The Effect of Social Networks on Migrants' Labor Market Integration:

A Quasi-Experiment

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ABSTRACT

Empirically identifying the causal effect of social networks on migrants' economic prospects is a challenging task due to the non-random residential sorting of migrants into locations with greater opportunities for (previous) connections. Our study addresses this selection-bias issue by using a unique quasi-experimental dataset of refugees and other migrants that were exogenously allocated to their first place of residence by German authorities. The empirical results reveal a positive causal effect of social networks on migrants' transition rate to the first job, but only if the networks are mobilized for the job search. This finding implies that the mere effect of more available networks is insufficient for migrants' labor market integration; it is when these networks are utilized that they become beneficial.

KEYWORDS: network social capital, quasi-experiment, refugees, first-generation migrants, labor market

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INTRODUCTION

Since the influential contribution of Granovetter (1973), many sociological studies have addressed the importance of social networks in job searches. By "conveying resources and providing signals to others" (Castilla, Lan, and Rissing 2013a:1013), social networks can influence labor market outcomes for both job seekers and job providers (Fernandez, Castilla, and Moore 2000; Petersen, Saporta, and Seidel 2000; see also, e.g., Bills, Di Stasio, and Gërxhani 2017). Additionally, the migration literature has established that social networks are influential in migrants' labor market integration (e.g., Kalter 2011; Massey and Espinosa 1997).

When studying the role of networks in migrants' labor market integration, a large body of migration literature examines prior connections with relatives or friends in the destination country (e.g., Aguilera and Massey 2003; Amuedo-Dorantes and Mundra 2007; Kalter and Kogan 2014), or uses either the size of co-ethnic enclaves in the destination country as a proxy for potential co-ethnic networks (Battisti, Peri, and Romiti 2016; Beaman 2012; Damm 2009; Edin, Fredriksson, and Åslund 2003; Munshi 2003), or the frequency and/or intensity of post-migration contacts with different population groups (e.g., Kanas et al. 2012; Kanas, Van Tubergen, and Van Der Lippe 2009). The main argument of this literature is that individuals' embeddedness in a network allows benefits from relevant (social) resources.

There is, however, a common issue in the literature on networks and (non-)migrant labor market outcomes: it remains challenging to identify the causal effect of social networks on the labor market prospects of (non-)migrants. As indicated first by McPherson, Smith-Lovin, and Cook (2001) and later by Mouw (2003, 2006), the endogeneity between social network variables and labor market outcomes is high. The rationale is that social contacts are chosen non-randomly; thus, much of the estimated effect of social networks may be driven simply by

selection effects. Selection bias in race and ethnicity represents one of the strongest divides in social networks (McPherson et al. 2001). As frequently found in the literature, migrant inflow into particular destinations and/or regions is often driven by connections with previously migrated family or friends (e.g., Borjas 1989; Kalter 2011; Palloni et al. 2001; Williams and Sofranko 1979), or by the presence of previously migrated co-ethnic groups (e.g., Cutler and Glaeser 1997; Dustmann and Preston 2001). In other words, if newcomers choose their location within the destination country themselves, their choice will likely be driven by the networks (such as those with family, friends or co-ethnic groups) they expect to have in different locations. Hence, any estimated effect of migrants' social connections may reflect a selection effect caused by an endogenous choice of networks.

In line with social resource theory (Lin 1999), a.k.a. "network social capital" perspective (Mouw 2006), we analytically distinguish between the possibility of having access to a network and the use of such networks. "The actual mobilization of resources, rather than the availability of resources, should play a more critical role in affecting the [labor market] outcome" (Lai, Lin, and Leung 1998:163). Our goal is to empirically examine whether this is the case regarding refugees and first-generation migrants[1], which will bring us closer to a causal test of the effect of network social capital on migrants' labor market integration.

As argued by Mouw (2003:890), such a causal test requires one to assume "that the use of contacts is exogenous [to the level of social capital], but that the benefit of contacts depends on the social capital embedded in those contacts". It is by now well established that showing that the use of networks is exogenous to their availability is a challenging task, both theoretically and empirically (Montgomery 1992), unless one applies experimental research strategies. These offer a controlled setting that allows one to draw causal inferences regarding the "true" effect of social networks (Castilla, Lan, and Rissing 2013b; Mouw 2006).

We have access to a unique quasi-experimental dataset, which is based on implementations of national dispersal policies that determine the residential allocation of refugees, ethnic German and Jewish migrants in Germany. In other words, these migrant groups are exogenously allocated by an external state authority, which minimizes the likelihood that migrant inflows into particular regions are driven by family and friendship ties or by the availability of a co-ethnic community. Moreover, our data from the German IAB-SOEP Migration Sample provide self-reported information about whether a migrant's first place of residence was determined by authorities, which increases our confidence that the allocation was exogenous. Such an allocation implies that any differences in (un)observable characteristics between those who have access to larger networks and those with access to smaller networks are unlikely, meaning that the identified effect is not a mere effect of self-selection. These unique features of our quasi-experimental design add important methodological – and hence theoretical – value to the predominantly observational knowledge about migration and social networks because experimental designs allow researchers to identify empirical regularities that, in turn, contribute to theory development (Davis and Holt 1993).

Finally, when studying migrants' labor market integration, the migration literature primarily considers migrants in different career stages and with very heterogeneous backgrounds in terms of, on one hand, the degree and type of labor market experience in the host country and, on the other hand, the accumulation of social contacts in the labor market. Such heterogeneity may, in turn, create path-dependent career trajectories by amplifying initial (dis-)advantages over time (cf. DiPrete and Eirich 2006). We focus on a specific and crucial life-course stage for migrants – their first job – because the effect of social networks is likely to be the "cleanest" and to be less confounded by other factors at the labor market entry stage. There is some empirical evidence that social networks are particularly important at the beginning of one's career and decrease in influence as experience is accumulated (Battisti et al. 2016; Dustmann

et al. 2016). For a more comprehensive picture of the role of social networks in migrants' labor market integration, we investigate the effect of social networks on both the transition rate to and the wages in migrants' first jobs.

A THEORETICAL AND METHODOLOGICAL OVERVIEW

Social networks and migrants' job opportunities

A growing body of sociological and economic empirical research has examined the role of social networks in newcomers' labor market opportunities. In the migration literature, connections with migrant networks in the destination country are considered the main source of "information about or direct assistance with migrating" (Garip 2008:593). Several studies have found positive effects of social networks on migrants' employment opportunities (e.g., Elliott 2001; Sanders, Nee, and Sernau 2002) and on job quality (e.g., Aguilera and Massey 2003; Amuedo-Dorantes and Mundra 2007; Drever and Hoffmeister 2008; Dustmann et al. 2016; Massey and Espinosa 1997). Other studies, however, report that social networks have either no effect on labor market outcomes (e.g., Kanas, Van Tubergen, and Van Der Lippe 2011; Xie and Gough 2011) or may even hinder newcomers' labor market integration (e.g., Cutler and Glaeser 1997; Drever and Hoffmeister 2008; e.g., Kalter and Kogan 2014; Kazemipur 2006; van Tubergen 2011). As we argue in the next section, these inconclusive findings in previous empirical studies may be due to reliance on different methodologies that cannot fully account for migrants' self-selection into social networks (see also Obukhova and Lan 2013 for a similar discussion).

Social resources theory and the endogeneity of social networks

Social resources theory (Lin 1999), or what is also known as the "network" social capital perspective (Mouw 2006:79)[2], argues that when analyzing the effect of social networks (i.e., social resources) on labor market outcomes, one needs to distinguish between the access to and

the use of social networks.[3] Hence, if individuals are part of a social network that they chose to be in, the use of this network to find a job, for instance, is endogenous to the network to which they may have access. As a consequence, one cannot conclude that using networks to find a job leads to more or better jobs. In a thought-provoking article, Mouw (2003:871) writes, "The results of social capital models suggest that individuals with well-connected social networks do better in the labor market. However, does this result reflect causality or merely the fact that similar people tend to associate with each other?"

The main problem in identifying the causal effect of social networks on labor market outcomes lies in isolating the effect of selection into networks from the effect of the network resources that can be mobilized for instrumental purposes (Lai et al. 1998). As argued above, most of the migration literature examines the role of social networks by looking at migrants' prior connections with relatives or friends in the destination country or by considering the size of migrants' co-ethnic enclaves in the destination country. This implies that migrants can choose to join these types of social networks, which in turn can have positive outcomes in their integration into the labor market. Because of this endogenous choice, however, it is unclear whether the effect of migrants' social networks simply reflects self-selection into the networks or that the use of such networks to find a job has a causal effect. Various methods that have been proposed since, to better address this issue, are summarized in the succeeding section.

Analytical tools to address the endogeneity of social networks

Self-selection into social networks has been addressed in the migration literature through the use of longitudinal data with individual fixed effects (e.g., Battisti et al. 2016; Beaman 2012; Dustmann et al. 2016; Garip, Eskici, and Snyder 2015; Kanas et al. 2011; Lancee 2016; Mouw 2002, 2003; Palloni et al. 2001; Yakubovich 2005). This approach allows one to account for unobserved time-constant heterogeneity (i.e., for unmeasured confounders that are likely to

affect both selection into social networks and job outcomes). However, if certain unmeasured time-varying factors (confounders) simultaneously drive access to social networks and the improved job outcomes, the causal effect of social networks is likely to be biased.[4]

Another way to address self-selection into social networks, particularly in the absence of longitudinal data, is to apply a matching method (e.g., propensity score matching, PSM; for a practical implementation of this method in the migration literature, see, e.g., Kalter and Kogan 2014). This method offers the opportunity to compare the labor market outcomes of individuals who use social networks with those of individuals who do not use social networks and who are otherwise equal in all observable attributes (relevant to job outcomes). The main problem is that matching is based on observable characteristics and requires larger samples to successfully identify matches based on many observables. The omission of correlated unobservables may still present a significant challenge.

The instrumental variables (IV) method can be a powerful tool to address the issue of correlated unobservables (Bollen 2012) and has frequently been used in the migration literature (e.g., Dustmann and Preston 2001; Munshi 2003). The challenge, however, is to find an appropriate instrument that correlates with the use of social networks but not with unobserved factors (and thereby with job outcomes). The greatest problem with the IV method is that there is no possibility to test the (theoretical) link between the instrument and the unobserved characteristics. If the instrument is weak, the identified effect of social networks may bias the estimates even more than a model that does not control for self-selection (Mouw 2006:92).

Applying these methods has improved our understanding of the relationship between social networks and labor market outcomes. Indeed, migration studies that accounted for self-selection into networks have shown that social networks are beneficial for migrants' labor market integration (e.g., Battisti et al. 2016; Dustmann et al. 2016; Mouw 2002; Munshi 2003).

Nevertheless, the results imply "suggestive associations, not causal links" mainly because of difficulties to identify peer effects (Garip et al. 2015:1080; see also Manski 1993 and Mouw 2006). Using an experimental design that varies from a random assignment of treatments (laboratory experiments) to an exogenous allocation of actors (natural or quasi experiments) has been recognized as the only means to truly identify the causal effect of social networks (Castilla et al. 2013b; Mouw 2006). This is because the randomized assignment eliminates the problem of individuals (e.g., migrants) selecting each other based on observable and unobservable characteristics. Natural experiments, on the other hand, are considered "particularly helpful for studying how unexpected exogenous changes in employment relations may affect network structures (e.g. sudden geographic relocations of companies)" (Castilla et al. 2013b:1021).

Indeed, contrary to the significant positive effects found in network studies that apply the above-mentioned methods, the few studies that rely on experimental data (e.g., regarding the random assignment of roommates) offer little evidence of a causal effect of social networks (e.g., Marmaros and Sacerdote 2002; Sacerdote 2001; see also Mouw 2006 for further reviews).

Causal effects of networks on migrants' transition to their first jobs and their wages

The experimental method thus ensures that the use of social networks to find a job is not endogenous to the availability or size of one's social networks. In line with the social resource theory, this would imply that if there is any effect of migrants' social networks on job-related outcomes, that effect would not be because of the size of social network a migrant may have access to but rather because of the resources embedded in that network, which the migrant can mobilize to achieve the desired job-related outcomes. Like in Mouw (2003), this theoretical possibility of a causal effect of social networks relies on the assumption that the relationship

between the access to and the use of networks is exogenous. In other words, migrants' use of their networks is independent of the size of networks.

The use of social networks can be beneficial to migrants for both, their propensity of job entry and their job quality (i.e., wages), because networks can transmit resources (e.g., information, support, and influence) or offer signals (on, e.g., ability, status, and trust) that can add value to employers (Castilla et al. 2013b). Employers, in turn, may be more likely to hire and/or offer a better job (e.g., with better pay or a higher occupational status) to the referred applicant (Gërxhani, Brandts, and Schram 2013; Lin, Ensel, and Vaughn 1981; Montgomery 1991). Therefore, it is through these properties of social networks that we expect the following:

> *H1:* Migrants with access to larger social networks will not differ from migrants with access to smaller social networks in their propensity to find a job and in their wages.
> *H1a:* The size of migrants' networks matters in their propensity to find a job and in their wages only when migrants mobilize their networks for that purpose.

GERMAN DISPERSAL POLICIES

To examine whether social networks have a causal effect on migrants' labor market integration, we focus on Germany for three main reasons. First, Germany has historically played an important role as a migration-receiving country in Europe and is characterized by a large proportion of migrants (see Kogan 2011 for an overview). Second, previous research on migration has revealed the deficiencies of integration policies in Germany. These policies have had disadvantageous outcomes for migrants, who face greater rates of unemployment, are concentrated in a lower occupational hierarchy (Kogan 2011), and have lower wages (Constant and Massey 2003) than natives. Social networks, however, seem to be an important instrument in countering migrants' economic disadvantage (Drever and Hoffmeister 2008; Dustmann et

al. 2016; Kalter and Kogan 2014; Kanas et al. 2011; Lancee 2012). A third and decisive reason for focusing on Germany relates to a quasi-experiment that we exploit for our research purposes, which is described below.[5]

According to their specific status in Germany, refugees, ethnic Germans, and Jewish migrants have been subject to national dispersal policies. The allocation of refugees, ethnic Germans, and Jewish migrants to their first place of residence was regulated by law (from the 1970s to the present for refugees and from 1989 to the end of 2009 for ethnic Germans and Jewish migrants). These migrants' allocation across German Federal States was based on a quota system, the so-called "Königsteiner Schlüssel".[6] Based on similar quota regulations, authorities in the federal states were responsible for the further allocation of the assigned migrants within their territory. In the case of family reunification (which applied to only married couples and their minor children), refugees, ethnic Germans, and Jewish migrants could request to join their (nuclear) families in a different reception center (in a different German Federal State). Such situations allow for deviations from these policies, which may undermine the exogenous allocation of migrants and increase the probability of self-selection. Some studies, that have attempted to exploit these exogenous allocation policies to identify the effect of social networks on migrants' labor market outcomes, suffer from self-selection because they have restricted their data to migrant groups subject to such policies only (e.g., Battisti et al. 2016 for Germany; Damm 2009 for Denmark; Edin et al. 2003 for Sweden). Contrary to these previous studies, our data provide a unique opportunity to properly test whether the results are robust to such endogeneity and selection bias. In the process of gathering the data, the respondents were asked whether their choice of the first residence place in Germany was driven by factors such as economic conditions, family living there, or whether they were allocated by German authorities. Hence, the respondents reporting having been assigned are evidently those who were truly allocated by German authorities, whereas respondents reporting family reasons

(even though they arrived, e.g., as refugees, ethnic Germans or Jewish migrants) were likely those arriving for family reunification.[7]

Refugees' first residential allocation was binding, and the obligation to reside in the district in which they were initially allocated could be abolished either upon the official recognition of one's refugee status or 24 months after arrival at the latest. The duration of the recognition procedure is time-consuming; this process took 22 months on average in 2005 (BAMF 2005). The first regional allocation was binding for ethnic German migrants (since 1996), and it could be abolished if these migrants showed proof of sufficient (permanent) job income three years after arrival at the latest. For Jewish migrants, there were no residential obligations.[8]

DATA AND METHOD

Data and sample

The empirical analysis is based on data from the IAB-SOEP Migration Sample,[9] a large longitudinal survey of migrants in Germany that was launched in 2013 and is conducted yearly. The anchor persons were drawn from administrative data (Integrated Employment Biographies, IEB, of the Institute for Employment Research, IAB) to be representative of the target population. The target population were individuals migrating to Germany between 1995 and 2010. All persons living in the same household were interviewed. The overall mean response rate amounted to approximately 32 percent and conforms to response rates of earlier SOEP samples (Kroh et al. 2015).[10] Mainly due to a panel-attrition problem, a refreshment sample was added to the original sample in the third wave, which was surveyed in 2015. This refreshment sample targeted migrants who arrived between 2009 and 2013 to Germany. For more information about the sampling procedure and further methodological issues, see Brücker et al. (2014) and Kroh et al. (2015).

For our analyses, we considered only the respondents from the third wave because information about their residential allocation was surveyed for the first time in this wave. We restricted our sample to foreign-born individuals who *reported being assigned to their first place of residence in Germany* (13 percent of the original data; 15 percent of the first-generation migrants). That is, their first residential allocation was determined by the German authorities. Although by this restriction we substantially reduce the sample size, this sampling ensured that the respondents' sorting across locations was exogenous and was not due to self-selection (i.e., individuals' personal choice or characteristics). The quasi-experimental opportunity in this empirical setting lies in the exogenous variations in the features of these first places of residence among the assigned group of migrants.

The sample of the assigned migrants (i.e., 536 respondents) was further restricted based on some additional criteria. Since our focus is on the first stages in the German labor market, we consider only migrants of working age at the time of their arrival in Germany. Following the definition of the German Federal Employment Agency (*Bundesagentur für Arbeit*), working age is defined between 15 years old till the pre-retirement age of 64 years old (Bundesagentur für Arbeit 2019). This led to a further exclusion of 108 respondents. Since the dispersal policies for some migrant groups (i.e., ethnic Germans and Jewish migrants) were first launched in 1989, we also excluded 12 respondents who had arrived before 1989.[11] Individuals who had not experienced a first job entry in Germany by the time of the survey and had no intention to work were not included in the analyses either (30 respondents).[12] To avoid bias in our results due to inconsistences in the respondents' information, the following 47 individuals were also excluded from the analyses: (1) individuals who reported "never having entered a first job in Germany" but who were "currently working", and (2) individuals for whom the reported date of first job entry appeared to be before the date they arrived in Germany. After all these

exclusions and the listwise deletion of missing values for the variables of interest (about 8 percent), the resulting final sample consisted of 309 individuals.

Dependent variables and empirical method

The first labor market outcome that we examine is migrants' *transition rate to their first jobs* in Germany, employing discrete event-history modeling for the empirical analyses (Allison 1982; Blossfeld, Golsch, and Rohwer 2007). The key statistical concept within the event-history approach is the transition rate (i.e., hazard rate), which represents the probability of experiencing an event (the labor market entry) in year t given that by the beginning of t, no entry had occurred. Accordingly, the transition rate tells us how rapidly individuals enter the labor market: the higher the transition rate, the faster the transition into the first job is. For the transition to the first job in Germany, the period of observation begins in the year of migration to Germany and either ends in the year of the first job-entry or is right-censored at the date of the interview (if entry into the first job has not yet occurred).[13] The data are organized in a person-year format, which means that each row of the dataset corresponds to a time period of one year.[14] This step leads to a total of 1063 person-year observations. The dependent variable is whether an individual entered his or her first job in Germany in a given year t. This event occurred for 267 of the person-years and is coded 1 (whereas 0 means no first job-entry in year t). The time dependency of the process of first job-entry is modeled using a piece-wise constant approach, which is useful to control for the dependency of duration (between arrival to Germany and first job-entry) while not requiring complex assumptions about the time dependence of the process (Blossfeld et al. 2007). Durations are assumed to follow an exponential distribution, which implies a time-constant hazard rate. By introducing five period-specific dummy variables (up to one year since arrival, two years since arrival, three to five years since arrival, six to ten years since arrival, and 11 years or more since arrival), the rate is allowed to vary across periods.

For our second labor market outcome - *quality of the first job* - we construct a measure of *real hourly wages* by using the monthly labor earnings and weekly hours worked in the first job in Germany. The information on both monthly labor earnings and weekly hours worked was available for 236 job entrants (88 percent of our sample). For migrants who entered their first job before 1999 (the year when the euro was introduced), the reported values were divided by the constant exchange rate for the Deutschemark to the euro (equal to 1.95583). To calculate real hourly wages, we use the Consumer Price Index (CPI) deflator with 2015 as the base year. For the empirical analysis, we rely on an ordinary least squares (OLS) regression model with the real log hourly wages in the first job as the dependent variable. We consider wages to be a good indicator of job quality since wages are often linked to consumption opportunities, and higher occupational status and job prestige (Weiss and Fershtman 1998). Moreover, in his theoretical work, Montgomery (1991) argues that social contacts operate as a channel for the transmission of information about unobservable characteristics between the employer and the potential employee, facilitating better match quality and, as a result, higher starting wages (Marsden and Gorman 2001). Accordingly, higher wages in the first job may approximate higher-quality matches.

Independent variables and confounders

The exogenous treatment in our sample is the local labor market migrants encountered in their first place of residence in Germany. We take account of an important possible variation in the features of these local labor markets, namely, the *co-ethnic network size* in the district of assignment in the year of arrival. In line with migration literature, we consider this measure a good proxy of individuals' embeddedness in a potential network they can extract resources from, like information on job opportunities or (non)financial support.[15] Contrary to this literature, however, our experimental setup deals with the endogeneity issue. In other words, our respondents' exogenous allocation across different local labor markets ensures that the

respondents' network structure with co-ethnics in these labor markets is exogenously determined. This setup eliminates the typical methodological problem of selection on the dependent variable (Fernandez and Weinberg 1997; Granovetter 1995; Montgomery 1992; Obukhova and Lan 2013).

The co-ethnic network size is measured via the number of previous working migrants by nationality (group)[16] as the share of total employment in each district in the year in which the migrant (last) arrived in Germany. Accordingly, the measure varies across origin-country-groups and districts of arrival, and it is fixed, for each migrant, to the value in the year of arrival. We consider working migrants instead of all migrants because information transmission from those employed is likely to be more beneficial for the labor market opportunities of the newcomers. By relating co-ethnic working migrants to the total working migrants in each district of arrival, we account for the potential "easiness" to thwart other co-ethnic groups. Consider a migrant assigned to a hypothetical district with 10 co-ethnics and 100 local residents and another migrant assigned to a district with 10 co-ethnics and 50 local residents. The latter setting can be more advantageous to the migrant than the former, as denser distribution of co-ethnics may facilitate potential access to and the information spreading within the co-ethnic networks.

To calculate the share of working immigrants' (groups) for each specific district and year, we rely on the full registry of employees in Germany (IEB). The number of districts in Germany is 401, with a mean (median) of 65,801 (43,643) workers per district. Our sample of assigned migrants is distributed across 112 districts of first arrival. Our measure of social network has an average size of 0.005, with a standard deviation of 0.007 and maximum of 0.057. The assigned migrants with the highest value of the average co-ethnic network size are those from Western Europe (0.025), followed by the Turkish (0.020) and Southeastern European migrants

(0.017).[17] For the empirical analyses, we standardize the social network variable; it has a mean of zero and a standard deviation of one.

To capture the *use of social contacts*, we use a survey question regarding the search methods that respondents used to find their first job in Germany. Accordingly, for respondents who have started their first job, use of social contacts is coded 1 if they found their first job via friends, acquaintances, relatives, or business relationships and 0 if they used other search methods. For those who had not entered a job by the time of their interview (19 percent), we examine the search methods they used to look for their first jobs. Because each respondent could employ several search methods, we consider the method through which each respondent had the highest expectations of finding a job to be his or her main search method. Hence, for respondents who were still looking for their first job at the time of the interview, 'use of social contacts' is coded 1 if they were looking for a job via friends, acquaintances, relatives, or business relationships and if they had the highest expectations of finding a job via this search method and 0 otherwise. More details about the variable coding strategy can be found in Appendix B.

The data we use do not capture any information about the social network characteristics, such as the networks' employment quality. However, the quasi-experiment ensures that the lack of information about network quality is randomly distributed (as is the case with the distribution of other confounders); hence, omitting this information does not bias our results for the sample of assigned migrants. However, a quasi-experiment such as ours allows for more noise than a controlled laboratory experiment. For this reason, we re-ran our estimates accounting for potential confounders that could lead to variations across the local labor markets the assigned group of migrants encountered at their first arrival in Germany. We control for a rich set of individual time-constant and time-varying characteristics including fixed effects for country-group-of-origin, district of assignment and arrival year that may affect labor market integration

and simultaneously correlate with the size or the use of social networks. This conservative analysis aims to test whether our results are robust to any misspecification or omitted variable bias.

More specifically, we account for gender (*female*), family-related characteristics (*partnership status at arrival* and the time-dependent[18] *number of children*), *age at last migration* (and its squared term), and visa category for entering Germany (asylum-seeker or *refugee*, ethnic German or other type of migrant). We further control for a set of pre- and post-migration human capital characteristics. The former characteristics include *educational attainment, German language proficiency*, *a good math score at school* and *having working experience*. Post-migration human capital characteristics include the time-dependent *new educational degree* and the time-dependent *recognition of foreign educational degree*. To further minimize the possibility of self-selection into migration, we control for the existence of *pre-migration connections in Germany* and for the main *reason to migrate* (grouped into political, family, economic, and other reasons). We also account for the *unemployment rate in Germany in the year before migration* to control for overall economic effects (e.g., the business cycle). An indicator of the *refreshment sample* (see the section *Data and sample*) is included to account for any differences across survey samples. As mentioned above, we further include country-group-of-origin fixed effects and assignment-district fixed effects, which should absorb any systematic differences in any characteristics across countries of origin and economic performance across districts. In the models regarding real hourly wages in the first job, we additionally account for *working fulltime* and *years before entry into the first job in Germany* (and its squared term).

RESULTS

The sociodemographic composition of assigned migrants in Germany

Table 1 provides several descriptives of the labor market integration and sociodemographic information about migrants who were assigned to their first residence place in Germany. Approximately 86 percent of the assigned migrants started their first job in Germany by the time of their interview. It took them, on average, three years to find these jobs, and their mean hourly wage was approximately six euros. In the year of their arrival to Germany, the average size of a migrant's co-ethnic network was 0.01. That is, approximately one percent of working population in each district of assignment consisted of migrants from the same country (group) of origin as the respondent's. Slightly less than half of the assigned migrants relied on social contacts to find their first job in Germany.

– Table 1 –

Turning to the sociodemographic composition of assigned migrants, there are fewer women than men (38 percent), most were married (66 percent) and young (29 years old), and almost half had children at the time of migration. Regarding their origin, 45 percent of migrants came from USSR successor states, 25 percent from Asia and the Middle East, 17 percent from Africa, and 13 percent from other countries (labeled "Western, Eastern, and Southeastern Europe"). In the latter group, 80 percent came from the Western Balkans, and the rest were from new EU member states. Slightly more than half of the assigned migrants arrived in Germany as refugees or asylum-seekers, while 25 percent arrived as ethnic Germans. However, only 39 percent reported that their main reason for migration was politically driven (such as discrimination, distress, persecution, or war), whereas 28 percent claimed to have come for economic reasons and 20 percent for family reasons. Half of the migrants had pre-migration connections to Germany (family or friends who resided in Germany).

Regarding their human capital characteristics, the highest share had a low educational level (60 percent), and only 15 percent had a high educational level. Three percent attained further educational credentials after migration, and four percent received recognition for their home-country education. Approximately 70 percent worked before migration and only seven percent of migrants had good or very good German language proficiency before migration.

Exogeneity between the size of co-ethnic networks and their mobilization by migrants

Before we turn to the test of our main hypotheses, we first corroborate the assumption that the relationship between the access to and the use of co-ethnic networks is exogenous. Recall that one can only claim a causal effect of social networks under this assumption (Mouw 2003). For corresponding empirical inquiry, we examine the relationship between the use of social contacts to find a first job in Germany (versus relying on other methods for job search) and the exogenously "assigned" size of co-ethnic networks. The results are presented in Table 2.

– Table 2 –

Model 1.1 provides a bivariate positive correlation between the two variables of interest. Note, however, that the co-ethnic network size varies across district, country (group) of origin and arrival year. This means that the bivariate positive correlation between co-ethnic network size and the use of social contacts for first job search might be attributed to confounding factors related to district, arrival year, or origin. Consider, for instance, a migrant who uses social contacts for job search and lives in a district characterized by a large co-ethnic group and a minimal presence of (state) job agencies; and another (identical) migrant who does not use social contacts for job search and lives in a district with a smaller co-ethnic group and many (state) job agencies. This means that not only availability of co-ethnic network varies by district but also that of state job agencies. In our example, a bivariate correlation between the use of social contacts for job search and co-ethnic network size would result in a positive relationship.

Yet, given that a minimal presence of (state) job agencies is likely to push individuals to rely on social networks for job search, the positive bivariate correlation between use of social contacts for job search and co-ethnic network size is likely to be spurious. Moreover, some origin groups tend to rely more often on social contacts than others because of cultural differences and attitudes towards activation of social networks (Ma, Huang, and Shenkar 2011; Sharone 2014).

Therefore, to absorb any systematic differences in any characteristics across country (groups) of origin and across district of assignment, we include the corresponding fixed effects in Models 1.2 and 1.3. As these models show, introducing fixed effects eliminates the positive (spurious) correlation between the size of co-ethnic networks and the use of social contacts for job search. In other words, other factors attributable to origin or the district of assignment seem to drive migrants to use social contacts for their job search. Hence, we conclude that the use of social contacts is exogenous to the size of co-ethnic networks. This conclusion holds when we also control for further socio-demographic characteristics of migrants (Model 1.3).

Co-ethnic networks and migrants' transition to their first jobs in Germany

Here, we present our empirical analysis that tests whether variations in the size of co-ethnic networks (our exogenous treatment variable) affect migrants' transition to their first job in Germany. Table 3 outlines the estimation results regarding the determinants of migrants' transition rate to their first jobs. Appendix C provides an overview of model specifications and likelihood ratio tests for the improvement of model fit from model to model. Appendix D includes robustness checks with an alternative sample and model specification.

– Table 3 –

In Model 2.1, we conduct a bivariate test by including our main variable of interest: size of co-ethnic network. What we find is that an increase in the network size by one standard deviation increases the transition rate to the first jobs by nine percent (=exp(0.09)-1). This effect is, however, not statistically significant. Thus, like we hypothesized (*H1*), the size of co-ethnic network is neither statistically nor substantially related to the transition rate to the migrants' first job. In Model 2.2, we add fixed effects for country-group of origin and district of assignment. Although doing so increases the effect of networks in size, such an effect remains statistically non-significant. Hence, having a larger co-ethnic network per se does not accelerate migrant's labor market entry.

In Model 2.3, we introduce the variable 'use of contacts for job search' (versus use of other search methods), and Model 2.4 includes an interaction term between size of co-ethnic network and use of social contacts variables. Likewise, Models 2.5 and 2.6 replicate Models 2.3 and 2.4 by introducing a full array of potential confounders (cf. section *Independent variables and confounders*). By interacting co-ethnic network size with the use of contacts, we are able to test our hypothesis *H1a,* that the size of migrants' networks matters in their propensity to find a job only when migrants mobilize their networks for that purpose. The test is shown in Model 2.6, which also exhibits a superior model fit, and, hence, is our preferred model. Accordingly, when controlling for all model covariates, among those who use social contacts for job searches, an increase in the co-ethnic network size by one standard deviation results in a 6.5-times faster transition rate to the first job (=exp(0.50+1.37)). In contrast, co-ethnic network size does not seem to affect the first job-entry rate among those who do not utilize the networks but use other search methods to find a job. Altogether, these results support our hypothesis *H1a*.

Co-ethnic networks and the hourly wages in migrants' first jobs in Germany

To test whether variations in the size of co-ethnic networks affect migrants' job quality, we model hourly wages as a function of co-ethnic network size, use of social contacts, their interaction term, and a set of controls. The empirical analysis follows the same steps as that for the transition to the first job. The results are presented in Table 4. An overview of model specifications and likelihood ratio tests for the improvement of model fit from model to model can be found in Appendix C. Likewise, Appendix D provides robustness checks with an alternative sample and model specification.

– Table 4 –

Similarly to the result on migrants' transition to their first job and confirming our hypothesis *H1*, the size of co-ethnic networks has no statistically significant effect on migrants' hourly wages (Model 3.1). Accounting for further covariates in Models 3.2, 3.3 and 3.5 increases the goodness of fit of the model but does not change the non-significant effect of co-ethnic network size. Contrary to the result on migrants' transition to their first job and to what we hypothesized (*H1a*), the interaction term between co-ethnic network size and the use of contacts for job search has no significant effect on migrants' hourly wages. Moreover, its inclusion does not increase the goodness of fit of the model (Models 3.4 and 3.6), rendering Model 3.5 our preferred specification. Overall, these results suggest that neither access to co-ethnic networks nor their mobilization for job search increase the quality of migrants' first jobs.

DISCUSSION

A growing body of sociological and economic research on migration stresses the importance of social networks for migrants' labor market integration. In line with social resource theory (Lin 1999), a.k.a., "network" social capital perspective (Mouw 2006), we argue that much of

the estimated effect of social networks on migrants' labor market integration may simply reflect selection effects due to the non-random sorting of migrants into locations in which they have more opportunities for prior or co-ethnic connections. This endogenous residential sorting, in turn, is likely to affect migrants' use of social networks to find a job.

In this study, we address the selection bias issue by using a unique quasi-experimental dataset of refugees and other migrants who were exogenously allocated to their first place of residence by German authorities. This data sampling accounts for the exogenous exposure of both migrant groups to different local labor markets that they encountered in their first place of residence. One such exogenous exposure concerns the size and potential availability of co-ethnic networks in these labor markets. In other words, our respondents' exogenous allocation across different local labor markets ensures that the respondents' network structure with co-ethnics in these labor markets is exogenously determined. Although an assumption can be made that because of the latter, the use of such networks for instrumental purposes – such as faster job entry or higher wages – is not endogenous to their social networks' availability (Mouw 2003), we are able to empirically confirm that indeed, migrants' use of their social contacts is independent of the size of co-ethnic networks.

As a result, this study manages to come closer to a causal test of network social capital on migrants' labor market integration. It does so by examining whether in case there is any effect of social networks on job-related outcomes, that effect is not because of the size of the social network to which one may have access but rather because of the resources embedded in that network that can be used to achieve the desired job-related outcomes.

To identify whether the effect of social networks is universal for different labor market outcomes, we study both the transition rate to and the (real) hourly wages in their first jobs. Our main finding is that having a larger co-ethnic network per se does not accelerate migrant's

labor market entry unless migrants use the social contacts to find a job. In contrast, neither access to, nor use of networks has any effect on migrants' wages in their first job, i.e., job quality. It seems reasonable to conclude that social networks do have a causal effect on migrants' speedy employment in the destination country, but only when the networks and the resources embedded in them are mobilized. Although we are aware that such resources could be related to either information, support and influence that social networks can provide or to signaling one's ability and status (Castilla et al. 2013b), the data does not allow us to distinguish which of these aspects play a role. However, the fact that we do not find an effect on job quality hints at a possible mechanism at play, namely, the job information transmitted through the networks rather than the quality, status or good match. All in all, our findings reinforce the main conclusion of the few studies that rely on experimental data, namely, that there is little evidence of a causal effect of social networks themselves. We thus join Mouw (2003:891) in wondering whether the results from the migration literature "are likely to overestimate networks' role in [migrants] labor market outcomes".

Future research must be more creative in its empirical testing because the problem of selection bias in social network research matters. Our results further indicate that we must broaden our focus of interest: the effect of social networks varies by outcome. Although our study highlights the importance of social networks through their use for migrants' first job entry, more research is needed to understand why such an importance fades away when it comes to the quality of their job and hence unravel the mechanisms that underlie these differences. Moreover, a more direct measure of migrants' actual network size could give further or different insights compared to the currently employed measure of potential network size. Finally, although our study makes a contribution to the European context, we cannot claim that the results are generalizable to other Western countries with diverse migration policies. We encourage more

studies that, like this one, take into account potential selection and endogeneity bias while considering the institutional differences across other European and non-European countries.

NOTES

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[1] Henceforth, the term "refugees" is used colloquially and includes all persons who move to another country for humanitarian reasons, irrespective of their legal status (e.g., refugee, asylum-seeker, or internally displaced person). For simplicity, the term "first-generation migrants" (foreign-born individuals who have immigrated to a new country for non-humanitarian reasons) is referred to as "other migrants". Note that when we use the term "migrants", we refer to all migrants, including both refugees and other migrants.

[2] Mouw (2006) uses Portes's (1998:7) definition of social capital: "the ability of actors to secure benefits by virtue of their membership in social networks or other social structures".

[3] The analytical distinction between the two has proven to be essential, with numerous empirical studies demonstrating in particular the relevance of the use of networks (e.g., Dustmann et al. 2016; Lancee 2016; Lin et al. 1981; Mouw 2003; Sanders et al. 2002; Yakubovich 2005).

[4] For instance, some exogenously driven changes in the propensity to accept a job (e.g., individual life-course events or a changing structure of the local labor market) might drive one to join a social network. Likewise, endogenous changes in the network (death, marriage, and residential mobility) may affect one's propensity to accept a job.

[5] Due to data restrictions, our study does not cover the recent refugee flow to Germany (i.e., those arriving from the fall of 2015 onwards). Therefore, the most recent integration policies and law changes launched from 2015 onwards will not be discussed.

[6] The quota is calculated annually based on the tax revenues and population size of each German Federal State, thereby specifying the shares of refugees, ethnic Germans and Jewish migrants in each.

[7] To check the robustness of our claim that the sample of migrants we look at is exogenously allocated, we replicated our analyses excluding those who migrated after their spouses. The results are robust to these sample restrictions (see Appendix D, Model 2.8 in Table D1 and Model 3.8 in Table D2). As we explain in detail later, arrivals of minor children are excluded per definition since we confine the data to those migrating to Germany at working age.

[8] A more detailed description of the allocation and integration policies for refugees, ethnic Germans and Jewish migrants is presented in Appendix A (see also Schacht and Hartmann 2017; Schneider 2012).

[9] We use the factually anonymous data of the IAB-SOEP Migration Sample Survey Data, wave 1-3. This IAB-SOEP Migration Sample is a joint project of the Institute for Employment Research (IAB) and the Socio-Economic Panel (SOEP) at the German Institute for Economic Research (DIW Berlin). Data access was provided via a Scientific Use File supplied by the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the IAB. DOI: 10.5684/soep.iab-soep-mig.2015. For data documentation, see Brücker et al. (2014).

[10] Previous research reveals that response rates from studies of migrants are lower than those of non-migrants (see Bethlehem, Cobben, and Schouten 2011).

[11] The results do not change substantially after including migrants who arrived before 1989.

[12] The rationale for excluding these individuals is that since they do not plan to work, they are not expected to look for a job and hence will not be exposed to job entry. Moreover, and most likely for these reasons, these individuals were not asked about their job-search method.

[13] Not all respondents had entered their first job in Germany by the time of their interview. This result does not necessarily mean that these individuals will never have a job in Germany. Exclusion of such right-censored cases (i.e., when the end of the episode is not observed) would seriously bias our results. By relying on event-history analyses – the method well suited to capture right-censoring – our analyses consider the information for those who had already begun and those who had not yet begun their first jobs in Germany.

[14] For an even more appropriate analysis of the timing of migrants' first jobs, it would have been more beneficial to consider monthly information, which is not available in the IAB-SOEP Migration data.

[15] Note that here we do not compare the benefits of having co-ethnic networks versus having cross-ethnic networks. Although this comparison could be very interesting, due to data availability we focus our attention only on the resources of having co-ethnic networks.

[16] We follow Battisti et al. (2016) and aggregate nationalities into seven country groups: (1) Western countries, including Western Europe, (2) Eastern Europe, (3) Southeastern Europe, (4) Turkey, (5) USRR, (6) Asia and Middle East, (7) Africa (see also Dustmann et al. 2016; Glitz 2014). The rationale for using country groups instead of single countries is that by using single countries, we would have had many empty cells. That is why we aggregated them by geographic proximity, which is likely to correlate with linguistic and cultural proximity (e.g., Giuliano, Spilimbergo, and Tonon 2006; Melitz and Toubal 2014). More importantly, having single countries would undermine the possibility to estimate the country fixed effects due to lower sample sizes (as per country) in the survey and in the sample of the assigned immigrants, in particular.

[17] In our sample of assigned migrants, there is only one respondent originating from Western countries (Greece), three from Eastern Europe (Poland), and one from Turkey. A replication of our analyses by excluding these migrants does not alter the conclusions (see Appendix D, Model 2.10 in Table D1 and Model 3.10 in Table D2).

[18] Time-dependent variables are used for the analyses of the transition rate to the first job in Germany and are measured for each person-year observation. For analyses of wages in the first job, time-dependent variables are captured in the year of the first job.

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