contacts" (friends/acquaintances/relatives) in Columns 1-3. In Column 4 the dependent variable is equal to one if the first job was found through "newspaper or internet". Finally in Column 5 the outcome is one if the first job was found via an "employment agency".[41] Looking at correlations between these channels and local network size allows us to investigate whether networks enhance the "personal contact" channel for finding a job.

From the survey we only know the method of finding one's first job, hence we can only use one

[40]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.

[41]This category includes the following methods: employment agency in Germany, employment agency in home country, employment agency for foreigners, private recruitment agency.

observation per individual. Our specification, therefore, includes the co-ethnic network at arrival-district not interacted with time-since-migration. The estimates in the row Netw_{cd_0} contain the coefficient on the size of the co-ethnic network in the district-of-arrival. In Column 1 of Table 4 we show the simple correlation, without including controls. In Column 2 we introduce the full set of fixed effects and controls. In Column 3 we additionally include the interaction of the network variable with dummies for being low skilled, medium and high skilled, respectively. We then estimate the same specification changing the job-finding channel: using "internet or newspaper" in Column 4, and using "employment agency" in Column 5. Our results show a significant positive correlation between initial network size and the likelihood that the first job in Germany was found through personal contacts. In particular, a one standard deviation increase in the co-ethnic network size at arrival corresponds to a 9.2 percentage point greater likelihood of having found the first job through contacts (Column 1). The unconditional correlation becomes smaller and less-precisely estimated once we include all of our controls (Column 2). Column 3, however, shows that networks have a significantly positive effect on finding the first job through contacts for immigrants with lower levels of education, in a regression that includes all the controls and fixed effects. For lower-educated immigrants, a one standard deviation increase in the network size increases the probability of finding a job through contacts (rather than by other methods) by ten percentage points. This magnitude corresponds to around 15 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 medium or high education levels find a first job via contacts (Column 3). Column 4 shows that the increased reliance on personal contacts for finding a job corresponds to a fall in "newspaper/internet".

5.4 Human Capital Investment

Higher employment rates associated with large initial networks disappear over time. After six years, immigrants who arrived in areas with small ethnic networks are as likely to be employed as those who arrived in areas with larger networks. Are there specific offsetting factors at work for individuals arriving in places with smaller co-ethnic networks? Using survey information on the full history of human capital investments of new immigrants, in this section we analyse whether there is a systematic relationship between social networks at district-of-arrival and investment in human capital. The main results of this regressions are presented in Table 5 and in Figure 3. The dependent variable is a dummy equal to one if the individual reports to be involved in learning activities and is not working in the same year more than 50 percent of the days. From this information we construct a yearly binary indicator of being in school/training for each respondent. We find significant evidence that initial network size negatively affects the likelihood of being in school/training during the first six years after arrival. The estimates of Column 2 of Table 5 show that immigrants first arriving in districts with co-ethnic networks that are one standard deviation larger are 3.1 percentage points less likely to be in school/training in their first three years after migration, where the baseline in our sample is around ten percent. This negative effect slightly declines (2.4 percentage points) but persists until six years after arrival. The average "static" effect estimated in Column 1 without accounting for years since arrival is negative and significant, albeit lower in magnitude at 1.7 percentage points.[42] Results are robust to the inclusion of current year fixed effects (Column 3), and to controlling for mobility (Column 4). Figure 3 shows the results of a more flexible specification where we interact the network variable with yearly dummies since migration. The results are consistent with those reported in Table 5 using the same set of controls as in Column 1. A one standard deviation increase in initial co-ethnic network size translates into a decrease in human capital investment of almost 4 percentage points in the first three years after migration. This negative effect then begins to shrink, becoming small and losing significance nine years after migration. This is consistent with the employment effects we find, and with immigrants in co-ethnic enclaves being employed but missing school-training in the early years.

These results are consistent with our model, which predicts that individuals exposed to larger initial co-ethnic networks are more likely to work and less likely to pursue more education/training. This could be because they have less time and opportunities to attend school and/or because they have less opportunities to realise the greater need for schooling to get a job. In Columns 5 and 6 of Table 5 we show the analysis on the restricted sample of refugees and Ethnic Germans. For this group, and consistent with the slightly larger employment effect, we also find a larger decline in human capital investment in the first three years, corresponding to a 4.3 percentage points reduction in the probability of investing in human capital.[43] In this case the under-investment seems

[42] To the best of our knowledge, this is the first estimate in the literature of the dynamic effects of co-ethnic networks at arrival on human capital investment of immigrants. While its magnitude appears reasonable we are not able to compare it to the literature.

[43] We are not able to add pre-estimated single- or two-way fixed effects in this specification because we lack a comparable bigger sample with information on human capital investments for immigrants.

Table 5: Network at Arrival and Investment in Human Capital

	Dependent Variable: Investment in Human Capital (dummy)					
	Full Sample			Excl. Movers	Restricted Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Netw}_{cd_0}\text{xYsm}_{0-3}$		-0.031***	-0.031***	-0.027**	-0.043**	-0.040**
		(0.009)	(0.009)	(0.011)	(0.019)	(0.020)
$\text{Netw}_{cd_0}\text{xYsm}_{4-6}$		-0.024***	-0.024***	-0.020**	0.002	0.000
		(0.006)	(0.007)	(0.008)	(0.018)	(0.018)
$\text{Netw}_{cd_0}\text{xYsm}_{7+}$		-0.006	-0.006	-0.004	0.034	0.033
		(0.006)	(0.006)	(0.008)	(0.021)	(0.021)
Netw_{cd_0}	-0.017***					
	(0.006)					
Observations	11664	11664	11664	7218	4182	4182
Clusters	239	239	239	161	138	138
R-squared	0.221	0.224	0.228	0.209	0.316	0.325
Mean dep var	0.050	0.050	0.050	0.045	0.060	0.060
Single FEs	yes	yes	yes	yes	yes	yes

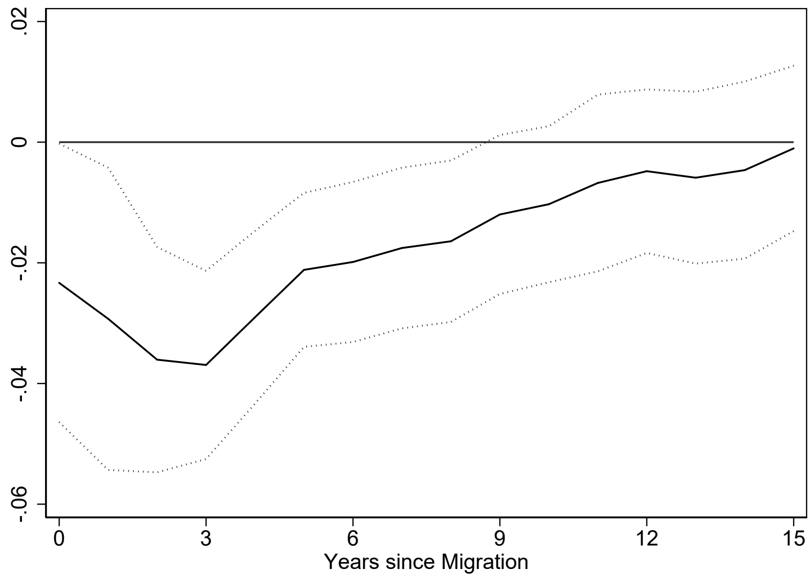
Note: The dependent variable is a dummy for being in education, defined as one if the individual reports to be in education and is not working in the same year more than 50 percent of the days. Single FEs refer to district at arrival, year of arrival, and country fixed effects. Column (1) reports the average "static" effect of network. Columns (3), (4) and (6) additionally include current year fixed effects. 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), average wage in the district of arrival. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Restricted sample 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.

stronger in the first three years. Refugees and Ethnic Germans in locations with large networks are significantly less likely to spend time in school/training in the three years after arrival. After six years this difference has disappeared and the point estimates are not significant.[44]

In Table 6 we report results by distinguishing the type of training in school-university education (Column 1) or vocational education (Column 2). The outcome is a dummy equal to one if an immigrant has pursued that type of training/education during the year and is not working more than 50 percent of the time. The estimates show that large co-ethnic network locations mainly reduce time in school/college education rather than time invested in vocational training. As vocational training in Germany is often connected to working and learning on the job, the trade-off between working and accumulating human capital is clearer and sharper in the case of proper schooling. Vocational training may actually be a complement of employment rather than an alternative choice. Those differences also seem to have stronger persistence. The negative effect of the network size

[44]In Table E.4 of our Online Appendix we show that results on employment and human capital investment are robust to (and slightly larger in magnitude when) restricting the sample to individuals who were younger than 40 at migration. This is the group for which human capital investment has the largest return.

Figure 3: Network at Arrival and Investment in Human Capital



Note: The figure shows the coefficients of the yearly dummies interacted with the network variable, Netw_{cd_0} obtained from specification (4) where year since migration is expressed as yearly dummies. The dependent variable is defined as in table 5. Solid lines refer to regression coefficients, dotted lines refer to 95% confidence intervals obtained using clustered standard errors at district level. The regression is estimated using the baseline sample as in Column 1 of table 5. Additional controls are those used in Column 1 of table 5: gender, age and age at migration (and their square), average wage in the district of arrival. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Single FEs: district at arrival, year of arrival, and country fixed effects. The figure is cut at year since migration equal to 15 for presentation purposes.

at arrival on school/formal education is long-lasting: a one-standard-deviation increase in network size upon arrival translates into a one-percentage point reduction in the probability of attending school and college, even in the long-run. The effects on vocational training are smaller, and shorter-lived: individuals arriving in areas with smaller networks do not appear to have taken advantage of vocational training right away, and may do so later. To the contrary, those who do not attend school in earlier years are unlikely to compensate for such missed opportunity later.

5.5 Language Proficiency

One of the channels through which initial co-ethnic networks may affect employment of newcomers is through effects on German language proficiency. Network size may affect both the opportunities and the incentives to learn the native language. In this section we investigate the effect of co-ethnic networks on language proficiency. Our survey data include separate observations of reading, writing

Table 6: Network at Arrival and Investment in Human Capital by Type

Dependent Variable	School-University	Vocational
	(1)	(2)
$\text{Netw}_{cd_0}\text{xYsm}_{0-3}$	-0.024***	-0.010*
	(0.007)	(0.005)
$\text{Netw}_{cd_0}\text{xYsm}_{4-6}$	-0.022***	-0.003
	(0.005)	(0.004)
$\text{Netw}_{cd_0}\text{xYsm}_{7+}$	-0.010**	0.004
	(0.005)	(0.004)
Observations	11664	11664
Clusters	239	239
R-squared	0.223	0.082
Mean dependent variable	0.029	0.021
Single FEs	yes	yes

Note: The dependent variable is a dummy for being in education, defined as one if the individual reports to be in education, distinguishing between school-university (Column 1) or vocational education (Column 2), and is not working in the same year more than 50 percent of the days. 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), average wage in the district of arrival, country of origin fixed effects, year at migration fixed effects, 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.

and speaking proficiency in German, measured on a five-point scale. We only observe language at two points in time, i.e. upon arrival and in the survey year. Table 7 presents the effects of initial network on current language proficiency, while controlling for a large set of regressors including language proficiency at time-of-arrival. The dependent variable is a proficiency index in reading (Columns 1 and 2), writing (Columns 3 and 4) and speaking (Columns 5 and 6). Columns 1, 3 and 5 show that larger initial networks tend to be associated with lower current language proficiency, especially for speaking, which is consistent with the idea that co-ethnic networks might reduce opportunities to speak German. In Columns 2, 4 and 6 we add an interaction term between initial network size and pre-migration language proficiency. Results show very consistently across reading, writing, and speaking that the negative effects of network size on current language proficiency are attenuated for people who have a better initial knowledge of German, and are more severe for individuals that have lower pre-migration proficiency.

These results are in line with those of Laliberté (2019), who uses longitudinal data from Australia to estimate the effect of linguistic enclaves on proficiency in English. Laliberté (2019) also finds linguistic enclaves to slow down language acquisition, and finds the channel to be informal social

Table 7: Network at Arrival and Language

	Dependent Variable: Proficiency in German Language					
	Reading		Writing		Speaking	
	(1)	(2)	(3)	(4)	(5)	(6)
Language_{t_0}	0.332***	0.321***	0.404***	0.392***	0.342***	0.333***
	(0.021)	(0.022)	(0.022)	(0.024)	(0.022)	(0.023)
Netw_{cd_0} (a)	-0.089	-0.190**	-0.105*	-0.209***	-0.102***	-0.189***
	(0.064)	(0.079)	(0.063)	(0.076)	(0.039)	(0.051)
$\text{Netw}_{cd_0}\text{xLanguage}_{t_0}$ (b)		0.048**		0.052**		0.042***
		(0.023)		(0.023)		(0.015)
Observations	1135	1135	1135	1135	1135	1135
Clusters	237	237	237	237	237	237
R-squared	0.570	0.573	0.581	0.584	0.576	0.578
Mean dep var	2.093		1.992		3.618	
(a)+(b)		-0.142		-0.157		-0.147
p-value (a)+(b)		0.034		0.016		0.001

Note: The dependent variable refers to current proficiency in German language. Each column uses a different definition of proficiency according to the heading. Columns (1)-(6) refer to speaking, reading, and writing, respectively. Each variable takes value between one and five, each value corresponding to "very poor", "poor", "sufficient", "good", "very good". "Language0" denotes measures of language proficiency at arrival. 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), current age (and its square), average wage in the district of arrival, country of origin fixed effects, year and district at arrival 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.

interactions rather than formal language education.

5.6 Effects by Education Group

Existing research (e.g. Glitz, 2014) shows that lower-educated immigrants are especially likely to locate where co-ethnic networks are large. Our model suggests that they largely benefit from large networks for job finding purposes. Table 8 breaks down the main sample by pre-migration educational levels. The three education categories are considered following the standard German classification: "lower education", corresponding to no vocational training, "medium education", corresponding to post-secondary vocational study, and "higher education" corresponding to college education and above. Columns 1-3 of Table 8 estimate network effect on employment probability, separately by education group. The positive initial effect of network on employment is stronger and more significant for individuals with lower levels of education. Medium-education immigrants still experience a significant but smaller short-run effect. Highly-educated immigrants show an effect which is close to zero and not statistically significant. Consistent with Table 3, effects disappear or are very strongly attenuated seven years after arrival. For less-educated immigrants, the effect of co-ethnic network on employment is quantitatively large: Column 1 of Table 8 shows that moving to a district with a one standard deviation larger co-ethnic network upon arrival corresponds to a 12.6 percentage point greater probability of being employed in the first three years, relative to an average of 66 percent. This effect largely disappears six years after migration. Columns 4-6 of Table 8 investigate the relationship between network size and human capital investment for individuals with different initial education levels. Consistently, results are stronger (more negative) for individuals with low and medium levels of education. In this case the largest point estimate and significance is for the medium-skilled at 4.1 percentage points. The point estimates are the lowest and not statistically significant for highly-educated workers. For less-skilled individuals, the effect is less significant but the point estimate is only slightly lower (less negative) than for medium-skilled. The results across educational categories need to be taken with caution due to relatively small sample sizes. A formal test shows that the point estimates of the relevant coefficients are statistically different only between less- and highly-educated categories for employment regressions. Overall, we can at least say that, within three years of arrival, less-educated immigrant workers arriving in districts with larger co-ethnic networks are more likely to find employment. These benefits of networks dissipate over time, likely because individuals in locations with smaller networks take advantage of more schooling and training, and improve their language skills. In the long-run, they have the same probability of being employed as migrants who started with a larger co-ethnic network.

In additional regressions we analyse whether the presence of co-ethnic networks with similar education levels yields a stronger impact on employment by skill groups. We separate immigrants by education group and 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 standardize for total employment by district-year. Table E.5 of our Online Appendix reports the results for each education 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 co-ethnic groups of low-skilled individuals; middle-skilled immigrants benefit from locating near networks with many individuals from "lower and medium-educated" co-ethnic groups; and the employment probability of the highly-educated is not affected by proximity to any co-ethnic skill group.

Table 8: Network at Arrival and Employment/Human Capital Investment by Education

Dependent Variable	Employment			Human Capital		
Education	Low	Medium	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
$Netw_{cd_0} xYsm_{0-3}$	0.126***	0.091*	0.005	-0.034*	-0.041***	-0.026
	(0.044)	(0.048)	(0.052)	(0.018)	(0.012)	(0.021)
$Netw_{cd_0} xYsm_{4-6}$	0.072**	0.062	-0.032	-0.018	-0.024**	-0.029
	(0.034)	(0.043)	(0.054)	(0.015)	(0.011)	(0.021)
$Netw_{cd_0} xYsm_{7+}$	0.045	0.034	-0.031	-0.001	-0.010	-0.015
	(0.032)	(0.044)	(0.050)	(0.015)	(0.012)	(0.021)
Observations	5454	3969	2348	5396	3947	2321
Clusters	177	154	132	177	154	132
R-squared	0.303	0.324	0.399	0.351	0.141	0.219
Mean dep var	0.662	0.703	0.695	0.069	0.025	0.047
Single FEs	yes	yes	yes	yes	yes	yes

Note: The dependent variable is a dummy for employment in Columns (1)-(3), and a dummy for being in education in Columns (4)-(6) as reported in the heading. Education refers to education at arrival. 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), average wage in the district of arrival. Pre-migration controls: employment, language proficiency, education, working experience (and its square). Single FEs refer to district at arrival, year of arrival, and country fixed effects. Standard errors in parenthesis are clustered at district level, * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

5.7 Placebo Exercise and Other Robustness Checks

One possible concern is that, despite the large set of fixed effects, our findings may still be in part driven by labour demand conditions in a specific district at a particular point in time. Some districts may have labour market conditions that are favorable to immigrants, and these might be persistently correlated with the size of ethnic communities. While including a district-time effect (two-way fixed effects) should reduce this concern substantially, we further examine this potential issue, here. Columns 2 and 4 of Table 9 presents results from a placebo exercise where we re-define our networks as the share of *non-co-national* foreign-born (foreign born individuals that are born in any region except that of the individual we are considering) in local employment. Columns 1 and 3 report our baseline estimates for comparison. Columns 1 and 2 show the estimates of the network variable and its interactions when using employment probability as the dependent variable. The estimates of Column 2 are not significantly different from zero, the point estimates are negative, and the magnitudes are small. Columns 3 and 4 perform an equivalent falsification test on the relationship between network size and human capital investment. Results of Column 4 do not point to any effect, either. We find these results very reassuring: it seems that co-ethnic networks specifically, and *not* the generic presence of immigrants that could be attracted by strong labour

Table 9: Falsification Test: non co-Ethnic Network

Dependent Variable	Employment		Human Capital	
Network	Baseline	Other	Baseline	Other
	(1)	(2)	(3)	(4)
$Netw_{cd_0}xYsm_{0-3}$	0.078***	-0.021	-0.031***	0.018
	(0.021)	(0.040)	(0.009)	(0.017)
$Netw_{cd_0}xYsm_{4-6}$	0.031	-0.024	-0.024***	0.020
	(0.020)	(0.038)	(0.006)	(0.014)
$Netw_{cd_0}xYsm_{7+}$	0.007	-0.045	-0.006	0.020
	(0.018)	(0.039)	(0.006)	(0.013)
Observations	11771	11771	11664	11664
Clusters	239	239	239	239
R-squared	0.250	0.246	0.224	0.220
Mean dependent variable	0.683	0.683	0.050	0.050
Single FEs	yes	yes	yes	yes

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), average wage in the district of arrival, country of origin fixed effects, year at migration fixed effects, 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, * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

markets, are the determinants of the employment effect found on new arrivals. The results of this exercise are consistent with the employment mechanism operating through co-ethnic networks and not through a generic correlation with labour market conditions.

As we look at the effect of co-ethnic networks upon arrival, remaining in the initial district exposes new immigrants to the channels we have discussed. If, instead, initial co-ethnic network size increases mobility, this would certainly affect the interpretation of its role. In order to address this issue, we estimate a regression model in which the dependent variable is a dummy for changing district of residence. The estimates in Table E.3 (Online Appendix) shows that our measure of initial network size does not predict the probability of changing district in the short, medium or long run, either using our full sample or our restricted sample. This in turn implies that the estimated effects on employment and human capital investment should not be driven by individuals leaving the original district.

An additional concern has to do with our sample: survey respondents need to give consent for the linkage of survey and administrative data. This may imply that our final sample is not fully representative of the underlying population. We follow a strategy similar to Lubotsky (2007),[45] and

[45]Brücker et al. (2020) adopt the same strategy to adjust for potential selection of individuals giving consent to the linkage of survey data to administrative records.

run our main regressions where we add the inverse of the estimated probability of giving consent (based on a set of characteristics) as regressions weights. Results from this exercise, available in Table E.7 of our Online Appendix, are very similar to our baseline results. We also check whether our results are robust to different geographical levels of aggregation. Throughout the paper we use districts as units, as they are a reasonable proxy for local labour markets. In Table E.8 of our Online Appendix we perform the analysis using municipalities instead, which are smaller units (there are about 12,000 in Germany) and capture interactions at the local level. The results show that our main estimates are robust to this modification.[46] 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. Table E.9 compares our baseline results (Columns 1 and 7 for employment and human capital, respectively) with results obtained 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 5-6, and 8). Results are very robust to these variations. We also consider alternative length for the interval of time allowed after arrival before the first appearance of each immigrant in the registry data. These checks reduce the number of immigrants for which we can impute the co-ethnic network upon arrival, but also reduce potential measurement errors. Table E.10 shows that the results are robust to more restrictive imputation windows, i.e. to only allowing the use of district information from the administrative data within one or two years after arrival according to the survey, as opposed to the baseline three year period.

6 Concluding Remarks

In this paper, we investigate whether the size of co-ethnic networks at the point of arrival affects employment and training/schooling of immigrants over time. We frame the interpretation of our empirical findings within a simple search model where individuals can search through a formal channel and a network-based channel. In the network-based channel, co-ethnic networks help indi-

[46]Other studies sometimes use even smaller units when analysing the role of networks. Bayer et al. (2008) for instance, use Census blocks, whereas Schmutte (2015) considers small neighbourhoods.

viduals find employment by providing referrals. Such a model predicts an initial lower probability of employment for individuals initially located in areas with smaller co-ethnic networks. Over time our model predicts employment differences between those with larger and those with smaller initial networks to decrease, because of different incentives to invest in their human capital. Our main dataset combines a recent survey of immigrants to Germany with administrative records of those individuals and of all other workers. This allows us to reconstruct their entire individual labour 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. However, they are also less likely to invest in human capital, especially in the form of schooling and college education, so that the employment rate advantage disappears 6-7 years after arrival. These effects are stronger for immigrants with lower initial levels of education.

Our analysis of the role of co-ethnic networks on human capital investment suggests that co-ethnic networks may give a larger initial boost to employment that attenuates over time. Moreover, co-ethnic networks may discourage long-run accumulation of general 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 particular. The benefits of a dense co-ethnic network seem short-lived in terms of employment, and an unintended consequence of encouraging settlement in co-ethnic enclaves may be that new immigrants have fewer 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) focused primarily on static earnings effects. Those studies mostly found a positive impact of networks on earnings, and they argued that dispersal policies have high costs for immigrants, worsening their labour market outcomes. The implications from our results, however, suggest a more nuanced story. While in the short-run employment probability is increased by the presence of co-ethnic networks, dynamically these networks may reduce human capital accumulation and lower the quality of job matches and, possibly, 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 isolate the 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. The initial positive employment effect and the negative investment in human capital effects are even somewhat stronger for refugees than for other immigrants, possibly due to their lower skill levels and greater need of initial connections to find jobs.

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